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AIX 1634 BAR-BA-DOES NEW ENG-LAND
NEW-JER-SEY VIR-GIN-IA OH-IO
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Twenty bokis I-clad in blak or red
Of Aristotle & his philoso-phie
Than robis ryche or fedele or gay sautrie
S. I.
U.S. National Museum
S. I.
Baldwin

NATURE

A WEEKLY

ILLUSTRATED JOURNAL OF SCIENCE

VOLUME II

MAY to NOVEMBER 1870

" *To the solid ground*
Of Nature trusts the mind that builds for aye."—WORDSWORTH

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INDEX

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Authors of papers read at the Liverpool Meeting of the British Association are requested to favour the Editor of "NATURE" *with copies or abstracts of their communications as soon as possible, addressed to him at the Post Office in the Reception Room, as by these means alone can an accurate and early notice be insured.*

The Editor appeals to men of Science to aid him in his attempt to give an account of their investigations to their fellow-workers throughout the world.

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A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

"*To the solid ground*
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One of the leading objects of this periodical is to awaken in the public mind a more lively interest in Science. With this end in view it provides original Articles and Reviews, written by Scientific men, and illustrated when necessary, expounding the grand results of scientific research, discussing the most recent scientific discoveries, and pointing out the bearing of Science upon civilization and progress, and its claims to more general recognition, as well as to a higher place in the educational system of the country. There will also be found in "NATURE" *complete Abstracts of all important Papers communicated to British, American, and Continental Scientific Societies and periodicals, and Reports of the Meetings of Scientific Bodies at home and abroad. In the columns devoted to Correspondence an opportunity is afforded for the discussion* *of scientific questions of general and popular interest. The Journal has now been nearly a year in existence, and having helped to promote many of the objects which all scientific workers have at heart, has, it is hoped, already made good its claim to the support of all who really desire the advancement of Science. Appended is a List of some of the eminent men who have already contributed to the pages of* "NATURE," *or have otherwise aided to establish it.*

ABEL, F. A., F.R.S., *H.M. Chem. Dep. Woolwich.*
AGASSIZ, PROF. L., *Museum of Comparative Zoology, Harvard College.*
ANDREWS, PROF. T., F.R.S. *Queen's Univ. Dublin.*
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BENNETT, A. W., F.L.S.
BERTHELOT, PROF., *Collège de France, Paris.*
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DANA, PROF. J. D. *Newhaven, Conn. U.S.A.*
DARWIN, C., F.R.S.
DAWKINS, W. BOYD, F.R.S. *Owens Coll. Manc.*
DUMAS, J. B. *Sec. Imperial Acad. of Sciences, Paris.*
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FOSTER, PROF. MICHAEL, *Royal Institution.*
FOSTER, PROF. G. CAREY, F.R.S. *Univer. Coll.*
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GALTON, DOUGLAS, F.R.S.
GEIKIE, A., F.R.S. *Geol. Survey of Scotland.*
GORE, G., F.R.S.
GRANT, PROF. R., F.R.S. *Direc. Glasgow Obser.*
GROVE, W. R., F.R.S., Q.C.
HARCOURT, A. VERNON, F.R.S. *Sec. Chem. Soc.*
HAUGHTON, REV. PROF., F.R.S. *Trin. Coll. Dublin.*
HIRST, PROF., F.R.S. *Gen. Sec. British Assoc.*
HOOKER, DR. J. D., F.R.S. *Director Royal Gardens, Kew.*
HUMPHRY, PROF., F.R.S. *Cambridge.*
HUXLEY, PROF. T. H., F.R.S. *Pres. Geol. Soc.*
JACK, PROF. *Owens College, Manchester.*
JEFFREYS, J. GWYN, F.R.S.
JENKIN, PROF. H. C. FLEEMING, F.R.S. *Edinburgh University.*
JEVONS, PROF. W. S., *late of Owens College, Manc.*
JONES, DR. H. BENCE, F.R.S. *Sec. Royal Inst.*
JOULE, J. P., F.R.S.
KÉKULÉ, PROF. *Chemisches Institut, Bonn.*
KINGSLEY, REV. CANON.
LANKESTER, DR. E., F.R.S.
LANKESTER, E. RAY.
LEVI, LEONI, F.S.S.
LEWES, G. H.
LIVEING, PROF. G. D., F.C.S. *Cambridge.*
LUBBOCK, SIR JOHN, BART., F.R.S.
MACMILLAN, REV. H.
MAGNUS, PROF. (*the late*), *Berlin.*
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MURCHISON, SIR RODERICK I., BART., F.R.S. *President Geog. Soc.; Direc. Geol. Survey.*
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OLIVER, PROF. D., F.R.S. *Royal Gard. Kew.*
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QUETELET, DR. A. *Secretary Royal Academy of Sciences Brussels.*
RAMSAY, PROF. A. C., F.R.S. *Geol. Survey.*
RODWELL, G. F., F.C.S.
ROLLESTON, PROF. G., F.R.S. *Oxford.*
ROSCOE, PROF. H. E., F.R.S. *Owens Coll. Manc.*
SCLATER, P. L., F.R.S. *Sec. Zoological Society.*
SHARPEY, DR., *Secretary Royal Society.*
SORBY, H. C., F.R.S.
SPOTTISWOODE, W., V.P.R.S.
STAINTON, H. T., F.R.S.
STEWART, BALFOUR, F.R.S. *Direc. Kew Observatory.*
STONE, E. J., *Royal Observatory, Greenwich.*
STRICKER, PROF. S. *University of Vienna.*

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TRISTRAM, REV. H. B., F.L.S.
TYLOR, EDWARD B., F.R.S.
TYNDALL, PROF. J., F.R.S. *Royal Institution.*
WALLACE, A. R., F.R.G.S.
WILLIAMSON, PROF. A., F.R.S. *Pres. Chem. Soc.*
WILSON, J. M., *Rugby School.*
WOODWARD, H., F.G.S. *British Museum.*
WRIGHT, DR. PERCEVAL, *Dublin University.*

The following are among the Articles which have appeared in "NATURE" *during the last few months:—*

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PROFESSOR HUXLEY, F.R.S.

Dust and Disease.—Pasteur's Researches on the Diseases of Silkworms.
PROFESSOR TYNDALL, F.R.S.

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Lectures to Working Men.—Scientific Education in Germany.
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The Relative Value of Classical and Scientific Training.
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The Medical Schools of England and Germany.
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The Natural Laws of Muscular Exertion.—The Labouring Force of the Human Heart.
REV. PROFESSOR S. HAUGHTON, M.D. F.R.S.

The Temperature and Animal Life of the Deep Sea.
DR. CARPENTER, F.R.S.

The Origin of Blood-letting.
E. R. LANKESTER, F.R.S.

The Polarization of Light.
W. H. S. RUSSELL, F.R.S.

Original Scientific Research in Relation to Employment for Workmen.
GEORGE GORE, F.R.S.

The Physical Constitution of the Sun.
DR. GOULD (*U.S.A.*)

The Snakes of Australia.
DR. GÜNTHER, F.R.S.

Whence come Meteorites?
PROFESSOR N. S. MASKELYNE, F.R.S.

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THURSDAY, MAY 5, 1870

TO OUR READERS

THE opportunity afforded by the commencement of a new volume is one we cannot allow to pass by without a few remarks on the work on which we are engaged, although it may be that such a course is not strictly in accordance with precedent, but our excuse lies in this—our journal is not according to precedent.

For, in fact, six months ago a scientific journal, in which the leaders of scientific thought, in this and other lands, gave week by week an account of their own and others' labours to their fellows and the general public, was a thing of the future, and, in the general opinion, to attempt to start such a journal was almost certain to end in signal failure. "Science is so small, her victories are so few," said some, "that a weekly account of them is altogether beside the question—the well would run dry." Others said: "Science is large, it is true, but her followers are not numerous. You may perhaps number your readers by hundreds, if you take care to appeal to scientific men only; but as for the outside world, they care nothing for science." On the other hand it was held that a popular scientific weekly journal would, be a certain success under certain conditions—some such as these: in the first place, the articles were to be light as air; each fact was to be clothed in a delicate atmosphere of adjective and imagery; next, each page was to be studded with beautiful pictures, correctness both in text and illustration giving way to a certain more or less subdued sensationalism; and lastly, and above all, every care was to be taken to spare the reader the least trouble in the matter of thinking.

We confess that we should have shrunk from our task in the face of such advice as this, had there not been certain Signs of the Times which did not seem difficult to read, and which were more in harmony with the encouragements we received to undertake it; and now that the first volume has been completed, we have the satisfaction of knowing that none of these gloomy forebodings have been realised.

A consideration of the facts brings us at once to our first duty, which is to tender to the scientific men, both at home and abroad, who have assisted us, our best thanks for all their help in the work we have undertaken. We willingly acknowledge the small part we have borne in what has been done. Thanks are due, not only for criticism and the contributions which have already appeared, but for many others which—Nature is so large, and our journal is so small—we have not as yet been able to place before our readers. It has been our endeavour to carry out our programme by making the journal useful to workers in science; worthy therefore of their perusal, and therefore, again, worthy of their contributions: and by thus extending our appeal beyond the limits of the scientific world on the one hand, and endeavouring to keep up the dignity of science herself on the other, we have already met with an encouraging response. Our subscribers now number nearly five thousand; that is, we have, on a moderate estimate, fifteen thousand readers. Though we think this an emphatic success, we shall not be satisfied if the increasing interest in Science, and an increased knowledge of the periodical, do not in a short time double our present circulation, and we trust not only that each worker will urge his neighbours to send us facts, but that each of our present readers will form a nucleus of new ones.

We state this, not only because the statement is almost due to our contributors as a justification of our demands upon their time, but because it indicates the work—we had almost said the noble work—which lies before them. Surely at a time when England would gain so much by the scientific education, not only of her Workmen but of her Ministers, an attempt to place Science before the Public, week by week, as Politics, Art, Music, and a hundred other things are placed before them, must not be suffered to flag; when the number of science-teachers and science-students is daily increasing, and the necessity for combined action and representation among scientific men themselves is being more and more felt, the popularisation of science becomes more important than ever, and every effort to gain these ends deserves a larger encouragement, for the most "practical man" will now soon be made to feel that Science dogs his every footstep, meets him at every turn, and twines itself round his life; nay, it may soon become evident that such a practical thing as a stagnation of trade may in some way be traced to the neglect of science.

Hence our endeavour in the future will be not only to make our journal a necessity in the Studies of the more thoughtful, and in our Schools, but a welcome visitor in the Homes of all who care for aught that is beautiful and true in the world around them.

EDITOR

THE VELOCITY OF THOUGHT

"As quick as thought" is a common proverb, and probably not a few persons feel inclined to regard the speed of mental operations as beyond our powers of measurement. Apart, however, from those minds which take their owners so long in making up because they are so great, rough experience clearly shows that ordinary thinking does take time; and as soon as mental processes were brought to work in connection with delicate instruments and exact calculations, it became obvious that the time they consumed was a matter for serious consideration. A well-known instance of this is the "personal equation" of the astronomers. When a person watching the movement of a star, makes a signal the instant he sees it, or the instant it seems to him to cross a certain line, it is found that a definite fraction of a second always elapses between the actual falling of the image of the star on the observer's eye, and the making of the signal—a fraction, moreover, varying somewhat with different observers, and with the same observer under differing mental conditions. Of late years considerable progress has been made towards an accurate knowledge of this mental time.

A typical bodily action, involving mental effort, may be regarded as made up of three terms; of sensations travelling towards the brain, of processes thereby set up within the brain, and of resultant motor impulses travelling from the brain towards the muscles which are about to be used. Our first task is to ascertain how much time is consumed in each of these terms; we may afterwards try to measure the velocity of the various stages and parts into which each term may be further subdivided.

The velocity of motor impulses is by far the simplest case of the three, and has already been made out pretty satisfactorily. We can assert, for instance, that in frogs a motor impulse, the message of the will to the muscle, travels at about the rate of 28 metres a second, while in man it moves at about 33 metres. The method by which this result is obtained may be described in its simplest form somewhat as follows:—

The muscle which in the frog corresponds to the calf of the leg, may be prepared with about two inches of its proper nerve still attached to it. If a galvanic current be brought to bear on the nerve close to the muscle, a motor impulse is set up in the nerve, and a contraction of the muscle follows. Between the exact moment when the current breaks into the nerve, and the exact moment when the muscle begins to contract, a certain time elapses. This time is measured in this way:—A blackened glass cylinder, made to revolve very rapidly, is fitted with two delicate levers, the points of which just touch the blackened surface at some little distance apart from each other. So long as the levers remain perfectly motionless, they trace on the revolving cylinder two parallel, horizontal, unbroken lines; and any movement of either is indicated at once by an upward (or downward) deviation from the horizontal line. These levers further are so arranged (as may readily be done) that the one lever is moved by the entrance of the very galvanic current which gives rise to the motor impulse in the nerve, and thus marks the beginning of that motor impulse; while the other is moved by the muscle directly this begins to contract, and thus marks the beginning of the muscular contraction. Taking note of the direction in which the cylinder is revolving, it is found that the mark of the setting-up of the motor impulse is always some little distance ahead of the mark of the muscular contraction; it only remains to be ascertained to what interval of time that distance of space on the cylinder corresponds. Did we know the actual rate at which the cylinder revolves this might be calculated, but an easier method is to bring a vibrating tuning-fork, of known pitch, to bear very lightly sideways on the cylinder, above or between the two levers. As the cylinder revolves, and the tuning-fork vibrates, the latter will mark on the former a horizontal line, made up of minute, uniform waves corresponding to the vibrations. In any given distance, as for instance in the distance between the two marks made by the levers, we may count the number of waves. These will give us the number of vibrations made by the tuning-fork in the interval; and knowing how many vibrations the tuning-fork makes in a second, we can easily tell to what fraction of a second the number of vibrations counted corresponds. Thus, if the tuning-fork vibrates 100 times a second, and in the interval between the marks of the two levers we count ten waves, we can tell that the time between the two marks, *i.e.* the time between the setting-up of the motor impulse and the beginning of the muscular contraction, was $\frac{1}{10}$ of a second.

Having ascertained this, the next step is to repeat the experiment exactly in the same way, except that the galvanic current is brought to bear upon the nerve, not close to the muscle, but as far off as possible at the furthest point of the two inches of nerve. The motor impulse has then to travel along the two inches of nerve before it reaches the point at which, in the former experiment, it was first set up.

On examination, it is found that the interval of time elapsing between the setting up of the motor impulse and the commencement of the muscular contraction is greater in this case than in the preceding. Suppose it is $\frac{2}{10}$ of a second—we infer from this that it took the motor impulse $\frac{1}{10}$ of a second to travel along the two inches of nerve: that is to say, the rate at which it travelled was one inch in $\frac{1}{20}$ of a second.

By observations of this kind it has been firmly established that motor impulses travel along the nerves of a frog at the rate of 28 metres a second, and by a very ingenious application of the same method to the arm of a living man, Helmholtz and Baxt have ascertained that the velocity of our own motor impulses is about 33 metres a second.* Speaking roughly this may be put down as about 100 feet in a second, a speed which is surpassed by many birds on the wing, which is nearly reached by the running of fleet quadrupeds, and even by man in the movements of his arm, and which is infinitely slower than the passage of a galvanic current. This is what we might expect from what we know of the complex nature of nervous action. When a nervous impulse, set up by the act of volition, or by any other means, travels along a nerve, at each step there are many molecular changes, not only electrical, but chemical, and the analogy of the transit is not so much with that of a simple galvanic current, as with that of a telegraphic message carried along a line almost made up of repeating stations. It has been found, moreover, that the velocity of the impulse depends, to some extent, on its intensity. Weak impulses, set up by slight causes of excitement, travel more slowly than strong ones.

The contraction of a muscle offers us an excellent objective sign of the motor impulse having arrived at its destination; and, all muscles behaving pretty much the same towards their exciting motor impulses, the results obtained by different observers show a remarkable agreement. With regard to the velocity of sensations or sensory impulses, the case is very different; here we have no objective sign of the sensation having reached the brain, and are consequently driven to roundabout methods of research. We may attack the problem in this way. Suppose that, say by a galvanic shock, an impression is made on the skin of the brow, and the person feeling it at once makes a signal by making or breaking a galvanic current. It is very easy to bring both currents into connection with a revolving cylinder and levers, so that we can estimate by means of a tuning-fork, as before, the time which elapses between the shock being given to the brow and the making of the signal. We shall then get the whole "physiological time," as it is called (a very bad name), taken up by the passage of the sensation from the brow to the brain, by the resulting cerebral action, including the starting of a volitional impulse, and by the passage of the impulse along the nerve of the arm and hand, together with the muscular contractions which make the signal. We may then repeat exactly as before, with the exception that the shock is applied to the foot, for instance, instead of the brow. When this is done, it is found that the whole physiological time is greater in the second case than in the first; but the chief difference to account for the longer time is, that in the first case the sensation of the shock travels along a short tract of nerve (from the brow to the brain), and in the second case through a longer tract (from the foot to the brain). We may conclude, then, that the excess of time is taken up by the transit of the sensation through the distance by which the sensory nerves of the foot exceed in length those of the brow. And from this we can calculate the rate at which the sensation moves.

Unfortunately, however, the results obtained by this method are by no means accordant; they vary as much as from 26 to 94 metres per second. Upon reflection, this is not to be wondered at. The skin is not equally sentient in all places, and the same shock might produce a weak shock (travelling more slowly) in one place, and a stronger one (travelling more quickly) in another.

Then, again, the mental actions involved in the making the signal may take place more readily in connection with sensations from certain parts of the body than from others. In fact, there are so many variables in the data for calculation that though the observations hitherto made seem to show that sensory impressions travel more rapidly than motor impulses (44 metres per second), we shall not greatly err if we consider the matter as yet undecided.

By a similar method of observation certain other conclusions have been arrived at, though the analysis of the particulars is not yet within our reach. Thus nearly all observers are agreed about the comparative amount of physiological time required for the sensations of sight, hearing, and touch. If, for instance, the impression to be signalled be an object seen, a sound heard, or a galvanic shock felt on the brow, while the same signal is made in all three cases, it is found that the physiological time is longest in the case of sight, shorter in the case of hearing, shortest of all in the case of touch. Between the appearance of the object seen (for instance, an electric spark) and the making of the signal, about $\frac{1}{5}$; between the sound and the signal, $\frac{1}{6}$; between the touch and signal, $\frac{1}{7}$ of a second, is found to intervene.

This general fact seems quite clear and settled; but if we ask ourselves the question, why is it so? where, in the case of light, for instance, does the delay take place? we meet at once with difficulties. The differences certainly cannot be accounted for by differences in length between the optic, auditory, and brow nerves. The retardation in the case of sight as compared with touch may take place in the retina during the conversion of the waves of light into visual impressions, or may be due to a specifically lower rate of conduction in the optic nerve, or may arise in the nervous centre itself through the sensations of light being imperfectly connected with the volitional mechanism in the brain put to work in the making of the signal. One observer (Wittich) has attempted to settle the first of these questions by stimulating the optic nerve, not by light, but directly by a galvanic current, and has found that the physiological time was thereby decidedly lessened; while conversely, by substituting a prick or pressure

* Quite recently M. Place has determined the rate to be 53 metres per second. This discordance is too great to be allowed to remain long unexplained, and we are very glad to hear that Helmholtz has repeated his experiments, employing a new method of experiment, the results of which we hope will soon be published.

on the skin for a galvanic shock, the physiological time of touch was lengthened. But there is one element, that of intensity (which we have every reason to think makes itself felt in sensory impressions, and especially in cerebral actions even more than in motor impulses), that disturbs all these calculations, and thus causes the matter to be left in considerable uncertainty. How can we, for instance, compare the intensity of vision with that either of hearing or of touch?

The sensory term, therefore, of a complete mental action is far less clearly understood than the motor term; and we may naturally conclude that the middle cerebral term is still less known. Nevertheless, here too it is possible to arrive at general results. We can, for instance, estimate the time required for the mental operation of deciding between two or more events, and of willing to act in accordance with the decision. Thus, if a galvanic shock be given to one foot, and the signal be made with the hand of the same side, a certain physiological time is consumed in the act. But if the apparatus be so arranged that the shock may be given to either foot, and it be required that the person experimenting, not knowing beforehand to which foot the shock is coming, must give the signal with the hand of the same side as the foot which receives the shock, a distinctly longer physiological time is found to be necessary. The difference between the two cases, which, according to Donders, amounts to $\frac{66}{1000}$, or about $\frac{1}{15}$ of a second, gives the time taken up in the mental act of recognising the side affected and choosing the side for the signal.

A similar method may be employed in reference to light. Thus we know the physiological time required for any one to make a signal on seeing a light. But Donders found that when matters were arranged so that a red light was to be signalled with the left hand and a white with the right, the observer not knowing which colour was about to be shown, an extension of the physiological time by $\frac{154}{1000}$ of a second was required for the additional mental labour. This of course was after a correction (amounting to $\frac{0}{1000}$ of a second) had been made for the greater facility in using the right hand.

The time thus taken up in recognising and willing, was reduced in some further observations of Donders, by the use of a more appropriate signal. The object looked for was a letter illuminated suddenly by an electric spark, and the observer had to call out the name of the letter, his cry being registered by a phonautograph, the revolving cylinder of which was also marked by the current giving rise to the electric spark.

When the observer had to choose between two letters, the physiological time was rather shorter than when the signal was made by the hand; but when a choice of five letters was presented, the time was lengthened, the duration of the mental act amounting in this case to $\frac{170}{1000}$ of a second.

When the exciting cause was a sound answered by a sound, the increase of the physiological time was much shortened. Thus, the choice between two sounds and the determination to answer required about $\frac{50}{1000}$ of a second; while, when the choice lay between five different sounds, $\frac{88}{1000}$ of a second was required. In these observations two persons sat before the phonautograph, one answering the other, while the voices of both were registered on the same revolving cylinder.

These observations may be regarded as the beginnings of a new line of inquiry, and it is obvious that by a proper combination of changes various mental factors may be eliminated and their duration ascertained. For instance, when one person utters a sound, the nature of which has been previously arranged, the time elapsing before the answer is given corresponds to the time required for simple recognition and volition. When, however, the first person has leave to utter any one, say of five, given sounds, and the second person to make answer by the same sound to any and every one of the five which he thus may hear, the mental process is much more complex. There is in this case first the perception and recognition of sound, then the bare volition towards an answer, and finally the choice and combination of certain motor impulses which are to be set going, in order that the appropriate sound may be made in answer. All this latter part of the cerebral labour may, however, be reduced to a minimum by arranging that though any one of five sounds may be given out, answer shall be made to a particular one only. The respondent then puts certain parts of his brain in communication with the origin of certain outgoing nerves; he assumes the attitude, physical and mental, of one about to utter the expected sound. To use a metaphor, all the trains are laid, and there is only need for the match to be applied. When he hears any of the four sounds other than the one he has to answer, he has only to remain quiet. The mental labour actually employed when the sound at last is heard is limited almost to a recognition of the sound, and the rise of what we may venture to call a bare volitional impulse. When this is done, the time is very considerably shortened. In this way Donders found, as a mean of numerous observations, that the second of these cases required $\frac{75}{1000}$ of a second, and the third only $\frac{39}{1000}$ over and above the first. That is to say, while the complex act of recognition, rise of volitional impulse, and inauguration of an actual volition, with the setting free of co-ordinated motor impulses, took $\frac{75}{1000}$ of a second, the simple recognition and rise of volitional impulse took $\frac{39}{1000}$ only. We infer, therefore, that the full inauguration of the volition took $\frac{75-39}{1000}=\frac{36}{1000}$. In rough language, it took $\frac{1}{28}$ of a second to think, and rather less to will.

We may fairly expect interesting and curious results from a continuation of these researches. Two sources of error have, however, to be guarded against. One, and that most readily appreciated and cared for, refers to exactitude in the instruments employed; the other, far more dangerous and less readily borne in mind, is the danger of getting wrong in drawing averages from a number of exceedingly small and variable differences.

M. FOSTER

CHOICE AND CHANCE

Choice and Chance. By the Rev. Wm. Allen Whitworth, M.A., Fellow of St. John's College, Cambridge. 2nd ed. Enlarged. 1870. (Deighton, Bell & Co.)

WE should think that not a few copies of the first edition of this work must have been purchased under the impression that it was an interesting story; and it is surprising that so neat and suggestive a title had not been long ago appropriated by some needy novelist. This work, however, is a very able elementary treatise on those puzzling branches of mathematics which treat of combinations permutations, and probabilities. The earlier chapters are quite within the comprehension of a schoolboy with a moderate knowledge of arithmetic; the appendices, which treat of distributions, derangements, the disadvantage of gambling, and a proof of the Binomial Theorem, founded purely on the doctrine of combinations, require some knowledge of algebra in the reader. So great is the clearness with which Mr. Whitworth states and explains the problems throughout, that it is almost impossible to misunderstand him. The appendix in which the disadvantage of gambling is demonstrated is very interesting, and often novel; and his explanation of the Petersburg problem is the most satisfactory which we have met.

Our only regret concerning the work is that Mr. Whitworth has not attempted more. Though the doctrines of combinations and probabilities lie at the basis of all mathematical and physical science, their value is chiefly theoretical, and it is hardly likely that time can be spared for their study in a school education. Had Mr. Whitworth enlarged his work so as to make it a pretty complete handbook of the theory of probabilities, he would have performed a great service to science. It is strange how little attention has been paid at Cambridge to the theory of probabilities. If we except Mr. Todhunter's valuable history, and Mr. Airy's special work upon its application to observations, we cannot call to mind any recent separate work devoted to rendering the subject of probabilities accessible to students. Mr. De Morgan's article in the Encyclopædia Metropolitana, his excellent work in the Cabinet Cyclopædia, the Useful Knowledge Society's essay, Galloway's treatise, and the translations of Quetelet's work, are what we have to depend upon as introductions to the subject; but they are all twenty or thirty years old at least, and difficult to meet with. Mr. Venn's logic of chance, being purely metaphysical, is not to be counted. We wish that Mr. Whitworth, or some mathematician at once as able, and possessed of as clear a style of exposition, would fill this gap in mathematical literature by producing a student's handbook of probabilities, including the theory of errors, the method of least squares, &c., with some of the applications to practice.

W. S. JEVONS

OUR BOOK SHELF

F. Hoppe-Seyler. Handbuch der physiologischen u. pathologischen Analyse. Third edition. (Berlin, 1870.)

WHILST modern chemical literature is abundantly supplied with publications on the analysis of mineral substances, works on the methods of chemical investigation of the products of animal life are comparatively few. Physiological chemistry is still in its infancy. By far the greatest number of the substances occurring in the animal body have as yet to be discovered, and even those already known exhibit but in few instances such characteristic reactions as serve for their detection and quantitative estimation equal in reliability to those we find in mineral chemistry. But however incomplete the analytical methods of the physiological chemist may be, they are highly valuable, not merely from a scientific, but also from a practical point of view, inasmuch as they aid the physician in the detection of those important changes in the chemical composition of animal fluids and excreta, which almost invariably accompany certain forms of disease. The scientific man as well as the medical practitioner will, therefore, take an equal interest in the re-publication in an enlarged form of a work on the application of chemical analysis to physiology and pathology, which has proved very valuable in its former editions.

The "Handbook" of Mr. Hoppe-Seyler's is adapted to the use of the advanced medical student as well as of the physician. That part of the book treating on the analysis, properly speaking, of animal fluids, tissues, &c., is preceded by some very useful chapters on the employment of chemical and physical apparatus; on re-agents and the mode of ascertaining the purity of the same; and on the composition, the properties, and detection of inorganic and organic chemical compounds occurring in the animal body. The great attention paid to the optical properties, of the various substances occurring in the body to the methods of their examination by means of the polariscope and spectroscope, forms a very remarkable and important feature of the book. Physiological chemistry claims a large share of the results which natural science owes to the application of these instruments, and a more extensive use of optical methods of research will certainly lead to further important discoveries. The author does not include the analysis of gaseous products, nor does he give an account of the methods used for the detection of poisons. The detection of blood-spots on wood, cloth, &c., is treated in an appendix. A chromolithograph, representing the spectra of the alkali metals, the absorption bands of hæmoglobine, and various tables and engravings, contribute to the usefulness of the work.

B. FINKELSTEIN

Search for Winter Sunbeams in the Raviera, Corsica, Algiers, and Spain. With numerous illustrations. By Samuel S. Cox. (London: Sampson Low, Son, and Marston. New York: D. Appleton and Co.)

THIS interesting book will be welcome to those who are seeking to find a home in a sunnier clime than our own The author points out the beauties and the medicinal qualities of the south. In his preliminary chapter he explains the title, "Sunbeams," giving the functions of light, music of light, analogy between light and sound, speaking especially of the life-giving power of the golden sunbeam. Quoting Prof. Maury's thoughts on light, he says, "that the organs of the human ear are so ordered that they cannot comprehend colour any more than the eyes can see sounds; yet, that we may hear over again the song of the morning stars, for light has its gamut of music! The high notes vibrate with the violet of the spectrum, and the red extremity sounds the bass; and though the ear may not catch the song that the rose, lily, and violet sing, it may, for aught we know, be to the humming-bird the butterfly, and the bee, more enchanting than that which 'Prospero's Ariel' sung to the shipwrecked mariner."

The author rapidly describes the well-known winter resorts, Nice, Mentone (of which, with its lovely flowers and fruits, he draws a most inviting picture), Monaco, with its roulette table, myths, and beautiful scenery; then comes Corsica, its chief town, Ajaccio, being renowned as the birthplace of Napoleon. Many interesting facts are here given of his mother, Madame Letitia, with incidents of his boyhood. The author then proceeds to Africa, passing through Algiers, visits the Kabyle people and Arabs, giving a description of the Blidah orange orchards, Algerine desert, the magnificent cedars and oaks on Mount Atlas, the Arab and Moorish women, different interesting old tombs, mosaics, and inscriptions. Our author travels on to Spain and compares it with Algiers.

Arrived at Murcia he witnesses a bull-fight, then he visits the Alhambra with its graceful architecture; *en route* for Madrid he passes many curious towns and castles. The following is a description of one:—"A mist obscured the mountains above. That old Moorish castle near the hill of the Pharos is called the Alcazaba. Its Puerta de la Cava is renowned, if not in history, in legend, as the scene of the suicide of Count Julian's daughter, whose woes brought on the Moorish invasion, and whose Iliad has been sung in prose by Irving. This castle is hid under a veil, even as Irving dropped over its rigid outlines the drapery of his genius. . . . The mist lifts a little. We see a streak of sunlight on a bleak, bright mountain ahead of us. We pass by gardens of immense fig-trees. The mountains begin to shine white. We are in the vine-hills again. . . . Cactus, oleander, orange, and pomegranate—all these appear." He then passes through the Basque country, St. Sebastian, and Biarritz, which the author considers "the very pearl of a summer resort." The work is ended with a fable recorded by Ford: "When San Ferdinand captured Seville from the Moor and bore the conquest to heaven, the Virgin desired her champion to ask from the Supernal Power any favour for Spain. The King asked for a fine climate and sweet sun: they were conceded. For brave men and beautiful women: conceded. For oil, wine, and all the fruits and goods of this teeming earth. This request was granted. 'Then will it please the beauteous Queen of Heaven to grant unto Spain a good Government?' 'Nay, nay, that can never be. The angels would then desert heaven for Spain!'"

The book is plentifully interspersed with good illustrations.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Sources of the Nile

FROM Mr. Keith Johnston's communication to NATURE of the 14th April, it appears that he agrees with me in opinion—though quite independently of me and by a different process of reasoning—that the great river Kassabi, Kassavi, or Kasai, of South-Western Africa, instead of flowing to the north and north-west, as it has hitherto been shown to do in all maps, has its course north-eastward as far as about the meridian of 27°30′ east of Greenwich, where it is joined by the river system of the Chambeze. Such being the case, the only material question between us is with respect to the lower course of the united stream of the two rivers, which Mr. Johnston carries round by a sharp curve to the north-west and west, so as to join the Zairé or Congo river, whereas I regard it as continuing northwards, and uniting with the Albert Nyanza, so as to form the upper course of the Nile.

While thus disputing the claim of the Kassavi and Chambeze to be the head-streams of the Nile, Mr. Keith Johnston advocates the rival claim of "the feeders of Lake Liemba." I do not, however, understand him to mean that these rivers, four in number, are to be considered the head-streams of the main body of the Nile, or to be anything but tributaries of that river, as is, in fact, shown in the map accompanying his paper. It being upon this point that the whole difference between us really hinges, I beg to be allowed to offer the following observations on the subject.

Dr. Livingstone, the discoverer of Lake Liemba, describes it as lying at an elevation of 2,800 feet above the sea, on the northern slope of the Balungu upland, in a hollow with precipitous sides 2,000 feet down, and as going away in a river-like prolongation, two miles wide, N.N.W., to Tanganyika, of which he rightly considers it to be an arm; the difficulty with respect to the elevation of the latter having been removed by Mr. Findlay, who makes it to be 2,800 feet, or the same as that of Lake Liemba.

Captain Burton, the discoverer of Tanganyika, says respecting this great lake: "The general formation suggests, as in the case of the Dead Sea, the idea of a volcano of depression—not like the Nyanza or Ukerewe formed by the drainage of mountains. Judging from the eye, the walls of this basin rise in an almost continuous curtain, rarely waving and infracted, to 2,000 or 3,000 feet above the water-level;" from which description it is evident that Liemba and Tanganyika are portions of one continuous fissure or "crack" in the table-land, of which table-land the elevation is 5,000 feet, or perhaps more, above the ocean.—Dr. Livingstone says 4,000 to 6,000 feet, sloping towards the north and west, but he had not seen any part of it under 3,000 feet of altitude. Further, Lake Tanganyika is described by Dr. Livingstone as passing northwards, by a river named Loanda, into Lake Chowambe, which lake he identifies with Sir Samuel Baker's Albert Nyanza.

The last-named expanse of water was found by its discoverer to have an elevation of 2,720 feet above the ocean, with a precipitous cliff of 1,200 to 1,500 feet on the east shore, whilst on the opposite side the faint blue mountains rose about 7,000 feet above the water-level.

The Albert Nyanza is, however, in nowise a continuation of the system of which Liemba and Tangányika form parts; for whilst the direction of these two lakes is from north to south, or nearly so, the general bearing of the Albert Nyanza is from about north-east to south-west; so that, as is shown on Mr. Keith Johnston's map, the former joins the latter at an angle of 45°; whilst the main body of water extends probably a hundred miles beyond the junction, or to about the 28th meridian. And the Albert Nyanza does not terminate here; for, in the latitude of Karagwe, between 1° and 2° S., it was said by the natives to turn to the west, in which direction its extent was unknown even to Rumanika, the King of Karagwe.

Quite independently, then, of the question of the junction of the joint stream of the Kassavi and Chambeze with the Nile, there is this preliminary question, which I would propound for Mr. Keith Johnston's consideration and answer:—Where would he place, even if only conjecturally, the head of this unknown western extension of the Albert Nyanza?—or, in other words, where would he trace the western limits of the Upper Nile basin?

As regards his objection to "the northward wall-like continuation of the Mossamba mountains on the 20th meridian to beyond the equator," shown in my sketch-map of the Upper Nile basin, in Part xv. (for March 1st last) of the "Illustrated Travels," I must explain that the same is so marked merely conjecturally, and that I do not think of maintaining it against—I will not say proof—but any reasonable argument. In my former maps of 1849, 1859, and 1864, I placed the conjectural western limits of the basin of the river somewhere about that meridian on or near the equator, thence continuing to about 10° N. lat., where the line was made to curve inwards towards the valley of the river. When I found the Kassavi, which I look on as the head-stream of the Nile, actually rising in the Mossamba mountains, on about the meridian thus indicated, I naturally extended my conjectural limits of the basin of the river along the same meridian from the equator southwards. But I repeat that, beyond what we actually know, all the rest is purely conjectural. If there is reason to carry the limits of the basins of the rivers of the West Coast of Africa to the east of the 20th meridian, on the equator or even as far south as the fifth parallel of south latitude, I have nothing to object to it, except that care must be taken to leave sufficient space for the western flank of the basin of the River Nile.

And this brings me to what I regard as an insurmountable objection to Mr. Keith Johnston's hypothesis. By causing, as he does, the Kassavi and Chambeze, after their union on the meridian of 27° 30′, to make a curve round to the north-west and west, so as to form the main-stream of the Congo River, he actually brings the course of this supposed river within 150 miles of the south-western extremity of the Albert Nyanza, as laid down on his own map. But to enable him to do this, he must, in defiance of Sir Samuel Baker's authority, deny the great westerly extension of this immense body of water; and by closing it up in that direction he renders it merely a "back-water" to Tanganyika, as Captain Speke imagined it to be to his Victoria Nyanza, instead of its being the main-stream of the Nile. Mr. Keith Johnston evidently has misgivings on this head, for he says: "If, however, the Albert Nyanza prove to have a great south-westerly extension, this one difficulty would be removed,"—namely, the sole difficulty in the way of the junction of the Kassavi and Chambeze with the Nile, for which I contend.

The argument founded on the comparative levels of Lakes Moero and Tanganyika I fail to appreciate. If the upland, 2,000ft. below which Liemba and Tanganyika lie, has a general elevation of 5,000ft., the waters of Lake Moero, instead of passing as they do through the crack in the mountains of Rua, could not by any possibility unite with those of Tanganyika, except by means of a similar crack in the mountains forming the western side of the latter; unless, indeed, Moero were supposed to lie on the upper level, to which supposition Mr. Johnston's argument is diametrically opposed.

In support of my own argument that the united stream of the Kassavi and the Chambeze continues northwards to join the Nile, instead of turning round to the north-west and west to join the Congo, I have really nothing to add. My opponent himself carries the joint stream for me to within 150 miles of the known south-western extremity of the Albert Nyanza: it is for him to show how the two are to be prevented from uniting. But in doing so he must bear in mind that the waters of Ulenge, made by him to be the recipient of the Kassavi, as well as of the Chambeze, are said by some of Livingstone's native informants to flow N.N.W. into Chowambe or Albert Nyanza, and that for the westerly extension of this body of water we have the authority of Baker.

On the subject of the Congo I have little to say. If it should be found that north of the fifth parallel of south latitude the basin of that river requires to be carried further eastward than the 20th meridian, I see no objection to it. Only I am bound to remark, that I do not consider there is any warrant for representing the Congo as a river having a low alluvial valley extending some 500 or 600 miles inland. I have not examined the subject of this river very closely, but my impression is, that, like the other rivers of the West Coast of Africa south of the equator, the rise of the level of its bed is rapid, and that it becomes considerable within a short distance from the ocean; so that there would not be sufficient fall for the waters of Ulenge in 27°30′ E. long. to join it. And, further, I cannot but entertain the opinion that the volume of water of the Congo has been greatly over estimated; in support of which opinion I will cite the following passages, in pages 147 and 148, of Captain Tuckey's Narrative:—"At the further end of the banza we unexpectedly saw the fall almost under our feet, and were not less surprised than disappointed at finding, instead of a second Niagara, which the description of the natives and their horror of it had given us reason to expect, *a comparative brook bubbling over its stony bed* . . . . The principal idea that the fall creates is, that the quantity of water which flows over it is by no means equal to the volume of the river below it; and yet, as we know there is not at this season a tributary stream sufficient to turn a mill below the fall, we can hardly account for this volume, unless we suppose, as Dr. Smith suggests, the existence of subterranean communications or caverns filled with water." Does this look like the lower course of the supposed second great river of Africa, with a basin of which the area is estimated to measure 800,000 square miles?

On a reconsideration, then, of the whole subject, I see no reason whatever to go from the opinion I have expressed, that the rivers Kassavi and Chambeze unite to form the upper course of the Albert Nyanza; that is to say, the main stream of the Nile; and as the former of those two rivers has the more direct course, and its source is the most remote of all, it is entitled to the honour which I claim for it, of being the hitherto undiscovered head of the great river of Egypt.

Bekesbourne, April 22 CHARLES BEKE

P.S., April 26.—Since the foregoing was written, Mr. Keith Johnston has obligingly sent me a copy of his "Map of the Lake Region of Eastern Africa," with notes, just published, in which I find a categorical answer to my question respecting the western limits of the Upper Nile Basin. He traces them as coming from the south of Lake Liemba and its feeders, and running close along the western side of Tanganyika as far as its northern end, where he gives them a curve to the westward not more than sufficient to include the south-western of the Albert Nyanza, and thence continuing along the high mountains on the west side of that body of water, the westerly extension of which, reported by Baker, he ignores entirely. To these views I need not reiterate my objections. C. B.

Why is the Sky Blue?

CAN any of your readers inform me why the sky is blue? Is it that the predominant colour of sunlight being orange, the regions devoid of sunlight appear of the complementary colour? If so, the planets of Sirius and Vega would have a black sky, those of Betelgeux a green sky, while those of the double stars would have different coloured skies at different times, according to their position with respect to their two luminaries. Or again, is the blueness merely the colour of our atmosphere, as Prof. Tyndall's experiments have led some to believe? In favour of the former explanation, is the fact that the maximum intensity of the light of the solar spectrum is in the orange, and indeed that the sun *looks* orange, and if we close our eyes after gazing a moment at him when high up in the sky, we see a blue image. When the sun is low, his colour changes from orange to red, and this would explain the green tints so often seen in the cloudless parts of the sky at sunset. Possibly Mr. Glaisher, who has seen the sky through a thinner stratum of air than most of us, could help us to a solution. H. A. N.

Hampstead, April 24

Curious Facts in Molecular Physics

SOME of the phenomena of photography present features of a very curious nature, yet seem to be very little known to philosophers who devote their time to researches in molecular physics. For instance, when a glass plate coated with collodion containing an iodide—say iodide of cadmium—is dipped into a "bath" solution of nitrate of silver, strength twenty-five grains to the ounce, in from three to four minutes a good dense precipitate of yellow iodide of silver is formed in the spongy collodion film, and the plate is ready for photographic use. But, let a plate be covered with collodion containing bromide of cadmium, (ten grains to the ounce) instead of iodide of cadmium, an immersion of ten or fifteen minutes is necessary to obtain a good film of bromide of silver, though the collodion skin upon the glass surface is only of the same thickness as in the former instance, and not only is this much longer immersion necessary, but the nitrate of silver solution must be increased in strength to about sixty grains to the ounce to get the best results. When the strength of the nitrate of silver is only twenty-five grains to the ounce, the bromide of silver forms more on the surface of the collodion than within it, and sometimes breaks away in scales from the collodion, and falls to the bottom of the bath.

Lastly, let chloride of cadmium be used instead of the bromide in the collodion, the strength of the nitrate of silver must be increased to about one hundred grains to the ounce of water, and an immersion of thirty or forty-five minutes is necessary to get a good photographic precipitate of chloride of silver. In this case, when a weak nitrate of silver solution is used, an uneven precipitate is formed upon the plate, and the tendency to burst out of film in scales is seen as in the former instance.

The three kinds of films just described vary in their photographic properties. The iodide of silver film requires the shortest exposure in the camera to produce a good picture, the bromide of silver film requires a longer exposure, and the chloride of silver film requires the most prolonged exposure to light of all.

Again, the iodide of silver film is more liable than the others to spots and markings, when there are particles of dust or other impurities on the glass plate or in the solution used; bromide of silver is not nearly so delicately sensitive to such disturbing influences; the chloride of silver film is even less sensitive in this respect than the bromide surface.

The reason of the differences of time of exposure just mentioned may possibly be accounted for on the supposition that chlorine binds itself to silver with more force than is exerted by bromine, and that the atom of bromine clings to the atom of silver with more tenacity than iodine clings to the metal. Hence the waves of light have more work to do in beating chlorine from silver than in beating iodine from silver. One very beautiful experiment, first made by Mr. M. Carey Lea, of Philadelphia, tends to prove that light will widen the distance between the hypothetical swinging atoms of iodine and silver, and that in darkness the atoms, with their attraction for each other thus partially overcome, will gradually fall together again. He prepared a film of absolutely pure dry iodide of silver, upon a glass plate, which film in the process of preparation had not been allowed to come into contact with the slightest trace of organic matter, in the washing water, or by any other means. On exposing such a film to light under a negative, and then applying what is known as the "alkaline developer," a picture came out; but if instead of developing the picture, the exposed plate were allowed to rest a day or two in the dark, the latent image died out, the film, so far as is known, returned to its primitive condition, and on exposure under another negative, a picture from it could be brought out, with no trace of the image impressed for a time through the first negative. The alkaline developer seems to "drink up" the iodine where its cohesion to the silver is loosened, thereby leaving a dark deposit of metallic silver, but where the light has not somewhat beaten the atoms asunder, the developer has no action, unless its strength be increased till it blackens the whole plate, whether the light has acted upon the film or not. The alkaline developer consists of a weak solution of pyrogallic acid, rendered alkaline by the addition of a few drops of carbonate of soda.

This is but one instance among many of the facilities offered by photographic phenomena to those who are trying to peer into the penumbral philosophical region of molecular physics.

WILLIAM H. HARRISON

SIR EDWARD SABINE'S CONVERSAZIONE

WHATEVER may be said by those to whom the grapes are sour, the gathering which met at Burlington House on April 23 to greet the President of the Royal Society, under animating circumstances, can hardly fail of beneficial results, whether regarded from the social, the moral, or the scientific point of view. It would not be easy to devise a happier way of bringing novelties at once under practical criticism—of making the outliers of science acquainted with the centre, of enabling investigators to compare operations and discuss facts and speculations, and of giving occasion for renewal of intercourse and removal of misunderstandings.

As usual, the range of articles exhibited was wide enough to include different branches of science, from astronomy to natural history, and from electro-magnetism to physiology, with achievements of fine art, and of arts mechanical. In an exhausted hydrogen tube placed across the poles of an electro-magnet, Mr. C. F. Varley produced a beautiful luminous arc, the dimensions of which he could vary at pleasure by a change in the size of the negative pole, and occasion a change of direction by a slight elevation of one end of the tube.

Spectroscopy, as we have more than once had occasion to record, owes much to the constructive skill of Mr. Browning. We shall return, on a future occasion, to his new automatic spectroscope.

Mr. C. W. Siemens's Electrical Resistance Pyrometer well maintains the reputation of the inventor for application of philosophical principles to mechanical uses. It is the very salamander of pyrometers, and will measure the temperature of the most highly heated fiery furnace; which must render it indispensable in operations where intense heat is required, and to all experimentalists who know the imperfections of the pyrometer in ordinary use. The construction of the new instrument is based on the physical fact that the resistance of pure metals to the electric current increases with increase of temperature in a simple absolute ratio. A platinum wire of known resistance is coiled upon a small cylinder of fireclay, and is covered by a tube of the same metal, which protects the wire from the destructive action of flame, without preventing access of heat. Thus constructed, the pyrometer is placed in the furnace, and is connected by wires with a Daniell's battery of two cells, and with a compact Resistance-measurer, specially devised by Mr. Siemens, on which the observer makes observations at his ease. As the fire burns, the electrical resistance of the platinum coil rapidly increases, communicates its progress to the measurer on which the indications of temperature may be read off as entirely trustworthy, even up to the melting point of platinum. The importance of such an instrument as this cannot fail to be recognised by practical men, whether among natural philosophers or workers in the pyrotechnic arts; and, for our part, we cordially welcome this new pyrometer as a logical sequence from the inventor of the regenerative gas-furnace with its fierce heat-producing capabilities.

Mr. Jerry Barrett, who relieves his hours at the easel with natural philosophy, exhibited an auxiliary air-pump, which appears to produce that essential desideratum, a perfect Torricellian vacuum. To an ordinary air-pump he attaches an air-chamber or reservoir, and, communicating therewith, two cylindrical glass vessels charged with mercury, and connected by a V tube. On working the pump the pressure of the air in the lower vessels compels the mercury to rise and fill the upper one, in which an ingeniously contrived platinum valve plays an important part. By continuing the process of filling and emptying (the details of which are not easy to describe), the desired vacuum is eventually obtained, and the exhausted tube on the top of the pump is ready for experiment. We learn that a well-known experimentalist was so favourably impressed by the capabilities of this pump that he intends to have a number of large tubes made for a series of experiments.

In these days of busy telegraphy, Mr. J. Parnell's new secondary battery is worth attention. It is so constructed as to be capable of a large amount of heavy work, having forty cells, each containing a pair of copper plates immersed in a solution of the impure carbonate of sodium, known in commerce as "soda." By this employment of an alkali, the electromotive force produced is supposed to depend on the electrolytic reduction of the sodium. The battery is arranged in ten compound cells of four couples each, and is charged by a small battery of five Grove cells, and after the connection has been established for a few seconds, a commutator of peculiar construction is brought into play, and excites the whole forty cells to activity. It is thought that a battery so constructed, which can be energised at pleasure by a brief communication with the small Grove, will be found of service in telegraphing through lines of great resistance.

Rear-Admiral Inglefield's contrivance for making the water in which a ship floats do the work of steering appeals to every Englishman, for are we not all interested in our navy, whether Royal or commercial? It is a contrivance which involves a large economy, for instead of a number of men labouring at two wheels, and with relieving tackles, it requires one small wheel only, and one man to steer the largest ship afloat; estimating roughly the pressure of the water as half a pound per square inch for every foot of the ship's draught. Admiral Inglefield admits the water through the bottom by a "Kingston valve" into a cylinder placed at the stern. The piston of this cylinder works a double-action force-pump, which sends the water to two hydraulic cylinders; these are connected with a tiller four feet in length, and thus, by movements of the small steering wheel, the ship is easily steered. Trials made with this apparatus on board the *Achilles*, one of the largest vessels in the navy, proved satisfactory; and in an improved form it is to be fitted to the *Fethi Bulend*, a corvette now building for the Turkish Government. Unfortunately for the visitors to the conversazione, the model exhibited, owing to lateness of delivery, could not be shown in work; but there was a skeleton of the corvette's stern, showing the position of the apparatus, and near it stood one of the small steering-wheels. By this and Admiral Inglefield's explanations, the naval men present could form an opinion of the new method, compare it with the existing method, and mark how surely the helm could be kept hard over during full speed, and how rapid and easy were its movements generally.

Mr. J. B. Rogers exhibited his life-saving apparatus, by which he has obtained the prize long offered by the Shipwrecked Mariners' Society, and furnished means of rescue, which, judging from the trials made near Portsmouth under authority of the Admiralty, are likely to render valuable service. With a mortar and a small charge of powder he throws out an anchor from the shore, and by means of the double rope thereto attached, a lifeboat can be hauled out through a heavy surf in weather when it would be impossible to launch her in the usual way; and with the further advantage that the hauling need not be done by the crew of the boat, who would consequently be fresh for their laborious task of rowing out to the ship in distress.

The Meteorological Office of the Board of Trade in carrying out their scheme of "ocean statistics," from which great advantage may be anticipated to navigation and to meteorological science, have constructed two charts, the value of which all whose business it is to go down to the sea in ships will appreciate. The wind chart is the first instalment of a series intended to show the best route for crossing the line in each month of the year. To facilitate reference, it is ruled in squares each representing a degree, with the direction and force of the prevailing winds. This is, we believe, the first attempt to show the *force* of the wind in a chart of this nature. The area embraced lies between the equator and 10°N. and 20° and 30°W., and contains the observations of five years for the month of November. When the other eleven months of the year are represented each by a chart, mariners will be able to choose a way across "the Doldrums" where they may be likely to find the most favourable winds and currents. From this it will be understood that the current chart is constructed in a similar style.

Principal Dawson, of M'Gill College, Montreal, who has just arrived with a fine collection of fossils, could not have desired a better opportunity for exhibiting them than was afforded by the conversazione. There, while showing his specimens to the *élite* of the scientific world, he could talk to them about the geological survey of Canada, and the Peninsula of Gaspé, with its cliffs of "Upper Silurian," 600 feet in height, its "Devonian sandstones" and "lower carboniferous deposits, and its arched rocks forming magnificent coast scenery. Among those fossils are two large tree-stems, *Protaxites Logani*, a species of *Psilophyton*, and a *Cyclostigma*, the latter a genus previously met with nowhere but in the Devonian rocks of Ireland. Other kinds include *Cordaites*, *Psaronius*, *Antholithes*, *Asterophyllites*, and a variety of ferns; and occurring in the animal remains, we find *Cephalaspis*, the first of the kind yet found in America, and *Machairacanthus*, and other large fishes. As Dr. Dawson is to read a paper on these important fossils at the Royal Society this evening, we may hope to see their story told in due time with suitable illustrations in the "Philosophical Transactions."

Dr. Carpenter exhibited with microscopes, with the actual specimens, and with a considerable breadth of well-executed diagrams, some of his treasures from the "deep, deep sea." In friendly neighbourhood, Prof. Tennant showed fossil specimens of some of the same creatures. And not far distant were hung Lieut. Palmer's clever drawings of living animals from the surface of the sea, captured in the China Sea, the Indian Ocean, and the Atlantic. These drawings testify to Lieut. Palmer's skill and industry. The animals are represented life-size and in their natural colours. Among them we observed the *Globigerina*, which may, perhaps, be taken as evidence that this creature does not, as some have supposed, exclusively inhabit the bottom of the sea. Considering that there is always room for natural history researches, the Admiralty should be able to find such employment for Lieut. Palmer as would exercise his artistic faculty and his habit of observation.

We are far from having exhausted the subject, but we must close here. Need we pause to draw a moral, or to point out that in such a conversazione as we have attempted to describe there is a tangible gain to science? It is well for inventors and experimentalists that they should hear what contemporaries say of their schemes and experiments, and much can be said and done with advantage amid the free talk of a general gathering which could not be permitted in the formal meeting of a scientific society. Let proper discrimination be used in the selection of articles for exhibition : science will then continue to benefit by soirées.

RECENT ACCESSIONS TO THE ZOOLOGICAL SOCIETY'S GARDENS

THE collection of living animals belonging to the Zoological Society of London and kept in their gardens in the Regent's Park contains, as most of the readers of NATURE are probably aware, by far the largest and most nearly complete living series of representatives of the various classes of Vertebrate animals that has ever been brought together in one spot. Great as the exertions that have been made of late years in some of the corresponding establishments on the Continent, the sister societies have never succeeded in rivalling the English collection as a whole, although they have occasionally bid fair to surpass it in some particular point.

The whole number of animals in the Zoological Society's Gardens usually somewhat exceeds two thousand—on the first of January last it was 2,031—consisting of 598 mammals, 1,245 birds, and 170 reptiles and batrachians, besides the fishes in the aquarium, which do not appear to be included in the annual census. Constant additions are made to the series, not only by purchase, but also by gifts of correspondents in every part of the world, and by exchange with the continental establishments. By these means the collection is kept up to its normal standard—the death-rate, as in all living zoological collections, being, in spite of every care and precaution, extremely heavy. During the past month of March 90 additions are recorded in the Society's register as having been made to the Menagerie. 33 of these were by gift, 33 by purchase, 4 by exchange, 5 by birth, and 15 were animals received "on deposit." The decrease during the same period by death and departures was 96, showing a total loss to the collection during the month of 6 individuals.

The most noticeable amongst the acquisitions to the Menagerie in March last were the four following :—

(1). Examples of two very fine new pheasants, recently discovered in Upper Assam by the well-known Indian ornithologist, Dr. J. C. Jerdon, and named by him *Lophophorus sclateri*, and *Ceriornis blythii.* These birds are both of very great interest, not only as being brilliant additions to the two magnificent groups to which they belong, but also as being *typical* specimens, *i.e.*, the identical specimens upon which Dr. Jerdon has founded these two species.

The "Monaul," or Impeyan pheasant of the southern slopes of the Himalaya, is one of the best known of Indian game-birds, and at the same time one of the most magnificently-coloured birds of British India, insomuch that Mr. Gould has chosen it as the representative bird for the cover of the numbers of his great work on the "Birds of Asia." For many years this bird was believed to stand quite alone, and to be the sole existing representative of the genus *Lophophorus.* A short time ago, however, Monsignor Chauveau, titular Bishop of Lhassa, who has recently found it necessary to retire from his Tibetan diocese into the confines of China, sent home from *Ta-tsien-lieu*, in the western part of the province of Sechuen, where he has taken up his abode, a collection of birds, amongst which were a pair of a very fine new species of Impeyan pheasant. These specimens, after being named in France *Lophophorus Lhuysi*, in compliment to M. Drouyn de Lhuys, the Minister of Foreign Affairs, whom we suppose the describer was anxious, for some good reason, to propitiate, passed into the collection of the British Museum, where they may be now seen in the Ornithological Gallery. It was thus proved that a second Impeyan pheasant is found on the northern slopes of the great central range of Asia, where it doubtless occupies a corresponding elevation and fulfils similar functions in the economy of nature to the well-known bird of the Indian Himalayas.

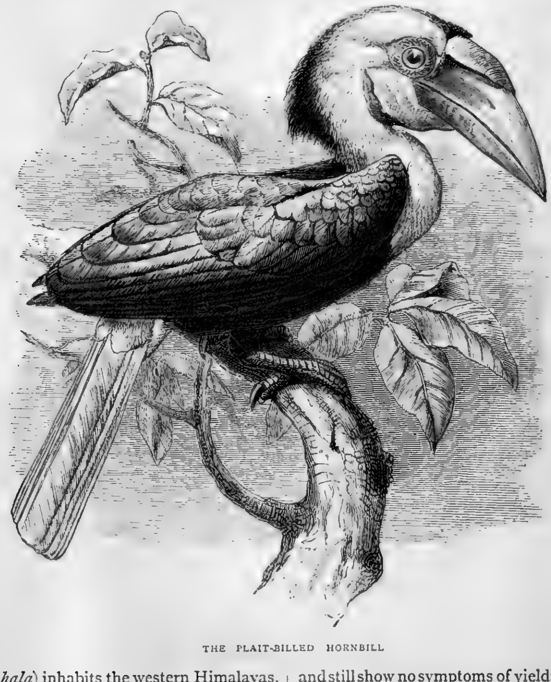
The discovery of the present bird by Dr. Jerdon, which, although somewhat different in certain details of structure from the two former, belongs strictly to the same genus, serves to further prove to us how much still remains to be done in zoological discovery, even amongst what are generally supposed to be the best-known divisions of the Vertebrata. Being crestless, Sclater's Impeyan, which has been named by Dr. Jerdon after the secretary of the Zoological Society of London, renders the old generic term *lophophorus* less applicable to the group. But in other points it does not materially differ, and at any rate is sufficiently near the common Impeyan to induce the only known individual of Sclater's Impeyan now in the Zoological Society's gardens to be quite ready to associate with a female of the latter which had been placed along with him.

Dr. Jerdon discovered this new bird during his residence at Shillong, a new sanitarium recently opened on the Khasya hills. The single example known was obtained from the Mishmees, a wild tribe which inhabits the hills of Upper Assam, by Major Montagu, of the Bengal Staff Corps, who, on being informed of the value of his acquisition, most liberally presented this unique specimen to the Zoological Society.

The new Tragopan which arrived in the Society's Gardens along with the last named bird, is hardly of less interest to the naturalist, and to the general observer a much more brilliant species in colour. It is likewise a gift of Major Montagu to the Society, having been obtained by him in the same district as the new Impeyan. The two Tragopans or "Argus Pheasants," as they are usually termed by Indian sportsmen, one of which (*Ceriornis melanocephala*) inhabits the western Himalayas, and the other (*C. satyra*) the Himalayas of Nipal and Sikim, have long been known as among the most splendid forms of the Gallinaceous order. The present bird, which has been named by Dr. Jerdon *Ceriornis blythii*, after one of the most distinguished of Indian naturalists, forms a third Indian species of the genus. There are likewise two Chinese Tragopans known—making in all five members of the group. Of one of these latter—Temminck's Tragopan (*C. temminckii*)—there are several pairs now in the Zoological Society's Gardens, and both this and the *Ceriornis satyra* have bred there in former years.

(2). Four young Hornbills belonging to three species (*Buceros bicornis*, *B. plicatus*, and *B. gracilis*), received March 18th.

A few years ago it was supposed to be impossible to keep alive in this country members of the great fruit-eating families of the Old-world and New-world Tropics—such as the Hornbills (*Bucerotidæ*), the Cotingas (*Cotingidæ*,) and others. Continued experience has, however, shown that the difficulties, formerly supposed to be insuperable, may be overcome by careful attention to diet and other matters, and in the case of the Hornbills the Zoological Society has succeeded in a very remarkable degree, some four or five of the finest species of the group having been successfully introduced into their aviaries, and kept in excellent health and condition. The most remarkable representatives of this group in the Society's Gardens at the present moment are, perhaps, the great Concave-casqued Hornbills (*Buceros bicornis*). A pair of these fine birds have inhabited one of the compartments of the large Eastern Aviary ever since the summer of 1864, and still show no symptoms of yielding to the inclemencies of the English climate. There are, indeed, great hopes of these birds breeding here, in which case the British public might have an opportunity of becoming acquainted with the very singular manners and customs of the Hornbills during the breeding season. No sooner has the hen commenced the labour of incubation, say several trustworthy observers on this subject, than the male walls up the hole in the hollow tree in which the hen is sitting on her eggs, until there is only room for the point of her bill to protrude, so that until her young birds are hatched she remains confined to her nest, and is in the meantime assiduously fed by her mate, who devotes himself entirely to this object. This habit has been testified to not only by Tickell, Layard, and other Indian naturalists concerning some of the Asiatic species, but is also spoken of by Dr. Livingstone



THE PLAIT-BILLED HORNBILL

in the case of Hornbills met with during his African explorations, and there appears to be no doubt of its authenticity. In Sumatra, in 1862, Mr. Wallace heard the same story from his hunters, and was taken to see a nest of the Concave-casqued Hornbill, in which, after the male bird had been shot while in the act of feeding its mate, the female was discovered walled up. "With great difficulty," Mr. Wallace tells us, "I persuaded some natives to climb up the tree, and bring me the bird. This they did, alive, and along with it a young one, apparently not many days old, and a most remarkable object. It was about the size of a half-grown duckling, but so flabby and semi-transparent as to resemble a bladder of jelly, furnished with head, legs, and rudimentary wings, but with not a sign of a feather, except a few lines of points indicating where they would come."

It would be certainly very delightful to be able to witness this imprisoning process in the Zoological Society's Gardens, and a fine moral lesson would at the same time be administered to such of the British matrons as are in the habit of running about neglecting their infant children.

Of the four Hornbills last received, one only belongs to the large species I have just spoken of. Two are the opposite sexes of a rather smaller bird, the Plait-billed Hornbill (*B. plicatus*), which is found in the Burmese peninsula, Sumatra and Java. The male, as in many other species, has the head and neck white or pale rufous, while the female these parts are black, like the rest of the body. It will be also remarked that the colour of the naked skin of the throat is not alike in the two sexes. The fourth and smallest bird is a female of the Slender Hornbill (*Buceros gracilis.*) Neither of these two last-named species have been previously exhibited in the Society's collection, which now contains twelve Hornbills of eight different species.

THE BURROWING OWL

(3.) Four Burrowing Owls (*Pholeoptynx cunicularia*), presented by George Wilks, Esq., C.M.Z.S., of Buenos Ayres. The Burrowing Owl is an American species of Day-Owl, well-known for its abnormal habits, and widely distributed in the New World. In the prairies of the far West, it lives in the "villages" of the Prairie-dog (*Arctomys ludovicianus*), residing in the forsaken burrows. "The burrow selected," says the well-known naturalist, Audubon, "is usually at the foot of a wormwood-bush (*Artemisia*), upon the summit of which the owl often perches, and stands for a considerable while. On being approached they utter a low chattering sound, start, and skim along the plain for a considerable distance. When winged they make for the nearest burrow, and when once within it, it is impossible to dislodge them." It is commonly said that rattlesnakes are likewise abundant in these "Prairie-dog villages," and that the beast, bird, and reptile, may not unfrequently be seen here in harmonious juxtaposition. In the pampas of South America, this little owl associates with another Burrowing rodent, which lives in communities in a similar manner to the Prairie-dog. This is the Vizcacha (*Lagostomus trichodactylus*). During the open day, Mr. Darwin tells us, but more especially in the evening, these owls may be seen in the pampas of the Argentine Republic in every direction, standing by pairs on the hillock next to their hole. If disturbed, they either retreat under-ground, or move with an undulatory flight to a short distance, and then turning round, steadily gaze at their pursuer.

The Burrowing Owl is, however, perfectly capable of making its own burrow, as Mr. Darwin tells us it always does where the Vizcacha is not found, and as it has done in the Zoological Society's Gardens. The first individual of this species which was received in 1868 from the same donor, was no sooner placed in a cage with a sandy floor than, "true to its habits, it excavated a hole in the soil at the bottom, into which it always retreated when threatened." The same habit may be witnessed on alarming the specimens of this bird now in the Society's gardens, although the burrow in the present instance has been, at all events partially, made artificially for their use.

(4.) An example of a rather rare Antelope from Western Africa—the Woodloving Antelope (*Cephalophus sylvicultrix*)—received in exchange March 24th. This is a representative of a group of Antelopes of small size which are found only in tropical or subtropical Africa, and are peculiar for having a little tuft of hair between their horns, as their generic name imports. Some eighteen or twenty species of this genus are known to science, and several of them are usually represented in the Zoological Society's collection. But the present animal, which is well marked by its white dorsal streak, has not been previously received alive by the Society. P. L. SCLATER

NOVEL TELEGRAPHY—ELECTRIFICATION OF AN ISLAND

A CURIOUS discovery has been made by Mr. Gott, the superintendent of the French company's telegraph station at the little island of St. Pierre Miquelon. There are two telegraph stations on the island. One, worked in connection with the Anglo-American company's lines by an American company, receives messages from Newfoundland and sends them on to Sydney, using for the latter purpose a powerful battery and the ordinary Morse signals.

The second station is worked by the French Transatlantic Company, and is furnished with exceedingly delicate receiving instruments, the invention of Sir William Thomson, and used to receive messages from Brest and Duxbury. These very sensitive instruments were found to be seriously affected by earth-currents; *i.e.*, currents depending on some rapid changes in the electrical condition of the island; these numerous changes caused currents to flow in and out of the French company's cables, interfering very much with the currents indicating true signals. This phenomenon is not an uncommon one, and the inconvenience was removed by laying an insulated wire about three miles long back from the station to the sea, in which a large metal plate was immersed; this plate is used in practice as the earth of the St. Pierre station, the changes in the electrical condition or potential of the sea being small and slow, in comparison with those of the dry rocky soil of St. Pierre. After this had been done, it was found that part of the so-called earth-currents had been due to the signals sent by the American company into their own lines, for when the delicate receiving instrument was placed between the earth at the French station and the earth at the sea, so as to be in circuit with the three miles of insulated wire, the messages sent by the rival company were clearly indicated, so clearly indeed, that they have been automatically recorded by Sir William Thomson's syphon recorder. Annexed is a facsimile of a small part of the message concerning the loss of the steamship Oneida, stolen in this manner.

It must be clearly understood that the American lines come nowhere into contact, or even into the neighbourhood of the French line. The two stations are several hundred yards apart, and yet messages sent at one station are distinctly read at the other station; the only connection between the two being through the earth; and it is quite clear that they would be so received and read at fifty stations in the neighbourhood all at once. The explanation is obvious enough: the potential of the ground in the neighbourhood of the stations is alternately raised and lowered by the powerful battery used to send the American signals. The potential of the sea at the other end of the short insulated line remains almost if not wholly unaffected by these, and thus the island acts like a sort of great Leyden jar, continually charged by the American battery, and discharged in part through the short insulated French line. Each time the American operator depresses his sending key, he not only sends a current through his lines, but electrifies the whole island, and this electrification is detected and recorded by the rival company's instruments.

No similar experiment could be made in the neighbourhood of a station from which many simultaneous signals were being sent; but it is perfectly clear that unless special precautions are taken at isolated stations, an inquisitive neighbour owning a short insulated wire might steal all messages without making any connection between his instrument and the cable or land line. Stealing messages by attaching an instrument to the line was a familiar incident in the American War; but now messages may be stolen with perfect secrecy by persons who nowhere come within a quarter of a mile of the line. Luckily, the remedy is simple enough. All owners of important isolated stations should use earth-plates at sea, and at sea only. This plan was devised by Mr. C. Varley many years ago to eliminate what we may term natural earth-currents, and now it should be used to avoid the production of artificial earth-currents which may be improperly made use of.

FLEEMING JENKIN

NOTES

WE regret to hear that Baron Liebig is very ill.

WE are informed that Messrs. Lyon Playfair, C.B., M.P., B. Samuelson, M.P., and Dr. W. A. Miller, will probably be among the members of the Royal Commission to inquire into the Present State of Science in this country.

PRINCIPAL DAWSON, of Montreal, who is now on a visit to this country, will deliver the Bakerian Lecture to-night before the Royal Society. The subject is the Pre-carboniferous Flora of North Eastern America. The opportunity of listening to so eminent a geologist on a subject which he has made especially his own, will doubtless draw together a large assembly of our men of science anxious to do honour to their distinguished *confrère*.

THE following gentlemen have been appointed by the University of London examiners and assistant examiners for 1870-

1871 in the various branches of science:—Logic and Moral Philosophy: Rev. Mark Pattison, B.D., and Prof. G. Croom Robertson, M.A. Political Economy: Prof. W. Stanley Jevons, M.A., and Prof. T. E. Cliffe Leslie, LL.B. Mathematics and Natural Philosophy: Prof. H. J. S. Smith, M.A., F.R.S., and Prof. Sylvester, M.A., F.R.S. Experimental Philosophy: Prof. W. G. Adams, M.A., and Prof. G. Carey Foster, B.A., F.R.S. Chemistry: Dr. Matthiessen, F.R.S., and Prof. Odling, M.B., F.R.S. Botany and Vegetable Physiology: Joseph Dalton Hooker, M.D., LL.D., F.R.S., and Thomas Thomson, M.D., F.R.S. Geology and Palæontology: Prof. Duncan, M.B., F.R.S., and Prof. Morris, F.G.S. Practice of Medicine: John Syer Bristowe, M.D., and Prof. J. Russell Reynolds, M.D., F.R.S. Surgery: Prof. John Birkett, F.R.C.S., and F. Le Gros Clark, F.R.C.S. Anatomy: Prof. William Turner, M.B., F.R.S.E., and Prof. John Wood, F.R.C.S. Physiology, Comparative Anatomy, and Zoology: Michael Foster, M.D., B.A., and Henry Power, M.B. Obstetric Medicine: Robert Barnes, M.D., and Prof. W. H. Graily Hewitt, M.D. Materia Medica and Pharmaceutical Chemistry: Thomas R. Fraser, M.D., F.R.S.E., and Prof. Alfred Baring Garrod, M.D., F.R.S. Forensic Medicine: E. Headlam Greenhow, M.D., and Thomas Stevenson, M.D.

PROFESSOR AGASSIZ is still seriously ill.

WE are rejoiced to hear that M. Janssen is to be provided with instruments wherewith to continue his observations on the sun by means of the new method. This was announced at the last meeting of the Paris Academy by M. Faye, who remarked: "El quelques mois il livrera à la Science cent fois plus de données précieuses que les astronomes n' auraient pu en recueillir, avant lui, par l'observation ordinaire des eclipses totales d'une vingtaine de siècles."

THE number of entries for the Examination for Women at the London University, which takes place during the current week, is 17, against 9 last year. Of these, 12 will be examined in London, and 5 at Cheltenham.

DR. A. VOELCKER and Mr. H. M. Jenkins, Secretary to the Royal Agricultural Society, reprint, from the Journal of the Society, a Report on the Agriculture of Belgium, containing much valuable information on its soil and climate, geological features, modes of agriculture, and rural economy.

"THE Body and its Health, a Book for Primary Schools," by E. D. Mapother, M.D., is a little elementary book on human physiology, prepared with greater care and attention to accuracy han is usually the case with primary scientific hand-books. It contains in a small space, and published at a low price, a mass of such information as ought to form a portion of the curriculum of all schools, both for boys and girls, and is illustrated by good woodcuts.

THE "Repertorium für Meteorologie," issued by the Imperial Academy of St. Petersburg, in the form of a 4to. volume, edited by Dr. H. Wild, Director of the Physical Central Observatory, contains a mass of tables respecting the meteorological phenomena of Russia, and a variety of other information.

WE have on our table the reports of several provincial scientific societies, and other papers of a like nature:—The Proceedings of the Berwickshire Naturalists' Club, for 1869; Transactions of the Norfolk and Norwich Naturalists' Society for 1869-70; the 37th Annual Report of the Royal Cornwall Polytechnic Society for 1869; and the Geology, Botany, and Zoology of the Neighbourhood of Alnwick, by George Tate, F.G.S. They all show the zeal with which the pursuit of natural history is followed in the provinces; these local societies have been the nursery of many a genuine naturalist who has rendered important service to the study of Nature.

AMONG the objects of interest exhibited at the soirée of the Linnean Society on the 27th ult., was a collection of plants made by Mr. W. W. Saunders, arranged in pairs; the plants forming each pair belonged to entirely different natural orders, but were so remarkably alike in the general form, and even in the marking of the foliage, as to be barely distinguishable even to a practised eye. One of the most strikingly "mimetic" pairs were a Conifer and a Selaginella, belonging to the two sub-kingdoms of flowering and flowerless plants.

ACCORDING to Mr. Kurtz, the Curator of the Herbarium of the Calcutta Botanical Gardens, the Andaman Islands are gradually sinking, the rate of subsidence being about one foot in a century. This inference is founded on the fact that trunks of trees still rooted in the ground may be seen in the water of the straits which separate the islands, belonging to species which never grow in mangrove swamps, but which are only found further inland. It is even possible to "trace in several places the stumps of the sunken trees in the sea, up to the state when the trees are just dying by the influence of the sea-water, and the subsequent change of the soil by the formation of the mangrove swamp." At the rate of subsidence indicated by Mr. Kurtz, a thousand years must elapse before the extensive convict establishment maintained on these islands will be in immediate danger of submersion.

IN a forthcoming number of his "Geographische Mittheilungen," by means of a map and a memoir, the geographer Petermann gives his rendering of the information contained in Dr. Livingstone's recent letters, taken in connection with the former travels of the Portuguese in Central South Africa. The new real geography of this map agrees remarkably well, in its general features, with a chart which lately appeared in this journal. There is, however, one main point of difference. A river named *Luvivi* was crossed by Pombeiro Baptista in his route to the Cazembe's town from that of the Muata Yanvo, and is distinctly stated by him to run into the Luapula, the river which is now known to unite Lakes Bangeweolo and Moero. Livingstone says that a large river named *Lufira* drains the western side of the great valley, and takes up the waters of Ulenge in the west of Tanganyika. On the foundation of the resemblance of these names—surely a very weak one in a region where duplicate river names are frequent—Dr. Petermann discards the Pombeiros statement, and uniting the Luviri with the Lufira, carries a great river through the midst of the country separating the valleys ruled over by the Muata Yanvo and the Cazembe, which all travellers here agree is a mountainous desert, and which the road joining these territories bends far southward to avoid. Again: The country of Usango is said by Livingstone to be on the east of the plateau which rises south of Tanganyika; on this map it is indicated to westward of that lake; and Lake Liemba, instead of being shown on the northern slope of that upland, is represented as lying in a valley directly continuing that of Tanganyika. The uncertainty as to the identity of the Chowambe Lake with the Albert Nyanza, and consequently of the union of Tanganyika with the Nile system, is considered too great to admit of any solution of the Nile problem as yet, and any attempt at this is characterised as plucking unripe fruit. This part also contains a very interesting account of a voyage by a Norwegian fisher named Captain E. H. Johannesen, who, in the summer of 1869, sailed completely round the island of Novaia Zemlia and through the Kara Sea. This gulf, believed till now to be constantly choked up by impenetrable ice-park, has been called the "ice-cellar" of the North Pole. In place of this, Johannesen found a mild atmosphere off the north-east of Novaia Zemlia, and throughout the whole Kara Gulf in July and August no ice was visible, but a heavy roll of the open sea came up from the south-east.

MR. B. WILLIAMSON, one of the Fellows of Trinity College, Dublin, is preparing a "Treatise on Mechanics," which will shortly be issued by the University Press.

Mr. James Glaisher has given two lectures for the "Sunday Lecture Society" on the evenings of April 24th and May 1st., on "The Balloon" and on "Rain." In the latter he gave an interesting account of how rain is derived and how measured ; some most carefully prepared tables were exhibited, together with diagrams and some excellent photographs of the varied forms of snow crystals. The lecture for May 8th will be by Mr. Henry Moody on "The Prevention of Infectious Diseases." The importance of this subject, in reference to the great object of the society, the social welfare of mankind, will be illustrated by the example of Bristol. From being one of the most unhealthy, Bristol has become one of the most healthy of cities. In the summer and autumn of 1832 the ravages there by cholera were enormous ; the deaths alone approaching to 1000 ! In 1866, on the contrary, when the poor at the East-end of London were dying by hundreds, the deaths in Bristol were but 26 ; and it will be pointed out that this vast difference has arisen from the precautions and general sanatory measures adopted by the inhabitants and authorities of the City of Bristol.

M. Quenault reports the discovery at Hauteville-sur-mer, near to a rock called Maulieu, of a bed of vegetable mould in which repose trunks of trees, still holding by their roots, along with a layer of turf. At high tide this bed is covered to the depth of about twelve inches. The oak alone has preserved its hardness, the other woods having become quite soft, but still preserving their colour and even their bark. He supposes the immersion to have taken place in the eighth century.

The ninth reunion of the learned societies of the Sorbonne took place on the 20th of April, when M. Le Verrier was appointed president, M. Milne-Edwards vice-president, and M. Blanchard, secretary of the Section of Sciences ; M. le Marquis de la Grange, president, M. Léon Renier, vice-president, and M. Chabouillet, secretary of the Section of Archæology ; M. Amédée Thierry, president, and M. Hippeau, secretary of the Section of History. M. Le Verrier stated that the lectures instituted at the Sorbonne, which have already been in existence ten years, have had already the most beneficial results ; they have formed bonds between the *savants* of France and of the other countries of Europe, and have contributed to raise scientific and literary labours to a higher and higher platform.

We have received Washington papers of the 13th, 14th, and 15th of April, containing a report of the 13th semi-annual session of the National Academy of Sciences, held in that city. The most important papers read were : "On the coming transits of Venus," by Prof. Simon Newcomb ; "On meridional arcs, measured in connection with the United States coast survey," by Mr. J. E. Hilgard ; "Craniological observations," by Dr. George Otis ; "The Northmen in Greenland," by Dr. Hayes ; "Considerations of the apparent inequalities of long periods in the moon's mean motion," by Prof. S. Newcomb ; "On the influence of the interior structure of the earth on precession and nutation," by Prof. J. G. Barnard ; "On a new classification of clouds," by Prof. Poey, of Havana ; "On fluctuations of the barometer," by Dr. B. F. Craig. Prof. Joseph Henry occupied the chair. We purpose giving abstracts of some of these pages on a future occasion.

A Natural History Society has recently been established at Winchester College ; and a Botanical Section has been formed in connection with the Hants and Winchester Scientific and Literary Society.

Mr. Llewellynn Jewitt has issued a prospectus of a proposed publication, by subscription, of an entirely new, large, and comprehensive history, topography, and genealogy of the county of Derby.

A paper appears in the last number of the "Proceedings of the Royal Society," by Dr. Herbert Davies, on the law which regulates the relative magnitude of the areas of the four orifices of the heart. He remarks that although to ordinary observation these orifices appear to exhibit no mutual relationship of size, there can be no doubt that an instrument so accurate in the adaptation of its valvular apparatus, and so exact in the working of its different parts, must reveal on close examination the existence of laws which not only determine the force required to be impressed upon the blood traversing its chambers, but also the relative sizes of these apertures to one another. On converting Dr. Peacock's measurements of the circumference of the several orifices into numbers representing these areas it is found that in the male the respective mean areas are

Tricuspid	$1\frac{3}{4}$ sq. inch.
Pulmonic	1 ,,
Mitral	$1\frac{1}{4}$,,
Aortic	$\frac{3}{4}$

and on pushing the inquiry further, it is found that there is a distinct law presiding over them which is discovered on comparing the ratios of the areas of corresponding orifices. Thus—

$$\frac{\text{Area of tricuspid}}{\text{Area of mitral}} = \frac{1.78}{1.27} = 1{\cdot}4 \text{ nearly}$$

$$\frac{\text{Area of pulmonic}}{\text{Area of aortic}} = \frac{1}{78} = 1{\cdot}3 \text{ nearly}$$

or in other words, the area of the tricuspid appears from these calculations to bear nearly the same relation to the area of the mitral, which the area of the pulmonic does to that of the aortic orifice ; *i.e.*, were the tricuspid, for example, twice the size of the mitral orifice in area, the pulmonic would be twice the size of the aortic orifice in area, the two ratios differing from each other only by one-tenth. The same law probably holds in the hearts of most animals, the areas of the four orifices bearing an exact mathematical relationship to each other, so that if the areas of any three of the openings be known, the area of the fourth orifice can be correctly calculated. A knowledge of this law, it is obvious, may prove of great importance in estimating the amount of contraction or dilatation of orifice which may be present in disease. Dr. Davis then proceeds to give the reasons for this arrangement, for which we must refer our readers to the original.

That the white corpuscles of the blood can pass through the walls of the blood-vessels is now admitted by so many observers, that it may fairly be regarded as an established fact. But the entrance of a solid body from the outside of the capillaries into their interior has, so far as we know, only been observed in one or two exceptional instances. Quite recently, however, M. Saviotti has published a paper in the *Centralblatt*, in which he describes the passage of entire pigment cells from the parenchyma of the web of the foot of the frog through the walls of the capillaries and smaller veins, into their lumen, where having arrived, they are swept away by the blood current as a phenomenon of common occurrence, and easily followed. To render this evident he excites local inflammation in the web by the application of a dilute (2 per cent.) solution of sulphuric acid. After the lapse of some time the pigment cells of the part affected are found to have congregated together round the minute vessels in a somewhat contracted condition, their long branching process becoming materially shortened. The contractility which these cells are known to possess, however, is not entirely abrogated. One or more of the processes may be seen to insinuate itself through the wall of the adjacent capillary. After penetration it may become much elongated, and be even altogether carried away by the current of blood, to the movements of which for a time it offers an impediment ; or it may, so to speak, drag after it the rest of the cell, which after remaining a little while adherent to the inner surface of the vessel at the point where it has entered, is swept, or itself swims away. Is not this a singular fact ? Of what nature must the capillary wall be, that will thus admit the ingress and egress of solid bodies, and yet retain the red blood corpuscles even under great increase of pressure.

M. E. LEFEVRE is engaged upon a monograph of the species of the genus *Clythra*, inhabiting Europe and its confines (including the Mediterranean region). As many new species have been discovered, especially in Algeria and the East, since the publication of Lacordaire's memoir in 1848, M. Lefèvre will be thankful for the loan of specimens of new or rare species, and for any information as to their geographical distribution. M. Lefèvre's address is 28, Rue Constantine, Paris-Plaisance.

WE quote the following from the *Scotsman*, of May 2:—"A correspondent writes: 'In Inverleith Row on Saturday night, exactly at a quarter-past eleven o'clock, my attention was attracted by a sudden and strange brightness overspreading everything around. Instinctively turning my eyes upwards, a grand sight met my gaze. A meteor of remarkable size, brilliancy, and distinctness, was seen shooting from the heavens, from about the zenith, and descending earthwards in a southerly direction. The form of this interesting object seemed elliptical, and it was of a bright yellow hue. It had a clearly-defined apex or point, which was of a deep red colour, and appeared to glow and sparkle in a wonderful manner. The phenomenon was visible for about two seconds, and lighted up everything around me. The night was fine and clear, with a decidedly frosty air, and there was a light, steady breeze blowing from the north-west at the time." On the same subject our Dunbar correspondent writes: 'A very brilliant meteor was observed here about eleven o'clock on Saturday night. When first seen, the meteor had the appearance of a star of the first magnitude. As it approached, however, it gradually increased in size until it assumed the appearance of a ball of five or six inches in diameter, and changing its colour from a pale silvery white to a bright blue flame. As it still increased, sparks seemed to be emitted from the circumference, giving it the appearance of being surrounded with a peculiar halo of dense silvery rays. It continued in this state for a second or two, and then shot across the heavens in a southerly direction, the ball increasing in brilliancy as it travelled, and leaving behind it a long train of lurid-coloured flame. From the time it was first seen approaching until it vanished about five or six seconds elapsed. The night was clear and cold at the time.'"

THE GRESHAM LECTURES

THE Lectures (three in number) were delivered by Dr. Symes Thompson, during the past week, at the Gresham College, Basinghall-street. The first was occupied with the consideration of Cough, an account being given of its etiology varieties, and general principles of treatment. The second was devoted to Tonics; whilst of the third, which treated of Climate and Health Resorts, we give an epitome as likely to prove interesting to some of our readers. Dr. Thompson remarked that the Romans very early discovered the use of mineral waters, as shown by many of their relics being found in the neighbourhood of such springs; whilst at Wiesbaden, tablets have been found with votive inscriptions. Some of the more common ingredients of mineral waters were then described, and their chemical properties demonstrated, including carbonic acid, iron, sulphuretted hydrogen, &c. The properties of ozone were then discussed in connection with pure air, and the lecturer passed on to the consideration of climate and health resorts, and said that it was a great mistake to suppose that any particular place was the best for a particular malady, for there is no specific action in the air of any place; and the physician, in recommending a resort to patients, had regard to the kind of climate there found, a dry climate being suitable for most bronchial membranes, and a moist climate for the reverse state of the membranes, and an approximation to such different climates can be obtained in our rooms. If the patient be feverish and excitable, he should be sent to an exciting varied climate; or, if languid and torpid, not to a quiet, mild, uniform climate. These two kinds of climate are obtainable at the south of France; for at Nice the climate is bright and exciting, while in the neighbourhood of the Pyrenees, at Pau, the atmosphere is very still, and eminently suitable to patients suffering from irritable membranes. The great advantage gained by persons going abroad is no doubt the regular daily outdoor exercise, not obtainable in the varied winter of our island, but found in Algiers, Riviera, Mentone, Nice, &c., at that season. Together with exercise may be linked the advantage derived from breathing pure air. There are, however, many places in England where almost the same benefit may be enjoyed. It was the former practice in cases of lung-disease, to shut up patients in close rooms, with fire burning, all through the winter, the consequence being that they became like hothouse plants, and on the first exposure to the open air in spring all these advantages were lost. Acting on the same principle, consumptive patients up to 20 years ago were sent to Madeira. Three years ago the inhabitants of Madeira, wishing to re-establish the value of their climate in lung-disease, solicited the authorities of the Brompton Hospital for Consumption to send them out patients as a matter of experiment. Twenty patients were sent out, and only those who were likely to be benefited by the climate of Madeira, being all in an advanced stage of lung-disease. Of this number 1 patient died suddenly, although up to the time of his death he seemed to be benefiting by the change; 4 were worse; 6 were stationary, and 7 were markedly benefited; 2 very much benefited. This has been regarded by some as unsuccessful; but Dr. Thompson was of different opinion, and thought it indicated that Madeira is a useful climate in certain cases. Since Madeira has been abandoned there has been a revulsion of feeling in favour of Canada as a resort for consumptives, but that this is no new doctrine is seen from the fact that the value of cold climates for consumption was advocated by Baron Larrey, Napoleon Bonaparte's physician. There are disadvantages as well as advantages in the most favoured health resorts; such as the dry, hot "mistral' of Nice, or the dry, dusty "brickfielder" of Melbourne,—winds which patients dislike as much as our east wind. Purity of air or absence of dust is of very great importance; and for this reason it is now the practice to send patients suffering from chest-disease for a long sea-voyage. It constantly happens that sick people—in whom the disease is far advanced—press their medical men to send them on a sea-voyage; but in their case the remedy has come too late, and so it happens that the death-rate at Melbourne is exceedingly high—more than half the deaths being due to consumption. Dampness favours consumption; so that dry air is another desideratum which can be obtained at certain altitudes—so that in certain districts it is found possible to resort to mountain-tops, where consumption does not occur. In the neighbourhood of the equator the so-called "immunity-level" is at a height of 9,000 feet; at Algiers, 6,000 feet; at London, 3,000 feet. The chief resorts in Europe are the Swiss mountains, where English people often go to spend the winter, in perpetual snow, but yet in an atmosphere so pure and clear that the most delicate invalids can go out in the open air. In Norway, Russia, &c., the immunity-level is only 1,000 feet above the sea.

SCIENTIFIC SERIALS

THE May number of the *Journal of Botany, British and Foreign*, commences with the first part of a *Clavis Agaricinorum* by the well-known fungologist, Mr. Worthington Smith. The general classification of the Agarics adopted by Fries and Berkeley is followed; but several new sub-genera are proposed. An ingenious tabular view accompanies the paper, presenting the salient features of the series and sub-genera of this vast genus at a glance. Dr. Seemann continues his revision of the natural order *Bignoniaceæ*; while the Hon. J. L. Warren contributes a paper on a sub-division of *Rubus*, a most intricate genus, to which he has paid special attention; and Dr. H. Trimen a description of a new British *Callitriche*. Other short notes and notices fill up the number, which maintains the interest for British botanists especially, promised on the commencement of the new series.

The *Revue des Cours Scientifiques* for April 23rd contains a report of an interesting address delivered before the University of Berlin, by M. du Bois Reymond on the Organisation of Universities; the conclusion of Fotherly's address to the Hunterian Society, and report of a lecture by M. Claude Bernard, on Suffocation by Charcoal. The number for April 30th is entirely occupied by M. Bouley's lecture at the Sorbonne, on Madness, and the conclusion of M. Bernard's lecture.

THE third number of the new Italian Geological Journal, or *Bolletino*, published by the "Comitato Geologico d'Italia," opens with M. Igino Cocchi's paper on the stratified rocks of the isle of Elba. It especially relates to the lower secondary, eocene, cretaceous, and post pliocene strata, and is illustrated with engravings. Among the bibliographical notices, Professor Omboni's work on the Geology of Italy, with eight maps, is well spoken of. The number also comprises translations of extracts from foreign memoirs.

THE *American Naturalist*, Vol. IV., No. 2, April 1870.—The April number of this Journal contains three exceedingly interesting articles, namely, a report on the Sea Otters of the north-west coast of America and Aleutian islands, by Captain C. M. Scammon; a paper on Parasitic Insects, from the able hand of Dr. A. S. Packard; and some notes on the Fresh-water Fish of New Jersey, by Dr. C. C. Abbott, which contains many valuable remarks. The last two papers are illustrated.—Dr. W. Wood contributes a popular article on Falconry.—The usual reviews and short miscellaneous notices complete the contents of the number.

SOCIETIES AND ACADEMIES

LONDON

Royal Society.—April 28.—"On the Organs of Vision in the Common Mole." By Robert James Lee.

"On an Aplanatic Searcher, and its Effects in improving High Power Definition in the Microscope." By G. W. Royston-Pigott, M.A., M.D.

The Aplanatic Searcher is intended to improve the penetration, amplify magnifying power, intensify definition, and raise the objective somewhat further from its dangerous proximity to the delicate covering-glass indispensable to the observation of objects under very high powers. The inquiry into the practicability of improving the performance of microscopic object-glasses of the very finest known quality was suggested by an accidental resolution in 1862 of the Podura markings into black beads. This led to a search for the cause of defective definition, if any existed. A variety of first-class objectives, from the $\frac{1}{16}$ to the $\frac{1}{4}$, failed to show the beading, although most carefully constructed by Messrs. Powell and Lealand. Experiments having been instituted on the nature of the errors, it was found that the instrument required a better distribution of power; instead of depending upon the deepest eyepieces and most powerful objectives hitherto constructed, that better effects could be produced by regulating a more gradual bending or refraction of the excentrical rays emanating from a brilliant microscopic origin of light. It then appeared that delusive images, which the writer has ventured to name *eidola**, exist in close proximity to the best focal point (where the least circle of confusion finds its focus).

I. That these images, possessing extraordinary characters, exist principally above or below the best focal point, according as the objective spherical aberration is positive or negative.

II. That test-images may be formed of a high order of delicacy and accurate portraiture in *miniature*, by employing an objective of twice the focal depth, or, rather, half the focal length, of the observing objective.

III. That such test-images (which may be obtained conveniently two thousand times less than a known original) are formed (under precautions) with a remarkable freedom from aberration, which appears to be reduced in the miniature to a *minimum*.

IV. The beauty or indistinctness with which they are displayed (especially on the immersion system) is a marvellous test of the correction of the observing objective, but an indifferent one of the image-forming objective used to produce the testing miniature.

These results enable the observer to compare the known with the unknown. By observing a variety of brilliant images of known objects, as gauze, lace, an ivory thermometer, and sparkles of mercury, all formed in the focus of the objective to be tested with the microscope properly adjusted so that the axes of the two objectives may be coincident, and their corrections suitably manipulated, it is practicable to compare known delusions with suspected phenomena.

It was then observed (by means of such appliances) that the aberration developed by high-power eyepieces and a lengthened tube followed a peculiar law.

A. A lengthened tube increased aberration faster than it gained power (roughly, the aberration varied as v^2, while the power varied as v).

B. As the image was formed by the objective at points nearer to it than the *standard distance of nine inches*, for which the best English glasses are corrected, the writer found the aberration diminished faster than the power was lost, by shortening the body of the instrument.

C. The aberration became negatively affected, and required a positive compensation.

D. Frequent consideration of the equations for aplanatism suggested the idea of searching the axis of the instrument for aplanatic foci, and that many such foci would probably be found to exist in proportion to the number of terms in the equations (involving curvatures and positions).

E. The law was then ascertained that power could be raised, and definition intensified, by positively correcting the searching lenses in proportion as they approached the objective, at the same time applying a similar correction to the observing objective.

The chief results hitherto obtained may be thus summarised. The writer measured the distance gained by the aplanatic searcher, whilst observing with a half-inch objective with a power of seven hundred diameters, and found it *two-tenths of an inch increase*; so that optical penetration was attainable with this high power through plate-glass nearly one quarter of an inch thick, whilst *visual* focal depth was proportionably increased. The aplanatic searcher increases the power of the microscope from two and a half to five times the usual power obtained with a third or C eye-piece of one inch focal length. The eighth thus acquires the power of a twenty-fifth, the penetration of a one-fourth. And at the same time the lowest possible eyepiece (3-inch focus) is substituted for the deep eye-piece formed of minute lenses, and guarded with a minutely perforated cap. The writer lately exhibited to Messrs. Powell and Lealand a brilliant definition, under a power of four thousand diameters, with their new "eighth immersion" lens, by means of the searcher and low eyepiece.

The traverse of the aplanatic searcher introduces remarkable chromatic corrections displayed in the unexpected colouring developed in microscopic test objects. The singular properties, or rather phenomena, shown by eidola, enable the practised observer in many cases to distinguish between true and delusive appearances, especially when aided by the aberrameter applied to the objective to display excentrical aberration by cutting off excentrical rays. Eidola are symmetrically placed on each side of the best focal point, as ascertained by the aberrameter when the compensations have attained a delicate balance of opposite corrections.

If the beading, for instance, of a test object exists in two contiguous parallel planes, the eidolon of one set is commingled with the true image of the other. But the upper or lower set may be separately displayed, either by depressing the false eidola of the lower stratum, or elevating the eidola of the upper. For when the eidola of two contiguous strata are intermingled, correct definition is impossible so long as the aperture of the objective remains considerable.

One other result accrues: when an objective, otherwise excellent, cannot be further corrected, the component glasses being already closely screwed up together, a further correction can be applied by means of the adjustments of the aplanatic searcher itself, all of which are essentially conjugate with the actions of the objective and the variable positions of its component lenses; so that if δx be the traversing movements of the objective lenses, δv that of the searcher, F the focal distance of the image from the objective when δx vanishes, f the focal distance of the virtual image formed by the facet lenses of the objective, then

$$\frac{\delta v}{\delta x} = \left(\frac{F}{f_1}\right)^2$$

The *appendix* refers to plates illustrating the mechanical arrangements for the discrimination of eidola and true images, and for traversing the lenses of the aplanatic searcher. The plates also show the course of the optical pencils, spurious disks of residuary aberration and imperfect definition, as well as some examples of "high-power resolution" of the Podura and Lepisma beading, as well as the amount of amplification

* From εἴδωλον, a false spectral image.

obtained by Camera Lucida outline drawings of a given scale.

"On a Cause of Error in Electroscopic Experiments." By Sir Charles Wheatstone, F.R.S.

To arrive at accurate conclusions from the indications of an electroscope or electrometer, it is necessary to be aware of all the sources of error which may occasion these indications to be misinterpreted. In the course of some experiments on electrical conduction and induction which I have recently resumed, I was frequently delayed by what at first appeared to be very puzzling results. Occasionally I found that I could not discharge the electrometer with my finger, or only to a certain degree, and that it was necessary, before commencing another experiment, to place myself in communication with a gas-pipe which entered the room. How I became charged I could not at that time explain; the following chain of observations and experiments, however, soon led me to the true solution. I was sitting at a table not far from the fireplace, with the electrometer (one of Peltier's construction) before me, and was engaged in experimenting with disks of various substances. To ensure that the one I had in hand, which was of tortoise-shell, should be perfectly dry, I rose and held it for a minute before the fire; returning and placing it on the plate of the electrometer, I was surprised to find that it had apparently acquired a strong charge, deflecting the index of the electrometer beyond 90°. I found that the same thing took place with every disk I thus presented to the fire, whether of metal or any other substance. My first impression was that the disk had been rendered electrical by heat, though it would have been extraordinary that, if so, such a result had not been observed before; but on placing it in contact with a vessel of boiling water, or heating it by a gas-lamp, no such effect was produced. I next conjectured that the phenomenon might arise from a difference in the electrical state of the air in the room and at the top of the chimney; and to put this to the proof, I adjourned to the adjacent room where there was no fire, and bringing my disk to the fireplace I obtained precisely the same result. That this conjecture, however, was not tenable was soon evident, because I was able to produce the same deviation of the needle of the electrometer by bringing my disk near any part of the wall of the room. This seemed to indicate that different parts of the room were in different electrical states; but this again was disproved by finding that when the positions of the electrometer and the place where the disk was supposed to be charged were interchanged, the charge of the electrometer was still always negative. The last resource was to assume that my body had become charged by walking across the carpeted room, though the effect was produced even by the most careful treading. This ultimately proved to be the case; for resuming my seat at the table and scraping my foot on the rug, I was able at will to move the index to its greatest extent.

Before I proceed further I may state that a gold-leaf electrometer shows the phenomena as readily. When I first observed these effects the weather was frosty; but they present themselves, as I have subsequently found, almost equally well in all states of the weather, provided the room be perfectly dry. I will now proceed to state the conditions which are necessary for the complete success of the experiments, and the absence of which has prevented them from being hitherto observed in the striking manner in which they have appeared to me. The most essential condition appears to be that the boot or shoe of the experimenter must have a thin sole and be perfectly dry; a surface polished by wear seems to augment the effect. By rubbing the sole of the boot against the carpet or rug, the electricities are separated, the carpet assumes the positive state and the sole the negative state; the former being a tolerable insulator, prevents the positive electricity from running away to the earth, while the sole of the foot, being a much better conductor, readily allows the charge of negative electricity to pass into the body. So effective is the excitation, that if three persons hold each other by the hands, and the first rubs the carpet with his foot while the third touches the plate of the electrometer with his finger, a strong charge is communicated to the instrument. Even approaching the electrometer by the hand or body, it becomes charged by induction at some distance.

A stronger effect is produced on the index of the instrument if, after rubbing the foot against the carpet, it be immediately raised from it. When the two are in contact, the electricities are in some degree coerced or dissimulated; but when they are separated, the whole of the negative electricity becomes free and expands itself in the body. A single stamp on the carpet followed by an immediate removal of the foot causes the index of the electrometer to advance several degrees, and by a reiteration of such stamps the index advances 30° or 40°. The opposite electrical states of the carpet and the sole of the boot were thus shown: after rubbing, I removed the boot from the carpet, and placed on the latter a proof-plate (*i.e.* a small disk of metal with an insulating handle), and then transferred it to the plate of the electrometer: strong positive electricity was manifested. Performing the same operation with the sole of the boot, a very small charge was carried, by reason of its ready escape into the body. The negative charge assumed by sole-leather when rubbed with animal hair was thus rendered evident. I placed on the plate of the electrometer a disk of sole-leather and brushed it lightly with a thick camel's-hair pencil; a negative charge was communicated to the electrometer, which charge was principally one of conduction, on account of the very imperfect insulating power of the leather. Various materials, as India-rubber, gutta-percha, &c., were substituted for the sole of the boot; metal plates were also tried; all communicated negative electricity to the body. Woollen stockings are a great impediment to the transmission of electricity from the boot; when these experiments were made I wore cotton ones. When I substituted for the electrometer a long wire galvanometer, such as is usually employed in physiological experiments, the needle was made to advance several degrees.

At the meeting of the British Association at Dublin in 1857, Professor Loomis, of New York, attracted great attention by his account of some remarkable electrical phenomena observed in certain houses in that city. It appears that in unusually cold and dry winters, in rooms provided with thick carpets and heated by stoves or hot-air apparatus to 70°, electrical phenomena of great intensity are sometimes produced. A lady walking along a carpeted floor drew a spark one quarter of an inch in length between two metal balls, one attached to a gas-pipe, the other touched by her hand; she also fired ether, ignited a gaslight, charged a Leyden jar, and repelled and attracted pith-balls similarly or dissimilarly electrified. Some of these statement were received with great incredulity at the time both here and abroad, but they have since been abundantly confirmed by the Professor himself and by others. (See Silliman's American Journal of Science, July 1858.)

My experiments show that these phenomena are exceptional only in degree. The striking effects observed by Professor Loomis were feeble unless the thermometer was below the freezing-point, and most energetic when near zero, the thermometer in the room standing at 70°. Those observed by myself succeed in almost any weather, when all the necessary conditions are fulfilled. Some of these conditions must frequently be present, and experimentalists cannot be too much on their guard against the occurrence of these abnormal effects. I think I have done a service to them, especially to those engaged in the delicate investigations of animal electricity, by drawing their attention to the subject.

Royal Institution, May 2. Annual meeting.—Sir Henry Holland, Bart, M.D., D.C.L., F.R.S., President, in the chair. The annual report of the Committee of Visitors for the year 1869 was read and adopted. The books and pamphlets presented in 1869 amounted to 255 volumes, making, with those purchased by the managers, a total of 388 volumes added to the library in the year, exclusive of periodicals. Forty-seven new members were elected in 1869. Sixty-three lectures and nineteen evening discourses were delivered during the year 1869. The following gentlemen were unanimously elected as officers for the ensuing year:—President: Sir Henry Holland, Bart., M.D., D.C.L., F.R.S. Treasurer: William Spottiswoode, M.A., F.R.S. Secretary: Henry Bence Jones, M.A., M.D., F.R.S.

Royal Geographical Society, April 25.—Sir R. I. Murchison, Bart., President, in the chair. The following new Fellows were elected:—Baron Osten Sacken, Secretary to the Imperial Geographical Society, St. Petersburg (Hon. Corresponding Member); Thomas M. Blackie; Lieutenant Evelyn Baring, R.A.; Colonel Shuckburgh Denniss; George B. Hudson; Lord Lawrence, G.C.B.; and John Fenton Taylor.

A paper was read, entitled "An Expedition to the Trans-Narym Country," by Baron Osten Sacken. This paper, which had been translated from the Russian by Mr. Delmar Morgan, contained a narrative of a journey, undertaken for the purpose of a reconnaissance survey, by General Poltoratsky, across the

Thian-Shan Mountains to the vicinity of Kashgar. This territory became part of the Russian dominions by the treaty of Pekin in 1860, by which the frontier line was fixed as extending from the east of Lake Issyk-Kul, along the southern spur of the Celestial Mountains, to the Khokand country; but the terrritory had never yet been visited by a European. Starting from Fort Vernoë, north of Lake Issyk-Kul, the party turned the western end of the lake, and then marched nearly due south. The country was very mountainous and picturesque, five distinct lines of elevation belonging to the Thian-Shan system being crossed in succession, some of them by passes upwards of 12,000 feet in height. The intervening valleys are traversed by streams, forming the head-waters of the Jaxartes, the largest of which is the Narym; and on the elevated ridges lie two beautiful alpine lakes, the Sou-Kul and the Chatir-Kul. Game is very abundant along the banks of the rivers, and the country is but thinly peopled by tribes of Kirghizes. The Russians did not gain possession of the new territory without a severe struggle with the forces of the neighbouring independent state of Khokand, who, in October 1860, marched an army of 40,000 men against the small Russian force, but were defeated. Baron Osten Sacken paid great attention to the botany of the country passed through, and noted the various zones of vegetation, from the wooded lower slopes of the Thian-Shan to the treeless plains below the snow-line. The alpine flora he described as extremely rich and beautiful in colour and form—amongst the plants he mentioned *Anemone narcissiflora*, Ranunculi, Geraniums, Potentillas, Gentians, and other genera—showing a great resemblance between the productions of the Thian-Shan and the Himalaya. The expedition reached to within two marches of Kashgar, and then returned to Fort Vernoë.

A second paper, on "Recent Russian Explorations in Turkistan," was read by Mr. Delmar Morgan. In the discussion which followed, M. Bartholomei, of the Russian Legation, spoke of the friendly rivalry which now prevailed between Russians and English in the exploration of Central Asia. Sir Henry Rawlinson enumerated three new expeditions to different parts of Turkistan, in which the Russians were now engaged, and the scientific results of which were freely communicated; and he congratulated the President, Sir Roderick Murchison, on the actual realisation of his anticipations of former years, when Russia and England would be friendly rivals in completing our acquaintance with the geography of the respective boundaries of each in their Eastern possessions.

Ethnological Society, April 26.—Prof. Huxley, F.R.S., in the chair.—Dr. Donovan read a paper on "The importance to the Ethnologist of a careful study of the Characters of the Brain."—Mr. E. B. Tylor then read a communication "On the Philosophy of Religion among the Lower Races of Mankind." Generalising from the lower religions of the world, the author stated the principle on which, in his view, was developed the philosophy of what may be called Natural Religion. Taking the doctrine of spiritual beings as the minimum definition of religion, he described it as *animism*, a term which fits with the theory put forward, that the conception of the soul as recognised by the lower races, is the starting-point of their religious philosophy. Such a soul, combining the ideas of ghost and vital principle, explains the phenomena of life, disease, dreams, visions, &c. This idea is extended to animals and inanimate objects, which are considered to have souls capable of appearing after death or destruction. On the analogy of the body and soul, the actions of nature are explained on the animistic theory as worked or controlled by soul-like spiritual beings. Of these beings an immense number are held to be actually human souls or manes. To such beings are ascribed the phenomena of disease, especially epilepsy and mania. Similar in nature, though different in function, are the spirits of trees, springs, &c. Hence the savage polytheist rises to expanded conceptions of greater deities, as Sun and Moon. At an early period he separates the cause of good from that of evil, and hence Dualism is rooted deeply in the religion of the lower races. The culminating conception of a Supreme Deity is well known to many of the lower races.—The President, Mr. Pusey, Mr. Howorth, and Dr. Hyde Clarke, joined in the discussion on this paper.

N.B.—It should have been stated in the report of the last meeting, that the paper "On the Danish Element in the Population of Cleveland" was written by the Rev. J. C. Atkinson, of Danby.

PARIS

Academy of Sciences, April 25.—M. Chasles presented a note by M. H. Durrande, on surfaces of the fourth order, and a communication from Mr. Spottiswoode concerning a theorem brought before the Academy on the 21st of March last, and of which he now gives the following enunciation:—"Every point of a surface is sextactic in ten of the sections made by the planes of a bundle of which the axis passes through the point." —M. de Saint Venant presented a memoir on the pressure of soils, containing a comparison of his estimates from the rational consideration of the limit of equilibrium, and by the employment of the so-called principle of least resistance of Moseley. —Several papers on subjects connected with physics were read. M. Becquerel communicated some experimental researches by MM. Lucas and Cazin, upon the duration of the electric spark. M. Jamin presented a note by M. A. Trève, on electric currents, containing some curious and interesting experiments on the action of currents in opposite directions, and when crossed in vacuum-tubes. A note by M. Rénou, on the latent heat of ice, deduced from the experiments of Laplace and Lavoisier, was communicated by M. C. Sainte-Claire Deville. The author referred to a note by M. Jamin, in which the correctness of the experiments made by Laplace and Lavoisier was maintained, and stated that the accordance of results obtained by M. Jamin could only be fortuitous, as the thermometers employed by the old experimenters were inaccurate.—M. H. Sainte-Claire Deville presented a note on the formation of liquid drops, by M. Duclaux. The author described experiments with distilled water, and with alcohols of different strengths, and stated that in the formation of drops phenomena of cohesion have but little action. Drops of water are formed much more rapidly in vapour of alcohol than in the air, and yet the amount of alcohol dissolved is very small, and hence the author concluded that the effect is produced only upon a very thin superficial layer of the drop, the tension of which constitutes the resisting power determining the size of the drop. He extended these considerations to the formation of emulsions, and to various liquids in the organism.—A paper on the fixed characteristic notes of the different vowels, by M. R. Kœnig, was presented by M. Regnault. The author discussed the results obtained by MM. Helmholtz and Donders, and gave the following as that of his own investigations into the musical notes of the vowels.

OU	O	A	E	I
(si♭)2	(si♭)3	(si♭)4	(si♭)5	(si♭)6

giving in round numbers of simple vibrations: 450, 900, 1,800, 3,600, 7,200.—Numerous papers relating to astronomical subjects were communicated, and M. Delaunay read a note on the discovery of a new telescopic planet at the Observatory of Marseilles on the 19th April. This is the 110th asteroid of the group between Mars and Jupiter, and M. Delaunay proposes for it the name of *Lydia*. Its position on the 19th April, at $10^h\ 33^m\ 13^s$, mean time at Marseilles, was as follows:—

$$A\ R = 12^h\ 2^m\ 39^s,\ 22.$$
$$D = +6°\ 50',\ 38''.\ 8$$

Horary movements { in right ascension — 1^s 77 / in declination + 2″. 20

Magnitude 12 — 13.

M. Faye presented three memoirs, namely: a report on the operations of M. Respighi in spectral observation of the solar protuberances; a note on the recent experiments of M. Wüllner on the spectra of hydrogen, oxygen, and nitrogen, with reference to those of the solar protuberances; and a note on the processes of photographic observation proposed by M. Paschen for the coming transit of Venus.—A letter from Father Secchi on the results of some spectral observations of the sun was also read.—M. Delaunay presented, on the part of M. Flammarion, a reply to the objections raised by M. G. Quesneville to his law of the rotatory movement of the planets.—M. Chapelas presented a note on a luminous meteor of great brilliancy observed at Paris on the night of the 19th of April. This meteor passed from near σ Herculis to the neighbourhood of $\delta\ \epsilon\ \zeta$ Cephei, describing a trajectory from S. to N. of 48°. Its colour was green, and it had a long train. Its disappearance was preceded by three noiseless explosions, accompanied by flashes which illuminated the hills round Paris. Its apparent size was 6 or 7 times that of Jupiter.—The subject of arctic explorations was treated by M. C. Grad, who suggested as an untried route for attempting to reach the North Pole, the passage through the Sea of Kara and the Siberian Ocean.—The chemical papers were the following: Researches upon new platinic derivatives of the phosphorus-bases, by MM. A. Cahours and H. Gall; on the utilisation of the secondary products obtained in the manufacture of chloral, for the preparation of the æthylamines on a large scale, by M. A. W. Hofmann; thermical investigations of iodic acid, by M. A. Ditte, communicated by M. H. Sainte-Claire Deville; and thermical investigations of the states of sulphur, by M. Berthelot, presented by M. Balard.—M. A. Béchamp communicated a memoir on geological "microzymas" of various origin, in which he described the action of different rocks in producing alteration and fermentation of starch-paste, and sugar. He maintained that in all limestones, from the Great Oolite to the most recent Tertiaries, there exist living organisms (for which he has proposed the name of *Microzymas*), of the nature of the molecular granules observed in certain fermentations, and that these are the agents which produce the changes described by him. He stated that pure carbonate of lime has no such action.—M. Milne-Edwards presented a note by M. Jourdain on the mode of action of chloroform, upon the irritability of the stamens of *Mahonia*. Exposure for from 1 to 3 minutes to the vapour of chloroform was said to destroy temporarily the irritability of the stamens; exposure for 10 minutes or a quarter of an hour kills the portion of the plant subjected to it.—A note on the primitive type of the mammalia, by M. A. Roujou was read.—M. C. Robin communicated a note by M. Girard-Teulon on the law of the rotations of the eye-ball in the associated movements of the eyes, in which the author supported the views of Donders, in opposition to those of Helmholtz and Listing, and indicated what, in his opinion, had led the latter writers to a false conclusion.—The Abbé Richard communicated an account of the discovery of a workshop for the manufacture of flint instruments in Palestine.—This workshop is near the village of El-Bire (the ancient Beëroth), about twelve kilometers from Jerusalem; the author found *haches*, scrapers, knives, and saws, the last said by him to be very remarkable.—Besides these, and two papers on medical and surgical subjects, several notes were read of which the titles only are given.

VIENNA

Imperial Academy of Sciences, Feb. 3.—Prof. P. Redtenbacher presented on the part of Prof. H. Will, "An investigation of white mustard seed." In place of the myronate of potash of black mustard seed, the white mustard contains *sinalbine*, decomposable into sugar, a sulphocyanic compound and an acid sulphate. The sulphocyanic compound contains the radical *akrinyle* $C^7 H^7 O$, and when freed from sulphur and treated as nitryle with alkali furnishes ammonia and the salt of an acid = $C^8 H^8 O^3$, which melts at 277° F., and is not identical with any known acid of the same formula. The acid sulphate contains sinapisine, and the author contrasts the products of black and white mustard as follows:—

	Myronate of Potash.		Sugar.		Mustard Oil.		Sulphate of Potash.
1.	$C^{10} H^{18} NS^2 KO^{10}$	=	$C^6 H^{12} O^6$	+	$C^4 H^5 NS$	+	$SO^4 K H$
	Sinalbine.		**Sugar.**		**Sulphocyanide of akrinyle.**		**Sulphate of sinapisine.**
2.	$C^{30} H^{44} N^2 S^2 O^{16}$	=	$C^6 H^{12} O^6$	+	$C^8 H^7 NOS$	+	$SO^4 (C^{16} H^{14} NO^5) H$

February 10.—Mr. Joseph Rauter forwarded a memoir on the "Developmental history of some of the hairy structures of plants belonging to various families of *Dicotyledons*." He noticed that in some cases the hairs are simple products of the epidermal cells; whilst in others, although the first rudiment of the hair takes its origin from an epidermal cell, at a later period the subjacent parenchyma and the neighbouring epidermal cells take part in its structure; and in others, again—such as the spines and glandular hairs of roses—the first rudiment of the structure springs from the subcuticular tissue. As examples of the first mode of development, he cites the woolly hairs of *Ribes*, *Rosa*, &c., the stellate hairs of *Hieracium pilosella*, and the glandular hairs of *Hieracium*, *Azalea*, &c.; of the second, the stings of the nettle, the clinging hairs of the hop, and some other forms.—Dr. Boué presented the first portion of his mineralogico-geognostic observations made during his travels in Turkey in Europe, and relating to North Albania, Bosnia, Hergezowina, and Turkish Croatia.—Prof. J. Redtenbacher communicated the results of an investigation of some Austrian hydraulic magnesian lime which had been made in his laboratory by M. P. G. Hauenschild. The material contained about sixty per cent. of carbonate of lime, and thirty per cent. of carbonate of magnesia; when burnt at about 752° F. it furnished an excellent hydraulic cement.—Prof. Hlasivetz made a preliminary communication "Upon a new acid from grape sugar." He referred to his discovery of lactonic acid by the treatment of sugar of milk with bromine, and indicated that the only difference between the two bodies consists in the presence of one more atom of oxygen in the acid. A similar acid was produced by treating other sugars with bromine, but the hydrobromic acid formed in the reaction prevented its separation. The author induced M. Habermann to treat grape sugar with chlorine instead of bromine, and he has obtained the expected acid, having the formula $C^6H^{12}O^7$.—Dr. S. L. Schenk communicated a memoir "On the distribution of gluten in the wheat-grain," and Prof. Lang some "Crystallographic-optical determinations" relating to thirteen substances, chiefly of organic origin.

Imperial Geological Institution, April 5.—Th. Fuchs on the fossil shells of the Congeria-beds from Radmanest, near Lugas (Banat). A comparatively large number of species (48, 32 of them hitherto unknown in Austria), but of small size, characterise this fauna, and correspond to the fauna of the so-called Upper Steppen-Kalk (limestone of Odessa) in southern Russia. The small amygdaloid *Congeria simplex*, recently described by Barbot de Marny, which alone forms whole beds of the Odessa-limestone, is also found abundantly in Radmanest. Among the small Cardiadæ of the Odessa-limestone, some species differ from the usual type of the genus by possessing a sinuated pallial line. Very interesting is therefore the discovery of a new species of *Congeria* at Radmanest, which differs in the same way from the generic type of Congeria.—Ch. v. Hauer on the coking of brown coal. A series of experiments with the brown coal from Fohnsdorf (Styria) gave very satisfactory results. Cokes were obtained with a heating power equal to that of the cokes of good black coal; the proportion of sulphur was essentially diminished, and the cokes are firm enough to support the pressure of a high furnace. V. Hauer thinks that, mixed with cokes of black coal, they would be applicable to the smelting of iron, a problem the solution of which would be of the utmost importance for the iron manufacturers in the Alpine iron districts.

April 23.—Ferd. Baron v. Andrian on the Volcanic Rocks of the Bosphorus. An accurate investigation of the geological relations, the mineralogical characters, and the chemical constitution of these rocks, which border the mouth of the Bosphorus to the Black Sea, served to distinguish a series of different varieties which in part are almost perfectly identical with the trachytic rocks of Hungary and Transylvania. In both countries generally three types may be distinguished, viz.: green andesites and dacites; black augit-andesites, and rhyalithes.—Prof. Ch. Zittel from Munich contributed some remarks on the tithonic strata. His important memoir on the fossils of the Stramberg limestone will be followed within a few weeks by another on the fossils of the cliff limestone (Klippenkalk) of the environs of Rogoznik and Csarsztyn (Galicia), and the tithonic cephalopod-limestone of Southern Tyrol and Italy. His studies have brought him to the conclusion that an exact line of demarcation does not exist between the Jurassic and the Cretaceous formation. Prof. Ch. Hoffmann of Pesth announced the discovery of Triassic fossils in the older Dolomite—and limestone-rocks of the environs of Ofen, which had formerly been thought to belong to the Rhaetic series. — M. Ch. Paul has examined the Lignite-beds of western Slavonia. In a thickness of more than 6 feet they are to be found along a line of 15 German miles in length, they belong to the upper Miocene formation (Congeria or fresh-water beds of the Vienna Basin), which contains many fossils, among them chiefly to be noticed a very large new species of Unio.—Dr. Em. Tietze, of Breslau, spoke about the fossils of the carboniferous limestone of Silesia. Nearly two hundred different species have been found therein, they will be described in a special memoir.—M. Fr. Posepny on the lead-mines of Raihl in Carinthia. The ores are imbedded in irregular masses in a stratified limestone of Triassic age. They form neither layers nor true veins, but are dependent on dislocations in the limestone-strata, and the over-lying schists.

BERLIN

German Chemical Society, March 28.—M. G. Krämer has investigated the products accompanying the formation of chloral from alcohol. Besides chloride of ethyl already recognised by Hofmann, both chloride of ethylene, and chloride of ethylidene, as also the monochlorinated substitution compound of both have been isolated. Chlorinated chloride of ethylene $C_2 H_3 Cl_3$ boils at 115° and yields with potash $C_2 H_2 Cl_2$ boiling at 37° and transforming itself into a solid polymeric modification.

Chloride of ethylidene and ammonia produce collidene. Professor Hofmann, in continuance of former researches, has transformed methylated and amylated sulpho-ureas into trimethylated and triamylated melamines by the action of oxide of mercury. This reaction however is but secondary, the first products being substituted (neutral) cyanamides, which by repeated evaporisation become suddenly transformed into alcaline melamides. The ethylic, and the phenylic sulpho-ureas behave in the same manner. The transformation consists in three molecules of cyanamide uniting into one of melamine—

$$3\,C\,H\,(C_2\,H_5)\,N = C_3\,H_3\,(C_2\,H_5)_3\,N_3$$

The same chemist, in conjunction with Dr. Olshausen, publishes researches on polymeric modifications of cyanetholine, and its homologues. These researches are connected with the foregoing paper by the following consideration. A certain analogy between ethylcyanamide $C\,N\,(C_2\,H_5)\,H\,N$ and cyanetholine—$C\,N\,(C_2\,H_5)\,O$ allows us to predict that the latter will treble, in the same way that the former does. This has been found to be the case. By passing chloride of cyanogen into methylate of sodium, the cyanetholine of the methylic series (an oil) forms at the same time, with crystals of the formula—

$$\left.\begin{matrix}(C\,N)_3\\(C\,H_3)_3\end{matrix}\right\}O_3$$

cyanurate of methyl. These crystals fuse at 134° but are transformed by distillation into the isomeric compound—

$$\left.\begin{matrix}(C\,O)_3\\(C\,H_3)_3\end{matrix}\right\}N_3$$

fusing at 175°. The former treated with potash yields cyanuric acid, and methylic alcohol; the latter carbonic acid and methylamine. The former, treated with ammonia, forms the dimethylic ether of amido-cyanuric acid—

$$C_3\,N_3\left\{\begin{matrix}C\,H_3\,O\\C\,H_3\,O\\H_2\,N\end{matrix}\right.$$

The same compound is formed (together with cyanurate of methyl) when chloride of cyanogen is passed into methylate of sodium, and may be separated from the cyanurate, by the action of ether, in which it is insoluble. The circumstance that the corresponding ethyl-compound dissolves in ether, renders the investigation of the transformation of cyanetholine more difficult. Analogous results have been obtained when chloride of cyanogen was passed into amylate and phenylate of sodium.

Professor Rammelsberg, in a paper on the phosphates of thallium, stated that isomorphism exists between

1. $H\,Tl_2\,PO_4\,H_2\,O$ and $H_2\,Na\,PO_4\,H_2\,O$
2. $H_2\,Tl\,PO_4$ and $H\,(NH_4)_2\,PO_4$
3. $H\,Tl_2\,PO_4$ and $H_2\,(NH_4)\,PO_4$

This he considers as the first proof of the isomorphism of hydrogen with monatomic metals. The same is stated of a phosphoborate of magnesium found in the saltlayers of Lüneburg, and analysed by Nöllner, who gives it the formula $Mg\,B_2\,O_4.\ 2\,H\,Mg\,PO_4$. De Koninck and Marquardt have investigated Bryonicine, one of the two bases contained in the roots of *Bryonia dioica*, and give it the formula $C_{10}\,H_7\,N\,O_2$. P. Marquardt described polybromides of tetraethyl ammonium. Dr. Coninck described modifications of Bunsen's sucking apparatus for filtering, and of Mitscherlich's potash bulbs for combustion. M. Ballo recommends the preparation of binitronaphthol by oxydising naphthylamin with nitric acid. By the action of monobrominated naphthalin on rosaniline, he has produced a violet colouring matter, not yet analysed. W. Doer has prepared azonaphthaline by heating nitronaphthaline with zinc powder. F. Rochleder has found four new colouring substances in madder, $C_{14}\,H_8\,O_4$ isalizarine; and its homologue, a very similar substance, $C_{15}\,H_{10},\,O_4$; a third called hydrisalizarin, $C_{28}\,H_{18}\,O_8$, and a fourth, homologous with the foregoing, $C_{29}\,H_{20}\,O_8$. The proportions in which these substances occur in madder are minute. N. Bunge on electrolysis communicates that nitrophenate of potassium yields to the anode nitrophenol and oxygen. Thiacetic acid and thiobenzoic acid yield bisulphide of acetyle, and bisulphide of benzoyl. But sulphocyanide of potassium, instead of yielding bisulphide of cyanogen, gives pseudo-sulphocyanogen. L. Henry has proved the identity of tribromhydrine of glycerine with the tribromide of allyle, from which it has hitherto been considered to differ. N. Lubarin, in submitting chloraluric acid to a renewed investigation, has arrived at the conclusion that it is impure parabanic acid mixed with chloride of ammonium. A. Ladenburg has found that, in support of Dr. Wanklyn's opinion, acetic ether perfectly free from water is not attacked by sodium below 100° C., and that in this reaction no evolution of gas takes place. For decomposing the water and alcohol generally contained in what is called pure acetic ether, the chlorides of silicium or of phosphorus were employed. Lastly, M. Vogel reported on Camuzet's experiments on gun-cotton, which differ so entirely from everything hitherto asserted, that they require confirmation. According to Camuzet, water dissolves the greater part of gun-cotton, separating at the same time the remaining part into a flocculent mass (the explosive ingredient of gun-cotton), and a granular non-explosive powder, which falls to the bottom of the vessel.

DIARY

THURSDAY, MAY 5

ROYAL SOCIETY, at 8.30.—On the Pre-Carboniferous Flora of North-Eastern America, and more especially on that of the Erian (Devonian) Period (Bakerian Lecture): Principal Dawson, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.—On the Date of the Discovery of the American Continent, by John and Sebastian Cabot: R. H. Major, F.S.A.

LINNEAN SOCIETY, at 8. CHEMICAL SOCIETY, at 8.

ROYAL INSTITUTION, at 3.—Electricity: Prof. Tyndall.

FRIDAY, MAY 6.

ROYAL INSTITUTION, at 8.—Star-grouping; star-drift; star-mist: R. A. Proctor.

SATURDAY, MAY 7.

ROYAL INSTITUTION, at 3.—Comets: Prof. Grant.

MONDAY, MAY 9.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

LONDON INSTITUTION, at 4.—Botany: Prof. Bentley.

TUESDAY, MAY 10.

ETHNOLOGICAL SOCIETY, at 8.30.—(Special meeting at the Museum of Practical Geology). Opening address: Prof. Huxley. On the Influence of the Norman Conquest in the Ethnology of Britain: Rev. Dr. Nicholas.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Discussion on the Strength of Iron and Steel. Recent Improvements in Regenerative Hot Blast Stoves, for Blast Furnances: E. A. Cowper.

ROYAL INSTITUTION, at 3.—On the Principles of Moral and Political Philosophy: Prof. Blackie.

PHOTOGRAPHIC SOCIETY, at 8.

WEDNESDAY, MAY 11.

GEOLOGICAL SOCIETY, at 8.

ROYAL MICROSCOPICAL SOCIETY, at 8.—On a new form of Binocular and Stereoscopic Microscope: Mr. Samuel Holmes.

ARCHÆOLOGICAL ASSOCIATION, at 8.

THURSDAY, MAY 12.

ROYAL INSTITUTION, at 3.—Electricity: Prof. Tyndall.

ZOOLOGICAL SOCIETY, at 8.30.—Notes on some points in the anatomy of certain Kingfishers: Dr. Cunningham.—On the taxonomic characters afforded by the muscular sheath of the œsophagus in Sauropsida and other Vertebrates: Mr. George Gulliver.—Notes on the myology of *Platydactylus Japonicus*: Mr. Alfred Sanders.—On the Hirundinidæ of the Ethiopian region: Mr. R. B. Sharpe.

LONDON MATHEMATICAL SOCIETY, at 8.—On the Mechanical description of a nodal bicircular Quartic: Prof. Cayley.—Concerning the ovals of Des Cartes: Mr. S. Roberts.

BOOKS RECEIVED.

ENGLISH.—Choice and Chance: Rev. W. A. Whitworth (Deighton and Bell).—Blanford's Natural History of Abyssinia (Macmillan and Co.).—The Lifted and Subsided Rocks of America, by Catlin (Trübner and Co.).—The Yosemite Guide-book: J. D. Whitney.

FOREIGN (through Williams and Norgate).—Ornithologie Nordost Afrikas Th. von. Heuglin: Elektrische Untersuchungen, achte Abhandlungen über die thermoelektrischen Eigenschaften des Topases: W. G. Hanke.—Bestimmung der Sonnenparallaxe durch Venus-vorübergange vor der Sonnenscheibe: P. A. Hansen.—La psychologie anglaise contemporaine: T. H. Ribot.—Das Verhalten der Eigenwärme in Krankheiten: D. C. A. Wunderlich.—Verhandlungen der k. k. Zoologisch-botanischer Gesellschaft in Wien 1869.—Berichte über die Verhandlungen Ost Afrika.

CONTENTS

ERRATUM.—By an error of the press, Prof. Duncan's Table of Madreporaria dredged up in the 'Porcupine' Expedition (No. 26, p. 663), was designated "Madreporaria of the Red Sea," instead of the "Deep Sea."

The INDEX and CONTENTS for Vol. I., will be published with an early number.

THURSDAY, MAY 12, 1870

A BUILDING FOR THE LEARNED SOCIETIES

THE Statistical Society has done good service to the cause of science in convening representatives of the learned societies, to consider whether it would not be possible to obtain a building for their accommodation worthy of the high position they occupy in this great metropolis. At this moment several societies are under notice to quit, others scarcely know where to look for shelter, and many more are utterly unable to find sufficient room for their libraries, instruments, and museums, though they pay a large portion of their income in rent and taxes. It is calculated that, jointly, upwards of 2,000*l.* a year is now paid in rent, enough, one would think, if properly managed, to supply most ample accommodation for very many societies, to say nothing of the great economy in service that would result from the joint occupation of a proper building. But so long as nothing is done to bring about some understanding and co-operation among the different societies, the evil is irremediable. Nor is it purely a question of finance. Many abstain from joining a learned society when its place of meeting is either inconveniently situated, or altogether too small for the usual attendance at the ordinary meetings. Not a few members of more than one society are unnecessarily driven from one place to another. The libraries for reference are not half utilised. Co-operation among men of science is almost impossible, and the action of each society is rendered thereby comparatively feeble and ineffective. On every ground, whether of convenience, economy, or utility, the learned societies would do well if they could combine in erecting a building sufficiently capacious for their joint accommodation.

Some learned societies have no reason to complain. The Royal Society, the Linnean, the Royal Astronomical, the Geological, the Chemical, the Society of Antiquaries, and a few others, are well accommodated, and a solid structure is being raised for them in Piccadilly. Those whose wants are yet to be supplied are the Statistical Society, the Institute of Actuaries, the Mathematical, Meteorological, Ethnological, Anthropological, Geographical, Archæological, and Juridical Societies, the Social Science Association, and as many more; and it is for them to consider whether it is better to go on as they are doing, paying one, two, or four hundred pounds a year each for their present rooms, or whether they would not do better by combining together for the erection of a proper building for them all. In calculating how many societies could unite for such a purpose, we must take into account the kindred character of their labour and inquiries. The statisticians, actuaries, and mathematicians might well meet together, and so it would be fitting that the antiquaries and archæologists should have a common habitation. But not so those that have nothing in common. Then the number of members and the space required for meetings, libraries, and museums are important elements. The Geographical Society, with its map-rooms and extensive library, would require space enough for a dozen other societies. And further, the frequency of meetings must be considered, when at the most only three or four commodious halls could be secured in any one building. We scarcely imagine, in fact, that any large number of societies could well be united in one building, and that will be a source of difficulty, especially in a financial aspect.

But why should there be any financial difficulty? Surely the erection of one or more buildings for purposes of science is a duty which may well rest with the Government. Nowhere does the State do so little for science as in this country. The estimates for 1870-71 give the entire sum to be applied to the learned societies at 2,370*l.*—a sum distributed among very few of them. Of this 500*l.* goes to the Royal Geographical Society, to provide suitable rooms in which to hold their meetings, and to exhibit to the public, free of charge, their collection of maps; 300*l.* is given to the Royal Society of Edinburgh; 500*l.* to the Royal Academy of Music; and 70*l.* to the Irish Academy of Music. In addition to this 160,000*l.* are appropriated to a building for certain learned societies in Burlington House; but it will serve for very few of them; and if we are rightly informed, the Government will reoccupy all the buildings in Somerset House now used by learned societies.

The chairman of the meeting at the Statistical Society stated that communications had passed between himself and the Chancellor of the Exchequer, and that no encouragement whatever was given for any application of this nature. But it is the clear duty of the societies not to rest satisfied with this, but to get a decided expression of opinion on the subject. By no means should the most natural and proper channel for obtaining the requisite sum for a building so essential to the well-being of the country be neglected.

But supposing the Government should turn a deaf ear to the application of the learned societies, are there no means available within these bodies themselves for getting the amount? It has been estimated that the probable cost of a building sufficiently commodious, though not ornamental, in some eligible locality near Charing-cross, will be, with the ground-rent, 30,000*l.* to 40,000*l.* Why should not a joint-stock company be formed for the purpose, and a large number of shares be taken up by the members of the societies interested? Some societies have moreover an accumulated fund of considerable importance. The Geographical Society has, it is understood, upwards of 20,000*l.* What more natural than to apply such investments in a palace of science, with an income so well guaranteed by the rental of the learned societies? The financial part of the question must be carefully but fearlessly approached. No insurmountable difficulties stand in the way of obtaining any reasonable amount for such a purpose.

What we want is a prompt and vigorous action on the part of the learned bodies. Heaven helps those who help themselves. The delegates at the meeting at the Statistical Society unanimously resolved in favour of co-operation on the subject, but by an unfortunate introduction of too cautious a spirit, they let the opportunity slip without naming a committee to prosecute the necessary inquiries and to digest a suitable scheme. Such a committee could not have committed the parties to any course of action. Its object would have been to place the proposal on a practical basis, so as to enable the societies to come to a right decision. As it is, the Conference decided to invite the societies—first, to confirm by their separate vote the joint resolve of their delegates, and then to give proof of their interest in the attainment of the object by nominating one or more members to act on a committee to be appointed for the purpose. The evil of such a course is, that much valuable time is lost in correspondence and negotiation before any practical step is taken in the matter. After all, however, the delay may be useful in ripening opinion on the subject. What is wanted in the steps eventually to be taken is energy of purpose and promptitude of action, for we are sure that the object in view will be eminently conducive to the welfare and progress of Science in the United Kingdom.

FOSSIL OYSTERS

Monographie du Genre Ostrea—Terrain Cretacé. Par Henri Coquand, Docteur és-Sciences, Professeur de Geologie et Mineralogie. (Paris: Baillière, 1869.)

OF the many able geologists whom France has produced, few have had better opportunities of observation, or have availed themselves of them to better purpose, than the author of this monograph. Distinguished alike by his skill and long experience as a palæontologist, and by his extensive knowledge of practical geology, M. Coquand has laboured long and well, and far and wide—not only in Provence, and Italy, and Germany, but in far distant regions in Spain and Africa, in valleys and mountains never before resounding to the blows of the geologist's hammer.

Those who are only acquainted with the chalk as it is seen exposed in quarries and cuttings on the green downs and wolds of England, can form but a very imperfect notion of its true character. As M. Coquand has shown in a paper lately read before the Geological Society of London,* the chalk of England, extensive as we are accustomed to regard it, is but a fragment when compared with that which is seen in the South of France. The utmost thickness of the English Cretaceous beds is found to be about 900 feet, while in Provence the same, or rather the equivalent beds are more than 4,000 feet thick. In England we are accustomed to arrange the chalk into three or four divisions, while in France their more extended development requires an entirely different arrangement, and thus we find no less than eleven different beds, the character and limits of which are now ascertained with great accuracy.

The French strata being thus so much more largely developed than the English, the character of the fauna is, as might be expected, infinitely more varied. The difference of nearly three thousand feet is principally represented in France by several marine and freshwater beds altogether unknown in England. In some places, as at La Cadière and Martigues, we find extensive beds of Hippurites and Radiolites—fossils almost unknown in England, lying ranged in close order as when they lived; and again, while we have been accustomed to regard the chalk as altogether of marine origin, we find in Provence a district of about 250 square miles, in which the upper chalk strata of England and the Charentes of France are represented by freshwater deposits 1,400 feet thick. These contain several hundred species of land and freshwater shells unknown elsewhere, associated with beds of lignite as compact as our own Newcastle coal, and like it worked extensively for fuel. Both from the palæontological and the geological evidence it would seem as if, at some time, while our Cretaceous deposits were interrupted and stationary, others of great magnitude, with a succession of faunæ essentially differing, as well from our own as from each other, were accumulating in the South of France, alternately depressed and elevated—sometimes a deep sea, sometimes a great lake, and not improbably at one time dry land.

* Quarterly Journal G. S., Aug. 1869, vol. 25, part 3.

After a careful study of the Cretaceous systems of many countries, M. Coquand, undeterred by the dread of taking charge of a family at once so numerous and so troublesome, has now been induced to prepare this monograph of all the Cretaceous oysters wherever found, to be followed by like monographs of the Tertiary, Jurassic, Triassic, and Permian formations. It is, we believe, the first, or at least the most important attempt to give a synopsis of any one genus occupying so extensive a range.

The results of M. Coquand's researches are sufficiently striking; he describes no less than 255 distinct species of chalk oysters (including Gryphæa and Exogyra), and of these he has given excellent figures in an atlas of 75 plates, in folio. As regards England, he has disclosed the poverty of the land as compared with our neighbours. Our chalk oyster beds have been examined as assiduously as the French, but they have been found much less prolific, While France possesses 115 well-marked species, England. according to Mr. Morris's catalogue, can show but 25, all of which, except one (*O. triangularis* of Woodward), seem to be found also in France.

Nor is the range of some species less remarkable than their abundance. Two of them (*vesiculosa*, and *ungulata* or *larva*) appear to be altogether cosmopolitan; the former being found alike in England, France, Algeria, Belgium, Spain, Poland, Russia, Sweden, North America, and Mexico; and the latter having been also traced through all these countries (except Poland and Mexico), and extending its range also to India.

But while some species are thus prone to wander, others are to be noted for their domestic habits. Out of 49 American species, five only have been met with in Europe; and of 27 in Russia, and 23 in Spain, no less than 10 in each of these countries are not found elsewhere.

It seems evident from our author's observations, that so far as these fossils are concerned the several zones of chalk which he has described are divided by "a hard and fast line," marking the limits of each as clearly as the Tertiaries are separable from the Secondary rocks. Of the several *Dordonien* species, not one is found in the *Campanien* beds, and of ninety-five found in the *Campanien* none are found in the lower beds, and the same observation applies to each of the seven or eight inferior deposits. Although it transcends all our powers of calculation to form even a conjecture, much less an approximate estimate of the ages of ages that should be allowed for the *creation* (or, if that word be not allowable, for the introduction or evolution) of these various forms, and the extinction of their predecessors, we may yet gather from these materials a somewhat better, although still utterly inadequate notion of the extreme deliberation, so to speak, exhibited in building up this portion of the earth's fabric.

M. Coquand's memoir can hardly fail to be welcomed as a valuable addition to our palæontological and geological literature, both for what it is, and for what it suggests. Monographs of the fossils of any one country can only be regarded as so many *Mémoires pour servir*—words and lines, rather than pages—of the geological record. Incomplete as that vast history must ever remain, it would be found far more available than it is if we could have a synopsis like the present of every important genus. Owing to their wide range, and the usually good state of preservation in which they are found, the study of these fossils cannot fail to be of value with reference to some questions of much present interest. As compared with the fauna of the Cretaceous seas, the fossil mammals of the Quaternary period, to which reference is so often made in these discussions, afford but very imperfect materials for testing the various theories which are from time to time put forward as to the succession of species. These Quaternary beds usually exhibit but broken fragments—*disjecta membra*, which, while lying on the surface, or tossed about in company with river or deluge gravels, have been subjected to so many chances and changes that the order of succession is often difficult, if not impossible, to ascertain; while, on the other hand, the fossils of the chalk, slowly accumulating during countless ages in the quiet depths of their seas, exhibit the exact order in which their multitudinous genera and species successively made their appearance and, having endured for their appointed seasons, finally disappeared.

If we could have monographs of other important families arranged upon a plan as comprehensive as M. Coquand's, how much less unsatisfactory might our speculations be upon the perplexed and perplexing questions of the origin, distribution, and extinction of species. When we consider the subjects of this memoir, their great variety and wide dispersion, although we might perhaps think it possible that, as we have heard, an oyster should be "crossed in love," we find it difficult to imagine the creature as existing under such conditions that one species, while engaged in "the struggle for existence," should starve out and extinguish another; or that any process of "natural selection" should avail to alter the formation of the hinge as well as the internal and external structure of the shell. Indeed, if any such change did occur, it must have been *per saltum*, since with these mollusks, numerous as they are, there are no forms that can fairly be recognised as transitional; for just as each zone or region of the chalk is marked by the presence of its peculiar fauna, so each species of this numerous family has a character of its own; it is *sui generis*, apparently without ancestors and without descendants. If, indeed, all the members of this great family, by virtue of some law or process of evolution, did descend from one common ancestor, we should expect to find their forms varied and numerous, instead of being, as to our sorrow we find them, more simple and far less numerous; so that instead of being permitted to choose from the two hundred and fifty-five kinds described by M. Coquand, we are reduced to the pitiful allowance of one poor "*native*," and from what we see and hear of *him* it seems not unlikely that he is to be the last of his race, and that ere long, Oysters, like Mastodons will be things of the past.

J. W. FLOWER

OUR BOOK SHELF

Mrs. Loudon's First Book of Botany, for Schools and Young Persons. New edition, revised and enlarged. By David Wooster. (London: Bell and Daldy, 1870.)

WE wish we could speak more favourably of this prettily got-up little book. Mrs. Loudon's writings did good service in cultivating a love of plants among the last generation; but when a new edition of an old manual is brought out, with the date of the current year on the title-page, and an editor's name as having "revised" it, we expect that it will be corrected by the light of the present state of scientific knowledge. In the present instance this has not been adequately done; of the inadequacy we may give but two instances. At p. 18 prickles are described as metamorphosed leaves, instead of, as they really are, indurated hairs, or processes of the epidermis. But a more serious erroneous description occurs in the case of the spores of ferns, which are said to differ from seeds "in not requiring to be fertilised by pollen" (do seeds require to be fertilised by pollen?) The reader is left to suppose that the young fern-plant springs direct from the spore, no reference whatever being made to the recent discoveries of the functions of the *pro-thallium*, *archegonia*, and *antheridia*. The arrangement is good, as also are some of the illustrations; but the book cannot be used as a manual by teachers or lecturers, without the errors being corrected from some other handbook.

A. W. B.

The Birds of Asia. By John Gould, F.R.S.

THE twenty-second part of this magnificent work has just been issued to the subscribers. It contains fifteen plates coloured by hand, including the great alced, four owls, two pheasants, three buntings, three piculets, Franklin's barbet, and the long-billed wren, accompanied by letter-press descriptions. Among so much that is beautiful and interesting, it is very difficult to particularise; but we cannot help referring to the charming little owlet dedicated to the late Sir Benjamin Brodie, the eminent surgeon, and named *Athene Brodiei*. Among the peculiarities of the bay owl found in Nepaul and the northern confines of India, Mr. Gould notices its friendship for wild animals, living on good terms with the tiger, and sometimes alighting on its back. We learn that one of the pheasants, the Chinese *Crossoptilon*, or Dallas's eared pheasant, is now domesticated in our Zoological Gardens; also that some eggs have been hatched there, and that female birds may be purchased for 15*l*. The long-billed wren (*Rimator malacoptilus*, Blyth), a small reddish-brown bird, with a droll apology for a tail, is said to be excessively rare, and one of the most curious and highly-interesting species in the Indian avi-fauna.

Zoologie et Palæontologie générales. Par Paul Gervais, Prof. d'Anatomie Comparée au Museum d'Histoire naturelle de Paris. Première Série. 4to. Planches 50. (Paris: Bertrand, 1867–69.)

THIS handsome volume, with its carefully executed plates, is, as the author states, an endeavour to make the treasures accumulated in the Museum of Natural History available for the advance of science. The present part is occupied with the consideration of various living and fossil vertebrated animals, and is introduced by a long account of the arguments, most of them familiar to our readers, respecting the duration of man's habitation of the earth, together with minute descriptions of bones of the animals found in various caverns in France. The second chapter treats of the Fossils of Armissan (Aude); the third of animals living at the present time in the French possessions in the North of Africa; the fourth of some fossil reptiles of the secondary period, especially including the archæopteryx; the fifth and last considers the different species of fossil reptiles.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The late Captain Brome

A SHORT time since I announced in your columns the decease of Captain Fred. Brome, late of Gibraltar, and well known to many of your geological readers for his great and successful labours in the exploration of the caves and fissures of the Rock. I then stated that Captain Brome had left a widow and eight children, wholly unprovided for; and this is literally the case. My object in this communication is to state that his numerous and warm friends in Gibraltar, and at Weedon where he died, have already commenced the collection of a fund for the relief and maintenance of his helpless widow and family, and to request that you will allow me space to say, that I shall be happy to receive and forward any contributions in aid of this fund.

Captain Brome was for twenty-two years Governor of the Military Prison at Gibraltar, from which post he was displaced towards the end of 1868. His removal to England last year with his large family necessarily involved him in considerable expense, incurred in the hope that his new appointment at Weedon might afford him a home and some prospect of providing for his children's education. These hopes, however, were destroyed in less than twelve months by the announcement that the prison at Weedon was to be disestablished. The anxiety lest he should thus be left without prospect of employment, and, as he feared, without any provision for the wants of his family, caused him such distress that, although a strong and energetic man and in the prime of life, he gradually sank, and died from mental depression on the 4th of March.

It is impossible to conceive a case more deserving of sympathy and support than that of his unhappy widow and children, or one more deserving of recognition by all lovers of science than that of Captain Brome, who had gratuitously devoted several years of his life, and the most unwearied personal labour, simply because he believed, and truly believed that he was promoting a scientific object.

Subscriptions will be received by me, and, I am kindly permitted to say, by Mr. W. S. Dallas, at the apartments of the Geological Society, Somerset House.

32, Harley Street, May 6. GEO. BUSK

Relations of the State to Scientific Research.—II.

SCIENTIFIC men are of three kinds: the young, the middle aged, and the old. It is difficult to say which needs help the most; but there is one work in which they can all severally take part, and from which they can each obtain that comfortable leisure which is the one thing needful for original research. That work is simply the work of teaching. And here let me not be misunderstood. By teaching science, I do not mean the miserable practices now carried on, but teaching on a scale commensurate with the great needs of a great nation, and in a way calculated to bring about the blessings that follow inevitably on true, thorough scientific knowledge.

To illustrate my meaning, let me take a particular science, chemistry, as an example. Of the whole population of England there will certainly be a certain number of men whose minds are so set on chemistry that they would be willing to accept, while in the prime of life, with gladness the offer of posts which, while taking up about half their time in teaching chemistry, would enable them to devote the entire other half to original work, and yet bring up their families in decency and order. What the exact number of such men would be, I do not care to know; it probably would never be very great; it certainly would never exceed the demand for men to fill chairs of chemistry at properly organised laboratories established at various points all over the kingdom.

Now a livelihood may be gained, and even a fortune made, by teaching, but this can never be done by working half time. But such a man as we are picturing must work half-time only; and therefore the receipts from his actual teaching must be subsidised from elsewhere—the chair must be endowed by the Government, either local or imperial.

The occupant of such a chair would not be an idler. No idler would seek a post which would always entail a large amount of labour, and never would bring wealth. At the very worst, even if he did no original work, he would earn his salary by teaching. If he taught badly, that is a thing which can readily be recognised by competent persons, and he could be dismissed. The very desire of a man to take such a position would be of itself almost a guarantee that he would perform its duties properly, and bring forth the fruit expected of him. And calling to mind the well-known law of human nature that the more work a man has to do, the more he over-abounds in work, we may feel sure that the half life which teaching leaves to such a man will be filled with a whole life's exertion.

The work of such a man would lie almost exclusively in the way of systematic lectures and general superintendence of the laboratory. I need hardly say that that ought to be a small part only of the total teaching done in the place. There must be attached to the professor two, three, or more recognised assistants, who would be always in the laboratory, who would personally direct and nurse the students, who would carry on original work, partly on their own account and partly on behalf of their master, and who would receive a moderate fixed salary, sufficient to enable them to live without having to look to any other extraneous sources of income. Such men would of course be embryonic professors; and I know of no more pressing need than this, of finding livelihoods for young promising men in the interval between the studentship and the professorship. I weep when I think of how many admirable young men become outcasts to science for lack of these. It has been so with myself: full of zeal for science in my youth, and, what is more important, rich in the germs of large ideas, which I have since seen flourishing in other men's minds and bringing forth fruit of fame, I could find no resting place. I threw myself into practical money-getting life, with the hope that after a while my gains would provide me a comfortable afternoon of old age, in which I might return to my former love. I now have both time and money; but, alas! my mind has grown stiff in the ways of the world: the old ideas of my youth are now vain shadows which I cannot grasp. I find myself a wretched puddler, full of egotistic hobbies, productive of little oddities and trifling curiosities, but bringing forth nothing of real value or permanent worth. The young men make fun of me, and the chief men treat me with a courtesy which is at once patronizing and forced.

What is true of chemistry is, with minor differences, true of the other sciences. Under such a scheme as I have pictured, both young and middle-aged would be provided for. With a sufficient number of laboratories, some large and some small, some with eminent, some with useful men at their head, some with many, some with few assistants, it would come to pass that on the one hand the younger men would work under the beneficial influence of their chiefs, while on the other the men full of thoughts would find heads and hands near them to carry out their ideas. Is it not a crying shame that at the present time such a man as Huxley is completely isolated from the younger biological workers, and instead of, like Cuvier, having a large laboratory manned by an enthusiastic body of scholars, ready to dissect everything after its kind, is penned up in an abominable den in Jermyn Street, and distracted by the demands of triflers; has, in fact, to work upon the world through the bars of a prison cage? Is it not also a shame that one of the acknowledged foremost teachers of mathematics in Europe, in the focus of our national life, should feel himself compelled to forsake the work of teaching for a subordinate unscientific appointment in a University, when his right place would have been as instructor of the rising mathematicians of England?

But besides teaching, there is the task of examining the taught. And here again is a source of easy livelihood. I do not mean such kind of examinations as are carried on at present; that wretched system of papers, worked through at the rate of so many dozen a day and paid for at so much a hundred—work done by steam and ending in smoke. I mean a thorough system of *practical* examinations, carried on slowly and quietly, by a staff of professors and their assistants, and paid for in respect of the immense contingencies that hang upon the result and of the vast responsibilities of the examiners. I have not space to dwell on this; but it is a point which wants working out thoroughly and well. The task of examining ought to be one of the richest sources of income to a large number of scientific men, instead as now the odd pence of a few.

I maintain, then, that teaching and examining combined would support all the young and middle-aged scientific men in this country that have sound reasons for devoting themselves to a scientific life, and support them honourably and productively.

Touching the old, little need be said. Every man who has filled one of the above posts worthily while he had force to work ought to be pensioned when he gets old. The matter lies in a nutshell and needs no more words. But there will always be a few old men who for their eminence and their services would require special provision, and that not so much on their own account as for the sake of the younger men of their time. There ought to be some *rewards* for a scientific life, but they should be few and very carefully allotted. At all times, moreover, there will be a few, a very few men whose genius ought to receive plenteous and present recognition. Such men with the older distinguished men might form a small consulting body whose services in the way of advice would be at the command of Government, and the members of which would draw salaries on a scale feebly imitative of those of other Government officials. I believe the legal advisers of Government are pretty well paid, and yet scientific advice is altogether unrewarded. Such men would be then at liberty to work out their ideas; the best means being, of course, taken to choose those men only into whose soul the iron of science has entered, men whom it is impossible to keep from work.

Two more remarks and I have done. It may be objected that this scheme would make scientific success in large measure dependent on the power of teaching, and that original work would thereby go to the wall. I reply that that is altogether a fallacy, and if I had time I could show it.

Lastly, the question of expense of apparatus and other means of inquiry is altogether a secondary one. Government ought of course largely to provide these; but there would be no difficulty in distributing them on a plan similar to that of the grant to the Royal Society. It is the question of "scientific careers" that is the pressing one, and the one most difficult to settle.

IN SICCO

Tails of Comets

In NATURE of 16th December, Prof. Tait advances the opinion that the tail of a comet consists of nothing but meteorites; mentioning in proof of this that the orbits of the August and November meteors have been determined, and found to be identical with those of two known comets. I do not question the importance of this most remarkable fact, but I think the older opinion, that the tail of a comet is gaseous, is demonstrably true. Sir John Herschel, in his "Elements of Astronomy," remarks with wonder how the tail, in the comet's perihelion passage, is *whisked round* in apparent defiance of the law of inertia, so as always to keep pointing away from the sun. Were the comet an assemblage of meteorites this would be impossible; the tail would, in that case, always lie parallel to the direction of the comet's orbit. The fact just mentioned as to the perihelion motion of the tail is, to my mind, a conclusive proof that the tail is not formed once for all, but is a cloud which is constantly in process of formation, and as constantly evaporated. This view is supported by the fact that Halley's Comet was seen to increase in apparent magnitude as it receded from the sun, in consequence, as was suggested, of the conversion of invisible vapour into visible cloud as the heat grew less intense.

Dr. Tyndall's suggestion, that the tail may be a cloud produced by actinic precipitation from an invisible atmosphere is, to my mind, the only plausible suggestion yet made on the subject.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, May 4

Left-Handedness

IN reference to the letters which lately appeared in your periodical on "Right and Left-handedness," I beg to draw your attention to some remarks of Professor Hyrtl, the celebrated anatomist of Vienna, which were published several years ago, and the substance of which I now quote from the 4th edition of his "Handbuch der topographischen Anatomie," 2 vol. 1860.

"It happens in the proportion of about two in a hundred cases that the left subclavian artery has its origin *before* the right, and in these cases left-handedness exists, as it also often actually does in the case of complete transposition of the internal organs (Professor Hyrtl describes two cases), and it is found that the proportion of left-handed to right-handed persons is also about 2 to 100. Professor Hyrtl thinks that ordinarily the blood is sent into the right subclavian under a greater pressure than into the left, on account of the relative position of these vessels, that in consequence of the greater supply of blood the muscles are better nourished and stronger, and that therefore the right extremity is more used. In cases of anomalous origin of the left subclavian, &c., the reverse occurs, and therefore the left hand is employed in preference.

Kensington, May 3 ADOLF BERNHARD MEYER

Strange Noises heard at Sea off Grey Town

IN submitting the following to the notice of your readers, I am guided only by the desire of seeking a solution of what to me and to many others appears a very curious phenomenon. The facts related can be vouched for by numbers of the officers and crews of any of the R. M. Company's ships.

I must premise that this phenomenon only takes place with iron vessels, and then only when at anchor off the port of Grey Town. At least, I have never heard of its occurring elsewhere, and I have made many inquiries.

Grey Town is a small place, containing but few inhabitants, situated at the mouth of the river St. Juan, which separates Nicaragua from Costa Rica, and empties itself into the Atlantic, lat. 10° 54′ N., and long. 83° 41′ W. In this town there are no belfries or factories of any kind.

Owing to a shallow bar, vessels cannot enter the harbour or river, and are therefore obliged to anchor in from seven to eight fathoms of water, about two miles from the beach, the bottom consisting of a heavy dark sand and mud containing much vegetable matter brought down by the river. Now, while at anchor in this situation, we hear, commencing with a marvellous punctuality at about midnight, a peculiar metallic vibratory sound, of sufficient loudness to awaken a great majority of the ship's crew, however tired they may be after a hard day's work. This sound continues for about two hours with but one or two very short intervals. It was first noticed some few years ago in the iron-built vessels *Wye, Tyne, Eider*, and *Danube*. It has never been heard on board the coppered-wooden vessels *Trent, Thames, Tamar*, or *Solent*. These were steamers formerly employed on the branch of the Company's Intercolonial service, and when any of their officers or crew told of the wonderful music heard on board at Grey Town, it was generally treated as "a yarn" or hoax. Well, for the last two years the company's large Transatlantic ships have called at Grey Town, and remained there on such occasions for from five to six days. We have thus all had ample opportunity of hearing for ourselves. When first heard by the negro sailors they were more frightened than astonished, and they at once gave way to superstitious fears of ghosts and Obeihism. By English sailors it was considered to be caused by the trumpet fish, or what they called such (certainly not the *Centriscus scolopax*, which does not even exist here). They invented a fish to account for it. But if caused by any kind of fish, why only at one place, and why only at certain hours of the night? Everything on board is as still from two to four, as from twelve to two o'clock, yet the sound is heard between twelve and two, but not between two and four. The ship is undoubtedly one of the principal instruments in its production. She is in fact for the time being converted into a great musical sounding board.

It is by no means easy to describe this sound, and each listener gives a somewhat different account of it.

It is musical, metallic, with a certain cadence, and a one-two-three time tendency of beat. It is heard most distinctly over open hatchways, over the engine-room, through the coal-shoots, and close round the outside of the ship. It cannot be fixed at any one place, always appearing to recede from the observer. On applying the ear to the side of an open bunker, one fancies that it is proceeding from the very bottom of the hold.

Very different were the comparisons made by the different listeners. The blowing of a conch shell by fishermen at a distance, a shell held to the ear, an æolian harp, the whirr or buzzing sound of wheel machinery in rapid motion, the vibration of a large bell when the first and louder part of the sound has ceased, the echo of chimes in the belfry, the ricocheting of a stone on ice, the wind blowing over telegraph wires, have all been assigned as bearing a more or less close resemblance; it is louder on the second than the first, and reaches its acme on the third night; calm weather and smooth water favour its development. The rippling of the water alongside and the breaking of the surf on the shore are heard quite distinct from it.

What is, then, this nocturnal music? Is it the result of a molecular change or vibration in the iron acted on by some galvanic agent peculiar to Grey Town? for bear in mind that it is heard nowhere else, not at Colon, some 250 miles distant on the same coast, not at Porto Bello, Carthagena, or St. Marta. The inhabitants on shore know nothing of it. If any of your numerous readers can assign a likely cause, will they be pleased to state by what means, if any, its accuracy may be tested? If required, I can forward a specimen of the mud and sand taken from the anchor.

CHARLES DENNEHY, M.R.C.S.I., R.M.S. *Shannon*

[Our correspondent should dredge.—ED.]

The Newly-Discovered Sources of the Nile

RELUCTANT as I am to meddle with geographical discoveries made by the 'high *priori* road,' I cannot refrain from protesting against erroneous statements, which, if left uncontradicted, may acquire currency. The zealous geographer of former times sought for truth and accuracy. Treasuring truth, his knowledge increased with his information. But fashions are now changed. It has been found that one who starts in ignorance may every day alight on some novelty and wonder; and that since anything may be proved by data made for the purpose, the best mode of treating preceding information is to corrupt, change, and distort it as the case may require, so that instead of fettering invention, it may serve as proof of endless new discoveries. Captains Burton and Speke examined the northern end of what they called Lake Tanganyika. They saw it narrowing to a point and enclosed by hills, called by the latter officer the Mountains of the Moon. Six rivers, they learned, flowed into it from those hills. They did not examine nor approach the southern end of the lake; they differed in their accounts of it; and Captain Burton, in writing that it was often circumnavigated by the Arabs, made a statement repugnant to common sense. The pedlar Arabs cross the lake in ill-built boats, with savage crews, navigating only in daylight. They navigate it no more than is absolutely necessary for their trade with the interior, and not for pleasure or scientific purposes. Captain Speke measured the altitude of the lake, and on his second journey, going to a great extent over the same ground, he saw no reason to be dissatisfied with his previous hypsometrical observations. The result of his observation at Gondokoro was thought to prove the accuracy of his instruments. Yet the account given of the northern end of the lake is now rejected, while that of the southern end is obstinately adhered to; and as to the elevation of the lake, the *à priori* geographers find it convenient to add 1,000 feet to that assigned by Captain Speke.

Among the geographers of the new school, no one holds a higher rank than Dr. Beke. In devoting his labours to the mystery of the Nile, he very properly began at the base. He first adjusted the Mountains of the Moon and their everlasting snows. A warm admirer of Ptolemy, he nevertheless found it expedient to correct a mistake of the old Grecian, who thought that those mountains extended from W. to E. in lat. 12° 30′ S., whereas Dr. Beke discovered that they actually lie in a meridional line across the equator, and not far from the eastern coast. With the boldness of genius he set this chain of mountains, on the alleged authority of the East African missionaries, in a region where these missionaries emphatically declare that there is nothing of the kind. But having removed the Mountains of the Moon from the famed Land of the Moon, he now unaccountably removes the sources of the Nile as far as possible from the mountains supposed to give birth to them. He places them 1,000 miles S.W. of those mountains, on the eastern frontier of Benguela; and this he does, forsooth, because Dr. Livingstone announces the discovery of the real Lakes of the Nile (a batch of 20), just where Ptolemy set them, between lat. 10° and 12° S. Had Dr. Livingstone an opportunity of looking at Ptolemy's map he would have therein seen only two lakes, nine degrees asunder, and respectively in lats. 6° and 7°. But with time to study and understand his author, he would also have perceived that the positions thus indicated in false graduation are really close to the equator, respectively in 11′ N. and 39′ S. In Dr. Livingstone such a mistake is not surprising; in Dr. Beke it is inexcusable. But the latter, being inspired with a new hypothetical discovery, eagerly seizes on anything that will help him to develope it. The river Casabi he deems the chief, as being also the most remote source of the Nile. Its course eastward he concludes on the authority of Ladislaus Magyar, whose scientific attainments and reliability he, of course, rates highly. But the career of the Hungarian proves only his leaning to savage life. From the Brazilian navy Ladislaus passed into the service of the King of Calibar; and thence again he made his way to the interior of Benguela, where, marrying the daughter of a chief, he found himself in a short time the leader of a band of expert hunters. In 1850 he started, with his wife and 280 armed followers, on an excursion to the interior. The province of Kiboque, in which are the sources of the great river Casabi, was soon reached. Its forests, he says, extend far and wide, in lat. 6°S. But as the province in question reaches little north of the 12th parallel, it is evident that the Hungarian's science deserted him at first starting. He continued his march through Bunda, south of the river Lungobungo, in lat. 10° 6′ (13° would be nearer the truth), and at length after a 33 days' march, crossing the Liambegi, he arrived at Ya Quilem, in Kilunda. Now, the Portuguese traveller, Graça, travelling from Bihé in a parallel route, arrived in 33 days at Catende, 100 miles west of the Liambegi, so that we cannot doubt that Ya Quilem was not far east of that river, and not to the north, but probably much to the south of the 11th parallel. Yet Ladislaus places it in 4° 41′ S.! Such is the science on which Dr. Beke relies. When the latter says that in lat. 6° 30′ Ladislaus learned the eastward course of the Casabi, he totally misrepresents the facts. The Hungarian was much further south when he embraced the belief that the great river runs to Nyanza and Lake Mofo (near the Cazembe), that is, that it occupies the valley of the Luapula. Graça, who followed the river down a long way to the north, states his opinion (entirely mistaken by Mr. Keith Johnston), that the Casabi and Lulua are the head-waters of the Rios de Sena (the Zambeze). To these two foolish attempts at a great discovery must be added a third. Dr. Livingstone proclaimed that the Luapula flows into the Liambegi, and deforming their names, he reckoned among its tributaries rivers which run into the Lulua. If the concurrent and invariable testimony of three centuries can make anything certain, it is certain that the Casabi falls into the river of Congo, commonly called the Zaire. From the first visit of the Portuguese to Congo to the present day, the natives, when interrogated respecting the origin of their great river, have always answered that it comes from the Lake (Lobale) a-Kilunda (of Kilunda). This is properly the name of the country about the head-waters of the Liambegi, but the Portuguese, copied by Dr. Livingstone, apply it much more widely. The Casabi certainly does not rise in Kilunda, but it receives many streams from it, and unites with the Lulua, which is swelled by many more. The chief river of Kilunda is, we believe, the Lualaba, which turns westward to join the Lulua, while 8 or 10 days' journey further east the Luviri, a smaller, but still an important river, flows north-eastward to the Luapula. Between them, in about the meridian of 25° E., is a well-marked water-parting. The Lualaba is bordered by extensive salt marshes. One of its affluents—the Luigila—is said to flow over a bed of rock-salt. Hence, the Lulua or Lolo, which collects these waters, is, as its name implies, a salt river, and remarkable for its excellent fish. Lake Dilolo (the cerebral *d* here takes the place of *r*), has, for the same reason, an equal reputation. Fish, salt, and copper are the products which chiefly support the trade of the African interior, and the great emporium of this trade is Katanga, on the River Luviri. I now turn to Mr. Keith Johnston, who rejects, but not on the best grounds, Dr. Beke's hypothesis that the Casabi is the source of the Nile, and at the same time proposes another equally objectionable, namely, that the Chambezi, that is, the Luapula, flows round by the north and west into the river of Congo. Surely such extravagant conjectures would never be brought forward if, in the quarters that exercise an influence on geography, fair play were allowed to the information and common sense of all parties. An ignorant and overbearing patronage has the power of spreading darkness around. Mr. K. Johnston unfortunately fixed his attention on a sentence of Dr. Livingstone's letter, which is fitted only to mislead—a sentence, the dangerous indistinctness of which was pointed out by me in an early number of this paper (NATURE No. 3). Dr. L. plainly says that the Luapula flows down north past the town of the Cazembe, and 12 *miles below it*, enters lake Moero. The traveller here states not what he saw, for the Luapula is some miles west of the Cazembe's town, but what he misconceived. He may have meant that twelve miles from the town, towards the S.W., the river issues from the lake. It is easy to show that Lake Moero (a name made for convenience by strangers, but not used by the natives) lies to the S.W. of the Cazembe. Dr. L., when he first visited the Cazembe, passed (northwards) up the east side of the lake. He tells the difficulties created by the flooded rivers at its north end; one of these was the Luo, mentioned, as I have elsewhere pointed out, by the Portuguese as five days' distant from the Cazembe. Again, further north Dr. L. had to wade through the Chungu near an old site of the chief's town and where Larcerda died. Now, Father Pinto, when he left the Cazembe on his way homewards, did not reach the Chungu where he disinterred Lacerda's bones (subsequently lost in the retreat), till the 3rd day. Thus it is quite evident that the north end of Lake Moero is south of the Cazembe's residence. With respect to the course of the Luapula northwards, Mr. K. Johnston may rest assured that Dr. L.'s statements have not the slightest foundation. The Luapula does not take the name of Lualaba, nor does it join the Luviri towards the north. That the Chambezi falls into the Luapula was ascertained by Dr. Lacerda 70 years ago, and all that Dr. Livingstone has ascertained is that his own views on the subject were erroneous. As to the further course of these rivers let us take the evidently unbiassed evidence of the Arabs who met Dr. Livingstone in the interior of Africa, and a brief account of whose travels appeared in the transactions of the Geographical Society of Bombay, 1862. From the western shore they travelled in 27 days to the broad river Maroongo. This is the Luapula, which, by strangers reaching it from the north, is named after the Arungo, who dwell on its western side. "Roonda (Lunda) is on the banks of the Rooapoora, which runs north to Tanganyika." Here the rude observers confound the river which does reach the Nyanza, with Lake Mofo. Neither need we believe that the place is called Lunda by the natives. But this among African traders is a wide-spread name. By the Portuguese and their native agents the dominions of the Muata-ya-Nvo and of the Cazembe are all called Lunda. "25 days west (SW) of the Cazembe are the copper mines and the town Katanga. The river Rafira (Luviri) flows past Katanga, and joins the Rooapoora to the N." There is much reason for believing that the Luviri flows south-eastwards from Kanyika, but perhaps the author's meaning is that the river wheels round to the north. Dr. Livingstone saw nothing of Lake Bangweolo. Lake Moero he saw only on the east and north, and not connectedly; most of his statements respecting its size, &c., must be due to hearsay. The mountains that he speaks of are the hills Chimpire, noted in two groups by the Portuguese. It is remarkable that when in his missionary travels he met with the name Mpire, a hill, he supposed it to be the Sichuana numeral mbili, *two* (hills), a flagrant mistake. The Portuguese reported that the elevated country on the way to the Cazembe lies in ridges, with pools of water in the successive hollows. They learned that there was a great marsh at the confluence of the Chambeze and Luapula. Further north they saw numerous swamps and lagoons, and heard of more. They were told that to the west lay the great lagoon which Caetano Pereira spent a whole day in wading through. This was Carucuize, the nucleus of the Moero. The pombeiros, or native commercial travellers from Angola to the Cazembe, marched down the eastern bank of the Luapula five days before they turned eastward to Lake Mofo. That river, therefore, does not ordinarily flow through a lake. Dr. Livingstone evidently found the country in a state of unusual flood, with the fens and lagoons united into great lakes. From the district of the Fumo Moiro, at the north-east margin of the flood, he has made the name of the lake. As he finds every pool to belong to the system of the Nile, it is natural that, in his exalted imagination, the hills should rise into mountains. The Alunda, or Balunda as he calls them, being originally from the banks of the Lualaba, nourish a superstitious regard for that river. While the traveller, therefore, thought of nothing but the Nile, his native hearers knew of no great rivers but the Lualaba and its immediate neighbours, the Luviri and Luburi (in the printed letter misread Soburi.) His inquiries pointed to the N. or N.E.; they answered respecting the S.W. They mistook the object of his ardent curiosity, and he was only too ready to misinterpret their communicativeness. Hence the confusion of rivers, right and left, the lakes Ulenge, Chowambe, &c., of which the less said at present the better. After such a conclusion it may perhaps be consolatory to remark that it would be labour thown away to lead all the great rivers of south tropical Africa to the river of Gondokoro. Dr. Peney, who studied the character of this stream, found that it varied often, but that it never rose in flood more than two feet above its mean level. This increase in a wide spreading river near the equator barely suffices to compensate the loss by evaporation. Consequently, the floods at Gondokoro have no perceptible effect on the river a few degrees lower down. The river of Gondokoro, therefore, contributing nothing whatever to the floods of Egypt, must be regarded as a very subordinate branch of the Nile.

Dr. Beke wonders (NATURE, No. 9) why I give the name Nyanza to Lake Tanganyika. He here touches upon an important subject, interesting in its bearing both on geography and on the intrigues of geographical coteries. The assertion that in the name Lake Tanganyika there lurks some fraudulence, will of course be received with incredulity, and therefore its justification will be impossible without some historical development. But if encouraged, I am prepared to show that, with respect to the lake, the geographical world labours under a delusion designedly produced.

W. D. C.

Apparent Size of the Moon

MY original intention was to put together several *veræ causæ*, which might be found, concurrently, to contribute to the universal impression that the moon's disc is larger or smaller, according as it is nearer to the horizon, or to the meridian. I shall content myself, however, with calling attention to what I am now persuaded is the nature of that impression. "Sweet are the uses of adversity." An attack of hemiopsia is always serious, and may be dangerous (see NATURE, Feb. 24th, 1870; p. 444). I think I owe to it the discovery (for such it was to me), that the variable standard of angular magnitude which infects our visual judgment, can be detected in a small room as certainly as in view of the celestial vault. The distressing affection which succeeds the hemiopsia, as soon as it forms a broken arch around the central hole of the retina, is an instructive spectre in regard to the question I am considering. Being referred to two equally distant sites on the wall of the room, one horizontal and the other considerably elevated, the spectre seems larger in the former than in the latter. I soon proved that this was no accident case. I extemporised a very rough experiment on this wise—I placed a disc $14\frac{1}{4}$ inches in diameter on the wall 7 feet from the ground, and selected a horizon so that the base of the disc and the horizon were equidistant from a fixed point of observation. I found the disc was about 30° above the horizon. I now took six persons successively, and made each person take an observation from that point, first looking at the disc, and then transferring it in mind to the horizon, where I carefully marked the estimated size. The maximum was $13\frac{5}{8}$, the minimum $10\frac{5}{8}$, and the mean $12\frac{1}{2}$ inches. This result is, of course, equivalent to saying that had an equal-sized disc been placed on the horizon, its diameter would, taking the average, have appeared to be $1\frac{3}{4}$ inch greater than when elevated 30° above it. I think it is worth while making this experiment with greater accuracy, and with a greater number of persons. I have no doubt Mr. Abbott's view of the case would be fully borne out. But I do not understand why the augmentation on the horizon is so much greater in the case of the moon and the sun; nor yet why the rising sun does not present so striking an augmentation as the rising moon. The augmentation of the latter may be partly an effect of external conditions; but the *fact* of augmentation, in what I have called visual judgment, is a question for the physiologist. I should much like to know what, for instance, such an authority as Helmholtz has to say on the matter.

The great fault of physicists, *me judice*, is, and ever was, their inability to see more than one side or aspect of a subject. Metaphysicians, on the contrary, may see all round it, but do not see all sides clearly. Mr. R. A. Proctor (NATURE, March 3rd, 1870; p. 462) affords me an apt example of the former. "The mind," he says, "instinctively assigns to the celestial vault a somewhat flattened figure, the part overhead seeming nearest to us." That is, taking the angular measure of the moon's diameter as a constant quantity, since she seems larger on the horizon than on the meridian, we must (unconsciously) refer her in the former case to a maximum distance. So far so good. But the argument is a thesis admitting of an equally valid antithesis. The case with me is this, that the moon appears to me to be much nearer on the horizon than in any other position! Nor am I singular in this. A lady proposed to me as an explanation of the apparently augmented size of the horizontal moon, that "probably she is nearer to us there than anywhere else!" Here we have the antithesis. We may say, the mind instinctively assigns to the celestial vault a somewhat prolate figure, the part overhead being furthest from us. That is, assuming (erroneously of course) a greater size for the moon's disc on or near the horizon (though she is not visually larger in one place than another), therefore we must infer that she is approaching us when she nears the horizon, thus showing us her size looming larger as she gets nearer. The arguments cut each other's throats, and can be of no use whatever.

Ilford, E., May 1 C. M. INGLEBY

Cross Fertilisation

THE following peculiarities in the flowers of *Helleborus niger* bear upon the same subject as Mr. Hartog's observations on hazel catkins (NATURE, No. 23), and may be worth noticing.

The tubular nectaries by which the petals are replaced are more or less completely hidden by the stamens. The sepals remain for some time half open, and I found that in every case, while the flowers were in this state, pollen readily adhered to the stigmas; and on account of the curved shape of the latter, reaching almost to the half-closed sepals, an insect visiting the nectaries must generally touch them. In such flowers, though the nectaries were full, the anthers had not burst, while in those more fully open, pollen did not so readily adhere to the stigmas, so that in most cases the flowers would be fertilised by pollen from older ones, and probably from distinct individuals. In old and widely-opened flowers, whose anthers had burst, an insect could hardly reach the nectar without being dusted with pollen, while it would probably not touch the stigmas. I may as well mention that I saw a bee visiting these flowers in February last, and in the same month I found a spider in a half-opened flower of *H. fœtidus*.

Mr. Darwin has noticed a case (*Spiranthes autumnalis*, "Fertilisation of Orchids," ch. iii.) in which older flowers are generally fertilised by pollen from younger ones.

CHRISTOPHER J. HAYDEN

Trinity College, Cambridge, April 25

Chamounix

MANY of the readers of NATURE are no doubt preparing for a visit to Mont Blanc; permit me to say to them that the season for making the ascent will, in all probability, be earlier this year than usual, on account of the remarkably fine and hot weather; it is two months within a day or so since any rain fell; but to-day we have had a refreshing shower of three hours' duration, which will prove of infinite service to the little farms in the valleys of Chamounix, St. Gervais, and Sallanches.

Early yesterday morning, accompanied by a guide and my daughter, a nimble girl of 15, I crossed the Glacier de Boissons, at an elevation of 3,000 feet; there was comparatively little snow, the blue ice being repeatedly visible. We made the best of our way up the moraine, and descended through the forest into the valley of Chamounix, where the heat of the sun was oppressive; beetles on the earth and butterflies in the air were numerous.

The cherry, plum, and pear trees, so plentiful near Sallanches, are all in full blossom and doing well—the bees know it.

The ice grotto at the foot of the Glacier de Bois is already diminishing, and a serious-looking crevasse appears at the portal; the *Arve*, which rises from this glacier, is already considerably swollen. Perhaps the most gratifying news to send is the intelligence that the new road from Sallanches to Chamounix is all but finished—a mighty work, worthy of the new ruler of Haute Savoy. S. P.

April 23

PHYSICAL SCIENCE AT CAMBRIDGE

A MEETING of members of the Senate took place here on Saturday last, which is likely to have a considerable influence on the fortunes of Physical Science in this University. About a year ago a Syndicate, or committee of members of the Senate, was appointed to consider in what manner the funds could best be raised requisite for maintaining a Professor of Physical Science, and for providing suitable buildings and apparatus, also for certain other University objects. As the revenues of the University are known to be not more than adequate to maintain the educational machinery already in operation, the appointment of the Syndicate was a tacit adoption of the principle that *College* endowments ought to be made available in order to extend the area covered by professional instruction the advantages of which are open to the members of every college. The Syndicate's report, issued at the end of last term, formed the subject of discussion at the meeting called on Saturday.

The Syndicate informs the Senate that after estimating the sum which would be required to carry out the objects which it was desired to attain, they "decided upon addressing a communication to the several colleges of the University to inquire whether they would be willing, under proper safeguards for the due appropriation of any moneys which might be entrusted to the University, to make contributions from their corporate funds for these objects." The answers to this communication received from the governing bodies of the Colleges are considered as private by the Syndicate, and are not printed in this report. In the opinion of the Syndicate, however, "they indicated such a want of concurrence in any proposal to raise contributions from the corporate funds of Colleges by any kind of direct taxation, that the Syndicate felt obliged to abandon the notion of obtaining the necessary funds from this source." Accordingly they propose another plan for providing the needful funds, or at any rate a portion of them, with which I need not trouble your readers, as it involves technical details, and moreover is in itself so unjust and objectionable that it has not the slightest chance of being adopted.

The one point of real importance on which the discussion on Saturday turned, was whether the Senate ought to acquiesce in the conclusion of the Syndicate, and abandon the notion of obtaining contributions from the corporate funds of the Colleges for University objects. I am glad to be able to inform you that the opinion of the meeting was very decidedly expressed in favour, not of abandoning, but of carrying out this notion. The Master of St. John's said it was a settled matter that the funds requisite for the efficient teaching of Physical Science must be provided by the Colleges, and that the only question was whether they could arrange among themselves some plan for contributing in proportion to their means. Failing this, he held it to be quite certain, that they would be compelled to part with some portion of their corporate revenues for these objects by parliamentary coercion. The Master of Trinity insisted, in the strongest terms, on the urgent necessity of immediate action, if the University was to retain its position in the van of educated opinion. The Vice-Master of Trinity College disputed the statement made by the Syndicate, that the replies of the Colleges were on the whole opposed to making contributions out of their corporate revenues. He said that his own College had at once announced its willingness to contribute, and expressed his belief that a majority of Colleges were in favour of a College contribution, though they were not, as yet, agreed as to the proper principle of assessment. On this point, as the Syndicate has thought fit to withhold from the Senate the answers returned by the governing bodies of the several Colleges to the communication addressed to them, we have no means of forming an independent judgment. Every speaker seemed thoroughly to recognise the urgent and paramount claims of Physical Science to be placed on an effective footing in the University, except indeed the registrars, who urged the superior claims of ecclesiastical history and pastoral theology, and Mr. Perowne, of Corpus, who deprecated exaggerated statements in favour of Physical Science as a disparagement of classics and mathematics, and spoke with effusion of the gratitude which would be earned by "some *rich* College," which should make a present to the University of the funds required, and so save the other Colleges (not so painfully *poor* after all) the pang of putting their hands into their own pockets—a sentiment which drew from Mr. Blore, of Trinity, an amusing sally as to the want of similar benefactors of the common race in the matter of the *Income-tax*.

It seems to be the general impression at Cambridge that the Council will have to appoint a fresh Syndicate to make a second search for a common ground on which the Colleges may be brought to agreement. Meantime, the very general interest excited on the subject throughout the University is an encouraging sign that the just claims of Physical Science will before long be satisfied.

SEDLEY TAYLOR

THE TRANSIT OF VENUS AND THE ANTARCTIC REGIONS

DR. NEUMAYER has recently been in London, looking after apparatus, and making arrangements, for the proposed Austrian expedition preparatory to the one to the Southern Seas for the purpose of observing the Transit of Venus in 1874. The object is one in which every English man of science will feel a warm interest. The lethargy of our own Government has been described by German astronomers and naturalists by the expressive but not complimentary term "Philistinism."

At the sitting of the Vienna Academy of Sciences on March 10th, Dr. Neumayer submitted a proposal for the preparatory arrangements for the observance of the Transit. A map of the circumpolar regions shows that the best points in the Southern Hemisphere for these observations will be the region south of the Indian Ocean, near the circumpolar district. Dr. Oppolzer has established that the most favourable localities for observing the *immersion*, both as to parallax and altitude, can be connected by a curve passing by the great gulf of Australia to the Macdonald Islands, and from these to a point situated in 36° 52′ S. latitude, 43° 23′ E. longitude. The points best adapted for observation of the *emersion* will also be found in a curve passing from the centre of the Indian Ocean to a point situated in 180° E. long., and 79° S. lat.; and from there to another point, 64° 55′ S. lat., and 244° 39′ E. long. The point of intersection of these two curves (48° 5′ S. lat., 99° 3′ E. long.) will evidently be the one most favourable for the observation of the transit in its totality. In this case, the factor of the parallax and of the altitude will be 0°·67, and 48°·0 for the immersion; and 0°·47 and 62°·5 for the emersion. The nearest station to this point will be the Macdonald Islands, situated nearly in 53° S. lat., and 12° E. long. (from Greenwich). M. Neumayer, who visited these islands in 1857, was struck with their relatively high temperature; and has ascertained, by a close examination of the tables of temperature published by the authority of the Dutch Government, that the current of Agulha must terminate near them. The summer and winter isotherms confirm these facts, and there can be no doubt that it is under the meridian of the islands of Macdonald and Kerguelen that the most favourable region must be sought for a route towards the South Pole, in the same manner as Sir James Ross followed, with the same object, a new current which set out from the shores of New Zealand. The map of the southern circumpolar regions, published by Petermann, furnishes very precise information for the equatorial limit of the floating ice, the curve showing two points of depression towards the pole; one under the meridian of Kerguelen's Land, the other under that of New Zealand. It may, however, be said that because floating icebergs have once or twice been found in a locality, these are not sufficient definitely to fix the relations of the floating ice, which depends especially on currents, and which secondary causes, such as winds, can draw into regions ordinarily free of ice. It is the frequency of the ice that must settle the limits in such cases. At the points which have been named the limit of floating ice bends back upon itself as high as 60° S. lat.; and this is an important fact for the determination of the warm currents setting from the north. The position of the limit of maximum density of sea-water, and the presence of spermaceti whales (*Physeter macrocephalus*) which, as is well known, seek in preference warm waters, on the coasts of Termination Land, permit the supposition that the current in question continues towards the South Pole as far as that land and Kemp Island. Admiral Sir John Ross also saw spermaceti whales at the approach towards South Victoria; while Wilkes, Dumont d'Urville, and Ross, only met with few and isolated individuals in the intermediate seas. M. Neumayer thinks that it will be advisable to despatch a small reconnoitring expedition without delay to these regions, and to establish a scientific station on the Macdonald Islands, the first object of which should be to determine the absolute longitude, to serve as a basis for Delisle's method. It would be occupied during the months of November, December, January, and February, with a series of meteorological observations, and with everything relating to physical geography. He proposes that, for this purpose, the Academy should make application to the Government for the fitting out of the expedition, the expense of which would amount to 35,000 florins (87,500 francs.) This has been granted, and the expedition will sail equipped for physical and natural history observations.

THE NATURAL HISTORY OF THE ABYSSINIAN EXPEDITION*

THE Abyssinian campaign will always be an interesting little episode in history. Unlike so many of our military expeditions in bygone times, it was vigorously conceived, energetically carried out, and successfully concluded, and will, we can entertain no doubt, effectively protect us against a repetition of the outrage which led to its organisation. But even if no other advantage resulted from it, the acquirement of so much additional information, both in regard to the zoology and geology of Abyssinia, as is contained in the work before us, would in great measure reconcile all enthusiastic naturalists to the additional trifle of income-tax they have had to pay as their contribution to the expenses of the war; and to all such, we would recommend Mr. Blanford's book, as enabling them at a small outlay to recompense them selves for the annoyance they have experienced.

The author left Bombay for Abyssinia in December 1867, and did not return till the following September, after an absence of nine months and a half, eight of which were spent in Africa. Upon the whole, he appears to have enjoyed unusual advantages in the collection of objects of natural history. He has collected no less than 1,700 specimens of Vertebrata, representing 350 species, besides about 3,500 of Mollusca and Articulata, representing about 500 species. The work is divided into three parts: the first being a personal narrative, the second devoted to the Geology, and the third to the Zoology of the regions traversed.

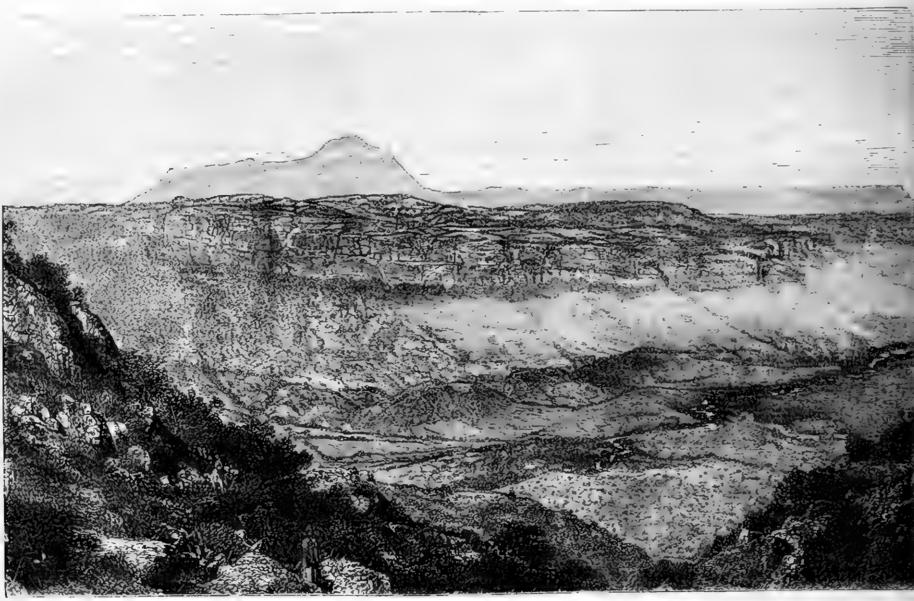
On arriving at Malkatto, in Annesley Bay, he at once set to work to collect specimens. In the vicinity he found larks, chats, shrikes, wagtails, white-breasted crows, kites, and vultures, constituting the commonest land birds, whilst on the shore there were abundance of gulls, pelicans, terns, ring-plovers, curlews, egrets, stints, and sand-pipers, with a little green bee-eater, which frequented the mangroves. Further inland, amongst the thorny acacia trees, he obtained a lovely little Nectarinia, the long-tailed robin, a weaver bird (*Hyphantornis galbula*), and two species of Avadavats (*Pytelia citerior* and *Estrelda rhodopyga*). Amongst the Mammals were hyænas, jackals, gazelles, hares, and Jerboa mice, which, finding unwonted supplies of food in the commissariat stores, increased and multiplied until the ground around the huts and tents was riddled with their holes. The only common reptiles were

* "Observations on the Geology and Zoology of Abyssinia made during the progress of the British Expedition to that country in 1867-68." By W. T. Blanford, F.G.S., late geologist to the Abyssinian Expedition, with illustrations and geological map. (London, Macmillan and Co., 1870.)

a small lizard (a kind of *Acanthodactylus*), and a very venomous little viperine snake (*Echis arenicola*). After the lapse of a few days, he started for the interior, and soon reached Hadoda. On waking the next morning, he saw a large troop of dogfaced baboons (*Cynocephalus hamadryas*), hunting for corn that had been dropped where the horses had been picketed. In the early part of January he was sent forward to examine the water supply, which proved to be abundant, and was obtained in places where there was no running water, by means of Norton's American pumps, and subsequently by an improved kind of chain pump (Brasyer's).

The pass which was selected for the road to the Abyssinian highlands commences at Komayli, situated on the verge of the coast plain, and extends to Senafé, a distance of about fifty miles. At Undul Wells, which is 3,400 feet above the sea, the subtropical fauna was entered, containing some of the animals peculiar to the Abyssinian highlands. Amongst these may be mentioned the *Corvus affinis*, a king crow, a noisy yellow-billed hornbill, a crateropus, a large partridge, and a very handsome bee-eater. A small plain covered with bush jungle, and partly with an aloe-like plant, was haunted with wart-hogs, hyænas, and Beni Israel. At a height of 5,000 feet, the splendid Abyssinian Plantain-eater (*Turacus leucotis*) appeared with a handsome francolin. Senafé itself, at the head of the pass, is 9,050 feet above the sea-level. Here he found the Hyrax, Ichneumon, Klipspringer, and Koodoo.

The drawing shows the Hamas Valley west of Senafé. The lofty hill in the distance is Hasheyat, or, as it is spelt in the excellent geological map which accompanies the work, Kishyat-hill, composed of columnar trachyte, and therefore of volcanic origin. The terrace on the opposite side of the valley is sandstone, while the bottom of the valley lies on metamorphic rocks. The picturesque character of the scenery of this region is here well shown. Leaving Senafé, the road traverses a plain of slaty metamorphic rocks, and presents few points of interest till the valley of Guna Guna is reached, where the scenery becomes very grand, and increases in beauty near Fokada, close to which there is a fine hill of columnar trachyte; and where the road winds round the western side of this, the view over the valley to the westward, exhibited in our second illustration, is one of unusual interest and beauty. The valleys, as usual, are deeply cut into the metamorphics; the flat hill-tops are of sandstone. To the southward, above the sandstone-bed, rise the terraced trap hills of the Harat range, and in the far distance are the strangely-shaped hummocks of the Adowa mountains.



VIEW OF THE HAMAS VALLEY, WEST OF SENAFE

Starting rom Fokada, our traveller, following the track of the army, passed through Adigrat; "a considerable town, with a fine church containing some remarkable mural paintings, in which Scriptural scenes are portrayed as they might have appeared, perhaps, had the scene been Abyssinia and the actors Abyssinians; just as the Italian painters of the Middle Ages introduced the costumes of Italy and the great buildings of Florence and Sienna in the representation of events which occurred in Palestine."

We need not follow Mr. Blanford's progress step by step, as the several camping-grounds are already known to the public through Markham's Abyssinian Expedition, and the correspondent of the *Illustrated London News*.

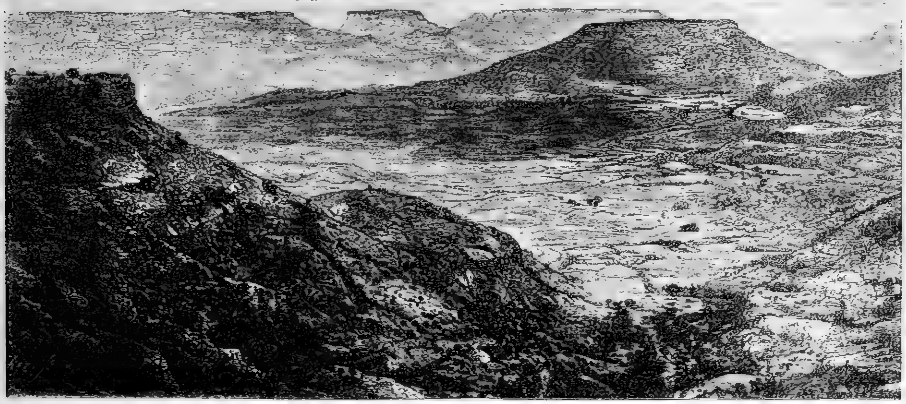
He describes the scenery as being almost everywhere strikingly beautiful; now bold and romantic—now resembling the undulating character of western England.

Near Bethor (not far from Magdala) they came suddenly on the brink of the mighty chasm in which the Jitta river runs, describing which he says,—

"Of all the grand scenery met with in Abyssinia, none equalled this wonderful gorge. It is 3,500 feet deep, and looks scarcely a mile across. The sides are extremely steep, and in places nearly perpendicular. The horizontal beds on both sides appear to correspond exactly. Half-way down there is a well-marked terrace, evidently formed by the same bed on both sides of the river. At the bottom of the ravine ran a beautifully clear stream in a pebbly bed." He entertains no doubt that this gorge has been formed by the river.

Magdala fell on the 16th April, and the retreat was so hasty that Mr. Blanford's opportunities of procuring specimens became much limited, especially as Lord Napier would allow no expeditions into the interior. Meeting, however, at Senafé, on the return journey, with Mr. Jesse, the Zoological Society's naturalist, who had been detained by illness and want of transport, they organised, with Lieut. Mockler, a trip to the Bogos territory, about 100 miles N.W. of Massowah, the details of which form the most interesting part of the work.

At Ailat they found a hot spring, the water with a temperature of 140° F., and perfectly tasteless. Here they had a tragical adventure with a leopard, the details of which are thus given:—"On the early morning of the 29th June, one of my servants rising before daybreak, was scratched in the face by some wild animal which had come into the camp. The track resembled that of a large cat. We thought nothing of this at the time, but on the following night we were all aroused by an outcry and shouting, and an alarm was given that a lion had seized one of our men. Mockler fired off his rifle to frighten away the beast, which rushed roaring past our tent. On enquiry we were horrified to find that an Abyssinian servant of Jesse's had been killed while asleep, and no alarm had been created until the animal attempted to drag away the body. The unfortunate man had two large tooth-holes in his throat, and must have been either so seized that he was unable to cry out, or else, as is probable, his neck was broken. The assailant was doubtless a leopard, very probably the same small animal which had scratched my servant the night before. We had a low thorn fence round three sides of our camp, and the camels occupied the open side—the usual plan in this part of Africa—but we had no fires, a most necessary precaution, and one we never neglected after this sad lesson."



VIEW OF THE PLATEAUX AND VALLEYS WEST OF FOKADA

Subsequently the adventurers had some fine sport with rhinoceroses.

In regard to the Geology of Abyssinia, the various rocks observed in ascending order were: 1, Metamorphic rocks; 2, Adigrat Sandstones; 3, Antalo limestones; 4, Trappean series, including the Magdala and the Ashangi group; 5, the Aden series of Volcanic rocks bordering the Red Sea; and 6, Recent formations—soils of the highlands, coral islands of the Red Sea, and alluvial deposits near the coast.

We need scarcely add that the Zoological portion is carefully drawn up, whilst the plates, which include illustrations of the *Hirundo æthiopica*, *Phylloscopus abyssinicus*, *Ruticula fuscicaudata*, *Pratincola semitorquata*, *Alauda prætermissa*, *Crithagra flavivertex*, and some fossils and horns, are admirably coloured, and very expressive.

NOTES

PROFESSOR STOKES will be a member of the Royal Commission to inquire into the present State of Science in this country. Up to the present time then, so far as we are informed, the Commission stands as follows :—President, the Duke of Devonshire. Members: Professors Huxley, Stokes, and W. A. Miller; Dr. Sharpey, Sir John Lubbock, Bart., M.P.; Messrs. Lyon Playfair, M.P., and B. Samuelson, M.P.

THE following is the list of candidates recommended by the Council for election into the Royal Society:—W. Froude, C.E.; E. Headlam Greenhow, M.D.; J. Jago, M.D.; Nevil Story Maskelyne, M.A.; M. Tylden-Masters, M.D.; A. Newton, M.A.; A. Noble, Esq.; Capt. Sherard Osborn, R.N.; Rev. S. Parkinson, B.D.; Capt. R. Mann Parsons, R.E.; W. H. Ransom, M.D.; R. H. Scott, Esq.; G. F. Verdon, C.B.; A. Voelcker, Ph.D.; S. Wilks, M.D.

AT the meeting of the Royal Geographical Society on Monday Sir Roderick Murchison announced that the Earl of Clarendon had decided, on the part of Her Majesty's Government, to provide means for relieving Dr. Livingstone by land from Zanzibar, news from the doctor having been cut off some time since by the outbreak of cholera on the caravan route. Sir Roderick also announced that the annual general meeting would take place on the 23d inst., and that the Royal gold medals would be awarded to Mr. G. W. Hayward for his journey to Yarkand and Kashgar, and to Lieutenant Garnier, of the French navy, for his merits in bringing the great French expedition from Cambodia to Yangtsin after the death of its chief.

SCIENCE is lively at the Royal Society. Last week we had Principal Dawson's very interesting Bakerian lecture; this week we are to have the Croonian lecture by Dr. Augustus Waller, F.R.S., "On the Results of the Method (introduced by the lecturer) of investigating the Nervous System, more especially as applied to the elucidation of the functions of the Pneumogastric and Sympathetic Nerves in Man." The subject is one of which physiologists will recognise the importance.

THERE is a considerable number of papers on hand at the Royal Society, and there remain but two more evening meetings, May 19, and June 16. The question has been asked whether Dr. Bastian's paper on Spontaneous Generation is to be brought forward at one of those meetings. And the same question might be asked in the case of other papers, and it is not the only question by any means. Were not the present arrangements of the Royal Society (including the break from May 19 to June 16) made to meet a condition of things which has long passed away? And since the flow of papers into the Society has largely increased, why should not the outflow be a little accelerated, and why should not the Bakerian and other lectures be given on extra nights, so that common justice may be done to the authors of other papers? One would have thought that the Royal Society would have gladly hailed the many signs of increased scientific activity, and made arrangements accordingly.

MEDICAL Science has sustained a heavy loss in the death of Sir James Young Simpson, Bart., Professor of Midwifery at the University of Edinburgh, and discoverer of the anæsthetic properties of chloroform. He died on Friday last of disease of the heart, in the 59th year of his age.

M. VILLEMAIN, the Perpetual Secretary of the French Academy, died on Sunday morning last, at the age of 80.

THE practical importance to the State of the progress of scientific discovery was happily illustrated in the debate in the House of Commons on Friday evening last, on the question of opening the Public Galleries during certain hours of the evening. The Chancellor of the Exchequer threw out a suggestion that some mode of lighting might, before long, be discovered as efficacious as gas, and not liable to the objection of endangering the safety of the building. "Science," said he, "was advancing very rapidly, and difficulties which seemed insuperable in these matters to-day, might, if we waited patiently for a few years, be met and overcome." It is needless to point out to so acute a reasoner as Mr. Lowe, that in thus proposing to utilise the possible future discoveries of Science, he furnishes the strongest possible argument for that assistance to Science by the State which we so persistently urge.

IN the same debate Mr. Lowe gave some hope that he might, in the course of the present session, state definitely the views of the Government with regard to the removal of the Natural History collections in the British Museum, a removal which he characterised as a "crying necessity." A considerable space of ground has been purchased by the Government behind the National Gallery, which will be used for some such public purpose.

THE Council of the Scientific Association of France at its last meeting appointed a mixed committee, representing the different branches of science, to perform the functions of the old commissions of astronomy, physics, and meteorology, entrusting to it at the same time the executive power, in the sense of regulating the public sittings, receiving requests for grants, &c. The committee, to which, at its own request, the Council has the power of adding the names of *savants* not belonging to its body, is composed as follows :—Astronomy, MM. Puisseux and Tissot; mechanics, MM. Eichens and Haton de la Goupillière; physics and chemistry, MM. Lissajous, Troost, and Cazin; meteorology, MM. Belgrand and Renou; geography, MM. Mouchez and Ploix; geology and palæontology, MM. Elie de Beaumont and Delafosse; botany, M. Lestiboudoix; zoology and zootechny, MM. Milne-Edwards and André Sanson; agronomy, MM. Payen and Barral. The *locale* of the association is at present at M. Le Verrier's, 1, Rue des Saints-Pères, Paris.

THE discussion which followed Principal Dawson's lecture of which we give an abstract in another column),) was sustained by Sir C. Lyell, Sir R. Murchison, and Dr. Hooker. Sir R. Murchison objected to the adoption of the term "Erian" for the series of pre-carboniferous rocks of North America, corresponding to our Devonian or Old Red Sandstone, as an unnecessary innovation; while Dr. Hooker thought that caution was necessary before concluding that the apparently exogenous fragment of wood found in a bed occupying the centre of the "Erian" series must necessarily belong to a dicotyledonous tree. He considered it quite possible that the structure of the wood of some of the higher Cryptogams of this early period might closely resemble that of dicotyledonous Phænogams.

WE print the following note from St. John's College, Cambridge :—"The following have been placed in the first class of the college examination in Natural Science: Blunt, Garrod, and Read. The names are arranged in alphabetical order, and the examiners are the same as for the open exhibition adjudged on Friday last. The second-class contains four names, and the third three."

THE fiftieth anniversary of the Leeds Philosophical and Literary Society was commemorated during last week, in the usual English manner, by a dinner.

THE following Special Meetings of the Ethnological Society will be held during the month of June :—Wednesday, June 1st, at the Royal United Service Institution, Whitehall-yard; "Report on the Prehistoric Antiquities of Dartmoor," by Mr. C. Spence Bate, F.R.S. Tuesday, June 7th, at the Museum of Practical Geology, Jermyn-street; "On the Geographical Distribution of the chief Modifications of Mankind," by Prof. Huxley, President. Tuesday, June 21st, at the Royal United Service Institution, Whitehall-yard; "On the Aymara Indians of Bolivia and Peru," by Mr. David Forbes, F.R.S. The meetings will commence punctually each evening at 8.30 P.M.

THE planet Lydia (No. 110), discovered by M. Borelly at the Marseilles Observatory on the 19th of April, had at $10^h33^m13^s$ mean Marseilles time, the following position:—Right ascension $12^h2^m39^s{\cdot}22$; north declination $6^\circ50'38''{\cdot}8$. Its horary motion has been determined as follows:—In right ascension $-1^s{\cdot}77$, in declination $+2''{\cdot}20$; its magnitude is between 12 and 13. M. Borelly had previously discovered two planets, bearing the numbers 91 and 99 in the system of asteroids revolving between Mars and Jupiter. These two planets had long been nameless, in consequence of the persistent refusal of M. Le Verrier to permit the astronomers under his jurisdiction to bestow any name upon them. The 91st has now received the name of Egina, the 99th that of Dikë.

THE planet which bears the number 109 in the series of asteroids, and which was discovered at Clinton by Mr. C. H. F. Peters, on the 9th of October last, has received the name of Felicitas. The following are the new elements of its orbit, which have been calculated by Mr. William A. Rogers, from three positions, on the 9th of October, 28th of November, and 22nd of January last:—

Epoch: 1869; Oct. 9th, mean Washington time.

Mean anomaly	$339^\circ\ 5'\ 45''{\cdot}21$
Longitude of perihelion	$55\ 56\ 3\ {\cdot}35$
Longitude of ascending node	$4\ 56\ 4\ {\cdot}35$
Inclination	$8\ 2\ 56\ {\cdot}10$
Angle (sine = eccentricity).	$17\ 27\ 2\ {\cdot}67$
Logarithm of half of the greater axis	$0{\cdot}4304068$
Mean diurnal motion	$802''{\cdot}41019$

WE have received the first sheet of Messrs. W. and A. K. Johnston's new Illustrations for Botanical Lectures, selected and arranged by Professor Balfour. It is occupied with a general ideal drawing of the various organs of a plant; and with illustrations of the embryo plant, cells and vessels, root and stem. If the series fulfil the promise of the first sheet, it will supply a desideratum for the botanical lecturer. The sketches are clear and well executed, and if they are too crowded, it is difficult to see how this could be avoided without making the series of an unwieldy size. We would suggest that their utility would be increased if they are also issued in sheets and uncoloured. The setting on a roller is not always the most convenient, and many would be glad to save the expense of the colouring. The descriptive pamphlet of text accompanying the sheet, by Professor Balfour, is in itself almost an elementary handbook of botany.

IN the last report of the Registrar-General, he gives a valuable classification of the geographical distribution of various diseases in the different districts of England, and concludes by remarking: "It is true that the returns of deaths can never furnish such immediate notice of the origins of epidemic diseases as returns of cases of disease; but it is not true that the information of the death register is necessarily too late; it is too late as regards the individual, but it is not too late as regards the community, which can immediately adopt measures to quench the sparks before they involve it all in flames. The seas which divide this island from the rest of the world no longer ward off diseases, which are landed every day on her shores, and can no more be shut out than the east winds. The nation is associated with all races and nations by its maritime population, and with many by empire. And however much men may indulge the natural pride of nationality, in one respect their solidarity admits of no dispute; they are all subject to the same diseases, and are all interested equally in the mitigation of the sufferings and losses those diseases occasion. How can those evils be mitigated unless their origin is known, and unless science determine the laws by which they are governed? And recorded observation on a European scale is as necessary for the determination in this field of life as observation of the skies in astronomy, without which Copernicus, Newton and Laplace could never have built up the system of the universe, or have given the navigator the means of avoiding shipwreck and finding his destination over the ocean. England is the only country in the world at the present time which publishes weekly and quarterly observations on an extensive scale in time to be available for immediate administrative use. But the Registrar-General hopes soon to get the co-operation of other countries, and in a few years to see in operation among several of the principal nations of the world one well-concerted series of reports of their marriages, births, deaths, and most controllable diseases."

IN the *séance* of the 11th April M. Duchemin brought before the Academy of Sciences of Paris the following curious fact in Natural History:—In the park of the Château de Montigny (Eure) belonging to M. Deroche, there is a large piece of water, through which a gentle current of beautifully clear water flows. In this lake numerous carp are reared, which thrive well, except during the first days of spring, when each year an extraordinary mortality occurs amongst them. In each animal one morbid symptom is always observable in the dead animals as they float on the surface of the water. In every case the animal is blind; a kind of film covers the eyes and even a part of the head. An examination of the body brings to light no internal disease, beyond a slight fatty degeneration of the tissues. The viscera appear healthy, and contain no intestinal worms. The cause of this strange malady has not hitherto received any notice; but from M. Duchemin's researches, in conjunction with M. Deroche, it seems that the toad (*Bufo calamita*) is an enemy, if not of all fishes, at least of the carp in spring. It attacks it, exhausts it, conquers, and kills it. To determine the point, they examined all the carp in the pond, and found squatting on the head of each of those that were diseased an enormous toad, the fore-paws of which were placed on the two eyes of the unfortunate fish. Thus, this ugly Batrachian, which presents so stupid an aspect, has yet sufficient intelligence to assume the offensive, and to overcome a large fish. If it has not agility and energy, it has cunning and perseverance. It would appear to kill by exhaustion, but it remains to be ascertained whether the acrid secretion of its skin assists in the conquest.

IN a still more recent *séance* of the Academy of Sciences, M. Duchemin, reverting to the above communication in regard to the mortality of the carp being in some instances due to the attacks of the toad, supplies observations which have been forwarded to him in support of his statements, and relates that from investigations undertaken at the Château de Montigny, the toad does not always remain permanently fixed on the head of the dead fish, but only so long as it gives signs of life. He observes, too, that all the carps from which the attacking toads had been removed were more or less blind. They were placed with care in another pond, but none of them recovered from the injuries received. No author has hitherto noted this animosity of the toad for the carp, who perhaps themselves consume the eggs of the toad. He has obtained additional evidence from M. Mermet, Directeur des Eaux at Contrexville (Vosges), who states that it has been found impossible to preserve carp in a sheet of water in that neighbourhood in consequence of the presence of numerous toads. M. l'Abbé Caillet, Curé of Rosoy (Haute Marne), whilst confirming the above statements, writes to him, "The toad is a villanous beast. One day I observed one that had crawled beneath a hive. There, with his two forepaws advanced and his throat wide open, he attracted the innocent bees, with which his sides were distended."

THE traffic of the Tower Subway—of the engineering features of which we recently gave an account (see NATURE, Vol. i., No. 11, p. 280)—has not been proceeding very smoothly, owing, we think, to defects in its management, and indeed was altogether suspended for a week or two. The passage through the tube is stated to be often interrupted by the breakage of the wire rope; for this purpose two engines are used, one at each end of the tunnel, while the obvious and ordinary arrangement would be to employ only one engine driving a shaft with two drums and an endless rope. The arrangements might be very similar to those adopted in mines for raising and lowering the "cage," except that in this case the cage would be the carriage, and would travel on nearly a level line instead of up and down a shaft. The mode adopted for raising and lowering passengers in the shafts attached to the subway is by means of a chain which draws the carriage up and down, various "safety" arrangements being adopted in case of the giving way of the chain. For raising and lowering passengers, a chain or rope is not however the best means. In the apparatus in use in all the best hotels in America, there are no chains or ropes, no catches, springs, or buffers in case of accident. There is a vertical hollow cast-iron column reaching through the whole length of the shaft; in that shaft is the thread or helix of a screw projecting a couple of inches from its surface; the cage forms part of the nut, which rests in the screw or shaft. The shaft is turned by a small engine, controlled by the guard, who travels with the cage; he can moderate, stop, or reverse the motion, and accidents in the ordinary sense are out of the question. It will thus be seen that the problems to be solved as to the best means of transporting passengers through the Tower Subway are of the simplest kind.

WE understand that the late Mrs. Appold has left to the Institution of Civil Engineers, a legacy of 1,000*l*., payable at the same time as the legacy for a similar amount from her husband, the late Mr J. G. Appold, F.R.S., Assoc. Inst. C.E. It is believed that both bequests have been made "for the general use and benefit of the Society," without being fettered with any conditions.

To those of our Scientific Societies who annually assure themselves, and others, of their continued existence by a dinner, we commend the "Report of the Speeches at the Annual Dinner of the Institution of Civil Engineers, May 4, 1870," which we have just received,—as an indication of what a little energy can make even of a dinner. Professor Tyndall answered for Science, showing in a clear way how physical research lies at the root of all conquests of Nature, gaining help in turn from the practical man. "Thus does the human intelligence oscillate between sound theory and sound practice, gaining by every contact with each an accession of strength. These two things are the soul and body of science, as far as you and I are connected with it. Sever sound theory from sound practice, and both die of atrophy. The one becomes a ghost, and the other becomes a corpse."

WE have received the first and second volumes of Dr. L. Lindenschmit's "Die Alterthümer unserer heidnischen Vorzeit," from originals in public and private collections, published under the authority of the Roman-German Central-museum at Mayence. The illustrations are most copious, and the work admirably done.

DR. R. CASPARY reprints from the Transactions of the Natural History Society of Halle a paper on the genus *Nuphar*. In examining the water-lilies of the Black Forest, as well as of Prussia and other districts of Northern Europe, he finds an intermediate form between *Nuphar luteum* and *N. pumilum*, which he regards as a true natural hybrid between two distinct species, and not as a mere transitional form. Another reprint from the Transactions of the same society is an essay on the Lennoaceæ, by H. Graf zu Solms-Laubach.

KONER'S *Zeitschrift der Gesellschaft für Erdkunde zu Berlin* contains a number of most valuable geographical papers, accompanied with carefully-executed maps; and a list of all geographical works, maps, and plans in all languages published between December 1868 and November 1869.

THE PHYSICAL CONSTITUTION OF THE SUN

DR. GOULD has addressed an important letter on the above subject to the Journal of the Frankland Institute. In the first part he refers to the new light recently thrown on the sun's physical constitution by the observations of Mr. Lockyer, and agrees with him and Dr. Frankland, both as to the absorption taking place in the chromosphere and photosphere itself, and also as to the possible telluric origin of the corona.

He then proceeds with regard to the probable age of the sun:—

"The researches of Helmholtz and Thomson regarding the age of the sun as a source of cosmical heat have shown us limits within which, in the absence of more decisive evidence, we must restrict our theories as to the length of time during which he has warmed the earth. The contraction-theory has been most ably discussed by these eminent physicists, and seems to afford the only satisfactory mode of accounting for the solar light and heat, now that we know both that the meteors generally revolve in cometary orbits, and that the habitability of the earth, as well as the apparent unchanged mutual attraction of the planets, bears testimony to the incorrectness of the meteoric theory. From Pouillet's data (derived from experiments which ought to be repeated in some year when the solar spots are at a minimum) Helmholtz has shown that, even were the sun's density uniform, a contraction of $\frac{1}{10}$ per cent. in his diameter would evolve 20,000 times the present annual supply of solar heat. But when the sun was hotter the same proportional contraction would have evolved yet more heat; so that we must consider the above estimate as a minimum.

"The expansibility of hydrogen gas for 100° C. is 0·3661. No gas appears to have so small a coefficient as 0·360, which would correspond to a linear expansion of 0·108. The expansibility of glass, the smallest known, I believe, even for a solid, is about $\frac{1}{150}$ part as great; say 0·00244 in volume, or 0·00081 linear. Therefore for glass even, a contraction of 1 per cent. in diameter would imply a fall of temperature by 1230° C., and a mean specific heat of 218. This seems certainly a minimum value.

"But if we suppose the expansion coefficient to be as large as that of hydrogen, a contraction of 1 per cent. would correspond to a change of temperature by 8·2° C. or a mean specific heat of 32,700, if equivalent to 20,000 years' supply. This is out of the question.

"Now Thomson has computed that bodies smaller than the sun, falling from a state of relative rest at mutual distances which are large in comparison with their diameters, and forming a globe equal to the sun, would generate 20,000 times the present annual supply. This would be greater did we consider the unquestionable increase of the sun's density towards his centre. And since it seems out of the question that resistance and previous minor impacts could have consumed more than one-half the heat, he inferred ten million times a year's supply to be the lowest, and one hundred million times to be the highest, estimate of the sun's initial heat.

"Now we have every reason for the belief that radiation is proportional to temperature. Assuming this and taking the temperature of the sun's photosphere as 14,000° C.,

10,000,000 times the present annual supply would be radiated
in 3,650,000 years if the specific heat were 218,
in 7,280,000 ,, ,, ,, ,, 1000.
100,000,000 times the present annual supply would be radiated
in 8,250,000 years if the specific heat were 218,
in 25,500,000, ,, ,, ,, ,, 1000.
500,000,000 times the present annual supply would be radiated
in 11,700,000 years if the specific heat were 218,
in 38,900,000 ,, ,, ,, ,, 1000.

"For vapours, other than hydrogen, the greatest known specific heat, so far as I am aware, is 0·508 (ammonia); and hydrogen, which has less than 3·5, cannot form any considerable portion of the sun's mass.* A specific heat so high as 1,000 seems altogether out of the question; yet it will be seen that, even on this supposition, an amount of initial heat equal to 500,000,000 the present annual supply, would have been radiated in less than forty million years, were the sun's radiative capacity proportional to his temperature. Taking the more probable age, 10,000,000 years, we should find 226 million times the present annual supply to have been radiated within this period if the specific heat were not greater than 218; and even were the specific heat 1,000, the total radiation would have been eighteen million times a year's radiation at present.

* It seems to form certainly not more than the 18,000th part of the mass of the earth.

"Thus the limit given by Thomson, although so vastly below that afforded by the speculations of some geologists, would appear itself to demand a considerable additional reduction. And I cannot see how we can well suppose the sun in its present form to have radiated heat for more than twenty millions of years, while three or four millions would seem to be a far more probable estimate, unless the thermic laws be totally different in those exalted temperatures which we must suppose to have existed at some past epoch.

"The very great diversity of the limiting values for the specific heat seems to afford ample scope for every needful allowance on account of the natural action of the particles within the body of the sun, even conceding to this the immense effect (analogous to the increase of specific heat) which has been assigned to it by some investigators. Even did we conceive a primitive heat equal to 200,000,000 times the amount now yearly radiated, and a specific heat 10,000 times as great as is possessed by any known gaseous body excepting hydrogen, we could not deduce so long a period as 80,000,000 of years for the past duration of the sun's heat."

SCIENTIFIC SERIALS

THE *Geological Magazine* for May (No. 71, or Vol. vii., No. 5) commences with a biographical sketch of Mr. G. Poulett Scrope, whose investigations into the phenomena of vulcanicity certainly entitle him to a distinguished place among eminent living geologists. This article is illustrated with an admirable portrait. Mr. Jenkins communicates an article on the surface geology of Belgium, in explanation of his map, a reprint of which appeared in the April number of the magazine. Mr. Maw notices two sections on the borders of Shropshire and Cheshire, in which Rhætic beds with characteristic fossils are exposed. In a paper on the Lower Silurian rocks of Galashiels, of which only the first part, illustrated with a map, is here published, Mr. Lapworth furnishes an important contribution to the elucidation of this confused group of rocks. The article in this number which will be generally read with most interest is one by Mr. James Croll, upon the boulder clay of Caithness, which he maintains to be a product of the action of land ice. This paper also is incomplete. Professor Rupert Jones notices and figures the species of Entomostraca from the coal measures of South Wales; several of the species are described as new. Lastly, Mr. Judd's paper, on the use and application of the term Neocomian, contains a good discussion of a matter which, although it seems to be merely a question of terminology, is really, especially at the present moment, one of considerable importance to geologists. Besides the usual reviews, notices, &c., the present number contains a supplementary paper by Mr. Samuel Hyde, on deep-mining in the south-west of Ireland, which possesses much economical interest.

THE *Ibis*, a Quarterly Journal of Ornithology, New Series, No. 22, April 1870. (Van Voorst.)—This number contains :—(10) "Notes relating chiefly to the Birds of India," by Mr. Blyth—the results of an examination of the specimens in the Leyden Museum; (11) "Note on the Systematic Position of *Indicator*," by Mr. P. L. Sclater; (12) "Stray Notes on Ornithology in India," by Mr. Allan Hume; (13) "On New and Little-known Birds collected during the Voyage of the *Magenta*," by Drs. Giglioli and Salvadori; (14) "A List of the Birds of Turkey" (continued), by Capt. Elwes and Mr. T. E. Buckley; (15) "On Rare and Little-known *Limicolæ*," by Mr. J. E. Hasting, determining and discriminating two puzzling species of *Eudromias*, *E. asiaticus*, and *E. veredus*; (16) "On the *Oriolidæ* of the Ethiopian Region," by Mr. R. B. Sharpe—a very elaborate article; (17) "On the Ornithology of Hainan" (continued), by Consul Swinhoe; (18) "On existing Remains of *Alca impennis*," by Prof. Newton, showing that there remain to us of this supposed extinct bird 71 or 72 skins, 9 skeletons, detached bones of 38 or 41 individuals, and 65 eggs. (19) "Notices of Recent Ornithological Publications"—English, French, Dutch, German, Russian, and American, wherein more than twenty works are briefly reviewed; and (20) "Letters, &c.," from Messrs. Layard, Hume, Brooks, and R. Gray, Col. Tytler, Lord Walden, Mr. C. Horne, Capt. Fielden, Herr von Pelzeln, Dr. Salvadori, and Messrs. P. L. Sclater, Harvie Brown, Hawkins, H. Saunders, Elliot, Tristram, and Skeat—the last a communication which will interest others than ornithologists, for it explains the etymology of the name "Grey Lag Goose"—the goose that *lagged* behind the others bred in this country when its congeners had departed for their summer quarters. The number is illustrated by some woodcuts, and by five beautiful coloured plates, by Mr. Keulemans, representing eight species of birds, of which six have never been figured before, and the other two in imperfect plumage only.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 5.—The Bakerian Lecture, "On the Pre-Carboniferous Floras of North-Eastern America, with especial reference to that of the Erian (Devonian) Period." By J. W. Dawson, LL.D., F.R.S., &c., Principal and Vice-Chancellor of M'Gill University, Montreal.

The attention of the author was first directed to the Devonian as distinguished from the Carboniferous flora, by the discovery, on the part of Sir W. E. Logan, in 1843, of some remarkable remains of plants in the Sandstones of Gaspé, Canada. In 1859, after visiting Gaspé to study these plants *in situ*, descriptions of them, and more particularly of the two characteristic Lower Devonian genera *Prototaxites* and *Psilophyton*, were published in the Journal of the Geological Society. Subsequently additional material was obtained by personal investigation of the Devonian of Maine and New Brunswick, and through the kindness of Prof. James Hall, from that of New York. These additional plants were also published in the Journal of the Geological Society. Still more recently, a thorough re-examination of the Gaspé beds, the systematic exploration of the plant-bearing beds near St. John by Prof. Hastt, and fresh collections made by Prof. Hall, have enabled the author to prepare a catalogue of 121 species, and to attempt a thorough revision of the Erian flora, and an investigation of its conditions of growth and relations to the Carboniferous flora.

The term "Erian" is applied to the formations included between the top of the Upper Silurian and the base of the Carboniferous, on account of the uncertainties which have attended the subdivision and limitation of the Devonian of Europe, and also on account of the immense area occupied by these beds on the south and west of Lake Erie, and their admirable development with regard to subdivisions and fossils. The name "Erie Division" was also that originally applied to this typical series by the geologists of the Survey of New York.

A large part of the paper was occupied with the revision of the Erian flora, including the description of twenty-three new species, and more ample descriptions of others previously known only in fragments. Large trunks of *Prototaxites*, from the base of the Lower Devonian, were described, and full details given of the form, structures, and fructification of two species of *Psilophyton*. The new genus *Ormoxylon* was described. The genus *Cyclostigma* was noticed, as represented by two species in America, and its foliage and fruit described for the first time. The genera of the Erian Ferns were examined and corrected, and several interesting trunks and stipes belonging to Tree-ferns were described. The fruits of the genus *Cardiocarpum* were illustrated with reference to their structure. The occurrence of *Lepidophloios*, *Calamodendron*, and other forms in the Middle Devonian was noticed for the first time.

The third part of the memoir was occupied with comparisons and general conclusions. At the close of the Upper Silurian period there was a great subsidence of the land in Eastern America, proved by the wide extent of the marine beds of the Lower Helderberg (Ludlow) group. It was on the small areas of Lower Silurian and Laurentian land, remaining after this subsidence, that the oldest land plants known in the region flourished. Re-elevation occurred early in the Devonian period, and the known flora receives considerable extension in the shallow-water beds of the Lower Erian. The subsidence indicated by the great Carboniferous limestone interrupted these conditions on the west side of the Appalachians, but not on their eastern side. At the close of this we find the rich Middle Devonian flora, which diminishes toward the close of the period; and after the physical disturbances which on the east side of the Appalachians terminated the Erian age, it is followed by the meagre and quite dissimilar flora of the Lower Carboniferous; and this, after the subsidence indicated by the Carboniferous limestone, is followed by the Coal-formation flora.

If we compare the Erian and Carboniferous floras, we find that the leading genera of the latter are represented in the former, but, for the most part, under distinct specific forms; that the Erian possesses some genera of its own, and that many Carboniferous genera have not yet been recognised in the Erian. There is also great local diversity in the Erian flora, conveying the impression that the conditions affecting the growth of plants were more varied, and the facilities for migration of species less extensive than in the Carboniferous.

In comparing the Erian flora of America with the Devonian of Europe, we meet with the difficulty that little is known of the plants of the Lower and Middle Devonian in Europe. There are, however, specimens in the Museum of the Geological Survey which show, in connection with facts which can be gleaned from the works of continental writers, that *Psilophyton* occupied the same important place in Europe which it did in America; and in the Upper Devonian the generic forms are very similar, though the species are, for the most part, different.

In Eastern America no land flora is known below the Upper Silurian; and even in that series the plants found are confined to the genus *Psilophyton*. Independently, however, of the somewhat doubtful Lower Silurian plants stated to have been found in Europe, there are indications, in the Lower Erian flora, that it must have been the successor of a Silurian flora as yet almost unknown to us; and the line of separation between this old flora and that of the Devonian proper, seems to be at the base of the Middle Devonian.

In applying these facts and considerations to the questions relating to the introduction and extinction of species, and the actual relations of successive floras, it was proposed to compare what might be called specific types, that is, forms which in any given period could not be rationally supposed to be genetically related. Of these specific types, at least fifty may be reckoned in the Erian flora; of these, only three or four are represented in the Carboniferous by identical species, while about one half are represented by allied species. The remainder have no representatives.

A Table of specific types of the Erian was given, and its bearing shown on the questions above referred to; and the hope was expressed that by separating such types from doubtful species and varietal forms, some progress might be made towards understanding, at least, the times and conditions in which specific types were introduced and perished, and the range of varietal forms through which they passed.

Royal Institution, May 9.—Sir Henry Holland, Bart., F.R.S., president, in the chair.—T. W. Boord, F.S.A., Miss Eliza Bowman, Miss Margaret Graham, Rev. Brenchley Kingsford, M.A., H. F. Makins, R. Heber Prance, the Earl of Rosse, F.R.S., the Hon. Capt. R. Talbot, M.P., the Hon. P. S. Wyndham, M.P., were elected members of the Royal Institution. John Tyndall, LL.D., F.R.S., was re-elected as Professor of Natural Philosophy.

Geological Society, April 27.—R. A. C. Godwin-Austen, F.R.S., vice-president, in the chair. The following communications were read:—1. "On the species of rhinoceros whose remains were discovered in a fissure-cavern at Oreston in 1816." By George Busk, F.R.S., F.G.S. The object of this paper was to show that the rhinoceros whose remains were discovered by Mr. Whidbey in a fissure-cavern at Oreston, near Plymouth, in the year 1816, and described by Sir Everard Home in the "Philosophical Transactions" for 1817, belonged, not as has hitherto been supposed by every one except the late Dr. Falconer, to *Rhinoceros tichorhinus*, but to *Rh. leptorhinus*, Cuv. (*R. megarhinus*, Christol). The remains in question are in the Museum of the Royal College of Surgeons, and consist of between thirty and forty, more or less, broken portions of the teeth, and of numerous bones of the skeleton. The greater number being hardly in a condition to afford satisfactory diagnostic specific characters, the remarks in the paper were limited to the teeth and to a perfect metacarpal bone, which appeared amply sufficient for the purpose. The teeth mainly relied upon were the first or second upper molars (m^1 or m^2) of the right and left sides. Both the teeth were broken, but what was wanting in one was supplied by the other. The characters exhibited were shown to be unlike those of *R. tichorhinus*, and quite in accordance with those of *R. leptorhinus*. These were the thinness and smoothness of the enamel, the configuration of the dorsal surface, the form and size of the columns, and the disposition and relations of the "uncus" and "*pecten*" ("*crochet*" and "anterior combing-plate);" and the consequent absence of the characteristic "*tichorhine pit*" or *fossette*. The less strongly marked characters by which the teeth could be distinguished from those of *R. hemitœchus*, Falc., and *R. etruscus*, Falc., were also pointed out. The metacarpal bone selected for the illustration of the diagnosis is 9½ inches long, and remarkable for the compression of the shaft and its comparative slenderness, as contrasted with the same bone in *R. tichorhinus*, specimens of which were exhibited on the table, and which, in no case within the author's knowledge, ever exceeds 7½ or 8 inches in length, and is proportionately much thicker than in *R. leptorhinus* or any other extinct species. The size and form of the bone also showed that the species could not be either *R. hemitœchus* or *R. etruscus*, for although the means of direct comparison with the third metacarpal of those species did not, to the author's knowledge, exist in London, its probable general dimensions and proportions could be deduced from those of the corresponding metatarsal, of which bone numerous specimens were available. It was further shown that the Oreston metacarpal exactly corresponded with those of *R. leptorhinus*, from Grays Thurrock, in the British Museum. The determination of the species appears to be of considerable interest, inasmuch as it affords an additional instance of the occurrence in England of the great southern Rhinoceros. This is also the only example of the discovery of that species, except in river or other deposits, either in this country or on the Continent. The Chairman remarked that at one time the Oreston *Rhinoceros* was referred to *R. tichorhinus*, but that Buckland, although mentioning the *Rhinoceros*, never gave it a specific name. The Chairman also said that the Oreston fissures were not caves, but mere fissures which had been filled in; an entire skeleton occurred at one spot, and the animal must have fallen in. Mr. Boyd Dawkins had been struck by the non-tichorhine character of the Oreston specimens some years since. He confirmed Prof. Busk's determination, and remarked that five British species of *Rhinoceros* are known, namely: 1. *R. Schleiermacheri*, from the Red Crag of Suffolk (in the Miocene at Darmstadt); 2. *R. etruscus*, from the Forest Bed = *R. Merckii* (Von Meyer); 3. *R. megarhinus* (Christol) = *leptorhinus* (Cuv.); but the latter name includes also *R. etruscus* and *R. hemitœchus*; so that the adoption of De Christol's name gets rid of a difficulty; 4. *R. hemitœchus*; and 5. *R. tichorhinus* = *R. antiquitatis* (Blum.). Prof. Busk, in reply, stated that Oreston was a fissure-cavern, and noticed the successive openings in 1816, 1821, and 1826. He did not agree with Mr. Boyd Dawkins in preferring the name *megarhinus* to Cuvier's *leptorhinus*. He did not know of the occurrence of two species of *Rhinoceros* at Oreston.

2. "On two Gneissoid series in Nova Scotia and New Brunswick, supposed to be the equivalents of the Huronian (Cambrian) and Laurentian." By H. Youle Hind, M.A.

This paper described the relations of two gneissoid series in Nova Scotia and New Brunswick, which have hitherto been regarded as intrusive granites and syenites, and have been thus represented on the published geological maps of those provinces. The author considered that these gneisses were in the main of Laurentian age, the Huronian or Cambrian rocks occurring only in patches over a vast area of Laurentian porphyroid gneiss. The old gneiss was stated to be brought to the surface by three great undulations between the Atlantic coast of Nova Scotia and the Laurentian axis of America north of the St. Lawrence. These axes were rudely parallel to one another, and in the troughs which lay between them the Silurian, Devonian, and Carboniferous series occurred in regular sequence, the New Brunswick Coal-field occupying the central trough. On the line of section, in the troughs to the north-west and south-east, the Lower Carboniferous was stated to be the highest rock series which has escaped denudation. The gold-bearing rocks of Nova Scotia are of Lower Silurian age, and rest either on Huronian strata or, where these had been removed by denudation, on the old Laurentian gneiss. The gold is found chiefly in beds of auriferous quartz of contemporaneous age with the slates and quartzites composing the mass of the series, which, in Nova Scotia, is 12,000 feet thick; and the auriferous beds are worked, in one district or another, through a vertical space of 6,000 feet. Besides auriferous beds of quartz, intercalated beds and true veins are found to yield gold, and are worked. A series of sharp and well-defined anticlinals ridge the province of Nova Scotia from east to west, while another series of low broad anticlinals of much later date have a meridional course. At the intersection of these anticlinals the gold districts are situated, because there denudation has best exposed the upturned edges of the auriferous beds of quartz, and rendered them accessible, sometimes exposing also the underlying gneiss. Plans of Waverley and Sherbrooke gold districts were exhibited, showing the outcrop of the edges of the slates and auriferous beds of quartz in semi-elliptical forms, with the gneiss at the base of the ellipse. On this ground it was suggested that a correct mapping of the gneisses of Nova Scotia would have an important influence on the development of the mineral resources of the province. A plan of some of the lodes in the Waverley gold district showed the result of operations in 1869, subsequently to the publication of a geological map and sections of the district furnished to the Department of Mines by the author in 1868. Citations were made from the annual reports just issued of the Chief Commissioner of Mines and of the Inspector of Mines, confirming the correctness of the author's plans exhibiting the geological structure of Waverley, which is a type of all the Nova Scotian gold districts. Principal Dawson spoke in confirmation of the fact that the Palæozoic rocks are underlain by Laurentian gneiss, &c., quite to the eastern coast of British North America, and stated that the same relation occurred in Newfoundland, and had been traced southwards into Massachussetts. He confirmed Mr. Hind's views generally, and stated that the Lower Silurian of Nova Scotia includes no great fossiliferous limestone, like that of the interior of North America. The supposed *Eozoon* discovered by Dr. Honeyman, was probably distinct from *E. canadense*, but was certainly a Foraminiferal organism allied to *Eozoon;* but as *Eozoon bohemicum* is of later date than *E. canadense*, the presence of *Eozoon* did not necessarily indicate Laurentian age. Prof. Ramsay suggested that other organisms besides *Eozoon* aided in building up these great calcareous masses. He inquired as to the mode of occurrence of gold, and suggested that the gold is obtained at the anticlinals merely because the exposure is better, and that it will be found to pervade the synclinals also. Mr. Henry Robinson had visited the Waverley district in company with Prof. Hind, in the winter of 1868, at which time the mining on the lodes referred to in the map before the society was at a standstill, the lodes having been lost by reason of a fault. He thought it was very satisfactory to find that the explorations of Prof. Hind, and the theoretical position which he assigned to the lodes, had been completely verified. Mr. Robinson also stated that gold is being mined in the synclinals by sinking shafts and driving cross-cuts. Mr. Hind remarked that all the Lower Silurian in Nova Scotia was auriferous, and that the gold was derived from the underlying Laurentian rocks. He stated that Sir W. E. Logan had indicated an auriferous zone in the Laurentian of Canada. Gold was finely distributed in the slates of Nova Scotia, as in Victoria, in the neighbourhood of lodes, according to Mr. R. Brough Smyth.

Chemical Society, May 5.—Prof. Williamson, F.R.S., President, in the chair. The following gentlemen were elected fellows: G. Matthey, T. Steel, T. Allen.—Mr. Brown read a paper on "Vapour densities," wherein he gave a historical review of the various methods employed for the determination of such densities.—Mr. Church communicated the analyses of two Cornish minerals. The one, Restormelite, may be regarded as a variety of kaolinite, standing nearest to the lithomarge group. The analysis gave the following figures:—

H_2O	11·68	per cent.
SiO_2	45·21	,,
Fe_2O_3	1·11	,,
Al_2O_3	35·10	,,
MgO	0·85	,,
K_2O	2·30	,,
Na_2O	4·12	,,

This corresponds pretty well with the formula of kaolinite, Al_2O_3, $2\ SiO_2+2$ aq., if we suppose a partial replacement of hydrogen by sodium or potassium, and of aluminium by iron. Restormelite may be considered as preserving in its alkalies more evident traces of its feldspathic origin than are usually found in such alteration products. The second of the above-mentioned minerals is Chalcophyllite. The recorded analyses of this mineral were so unsatisfactory that Mr. Church thought it worth his while to submit to a new investigation. The figures he obtained in his analysis led him to assign to chalcophyllite the formula $8\ CuO$, $Al_2\ O_3$, $As_2\ O_5 + 24$ aq. The mineral cannot be dried even in vacuo without an entire change in its appearance. The beautiful green and transparent crystals become of a more bluish tinge, and quite opaque. This change corresponds to a loss of 13·79 per cent. of water.—Messrs. Bolas and Gloves communicated a paper on their newly-discovered tetrabromide of carbon. This compound is obtained by heating bisulphide of carbon with bromide of iodine in a sealed tube to a temperature of 150° C for about forty-eight hours, adding afterwards caustic soda to the contents of the tube, and submitting the mixture to distillation, when the tetrabromide of carbon will distil over. Bromoform and bromopicrin, when treated with bromide of iodine, yield the same result. The bromide of iodine can be replaced by antimony terbromide. Tetrabromide of carbon is a white crystalline substance, melting at 91° C., insoluble in water, but readily soluble in ether, hot alcohol, benzol, American oil, bromoform, and chloroform. Sodium amalgam reduces it, first to bromoform, then to methylene dibromide. The authors propose to carry on their investigations of this interesting compound.

Anthropological Society of London, May 3.—Dr. R. S. Charnock, V.P., in the chair. Moore A. Cuffe, LL.D., 9, Camden Crescent, Bath, was elected a Fellow. A paper was read by Major W. Ross King, F.R.G.S., F.S.A.S., on the "Aboriginal Tribes of the Nilgiri Hills," namely, the Todas, Khotas, Erulas, and Kurumbas, especially noticing the former, as being the most singular and important. The author, who was three years among these tribes, described in turn the characteristic features and peculiarities of each, with detailed information as to their very curious social customs, and religious rites and ideas; showing the marked distinction existing in every point between tribes occupying one and the same area, and in constant communication with each other; pointing out the fact that each people retained its own language; and their remarkable isolation from the surrounding enormous population of the plains. The striking similarity between the rites, practices, and monuments of the Todas and those of the ancient Celts of Britain was shown; a passing allusion was made to the evidences of an early western migration as traceable through intervening countries in the existence of similar rites and customs; and the presence on the Nilgiri hills of Druidical circles, cromlechs, kistvaens, and tumuli was described, precisely similar to those so well-known in our own country. While commenting on the analogies thus apparent between the ancient Celts and some of these Hill Tribes, the author took occasion also to remark on their similarities in other respects to the Jews of old, to the Kaffirs, and to the ancient Romans, not as being likely to lead to any theory of origin in those quarters, but as possibly qualifying the reliance to be placed on every point of Celtic resemblance. In conclusion, the author, who illustrated his paper by the exhibition of several drawings, and of some interesting native ornaments, &c., summed up the various theories prevailing as to the probable origin of these tribes, of whose history we are still so ignorant, and recommended the subject to the Society as one worthy of their investigation.

Linnean Society, May 5.—The following foreign members were elected in the place of those who have died during the past year:—Prof. Spencer F. Baird, of Washington; Herr George Ritter von Frauenfeld, of Vienna; Dr. William Lilljeborg, Prof. of Zoology at Upsala; Dr. Charles Naudin, of Collioure, Pyrenees; and Sig. Roberto di Visiani, Prof. of Botany at Padua.—A letter was read from Dr. Ernst, of Caracas, on a peculiar plant belonging to that country known as "incense," a small tree forming a striking feature in the scenery. It was described by Humboldt and De Candolle under different names, its affinities not having been accurately determined. Dr. Ernst has established its right to form a distinct genus, to which he gives the name of *Libanothamnus*.—Dr. Hooker read a communication from Dr. Kirk, vice-consul at Zanzibar, on "Copals." One characteristic by which fossil copal is known from the recent resin, in addition to its greater transparency, is the so-called "goose-skin." Dr. Kirk has ascertained that the fossil copal shows no trace of this goose-skin when first dug out of the earth, but that it makes its appearance only after cleaning and brushing the outer surface. Specimens exhibited of both recent and fossil copal contained imprisoned flowers, leaves, and insects, in a beautiful state of preservation. Captain Grant states that the true copal gum-tree is a climber which climbs to a great height among the forest trees, and finally becomes completely detached from its original root, when the copal exudes from the extremities of these detached roots. Large pieces of the resin fetch a very high price even in that country.

Zoological Society, April 28.—John Gould, F.R.S., V.P., in the chair. The Secretary read some notes on the principal additions to the Society's Menagerie during the month of March, and called particular attention to four Burrowing Owls presented by G. Wilks, Esq., C.M.Z.S., and to a wood-loving antelope (*Cephalophus sylvicultrix*), obtained by purchase.—Mr. J. E. Harting, F.Z.S., exhibited an unusually fine specimen of the Dusky Redshank (*Totanus fuscus*) in summer plumage, recently killed near London.—The Rev. H. B. Tristram exhibited two skins of *Salicaria melanopogon*—a rare European warbler, obtained near Ettawah, north of Agra, being the first recorded occurrence of this species in Central India.—Dr. E. Hamilton communicated an extract from a letter addressed to him by his nephew, Capt. Hamilton, lately commanding detachment at Port Blair, concerning the true locality of the so-called "Andaman Monkey," now in the Society's Gardens, which was stated to have been imported into the Andaman Islands from Burmah. —A letter was read from Dr. John Anderson, F.Z.S., of the Indian Museum, Calcutta, announcing that he had obtained a specimen of the dolphin of the Irrawaddi, which turned out to be a species of the genus *Globiocephalus*.—Mr. St. George Mivart read a memoir on the axial skeleton of the tailed batrachians, containing observations on the development and mode of formation of the spinal column of these animals.—A communication was read from Mr. Gerard Krefft, C.M.Z.S., containing the description of a new and very remarkable animal, allied to *Lepidosiren*, recently discovered in the freshwaters of Queensland. Mr. Krefft considered this animal to be an Amphibian, and referred it to the genus *Ceratodus* of Agassiz, proposing to call it *Ceratodus Forsteri*, after Mr. Wm. Forster, its discoverer.—Mr. R. Swinhoe, F.Z.S., read a paper on the Mammals of Hainan, as observed during his recent visit to that island. The number of species enumerated was 21, amongst which was a hare, believed to be undescribed, and proposed to be called *Lepus hainanus*.—A second communication was read by Mr. Swinhoe, being a list of reptiles and batrachians found in the same island, with notes on their habits. The species had been determined by Dr. Günther. — Mr. D. G. Elliot, F.Z.S., read a paper on some new genera and species of birds belonging to the families *Formicariidæ*, *Pachycephalidæ*, and *Sylviidæ*. These were proposed to be called *Xenorhynchus pachycephaloides* (from New Caledonia), *Clytoctantes alixii* (from Ecuador), and *Calamoherpe subflavescens* (from Dahouria).—Messrs. Sharpe and Dresser read a paper "On some new or little-known points in the economy of the common swallow" *Hirundo rustica*). The authors drew special attention to the changes of plumage through which this species passed during its residence in Southern Africa.—Mr. G. B. Sowerby communicated descriptions of 48 new species of shells from various localities.

CARDIFF

Naturalists' Society, April 5.—A paper was read on "Water in its different forms," by Mr. Vivian. A large number of very interesting objects contained in various descriptions of water were shown under the microscope, among which the most interesting were the contents of two vials, both from a shallow, muddy-looking bog on Splottland Moor, which furnished a puzzle for the members of the Society, and a satisfactory solution of which is still a desideratum. One of these was filled with the yellow gelatinous substance which deposits the famous bog iron, consisting chiefly of a very minutely twisted conferva, which Sir C. Lyell, after Ehrenberg, called *Gallionella*, but which is now more commonly named with Griffith *Didymohelix ferruginea*. Within this yellow substance was an innumerable swarm of bluish-green animalcules (*Stentor polymorphus*), with several specimens of two other kinds of Vorticellineæ—viz., *Urocentrum turbo* and *Cœnomorpha medusula*. This is a very funny living parasol, worth seeing. The other vial, except the yellow stuff, which was eliminated on purpose, had the same trumpeters (*S. polymorphus*), which, wonderful to say, on being corked disappeared, collapsing all at once, and leaving nothing behind but a milky, bluish-green water, which still keeps its colour after several days. We shall wait to see if any living creature will ever come out of it by spontaneous or hemigerminal generation. From another vial a good harvest of phytozoa (*Euglenæ*) was expected, all the water looking deep yellow green; but this water, too, never settled as it does when living *Euglenæ* are collected, a clear proof that here also the animals come to grief—when and how? —that is the question the members of the Cardiff Naturalists' Society wish to have solved by some of our readers. In reference to the contents of a single drop of water, Prof. Gagliardi remarked:—It was in the same gathering that an extremely minute protoplasmic bit of living matter was seen. Under a magnifying power of 500 diameters this floating atom looked like a little comma, scarcely half the size of the *Surirella minutæ* that was living with it. Another unusually large specimen of *Amœba* came out of a gathering in a pond in Cathays; it looked somewhat like a streaming worm. I have no doubt that it was but a stronger variety of the *Amœba princeps*; yet, seeing how steadily it kept to the vernicular form, with very slight changes now and then, I should call it rather *A. vermicularis*.

GLASGOW

Geological Society, April 14.—Professor John Young, President, in the chair.—Mr. James Geikie, Vice-President, read a letter from Mr. Croll, of the Geological Survey of Scotland, referring to a paper contributed by him to the transactions of the Edinburgh Geological Society, on "Ancient River Channels buried under Drift," and on which Mr. John Young had made some remarks at a previous meeting. An animated discussion ensued on the points referred to in the letter.—Mr. D. Bell read a paper entitled "Aspects of Clydesdale during the Glacial Period." He gave a sketch of the succession of events which had been made out from the dawn of the glacial epoch, down to a comparatively recent geological time—beginning with the period of land-ice, and ending with the "last elevation" of the land. As to the period of land-ice, he thought the first point which they had to fix in their minds and try to get some adequate notion of, was the great thickness and mass which the ice attained. He did not know where they could get a better or more impressive idea of this than by ascending Ben Lomond. He described the marks of the ice, the grooved and polished surfaces, that may be traced from the shore at Rowardennan to a great height on Ben Lomond, observing that the ice evidently did not come *down* the mountain, but moved *along* or *across* it. He also referred to similar markings on the neighbouring hills, and the conclusion these all led to was, that the entire hollow in which Loch Lomond now lies was at one time filled from side to side with a mass of ice which only the higher mountains overtopped, and from which Ben Lomond itself only rose as a little rocky islet. Having shown that this was quite in harmony with what had been observed in other parts of the country, among the mountains of Perthshire, Aberdeenshire, and Argyleshire, he said he had no doubt the great depth of Loch Lomond in its upper part, where it is not less than 100 fathoms, was due mainly to the action of the ice, which was there compressed and imprisoned, forcing its way between the hills. In the lower part it got spread out more, so that although it had softer rocks to deal with, it produced comparatively a less effect. He then alluded to similar proofs of glacial action in the neighbouring parts of the Firth of Clyde—at Garelochhead and along the shores of that loch, of Lochlong, Lochgoil, and the Holy Loch—on the hills behind Gourock, Greenock, and Port-Glasgow—on Dumbarton Castle rock—on the flanks of the Kilpatrick hills—on the opposite side of the river near Bishopton, and all over the lower grounds from the Gleniffer and Cathkin braes on the one side, to the Campsie hills on the other. The persistency and uniformity of direction of these markings, alike in the valley and over the neighbouring hills, sufficiently proved the great volume the ice must have attained. He then proceeded to consider the formation of the boulder clay, holding that the lower till or clay was the product of this great sheet of land ice, and that the upper boulder clay was more probably due to sea ice during the period which followed, when the land was submerged to a depth of several hundred feet beneath its present level. He referred to the beds of sand found interspersed throughout the boulder clay, and thought that whether the theory of land or sea ice were adopted, these might be explained without supposing, as some did, that there had been so many distinct "breaks" in the glacial period. He pointed out the narrowness of the basis on which such conclusions rested, only a very few of the borings which had been adduced showing more than one or two beds of sand; and maintained that the one "break" of which we seemed to have evidence, between the first and second depression of the land, was sufficient, if we considered and gave due weight to the gradual advance and retreat of the ice in each case, and the accumulations of water that must have been caused thereby, to account for all the beds of sand that had been described. Coming to the "shell-beds" which had been found at various heights in the Clyde valley, from Airdrie, at 510 feet, down to Paisley, and from that to the present sea margin, he was of opinion that the theory of "unequal elevations," which had been proposed to account for these beds being found at so many different levels, was quite untenable, being a most objectionable and unphilosophical theory in every respect. He believed the true explanation was to be sought, not by supposing the beds to have been strictly *contemporaneous*, or formed at the same level, and afterwards "unequally elevated," but by considering them to have been *successive*, formed at different levels during the gradual sinking or rising of the land, as the depth of the sea, its freedom from ice, and other conditions, became favourable to the various forms of marine life, whose remains are found in the beds referred to.—Mr. John Young then exhibited some specimens of finely-laminated clay from the excavations in the College grounds, pointing out the traces of organisms which they presented, some of which were supposed to be of Annelides, others of Crustacea. Arrangements for the society's excursions during the summer months were afterwards announced, and the proceedings terminated.

PARIS

Academy of Sciences, May 2.—M. Le Verrier communicated a note by M. Aoust on roulettes in general.—The following papers on physical subjects were read :—A memoir by M. Becquerel on the cause of the electrical effects produced by the contact of metals with distilled water, in which the author, after discussing the opinions of previous writers on the subject, and describing his experiments, comes to the conclusion that these effects are due to the reaction of the water upon the gases absorbed by the inoxidisable metals, whilst those furnished by oxidisable metals arise from the presence of a slight coat of oxide upon their surface, which renders them positive relatively to metals not so protected. By M. J. Jamin and M. Cornu, notes in opposition to the results obtained by M. Croullebois with regard to the index of refraction of water; and a a reply by M. Jamin to the recent note by M. Renou on the latent heat of ice. A memoir by M. Lecoq de Boisbaudran on the constitution of luminous spectra, containing a comparison of the spectra of chloride, bromide, and iodide of barium, showing that an augmentation of mean wave-length in some degree proportional to the augmentation of molecular weight, is caused by the substitution of one halogen for another, as well as by the substitution of one metal for another. A continuation of M. P. Desains' researches upon calorific spectra. A note by M. E. Bouchotte, communicated by M. E. Becquerel, on the estimation of the relation existing between the dynamic work expended and the quantity of electricity produced in Holtz's machine, in continuation of a note upon the same subject presented in February last; and a note by M. Limouzin, presented by M. Bussy, relating to a communication by M. Duclaux on the formation of liquid drops, and remarking that the author, more than a year ago, presented to the School of Pharmacy an alcoholometric apparatus constructed upon the principle indicated by M. Duclaux.—A note by M. Lacoine, on the effects produced by the Aurora of the 5th April on the Turkish Telegraphic lines, was presented by M. Leverrier. The author observed a complete stoppage of transmission in the line from Pera to Semlin, the line being traversed by a strong current in the opposite direction, indicating a terrestrial current from north to south.—M. Descloizeaux presented a note on the crystalline form and optical properties of a compound of protochloride of platinum and triethylophosphine analogous to Magnus's salt.—The following strictly chemical papers were also read :—A note by M. Morren to the president on the combustibility of the diamond, and the effects produced upon it by high temperatures. The author stated that when heated by means of common coal, or brought to a white heat in a current of coal gas, diamonds become blackened on the surface, but without change of weight; with pure hydrogen no alteration is produced; with carbonic acid they lose lustre and weight. He added that diamonds burn readily when exposed to the blow-pipe flame of a glass-blower's lamp upon a piece of platinum, and that the whole substance does not burn with equal readiness, so that if the operation is interrupted, the surface of the residue shows numerous small equilaterally triangular faces belonging to minute octahedra.—A note on the solubility of chloride, bromide, and iodide of silver in salts of mercury, by M. H. Debray.—A memoir on a new process for the volumetric determination of copper, by M. F. Weil, communicated by M. Dumas. This process depends on the facts that in presence of an excess of free hydrochloric acid, and at a boiling temperature, the least trace of bichloride of copper gives a distinct greenish yellow tinge to its solution, and that under these circumstances protochloride of tin instantly converts the salts of binoxide of copper into colourless proto-salts. The termination of the reaction is determined by means of bichloride of mercury, which produces the characteristic white precipitate of calomel with the slightest excess of chloride of tin.—A paper on the products of the fermentation of pyrotartaric acid and its homologues, by M. A. Béchamp. The author stated that as succinate of lime by fermentation furnishes butyric acid, with evolution of hydrogen and carbonic acids, its homologue, pyrotartaric acid, might also be expected to produce butyric acid, but that at the close of the operation the apparatus contains only carbonate of lime, whilst the gases evolved are carbonic acid and marsh gas. He also noticed the behaviour of several organic acids when fermented by means of chalk in presence of a small portion of flesh.—A note by the same author on the preparation of pyrotartaric acid. He operates upon anhydrous tartaric acid mixed with pumice, and obtains about 20 per cent. of pyrotartaric acid.—A note by M. F. Pisani on the minerals obtained in the copper mine of Cap Garonne (Var) was communicated by M. Descloiseaux. These minerals are Adamine (of which the author gives analyses), Chalcophyllite, Lettsomite, Brochantite, Olivenite, Mimetese, Azurite, Malachite, and Barytine.—M. Prunières forwarded some specimens of charcoal and carbonised wood, collected in the Lozère from a sedimentary deposit between granite and basalt, at a depth of 40 metres. Some of them bore remarkable notches, "which will have to be studied from another point of view."—M. Duméril communicated some observations by M. E. Moreau, on the structure of the *chorda dorsalis* in *Amphioxus lanceolatus*. Appended to the *chorda dorsalis* in this fish, the author finds neurapophyses and hæmapophyses; he also describes the sustaining pieces of the fins, especially the dorsal, which he regards as representing fin-rays amalgamated with interspinous pieces.—M. Brongriart communicated a memoir by M. A. Gris, containing anatomical and physiological observations on the pith in ligneous plants. The author distinguishes three medullary elements, namely, *active*, *inert*, and *crystalligenous* cells. When the first and third of these are present, he calls the pith *homogeneous medulla*; the first and second constitute a *heterogeneous medulla*. From the presence of starch in the active cells of the pith in large branches and trunks showing from eighteen to twenty-eight circles of growth, the author concludes that the supposed inertia of the medulla is by no means certain. He regards it as an organ of reserve.

BERLIN

Royal Prussian Academy of Sciences, January 6.—The following scientific paper was read :—On the theory of the newest Electrophorous machines and on supernumerary conductor. By M. Riess.

January 17.—Professor W. Peters read a memoir on the Ductus pneumaticus of the lower jaw in the crocodile.

February 10.—Professor W. Peters read a memoir on the African monitors and their geographical distribution, in which he indicated the synonymy and distribution of the following species:—*Monitor niloticus*, Hasselqu.; *M. saurus*, Laur.; *M. albogularis*, Daud.; *M. ocellatus*, Rüpp.; *M. exanthematicus*, Bosc; and *M. griseus*, Daud. He adopts the Cuvierian name for the genus, as it is three years earlier than Merrem's *Varanus*. Prof. Peters also read a contribution to the knowledge of the herpetology of South Africa, including a list of a few species of lizards, snakes, and batrachia, chiefly from Hantam, in the Calvinia district. A new species of gecko, *Chondrodactylus angulifer*, is described and figured by the author; it is the type of a new genus, allied to *Stenodactylus*, but destitute of claws. The author also remarks upon the characters and synonymy of *Agama hispida*, Linn.; *A. atra*, Daud.; *Eremias capensis*, Smith; and *Euprepes vittatus*, Oliv.; and figures two small species of tree frogs, namely, *Arthroleptis Wahlbergii*, Smith; and *Hyperolius tuberilinguis*, Sundevall. He proposes to change the name of his *Hemidactylus variegatus* to *H. picturatus*.—A memoir by M. Kostka, on the determination of the ellipsoidal figure of equilibrium of a homogeneous mass of fluid rotating round a fixed axis, when its density and period of rotation are known, was presented by M. Weierstrass.

February 14.—Professor Dove read a note on the compensation of the cold observed in Europe in January of the present year, by an unusual elevation of the temperature in America.—Professor Ehrenberg made a preliminary communication on the beds of Bacillariæ in the high lands of California, in which he noticed the occurrence of great beds consisting wholly of Diatomaceæ in various parts of the Californian territory.—M. Weierstrass presented a memoir by M. Ketteler on the influence of ponderable molecules upon the dispersion of light, and upon the import of the constants of the dispersion formulæ.

February 17.—The papers read at this meeting were chiefly of historical or antiquarian interest, but they included an important contribution to the history of algebra in Germany, by Prof. Gerhardt, of Eisleben.

February 24.—Prof. A. W. Hofmann read a paper on the preparation of the ethylamines on the large scale. The author finds that the most volatile of the subsidiary products of the manufacture of chloral, if condensed and digested at 212° F. with a strong alcoholic solution of ammonia, furnishes, by a simple subsequent treatment described by him, a considerable proportion of hydrochlorates of the amine bases, which may be isolated by the addition of concentrated solution of soda. Professor Hofmann also read some supplementary remarks upon the products of the desulphurisation of diphenylosulphocarbamide.

German Chemical Society, April 11.—Two papers by L. Carius were communicated. The first describes a new method of preparing dibrominated acetic ether, by the action of bromine on acetic ether. The second announced new syntheses of maleic and phenaconic acids, by the use of disodic acetic ether, $C\ H\ Na_2\ C\ O\ O\ C_2\ H_5$ on bibromoacetic ether, and on bibromosuccinic ether.—Messrs. Schneider and Erlenmeyer have investigated normal iodopropionic acid. Treated with acetate of silver, this acid yields acetoxypropionic acid.—L. Fleury publishes researches on new derivations of allyle, viz.: $C_3\ H_5\ Cl_2\ N\ O_2$, $C_3\ H_5\ I\ Cl_2$ and $C_3\ H_6\ O\ H\ H\ Cl\ O$.—A. Ladenburg has discovered a distannic ethide, $Sn_2\ (C_2\ H_5)_6$. The vapour density serving to establish the formula of this compound was taken by Hofmann's method, the constant temperature being produced by distillation of oil of cloves. Chlorine and iodine separate the molecule producing

$$Sn\ (C_2\ H_5)_3\ Cl,\ \text{or}\ Sn\ (C_2\ H_5)_3\ I.$$

C. Liebermann reported on an easier method patented by himself, in conjunction with Messrs. Graebe and Caro, for preparing artificial alizarine. Instead of brominating anthracene they treat it with sulphuric acid. According to the quantities employed, either one, two, or three atoms of hydrogen are replaced by the group HSO_4. $C_{14}\ H_8\ (SO_3H)_2$ fused with potash yields $C_{14}\ H_8\ (OH)_2$, and this is oxydised into alizarine $C_{14}\ H_6\ (HO)_2\ O_2$. Or they transform anthracene $C_{14}\ H_{10}$ first into anthrachinone $C_{14}\ H_8\ O_2$, and treat this substance with sulphuric acid. The compound $C_{14}\ H_6O_2\ (HSO_4)_2$ may then be transformed by fusion with potash into $C_{14}\ H_6O_2\ (OH)_2$. The compound $C_{14}\ H_5\ O_2\ (HSO_4)_3$ is transformed by this process into purpurine. A process lately patented by Bronner and Gubzkow for preparing alizarine was then severely criticised by Mr. Liebermann; this process, consisting in fusing anthrachinone with potash, yields only a trace of a blue colouring matter, but no alizarine. He intends to return to this subject.—Professor Rammelsberg reported on the action of periodic acid on the oxides of thallium. Protoxide of thallium treated with periodic acid is partly converted into the iodate, and partly into peroxide of thallium. Sesquioxide of thallium, on the contrary, combines with periodic acid.—V. Meyer has continued his researches on the synthesis of organic acids, by treating sulpho-salts with formiates. Sulphonaphthalate of potassium, when fused with formiate of sodium, produces acid sulphite of potassium and naphtalinecarbonate of sodium. Chlorosalylate of potassium treated in the same way, however, yields chloride of potassium and benzoate of sodium.

April 25.—Messrs. Krämer and Pinner have continued their researches on aldehyde by submitting it to the action of chlorine-gas. Conducted in this way, the reaction takes place in a different manner from that described by Wurtz, who, pouring an excess of aldehyde into large vessels filled with chlorine, obtained chloride of acetyle and its compound with aldehyde. Neither of these substances has been obtained by Messrs. Krämer and Pinner. Nor is ordinary chloral obtained by this reaction, the aldehyde being entirely converted into the chloral of the condensed aldehyde, C_4H_6O, known as crotonic aldehyde. Crotonic chloral is a liquid, boiling at 165°, and forming with water, but not with alcohol, a crystalline compound. By oxydation it forms trichlorocrotonic acid. Caustic potash transforms it into the corresponding chloroform $C_3\ H_2Cl_3$ and its derivative $C_3\ H_3\ Cl_2$ (bichlorinated allylene?) boiling at 78°.—C. Martius has studied the combinations of chloral with alcohols. Amylic alcohol forms with it a beautifully crystallised compound. Mercaptans also combine with chloral.—F. Rüdorff communicated a method of determining with great exactness the quantities of pure glacial contained in acetic acid of different degrees of concentration. It is founded on the melting-points of pure acetic acid (16°·7 C.) and its mixtures with water. Commercial glacial acetic acid contains often as much as 10 per cent. of water, and then melts at 10°·3 C., or even 15 per cent., and then melts at—0°·2.

DIARY

THURSDAY, May 12.

Royal Society, at 8.30.—On the Results of the method of investigating the Nervous System, more especially as applied to the elucidation of the Functions of the Pneumogastric and Sympathetic Nerves in Man: Dr. A. Waller (Croonian Lecture).

Society of Antiquaries, at 8.30.—On recent Discoveries at Rome: J. H Parker.

Mathematical Society, at 8.—Mechanical description of a nodal bicircular Quartic: Prof. Cayley.

Zoological Society, at 8.30.—Notes on some points in the Anatomy of certain Kingfishers: Dr. Cunningham.—On the taxonomic characters afforded by the muscular sheath of the œsophagus in Sauropsida and other Vertebrates: Mr. George Gulliver.—Notes on the myology of *Platydactylus Japonicus*: Mr. Alfred Sanders.—On the Hirundinidæ of the Ethiopian region: Mr. R. B. Sharpe.

Royal Institution, at 3.—Electricity: Prof. Tyndall.

FRIDAY, May 13.

Royal Institution, at 8.—Descent of Glaciers: Rev. Canon Moseley.

Royal Astronomical Society, at 8.

Quekett Microscopical Society, at 8.

SATURDAY, May 14.

Royal Institution, at 3.—Comets: Prof. Grant.

MONDAY, May 16.

London Institution, at 4.—Botany: Prof. Bentley.

TUESDAY, May 17.

Institution of Civil Engineers, at 8.—Discussion upon Mr. Briggs paper on Rotary Fans.—On Recent Improvements in Regenerative Hot Blast Stoves for Blast Furnaces: Mr. E. A. Cowper.

Royal Institution, at 3.—Moral Philosophy: Prof. Blackie.

Anthropological Society, at 8.—Music considered as a Racial Characteristic: Mr. H. F. Chorley.

Statistical Society, at 8.—On the incidence of Local Taxation in the United Kingdom: Prof. Thorold Rogers.

THURSDAY, May 19.

Royal Society, at 8.30.

Society of Antiquaries, at 8.30.

Royal Institution, at 3.—Electricity: Prof. Tyndall.

Chemical Society, at 8.—On some Bromine Derivatives of Coumarine: W. H. Perkins, F.R.S.

BOOKS RECEIVED

English.—Other Worlds than ours: R. A. Proctor (Longmans.)—A New Manual of Logarithms; Dr. Bruhns (Williams and Norgate.)—Donkin's Acoustics (Macmillan.)—Thorell on European Spiders, Part 1 (Williams and Norgate.)

Foreign (through Williams and Norgate).—Baron Von der Decken's Reisen in Ost-Afrika; 4ter Band, Die Vögel Ost-Afrikas—Beiträge zur vergleichenden Anatomie und Histologie der Ohrtrompete: Prof. Ridinger.—Die Reinigung und Erwässerung der Stadt Heidelberg: Prof. Friedreich.—Deutsche Vierteljahrsschrift für offentliche Gesundheitspflege; 2er Band, 1tes Heft.—Baillon's Histoire des plantes, Papilionacées: Zeitschrift für Parasitenkunde, Vol. 1.—Untersuchungen aus dem Institute für Physiologie und Histologie in Graz: A. Rollett.—Etude préhistorique sur la Savoie: A. Perrin.—Die Fische Deutschlands und Schweiz: J. C. Weber.—Grundriss der Physiologie des Menchen: Dr. L. Hermann.—Annalen der Oenologie 1er Band 2tes und 3tes Heft.—Beiträge zur Anatomie und Physiologie: C. Eckkard.

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THURSDAY, MAY 19, 1870

SCIENTIFIC EDUCATION

THAT the Government of this country is anxious to advance Science education is plainly manifest from what it has already done, in making large annual grants to institutions which it established, and which it maintains as its own. That it does not consider the present arrangements for this purpose as final or sufficient, is clear from the recent appointment of a Royal Commission to inquire into the whole question of Government aid to Science. The movement which resulted in the appointment of this Commission arose, as we have already explained, from a recommendation of the Council of the British Association for a formal inquiry into the existing state of Science education in this country; and the resolution stated: "That no such inquiry will be complete which does not include the action of the State in relation to scientific education, and the effects of that action upon independent educational institutions." Before the Commission meets, it seems desirable that those interested in advancing Science and Education generally, should seriously consider the different position in which Science now stands, as a means of education, to that which it formerly occupied. The time was, and not long ago, when Science was regarded as a thing by itself, having no connection with other branches of education, and useful mainly as a means for rendering men better machinists, better artisans, or discoverers of processes for the advancement of arts and manufactures. Many doubtless hold these opinions at present, and one concludes this to be the case from the very limited view which is expressed by the term "technical education" which is so generally used. Now, if it be desired to promote this view only, and to teach, Science alone, and not as a part of general education Government has established perhaps such schools as might meet the wants of the case, if it can be shown that they fulfil the expectations with which they were founded. But if the higher view, that Science is in its way as important a means of mental training as any other of the branches taught in our schools and universities, then some other method of extending Government assistance for its promotion must be adopted; and it is to this consideration we earnestly hope that inquiries will be directed. Since the first Report of the Science and Art Department, in 1854, sufficient time has been given to show whether the system then originated has answered its purpose. At page 2 of the Report, it is stated that its system will be "in the main self-supporting; while the advantages will be distributed over every part of the United Kingdom; and the assistance received from Parliament be applied for the general good of all." It is generally believed that the system is *not* self-supporting, but that every associate of the School of Mines costs the Government a considerable sum of money. There can be no question that the advantages of the system are very great, directed as it is, in the several branches, by men of the highest possible eminence; but it is urged that they are not to any great extent distributed over the whole country, but mainly collected for the benefit of the technical schools founded by Government, and this tendency to force the official plan of education upon the country is regarded by many connected with other educational establishments as unfair. In fact, there is a threatened crusade against the Government professors.

To such we would remark, that the quantity of Science taught is so small, that it is not wise to attempt in any way to reduce that quantity; but it is certainly to be wished that the Commission should carefully inquire whether this method is one calculated to extend a sound Science education over the whole country, and whether it is possible to judge of a person's fitness to teach, without practical examination in subjects which are eminently practical, and without some guarantee that he has received a sound general education.

The Report further says: "It is essential that the institution should be supported to a considerable extent by the fees of pupils." This, it is urged, is not the case with the London Government school (and still less with the sister College of Science in Dublin), where the fees are not sufficient to pay the working expenses, to say nothing of the salaries of professors, and the scholarships of £50 per annum each, which are held by so large a number of students. At page 9 of the Report, the same important subject is dwelt upon: "My lords concur in the views expressed by the Lords of the Committee of Trade, that every means should be used to render these institutions as much self-supporting as possible, and that, in the plans adopted, that object should always be borne in mind. My lords adopt this view, not only because they feel it incumbent upon them to confine the public expenditure to the lowest limit, but also because they entertain a belief that the utility of such institutions is great in proportion as they are self-supporting." It may not be generally known that large sums have been expended, and further large sums are to be expended, in building and fitting up laboratories, lecture-rooms, &c., at South Kensington. The present time, then, is a very fitting one for an inquiry as to the present wants and resources of the country in relation to the higher science teaching, and the means best calculated to utilize and develope them with due regard to efficiency and economy. If it can be shown that the School of Mines has really done more work than unendowed schools, in proportion to the sums spent upon it, let its sphere of action be enlarged—at South Kensington or elsewhere—and let its usefulness be increased. But it must always be remembered that it professes to give none other than a *special* training: that it in no way supplies the place of universities, colleges, and schools of general education. If, on inquiry, it is found that it has attained the object for which it was established, still that does not touch the recommendation of the Council of the British Association, which includes "the action of the State in relation to scientific education, and the effects of that action upon *independent educational* institutions." It certainly ought to be a subject of serious inquiry, whether or not such colleges as University College, London, which has for forty years trained and sent out into the world some of the most distinguished teachers of Science, which in fact originated the present system of scientific education in union with other branches of education; or King's College, which has in like manner contributed so largely to advance modern education; or Owens College, Manchester, whose students show so well the nature of the education they there receive by the honours and prizes they gain at the London University; or many other flourishing colleges—should have their hands strengthened by Government help. In these institutions a thoroughly sound education, in all branches, is given. They have hitherto depended entirely on voluntary support, but the time has come when larger aid is needed to meet the modern requirements. Scholarships given by Government as incentives to work, and as helps to the many industrious students whose means are limited; stipends to professors, in order that they may obtain teaching assistance of a high character, of which they stand sorely in need, for it is absolutely impossible for them to teach effectively the large classes who place themselves under their guidance; grants for apparatus, and enlarged accommodation for the extension of original research—these are subjects which must occupy the attention of any committee appointed to inquire into the existing state of education. It is now pretty generally admitted by scientific men that no exclusively scientific education can meet our present requirements. On the Continent it is felt that it is only in universities and schools where *all* branches of knowledge are taught, that a really scientific education can be given; and we are glad to find that this opinion has gained ground in this country, together with a conviction that Science studies in their turn render students more apt in the acquisition of other branches of knowledge.

In Germany there is a strong feeling against the establishment of mere technical schools. It is maintained that boys should receive the same training up to a certain stage, and that they should afterwards enter for the special branch they design tofollow. Professor Köchly, of Heidelberg, Professor of Greek, proposes that there should be a thorough but limited instruction in classics, a more extended development of mathematics, a course of instruction in the natural sciences, and systematic instruction in modern languages. Professor Hofmann, who is well known in this country, considers that the best safeguard against the vulgarising of Science, when it is taught with too special a regard to its applications, is to be found in a sound general school training; and he believes that the old gymnasium system is of inestimable value. He asserts that in scores of instances he has seen youths who have come to the chemistry classes in the University of Berlin, with scarcely a knowledge of the meaning of the word chemistry, but who have been well trained in a gymnasium, in a short time completely surpass their fellows, who, in a school of another kind, have acquired considerable knowledge of the elements of chemistry. All the Polytechnic Schools of Germany are rapidly approaching the university type;—the teaching of the *principles* of Science, and not of the applications, is becoming more and more the main object.

LEONARDO DA VINCI AS A BOTANIST

FEW men have better earned the title of universal genius than Da Vinci. An ardent disciple of Nature, disdaining mere superficial knowledge, he went to the root of whatever he took up, and attained an intimate acquaintance especially with everything that bore on his beloved art of painting. And this art was understood by him in its widest sense. Not content with representing the mere outward appearance of Nature or of the human form, he considered it a part of his business as a painter to investigate the laws which produce those appearances or which govern that form in its healthy state. To the long list of his acquirements given in the catalogue of the Louvre collection, as painter, sculptor, architect, engineer, physicist, writer, and musician, may now be added that of botanist. In the first number of a new botanical journal, *Nuovo Giornale Botanico Italiano*, published at Florence, Sig. G. Uzielli has given some interesting extracts from a work by Da Vinci, from which he would appear to have anticipated the discovery of certain botanical laws generally attributed to writers of a later age. These extracts are taken from a section of his great treatise on painting, entitled "On Trees and Vegetation," which, however, is found only in one edition of that work, the Roman. The following are the points on which the originality of his observations deserves especial mention.

1. The laws of Phyllotaxis, or of the arrangement of leaves on the stem. Da Vinci appears to have been the first to observe that the order of growth of the leaves is uniform in the same species; and that their modes of arrangement can be divided into three principal forms—the opposite, the whorled or verticillate, and that usually denominated in text-books the alternate, but which should rather be called the spiral. He also pointed out that in the case of leaves growing in opposite pairs, they are generally arranged in a "decussate" manner, that is, each pair grows at right angles to the pairs immediately above and below it; that when leaves are verticillate, those in each whorl are seldom in a direct line with those in the whorls immediately above and beneath; and that a very common form of the spiral arrangement is that sometimes called "quincuncial," where the cycle is completed by five leaves, the sixth being in a direct line with the sixth above and beneath. Another observation of the great painter's is, that inasmuch as branches grow from buds generated in the axils of leaves, the arrangement of the branches on the trunk necessarily corresponds to that of the leaves on the stem.

In botanical works it is generally stated that Sir Thomas Browne, in his quaint little treatise "The Garden of Cyrus, or the Quincuncial Lozenge," published in 1658 (a work not mentioned in Pritzel's "Thesaurus Litteraturæ Botanicæ"), was the first to describe the spiral disposition of leaves, which was afterwards noticed contemporaneously by Grew and Malpighi. Bonnet,* however, in 1754 followed out the laws of phyllotaxis in a far more exact manner; and the subject has been still further elucidated by Goethe, Schimper, Braun, Steinheil, the brothers L. and E. Bravais, and Martins. To Da Vinci, however, who lived from 1452 to 1519, is clearly due the priority in the discovery of these laws; although, as might be expected, many of his observations show a crudeness and imperfection which have been corrected by more recent writers.

2. The manner in which, from the structure of the trunk of exogenous trees, their age can be determined. This fact, although now familiar to unscientific persons, appears to have been unknown to the ancients; since Theophrastus makes no mention of it, nor does Pliny, who

* Bonnet, Ch., Recherches sur l'usage des feuilles dans les plantes.

cites examples of trees which have been known for a great length of time. The discovery is usually attributed to Malpighi and Grew, who published their works, the former in 1675, the latter in 1682; it was, however, known earlier; for Montaigne, passing through Pisa in 1581, learnt the fact from a jeweller of that town, in terms which recall those used by Leonardo. I transcribe the description of Montaigne:—

"The workman, an ingenious man, and famous for the manufacture of beautiful mathematical instruments, informed me that every tree bears as many circles as the years it has lived, and he showed me this in all the specimens of wood which he had in his shop. And the part which is exposed to the north is firmer, and the rings closer and more dense than the rest. By this means he professes to be able to judge of any piece of wood that is brought to him, both the age of the tree, and in what situation it grew." *

The following are the words of Leonardo:—

"The southern part of the plant shows more vigour and youth than the northern. The rings of the branches of trees show how many years they have lived, and their greater or smaller size whether they were damper or drier. They also show the direction in which they were turned, because they are larger on the north side than the south; and for this reason the centre of the tree is nearer the bark on the south than on the north side."

From this it will be seen that both the observations on the age, and those on the eccentricity of the trunks of trees, attributed hitherto by De Candolle † and others to Malpighi, had been previously made by Leonardo da Vinci.

3. The growth of exogenous stems by the formation of new wood beneath the bark. This he describes in the following sentence:—

"The growth in the size of plants is produced by the sap, which is generated in the month of April between the outside coating (*camisia*) and the wood of the tree. At the same time this outside coating becomes converted into bark, and the bark acquires new crevices of the depth of the ordinary crevices."

It will be seen that, although the painter correctly indicated the portion of the trunk in which the increase takes place, he nevertheless failed to detect the cambium, and the important part which modern researches have shown that it plays in the formation of new wood.

For the above illustrations of the botanical knowledge of Da Vinci, we are mainly indebted to the article already named by Uzielli, who states that he might cite from the "Treatise on Painting" many other observations, generally correct, on the structure and development of plants, on the symmetry of their secondary axes, and on the influence which external agents have upon their growth. Uzielli remarks that it is strange that Venturi does not mention these botanical observations, he having had Leonardo's MSS. for a long time under his hand, not even referring to them in his "Essay on the physico-mathematical works of Leonardo da Vinci," where he claims for the painter the character of a great *savant*, and one of the founders of the experimental method. Amoretti, and all the other illustrators of his life and works, are also silent; and Libri, who wrote after the publication of the Roman edition of the work on Painting, mentions only that Leonardo records in it some botanical observations. Libri was, however, the first to publish the important experiments of Da Vinci relative to the action of poison on plants, discovered in the MSS.* preserved in the Library of the Institute at Paris, in which he also alludes to an ingenious process of drying plants, and reproducing their form easily on paper. Not only these MSS., but those also in the Ambrose Library at Milan, in the British Museum, and at Windsor, and those to be found in some private libraries, would doubtless repay a more careful research than has at present been bestowed upon them; and we would commend the subject to the attention of whoever takes up the thread of the life of Da Vinci, broken by the lamented death of Mr. B. B. Woodward.

Sir Charles Lyell † refers to Leonardo da Vinci as one of the first who applied sound reasoning to the facts of Geology, and who taught the organic origin of fossils. His botanical and geological theories are alike evidence of the spirit in which he applied all the powers of his mind to the observation of the phenomena that surrounded him, and which prompted him to counsel his pupils and readers invariably to have recourse to Nature rather than to the works of man, as their guide and the source of their inspiration.

ALFRED W. BENNETT

* Journal of Travels in Italy, by M. Montaigne.
† Organographie végétale, vol. 1. p. 324. Paris, 1827.

* MSS. of Leonardo da Vinci, vol. N, fos. 11 and 71.
† Principles of Geology, 10th ed. vol. 1. p. 31.

THE RACES OF INDIA

Memoirs on the History, Folk-lore, and Distribution of the Races of the N.W. Provinces of India. By the late Sir Henry M. Elliot. Edited by J. Beames. (2 vols. Trübner and Co.)

THE above work dates from the time of the old East India Company, bearing ample witness anew to that glorious fertility of genius produced in the full flow of an activity directed seemingly to the development of a purely mercantile policy of the most practical kind—the utilising of a distant continent for the enrichment of a handful of merchants sitting at home at their ease. Such, at least, was the repute enjoyed by the Honourable Court of Directors in their day, and it required no less a change than the transfer of power to as methodical a form of government as that which rules India nowadays to make us see matters in their true light, and bless the memory of John Company. This remark is made, of course, from a scientific point of view, for in every other respect, doubtless, India has at large been the gainer. The Company had served its term, and had to give way to a more central power in the interest of the empire generally. One cannot help contrasting, however, the times that are gone by, when upon the horizon shone such stars of first magnitude in science and literature as Sir Charles Wilkins, Sir William Jones, Gilchrist, Lumsden, Colebrooke, Wilson, Ballantyne, Charles Philip Brown, Roer, Sprenger, with the days that be, when examination tests of the severest kind are in the ascendant, but followed, alas! by no apparent results as far as growth of scientific knowledge is concerned, whatever advantage the service generally may be found to derive from them. Men there are, no doubt, who in the proper spirit, and with no less self-devotion, have continued the work of the past. Such names as Cowell, Nassau Lees, Buehler, Burnell, need only be mentioned to give us hope in the future. But what encouragement have their efforts met with? Unsupported as they are by any government aid, will not such efforts go sadly to waste? We do not mean to insinuate that the State should constitute itself a "Bureau de Surintendance," for the better direction and advancement of science and learning. It may be all very well in its way if the "Ministère de l'Instruction Publique" appoints a "Commission pour l'exploration scientifique de l'Algérie," but in a country of parliamentary government, where most things are left to individual initiative, such a state of things is supposed to be anomalous. A great deal might meanwhile be achieved if the range of knowledge required of an Indian civil servant were narrowed, and if he were plied more amply with knowledge of more immediate use for his future career. Instead of being obliged, as now, to occupy himself *de omnibus rebus et quibusdam aliis*, let his attention be directed to such knowledge as will more immediately concern him, and which, if properly followed up, could not but add greatly to our acquaintance with India. Practically speaking, indeed, such a course seems to be not only advisable, but absolutely necessary. That our authority throughout that region is diminishing according as our military power is less displayed, it would be useless to deny. The greater, consequently, seems the necessity for drawing closer the bonds of union, by employing ourselves more fully with the concerns of the people—not in the carping spirit too often assumed by missionaries, but with the unprejudiced mind of scholars. Occasion has been given to these remarks by the perusal of Sir Henry Elliot's book on Indian races, which, although cast in a form anything but grateful to the ordinary student, teems with most interesting information, not to be met with elsewhere in so condensed a form or backed up by such reliable authority. The work resulted from an order issued by the then Government to the Sudder Board of Revenue, N.W.P., bearing date 14th Dec., 1842, and directing them to compile a glossary of Indian terms in accordance with a comprehensive scheme which comprised not only terms relating to the revenue, but also to matters mythological, and to geographical nomenclature. The plan being but insufficiently carried out by his subordinates, Sir Henry of his own accord took it upon himself, in 1844, to complete the parts submitted to the Government, and reaching down to the letter J, without waiting for the completion of the whole—which, indeed, never seems to have been published —limiting his attention mainly to "tribes, customs, fiscal and agricultural terms." He not only added a great many new headings, but enriched the whole with contributions from his own vast store of historical knowledge, compiled from Mohammedan sources, principally from the "Ayin-i-Akbari," the work of the well-known Minister of the Emperor Akbar, who was the founder of a new era in Indian administration. So far as it goes, it is the nearest approach to an encyclopædia of modern Hindooism we can think of. But to contend that it is anything more than a most convenient book of reference in *practised* and *skilful* hands, would be going beyond the mark. In spite of the more practical arrangement adopted by the present editor, one must have struggled for some time with the difficulties which haphazard transcription of native words into English has put in one's way, in order to know where to find what is sought for, or to identify it if one has come across it by chance. This is the first attempt at a rational way of transcription; but, just because it is the first, it is not yet so consistently carried out as might be wished.

To the editor, for whom we have the greatest respect, and who, by his "Outlines of Indian Philology," has shown how earnestly he goes to work in such matters, we in no wise wish to be unfair. The short space of two months, however, allotted to him for editorial work, was far too short to allow him to think of such a fundamental change as that of digesting the whole of the additional matter, and making it conformable with the rest. To do this would involve immense labour, much more, at all events, than one is called upon to bestow when merely editing another man's work. This leads to another point of the utmost importance for our scientific knowledge of India, the bewildering confusion regarding geographical and other names in their English garb. We have been long in the habit of laughing at philologists pouring showers of abuse on each other on the question whether a certain letter in English transcription ought to have its dot over or under the line. But if we do not adopt a little of their pedantry, we shall see no end of confusion in scientific terminology. Since the days of Gilchrist, in 1802, when he made an appeal to European scholars to adopt a uniform system of transcription, no visible improvement has yet generally taken place. If we should not see the urgent necessity of such a change, he gave us the counterfeit of the Hindustani people spelling and pronouncing *udbikut* for advocate, *usishtun* for assistant, *kotmasool* for court-martial, etc. Nevertheless we go on spelling native names in all manners of ways. No two gazetteers, not even of India proper, agree in their orthography, and we may even say, not one gazetteer is consistent with itself. Look at the index to Allen's map of India, which, after all, is still one of the best. We often find there, under different letters of the alphabet, two places, at only a few minutes' distance from each other, which, if it had not been for the strange disguise in which different surveyors chose to put it from the way in which the name struck their ear, would never have been put down as two, but would at once have led to a more accurate measurement of longitude and latitude. The only way to rectify these errors is now afforded, by comparing those maps in Hindustani and Devanagari character, which are issued from the Surveyor-General's offices at Agra and Allahabad, with our English maps, and rectifying the latter in a systematical manner. We are no better off if we turn to botany and the pharmacopœia. Mr. Watson's index, which was lately compiled with a view of collecting the material, swarms with the most glaring mistakes, which, it is true, will do no harm to the learned in these matters, but it is just for them that such books are *not* published. They are intended for those who, in our busy days, have no leisure to settle all this detail for themselves.

To turn back to our book. Of the hundreds of geographical names contained in it, there are perhaps not ten which one would find in this form on our maps of India. But after more or less experimenting, you would not only find every one of them on the maps, but also in some volume of the latest edition of "Thornton's Gazetteer," and perhaps in the volume in which you least expect to find it. To make the book useful for the general public, therefore, a careful index of all the possible spellings, and reference to the correct one, ought to have been annexed to it. Another English index arranged according to subject matter, such as for instance the one to "Rich's Dictionary of Roman and Greek Antiquities," is still a great desideratum, even after the new distribution in four chapters by Mr. Beames. To give only two or three instances out of many: How is the ordinary reader to know that Bareilly (II., 143) is the same as Barelî (141), and that the latter is the correct form? or that the Jadubans (I., 3) are the same as the Yadbansî's (350), and that Kayat, Kayath, Kayeth, Kaisth and Kaith, as they are spelt in different parts of the book, are the same, namely, Káyastha, and that the name is not composed, as stated, on I., 305, from *kai* and *stittei*? And how, without an alphabetical table of contents, are you to know that contributions to Persian and Slang lexicography are hidden away in pages 178 of the second, and 160 of the first volume?

All this does not, however, detract from the value of the work, which we consider, with the author, as "a basis and starting point" very well worth imitating for all the civilians who go out to India. If every secretary of a Sudder Board of Revenue in India were presented with a copy of this work, and if an injunction were made that either he himself or one of his assistants who is well qualified for the task should from time to time send in reports of what he sees and hears after the pattern of the present book, we might, without outlay to the Government, soon see the book completed to the letter Z, and the same thing done for other presidencies too. But the case occurring, we must beg one thing, that the right man be put in the right place, and that we are to have no more of that gentleman's reports who tries to pass off the names of five great districts and of five great languages for so many "great families." (I. 342). We fancy we see him in our mind's eye sitting down to a task utterly ungenial to him, and after a strenuous effort to huddle it through, only heaping blunder upon blunder. Such discoveries shake our faith in the reliability of his other statements in cases where we have no means to test them by facts established elsewhere, and resting on sufficient authority by themselves.

OUR BOOK SHELF

A Catalogue of British Neuroptera. Compiled by Robert McLachlan, F.L.S. The Ephemeridæ by the Rev. A. E. Eaton, B.A. Published by the Entomological Society of London. 8vo. (London: Longmans. 1870.)

ENTOMOLOGISTS will give a cordial welcome to this first instalment of the catalogue of British insects, the preparation and publication of which has been taken up with such commendable zeal by our Entomological Society. The subjects coming under the domain of entomology are so infinitely numerous, and the literature of the science has increased so enormously of late years, that for any one man to attempt to grapple with it specifically would be almost an act of insanity; and the authorities of the Entomological Society have therefore very wisely entrusted the preparation of different parts of their projected catalogue to those British entomologists who have most successfully studied particular groups. Mr. McLachlan as a zealous student of the Neuroptera is so well known both in this country and on the Continent, that no one else could well have been selected for this part of the task, and he has associated with himself, in the preparation of the list of Ephemeridæ (the well-known May-flies of the angler) a gentleman who, if his published writings are less numerous than those of his colleague, has certainly shown in them that he possesses in a high degree the qualities necessary for the investigation of a rather difficult group of insects.

The order Neuroptera, as understood in this catalogue, possesses the same signification that was originally given to it by Linnæus—that is to say, it embraces, besides the true Neuroptera with a complete metamorphosis, those forms, such as the dragon-flies, May-flies, and some others, which, from their imperfect transformation and certain structural characters, have of late years frequently been placed with the Orthoptera, under the name of "Pseudo-Neuroptera." Under this subordinal or tribual name they figure in this catalogue, and in the present state of our knowledge of the classification of these forms of insects, this is perhaps as good a place for them as any. We have still much to learn as to the affinities of these creatures before any satisfactory arrangement of the families and higher groups can be made, and long and persevering labours, probably in the genealogical direction indicated by Darwinian views, will be necessary before we can clearly understand their true relations, which, however, are the more interesting, as it is undoubtedly in this neighbourhood that we have to seek for the primitive type or types of the whole world of insects. Towards such a happy consummation as the final settlement of so knotty a question as the true classification of the insects comprised under the orders Orthoptera and Neuroptera, such conscientious work as has been put into this catalogue by its authors must greatly contribute.

There is one other point on which we may congratulate the Entomological Society, namely, their adoption of an order of Entomological pariahs, if we may so speak, for the *début* of their catalogue. In Entomology, perhaps more than in any other department of Natural History, fashion rules the day, and the great majority of its votaries devote their whole attention either to Lepidoptera or to Beetles. The fact that one of the most neglected groups of insects has been taken for the commencement of this catalogue of British insects, is, we hope, a sign that the order of publication will continue to be in the inverse ratio of the popularity of the subjects, as we feel convinced that there are many who with any tolerable guidance would be only too glad to acquire some knowledge of the forms of insect-life which lie outside the limits of their present studies.

Ost Afrika: Erinnerungen und Miscellen aus dem abyssinischen Feldzüge. Von Dr. J. Bechtinger. (Wien. 1870.)

DR. BECHTINGER furnishes an account in a light sketchy style of his experience in the Abyssinian campaign as an acting assistant surgeon. The contents of this work are of a very miscellaneous nature, and are not particularly well arranged, comprising scraps of information respecting the diseases of the troops, the treatment adopted for the *Filaria medinensis*, the Yemen ulcer, the character and habits of the Abyssinians, and the incidents of the journey. The descriptions of the scenery are few and short, and there are scarcely any observations of scientific value. We scarcely know whether the book is intended for the general or the professional reader. For the former it contains too much medicine and surgery; for the latter it is almost worthless, and we think the author need not have been so particular in reserving the right of translation and reproduction.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Strange Noises heard at Sea off Grey Town

I AM glad to see that the vexed question of the noise heard from under the sea in various parts of the Atlantic and Pacific has been re-opened by a gentleman so accurate and so little disposed to credulity as Mr. Dennehy. The fact that this noise has been heard at Grey Town only on board the iron steamers, not on board the wooden ones, is striking. Doubtless if any musical vibration was communicated to the water from below, such vibration would be passed on more freely to an iron ship than to a wooden one. But I can bring instances of a noise which seems identical with that heard at Grey Town being heard not only on board wooden ships, but from the shore.

I myself heard it from the shore, in the island of Monos, in the Northern Bocas of Trinidad. I heard it first about midnight, and then again in the morning about sunrise. In both cases the sea was calm. It was not to be explained by wind, surf, or caves. The different descriptions of the Grey Town noise which Mr. Dennehy gives, will each and all of them suit it tolerably. I likened it to a locomotive in the distance rattling as it blows off its steam. The natives told me that the noise was made by a fish, and a specimen of the fish was given me, which is not *Centriscus scolopax*, the snipe-fish, but the trumpet-fish, or *Fistularia*. I no more believe that it can make the noise than Mr. Dennehy believes (and he is quite right) that the *Centriscus* can make it.

This noise is said to be frequently heard at the Bocas, and at Point à Pierre, some twenty-five miles south; also outside the Gulf along the Spanish main as far as Barcelona. It was heard at Chagreasancas (just inside the Bocas) by M. Joseph, author of a clever little account of Trinidad, on board a schooner which was, of course, a wooden one, at anchor. "Immediately under the vessel," he says, "I heard a deep and not unpleasing sound, similar to those one might imagine to proceed from a thousand Æolian harps; this ceased, and deep and varying notes succeeded; these gradually swelled into an uninterrupted stream of singular sounds, like the booming of a number of Chinese gongs under water; to these sounds succeeded notes that had a faint resemblance to a wild chorus of a hundred human voices singing out of time in deep bass."

He had, he says, three specimens of the trumpet-fish, said to make the noise, either by "fastening the trumpet to the bottom of a vessel or a rock," or without adhering to any object. The whip-like appendage to the tail, which he describes, marks his specimens at once as *Fistularias*.

Meanwhile, it is but fair to say that Mr. W. W. Spicer, a few weeks since, called attention to this "Sirene," or musical fish, in Hardwicke's *Science Gossip*, commenting on an account of its being heard commonly in the Bay of Pailon, Esmeralda, on the Pacific shore, in latitude 4° north. I replied shortly in the same excellent magazine, and offered to write further, a promise which I should have redeemed, had not I understood that my learned friend Dr. Günther, in the meanwhile, was about to write on the matter himself, telling far more than I could have told.

Another instance of this sound being heard on board a wooden ship (and this time again in the Pacific) is given (in p. 304 of Mr. Griffith and Colonel Hamilton Smith's edition of Cuvier's Fishes, on no less an authority than that of Humboldt who (say the editors and authors of the Appendix) did not suspect the cause. "On the 20th of February, 1803, toward seven in the evening, the whole crew were astounded by an extraordinary noise, which resembled that of drums beating in the air. It was at first attributed to the breakers. Speedily it was heard in the vessel, and especially toward the poop. It was like a boiling, the noise of the air which escapes from fluid in a state of ebullition. They then began to fear that there was some leak in the vessel. It was heard unceasingly in all parts of the vessel, and finally, about nine o'clock, it ceased altogether. From the narration (says Cuvier) which we have extracted, and from what so many observers have reported touching various Sciænoids, we may believe that it was a troop of some of these species which occasioned the noise in question."

For there is, without doubt, a great deal of evidence to show that certain Sciænoids make some noise of this kind. The *Umbrinas*, or "maigres" of the Mediterranean and Atlantic are said to be audible at a depth of twenty fathoms, and to guide the fishermen to their whereabouts by their drumming. The fishermen of Rochelle are said to give the noise a peculiar term, "seiller," to hiss; and say that the males alone make it in spawning time; and that it is possible, by imitating it, to take them without bait. The "weak-fish" of New York, (*Labrus squetaquee* of Dr. Mitchell) is said to make a drumming noise. But the best known "drum-fishes" are of the genus *Pogonias*, distinguished from *Umbrina* by numerous barbules under the lower jaw, instead of a single one at the symphysis. M. Cuvier names them *Pogonias fusca*, and mentions that "it emits a sound still more remarkable than that of the other Sciænoids, and has been compared to the noise of several drums." The author of the Appendix states that these "drum-fish" swim in troops in the shallow bays of Long Island; and according to Schœpf (who calls them *Labrus chromis*) assemble round the keels of ships at anchor, and then their noise is most sensible and continuous. Dr. Mitchell, however, only speaks of their drumming when taken out of the water. Species of the same genus, if not identical, are found as far south as the coast of Brazil; and it is to them, probably, that that noise is to be attributed which made the old Spanish discoverers report that at certain seasons the nymphs and Tritons assembled in the Gulf of Paria, and made the "Golfo Triste glad with nightly music."

How this noise is produced, if the theory be true, I cannot say. Early naturalists looked, naturally, towards the large and strong swimming bladders, observing, at the same time, that these have no communication with the intestinal canal, nor with the exterior generally.

It only remains to me now to quote the opinion of Dr. Günther, to whose courtesy I owe the sight both of the fish and of its pharyngeal and vomerine teeth. He thinks, with later naturalists, that the noise might be made simply by large shoals of "drums" grinding these teeth together, whether in masticating the crabs, &c., on which they feed, or for mere sport.

I would, therefore, request Mr. Dennehy, or any officers of the Royal Mail steamers who may visit Grey Town, to try if they cannot catch a *Pogonias* or two. Of course, finding them there will not prove that they make the noise, but it will be at least one fresh link in a long chain of evidence.

And so I leave the matter, apologising for having quoted from no later authority than the Cuvier of 1834, which is the only book accessible to me; and for myself, "holding it for rashness hastily to avouch or deny aught amid such fertility of Nature's wonders."

C. KINGSLEY

[Mr. Kingsley will find references to all the various authors who have written on Sciænoids generally, and "Drum Fishes" especially, in Dr. Günther's "Catalogue of Fishes," vol. ii., p. 270 *et seq.*]—ED.

WITH reference to the communication published in last week's NATURE, on "Strange Noises heard at Sea off Grey Town," it does not appear necessary to refer these noises to any occult galvanic agency, or magnetic influence in connection with iron ships, although at first sight, and more especially as there is much ferruginous sand in the vicinity, and as the sounds are heard only in iron ships, and not in wood-built, copper-bottomed vessels, there seems ground for such an idea. The solution I would venture to offer is that these noises proceed from "musical fish" or shells.

Musical sounds proceeding from under water, agreeing in character with those described by Mr. Dennehy, appear to be known on the western coast of India and on the coast of Chili. A very interesting account of these musical sounds will be found in Sir Emerson Tennent's work on Ceylon, from the author's own experiences at Batticaloa in that island. His impressions as to the gentleness and harmony of the sounds are as vividly described as those of your correspondent from the Royal Mail Ship *Shannon*: and although Sir E. Tennent throws no light on the remarkable periodicity of the phenomenon, yet he gleaned by his inquiries that the sounds were heard at night, and most distinctly when the moon was nearest the full. Your readers will find the details at p. 468 *et seq.*, 2nd vol., Edition of 1859.

The iron ship is, in all probability, from the thinness of the plates, a far better musical sounding-board than the thick-bottomed wooden ships, and here we may have the reason of the delicate sounds not being heard in the latter class of vessel.

It has always appeared to me that this particular locality of the River San Juan and its embouchure at Grey Town, offers a rare field for research to the naturalist and speculative geologist.

In my early service—1834—I was engaged in the Admiralty nautical survey of the then harbour at the entrance of the River San Juan, subsequently well known as Grey Town. At that time it afforded a secure and fairly spacious anchorage for a few vessels of even 24ft. water (sheltered by a sandy peninsula) with a wide and clear approach.

Between 1834 and 1839 the end of this sandy peninsula, Arenas Point, advanced considerably across the entrance towards the opposite shore, in a depth of five-and-a-half fathoms. In 1859, the point had reached to within a cable's length of the main land, or over 6000ft. in advance of its position twenty-five years previously, in depths varying from 33ft. to 18ft., practically closing the port except to a small description of vessel.

These great geological changes—if they may be so called —in so short a period of time, destroying as they have done a useful port, are an interesting, and, so far as I know, an unique fact; but the point to which I would wish to refer in connection with the musical fish (?) is the vast amount of animal life observed at the time of the original survey alluded to. The port literally swarmed with fish, but we could not venture to haul the seine more than twice, from the circumstance that large alligators came up in it, to the consternation of the fishermen and the destruction of the nets. Sharks of huge size rendered precaution from falling overboard a matter of some moment, as an unfortunate pet monkey discovered by being instantly seized. This abundance of life, proof of good feeding-ground, may have some connection with a well-developed species of musical fish; but this speculation must be left for your naturalist readers. F. J. EVANS

PERHAPS your correspondent, Charles Dennehy, M.R.C.S.I., R.M.S. *Shannon*, may find an interpretation of the nocturnal musical phenomenon (mentioned in NATURE, No. 28) experienced by iron ships when at anchor in seven or eight fathoms, with a bottom consisting of *a heavy, dark sand and mud containing much vegetable matter*, in the following natural system of gas-escape. In examining certain pools of water in the East, notorious for their poisonous qualities at certain seasons of the year, I was aware of intermittent risings of vast quantities of bubbles. The waters rested on vegetable deposits; if these were stirred up, large globules rose with considerable force, and I came to the conclusion that these air risings were due to the escape of gases from the decomposing vegetable matter. If any metallic body had met these bubbles as they rose, some sound would have been produced, the nature of it depending on various causes. The reason of the sound being heard on board ship between twelve and two, and not between two and four, is owing to a very simple, but beautiful rule of law: as the gases are at all times collecting, we might suppose that they would be at all times escaping, but as the surface of the bottom is of an elastic nature, the water pressure imprisons the gas as if it were within a valve; but when the force of the gas overpowers the water pressure, there is a bubbling escape till the collected gases are expended, and thus I account for the sounds continuing "*about two hours, with but one or two very short intervals*." It is by no means improbable that the musical performance occurs more than once in the twenty-four hours, though the ordinary noises of shipboard prevent its being audible. I believe there is no other way of accounting for this incident; but the test I would propose is to stir up the bottom on a calm day with considerable force; if large quantities of air-bubbles arise, the sailors may rest satisfied that the concert is not given by ghost, mermaid, or siren, but simply by a continued contact of myriads of gas globules against the ship's bottom. The stirring up will not necessarily cause the sound, as the bubbles may be diverted by undercurrents. H. P. MALET

The Sources of the Nile

IN the fifth (May) number of the *Geographische Mittheilungen*, I publish an article and two maps on "Livingstone's Travels and Discoveries from 1840 to May 1869," one of the maps being carefully compiled from the original Portuguese publications of the Portuguese journeys since 1798—viz., those of Dr. Fr. José de Lacerda e Almeida, the Pombeiros Joao Baptista and Pedro, Major Monteiro, and others, showing all that is at present known of those regions. Both maps and text keep aloof from theories and speculations as to the connections of rivers and lakes discovered by Livingstone and the Portuguese with the Nile.

Of the two points at issue, the one as to the connection of the Lake Tanganyika with Albert Nyanza, Livingstone says:—"Tanganyika and Nyige Chowambe (Baker's?) are one water;" but gives no proof of it, having evidently derived his information from hearsay. The most reliable information on this point seems to me that supplied by Burton (Journal R. G. S., vol. 29, p. 254):—"At the head (northern end) of the Tanganyika lies the land of Uzige," in which land, "according to the guides, six rivers fall into the Tanganyika in due order from the east—the Kuryamavenge, the Molongwe, the Kavinvira, the Kariba, the Kibaiba, and westernmost, the Rusizi or Lusizi. The latter is the main drain of the northern country, and the best authorities, that is to say those nearest the spot, unanimously assert that it is an influent."

Regarding the Kassabi, the upper course of which was explored by Livingstone, Ladislaus Magyar, and Rodriguez Graça, it appears to me that the most reliable information we possess of its lower course is that supplied by Livingstone, as collected by him when at Cabango in 1855 (Livingstone's Missionary Travels in South Africa, pp. 457 and 458):—"Several of the native traders here having visited the country of Luba, lying far to the north of this, and there being some visitors also from the town of Mai, which is situated far down the Kasai, I picked up some information respecting those distant parts. In going to the town of Mai, the traders crossed only two large rivers, the Laojima and Ohihombo. The Kasai flows a little to the east of the town of Mai, and near it there is a large waterfall. They describe the Kasai as being there of very great size, and that it thence bends round to the west. On asking an old man, who was about to return to Mai, to imagine himself standing at his home, and point to the confluence of the Guango and Kasai, he immediately turned, and pointing to the westward, said, 'When we travel five days (thirty-five or forty miles) in that direction, we come to it.' He stated also that the Kasai received another river, named the Lubilash. There is but one opinion among the Bovonda respecting the Kasai and Guango. They invariably describe the Kasai as receiving the Guango, and beyond the confluence assuming the name of Zairé or Zerézeré. And the Kasai, even previous to the junction, is much larger than the Guango, from the numerous branches it receives. Besides those we have already crossed, there is the Chihombo, at Cabango, and forty-two miles beyond this, eastward, runs the Kasai itself; fourteen miles beyond that the Kaunguesi; then, forty-two miles further east flows the Lolua; besides numbers of little streams, all of which contribute to swell the Kasai. The town of Mai is pointed out as to the N.N.W. of Cabango, and thirty-two days or 224 miles distant, or about lat. S. 5° 45'. It is evident, from all the information I could collect both here and elsewhere, that the drainage of Londa falls to the north and then runs westward. The countries of Luba and Mai are evidently lower than this, and yet this is of no great altitude, probably not much more than 3,500 feet above the level of the sea. Having here received pretty certain information on a point in which I felt much interest, namely, that the Kasai is not navigable from the coast, owing to the large waterfall near the town of Mai * * *" AUGUSTUS PETERMANN

Redaction der Mittheilungen aus
Justus Perthes geographischer Anstalt,
Gotha, May 2

Scandinavian Skulls

IN his recent lecture on the "Forefathers of the English People," Professor Huxley says, "It is a very remarkable circumstance that the skulls of the existing Scandinavians . . . are *long*;" and he contrasts their dolicephalous type with the round forms of South German, Swiss, and ancient Belgic heads. He also thinks it likely that the Scandinavian invasions of England brought a "longer form of head" into fashion amongst us. The same doctrine is taught by Sir Charles Lyell, in his "Antiquity of Man," and even in the sixth edition of his "Elements of Geology," he says that the Scandinavian skulls of the dolmen period are brachycephalous, or round; those of the iron age being dolicephalous, or long.

Such notions were once current in Sweden and Denmark, but they are now exploded. Originally deductions from history, they rest on no basis of observed fact, and archæology plainly contradicts them. Thirty or forty years ago, Scandinavian *savants* believed, on historical and philosophical grounds, that Lapps and Finns were the earliest inhabitants of the Baltic North, that after them came a Celtic, and finally a Gothic invasion. As Scandinavian archæology grew into a science, and the remains discovered were seen to fall into a stone, a bronze, and an iron series, the three groups collected were, in obedience to the previously existing historic theory, respectively labelled Finnish, Celtic, and Scandinavian. Now, it happened that the first *two* obviously prehistoric skulls found in Scandinavia were of the round type, on which circumstance a learned person (Retzius, if I remember rightly), jumped—in characteristic nineteenth-century style—to the conclusion that the *whole dolmen race* was round-skulled. Never was a more monstrous generalisation built on a miserable collection of two particulars. There are known to exist in museums about 80 skulls attributed to dolmen men, some of which are, perhaps, of questionable origin. These crania are of every conceivable type, being, in fact, identical with modern churchyard skulls. No one pretends that their form is short, or Celtic, or Finnic; and some authorities allege that they are mostly long. These facts are notorious enough in the North. They were ventilated at the late Copenhagen Archæological Congress, and it was not denied on any side that the stone age skulls would suit modern Danes and Swedes. The short skulled stone man is in fact gone the way of the basilisk and the Vital Force, and it is time for him to take his departure from the authoritative scientific teaching of Great Britain.

Professor Huxley appears to believe that the Northern Bronze, or Iron Man, was long skulled. Although this view is quite unsupported by facts, it may be backed by an argument from the domain of the Higher Criticism. In the Museum of Rosenberg are casts of the so-called Gold Horns, the originals of which precious runic articles were stolen nearly 70 years ago. One of these horns bears the inscription—"Ek hleva gastim holtingam horna Favido," which every runologist can read easily enough. But no two runologists agree even approximately in their versions, so that no date can be given to the horns from inferences built on the style of the inscription. However, Professor Steenstrup has pointed out that some figures of men engraved on the horns have heads of a longish appearance, which conclusive fact tells in favour of Professor Huxley's dolicephalous doctrine, although some learned Danes consider that the skulls represented on the horns were obviously Oriental and not Scandinavian.

According to the highest Copenhagen authorities, there is no ground whatever for the assertion that modern Scandinavian skulls are of the long type. It is equally incorrect to say that Scandinavians are fair-haired and blue-eyed

Copenhagen G. STRACHEY

The Anglo-Saxon Conquest

IN an interesting paper, quoted at p. 661 of NATURE, Prof. Rolleston dwells upon the proportion of short-lived male skeletons, found in Anglo-Saxon interments, as contrasted with the older character of the Romano-British interments, deducing therefrom a conclusion as to their respective longevity. The writer appears to have forgotten that the youth of Romano-Britain had for many generations been forcibly expatriated—drafted abroad to feed the armies of Imperial Rome. A. HALL

Analogy of Colour and Music

MR. W. S. OKELY accuses me of having criticised his letter "far too hastily," and writes that he does *not* compare the *diameters* of Newton's rings with one another, but their *cubes*. On referring to his letter in NATURE for Feb. 10, I read as follows:—"Professor Zannotti, of Naples, gives for the *diameters* of the rings from red to red, the cube-roots of the numbers 1, $\frac{8}{9}$, $\frac{5}{6}$, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{3}{5}$, $\frac{9}{16}$, $\frac{1}{2}$. The intervals between these, taken successively, are $\frac{9}{8}$, $\frac{16}{15}$, $\frac{10}{9}$, $\frac{9}{8}$, $\frac{10}{9}$, $\frac{16}{15}$, $\frac{9}{8}$." Your readers can now judge whether my failure to apprehend Mr. Okely's measures was due to my undue haste or his obscurity of expression. When Mr. Okely speaks of my "doubting the accuracy" of Professor Zannotti and M. Biot, he is drawing entirely on his own imagination; what I *did* doubt was the value of the deductions drawn by Mr. Okely from their figures. I *now* doubt his power of distinguishing between external facts and those evolved from his own moral consciousness.

Trin. Coll. Cambridge, May 4 SEDLEY TAYLOR

Colour of the Sky

YOUR correspondent "H. A. N." will find some interesting remarks on the blueness of the sky in Professor Tyndall's "Glaciers of the Alps" (p. 257, &c.), and one or two additional notes in my "Alpine Regions," p. 150. With regard to the colour of the sky at great heights, I can inform him that in fine weather the blue becomes deeper as one ascends, as has been noticed by many persons accustomed to mountain climbing. The most striking instance that I have seen was during an ascent of Monte Rosa, 15,217 feet. On this occasion the colour was so deep as almost to approach a black, as deep as or deeper than the richest hues of *Gentiana acaulis*. This intensity of colour was only very conspicuous during the last few hundred feet of the ascent; and in expeditions to mountains of nearly the same height I have not often seen it approached, never surpassed. Mr. Hincliff in his "Summer Months among the Alps," p. 111, calls attention to the same phenomenon on Monte Rosa, and very appositely quotes Shelley:

> The sun's unclouded orb
> Rolled through the black concave.

T. G. BONNEY

The Royal Society

I CANNOT but think that the list of candidates recommended by the Council for election into the Royal Society published in your last number will be read by the outside world with considerable surprise. I look in vain in it for the names of two men, at least, of world-wide reputation, and well known as no mere *dilettanti* in their respective sciences, who were among the candidates, while the names of others are found there, which are on everybody's lips with the thought, What have they done to merit the scientific distinction which is looked on by every lover of science as almost an opening of the gates of paradise? Is it possible for us outsiders to learn anything of the considerations which govern the election? NOT AN F.R.S.

The Origin of Species and of Languages

ALTHOUGH the origin of languages is due, doubtless, to the gradual variation, selection, and combination of a few primary sounds, partly emotional, partly imitative; yet the process differs essentially from the Darwinian in one all-important respect—that it is carried on by the countless efforts of *rational* beings. No irrational animal, though capable of uttering emotional sounds that are quite intelligible to its fellows, and though in some instances capable of imitating both natural and articulate sounds in a remarkable degree, has ever formed a language, simply because it wants reason. Therefore the analogy, in so far as it really holds, seems to tell against the Darwinian theory, in as far as that ascribes the origin of species to *reasonless* variation and selection.

To me this seems a most important consideration. But I cannot trespass further on your space.

Stirling WILLIAM TAYLOR

TAUNTON COLLEGE SCHOOL

THE educational scheme which occupied much of the late Lord Taunton's attention during the last years of his life, but of which he only saw the beginning, has now come into practical working. Under ordinary circumstances the development of an ancient Grammar School into a modern Public School would merely pass as one of the now frequent symptoms of advance in English higher education. Thus the removal of Bishop Fox's foundation (A.D. 1522) to a fine range of buildings outside Taunton, would hardly demand notice here. Our readers, however, whose attention was taken by Mr. Tuckwell's paper on Science Teaching in Schools (NATURE, No. 1), will see that the application of his system on a much enlarged scale is likely to affect considerably the position of science in the West of England. While calling public attention to the admirable educational arrangements of this particular school, we wish to remark on science teaching in schools in general, with regard to two points which we observe to be often misconceived by the very teachers and parents whom they especially concern.

First, as to the amount of other work displaced by the introduction of Physical Science as one of the regular parts of the school course. In the mediæval system, still not extinct, the forty-two working hours per week were given almost exclusively to classics. In any really good school these hours have now to hold at least an equal average of classical learning, beside English and French, sometimes German, as well as mathematics, geography, history, &c. The problem of doing much in the same time as used to be spent in doing little, is, to a great extent, solved by mere improvement of method. The old classical teaching was so clumsy and repulsive, that its results, so far as it suits the new system to strive for them, may be obtained in one quarter to one half of the time formerly allowed. In addition to this, there are some products of the old system which the new must almost perforce abandon. Latin verses demanded a minimum of ten hours per week out of a total of forty-two working hours. Now, it cannot be too clearly impressed on the minds of persons interested in education, that the time required for giving a well-grounded acquaintance with elementary science is four hours per week.

Second, it is often thought by parents that science, while valuable to boys about to pass certain examinations, or to enter certain professions, is merely of the nature of an "extra accomplishment," not affecting the rest of the educational course. Nothing can be less true. It would be nearer the fact to say that the especial importance of science-teaching in schools, is in its serving beyond any other known means to open children's minds, to stimulate their reasoning powers ; not to teach dull formulas, learnt by a *memoria technica*, but to start boys and girls on a course of realising and comprehending life and nature. This statement (as is right with a statement concerning physical science) is one to be tested by direct experiment. Take a class of children brought up to learn Latin and Greek, Geography and History, and Mathematics, but on whose minds the idea has scarcely dawned that these matters concern real places and people and things. Unfortunately, nothing is easier than to find such classes, grinding on, year after year, in the fond belief that, because school work is dull and toilsome, it must be profitable. Now, let an intelligent teacher give these unlucky children an elementary science lesson : for instance, how it is that bodies fall, what causes summer and winter, how the thermometer does its work. In half an hour's time it will be seen in the very faces of the children, that the lesson, independently of its value for itself, has actually repaid the time spent on it, in the newly-aroused attention and reflection it has gained for other studies. It is no exaggeration to say that four hours of really live teaching in science fully pays for itself in the improved quality of the rest of the week. For this cause it is that science is not made optional at the Taunton School. It is taught simply, and with inexpensive apparatus; but every boy is required to collect his own specimens, to perform his own experiments, and to show at every step that he knows what he is doing.

It seems to us that in some few of the really enlightened public schools, such as that we are now writing of, nearly the highest ideal of training has been attained to, compatible with the present habits of English life. When boys, fresh from an intelligent governess at home, or from a ladies' preparatory school of the best sort, go through the full school course from 11 to 17, working steadily on without flagging and without strain or hurry, at an education which they understand to be the direct and purposeful preparation for active business or professional life, such boys start with success in their hands. It is the result of such education that the professional "crammer" tries to simulate when he endeavours to make 12 months' over-work, ruinous to body and mind, produce at the examiners' table the semblance of seven years of steady mental growth. The examiner knows better, and the real business of after-life shows before many years are out the difference between cram and real education.

THE SCIENCE OF EXPLOSIVES AS APPLIED TO WARLIKE PURPOSES

II.—RECENT IMPROVEMENTS IN THE MANIPULATION AND FIRING OF EXPLOSIVE CHARGES

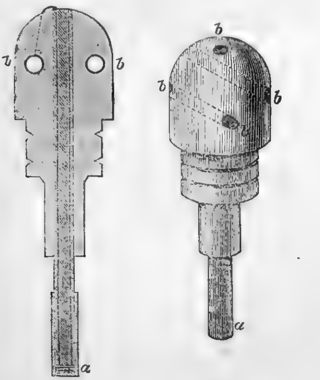
AS we have already shown, the employment of explosive charges is a branch of warfare to which the attention of military engineers has been directed for some time past. For warlike purposes, as also for the destruction of wrecks or other submarine obstructions, explosions have been frequently applied in earlier times, but the methods resorted to for igniting them were, as may be supposed, exceedingly crude and primitive ; sometimes a clock-work arrangement was used, sometimes an encased slow match, and, occasionally, explosion was brought about by means of heated shot dropped down a metal tube. As would naturally be inferred, the difficulties and the frequently unsatisfactory results attendant on explosions of this kind rendered their profitable application a matter of considerable doubt, and few successful records of their employment are to be found.

During the past few years our knowledge of the science of explosives has been considerably enlarged. Not only has the subject of igniting charges been studied to such an extent as to form, at the present time, almost a science of itself, but the nature of the combustible material has likewise been changed and improved ; and military and naval engineers have thus been placed in possession of a source of power, of which we hardly know whether to admire more its unlimitable force, or its wonderful plasticity.

The first practical application of electricity to igniting gunpowder, although such a proceeding was considered possible both by Franklin and Priestley, was not made until about thirty years ago, when some French military engineers employed a voltaic battery as a means of explosion. The method used by these officers was the simple and well-known one of connecting the two conducting wires by a thin platinum thread, the resistance offered to the passage of the electric current by that metal causing its temperature to be raised to a degree sufficient to ignite any charge of gunpowder in contact with it. This manner of applying the electric current as a source of heat is both simple and practical, but it frequently lacks, besides other qualities, the essential virtues of certainty and instantaneity, and it was for this reason that further investigations of the subject have been from time to time carried on. Among others, Colonel Verdu, a Spanish officer, made some progress in the matter, and was successful by the aid of a Ruhmkorff induction coil in exploding several charges simultaneously. This officer's first attempt was to fire the gunpowder by simply allowing a powerful spark to pass from one pole to the other of two wires imbedded in the charge ; he found, however, that ignition in this manner was by no means to be relied upon, but that by covering the poles with fulminate of mercury, a substance more readily inflamed, the desired result was readily secured. Some successful results were also obtained about the same time by employing a particular electric fuze, known as Statham's.

A few years later, in 1856, Sir Charles Wheatstone and Mr. Abel devoted considerable time to the prosecution of further researches, and each of these gentlemen contributed an important discovery, which had the effect of perfecting this interesting application, and rendering it a practical and valuable aid to military science. The employment of electricity induced by magnetism was suggested by Sir Charles, who, after some preliminary experiments, constructed an exploding instrument in which the electricity was created by the rapidly revolving armatures of a compound magnet; and the successful application of this machine was effected through the agency of an electric fuze, devised by Mr. Abel, and of which a sketch is here given.

The chief condition to be fulfilled in the construction of an electric fuze was the preparation of a compound which should combine high conducting power with great susceptibility to ignition, and this, after patient and renewed investigation, was ultimately accomplished. A mixture of subphosphide of copper and subsulphide of copper with chlorate of potash afforded a composition which was exploded with perfect ease and certainty by a current from a small magneto-electric machine, a larger apparatus of the same kind being capable of igniting twenty or thirty of these fuzes almost instantaneously. The means at command afforded, under certain circumstances, by a Wheatstone Exploder and the Abel fuzes, can scarcely be valued too highly, and it has been stated that the Governor of Malta, if provided with these valuable aids, might by himself conduct the defence of Valetta Harbour from his own drawing-room window. This may indeed be a vainglorious boast, but as a matter of hard fact we may mention that the Time Guns at Newcastle, Edinburgh, and other northern towns, have been ignited for some years past from Greenwich Observatory, through many hundred miles of wire, by an Abel fuze with infallible precision and certainty.



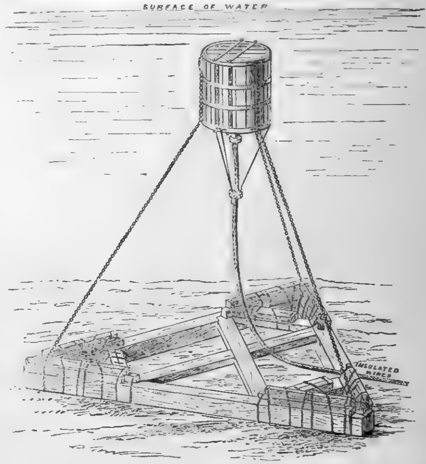
Section *Perspective view*

ABEL FUZE

a, cap of composition, into which the two poles of the wires are fixed.
bb, metal eyes for connecting the insulated wires with the fuze.

Since the date of these improvements, much further experience has been gained in regard to the most suitable description of apparatus to serve as exploders, frictional and dynamo-electric machines, as also miniature voltaic batteries taking the place of the Wheatstone instrument; the electric fuze has however stood its ground in spite of many rivals, and still continues to be used for the ignition of explosive charges, including torpedoes and submarine mines.

As mentioned in the first part of this paper, the Americans employed electricity for firing some of their torpedoes, but they are by no means the only nation who has done so. In the Austro-French war, when the capture of Venice was feared by the Austrians, a very ingenious system of electric torpedo defence was organised at that seaport by a distinguished officer of Engineers, Colonel von Ebner. A camera obscura was erected in proximity to the harbour in such a manner that the horizontal table of the instrument reflected the whole area of the channel. Large wooden cases, each containing 400 pounds of gun-cotton, were lowered at certain fixed distances into the water, and as these disappeared one by one, a small row-boat described at the time a circle round the spot to indicate the extreme confines of the distance at which the torpedo would prove effective; an observer was stationed in the camera as these operations were going on, carefully watching their reflection in the instrument, and as each torpedo disappeared into the water, he marked with a pencil its precise locality on the white table, tracing also the ring formed by the row-boat. Thus a series of circles was formed in the camera, each of which was marked with a distinctive number, and in this way a miniature, but exceedingly correct, plan of the obstructions in the harbours was prepared; the wires in connection with the torpedoes were afterwards led up into the camera obscura and furnished with numbers to correspond with the circles. By means of this arrangement a sentinel stationed in the apparatus might at once explode any one of the torpedoes, as soon as he observed the reflection of an enemy's ship pass within the limits of the circles marked upon the table. The channel itself was quite clear of any suspicious buoys and beacons, and appeared to the enemy wholly free from obstruction.



GUN-COTTON ELECTRIC TORPEDOES CONSTRUCTED AT VENICE IN 1859

In our own country the question of torpedo warfare has for some years past been the subject of study and investigation, and a system has lately been elaborated which owes its origin mainly to Mr. Abel, the scientific referee of the War Department, and which is at once so simple and practical that it cannot fail, in the future, to form a new and interesting branch of war science.

Mechanical torpedoes present so many serious defects (as for instance their liability to get out of order, the risk incurred in mooring them, and the danger they involve, when once sunk, to friend as to foe), that all idea of their employment was at once abandoned by our authorities, and the investigations confined to the devising of efficient electric torpedoes. Of these many descriptions have been designed, but the two kinds held most in favour are the self-exploding instruments, and those which are capable of sending a signal when touched by a passing vessel to indicate the proper time of effecting their ignition from the shore.

The self-acting electric torpedo is of very simple construction, the following being a general outline of one form of it. An Abel fuze is fixed in the torpedo, one pole of which is connected to a constant battery on shore by means of an insulated wire, while the other pole is in communication with an insulated metal plate fixed inside a pivot in the upper part of the machine. Upon this pivot swings a moveable hood, or cage, and the latter, though not affected by the motion of waves, will, upon being struck by a passing vessel, swerve round and come into metallic contact with the insulated plate above mentioned, thus completing the electric circuit with the earth, or, more strictly speaking, with the water. As will be readily perceived therefore, in this case, a single wire only is needed to connect one element of the battery with the fuze, the other element being of course allowed to pass to earth. In the other description of torpedo, a *circuit closer* of the same construction is used, and this on being struck furnishes a signal to the shore, whence a sentinel at once explodes any charge, or charges, which may be in the vicinity of the submerged machine. When disconnected from the batteries, these torpedoes naturally cease to be a source of danger, and herein lies one of the most valuable qualities of the electric exploding method. If considered desirable, the machines need in fact never be put into an active state except in a case of imminent danger. Thus, if a fleet of friendly vessels were pursued by hostile ships, the sentinel on the look-out would not connect his batteries until the former had passed over the torpedoes, and when the machines were well left behind, by simply turning a switch arrangement he would be enabled instantly to close the line of defence, and set up a formidable barrier not to be passed with impunity.

In the simplest form of electric torpedoes (such as the majority of those used in America) where ignition is brought about by simply sending a current through the circuit, one wire leading from the torpedo to the battery and another to earth, the employment of the Abel fuze presents one very important advantage. Explosive machines fitted with these appliances may, when in position, be tested at any moment to ascertain their state of efficiency, and the operator is thus made cognisant of the serviceableness or otherwise of his apparatus and batteries; this operation is effected by simply passing a weak current through the wire and fuze, which although insufficient to produce ignition, is yet powerful enough for the transmission of signals.

Where a large number of torpedoes are grouped together, it is found undesirable, except in special cases, to use either the frictional or dynamo-electric machines for exploding the fuzes, for the reason that a current sent from one of these instruments to ignite a specific charge, induces similar currents in adjacent wires and at once causes a wholesale explosion. Constant voltaic batteries or piles are therefore generally resorted to, and the construction of simple forms of these from rough, handy materials (some sheet zinc and copper, a few pieces of wood, and a little vinegar and common salt) is a favourite occupation among sailors who have received elementary instruction in this system of warfare.

By employing in torpedoes, instead of powder, a heavy charge of gun-cotton, and exploding this by the newly-discovered method of detonation, a force is developed which, it is no exaggeration to say, would prove fatal against a vessel of the strongest and most cunning construction.

NOTES

WE are glad to be able to announce that the arrangements for the Eclipse Expedition are progressing very rapidly and satisfactorily, and that there seems every chance of everything being done which can insure success. In response to their circular, the Council of the Royal Astronomical Society have received upwards of sixty applications from observers anxious to help in an examination of the phenomenon. It is proposed that, if possible, there shall be two expeditions; one to Spain, the other to Sicily. The desirability of this is obvious, as the chances of bad weather are thereby considerably reduced. Unfortunately, those who know Sicily well state that the region to be visited is so brigand-ridden that other precautions besides those usually employed in Eclipse Expeditions will be desirable. The Italian Government, which will also, we believe, send an expedition to Sicily, will, doubtless, look to this. The French Expedition will observe in Algeria.

OUR Berlin Correspondent writes that Baron Liebeg has recovered from his recent severe illness.

WE regret to learn that Mr. Archibald Geikie, who recently left England to investigate the Geology of the Lipari Islands, was prostrated by fever as soon as he arrived there, and is in such a weak state of health, that he has been ordered back to England.

AN Imperial decree has been published in Paris, ordering that the Minister of Fine Arts shall henceforth bear the title of Minister of Literature, Science, and Art, and also that his department shall include the superintendence of the Institut de France, Academie des Sciences, the libraries, learned societies, and the like. When shall we get *our* Ministry of Literature, Science, and Art?

THIS will be a week of Anniversary Meetings. On Monday the annual réunion of the Royal Geographical Society will be held at one o'clock, and of the Victoria Institute at four; and on Tuesday the Linnean Society will celebrate its anniversary at three, and the Ethnological at four.

THE *British Medical Journal* states that the chair of Physiology, in the University of Prague, vacant by the death of the celebrated Purkinje, has been filled by the appointment of Dr. Hering, of Vienna. It was offered to Professor Helmholtz, who, however, preferred to remain at Heidelberg.

AT the annual meeting of the Newcastle Natural History Society on the 10th inst., a discussion took place on the present position of the Alder Memorial Fund. It was stated that while the original intention was to raise 600*l.* to carry out the memorial scheme, only about 300*l.* had been collected since March 1867. After some discussion, it was agreed to make efforts to raise an additional 100*l.*, which was considered a sufficient sum to carry out the objects proposed.

AT the recent general examination for women, held by the University of London, five passed in the "Honours" Division and four in the First Division. Of the seventeen candidates, five were from the Cheltenham Ladies' College, all of whom were successful, two being placed in the Honours and three in the First Division.

MR. J. W. ELWES, of King's College and the London University, and Mr. W. T. Sollas, of the Royal School of Mines, have been elected (equal) Exhibitioners in Natural Science, at St. John's College, Cambridge. There were eight candidates; the examiners being Prof. C. C. Babington (Botany), Prof. Humphry (Physiology), Prof. W. G. Adams (Physics), Mr. Bonney (Geology), and Mr. Main (Chemistry).

A RECENT number (94) of the German series known as "A Collection of Popular Scientific Treatises, edited by R. Virchow and Fr. von Holtzendorff," is a lecture on the Glacial Period (*Die Eiszeit der Erde*), by Alexander Braun. It gives a clear and concise history of the observations and arguments by which geologists have been led to the conclusion that a lengthened period of extreme cold overspread the greater part of Europe before the commencement of the historical epoch.

MR. C. P. SMITH reprints, as a separate publication, an epitome of a paper read before the Brighton and Sussex

Natural History Society on Nov. 11, 1869, under the title of "The Moss Flora of Sussex, together with Notes on the Structure and Reproduction of Mosses."

THE first volume is published of Dr. Oppolzer's "Lehrbuch zur Bahnbestimmung der Kometen und Planeten."

A PAMPHLET lies on our table entitled "History of Modern Anæsthetics, a second letter to Dr. Jacob Bigelow, by Sir J. Y. Simpson, Bart." Without entering into the merits of the controversy between the Scotch and American doctors, it is but just to the memory of Sir James Simpson to say that it appears to have been conducted by him in an admirable spirit of courtesy which is not always found in scientific discussions. It is admitted on both sides that the first case of an anæsthetic operation under sulphuric ether occurred at Boston on the 30th of September, 1846; and the first case of an anæsthetic operation under chloroform occurred at Edinburgh on the 15th of November, 1847. The last sentence of Sir James's letter to Dr. Bigelow, written when the grave was almost closing upon him, is full of touching pathos:—"With many of our profession in America I have the honour of being personally acquainted, and regard their friendship so very highly, that I shall not regret this attempt—my last, perhaps—at professional writing, as altogether useless on my part, if it tend to fix my name and memory duly in their love and esteem."

IN the *North American Review* for April appeared an article entitled "Darwinism in Germany," from the pen of Mr. Charles L. Brace, giving a *résume* of the present state of biological speculation on the Continent.

MR. WILLIAM HUGHES, Professor of Geography in King's College, London, reprints "Geography in its relation to History," a lecture delivered at the Birkbeck Institution; and "Geography, what it is, and how to teach it," a paper read before the College of Preceptors.

THE *Food Journal* for May commences a somewhat minute description of Mr. Twining's Museum of Domestic and Sanitary Economy at Twickenham, one of the most interesting and really valuable collections ever brought together by private enterprise.

DURING the present year, the following medals will be awarded for the encouragement of photographic discovery:—A large silver medal, by the French Photographic Society, for the best transparent pellicle that can be devised for the transfer of *cliches;* a large gold medal, by the Vienna Photographic Society, for the best dry process; and two silver and two bronze medals for other deserving inventions. The Hamburg Society also promises medals for important discoveries.

AT the sitting of the Paris Academy of Science for May 2, the President announced the death of Professor Lamé, a member of the Institute since 1843. The deceased, a very celebrated physicist and mathematician, was born in 1795, educated at the Ecole Polytechnique, and was for some time engineer in the Russian service. On his return to France, he was appointed Professor of Physics at the above-named school, and remained in that capacity until the year 1845, when he was elected Examiner at the school. In the year 1848 he was appointed Professor in the Faculty of Sciences at Paris. Among his very many published works those on mathematics and the elasticity of bodies are the most celebrated.

ACCORDING to the *British Medical Journal*, the weight of the late Sir James Simpson's brain, including the cerebellum, was 54 ounces. While, as is well known, the ratio between intellect and size of brain is by no means close, yet there can be no doubt that it is very important. Most of our great men have had large crania. The male brain ranges chiefly between 46 and 53 ounces, its average being $49\frac{1}{2}$ (Quain and Sharpey). That of Cuvier is stated to have weighed 64 ounces, and that of the late Dr. Abercrombie 63 ounces, but it is possible that some error may have crept in through the use of weights of different standards. If not, Sir James's brain, whilst much above the average, did not nearly reach those of the celebrated men we have mentioned; but at the same time, the convolutions were remarkably numerous; they were, says a correspondent, "twisting and twining round on each other as if they could not find room within the head. The island of Reil was very wonderful."

THE frontispiece to the *Photographic Art Journal* for May is the first published example of Mr. Woodbury's new patent process of photo-mechanical printing in printing-ink. It was printed in a copper-plate press from a plate produced at the establishment of MM. Goupil, at Paris. It is entitled "Orpheline," and is a copy of a drawing by Girardet, an eminent modern French artist. The other illustrations in the same journal are a photograph by Messrs. Edwards and Kidd's photo-mechanical or surface-printing process, of a drawing made at Chartres last October by Mr. A. E. Browne; and a copy by the photo-engraving process of M. H. Garnier, of Paris, of an old lithograph by the celebrated French painter Géricault.

AN interesting application of photography to legal evidence has just taken place. The Spanish Government having refused to give up the *Tornado*, an English vessel captured some time since, or to give compensation to the owners, our own Government has acquiesced in the decision, a photographic copy of the private instructions given to the captain by the owners having proved conclusively the more than doubtful character of the vessel.

THE *American Entomologist* for April appears under the new title of the *American Entomologist and Botanist*. Mr. Charles V. Riley continues the editorship of the entomological department, while the botanical section is undertaken by Mr. George Vasey, of Richview, Illinois, who has long been known in the West as a careful botanist. The paper is published at the enterprising south-western capital, St. Louis, Missouri, a town which also supports the *Grape Culturist*, a monthly journal devoted exclusively to grape culture and wine making, and the *St. Louis Journal of Agriculture*, published weekly. An epitome of its contents will be found under the head of "Scientific Serials."

AT a recent meeting of the Paris Chemical Society, M. Scheurer-Kestner read a paper on the composition of fossil and recent bones. He finds that bones which have been buried for long periods contain, besides ossein, which is insoluble in water, another organic nitrogenous substance, soluble in water, and into which he supposes ossein to be slowly changed. Running water gradually removes this soluble modified ossein, and consequently the ancient bones found in loose impervious soils contain very little organic matter, while those buried in compact clay may retain a large quantity of it. The rate of decomposition thus varies with the nature of the soil; but in the same soil M. Scheurer-Kestner believes that the relative age of different bones can to a considerable extent be determined by their chemical composition.

THE *Scientific American* states that Mr. Sherwood has invented an ingenious method for the separation of animal fibre from vegetable. The process does not alter the colour or structure of the animal fibre, and permits the use of cotton or linen separated from it for numerous purposes. It is sufficient to suspend the goods in an atmosphere of nitrogen or carbonic acid, and to cause the vapour of perfectly dry sulphuric, phosphoric, or hydrochloric acid to enter the room. These fumes disentangle the vegetable fibre, and leave intact the animal—the two fibres can thus be separated and appropriated to their respective uses.

ACCORDING to the *Chamber of Agricultural Commerce*, Belgium sent us during the year 1869 3,000 tons of meat, poultry, and rabbits; and the birds, at any rate, we might as well have fed and hatched at home. Belgium exported in the same year 34,375 tons of raw beet-root sugar. What prevents us making sugar for ourselves in the United Kingdom? Certainly not climate, for the Hon. Agar Ellis, M.P., has grown sugar-beets this year in several parts of Kilkenny county; and Dr. Völcker's analysis of the roots finds a proportion of 8·94 to 10·91 per cent. of crystallisable sugar, while a proportion of only 8·5 per cent. is said to be sufficient to remunerate the sugar manufacturer. The sugar-beets grown near Lavenham, Suffolk, in 1868 contained, according to the same analyst, from 9·62 to 12·84 per cent. of crystallisable sugar.

A PARLIAMENTARY return has just been issued showing to what extent the Act for establishing libraries and museums has been adopted. At Manchester the Public Free Library was opened on the 6th of September, 1852; the public subscription amounted to 12,823*l.*, of which 813*l.* 18*s.* was contributed by artisans and workpeople, numbering upwards of 20,000 persons employed in the various industrial establishments in the city and neighbourhood. On the question of maintaining the library, the number of votes recorded in favour of the adoption of the Act was 3,962; against its adoption, 40. The news-rooms during the evening hours are constantly crowded; from the published tables it appears that the total number of readers had increased from 39,944 in 1853 to 135,877 in 1868. Between 1863 and 1864 the numbers fell from 91,121 to 58,589, showing to how great an extent the libraries are used by the classes affected by the cotton famine. The number of borrowers, though subject to a slight fluctuation at the same period, has shown a comparatively steady increase from 2,000 in 1853 to 27,749 in 1868. The daily average of visitors in 1868 was 5,575, or upwards of 1,700,000 during the year. The total number of volumes in the library in June 1869, was 40,498 in the reference library, and 49,791 in the lending department.

THE Prussian Government appears to have definitely decided on introducing the Vautherin (iron) sleepers in the place of wood on the State railways. A contract has been recently entered into with the Saarbrück works, for the supply of 30,450 sleepers, intended for the railways from Trèves to Saarbrück, from Saarbrück to Neunkirchen, and from Neunkirchen to Bingerbrück. By this means it is thought also that the inconvenience will be avoided which has been experienced in France from the excessive destruction of the forests, and the great excess of the consumption of timber over the native production.

THE gradual destruction of fish in the rivers of Germany by the unrestricted fisheries has begun to attract serious attention, and the evil is beginning to be felt also in the coast fisheries. M. Schmarda, who has been commissioned by the Austrian Government to inspect and report on the French fisheries, estimates the quantity of fish caught on the coast between Cherbourg and Toulon at upwards of 58,000,000 kilogrammes in the year.

IT is stated that the Roman Catholics of San Francisco are building an "earthquake-proof" church. The side walls above the basement are only 30 feet high; at this height a roof rises, which, with the main roof, is supported independently of the walls, by two rows of pillars inside of them. Both roofs are firmly bound to the pillars, and the pillars are fastened together by iron cross-beams, secured with heavy iron bolts, forming a network of great strength. The theory of the plan of construction is, that should the pillars be shaken down, the roof would be launched outside the walls, thus giving a chance of escape from the ruins. In thus falling, the roof would be carried aside a distance of 80 feet, the length of the pillars.

IT can scarcely be said that any amount of water is too great for the supply of public and private wants, but as the expenses increase rapidly with the volume supplied, a limit is in practice soon reached and cannot readily be exceeded. The average amount of water consumed per diem for each person is estimated at about three-and-a-half pints, below which proportion physical suffering commences. The quantity required for washing purposes is estimated at about one gallon. A considerable quantity is also required for the consumption of animals, for watering the streets, gardens, &c., the extinction of fires, and other purposes. The following table shows the quantity supplied in different cities to each inhabitant per diem, expressed in litres (=1·7 pints).

Rome........	944	litres	derived from	springs
New York...	568	,,	,,	springs and rivers
Marseilles ..	470	,,	,,	ditto
Besancon ...	246	,,	,,	ditto
Dijon.........	240	,,	,,	Rosoir springs
Bordeaux ...	170	,,	,,	springs and rivers
Metz	125	,,	,,	ditto
London	95	,,	,,	rivers
Lyons	85	,,	,,	springs and rivers
Brussels	80	,,	,,	ditto
Geneva	74	,,	,,	rivers
Grenoble.....	65	,,	,,	springs
Paris..........	60	,,	,,	springs and rivers
Montpellier.	60	,,	,,	springs
Havre.........	42	,,	,,	ditto
Liverpool...	28	,,	,,	ditto

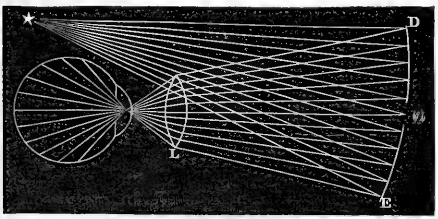
THE *Field* quotes the following from the *Toronto Globe*, illustrative of the much-disputed fact that the queen-bee can be fertilised within the hive. The observer is Mr. Malone, of Garden Island:—"I first made some small nuclei hives, and inserted three frames in each (with brood and comb in them), and placed a queen cell in each in such a manner that by turning a button I could see the cell. As soon as the queen was hatched I caught her and placed her in a cage 6in. square by 8in. long, two sides of the cage being wood, the rest wire, and placed a good number of worker bees in with her, and put the cage on the top of the frames in a hive containing a good swarm of bees, having first removed their queen. When the queen was five days old, that is, on the fifth day, I took out all the worker bees from the cage, and placed seven nice large drones in with the queen. I left the queen and drones together forty-eight hours in the cage, having placed them back again on top of the frames, and replaced the cover and plugged up the ventilators which are in the sides of the cover, to keep out the light. Of course I put some honey in the cage, out of the reach of the bees below in the hive, to keep the queen and drones from starving. Each time on examination, I found (with one exception) a dead drone, having all the end of his abdomen burst open, and twice I noticed evidences of impregnation. To make myself doubly sure that they were fertilised by this method, I introduced the queens into new swarms, and closed the opening so that nothing but a worker bee could go in and out, and all the queens (with one exception, as mentioned above) in a few days commenced laying, and reared nicely-marked Italian workers."

THE REV. E. O'MEARA, A.M., is preparing a catalogue of Irish Diatomaceæ. This catalogue will appear in the form of a report to the Royal Irish Academy, and will, we presume, be published in its Transactions. A synonymy of all the species will be given, and figures of all the new species, or of species the figures of which are not easily accessible. This catalogue, which will be the result of many years' labour, will, we believe, form a worthy supplement to "Smith's British Diatomaceæ."

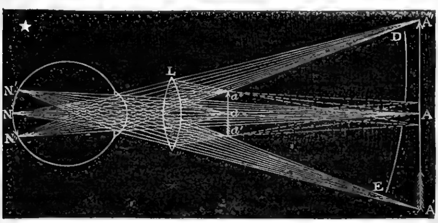
COMPLAINTS have appeared in some of the Dublin papers that the library of the Royal Dublin Society was closed for a week during the Dublin Cattle Show. It is also rumoured that a complaint of the same kind was forwarded to the authorities at Kensington. When it is recollected that this is, perhaps, the only equally large library in Europe that is open to the public from 10 A.M. to 10 P.M. throughout the year, it will be seen that the public have little real cause of complaint in its being closed for eight or ten days in the twelve months.

A NEW FORM OF OPHTHALMOSCOPE

THE principal steps that have been made during the last twenty years in the knowledge of the healthy and of the diseased conditions of the eye have been effected by the employment of the ophthalmoscope, an instrument so simple, and yet so valuable, that, like other discoveries, it is only remarkable that the knowledge of the facts on which its construction depends should have so long remained unfruitful. Under all ordinary circumstances, when we look into the pupil of the eye of another person, however widely dilated it may be, it appears of an intense black hue, because the degree of illumination is insufficient to render parts so deeply seated visible, the principal portion of the light being intercepted by the head of the observer. An exceptional instance, however, is sufficiently familiar to every one, in which a brilliant reflection may be observed to occur from the back of the eye. It is that of an animal crouching in the corner of a cellar, whilst the observer is standing at the door, or looking towards a window, to which the back of the observer is turned. The principle on which the ophthalmoscope is founded is identical with this, the eye under observation being illuminated by a pencil of light proceeding, as it were, from the eye of the observer. This is accomplished by placing a steady source of light at the side of or above and somewhat behind the head of the person under observation, whilst the observer reflects its rays into the eye of the subject by means of a plane or concave mirror, the centre of which is perforated by a small opening through which he looks. The back, or fundus of the globe, then comes into view, presenting a red, or greyish red glare, the illumination being greatly increased by the use of a lens at L, as shown in the accompanying little



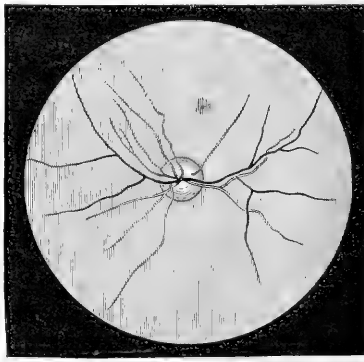
woodcut, from the recent work of Dr. Williams of Boston, where the rays of light emanating from the star are reflected from the concave mirror DE, and rendered convergent by the lens L, lighting up the whole of the posterior surface of the globe; some of the rays returning from this pass through the opening in the mirror, and are seen by the observer at O. The precise mode in which the image is formed is shown in the following cut, borrowed



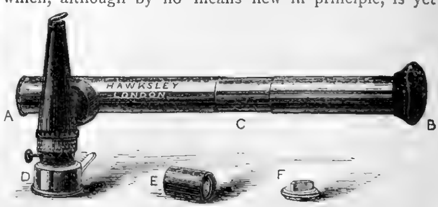
from the same work. The rays returning from N, N′ N″ representing a portion of illuminated fundus, are brought to a focus by the convex lens L, at A, A′ A″, and then form, the inverted aërial image of the fundus, which is seen by the observer.

The image which comes into view under these circumstances, especially if, as is usual, the pupil be dilated by the employment of a little belladonna or solution of atropine, is represented in the following woodcut, which we have carefully drawn from a child of twelve years of age. The reader must imagine the general surface to be of an orange vermilion, or scarlet vermilion tint, though in the negro it is of a very dark vermilion; the colour being produced by the reflection of the light from the capillary blood-vessels of the choroid.

In the centre is a yellowish white spot, which is the optic disc, or point of entrance of the optic nerve. This is perforated by the branches of the central artery and veins of the retina, the lighter double lines representing the arteries and the darker the veins. Above and a little to the right is a spot which is the true centre, and at the same time the most sensitive part of the eye; from its colour it is sometimes called the macula lutea, or from its being



slightly depressed below the surface it is termed the fovea centralis. The changes which the optic disc, the blood-vessels, and the retina undergo in disease can of course be readily followed, and may thus enable a positive opinion to be pronounced on cases which were formerly incapable of being distinguished even by the most acute observer. Nay, it has recently been suggested by M. Poncet to employ it as one of the most reliable means of ascertaining that *death* has really taken place. A great variety of forms of the instrument have been suggested, but the ordinary hand ophthalmoscope has proved the most convenient in practice, requiring only that the room should be darkened, and that there should be some steady source of light. Dr. Beale, however, has lately suggested a form of self-illuminating ophthalmoscope, which, although by no means new in principle, is yet



convenient in application, doing away with the necessity of a dark room, and furnishing a very steady and good light. It is represented in the preceding figure, and consists of a tube, A B C, of which the extremity B is closely applied to the eye to be examined, thus shutting out all extraneous light. D is a paraffin lamp which can be easily connected or removed, the rays falling on a perforated mirror, and being reflected towards B, a convex lens being interposed near C, which can be approximated to or made to recede from the eye by the movement of the draw-tube B.

We have tried the instrument in a number of cases, and have found that while it enables a very good view of the fundus to be obtained with facility, it has the drawbacks of smelling disagreeably and also of becoming rather unpleasantly hot, from the proximity of the lamp to the eye of the observer—an inconvenience that might be remedied by placing the lateral tube at a greater distance from the end A.

H. POWER

SCIENTIFIC SERIALS

THE PROGRESS OF CHEMISTRY (*Jahresbericht über die Fortschritte der Chemie und verwandter Theile anderer Wissenschaften.*) Unter Mitwirkung von Th. Engelbach, Al. Naumann, W. Städel; herausgegeben von Adolph Strecker. Für 1868. Erstes Heft. Ausgegeben am 15 Februar, 1870. Giessen J. Ricker'sche Buchhandlung. 1870.

Of the 480 pages of this part, 133 are occupied by general and physical, and 155 by inorganic chemistry, the remainder being devoted to organic chemistry.

In the first section we find an account of the method devised by Hofmann for determining the vapour densities of solids and liquids in a Torricellian vacuum, and which promises such valuable results in many cases. Tomlinson's observations on the properties of chemically clean surfaces are here noticed; and also Guthrie's experiments on the conduction of heat by liquids. A long abstract of Becquerel's papers on electro-capillary action opens the section on electro-chemical investigations, and in the division on optical chemistry Tyndall's researches on the chemical action of light find a place. Two of the channels into which much chemical thought has been directed during the last few years are clearly indicated by the notices of numerous papers on dissociation and spectrum analysis.

In the section on inorganic chemistry we have some additional experiments in support of Frankland's theory of the cause of the light emitted by luminous flames; the combustion of hydrogen and carbonic oxide in oxygen under a pressure of ten atmospheres, and the burning of heated phosphorus vapour in hot chlorine being mentioned. Graham's researches on the occlusion of hydrogen by metals, and on the curious combination which this element forms with palladium are noticed at considerable length. The next paper to which we shall direct attention is one which at present stands amongst those on inorganic chemistry, but the compounds herein described bid fair shortly to occupy a section by themselves in a position intermediate between mineral and organic bodies; we refer to the researches of Friedel and Ladenburg on silicic oxychloride, and silicium iodoform; the study of compounds having the same constitution as well-known organic bodies, but in which silicon plays the part of carbon, promises to enable us, ultimately, to explain the constitution of that very extensive class of complex bodies containing silicon, which are found so abundantly in nature. We have a long notice of Meyer's investigations of indium and its compounds, and one on Mills's researches on the ammoniacal cobalt compounds.

Among the papers on organic chemistry, we have that of Troost and Hauteufeille, on the decomposition of mercuric cyanide at different temperatures and pressures; and the very important one by Berthelot on the synthesis of hydrocyanic acid, which he obtained by the passage of electric sparks through a mixture of acetylene and free nitrogen. A considerable space is devoted to the researches of Jungfleisch on the chlorine derivatives of benzol, and to Oppenheim's investigations of the isomeric compounds of allyl and propylene. Berthelot's valuable researches on the hydrocarbons are continued, his observations on styrol and its compounds being given. The foundation of one of the most interesting and valuable discoveries of the last few years is contained in this volume, viz., the conversion of alizarin into anthracene by its distillation from powdered zinc. Since this paper was published, its authors, Messrs. Graebe and Liebermann, have succeeded (as our readers well know) in effecting the inverse transformation, and thus producing the colouring principle of madder from one of the products of the destructive distillation of coal. We also find here the discovery of the first acetylene of the aromatic series, by Glaser in acetenylbenzol or phenylacetylene. Linnemann's preparation of normal propylalcohol and the synthesis of butylalcohol by Lieben, are noticed, as are the researches of Stenhouse on tetra- and tri-chlorochinon.

All students of chemistry will heartily welcome the appearance of the Jahresbericht, though the usefulness of the first part is much impaired for want of the index. It is very much to be regretted that this very valuable book does not appear at an earlier date; in this case the first part of the report has been published thirteen months and a half after the expiration of the year to which it refers. It must be allowed that the labour required for such a work is very arduous, but it would be a great convenience to chemists if it were possible to expedite its publication. It is reported that the Council of the Chemical Society of London intends to issue fortnightly, or monthly, short abstracts of all papers on chemistry and the allied sciences published in England and abroad. These abstracts will be made by competent chemists, and will be printed as soon as possible after the appearance of the original memoirs. Such a collection of abstracts would be invaluable to many who, in consequence of their living away from London, or owing to other circumstances, have no opportunity of reading the periodicals: it would also be extremely useful to many who, though within reach of journals, have not the time to devote to the perusal of the very numerous papers published at the present time. All chemists, and many others taking a general interest in science, must wish every success to the efforts of the Council of the Chemical Society, though it is to be feared that such a journal would scarcely, at present at least, prove a commercial success.

THE *Revue des Cours Scientifiques* for May 7 contains M. Blanchard's lecture before the *Réunion des Sociétés Savantes*, at the Sorbonne, on Scientific Work in the Departments; the continuation of Bernard's lecture on Suffocation by Charcoal fumes; and the conclusion of M. Bouley's on Madness. In the number for May 14 we find M. Kuhne's lecture on the Science of Life, delivered on the occasion of the inauguration of the Physiological Laboratory at the University of Amsterdam; a paper by M. E. Fournier on the Ergot of Rye, being the first of a series on the parasites of cereals; and a continuation of M. Bernard's paper.

THE *American Entomologist and Botanist* for April contains several good articles. A paper entitled "Wheat-rust and barberry rust" (placed singularly in the entomological department), defends the accuracy of the statement well known to European botanists, but which appears to have been attacked in America, that the neighbourhood of barberry trees is a prolific cause of rust in wheat; the fungus which causes the latter disease, *Puccinia graminis*, and the fungus which produces the bright yellow spots on the leaves of the barberry, *Œcidium berberidis*, being, in fact, different conditions of the same plant. An article entitled "Scientific Language," justly rebukes the tendency to use long latinised words where plain English words would do just as well, and especially the coining of barbarous compound terms, derived from two or three different languages. There are also several good descriptive papers, both entomological and botanical, specially interesting to American naturalists and collectors.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, May 2.—Mr. A. R. Wallace, president, in the chair. Mr. Hewitson exhibited a collection of new and rare butterflies, from Tropical America.—Mr. Frederick Smith exhibited a collection of *Hymenoptera*, from Japan.—Mr. McLachlan exhibited some exotic dragon-flies.—Mr. Bates exhibited several new exotic *Copridæ*.—Mr. G. R. Crotch sent for exhibition British specimens of *Trachyphlœus laticollis*, a beetle not previously recorded as indigenous to this country.—Papers were read, on Equatorial *Lepidoptera*, by Mr. Hewitson; on some new *Neuroptera Odonata*, by Mr. McLachlan; on new *Copridæ*, by Mr. Bates; and on Australian *Curculionidæ*, by

Mr. Pascoe.—"A Catalogue of British Neuroptera," compiled for the society by Mr. McLachlan, and published by the society, was on the table.

Ethnological Society, May 10.—Special meeting held at the Museum of Practical Geology, by permission of Sir R. I. Murchison, Bart. Prof. Huxley, LL.D., F.R.S., in the chair. Dr. O'Callaghan was announced as a new member.—Col. Lane Fox read a letter from Lieut. Oliver, R.A., relative to the destruction of the fine menhir of Le Quesnel, in Jersey, described in the last number of the Society's Journal.—Sir J. Lubbock, Bart., and Mr. J. S. Mackie made remarks on the importance of the labours of the society in obtaining reports on the present condition of our megalithic monuments.—Prof. Huxley then delivered an address on the Ethnology of Britain. He showed that the early accounts of the inhabitants of these islands, such as that given by Tacitus, prove the existence of two types of people physically distinct—the one being tall, fair, yellow-haired, and blue-eyed, whilst the other was short and dark, with dark hair and black eyes. This dark type, as exemplified in the ancient Silures, closely resembled the people of Aquitania and Iberia, whilst the fair type of south-east Britain was physically related to the Belgæ of north-east France and what is now Belgium; and these again resembled the old Germani, who dwelt on the east bank of the Rhine. In this country both the fair and the dark people spoke Celtic—probably Cymric in Britain and Gaelic in Ireland. But on the Continent the dark type spoke a Euskarian or Basque tongue, while the ancient Gauls spoke Celtic and the Germani Teutonic. The Celtic and Teutonic languages both belong to the Aryan family, but the Euskarian appears to have no affinity to any other Eur-asiatic language. None of the invasions to which Britain has been subjected has introduced any new race-element. It is doubtful whether the Romans strengthened the fair or the dark type of the pre-existing population, but it is certain that the invasion of the Low Dutch from the shores of North Germany bordering on the Baltic and the North Sea strengthened the fair element, as also did the incursions of the Danes. The effect of the Norman conquest would be shown by Dr. Nicholas.—The Rev. Dr. Nicholas then read a paper "On the Influence of the Norman Conquest on the Ethnology of Britain." He first inquired what race-elements were present in Britain prior to the Conquest, and concluded that the blood preponderated considerably in favour of the ancient British race—a race which he did not hold to be purely Celtic. He then sought to determine what were the elements in William the Bastard's so-called "Norman" army, and showed that they were mainly Gaelic and Cymric. Hence the conclusion that the effect of this conquest was in the gross greatly gainful to the old British or Gallo-Celtic population.

MANCHESTER

Literary and Philosophical Society, March 22.—The following extract of a letter, dated March 21, 1870, from Sir William Thomson, D.C.L., F.R.S., hon. member of the Society, was read:—

"I have now at last got into good working order measurements of electrostatic capacity (which, perhaps, you may remember I was working on the first time you ever came to see me, and more or less almost ever since). I have two students of last year, junior assistants in my laboratory, measuring electrostatic capacities of condensers, and variations of specific inductive capacities of resistance, with sensibility of $\frac{1}{10}$ per cent., and with constancy in spite of accidental variations, generally within $\frac{1}{4}$ or $\frac{1}{3}$ per cent. My occupation on the Kinetic theory of gases has led me at last to come to definite terms as to the size of molecules. Ever since about the first year of my professorship I have taught my students that Cauchy's theory of Dispersion proves heterogeneousness, or molecular structure, to become sensible in contiguous portions of glass or water, of dimensions moderately small in comparison with the wave-lengths of ordinary light. I have spoken to you also, I think, of the argument deducible from the contact electricity of metals. This, I now find, proves a limit to the dimensions of the molecules in metals quite corresponding to that established for transparent solids and liquids by the dynamics of dispersion. In experiments made about ten years ago, of which a slight sketch is published n the Proceedings of the Literary and Philosophical Society of Manchester, I found that a plate of zinc and a plate of copper kept in metallic connection with one another (by a fine wire or otherwise) act electrically upon electrified bodies in their neighbourhood, and upon one another, as they would if they were of the same metal and kept at a difference of potentials equal to about three-quarters of that produced by a single cell of Daniell's. Hence, and from my measurement of the electrostatic effects of a Daniell's battery, published in the Proceedings of the Royal Society, for February and April, 1860, I find that plates of zinc and copper held parallel to one another at any distance, D, apart which is a small fraction of the linear dimensions of their opposed surfaces, and kept in metallic communication with one another, exercise a mutual attraction equal to

$$2 \times 10^{-10} \times \frac{A}{D^2} \text{ grammes weight.}$$

Hence, if they were allowed to approach from any greater distance, D', to the distance D, the work done by their mutual attraction is

$$2 \times 10^{-10} \times \frac{A(D'-D)}{D'D} \text{centimetre grammes;}$$

which, if D is very small in comparison with D', is very approximately equal to

$$2 \times 10^{-10} \times \frac{A}{D}$$

Now suppose a pile to be made of a great number (N + 1) of very thin plates alternately of zinc and copper, kept in metallic connection while they are brought towards one another. Let their positions in the pile be parallel, with narrow spaces intervening. For simplicity let the thickness of each metal plate and intervening space be D. The whole work done will be

$$2 \times 10^{-10} \times N\frac{A}{D}$$

The whole mass of the pile (if we neglect that of one of the end plates) is

$$NAD\rho,$$

where ρ denotes the mean of the densities of zinc and copper. Hence, if h be the height to which the whole mass must be raised against a constant force equal to its weight at the earth's surface, to do the same amount of work, we have

$$NAD\rho h = 2 \times 10^{-10} \times N\frac{A}{D}$$

which gives

$$h = \frac{2 \times 10^{-10}}{\rho D}$$

or, as $\rho = 8$, nearly enough for the present rough estimate,

$$h = \frac{1}{(200000D)^2},$$

Hence, if

$$D = \frac{1}{200000} \text{ centimetre,}$$
$$h = 1 \text{ centimetre.}$$

The amount of energy thus calculated is not so great as to afford any argument against the conclusion which general knowledge of divisibility, electric conductivity, and other properties of matter ndicates as probable: that, down to thicknesses of $\frac{1}{200000}$ of a centimetre for the metal plates and intervening spaces, the contact electrification, and the attraction due to it, follow with but little if any sensible deviation the laws proved by experiment for plates of measurable thickness with measurable intervals between them. But let D be a two-hundred-millionth of a centimetre. If the preceding formulæ were applicable to plates and spaces of this degree of thinness we should have

$$h = 1{,}000{,}000 \text{ centimetres or 10 kilometres.}$$

The thermal equivalent of the work thus represented is about 248 times the quantity of heat required to warm the whole mass (composed of equal masses of zinc and copper) by 1° Cent. This is probably much more than the whole heat of combination of equal masses of zinc and copper melted together. For it is not probable that the compound metal when dissolved in an acid would show anything approaching to so great a deficiency in the heat evolved below that evolved when the metallic constituents are separately dissolved, and their solutions mixed; but the experiment should be made. Without any such experiment, however, we may safely say that the fourfold amount of energy indicated by the preceding formula, for a value of D yet twice as small, is very much greater than any estimate which our present knowledge allows us to accept for the heat of combination of zinc and copper. For something much less than the thermal equivalent of that amount of energy would melt the zinc and copper; and, therefore, if in combining they generated by their mutual attraction any such amount of energy, a mixture of zinc and copper filings would rush into combination (as the ingredients of gunpowder do) on being heated enough in any small part of the whole mass to melt together there. Hence we may infer that the electric attraction between metallically-connected plates of zinc and copper of only $\frac{1}{400000000}$ of a centimetre thickness, at a distance of only $\frac{1}{400000000}$ of a centimetre asunder, must be greatly less than that calculated from the magnitude of the force and the law of its variation observed for places of measurable thickness, at measurable distances asunder. In other words, plates of zinc and copper so thin as a four-hundred-millionth of a centimetre from one another, form a mixture closely approaching to a molecular combination, if, indeed, plates so thin could be made without splitting atoms. Wishing to avoid complication, I have avoided hitherto noticing one important question as to the energy concerned in the electric attraction of metallically connected plates of zinc and copper. Is there not a change of temperature in molecularly thin strata of the two metals adjoining to the opposed surfaces, when they are allowed to approach one another, analogous to the heat produced by the condensation of a gas, the changes of temperature produced by the application of stresses to elastic solids which you have investigated experimentally, and the cooling effect I have proved to be produced by drawing out a liquid film which I shall have to notice particularly below? Easy enough experiments on the contact electricity of metals will answer this question. If the contact-difference diminishes as the temperature is raised, it will follow from the Second Law of Thermodynamics, by reasoning precisely corresponding with that which I applied to the liquid film in my letters to you of February 2nd and February 3rd, 1858,* that plates of the two metals kept in metallic communication and allowed to approach one another will experience an elevation of temperature. But if the contact difference increases with temperature, the effect of mutual approach will be a lowering of temperature. On the former supposition, the diminution of intrinsic energy in quantities of zinc and copper, consequent on mutual approach with temperature kept constant, will be greater, and on the latter supposition less, than I have estimated above. Till the requisite experiments are made, further speculation on this subject is profitless: but whatever be the result, it cannot invalidate the conclusion that a stratum of $\frac{1}{200000000}$ of a centimetre thick cannot contain in its thickness many, if so much as one, molecular constituent of the mass. Besides the two reasons for limiting the smallness of atoms or molecules which I have now stated, two others are afforded by the theory of capillary attraction, and Clausius' and Maxwell's magnificent working out of the Kinetic Theory of gases. In my letters to you already referred to, I showed that the dynamic value of the heat required to prevent a bubble from cooling when stretched is rather more than half the work spent in stretching it. Hence, if we calculate the work required to stretch it to any stated extent, and multiply the result by $\frac{3}{2}$, we have an estimate, near enough for my present purpose, of the augmentation of energy experienced by a liquid film when stretched and kept at a constant temperature. Taking ·08 of a gramme weight per centimetre of breadth as the capillary tension of a surface of water, and therefore ·16 as that of a water bubble, I calculate (as you may verify easily) that a quantity of water extended to a thinness of $\frac{1}{200000000}$ of a centimetre would, if its tension remained constant, have more energy than the same mass of water in ordinary condition by about 1,100 times as much as suffices to warm it by 1° Cent. This is more than enough (as Maxwell suggested to me) to drive the liquid into vapour. Hence if a film of $\frac{1}{200000000}$ of a centimetre thick can exist as liquid at all, it is *perfectly certain* that there cannot be many molecules in its thickness. The argument from the Kinetic Theory of gases leads me to quite a similar conclusion.

April 19.—Annual meeting, Dr. J. P. Joule, F.R.S., President, in the chair. The report of the council having been read, the following gentlemen were elected officers of the society for the ensuing year:—President: E. W. Binney, F.R.S., F.G.S.; Vice-presidents: Dr. J. Prescott Joule, F.R.S., Dr. E. Schunk, F.R.S., Dr. R. Angus Smith, F.R.S., Rev. W. Gaskell, M.A.; Secretaries: Dr. H. E. Roscoe, F.R.S., &c., J. Baxendell, F.R.A.S.; Treasurer: T. Carrick; Librarian: C. Bailey. Of the Council: P. Spence, F.C.S., G. Venables Vernon, F.R.A.S., J. B. Dancer, F.R.A.S., W. Leeson Dickinson, H. Wilde, R. Dukinfield Darbishire, F.G.S. A paper on "Infant Mortality in Manchester" was read by Mr. J. Baxendell, F.R.A.S., who combated the assertion that the high death-rate of Manchester and other towns in the cotton manufacturing districts is due to the mortality among infants and young children being relatively much greater than in large towns in other parts of the country, a careful examination of the mortality returns showing that a much larger proportional number of deaths of infants and young children takes place in other towns, where the general death-rate is decidedly lower.

* Proceedings of the Royal Society for April, 1858.

Cambridge

Cambridge Philosophical Society, May 2.—Clement Higgins, B.A., Downing College, was elected a fellow. Communications:—By Professor Miller, F.R.S.: "On the best form for the ends of measures *à bouts.*" The author found that the best form was that of two 'knife edges,' whose edges were in planes perpendicular to each other, and were not straight lines, but arcs of circles, whose centre was the opposite end of the axis of the bar. With this form the error (l being length of bar, and e distance between real and assumed position of the point where the axis intersects the bounding surface) $= \frac{e^4}{4l^3}$ while in the forms commonly used it was either $\frac{e^2}{l}$ or $\frac{2e^2}{l}$

By Mr. Bonney (St. John's College): Note on supposed Mollusc Borings in the limestone of Derbyshire. The author first described a large number of burrows which he had examined in the hills above Matlock; these were generally directed upwards, and too irregular in form to be *Pholas* burrows, as had been asserted; he then described some burrows observed in a scarp of limestone about nine feet above the stream in Miller's Dale, by the road-side. He believed this scarp to be artificial; but whether so or not, the gorge was most distinctly one of fluviatile erosion, and he maintained that this case was fatal to the *Pholas* theory. He considered the burrows to be the work of *Helices.*

Brighton

Brighton and Sussex Natural History Society, April 14.—Mr. Glaisher, Vice-president, in the chair. A communication from Mr. Gwyn Jeffreys respecting the Sars testimonial, was read by the Hon. Sec., after which a subscription was made among the members present, the Society not having power to vote its own funds for such a purpose. Mr. T. H. Hannah, the President, read a report on "Soundings made by Sir (Captain) E. Parry in 1818." These soundings were purchased some years since among the geological specimens of Sir E. Parry, from his widow, by Mr. J. Cordy Burrows, of Brighton, who deposited the geological specimens in the Brighton Museum, and gave the soundings to Mr. Peto. The discoveries of last year giving a prominence to sea-soundings, they were placed in January in Mr. Hannah's hands to examine microscopically, and report on their contents to the society. The soundings were made in Davis's Straits, and Lancaster Sound between 68° and 71° 15′ N. lat. and 73° and 78° 34′ W. long. at depths between 22 fathoms and 1,058 fathoms. Those under 58 fathoms contained nothing but stones and corals, some of the stones had evidently been rubbed by the action of a strong current; those from 201 fathoms were much less disturbed and consisted chiefly of a sandy material; from 674 fathoms there was an almost absence of inorganic débris, showing the non-existence of currents, and plenty of the testa of arenaceous foraminiferæ. In all the deep ones diatomaceæ were very beautiful, as well as sponge spicules and arenaceous foraminiferæ; but polycystinæ and cretaceous foraminiferæ were but sparsely found. He had also met with casts similar to those found in the greensand, and some highly organised spicules which he had not yet identified. At one of the microscopical meetings the slides would be exhibited. He much regretted they were not examined earlier, as they would then have preceded the discoveries of Carpenter and others, who, by the deep-sea soundings recently made, had opened out new ideas of life at great depths. Reference was then made to what had been done by Carpenter, Jeffreys, Thomson, and others in adding to our knowledge of the conditions of existence in deep sea; the nature of the animals and their economy together with the chemical conditions at great depths were also touched on, and the fact pointed out that changes of fauna depended not on latitude, but on the variations in the bottom temperature.—Mr. Wonfor, hon. sec., announced that the "Moss Flora of Sussex" was ready for distribution among the members, and that duplicate copies could be had at a nominal price, by applying to him, at 38, Buckingham-place, Brighton.

DUBLIN

Natural History Society of Dublin, May 4.—W. Andrews, Chairman of the Natural History Committee of the Royal Dublin Society, in the chair. The Chairman read a paper entitled "Ichthyological Notes." There are no subjects fraught with greater interest than discoveries which arise from a practical knowledge of any branch of science, whether it relates to botany, geology, or to any of the orders or genera of zoological investigations. When we contemplate the vast scope of the branches of the natural sciences, we cannot fail to meet most perplexing difficulties in the determining of correct classification, the opportunity of practical investigation not being afforded, for without that most essential aid, no certainty can possibly be arrived at, especially true characteristics depending upon habit, geographical range, seasons, depths, and peculiarities of soundings, and those natural causes which influence forms and changes of animal and vegetable life. The subject of the paper this evening relates to deep-water species, the present observations being with reference to notes on some fish occurring on the south-west coast of Ireland, in Dingle Bay, and off the coast of Kerry. Some most interesting crustaceans and species of rare fish have been met with, which are confined to peculiar soundings in deep water; yet I may say that at a depth beyond eighty fathoms much of interest as to variety of forms ceases, and the dredge in soundings of 100 fathoms, and beyond that depth, rarely brings up anything but remains belonging to shallower soundings, or those forms of Foraminifera Globigerina, which require microscopic manipulation in the determining of their numerous forms. It is singular, however, from what extreme depths minute crustacea, echinoderms, sponges, and corals are brought up; therefore I may say that when we proceed further than a depth of eighty fathoms, the interest, so far as the ichthyologist is concerned, closes. The object of this paper is to bring to notice a most interesting species of the Clupæidæ, the true anchovy (*Engraulis encrasicholis*) the first placed on record as captured on the shores of Ireland. I had heard of a species of small herring that had been taken in the herring nets, of a peculiar silvery brightness. I was delighted to obtain a specimen last autumn off Ventry Harbour, being another among the many interesting additions that have been made on that part of the coast of fish peculiar to the Mediterranean. The specimen exhibited is of full size, being in length six inches, thicker in proportion than the herring, no serrations on the abdomen, and the sides and belly being of the most silvery brightness, with no apparent scales. The back was a dark bluish green; the mouth with a remarkably wide gape; teeth exceedingly minute in the maxillary; none in the lower jaw; snout much projecting. At first examination I thought it might be a species of melletts, as it did not bear sufficient resemblance to several of the figures of the anchovy in works on ichthyology.—Dr. Foot then read a paper on the breeding of the Cereopsis Geese and Emus, in the Zoological Gardens, Dublin, and also laid before the meeting a paper on "Animal Luminosity," after which the meeting adjourned.

Royal Irish Academy, Monday, May 9.—Rev. Professor Jellett, B.D., president, in the chair. Dr. Stokes, V.P., F.R.S., read a paper on putrefaction occurring in some closed cavities, without the admission of air, and on the germ theory of Professors Lister and Tyndall.

Royal Geological Society, Wednesday, May 11.—Mr. G. Sanders, in the chair. Rev. Professor Haughton, M.D., read a paper on the probable geological effects of the permanent opening of the Suez and Darien Canals. The author regarded the Suez Canal as an interference with nature. He gave a graphic description of the two oceans, the Atlantic and Indian, with their two great offshoots, the Mediterranean and the Red Sea—seas with not a very limited rainfall area, and subject to immense evaporation; in fact, having all the circumstances in their favour to make them the seat of strong currents, the natural tendency of the meeting of which is to build up that vast sand or mud bar, the Isthmus of Suez. The new canal he regarded as a scratch of a spade by the hand of a child across the bar of two oceans, and the re-formation of that bar he regarded as certain—it would probably be re-formed beyond Port Said. He thought the Darien Canal would have a totally different success. The idea of forming this canal originated with Dr. Cullen, of Dublin. Nature, in scooping out the great gulf in which the West Indian islands lie, showed her determination to break through the Isthmus somewhere, and the great South Atlantic current would keep any canal open that was cut. The Americans thought that they would lower the temperature of Europe by removing so much of the Gulf Stream from us. But if their canal was one mile wide and 100 feet deep, they would take from us but $\frac{1}{1000}$ part of the heat we already possessed; and to make up this deficiency would cost the inhabitants of Dublin not more than *2s. 4d.* for additional fuel to each family—a sum they might well lose for the good of the whole race. The palæontological effects would not be of much interest in the case of either canals: a few Red Sea cockles might wander into the Mediterranean, and even this would not occur in the Darien Canal, as the same species of fish and molluscs were found on both sides of the Isthmus of Panama; and man must not forget that he has no power over nature.—The Rev. Maxwell Close read a paper "On some Corries, and their Rock Basins, in Co. Kerry."

Institution of Civil Engineers of Ireland, May 11.—John Bailey, C.E., in the chair. Mr. Alex. McDonald read a paper, "Notice of Le Chatelier's Counter-pressure Steam System." Professor Downing, LL.D., read "Notes on the Transport of Minerals by wire ropes." The author suggested the use of stationary wire rope, the mineral boxes to be pulled up by a common hempen rope, and to slide up the wire rope.

EDINBURGH

Royal Physical Society, April 27.—R. F. Logan, president, in the chair. Thomas Edmonston of Buness, Shetland, was elected a member of the society; and the Rev. Samuel Fraser, Melbourne, Australia, a corresponding member. The following communications were read to the meeting:—I. Our Pets in Unst, Shetland, by Thomas Edmonston of Buness, F.R.S.L. A vote of thanks was given to Mr. Edmonston for his interesting communication, notice being taken by Mr. Scott Skirving of the fact that the thanks of all naturalists were due to him as the preserver, on his estate in Shetland, of one of the few breeding places still remaining in Britain of the Great Skua Gull. II. Australian Entomology.—Notices of the Tarantula (Spider) and Hornet, by A. F. Grieve, Brisbane, Queensland. Communicated by D. Grieve. The President (Mr. Logan) said the insect called a hornet by Mr. Grieve was a species of *Pelopæus*, belonging to the family *Sphegidæ*; it was probably *P. lactus*. The habit of this genus is to store up spiders in mud cells, but surely it could not do this with the immense *sleygale*, which Mr. Grieve had also described. III. (1.) On the Skull of the Ringed Seal (*Pagomys fœtidus*, Gray), with special remarks on the osteological characters of the lower jaw. By James M'Bain, M.D., R.N. (2.) Notice of Clay Nodules, found at a depth of 292 feet in boring for an Artesian Well at Umballah, in India. By Thomas Login, C.E., F.R.S.E. Communicated by James M'Bain, M.D., R.N. IV. (1.) Note of a saline incrustation sold by the natives at Old Calabar, Africa. (Specimens sent to Dr. Smith by the Rev. Dr. Robb were exhibited.) (2.) Ornithological notes. (Specimens exhibited.) (3.) Notice of the capture of the Spiny Lobster (*Palinurus vulgaris*) on the west coast of Scotland. (Specimen exhibited.) (4.) Note of the capture of the *Seymnus borealis*, the Greenland shark, in the Frith of Forth. By John Alexander Smith, M.D. V. Notes of various species of *Crustacea* from Shetland, Caithness, &c. By C. W. Peach. (Specimens exhibited.)

Botanical Society, April 14.—Sir Walter Elliot, president, in the chair. The following communications were read:—On the Flowering and Fruiting of *Aucuba Japonica*, by Mr. P. S. Robertson. The author had observed that recently-introduced female plants from Japan (grown in a cold pit) came into flower in January and February, while the male plants, grown in the same circumstances, never came into flower till the middle of March. Yet he had every year obtained a crop of young seedlings from the berries, although the female flowers were quite shrivelled before the male ones expanded. He found that the common spotted variety, long grown in this country, does not flower till May or June, although grown in the pit or house with the others, and begins to expand its flowers when the males are getting past; yet it also never fails to produce a crop of fruit with perfect seeds. He thought that the pollen must lodge for some time in the scales of the unopened flower-buds, or must reach the pistils before the flowers are expanded; but how to account for the fertilising of the early flowering varieties, he was at a loss. This year he has forced on the flowering of the male plants by placing them in strong heat, and has all the varieties of the male and female plants in full flower at very nearly the same time, and accordingly he anticipates a much larger produce of berries than in former years, when they were left to the ordinary course. He exhibited a branch bearing berries with perfect seed; yet when that plant came into flower, there had not been a male plant in the house where it grew for fully a month previously. Mr. Sadler stated that he had been informed by the Messrs. Lawson that when there was a great lapse of time between the flowering of male and female Aucuba plants, they frequently collected the pollen and kept it wrapped in paper until such time as the female flowers were ready for fertilisation, when it was applied to the stigmas, and thus secured invariably a crop of fruit with perfect seeds. By grafting the male plant on the female, the two kinds of flowers might expand nearly at the same time.—"Remarks on *Grimmia pruinosa* (Wilson's MSS.)." By Mr. William Bell.—"Remarks on *Bœhmeria nivea* (*Urtica nivea* of Linnæus)." By Mr. Sadler.—"Memorandum on Ipecacuanha." By Mr. Clements R. Markham.—"Report on the Open-air Vegetation at the Royal Botanic Garden." By Mr. M'Nab.—"Remarks on the Embryos of the White Water-Lily (*Nymphœa alba*), and the Date Palm (*Phœnix dactylifera*)." By Professor Dickson.

Paris

Academy of Sciences, May 9.—The following mathematical papers were read:—"Some results obtained by the infinitely small displacement of an algebraical surface," by M. A. Mannheim, presented by M. Chasles; "On the division of hyperelliptical functions," by M. C. Jordan; and "On the existence of new classes, each containing an unlimited number of plane algebraical curves, the arcs of which present an exact representation of the elliptical functions of the first kind," by M. Allibert.—MM. Croullebois presented a reply to the remarks of M. Jamin, on the index of refraction of water.—A note, by M. G. Guéroult, on harmonic and melodic intervals, was presented by M. H. Sainte-Claire Deville.—A memoir, by M. C. A. Valson, on molecular actions, based on the theory of capillary action, was also presented by M. H. Sainte-Claire Deville.—M. C. Sainte-Claire Deville presented a note by M. E. Renou, on the latent heat of ice, in reply to M. Jamin's note, read at the last meeting.—M. Fizeau called the attention of the Academy to some errors which, he thought, had slipped into a communication by Father Secchi.—M. Regnault presented, in the name of M. Pfaundler, a claim of priority in the method employed by M. Jamin for the determination of specific heats.—M. Delaunay presented a note on the sun's spots, by M. Sonrel, in which the author called attention to some photographs exhibited by him, by which the perturbations of the surface of the sun are shown to have been lately remarkably active; a note, by M. H. Tarry, indicating the chief points of M. Respighi's theory of scintillation; and a second note, by the same author, on showers of dust and blood-rains. The author ascribed these phenomena to the action of cyclones upon the desert of Sahara.—MM. A. W. Hofmann and O. Olshausen communicated a memoir on the isomers of the cyanuric ethers.—M. Wade communicated a note confirmatory of M. Duchemin's remarks on the destruction of carp by toads; he referred to toads found fixed upon pike. M. d'Esterno remarked that the toads thus found attached to fishes were all males, and that this attack took place only at the breeding-time of the toads.—M. C. Robin presented a note by MM. Legros and Onimus, on the choreimorphic movements of the dog; and M. Balard communicated an extract of a letter from M. Castelhaz, on the employment of bromide of sodium, instead of bromide of potassium, as a medicine. The following papers were also read:—"A memoir on solids subjected to flexion," by M. L. Aubert; and "A note on the preparation of optically neutral sugar," by M. Maumené.

Vienna

Imperial Academy of Sciences, April 7.—Prof. Barth presented a memoir on isomeric cresoles.—Dr. Boué made some propositions for the purpose of getting rid of the ignorance which prevails as to the intellectual doings of certain foreign nationalities. He suggested that the transactions of academies established in certain places should be accompanied by translations or abstracts in French, German, or English.—M. G. Tschermak presented a memoir containing the results of an investigation of the meteorite of Lodran, near Mooltan, which fell on the 1st October, 1868, with a notice of a specimen of meteoric iron presented to the Mineralogical Museum, from the desert of Atacama.—A memoir, having the title of *Formicidæ neogranadenses*, by Dr. Gustave Mayr, was read, containing an account of the species of ants found in New Granada, and referring especially to those forms which throw light upon the affinities of the Formicidæ.—Dr. E. Reitlinger communicated the results of an investigation upon the spectra of negative electrodes, and of Geissler's tubes which have been long in use, which he had carried on in conjunction with Prof. M. Kuhn. The observers compared the spectra of the negative electrodes in Geissler's tubes with nitrogen, hydrogen, and oxygen, with those of the other parts of the tube, and found them to differ. To Wüllner's observations on the production of a new spectrum in hydrogen tubes after long use, the observers added similar results with nitrogen tubes. The fluorescence of the glass extends in these tubes to other parts than those immediately around the negative pole.—M. F. Unferdinger presented a memoir on the transformation and determination of the integral

$$\iiint F\left(\frac{x^2}{a^2}-\frac{y^2}{b^2}-\frac{z^2}{c^2},\ \alpha x+\beta y+\gamma z\right) dx\, dy\, dz$$

—Prof. Biesiadecki presented some investigations upon vesicle-formation and regeneration of the epithelium in the swimming membrane of the frog.—The report of the Central Observatory for meteorology and terrestrial magnetism for the month of March was communicated to the meeting.

April 21.—A memoir was read on some Pleuronectidæ, Salmonidæ, Gadoidæ, and Blennidæ, from Decastris Bay and Viti Levo, by Dr. F. Steindachner and the late Prof. R. Kener. Also one on the solution of dead organic matters, and one on the process of development and the structure of the walls of woody fibre, by D. T. Hartig, and one on the construction of a conic section, when this is defined by imaginary points and tangents.—Dr. Fitzinger presented the first part of his critical revision of the true Bats (*Vespertiliones*), containing the genera *Diclidurus*, *Taphozous*, *Saccolimus*, *Emballonura*, *Uroccyptus*, *Mystacina*, *Centronycteris*, *Saccopteryx*, and *Mosia*.—Dr. Horwath presented a paper on the production of inanition in animals by deprivation of heat.

April 28.—The following memoirs were read:—On the fish fauna of the Senegal (conclusion), by Dr. F. Steindachner; on the perfecting of involutions of high orders, by Dr. Emil Weyr; and on the determination of the sum of the angles of plane polygons, by M. A. Steinhausen.—M. von Haidinger reported upon some new observations relating to meteorites.—Dr. Boué again referred to his suggestion that the academies of northern and eastern Europe should furnish translations or abstracts of their memoirs in one of the three most familiar European languages.—Dr. T. Oppolzer presented a memoir on the transit of Venus in the year 1874.

New Zealand

Wellington Philosophical Society, January 29.—Annual meeting, Mr. J. C. Crawford in the chair. The election of the following office-bearers for the year took place, viz.:—President—Hon. W. B. D. Mantell, F.G.S. Vice-Presidents—J. C. Crawford, F.G.S.; R. Pharazyn, F.R.G.S. Council—W. T. L. Travers, F.L.S.; James Hector, M.D., F.R.S.; J. Kebbel; Wm. Lyon, F.G.S.; W. L. Buller, F.L.S.

Dr. Knox then gave a description of a specimen of *Bernardius Arnuxii*, which had been recently captured in the harbour, and the most important parts of the skeleton of which he had secured for the Museum. The animal was evidently full grown, and measured thirty-three feet, and what he desired to draw special attention to was the fact that while it presented well-developed teeth, yet they did not project through the gums, but were included in a deep socket with the tip covered in by thick fleshy gums. The use of such teeth, he considered, must be limited to the stimulation of the salivary glands by a reflex nervous process, as they could neither seize, divide nor masticate the food of the whale. Messrs. Mantell and Travers both considered that the teeth exhibited marks of the attachment of the gum round a projecting point of polished enamel, and that the teeth were probably enclosed in the retractile sheath of the gum. Dr. Hector stated that, in a recent description by Dr. Haast of a whale of the same species, it was stated to have shown its teeth when infuriated, which supported the view that the teeth were not completely undeveloped externally.—The next communication by Dr. Hector, on the interior of the North Island, gave the leading features of the geology of the Kaimanawa and Ruahine ranges, which had been recently examined by him. The modern tertiary rocks that form the eastern portion of the Hawke's Bay province, were described as rising in the interior to an altitude of 2,700 feet, but that it was probable that the Kaimanawa range and certain parts of the Ruahine mountains had always remained as islands above the tertiary sea. The tertiary rocks comprise three groups—1. Limestone containing a large percentage of existing shells; 2. Clay marls, containing few shells; and 3. Sandstones and conglomerates with irregular seams of coal, some of which might yet prove valuable as fuel. The upper group is of much later date than the others, but all are distinctly tertiary. The axis of slate rocks, which divided the tertiary series at the time of the development of the conglomerates, is within twenty miles of the present East Coast line, but is broken through by several modern rivers which rise in the Taupo plains; so that easy passes exist from Napier to the interior, a circumstance which has an important bearing on the opening up of the country. The Kaimanawa range is formed of the same slate and sandstone rocks as the Ruahine, but it lies at a considerable distance to the west of the proper axis of the island. The space left between them is occupied by the same tertiary rocks as on the east side, and which slope gradually to the sea-coast at Wanganui. As the tertiary rocks are quite free from any trace of volcanic matter, the eruption of the central volcanoes must have commenced after their deposit was completed. In referring to the auriferous specimens which had been found on Mr. Lyon's run at Kereru, Dr. Hector stated that chemical analysis had proved that, notwithstanding its granitic appearance, the rock to which the gold quartz was attached was only an altered form of the sandstone, as it contained traces of graphite, and 91 per cent. of silica. This is strongly in favour of the view that it is derived from the Ruahine range, as the sandstones in them have been previously mistaken for granite. After alluding to the recent increase in the activity of the volcanic forces in the Tongariro district, Dr. Hector described the route to the West Coast from the interior, and drew attention to maps and reports by Mr. Geo. Swainson and Mr. Field. He also exhibited a new geological map of the central district. The Hon. Mr. Fox considered that there was no doubt of the practicability of a route to Taupo district from the Wanganui coast. He believed that the track through the bush country was almost completed, and he was glad to find that no insurmountable obstruction would be encountered beyond that point. In reply to Mr. Mantell, Dr. Hector stated that he did not think that any rich auriferous quartz had been obtained in the Kaimanawa, but that his opinion remained unchanged as to the probability that gold would yet be met with in the district he had described.

PHILADELPHIA

American Philosophical Society, March 4.—A paper was read entitled "On the Periods of certain Meteoric Rings," by Prof. Daniel Kirkwood. Mr. P. E. Chase discussed the subject of the tides, referring to the recently-published theory of Prof. Challis, and contrasting his views with those of Airy and others. Prof. Cope read a paper "On *Adocus*, a genus of Cretaceous Emydidæ." Dr. Brinton read a paper entitled "Contributions to a grammar of the Muskokec language."

March 18.—Prof. Cope described the disinterment of a number of human remains from a pit in New Jersey, which probably belonged to some of the first European emigrants, whose history has not been preserved. He also exhibited photographs of human foot-tracks, sculptured in Cretaceous rocks of Kansas, and made observations on vertebræ of a large gavial from New Jersey. Dr. Brinton made some observations on a dictionary of the Maya language.

April 1.—Dr. F. V. Hayden described the position and appearance of the Tertiary strata on Green River, Wyoming Territory, mentioning the highly bituminous character of the shales. Prof. Cope exhibited two species of fishes from them, which he regarded as new, and named *Cyprinodon levatus*, and *Clupea pusilla*, and stated that their presence indicated connection with tide-water. Dr. Hayden mentioned the occurrence of insects and Myriopoda in the same shales.

DIARY

THURSDAY, MAY 19.

ROYAL SOCIETY, at 8.30.—Experiments on the Use of Alcohol (ethyl alcohol) in the Human Body: Dr. Parkes and Count C. Wollowicz.—On the Cause and Theoretic Value of the Resistance of Flexure in Beams subjected to Transverse Stress: Mr. W. H. Barlow.—On Deep-sea Thermometers: Commander J. E. Davis, R.N.—On the Difference between a Hand and a Foot, as shown by their Flexor Tendons: Rev. Dr. Haughton, and other papers.

SOCIETY OF ANTIQUARIES, at 8.30.—On recent discoveries in the Roman Wall, comprising Eighteen inscribed Altars: Rev. C. J. Bruce.

CHEMICAL SOCIETY, at 8.—On some Bromine Derivatives of Coumarine: W. H. Perkin, F.R.S.

ANTHROPOLOGICAL SOCIETY, at 8 (at St. James's Hall).—Race in Music: Henry F. Chorley.

ROYAL INSTITUTION, at 3.—Electricity: Prof. Tyndall.

FRIDAY, MAY 20.

ROYAL INSTITUTION, at 8.—Atoms: Prof. Williamson.

SATURDAY, MAY 21.

ROYAL INSTITUTION, at 3.—Comets: Prof. Grant.

MONDAY, MAY 23.

ROYAL GEOGRAPHICAL SOCIETY, at 1.—(Anniversary Meeting.)
VICTORIA INSTITUTE, at 4.—(Anniversary Meeting.)
LONDON INSTITUTION, at 4.—Botany: Prof. Balfour.

TUESDAY, MAY 24.

LINNEAN, at 3.—(Anniversary Meeting.)
ETHNOLOGICAL SOCIETY, at 4.—(Anniversary Meeting.)
INSTITUTION OF CIVIL ENGINEERS, at 8.—Discussion on Hot Blast Stoves.—On the Relative Safety of different Methods of Working Coal: George Fowler.—On Coal Mining in Deep Workings: Mr. Emerson Bainbridge, Stud. Inst. C.E.

WEDNESDAY, MAY 25.

GEOLOGICAL SOCIETY, at 8.

THURSDAY, MAY 26.

SOCIETY OF ANTIQUARIES, at 8.30.

ZOOLOGICAL SOCIETY, at 8.30.—On Dinornis (Part XVI.), containing Notices of Internal Organs of some Species, with a Description of the Brain and some Nerves and Muscles of the Head of the Apteryx australis: Professor Owen, F.R.S.—Notes on the Anatomy of the Prongbuck (Antilocapra americana): Dr. J. Murie.—Some Remarks on the Poison Glands of the Genus Callophis: Dr. A. B. Meyer.—Notes on some Fishes from the Western Coast of India: Surgeon Francis Day.

ROYAL INSTITUTION, at 3.—Electricity: Prof. Tyndall.

BOOKS RECEIVED

ENGLISH.—Balfour's Class Book of Botany, 3rd Edition (A. and C. Black).—Meteorology: Sir J. F. W. Herschel (A. and C. Black).—How Crops feed: S. W. Johnson (Trübner and Co.).—Lecture Notes for Chemical Students, Vol. 1.; Inorganic Chemistry, by E. Frankland (Van Voorst).—Trowbridge's Annual of Scientific Discovery for 1870 (Trübner and Co.).—Stanford's Family Atlas; Stanford's Complete Atlas; Stanford's Cyclopædian Atlas (E. Stanford).—Mammalia: their various Orders and Habits, by L. Figuier (Chapman and Hall).—Researches into the Early History of Mankind, new edition, by E. B. Tylor (Murray).

FOREIGN (through Williams and Norgate).—Malacologia del Mar Rosso: A. Issel.—Lecons sur la Physiologie et l'Anatomie comparée de l'Homme et des Animaux, Tome ix. pt. 2: M.-Edwards.—Troschl's Archiv für N tur-geschichte, 1870, pt. 1.—Publicato per cura dei Professori S. Richardi e G. Canestrini, Vol. II. Sect. 2.—Lehrbuch der chemischen Technologie zum Unterricht und Sebst-studium: D. F. Knapp.—XV. Tafeln zu H. Engelhardt's Flora der Braunkohlen-formation im Königreich Sachsen.

CONTENTS

THURSDAY, MAY 26, 1870

THE NEW NATURAL HISTORY MUSEUM

WITHIN the last few days two members of Her Majesty's Government, the Chancellor of the Exchequer and the Premier himself, have declared, in their places in Parliament, that it is the wish and intention of the Ministry to carry into effect, without further delay, the long-talked-of project of erecting a special building to contain the Natural History Collections of the British Museum.

That this announcement will be received with great satisfaction by the large body of persons interested in the various branches of natural history in this country there can be no doubt, although there may be misgivings in some quarters lest the fullest advantage should not be taken of so grand an opportunity of making the very best museum of the kind in the world. These misgivings are in a great measure justified by the present condition of not only our own, but nearly all other European Zoological Museums, and more especially by the plans that have at various times been put forth in a semi-official manner, as representing the ideal of what such a museum should be.

Long and extensive experience has, or ought to have, taught us the best principle of construction as applied to libraries and picture galleries; but the natural history sciences are in their infancy as compared with literature and art, and the best methods by which their treasures can be housed and exhibited have yet to be learned. The way in which this is done in the new Museum will exercise so great an influence upon the progress of these sciences, that it should not be determined upon without full consideration by those who are most conversant with their present state, and with the probable wants of a future generation of students.

Let the reader imagine what a public library would be if the books, instead of being shut up and arranged on the shelves for consultation when required, had every single page framed and glazed and hung on the wall, so that the humblest visitor as he passes along the galleries has only to open his eyes and revel in the wealth of literature of all ages and all countries, without so much as applying to a custodian or opening a case. There is something truly heroic in the conception of such a scheme; but laying aside all questions of space and cost, what would be its real utility to those who are able to appreciate and make true use of its contents! All the inconveniences, all the impossibilities, I may say, of a library arranged upon such a plan would be found in a museum containing anything like an adequate number of objects for the purposes of really enlarging the boundaries of scientific zoology, in which every specimen contained in it were exposed to the gaze of all who choose to enter its walls.

We are only beginning to form any idea of the enormous numbers of specimens actually required to enable us to rest our generalisations relating to most zoological problems upon a firm basis, and of the importance of keeping these specimens in such a condition, and so placed, that they can be examined and compared with the greatest facility. Provision should, therefore, be made in the new Museum for the great bulk of the collection being thus treated. It would be quite a mistake to suppose that they would then be shut up and put away, and that the public have no further concern in them, and ought not to be expected to pay for their accommodation. They would be in exactly the same circumstances as the books in a well-arranged library, and ought to be equally accessible under suitable regulations; and there are thousands of people who will read with interest the conclusions that scientific men will draw from their study of them, who would never care to see, or if they did, never learn anything from, the specimens themselves.

At the same time, the other essential function of a Natural History Museum, the diffusion of knowledge among the general public, should be carefully provided for, and means should be taken by which these two objects may be carried on simultaneously, and, at all times, without interfering with each other, instead of continuing the present excessively inconvenient system of alternate closed and open days. This, and many other advantages, upon which I need not dwell at present, can be readily secured by the admirable plan, first suggested, I believe, by Mr. Sclater, of having all the glass-cases hermetically closed on the side towards the public galleries, and opening behind into the working-rooms in which the unexhibited portions of the same series are arranged in drawers or cabinets. In this way, any exhibited specimen, if required for examination or comparison, could be readily removed and replaced, without inconvenience to the visitors, and without letting in the dust which necessarily fills the air of the crowded public galleries. One of the principal objections to lighting the Museum with gas in the evening would also be obviated if this were carried out.

Another important requirement in the arrangement of a new Museum of Natural History is the abolition of the special department of palæontology. There might be a comparatively small *geological* series, consisting of characteristic fossils arranged stratigraphically, but the great bulk of extinct animals should be incorporated with the zoological series, so that they may be studied side by side with their existing representatives, not only by the sight-seeing public and specially-instructed visitors, but also by those who have charge of the collections. If the construction of the new museum tends to perpetuate the present artificial distinction between extinct and recent species of animals, it will hinder instead of promoting the progress of any general conception of the organic world as a whole, and will also impose unnecessary difficulties in working out the minute comparisons by which the affinities and gradations between the various units of which that world is composed are recognised.

It would probably be premature at the present time to enter into any further discussion of details, since the subject has so often seemed nearly as far advanced as now, and has as often receded again into the far-off distance. But after the declaration of last Friday evening it should be in a fairer way of being realised than ever before it, and it behoves the working naturalists of the country to lend their aid in furthering the excellent intentions of the Government by making known their well-considered opinions upon this most important question.

W. H. FLOWER

ON THE EXTRACT OF MEAT

AN article of food has lately been introduced which has found its way into every grocer's and chemist's shop in the country, and for which there is in all parts of the world a vast demand. This substance is variously called the Extract of Meat, the Juice of Flesh, Liebig's Extract, and in Latin, *Extractum Carnis Liebigii.* The name of Baron Liebig, the great chemist, is more especially connected with this compound, as he has undoubtedly the merit of having first called attention to it as a valuable article of diet. In his "Familiar Letters on Chemistry," he devotes a letter to vegetable and animal food, and gives an account of their various chemical components. He shows with regard to all animal flesh, that besides fibrine, albumen, gelatine, and fat, it contains certain other constituents which may be separated from these by a simple process of infusion, straining, and evaporation. The substance thus obtained is the extract of flesh. This compound was known to chemists previous to the researches of Liebig, and he especially mentions those sagacious and experienced physicians, Parmentier and Proust, who had long ago endeavoured to introduce a general use of the extract of meat. They, however, regarded it as a remedy for disease and exhaustion, and recommended it as a resource for the diseased and wounded soldier on the field of battle or in camp. "In the supplies of a body of troops," says Parmentier, "extract of meat would to the severely wounded soldier be a means of invigoration, which with a little wine would instantly restore his powers, exhausted by great loss of blood, and enable him to bear being transported to the nearest field hospital." "We cannot," says Proust, "imagine a more fortunate application. What more invigorating remedy, what more powerfully acting panacea than a genuine extract of meat dissolved in a glass of noble wine? Ought we then to have nothing in our field hospitals for the unfortunate soldier whose fate condemns him to suffer for our benefit the horrors of a long death-struggle amidst snow and the mud of swamps?" That which these sagacious physicians recommended for dying soldiers is now a common article of daily consumption in the households of Europe. That which was amply demonstrated to be of use to the dying soldier, was found no less adapted to restore the vital powers of the poor in our hospitals, and that which proved of benefit to the exhausted nervous powers of the poor was soon found to be of value to the exhausted nervous powers of the rich. The doctor, from prescribing it to the poor in hospitals, learned to prescribe it to his patients among the rich. The result of the action of this substance on exhausted nervous systems and debilitated frames is no delusion, it is no influence of imagination, no simple belief in doses without power, but a real experience which is accumulating from day to day, and making demands on the manufacturers of this all-potent juice, which their utmost industry cannot meet.

Let us now inquire how this is. To the unlearned there is a ready reply: the extract of flesh is all the nutritive power of flesh concentrated, and if one pound of juice is got from thirty pounds of flesh it must be thirty times as nutritious. But it is not so, and it will be surprising to those who believe in this doctrine to hear that the extract of meat contains little or nothing of what may be said to be nutritious at all. The substances which go to form nourishment for the body, which are contained in meat, are fibrine, albumen, and fat, but these are not present in the extract of meat.

One hundred parts of beef contain the following constituents:—

1. Fibrine	4
2. Albumen	4
3. Gelatine	7
4. Fat	30
5. Mineral matters	5
6. Water	50
	100

Let us contrast with this the composition of a hundred parts of Liebig's Extract of Meat:—

1. Creatine, Creatinine, Inosic Acid, Osmazome, &c.	51
2. Gelatine	8
3. Albumen	3
4. Mineral matters	21
5. Water	17
	100

The difference will be seen at a glance. The water has diminished by half, the albumen is less, and there is four times the quantity of mineral matter, and a set of bodies is introduced which occupy half the bulk of the compound, which are not noticed in the composition of beef at all. If, then, the Extract of Meat differs from beef and from all other nutritious articles of diet, it is not in containing nutritious matters, but in containing the chemical compounds just mentioned in large quantities, and mineral matters. It is to these, then, we must look for the explanation of the marvellous powers which the extract of flesh exerts on the human system.

What, then, are the creatine, and its associates creatinine, inosric acid, &c? All we know of creatine is that it is a crystallisable body, and that it has an alkaline action, and is capable of combining with acids to form salts. In this respect it is like quinine, morphine, strychnine, and other substances from the vegetable kingdom capable of exerting a great influence on the nervous system. It seems to stand between these latter bodies and theine, which is contained in tea and coffee, and which has not the power of combining with acids. Whatever may be its chemical character, we know little of its action on the human body. It is easily resolvable into urea, and seems to be one of those compounds which are the result of the decomposition of albumen and fibrine into nerves and muscles before these are ultimately removed from the body. Whatever may be the true chemical or physiological relations of creatine, we cannot but regard the presence of this substance in the extract of flesh as playing an important part in the action of the latter on the human system.

When creatine is boiled with mineral acids another product results, which differs from creatine, and is called creatinine. This again may be decomposed, and forms sarkosine. The special action of these substances on the animal system is unknown, but we know they are contained in the juice of flesh. Besides these substances, there is inosic acid, and inosite, or muscle sugar, found in the juice of flesh, and probably there are other compounds not yet made out, and of whose special action on animal organisms we as yet know nothing. But although our knowledge of the action of these things is very imperfect, there is one thing we know, and that is, that the albumen, the fibrine, the fat, and the gelatine, will none of them act separately or together, as they do when combined with the juice of flesh.

Many experiments have been performed in France, Belgium, and Germany, which show that fibrine alone will not support life, that albumen alone (as in white of eggs) will not support life, that gelatine alone will not support life; we are thus driven to the conclusion, seeing that all these substances are easily digested and appropriated when combined with the juice of flesh, that the alkaline and other substances referred to perform a most important part, if not in ultimate nutrition, at least in the previous process of digesting food.

If we study a little our individual experience in the matter of digestion, we may find perhaps an approximate solution of the mode in which Liebig's Extract acts in giving strength to the weak, and new life to the exhausted. If we are hungry and eat dry bread the appetite soon palls, and we give up the effort; if we take some cold water we can consume more of the bread, and even with warm water, especially if flavoured with tea and sugar, still more. The latter evidently acts as an incentive. If we add salt to the water the same effect is produced. But if we now take a basin of soup—for soup is but a solution of the juice of flesh—we shall find that we can take into our stomachs with relish four or five times the bread we could have eaten dry or with cold water. How is this? We are all aware of the fact without being able to give the explanation. It is evident that an effect has been produced upon the nerves of the stomach and its glandular apparatus, which has enabled it to digest and deal with food which before was a mere inanimate burden in its cavity. If the nervous system is excessively exhausted or unable to act, as it is sometimes in disease, the glass of "noble wine" recommended by Proust will increase the effect upon the paralysed nerves. It is in this way, it appears to me, that the extract of flesh taken with food acts in so beneficial a manner, as compared with tea, coffee, cocoa, beer, wine, or spirits. All these, whilst stimulating the nerves of the stomach to higher action, are attended with subsequent depressing and sedative effects, of which we see no sign in the action of a dilute solution of the juice of flesh.

There does not appear to exist any evidence of the subsequent beneficial action of the organic substances found in the Extract of Meat. Not that this ought to be denied to them. They may, like theine and quinine, supply more readily materials for the manufacture of working muscle and nerve than can be readily obtained otherwise than from the blood. The theory that these salts assist in nourishing the nerves has recently been put forward, with his accustomed ingenuity, by Professor Agassiz; and as the flesh of fish is known to contain more creatine than that of other animals, he recommends a diet of fish as especially adapted for the food of philosophers and those who work much with their brains.

But whatever doubts may arise as to the action of creatine and its consequences on the ultimate nutrition of the nerves and muscles, there can be no doubt of the beneficial action of the mineral matters contained in Liebig's Extract. We eat salt because we do not get enough in our ordinary food. Besides salt, which contains chloride and sodium, we require other elements in our bodies. We require phosphorus, calcium, potassium, sulphur, and iron. Now, we do not add these artificially to our food as we do the chloride of sodium, and yet in our ordinary system of cooking and feeding we may deprive our bodies of these necessary elements. In soup we supply them, and they are contained in the juice of flesh. Whilst one hundred pounds of beef contain five pounds of mineral matters, one hundred pounds of Liebig's Extract contain twenty-one pounds of the same substances. Above seventy per cent. of these consist of phosphorus and potassium, whilst the remainder consists of lime, iron, sulphur, and magnesia. Here then, perhaps, we may find a nutritive action for the Extract in supplying those elements to the nerves and muscles which are constantly being removed by the changes of composition in the tissues, through the vital activity of the body.

From the above statement it will be seen that the juice of flesh presents after its manufacture no new product, but that it contains the same constituents that are ordinarily met with in the flesh of animals. The great advantage that it confers is that it is already fit for use. A teaspoonful of the Extract in a pint of hot water is a stock for any kind of soup, and may be prepared in a few minutes. To this may be added bread, potatoes, vegetables, eggs, meat, or flavouring essences of any kind, and the most agreeable of soups can be thus prepared. Its use in this direction is not confined to the sick-room; it may be used economically for the daily manufacture of soup for the table, and where the speedy preparation of hot food is desirable there is nothing to equal it. For the dyspeptic, and those whose stomachs have become paralysed by the use of theine in tea and coffee, it quickly restores the digestive powers; and for a permanent beverage, morning and evening, it is better than tea or coffee. Of course, this solution should always be taken warm, although after cooling it is perfectly thin, and is not like soup made from meat, which becomes thick on cooling, an effect due to the gelatine, and greasy, from the fat floating on the surface.

The Extract is sold for ten or twelve shillings a pound, and a pound of Extract represents thirty pounds of lean beef. It is therefore no economy to make it and sell it at this price in England; but as it can be made in South America and Australia, where cattle and sheep are in abundance, even at the low price of ten shillings a pound, including carriage, a large profit is made. Within the last few years an establishment has been erected at Frey Bentos, in South America, for the manufacture of this Extract from the wild cattle of that part of the world. There are also two distinct manufactories on the Clarence, New South Wales. These places are worked by companies which supply immense quantities for public use. There are also private manufactories in many parts of America and Europe, and one in Scotland, supplying the same substance. As far as published analyses have gone, the Extract has the same general composition, and on that account one is not to be preferred above another. But there is a difference in flavour, and that which is preferred in that point will fetch the highest price and have the largest sale. Here, as in all other kinds of food, it is the flavour that makes the quality. It is the *bouquet* of wine and not the alcohol that constitutes its value.

E. LANKESTER

THE SNAKES OF AUSTRALIA

The Snakes of Australia: an Illustrated and Descriptive Catalogue of all the known Species. By Gerard Krefft, F.L.S., C.M.Z.S., &c., &c., Curator and Secretary of the Australian Museum. Large 8vo. pp. 100, with 12 lithographic plates. (Sydney, 1869. London: Trübner and Co.)

WHEN we consider how very small is the number of zoologists who take an interest in, or make a special study of, the animals of the class Reptilia, and how little attraction this branch of zoology appears likely to have for the public, we cannot but feel surprised when, now and then, one bolder than his fellow-labourers prepares a comprehensive account of some portion of these animals, and ventures to put it forth in the shape of a goodly volume, which must have cost the author a vast amount of unappreciated labour, and the publisher a round sum of money without a prospect of its speedy return. Thus, on examining the work which has just been published under the above title by the Curator of the Sydney Museum, we find that the investigations on which it is based have been carried out by fourteen zoologists only, of whom not more than one half belong to the present generation, whilst the other half have only described a species or two incidentally.

The causes of this neglect of the study of reptiles are obvious. In Europe, a boy whom Nature has endowed with a taste for contemplating her works, begins to collect the objects most accessible in his neighbourhood, and most attractive by their variety of form or colour; he collects, and perhaps studies, birds and their eggs, beetles, butterflies, shells, or plants. What is more natural than that he should continue to devote himself to the same particular branch, if the duties of more mature years allow him to develop the fancy of his boyhood into scientific research? Consequently, ornithology, entomology, conchology, and botany are *popular* pursuits.

There are but few who become connected with public collections, and who, from more expanded views or duty, enter into the study of animals which have but rarely formed part of private collections. A boy in England would soon get tired of his taste for natural history, if he had to develop it through the scanty means afforded by the small number of British reptiles; and Ireland, as far as we are aware, has not yet produced a single herpetologist (although, as Mr. Krefft informs us, that island is inhabited by snakes—a fact which is certainly new to us).

On the other hand, we may predict that herpetology will become a more popular science in Australia, where reptilian life abounds. Snakes must be numerous there, for we are told that, "from six to ten specimens, belonging to different species, were captured some years ago under a single stone not many miles from the city of Sydney;" that, "to go snake-hunting has been a pastime with school-boys for years," and that "the collecting-bag often forms part of the outfit of the wallaby-hunters, by whom the old sport of boyhood is not forgotten." Snakes in Australia must also play quite as important a part in relation to mankind as in tropical countries; for not less than two-thirds of the species, and fully nine-tenths of the individuals, are venomous. Ten years ago only some forty species of Australian snakes were known; and it is chiefly due to the energy of Mr. Krefft, as collector and curator of the Australian Museum, that this number is now doubled.

The work begins with a copious introduction, in which the natural history of snakes generally is treated in a popular manner; then follow technical descriptions of the eighty species known, and their geographical distribution and habits are indicated, the volume being illustrated by twelve lithographic plates. The descriptions are chiefly reproductions of the original diagnoses given by the various authors; and we do not notice any species which has not been described elsewhere. Thus, whilst we bear witness to the great progress in Australian herpetology due to Mr. Krefft's labours, we must add that he has effected it previously to and independently of the publication of his book. But, like all conscientious compilations, it will be useful to the student, and will supply a real want among residents in Australia desirous of acquainting themselves with objects which daily come under their notice.

Great credit is likewise due to Mr. Krefft for the caution used in working up his materials. European collections contain by far the greatest number of the typical specimens of the species described within the last century; and men working at a distance from this principal source of information, and more or less dependent on descriptions, are only too much exposed to the risk of failing in the determination of species, applying old names to really new species, and describing old ones as new. No end of labour in rectifying these errors is caused to European naturalists by such premature publications. But Mr. Krefft has been for years in constant communication with his fellow-labourers in England and Germany, sending duplicate examples for identification; and thus creating a well-determined collection, he has laid a solid basis for his own future researches and for the instruction of Australian students. We have heard an authority on the subject express the belief that there is not in the book a single species erroneously determined.

The plates which accompany the volume are the work of two ladies, Miss Scott and Mrs. Edward Ford, who, considering the peculiar difficulty of drawing snakes, have accomplished their task extremely well.

In conclusion, we must congratulate the trustees of the Sydney Museum on having found so able and zealous a curator as Mr. Krefft; and express the hope that his book may lead to new discoveries sufficiently numerous to call for a second edition. It is a good sign that the scientific literature of our colonies already contains such books as the one under review. May the number soon be largely increased.

A. GUNTHER

OUR BOOK SHELF

The Bottom of the Sea. By L. Sonrel. Translated and Edited by Elihu Rich. Pp. 402, 67 Illustrations. (London: S. Low, Son and Co. 1870.)

WE cannot do better than quote part of the translator's preface, wherein he states that the book "bears the same relation to the strictly scientific treatment of the subject as a popular lecture on art to instruction in the studio; a ramble through a museum to a lecture on science; or a short pleasure-sail on the coast, with here and there an opening glimpse of the scenery," &c. M. Sonrel devotes the first portion of his book to submarine orography, with full explanations of deep-sea soundings, configuration of sea-bottom, submarine scenery, the various charts of the sea-bottom, and the like. The phenomenon of phosphorescence is explained; and the colour and temperature of the ocean are also dwelt on; next comes submarine life, with a long description of wonderful sponges, polypi, and corals. He relates, also, many legends with regard to marine monsters. Then we have man, and his work at the bottom of the sea, divers, diving apparatus, raising of ships, construction of bridges, submersion of towns, submarine volcanoes—all are graphically described. The last part is devoted to the action of rivers and currents on the sea-bottom, the dunes of Gascony, and villages buried beneath them. M. Sonrel lastly illustrates the insignificance of man compared with the ocean, by telegraphic cables, with an engraving of a fossilised cable. The following passage ends this interesting volume:—"If the intelligence of man has placed him at the head of the creation, the feeble influence that he can exercise over Nature ought to humble his pride. All that he can accomplish by physical labour is almost imperceptible by the side of the work effected by the microscopic infusoria; man, the giant, is dwarfed in results by the almost invisible atom!" This book is well illustrated throughout, and is admirably adapted to those who require light scientific reading.

Lehrbuch der Chemie für Land und Forstwirthe. Von S. J. Möser. Large 8vo., pp. x. and 355. (Vienna: Braumüller, 1870.)

IN this work, which was written for agricultural students, Dr. Möser has made it his aim to supplement the educational deficiencies under which his German pupils labour; and as the time which they can devote to purely chemical study is (he informs us) unduly limited, he brings into prominence in this manual only the more general and important facts, while the minor details, which are described in a smaller type, are kept somewhat in the background. The inorganic part is comprised in 183 pages, inclusive of an appendix, in which we are pleased to notice special sections devoted to the formation of saltpetre and the soil. The organic part contains 202 pages, and is consequently cut very short; but certain parts of it and its appendix are occupied very fully with physiological chemistry, and seem to have been ably executed,—perhaps more *con amore* than the rest of the *Lehrbuch.* Dr. Möser offers an apology for adopting the old notation; but we think his views on this subject are likely to alter with a new edition.

La Chambre Noire et le Microscope: Photomicrographie pratique. Par Jules Girard. (Paris: F. Savy. London: Williams and Norgate.)

THIS little volume contains a useful description of the apparatus required for photographic representation of microscopic objects, and a detailed account of the various operations involved in this art. It also describes the application of photomicrographic plates for lectures and educational purposes, by means of the oxyhydrogen light and the lantern. The book is illustrated with several well-executed woodcuts. A translation of it would be very useful to those engaged in this kind of work.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Oysters of the Chalk, and the Theory of Development

THE interesting notice, in your last number, of M. Coquand's "Oysters of the Chalk," draws inferences unfavourable to the Theory of Development or Evolution which scarcely seem warranted by the facts. It need not be "difficult to imagine the creature as existing under such conditions, that one species, while engaged in 'the struggle for existence,' should starve out and extinguish another;" for however widely we may find a fossil species dispersed, it is not probable that it occupied the whole of its territory at one and the same time, and in the limited area occupied immediately before its extinction, new varieties may have prevailed over and displaced the old by some slightly superior adaptation to the food-supply of the region. The extinction of any particular species may in some instances have been due to the extinction, or loss by other means, of its own appropriate food. Again, it is not necessary to suppose that the hinge, or the internal or external structure of the shell of an oyster, has been altered by what may be called the direct action of "Natural Selection," since by the well-established principle of "correlation" the variation in one part of an organism is nearly or quite certain to produce variations in other parts. "If any such change did occur," it is argued, "it must have been *per saltum*, since with these mollusks, numerous as they are, there are no forms that can fairly be recognised as transitional." But this appeal to the evidence of facts is somewhat premature. The immense difference pointed out between the geological records of England and France in regard to these very oysters of the chalk, leaves it perfectly open for us to suppose that even the comparatively full French record is itself exceedingly imperfect, and that the transitional forms have either not been preserved, or remain yet to be discovered. Mr. Darwin gives reasons for believing that when variation once begins it continues with some vigour; hence, between two settled widespread species connected genealogically together we might expect a large number of transitional varieties, each represented by only a few individuals, so that the whole number of these transitional forms might well be lost to the geological record.

Finally, the objection from the scarcity of oysters at the present day, compared with the great abundance of species in the past, does not really touch the theory of development, which is concerned to explain how species come into existence, not how they go out of it. That varieties, species, genera, have been superseded or extinguished, within longer or shorter periods, is a fact admitted on all hands. The general principle of natural selection will account for this in the rough, maintaining as it does, that fresh varieties, species, and genera better adapted to the surrounding circumstances have arisen, and by their superior adaptation, unavoidably ousted the older forms. Digging down into the records of history we find a time when the Romans were supreme in the civilised world; no two consecutive years of the interval present any remarkable divergence of the prevailing conditions, yet now we may say of that Roman supremacy in the civilised world, that, "like the Mastodon, it is a thing of the past." May it not be that both in races of men and every other race of creatures, there is a certain store of vitality and vigour, capable of very extensive and long-continued development, but capable also of exhaustion?

Torquay, May 14 THOMAS R. R. STEBBING

Euclid as a Text-book

THERE are many engaged in the work of education in this country, besides those who have come prominently forward in the matter, who feel strongly that Geometry as now taught falls far short of being that powerful means of education in the highest sense which it might easily be made. They find themselves, in the majority of cases, compelled to use in their classes a text-book which should long ago have become obsolete.

We have lately had instances in abundance of the power of combined action. If the leaders of the agitation for the reform of our geometrical teaching would organise an Anti-Euclid Association, I feel sure they would meet with considerable and daily-increasing support.

We of the rank and file do not feel strong enough to act alone, and yet think we might do something to help forward the good cause by co-operating with others.

The immediate objects of such an association should be in my opinion (1) To collect and distribute information connected with the subject; (2) To induce examining bodies to frame their questions in geometry without reference to any particular textbook.

RAWDON LEVETT

King Edward's School, Birmingham

Philology and Darwinism

MR. FARRAR'S interesting communication on this subject in a recent number of NATURE, suggests to me a few remarks. As one who has paid considerable attention to the various dialects existing throughout Scotland, as well as to the manner in which our Gaelic-speaking population is by stern necessity obliged to attempt the pronunciation of Anglo-Saxon words, I have become thoroughly convinced that the growth, life, and death of languages are subject to fixed laws. The Highlander whom I meet and who tells me this is a "Koot tay" is as unconsciously obeying Grimm's law of the transposition of consonants as the sun above him is obeying Newton's law of gravitation.

There is this difference, however, between Mr. Darwin's teachings regarding animals and the changes of language, that whereas those animals whose breeding and training are most subject to man's conscious action exhibit the greatest amount of variability, those dialects which are the most neglected and despised by educated men, and which, in this respect, may be called the weeds of speech, are far more variable than Queen's English, watched over as the latter is by such a host of schoolmasters, lexicons, and grammars. The difference of pronunciation of the same words in different counties is great, while many words in the dialect of one county are quite unknown in that of another, in this respect presenting a striking analogy to the Flora of the country. Even in words formed from sound, in which case, at first thought, we might expect considerable uniformity, the difference is often very marked, as between Scotland and England. The words imitative of the animal voice, or of the different cries of the same animal under different sensations are sometimes unlike, for the horse *neighs* and the cat *mews* in England, whereas they respectively *nicher* (*ch* guttural), and *nyioo* in Scotland. Sometimes the imitative word is from the sound of different organs, as *lapwing* in English from the flap of the wing; *peewit* in Scotland from the sound of the voice. Generally people so ignorant as to be necessitated always to express their thoughts in a rustic dialect, do so with the assistance of more or less gesture, and even this gesture is not quite whimsical, but has family and county resemblances. In conclusion, my impression is that the dialects which run wild are much more variable than those under man's care, which is the reverse of the case with wild and domestic animals and plants.

S. J.

Xanthidia in Flint

DR. CARPENTER, in his recent lecture at the Royal Institution on "the Temperature and Animal Life of the Deep Sea," speaks of the resemblance of the globigerina mud to chalk as being "greatly strengthened by the recognition of several characteristically cretaceous types among the foraminifera scattered through the mass of *globigerinæ*, of which it is principally composed; as also of the *Xanthidia* frequently presented in flint. (NATURE, vol. i., p. 564.)

The precise nature of the spinous orbicular bodies found in flint, and generally called *Xanthidia*, has hitherto been a matter of some doubt. Ehrenberg described them as fossil species of his genus of supposed polygastric animalculæ, *Xanthidium*. Their structure, however, differs in many respects from true *Xanthidia*, which form a genus of *Desmidieæ*, now universally admitted to be vegetable organisms, and, like nearly all desmids, having compressed bipartite cells, whilst the fossils have globose and entire cells. The most recent opinion has been that they are the fossil sporangia of other species of *Desmidieæ*, and they do indeed bear considerable resemblance to the sporangia of various species of the genera *Micrasterias*, *Euastrum*, *Staurastrum*, *Cosmarium*, and *Closterium*. An objection to this opinion arises from the fact that *Desmidieæ* are (so far at least as at present ascertained) exclusively fresh-water plants, and do not appear at all adapted for a deep-sea existence; whereas on the other hand, the chalk containing the flints may now be said to have been conclusively proved to have been formed at a considerable depth at the bottom of the sea: and the other organisms with which the fossils in question are found associated are undoubtedly marine.

The discovery of these *Xanthidia* (?) then, at an ocean depth of 767 fathoms, is a most important fact, whatever their nature may be; what that nature is, I, and I doubt not many others of your readers, would be glad to learn from Dr. Carpenter, or others who have had an opportunity of seeing the specimens he alludes to.

Winchester, May 14 FRED. J. WARNER

What is a Boulder?

WITH all terms in ordinary use there is a looseness of meaning, which, while not in the least degree inconvenient in common language, becomes so when transferred to the would-be-exact nomenclature of science.

Hobbes says, "A name is a word taken at pleasure to serve for a mark which may raise in our own mind a thought; like to some thought we had before, and which being pronounced to others may be to them a sign of what the speaker had or had not before in his mind."

It is obvious, however, that a name pronounced by a speaker, or written by an author, can only raise, in the minds of others, the proper thought, when the exact meaning of such a name has been agreed upon by those who make use of the name; and it appears to me desirable that in all cases when names in common use are transferred to the language of science, their exact meaning should be stated. In some sciences, as in botany, this is done, where such terms as leaf, stem, root, &c., are defined apart from their ordinary signification, but much remains to be done in this way. Thus, take the query at the head of this letter. I have tried to find out exactly what a boulder is, and I completely fail. According to Dance a boulder is "a mass of rock, whether rounded or not, which has been transported by natural agencies from its native bed." This definition is excellent at first sight, but it fails, as the term "mass of rock" conveys no clear idea as to size. Ask half-a-dozen persons the smallest size they would attribute to a mass of granite, and the answers would vary considerably. One cannot see why the smallest piece that would contain the constituents of the rock should not be called a mass, and in that case many of the large grains of sand on a granitic coast would be included in the term "boulder." But this is absurd, for we might then speak of carrying half a dozen boulders in the waistcoat pocket, or a geologist might suffer from having a boulder blown accidentally into his eye! There is apparently no determinate size at which a boulder begins or ends; but it seems to me desirable that some idea of size included should be given in the definition, and I would ask some of your readers to be good enough to state what they understand by a "boulder," with a view of getting an exact idea of the meaning of this frequently used term.

Midland Institute, May 9 C. J. WOODWARD

The Anglo-Saxon Conquest

A CORRESPONDENT, writing under the signature "A. Hall," in your issue of last week, suggests that in laying certain statistics as to the longevity of the Romano-Britons before the Royal Institution, I had "forgotten that the youth of Romano-Britain had for many generations been forcibly expatriated, drafted abroad to feed the armies of Imperial Rome." Your correspondent "appears to have forgotten" what the *résumé* of my lecture stated, viz., that my observations related to the time of Cerdic; and he will now no doubt recollect that a space of about three generations intervened between this period and the one to which he refers. Less than three generations is a sufficiently long period to allow of the balance which the Romano-British population is supposed to have suffered by being drafted into the Roman armies, righting itself.

I have, in a memoir which the Society of Antiquarians has honoured me by publishing in the "Archæologia" of this year, given other reasons, and these at greater length, for holding the explanation which your correspondent suggests for my statistics to be erroneous. I do not in that memoir quote, in illustration of and as a means of expressing that explanation, some lines from Mr. Henry Lushington's poem "Inkermann." These lines I did quote in my lecture, and will quote them here, as they may give pleasure to some of your readers. Speaking of the Russian slain, Mr. Lushington says:—

> Lay them there like soldiers—
> Men that did not blench.
> Many a sad serf-mother
> Yearns for these at home;
> Yet she thinks, "My children
> Never more shall come.
> Few, alas, of many
> Come back from the wars.
> There they die, fulfilling
> God's will and the Czar's!"

Oxford, May 23 G. ROLLESTON

Carp and Toads

IN reference to *Bufo calamita* attaching itself to the carp, and pressing its thumbs into the fish's eyes (see NATURE, May 12th), I would mention that the male Batrachia in the spawning season often attach themselves to any object, pressing the hands, on which is developed, at this season, a peculiar, black, wart-like structure, into the object which they seize—a stick, a human finger, or a carp as it appears, being sometimes hugged with spasmodic violence. A curious illustration of the reflex nature of this movement, and the inhibitory function of the cerebrum in regard to reflex actions, was witnessed by me lately, on cutting through the neck of a male toad. My finger was between the animal's fore legs, but on account of fright, or some other cerebral operation, no hugging action took place; but directly the connection between the spinal cord and cerebrum was severed, the arms joined closely upon my finger, the thumbs being pressed into it in the usual way, and the headless body held firmly to me with considerable muscular power. Just as the leg of the brainless frog is withdrawn more rapidly from acidulated water, than is that of a perfectly sound and healthy specimen, so did the hugging action of the forearms fail to be brought about by contact of the extremities with a foreign body whilst the animal was whole, but took place immediately upon the severance of the cerebrum from connection with the rest of the cerebro-spinal axis.

Hampstead, May 16 E. RAY LANKESTER

WITH reference to the disease existing amongst the carp at the Château de Montigny, and its presumed connection with "the first days of spring," and the animosity of the toad for the carp, permit me to take up the cudgels, not for the early spring but for the poor maligned toad, which like other possessors of jewelled heads has already but too many enemies. On two occasions I have noticed the curious train of symptoms detailed by M. Duchemin, commencing with blindness, and ending in death. One occurred in Norway, and the trout and grayling were the only fish affected; the other in Lord Bathurst's park in this town, when the pike only were attacked whilst the perch and tench escaped. In both these instances I instituted a series of experiments to ascertain the probable cause of death. No intestinal worms were discoverable, nor did any of the tissues appear congested or otherwise diseased, with the exception of the eyes, in which the cornea became opaque and friable so that on very slight pressure the crystalline lens escaped. The long duration of this blindness before death supervened, rendered it probable that starvation was at least usually the cause of death. In both cases spring was the time of the attack, but in neither were toads observed in proximity to the diseased fish; indeed, in that part of Norway where the disease existed, toads are almost unknown. One cause only has as yet suggested itself to me, and that is the presence of diffused mud in the water. As both in Norway and Cirencester, works had been undertaken, just previous to the outbreak of the disease, which had had the effect of introducing a large amount of clay into the water, and the "early days of spring" were so far implicated that they were days of rain and melting snow, and thus the products of degradation were added to the mischief caused by the hand of man.

Cirencester, May 14 W. D. CROTCH

Meteorological Phenomenon

ON the afternoon of Sunday, the 22nd, a very curious appearance was noticed by many. The sky was hazy, and the sun was seen through the haze of a pink colour, inclining to purple. I see by a newspaper that the same was noticed at Dublin. A red or orange sun is common, but I never before saw its colour on the purple side of red. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, May 24

Keen Sight of Fish

THE following extract from the remark book of Captain Robert A. Parr, of H.M.S. *Lyra*, may be of interest to your readers, bearing, as it does, such remarkable and trustworthy evidence of the keen sight of fish:—

"December 17th, 1867.—At noon, in lat. 0° 33′ S., long. 46° 13′ E., caught an Albacore, with 28 pistol bullets in its stomach. The ship's company had been exercised at pistol practice during the forenoon." G. F. M'DOUGALL

Hydrographic Dept., Admiralty, May 24

THE MULBERRY TREE

AN effort is being made to introduce once more into England the cultivation of the mulberry tree, and as the leaves of either the white or black variety (but especially those of the former) afford food for the silkworm, and both kinds will flourish in a tolerably mild and moist climate, there seems to be no reason why we should be altogether dependent upon foreign sources for a supply of raw material for the looms of Coventry and Macclesfield. Certainly the present is a favourable opportunity for making the experiment, as the price of silk has been largely enhanced by disease amongst the worms in the south of Europe, and by the destruction of the mulberry trees in China during the rebellion. At Yately, in Hampshire, Captain Mason has for the last three or four years been successfully engaged in rearing silkworms, and he calculates that if his mulberry plantations had been made upon a sufficiently extensive scale, a profit of 10*l.* an acre might have been easily realised. King James I. preceded him in this speculation, and imported ship-loads of mulberry trees from France with the view of encouraging the production of silk in England. In 1629 Walter Lord Aston was appointed "to the custody of the garden, mulberry trees, and silkworms near St. James's, in the county of Middlesex." But the scheme, like many others framed by the same monarch, proved abortive, and within a few years the mulberry garden became, in the words of Evelyn, "the only place of refreshment about the town for persons of the best quality to be exceedingly cheated at." Pursuing its history a little further we find the gardens converted into the site of Buckingham House, and in our own time Dr. King's allusion, written a century and a half ago, is a good deal more true than when he penned it:—

> The fate of things lies always in the dark;
> What cavalier would know St. James's Park?
> A princely palace on that space does rise,
> Where Sedley's noble muse found mulberries.

Within our own memory a similar experiment was tried in the neighbourhood of Slough, but it failed, not from any deficiency in the supply of food, but from the difficulty and expense incurred in tending the worms and carding the silk. Mechanical processes have now, in a great measure, removed these drawbacks, and the whole process of cultivation is one which would afford suitable employment for women and children.

The vicissitudes of fortune experienced by the mulberry tree in England belong to a curious chapter in the yet unwritten history of Botany. In common with the vine and several other trees, the mulberry has been alternately fostered and neglected: but in spite of royal favour and many intrinsic merits (for its fruit is wholesome and its timber valuable), it has now become a rare tree in Britain. The specimens at Sion House enjoy the reputation of being the first planted in England; but the probability is that the mulberry was introduced by the Romans, for the Saxon name for the tree "mor-beam," is little more than an echo of the Latin *morus*, which again can be traced to a still more Eastern source. The Sion House trees were, perhaps, some of those imported by James I., but their interest is far inferior to that which attaches to the celebrated tree planted at Stratford by Shakespeare's own hand, and ruthlessly destroyed by a Goth of modern times. C. J. ROBINSON

NEW TREES AND SHRUBS FOR ENGLISH PLANTATIONS *

WITHIN the last twenty years a complete revolution has taken place in the character of our out-of-door planting of ornamental trees and shrubs; trees which twenty years ago were rarities that a lover of arboriculture would go miles to see, are now to be met with in every gentleman's shrubbery or lawn with any pretensions to artistic arrangement. A really good hand-book was greatly wanted, to enable a planter to choose the species best suited to the climate, and best adapted to the special circumstances of his own particular estate. Such a hand-book Mr. Mongredien's "Planter's Guide" proposes to furnish, and to a great extent successfully. The plan of the book is admirable. We first have a list of 621 species of trees and shrubs, "selected from the large multitude which from time to time have been introduced from all parts of the world, and of which the vast majority are not worth cultivation for ornamental purposes." A brief description accompanies each name, with instructions as to the best aspect or position, and other needful particulars. These 621 species are then looked at from different points of view, and in the second part are classified accordingly: first, as to their height, then as to their foliage, next as to their flowers, and finally as to their fruit. Furthermore, we have selected lists of species remarkable for singularity of aspect, for rapidity of growth, suitable for hedges, thriving under the drip of trees or in the smoke of cities, adapted for different soils, &c. On looking closer into the manner in which this programme is carried out, we certainly find defects, as might be expected in a work which covers so much ground; but the defects are such as the author is sure to have brought under his notice, and which may easily be remedied in a second edition. Thus, although the list seems an extensive one, we miss many species, either old favourites or newly introduced, which ought to have had a place in it for their beauty or their useful properties: such as, among flowering shrubs, the *Berberis aquifolium*, a desideratum in every shrubbery, from its early flowering and the beautiful gloss of its evergreen leaves; *B. vulgaris*, the scarlet fruit of which is one of the most beautiful ornaments for the table; and the butcher's broom, *Ruscus aculeatus*, very easy of cultivation, and striking from the weirdness of its appearance, and the very peculiar growth of its flowers: and among climbing-plants, the common hop, used in some of the London parks in a very graceful manner to cover the stems of the poplar-trees. The list of plants thriving in the smoke of cities might also have been more than doubled by any of the author's friends who happen to have a London suburban garden. The illustrations interspersed here and there are very pretty; the frontispiece, however, a magnificent specimen of *Araucaria imbricata*, is unfortunate. If really taken from that tree, and not from another species of *Araucaria* not mentioned in Mr. Mongredien's list, it is very badly drawn.

ABIES PINSAPO BIOTA PENDULA

The introduction from China, about thirty years since, by Mr. Fortune, of a number of perfectly hardy evergreen conifers, previously unknown in this country, set the fashion among gardeners and planters strongly in that direction; and the proportion of this class, recommended in the "Planter's Guide," is very large. Out of the 621

* "Trees and Shrubs for English Plantations; a selection and description of the most ornamental trees and shrubs, native and foreign, which will flourish in the open air in our climate." By Augustus Mongredien. With Illustrations. (London: Murray, 1870.)

species, 254 are evergreens, 85 of them belonging to the coniferous tribe. As the author remarks, however, there are both advantages and disadvantages connected with the choice of evergreens for ornamental planting; while the persistent leaves of evergreens are generally of a dark and sombre hue, the young leaves put forth by deciduous trees in the spring are of a much brighter livelier tint, and during the summer months add much more to the freshness and beauty of the landscape. It seems probable that the great rage for new conifers is now somewhat going by, and that more attention will in future be paid to shrubs remarkable for the beauty of their flowers or fruit. A great acquisition has been the recent introduction of male plants of the *Aucuba japonica*, or common "variegated laurel," which thrives in every London garden or area. Till recently female plants only had been known in this country, which consequently never bore fruit. The fertilising of these by pollen, or the planting of a male plant, will ensure their being covered in the summer and autumn with a quantity of ornamental red berries.

There is much yet to be learnt with regard to the laws of acclimatisation and naturalisation. It appears by no means always to follow that we must look for trees and herbaceous plants suitable for introduction into our own country to those regions of the earth, the climate and soil of which most closely resemble our own. On looking over Mr. Mongredien's list, the countries which seem to have furnished the greatest number of perfectly hardy trees and shrubs are China and Japan, Northern India, Chili, California, and the more southern of the United States. Australia and New Zealand, on the other hand, have sent us very few species. A very large number of new kinds were introduced between 1840 and 1850; and we have therefore had no opportunity yet of knowing whether they will attain with us the size that some of them do in their native forests. An elm-tree eighty feet high is with us a fine tree; a very large number of the conifers are described in this work as attaining a height of from 100 to 140 feet, while the *Wellingtonia gigantea* of California, the monarch of trees, rears its head to the enormous altitude of 350 or 400 feet. Should our descendants witness their growth to their normal size, they will probably in many cases regret the want of forethought in their ancestors who planted them so near their houses; and at all events our parks and pleasure-grounds will present a very different appearance to what they do now. There is little doubt that a considerable number of trees and shrubs which are reckoned by gardeners to be half-hardy will, with a little care, grow very well out of doors in the southern counties of England. Even the Camellia requires, according to our author, protection for the first year or two only, to become a permanent and magnificently ornamental denizen of our shrubberies.

As a specimen of the author's style we may quote his description of the elegant *Cupressus Lawsoniana*:—

"California, 1852. Tree 60—80 feet. Leaves in alternate opposite pairs, closely adpressed, of a glaucous green. Branchlets slender, flattened, thickly clothed with leaves, gracefully pendulous, the leading shoots (as in the cedars) drooping until the ensuing season's growth; cones of the size of a large pea, with a glaucous bloom while young. This is one of the most beautiful trees of a beautiful tribe. It is very hardy, a rapid grower, and should find a place in every collection. It is frequently so laden with its beautiful cones (which, however, have more the appearance of berries) that the fruitful branchlets are quite borne down by their weight, like the boughs of a prolific apple-tree. Nothing can be more graceful or attractive."

GLEDITSCHIA TRIACANTHOS

Mr. Mongredien's book should be in the hands of every one interested in the planting of trees—and who is not who has the money to spend and the space to spare? The man who introduces on an extensive scale a new ornamental tree adapted to our climate performs a service to mankind, not only to his contemporaries, but to his descendants for many generations.

NOTES

HERE is some welcome news from the *London Gazette*—"The Queen has been pleased to appoint the most Noble William, Duke of Devonshire, K.G.; the Most Honourable Henry Charles Keith, Marquis of Lansdowne; Sir John Lubbock, Sir James Phillips Kay-Shuttleworth, Bernard Samuelson, Esq., William Sharpey, Esq., M.D., Thomas Henry Huxley, Esq., Professor of Natural History in the Royal School of Mines; William Allen Miller, Esq., M.D., Professor of Chemistry in King's College, London; and George Gabriel Stokes, Esq., M.A., Lucasian Professor of Mathematics in the University of Cambridge, to be Her Majesty's Commissioners to make inquiry with regard to Scientific Instruction and the Advancement of Science, and to inquire what aid thereto is derived from grants voted by Parliament, or from endowments belonging to the several Universities in Great Britain and Ireland, and the Colleges thereof, and whether such aid could be rendered in a manner more effectual for the purpose."

WE believe that the Government will propose to Parliament that the New Natural History Museum shall be built on the site occupied by the Exhibition of 1862, south of the Royal Horticultural Gardens. The ground was purchased some time ago for the sum of 120,000*l*.

THE annual visitation of the Royal Observatory, by the Board of Visitors is fixed for the 4th of June, at 3 p.m. The visitors, according to custom, will dine afterwards at the Ship.

MR. ERASMUS WILSON, who some time ago presented to the Council of the Royal College of Surgeons, of which institution he is a Fellow, the handsome sum of 5,000*l*., with which to endow a Professorship of Dermatology, has now devoted a further sum to the liquidation of the debt incurred by the College in fitting up the costly cases in the museum for the large and valuable collection of dermatological preparations since presented by him.

WE are requested to state that, to provide space at the South Kensington Museum for the examination and exhibition of the National Competition Drawings of the Schools of Art in the United Kingdom, the Gallery of Raphael's Cartoons will be used, and must be closed for a short time.

THE Council of the Royal Society have decided to recommend J. A. Ångström, of Upsala, and J. A. F. Plateau, of Ghent, to fill the two vacancies in the list of Foreign Members of the Society.

THE Commission of the Imperial Observatory of France for 1870 consists of the following members :—Members of the Council : President, M. Delaunay; Vice-president, Rear-Admiral de Penhoat; M. Balard, of the Academy of Sciences; M. Puiseux, of the Bureau of Longitudes ; M. Yvon Villarceau, M. Marié-Davy, M. Loevy, M. Wolff. Members of the Committee of Examination : President, Vice-Admiral Touchard, Rear-Admiral Baron Didelot, M. Faye, and M. Serret, members of the Institute; M. Briot, professor at the Faculty of Sciences, and M. Delaunay, director of the Imperial Observatory.

AS mentioned in our last number, the French Minister of the Fine Arts is in future to bear the title of Minister of Letters, Sciences, and the Fine Arts. The following duties and institutions are separated from the Ministry of Instruction to be attached to the new ministry :—The Imperial Institute of France ; the Imperial Academy of Medicine ; the Museum and Library of Algiers, and instruction in living Oriental languages ; the Imperial School of Letters Patent ; the Imperial Library and curriculum of Archæology which are attached to it ; the Mazarin library, and those of the Arsenal and St. Geneviève ; the general care of the libraries, and editing the catalogues of the libraries of the departments ; the learned societies of Paris and of the departments ; the *Revue des Sociétés savantes*, and library of the committee of historical works, and of the learned societies ; the *Journal des Savants* ; subscriptions to scientific and literary works, and distribution of these works among the public libraries ; the consulting committee of subscriptions and committee of historical works ; encouragement and assistance to scientific and literary men ; subsidies and encouragements for voyages and scientific and literary missions ; publication and distribution of the unpublished documents concerning the history of France and topographical map of Gaul ; the legal dêpot ; reception and distribution of works proceeding from the legal dêpot. And all this time *we* have not even a Minister of Public Instruction !

THE annual Rede lecture was delivered on the 18th inst. in the Senate House, Cambridge, by Prof. W. A. Miller, of King's College, London, to a numerous audience, the subject being, "Aniline, and the various Products of Coal-tar." The lecture, which was eloquently delivered, and was illustrated by many beautiful and successful experiments, was listened to with great attention ; and at the conclusion a vote of thanks, proposed by the Vice-Chancellor and seconded by Professor Sedgwick, was carried enthusiastically. The honorary degree of LL.D. was afterwards conferred on Prof. Miller and on Mr. Huggins, the Rede lecturer of last year.

THE council of the Senate of the University of Cambridge have referred the report of the Physical Science Syndicate back to that body, with a request that they would reconsider it, having regard to the late debate in the Arts Schools, the general tone of which was decidedly adverse to the scheme recommended by the Syndicate, and favourable to the general principle of the fund being raised by some system of taxation of the College revenues.

WE are extremely glad to learn that Trinity College, Cambridge, has instituted a Prælectorship of Pure Physiology, and that Dr. Michael Foster, who has for some years been Professor of Practical Physiology at University College, has been appointed the first occupant of the chair. This will, we trust, give great impulse to the study of Natural Science, especially of Biology, in Cambridge. Trinity College could scarcely have found a more earnest, able man, and a better teacher than Dr. Foster.

DR. CARPENTER will deliver a lecture "On the Physical and Biological conditions of the Deep Sea," in the Senate House of the University of Cambridge, to-morrow, at 2.30 P.M.

THE "Sars Fund" in this country now amounts to 343*l*. 12*s*. 10*d*., and we understand that it will soon be closed. Every zoologist and geologist has subscribed ; but there are many lovers of science and of Scandinavia to whom we may venture to make a last appeal on behalf of this truly deserving and interesting case. Some copies of the *carte de visite* of the late Professor Sars will soon be received by Mr. J. G. Jeffreys for those subscribers who may care to possess such a memorial.

THE Admiralty have acceded to the request of the Royal Society by again placing Her Majesty's surveying steam-vessel the *Porcupine* at their disposal for another deep-sea exploration, to commence in the latter part of June. Mr J. Gwyn Jeffreys will take charge of the first cruise, which is intended to be across the Bay of Biscay, along the coasts of Spain and Portugal, to Gibraltar. Dr. Carpenter will there succeed him in the beginning of August, and proceed into the Mediterranean, after endeavouring to trace the direction and nature of the currents at the Straits. Prof. Wyville Thomson will probably join Mr. Jeffreys. A photometric apparatus has been contrived by Mr. Siemens for the purpose of ascertaining the depth to which solar light penetrates the sea ; and other questions of considerable interest will be investigated in this expedition. But we regret to find that the time is so limited for such an important object.

AT the anniversary meeting of the Royal Geographical Society held on Monday last, the Founder's Medal was awarded to Mr. George W. Hayward, the Society's envoy to Central Asia, for the map of his journey across the Kuen Lun into Eastern Turkistan, and for the perseverance with which he is endeavouring to carry out his object of reaching the Pamir Steppe. The Patron's, or Victoria Medal, to Lieutenant Francis Garnier, of the French navy, second in command of the French Exploring Expedition from Cambodia to the Yang-tsze-Kiang, for the part he took in the extensive surveys executed by the commission, for his journey to Tali-fu, and for the ability with which, after the death of his chief, Captain de la Grée, he brought the expedition in safety to Hankow. The medals presented by the society to the chief public schools were awarded to G. G. Butler, Liverpool College; M. Stuart, Rossall School; G. W. Gent, Rossall School; and J. H. Collins, Liverpool College. Sir Roderick Murchison, in the course of his opening address, said that he grieved at being unable to offer some encouraging sentences on the prospect of speedily welcoming Dr. Livingstone home; at the same time (he proceeded) there is no cause for despondency as to his life and safety. He had been for some time at Ujiji, on the Lake Tanganyika, whence he wrote home on the 30th May last, though unable to make any movement for want of carriers and supplies. These were, indeed, forwarded to him by Dr. Kirk from Zanzibar, when an outbreak of cholera stopped and paralysed the relieving party. Recent intelligence, however, has reached the Foreign Office to the effect that the pestilence had subsided to so great an extent that we may presume the communication between the coast and Ujiji has before now been reopened. The work which still lies before Livingstone has been often adverted to, and it is hoped that he will live to advance to the north end of the Tanganyika, and there ascertain if its waters flow into the Albert Nyanza of Baker. If the junction should be proved, we may indulge the thought that, informed as Livingstone must now be of the actual carrying out of the great project of Sir Samuel Baker, he may endeavour to meet his great contemporary. The progress of the great Egyptian expedition of Baker having been delayed in its outset, we know that it only left Khartoum to ascend the White Nile in February. After reaching Gondokoro, as was expected to be the case in the first days of March, some time must necessarily elapse in establishing a factory above the upper rapids, and beyond the tributary Asua, where the steam-vessels are to be put together before they are launched on the Nile water, on which they are to pass to the great Lake Albert Nyanza. As soon, however, as a steamer is on that lake, we may be assured that Baker, with his well-known energy and promptitude, will lose not a moment in the endeavour to reach its southern end, in the expectation of there giving hand and help to Livingstone.

THE paragraph inserted among our "Notes" of April respecting Mr. Wilson Saunders's collection of "mimetic" plants at the recent Linnean *soirée* having been copied by our able contemporary the *Gardener's Chronicle*, a correspondent in that paper asks for a complete list of the mimetic pairs. In giving us the list, Mr. Saunders states that the plants were none of them grown for the purpose, but simply selected from his greenhouse on the spur of the moment for the purposes of the *soirée*.

Olea europæa	Oleaceæ
Swammerdamia Antennaria	Compositæ
Kleinia ficoides	Compositæ
Cotyledon tricuspidata	Crassulaceæ
Thujopsis lætevirens	Coniferæ
Selaginella circinata	Lycopodiaceæ
Phyllanthus Xylophylla	Euphorbiaceæ
Polygonum platycladon	Polygonaceæ
Peperomia sp. Brazil	Piperaceæ
Nematanthus longipes	Gesneraceæ
Haworthia planifolia	Liliaceæ
Cotyledon (Echeveria) aloides	Crassulaceæ
Gymnostachyum Verschaffetii	Acanthaceæ
Echites rubro-venosa	Apocyneæ
Sempervivum arenarium	Crassulaceæ
Haworthia atrovirens	Liliaceæ
Echinocereus Blankii	Cactaceæ
Euphorbia echinata	Euphorbiaceæ
Aralia sp. Bahia	Araliaceæ
Philodendron sp. Trinidad	Araceæ
Dorstenia sp. (near villosa) Brazil	Moreæ
Eranthemum sp. n. Brazil	Acanthaceæ
Grevillea sp.	Proteaceæ
Acacia chordophylla	Leguminosæ
Euonymus latifolius	Celastraceæ
Hedera canariensis var.	Araliaceæ
Ilex Aquifolium var.	Aquifoliaceæ
Osmanthus Aquifolium var.	Oleaceæ

AN admirable article appears in the *British Medical Journal* for May 21st, on Government Honours to Medical Science. Starting with Faraday's reply, when consulted by the Government of the day as to the propriety of a more liberal distribution of titles and other honours amongst men of science, that "Government should, *for its own sake*, honour the men who do honour and service to the country," the writer shows how this principle might be carried out by the appointment of Medical Sanitary Inspectors for the whole country, instead of merely for large towns; of State Inspectors of Civil Hospitals; and other similar arrangements.

WE quote from the *Moniteur Scientifique*:—"M. R. Wolf, of Zürich, has just published the *résumé* of his observations on the solar spots made since 1864. The minimum occurs in 1867, and agrees with the period of $11\frac{1}{9}$ years, found by Sabine and himself. Designating the relative frequency of the spots by r, M. Wolf expresses the variation of the magnetic declination at Christiania by the formula $v = 0'\cdot0413r + 4'\cdot921$, which does not, however, completely agree with observation."

WE quote the following from the *Pall Mall Gazette* for the benefit of our Darwinian readers:—"Two new birds have arrived at the Zoological Gardens the alleged habits of which afford a curious theme for speculation, and serve to supply an illustration to poets and philosophers. The male has a strong, short, curved beak; the female, a much longer bill. The naturalists tell us that the male breaks open the bark of the tree, within which lies hid the grub on which they feed; and the female pulls out the worm and presents her mate with half the meal. Here is a delightful instance of the essential incompleteness and mutual helpfulness of the sexes, the two forming one, as we are told they should, in perfect conjugal union. We hope that observation may confirm the tale; but animals at the Zoological Gardens are painfully apt to disappoint the expectations which we have been led to form of them. There is the aye-aye, for instance. Every one has heard of its marvellously long nail, and its singular adaptation to the necessities of the creature's existence. Professor Owen has founded an exquisite argument on the use of the long nail in extracting the creature's food from the deep crevices in which it is supposed to find it. It is an admirable instance of design. But although all sorts of ingenious devices have been adopted to induce the aye-aye to use its nail for these purposes, it seems to have a rooted objection to do so, and has never been known to do anything else than scratch its nose with it, which nobody can suppose to be a final cause."

WE have received from the Colonial Government of Victoria a copy of the Reports of the Mining Surveyors and Registrars for the quarter ending 31st December 1869, containing the Gold Mining Statistics for the quarter, the estimated yield of gold and quantity of gold exported; the summary of yield of gold from quartz, quartz tailings, &c., crushed; and the number and distribution of miners on the gold fields of the Colony on the 31st of December.

THE Botanical Garden at Rotterdam is about to be suppressed by the communal administration. M. Rauwenhoff, the director, and M. Witte, the head gardener, have given this information to their European *confrères*.

A JOURNAL of Horticulture has been started in Portugal under the title of *Journal de Horticultura Pratica de Portugal*, by M. José Marques Loureiro. M. Welwitsch has shown how wide a field for further exploration by botanists still remains in the Portuguese settlements in Africa.

A NINTH supplement to the Annals of the Munich Observatory is published, containing the particulars of 4793 telescopic stars between -3° and -9° declination, together with the observations of Lalande, Bessel, Rümker, and Schjellerup.

THE ABRADING AND TRANSPORTING POWER OF WATER

III.—PRACTICAL CONCLUSIONS

HAVING on two former occasions, when treating of the abrading and transporting power of water (which is supposed to increase as the velocity increases, but to decrease as the depth increases), dwelt on the mechanical property of water, and shown how it rolls rather than slides: the following conclusions may be arrived at:—

I. That all particles of water have an affinity to each other as well as to other bodies, and that force is required to separate them;

II. That friction sets these particles rotating in all directions in larger or smaller circles, and that the friction or force increases in some proportion to the area of surface exposed;

III. That this rolling motion becomes rarer the larger the diameter of the circles may be, that is, the resistance decreases as the depth and breadth of a stream increase, or in other words, the velocity increases proportionally to the "hydraulic mean depth;"

IV. Lastly, that any increase to the rapidity of this rotatory motion, must increase the abrading and transporting power of water, by enabling it to remove from the channel of a stream grains of solid matter, and hold them in suspension.

The following deductions are arrived at:—

1. That a smooth surface offers the smallest area for the water to attach itself to, and fewer irregularities; consequently the rotatory motion given to the water is reduced to a minimum, that is, the power expended is least, or the friction among the particles of water flowing through a smooth uniform channel is less than when it flows through an irregular and rough one.

2. That in the lines of a ship not only should there be no sudden changes in direction, but the surfaces should be as smooth as possible.

3. That the area of this surface should be as small as possible; hence convex lines are preferable to concave ones, as with the same area they afford greater buoyancy, while there would be less friction for the water to roll along a convex surface than a concave one.

4. That additional length given to a ship, leaving out all other questions, must retard a ship passing through the water, by increasing the area exposed to friction; consequently there is probably some limit owing to this increased resistance, where the length midships should not exceed certain proportions of the midship section.

5. That a ship passing over shallow water must be retarded, as the diameter of the vertical circles revolving under her bottom must be less than the diameter of the circles where the water is deep; hence the smaller circles will be set in quicker rotation, and therefore loss of power ensues.

6. That the same will be the effect from the same cause where the channel is narrow and contracted.

These deductions apply to cases where the abrasion may be considered "nil," such as the discharge of water through pipes, and the sailing of ships; and the practical conclusion is, that for pipes with glazed surfaces, and ships having coppered bottoms, the water passes with the least friction.* In the case of ships, *speed* is not the only question to be considered, so the subject becomes very complicated, and though believing in the general soundness of the above deductions, the solution of these problems may be left to the naval architect to consider.

Viewing the subject on the large scale, very important conclusions are arrived at from these facts—namely, that the depth of a river depends on the nature of the materials it has got to transport; thus those which have to carry down coarse sand should be broad and shallow, while those which have to convey fine mud would naturally be narrow and deep. And as this depends on the geological nature of the catchment basin of the river, are we not naturally led to the conclusion that where we find rivers navigable, the rocks of the catchment basin which predominate are of an aqueous formation, while those rivers which are difficult of approach from the sea must drain a country where crystalline rocks predominate? Judging, therefore, on this hypothesis, are we not right in conjecturing that the rocks of Central India, and also of that vast, but hitherto almost unexplored, country, Central Africa, must be generally of a crystalline nature?

This interesting question, however, like that of the best form of ships, had better be left for the consideration of the professional inquirer, whose investigations lead him to study that branch of geology which treats of the denudation of rocks now going on on the earth's crust, and the deposits now being formed. I pass on to those questions which affect the hydraulic engineer, and they are so numerous that it would be difficult even to enumerate and classify them, while their importance is so great that it can hardly be over-estimated. On this occasion only one or two of the more prominent subjects will be glanced at, more for the purpose of leading to future investigation, than to lay down rules for guidance, which at this present stage it would be premature to attempt.

From the foregoing remarks, suggestions, and deductions, it may be supposed that there are certain laws of nature which adapt each case to its own particular circumstances. Take, for example, the course of a river. It has been before said that streams which have to transport coarse, solid matter, such as sand, are usually broad and shallow, while those which convey chiefly fine mud are deep and narrow. The reason why those streams which convey a large proportion of sand should be broad and shallow, is that the water has thus sufficient power to hold the solid matter in suspension; and to still further aid them in this, it will be often observed that Nature generally gives such rivers comparatively speaking straight channels, in comparison to those which convey fine mud. The object in this case would appear to be that Nature in the former instance takes the shortest route, so as to obtain as great a fall as possible in the bed of the stream; while with the deep muddy stream, to prevent the water rushing off too fast, and so to keep up the surface of the stream to a level with the banks, or in floods often above them, Nature takes her tortuous courses, and is thus enabled year after year to deposit those fine grains of mud which add so much to the fertility of the soil.

So evidently is this the case, that in Egypt the irrigation canals are all carried in a zigzag direction, so as to check the velocity, and prevent the coarser particles of solid matter from being transported, while at the same time the surface of the water is kept at a sufficient elevation, so as to admit of easy irrigation. Thus, probably, Joseph, or whoever started irrigation in the land of the Pharaohs, took a leaf out of Nature's book; and it is by the study of this volume that the engineer of the present day will be most certain to arrive at satisfactory results.

That rivers have certain general principles by which they are governed, as to breadth, depth, slope, velocity, and load of solid matter held in suspension, it appears reasonable to suppose; and any change introduced in any

* On reading over the above conclusions to an experienced ship-builder here in London, he said that one of the reasons why Aberdeen clippers sailed so fast, was owing to the smoothness of the ships' bottoms, which were first planed before the copper was put on. He also remarked that where the copper is very smoothly put on, the first place where the sheet wears through is just behind where the sheets overlap, showing that even an irregularity of $\frac{1}{16}$ of an inch causes an extra action, not as might be supposed at the point of greatest obstruction, but just beyond it, proving that there must be this whirling motion which causes this abrasion. This gentleman also observed that experience showed that the speed of a ship chiefly depends on the fineness of the lines of the after rim of a ship, and that "ingoing" or concave lines should be avoided if possible.

one of these proportions must cause a corresponding change in one or all of the above conditions.

Thus, let there be a stream which in flood contains 5 per cent. by weight of solid matter, and let it be 8 ft. deep, discharging 50,000 cubic feet a second, with a mean velocity of $7\frac{1}{2}$ ft. a second—the breadth of this stream would be $\frac{50,000}{8 \times 7\cdot5} = 833\frac{1}{3}$. To add one-fourth to the discharge of this stream of pure water would increase the discharge from 50,000 to 62,500 cubic feet a second, and the proportion of solid matter, instead of being 5 per cent., would be only 4 per cent. But, by the example before given, a depth approaching 9 ft. instead of 8 ft. would be the natural depth, so the bed would be lowered 1 ft., and the breadth would only be increased $\frac{12,500}{2 \times 9 \times 7\frac{1}{2}} = 92$ ft., instead of having to give an increase of one-fourth more, or $\frac{833}{4} = 208$ ft., and still keep matters much as they are found in nature.

So in bridging such a stream the whole additional length of viaduct would be only 92 ft., or 116 ft. would be saved by simply sinking the foundations 1 ft. more. But as water rolls rather than slides, and never flows in straight lines, the shape of the section of a stream can be changed at pleasure; so the depth may be increased without much danger by decreasing the breadth. Taking, then, the same stream which it was proposed to make $833 + 92 = 925$ ft. broad by 9 ft. deep, suppose the mean depth be made 15 ft., this increase of depth would decrease the transporting power, while at the same time the velocity would also be increased; and suppose it to be now 10 ft. a second, instead of $7\frac{1}{2}$ ft., and with a depth of 15 ft. with such a velocity only 4 per cent. of solid matter could be held in suspension, and the waterway would be $\frac{62,500}{15 \times 10} = 416\frac{2}{3}$ ft. broad instead of 925 ft., or less than one-half. That is, by adding 8 ft. to the general depth of a stream where the river discharges 50,000 cubic feet a second in the main channel, and 12,500 cubic feet a second of inundation water, which is comparatively free of silt, the whole volume or 62,500 cubic feet could be passed through a bridge only half the breadth of the original stream, which was 833 ft. broad. So the whole question reduces itself now into one of cost.

The question is whether it be cheaper to sink the foundations an extra 8 or 10 ft., or to double the length of viaduct. By the use of the sand-pump foundations can now be sunk through sand at a very moderate cost; so it is believed that the extra sinking would not involve anything like the cost of the shallower foundations for the bridge built on the extended plan; thus the whole cost of the superstructure and extra girders could be saved—that is speaking approximately. In such a case the bridge built on this deep foundation principle could be built at nearly half the cost of the old plan; but to guard against accidents, and scooping out to excessive depths, it would appear that at least one-third may be saved by building bridges on this principle; while the river, by having a deep channel under the bridge, could be kept in better control than by the present extended method, as it would not have such a tendency to desert its course, but would always keep to the deep channel.

Several other examples may be brought forward to illustrate the practical advantages that a better knowledge of the action of flowing water would be sure to confer on science and hydraulic engineering; but it is hoped that the foregoing will assist in bringing the importance of the subject more prominently forward. When once the subject is properly discussed I am convinced its importance will be manifested.

T. LOGIN

SOCIETIES AND ACADEMIES

LONDON

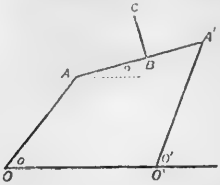
Royal Society, May 19.—"Experiments on the Effects of Alcohol (*Ethyl Alcohol*) on the Human Body." By E. A. Parkes, M.D., F.R.S., Professor of Hygiene in the Army Medical School, and Count Cyprian Wollowicz, M.D., assistant-surgeon, Army Medical Staff.

"On Deep-sea Thermometers." By Staff-Commander John E. Davis, R.N. Communicated by Captain Richards, R.N., Hydrographer of the Admiralty. The results of thermometric observations at great depths in the ocean not being of a satisfactory nature, the attention of the hydrographer of the navy was directed to the defects in the construction of the Six's self-registering thermometers then in use, and also to the want of knowledge of the effects of compression on the bulb; and as it was known that a delicate thermometer was affected *in vacuo*, it was natural to suppose that an opposite effect would be had by placing them under pressure, and particularly such as they would be subjected to at great depths. Several thermometers, of a superior construction, were made by different makers, and permission was granted to make experiments by pressure in a hydraulic press; but much delay was caused by not being able to obtain a press suitable to the requirements, until Mr. Casella, the optician, had a testing-apparatus constructed at his own expense, and the experiments were commenced. Previous to the experiments being made, Dr. W. A. Miller, V.P.R.S., proposed, or rather revived, a mode of protecting the bulb from compression by encasing the full bulb in glass, the space between the case and the bulb being nearly filled with alcohol.* A wrought-iron bottle had been made to contain a thermometer, for the purpose of comparison with those subjected to compression; but it failed, and finally burst under great compression; it proved, however, of but little consequence, as those designed by Dr. Miller showed so little difference under pressure that they were at once accepted as standards. Two series of experiments were then most carefully made, at pressures equal to depths of 250, 500, &c., to 2,500 fathoms, the results of which satisfactorily proved that the strongest made unprotected thermometers were liable to considerable error, and therefore that all previous observations made with such instruments were incorrect. Experiments were also made in the testing-apparatus with Sir Wm. Thomson's enclosed thermometers, to ascertain the calorific effect produced by the sudden compression of water, in order to find what error, if any, was due to compression in the Miller pattern: an error was proved to exist, but small, amounting to no more than $1^\circ\cdot4$ under a pressure of 3 tons to the square inch. The dredging cruise of the *Porcupine* afforded an opportunity of comparing the results of the experiments made in the hydraulic testing apparatus, with actual observation in the ocean, and a most careful series of observations were obtained by Staff-Commander E. K. Calver at depths corresponding to the pressure applied in the testing apparatus; the result was that, although there was a difference in the curves drawn from the two modes of observation, still the general effect was the same, and the *means* of the two were identical. From these experiments and observations a scale has been made by which observations made by thermometers of similar construction to those with unprotected bulbs can be corrected and utilised, while it is proposed that by means of observations made with the Miller pattern in the positions and at the same depths at which observations have been made with instruments not now procurable for actual experiment, to form a scale for correcting all observations made with that particular type. In conclusion, it is suggested that to avoid error from the unsatisfactory working of the steel indices, which, from mechanical difficulties in their construction, cannot always be depended on, two instruments should be sent down for every observation; and although their occasional disagreement of record may raise a doubt, a little experience will enable the observer to detect the faulty indicator, while their agreement will create confidence.

London Mathematical Society, May 12.—Prof. Cayley, president, in the chair. The Hon. Sir J. Cockle, Chief Justice of Queensland, was proposed for election.—The President (Mr.

* *Vide* Proceedings of the Royal Society, vol. xvii. No. 113, June 17, 1869.

Spottiswoode, V.P., having taken the chair) gave an account of his paper "On the Mechanical Description of a nodal bicircular Quartic." Take a quadrilateral OAA′O′, in which the adjacent



sides OA, AA′ are equal to each other, and the other two adjacent sides OO′, O′A′ are also equal to each other. O, O′ are fixed points, and we have thus a link, AA′, the extremities of which are connected with the radii OA, O′A′, respectively, and consequently describe circles about the centres O, O′ respectively, the radius OA of the one circle being equal to the length AA′ of the link, and the radius O′A′ of the other circle being equal to the distance OO′ of the centres. The theorem is that any point C rigidly connected with the link AA′ describes a nodal bicircular Quartic; that is, a quartic curve with three nodes, two of the nodes being the circular points at infinity. Any such curve is the inverse of a conic, and it is also the antipode of a conic. For the analytical investigation, the origin is taken at O, the axis of x in the direction OO′, and the axis of y at right angles thereto, upwards from O. The inclinations of OA, AA′, O′A′ to the axis Ox are taken to be θ, ϕ, θ' respectively. Also write OA = AA′ = a, AB = b,

BC = c, OO′ = O′A′ = a^1, and make $m = \dfrac{a^1 - a}{a^1 + a}$ then

we have for the locus of C—

$$x = a \cos\theta + b \cos\phi - c \sin\phi$$
$$y = a \sin\theta + b \sin\phi + c \cos\phi;$$

since $\theta^1 = \theta + \phi$. Also we have $\tan\frac{\theta}{2} \tan\frac{\phi}{2} = m$;

whence writing $\tan\frac{\theta}{2} = u$, we have $\tan\frac{\phi}{2} = \frac{m}{u}$ and the equation to the locus may be written—

$$x = -a\frac{u^2 - 1}{u^2 + 1} + b\frac{u^2 - m^2}{u^2 + m^2} - c\frac{2mu}{u^2 + m^2}$$

$$y = a\frac{2u}{u^2 + 1} + b\frac{2mu}{u^2 + m^2} + c\frac{u^2 - m^2}{u^2 + m^2};$$

which equations show that the locus is a bicircular quartic. The author then proceeds to show the existence of a third node, to express the locus as the inverse of a conic, and to exhibit it as the antipode of a conic. In the discussion on the paper Mr. Roberts gave some additional results bearing on the subject; and Mr. Spottiswoode stated that he recognised many of the curves (exhibited by Messrs. Cayley and Roberts) as having come under his notice in the course of experiments he had recently made with elastic strings.

Mr. Roberts then read his paper "On the Ovals of Des Cartes." It will suffice in this notice to give the heads under which the author treated the subject. On the polar equation and its interpretation; on the description of the curves by the transformation of a circle; on certain systems of the curves; on the normals; properties deducible from the general interpretation of the polar equation. Under the last head Mr. Roberts stated some interesting results: the sum of the areas of the Ovals—that is to say, in an obvious sense, the area of the Cartesian is equal to twice the area of the circle whose centre is at the triple focus, and which passes through the points of contact of the double tangent; the lengths of the ovals of a Cartesian are expressed by Zyzygetic relations between two elliptic quadrants; the difference of the lengths of the loops of a nodal Limaçon is four times the distance between the vertices.—Dr. Henrici exhibited a plaster cast of the surface, $xyz - \left(\frac{3}{7}\right)^3 \left(x + y + z - 1\right)^3 = 0$.

Royal Astronomical Society, May 13.—W. Lassell, F.R.S., President of the Society, in the chair. Thirty-one presents were announced, and the thanks of the society were voted to the respective donors. Prof. Selwyn read a communication from Sir John Herschel, referring to a chart in which the sun's spots observed by Mr. Carrington during his long study of the sun have been referred to their proper solar longitude and latitude. Sir John Herschel's object in preparing this chart was to determine what signs there are that any particular portions of the sun within the sun-spot latitude are more liable than others to be the scene of spot-formation. He found very few signs of any such arrangement. Mr. Proctor remarked that Sir John Herschel's communication seemed to dispose effectually of Prof. Kirkwood's recently propounded theory in explanation of the periodicity of sun-spots.—Papers on the occultation of Saturn, as observed by Messrs. Talmage, Joynson, and Captain Noble, were then read. Captain Noble remarked on the singularly clear definition of Saturn, "right up to the moon's limb," as being strikingly opposed to the theory brought forward by Mr. Watson at the last meeting of the society.—A paper by Mr. Penrose, on the Sun-spots recently seen, led to some controversy about the real nature of the spots. Mr. Howlett dwelt on the exaggerated character of the saucer-shaped depressions usually shown in text-books of astronomy. Mr. De La Rue mentioned the preponderance of evidence in favour of the spots being depressions. The Astronomer Royal also spoke in favour of this view. Mr. Lockyer dwelt on the evidence bearing on the subject derived from his method of detailed spectroscopic examination, and mentioned the interesting circumstance that so far as his spectroscopic observations had hitherto gone, it seemed almost as though the depressions were shallower now the spot period was nearly at its maximum than they were a year and a half ago. The Astronomer Royal expressed his admiration of the persistence and patience with which Mr. Lockyer had continued his solar observations, and stated his belief that in the course of the next two or three years we should gain much more new knowledge from this application of spectroscopic analysis to the sun.—At the President's request, Mr. Airy then referred to the expedition to be made to observe the eclipse of December next. He stated that the names of about sixty volunteers had been received. According to present arrangements, as many as twenty-six would be wanted at each station for general, spectroscopic, polariscopic, and photographic work. He expressed his anxiety that the observers who were to take part in the observations on the polarisation of the corona's light should understand their work, and clearly recognise—1st, what polarisation is; 2nd, what polarisation in a plane means; and, 3rd, when light *is* polarised in a plane how to recognise that plane. Mr. De La Rue spoke very favourably of the list of volunteers, saying that it included just those classes of observers which were really wanted. It was mentioned that the Poet Laureate had volunteered; and, in answer to a question by Prof. Grant, that good draughtsmen would accompany the expedition.—A paper by Mr. Lynn, on a star in Ursa Major, which has a very large proper motion, according to the researches of Argelander, was then read.—Mr. Proctor read a paper, "On the Resolvability of Star-groups regarded as a Test of Distance," dwelling on the evidence we have of variety of constitution as at least equally available to explain differences of resolvability as variety of distance can be. Mr. Stone said he thought Mr. Proctor exaggerated the theory of uniformity of stellar distribution; in reality, astronomers regarded the existence of very various degrees of real magnitude among stars as not only possible but probable, or even certain. Mr. Proctor said he was fully aware of that, and his views touched on quite a different matter. What he pointed to as wrong in accepted theories was that those theories failed to recognise the existence of definite aggregations of stars, in streams, clusters, groups, and so on, showing all degrees of richness of aggregation, &c.—Mr. Williams then described some antique telescopes of Campani's construction, amazingly long, with object-glasses varying apparently from about half an inch to an inch in diameter.

Zoological Society, May 12.—Prof. Newton, vice-president, in the chair. The secretary read some notes on the principal additions to the Society's menagerie during the month of April, and called particular attention to a Vulturine Guinea-fowl (*Numida vulturina*), presented by Dr. John Kirk, C.M.Z.S., being the first living specimen of this rare species received in England.—A communication was read from Dr. R. O. Cunningham, C.M.Z.S., on some peculiarities in the anatomy of three kingfishers, *Ceryle stellata*, *Dacelo gigas*, and *Alcedo ispida*.—A communication was read from Mr. George Gulliver, F.Z.S., on the taxonomic characters afforded by the muscular sheath of the œsophagus in sauropsida and other vertebrates.—Mr. R. B. Sharpe read a paper containing a full account of the swallows

(*Hirundinidæ*) of Africa, and pointed out their characters and geographical distribution. Particular attention was drawn to the affinities of the African *Hirundinidæ* with those of the New World, and also to the representation of various species by smaller races or sub-species throughout the Æthiopian region. Thirty-eight species of swallows were enumerated, of which number thirty were stated to be peculiar to the continent of Africa, and two to Madagascar and the adjacent islands. Two species only were common to India and Africa, and the remaining four were migratory throughout the Palæarctic and Æthiopian regions.—Dr. O. Finsch, C.M.Z.S., communicated the description of a new species of penguin in the collection of the Counts Turati, of Milan, which he proposed to call *Dasyrhamphus herculis*.—Messrs. P. L. Sclater and O. Salvin read descriptions of seven new species of birds collected by Dr. Habel during a recent expedition to the Galapagos Islands. These new species were mostly from Bindloes Island and Abingdon Island, which had not been visited by former explorers, and belonged principally to a peculiar group of *Fringillidæ*, containing *Geospiza* and its allied forms, which is characteristic of the Galapagoan Archipelago.—Mr. P. L. Sclater, F.R.S., read a paper on some new or little known species of South American birds, amongst which was a new woodpecker, proposed to be called *Melanerpes ulcher*, from New Granada.—Prof. Flower, F.R.S., communicated some additional notes on the specimen of the common fin whale (*Physalus antiquorum*) recently stranded in Langston Harbour.—Prof. Newton read a paper "on *Cricetus nigricans* as an European species," and exhibited a specimen of this mammal which had been lately killed in Bulgaria by Mr. T. E. Buckley, F.Z.S., and had been presented by him to the Cambridge Museum.

Leeds

Philosophical and Literary Society, May 3.—At the annual meeting, being the close of the fiftieth session of the Leeds Philosophical and Literary Society—Dr. Heaton, the retiring president, in the chair—it was announced that the following gentlemen had been elected as officers, council, and members for 1870-71:—President, John Deakin Heaton, M.D., F.R.C.P.; Vice-Presidents, C. Chadwick, M.D., F.R.C.P., Thomas Nunneley, F.R.C.S.; Treasurer, Henry Oxley; Honorary Secretaries, Thomas Wilson, M.A., Richard Reynolds, F.C.S.; Honorary Curator in Geology, J. G. Marshall, F.G.S.; Honorary Curators in Zoology, Edward Atkinson, F.L.S., T. C. Allbutt, M.A., M.D., F.L.S., F.S.A.; Honorary Curator in Ethnology and Works of Art, &c., Thomas Nunneley, F.R.C.S.; Honorary Librarian, John Deakin Heaton, M.D., F.R.C.P. The above officers, and the following gentlemen, compose the Council:—John Edwin Eddison, M.D., E. Filliter, M. Inst. C.E., F.G.S., T. M. Greenhow, M.D., Joshua Ingham Ikin, F.R.C.S., Rev. J. H. M'Cheane, M.A., John Manning; T. W. Stansfeld, Rev. H. Temple, William Sykes Ward, F.C.S., Rev. Canon Woodford, D.D., General Curator and Assistant Secretary, Henry Denny, A.L.S.

Paris

Academy of Sciences, May 16.—The following mathematical papers were read:—Researches on pencils of right lines and normals, containing a new exposition of the theory of the curvature of surfaces, by M. A. Mannheim, communicated by M. Bertrand; a note on a peculiar property of the cassinoid with 3 foci, $p^6 - 2\,mp^3 \cos 3\,\theta = \pm 1$, by M. Allegret; and a note on the solution of the problem consisting in finding three whole numbers, such that the square of the one shall equal the sum of the squares of the other two. M. Bertrand also presented a report on a memoir by M. Moutard on the theory of partial differential equations of the second order.—M. Lecoq de Boisbaudran presented some remarks on the spectra of nitrogen, in which, after noticing his observations on this subject, he concluded that the changes of the spectra of this, and probably of other bodies, depend upon variations of temperature rather than of pressure, a circumstance which ought to lead to great caution in the astronomical application of spectrum analysis.—M. Regnault presented a note on the maximum of density and the temperature of congelation of solutions of alcohol in water, by M. F. Rossetti. These temperatures decrease in proportion to the amount of alcohol in solution.—M. Leray communicated a theory of the elasticity of media deduced from the principle that equal currents crossing in all directions exist in the midst of ether when not influenced by surrounding bodies; and M. Delaurier presented a description of a battery for ringing telegraphic bells.—A communication from Father Secchi, containing the rectification of a numerical error in his last paper, was read, and in connection therewith M. Fizeau communicated some remarks on the displacement of the spectral rays by the movements of the luminous body or of the observer. A memoir on the theory of the tides, by M. Roumiantzoff, was read; also a note by M. Trémaux on some questions relating to the movements of the planets.—M. Guyon presented a note on a meteorological observation made by the washerwomen of the south coast of Spain. The observation was that with a southerly wind linen put out to dry always acquires a yellow colour; the author accounted for it by assuming that this coloration is due to impalpable dust conveyed from the African deserts.—M. A. Wurtz described a process for obtaining solid cresole, and some curious properties possessed by that body.—M. F. M. Raoult communicated a note on the composition of the gas of the burning spring of Saint Barthélemy (Isère) It consisted chiefly of marsh gas, 98·81 per cent. by volume, with small quantities of carbonic acid, nitrogen, and oxygen probably accidentally mixed with it.—M. Guyot presented a note on the examination of ammonia and nitric acid by means of rosolic acid and bromomercurate of potash.—M. Belgrand communicated a general account of the contents of his great work on the basin of Paris in prehistoric times, which has lately been published by the municipal administration of Paris.—M. Farez recommended the employment of silicate of potash for the purpose of giving solidity and cohesion to friable fossil bones.—M. Brongniart presented a report on a memoir by M. B. Renault, on some silicified plants of the neighbourhood of Autun.—A note was read from M. Marey, admitting that Dr. Pettigrew had the priority over him with regard to the figure of 8 described by the wing of an insect during flight; and M. A. Dumeril presented a note by M. S. Legouis on the pancreas of the osseous fishes and the nature of the vessels discovered by Weber. The author described three forms of pancreas occurring in osseous fishes, which he called the *disseminated*, *diffused*, and *massive* pancreas; the vessels discovered by Weber are the excretory ducts of the first two forms. The Plagiostomi have a pancreas resembling that of the higher animals.—A note from M. Didierjean was read, calling attention to the fact that milk is a preservative from the poisonous effects produced by lead upon the workmen who are engaged in the preparation of its compounds.—M. E. Decaisne presented a note on the use of the sewing machine and its influence on the health of workwomen. He considers that the ill effects of working with the sewing machine have been greatly exaggerated, and that the health of women working with the machine is quite as good as that of needlewomen.

Berlin

German Chemical Society, April 25.—H. Limpricht published a paper on oxyde of toluylene, to which he ascribes the formula $C\,H_2\,O\,C\,(C_6\,H_5)_2$.—M. Topsoë sent in a paper on preparing concentrated bromhydric acid, and a table on the boiling points of hydrobromic acids of different concentration.—A. Claus reports on the constitution of acroleïne, looked upon by Kolbe as an acetone, on account of its transformation by hydrogen into isopropylic acid. This Prof. Claus denies to be the case; the alcohol obtained forming no acetone by oxidation.—W. Henneberg has found a source of error in using Pettenkofer's apparatus for determining the products of respiration; the respiration of people approaching the apparatus influencing the amount of carbonic acid.—H. Schiff has found that acetic acid and oil of bitter almonds form cinnamic acid when treated with a minute quantity of HCl or $Zn\,Cl_2$. A larger quantity of these substances transforms the acid into metastyrol.—A. Oppenheim recommends the action of bibromide of copper for transforming organic iodides into bromides. With an alcoholic solution of this salt and iodide of allyle, the transformation is instantaneous, protoiodide of copper being precipitated at ordinary temperatures. An aqueous solution acts in the same way at a higher temperature. By the transformation of the bibromide into the protoiodide of copper, however, bromine is liberated, which will combine with non-saturated bromides. The method is, therefore, only applicable to saturated compounds.—C. Rammelsberg reported on isomorphous mixtures of selenium and sulphur.

May 9.—V. Meyer, in order to decide whether the hydrate of chloral is a molecular combination—thus, $C\,Cl_3 - C = O\,H + H_2O$—or whether the water enters into the atomic constitution of the body—thus, $C\,Cl_3 - C\,H\,(O\,H)_2$—has tried its action on chloride of acetyl. H_2O is thereby separated, and one molecule of chloride of acetyle takes its place. The first supposition, therefore, appears the more probable of the two.—P. Groth has entered into a long series of important and difficult researches to investigate the connection between the chemical constitution and crystalline form of organic bodies. He communicates his first results, founded on his measurement of the form of benzol during the great cold of last winter, and comparing with it the forms of benzolic derivatives, in which one or more hydrogen are replaced by O H, N O_2, or Cl. He has come to the conclusion that, by these substitutions, *the numeric relations of two axes remain intact, the third axis only increasing or decreasing with the chemical substitution* The influence thus exercised by certain elements or groups taking the place of hydrogen, he calls their *morphotropic* power. The morphotropic power of chlorine appears far greater than that of N O_2 or of O H. The morphotropic changes, however, depend likewise on the position the element or group occupies in the molecule, and on the crystallographic system of the primary substance. The author has also investigated certain combinations of naphthalene in the same sense. They bear out the law quoted above. For the conclusions drawn from these highly interesting results we must refer to the original paper.—M. Lex has found that phenol mixed with nitrite of potassium and a reducing substance (such as sugar and lime, or hydrochloric acid and zinc), and then exposed to the air, or the oxidising action of chloride of lime, gives rise to a blue colour, much like indigo, but very unstable.—A. W. Hofmann reported on the curious researches of Prof. Church on the red colouring matter of the feathers of the Turacon.—C. Rammelsberg has analysed Indian steel, or wootz, without finding in it a trace of aluminium.—M. Topsöe has analysed platinic acid and platinate of barium, to which he gives the formula: Pt O_2, 2 H_2 O and Ba Pt O_3 + 3 H_2O respectively.—M. Clemenks, by treating the impure choride of pyroracemic acid with alcohol and with ammonia, has obtained the corresponding ether and amide.—MM. Kékulé and Quinke report on some reactions of metaldehyde and of paraldehyde.—M. Czumpelik has prepared the cyanide of nitrobenzyle and of amidobenzyle, and some derivatives of cuminic and of oxycuminic acids. The same author, by introducing one atom of chlorine into cymol, and treating the compound with acetate of potassium, obtained the corresponding organic acetate. —Mr. Buchanan, from Glasgow, has studied the complex action chloride of phosphorus shows with hyposulphite of lead.

Montreal

Natural History Society, April 25.—The Rev. Dr. DeSola presiding. Dr. Smallwood read the first paper, "On some phenomena of the Solar Eclipse of August 1869." It was intended to illustrate more fully a paper which he had contributed to the *Canadian Naturalist*, referring to the rose-coloured prominences of the sun's chromosphere, and their appearance before first contact. He exhibited diagrams of the several eclipses of 1851, 1860, 1868, and 1869, which showed the various shapes of the protuberance, and referred more particularly to the large one observed during the eclipse of last August, some 30,000 miles high, which was seen in a direct line with the passage of the moon across the sun's disc. He attributed the appearances which were observable a few seconds before the first contact of the Moon with the Sun's true limb to this circumstance, and cited and illustrated the experiments of Mr. J. N. Lockyer and Janssen, in confirmation of this opinion. The remarks of the Astronomer Royal, on the causes which, up to 1861, had prevented these prominences being seen, except during a solar eclipse, were quoted from the Transactions of the British Association. The spectroscope and other optical appliances have now made it possible to examine these phenomena at any time when the sun is shining. The experiments of Lockyer and Janssen seem fully to bear out the conclusions to which Dr. Smallwood had arrived, and the lecturer ended by expressing a hope that observations made on the eclipse of next December would tend to illustrate further the somewhat unusual appearances which he had recorded.—The Acting President made some remarks on the above paper, and expressed his regret at the want of good astronomical instruments in the city.—Mr. A. S. Ritchie then read a paper entitled "Aquaria Studies, No. 1." After some preliminary remarks, the principles upon which a fresh-water aquarium should be constructed and stocked were explained in detail, and particulars were given as to how the balance between animal and vegetable life might be best maintained. The author went on to describe some of the peculiarities of the larger tenants of his own aquarium. Commencing with the fishes, the various points of interest connected with the habits of several of the smaller Canadian fresh-water fishes were dwelt upon at some length. The species described were a new Stickleback, lately described by Principal Dawson in the pages of the *Canadian Naturalist*; the Darter, a fish which has no air-bladder, and swims by jerks; the Striped Minnow, the Sun-fish, American Perch, Black Bass, Cat-fish, Pond-sucker, the Black Minnow, a species allied to the Pike; and, although not a Canadian species, the Gold-fish. Illustrations were also given of the behaviour in captivity of the Painted Turtle, the Water Newt, the Shad Frog, and the American Crayfish. In conclusion the lecturer stated that in part No. 2 of Aquaria Studies, he hoped to give descriptions of the microscopical denizens of his miniature pond.

DIARY

THURSDAY, May 26.

Society of Antiquaries, at 8.30.—Election of Fellows.
Zoological Society, at 8.30.—On Dinornis (Part xvi.), containing Notices of Internal Organs of some Species, with a Description of the Brain and some Nerves and Muscles of the Head of the Apteryx australis: Professor Owen, F.R.S.—Notes on the Anatomy of the Prongbuck (Antilocapra americana): Dr. J. Murie.—Some Remarks on the Poison Glands of the Genus Callophis: Dr. A. B. Meyer.—Notes on some Fishes from the Western Coast of India: Surgeon Francis Day.
Royal Institution, at 3.—Electricity: Prof. Tyndall.

FRIDAY, May 27.

Royal Institution, at 8.—Primitive Vegetation of the Earth: Principal Dawson.
Quekett Microscopical Society, at 8.

SATURDAY, May 28.

Royal Institution, at 3.—Comets: Prof. Grant.

MONDAY, May 30.

London Institution, at 4.—Botany: Prof. Bentley.

TUESDAY, May 31.

Anthropological Society, at 8.—On the Armenians of Southern India: Dr. John Shortt.—The Races of Morocco: J. Stirling, M.A.
Royal Institution, at 3.—Present English History: Prof. Seeley.

WEDNESDAY, June 1.

Ethnological Society, at 8.30 (at the Royal United Service Institution, Whitehall Yard).—Report on the Prehistoric Antiquities of Dartmoor: C. Spence Bate.

THURSDAY, June 2.

Royal Society, at 8.30.
Society of Antiquaries, at 8.30.
Chemical Society, at 8.—On the Platinum Ammonias: Dr. Odling.
Linnean Society, at 8.—On some New Forms of Trichopterous Insects.
Royal Institution, at 3.—Electricity: Prof. Tyndall.

BOOKS RECEIVED

English.—On the Strength of Beams, Columns, and Arches: B. Baker (E. and F. Spon).—Flint Chips, a Guide to Prehistoric Archæology: E. T. Stevens (Bell and Daldy).—The Interior of the Earth: H. P. Malet (Hodder and Stoughton.)

Foreign (through Williams and Norgate).—Palæontographica, Beiträge zur Naturgeschichte der Vorwelt; Supplement (Die Fauna der ältern Cephalopoden): R. A. Zittel.—Protozoe Helvetica: W. A. and C. F. Ooster.

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THURSDAY, JUNE 2, 1870

WHENCE COME METEORITES?

M. STANISLAS MEUNIER, the *collaborateur* of M. Daubrée, at the *Jardin des Plantes*, has worked with much assiduity during the past two years at the analysis of certain of the meteorites contained in the now important collection of these bodies in Paris; and in a recent number of this journal an account was given of a view propounded in *Cosmos* by M. Meunier as an answer to the question, Whence come Meteorites?

Whether M. Meunier's theoretical conclusions are to be looked on with the same favour that we heartily accord to his practical work is what we are about to discuss.

Briefly stated, his view is, that the character of the meteorites that fall on the earth has, during the short period embraced by human history, undergone a change. He supposes that formerly—by what he deems to have been a providential arrangement for the supply of metallic iron to our earliest ancestors, ere metallurgy had become an art—the meteorites that fell were of iron. Subsequently, and in our own particular age, stony meteorites have been descending to us of what M. Meunier terms the *type commun*. We may, perhaps, more accurately describe them as fine grained mixed rocks often presenting spherular structure, and consisting of magnesian and ferro-magnesian silicates, associated with small quantities of augitic and felspathic minerals, nickeliferous iron, and ferrous monosulphide (troilite), the last two sporadically disseminated in variable amounts.

These, then, are the meteorites that are falling now. Already M. Meunier thinks he sees the beginning of a new order of meteoric falls, though he does not give a single fact to show that such is the case, in the occasional visit to our earth of what he calls "the lavas" of the type of the meteorite of Stannern (which fell 22nd May, 1808), and of the carbonaceous meteorites, like that of Cold Bokkveldt (of 13th October, 1838). The former of these two very dissimilar kinds of meteorites consists, we may state, chiefly of a mixture of augite and anorthite, with a scarcely discernible amount of nickel-iron; in the latter kind graphite and solid hydro-carbon, strange to say, are met with, mixed with enstatite or olivine. These M. Meunier tells us are to be the meteorites of the immediate future. We fail, however, to see that these are less our contemporaries than are those of the *type commun*, except, indeed, that there are fewer of them. After they have had their day, our children may begin to look out for granitoid rocks, and perhaps even for portions of stratified deposits; and if we are disposed to ask "What next?" we must push M. Meunier's hypothesis to the logical conclusion his modesty seems to shrink from, and leave it to our remoter descendants to search diligently among the meteoric falls of their time for the fossil relics of organisms that may once have flourished on a now demolished world—providentially, let us suppose, reserved till those latter days, that they may reveal the answer to that keenly debated problem which Sir David Brewster linked with the hopes of the philosopher and the faith of the Christian!

For M. Meunier supposes meteorites to be the shattered morsels of a satellite smaller than and perhaps subordinated to the moon, which has run its course and been broken up by those internal throes of volcano and earthquake that form the true "defectus solis varios lunæque labores" which are in turn, according to M. Meunier, to break up Moon, and Earth, and Sun himself.

The fragments of this satellite he believes to be now careering in every direction, retrograde as well as direct, around our world, and gradually falling into its surface; the iron masses, as possessing the greatest specific gravity, having already descended; the rest following and to follow in the order of their densities. Certainly, we may observe, *in limine*, that this satellite must have been a very minute one, or its metallic ingredient in very small proportion to its other materials, if the iron meteorites that have reached the earth in historic times represent in any considerable proportion the amount of that ingredient. And it is not very easy to see what is to bring to the earth masses of matter, however small, moving in orbits round it, unless it be the retardation caused by their coming into contact with its atmosphere; an influence that would of course act in precisely the inverse manner to that assumed by M. Meunier, as the masses of lowest specific gravity would be the first to succumb to it.

But what are M. Meunier's grounds for this hypothesis? Does he explain anomalies in the moon's motion by it? Or does he rest it on what would certainly be at least a plausible ground for linking the meteorites by some close bond of attraction and direction of motion with the earth—on the fact, namely, that over one region in India, near to the greatest mountain protuberance on our globe, the recorded falls of meteorites are more numerous than on any other spot? a fact, however, that we believe has an altogether different explanation. M. Meunier does not even hint at such arguments as these. He goes to tradition, to a fragment of Æschylus, and to such comparatively modern evidence as that of an Icelandic Saga. In the former allusion is made to the well-known stone-strewn plain of the Crau, north-west of Marseilles, which according to the ancient myth was the scene of the contest between Hercules and the Ligures, when Zeus rained down a shower of stones to aid the hero. Now, if a myth venerable four centuries before Christ has any bearing on the question, this passage would certainly seem to hint that the *grêle de galets*, which M. Meunier quotes from Bouillet's translations (in the original it is νιφάς στρογγύλων πέτρων), records a familiarity in those ancient days with falls of stones rather than of masses of iron, and certainly the quotation M. Meunier gives from a translation of the Edda might embody an allusion at least as happily to the aurora or to an eruption of Hecla as to a fall of meteorites. M. Meunier does not allude to the meteorite of Troy or of Ægospotamos or to the image of the Ephesian Artemis; and these were surely stones. So was the meteorite of Emesa, and if it be meteoric, such must be that other venerable fetish the Caaba stone. We need not perhaps discuss M. Meunier's wonderful attempt to connect by etymology the Latin *sidus* with the Greek σίδηρος, if this is what he implies by saying that σίδηρος had the double meaning of a star and of iron. But dismissing these cloudy reasonings, we may consider two other arguments brought forward by M. Meunier. One of these consists in the paucity of the number of iron meteorites that have been seen to fall as compared with that of stones of which the falls were witnessed: the proportion being only about 4 or 5 per cent. Thus in the collection at the British Museum we have 106 specimens of iron (siderites and siderolites together) indubitably meteoric, of which four only were seen to fall, while there are 179 of meteoric stones, of which only five have *not* been seen to fall. Surely, however, we have a sufficient explanation of this in the nature of the bodies themselves. The one, a mass of solid iron, besides possessing far greater permanence than a soft and porous rock permeated by small particles of readily rusting metal, would, if lying on the ground, be at once recognised by any one familiar with the metal, and would be preserved for use or as a curiosity; while the meteoric stone so found would equally naturally be neglected, unless the finder knew a meteorite as well as M. Meunier. Hence the iron meteorite is both better preserved and more surely recognised; and hence though its fall is a far rarer event in nature than that of the meteoric stone, our collections are comparatively rich in iron meteorites. That the American Continent has furnished so many meteoric irons to our collections is, no doubt, due to the ignorance of the uses of iron on the part of the ancient inhabitants of that continent, and to the comparatively unpeopled nature of the country. It is in the United States, and scattered over the plains and valleys of Mexico, or lying unrusted under the clear dry air of the Cordilleras of the Andes, that most of these iron masses have been found. They afford an ocular proof that, though after a longer or shorter time such irons must sink into a shapeless mass of oxides, yet under favourable conditions they can and do last through long generations before this destructive process is consummated. Indeed, if they do not, what becomes of M. Meunier's main argument? And if they do, the anomaly of their coming into our collections, while the stones, the fall of which has not been witnessed, are absent, does not seem so inexplicable.

M. Meunier advances another argument in support of his theory founded on the similarity of composition of certain meteorites that he has examined, and in which he recognises what he terms a stratification of different recurring varieties.

Assuming the correctness of this statement, we fail to see its logical connection with his theory. It is no new fact in the mineralogy of meteorites, whether of stone or iron, that the same minerals and combinations recur in them, and that certain of them look like chips from the same block. It is on this very account that a community of origin for those belonging to each group of them, if not indeed for the whole of even these groups themselves, has been so long suspected. But is not this a community of origin that links them not only one with another, but probably also to other bodies in space, and that by a much further reaching chain than one which would bind them down only to our tiny orb and its satellite? Indeed, the very remarkable parallelism between their constituent elements and those which have been revealed by prismatic analysis as existing in activity on the surface of the sun, gives to this question of the origin of meteorites an interest of that expectant kind which holds us, as it were, listening for the announcement of what may be the next new discovery in solar physics—some fact that may illuminate as by electric light the whole solar system, and, clearing up the mystery that surrounds the comets, the zodiacal light, the solar corona, and even our aurora, may tell us why the "fiery tears of St. Lawrence" and other meteor showers do not descend on us as νιφάδες πέτρων, and even explain the source whence the meteorites do really come. We shall then be able better to decipher the characters in which the history of the meteorites is written, a history which assuredly is engraved, though in hieroglyphic language, on these messengers from space to our world. Expectation of this kind should surely invest our reasoning with the kind of caution which befits men who, feeling in the twilight after a "quest" of this kind, are conscious that they cannot be very far off from touching it in very truth, and, as it were, with their hands. We venture to think that M. Meunier has not, on this occasion, succeeded in attaining to the object of that quest.

N. S. MASKELYNE

WHAT IS ENERGY?

II.

IN our first article it was shown that energy, or the power of doing work, is of two kinds, namely, energy due to actual motion, and that due to position. We ended by supposing that a stone shot vertically upwards had been caught at the summit of its flight and lodged on the top of a house; and this gave rise to the question, What has become of the energy of the stone? To answer this we must learn to regard energy, not as a *quality*, but rather as a *thing*.

The chemist has always taught us to regard quantity or mass of matter as unchangeable, so that amid the many bewildering transformations of form and quality which take place in the chemical world, we can always consult our balance with a certainty that *it* will not play us false. But now the physical philosopher steps in and tells us that energy is quite as unchangeable as mass, and that the conservation of both is equally complete There is, however, this difference between the two things—the same particle of matter will always retain the same mass, but it will not always retain the same energy. As a whole, energy is invariable, but it is always shifting about from particle to particle, and it is hence more difficult to grasp the conception of an invariability of energy than of an invariability of mass. For instance, the mass of our luminary always remains the same, but its energy is always getting less.

And now to return to our question,—What has become of the energy of the stone? Has this disappeared? Far from it; the energy with which the stone began its flight has no more disappeared from the universe of energy, than the coal, when we have burned it in our fire, disappears from the universe of matter. But this has taken place:—the energy has changed its form and become spent or has disappeared as energy of actual motion, in gaining for the stone a position of advantage with regard to the force of gravity.

If we study this particular instance more minutely, we shall see that during the upward flight of the stone its energy of actual motion becomes gradually changed into energy of position, while the reverse will take place during its downward flight, if we now suppose it dislodged from the top of the house. In this latter case the energy of position with which it begins its downward flight is gradually reconverted into energy of actual motion, until at last, when the stone reaches the ground, it has the same amount of velocity, and, therefore, of actual energy, which it had at first.

Let us now revert, for a moment, to the definition of energy, which means the power of doing work, and we shall see at once how we may gauge numerically the quantity of energy which the stone possesses, and in order to simplify matters, let us suppose that this stone weighs exactly one pound. If therefore, it has velocity enough to carry it up one foot, it may be said to have energy enough to do one unit of work, inasmuch as we have defined one pound raised one foot high to be one unit of work; and in like manner if it has velocity sufficient to carry it 16 feet high, it may be said to have an energy equivalent to 16 units of work or foot-pounds as those units are sometimes called. Now, if the stone be discharged upwards with an initial velocity of 32 feet per second, it will rise 16 feet high, and it has therefore an energy represented by 16. But if its initial velocity be 64 feet per second it will rise 64 feet high before it turns, and will therefore have energy represented by 64. Hence we see that by doubling the velocity the energy is quadrupled, and we might show that by tripling the velocity the energy is increased nine times. This is expressed in general terms by saying that the energy or quantity of work which a moving body can accomplish varies as the square of its velocity. This fact is well known to artillerymen, for a ball with a double velocity will penetrate much more than twice as far into an obstacle opposing its progress.

Let us now take the stone or pound-weight having an initial velocity of 64 feet per second, and consider the state of things at the precise moment when it is 48 feet high. It will at that moment have an actual velocity of 32 feet per second, which, as we have seen, will represent 16 units of work. But it started from the ground with 64 units of work in it: what therefore has become of the difference—or 48 units? Evidently it has disappeared as actual energy; but the stone, being 48 feet high, has an energy of position represented by 48 units; so that at this precise moment of its flight its actual energy (16), *plus* its energy of position (48), are together equal to the whole energy with which it started (64).

Here, then, we have no annihilation of energy, but merely the transformation of it from actual energy into that implied by position; nor have we any creation of energy when the stone is on its downward flight, but merely the re-transformation of the energy of position into the original form of actual energy.

We shall presently discuss what becomes of this actual energy after the stone has struck the ground; but, in the meantime, we would repeat our remark how intimate is the analogy between the physical and the social world. In both cases we have actual energy and energy of position, the only difference being that in the social world it is impossible to measure energy with exactness, while in the mechanical world we can gauge it with the utmost precision.

Proteus-like, this element energy is always changing its form; and hence arises the extreme difficulty of the subject, for we cannot easily retain a sufficient grasp of the ever-changing element to argue experimentally regarding it. All the varieties of physical energy may, however, be embraced under the two heads already mentioned, namely, energy of actual motion and of position. We have chosen the force of gravity, acting upon a stone shot up into the air, as our example; but there are other forces besides gravity. Thus, a watch newly wound up is in a condition of visible advantage with respect to the force of the main-spring; and as it continues to go it gradually loses this energy of position, converting it into energy of motion. A cross-bow bent is likewise in a position of advantage with respect to the spring of the bow; and when its bolt is discharged, this energy of position is converted into that of motion. Thus again, a meteor, a railway train, a mountain torrent, the wind, all represent energy of actual visible motion; while a head of water may be classed along with a stone at the top of a house as representing energy of position. The list which represents visible energy of motion and of position might be extended indefinitely; but we must remember that there are also invisible molecular motions, which do not the less exist because they are invisible.

One of the best known of these molecular energies is *radiant light and heat*—a species which can traverse space with the enormous velocity of 186,000 miles a second.

Although itself eminently silent and gentle in its action it is, nevertheless, the parent of most of the work which is done in the world, as we shall presently see when we proceed to another division of our subject. In the mean time we may state that radiant light and heat are supposed to consist of a certain undulatory motion traversing an etherial medium which pervades all space.

Now, when this radiant energy falls upon a substance, part of it is absorbed, and in the process of absorption is converted into *ordinary heat*. The undulatory motion which had previously traversed the thin ether of space has now become linked with gross palpable matter, and manifests itself in a motion which it produces in the particles of this matter. The violence of this rotatory or vortex-like motion will thus form a measure of the heat which the matter contains.

Another species of molecular energy consists of *electricity in motion*. When an electric current is moving along a wire, we have therein the progress of a power moving like light with enormous velocity, and, like light, silent in its operation. Silent, we say, if it meets with no resistance, but exceedingly formidable if it be opposed; for the awe-inspiring flash is not so much the electricity itself as the visible punishment which it has inflicted on the air for daring to impede its progress. Had there been a set of stout wires between the thunder-cloud and the earth, the fluid would have passed into the ground without disturbance.

The molecular energies which we have now described may be imagined to represent motion of some sort not perceived by the outward eye, but present nevertheless to the eye of the understanding, they may therefore be compared to the energy of a body in visible motion, or actual energy as we have termed it.

But we have also molecular energies which are more analogous to the energy of position of a stone at the top of a cliff.

For instance, two bodies near one another may be endowed with a species of energy of position due to *opposite electrical states*, in which case they have a tendency to rush together, just as a stone at the top of a cliff has a tendency to rush to the earth. If the two bodies be allowed to rush together this energy of position will be converted into that of visible motion, just as when the stone is allowed to drop from the cliff its energy of position is converted into that of visible motion.

There is finally a species of molecular energy caused by *chemical separation*. When we carry a stone to the top of a cliff, we violently separate two bodies that attract one another, and these two bodies are the earth and the stone. In like manner when we decompose carbonic acid gas into its constituents we violently separate two bodies that attract one another, and these are carbon and oxygen. When, therefore, we have obtained in a separate state two bodies, the atoms of which are prepared to rush together and combine with one another, we have at the same time obtained a kind of energy of molecular position analogous on the small scale to the energy of a stone resting upon the top of a house, or on the edge of a cliff on the large or cosmical scale.

BALFOUR STEWART

FORMS OF ANIMAL LIFE

Forms of Animal Life; being Outlines of Zoological Classification, based upon Anatomical Investigation, and illustrated by Descriptions of Specimens and of Figures. By George Rolleston, D.M., F.R.S., Linacre Professor of Anatomy and Physiology in the University of Oxford. (Oxford: Macmillan and Co., 1870; Clarendon Press Series.)

I.

THIS long-promised and hoped-for book has at last appeared, and we may say at once that it fully maintains the well-earned reputation of its learned author. It will probably be most useful to his own pupils, for whom it seems to have been originally designed; and to those students of Comparative Anatomy who teach as well as learn.

The work consist of three parts: first, an enumeration of the anatomical characters of each sub-kingdom and class, arranged in a descending order from Mammalia to Gregarinæ—a plan less useful in most respects than the reverse one which is now generally followed. Next comes a minute description of certain dissected specimens in the new Museum at Oxford; and lastly an explanation of twelve plates, most of them original, which together supply a tolerably detailed account of the anatomy of at least one specimen of almost every class. "The distinctive character of the book consists in its attempting so to combine the concrete facts of Zoötomy with the outlines of systematic classification, as to enable the student to put them for himself into their natural relations of foundation and superstructure. The foundation may be made wider, and the superstructure may have its outlines not only filled up, but even considerably altered by subsequent and more extensive labours; but the mutual relations of the one as foundation and of the other as superstructure, which this book particularly aims at illustrating, must always remain the same." (Preface, p. vi.) This is very true, and it would have been well if all systems of classification had been thus based on anatomical facts. We may even suggest that the mutual relation of the foundation and the superstructure would have been still more obvious if the anatomical had preceded the systematic parts of the present work.

We propose, however, to follow Professor Rolleston's order, and to discuss here the classification he adopts, reserving an account of the second and third parts for a future article.

The question whether a perfect Zoological classification would be merely, like a perfect Nosology, a convenient method of stating and remembering a number of concomitant variations, or whether it would represent real genealogical relations between the several groups of animals, is one which has acquired great importance since the facts adduced by Darwin and Wallace have given probability to a modified form of the old theory of evolution. Most German naturalists fully accept this hypothesis, and employ their skill in constructing genealogical trees of each class. Dr. Rolleston holds that acceptance or rejection of the modern theory of evolution will depend on the particular constitution of each mind to which it is presented: "but," he adds, "whether the general theory be accepted as a whole or not, it must be allowed that in the face on the one hand of our knowledge of the greatness of the unlikeness which may be compatible with specific identity, and on the other of our ignorance of the entirety of the geological record, the value of the special 'phylogenies' reaching far out of modern periods are [qy. is] likely to remain in the very highest degree arbitrary and problematical."

It must, however, be remembered that, apart from its truth, a scientific theory may be very valuable by the accumulation of facts and the clearing of conceptions, to which it leads. Judged by this standard, the Darwinian theory is abundantly justified, not only by the observations of its illustrious author himself, but by the mass of excellent work it has evoked from others, especially in Germany. Indeed, from the results already gained, we may almost rank the theory of Natural Selection on a level with the teleological views which led Harvey to his great discovery, or with the belief in ideal archetypes by which Goethe was led to discover the presence of a præmaxillary bone in man, the "vertebral" construction of the skull, and the true morphology of a flower.*

Again, Dr. Rolleston enumerates the many similes by which men have endeavoured to represent the system of nature, and prefers the comparison of the groups of animals to the islands of an archipelago. Most readers will probably find the metaphor of a tree with its branches more useful, especially if existing forms are regarded according to Prof. Flower's ingenious suggestion, as the transverse section of such a tree, cut off at the present stage of the world's history. But, after all, by far the most natural, convenient, and almost inevitable metaphor

* Die Descendenztheorie wird so eine neue Periode in der Geschichte der vergleichenden Anatomie beginnen. Sie wird sogar einen bedeutender en Wendepunct bezeichnen als irgend eine Theorie in dieser Wissenschaft vorher vermocht hat, denn sie greift tiefer als alle jene, und es gibt kaum Einen Theil der Morphologie, der nicht auf's Innigste von ihr berührt würde. (Gegenbaur: Grundzüge der Vergl. Anat. 1870, p. 19.)

is that of a great nation of common descent, not divided by one of the subjects into the arbitrary "provinces" of a kingdom, or "classes" of an army, but falling naturally into "alliances" which are those of blood, "families" which depend on common parentage, and "orders" which are hereditary. Now it is a great point gained to know that, probably, at least, such natural and convenient language is no metaphor at all, but strictly and literally true.

Prof. Rolleston follows Gegenbaur in elevating *Echinodermata* to the rank of a primary division, and its various orders to classes; and also in including under the head *Vermes*, not only the *Rotifera* and *Helminthes*, which, as "Scolecida," form, with Echinodermata, Prof. Huxley's Annuloid group; but the *Annulata* as well, which have been associated with the Arthropod classes by all other naturalists since Cuvier (see the long and valuable note on pp. 152-157). On the other hand, *Tunicata* and *Polyzoa* are not also placed in the same heterogeneous crowd of "Würmer," but are retained in their probably more natural position among the Mollusks. Indeed, the classes included by Dr. Rolleston under Vertebrata, Arthropoda, Mollusca, Cœlenterata, and Protozoa, are almost precisely the same as those recognised by Prof. Huxley, and generally admitted in this country. The only variation is in re-admitting Radiolaria among Rhizopods, and making "Ctenophorae" (why not Ctenophora?) a separate class, instead of an order of Anthozoa.

The "characteristics of Vertebrata" are given with the fulness and accuracy which mark the author's work. The eight closely-printed pages devoted to this section may be advantageously compared with the very short, but masterly account of the same group in Prof. Huxley's "Introduction to the Classification of Animals." Indeed, we would advise all students to read thoroughly the latter work before beginning the one under review, which would then admirably fill up the details of the London professor's sketches. At p. xxxv., it is implied that the kidney of Amphibia and most fishes answers to the permanent one of other vertebrates; its homology with the Wolffian body is, however, rightly stated at p. lxiv. We would suggest that the terms "outer" and "inner," are better than "uppermost" and "lowermost," to denote the serous and mucous layers of the germinal membrane; and at p. xl. "ventral" should replace "anterior,"—a term which confuses by mixing up the relations of human anatomy with those of general zootomy. We are not surprised that Dr. Rolleston refuses to accept Hamatocrya as a natural group of Vertebrata; he follows Prof. Huxley's division into Branchiata and Abranchiata, and of the latter into Mammalia and Sauropsida. Anammiota is a better alternative name for Branchiata than Anallantoidea; not only for euphony, but because an allantois is certainly present to some extent in certain Ichthyopsida, while its development and importance in mammals is far less than in birds and reptiles. In describing the characters of Mammalia, we notice that the author endorses Prof. Huxley's revised opinion that the malleus, not the incus, represents the quadrate bone, although the opposite is stated at p. 25 of the second part; but he appears to regard the marsupial bones as part of the pelvis, and not as mere ossifications of the internal pillar of the abdominal ring. The orders of the vertebrate classes are adopted from those given in the "Introduction to Classification" before referred to. That most of these will receive general assent there can be little doubt; but with respect to the placental classification of Mammalia, we venture to suggest some objections. In the first place the structure of the uterus and placenta is not strictly an embryonic character, but belongs to the parent organs of generation, which, in other classes, are found to be of minor importance in classification. Then it is very difficult to get accurate information as to the condition of the placenta—opportunities for observing parturition are, of course, much less frequent than for studying almost any other process—so that it will be long before we have any facts as to whole groups of animals, *e.g.*, the Sirenia. Moreover, if here opportunity is fleeting, judgment would seem to be often difficult: thus the placentation of so common a mammal as the rat was completely misunderstood by an eminent naturalist, until his account was corrected by Prof. Rolleston's own dissections. Compared with the skeleton, the teeth, or even the brain, the placenta is a far less available criterion, and far more liable to misinterpretation. But even if this were not the case, a grave objection to the placental classification remains in the fact that it necessarily excludes all fossil forms, the study of which has been so well used as a help in tracing the affinities of living animals, and by none more than Prof. Huxley himself. Lastly, judging this system by the test of concomitant variation, it is surely sufficiently condemned by compelling together animals so different as Primates and Rodentia, Orycteropus and Simia, while it separates Hyrax from Rodents and Perissodactyla, to unite it with Carnivora. The better plan we conceive is that followed by Prof. Flower in his recent lectures, to place the several orders in as natural juxtaposition as may be, and to put placental characters on the same level only as those afforded by the brain or the extremities.

In the careful description of the Mollusca which follows, Dr. Rolleston does not divide the Gasteropoda into two classes; nor does he admit the constancy of the primary flexures of the intestine, which Professor Huxley has made an important criterion of all the Molluscous series. (See pp. 58, 68, and 235.) In the *Ascidia* this question of the intestinal flexure depends upon the view taken of their great branchial sac. Dr. Rolleston does not admit Prof. Huxley's theory of its being morphologically a pharynx, but with Mr. Hancock regards it "as homologous not with a dilated pharynx, but with the branchial cavity, and the inhalent aperture to represent not the mouth, but the inhalent syphon of the Lamellibranchiata" (p. 69). If this view be accepted, it unites the Molluscoidea more closely to the Mollusca proper, and is an additional argument against their association with Vermes. The remarkable observations of Kowalewsky and Kupffer on the resemblances to vertebrate structures in the larva of Phalusia are duly noted (pp. ci. ciii.) Dr. Rolleston says of the Arthropoda that they "have frequently been classed together with more or fewer of the Vermes in one subkingdom, that of the 'Annulosa;' and whilst by such highly organised forms as the marine *Polychæta* an approximation appears to be made to certain of the less specialised of the Crustacea; or even of the Myriopoda, or the larvæ of insects, amongst the air-breathing Arthropoda; the microscopic Rotifera connect the Vermes, to which Sub-kingdom they are to be referred, very closely to the Crustacea"* (p. cvii.)

Although Gegenbaur's union of the Polyzoa and Tunicata with Vermes is not adopted, Huxley's group of Scolecida is divided into the three classes—Nematelminthes, Rotifera, and Platyelminthes. The Annulata again are divided into Annulata proper and Gephyrea. Dr. Rolleston ranks the remarkable genus *Sagitta* under the Nematelminthes (p. cxxxvii.), again following Gegenbaur, instead of placing it in a class by itself, the Chætognatha of Huxley.

We would here venture to question the advantage of the practice so generally followed by zoologists of making a separate order or even class—which generally entails at least one new name—for every aberrant genus. If *Sagitta* cannot be ranked with Annulata or Vermes, it might well stand under its generic head, or as the representation of an isolated family. In the same way we would deal with Sir John Lubbock's genus *Pauropus*, in relation to the two orders of Myriopoda, with *Hyrax* among mammals, *Archæopteryx* among birds, and *Amphioxus* among fishes. It is in vain to try to make all our classes or orders "of equal value." When natural families have been defined and grouped around a typical genus, the ordinal arrangement should, to a great extent, depend upon the number of species and other points of practical convenience. We learn nothing more of the single animal *Amphioxus lanceolatus* by a special order or sub-class, variously named by each classifier, being framed for its reception. So again we do not see the necessity of marking the distinction of *Marsupiata* and *Monotremata* from other mammals by the invention of fresh names—names which in this case have been singularly inappropriate, since several placental mammals are "didelphous," and the word "ornithodelphia" implies that birds have a uterus, and conceals the sauroid rather than ornithic affinities of Monotremata.

The description given by Professor Rolleston of the Cœlenterata is somewhat meagre, but that of Echinodermata is remarkably full, and when read in conjunction with the descriptions of *Asterias* and *Pentactes* (pp. 141-158, and 223-229, Plate x.), constitutes a valuable monograph of this complicated and interesting group of animals. Here, however, as in many other parts of the book, a few rough diagrams like those in the "Introduction to Classification," and in Prof. Greene's admirable monograph of Cœlenterata, would have been exceedingly useful, especially in explaining the more difficult points of embryology.

In treating of the Protozoa, with which the Infusoria are, we think, rightly associated, Professor Rolleston introduces a valuable disquisition upon the limits of the animal and vegetable kingdoms with the admission that "it is not rarely difficult to differentiate a unicellular organism as animal or vegetable, unless we happen to be acquainted with its past or future history" (p. clxii.). He does not admit Häckel's intermediate kingdom Protista, agreeing with almost all English naturalists in regarding *Monera* and *Protoplasta* as allied to Rhizopoda, and *Myxomycetæ* and *Flagellata* as vegetable organisms. He justly regards the chief difficulty to lie in the establishment of such statements as that animalcules as high as *Actinophrys* have at one period undoubtedly vegetable characters; but at the utmost the indeterminate groups would include very few of the organisms claimed by Prof. Häckel for his new kingdom.

In addition to the criteria usually given between animals and vegetables, it would seem that in all cases of true ovulation, the animal embryo absorbs its yelk from inside, while that of a seed is itself surrounded by the albumen; if this difference proves to be universal, it would be a remarkable foreshadowing of the mode of nourishment of adult animals and plants respectively.

* This sentence is a fair specimen of the author's compressed and parenthetical style, which sometimes reminds the reader of Lord Bacon and sometimes of S. Paul. A large insertion of brackets and dashes, of which there is scarcely one throughout the book, would often make plain the difficulties of a Thucydidean sentence, but even then only persons of great vital capacity could read the book aloud.

OUR BOOK SHELF

The Handy Book of Bees, being a Practical Treatise on their Profitable Management. By A. Pettigrew. (William Blackwood and Sons, Edinburgh and London.)

THIS book will be invaluable to the beginner in beekeeping, and will probably contain many useful hints to the more experienced. The author is one of a family of beekeepers, who have always made a large profit from their bees. He is eminently practical, and the greater part of the work consists of careful notes on the various details of successful bee management. In the descriptive parts he is also very good, but is not quite so successful when he comes to treat of some disputed points in the economy of bees. For example, he maintains the theory that the eggs of bees are of no sex, and can be made into queens, workers, or drones, as the wants of the community render necessary. In this he is opposed to all the great authorities who have studied bees; and he even gives a series of letters from Mr. Woodbury, of Exeter, on the question, which are almost conclusive as to eggs being of two sorts when laid, one producing drones only, and not capable by any subsequent treatment of producing anything else; the other capable of producing workers or queens, according to the treatment they receive. His arguments against this view are of the weakest, and he suggests an experiment, which, he says, "is within the reach of very inexperienced persons," and which would completely settle the question; and yet he writes a book in which he brings up the subject, and opposes the best authorities, without having first taken the trouble to make the experiment himself! Again, he states positively that worker-bees live nine months only—never more; yet he gives no account of how this can be ascertained, or refers to the variety of opinion that exists as to their longevity.

As an example of the valuable matter in the practical part of the work, we quote his recipe for fumigation. "A few puffs of smoke from a bit of corduroy or fustian rolled up like a candle, stupefies and terrifies bees so much that they run to escape from its power. Tobacco smoke is more powerful still, but it has a tendency to make bees dizzy, and reel like a drunken man; besides, it is far more expensive, and less handy. Old corduroy or fustian is better than new, unless the matter used to stiffen it be completely washed out. The stiffening matter won't burn. The old worn-out and castaway fustian and corduroy clothes of labouring men cannot be surpassed for the purpose of stupefying bees. Let me ask the most timid beekeeper in the country to try it. Get a piece the size of a man's hand, rolled up rather tight, and fired at one end—not to blaze, but simply to smoke. Let him now place the smoking end so close to the door of a hive that all the smoke may go in when he blows on it. After six or eight puffs have been sent into the hive, let him lift it off the board, turn it gently over upside down, so that the bees and combs stare him in the face.

By holding and moving the smoking ends of the rags over the face of the bees and blowing the smoke among them, they run helter skelter down amongst the combs far more afraid than hurt. Now he can carry the hive round the garden under his arm without being stung. Whenever the bees are likely to rise they should be dosed again. The bee-keeper will now find he has got the mastery over his bees, and can do what he likes with them. He will be able to drive them out of a hive full of combs into an empty one, and moreover shake them back, or tumble them back, or spoonful them back into the old hive or another, as men take peas from one basket to another. The smoke does not injure the health of the bees, does not stop them from work more than two or three minutes, and the use of it is so simple, easy, and efficacious, that we have no wish to find anything better for stupefying bees."

Hives, their material, size and position; their covers, boards, supers, ekes and nadirs; the times and modes of swarming bees artificially; how to feed them, and how to take the honey; how to combine separate hives, and how best to preserve them during winter, with many other details of bee-management, will be found so fully and clearly described, and with such good reasons for every step, that we think this work may do much to render profitable beekeeping far more common than it seems to be at present.

A. R. WALLACE

Malacologia del Mar Rosso. Arturo Issel. 8vo. With five lithographed plates. (Pisa, 1869.)

WE have lately read and heard much about that great undertaking, the Suez Canal, and of its being the means of facilitating the commerce of the human race in Europe and India. Something may also be said as to the interchange of the marine fauna of the Mediterranean and Red Sea, which will probably result from this artificial mode of communication. Geology teaches us that these two seas were once (in the post-tertiary or quaternary period) connected by a natural channel; for several species of shells now inhabiting the Mediterranean, and common there, occur in a fossil state throughout the Isthmus or Desert of Suez. These are:—*Arca Noæ*, *A. lactea*, var. *erythræa*, *Donax trunculus*, *Solecurtus strigilatus*, *Gastrochæna dubia*, *Patella cærulea*, *Calyptræa Chinensis*, *Nassa mutabilis*, *N. costulata*, *Murex trunculus*, var., and *Cypræa annulus*. Now it is a remarkable fact that scarcely any species in a living state are common to the Mediterranean and the Red Sea, even after making every allowance for the range of local variation. Dr. R. A. Philippi, indeed, in the second volume of his admirable work on the Mollusca of the Two Sicilies (published in 1844), gave a list of all the marine shells which he had examined in the collection made by Hemprich and Ehrenberg in the Red Sea; and of these he identified no less than 75 species as living both in the Mediterranean and the Red Sea. According to him the number of Red Sea species found by Hemprich and Ehrenberg was 408. But it now appears that these explorers collected at Alexandria also on their way home, and that by some carelessness or mischance many of the labels indicating the localities got intermixed; so that no reliance could be placed on the collection in a geographical point of view when it was examined by Philippi.

The present work gives 574 recent or living species, of which 64 are for the first time described and 34 figured. As might be expected, nearly all are tropical and belong to the Indian Ocean. Besides these, 232 fossil species are enumerated, 25 being described as new to science, and 31 figured. The author collected 191 species on the shore at Suez in the spring of 1865; 141 were collected by the Marquis G. M. Arconati in the Gulf of Akaba, as well as at Suez; public museums and private cabinets at Berlin, Paris, Pisa, Turin, and Genoa furnished additional material; while the catalogues of Ehrenberg, Rüppel, and Vaillant, with the descriptions and plates of Philippi, Reeve, Sowerby, Kiener, and others, served for comparison and reference. Professor Issel is again gone to Suez for the purpose of continuing this interesting and useful research. His figures are very good, drawn on tinted paper. All general conchologists ought to possess the work.

I may remark that one of the Red Sea species (*Cæcum annulatum*) here stated to inhabit "Aden, Indie occidentali, Irlanda, Inghilterra"—the last two localities being, on the authority of Brown, Forbes and Hanley, and Philip Carpenter—has been only found in Great Britain among the sand from bath-sponges!

It should be known that Mr. M'Andrew dredged for several months last year in the Gulf of Suez, when he made a very extensive collection of Mollusca, including a great number of then undescribed species. I hope he will soon publish his discoveries. No one is more competent to do so.

J. GWYN JEFFREYS

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The New Natural History Museum

I AM informed that the plan of fitting a museum with cases sealed on the side facing the public galleries, alluded to in last week's NATURE, was suggested by Dr. Hooker, in an article signed "A Metropolitan Naturalist," in the *Gardener's Chronicle* for 1858, p. 749, which also contains many other good suggestions as to the requirements of the museum.

W. H. FLOWER

The "English Cyclopædia"

IN my youth I took in "The Penny Cyclopædia," in my manhood I purchased its progeny, "The English Cyclopædia," and now, in comparative old age, I have acquired two supplementary volumes to the latter; and I have never had reason to complain of any of these books, until the supplement to the Natural History division appeared a month or two ago. This supplement embraces a period of sixteen years, from 1854 to 1870, during which, probably, more good scientific work has been accomplished than in any preceding half-century. Many subjects on which I expected to find important articles are passed over without a reference, and others are, as I shall endeavour to show, treated of in a most imperfect and unsatisfactory manner.

I looked in vain for articles on (1) *Acclimatisation*, (2) *Ants*, (on which Bates, Lespes, Lincecum, Norton, F. Smith, Sumichrast, and many others have written since 1854), (3) *Axolotl* (whose remarkable metamorphoses have been studied by Dumeril and others), (4) *Cephalopoda* (on which much has been written since the Cyclopædia article appeared, when the Hectocotylus had not become a subject of discussion), (5) *Darwinism*, (6) *Deep-sea Dredging*, (7) *Dimorphism in the Animal Kingdom*, (8) *Eophyton*, (9) *Eozoon*, (10) *Eugerion* (a fossil insect that from its puzzling form has been compared with the Archæopteryx), (11) *Fungous Origin of Diseases*, the cholera-fungus, scarlatina-fungus, ague-fungus, the fungi in skin-diseases, &c., and (12) *Hyalonema* (on which several articles holding the most opposite views have lately appeared); (13) *Hybridity in animals and plants* (on which Broca, Masters and others have written elaborate works, and on which, as in the case of rabbits and hares, many remarkable experiments have been made), (14) *Mimicry in the Animal Kingdom*, (15) *Monera*, (16) *Ornithoscelida*, (17) *Parthenogenesis* (on which, during the last sixteen years, there have been published Siebold's "True Parthenogenesis in Lepidoptera and Bees," Owen "On Parthenogenesis," Leuckart "On the recognition of Parthenogenesis in Insects," De Quatrefages' "Metamorphoses of Man and the lower Animals," and the contributions of Huxley and Lubbock to Transactions of the Linnean Society and to the Philosophical

Transactions, besides numerous articles on special cases of Parthenogenesis in certain gall-flies, solitary wasps, spiders, and mites), (18) *Protista*, (19) *Protoplasm*, (20) *Rhizocrinus*, which sent forth our late deep-sea dredging expeditions, (21) *Sauroids* and *Sauropsida*, (22) *Sphegidæ* (insects whose marvellous instincts have been described by Lespes and other observers), and (23) *Vivisection* and its results.

A few of these subjects are discussed in articles devoted to more general matters; for example, (8) is noticed, but not figured or even systematically described, under *Foraminifera, Laurentian Formations*, and *Palæontology*, and (9) is referred to in the last-named article; (12) is mentioned in *Spongiadæ*, and more fully described, but not figured, in *Alcyonaria*; (17) is alluded to, in so far as the researches of Huxley and Lubbock go, in the article *Aphis*; (19) is noticed under *Cells*; and (21) *Sauropsida* is defined in the article on *Birds*.

Cross references, which, like illustrations, have been far too scantily employed in these volumes, would have partly removed this source of complaint, as for example *Sauropsida* [Birds, E.C.S.]

On the following subjects our knowledge has not been brought up to what can with every allowance, be called a recent date:—(1) *Aerolites*, latest reference 1861, and no bibliography; (2) *Alca*, latest reference 1861; (3) *Annelida* contains no reference to Claparède's appalling criticism on De Quatrefages' researches; (4) *Archæopteryx* has no reference to Huxley's papers; (4) *Blood* contains no reference to late researches on the structure of the corpuscles, to the occurrence of *Protagon* in them, or to the remarkable colour-test for blood, discovered by Dr. Day of Geelong, which has succeeded in detecting old blood spots, when even spectrum analysis in the hands of its great master, Mr. Sorby, has failed; and Dr. Richardson is stated to hold the opinion that the fibrin is held in solution in the body by ammonia, although it is well known that, with a moral courage which cannot be sufficiently commended, he publicly (at a meeting of the British Association some years ago) renounced that opinion as soon as he found it was untenable; (5) *Birds of Paradise* would have been a more satisfactory article if it had had the benefit of Mr. Wallace's supervision; (6) *Foraminifera* would have been all the better if the writer had been acquainted with Häckel's splendid monograph on the *Radiolaria*; (7) *Nervous System* is perhaps the most imperfect article in the whole volume. It contains no reference to the labours of Gratiolet, Lockart Clarke, Brown-Sequard, Claude Bernard, Robin, Philippeaux, or Vulpian on the minute structure and the physiology of the nerves, while the chemistry of the brain is discussed without a reference to Protagon or Neurine. The synthetical formation of the latter is surely of sufficient interest to deserve notice.

Regard for the value of space in your columns alone prevents me from prolonging the list of imperfect articles.

The English Cyclopædia is, as I presume everyone will admit, intended for "all sorts and conditions of men," for "women labouring with child" (if we use the phrase in the same sense as a German governess, who is said to have expatiated to a popular bishop on the comprehensiveness of a church-service that did not even overlook the daily cares of those who devoted themselves to the duties of early education), and even for children; at all events I read the "Penny" with great pleasure as a boy. Hence it should be a source of knowledge from which we might expect to find information in all cases of ordinary difficulty. To decide how far this assumption is correct, I put it to the test in the following way:—I read Huxley's splendid address "On the recent progress of Palæontology" which lately appeared in your columns, and the "Report on a Close Time for Birds" in the last volume of the British Association Reports. I freely admit that I am not learned in Natural History; but as an old country doctor, I probably picked up quite as much knowledge in my youth, as the average class of "Cyclopædia" readers. None of the following terms taken from Huxley's address are to be found either in the Index to the Cyclopædia, or in the Supplement:—*Amphycion, Anchitherium, Anthracosaurus, Artiodactyle, Cainotherium, Chœropotamus, Coccoliths, Coccospheres, Compsognathus, Coryphodon, Dicynodes, Didelphia, Dinosauria, Discoliths, Elasmobranchs, Eophyton, Eozoon, Evolution, Galeosaurus, Globigerinæ, Hipparion, Hipparitherium, Homotaxis, Hyænictis, Hyenoartos, Hyopotamus, Hyracotherium, Ictitherium, Mesopithecus, Microlestes, Monadelphia, Omalodotherium, Ornithodelphia, Ornithoscelida, Orycteropus, Perissodactyle, Phascolotherium, Pliolophus, Pterosauria, Sauropsida, Stereognathus, Typotherium.*

I will not go further into the "Close Time" Report than to state that I learn from it the important fact that owls eat, *inter alia*, "*Arvicolæ, Crocidura, Crossopi, Hypudæi, Sorices*, shrews and voles." I look in vain for all these inviting edibles, and I find only *Hypudæus* in the Cyclopædia, and what benefit do I derive from my search? Only that *Hypudæus* is sometimes spelt *Hipudæus*. A learned friend, who is ever ready and able to remove difficulties from the paths of his weaker brethren—the genial guardian of Kent's Cavern—suggested that it was something in the mouse or rat line, and so I turned to *Muridæ*, where I found the required information regarding that animal and the voles.

It is not for the purpose of depreciating the Supplement to the English Cyclopædia that I have noticed the above omissions and deficiencies, but with the object of pointing out how they may still be remedied. The Supplement has evidently been drawn up without any editorial care. Let a duly-qualified editor obtain a list of *desiderata* from some botanist, geologist, and zoologist of eminence; and let him fix upon the articles that he deems the most important, and give them to qualified writers. Let him attend duly to the compilation of bibliographies of the most important subjects, and let him increase to an enormous extent the amount of cross references. The article *Muridæ*, from which I exhumed *Hypudæus*, would probably yield fifty references.

A few subjects—as birds and hydrozoa—are fairly supplemented. Why should not similar articles be given us on the progress, during the last sixteen years, of our knowledge of the crustaceans, insects, fishes, reptiles, &c.? An additional supplement, such as I have here suggested, would probably not occupy more than 100 pages.

NEMO

South Devon

ADMIRAL MANNERS

ADMIRAL RUSSELL HENRY MANNERS was born in London on the 31st of January, 1800, entered the Royal Naval College the 6th of May, 1813, and embarked March the 6th, 1816, as a volunteer on board the *Minden*, 74, Captain Paterson, in which, after assisting at the bombardment of Algiers, he proceeded to the East Indies, where he served under the flag of Sir Richard King, until nominated midshipman, the 1st of July, 1818, to the *Orlando*, 36, commanded by Captain John Clavell, with whom, in 1819, he returned to England on the *Malabar*, 74. After an intermediate employment on the Channel and West India stations in the *Spartan* and *Pyramus* frigates under Captains William Furlong Wise and Francis Newcombe, he became, the 29th of July, 1822, Acting Lieutenant of the *Tyne*, 26, Captain John Edward Walcot, to which vessel the Admiralty confirmed him the 19th of October following. In May 1823, he rejoined the *Pyramus*, still commanded by Captain Newcombe, under whom he continued until he obtained his promotion on the 16th of August, 1825. His last appointment was on the 21st of October, 1827, to the command of the *Britomart*, 10. The *Britomart* was first employed and intended for the Channel service under the order of the Commander-in-Chief, the Earl of Northesk, at Plymouth. She accompanied the squadron of ships escorting Don Miguel to Lisbon in the early part of 1828. In consequence of the revolution that followed in Portugal on Don Miguel declaring himself absolute, the *Britomart* was stationed at and off Oporto to watch the British interests there. The Constitutional party, failing to restore the Constitution against the usurped position of Don Miguel, the British Government withdrew her Minister from Lisbon, leaving the British interests in the hands of the Consul only, and Capt. Manners was selected to be in readiness to support him in case of need by keeping in sight of signals from Lisbon as long as the safety of the vessel permitted, but not to anchor within any Portuguese port unless absolutely necessary. This involved a long and vigilant cruising off and on the coast for about eight months, and through the whole of the winter. The only place communicated with during that time was Gibraltar, and then only to receive a supply of provisions and water from the dockyard. The yellow fever unfortunately breaking out at Gibraltar just before going there for this object, no communication could be had with the town, and the stay was confined to from twenty-four to forty-eight hours. The zeal and ability with which this service was carried out by Capt. Manners, as witnessed by Sir George Sartorius, there in command of the Portuguese Constitutional Squadron, and under whose orders in some degree the *Britomart* was placed, led to Capt Manners receiving his Post-rank on the 4th of March, 1829. He retired from active service in March 1849, became Rear-Admiral in July 1855, Vice-Admiral in April 1862, and Admiral in September 1865.

Admiral Manners was the only child of the late Mr. Russell Manners, M.P., and married in 1834 Louisa Jane, daughter of Count de Noé, Peer of France, who survives him, and by whom he has two sons and a daughter.

From the time he attained his Post-rank to the time of his death he devoted himself to scientific pursuits. He was elected a member of the Royal Astronomical Society in 1836. At a very early period he took an active interest in its administration, and after being on the Council for some time, was elected one of the honorary secretaries in February 1848, an office which he filled until 1858, when he accepted that of Foreign Secretary. This was a post for which his knowledge of foreign languages and his position in society peculiarly fitted him, and during his tenure of office he formed by active correspondence a connecting link between English and foreign astronomers. He was much esteemed abroad, so much so indeed that one of the presidents, in asking Admiral Manners to transmit one of the Society's medals to a foreign recipient, deemed it just to preface his remarks with the following well-deserved compliment :—

"Admiral Manners,—It has been my good fortune to visit the majority of European Observatories, and to make the acquaintance of their directors and other gentlemen connected with them, and it has in consequence become known to me how high in their esteem our Foreign Secretary stands. Your urbanity and promptitude in carrying out our foreign business has indeed become proverbial."

Admiral Manners was, on more than one occasion, asked to accept the chair of President, which, after some hesitation, he consented to do, and he was elected to that position in 1868. None of his predecessors was more highly esteemed by the Fellows of the Society, and no one filled the chair more admirably than he did. His mathematical attainments were considerable, more so than one might be apt to infer from his quiet demeanour. He was well versed in the astronomical literature of the day, and took a deep interest in the progress of astronomical science, both in England and on the Continent; and his active influence was always available for the promotion of any object connected with it.

On presenting the gold medal of the Society to Mr. Stone, first assistant of the Royal Observatory, Greenwich, Admiral Manners delivered a most able and exhaustive summary of that able astronomer's labours, and evinced a complete knowledge of the history of the solar parallax, for the investigation of which the medal was mainly awarded. Illness overtook him before he could complete his second year of office, and he was compelled to forego the gratification of delivering the address to M. Delaunay for his researches on the lunar theory; but he made it a point of duty and pleasure to receive M. Delaunay at his house, and although he was compelled to delegate to the friendly hand of Prof. Adams the drawing up of the address, yet he read and approved of what was written before it was delivered.

Admiral Manners in all his relations was a pure-minded, courteous, and sympathetic man, and in the fullest sense of the word a gentleman.

THE PRIMITIVE VEGETATION OF THE EARTH

TWENTY years ago scarcely anything was known, even to those engaged in the study of vegetable fossils, of a land flora older than the great coal-formation. In 1860, Goeppert, in his Memoir on the plants of the Silurian, Devonian, and Lower Carboniferous, mentions only one land plant, and this of doubtful character, in the Lower Devonian. In the Middle Devonian he knew but one species; in the Upper Devonian he enumerated fifty-seven. Most of these were European, but he included also such American species as were known to him. The paper of the writer on the Land Plants of Gaspé was published in 1859, but had not reached Goeppert at the time when his memoir was written. This, with some other descriptions of American Devonian plants not in his possession, might have added ten or twelve species, some of them Lower Devonian, to his list. In the ten years from 1860 to the present time, the writer has been able to raise the Devonian flora of Eastern North America to 121 species, and reckoning those of Europe at half that number, we now have at least 180 species of land plants from the Devonian, besides a few from the Upper Silurian. We thus have presented to our view a flora older than that of the Carboniferous period, and, in many respects, distinct from it; and in connection with which many interesting geological and botanical questions arise.

Geologists are aware that in passing backward in geological time from the modern to the Palæozoic period, we lose, as dominant members of the vegetable kingdom, first, the higher phænogamous plants, whether exogenous or endogenous; and that, in the Mesozoic period, the Acrogens, or higher cryptogams, represented by Ferns, Club-mosses, and Equiseta, share the world with the Gymnosperms, represented by the Pines and Cycads, while the higher phænogams on the one hand, and the lower cryptogams on the other, are excluded. Hence, the Mesozoic age has been called that of Gymnosperms, while the Palæozoic is that of Acrogens. These names are not, however, absolutely accurate, as we shall see that one of the highest forms of modern vegetation can be traced back into the Devonian; though the terms are undoubtedly useful, as indicating the prevalence of the types above mentioned, in a degree not now observed, and a corresponding rarity of those forms which constitute our prevalent modern vegetation.

It is my present object shortly to sketch the more recent facts of Devonian and Upper Silurian Botany, and to refer to a few of the general truths which they teach. The rocks called Devonian in Europe being on the horizon of the Erie division of the American geologists, which are much more fully developed than their representatives on the Eastern Continent, I shall use the term *Erian* as equivalent to Devonian, understanding by both that long and important geological age intervening between the close of the Upper Silurian and the beginning of the Carboniferous.

Just as in Europe the rocks of this period present a twofold aspect, being in some places of the character of a deposit of "Old Red Sandstone," and in others indicating deeper water, or more properly marine conditions, so in America, on a greater scale, they have two characters of development. In the great and typical *Erian* area, extending for 700 miles to the westward of the Apalachian chain of mountains, these rocks, sometimes attaining to a thickness of 15,000 feet, include extensive marine deposits; and except in their north-eastern border are not rich in fossil plants. In the smaller north-eastern area, on the other hand, lying to the eastward of the Apalachian range, they consist wholly of sandstones and shales, and are rich in plant remains while poor in marine fossils. Hence it is the Devonian of Gaspé, of New Brunswick, and of Maine, with that of eastern New York, which have chiefly afforded the plants to be described below; and it is exclusively in these areas that we find underclays with roots, or true fossil soils. Most of the localities of fossil plants in the districts above mentioned have been visited, and their plants studied *in situ* by the writer. The Gaspé sandstones were first studied and carefully measured and mapped by Sir W. E. Logan. The Devonian beds of St. John's, New Brunswick, have been thoroughly examined and illustrated by Prof. Hartt and Mr. Matthews, and those of Perry by Prof. Jackson, Prof. Rogers, and Mr. Hitchcock. Prof. Hall, of the Survey of New York, has kindly communicated to me the plants found in that State, and Prof. Newberry has contributed some facts and specimens illustrative of those of Ohio.

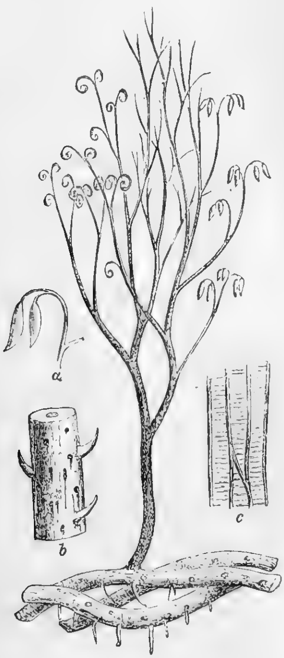

FIG. 1.—*Psilophyton princeps*—the oldest known plant of America, restored. (*a*), Fruit, natural size; (*b*), Stem, natural size; (*c*), Scalariform tissue of the axis, highly magnified. In the restoration one side is represented in vernation, and the other in fruit.

In the Sandstone cliffs of Gaspé Bay, Sir W. E. Logan recognised in 1843 the presence of great numbers of apparent roots in some of the shales and fine sandstones. These roots had evidently penetrated the beds in a living state, so that the root-beds were true fossil soils, which, after supporting vegetation, became submerged and covered with new beds of sediment. This must have occurred again and again in the process of the formation of the 4,000 feet of Gaspé sandstone. The true nature of the plants of these fossil soils I had subsequently good opportunities of investigating, and the most important results, in the discovery of the plants of my genus *Psilophyton*, are embodied in the restoration of *P. princeps* in Fig. 1. This remarkable plant, the oldest land plant known in America, since it extends through the Upper Silurian as well as the Devonian, presents a creeping horizontal rhizome or root-stock, from the upper side of which were given off slender branching stems, sometimes bearing rudimentary leaves, and crowned, when mature, with groups of gracefully nodding oval spore-cases. The root-stocks must in many cases have matted the soils in which they grew into a dense mass of vegetable matter, and in some places they accumulated to a sufficient extent to form layers of coaly matter, one of which on the south side of Gaspé Bay is as much as three inches in thickness, and is the oldest coal known in America. More usually the root-beds consist of hardened clay or fine sandstone filled with a complicated net-work or with parallel bands of rhizomes more or less flattened and in various states of preservation. In all probability these beds were originally swampy soils. From the surface of such a root-bed there arose into the air countless numbers of slender but somewhat woody stems, forming a dense mass of vegetation three or four feet in height. The stems, when young or barren, were more or less sparsely clothed with thick, short, pointed leaves, which, from the manner in which they penetrate the stone, must have been very rigid. At their extremities the stems were divided into slender branches, and these when young were curled in a crosier-like or circinate manner. When mature they bore at the ends of small branchlets pairs of oval sacs or spore-cases. The rhizomes when well preserved show minute markings, apparently indicating hairs or scales, and also round areoles with central spots, like those of Stigmaria, but not regularly arranged. These curious plants are unlike anything in the actual world. I have compared their fructification with that of the Pilulariæ or Pillworts, a comparison which has also occurred to Dr. Hooker. On the other hand, this fructification is borne in a totally different manner from that of Pilularia, and in this respect rather resembles some ferns; and the young stems by themselves would be referred without hesitation to Lycopodiaceæ. In short, Psilophyton is a generalised plant, presenting characters not combined in the modern world, and, perhaps illustrating what seems to be a general law of creation, that in the earlier periods low forms assumed characteristics subsequently confined to higher grades of being.

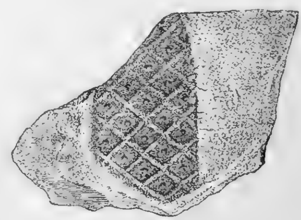

FIG. 2.—*Leptophleum rhombicum*—a Lycopodiaceous tree of the Devonian.

A second species of Psilophyton (*P. robustius*), also abundant at Gaspé, shows stouter stems than the former, more abundantly branching and with smaller leaves, often quite rudimentary. Its spore-cases are also of different form and borne in dense clusters on the sides of the stem. Masses of very slender branching filaments appear to indicate a third species (*P. elegans*) which is also found in the Devonian of St. John, New Brunswick. These species of Psilophyton occur both in the lower and middle Devonian, and, as will be mentioned in the sequel, they extend also into the Upper Silurian.

Decorticated and flattened stems of Psilophyton cannot be readily recognised, and except when their internal structure has been preserved, might be mistaken for algæ, a mistake which I believe has in some instances been made. Specimens of the barren stems (*var. ornatum*) might readily be referred to the genius *Lycopodites*.

Another genus of generalised type is that named by Haughton *Cyclostigma*. As found at Gaspé it presents slender stems with rounded scars, placed either spirally or in transverse rows, and giving origin to long rigid leaves. It had a slender axis of scalariform vessels, and fructification of the form of elongated spikes or strobiles is found with it. In many respects these plants resembled Psilophyton, and their affinities were distinctly Lycopodiaceous. Specimens from Ireland, in the Museum of the Geological Society, kindly shown to me by Mr. Etheridge, appear to show that in that country these plants attained the dimensions of trees, and had roots of the nature of Stigmaria. Mr. Carruthers has even suggested that they may be allied to *Syringodendron*, a group of Carboniferous trees connected with the *Sigillariæ*.

The genus *Lycopodites* is represented by a trailing species, bearing numerous oval strobiles (*L. Richardsoni*), a species quite close to many modern club-mosses (*L. Matthewi*), and a remakable pinnate form (*L. Vanuxemii*), which, though provisionally placed here, has been variously conjectured to resemble Ferns, Cycads, Algæ, and Graptolites. But the most remarkable Lycopodiaceous plants are the gigantic arboreal *Lepidodendra*, plants

FIG. 3.—*Cyclopteris* (*Archæopteris*) *Jacksoni*—a Devonian Fern, the American representative of *C. Hibernicus*.

which, while they begin in the Middle Devonian, become eminently expanded in numbers and magnitude in the Carboniferous. The common species in Eastern America (*L. Gaspianum*) was of slender and delicate form, very elegant, but probably not of large size. In the same family I would place my new genus *Leptophleum*, a portion of whose curiously-marked bark is represented in Fig. 2.

The *Calamites*, afterwards so largely developed in the Carboniferous, and to be replaced by true Equiseta in the Trias, make their first appearance in a large species (*C. inornatum*) in the Lower Devonian, and are represented in the middle and upper parts of the system by two other species, which extend upward into the Carboniferous. They are also represented in the Devonian of Germany and of Devonshire. The peculiar type indicated by the internal casts known as *Calamodendron* is likewise found in the Devonian.

More beautiful plants were the *Asterophyllites*, with more slender and widely branching stems, and broader leaves borne in whorls upon their branches. These plants have been confounded with leaves of Calamites, from which, however, they differ in form and nervation, and in the want of the oblique interrupted lines common to the true leaves of Calamites and to the branchlets of Equisetum. The Asterophyllites, and with them a species of *Sphenophyllum*, appear in the Middle Devonian.

No plants of the modern world are more beautiful in point of foliage than the Ferns, and of these a great number of species occur in the Middle and Upper Devonian. I must refer for details to my more full memoirs on the subject, and in the present paper shall content myself with a few general statements. Some of the generic forms of the Devonian, and perhaps a few of the species, extend into the Carboniferous; others are peculiar to the Devonian; and among these, forms allied to the modern Hymenophyllum and Trichomanes appear to prevail. One remarkable type, *Cyclopteris* (*Archæopteris*) *Hibernicus*, with its American allies, *C. Jacksoni*, &c., extends in the Upper Devonian over both continents, yet is wanting in the Carboniferous. Tree ferns also existed in the Devonian. Two species have been found by Dr. Newberry in Ohio, and remarkable erect trunks have been obtained by Professor Hall from Gilboa, in the State of New York. The latter are surrounded by aerial roots, and thus belong to the genus *Psaronius;* a genus which, however, must be artificial, since in modern tree ferns aerial roots often clothe the lower part of the stems while absent from the upper part. The only indication as yet of a tree fern in the Old World is the *Caulopteris Peachii*, of Salter, from the Old Red of Scotland. It is further remarkable that the ferns of the genus Archæopteris are much more large and luxuriant in Ireland than in America, and that in both regions they characterise the upper member of the system.

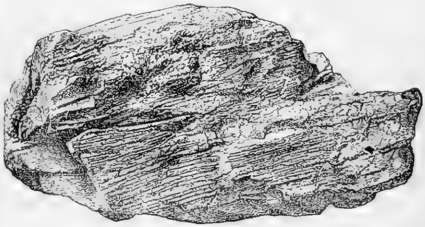

FIG. 4.—*Prototaxites Logani*—the oldest known tree. (Fragment of the trunk, much reduced.)

Of the plants of the Palæozoic world, none are more mysterious than those known to us by the name *Sigillariæ*, and distinguished by the arrangement of their leaves in vertical series, on stems and branches often ribbed longitudinally, and by the possession of those remarkable roots furnished with rootlets regularly articulated and spirally arranged, the Stigmariæ. It seems evident that this group of plants included numerous species, differing from each other both in form and structure. Still, as a whole, they present very characteristic forms dissimilar from those of their contemporaries, and still more unlike anything now living. I believe that many of them were Gymnosperms, or at the least, Acrogens with stems as complicated as those of Gymnosperms. In the Carboniferous period these plants have a close connection with the occurrence of coal. Nearly every bed of this mineral has under it a "Stigmaria underclay," which is a fossil soil on which a forest of Sigillariæ has grown, and the remains of these trees are very abundant in the coal and the accompanying beds. Hence the Sigillariæ of the coal-period are regarded as the plants most important in the accumulation of coal. In the Devonian, as far as we yet know, they did not attain to this utility, and in the lower part of the system at least, the rhizomata of Psilophyton seem to have occupied the place afterwards held by the Stigmariæ. In connection with this, it is to be remarked that the Sigillariæ of the Erian period seem to have been few, and of small dimensions in comparison with those of the coal.

Rising still higher in the vegetable kingdom, and arriving at unquestionable Gymnosperms, we find in the Devonian of Eastern America, and also, I believe, in that of Scotland and Germany, trunks which may be referred to Coniferæ. In the Middle and Upper Devonian these present the structure of modern Araucarian pines, or that modification of it belonging to the Carboniferous trees of the genus *Dadoxylon*. In the Lower Devonian we have what seems to be a simplification of the Coniferous structure, in the cylindrical wood-cells, marked only with spiral threads, found in the genus *Prototaxites*. These trees are very abundant as drift trunks in the Lower Devonian, down almost to its bottom beds, and sometimes attain to a diameter of three feet. Though of a structure so lax that it is comparable only with the youngest stems of ordinary Coniferæ, these trees must have been durable, and they are furnished both with medullary rays and rings of annual growth. Unfortunately we know nothing of their foliage or fruit. Fig. 4 represents a fragment of the wood of one of these trees, mineralised by infiltration of the tissues with silica, so that the structure is preserved.

But for one little fragment of wood, we should have had no indication of the existence in the Erian of any trees of higher organisation than the Conifers. This fragment, found by Professor Hall at Eighteen-mile Creek, Lake Erie, has the dotted vessels characteristic of ordinary Exogens, and unquestionably indicates a plant of the highest kind of organisation. Until confirmed by other facts, this discovery may be received with doubt, but I believe it can be relied on.

Our knowledge of the flora of the Upper Silurian is at present nearly in the same state with that of the Middle and Lower Devonian ten years ago. I know in the Upper Silurian of Canada but two species of *Psilophyton*, both apparently identical with Devonian forms. In England, besides the spore-cases known by the generic name *Pachytheca*, there exist in the collections of the Geological Survey fragments of wood and bark which I believe indicate two additional species. In Germany three or four species are known in rocks of this age. All of these plants appear to be Acrogens allied to Lycopodiaceæ. That these few species constitute the whole flora of the Upper Silurian we can scarcely believe. They occur in marine formations, and were probably drifted far from the somewhat limited land-surfaces which existed in the explored parts of the Upper Silurian areas. When we obtain access to deposits of this age formed in shallows or estuaries, we may hope to find a flora of greater richness; and, judging from present indications, not dissimilar from that of the Lower Devonian.

With the exception of some remains which I believe to be of very doubtful character, the Lower Silurian has as yet afforded no remains of land plants, and in North America, at least, this is very significant, inasmuch as we have, in the Potsdam sandstone, extensive sandy flats of this period, in which we might expect to find drifted trunks of trees, if such had existed. But the search is not hopeless, and we may yet find some estuary deposit on the margin of the ancient Laurentian continent, in whose beds the plants of that old land may occur.

Lastly, for reasons stated in a paper lately published in the Proceedings of the Geological Society, I believe that the extensive deposits of graphite, which exist in the Laurentian of Canada, are of vegetable origin, and possibly in part produced by land plants, as yet altogether unknown to us. If the Palæozoic was the age of Acrogens, the Eozoic may have been that of Anophytes and Thallophytes. Its plants may have consisted of gigantic mosses and lichens, presenting us with a phase of vegetable existence bearing the same relation to that of the Palæozoic, which the latter bears to that of more modern periods. But there is another and a more startling possibility, that the Laurentian may have been the period when vegetable life culminated on our planet, and existed in its highest and grandest forms, before it was brought into subordination to the higher life of the animal. The solution of these questions belongs to the future of geology, and opens up avenues not merely for speculation but also for practical work.

The above must be regarded as merely a sketch of the present aspect of the subject to which it relates. Details must be sought elsewhere. J. W. DAWSON

NOTES

THE Royal Commission on Scientific Instruction and the Advancement of Science has already begun work. A second meeting was held on the 31st ult. at Devonshire House. Present: The Duke of Devonshire, K.G., chairman; the Marquis of Lansdowne; Sir J. Lubbock, Bart., M.P., F.R.S.; Sir J. P. Kay Shuttleworth, Bart.; Mr. B. Samuelson, M.P.; Dr. Sharpey, Sec. R.S.; Professor Huxley, F.R.S.; Dr. W. A. Miller, Treas. R.S.; and the secretary, Mr. J. Norman Lockyer, F.R.S.

A MEETING of the Syndicate appointed to consider the ways and means of establishing a Chair of Experimental Physics at Cambridge was held on Tuesday last.

WE may remind our Astronomical readers that the Visitation of the Royal Observatory at Greenwich takes place at 3 P.M. on Saturday.

THE annual meeting for the election of Fellows into the Royal Society was held this morning.

THE Geographical Society of Paris has bestowed a well-earned honour on our countryman, Mr. Alfred R. Wallace, by awarding him one of their gold medals for his researches in the Malay Archipelago.

AT the meeting of the French Academy, held on the 23rd ult., the following list of candidates for the place vacant by the death of Professor Magnus was presented by the Secret Committee:—1, Mr. Joule, 2, MM. Ångström, Billet, Dove, Grove, Henry, Jacobi, Lloyd, Riess, Stokes, Tyndall, Volpicelli, and Sir William Thomson.

THE Gresham lectures for the present week at the Gresham College, Basinghall Street, are by Dr. Symes Thompson, on the 2nd, 3rd, and 4th of June, at 7 P.M., on "The Epidemics of the Middle Ages," on "Sedatives," and on "Narcotics." They are free to the public, and will be illustrated with diagrams, tables, and chemical experiments.

THE municipal administration of Paris has decided on publishing a series of documents on the history of the capital. The commission appointed for this purpose has placed at the head of its programme a sketch of the pre-historic epochs, and has entrusted this labour to M. Belgrand, who is well known in the scientific world by his important works on the basin of Paris. In presenting to the Academy a *résumé* of his work, he divides it into four parts—the diluvian epoch, the great water-courses of the age of stone, the history of the peat-mosses, and the palæontological history of the basin of the Seine during the quaternary epoch. Our Common Council of London has a splendid opportunity of distinguishing itself in the same way; is it too much to expect of such an august body?

WE perceive with great pleasure that the Radcliffe Library at Oxford is being adapted more completely to the wants of students of science. Students in any department of natural knowledge, who need scientific works, maps, or plans which they do not find in the library, are invited to record their wants in a book kept for the purpose. There is a standard microscope, by Powell and Lealand, attached to the library, for the comparison of objects with the illustrated works of the library.

THE Royal Dublin Society have commissioned Mr. Catterson Smith to paint a portrait of Dr. Joly, to be placed in their library. It will be remembered that Dr. Joly presented the Royal Dublin Society with a large and valuable library of modern books. The society already possessed a library of works on Science and Fine Arts, which, however, was very deficient in works of modern literature. It will now, with the addition of the Joly library, be one of the most useful public libraries in Great Britain and Ireland.

THE Surgical Society of Ireland gave a grand *conversazione* to its members and to the medical men of Dublin on Tuesday, the 24th ult. His Excellency the Lord Lieutenant was present. The *conversazione* was held in the Royal College of Surgeons. The small museum of the college was lit up, and looked uncommonly well. A series of interesting microscopical objects were exhibited by Dr. Barker, Mr. William Archer, Rev. Eugene O'Meara, and others.

M. DE CALIGNY called the attention of the French Academy, at its meeting of the 16th of May, to two prizes founded in 1867 by the Marquis d'Ourches, for the indications of the means of preventing people from being buried alive. The first is a prize of 20,000 frs. for the discovery of a simple method of recognising with certainty the signs of actual death. This method must be adapted to the capacities of the most ignorant. The second, a prize of 5,000 frs., is for the discovery of a means of recognising death by means of electricity or galvanism, or by some other process requiring knowledge for its application. These prizes are to be decided by the Imperial Academy of Medicine, within five years from the 22nd April, 1868.

ONE of the best signs of the times we have seen lately is the third annual report of the Rugby School Natural History Society for 1869, as giving satisfactory evidence of the interest taken in natural science by at all events a few Rugbeians. The botanical report for 1869 records the dates of flowering of 340 plants, which seem to have been carefully observed for three successive years, and a few new localities or plants new to the neighbourhood. We find also a thoroughly Darwinian paper by Mr. J. M. Wilson on "A remarkable instance of protective mimicry among the *Lepidoptera*;" the well-known example of the female of *Papilio Merope*, with an illustration; one on "English Snakes and the Blind-worm," by Mr. N. Masterman, secretary of the society; an elaborate account by Mr. J. M. Wilson of the "Drifts, Gravels, and Alluvial Soils of Rugby and its neighbourhood," illustrated by some carefully-executed drawings; a paper by the Rev. T. N. Hutchinson "On Spectrum Analysis and what it has done;" and one on "Norway," by Mr. Sedgwick. Other papers not reported appear to have been read at the meetings, which must have been interesting ones. We believe no greater service can be rendered to the spread of a taste for natural science than an encouragement of these efforts at our public schools.

A NATURAL History Society has been organised in Baltimore, to be called the Maryland Academy of Sciences; its object being to promote scientific research, and to collect, preserve, and diffuse information relating to the sciences, especially those connected with the natural history of Maryland. Its first president is Philip T. Tyson; Vice-president, Dr. John G. Morris; Corresponding Secretary, Dr. Charles C. Bombaugh.

THE *Feuille des Jeunes Naturalistes* is a praiseworthy attempt by a small body of French naturalists to establish an international school journal of science and natural history for the boys of France, Germany, and England. Contributions, written in either of the three languages, are invited from any schoolboy; and they will be received from no other source. The proprietors are especially anxious to interest our English schools in their enterprise; and we gladly give publicity to a very novel and very admirable scheme. The subscription is four francs per annum. The editor is M. E. Dollfus, Dornach, Haut Rhin.

IN reference to Mr. Murphy's communication in our last number respecting the purplish pink colour of the sunlight, we learn from correspondents that it was noticed also at Tynemouth, at 5 P.M., on Sunday, the 22nd ult.; at Cambridge, at 10 A.M., on Monday, and in Gloucestershire on both these days. In all these cases the sky is described as being hasy at the time.

THE *American Gas-Light Journal* speaks of an invention which aims at the entire abolition of oils and all other lubricating material for boxes, slides, and every condition of motion where metallic friction is to be overcome or expected. It is claimed that such a result has been fully achieved, and there are engines now running with this material, which the proprietors aver have worked to complete satisfaction for weeks and months. It is the work of a scientific and practical gentleman, well known both in America and in Europe, who has spent a great many years in the study of physical forces and their effects, with especial reference to metals. The exact nature of the present invention cannot be given, for the reason that patents are being sought for in several countries in Europe, and any clear description of the materials and processes would be likely to defeat that end. It may, however, be said, in a general way, that the discovery—which has received the name of Metalline—consists of such combinations and manipulations of various metallic substances, as to make a surface on which the ordinary axles, cranks, pins, slides, &c., of iron, steel, brass, or any other metal, will run with much less friction, without heat that comes within the slightest possibility of danger, and without increase (in fact an actual decrease is claimed) of the motive power used. These, briefly, are the claims, and the inventor refers to a large number of trustworthy gentlemen who have examined and tried the thing, and speak from actual knowledge.

A MOVEMENT is being set on foot in Germany for an alteration in the laws regulating the sending of dangerous substances by rail. Hitherto ether, alcohol, phosphorus, &c., can only be sent by special trains plying but once a week between the principal stations. Dry guncotton is entirely excluded from the railways; manufacturers are in the habit of sending it moistened with a certain quantity of spirits of wine; it thereby loses its explosive properties and burns like ordinary cotton.

WE are glad to see "Cassell's Popular Educator" still devoting a large proportion of its space to science. In the June number articles occur devoted to the following natural sciences:—Ethnology, geology (illustrated with woodcuts, which we recognise as taken from well-known handbooks), botany (a continuation of a series of excellent elementary articles), mineralogy, meteorology, and the science of heat.

DR. C. H. SCHAIBLE, of the Royal Military Academy, Woolwich, sends us "The State and Education," an historical and critical essay, with special reference to Educational Reform. The subject is exhaustively treated under the heads—Historical Sketch of State education; Compulsory Instruction; State Instruction; Organisation of State Instruction in Germany; Voluntary Instruction; State Control of Instruction; and Reform of Education in England; and the general conclusion arrived at may be stated to be the necessity of general, compulsory, unsectarian, primary instruction for all children from six to fourteen years of age, under State control, and gratuitous for the poor.

AT a recent meeting of the French Horticultural Society, M. Duchartre gave a history of the investigations into the nature of the phenomenon of variegation. He considers it to be now completely established that variegation is of the nature of a contagious disease, which in the case of grafted plants can be communicated both from the stock to the graft and from the graft to the stock.

It has long been laid down as a maxim in botanical hand-books that variegation and double flowering never go together. Many botanists have, however recently doubted whether the law always holds good, and that the double phenomenon may sometimes occur appears now to be definitely established by an article contributed by Prof. Morren, of Liège, to the April and May number of the *Belgique Horticole*, in which he gives a description, accompanied by a drawing, of a wall-flower possessing both double flowers and variegated leaves. The plant has now been grown for several years by M. Em. Rodigas, of St. Trond.

M. Schutzenberger, Professor of Clinical Medicine at Strasburg, publishes an essay on higher instruction, in which he compares the system in vogue in France to those pursued in Germany, England, and America, and discusses the influence of the Napoleonic University system on the institutions for higher instruction.

Dr. R. Wolf, Professor at Zürich, reprints "The Discoveries of the Telescope, and their results in Astronomy," a lecture given at the public hall in Zürich.

From the same author we have a second part of a Manual of Mathematics, Physics, Geodesy, and Astronomy, copiously illustrated with woodcuts.

Professor Miquel, of the Hague, publishes the first part of a "Catalogus Musei botanici Lugduno-batavi," being a catalogue of the flora of Japan, with lists of the Japanese collections contained in the Leyden Herbarium.

The eighth vo'ume of Dr. Frisch's collected works of Kepler, containing the "Collectanea ex codicibus Pulkoviensibus," the "Judicium matris Kepleri," and some smaller treatises, lies on our table.

A Magazine is now published at Heidelberg under the title *Annalen der Oenologie* devoted, entirely to the discussion of the cultivation of the vine and the manufacture of wine. It is edited by Dr. Blankenhorn and Dr. Rösler.

Perhaps the most extraordinary instance of excessive and depraved appetite on record is that of a French soldier, named Tarare, whose case is described in vol. xxi. of the *Dictionnaire des Sciences Médicales*, by Dr. Percy. He was born near Lyons, and came up to Paris, where his first exploit was to eat a basket of apples—at a friend's expense. On various occasions he swallowed a series of corks and other indigestible materials, which produced such violent colic that he was obliged to attend the Hotel Dieu, and whilst being examined almost managed to swallow the watch-chain and seals of the surgeon in attendance, M. Giraud. Desault, on the occasion of one of these attacks of colic, tried to frighten him out of his gross habits by declaring that it would be necessary to open his stomach, and arranged the instruments; he ran away, and relieved himself by copious draughts of warm water. Soon after he found that his appetite had really increased to an excessive amount, pro› bly owing to the continued irritation produced by these absurd tricks. At 17 years of age, when only weighing 100lb., he could eat 24lb. of beef in as many hours. He now entered the army, and being recognised by the Surgeon-Major, M. Courville of the 9th Regiment of Hussars, he was detained for the sake of curiosity. From the day of his admission, he was ordered quadruple rations, with pickings and waste meat, but often slipped into the dispensary to finish off a poultice or two. One day he was observed to seize a large cat: and, after sucking its blood, left, in a very short time, only cleanly picked bones, the hair being rejected in the course of about half an hour, like other carnivora. He was fond of serpents and eels, swallowing them whole. On another occasion he consumed in a few minutes a repast, spread out for fifteen German workpeople, of milk, &c., after which he was blown out like a balloon. In the presence of some officers he swallowed, at one sitting, 30lb. of liver and lights. His insatiable appetite was for once in his life made useful, by his being selected to convey a correspondence between General Beauharnais and a French colonel, which was inserted in a box and swallowed; but he was caught and soundly thrashed. He fell under suspicion of having eaten a child fourteen months old. It is stated that he was of mild and gentle manners and aspect. After death his stomach was found in a very diseased condition.

The Tyneside Naturalists' Field Club is not only one of the most vigorous and truly scientific in the country, but is nearly the oldest, and probably one of the largest, numbering 600 members. At its 24th anniversary meeting recently held, the following gentlemen were elected as officers, viz.:—President—Mr. George S. Brady; Vice-Presidents—the Revs. W. Featherstonehaugh, B.A., J. F. Bigge, M.A., H. B. Tristram, LL.D., W. Greenwell, M.A., G. C. Abbs, M.A., A. M. Norman, M.A., J. C. Bruce, LL.D., A. Bethune, M.A., and R. F. Wheeler, M.A., Sir W. C. Trevelyan, Bart., Drs. Embleton and Charlton, and Messrs. R. B. Bowman, Richard Howse, George Hodge, Ralph Carr, R. Ingham, T. Sopwith, Rowland Burdon, George Wailes, and E. J. I. Browell. Treasurer—Mr. R. Y. Green. Local Secretaries—Durham, Mr. John Booth; Hexham, the Rev. W. T. Shields; Morpeth, Mr. W. Creighton. Committee—Messrs. Thomas Atthey, Jos. Blacklock, T. J. Bold, James Clepham, John Coppin, W. Dinning, D. O. Drewett, Albany Hancock, John Hancock, Jos. Watson, A. F. Marreco, and Dr. Philipson. Auditors—Messrs. J. S. Foster, and T. P. Barkas.

We have received from the Canadian Government Emigration Office a copy of the Year-Book of Canada for 1870, containing a vast mass of information respecting British North America, which will be very useful to those interested in the country. It also contains an interesting paper on the Climatology of British North America, written by Mr. G. T. Kingston, the director of the Magnetic Observatory at Toronto; an account of the Educational Establishments, by Mr. J. C. Patterson, of Ottawa; and a paper by Mr. T. Cross on Mining in Canada, both of which are of interest. One of the chief points in the latter paper is the statement that mining in Canada is every year assuming a more steady and settled character. The gold working in Nova Scotia has given less favourable results than in 1867, but the falling off is ascribed to the depth at which the lodes were being worked, and the inadequate nature of the machinery employed. The total yield in 1868 was 20,541 ounces; and the inspector expresses the opinion that gold mining in the province is yet far from that development which may be expected.

M. l'Abbe Vaullet, director of the hospital of Annecy, believes, says *Les Mondes*, that he has proved that the temperature of the department of Haute Savoie has gradually risen in a very appreciable manner during the last forty years. The mean temperature, formerly 8° to 9° C., now exceeds 10·5°. The proofs he adduces are the advance of the cultivation of the vine, and of grain, and the retreat of the glaciers. The causes of this amelioration of the climate M. Vaullet considers to be the disforesting, the destruction of hedge-rows, the clearing of uncultivated lands, the multiplication and maintenance of roads, the draining of marshes, and the increase of population and of cattle.

Dr. Thomas Andrews has recently delivered two lectures in the chemical lecture-room of Queen's College, Belfast—the first on carbon, the second on carbonic acid and carbonic oxide. They form a portion of a course of strictly scientific lectures, the attendance being confined entirely to working men, the admission by ticket, but without charge. On each occasion the hall was crowded.

ON THE PROGRESS OF BOTANY DURING 1869 *

I.

It had been my intention on the present occasion to have carried on the sketches of the general progress of biological science which I had attempted in 1862, 1864, 1866, and 1868; but I have, from various causes, being unable to bestow so much time as usual to the preparation of my address, and feel obliged to confine myself to a few points connected with subjects of special interest to myself, which, within the last two or three years, have made considerable advances.

The most striking are, without doubt, the results obtained from the recent explorations of the deep-sea faunas, and from the investigation of the tertiary deposits of the Arctic regions, which, although affecting two very different branches of natural science, I here couple together, as tending both of them to elucidate, in a remarkable degree, one of the most important among the disputed questions in biological history, the continuity of life through successive geological periods.

An excellent general sketch of the first discovery and progressive investigation of animal life at the bottom of the sea at great depths, up to the close of the season of 1868, is given by Dr. Carpenter in the Proceedings of the Royal Society, vol. xvii. No. 107, for December 17, 1868; the results of the still more important expedition of the past year have as yet been only generally stated by Mr. Gwyn Jeffreys, in the numbers of NATURE for Dec. 2 and 9, 1869; and by Dr. Carpenter in a lecture to the Royal Institution, published in the numbers of *Scientific Opinion* for March 23 and 30, and April 6 and 13 of the present year; and further details, as to the Madreporaria, are given by Dr. Duncan in the Proceedings of the Royal Society, 1 vol. xviii., No. 118, for March 24 of the present year; whilst, in North America, the chief conclusions to be drawn from those researches into the deep-sea fauna are clearly and concisely enumerated by Prof. Verrill, in the number of *Silliman's Journal* for January last, and some of the more detailed reports of the American explorations by Louis and Alexander Agassiz, and others, have been published in the Bulletin of the Museum of Comparative Zoology at Harvard College, Nos. 6, 7, and 9 to 13. For the knowledge of the data furnished by the Tertiary deposits of the Arctic regions we are indebted almost exclusively to the acute observations and able elucidations of Prof. O. Heer, in his "Flora Fossilis Arctica," in his paper on the fossil plants collected by Mr. Whymper in North Greenland, published in the last part of the Philosophical Transactions for 1869, and in the as yet only short general sketch of the results of the Swedish Spitzbergen expeditions, contained in the Genevan *Bibliothèque Universelle Archives Scientifiques* for December 1869.

It would be useless for me here to retrace, after Dr. Carpenter and Prof. Verrill, the outlines of the revolution which these marine discoveries have caused in the previously conceived theories, both as to the geographical distribution of marine animals, and the relative influences upon it of temperature and depth, and as to the actual temperature of the deep seas, or to enter into any details of the enormous additions thus made to our knowledge of the diversities of organic life; and it would be still further from my province to consider the geological conclusions to be drawn from them. My object is more especially to point out how these respective dips into the early history of marine animals and of terrestrial forests have afforded the strongest evidence we have yet obtained, that apparently unlimited permanency and total change can go on side by side without requiring for the latter any general catastrophe that should preclude the former.

There was a time, as we learn, when our chalk-cliffs, now high and dry, were being formed at the bottom of the sea, by the gradual growth and decay of Globigerinæ and the animals that fed on them; amongst others, for instance, *Rhizocrinus* and *Terebratulina Caput-serpentis*; and when the upheaval of the ground into an element where these animals could no longer live, arrested their progress in that direction, they had already spread over an area sufficiently extensive for some part of their race to maintain itself undisturbed, and so on from that time to the present day, by gradual dispersion or migration, in one direction or another, the same *Rhizocrinus* and *Terebratulina* have always been in possession of some genial locality, where they have continued from generation to generation, and still continue, with Globigerinæ and other animals, forming chalk at the bottom of the sea, unchanged in structural character, and rigidly conservative in habits and mode of life through the vast geological periods they have witnessed. So also there was a time when the hill sides of Greenland and Spitzbergen, now enveloped in never-melting ice and snows, were under a genial climate, clothed with forests, in which flourished *Taxodium distichum* (with *Sequoiæ*, *Magnoliæ*, and many others), and when these forests were destroyed by the general refrigeration, the Taxodium occupied an area extensive enough to include some districts in which it could still live and propagate; and whatever vicissitudes it may have met with in some parts, or even in the whole, of its original area, it has, by gradual extension and migration, always found some spot where it has gone on and thriven, and continued its race from generation to generation down to the present day, unchanged in character, and unmodified in its requirements. In both cases the permanent animals of the deep-sea bottom and the permanent trees of the terrestrial forests have witnessed a more or less partial or complete change in the races amongst which they were commingled. Some of these primitive associates, not endowed with the same means of dispersion, and confined to their original areas, were extinguished by the geological or climatological changes, and replaced by other races amongst which the permanent ones had penetrated, or by new immigrants from other areas; others again had spread like the permanent ones, but were less fitted for the new conditions in which they had become placed, and in the course of successive generations have been gradually modified by the Darwinian process of natural selection, the survivors of the fittest only among their descendants. If, in after times, the upheaved sea-bottom becomes again submerged, the frozen land becomes again suited for vegetation, they are again respectively covered with marine animals or vegetable life, derived from more or less adjacent regions, and more or less different from that which they originally supported, in proportion to the lapse of time and extent of physical changes which had intervened. Thus it is that we can perfectly agree with Dr. Duncan, that "this persistence (of type and species through ages, whilst their surroundings were changed over and over again) does not indicate that there have not been sufficient physical and biological changes during its lasting to alter the face of all things enough to give geologists the right of asserting the succession of several periods;" but we can, at the same time, feel that Dr Carpenter is in one sense justified in the proposition, that we may be said to be still living in the Cretaceous period. The chalk formation has been going on over some part of the North Atlantic sea-bed from its first commencement to the present day, in unbroken continuity and unchanged in character.

If once we admit as demonstrated the coexistence of indefinite permanency, and of gradual or rapid change of different races in the same area and under the same physical conditions, according to their constitutional idiosyncrasies, and also that one and the same race may be permanent, or more or less changing, according to local climatological or other physical conditions in which it may be placed, we have removed one of the great obstacles to the investigation of the history of races, the apparent want of uniformity in the laws which regulate the succession of forms. We may not only trace, with more confidence, such modifications of race through successive geological periods as Prof. Huxley has recently exhibited to us in respect of the horse, but we can understand more readily the absolute identity of certain species of plants inhabiting widely dissevered areas, of which the great majority of species are more or less different. One of the arguments brought forward against the community of origin of representative species in distant regions, such as temperate Europe and the Australian Alps, the Arctic Circle and Antarctic America, the Eastern United States and Japan respectively,—an argument which long appeared to me to have considerable weight,—was this:—that if disseverance and subsequent isolation results necessarily in a gradual modification by natural selection, how is it that when all are subjected to the same influences, the descendants of some races have become almost generically distinct in the two regions, whilst others are universally acknowledged as congeners, but specifically distinct, and others again are only slight varieties or have remained absolutely identical? To this we can now reply, with some confidence, that there is no more absolute uniformity in the results of natural selection than in any other of the phenomena of life. External influences act differently upon

* Address of G. Bentham, F.R.S., President of the Linnean Society, read at the Anniversary Meeting, May 24, 1870.

different constitutions. Were we to remove the whole flora and fauna of a country to a distant region, or, what comes to the same thing, change the external conditions of that flora and fauna, as to climate, physical influences, natural enemies, or other causes of destruction, means of protection, &c., we should now be taught to expect that some of the individual races would at once perish; others, more or less affected, might continue through several generations, but with decreasing vigour, and, in the course of years or ages, gradually die out, to be replaced by more vigorous neighbours or invaders; others again might see amongst their numerous and ever-varying offspring some few slightly modified, so as to be better suited for the new order of things; and experience has repeatedly shown that the change once begun may go on increasing through successive generations and a permanent representative species is formed, and some few races may find themselves quite as happy and vigorous under their new circumstances as under the old, and may go on as before, unchanged and unchanging.

Taking into consideration the new lights that have been thrown upon these subjects by the above investigations and by the numerous observations called forth by the development of the great Darwinian theories, amongst which I may include a few points adverted to in a paper on Cassia which I laid before you last year, it appears to me that in plants, at least, we may almost watch, as it were, the process of specific change actually going on, or at least we may observe different races now living in different stages of progress, from the slight local variation to the distinct species and genus. As a first step we may take, for instance, those races which are regarded by the majority of botanists as very variable species, such as *Rubus fruticosus*, *Rosa canina*, *Zornia diphylla*, *Cassia mimosoides*, &c.: we shall find in each some one form, which we call typical, generally prevalent over the greater part of the area of the race, whilst others more or less aberrant are more or less restricted to particular localities, the same varieties not occurring in disconnected stations with precisely the same combinations of character; and in the same proportions local and representative varieties and sub-species are being formed, but have not yet obtained sufficient advantages to prevent their being kept in check by their inter-communication (and probable cross-breeding) with their more robust type. The British rubologist or rhodologist transported to the south of France or to Hungary will still find one, or perhaps two or three, forms of bramble and dog-rose with which he is familiar; but if he wishes to discriminate the thirty or forty varieties or sub-species upon which he had spent so much labour and acuteness at home, he will find that he must recommence with a series of forms and combinations of characters quite new to him. The species is still the same; the varieties are changed. As examples of what we may call a second stage in the formation of species, we may adduce such plants as *Pelargonium australe* or *grossularoides* and *Nicotiana suaveolens* or *angustifolia*, to which I alluded in the above-mentioned paper on *Cassia*. Here we have one race, of no higher than specific grade in the ordinary acceptance of the term, inhabiting two countries which have long been widely dissevered (in the one case South Africa and Australia, in the other Chili and Australia), which, if originally introduced by accident from one country to the other, have been so at a time so remote as thoroughly to have acquired an indigenous character in both; in both are they widely spread and highly diversified, but amongst all their varieties one form only is identical in the two countries (*Pelargonium australe*, var. *erodiodes*, and *P. grossularioides*, var. *anceps; Nicotiana suaveolens*, var. *angustifolia*, and *N. angustifolia*, var. *acuminata*), and that so comparatively a rare one that it may be regarded as being in the course of extinction; whilst all other varieties, some of them very numerous in individuals over extended areas, and all connected by nice gradations, diverge nevertheless in the two countries in different directions and with different combinations of characters, no two of them growing in the two countries being at all connected but through the medium of that one which is still common to both. When that shall have expired the distinct species may be considered as established. A still further advance in specific change is exemplified in *Cassia* itself, in which I have shown that no less than eight or nine different modifications of type, sectional and sub-sectional, are common to South America, tropical Africa, and Australia, but without any specific or, at least, sub-specific identity, except perhaps in a few cases where a more modern interchange may be presumed. The original common specific types are extinct, the species have risen into sections. Common types of a still higher order have disappeared in the case of Proteaceæ, an order so perfectly natural and so clearly defined that we cannot refrain from speculating on the community of origin of the African and of the Australian races, both exceedingly numerous and reducible to definite groups—large and small well-marked genera in both countries, and yet not a single genus common to the two; not only the species, but the genera themselves, have become geographical. As in the varieties of *Pelargonium* and *Nicotiana*, so in that of the species of *Cassia* and of the genera of Proteaceæ, it is not to be denied that precisely similar modifications of character are observed in the two countries; but these modifications are differently combined, the changes in the organs are differently correlated. In Asiatico-African *Chamæcristes* a tendency to a particular change in the venation of the leaflet is accompanied by a certain change in the petiolar gland; in America the same change in the gland is correlated with a different alteration in the venation. In Australian Proteaceæ the glands of the torus are constantly deficient, with a certain inflorescence (cones with imbricate scales), which is always accompanied by them in Africa.

In selecting the above instances for illustration of what we may, without much strain upon the imagination, suppose to be cases of progressive change in races, it is not that they are isolated cases or exceptionally appropriate; for innumerable similar ones might be adduced. In the course of the detailed examination I have had successively to make of the floras of Europe, N.W. America, tropical America, tropical Africa, China, and Australia, I have everywhere observed that community of general type, in regions now dissevered, is, when once varied, accompanied by more or less of divergence in more special characters in different directions in the different countries.

G. BENTHAM

SURFACE-OCEANIC LIFE

IN the waiting-room at the Admiralty is a drawing 12 feet by 8 feet, which is attracting the attention of numerous scientific and naval men, who thoroughly appreciate the novel and complete manner in which the several groups of interesting marine life have been arranged, and the system and regularity upon which the arrangement has been carried out, and we may also add, for the benefit of the curious, that the beauty and colour of these grotesque forms would exceed the imagination of Gustave Doré. The work was entirely executed in H.M.S. *Rodney*, on her passage from China to England during the last six months, and extends over the China Sea, Indian and Atlantic Oceans. The subject of surface-oceanic life is particularly acceptable at the present time, as Dr. Carpenter, Mr. Gwyn Jeffreys, and Professor Wyville Thomson were last season engaged in examining the deep-sea life of the neighbouring ocean, and are likely to extend their investigations into the Bay of Biscay and Mediterranean Sea during the summer. These deep sea explorations should be energetically pursued, and we may earnestly hope that it will not be long before an honest rivalry is maintained in the Atlantic and European seas, and that other oceans and parts of the world may be dipped into by voyagers, for contributions to this useful branch of science.

Those who only know the sea under the aspect which it usually presents round our own coasts will hardly be acquainted with the fact that the surface of the ocean forms a world in itself, inhabited by myriads of strange and delicate creatures, as distinct in its conditions from the shore world as from the inhabitants of the dark mysterious depths whose oozy plain, shut off from the day by three miles' thickness of water, is tenanted by the lingering and stunted refugees of a world of animals now for the most part extinct. The creatures which inhabit the surface of the ocean are very many of them born and bred there; others, on the contrary, have left their parents at a very early age, being carried away from the shore by surface currents and drifted out to sea, there to pass through ever-changing forms, until the time comes for their return to shallower places and a life of grovelling on the ground. Although this picture contains more than six hundred drawings of marine animals, it does not represent much more than one-third of the actual labour incurred, duplicate and fac-simile drawings of all the creatures having been originally made. The author of this picture, Mr. Francis Ingram Palmer, has been employed surveying the coasts of Japan and China, and it was on his passage home that he devoted his attention to this subject.

SCIENTIFIC SERIALS

Max Schultze's Archiv für Mikroskop Anatomie, Band vi. Heft. 2, 1870, is a paper by M. J. C. Eberth on the termination of the Cutaneous Nerves. Eberth's experiments were undertaken upon the skin of man, of rabbits, guinea-pigs, dogs, and cats, but chiefly on that of man and of Albino rabbits; the processes of pigment cells in the other animals often closely resembling nerves when stained with gold chloride. The strength of the solution that was used varied from ½ to 1 per cent., in which portions of skin were allowed to soak for from ¼ to 4 hours. In the cutis of man the nerves form first a rich web of dark-edged fasciculi, which break up into a plexus of fine fibrils and small fasciculi of fibrils. These soon lose their medullary sheath, and enter more or less vertically into the papillæ in the form of fine axis cylinders with fusiform nuclei lying upon them. He particularly insists that the final finest terminations which can be followed to the attached surface of the epithelium are free and do not form a plexus. He corroborates the statements of Langerhans respecting the presence of peculiar cells in the deeper parts of the epidermis of stellate and fusiform shape; often with a distinct nucleus. They blacken with chloride of gold; but neither Eberth nor Langerhans have been able to trace their connection with nerves. These cells usually send off from five to eight simple or branched processes towards the surface, but only one or two towards the cutis.

IN the *Annales des Sciences Naturelles, Zoologie*, Paris, 1870, p. 1, is a contribution by M. Léon Vaillant to the anatomical investigation of the genus Pontobdella, the principal points of which we extract. M. Vaillant states that he has had peculiar opportunities of observing this genus of the Hirudinidæ, and the particular species he has investigated is that of the *P. verrucata*, so called on account of the proper zoonites or segments of the animal supporting four tubercles, though the *cutaneous* segments or zoonites only bear two. The total number of cutaneous zoonites is 67. The anterior orifice of the digestive system is placed at the centre of the anterior sucker. The posterior orifice opens dorsally just in front of the posterior sucker. The skin presents a dermis and an epidermis, the latter being composed of a delicate cuticle and of a layer of epithelial cells, corresponding to the pigmentary layer of Moquin Tandon. The dermis is composed of cells concealed by a network of what appear to be anastomosing tubes. Beneath the skin, and almost forming part of it, is a dense layer of smooth muscular tissue, the external fibres of which are circular, the deeper longitudinal. By the agency of these the locomotion of the animal is chiefly effected. Between the muscular layer and the digestive tube an immense number of yellow granules are found, which appear to be of the same nature as the unicellular glands described by Leydig, possessing fine ducts, that can in some instances be followed to the skin, and therefore almost precluding the idea of their being hepatic organs. The nervous system presents 22 ganglia, excluding the œsophageal collar; the last one is the largest, and is found in the anal sucker. No eyes have been discovered in them, and their relations to the outer world appear to be restricted to those derived from the sense of touch. The digestive organs present no remarkable deviations from that of the leeches in general. Its divisions are a proboscis, with its sheath; a crop; the gastro-ilial portion, and the rectum. The jaws are reduced to three minute projecting points. The crop extends quite to the posterior part of the body, and presents a series of constrictions. The gastro-ilial portion is a single tube lying above the *cul de sac*, formed posteriorly by the ingluvies, and appears to correspond to the true stomach of other animals. The circulation is effected through a closed system of vessels, and the contents of these vessels are colourless, and destitute of corpuscles. M. Vaillant considers that the blood is represented properly by the fluid contained in the general cavity of the body, which contains definite morphological elements. There are four principal vessels, a dorsal, ventral, and two lateral, and these lie in the muscular layers. The dorsal and ventral vessels communicate freely by large branches; the lateral vessels receive their blood from a delicate plexus of vessels distributed on the intestine, which, however, communicates with the dorso-ventral system; and it is probable that an oscillation of the fluid is constantly occurring from one set of vessels into the other. On the whole, the vascular system is much less complicated here than in the leech. The respiratory function is effected essentially if not exclusively by the skin, and there is no special organ for its performance. In regard to the secretions, reference has already been made to the unicellular glands of the skin; and the only others are some peri-œsophageal glands, which are generally considered to be salivary, and the muciparous follicles, which are ovoid vessels, six in number, on each side, placed in the testicular region, and opening externally with a ciliated orifice. The sexes are united in the same individual. The eggs are deposited either separately or several together enveloped in a common capsule.

IN the third part of M. Brown Séquard's "*Archives de Physiologie*" are the results of an investigation of the mode in which nerves terminate in smooth muscular tissue, by M. Hénocque. He has examined the smooth muscles of numerous vertebrate animals and of man, with a great variety of reagents, as serum, pyroligneous acid, chromic acid, and chloride of gold and potassium, which in a strength of one part in 200 he particularly recommends. He finds that the appearances presented are the same in all animals, and in all organs; and states that in following out the nerves towards their peripheral terminations, they may be found to form three plexuses or networks—namely, a fundamental plexus, with which numerous ganglia are associated, and which is situated *outside* the smooth muscle; an intermediate plexus; and lastly an intra-muscular plexus situated in the interior of the fasciculi of smooth fibres. The terminal fibres are everywhere identical, they divide dichotomously or anastomose, and end in a slight button or swelling, or in a punctiform manner. These little buttons are seated in various parts of the smooth muscular fibre, more frequently round the muscles or at the surface of the muscular fibres, or, finally, between them.

THE *American Naturalist* for May commences with an interesting article on the Indians of California, their manners and customs, by Edward E. Chever; followed by one on the "Time of the Mammoths," by Professor N. S. Shaler, in which he gives a full account of the geological distribution on the American continent of the different species of the genus *Elephas*. W. G. Binney contributes a paper on the "Mollusks of our Cellars."

IN the *Revue des Cours Scientifiques* for May 21 we have M. Faye's important paper on the form of Comets, which occupied one of the *Soirées Scientifiques de la Sorbonne*, and a continuation of M. Bernard's series on Suffocation by the Fumes of Charcoal. In the number for May 28 is an epitome of M. Belgrand's *résumé*, presented to the Academy of Sciences, of the prehistoric history of the Paris basin, to which we refer in another column; the Rectorial Address, by M. H. Kopp, to the University of Heidelberg, on the State of Science during the Middle Ages; and a paper by M. Bert on Physiology and Zoology.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 19.—A paper was read, entitled "On the Effects of Alcohol (*Ethyl Alcohol*) on the Human Body," by Dr. Parkes and Count Cyprian Wollowicz. The experiments given in detail by the authors were undertaken with a view of testing the physiological and especially the dietetic effects of alcohol, and to clear up some points left doubtful by previous observers. They were fortunate in obtaining as the subject of experiment a healthy and very intelligent soldier at twenty-eight, 5 feet 6 inches in height, weighing from 134lb. to 136lb., with a clean, smooth skin, a clear bright eye, good teeth, largely developed, powerful muscles, and but little fat. As he had been accustomed to smoke, he was allowed half an ounce of tobacco daily, lest the deprivation of it might disturb his health. The amount of alcohol administered varied, but it was never carried so far as to produce any extreme symptoms of narcotism.

The plan of observation was as follows:—For twenty-six days the man remained on a diet precisely similar as to food and times of meals in every respect, except that for the first eight days he took only water (in the shape of coffee, tea, and simple water); for the next six days he added to this diet rectified spirit, in such proportion that he took, in divided quantities, on the first day one fluid ounce (= 28 c. c.) of absolute alcohol; on the second day two fluid ounces; on the third day four ounces, and on the fifth and sixth days eight ounces on each day. He then returned to water for six days, and then for three days took on each day half a bottle (= 12 ounces, or 341 c. c.) of fine brandy, containing 48 per cent. of alcohol. Then for three days more he returned to water.

There were thus five periods, viz., of water-drinking, alcohol, water, brandy, water.

Before commencing the experiments, the man, who had been accustomed to take one or two pints of beer daily, abstained altogether from any alcoholic liquid for ten days.

During the first few days there was a gradual increase in weight, owing probably to the food being rather greater and the exercise less than before; equilibrium was reached on the eighth day, and the weight remained almost unchanged during the alcoholic period. There was slight decrease after alcohol; and on the last brandy day a slight increase, which was maintained in the after period. The general result appears to be that (other conditions remaining constant) the effect of alcohol in modifying weight is quite unimportant. The results of the experiments may best be given in relation to the different functions of the body; and first in regard to the temperature of the axilla and rectum, it appeared that when taken as above described, alcohol and brandy produced little change in the temperature of either the axilla or rectum; but what effect there was, was rather in the direction of increase than of diminution. Secondly, in regard to the circulation, it was found that the pulse was increased both in frequency and volume, rising in number from 77·5 before alcohol to a max. of 94·7 with the largest doses. The capillary circulation was increased, shown by flushing of face and neck, &c. As conclusions from the sphygmographic observations that were made, it followed that there was increased frequency of the ventricular contractions of the head, and increased rapidity of each contraction, the ventricle therefore doing more work in a given time, the period of the heart being much shortened, and the blood moving more freely through the capillaries, so that the increased quantity of blood which it is to be presumed was thrown into the arteries, was very quickly got rid of. Thirdly, in regard to its action on the renal secretions, the authors show there was a decided increase in the amount of water eliminated; but they demonstrate in opposition to previous experimenters that, as long as the ingress of nitrogen is the same, 8oz. of absolute alcohol and 12oz. of brandy have no effect, or only a trifling effect, on the processes which end in the elimination of nitrogen by the urine, and most decidedly do not lessen the elimination. Further, the influence of alcohol on the elimination of chlorine and phosphoric acid, and upon the free acidity of the urine, is inconsiderable. The action of alcohol on the elimination of nitrogen by the alvine discharges was probably inconsiderable, and no experiments were made upon its effects upon the pulmonary excretion.

Putting together the evidence derived from the pulse as felt by the finger, from the state of the cutaneous vessels, and from the sphygmographic tracings, it seems fair to conclude that the chief effects of alcohol on the circulation in health are on the ventricles (the rapidity with which contractions are accomplished being greatly increased), and on the capillaries (which are dilated and allow blood to pass more freely through them).

As regards the mode in which alcohol is eliminated from the body from the application of a colour test, they are of opinion that a good deal must be eliminated by the lungs, and still more by the skin. Some also, though only a small proportion, must be given off by the renal and alvine discharges.

They found that one or two fluid ounces of absolute alcohol, in divided doses, increased the appetite; 4oz. lessened it, and larger quantities almost entirely destroyed it.

Estimating the daily work of the heart at 122 tons lifted one foot, the heart during the alcoholic period did daily work in excess equal to lifting 15·8 tons one foot, and in the last two days did extra work to the amount of 24 tons lifted as far. After the alcohol was omitted, the heart showed signs of weakness.

From the general results of the experiments, it appears, that any quantity over 2oz. of absolute alcohol would certainly do harm to this man, and that as every function was performed perfectly without it, its use was wholly unnecessary. They concluded by remarking that they were hardly prepared, notwithstanding their previous experience, for the ease with which appetite may be destroyed, the heart unduly excited, and the capillary circulation improperly increased. Yet they recognise the great practical benefit that may be derived from the use of alcohol in rousing a failing appetite, exciting a feeble heart, and accelerating a languid capillary circulation, though, for these objects to be fulfilled satisfactorily, there is necessity for great moderation and caution.

Ethnological Society, May 24.—Anniversary meeting, Prof. Huxley, president, in the chair. The report of the council and the treasurer's report were read and adopted. These reports showed that the position of the society was highly satisfactory. The President delivered an address, in which he gave a history of the efforts which have been made for amalgamating the Ethnological and Anthropological Societies, and hinted at the desirableness of union being effected between several societies having kindred objects. He also referred to the encouragement which the British Association had, since the Nottingham meeting, given to ethnological science, by allowing the Biological section (D) to resolve itself into departments. The following is the result of the ballot for officers and council:—President, Prof. Huxley, LL.D., F.R.S. Vice-presidents: Dr. A. Campbell, Sir John Lubbock, Bart., M.P., F.R.S., E. B. Tylor, Thomas Wright. Honorary Treasurer: H. G. Bohn. Honorary General Secretary: Colonel A. Lane Fox. Honorary Foreign Secretary: Hyde Clarke. Council: W. Blackmore, Prof. Busk, F.R.S., G. Campbell, Dr. Barnard Davis, W. Boyd Dawkins, F.R.S., J. Dickinson, Robert Dunn, J. W. Flower, David Forbes, F.R.S., A. W. Franks, Rev. Canon Greenwell, A. Hamilton, F. Hindmarsh, T. McK. Hughes, Dr. Richard King, Sir R. I. Murchison, Bart., K.C.B., J. F. McLennan, Rev. Dr. Nicholas, E. B. Pusey.

Linnean Society, May 24.—Anniversary meeting. The officers and council for the year 1870-71 were elected as follows:—President, G. Bentham, F.R.S. Treasurer, W. Wilson Saunders, F.R.S. Secretaries: F. Currey, F.R.S., and H. T. Stainton, F.R.S. Members of the Council: Thos. Anderson, M.D., John Ball, F.R.S., J. J. Bennett, F.R.S., George Busk, F.R.S., M. Foster, M.D., A. Grote, J. D. Hooker, M.D., F.R.S., Henry Lee, Major Parry, R. C. A. Prior, M.D., T. Thomson, M.D., F.R.S.

Short obituary notices having been read of fourteen fellows and five foreign members who had died during the year, the President proceeded to deliver his annual address, of which a full report will be found in another column.

Geological Society, May 11.—Joseph Prestwich, president, in the chair. Sir William Bagge, Bart., M.P., Colonel James Leslie Tait, and Dr. C. C. Carwana, were elected Fellows of the Society.

"Notes on some specimens of Lower Silurian Trilobites." By E. Billings, F.G.S., Palæontologist of the Geological Survey of Canada.

The author first described a specimen of *Asaphus platycephalus*, in which the hypostome was not only preserved *in situ*, but also the remains (more or less well preserved) of eight pairs of legs, corresponding with the eight segments of the thorax, to the underside of which they had been attached. The appendages take their rise close to the central axis of each segment, and all curve forwards, and are thus most probably ambulatory rather than natatory feet. They appear to have had four or five articulations in each leg. Three small ovate tubercles on the pygidium may, perhaps, indicate the processes by which the respiratory feet were attached. Mr. Billings referred to the large number of Trilobites which have been examined, and expressed his belief that only the most perfectly preserved specimens are likely to have the organs on the underside preserved. Mr. Billings next described the doublure or pleura in the Trilobites, comparing it to that of *Limulus*. He then proceeded to describe a row of small scars and tubercles on the underside of the pleuræ, to which both Dr. Volborth and Dr. Eichwald believed soft swimming feet or hard horny legs had been attached. As these were first seen by Dr. Pander in a Russian Trilobite, Mr. Billings has called them "Panderian organs." He thinks, soft natatory appendages may have been attached to these scars. Mr. Billings directed attention to the *Protichnites* and *Climactichnites*, which he thinks may now be referred to *Crustacea*, belonging to the division *Trilobita*. Finally, he described a section of a rolled-up *Calymene senaria*, the interior cavity of which appears to be full of minute ovate bodies, from $\frac{1}{80}$ to $\frac{1}{100}$ of an inch in diameter. These small ovate bodies the author believes to be eggs.

"Note on the palpus and other appendages of *Asaphus*, from the Trenton Limestone, in the British Museum." By Henry Woodward, F.G.S., F.Z.S.

Mr. Woodward, when comparing the Trilobite sent over by Mr. Billings with specimens in the British Museum, presented by Dr. J. J. Bigsby, F.R.S., discovered upon the eroded upper surface of one of these, not only the hypostome exposed to view, but also three pairs of appendages, and what he believes to be the palpus of one of the maxillæ. This furnishes an additional fact to Mr. Billings's most interesting discovery, besides confirming its correctness. Mr. Woodward considers the so-called "Panderian organs" to be only the fulcral points upon which the pleuræ move, and showed that such structures exist in most recent Crustacea. He considered that the evidence tended to place the Trilobita near to, if not in, the Isopoda Normalia, and remarked that the prominence of the hypostome reminded one strongly of that organ in *Apus*, and suggested that we might fairly expect to find that the Trilobita presented a more generalised type of structure than their representatives at the present day, the modern Isopoda.

"On the Structure and Affinities of *Sigillaria*, *Calamites*, and *Calamodendron*." By J. W. Dawson, LL.D., F.R.S., F.G.S., Principal and Vice-Chancellor of M'Gill University, Montreal. The object of this paper was to illustrate the structure and affinities of the genera above named, more especially with reference to the author's previous papers on the "Structures in Coal," and the "Conditions of Accumulation of Coal," and to furnish new facts and conclusions as to the affinities of these plants. With reference to *Sigillaria*, a remarkably perfect specimen of the axis of a plant of this genus, from the coal-field of Nova Scotia, was described as having a transversely laminated pith of the *Sternbergia* type, a cylinder of woody tissue, scalariform internally, and reticulated or discigerous externally, the tissues much resembling those of Cycads. Medullary rays were apparent in this cylinder; and it was traversed by obliquely radiating bundles of scalariform vessels or fibres proceeding to the leaves. Other specimens were adduced to show that the species having this kind of axis had a thick outer bark of elongated or prosenchymatous cells. The author stated that Prof. Williamson had enabled him to examine stems found in the Lancashire coal-field, of the type of Binney's *Sigillaria vascularis*, which differed in some important points of structure from his specimens; and that another specimen, externally marked like *Sigillaria*, had been shown by Mr. Carruthers to be more akin to *Lepidodendron* in structure. These specimens, as well as the *Sigillaria elegans* illustrated by Brongniart, probably represented other types of Sigillarioid trees, and it is not improbable that the genus *Sigillaria*, as usually understood, really includes several distinct generic forms. The author had recognised six generic forms in a previous paper, and in his "Acadian Geology;" but the type described in the present paper was that which appeared to predominate in the fossil Sigillarian forests of Nova Scotia, and also in the mineral charcoal of the coal-beds. This was illustrated by descriptions of structures occurring in erect and prostrate *Sigillariæ*, on the surface of *Sternbergia*-casts, and in the coal itself. The erect Calamites of the coal formation of Nova Scotia illustrate in a remarkable manner the exterior surface of the stems of these plants, their foliage, their rhizomata, their roots, and their habit of growth. Their affinities were evidently with *Equisetaceæ*, as Brongniart and others had maintained, and as Carruthers and Schimper had recently illustrated. The internal structure of these plants, as shown by some specimens collected by Mr. Butterworth, of Manchester, and soon to be published by Prof. Williamson, showed that the stems were more advanced in structure than those of modern *Equiseta*, and enabled the author to explain the various appearances presented by these plants, when the external surface is preserved, wholly or in part, and when a cast of the internal cavity alone remains. It was further shown that the leaves of the ordinary Calamites are linear, angular, and transversely wrinkled, and different from those of the *Asterophyllites* properly so called, though some species, as *A. comosus*, Lindley, are leaves of Calamites. The *Calamodendra*, as described by Cotta, Binney, and others, and further illustrated by specimens from Nova Scotia, and by several interesting and undescribed forms in the collection of Prof. Williamson, are similar in general plan of structure to the Calamites, but much more woody plants; and, if allied to *Equisetaceæ*, are greatly more advanced in the structure of the stem than the modern representatives of that order. Specimens in the collection of Prof. Williamson show forms intermediate between Calamites and *Calamodendron*, so that possibly both may be included in one family; but much further information on this subject is required. The tissues of the higher *Calamodendra* are similar to those of Gymnospermous plants. The wood or vascular matter of the thin-walled Calamites consists of multiporous cells or vessels, in such species as have been examined. In conclusion, a table was exhibited showing the affinities of *Sigillariæ* on the one hand, through *Clathraria* and *Syringodendron* with *Lycopodiaceæ*; and on the other hand, through *Calamodendron* with *Equisetaceæ*; while in the other direction they presented links of connection with Cycads and Conifers.

"Notes on the Geology of Arisaig, Nova Scotia." By the Rev. D. Honeyman, D.C.L., F.G.S. The author referred to a previous paper on the Upper Silurian Rocks of Nova Scotia, which he stated appeared to him now to be generally repetitions of his Arisaig series. He noticed the occurrence of fossils in one of the beds previously supposed to be almost destitute of organic remains, and described the occurrence, in Arisaig township, of a band of crystalline rocks which appeared to contain *Eozoon*, and were probably of Laurentian age. A note from Prof. Rupert Jones, giving an account of the fossils referred to by Dr. Honeyman, was also read.

Chemical Society, May 19.—Warren De la Rue, F.R.S., vice-president, in the chair. Mr. S. H. Johnson was elected a fellow. Mr. Griffin exhibited and explained a new gas furnace which is capable of melting about three pounds of iron in little more than one hour.—Mr. Walenn described an advantageous method for coating cast-iron objects by electrolysis with copper or brass. The special peculiarity of the method consists in the circumstance that no hydrogen is evolved during the process. A calico printing valse and other articles, coated with brass, in this manner, were submitted to the inspection of the assembly.—Mr. Tookey, Assayer in the Japanese Imperial Mint, communicated a paper "On the Manipulations of Assays of Gold and Silver Bullion." The number of separate processes from the first weighing in of a piece of gold bullion to the second weighing out before its value can be ascertained are well known to all assayers. The author saves a good deal of time by proceeding in the following manner:—The bullions are placed in conical shaped platinum tubes, which, at their narrow ends, are closed with a perforated plate, and at the wider end are provided with a shoulder, so that they can be supported by a porcelain tile having circular holes. The tubes and holes are numbered. The entire arrangement is then immersed in hydric nitrate, &c.; in short, proceeded with as if a single bullion, instead of a batch of them had to be treated. With regard to the assay of silver bullion, the hammering and brushing of the buttons after they have been detached from the cupels, may be dispensed with by placing those buttons into the perforated cavities of a platinum plate, where they are fastened by a platinum wire, and immersing the plate in pure hydric chloride, which will dissolve all the bone ash adhering to the buttons. The cavities of the plate are numbered to correspond with the cupels in the muffle. —Mr. Perkin read a note "On some Bromine derivatives of Coumarin." The following definite compounds have been obtained by treating coumarin with bromine in different ways: dibromide of coumarin, $C_9 H O_2 Br_2$; bromo-coumarin, $C_9H_5 BrO_2$, and dibromo-coumarin, $C_9H_4Br_2O_2$. All the three compounds are easily soluble in hot alcohol, from which they crystallise out on cooling. Dibromide of coumarin fires at about 100°, bromo-coumarin at 110°, dibromo-coumarin at 174°. The two latter compounds form, when boiled with an aqueous solution of potassic hydrate, potassium salts of new acids, probably bromo, and dibromo-coumaric acids.—Dr. Divers made some remarks "On the precipitation of solutions of ammonic carbonate, sodic carbonate, and ammonic carbonate by calcic chloride." The results of these experiments are chiefly of interest as supplying a characteristic reaction for the carbonate. When ammonic carbonate is added in excess to an aqueous solution of ammonia and calcic chloride, the calcic carbonate is precipitated very slowly in the cold, whilst such precipitate is instantly produced when ammonic carbonate is added to a solution of caustic ammonia and calcic chloride in water.—Dr. Thudichum made a short communication about having obtained hydric acetate from fresh urine, which fact contradicts the statements of Berzelius, Lehmann, and Liebig.

EDINBURGH

Institution of Engineers, April 19.—Professor Macquorn Rankine, C.E., LL.D., president, in the chair.—"On the Patent Law." By Mr. R. S. Newall. After some general remarks Mr. Newall proceeded as follows:—In brief, then, my propositions are:—The appointment of a standing commission who shall examine, in public, all petitions and specifications, &c., before the granting of a patent. That when once a patent is granted, it shall be held as valid, if not assailed within two years, under certain conditions. I do not see why the inventor's letters patent should not be made as secure and held as sacred as the title-deeds of any kind of property. The commissioners to be selected by the Privy Council from among men who are intimately acquainted with the arts and the sciences. I would extend the term of the patent to twenty years, and make the cost payable in four periods of five years, charging 50*l.* for each. This also might tend to restrict the number of patents applied for. I would leave the granting of licenses entirely to the patentee. It may suit his convenience to carry on the manufacture of his invention himself, better than to grant licenses to others to oppose him; and if he has the monopoly conferred upon him, he ought to be allowed to make use of it as he thinks proper. You have no more right to demand that a monopolist should grant a license than I have to drive my cattle into my neighbour's field. It appears to me to be absolutely impossible to fix the price for royalty on the granting of a patent; that, of course, must altogether depend upon the value of the invention, which cannot possibly be ascertained until some years after the patent is granted. We might as well attempt to fix the price for ten years of any commodity sold by shopkeepers, or of land to be sold. I propose to grant patents for inventions, whether they are made by foreigners or British subjects. I would propose the infliction of imprisonment in the case of any one infringing a patent, and having previously been convicted of the same offence. If this remedy were adopted we should have fewer rogues to deal with in patent cases, and the inventor might have the enjoyment of his monopoly. Since I began my proposed amendment of the patent laws in 1848, I have had more dishonesty to contend with than I hope may ever fall to the lot of any patentee. I have had fourteen years' litigation in defending the patent for my invention "for laying down submarine cables" against infringement, by Glass and Elliot, the Telegraph Construction Company, and others. Instead of being rewarded for a most valuable invention, without which the Atlantic, Indian, and other cables could not have been laid down, I have had to spend years in attending to law, and the expenses have amounted to thousands of pounds, whilst the pirates have made large fortunes by means of my invention.

PHILADELPHIA

American Philosophical Society, April 15.—Prof. Cope exhibited the greater part of the skeleton of an extinct crocodilian from the Cretaceous Greensand of New Jersey, which represented a new species of the genus *Bottosomus*, which he called *B. tuberculatus*. It displayed marked characters of the genus not before ascertained. Dr. Hayden called attention to numerous points in the topography and geology of the Rocky Mountain region, exhibiting photographs in illustration of them.

May 6.—A paper by Prof. Alex. Winchell, of the University of Michigan, was read, entitled, "On the Geological Character and Equivalents of the Marshall Group in the United States."—An obituary notice of Horace Binney, jun., was read by Prof. C. J. Stille.—A description of some beads of complex construction found in an Indian grave on the Susquehanna River, in Pennsylvania, by Prof. S. S. Holdeman, was read by the Secretary. They were described as ovoid, apparently made from parts of four concentric cylinders of blue and white material, the blue ridged so as to give a striated appearance to the coloration.—Prof. Harrison Allen read a paper entitled, "Some effects of Age, as observed in the Osseous System," illustrated by changes in the forms of the pterygoid alæ, malar bone, &c.—Prof. Cope read a paper "On the Fishes of the Tertiary Shales of Green River, Wyoming territory," in which the fragment of the fauna described was indicated as presenting resemblances to that of Monte Bolca. Prof. Cope also exhibited the cranium of a Dicynodont reptile from the Cape Colony, which he regarded as new, and called it *Lystrosaurus frontosus*. The genus was near Ptychognathus, but differed in the horizontal shovel-like mandible received within the upper. The species was near the *Pt. latifrons* (Owen), but differed in the breadth of cranium exceeding the length, great interorbital width, prominent orbital tuberosities, very narrow front, &c. He exhibited specimens of more or less imperfect tusks from the Trias of Pennsylvania, which he referred to Dicynodont reptiles.—Prof. F. V. Hayden communicated an essay on the stratigraphy of certain tertiary rocks on the line of the Pacific Railroad, including, among others, a section of a remarkable anticlinal in the basin of Utah. The strata exhibited in this section embrace two hundred distinct layers, varying from two inches to four feet in thickness. At the eastern extremity they are vertical, at the western they are bent in the form of a bow. It is a remarkable illustration of an arch unaffected by heat that Dr. Hayden had seen in the West. Some of the layers were composed of fossil shells, among others, Unio, Paludina, Corbula, &c.; the species few, but the individuals numerous.

DIARY

THURSDAY, JUNE 2.

LINNEAN SOCIETY, at 8.—On some New Forms of Trichopterous Insects.
CHEMICAL SOCIETY, at 8.—On the Platinum Ammonias: Dr. Odling.
ROYAL INSTITUTION, at 3.—Electricity: Prof. Tyndall.

FRIDAY, JUNE 3.

ROYAL INSTITUTION, at 8.—Migration of Fables: Prof. Max Müller.
GEOLOGISTS' ASSOCIATION, at 8.

SATURDAY, JUNE 4.

ROYAL INSTITUTION, at 3.—Comets: Prof. Grant.

MONDAY, JUNE 6.

ENTOMOLOGICAL SOCIETY, at 7.
LONDON INSTITUTION, at 4.—Botany: Prof. Bentley.

TUESDAY, JUNE 7.

ROYAL INSTITUTION, at 3.—Present English History: Prof. Seeley.
ETHNOLOGICAL SOCIETY, at 8.30 (at the Museum of Practical Geology).—On the Geographical Distribution of the Chief Modifications of Mankind: Prof. Huxley.

WEDNESDAY, JUNE 8.

GEOLOGICAL SOCIETY, at 8.
ROYAL MICROSCOPICAL SOCIETY, at 8.—Experiments on Fermentation and Parasitic Fungi: John Bell.—A New Form of Binocular Microscope: John W. Stephenson.

THURSDAY, JUNE 9.

ZOOLOGICAL SOCIETY, at 8.30.
MATHEMATICAL SOCIETY, at 8.
ROYAL INSTITUTION, at 3.—Electricity: Prof. Tyndall.

BOOKS RECEIVED

ENGLISH.—The Interior of the Earth: H. P. Malet (Hodder and Stoughton.)—The Modern Practical Angler; H. C. Pennell (Warne and Co.).—Primitive Man: L. Figuier (Chapman and Hall).—Water Analysis: J. A. Wanklyn (Trübner and Co.).—Rustic Adornments for Homes of Taste: Shirley Hibberd, new edition (Groombridge and Co.).—The Student's Flora of the British Islands: Dr. J. D. Hooker (Macmillan and Co.).

FOREIGN (through Williams and Norgate).—Die Pflanzenstoffe, zweite Lieferung: Dr. A. Husemann.—Précis de Paléontologie humaine: E. T. Hamy.—Die Gestaltung der Erdoberfläche nach bestimmten Gesetzen: O. Reichenbach.

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THURSDAY, JUNE 9, 1870

THE NATURAL HISTORY COLLECTIONS

WE have been favoured with a copy of a memorial, drawn up as long ago as 1858, by some of the most distinguished geologists and botanists in England, on the subject of the organisation of the British Museum Natural History Collections, the removal of which was then thought to be imminent. As our readers are aware, no steps towards this removal have yet been taken, but as the subject has been brought again before Parliament, it would be well that so carefully considered a document as this appears to be should be weighed in reference to any contemplated change in the governance and disposition of our national Natural History Collections.

It is further most desirable that gentlemen interested in this subject should communicate their views upon the proposals embodied in this memorial to the public, and such will be thankfully received by the Editor of NATURE. Of the nine memorialists, four are zoologists, all happily alive. Of the five botanists, on the other hand, Mr. Bentham alone survives; it is especially desirable that the opinions of botanists on so important a question should be heard.

Copy of a Memorial addressed to the Right Honourable the Chancellor of the Exchequer.

SIR,—The necessity of the removal of the Natural History departments from the British Museum having been recently brought prominently before the public, and it being understood that the question of their reorganisation in another locality is under consideration, the undersigned zoologists and botanists, professionally or otherwise engaged in the pursuit of natural science, feel it their duty to lay before Her Majesty's Government the views they entertain as to the arrangements by which national collections in Natural History can be best adapted to the two-fold object of the advancement of science, and its general diffusion among the public,—to show how far the Scientific Museums of the metropolis and its vicinity, in their present condition, answer these purposes, and to suggest such modifications or additional arrangements as appear requisite to render them more thoroughly efficient.

The Scientific Collections or Museums, whether Zoological or Botanical, required for the objects above stated, may be arranged under the following heads :—

1. A general and comprehensive Typical or Popular Museum, in which all prominent forms or types of Animals and Plants, recent or fossil, should be so displayed as to give the public an idea of the vast extent and variety of natural objects, to diffuse a general knowledge of the results obtained by science in their investigation and classification, and to serve as a general introduction to the student of Natural History.

2. A complete Scientific Museum, in which collections of all obtainable Animals and Plants and their parts, whether recent or fossil, and of a sufficient number of specimens, should be disposed conveniently for study; and to which should be exclusively attached an appropriate *library*, or collection of books and illustrations relating to science, wholly independent of any general library.

3. A comprehensive Economic Museum, in which Economic Products, whether Zoological or Botanical, with Illustrations of the processes by which they are obtained and applied to use, should be so disposed as best to assist the progress of Commerce and the Arts.

4. Collections of Living Animals and Plants, or Zoological and Botanical Gardens.

The Typical or Popular Museum, for the daily use of the general public, which might be advantageously annexed to the Scientific Museum, would require a large building, in a light, airy, and accessible situation. The collections should be displayed in spacious galleries, in glass cases, so closed as to protect them from the dirt and dust raised by the thousands who would visit them; and sufficient room should be allowed within the cases to admit of affixing to the specimens, without confusion, their names, and such illustrations as are necessary to render them intelligible and instructive to the student and the general public.

The Economic Museums and living Collections in Botany might be quite independent of the Zoological ones.

The Scientific Museum, in Zoology as in Botany, is the most important of all. It is indispensable for the study of natural science, although not suited for public exhibition. Without it the naturalist cannot even name or arrange the materials for the Typical, Economic or Living Collections, so as to convey any useful information to the public. The specimens, though in need of the same conditions of light, airiness, &c., as, and far more numerous than, those exposed in the Typical or Popular Museum, would occupy less space; and they would require a different arrangement, in order that the specimens might, without injury, be frequently taken from their receptacles for examination. This Scientific Museum, moreover, would be useless unless an appropriate library were included in the same building.

The union of the Zoological and Botanical Society Museums in one locality is of no importance. The juxtaposition of each with its corresponding Living Collection is desirable, but not necessary; although, in the case of Botany, an extensive Herbarium and Library are indispensable appendages to the Garden and Economic Museum.

The existing Natural History Collections accessible to men of science and to the public, in or near the metropolis, are the following :—

In Botany.—The Kew Herbarium, as a scientific collection, is the finest in the world, and its importance is universally acknowledged by botanists. It has an excellent scientific library attached to it; it is admirably situated; and being in proximity with, and under the immediate control of the head of, the botanic garden, it supersedes the necessity of a separate herbarium for the use of that garden and museum. But a great part of it is not the property of the State; there is no building permanently appropriated for its accommodation, and it does not include any collection of fossil plants.

The Botanical Collection of the British Museum, consisting chiefly of the Banksian Herbarium, is important, but very imperfect. It is badly situated, on account of the dust and dirt of Great Russell Street; and the want of space in the existing buildings of the British Museum would prevent its extension, even were there an adequate advantage in maintaining, at the cost of the State, two Herbaria or Scientific Botanic Museums so near together as those of London and Kew. The British Museum also contains a valuable collection of fossil plants, but not more readily available for science than its zoological collections.

There exists no typical or popular Botanical Museum for public inspection.

The efficiency of the Botanical Gardens and Museum of Economic Botany at Kew, as now organised, and the consequent advantages to science and the public, are too generally recognised to need any comment on the part of your memorialists.

In Zoology.—The British Museum contains a magnificent collection of recent and fossil animals, the property of the State, and intended both for public exhibition and for scientific use. But there is no room for its proper display, nor for the provision of the necessary accommodation for its study—still less for the separation of a *popular typical* series for public inspection, apart from the great mass of specimens whose importance is appreciated only by professed naturalists. And, in the attempt to combine the two, the public are only dazzled and confused by the multiplicity of unexplained objects, densely crowded together on the shelves and cases; the man of science is, for three days in the week, deprived of the opportunity of real study; and the specimens themselves suffer severely from the dust and dirt of the locality, increased manifold by the tread of the crowds who pass through the galleries on public days,—the necessity of access to the specimens on other days preventing their being arranged in hermetically closed cases.

A Museum of Economic Zoology has been commenced at South Kensington.

There is an unrivalled Zoological Garden or living collection, well situated in the Regent's Park, but not the property of the State, nor receiving any other than indirect assistance, in the terms on which its site is granted.

The measures which your memorialists would respectfully urge upon the consideration of Her Majesty's Government, with a view to rendering the collections really available for the purposes for which they are intended, are the following:—

That the Zoological Collections at present existing in the British Museum be separated into two distinct collections,—the one to form a Typical or Popular Museum, the other to constitute the basis of a complete Scientific Museum.

These Museums might be lodged in one and the same building, and be under one direction, provided they were arranged in such a manner as to be separately accessible; so that the one would always be open to the public, the other to the man of science, or any person seeking for special information. This arrangement would involve no more trouble, and would be as little expensive as any other which could answer its double purpose, as the Typical or Popular Museum might at once be made almost complete, and would require but very slight, if any, additions.

In fact, the plan proposed is only a further development of the system according to which the Entomological, Conchological, and Osteological collections in the British Museum are already worked.

That an appropriate Zoological Library be attached to its Scientific Museum, totally independent of the zoological portion of the Library of the British Museum, which, in the opinion of your memorialists, is inseparable from the general library.

That the Scientific Zoological Museum and Library be placed under one head, directly responsible to one of Her Majesty's Ministers, or under an organisation similar to that which is practically found so efficient in regard to Botany.

That the Museum of Economic Zoology at South Kensington be further developed.

Your memorialists recommend that the whole of the Kew Herbarium become the property of, and be maintained by, the State, as is now the case with a portion of it—that the Banksian Herbarium and the fossil plants be transferred to it from the British Museum—and that a permanent building be provided for the accommodation at Kew of the Scientific Museum of Botany so formed.*

This consolidation of the Herbaria of Kew with those of the British Museum would afford the means of including in the Botanical Scientific Museum a geographical botanical collection for the illustration of the colonial vegetation of the British Empire, which, considering the extreme importance of vegetable products to the commerce of this country, your memorialists are convinced would be felt to be a great advantage.

Your memorialists recommend further, that in place of the Banksian Herbarium and other miscellaneous botanical collections now in the British Museum and closed to the public, a Typical or Popular Museum of Botany be formed in the same building as that proposed for the Typical or Popular Museum of Zoology, and, like it, be open daily to the public.

Such a collection would require no great space; it would be inexpensive, besides being in the highest degree instructive; and, like the Typical or Popular Zoological Collection, it would be of the greatest value to the public, and to the teachers and students of the Metropolitan Colleges.

That the Botanical Scientific Museum and its Library, the Museum of Economic Botany, and the Botanic Garden, remain, as at present, under one head, directly responsible to one of Her Majesty's Ministers.

The undersigned memorialists, consisting wholly of Zoologists and Botanists, have offered no suggestions respecting the very valuable Mineralogical Collection in the British Museum, although aware that, in case it should be resolved that the Natural History Collections generally should be removed to another locality, the disposal of the minerals also will probably come under consideration.

GEORGE BENTHAM, V.P.L.S.

W. H. HARVEY, M.D., F.R.S. & Z.S., &c., Professor of Botany, University of Dublin.

ARTHUR HENFREY, F.R.S., & L.S., &c., Professor of Botany, King's College, London.

J. S. HENSLOW, F.L.S. & G.S., Professor of Botany in the University of Cambridge.

JOHN LINDLEY, F.R.S. & L.S., Professor of Botany in University College, London.

GEORGE BUSK, F.R.S. & Z.S., Professor of Comparative Anatomy and Physiology to the Royal College of Surgeons of England.

WILLIAM B. CARPENTER, M.D., F.R.S. & Z.S., Registrar of the University of London.

CHAS. DARWIN, F.R.S., L.S. & G.S.

THOMAS HUXLEY, F.R.S., Professor of Natural History, Government School of Mines, Jermyn Street.

Nov. 18, 1858

* Since this Memorial was written great changes have taken place in the extent and position of the Botanical collections both at Kew and the British Museum, and the above recommendations would require some modification. This applies especially to the fossil plants, which it seems highly desirable to retain within an easy distance of the principal geological collections, and which might be fully illustrated by including the geographical botanical collection in the typical museum in London.—[G. B., June 1870.]

LONGEVITY IN MAN AND ANIMALS

On Comparative Longevity in Man and the Lower Animals. By E. Ray Lankester, B.A., Junior Student of Christ Church, Oxford. (London: Macmillan and Co. 1870.)

IN this interesting little essay Mr. Lankester appears to have accumulated most of the facts with which we are at present acquainted, in respect to the duration of life. He defines longevity to be the length of time during which life is exhibited in an individual; but does not, of course, apply the term individual to entire masses proceeding, as in the case of *alsinastrum* and many polypes, from a process of asexual generation; and he proceeds to point out that there is a longevity belonging to the species, and a longevity characteristic of the individual, and further, that the average longevity of a species never equals its potential longevity, since a thousand accidents happen to destroy individuals at an early period of their lives; he then distinguishes between normal and absolute potential activity, and shows that in man alone do these two nearly coincide.

The two chief circumstances which favour longevity are high individualism, for this in itself requires time; and small expenditure, the latter embracing the wear and tear put forth in the procurement of food and in the reproduction of the species. In support of these statements Mr. Lankester then adduces a considerable number of the more trustworthy observations that have been made in reference to the longevity of individuals belonging to different classes of the animal kingdom, some of which we here append. Our knowledge, it appears from these tables, of the duration of life in the lower classes, is very imperfect. Amongst the Protozoa, *Spongilla fluviatilis* dies yearly, leaving gemmules. Amongst the Cœlenterata, *Hydra viridis* reproduces sexually in autumn and then dies. An *Actinia mesembryanthemum* has been living forty-two years in an aquarium and is still alive. Amongst the Crustacea, some of the larger crabs and lobsters must have attained a great age; but Mr. Lankester has observed one species, *Cheirocephalus diaphanus*, which developes from the egg, reproduces and dies in from two to three months. In the Insecta, the imago, as a rule, lives part of a year, from six months to a few hours, dying on reproduction. The length of life of the larva varies greatly in closely allied forms, from four years or more to a week. Fleas may live as long as nine months. Scarcely any observations have been made on the length of life in the molluscoidea and mollusca. Fish appear to have great tenacity of life. Thus, the carp is believed to have attained the age of 150 years, and the pike 267 years, if a ring with the following inscription is genuine—"I am the fish which was first of all put into this lake by the hands of the Governor of the Universe, Frederick II., the 5th of October, 1230." It weighed 350lbs., and was 19 feet long. Its skeleton was exhibited at Mannheim, and it was taken at Halibrun in Suabia in 1497. Of the Amphibia, the toad lives 36 years, the frog 12 to 16, and various tortoises are inferred to be of great age from their size. Amongst birds, the parrot, goose, falcon, and raven are long-lived, the two former reaching 100 to 120 years, and the two latter exceeding 150 years. Wrens only live two or three years. Amongst mammals, the whale and elephant have the longest term of life, both probably exceeding 100 years, and possibly reaching 200; horse 25, but occasionally reaching 40 years; ox 15 to 20 years; sheep and goat 12 years; lion 20 to 50 years; cat 9 to 18 years.

Mr. Lankester comments on these tolerably well ascertained facts, and shows how they support the theory that longevity depends on the influence of generative and personal expenditure.

The last part of the work is devoted to the longevity of man, satisfactory conclusions respecting whom are almost limited to the very highly civilised nations. Mr. Lankester appears to entertain no doubt that cases have occurred where the age of one hundred has been exceeded. We have limited ourselves to a brief epitome of the contents of Mr. Lankester's work, and cordially recommend it to our readers.

H. POWER

OUR BOOK SHELF

The Fuel of the Sun. By W. Matthieu Williams, F.C.S. (London: Simpkin, Marshall, and Co.)

WE have in the work before us a proof that a very interesting and readable volume may be produced, although the hypothesis which has called it into being may be one with which we do not agree. Mr. Williams discusses at great length the very perplexing question of the sun's fuel, nevertheless we do not think that his hypothesis is an improvement upon that of Helmholtz and Thomson. But let us hear the writer speak for himself. After having come to the conclusion that an atmosphere very similar to our own, but only more attenuated, pervades all space, he supposes that the sun, in its progress through space, encounters new portions of this atmosphere, and then asks the following question—"Does there exist in the actual arrangements of the solar system any machinery for stirring in an important quantity of the new atmospheric matter and ejecting the old? If so, the maintenance of the sun's heat may be fully accounted for." The question is answered in the affirmative; the atmosphere is supposed to be the sun's fuel, and the planetary attendants of the sun are supposed to perform the duty of stokers with untiring vigilance and efficiency. The mode of action of this atmospheric fuel in furnishing heat is supposed to be as follows:—"It is evident then that the first result of the great evolution of heat from mechanical condensation of the mixed atmosphere of aqueous vapour, carbonic acid, and free oxygen and nitrogen, will be the dissociation of the water and the carbonic acid. But there must somewhere be a height at which the temperature capable of effecting dissociation terminates; where the atmosphere of elementary gases fringes upon that of combined aqueous vapour, and where these separated gases must revert into reunion with a furious chemical energy which will be manifested by violent combustion. Thus we shall have a sphere of dissociated gases and a sphere of compound vapours separated by an interlying stratum of combining gases, a spherical shell of flame, constituting exactly what solar observers have described as the 'photosphere.'" In fine, Mr. Williams' hypothesis is "a perpetual bombardment of 165 millions of millions of tons of matter per second without in any degree altering the density, the bulk, or any other element of the solar constitution."

B. STEWART

Einleitung in die Physik. Bearbeitet von Professoren G. Karsten, F. Harms, und G. Weyer.

THE volume before us is introductory to the "Allgemeine Encyclopädie der Physik," which is in course of publication, under the general editorship of Professor Karsten. The authors endeavour to supply whatever would not naturally be found or expected in the separate treatises of which the Encyclopædia is made up, which have been written independently by specialist authors from their individual points of view. They add a systematic treatment of everything that may be considered auxiliary to the entire group of the physical sciences.

Professor Harms is the author of the most important part of the work—a philosophical and historical introduction to the whole subject. The discussion ranges over three principal heads—

1. What are the proper limits, and the true relations of physical science, and what distinctions can be drawn between it and the other sciences of matter?

2. What are the methods of physical inquiry, with a critical estimate of induction, of speculation or deduction, and of the theory of cognition (Erkenntniss-theorie) which has arisen in Germany since the days of Kant. This discussion is naturally conducted both historically and metaphysically. The rapid but exhaustive reviews of Bacon,

Locke, Hume, Sir J. Herschel, Mill, and Whewell—the only authors quoted on the subject of induction, will be specially interesting to English readers.

3. The philosophical basis of the conceptions at the root of the natural sciences, with a full treatment of Idealism and Materialism, and a discussion of the differences between matter passive and without force, and matter active, bound up, that is to say, with capacities of change of state.

Professor Karsten's contributions involve an enormous amount of statistical and bibliographical labour. Fifty pages are occupied with a complete catalogue of the literature of general physics. All the encyclopædias, all the scientific periodicals and collections, all the books on the history of science, and all the handbooks and general treatises of all modern nations, are gathered together in one most useful and naturally bewildering list. Germany, Switzerland, England, the United States, France, Belgium, Holland, Denmark, Sweden and Norway, Russia, Italy, Spain and Portugal, are the countries which contribute. The order in which we have given them exhibits the civilised world from the German professor's point of view.

A second treatise by the same author deals with all of what are called the universal properties of matter, and discusses in full the problems of chemical affinity and the newest theories of atoms. Little is really carried lower than the year 1860, but references are given to all books of importance published as late as 1867.

His third treatise gives us the methods of measurement, with full descriptions of the instruments and copious tables of comparison between the units of different countries. Professor G. Weyer finally supplies a separate work on the determinations of space and time. All questions of latitude and longitude, of apparent and real magnitude, are fully discussed, and the astronomical data which affect our estimates of time are exhibited in full.

We have preferred to give our readers a simple statement of what is contained in the closely-printed volume of 900 royal octavo pages before us. Detailed criticism of five separate treatises, in the space at our disposal, is a mere impossibility. It is sufficient to say here that every subject discussed is worked out in all the painstaking and exhaustive detail to which the separate volumes of Karsten's Encyclopädie previously published, have accustomed us. Such works are of the greatest possible service to literature. They are not produced in England. Our scientific men are too busy conquering new worlds, and lecturing on the exciting incidents of every fresh conquest. There is not a man too many thus engaged, but we confess that we sometimes turn with desire to our two great mediæval universities, where the liberality of our forefathers has established hundreds of fellowships, expressly that men might have leisure to devote themselves to lifelong studies. How is it that Oxford and Cambridge leave us to sigh for impossible translations of laborious books like this, which has been sent us principally by the University of Kiel?

W. J.

De l'abus des boissons alcooliques. Par L. F. E. Bergeret. (Paris: Baillière et fils.)

THE author, who is the senior physician of the Hospital D'Arbois (Jura), has for the last thirty years devoted special attention to the effects produced by the excessive use of alcoholic liquors. Though denying that alcohol is in any form a necessary of life, he fully admits that the moderate use of alcoholic liquors has its advantages, and the work has been written chiefly with the object of affording a popular illustration of their physiological action, and of exciting a wholesome fear of their abuse. The volume contains a large amount of very interesting information, and the results of much personal observation relating to the consequences of habitual intoxication.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

The Cretaceous Epoch

THE President of the Linnean Society having been good enough to credit me, in the interesting address which has just appeared in NATURE, with the doctrine that the formation of chalk has been going on continuously over some part of the North Atlantic sea-bed from the Cretaceous epoch to the present time, I feel it due to my friend and colleague, Prof. Wyville Thomson, to disclaim most explicitly the merit of having originated this doctrine, which entirely belongs to him. I regret that the form in which it was promulgated in my report of the *Lightning* expedition should have led to this misapprehension; but that form was adopted at my friend's express desire; and I have on every occasion (as in my recent lecture at the Royal Institution) spoken of the idea as exclusively *his*. Whilst myself fully accepting and advocating it, I am the more anxious that there should be no mistake in this matter, as it seems to me that the idea is one which must exert so important an influence on the future course of geological inquiry that its introduction will be one of the landmarks in the history of the science.

The *similarity* of the globigerina-mud, at present in process of formation, to the mesozoic chalk, had been recognised by various microscopists who had studied both—as Ehrenberg, Bailey, Williamson, Huxley, Wallich, and Sorby. But no one, so far as I am aware, had ventured to advocate the *unbroken continuity* of the chalk formation throughout the Tertiary and Quaternary periods, until it was pointed out by Prof. Wyville Thomson that there is no adequate evidence of its ever having ceased, though its locality has changed.

This doctrine has received most striking confirmation from the discovery of the persistence of numerous cretaceous types, more or less modified, not merely in our own explorations, but in those carried on by the United States Coast Survey in the Gulf of Mexico. That we could not expect to find the cretaceous fauna *as a whole* in our modern chalk is evident from the considerations so admirably set forth in Mr. Bentham's address; and if the cretaceous epoch is to be limited by the duration of that particular *ensemble*, it may, of course, be affirmed to have closed long since. But if there has been a continuous production of globigerina-mud from the time when the cretaceous area of Europe was a deep-sea-bed, the elevation of that sea-bed, so as to bring a large part of it above the surface, being probably coincident with the depression of what is now the North Atlantic Basin, to which the globigerinæ then migrated, and if there be found in the newly-formed chalk so large a number of representatives of the types most characteristic of the old, as indicates the continued prevalence of the same general, physical, and biological conditions, there is, I submit, a fair justification of the assertion (the pregnant words of which are Professor Wyville Thomson's) that "we may be said to be still living in the cretaceous epoch." And, as I ventured to put forth in the lecture referred to, the *onus probandi* now seems to me to rest on those who assert that this continuity has been interrupted, and that the chalk formation now in progress is anything else than a continuation of that of which Dover Cliff is composed.

Whilst "on my legs," I would venture to call the attention of geologists to the question which has been much considered by Professor Wyville Thomson and myself, whether we are not justified, by the probabilities of the case, in carrying *backwards* the continuity of the accumulation of Foraminiferal mud on the deep-sea bed, into geological epochs far more remote; since there must have been deep seas in all periods, and the changes which modified the climate and depth of the sea-bottom must have been for the most part sufficiently gradual to admit of the migration of animals to whose continued existence in the same locality those conditions were no longer favourable. It is a most interesting confirmation of the view we are disposed to entertain on this point, that, as I have recently learned from Sir William Logan, coccoliths and coccospheres have been discovered in some of the most ancient Palæozoic limestones of North America.

WILLIAM B. CARPENTER

[See also Prof. Gümbel's letter to Prof. Huxley on this subject in NATURE, vol. I. p. 657.—*Ed.*]

The Aye Aye

I FIRST saw the quotation from the *Pall Mall Gazette* in the columns of NATURE concerning the long finger nail of the Aye Aye, and the "exquisite argument" founded by Professor Owen upon it.

As a simple matter of fact, allow me to state that I kept a living Aye Aye (now preserved in the British Museum) in a large cage in the Mauritius, and as its food I gave it the maggot that infested branches of a species of Acacia. The animal used to spend its evenings in feeding, as follows. It listened attentively at the branches, tapping occasionally the most perforated parts; it then tore off pieces of the wood around the maggot hole, inserting the peculiar long finger as a probe from time to time, and ended by extracting the maggot by means of this long finger and its strong rodent teeth.

I have seen the operation scores of times.

Athenæum, May 28 HUMPHRY SANDWITH

Carp and Toads

IN the last number of NATURE you give an abstract of a paper by M. Duchemin on the destruction of carp by toads. The fact that carp are so destroyed is, or was, well known. Walton, in his "Complete Angler," says:—"And I have known of one (person) that has almost watched the pond, and at the fishing of the pond, found of seventy or eighty large carps not above five or six; and that he had foreborne longer to fish the said pond, but that he saw on a hot day in summer, a large carp swim near the top of the water with a frog upon his head; and that he, upon that occasion, caused his pond to be let dry; and I say, of seventy or eighty carps, only found five or six in the said pond, and those very sick and lean, and with every one a frog sticking so fast on the head of the said carps, that the frog would not be got off without extreme force or killing." Walton also mentions that pike are attacked and destroyed in the same manner. Walton wrote his "Complete Angler, or Contemplative Man's Recreation" in 1653. The confusion between frogs and toads was one likely to be made at a time when natural history was so little studied. In all other respects Walton's account agrees with M. Duchemin's. C. H. G.

Anticipated Destruction of the Cheesewring

I AM sure your readers will pardon me for drawing their attention to the very perilous situation of that remarkable pile of rocks, six or seven miles north of the town of Liskeard, in Cornwall, and known as the Wring-cheese or Cheesewring. Wilkie Collins, in his "Rambles beyond Railways," thus describes the general appearance of this natural curiosity:—"If a man dreamt of a great pile of stones in a nightmare, he would dream of such a pile as the Cheesewring. All the heaviest and largest of the seven thick slabs of which it is composed are at the top; all the lightest and smallest at the bottom. It rises perpendicularly to a height of thirty-two feet, without lateral support of any kind. The fifth and six rocks are of immense size and thickness, and overhang fearfully all round the four lower rocks which support them. All are perfectly irregular; the projections of one do not fit into the interstices of another; they are heaped up loosely in their extraordinary top-heavy form on slanting ground, half way down a steep hill." Of late years this hill has been so extensively quarried for granite that the workmen are now within a few paces of the Cheesewring itself. When a lease of the ground was first granted, it was stipulated that no stone should be removed within a certain distance of this well-known landmark, so as to prevent any possibility of its being destroyed. Now, however, the boundaries of the quarry have been so extended that powerful blasting operations are continually being carried on within a short distance of it, not without very great risk to the whole structure. In fact, it is on the eve of being destroyed, unless a vigorous and influential attempt is made to save it. Six months ago the Royal Institution of Cornwall sent a deputation of its members to report on the exact state of the Cheesewring; and although a memorial was addressed to the authorities of the Duchy of Cornwall, the owners of the property, praying that some means be adopted for the preservation of this extraordinary geological formation, no satisfactory reply has been hitherto received. Since, therefore, *local* influence appears to be of little or no avail, it is to be hoped that the matter will be taken up by those who are especially interested in the preservation of remarkable objects of this kind. The untimely fate of the great Tolmên last year should be remembered; and measures immediately adopted to avoid a repetition of a similar catastrophe within so short a period. Whatever is done should be begun speedily, or in the meantime the impending calamity may actually take place. E. H. W. DUNKIN

Greenwich

Left-handedness

I HAD been intending, if no one had anticipated me, to suggest what I have little doubt is the true explanation of the destruction of carp by toads. I see in p. 59 of NATURE for May 19, that Mr. Wade hints at this solution of the difficulty.

Let me take this opportunity of saying a word on Left-handedness. The late eminent anatomist, Professor Gratiolet, maintained that in the early stages of fœtal development, the anterior and middle lobes of the brain on the left side were in a more advanced condition than those on the right side, the balance being maintained by an opposite condition of the posterior lobes. Hence, in consequence of the well-known decussation of the nerve-roots, the right side of the body—so far as it is influenced by brain force—would, in early fœtal life, be better supplied with nervous power than the left side; and movements of the right arm would precede and be more perfect than those of the left. If Gratiolet's view is regarded as established (it has, I believe, been disputed), we have a physiological clue to the explanation of Right-handedness. In rare cases the development of the interior and middle lobes of the right side may precede that of the left; we shall then have Left-handedness.

May 21 M.A., CANT.

IN answer to Mr. Meyer's letter on this subject, I can only say that the question of the dependence of Left-handedness on abnormality of the subclavian artery cannot be settled by the authority of even so eminent an anatomist as Professor Hyrtl, when it is adduced against the facts I mentioned in my last note. That there have been cases like that quoted from Dr. Buchanan of transposition of viscera and left-handedness occurring in the same individual, and that they will be observed again, I do not doubt. Otherwise the conclusion would be that such abnormalities prevent left-handedness, which no one pretends. What I venture to think the cases already on record prove is, that the one condition has no relation to the other in either causing or preventing it.

With regard to the origin of the right subclavian direct from the end of the transverse aorta, though I have met with several cases of this variation, they were in subjects whose history was unknown; while, unlike complete transposition of the viscera, the condition cannot of course be recognised during life. But at the last meeting of the Pathological Society, a case of aneurism with this abnormality was brought forward; and, by the kindness of Dr. Peacock, who exhibited it, I am informed that the patient was undoubtedly right-handed during life.

Even apart from facts like this, one would scarcely expect to find the explanation of left-handedness in abnormalities affecting only the upper extremity. The condition is one of the eye, the leg, and the whole body. This one discovers in using the microscope, shooting, and batting; and the facts of aphasia appear to show that we are all, to use M. Broca's phrase, *gauchers de cerveau*. In fact, we must first explain normal right-handedness, and that not only as seen in man, but also in the paces of the horse, and in many other alternate or otherwise unsymmetrical movements of animals.

The only anatomical explanation of right-handedness I have met with is the unconfirmed statement of Gratiolet that the left half of the brain is developed more rapidly than the right. But even if true, this would only throw back the question to the origin of such a condition of the brain.

That the primitive habit of bilaterally symmetrical animals is to use right and left organs equally, seems probable, and also that when more complicated movements have to be performed, one limb alone must be chosen and so in time become more skilful. This preference may be transmitted hereditarily. But, granting in addition that the convenience of the community requires that all its members should select the same limb, it yet is not clear by what process of natural selection the right one has come to be universally preferred to the left—how, in fact, except from practice, right-handedness has come to be dexterity, and left-handedness *gaucherie*.

This question of the reason of symmetrical organs having different functions might perhaps be elucidated by a study of the conditions under which deviation from bilateral symmetry occurs in the *structures* of animal life, even among the highest arthropods and vertebrates. P. S.

May 23

The "Chromatic Octave"

RENEWED attention to the "chromatic octave" tempts me to suggest an experiment. There used to be a gentleman, Smith, I believe, by name, who refuted the undulatory theory by means of a disc, divided into black and white sections, which he whirled with very high velocities, producing colours (so the *Times* positively stated) varying according to the velocities. It is plain that such a result might on the contrary confirm the theory, if, for instance, the disc were divided into 400 black and 400 white sectors, and whirled at the rate of one or two million million times a second. It is also plain that Mr. Smith, in the words of an authority who has been quoted in your columns for weightier judgments, would have blown his disc into smoke first. But *once* a second is only 40 octaves below a million million times; and it is just possible that something practicable between the two might throw light on the "chromatic octave," among other things. There are some obvious objections: the question is, whether they make it worth while to repeat Mr. Smith's experiments.

I have just received the number of *Poggendorff* (Oct. 1867, CXXXI.) containing Professor Listing's paper (p. 564), referred to by Mr. Barrett in NATURE for Jan. 13 and March 31. I did not expect it would support Mr. Barrett, but I was surprised to find how directly it contradicts him. "Listing," he says, "concludes that, although physiologically and psychologically there may be differences, yet there is an indisputable *physical* basis for the analogy between tones and colours." I had myself, at the end of my remarks in NATURE for Feb. 3, admitted the "physical basis" in that sense "of the word *physical* which excludes biological relations," and the remark is too trivial to have formed Listing's *conclusion*. It forms part of a sentence in the first page of the paper. "The analogy between tones and colours, which has often been pursued with excessive predilection, and which certainly has an indisputable physical basis, has against it numerous points of disagreement (*Discongruenzen*), even now not in general sufficiently attended to, which depend rather on the physiological and psychological aspect of the phenomena." In the same page of the number it clearly appears how much is meant by the *physical basis*. "Physically," he says, "it is the period of vibration that determines both tone and colour; but the physiological effects stand in very different relations to the common element in the two cases." He proceeds to show, as correctly explained by Mr. Barrett, that the several colours divide the spectrum in an arithmetical progression of their rapidities of vibration; and at the end of the paper, contrasting this phenomenon with the geometrical progression of a uniform series of tones, he says: "This point of disagreement, a very vital one in my opinion, between the scales of tone and colour, may be briefly stated thus: *In the musical scale* (chromatic and with equal temperament*) *the logarithms of the tones are in arithmetical progression, in the scale of colour the colours themselves.*" That this should mean what Mr. Barrett understands it to mean, you must read *agreement* for *disagreement.*

Of the reality of Listing's result, I suppose there can hardly be any doubt; and I am glad that Mr. Barrett has corrected my suggestion that it probably represented a conventional demarcation. There does seem something arbitrary in the number of divisions made, but their positions represent a mean among the impressions of different observers as to the boundaries between colours answering to the names assigned; and the accuracy of these determinations may be fairly estimated by likening them to the case of a person who, having to divide a space of nine inches into nine equal parts, should be correct *as often as not* within one 24th of an inch. But the most important point is this, that the observers would not be aided, but must rather have been distracted, by the spaces actually occupied by the colours in the spectrum. For the observations were made on two different spectra, the irregular one obtained from the prism, and the diffraction spectrum in which the colours proceed uniformly by wave-lengths; and the result was a division into equal spaces, not on either of these visible spectra, but on the ideal spectrum, which should proceed uniformly by rapidities of vibration. It would have been in the spirit of good German precedents if we had been given some measure of the variation between different observers.

It must be confessed that all this is damaging to the theory of a "chromatic octave," essentially a theory of geometrical progression. Still more obviously damaging is the fact that "lavender" would be the octave above something so unlike it as "brown," or "brown" and "red."

Mr. Murphy's argument (NATURE, April 28) seems to assume that complementary wave-lengths must be in *some* constant ratio. His theory is, at any rate, inconsistent with his author's; for primary red and blue would be nearly complementary, so that "true white" could not be produced by any mere preponderance of blue, and would be white only to the green-blind.

C. J. MONRO

* If this is what is meant by "chromatischen gleichschwebenden."

IN Mr. Murphy's interesting letter in No. 26 of NATURE, April 28, 1870, he assumes that the number expressing the frequency of vibration producing a colour complementary to another, is the geometrical mean between the frequency of vibrations corresponding to that other, and its double. By this means he does not get colours complementary from sunlight. Thus red and bluish green (whose numbers are respectively 36·4, 48·3) are not complementary on his hypothesis; which would require the number for bluish green to be 51·47. So for yellow and indigo, the numbers are 41·4, 54·7, but should be 41·4, 58·4. This he attributes to the impurity of the solar spectrum. There seems as much reason, however, for taking the *harmonic mean* instead of the geometric; and, on this supposition (the harmonic mean between two quantities being twice their product divided by their sum), the numbers would be red, 36·4; bluish-green, 48·5; yellow, 41·4; indigo, 55·2. The second and fourth, 48·5, 55·2, are not very different from 48·3, 54·7. Taking then a colour twice over in the spectrum and its intermediate complementary, the relation between the three would be that of a musical note, its fifth and its octave.

Little Wratting, Suffolk, May 16 M.A.

The Colour of the Moon by Day and by Night

CAN any of your readers give me a full explanation of the reason why the moon looks white by day and yellow by night? The light that proceeds from it is of course the same at both periods; whence does the change in appearance arise? Two reasons occur at first thought, but they do not completely satisfy the many requirements of the problem. The one is, that the light, being really somewhat yellow, though less so than it often appears to be, passes in daytime through an atmosphere made blue by the solar rays, and the blue and yellow neutralising each other, the moon looks white. The other reason is, that as the evening closes in, the twilight becomes purple, and the moon being but moderately yellow in itself, looks more intensely yellow by contrast. All this is correct so far as it goes; but I do not see why the moon should often look extremely yellow in the middle of the night after twilight has quite disappeared. Does it show that the light, one knows not exactly whence it comes, which is found even on clouded and moonless nights, is purple? There are some grounds for this hypothesis, because the moon almost always, as I have been assured by a practical astronomer, looks comparatively white through a telescope, which of course isolates the field of vision. Also, it seems to me that the street gaslights are just as yellow at midnight as in twilight; the stars, also, commonly look yellow all the night through. It is strange that the very frequent and beautiful phenomenon of the white moon of the day suddenly turning yellow as the evening closes in, should not have long since attracted scientific comment.

F. G.

What is a Boulder?

A CORRESPONDENT in your journal of the 26th of May inquires about the size of boulders, and states that he cannot find any definition of the word which gives a notion of its size accurate enough for scientific purposes.

There are several definitions of boulder-stones given by geologists and others, which determine their size within tolerably narrow limits.

Dr. Page defines boulders as being "any rounded or water-worn *blocks* of stone, which would not, from their size, be regarded as pebbles or gravel;" or generally, "any *large* water-worn and smoothed blocks, found embedded in the clays and gravels of the drift formation." The maximum size he assigns to gravel is the size of a hen's egg, and pebbles may be ten times as large as this, so that the smallest boulder may be regarded as being of more than a foot diameter. Again, the same author describes boulders as being "often of great size, and weighing many tons."

Dr. Nuttall, in his Pronouncing Dictionary, defines boulders as being "large round pebbles;" and it would not often be possible to put even a small pebble in one's waistcoat pocket, and it must be a high wind that could blow a large pebble for any distance.

Chambers gives, as the etymology of boulder, or bowlder, as it is sometimes spelt, the verb "to bowl," and so states that it means a rolling stone, and, afterwards, one rounded by water. Now boulders are very often found separate, and at some distance from each other, and how could they have been rounded separately, without friction with each other, if they had been otherwise than large?

These arguments may be enough to show that boulders cannot be very small, and, in fact, may vary from stones 18 inches diameter, or thereabouts, to immense rocks of 10 feet diameter, "weighing many tons."

Christ's Hospital, May 27 J. W. CRAWLEY

Scandinavian Skulls

MR. G. STRACHEY, commenting in your columns on a recent lecture of Professor Huxley's, makes the following statements:—

"According to the highest Copenhagen authorities, there is no ground whatever for the assertion that modern Scandinavian skulls are of the long type. It is equally incorrect to say that Scandinavians are fair-haired and blue-eyed."

Without at all wishing to endorse Prof. Huxley's views on British ethnology, which I regard as rather bold than sound, I may be allowed to express my astonishment at the statements just quoted.

The skull measurements of Retzius, Van der Hoeven, and Barnard Davis all exhibit the Swedes as a more or less dolichokephalous people. My own measurements of the living head (Anthrop. Memoirs, ii. 351, and iii. 378) tend to controvert both Mr. Strachey's facts and Prof. Huxley's theory. Lastly, I should be glad to know where in the world fair hair and blue (or light) eyes are to be found, if not in Scandinavia. They are not universal there, nor any where else, but I do not think they are anywhere more common, except possibly in Lithuania or Esthonia.

Clifton, June 7 JOHN BEDDOE

Formation of Caverns

IN NATURE of 21st April, Mr. W. Boyd Dawkins, writing of caves in Yorkshire, tells us:—"All have been at one time or other subterranean water-courses." In the *Popular Science Review* for October 1869, the same gentleman writes:—"The ceiling, at the time of its deposition, must have been supported by a layer of cave earth." With your permission I will explain the character of a phenomenon, which I have published in my "New Pages of Natural History" (Newby, 1868), which may suggest to Mr. Dawkins the manner in which the caves at Streddle and Kent's Hole were formed. In the province of Poona, Bombay, the Ghar Nuddee (white river) has, in the mountains near its source, several lime formations spanning its level. In the dry season the river runs below as a subterranean stream; in the Monsoon it runs over this sheet of lime, which varies in thickness from a few inches to two feet along the centre or crown of the cavity, increasing in thickness towards the sides; there are several fissures on the surface; the hole at the upper end is smaller than that at the lower; the whole formation is in layers, and is due to water containing vast quantities of lime in solution. Originally there was a dip or hollow place on the spot, which gradually filled with all sorts of materials, till they grew nearly to the level of the dry season stream. In this condition the first thin sheet of lime was deposited on them, till by successive seasons the formation grew into a substantial covering composed of yearly layers. As there were perishable materials down below, they subsided, so that the lime covering, having in places no support, gave way to the force of the water, or to the weight of boulders hurled upon it, and water found admission between the lime sheet and the buried materials; as these were more moveable than the covering, they gradually washed out, and left the river to resume its ancient course beneath a covering of its own formation. Of course subterranean streams may excavate caverns; but if these streams owe their origin to percolation only, no large organic remains will be found in the caverns. If these are formed after the fashion of the Ghara caves, some organic remains may still be found, though waters have washed through them for years. When these lime formations withstand all the forces to which they are subjected, and have grown into large hills, the materials which formed the mould of the cave are still *in situ*, the perishable portions have changed into an oily, loamy soil; but bones, pebbles, and other materials are there mixed up with the stalagmite, which originally forming upon the surface of these materials, sunk and broke up as the supports failed, and remain, as Mr. Dawkins found them in Kent's Hole. H. P. MALET

The Anglo-Saxon Conquest

I SAW, with perfect clearness, the grounds upon which Prof. Rolleston now rests his defence. It is assumed that we can identify, to a nicety, the precise period of an interment, having no recording date, that took place fourteen or fifteen hundred years ago. I need not contest or discuss this point, but I am at liberty to doubt it.

The Britons petitioned Rome for assistance in A.D. 446. Cerdic landed in 494; the *groans of the Britons* were a piteous wail of "old men and children." The interval, taking it at three generations, passed in constant warfare, would not restore the balance of youth removed by conscription prior to A.D. 418, which circumstance therefore remains in part as an element to affect the general average of assumed longevity in the time of Cerdic. I urge this, with all respect, not as affecting, in any way, the real facts discovered or expounded by Prof. Rolleston, with whose address I was very much gratified; but only as affecting certain conclusions sought to be founded thereon. A. HALL

Curious Effect of the Words "Carmine" and "Germinal Matter"

DURING the twelve or thirteen years that Mr. Huxley has performed the duties of Examiner at the University of London, it appears that he has been much disturbed by the frequent use the candidates have made of the words, "cell," "germinal matter," and "carmine." He says: "*I declare to you I believe it will take me two years at least of absolute rest from the business of an examiner to hear either of the words without a sort of inward shudder.*" * This is surely a very remarkable declaration on the part of the examiner, since it is extremely doubtful if hearing these words ever made anyone else shudder, and if any other words written or spoken ever produced an effect so exceptional upon Mr. Huxley. LIONEL BEALE

Holly Berries

WILL some learned botanist, or Darwinian theorist, kindly inform me through the columns of NATURE why some holly berries appear obnoxious to birds? This is a great holly neighbourhood, and there are at present several trees actually loaded with ripe berries; the ground is also thickly strewed with berries beneath the trees, and yet not a single species of bird appears to eat them. Last winter the holly trees bore an abundance of berries, but the majority of the trees were stripped by the migratory *Turdi*, &c., as early as the beginning of February. I presume, in the "struggle for existence" these berries, obnoxious to birds, will stand a better chance of propagating and increasing that peculiar variety, and in course of time raise a distinct and well marked species.

Thruxton, May 23 HENRY REEKS

Origin of Languages

IT seems to me that your correspondents, Mr. Taylor and S. J., have discovered merely imaginary differences between the origin of species and the origin of languages. Mr. Taylor sees an essential difference in the fact that in the one case the process "is carried on by the countless efforts of *rational* beings," whilst in the other case there is "*reasonless* variation and selection."

* Address on Medical Education," *Lancet*, June 4th, 1870.

But I think the analogy still holds good, inasmuch as the "gradual variation, &c., of a few primary sounds," is not the result of an *intention* to originate a new language, any more than the origination of a new species of animal by natural selection is *intentional* on the part of the animals engaged in the struggle for life.

S. J.'s "impression is that the dialects which run wild are much more variable than those under man's care, which is the reverse of the case with wild and domestic animals and plants." But it must be remembered that it is the object of the "host of schoolmasters, lexicons, and grammars," who "watch over the Queen's English" to make that language uniform, to check its variations. If breeders of cattle made an effort to obtain perfect uniformity in a certain species of animal, I have no doubt that the wild herds of that animal, if allowed to exist under different conditions, would show much more variation "than those under man's care." But I question whether, in spite of the conservative influences of "schoolmasters, lexicons, and grammars" languages do not undergo as much variation through the *artificial selection* of writers, whose aim it is to make language more and more expressive, as animals and plants undergo through the artificial selection of breeders and agriculturists.

King's Lynn, May 28 ARTHUR RANSOM

STONE IMPLEMENTS FROM BURMA

SOME notes on the stone implements of Burma, by W. Theobald, jun., of the Geological Survey of India, contained in the number of the Proceedings of the Asiatic Society of Bengal for July, 1869, seem worthy of notice in these pages. "The implements are curious as differing in form and type, not only from anything found in India, but from anything hitherto described from any part of Europe, though any implement yet found in India has its precise analogue in Europe." According to Mr. Theobald, not only is the form but the material remarkable, as these Burmese implements are fashioned either of basalt or some schistose rock, quite unlike anything to be met with in the district where the implements themselves occur; a fact which he thinks points to their having been brought down from Upper Burma (where such implements are common) by the original settlers of the country.

That curious superstition which prevails over almost the whole of the globe, and connects the origin of these stone implements with the "thunderbolt," is found also in Burma. They are there called *mo-gio*, or thunderbolts, and are believed to accompany the lightning. If a flash of lightning is seen to strike the ground, and an earthen vessel is inverted over the spot, in the course of a year or so the *mo-gio* will be found in it, having worked its way back again to the surface by its own recoil.

The classical, or rather Plinian, view of this subject has been well given by Bishop Marbodæus, who wrote his Book "De Gemmis" early in the 12th century, and who thus speaks of the Ceraunius:—

> Ventorum rabie cum turbidus æstuat aër,
> Cum tonat horrendum, cum fulgurat igneus æther,
> Nubibus elisus cælo cadit ille lapillus,
> Cujus apud Græcos extat de fulmine nomen.

Its virtues were great in Europe as preserving from injury by lightning or shipwreck, and they had even aggressive as well as prophylactic powers, enabling the possessors to take beleaguered cities and to destroy hostile fleets.

In Burma they are also highly valued, but are put to crucial tests to prove them to be the genuine article, before a purchaser pins his faith to them. One test is that if wrapped in cloth and fired at with a gun, no effect will be produced either on the cloth or its contents, however near the aim may be taken, and it is from its presence producing invulnerability in its wearer that the *mo-gio* is mainly valued. It may be observed that it is not stated whether it is the seller who is entrusted to take aim with the gun. Another test of its celestial origin is placing it on a mat with a quantity of rice. If genuine, no fowl or other creature will venture near it. Again, a plaintain tree cut down with it ought to die, and not, as is usual, to send up a new shoot. If genuine, it preserves from fire, but it has also great medicinal virtues, and a small chip administered internally is considered a cure for inflammation of the liver or other internal organs, and is also a specific for ophthalmia. The virtues of stone axes in Germany, as summarised by Preusker in his "Blicke in die vaterländische Vorzeit," are curiously similar to those of the Burmese *mo-gios*. They preserve the house in which they are from lightning, they perspire when a storm is approaching, they are good for diseases of man and beast, they increase the milk of cows, they assist the birth of children, and powder scraped from them may be taken with advantage internally as a remedy for certain diseases.

The types of these Burmese instruments described by Mr. Theobald seem susceptible of arrangement under four heads.

1. "A rough, stout, wedge-shaped instrument," which, to judge from the figure, resembles closely the better finished specimens of flint hatchets, of the type which occurs in the Danish Kjökkenmöddings.

This form is very rare.

2. A hatchet with flat sides converging towards the base, which is square, and with a segmental edge, much like a common German form.

This type is common.

3. A long adze, with square, slightly converging sides, and a bevelled segmental edge, in character much resembling some of the implements discovered in Java, Borneo, and Sumatra, and also a New Zealand form; and—

4. Implements of the same character so far as the edge and sides are concerned, but having the butt end reduced in width so as to produce a square shoulder on each side of the blade. In some this reduction in width extends more than half the length of the blade, so as to produce a T-shaped form. These shorter specimens are the most common. This form appears to be peculiar to Burma. One of them has been figured by the Society of Antiquaries (Proc. N.S. vol. ii. 96).

In some cases the lashings used to fasten them to their hafts have left traces on the stone. The implements are usually picked up on the surface of the hills, in the fields, or clearings made for cultivation, and not in the plains. Mr. Theobald seems inclined to doubt whether, without the use of iron also, those who made these implements could have effected clearances in the gigantic forests of Pegu; but it may be urged against this view that by calling in the aid of fire the efficiency of such tools is almost as great as if they had been formed of metal, and it is difficult to conceive a people in possession either of bronze or iron bestowing the necessary time and trouble on the fashioning of stone tools, when those of metal were at their command, which, whether fire were employed in the clearance or no, were for general purposes so much more effective. If the makers of these stone tools had been in possession of other means for clearing the hill sides, then Mr. Theobald would be inclined to regard the stone relics as agricultural implements used in hand agriculture, at the end of sticks, as a kind of spade, to form the shallow holes for the cultivation of "hill rice." If not explained in this manner, he argues, we must regard them as weapons of the chase and war, though this use is, he thinks, negatived by their thoroughly inefficient character for such purposes. To this may be objected, first, that the material of which they are usually formed is basalt, a stone constantly used as a material for cutting tools; secondly, that the presence of the square shoulders, so like those on the horn sockets for hatchets of the Swiss Lake-dwellers, seems to testify to the tools having been used as adzes or axes, or possibly chisels; and thirdly, that if they had been required merely for hoeing or digging, the trouble of grinding and polishing might and would have been saved. We will only add that the paper is a valuable contribution to our knowledge of Eastern Neolithic implements, and that our present remarks are, like those of Mr. Theobald, "merely tentative, and designed to elicit additional information."

J. EVANS

M. FIZEAU'S EXPERIMENTS ON "NEWTON'S RINGS"

A COMPARISON of the values given by Professor Ångström (in his magnificent *Recherches sur le spectre solaire*) for the wave-lengths corresponding to the two principal components of Fraunhofer's line D, with some observations made eight or nine years ago by M. Fizeau, not only reveals a remarkable agreement between the results of these two distinguished investigators, but yields one of the most striking confirmations of the truth of the undulatory theory of light that recent optical research has afforded.

The experiments of M. Fizeau to which we refer were, essentially, the following. He produced the phenomenon of "Newton's rings," by laying a convex lens of very long focus upon a piece of glass with plane parallel surfaces, and illuminating the combination by the yellow flame of spirit of wine containing a little common salt. The lens was so arranged that it could either be made to touch the glass plate or be separated a short distance from it, its position being regulated by a micrometer screw. On gradually separating the lens from the glass plate, the rings were seen to contract and move in towards the centre of the lens, where they successively disappeared, while their place was supplied by fresh rings which made their appearance at the circumference of the lens. So far, all was in accordance with what was well-known before. But M. Fizeau found that when the phenomenon was observed with sufficient care, nearly 500 rings could be counted, flowing inwards one after another, but that after about this number the rings ceased to be visible, the surface of the glass showing a nearly uniform illumination all over instead of a sharply defined alternation of light and dark bands. When, however, the distance between the lens and the glass plate was further increased the rings re-appeared, getting gradually more and more distinct, until when nearly another 500 had passed they had become as sharp as at first; but a still further increase of distance caused them again to become confused, and they ceased a second time to be discernible at about the 1,500th. With a still greater separation of the glasses, however, they reappeared again, and became quite sharp at about the 2,000th, after which they for a third time got gradually confused and became indistinguishable at about the 2,500th.

So the phenomenon went on, the stream of rings inwards towards the centre of the lens, followed by fresh ones from the circumference, continuing as the lens was moved further and further away from the glass plate; but the succession of rings was not uniform, but broken up into batches of about 1,000 each, separated by short intervals of confusion in the way that has been described. The rings did not finally cease to be distinguishable until *fifty-two* such batches had been counted, and the two glasses were at a distance of about fifteen millimetres (more than half an inch) from each other.

This remarkable phenomenon of the alternate periods of distinctness and confusion of the rings is easily explained, as M. Fizeau points out, when we remember that the light employed was not strictly homogeneous, but consisted of two portions of nearly, but not quite, equal degrees of refrangibility. If either of these two constituent parts of the light had been used by itself, it would have produced a set of rings, but the rings of one set would have been a very little larger than the corresponding rings of the other. Hence if the two sets of rings are put together (as they were in Fizeau's experiment), they will nearly, but not quite, fit each other. If we examine a few rings at the centre, when the two glasses are in contact, they will appear to coincide precisely; but if they are traced to a sufficient distance from the centre, the coincidence is seen not to be exact. For although the *twentieth* ring (say) of one set is not perceptibly bigger than the twentieth ring of the other set, the *five-hundredth* of one set is perceptibly bigger than the five-hundredth of the other, and, when put upon it, falls almost exactly halfway between the five-hundredth and five-hundred-and-first of this set. Consequently, at about this part of the phenomenon, the bright spaces of one set of rings will occupy the same position as the dark spaces of the other set, and they will mutually obliterate each other. But since the *thousandth* ring of one set is nearly the same size as the thousand-and-first of the other, the two sets of rings will appear to fit each other again about this point; the *fifteen-hundredth* of the first set, however, is larger than the fifteen-hundred-and-first of the second set, but not so large as the fifteen-hundred-and-second; and hence, at about the position of this ring, the rings of the two sets will overlap each other, and mutually efface each other's outlines. And, carrying such considerations further, it is evident that the apparent coincidence and overlapping of the two systems of rings would recur alternately at regular intervals.

In order to simplify this explanation, we have tacitly assumed the lens to be so large that several thousand rings could be seen between its centre and its circumference. Practically, this would be impossible; but, by gradually separating the lens from the plane glass, we can, as it were, draw in towards the middle the rings which, with a larger lens, would be formed at a great distance from the centre.

Now, according to the explanation which the undulatory theory gives of the formation of "Newton's rings," the distance by which the interval between the glasses must be increased, in order that a given ring may come into the position previously occupied by the next smaller ring, must be equal to half the wave-length of the kind of light used for the experiment; and the distance of 0·28945 millimetres, through which, as M. Fizeau found by actual measurement, it was necessary to vary the space between the glasses, in order to make the rings go through one of the recurrent periods above described, that is to say, pass from sharpness to confusion and become sharp again, must contain just one more half wave-length of one portion of the light by which the rings were formed than it does of the other.

This brings us to the point of contact between M. Fizeau's observations and those of Prof. Ångström, to which we referred at the beginning. According to the latter, the wave-lengths of the two principal constituents of the light emitted by a flame containing the vapour of sodium (such as the flame employed by M. Fizeau) are respectively—

Millimetres
0·000589513
and 0·000588912.

Now, if we divide 0·28945 by half the former of these numbers, we get as the quotient 982; and if we divide it by half the second, we get as the quotient 983. That is to say, we find, precisely as the undulatory theory requires, that the distance measured by M. Fizeau contains exactly one more half wave-length of the more refrangible constituent of the light of a sodium-flame than it does of the less refrangible part. And, moreover, if we calculate, from Ångström's determination of the wave-lengths, the number of rings which must intervene between the positions of greatest confusion and greatest distinctness, we find 491 of the one set and 491½ of the other, which agrees entirely with M. Fizeau's estimated round number 500.

G. C. FOSTER

THE NEW AUSTRALIAN MUD-FISH

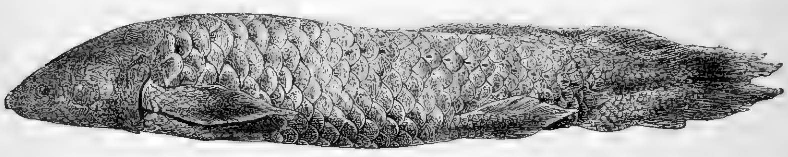
IN his well-known essay upon the arrangement of the extinct fishes of the Devonian epoch, published in the Memoirs of the Geological Survey* Professor Huxley, after showing that the *Polypterus* of the African rivers is probably the descendant of the Crossopterygian Ganoids with rhomboidal scales, continues as follows :—

"It is singular that while the line of the rhombiferous *Crossopterygidæ* has so distinct a modern representative, the cycliferous *Crossopterygidæ* seem to have died and left no issue at the end of the Tertiary epoch. But, without wishing to lay too much stress upon the fact, I may draw attention to the many and singular relations which obtain between that wonderful and apparently isolated fish, *Lepidosiren*, sole member of its order, and the cycloid glyptodipterine, ctenodipterine, phaneropleurine, and cœlacanth *Crossopterygidæ*. *Lepidosiren* is in fact the only existing fish whose pectoral and ventral members have a structure analogous to that of the acutely lobate paired fins of *Holoptychius*, of *Dipterus*, or of *Phaneropleuron*, though the fin-rays and surface-scales are still less developed in the modern than in the ancient fish. The endoskeleton of *Lepidosiren*, again, is as nearly as possible in the same condition as that of *Phaneropleuron*, and is more nearly similar to the skeleton of the Cœlacanths than that of any other recent fish ; while perhaps it is not stretching the search for analogies too far to discover in the stiff-walled lungs of *Lepidosiren*, a structure more nearly representing the ossified air bladder of the Cœlacanths than any with which we are at present acquainted, among recent or fossil fishes. Furthermore, *Lepidosiren* is the only fish whose teeth are comparable in form and arrangement to those of *Dipterus*. Though *Lepidosiren* may not be included among the Crossopterygidæ, nor even in the order of the Ganoids, the relations just pointed out are not the less distinct ; and perhaps they gain in interest when we reflect that while *Polypterus*, the modern representative of the rhombiferous Crossopterygidæ, is that fish which has the most completely lung-like of all air-bladders ; *Lepidosiren*, which has been just shown to be, if not the modern representative of the cycliferous Crossopterygidæ yet their 'next of kin,' is the only fish which is provided with true lungs. These are unquestionable facts. I leave their bearing upon the great problems of zoological theory to be developed by every one for himself."

In this remarkable passage, written some ten years ago, we may almost say that Prof. Huxley prophesied the discovery, announced in these columns some weeks since,† of the new Australian Mud-Fish, which appears to unite the dentition and other peculiarities of some of the extinct Crossopterygians with the external form of *Lepidosiren*, and thus to connect what, in the opinion of some naturalists, are two very widely separated forms of the order Pisces. Since the announcement of this discovery appeared, a paper has been read before the Zoological Society of London, containing a preliminary account of this wonderful fish, by Mr. Gerard Krefft, the curator and secretary of the Australian Museum, Sydney. Mr. Krefft proposes to call it *Ceratodus forsteri*, "after the Hon. William Forster, M.C.A., its discoverer."

The general form of *Ceratodus forsteri*, and its striking resemblance to *Lepidosiren*, will be at once seen by the accompanying figure, which has been reduced from one of the photographs forwarded to England by Mr. Krefft. The length of the specimen described (which, at the time the paper was written, was the only individual yet obtained) was about three feet ; it has a broad flat head, small eyes, and four limbs in the shape of flappers. The body is stated to be covered with large cycloid scales, ten rows on each side. A large gill-opening in front of the pectoral limb contains well developed branchiæ, but their accurate examination was not possible, on account of the bad condition of the specimen. A rather large pair of nostrils, situated just below the upper lip, communicates by a short tube with the roof of the mouth.

The skeleton of this fish is partly ossified and partly cartilaginous, the vertebræ being pure cartilage, and the ribs hollow tubes, filled with cartilaginous substance. The palate and upper part of the skull are bony, and the head is covered by two large scales. The tongue is very small, and is attached to what appears to be a large hyoid bone, ossified externally. The rays which support the dorsal and caudal fin consist of hollow tubes filled by cartilage. In the upper jaw are two large teeth, which Mr. Krefft terms incisors, and which are obviously the representatives of the peculiar teeth in the corresponding position in *Lepidosiren*.* Behind these are dental plates, divided on each side into six tooth-like projections. The lower jaw is provided with similar dental plates, but has no teeth in front ; the rami are joined only by a tough skin.



CERATODUS FORSTERI, THE NEW AUSTRALIAN MUD-FISH

Such is an abstract of Mr. Krefft's preliminary notice of this strange animal, which is certainly one of the most remarkable zoological novelties that Australia (that land of wonders) has yet produced. It is singular, indeed, that a creature like this, which appears to have been long well known to the squatters of Queensland, should have hitherto altogether escaped the observation of naturalists. It is said to have flesh of the colour of salmon, and to be excellent eating, so that the settlers have named it the "Burnett," or "Dawson salmon," from the two Queensland rivers in which it is principally found. The native name is given by Mr. Krefft as *Baramoonda* or *Baramoondi*. The fish is stated to attain sometimes a length of six feet and upwards.

* Decade x., 1861.

† See NATURE, No. 28, Vol. ii. p. 38.

* See Dr. Cobbold's discussion of these teeth, in his article on the cranium of *Lepidosiren* : Proc. Z.S., 1862, p. 129.

As regards the correct position of the *Ceratodus forsteri* in the "Systema Naturæ," we must, of course, wait until more specimens are procured for the examination of competent naturalists, the single example in the Australian museum being in an incomplete condition, from the internal organs having been removed. Mr. Krefft calls it a "gigantic amphibian," not being aware probably that all the best authorities now follow Johann Müller in classing *Lepidosiren* as a fish. It is, however, certain that, as Dr. Günther pointed out at the meeting of the Zoological Society when Mr. Krefft's paper was read, this fish must either be placed, along with *Lepidosiren* and its African representative *Protopterus*, in the order Dipnoi, or that it must form of itself a new division of the Ganoid fishes. We must know more of its internal structure, and in particular of the organs of circulation, before this question can be decided. Mr. Krefft has referred the fish to the genus *Ceratodus*, a name established by Agassiz in his "Poissons Fossiles" for the indication of certain teeth which were then supposed to be those of some kind of shark. Dr. Günther, our best living authority on the class of fishes, is, I believe, of opinion that, so far as the structure of *Ceratodus* is known, there is nothing to show that Mr. Krefft's decision is wrong, though it would appear to me to have been better to have proposed a new generic name for this animal.

In conclusion, I may express a hope that this short notice may have the effect of calling the attention of some of the colonists of Queensland to the wonderful nature of this relic of the Devonian epoch that is now swimming about beneath their noses, and that they will cease, for the present at least, to kill it and eat it as "salmon." Any specimens that may "rise to their fly" should be carefully kept out of the way of the cook, preserved in alcohol and transmitted to the British Museum or some other scientific institution. When the existence of *Ceratodus forsteri* becomes more widely known, there will be no lack of applicants for examples of it.

P. L. SCLATER

NOTES

THE Syndicate appointed by the University of Cambridge to consider the question of the manner in which provision may be made for the establishment of a Professorship of Physical Science, and increasing the facilities for instruction in it, have again reported to the Senate. It may be remembered that the former report, after having been discussed in the Schools, was referred back to the Syndicate by the Council. The present report, however, only differs in one important point from the former, in recommending that the increase in the tax levied upon the M.A. members of the University should be temporary instead of permanent. It had been hoped by many that the Syndicate would have taken courage from the general tone of the debate, have cancelled their former report, and proposed some bolder and more thorough measure. It is, however, not very probable that the University will accept the report. Several influential members of the Syndicate have not signed it; and there appears to be a growing feeling in Cambridge that, notwithstanding the obvious evil of delay, to postpone for a few months the appointment of a Professor would be better than to carry at once a temporary and unsatisfactory scheme.

THE Council of the Society of Arts at their meeting on Monday last, awarded the Albert gold medal for the present year, to M. F. de Lesseps, "for services rendered to arts, manufactures, and commerce, by the realisation of the Suez Canal."

WE regret to learn that Baron Liebig continues dangerously ill. He has recently submitted to two operations for a very painful abscess in the shoulder, and continues very weak, but perfectly clear and cheerful, although believing that death is near.

THE new lectures on physiology at Trinity College, Cambridge, by Prof. Foster, will, we understand, be open to all members of the University, so that this liberal act on the part of the college is practically equivalent to the foundation of a professorship of physiology in the University.

THERE will be an examination at St. Peter's College, Cambridge, on Tuesday, June 14, for a Natural Science Scholarship, of the value of 60*l.* per annum. It will be open to all persons who may desire to enter at the University. The subjects are chemistry, comparative anatomy, physiology, and botany. Names of candidates must be sent a week previously to Rev. J. Porter, tutor of the college, from whom further information may be obtained.

AT the meeting of the French Academy of Sciences, held on the 30th ult., Mr. Joule was elected a correspondent in the Physical section by 32 votes out of 43. Of the remainder, Professor Lloyd obtained eight, and Professors Angström, Dove, and Volpicelli one each.

THE Senate of the Queen's University in Ireland has unanimously passed a resolution conferring the honorary degree of Doctor in Science on Prof. William King, in recognition of his scientific attainments.

WE learn from the *Moniteur Scientifique*, that M. R. Radau has been charged with a commission from the French Minister of Instruction to visit Germany, for the purpose of studying the organisation for instruction in the higher mathematics in the principal universities of that country, and prepare a report.

A MONUMENT to John Kepler, at Weil der Stadt, the birthplace of the great astronomer, will be uncovered on the 24th inst. Subscriptions have been received from all parts of Germany, France, Russia, and even North America.

ON Friday, May 26, Dr. Carpenter delivered a lecture on the Physical and Biological Conditions of the Deep Sea, in the Senate House, Cambridge. His remarkably lucid and interesting account of the results of the late expeditions, illustrated by some beautiful diagrams, was listened to with great attention by a very large audience, and at the conclusion, the Master of Christ's College (who occupied the chair in the unavoidable absence of the Vice-Chancellor) proposed, and Prof. Sedgwick seconded, a vote of thanks to the lecturer, which was carried with enthusiasm. The latter made some remarks upon the bearing of Dr. Carpenter's discoveries upon Geology, which showed that notwithstanding the burden of eighty-five years, the old fire was still burning brightly.

DR. HASSKARL, formerly superintendent of the Botanic Gardens, and introducer of the Cinchona plant into Java, publishes a monograph of the Indian *Commelinaceæ*, especially those of the Indian Archipelago, with a few other species.

WE have received papers of Port Louis, Mauritius, for April 9th, containing a report of the meeting of the Meteorological Society, held March 24, when a valuable paper was read "On the Origin of Storms in the Bay of Bengal," by the secretary, Mr. C. Meldrum, with special reference to the monsoons and cyclones of the Indian Ocean.

THE last volume of the "Transactions of the Linnean Society," just published, contains Mr. Carruthers's long-expected paper "On Fossil Cycadean Stems from the Secondary Rocks of Britain."

MR. CHARLES T. BROWN, of the Geological Survey of Demerara, has lately returned from a journey of three months' duration in the interior. He has examined the Potaro, Siparunie, and Burroburro rivers, and the country beyond the heads of the latter two, which he finds to be table-land, composed of slightly-inclined beds of sandstone and conglomerate. On the Potaro river he met with a magnificent fall, hitherto unknown.

It is formed by the river falling from a table-land 1,375 feet above the sea, perpendicularly, in an unbroken fall of about 900 feet. The river is 100 yards wide, and from 10 to 15 feet deep in its deepest parts. It is with much regret we hear that the Combined Court of this colony have determined that this survey shall be discontinued.

IN the *Artisan* for June is an article by Mr. John Scott Russell, on "International Communication by Railway Steamships," a mode of performing the Channel transit which he has long advocated. The real point of difficulty he states to be this: Dover Harbour is the property of the Government; Calais Harbour is also the property of the French Government. He believes that if the two Governments lay their heads together, and simply make their harbours accessible, which can be done at a moderate expense, a great international communication can be easily established.

IN his annual address as President of the Canadian Institute, the Rev. Wm. Hincks makes use of the following argument in opposition to the Darwinian theory of Natural Selection:—"Nothing is to me more evident than that both seemingly permanent specific and higher differences, and varieties which have no pretensions to permanence, depend on the comparative development of different elements of a common plan; from which it seems to follow both that the non-existence from the commencement of living nature, of all the distinct plans of structure is in the highest degree improbable, and that the tendency of development, sometimes in one direction, sometimes in another, among the same primitive elements, must produce a harmonious system; whilst the preservation of the forms best adapted to a situation amongst a great number of variations arising without order must produce a confused mass of objects having no regular relations and incapable of being reduced to a common system. Which of these prevails in nature I cannot for a moment hesitate in deciding, and consequently I must maintain that, if there is variation, it must be within definite limits, and according to a fixed plan, so as to maintain a uniform order and harmony in the whole system."

A REMARKABLE *mirage* was seen at Ostend on May 19. Ships riding in the roads in the horizon were reflected in the sky as by a gigantic mirror; a brig, a steamer, and several fishing-boats appeared to have other similar vessels attached to the summits of their masts in a reversed position. The phenomenon lasted through the whole of the afternoon and evening, the wind being light from the N.N.E. From 5 to 6.30 P.M. the French coast was visible as far as Dunkirk, the houses being quite distinct, and the *dunes* appearing suspended in the air at a height of several metres, and somewhat moved in position towards the N.W. The port of Niewport seemed so near that the bridge was distinctly visible.

IN the last number of the *Revista das Obras Publicas* (Review of Public Works), Senhor C. A. Beirao mentions an interesting phenomenon of terrestrial refraction, observed by him on the evening (or afternoon, *vespera*) preceding the storm which caused such damages on the Tagus on Easter Day. Here is what Senhor Beirao says about the phenomenon:—"From the house in which I live, in the Rua (street) of S. Joao da Praça,* only about a third of the upper part of the Bugio Lighthouse is to be seen, all the rest is interrupted by the roofs of the buildings in the Praça do Commercio (called by the English 'Black Horse Square,' where all the public departments stand). On that occasion (half-past four o'clock P.M. on Saturday of Alleluia) as I by chance directed my spy-glass towards the tower (*i.e.* of the Bugio), I remarked that not only the latter was completely in view, but also the surface of the water to a distance of many metres on this side the Tower, which indicated, with respect to the latter, an elevation of fifty to sixty metres, at least, over the line of normal vision."

* This street is about four-and-a-half miles from the said Bugio lighthouse.

THE second part of the Proceedings of the Bristol Naturalists' Society contains a notice, by Dr. H. E. Fripp, of recent observations of Amœbæ and Monads by Greef and Cienowski, in which he describes the species of Amœbæ and Rhizopods recently discovered living in the earth; a paper by S. H. Swayne on the Scales and other tegumentary organs of Fish; an analysis by Mr. Sanders, of the Report on Theories of Elevation and Earthquakes, presented to the British Association by the late Professor Hopkins; a note by a lady associate on a novel application of tea leaves for promoting the growth of plants; and a paper on the Descent of Glaciers, by the Rev. Canon Moseley, in which he refers the motion of glaciers to a succession of small alternations of temperature, causing expansion and contraction with a cumulative effect. There is also a paper, by Mr. Stoddart, on Rain-water collected in Bristol, showing the presence of considerable amounts of saline material in the water.

AN icono-photograph album, containing numerous figures of the appearances presented by sections of the nervous centres, has just been presented by Dr. Ducheune, of Boulogne, to the French Academy of Medicine. He states he has obtained excellent results from sections of the great sympathetic nerve, the spinal ganglia, the spinal cord, and of the medulla oblongata when magnified from 8 to 500 times. The plan was suggested some years ago by Dr. Ducheune himself; but it was found that the photographs obtained in the ordinary method were not persistent. He therefore fixed them on stone by a process he terms photo-autography, the details of which, however, he does not communicate. It is satisfactory to find him stating that the results of his experiment and photographs only confirm the substantial accuracy of the beautiful drawings made by Dr. Lockhart Clarke on the central parts of the nervous system, and especially upon the medulla oblongata. In his later experiments Dr. Ducheune has adopted Dr. Clarke's mode of preparation with chromic acid and carmine. He states that certain micrographic details come out with wonderful clearness in the photographs, and that by this means some important additions may be made to our knowledge. Thus he has ascertained that in the white substance of the medulla oblongata there are a large number of very small nerve tubules (0^{mm}, 0033) diameter mingled with others of average and of large diameter 0^{mm}, 0 1 to 0^{mm}, 0 2 and ·0·3.

FROM the proceedings of the Institute of Lombardy, reported in the *Imparziale* of the 16th May, we extract the following results of the important experimental researches of Prof. Mantegazza on the action of the essences of flowers on the production of atmospheric ozone. 1. The essences of mint, turpentine, cloves, lavender, bergamot, anise, juniper, lemon, fennel, nutmegs, cajeput, thyme, cherry laurel, in contact with atmospheric oxygen in light, develop a very large quantity of ozone, equal if not superior in amount to that produced by phosphorus, by electricity, and by the decomposition of permanganate of potash. 2. The oxidation of these essences is one of the most convenient means of producing ozone, since even when in very minute quantity they can ozonise a large quantity of oxygen, whilst their action is very persistent. 3. In the greater number of cases the essences, in order to develop ozone, require the direct rays of the sun; in a small number of cases they effect the change with diffused light; in few or none, in darkness. 4. In some cases, however, the action just commenced in solar light was found to persist to some extent when the essence was placed in darkness. 5. In some cases a vessel perfumed with an essence and afterwards thoroughly washed with alcohol and perfectly dried, could still develop a proportionate quantity of ozone, provided that it retained a slight odour of the essence. 6. The essences that developed the largest quantity of ozone were those of cherry laurel, palmarosa, cloves, lavender, mint, juniper, lemons, fennel, and bergamot; those that gave it in less quantity were anise, nutmeg, cajeput, and thyme. 7. Camphor, as an ozonogenic agent, is inferior to all the above-named essences. 8. Eau de Cologne, honey water, and other perfumes, or aromatic tinctures, develop a proportionate quantity of ozone when they are exposed to the direct rays of the sun. 9. The flowers of the narcissus, hyacinth, mignonette, heliotrope, lily of the valley, &c., develop ozone in closed vessels. Flowers destitute of perfume do not develop it, and those which have but slight perfume develop it only in small quantities. As a corollary from these facts the professor recommended the use of flowers in marshy districts and in places infected with animal emanations, as the powerful oxidising influence of ozone may destroy them. The inhabitants of such regions should surround their houses with beds of the most odorous flowers.

THE fourth part of the "Researches undertaken in the Physiological Laboratory of Wurzburg," edited by Dr. Richard Gscheidlen, has been published. It contains a kindly notice of the life of Albert von Bezold, so untimely snatched away by death; a most profound investigation on nerve and muscle electricity, an abstract of which, we fear, would be unintelligible to our readers, by Worm-Müller, of Christiania; an essay on the Calabar bean, by the editor, and some remarks on the movements of the Iris, by Dr. Engelhardt. In this last it is shown that atropine paralyses the extremities of the third nerve, whilst Calabar bean acts as an irritant upon them, and he holds that there is a ganglionic nervous centre or organ imbedded in the iris, and intercalated between the fibres of the third and the sphincter which acts as an inhibitory centre on the sympathetic.

PRIZE MEDALS OF THE ROYAL GEOGRAPHICAL SOCIETY

THE Royal Geographical Society of London having taken into consideration the fact that a knowledge of geographical facts, and still more a knowledge of the science of geography, is not so common among educated persons as it ought to be, determined, about two years ago, to offer prizes in the shape of gold and bronze medals, to be competed for by boys from certain selected public schools. The principle which governed the choice of the schools, was the number of boys who were receiving education therein; no school having less than 200 boys being chosen for the competition.

An effort was made, at the time that the matter was under discussion, to get girls admitted as candidates. It was, however, objected that there were no public schools for girls which could be invited to compete, and it was also said that it might be well to see how the experiment would work with boys before inviting girls to the competition.

These questions being settled, the next thing to arrange was the manner in which the examinations should be conducted. It was obvious that to bring a number of boys from the various schools scattered all over the country to London, would be practically impossible; and it was equally obvious that some process of selection would have to take place in the schools themselves, so that the best boys only should have their papers sent to the examiners appointed by the Council of the Royal Geographical Society. It was therefore determined that the subjects of examination should be divided into Political Geography and Physical Geography, and that no candidate should be permitted to be examined in both subjects at the same time. Moreover, not more than four candidates in each subject were allowed to take the papers; these four, of course, having been selected by the masters of the various schools from among those most likely to pass. The Council of the Geographical Society could not undertake to direct any preliminary examination—that had to rest entirely with the authorities of the various schools.

It was ultimately determined that the examination should take place by means of papers of questions, which should be sent in sealed packets to the Head Masters of the selected schools; and that these papers should be given out and worked simultaneously at all the schools. The superintendence of the examinations was to be done by a master of each school, who was to be present during the whole time the paper was worked. In order to ensure, as far as possible, the strict fulfilment of the conditions under which the examination was to take place, a declaration was required to be signed by all the masters who were present while the work was going on. This declaration set forth that the candidates worked the papers without assistance, and that there were no globes or maps in the room where they were written. It also declared that the specified masters were present during the whole of the time.

Thirty-seven schools, containing in the aggregate about 12,700 boys, were invited to compete at the first examination. These schools included the nine schools of the Royal Commission of 1864, and all others in England, Scotland, and Ireland which, according to the latest edition of the "Public Schools Calendar," contained not less than 200 boys.

Twenty-one of the invited schools sent candidates; no school being permitted to send more than eight boys, four candidates in the subject of Physical, and four in that of Political Geography. Many schools did not send the maximum number. In fact, four schools had only one candidate each; and out of the twenty-one schools, only four presented the highest number of candidates allowed. Last year forty-two schools were invited to take part in the examinations, but only nineteen sent candidates.

The papers set, both in Physical and Political Geography, bore a remarkable resemblance to each other, and in looking over the syllabus published by the Geological Society, it is impossible not to feel that the line between the two is very feeble, and, in some instances, indefinitely indicated. The special subject appointed for next year is North America.

The examiners appointed for the first year were the Rev. W. G. Clark, F.R.G.S., Public Orator of the University of Cambridge, for Political, and Mr. A. R. Wallace, F.R.G.S., for Physical Geography; and for the second year the Dean of Chester for Physical, and Mr. Wallace for Political Geography.

The successful candidates received their awards last year and this at the anniversary meetings of the Royal Geographical Society.

Rossall School and the Liverpool College have been very distinguished in the examinations. In the first year Rossall carried off both the gold and the bronze medals for Physical Geography; and in the second year the bronze medal for Physical, and the gold medal for Political Geography, both fell to pupils of the same school. The Liverpool College gained the gold medal for Political Geography in the first year, and the gold for Physical, and bronze for Political, in the second year. Honourable mentions have also been gained by the pupils of various other schools.

A very remarkable fact, to be noticed as the result of these examinations, is that the schools which obtained the prizes are not those which the general public is accustomed to look upon as the leading schools of England; while Eton, Harrow, and Rugby are among those who have, as yet, sent no candidates at all. This is the more noticeable as, to Englishmen, the travellers of the world, and the subjects of a monarch on whose dominions the sun never sets, it does appear as if geography ought to be a subject of vast importance instead of being one in many cases almost neglected. Sir R. Murchison, in presenting the medals at the recent meeting of the Royal Geographical Society, hoped that Eton, Harrow, Rugby and other great schools might, in future years, send candidates, "for, without geography, a man cannot be said to be educated at all." Westminster School, it may be stated, to the disgrace of the rest, is the only public school represented in the competition, and has twice had honourable mention.

The Royal Geographical Society has made a wise step in inaugurating this movement, which will give, it is to be hoped, a powerful stimulus to the popular study of geography. The learned societies are at present too much dissociated from the general education of the country. Science has so few votaries among the bulk of the population, that a knowledge of scientific facts, with anything like accuracy, or an acquaintance with scientific methods of working, is almost totally absent from the education of the majority. The scientific man is still to too many persons a species of magician, arriving at his information in occult ways, not to be penetrated by the ordinary observer.

Government is called upon from all sides to do this and that in the matter of education. But Government is slow to move, and is quite sure not to please everybody when it does. In the meantime let scientific societies, each anxious for the spread of knowledge on its own subject, take example by the Royal Geographical Society. Let prizes and honourable mentions be offered, let them be somewhat difficult of attainment, and let the distinction be matter of public award; and it will soon be seen that the scientific education of the country will have received a healthy and vigorous impulse, which will do much to spread the desired instruction through all classes of the nation.

J. A. CHESSAR

ON THE PROGRESS OF BOTANY IN 1869

II.

WITH regard to the succession of races which have undergone a complete specific change through successive geological periods, we have not in plants, in as far as I am aware, any such cases of "true linear types or forms which are intermediate between others because they stand in a direct genetic relation to them," as Professor Huxley appears to have made out in favour of the pedigree of the horse in his last anniversary address to the Geological Society. And I may, in regard to plants, repeat with still greater emphasis his dictum, that "it is no easy matter to find clear and unmistakable evidence of filiation among fossil animals; for in order that such evidence should be quite satisfactory, it is necessary that we should be acquainted with all the most important features of the organisation of the animals which are supposed to be thus related, and not merely with the fragments upon which the genera and species of the palæontologist are so often based." The difficulty is much greater in the case of fossil plants; for instead of bones, teeth, or shells, portions of internal or external skeletons, the parts preserved to us from the Tertiary period are generally those least indicative of structural organisation. Mr. Carruthers has recently (*Geological Magazine*, April and July 1869, and *Journal of the Geological Society*, August 1869) adduced satisfactory evidence of the close affinity of *Sigillaria* and the allied genera of the coal-period with the living Lycopodiaceæ, formerly suggested by Dr. Hooker, but, as he informs me, no connecting links, no specimens indeed of the whole order, have as yet been found in any of the intermediate Cretaceous or Tertiary deposits. Among the latter the presence of numerous types, to which we may plausibly refer as to the ancestors of living races, is established upon unimpeachable data; but I have been unable to find that a single case of authentic pedigree, as successively altered from the Cretaceous through the abundant deposits of the Eocene and Miocene period to the living races, has been as yet as satisfactorily made out as that of the absolute identity of *Taxodium* and others above mentioned, although I feel very little doubt that such a one will yet be traced when our palæontologists will have ceased to confound and reason alike upon the best proved facts and the wildest guesses. Our late distinguished foreign member, Professor Unger, whose loss we have had so recently to deplore, had indeed, shortly before his death, published, under the name of "Geologie der Europäischen Waldbäume, part I. Laubhölzer," no less than twelve tabular pedigrees of European forest races; but it seems to me that in this, as in another of the same eminent palæontologist's papers to which I shall presently have to refer, his speculations have been deduced more freely from conjectures than from facts. There is no doubt that the presence of closely allied representatives of our Beeches, Birches, Alders, Oaks, Limes, &c., in the Tertiary deposits of central and southern Europe is fully proved by inflorescences and fruits as well as leaves; but how can we establish the successive changes of character in a race when we have only the inflorescence of one period, the fruit of another, and the leaf of a third? I do not find a single case in which all three have been found in more than one stage, and by far the great majority of these fossil species are established on the authority of detached leaves or fragments of leaves alone.

Now let us consider for a moment what place a leaf really holds in systematic botany. Would any experienced systematic botanist, however acute, on the sole examination of an unknown leaf, presume to determine, not only its natural order and genus, but its precise characters as an unpublished species? It is true that monographists have sometimes published new species founded on specimens without flower or fruit, which from collateral circumstances of habitat, collector's notes, general resemblance, &c., they had good reason to believe really belonged to the genus they were occupied with; but then they had the advantage of ascertaining the general *facies* derived from insertion, relative position, presence or absence of stipular appendages, &c., besides the data supplied by the branch itself. And with all these aids even the elder De Candolle, than whom no botanist was more sagacious in judging of a genus from general aspect, was proved to have been in several instances far wrong in the genus, and even order, to which he had attributed species described from leaf specimens only. Palæontologists, on the other hand, have, in the majority of these Tertiary deposits, had nothing to work upon but detached leaves or fragments of leaves, exhibiting only outward form, venation, and, to a certain degree, epidermal structure, all of which characters may be referred to that class which Professor Flower, in his introductory lecture at the Royal College of Surgeons in February last, has so aptly designated as *adaptive*, in contradistinction to essential and fundamental characters. They may, when taken in conjunction with relative individual abundance, assist in forming a general idea of the aspect of vegetation, and thus give some clue to certain physical conditions of the country; but they alone can afford no indication of genetic affinity, or consequently of origin or successive geographical distribution.

Lesquereux, in speaking of Cretaceous "species, or rather forms of leaves," observes in a note to his paper on Fossil Plants from Nebraska (*Silliman's Journal*, vol. xlvi. July 1868, p. 103), that "it is well understood that when the word *species* is used in an examination of fossil plants, it is not taken in its precise sense, for indeed no *species* can be established from leaves or mere fragments of leaves. But as palæontologists have to recognise these forms described and figured, to compare them and use them for references, it is necessary to affix to them specific names, and therefore to consider them as species." But the investigators of the Tertiary floras of Central and Southern Europe have acquired the habit, not only of neglecting this distinction, and naming and treating these forms of leaves as species equivalent to those established on living plants, but of founding upon them theories which must fall to the ground if such specific determination proves inaccurate. Nothing can be more satisfactory than such determinations as that of *Podogonium* for instance, which Professor Heer has succeeded in proving, by numerous specimens of leaves, fruits, and even flowers, some of them still attached to the branches, which I had myself the pleasure of inspecting last summer under the friendly guidance of the distinguished Professor himself. This genus of Cæsalpineæ, from its evident affinity with *Peltogyne*, *Tamarindus* and others now scattered over the warmer regions of America and Africa, and more sparingly in Asia, tells a tale of much significance as to the physico-geographical relations of the Swiss Tertiary vegetation, confirmed as it is by some other equally, or almost equally, convincing examples. But the case appears to me to be far different from the theory so vividly expounded by Professor Unger in 1861 in his Address entitled "Neu Holland in Europa;" this generally admitted theory seems to me to be established on some such reasoning as this:—There are in the Tertiary deposits in Europe, and especially in the earlier ones, a number of leaves that look like those of Proteaceæ; Proteaceæ are a distinguishing feature in Australian vegetation; *ergo*, European vegetation had in those times much of an Australian type derived from a direct land communication with that distant region.

This conviction that Proteaceæ, belonging to Australian genera, were numerous in Europe in Eocene times, is indeed regarded by palæontologists as one of the best proved of their facts. They enumerate nearly 100 Tertiary species, and most of them with such absolute confidence that it would seem the height of presumption for so inexperienced a palæontologist as myself to express any doubt on the subject. And yet, although the remains of the Tertiary vegetation are far too scanty to assert that Proteaceæ did not form part of it, I have no hesitation in stating that I do not believe that a single specimen has been found that a modern systematic botanist would admit to be Proteaceous, unless it had been received from a country where Proteaceæ were otherwise known to exist. And, on other grounds, I should be most unwilling to believe that any of the great Australian branches of the order ever reached Europe. As this is a statement requiring much more than mere assertion on my part, I shall beg to enter into some detail, commencing with a short summary of my grounds of disbelief in European Tertiary Proteaceæ, and then examining into the supposed evidences of their existence.

The analysis and detailed descriptions I have had to make within the last few months of between five and six hundred Proteaceæ, and consequent investigation of their affinities and distribution have shown that the order as a whole is one of the most distinct and most clearly defined amongst Phanerogams. I do not know of a single plant intermediate in structure between that and the nearest allied orders, which I cannot say of any other of the large orders I have worked upon. There is, moreover, especially amongst the Nucamentaceæ, a remarkable definiteness in the majority of genera without intermediate species, whilst the whole order exhibits the greatest uniformity in some of its most essential characters, derived from the arrangement of the floral organs and the structure of the ovary and embryo, accompanied by a truly Protean foliage. All this points, in my mind, to unity of origin, very great antiquity, and long isolation in early times. And the species themselves appear to be for the most part constitutionally endowed with what I designated in my last year's address as individual durability rather than with rapidity of propagation. The order may be divided into about five principal groups, more or less definite in character, but very different in geographical distribution. First, the Nucamentaceæ (from which I would exclude *Andripetalum* and *Guevina*), which we may suppose to be the most ancient, and perhaps the only one in existence where Proteaceæ inhabited some land in direct communication, either simultaneously or consecutively, with extra-tropical Africa and Australia; for it is the only group now represented in the former. It is pre-eminently endowed with the characteristic definiteness and durability of the order. It is very natural as a whole; it has about 250 species in eleven distinct African genera, and nearly 200 species in twelve equally distinct Australian genera, no single genus common to the two countries, and the species mostly abundant in individuals in very restricted localities. In both countries it is chiefly confined to the south. Africa sends only one or two species northward, as far as Abyssinia. The Australian portion has extended to New Zealand, where it has left a single species, now quite differentiated from the Australian ones; very few species (not half-a-dozen) have reached tropical Australia; and, if ever it extended farther, no representatives have yet been discovered in America, Asia, or even in New Caledonia. The four remaining groups, constituting the Folliculares, must have all been formed since the isolation from Africa. 1. Banksieæ, two genera, with above 100 species, have the type of distribution of the Australian Nucamentaceæ, chiefly southern, local, and abundant in individuals, with three or four species penetrating into the tropics, but none beyond Australia 2. Grevilleeæ, in which the genera are somewhat less definite and the distribution more extended, have above 300 species in about eight genera, of which the greater portion are still southern and local; but yet a considerable number are tropical, and a few extend to New Caledonia, although none beyond that. 3. Embothrieæ, with about twenty-five species in half a dozen genera, form part of that southern, chiefly mountain, flora which extends from Tasmania and Victoria to New Zealand, Antarctic and Chilian America, a flora which comprises many species which we might imagine to have spread from the northern hemisphere down the Andes to Antarctic America, and thence to New Zealand and Australia, whilst others may have extended in a contrary direction; and amongst these we may conjecturally include the Embothrieæ, which in America are not found farther north than Chile; whilst in Australia, although chiefly from the southern and eastern mountains, two or three species are northern, and one or two more are found in New Caledonia, but none in the Indian Archipelago, nor in Continental Asia. 4. We have lastly the tropical form of Proteaceæ, the Helicieæ, which are but a slight modification in two different directions (modifications either of the flower or of the fruit) of the Grevillea type, probably of a comparatively recent date; and although now widely spread over South America and Asia, have, nevertheless, left representatives in the original Grevillea regions of Australia. There are nearly 100 species in about eight genera, almost all tropical or subtropical; three small genera are exclusively Australian; *Helicia* itself is Asiatic, chiefly from the Archipelago, extending, in four species, to tropical Australia; in one or two species to New Caledonia; in two or three northward to the mountains of Bengal and Sikkim; and in one species even to Japan. Two American genera, with about forty species, are represented in New Caledonia by one genuine species of each, and one of an allied genus or section; and in tropical Australia by one species showing still the Australian connection; and two small genera are, as far as hitherto known, exclusively American; and may have been there differentiated. No Helicieæ, nor indeed, as already observed, any Folliculares, have hitherto been discovered in Africa. If, therefore, Proteaceæ have really ever extended to Europe, it would naturally be in this Helicioid group that we should seek for them. As far, however, as I can learn, among the supposed century of European Proteaceæ, there is only one which palæontologists refer to it, the *Helicia sotzkiana* of Ettingshausen, founded on a single leaf, which Ettingshausen himself admits to bear much resemblance to the leaves of about twenty genera in thirteen different families; and, upon much consideration, he thinks it rather more like a *Helicia* than anything else, and therefore definitively names it as such, a decision in which it is difficult to concur.

In answer to the above negative considerations, which, after all, lead to presumption only, we are told that we have positive evidence of the existence of Proteaceæ in the Miocene, and still more in the Eocene formations of Europe, in leaves, fruits, and seeds. As none of these have been found attached to the branches nor even in sufficiently abundant proximity to be matched with anything like certainty, we must take the three separately. First, as to seeds, those referred by palæontologists to Proteaceæ are winged and samaroid, some of them probably real seeds, shaped, without doubt, like those of some *Hakeæ* and *Embothria*, but quite as much like those of several Coniferæ, or of certain genera of Meliaceæ, Sapindaceæ, and various other Dicotyledonous orders, there being no evidence of internal structure, conformation of the embryo, &c., by which alone these several samaroid seeds can be distinguished. Moreover, those figured by Ettingshausen in his paper entitled "Die Proteaceæ der Vorwelt" (Proc. Imp. Acad. Sc. Vienna, vii. 711, t. xxxi. f. 11, 12, 14, 15, and 8), have a venation of the wing very different from that of any Proteaceæ I have seen, and much more like that of a real samara of an ash. Next, as to fruits, the hard follicles or nuts of Proteaceæ are as remarkable for their durability as the capsules of so many Australian Myrtaceæ; and we should be led to expect that, where Proteaceous remains are abundant, they should include a fair proportion of fruits, as is the case with the Conifers, Leguminosæ, &c., which have been undoubtedly identified. These supposed Proteaceous fruits in the Tertiary deposits are, however, exceedingly rare. The only ones I have seen figured are: (1) a supposed Embothrium fruit figured by Heer in his Tertiary Flora of Switzerland (t. xcvii. f. 30), an outline impression, with a deficiency in the upper portion, and without indication of internal structure; if this deficiency were filled up, and the seeds inserted, as in the imaginary restoration, f. 31 (for which I see no warrant, and in which the seeds are in the wrong position), it would be something like, but to my eyes not much like, the follicle of an *Embothrium*, and quite as much like what Ettingshausen figures (t. xxxi. f. 5) as the veinless leaf of a *Lambertia*; and (2) the supposed *Persooniæ* and *Cenarrhenes* drupes figured by Ettingshausen (t. xxx.) The former, in the absence of all indication of structure, are quite as good, if not better, representations of young fruits of *Ilex*, *Myoporum*, and many others, as of *Persoonia*; and where, in figures *c* and *d* of the same plate, recent (unripe) Persoonia fruits are inserted, for comparison, with the fossil figures β, γ, and δ, it appears to me that in the latter the long point is the pedicel, and the short point the style, whilst in the former, on the contrary, the short point is the pedicel, and the long one the style. To suppose that fig. 5 of the same plant represents the fruit of a *Cenarrhenes*, which, as far as known, has always an obliquely globular drupe, requires indeed a great strain upon the imagination. I can find no other fossil Proteaceous fruit figured or described.

Lastly, with regard to leaves, necessarily the mainstay of palæontologists, I must admit that there is a certain general *facies* in the foliage of this order that enables us in most, but not in all cases, to refer to it with tolerable accuracy leafy specimens known to have come from a Proteaceous country, even without flowers or fruit; but as to detached leaves, I do not know of a single one which, in outline or venation, is exclusively characteristic of the order, or of any one of its genera. If we know the genus and section of a specimen, we may determine its species by the venation; and we may sometimes fairly guess at its genus if we know it to be Proteaceous; but that is all. Outline is remarkably variable in many species of Grevillea and others, and venation is not always constant even on the same individual. But then we are told, with the greatest confidence, that the structure of the stomata in these fossil leaves, as revealed by the microscope, proves them beyond all doubt to be Proteaceous. In reply to that, I can only refer to the highest authority on these curious organs, Hugo Mohl, who, in a very careful and elaborate memoir specially devoted to the somata of Proteaceæ, has the following passage ("Vermischte Schriften," p. 248):—"Striking as is the above-described structure of the stomata in Proteaceæ, we should, nevertheless, not be justified in regarding this as a peculiarity of this family; for all the variations which we meet with in the structure of the stomata in Proteaceæ are also to be found in plants belonging to widely distant orders."

From the above considerations, I cannot resist the opinion that all presumptive evidence is against European Proteaceæ, and that all direct evidence adduced in their favour has broken down upon cross-examination. And however much these Eocene leaves may assume a general character, which may be more frequent in Australia (in Proteaceæ and other orders) than elsewhere, all that this would prove would be, not any genetic affinity with Australian races, but some similarity of causes producing similarity of adaptive characters.

Another series of conclusions drawn by palæontologists from their recent discoveries, which appears to me to have been carried too far, relates to the region where a given species originated. The theory that every race (whether species or group of species derived from a single one) originated in a single individual, and consequently in one spot, from which it has gradually spread, is a necessary consequence of the adoption of Darwinian views; and when Mr. R. Brown ("On the Geographical Distribution of Conifers," Trans. Bot. Soc. Edin. x. p. 195) sneers at my having qualified it as a perfect delusion, he must have totally misunderstood, or rather misread, the passage he refers to in my last year's address. The expression is there specially applied to the idea of general centres of creation, whence the whole flora of a region has gradually spread, in contradistinction to the presumed origin of individual races in a single spot, which is there as distinctly admitted. The determination of where that spot is for any individual race is a far more complicated question than either geographical botanists or palæontologists seem to suppose. "Every vegetable species," as well observed by Prof. Heer, "has its separate history," and requires a very careful comparison of all the conclusions deducible as well from present distribution as from ancient remains. The very important fact that *Taxodium distichum*, *Sequoiæ*, *Magnoliæ*, *Salisburia*, &c., existed in Spitzberg in Miocene times, so satisfactorily proved by Heer, shows that the vegetation of that country then comprised species and genera now characteristic of North America; but it appears to me that the only conclusion to be drawn (independently of climate and geology) is, that the area of these species and genera had extended continuously from the one country to the other, either at some one time or during successive periods. The proposition that "Spitsberg appears to have been the focus of distribution of *Taxodium distichum*," because an accidental preservation of its remains shows that it existed there in the Lower Miocene period, would require at least to be in some measure confirmed by a knowledge of the flora of the same and preceding periods over the remainder of its present area, the greater part of which flora is however totally annihilated and for ever concealed from us. The fact that *Pinus abies* existed in Spitsberg in Miocene times, and that no trace of it has been found in the abundant Tertiary remains of Central Europe, is very instructive. It might show that that tree was of more recent introduction into the latter than the former country; but it cannot prove that it was not still earlier in some other region, whence it may have spread successively into both territories, still less that its course of dissemination was directly from Spitsberg over Northern and Central Europe. Moreover, the determination of *Pinus abies* is not so convincing as that of the *Taxodium*, resting as it does, if I correctly understand Prof. Heer's expression, on detached seeds and leaves, with a few scales of one cone, and may require further confirmation.

In the above observations it is very far from my wish to detract from the great value of Professor Heer's researches. Interested as I have been in the investigation of the history of races of plants, I have deeply felt my general ignorance of palæontology, and consequent want of means of checking any conclusions I may have drawn from present vegetation by any knowledge of that which preceded it, and the impossibility at my time of life of entering into any detailed course of study of fossils. Like many other recent botanists, I am obliged to avail myself of the general results of the labours of palæontologists, and if I have here ventured on a few criticisms, it is only as a justification of the hope that they may in some measure distinguish proved facts from vague guesses, in order that we may know how far reliance is to be placed on their conclusions.

Spontaneous generation, or Heterogeny, is a question which continues to excite much interest. It has been the subject of detailed memoirs, of violent controversies, and of popular articles in this country, and still more on the Continent; but the solution of the problems still involved in doubt does not seem to me to have much advanced since I alluded to the opposing theories of Pasteur and Pouchet in my Address of 1863. The present state of the case appears to me to be this: in the higher orders of animals every individual is known to proceed from a similar parent after sexual pairing; in most plants, and some of the lower animals, besides the result of that sexual pairing which they all are endowed with, reproduction from the parent may take place by the separation of buds, by division, or sometimes by parthenogenesis; in some of the lower Cryptogams, the first stage in which the new beings are separated from the parent is that of spores termed agamic, from the belief that they never require previous sexual pairing, although the range of these agamic races is being gradually restricted, a remarkable advance having been recently made in this direction by Pringsheim in his paper on the pairing of the Zoospores in *Pandorina* and *Eudorina*. In all the above cases, in all organised beings which in their earlier stages are appreciable through our instruments, every individual has been proved to have proceeded in some stage or another from a similarly organised parent. But there are cases where living beings, Vibrios, Bacteria, &c., first appear under the microscope in a fully formed state, in decaying organic substances in which no presence of a parent could be detected or supposed: three different theories have been put forward to account for their presence: first, that they are suddenly created out of nothing, or out of purely inorganic elements, which is perhaps the true meaning disguised under the name of spontaneous generation, a theory not susceptible of argument, and therefore rejected by most naturalists as absurd; secondly, that they are the result of the transformation of the particles of the organic substances in which they are found, without any action of parent Vibrios or Bacteria; and this appears to be what is specially termed Heterogeny; thirdly, that there existed in these organic substances germs which had proceeded from parent Vibrios and Bacterias, but too minute for optical appreciation, and that their generation was therefore normal. The supporters of Heterogeny rely on the impossibility of accounting for the appearance of the Vibrios and Bacterias in any other manner; for they say that although you treat the medium by heat in a hermetically closed vessel in such a manner as to destroy all germs and intercept all access, still these beings appear. This their opponents deny, if the experiments are conducted with proper care. So it was seven years ago, and so it is still, although the experiments have been frequently repeated in this country, in France, and in North America, almost always with varying results. All reasoning by analogy is still in favour of reproduction from a parent; but Heterogeny has of late acquired partisans, especially in Germany, among those who are prepared to break down the barriers which separate living beings from inorganic bodies.

Brown's celebrated theory of the Gymnospermy of Conifers and allied orders has been of late the subject of keen controversy. Objected to by Baillon, Parlatore, and others, it had been strongly supported by Caspary, Eichler, and lastly, by Hooker in his important Memoir on *Welwitschia*, published in our Transactions in 1863. There the question seemed to rest till last year, when two detailed papers appeared, the one contesting, the other advocating the theory. The most elaborate is without doubt that of Gustav Sperk, in the "Memoirs of the Imperial Academy of Sciences at St. Petersburg." He gives a very fair *résumé* of all that had been published on the subject, and proceeds to record in detail his own observations on the structure and anatomy of the flower in a considerable number of Coniferæ, of *Ephedra alata*, *Gnetum latifolium*, and two species of *Cycas*, illustrated by well-executed analytical figures. He endeavours to prove, chiefly by their anatomy and development; that the coating which encloses the nucleus is carpellary, not ovular, of independent origin, always free, and often earlier developed than the nucleus—that what is wanting in gymnosperms is not the ovarium or carpellary envelope, but the ovular coating—that these plants are in fact gymnosperms in the sense of having naked nuclei and embryo-sacs, not naked ovules.

P. Van Tieghem, on the contrary, in the *Annales des Sciences Naturelles*, ser. 5, vol. x., considers the gymnospermy of the ovules of Conifers to be proved by the anatomical structures of the organs on which they rest. He says that, as in normal Dicotyledons, the ovules are developed from, and continuous with, the margins of carpellary leaves, but these carpellary leaves are open, variously or imperfectly developed, and constitute solitary leaves on a secondary branch in the axil of the subtending bract, this secondary branch being arrested in its development, and the carpellary leaf facing the bract; the paper is illustrated by a large number of diagrams. These two Memoirs, published simultaneously at St. Petersburg and at Paris, contain of course no reference to each other. How far each author may or may not have proved his case, I cannot now take upon myself to inquire into. Neither of them appears to have had any knowledge of the views of Professor Oliver, who in his review of Hooker's Memoir on *Welwitschia* (Nat. Hist. Review, 1863) suggests the analogy of the disputed organ with the axial developments known under the name of floral discs. Both writers, however, confirm the anomalous structure of the flower in this great class of plants, and the position of the plants themselves in many respects intermediate between the higher Cryptogams and Dicotyledons, their connection with the former being clearly shown by the researches of Carruthers and other palæontologists, and with Dicotyledons through *Welwitschia* by Hooker in his above-mentioned Memoir.

Teratology is a subject which has again risen into importance, as aiding in the history of the variations worked upon by natural selection in the formation of species. There had always been a tendency to attribute monsters and prodigies, whether in the organic or the inorganic world, to an infraction of the laws by which natural phenomena are regulated, by the intermediate interposition *ad hoc* of a supreme will for temporary motives inscrutable to man, in which all that the man of science was called upon to do was to establish their authenticity, and detail their abnormities. This, however, was considered by D'Alembert as sufficient to constitute Teratology as one of the great branches of Natural History taken in its most extended sense; for in his once celebrated "Système Figuré des Connaissance Humaines," *Histoire Naturelle* has three great branches—*Uniformité de la Nature*, or the study of the laws which govern the organic or inorganic world, terrestrial and celestial; *Ecarts de la Nature*, the science of prodigies and monsters; and *Usages de la Nature*, or arts and manufactures. Jeremy Bentham, in his "Essay on Nomenclature and Classification," of which I published a French edition now nearly half a century since, strongly criticised such a classification, "by which a middle-sized man is placed in one niche, a tall man and a short man together in another."* Mr. Galton however, in his recently published interesting researches on Hereditary Genius, shows us, after Quetelet, that even in this respect the laws which govern the deviations from the average height of man, both above and below that average, are uniform under similar conditions, and may well be studied together.

We may not, indeed, with D'Alembert, combine the history of animal and vegetable monstrosities with that of mineral monsters and celestial prodigies (whatever these may be); but the course which Biology has taken in the last few years has shown the necessity of accurately investigating in each branch all observed departures from what appears to be the ordinary course, before the real laws of that ordinary course can be ascertained. A work, therefore, in which these observed aberrations are carefully collected, tested, and methodised, cannot fail to be of great use to the physiologist, and such a work with regard to plants, the want of which, brought down to the present state of the science, I alluded to in my Address of 1864, has now been provided for us by Dr. Masters, in his "Vegetable Teratology,"—a work which we should especially like to see deposited in local libraries at home and abroad, to which observers resident in the country could have ready access. Monstrosities or deviations from the ordinary forms in plants are comparatively rare and evanescent; they can be best observed in their fresh state, and often require watching in the course of their development. Country residents have the best means of doing so, and to them it is very important to have a systematic work at hand by which they can ascertain whether the aberration they have met with is one well known or of frequent occurrence, or whether it presents any new feature, adding another item to our store of data, and therefore requiring closer observation and accurate record.

* Essay on Nomenclature and Classification, or *Chresstomathia*, part ii. 1817, p. 157, French Edition, 1823, p. 48.

In making use, however, of Teratology in explanation of structure and affinities, great care is required. It is not every one who can handle these phenomena with the tact of a Darwin. In the course of my systematic labours I have met with several instances where teratologists have been led into conclusions which have proved to be far wide of the truth, owing to their having confined themselves to teratology to the neglect of homology and organogeny. This importance of teratological facts to the physiologist who is able duly to appreciate their bearing, and the discredit cast on their study owing to their misuse in hasty and incautious speculations, are alluded to in Dr. Masters's Introduction. But beyond some explanations of causes suggested by the bringing together a series of facts showing a physiological connection with each other, and with more normal formations, he enters little into the various questions the solution of which has been more or less attempted by the aid of teratology. These questions, indeed, could not have been discussed without fully working out on each occasion normal organogeny, development, and homology, and thus leading him far beyond the object of the present work, which was to present to the future physiologist such a digested record of facts as should best show their relative bearing to each other, to normal conditions, and to any observed causes of disturbance. This object appears to have been well fulfilled, and the method adopted by the author probably the best suited to the purpose. A classification, founded upon the nature of the causes inducing the several changes, might, indeed, as he observes, have been theoretically the best, but is wholly impracticable until these causes shall have been satisfactorily ascertained. For the inquiry into these causes this teratological digest supplies a record of one class of facts, a necessary one, but only one of many classes on which it must be founded.

G. BENTHAM

SCIENTIFIC SERIALS

Journal of the Chemical Society, April 1870. This number contains a "Note on some Reactions of Alcohols," by Mr. E. T. Chapman. The author finds that on distilling with caustic soda a mixture of the rotating and non-rotating amylic alcohols to dryness, the distillate contains a larger proportion of the rotating alcohol than the original liquid; and, on adding water to the residue of sodic amylate and distilling the alcohol which passes over with the water is almost free from the rotating variety. A repetition of the process renders it quite pure. He also finds that repeated treatment of the rotating alcohol by caustic soda converts it into the non-rotating. On treating amylic alcohol to which about 2½ per cent. of water was added with a quantity of sodium just sufficient to decompose the water, and distilling, water first passed over, followed by amylic alcohol; sodic amylate almost free from caustic soda remaining in the retort; showing that the sodium replaces the hydrogen of the alcohol in preference to that of the water. Again, on distilling a solution of caustic soda in amylic alcohol, water passed over with the alcohol, the residue being sodic amylate.—"Note on the Organic Matter contained in Air," by Mr. E. T. Chapman. Several methods were tried for collecting the organic matter from the air before estimating its quantity. Passing the air through water in a Liebig's potash apparatus, or even in a tube with twenty-five bulbs, did not fix the whole of the organic matters. Cotton wool and gun-cotton failed on account of their invariably containing nitrogenous bodies, which vitiated the results; the condensation of steam in the air and washing with fine spray were better, but not satisfactory. Filtering the air through asbestos paper succeeded very well, but the asbestos was difficult to manage. The process finally adopted was to pass 100 litres of air through a quantity of finely powdered and moistened pumice stone, placed on a piece of wire gauze, fixed on the wide end of a funnel; distilling the pumice with dilute potassic hydrate and potassic permanganate, and determining the quantity of ammonia in the distillate by Nessler's test. In crowded rooms and near an untrapped sink, the air was found to contain organic bases as well as ammonia. 100 litres of air from crowded rooms contained quantities of nitrogenous substances, producing from 0·02 to 0·35 milligrammes of ammonia.—Then follows a lecture by Dr. Gladstone on "Refraction equivalents," which has already been noticed in these columns. The number concludes with a long paper by Dr. Thudichum on "Kryptophanic acid, the normal free acid of Human Urine." From the analysis of the salts it appears to be a dibasic acid of the formula $C_5 . H_9 \ N \ O_5$ or a tetrabasic acid containing $C_{10} H_{18} N_2 O_{10}$.

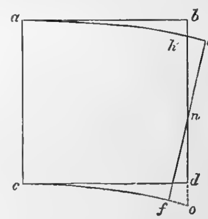
The May number of the *Journal of the Chemical Society* opens with an important paper by Mr. W. H. Perkin on "Artificial Alizarin." He first gives a sketch of the notions entertained by chemists as to the composition and relations of this important colouring matter prior to the investigations of Messrs. Graebe and Liebermann. These chemists showed that by treating alizarin obtained from madder with powdered zinc, a hydrocarbon was produced of the composition $C_{14} H_{10}$ and possessing all the properties of the anthracene of coal tar. By oxidising anthracene it is converted into anthraquinone $C_{14} H_8 O_2$; by the action of bromine dibromanthraquinone $C_{14} H_6 Br_2 O_2$ is produced, and by subsequent treatment with potassic hydrate at a high temperature, an alkaline solution of alizarin $C_{14} H_8 O_4$ is obtained—being the first instance of the artificial formation of a natural colouring matter. Mr. Perkin, in England, and Messrs. Caro, Graebe, and Liebermann, in Germany, have succeeded in obtaining the alizarin without the use of bromine. This may be effected by treating the anthraquinone with strong sulphuric acid, when disulphanthraquinonic acid $C_{14} H_6 O_2 (HSO_3)_2$ is formed, and this, when digested with potassic hydrate at 180°, gives rise to potassic sulphate and the potash compound of alizarin. The alizarin prepared from anthracene is identical with that extracted from madder. It has the same appearance, behaves in the same manner with reagents, produces the same effects in the dye bath, and exhibits the same absorption bands when examined spectroscopically. Two patterns dyed with artificial alizarin accompany the paper.—The second paper is by Mr. John Hunter, and contains some "Analyses of Deep Sea Water, and of some Ooze from the bottom of the Atlantic," collected during the expedition of H.M.S. *Porcupine*.—We then have a paper by Dr. Gladstone, on the "Refraction Equivalents of Aromatic Hydrocarbons and their Derivatives," being a continuation of the subject of the lecture reported in the previous number. Messrs. T. Bolas and C. E. Groves have experimented on the preparation of bromopicrin. They give exact directions for the preparation of this substance by acting on picric acid with bromide of lime, a compound analogous to bleaching powder. Bromopicrin has a very high specific gravity, 2·811 at 12·5°C. The authors announce that they have obtained the carbonic bromide CBr_4 by acting on bromopicrin with powerful brominating agents.—The concluding paper is by Prof. How, on an "Acidified Water from the Coal-field of Stellarton, Nova Scotia." This water was found to be distinctly acid, in consequence of its containing a considerable quantity of free sulphuric acid, probably produced from the iron pyrites present in the coal strata.

SOCIETIES AND ACADEMIES

London

Royal Society, May 19.—"On the Cause and Theoretic Value of the Resistance of Flexure in Beams." By W. H. Barlow, F.R.S.

The author refers to his previous papers, read in 1855 and 1857, wherein he described experiments showing the existence of an element of strength in beams, which varied with the degree of flexure, and acts, in addition to the resistance of tension and compression of the longitudinal fibres. It was pointed out that the ratio of the actual strength of solid rectangular beams to the strength, as computed by the theory of Leibnitz is, in cast iron, as about $2\frac{1}{4}$ to 1; in wrought iron as $1\frac{3}{5}$ and $1\frac{3}{4}$ to 1; and in steel, as $1\frac{3}{5}$ and $1\frac{2}{5}$ to 1. The theory of Leibnitz assumes a beam to be composed of longitudinal fibres only, contiguous, but unconnected, and exercising no mutual lateral action. But it is remarked that a beam so constituted would possess no power to resist transverse stress, and would only have the properties of a rope. Cast iron and steel contain no actual fibre, and wrought iron (although some qualities are fibrous) is able to resist strain nearly equally in any direction. The idea of fibre is convenient as facilitating investigation; but the word fibre, as applied to a homogeneous elastic solid, must not be understood as meaning filaments of the material. In effect it represents lines of direction, in which the action of forces can be ascertained and measured, for in torsion-shearing and "*angular deformation*" the fibres are treated by former writers as being at the angle of 45°, because it has been shown that the diagonal resistances have their greatest manifestation at that angle. Elastic solids being admitted to possess powers of resistance in the direction of the diagonals, attention is called to omission of the effect of resistance in the theory of beams. The author then states, as the result of his investigation, that compression and extension of the diagonal fibres constitute an element of strength equal to that of the longitudinal fibres, and that *flexure* is the consequence of the relative extensions and compressions in the direct and diagonal fibres, arising out of the amount, position, and direction of applied forces. Pursuing the subject, it is shown that certain normal relations subsist between the strains of direct fibres and their relative diagonals, evenly distributed strain being that in which the strain in the direct fibres is accompanied by half the amount of strain in the relative diagonal fibres. Any disturbance of this relation indicates the presence of another force. Thus tensile forces applied at *right angles* to compressive forces of equal amount, produce no strain in the diagonals. But if forces applied at right angles to each other are both tensile, or both compressive, the strain in the diagonal is as great as that in the direct fibres. It is also pointed out that in a given fibre *a*, *b*, *c*, the point may be moved with regard to *a* and *c*, thus producing plus and minus strains in the same fibre. Treating a solid as being made up of a series of laminæ, and showing that every change of figure can be represented by the variation in length of the diagonals, taken in connection with those of the direct fibres, the author proceeds to trace the effects of the application of tensile and compressive forces acting longitudinally on either side of the neutral plane, and shows that curvature is the result of the relation between the strains in direct fibres and those in the diagonals. The operation of a single tensile force applied along one side of the plate and a transverse stress are likewise traced out, and the conditions of "*elastic equilibrium*" referred to. The amount of resistance offered by the diagonal fibres is shown as follows:—



a, *b*, *c*, *d* represents a portion of a beam strained by transverse forces into the circular curve *a*, *e*. Two resistances arise. 1. That due to the extension and compression of the longitudinal fibres produced by the rotation of *b*, *d* about the neutral axis, which is the resistance considered in the theory of Leibnitz. 2. That due to the extension and compression of the diagonal fibres, caused by the deformation of the square *a*, *b*, *c*, *d* into the figure *a*, *h*, *o*, *c*, which is the resistance of flexure. It is then shown that in a solid rectangular beam, the second resistance is equal to the first, and that both resistances act independently, and consequently that the true theoretic resistance of a solid rectangular beam is exactly twice that arrived at by the theory of Leibnitz. The strength so computed is in general accordance with the results of experiments in cast iron, wrought iron, steel, and other materials, the maximum strength being found in cast iron, which is one-eighth above, and the minimum in glass, which is one-fourth below the calculated strength. The author considers this treatment of the subject as arising necessarily out of Dr. Hook's law "ut tensio sic vis," and that it is in effect completing the application of those principles which are only partially applied by Leibnitz. The paper concludes with some practical illustrations (accompanied by photographs) of the effect of diagonal action. The appendix contains the results of experiments on the tensile, compressive, and transverse resistances of steel.

"On some Elementary Principles in Animal Mechanics.—No. IV. On the difference between a Hand and a Foot, as shown by their Flexor Tendons." By the Rev. Samuel Haughton, M.D. Dubl., D.C.L. Oxon, Fellow of Trinity College, Dublin.

The fore feet of vertebrate animals are often used merely as organs of locomotion, like the hind feet; and in the higher mammals they are more or less "cephalised," or appropriated as hands to the use of the brain. The proper use of a hand when thus specialised in its action, is to grasp objects; while the proper use of a foot is to propel the animal forward by the intervention of the ground. In the case of the hand, the flexor muscles of the fore-arm act upon the finger tendons, in a direction from the muscles towards the tendons, which latter undergo friction at the wrist and other joints of the hand, the force being applied by the muscles to the tendon above the wrist, and the resistance being applied at the extremities of the tendons below the wrist by the object grasped by the hand. From the principle of "Least Action in Nature" we are entitled to assume the strength of each portion of a tendon to be proportional to the force it is required to transmit; and since, in a proper hand, these forces are continually diminished by friction, as we proceed from the muscle to the fingers, we should expect the strength of the tendon above the wrist to be greater than the united strengths of all the finger-tendons. Conversely, in a proper foot, the force is applied by the ground to the extremities of the tendons of the toes, and transmitted to the flexor muscles of the leg, by means of the tendons of the inner ankle, which undergo friction in passing round that and the other joints of the foot. In this case, therefore, we should expect the united strengths of the flexor tendons of the toes to exceed the strength of the flexor tendons above the heel. In the case of the hand, friction acts against the muscles; in the case of the foot, friction aids the muscles. I have measured the relative strengths of the deep flexor tendons of the hand above and below the wrist in several animals, and also the relative strengths of the long flexor tendons of the foot above and below the ankle, in the following manner:—I weighed certain lengths of the tendons above the wrist and ankle, and compared these weights with the weights of equal lengths of the flexor tendons of the fingers or toes, assuming that the weights of equal lengths are proportional to their cross sections, and these again proportional to the strengths of the tendons at the place of section. The difference between the weights above and below the joint represents the sum of all the frictions experienced by the tendons between the two points of section. Tables are given showing the results of measurements, *e.g.*, in the case of the Pyrenean Mastiff the amount of friction is 65·4 per cent., while in the Boomer Kangaroo it is *nil*. The foregoing animals all realise the typical idea of a true foot, with a variable amount of friction at the ankle-joint; this friction disappearing altogether in the Boomer Kangaroo, whose method of progression realises absolute mechanical perfection, as no force whatever is consumed by the friction of the flexor tendons at the heel. The only animals whose feet deviated from the typical foot were three, viz., the Alligator, common Porcupine, and Phalanger. In these animals the foot has the mechanical action of a hand, or grasping organ; and the flexor tendons above the ankle exceeded those below the ankle by the following amounts:—Alligator, 11·5 per cent.; common Porcupine, 20·0; Phalanger, 29·2. In the case of the flexor tendons of the hand results were obtained varying from 71·0 in the case of the Common Porcupine, to *nil* in the case of the Goat. It will be observed that the fore foot of the goat, regarded simply as an organ of locomotion, attains a perfection comparable with that of the hind foot of the kangaroo, no force being lost by friction at the wrist-joint. The only animal in which I found a departure from the typical hand was the Llama, in which the flexor tendons of the fingers exceed the flexor tendon above the wrist by 14·4 per cent. The bearing of the foregoing results on the habits of locomotion of the several animals will suggest themselves at once to naturalists who have carefully studied those habits. I shall merely add that the subject admits of being carried into the details of the separate or combined actions of the several fingers and toes, and that the habits of various kinds of monkeys in the use of certain combinations of fingers or toes may be explained satisfactorily by the minute study of the arrangement and several strengths of the various flexor tendons distributed to the fingers or toes.

PARIS

Academy of Sciences, May 23.—M. Jordan communicated a theorem on doubly periodical functions.—A note by M. L. Cailletet on the compressibility of gas at high pressures, was communicated by M. H. Sainte-Claire Deville.—M. Chapelas presented a note on the aurora borealis of the 20th May, which was observed to move from west to east, and was followed by great atmospheric disturbances coming from the south-west.—M. E. Lagout described a cheap equatorial sundial which he has invented for the purpose of regulating timepieces. It costs from 8 to 12 francs.—M. A. Trécul presented a note on the hailstones which fell at Paris during the storm of the 22nd May. These were pear-shaped, and very large, some measuring more than three-quarters of an inch in length. One of them showed at its larger end the form of an obtuse-angled rhombus.—A note on the clouds, fogs, and rains with sand, observed in Italy in 1869, by M. Zantedeschi, was read.—Several chemical papers were communicated, namely:—A memoir on the action of water upon iron, and of hydrogen upon oxide of iron, by M. H. Sainte-Claire Deville. A report by M. Chevreul on a memoir by M. Vétillart on the vegetable fibres employed in manufactures, describing the distinctive characters of the principal textile materials as evinced by microscopic observation and by treatment with iodine and sulphuric acid. A note by M. Schützenberger on the compounds of protochloride of platinum with oxide of carbon; one by M. M. Prudhomme on the action of acetylene upon acetohypochlorous anhydride (acetate of chlorine); and one by the same author, on the action of sulphuric anhydride upon protochloride and sesquichloride of carbon, all communicated by M. H. Sainte-Claire Deville.—Observations on the constitution of the flame of the fish-tail gas-burner, by M. A. Baudrimont. The author found that the obscure part of the flame possesses heat sufficient to fuse a platinum wire.—A note by M. A. Vezian, on the system of mineral veins of the Hundsrück, was read; and an extract from a letter by M. de Botella, noticing two recent cases of elevation of land in Spain. Upon the latter M. Elie de Beaumont made some remarks.—M. Cl. Bernard presented a contribution to the knowledge of the minute structure of the mammary gland, by MM. G. Giannuzzi and E. Falaschi. M. d'Abbadie suggested the desirableness of a decimal division of the circle and of time; and General Morin presented a note on some earthenware stoves, manufactured by Müller and Co., of Ivry, which, as he stated, utilise no less than 93 per cent. of the heat developed by the coke consumed in them. Several other papers and communications were read of which the titles only are given.

May 30.—A paper by M. E. Combescène on some differential formulæ was presented by M. Hermite; and a note on a formula of analysis, by M. F. Lucas, was communicated by M. Liouville. General Morin communicated a memoir by MM. C. Martins and G. Chancel on the physical phenomena which accompany the rupture of hollow projectiles of various calibres by the congelation of water contained in them. The numbers obtained by the authors are about one half those obtained by General Morin from the formulæ given by him in his lectures on practical mechanics. General Morin, M. Dumas, and M. Elie de Beaumont made some remarks upon this communication.—M. Jamin presented a note by MM. A. Cornu and E. Mercadier, on melodic and harmonic intervals, in reply to a paper by M. Guéroult, read on May 9.—M. Jamin also communicated two notes by M. Trève on electric currents. In one of these the author cited some further observations in support of his assertion that two currents cannot circulate in opposite directions in the same wire or in the same Geissler's tube; in the second he indicated a method of explaining the course of the currents in telegraphy when terrestrial communications are employed without a return wire. He maintained that the soil is to be regarded as a common reservoir rather than as a conductor.—A note by M. J. Mario on the phenomena of electrostatic induction was read. From his experiments he proposed a theory of terrestrial currents, according to which the sun would be a source of positive electricity acting by induction upon the earth.—A note by M. Neyreneuf, on the theory of electrical condensers, was also read.—The following papers on chemical subjects were read:—A note by M. Cloëz, claiming priority in the discovery of the cyanic and cyanuric æthers, presented by M. Cahours.—A note by MM. Gal and Gay-Lussac, also presented by M. Cahours, on some compounds homologous with tartaric and malic acids. These compounds were adipomalic, adipotartaric, suberomalic, and suberotartaric acids; they are obtained by the action of bromine upon adipic and suberic acids.—A note on the action of hydrochloric acid upon osseine, including researches upon determination of the quantity of osseine in fossil bones, by M. Scheurer Kestner, communicated by M. Balard. The author stated that the solubility of a portion of the osseine in bones is independent of the action of hydrochloric acid, which may be reduced to almost nothing by sufficient dilution. He replied to some remarks of M. Elie de Beaumont on his former paper on this subject.—A note, by M. Sacc, on the preparation of pyrotartaric acid. The process proposed by the author consists in disolving anhydrous tartaric acid in commercial acetic acid, and heating the mixture in a retort until it becomes syrupous; in a day or two the vessel is filled with acicular crystals.—A memoir was read on the organisation of silicified branches probably belonging to a *Sphenophyllum*, by M. B. Renault. The author described the structure of a stem, which he identifies with *Sphenophyllum* (naming the species *S. Charmassii*), and from the characters displayed by which he is led to remove *Sphenophyllum* from the Equisetaceæ.—M. Blanchard presented a memoir by M. Lacaze Duthiers containing his researches upon the evolution of *Molgula tubulosa.* The author stated that the larva of this species, instead of the tadpole-like form supposed to be common to the larvæ of the Ascidia, is an amœboid body which, as it were, flows out of the ruptured egg.—M. A. Dumèril presented a note, by M. G. Pouchet, on some monstrous gold fish (*Cyprinus auratus*) from China. This note related to the well-known doubling of the tail in these fish, which, according to the author, always occurs below the extremity of the vertebral column.—M. de Quatrefages presented a note, by M. Bordone, on the organisms which are developed in the silkworms attacked by the disease called *Morts flats.* M. Dumas made some remarks upon this paper.—M. Decaisne communicated a note, by M. J. E. Planchon, on the Phthiriosis of the vine known to the ancients, and on the Coccidæ of the vine of modern times. The cause of the vine-disease known to the Greeks as Phthiriosis is said by M. Koressios to be the *Phylloxera;* the author stated that it was a coccid, but not, as supposed by Walckenaer, the *Coccus Vitis.* (Linn.) He identified it with *Dactylopius longispinus*, of Targioni Tozzetti.—M. A Dumèril presented a note by M. E. Moreau, on the cranial region of *Amphioxus*, forming a continuation of his paper on the structure of the dorsal chord in that fish.—A note by M. N. Gréhant, on the rapidity of the absorption of oxide of carbon by the lungs, was commnuicated by M. Cl. Bernard. The author stated that the absorption of oxide of carbon into the blood commences immediately, and advances rapidly. The blood of a dog breathing air containing one-tenth of oxide of carbon, furnished a gaseous mixture containing 4·28 per cent. of oxide of carbon, in less than 25 seconds, and 18·41 per cent. in less than $1\frac{1}{2}$ minute.—M. C. Robin presented a note by M. J. Chéron, on the state of muscular contractibility, judged comparatively by means of continuous currents, and of currents of induction, in a certain number of paralyses.—M. C. Sainte-Claire Deville presented the first volume of the "Bulletin of the Meteorological Observatory of Montsouris," and made some remarks upon its contents.—Numerous other papers and memoirs were communicated, of which we have only the titles.

BERLIN

German Chemical Society, May 23.—M. Ador has obtained the radical of phthalic acid by treating its chloride with silver. Three molecules thus unite into one. The new body yields three different acids by oxidation, the last of which is phthalic acid.—A. Baeyer, in his own and in M. Emmerling's name, reported on the transformation of isatine into indigo. As isatine, when treated with nascent hydrogen, unites with it and forms indol, a substance not capable of uniting with the reduced substance was sought for, and discovered in phosphorus, the solvent employed being chloride of acetyle or of phosphorus. Real indigo-blue and indigo-red were thus produced. The latter stands in the same relation to the blue as purpurine does to alizarine. To complete the long hoped-for discovery of producing artificial indigo, all that remains to be done is now to transform indol into isatine.—Prof. Cannizzaro, of Palermo, sent in a paper on isomeric cyanuric ethers. Solid chloride of cyanogen acting on benzylic alcohol produces an ether isomeric with one formerly described, and belonging to the series lately investigated by Hofmann.—Messrs. Gomp-Besanez and Grinva have produced artificially the essential oil of rue by distilling together caprate of sodium $C_{10}H_{19}NaO_2$, and acetate of sodium. The acetonic nature and the formula $C_{11}H_{22}O$ (hitherto doubtful) of the oil are thereby fully established.—C. Rammelsberg, in a paper on meteorites, remarked on the absence of alkalis in these celestial rocks. The "Shalkah" stone found in India has lately been analysed, and consists of 22 per cent. of olivine and 88 per cent. of bronzit. He found that sulphuric acid only attacks the former mineral and leaves the latter untouched.—Messrs. Naumann and Vogt report on what Wurtz considered as a combination of chloride of cyanogen with hydrocyanic acid, but which according to them is a mixture.—A. Claus has studied the action of bromine on dichlorhydrine, and that of chloride of sulphur on aniline. The latter reaction has not as yet produced pure substances.—L. Henry by treating with nitric acid, glycol, lactic, and malic acids, has replaced the alcoholic group O H by the group NO_2.—A. W. Hofmann reported on the danger of preparing chloride of cyanogen by the action of chlorine on cyanide of mercury. Sometimes explosions take place during this preparation, particularly at the end of it, when the salt remaining behind and acted upon is a double salt of the chloride and the cyanide of mercury.—Messrs. Baeyer and Martins recommended the action of chlorine on hydrocyanic acid, and that of chloride of lime on cyanide of potassium for producing chloride of cyanogen.

DIARY

THURSDAY, JUNE 9.

ZOOLOGICAL SOCIETY, at 8.30.
MATHEMATICAL SOCIETY, at 8.
ROYAL INSTITUTION, at 3.—Electricity : Prof. Tyndall.

FRIDAY, JUNE 10.

ROYAL INSTITUTION, at 8.—The Ammonia Compounds of Platinum : Prof. Odling.
ROYAL ASTRONOMICAL SOCIETY, at 8.
QUEKETT MICROSCOPICAL SOCIETY, at 8.

SATURDAY, JUNE 11.

ROYAL INSTITUTION, at 3.—Comets : Prof. Grant.

MONDAY, JUNE 13.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

TUESDAY, JUNE 14.

ANTHROPOLOGICAL SOCIETY, at 8.—On the Kelts of Ireland : Dr. John Beddoe.—On the Irish Celts : Dr. H. Hudson.—Notes on the Race Elements of the Irish People ; G. H. Kinahan.
PHOTOGRAPHIC SOCIETY, at 8.

WEDNESDAY, JUNE 15.

METEOROLOGICAL SOCIETY, at 7.—Anniversary Meeting.

THURSDAY, JUNE 16.

ROYAL INSTITUTION, at 8.30.
ROYAL SOCIETY OF ANTIQUARIES, at 8.30.
LINNEAN SOCIETY, at 8.
CHEMICAL SOCIETY, at 8.
NUMISMATIC SOCIETY, at 7.—Anniversary Meeting.

BOOKS RECEIVED

ENGLISH.—Atlas and Handbook of Physical Geography : Keith Johnston, jun. (Johnston and Co.).—The Story of Aristæus and his Bees : R. M. Millington (Longmans).—Selections for Latin Prose : R. M. Millington (Longmans).

FOREIGN.—Lecons d'Optique physique : E. Verdet. Masson, Paris.—(Through Williams and Norgate).—Traité de Paléontologie Végétale, et Atlas. Tom ii. pt. 1re. : W. P. Schimper.—Beiträge zur Morphologie und Physiologie der Pilze : A de Bary and Woromin.

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ERRATA.—In No. 31, p. 95, second column, line 52 from top, for "fires" read "fuses;" lines 58 and 61 (*bis*), for "carbonate" read "carbamate."

THURSDAY, JUNE 16, 1870

THE SCIENTIFIC EDUCATION OF WOMEN

THE feature which will probably most clearly mark the year 1869 in the view of the future historian of Education, will be the definite recognition of the rights of woman to all the advantages of education accorded to men. The advance of public opinion within the last few years on all subjects relating to the legal, social, and educational position of woman, has indeed been so rapid, that the man whose words were only quite recently listened to by his friends with a condescending smile of pity, is now scarcely in advance of his times. As it is generally believed that the movement has yet far from reached its full development, and the course in which it has been so far directed having been in the main sound and excellent, we would still wish to suggest to its promoters whether the curriculum of subjects taught under the auspices of the various associations may not be somewhat widened by a more liberal infusion of the scientific element. The ability of women to appreciate instruction by the highest teachers of Natural Science has as yet hardly been tested. The high position occupied by a few women like Miss Martineau and Mrs. Somerville as writers on Political and Natural Science cannot be taken to prove the capacity of the whole sex; but we think that so far as opportunity has yet been offered, the evidence is entirely favourable. The programmes of the Lectures to Women on Physiography, Physics, and Botany, recently delivered at the South Kensington Museum by Professors Huxley, Guthrie, and Oliver, show at least no want of confidence in the capacity of their pupils. The first of these courses had already been given substantially to a mixed class of boys and girls at the London Institution, and in the results of the examination of that class, the girls had decidedly the advantage over the boys. In most of the large towns of Great Britain courses of lectures to ladies have now been delivered during the last two or three years by eminent professors of the various branches of literature; in many of these rigorous examinations have been held at the close of the courses; and where this has been done, there is but one expression of opinion as to the quality of the work executed. At London, Edinburgh, Manchester, Liverpool; in English literature, mathematics, experimental physics, mental philosophy, the testimony is uniform, that not only can women compete with men in the qualities essential for severe and successful study, but that in many respects their average attainments are higher than among the working members of a University. A careful examination of the reports of the various educational associations convinces us that this statement is decidedly within the mark. Among so many testimonies to the same effect, it seems almost invidious to pick out one; but we cannot forbear quoting from Professor Fraser's report of his class of logic and mental philosophy at Edinburgh:—"Sixty-five students enrolled. Forty-eight of these shared more or less in the examinations and essays of the class. I found, as the session advanced, that I had at the outset underrated the mental power and persistency of as able and zealous a set of students as I have ever had the good fortune to conduct. . . In the examination the average of marks gained was about 55 per cent.; one-twelfth of the class gained more than 80 per cent. of the marks, and only one had less than 25 per cent."

The Edinburgh Association stands out from most of its kindred societies in being formed on a decidedly more academical basis. The courses are longer, averaging about forty lectures each, and, consequently, deeper and more thorough: the teachers are all university professors, and the aim of the Association is, as it were, to form a distinct Faculty of the University. So far as we can judge, the success of the Association has justified the views of its founders. Many advantages no doubt result from immediate connection with a great centre of learning like the University of Edinburgh, a connection which has hitherto been denied to female education. We are inclined to think that the "College for Women" may have made a mistake in establishing itself in a locality "midway between London and Cambridge." The College will not share in the life of the University; the Cambridge professors will not feel the Hitchin College a portion of their own system, unless the College is locally associated with the University.

We have already alluded to the comparative absence of Natural Science from the programmes of the Ladies' Educational Associations; this is not so strikingly the case as it was last year. The London Association is making arrangements for some scientific classes next session; at Edinburgh Professor Balfour is trying the experiment of a class of botany; classes for zoology and geology are included in the Manchester curriculum for 1870-71; as well as one on logic by Professor Jevons; Natural Science has a place both in the entrance examination for the Hitchin College and in the College course, though it has not yet been taught; while chemical classes have already been conducted in several localities by Professors Williamson, Roscoe, and others, with marked success. We notice with great pleasure the movement at Cambridge for the instruction of women to which we have referred elsewhere. Here a wide field is opening for the future, and one which it will surprise us if women do not make especially their own. In the training of boys we have recently awoke to the discovery that a complete education implies something more than an intimate acquaintance with two dead languages. There is no danger that we shall ever underrate the value of a critical acquaintance with Latin and Greek, as requiring a mental training which no other studies can give; but while a classical education imparts the highest culture possible to the intuitive faculties, it scarcely brings into play the powers of observation. Now, it is in these very powers of perception, as distinct from conception, which the Natural Sciences cultivate, that woman has naturally the advantage over man; and we may therefore *a priori* conclude that their study will be specially within the range of her powers. Another consideration is also worthy of notice by those who are looking for "new careers for women." At a time when we are beginning to recognise the importance of a scientific training as an essential portion of a liberal education, we find that our teaching powers fail us. The number of really competent teachers of science has by no means kept pace with the extension of a desire for instruction; the leading men in every branch are overwhelmed with work; and the younger men to whom they can with confidence entrust a portion of their labours are by no means sufficiently numerous. It is thus not women only, but men, the whole human race, that is stunted in its intellectual development at a time when its growth should be the most rapid, by the practical restriction to one half of the race only, of the means of acquiring the ability to help in this development.

We must next touch upon a subject of great delicacy; we refer to the instruction of women in medicine and surgery. There is an important distinction between this and all other departments of science. While it is competent to any one to teach chemistry, geology, or botany, and his success as a teacher will depend on his competency, the teachers and practisers of medicine and surgery form a guild, a professional trades' union, protected and licensed by the Government. It is in the nature of guilds and monopolies to be exclusive; and when we find that the medical profession is united almost as one man (with a few honourable exceptions) to resist the admission of women into its ranks, it is only what might with confidence have been predicted. The instinct of self-defence is a strong one; and if any evidence is required of the extent to which self-interest has entered into the causes of the opposition by the profession to the medical training of women, we need only refer to the "seven reasons against the admission of ladies to the profession" given in the *British Medical Journal* for May 7th. Into the abstract question of the utility of monopolies we need not enter; those who are excluded from their benefits are perfectly justified in using every legitimate effort to overthrow them, and in claiming the assistance of those who believe in the universal adaptation of the principles of free trade. Seldom have greater persistence and self-denial been shown than by those few women who have laboured long and hard in this country, America, and France, in attempting to open to their sisters the doors of the medical profession. Careless of cruel misrepresentation, of public slander, of private persecution, they have held nobly on their course, and their services to mankind will one day be recognised.

Few have yet realised the enormous gain that will accrue to society from the scientific education of our women. If, as we are constantly being told, the "sphere of woman" is at home, what duty can be more clearly incumbent upon us than that of giving her the opportunity of acquiring a knowledge of the laws which ought to guide her in the rule of her house? Every woman on whom the management of a household devolves may profit by such knowledge. If the laws of health were better known, how much illness and sorrow might be averted! What insight would a knowledge of chemistry afford into the wholesomeness or unwholesomeness of different articles of food! What added zest would be given to a country walk with the children, or a month by the seaside, if the mother were able to teach the little ones intelligently to observe and revere the laws of Nature! Above all, what untold sufferings, what wasted lives, are the penalty we have paid for the prudish ignorance of the physiology of their bodily frame in which we have kept our daughters! These considerations have had far too little place with us at present.

We trust that a new era is dawning upon us; may the higher education of women be pursued in the admirable spirit of the last report of the Edinburgh Ladies' Educational Association:—"So far as we can see, cultivation does for women what it does for men—intensifies every moral attribute in proportion to the mental growth. Those who must go out into the world go out with a truer courage, founded upon a nobler estimate of work; those whose duties lie within the circle of home find them invested with a new and vivid significance from the higher elevation, and consequently larger views, of their own minds; and, finally, as 'woman is not undeveloped man,' we believe that womanhood can only be made more truly womanly, as manhood is made more truly manly, by the utmost use of the possibilities of high cultivation."

NATURAL HISTORY COLLECTIONS

ALLOW me to give in my adhesion to the "platform" established by the signers of the Memorial concerning the Natural History Collections, reprinted in your last number, and at the same time to request you to reprint a second Memorial on the same subject, presented in 1866 to the then Chancellor of the Exchequer. You will observe that this Memorial has likewise been signed by many distinguished men of science.

P. L. SCLATER

Copy of a Memorial presented to the Right Hon. the Chancellor of the Exchequer

SIR,—It having been stated that the scientific men of the Metropolis are, as a body, entirely opposed to the removal of the Natural History Collections from their present situation in the British Museum, we, the undersigned Fellows of the Royal, Linnean, Geological, and Zoological Societies of London, beg leave to offer to you the following expression of our opinion upon the subject.

We are of opinion that it is of fundamental importance to the progress of the Natural Sciences in this country, that the administration of the National Natural History Collections should be separated from that of the Library and Art Collections, and placed under one officer, who should be immediately responsible to one of the Queen's Ministers.

We regard the exact locality of the National Museum of Natural History as a question of comparatively minor importance, provided that it be conveniently accessible and within the Metropolitan district.

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London, May 14, 1866

FOSSIL MAMMALS IN NORTH AMERICA

The Extinct Mammalian Fauna of Dakota and Nebraska; together with a Synopsis of the Mammalian Remains of North America. By Dr. Leidy. With an Introduction on the Geology of the Tertiary Formations of Dakota and Nebraska; with a map. By Dr. Hayden. (Philadelphia, 1869.)

DR. LEIDY'S new work on the extinct mammalia and fauna of Dakota and Nebraska, to which is appended a Synopsis of the Mammalian Remains of North America, fills a wide gap in Palæontology. It occupies the whole of the seventh volume of the Journal of the Academy of Natural Sciences of Philadelphia, and is accompanied by a preface on the geology of the Tertiary Strata of Dakota and Nebraska, by Dr. Hayden. Altogether it forms the most important contribution to our knowledge of fossil mammals which has been made since Prof. Gaudry published his famous "Animaux Fossiles et Geologie de l'Attique." These two books, indeed, stand in close relation to one another, for in the one the chief interest centres in the Miocene fauna, which is the subject matter of the other. I propose to give an outline of Dr. Leidy's work, and to show the relation which the American Mammalia bore to those of Europe, from the Miocene down to the "Quaternary," or Post-glacial epoch.

At the close of the Cretaceous period, writes Prof. Hayden, the ocean which had "rolled uninterruptedly across the area now occupied by the Rocky Mountains" began to grow shallow, until at last a long barrier of land gradually rose above the waves, and separated the Atlantic from the Pacific. This elevatory movement culminated in the Rocky Mountain range in the United States, and probably has been going on from the Cretaceous period down to the present day. In the early Tertiary epoch enormous lakes occupied the basin of the Mississippi. The "great lignite basin," for instance, extends far southward, possibly even to California, westward far over the mountains to Utah, and possibly to the Pacific, and northward probably to the Arctic Sea, interrupted here and there by the upheaval of mountain ranges." The strata which testify to the former existence of this great lake, consist of layers of clay and sand, and numerous beds of lignite, varying in thickness from a few inches to twelve or fifteen feet. In its lower portion an oyster is the characteristic fossil, which by its stunted growth implies a change from salt to brackish water, while in the rest of the formation there are freshwater shells of the genera Melania and Corbicula. "The occurrence of immense fan palms, and many other plants now growing only in tropical climates, points directly to the conclusion that along the shores of this great lake there grew most luxuriant forests, equalled only by those now existing in Central America or Brazil." The date of this lignite formation is possibly Eocene, and certainly pre-Miocene. Some of these lakes continued to exist as late as the Pliocene epoch.

The "White River group" of rocks consists of white indurated clays, sandstones, and conglomerate marls and sands, upwards of a thousand feet thick, which occupy an area of at least 100,000 square miles on the Eastern flank of the Rocky Mountains. It is purely a fresh-water formation. In Nebraska it is eroded by pluvial and torrential action into quaint pinnacles and fantastic shapes of every sort, and into deep barren ravines that recall to mind Dr. Falconer's description of some parts of the Sevalik Hills. From the difficulty of traversing the 5,000 square miles which are cut up in this way the district is known to the Canadian voyagers as "Mauvaises Terres," and to the Indian hunters as the "Bad Grounds." It has furnished the larger portion of the Mammalia described by Dr. Leidy, and is unequivocally of Miocene Age. The Loup River Strata resting on the White River group, consist of sand and a few layers of limestone, and contain the remains of land and fresh water Testacea and Mammalia. In Nebraska, the sand is so incoherent that it forms a series of ever shifting dunes, which occupy an area of not less than 20,000 square miles. The remains of Elephant and Mastodon and others show that it belongs to the Pliocene, irrespective of the stratigraphical evidence. The Post-tertiary deposits are represented in the same region by a yellow siliceous marl, most fully developed along the Missouri River, and in the valley of the Plate. It is from three to five hundred feet in thickness, and contains well-known Post-glacial fossils.

In treating of the Mammalia yielded by these different formations, Dr. Leidy has followed the example of Prof. Agassiz in the needless multiplication of species. Naturalists fall into two classes, according to the ideas which they bring to bear on their work: the one fix their attention on the variability and points of resemblance manifested by suites of organic remains, and stretch the name of species as far as it will go; the other give specific value to minute differences of size and form, which, in a larger series of specimens, either recent or fossil, would be found valueless in classification. In this work Dr. Leidy has joined himself to the latter class, and has marked every small variation by a specific, and, in some cases, even a generic name, and by so doing has added, without any necessity, to the heavy burden of synonyms which scientific literature has to put up with. He believes that the American is a "peculiar fauna," and, that even if no difference between European and American fossils can be detected, their geographical separation is evidence that they belong to different species. For example, in the description of a new species, *Equus excelsus*, he admits that "it is not improbable that part of the specimens looked upon as fossils may be the remains of the Mustang, or recent wild horse of our western wilderness." Nevertheless, he holds to his own undefined specific name. This fault is carried to an extreme, in the case of the extinct family Anchitheriidæ. On the small foundation of one tooth, which, "in general proportions and construction," and "in size," is "nearly the same as the teeth of *Anchitherium Aurelianense*," a new genus, Anchippus, is based; while four milk molars, which "in form, mode of insertion, and general constitution and size, bear a near resemblance to those of the same species," constitute the basis of the second new genus Parahippus. This mode of treatment runs more or less throughout the work, and renders it of less value than might have been expected from the importance of the subject-matter. But, nevertheless, it is a mine of information to which Tertiary naturalists will resort for many long years to come.

I will now pass on to the consideration of the leading features of the Miocene fauna. On the borders of the ancient Miocene lake, from which the Mauvaises Terres were deposited, lived a most remarkable group of herbivores, all of which were, roughly speaking, less specialised than any now on the face of the earth. The Oreodon, a ruminant about the size of a large domestic sheep, was there in considerable abundance, and must have lived in herds, after the manner of the bison in the neighbouring region. It was an animal of a strangely composite kind; to the molar teeth of a ruminant it added the ulna and radius of a hog; it possessed a cranial and temporal region like that of the camel, and larmiers beneath the orbit as in the Cervidæ and the musk-sheep. Its canines were trilateral and worn like those of a pig, and its dental armature was complete all round. Three closely-allied forms, the Merychochœrus, Leptauchenia, and Agriochœrus, are associated with Oreodon, and form a group which, judged by existing forms of life, stands half-way between the pigs and the ruminants. It is an admirable instance of one of Prof. Huxley's "intercalary types." The camel or lama was represented by two allied forms, the Pœbrothere and Protomeryx, and the musk-deer by the Leptomeryx. The Artiodactyle division was present in very strong force. The Elotherium is allied to the hog, peccary, and hippopotamus. In a full complement of teeth it possessed a canine almost carnivorous in character. It was probably less omnivorous than any of the class now living. The Perchœrus, Leptochœrus, and Nanohyus, form members of the same class, together with the Hyopotamus and the Titanothere, which was possessed of a well-developed and separate ulna and radius. The remains of rhinoceros indicate one, and perhaps two forms, during the American Miocenes. The lowness of the crowns of their teeth, the large development of the incisors, and the absence of any trace of horn-basis on the skulls which have been preserved, imply that they belong to the hornless section Aceratherium, of Dr. Kaup, rather than to the true rhinoceros. Dr. Leidy has very rightly separated Hyracodon from the true rhinoceros, because it has the full complement of teeth in both jaws. The Anchithere represented the horse in this fauna; the Palœolagus the hare; and there were also squirrels, beavers, mice, and hedgehogs present.

There was also a corresponding development of the Carnivora. Two species of Amphicyon performed the function of the living foxes and wolves; the Hyænodon that of the hyæna; while the great Machairodus, and the allied form Dinictis, represented the lions, tigers, and other larger felines.

If this fauna be compared with that of the European Miocenes, several important differences and resemblances may be remarked. The whole group of antelopes, found in such numbers in the classic plains of Pikermi by M. Gaudré, and in central France, are absent. The giraffe also, and the family of the Cervidæ, and the horse, elephant, Mastodon and Hipparion of Europe, are equally absent. Other genera are common to both Europe and America. The *Elotherium Mortoni* of the Mauvaises Terres can scarcely be distinguished from the *E. Aymardi* of the Gironde. The *Anchitherium Bairdi* cannot be distinguished with certainty from the *A. Aurelianense* of France. The rhinoceros and Aceratherium of Eppelsheim and Pikermi find their analogues in the so-called *Rhinoceros occidentalis* and *R. meridianus* of America, while the Hyracodon recalls forcibly to mind the small rhinoceros from Sansan (*Aceratherium Sansanense*). The Lophiodon is also an American form. Of the Carnivores, the *Amphicyon vetus* is the equivalent of the *A. major* of De Blainville from Sansan, while the Hyænodon and the sabre-toothed Machairodus were the scourge of the Miocene herbivores in America as in Europe. The family of the Oreodontidæ, on the other hand, seem peculiar to America, as also the Titanothere and the small carnivore the Dinictis.

This distribution of life throws considerable light on the physical geography of the northern hemisphere during the Miocene period. The absence of the South American forms which were living at the time, the apes, the rodents, and the edentata, implies the presence of a barrier between North and South America, which prevented migration from the one to the other; and this barrier was most probably, as Prof. Huxley remarked in his last address to the Geological Society, an open sea. The forms of life common to Europe and North America imply a continuity of land between those now widely dissociated areas. Mr. Murray believes in the existence of a Miocene Atlantis, which has left the Sarghasso sea as a palpable monument of its existence in the mid-ocean. I should, however, be rather inclined to look for the continuity of land in the direction of Siberia, Behring's Straits, and, it may be, Greenland; and when the recent wonderful discoveries of temperate and sub-tropical vegetation in the now Arctic regions is taken into account, it appears to me extremely probable that the animals migrated from one area to the other by that pathway. But whether this be accepted or not, Prof. Heer has shown that during the Miocene times there was a vast extent of land, and a temperate climate in the now extremely high northern latitudes, which would imply conditions of life favourable for the migration of the Miocene animals. It is impossible to find out with any certainty the direction which the Miocene migration took, whether from America towards Europe and Asia, or *vice versa*. There is, however, one very significant fact to be observed, that the American Miocene fauna is less specialised than the European, or, in other words, that it is of an older type. It contains no true hyænas, nor deer, nor antelopes, nor any of the living genera which first appeared in the Miocenes of Europe. Possibly in point of time, or rather in homotaxy, it was older and more closely allied to the Eocene. The explanation which strongly suggests itself to my mind is that the migration set in from the old world, and that the above-named living genera sprang into being here, and are not found in the American Miocenes, because they had not time to reach that area. Thus the absence of certain extinct genera, such as mastodon and hipparion, may be accounted for. That eventually they found their way thither will be seen in the succeeding part of this essay relating to the Pliocene.

W. BOYD DAWKINS

NAUMANN ON THERMO-CHEMISTRY

Grundriss der Thermochemie. By S. Alex. Naumann. 8vo. pp. viii. and 150; price 3*s*. (Brunswick, 1869. London: Williams and Norgate.)

IT is not altogether without reason that modern chemists are accustomed to point, as a proof of their activity, to the amount of materials they have succeeded in accumulating. The fact, indeed, is sufficiently familiar to most students.

If any one will examine a popular chemical manual, he will find its pages occupied almost wholly with experimental results, connected by an imperfect and partial classification. Should he feel envious to know what laws have been acquired by the science, or how far it at present possesses a deductive form, the whole of the information he seeks is generally proffered in a few paragraphs. Nor can it be denied that, as a rule, the manuals have given a fair representation of the kind of chemistry that we are compelled to use for the purposes of teaching. But not only has this state of things been repeatedly predicted; the result has also been discerned. The most distinguished chemists have from time to time seen the fundamental identity that exists between their own science and that of physics, and have recommended the conjoint study of these subjects as likely to prove of the greatest advantage to each, until the apparent and illusive difference between them shall have vanished, and their separate efforts be blended in a single enterprise. In recent times, the special character formerly assigned to chemistry is advocated only by the few who are content with the prevailing style of research. Every one will, therefore appreciate the eager interest that has always been shown in any attempt to construct a firm and logical union between chemistry and physics. Such an union is now being accomplished by the science of Thermo-chemistry.

If we look back to the beginning of this century, we shall see how little reason we had to expect that the desired result would be brought about in such a manner. At that time, there were but few who did not regard heat as, in some way or other, a kind of matter, and not many who deemed its study of much importance. Lavoisier, it is true, touched the obscure topic with his restless fingers; but while we cannot withhold our respect from the inventor of the calorimeter, few will excuse the great opponent of phlogiston for his theory of caloric. Thénard, on the other hand, was the first to show, in his classification of the metals, the great importance to chemistry of the study of temperature. His principles and the fruit of his teaching can easily be traced in modern chemistry. But it was not in the study of pure thermotics that thermo-chemistry took its rise. That science could not furnish, what the weakness of the mind invariably demands, a conception on which to proceed. Heat might be a mode of motion. But motion of *what?* The passage in which Rumford announced his discovery is sublime in its simplicity and unsullied by any materialistic taint. The popular prejudice stripped that magnificent idea of its regal investiture, and clothed it in the garments of a corpuscular theory. Dalton's atomic doctrine, at first received with coldness, has long been almost universally accepted among chemists; but it is only, perhaps, within the last decennium (for Mayer and Joule must be omitted) that a few prominent physicists, among whom Naumann himself is to be enumerated, have given in their adhesion to Dalton's fundamental views, and constructed for themselves a new basis on which to work. Thus, then, has arisen the science of thermo-chemistry; and it is not, consequently, very surprising that it should teach us that heat is a kind of undulatory movement (*Bewegungsform*) of molecules or atoms. On the whole, we are inclined to regard the atomic constituent of the infant science as "accidental" (to borrow a term from mineralogy); in anything equal to a calculus, it will prove to be an intolerable hindrance to perspicuity, infecting the purely inductive part with a host of extraneous entanglements.

Professor Naumann is far too acute and experienced a reasoner not to perceive that it is precisely at this point that objections are most likely to be made. Accordingly he is careful to fortify his position by a well-digested chapter on the atomic theory, in which the usual arguments are advanced with much more than the usual thought and distinctness. It may aid the reader to form a judgment of how far his author has succeeded, if we notice one or two points in the discussion. Almost at the outset, we are warned that demands on the accuracy of a scientific theorist must not be severe or stern. "The essential value of a theory does not consist so much in its fundamental hypotheses, but in the connecting of known facts and enabling us to discover new relations." Further on there is sufficient confusion in the terms employed to mislead one into supposing that an element enters into combination with unaltered properties; whereas, of course, it must always gain or lose by that process. The existence of bodies having the same per-centage composition, but different reactions, is adduced to prove that the matter in them must be divided into discrete parts in each case. Does it not rather prove that matter has nothing whatever to do with chemical properties? Again, the passage beginning "If we now suppose the process of mechanical division to be carried on continuously, a limit must at length appear," &c., is as clear a case of *petitio principii* as we remember to have met with.

The work of Professor Naumann is intended as a sort of summary, in a form specially designed for the student, of what the science of thermo-chemistry has been able to achieve. The accomplished author, who has taken a most important practical share in the results he describes, has spared no pains to perfect his labour with such an end in view. We need hardly say that an effort, in itself so desirable and meritorious, is in its result both opportune and intelligible. We may as well, perhaps, point out that the chapters on Dissociation and kindred subjects have received, as was natural, the greatest amount of development.

The progress of biology has repeatedly been opposed by an obstacle which, under the name of "mind," it has scarcely known how to treat, but with respect to which it is just beginning to find its true position. In like manner, physical science, and chemistry particularly, has had to encounter a phenomenon which, under the name of "matter," has continually impressed upon it the heaviest and most severe of theoretical burdens. It will be strange, indeed, if biology should steal a march on physics.

E. J. MILLS

OUR BOOK SHELF

The Interior of the Earth. By H. P. Malet, F.L.C.S. 8vo, pp. 175. (London: Hodder and Stoughton, 1870.)

THIS is a very good example of a book which ought never to have been written. The author tells us in the introduction that, in approaching the most complicated problem of the condition of the interior of the earth, he has "culled from contemporary literature such extracts as fit" his subject, and that "his intention is to offer a new interpretation of observed phenomena." The "contemporary literature" which has afforded the materials for the "new interpretation" consists of Page's "Geological Manual," Phillip's "Vesuvius," and very many extracts from the current weekly and quarterly periodicals for the last year. With the exception of an allusion to M. Daubrée, obtained from an appendix to Professor Haughton's "Manual of Geology," the author shows no acquaintance with any of the French or German authorities who have worked so hard at his subject. He is, moreover, in ignorance of the labours of Sorby, and of the translation of Bischoff, published by the Cavendish Society. Had he read Lyell's "Principles" he would certainly not have been betrayed into writing such a strange book as the "Interior of the Earth."

We will give one sample of the quality of "the new interpretation." "Page tells us 'that the interstratified trap-tuffs, the basaltic outbursts, and the numerous faults and fissures testify to a period of intense igneous activity.' *We have a familiar example of this action in our hot-beds and our hay-ricks.*" Mr. Malet then proceeds to explain the earth's heat and volcanic phenomena by a like action on buried vegetable matter. To combat views such as these would be absurd. Their author has succeeded in gaining a place among geologists similar to that of the circle-squarer among mathematicians. Z.

Sitzungsberichte der Naturwissenschaftlichen Gesellschaft Isis in Dresden. (Jahrgang, 1869. Nos. 10—12.)

THIS part of the report of the Isis Natural History Society of Dresden contains as usual a great number of interesting notices in all departments of natural history, the most important being on botanical subjects, namely, the commencement of a prodromus of the Lichens of Saxony, Thuringia, and Northern Bohemia, by Dr. L. Rabenherst, and the conclusion of a synopsis of the Coniferæ, by M. Laessig. In the latter, diagnoses of the families and genera are given, and the general characters and geographical distribution of the species are indicated. In the continuation of a lecture on extinct mammalia, Dr. Günther noticed the following species:—*Canis familiaris fossilis*, *C. spelæus* (=*C. lupus*), *C. vulpes fossilis*, *Hyæna spelæa*, *Felis spelæa*, *F. antiqua*, Cuv., *F. minuta fossilis*, Wagn., *F. aphanista*, and *ogygia*, Kaup, and some species of *Mustela*. In a paper on recent explorations for rock-salt in Prussia, M. Otto noticed the occurrence of a vast bed of that mineral near Sperenberg, where a boring has been carried down 2,270 feet, of which 1,920 feet is through a salt bed. The boring has probably nearly reached the bottom of the deposit, as the material brought up now contains much anhydrite. Near Segeberg, in Holstein, a deposit of salt has been met with at a depth of 400 feet.

Milne-Edwards. Leçons sur la Physiologie et l'Anatomie comparée. Tome ix. 2me partie. (Paris: Masson et fils.)

WE gladly welcome another instalment of the long and great work, the "beautiful legacy," as Bernard has called it, of the venerable French naturalist. At a time when the "differentiation" of study is carried to such an extent that many physiologists know very little about other animals than frogs, rabbits, dogs, and men, and many zoologists have a very meagre acquaintance with the results of experimental physiology, such a work as this, which skilfully weaves together all the main facts of animal biology, is most wholesome reading. The present part continues the discussion of the Reproductive Organs of the Invertebrata, and contains two lectures on the Development of the Embryo. We trust the distinguished author may be spared still to preside over and finally to witness the conclusion of this great work.

M. F.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

The Apparent Size of the Moon

DR. INGLEBY is curious to know what Prof. Helmholtz would say on this vexed question. If Dr. Ingleby will turn to page 630 of the "Physiologische Optik," he will find that Prof. Helmholtz has anticipated his wishes. As others of your readers may be interested in seeing how the matter is treated by one who is *facile princeps* in this department, I subjoin a translation of the passage. If the curious experiment mentioned by Dr. Ingleby had referred only to the vertical diameter of the disc, it would have seemed to be another illustration of our inveterate tendency to ascribe an exaggerated value to vertical lines or angles, at or near the horizon. It is said that if ten men be required to fix off-hand on a star half way between the zenith and horizon, nine, at least, will choose one very much too low. If an exact square be cut out in paper and pinned against the wall opposite to the eye, the sides will appear longer than the top or bottom. If an equilateral triangle be placed in the same position, the angles at the base will appear larger than the angle at the vertex. If a line be drawn parallel to the bottom of a sheet of paper, and a second line, making with it an angle of 20° or 30°, any one attempting, without moving the paper, to draw a third line through the point of intersection, so as to make an angle with the second line equal to that which the second makes with the first, will make the second angle too large. (This experiment is guaranteed by Helmholtz.) After reading Helmholtz's theory, metaphysicians may be willing to allow that all these illusions are to be derived, after his example, from the clouds. As metaphysicians have, before now, contributed a good deal to the clouds, it is perhaps only fair that the clouds should contribute something to the metaphysicians.

W. T. RADFORD

"To this category also belongs the celebrated question why the moon appears larger when she is near the horizon than when she is high in the heavens, although, in point of fact, owing to atmospheric refraction, her vertical diameter ought in the former case to seem less than in the latter. Even Ptolemy and the Arabian astronomers were perfectly aware that the true reason why the moon appears larger when seen in the horizon, is that she then appears further off. The real question therefore is, why the sky should appear further from us at the horizon than it does at the zenith. Various causes have been assigned for this fact, and I am myself disposed to admit that there are several causes which combine to produce this effect, so that it may be difficult to say which of these causes predominates in any one case.

"First of all we must remember that there is no decisive reason why the starry firmament should appear to us to be a spherical surface. It certainly reveals to us objects (the stars) which are at an infinite distance; but hence we can only infer that it may assume the appearance of any such indeterminate surface as any motive whatever may lead us to ascribe to it. If we were floating in empty space, and could survey it in its whole extent at the same moment and in all directions, or if its movements were so rapid as to make a distinct impression on the senses, there might be more reason for assigning to it a spherical rather than any other kind of surface. In point of fact, however, its apparent form and apparent direction are constantly changing, according as the portion we happen to see is more or less enclosed by various terrestrial objects, and according as we fix our attention on a higher or a lower spot. We shall see further on that we are naturally disposed to regard it as a plane surface, at right angles to the line of sight, whenever both eyes are steadily fixed on one point.

"But with the canopy of cloud the case is entirely different. The clouds in general are so far from us that the criteria for judging of distance which binocular vision or the movement of our own bodies can supply are utterly useless. But the clouds are often disposed in parallel lines, they generally drift with a constant velocity and in the same direction; when near the horizon they appear like bars across the sky seen edgewise, and so lighted that it is easy to perceive they are bodies whose horizontal extension is foreshortened by perspective. All these indications serve to give us the impression that the true form of the canopy of cloud, at least in the zenith, is that of a very flat dome. On the horizon indeed these indications cease to serve us; there the clouds, like the mountains, appear to be evenly painted on a vertical or nearly vertical background, which gradually passes into the surface of the earth below, and into the firmament above. Now, since the senses supply no criteria by which we distinguish between the distance of the clouds and that of the sky, it seems only natural that we should ascribe to the one the ascertained form of the other, so far, at least, as we can separate them. This, I believe, is the way in which our conception of the sky, as a flat domelike vault, must originate, vague, variable, indefinite as that conception undoubtedly is.

"Moreover, the apparent increase in the size of the sun or the moon is never very striking or decided, except at those times when the air near the horizon is heavily charged with vapour, and when, as a necessary consequence, the heavenly bodies in question only shine with a very feeble light; we have then the very same effect with which we are perfectly familiar in the case of distant mountains. They appear more distant than they do when the air is clear, and therefore larger. Moreover, when suitable terrestrial objects happen to be placed near the horizon, they add very much to the effect. When, for instance, the moon sets near a tree some twenty feet in diameter, and about 1,000 yards off, as she subtends the same visual angle, and is known to be far more distant, she appears to be very much larger; whereas, when the moon sets behind a flat horizon, there is no object of comparison to enable us to perceive that her small *apparent* may represent a very great *absolute* magnitude.

"When I look at the moon reflected from a piece of parallel glass, so that her image appears to be very near the horizon, I do not find that the image looks decidedly larger than the moon herself seen directly high in the sky, although in this way it is easy to compare the apparent magnitude of the reflected image with that of the terrestrial objects seen together with it. In this case it is evident the reflected image has not the effect of being seen through the vaporous portion of the atmosphere.

"To my eye, the apparent increase in magnitude near the horizon is much more apparent in the case of the moon than in that of the sun. When the form of the sun can be distinguished at all, his light is generally so dazzling that we cannot look at him steadily, and consequently cannot compare him directly with any terrestrial objects that happen to be on the horizon. Even in the case of the moon when the sky is quite clear, the delusion is not so apparent. In all cases the delusion depends in a very great degree on the state of the atmosphere."

Occurrence of the Little Egret

An adult specimen of the Little Egret (*Ardea garzetta*, Linn.) was shot at the end of last month, on the mud-flats below Topsham, a town on the Exe, four miles below Exeter, and has unfortunately fallen into private hands. This is the first known occurrence of this beautiful bird on the Exe, but two or three specimens were recorded from the Dart and Tamar more than fifty years ago. The last specimen obtained in South Devon was killed in April 1851.

A nearly adult male Montagu's Harrier was shot near Christon, Devon, last month, and I have obtained it for the collection in this Institution. The female bird has since been seen.

W. S. M. D'Urban

Devon and Exeter Albert Memorial Museum, Queen Street, Exeter, June 13

Pinkish Colour of the Sun

In addition to the several accounts of the curious pinkish appearance of the sun, noticed in the numbers for May 26 and June 2 of your journal, it may perhaps interest your correspondents and the readers of Nature to know that the sun presented a round disc of a very unusual pinkish colour, here and at Cranbrook (about five miles north-east from Hawkhurst), in Kent, between five and six o'clock P.M. on the afternoon of Monday, the 23rd ult. It was so seen by myself at Cranbrook, in company with several others, who thought that the colour was quite unusual, shining through a thick haze of apparently low cirrostratus, but which was perhaps rain cloud, as the air at the time was light from the north, and cold, while the mist, or haze, seemed to be at no very great elevation above the ground, and considerably lower than those ordinary forms of cirrostratus in which halos and mock-suns are generally seen.

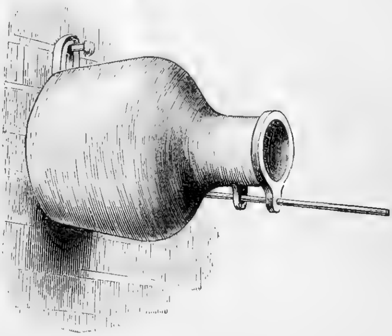
The colour observed here was a pinkish buff, or such a mixture of pink and yellow as to suggest the abundance of more blue and violet, and the absence of more yellow light than in the orange and reddish tints, generally seen in the setting sun, so as to resemble the colour of very pale blotting-paper, or a light flesh-colour. While the disc was still clearly seen of this colour, two or three sun-spots were visible upon it with the naked eye. These could no longer be distinguished at six o'clock, when the peculiar pinkish hue was also succeeded by the ordinary yellow of the sun's disc near the horizon, seen through a thick haze. On the same afternoon (of the 23rd) the appearance of the sun's round disc through a thick cloud of haze in the sky was noticed, for a considerable time, as visible with rare and unusual distinctness at Tunbridge Wells, in Kent.

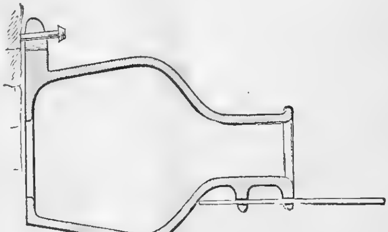
A. S. Herschel

Collingwood, Hawkhurst, June 8

La Petite Culture en Belgique

I enclose drawings of earthern pots, which I observed nailed against the south side of a farm-house near this. These pots are for sparrows' nests, and the young, when fledged, are taken





and eaten. I think this form of "La petite culture" cannot be commended in a country so swarming with insects as Belgium, and I infer from the careful make of the pots that the custom is not a new one, though it may be new to some of the readers of Nature.

N. A. Staples

Louvain, June 4

The Report of the Meeting of the British Association for the Advancement of Science

It would be a great boon to the English public if the papers which are read before the members of the Association, or rather those who have the means as well as the inclination to attend such important annual gatherings, could be published within the first few weeks immediately after the meeting, and at such a price as many in this land would by no means grudge. According to the present arrangement, some months have to pass before even such as can afford the heavy price placed upon these reports can obtain them, the result of which is, that the poor and meagre reports of the various newspapers are all that the majority of persons have to inform them about the advancement of science. Could not some such report as that authorised by the committee of the Church Congress be produced? The first Congress was reported fully, the papers entire, and the discussions almost so, in a well-printed volume of nearly 500 pages, for the moderate sum of half-a-crown, prepaid; or four shillings if purchased afterwards. There are many, I am convinced, who would gladly prepay three or even four half-crowns for the report of the papers only which are read before the British Association.

Birkenhead G. H. H.

TERRESTRIAL MAGNETISM

The great progress made during the last few years in our knowledge of the phenomena of terrestrial magnetism has naturally attracted not a little attention to this interesting subject, and the persevering efforts of many leading scientific men are the surest guarantee of the ultimate success of the labours undertaken in this cause. Hitherto theory has gone hand in hand with observation, and those most able to undertake the charge are now only waiting a more complete array of facts, in order to present us with a theory of terrestrial magnetism based on solid foundations, and equal in interest, in completeness, and in the utility of its practical applications, to any of its sister sciences. The required facts may be ranged under these two heads. First the actual values of the magnetic elements at all parts of the earth's surface, with the secular variations of these elements; and secondly, the daily, monthly, and yearly range through which these quantities vary, and the irregular perturbations to which they are subject. To meet the first demand, a complete set of magnetic observations must be taken in different countries, the stations of observation not being too far distant from each other; and a repetition of these at intervals of from ten to twenty years will supply an accurate record of the secular variations of the required elements. The second need can only be satisfactorily provided for by the establishment of permanent magnetic observatories, supplied with self-registering magnetographs, by which every change of the magnetic force can be correctly traced.

Fortunately much has already been effected, and more is actually being carried out for the furtherance of this latter object. This country alone possesses three such observatories, Lisbon another, Florence a fifth, and four others are at present in course of erection, at St. Petersburg, Bombay, Melbourne, and the Mauritius. The fact, now clearly established, that any disturbance of the magnetic needle traced by our magnetographs in England has a corresponding perturbation recorded at Lisbon and at Florence, is an earnest of what we may expect from a careful comparison of magnetograms from all quarters of the globe.

But the other branch of observation, on which the determination of the absolute values of the magnetic elements solely depends, has scarce kept pace with the giant strides of the fixed observatories. Doubtless a series of monthly absolute readings is being taken at each magnetic observatory to serve as a basis for the differential curves traced by the magnetographs; but such observatories will always be few in number, and the accurate determination of the magnetic elements of these few points, however useful it may be, will scarcely suffice to give us a complete knowledge of the magnetic condition of the earth's surface. The survey of the ocean we may well trust to the devoted and persevering enterprise of the naval officers, who are adding so much to our store of meteorological and magnetic knowledge; but for the land surveys we must mainly depend on the accurate observations of private observers. A very limited number of scattered observations made at stations remote from each other, and the careful survey of scarcely half a dozen countries of Europe, is all we can point to as at present accomplished. If it be true that labour and expense are the great obstacles to be overcome when an important enterprise has to be undertaken, we may well wonder that the more laborious and by far the most expensive of the observations needed for the science of terrestrial magnetism are being amply supplied by the establishment of fixed observatories, whilst the magnetic surveys, which may be now made an agreeable pastime, and need scarcely add perceptibly to our usual expenses, are nearly entirely neglected. Most men who are daily engaged in intellectual occupations, require yearly a few weeks of repose, or at least of mitigated application. For many, some interesting work that does not too much engross their minds, and that differs considerably from their usual routine duties, offers a means of relaxation far more enticing than a complete cessation from labour. For men of this class, who have had some little experience in the use of instruments of precision, a magnetic survey might offer the greatest attraction during a summer vacation. A brief sketch of a simple method of carrying this into effect, which has been found to work well during two successive vacations, may not therefore be without its utility and interest.

The country chosen for a rapid survey should be one that offers considerable facilities for railway communication; and a previous study of the direction of the lines will prevent much unnecessary expense and loss of time from travelling twice over the same ground. Should the railway arrangements of the country resemble those of France, a companion for the journey becomes an absolute necessity; for the instruments used are of too delicate a make to be entrusted to the tender mercies of foreign railway officials of the registered baggage department.

The instruments required for the survey are a dip circle, an unifilar, a small transit theodolite, and a good chronometer. Two tripod stands are nearly a necessity if time be any consideration. Three days might conveniently be allotted to each station; one for travelling, another for the observations, and the third for visiting the objects of interest in the vicinity. Twenty sets of observations would thus be completed in the course of two months; and two vacations so employed would furnish data sufficient for the accurate study of the magnetic condition of an extensive tract of country. But should the time at the observer's disposal be more limited, and he feel equal to the task, the day of travelling might reckon as a day of rest, and the public monuments be visited during the leisure hours of the day of observation.

Arrived at a station, the first thought should be about the choice of a fit place of observation. The garden of any large public institution will perhaps in general be found the most convenient and accessible, unless an extensive and somewhat retired garden be attached to the hotel. Inquiry must next be made about the position of any considerable mass of iron, such as large pipes, which serve not unfrequently to convey gas or water at a few feet below the surface of a gravel walk or a grass plot. For the observation of the dip and the intensity, a good shady spot is required; whilst, for the declination, a position well exposed to the sun, at the early hours of the morning, is the best possible. A tent may sometimes be required to supply the want of trees; but when a large canvas covering, with poles, &c., has been carried from the north of England to the Pyrenees without ever being asked to do service, one is more inclined to trust in future to the resources of any locality, rather than again to travel in company with such a burdensome luxury.

Provided the morning be not hopelessly cloudy, it will generally be found preferable to start work by observing the declination. This choice may at times save the travellers a day or more of precious time in changeable weather; for the sun then not unfrequently shows himself for a short time in the early morning hours, and when the clouds have been permitted to close around him, he remains persistently hidden for the remainder of the day. For finding the magnetic declination, the sun is our most useful helpmate; but during the rest of the observations his presence can with benefit be dispensed with. The unifilar is therefore at once put together, and whilst the torsion of the silk thread by which the declination magnet is about to be suspended is being removed, the theodolite is fixed on a tripod stand, and several transits of the sun are taken over the vertical wires. The azimuth circle having been read at each passage of the sun, the telescope is placed horizontal, and the position of a fixed mark determined. A few altitudes of the sun might then be taken, for finding the error of the chronometer, before removing the theodolite from the tripod stand to make way for the unifilar. Great care must of course be taken in levelling each instrument before commencing an observation; and, after railway travelling, all the screws should be well examined. The tripod stand remaining firmly fixed in its first position, the unifilar is placed upon it, and the telescope is directed to the fixed mark and its azimuth read. The magnet is then suspended, brought to rest, and the positions of the azimuth circle and of the magnet scale noted. A second reading of the scale is afterwards taken in the inverted position, and this invertion should be twice performed, for greater security. A glance at his note-book will at once tell the observer whether he has entered all the data necessary for the future calculation of the declination. The chronometer gives the azimuth of the sun for a known latitude and longitude, the sun fixes the chosen mark, and the angle between the magnet and the mark then tells us what are the bearings of the compass at the station of observation. Having thus secured the declination, an interesting set of scale readings may be taken whilst the magnet still remains suspended. These readings, if taken every five minutes during any considerable time, will furnish materials for a comparison with the magnetograms of the fixed observatories; and thus afford satisfactory data for determining the relation that exists between the magnetic perturbations at different points of the earth's surface.

The dip circle is the next instrument to which attention must be directed. The observation of the dip of the magnetic needle is in theory the simplest, and in practice the most trying work of the survey. To attempt it when the atmosphere is saturated with moisture may be an excellent exercise of patience, but can scarcely lead to any trustworthy results; and a dusty locality will easily give the observer hours of labour before he has finished with a single needle. At each station, two or three needles should be used; each observation being the mean of 32 readings taken in different positions of the instrument and needle. The method of observation can offer no difficulties; after levelling the instrument, the needle is magnetised and gently lowered until it rests on its agate planes. The dip circle is then turned round till the needle takes up a vertical position; the reading of the azimuth circle at this point, when increased by 90 degrees, gives the direction of the magnetic meridian. The instrument may now be clamped with the needle in the meridian, and the readings of the angle of inclination commenced. Whilst the dip is being found, the torsion may be removed from the silk thread that serves to suspend the vibration magnet.

A complete knowledge of the direction of the earth's magnetic force at the station of observation having been acquired, it remains to determine the intensity of this force. Knowing the direction, it suffices to measure the intensity of the force in any given direction, in order to ascertain the total effect of the force. The horizontal component of the intensity is then the only element that still remains to be observed, and this is found by the method of vibrations and deflections. A magnet in the form of a small telescope, having a delicately graduated scale at the end farthest from the observer, is hung by a thread, and made to vibrate through an arc of a few seconds. The time occupied by 100 such vibrations is noted by the chronometer to within the twentieth of a second, and this is repeated some twelve times to obtain an accurate measure of the time of one vibration. The square of this interval of time serves to determine the product of the horizontal component of the earth's magnetism, by the magnetic moment of the vibrating magnet.

The quotient of these same quantities is next ascertained from the deflecting power of the vibration magnet. For this purpose another magnet is suspended by a very delicate silk thread, from which the torsion has been removed, and the vibration magnet is placed on a graduated brass bar at known distances from the suspended magnet. The opposite poles of the deflecting magnet are then presented alternately to the centre of the free magnet, and at the same distance due east and west. The various readings of the azimuth circle, and of the scale reflected by the mirror of the deflected magnet, are noted for each position, and the changes of temperature, which enter into the results, are frequently recorded.

The series of observations is then completed, and, when the instruments have again been packed with all possible care, the observers are ready for the journey of the morrow. Very much of the success of the survey depends on the steady rate of the chronometer; and hence no pains should be spared to preserve it from all jolting movements whilst travelling, and altitudes of the sun should never be omitted. But it is, above all, advisable to lose no opportunity of comparing the chronometer at any good observatory that can conveniently be visited on the way.

A full set of the above observations may be taken by one person within the space of half a dozen hours, but then all must have gone on smoothly from starting to the end. A loud clock, a clear-toned bell, or even the rustling of the leaves in a high wind, will not unfrequently compel an observer to recommence his series of vibrations. Or again, the breaking clouds will entice him out with his theodolite, and then the sun will always seem purposely to hide himself at the moment he approaches the fatal wire. But the near approach of friends is what tries most the powers of endurance. The second has arrived for taking the observation, the eye and ear are all attention, and a person shows his interest in the work by spoiling all with an ill-timed question. Or again, the suspended magnet is just coming to rest after a tedious oscillation, the observer's patience is about to be rewarded by a perfect reading, when a violent movement of the magnet makes him aware that the bunch of keys or pocket-knife of a friend, who approaches with the most scupulous avoidance of all noise, have attracted the attention of the giddy little needle. But a good observer will never be overcome by such trifling difficulties.

A two months' vacation passed in this alternation of travel, observation, and repose, will witness the accumulation of data sufficient for the calculation of the isogonics, isoclinals, and isodynamics of a large extent of country, and the observer will return to his routine labours invigorated in mind and body, and with the consoling thought that he has added some little at least to that mass of well-established facts, which must ever be the only foundation of any true advancement in the Natural Sciences.

S. J. PERRY.

COFFEE

CONSIDERING the fact that the necessities of our daily life, whether as clothing, food, or medicine, are mostly provided by the vegetable kingdom, it is remarkable how little is generally known of the sources from whence we derive our most common articles of commerce. We propose in this article to say something about our Coffee, and especially about the mode of detecting whether the commercial article is pure or adulterated.

Although the specific name of the coffee plant, *Coffea Arabica*, appears to indicate the coffee tree to belong originally to Arabia, it is with good reason supposed to be a native of the mountainous part of the south-west point of Abyssinia, having been introduced from thence into Arabia, where it is said to have been first used about 1450. For about 200 years after this date the whole of the coffee used was grown on Arabian soil, from whence the Dutch introduced the plant into Batavia, after which it was carried into other eastern countries as well as into various parts of the western hemisphere. The introduction of coffee into Europe took place about the middle of the sixteenth century, fourteen years before the introduction of tea.

Fig. 1.—Branch of Coffee Plant.

The fruits of the coffee tree when ripe are gathered and taken to the pulping house, and placed in a machine called a pulper, by which the fleshy covering is removed, the beans or seeds pass into a cistern, and the pulps are carried off in another direction and are collected and preserved for manure; the seeds themselves are left to steep for several hours, so as to soak off any remaining mucilage or pulpy matter; they are then washed and dried, the parchment and the thin inner skin being removed by winnowing, after which they are packed in bags and ready for shipping.

The berries or seeds of true Mocha coffee, which is, however, now scarcely to be obtained in Europe, are usually more round than those of other varieties; they nevertheless vary much in form as well as in size and colour; and though the several commercial sorts are easily known to a practised eye, they are difficult to detect by an ordinary observer. The value to the consumer does not in all cases depend so much upon the size or shape of the seed as upon its flavour and the strength of its aroma, but these qualities cannot be discovered until after roasting; therefore in purchasing unroasted coffee, an important point is to see that the seeds are not damaged by sea water or mouldiness. In roasted "whole" coffee, the case is different, for a greater or lesser aroma of more or less fragrance can be detected, the volatile oil, and the peculiar astringent acid to which the aroma and flavour are due, and which before were latent in the seed, being developed

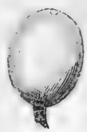


Fig. 2.—Coffee Berry (*nat. size*).

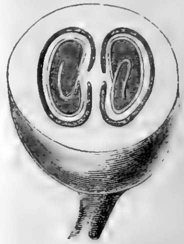


Fig. 3.—Section of Coffee Berry (*magnified*).

by the heat. In the process of roasting, the seeds lose about one-fifth in weight, but increase in bulk by about one-half.

The peculiar principle of coffee is called caffeine, and is identical with that of tea; it acts as a stimulant upon the brain, preventing sleep or drowsiness, and causing greater mental as well as bodily activity; it is also said by some chemists to repair or prevent in a remarkable degree the too rapid waste of the tissues, so that life can be sustained on a smaller quantity of food than would be the case without the use of coffee.

Understanding these principles which Nature has given to coffee, and which Science has revealed for our benefit, we cannot fail to see the great importance of obtaining the article in its genuine state. Upon microscopical examination, genuine coffee can be easily detected, the cells of the coffee-seed being very irregular in form, and having very thick walls with ragged sides. Some of these ragged projections belong to the true cell wall, while a few are composed of starch granules. Genuine coffee, then, should always present this appearance, for there are no tubes or spiral vessels in the true coffee seed as there are in the root of the chicory; and moreover, in the cellular part of the chicory root, the cells themselves are larger, the walls are shown as mere fine lines, closely fitting together by the pressure exerted upon them in the process of growth. This difference will be more readily understood by reference to Figs. 4 and 6. In the most genuine coffee, however, a certain portion of the skin must be present, the microscopical appearance of which is shown at Fig. 5; by reference also to Fig. 3, it will be

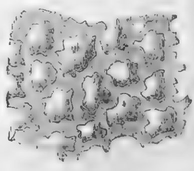


Fig 4.—Tissue of Coffee Seed, after being roasted and boiled.

Fig. 5.—Microscopical appearance of Coffee Skin, after being roasted and boiled.

seen how a portion of this skin is naturally enclosed in the folds of the seed, so that, while in the process of cleaning it is entirely removed from the surface of the seed, the enveloped portion remains, as it cannot be removed without breaking or injuring the seed. This, however, has been done in the trade, and a series of coffees was exhibited in the Great Exhibition of 1851, amongst which were roasted coffee seeds which had been divided longitudinally by a patented machine, and the folded skin taken out; these broken seeds were sold under the name of coffee nibs. We do not see any advantage in this; but on the contrary, the broken seeds would be much more liable than whole seeds to adulteration with damaged berries, and this skin is so thin that it adds little to the weight of the coffee, and the small proportion in which it occurs does not affect either the pocket or the health of the consumer; indeed, we are told by some travellers, that neither the skin nor the parchment itself deteriorates the quality of the coffee, but rather adds to its value, for in some parts of Arabia the parchment is preferred before the seed itself.

Therefore genuine coffee, when seen under a microscope, will exhibit an appearance similar to that shown at Fig. 4, with the addition, in nearly all cases, of a few small bodies like those at Fig. 5, scattered here and there.

A very simple test for the presence of chicory in ground coffee, is to drop a little in a tumbler of clean cold water. Do not stir it, but if chicory is present the particles will immediately drop to the bottom of the tumbler, imparting at once to the water a deep amber colour; the coffee particles will float for a much longer time, and the water

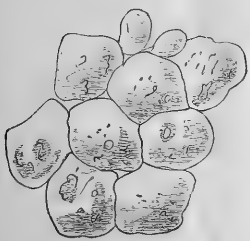


FIG. 6.—Cellular Tissue of Chicory.

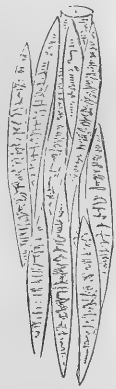


FIG. 7.—Vascular Tissue of Chicory.

will be but slightly coloured. The most satisfactory way of purchasing coffee, however, is in the whole state, and to grind it as it is wanted, when all the freshness of the aroma is obtained in the infusion. For examination under the microscope, coffee should be previously soaked in water, or boiled in a weak solution of potash; this both softens the tissues and makes the substance more transparent. For persons unacquainted with vegetable structure, it will help them very much in determining the microscopical appearance of genuine roasted coffee, to examine first both the fresh coffee seed and the fresh chicory root; for this purpose a slice should be cut from each as thin as possible, moistened with water, and placed on a glass slide with an ordinary thin glass covering dropped on the top, and gently pressed down with the fingers so as to exclude all air bubbles. A half-inch objective may be used, and with an ordinary amount of perception adulterations may be detected. Other substances than chicory may be mixed with coffee, but none will present such microscopical appearances as those we have shown to belong to genuine coffee.

J. R. JACKSON

NOTES

WELL-WISHERS of the University of Oxford will rejoice to hear that the honorary degree of D.C.L. has been offered to Mr. Darwin. The state of Mr. Darwin's health unfortunately precludes him from accepting the proffered honour, but the scientific naturalists of this and other countries will none the less appreciate the compliment which has been paid to their great leader. It is all the more graceful as Mr. Darwin is not an Oxford, but a Cambridge man, a circumstance which the University of Cambridge seems to have forgotten; though by-and-by it will be one of her claims not to be herself forgotten.

DR. HOOKER, F.R.S., and Professor W. H. Flower, F.R.S., have been appointed examiners in botany and anatomy for the Natural Sciences Tripos at Cambridge. The other examiners are Professor Miller, Sec. R.S., in mineralogy, Mr. Trotter, fellow of Trinity College, in chemistry and physics, Mr. Danby, fellow of Downing College, in geology.

WE are glad to announce that Mr. Geikie has arrived in England, and is in a fair way of recovery.

THE Sars subscription fund has now reached 343*l.* in England, and 11,666 francs in France. It is very desirable that intending contributors should forward their subscriptions without delay.

IT is reported that the Secretary of State for India has determined upon establishing in this country a complete College of Science for civil engineers, for the education of those who are to be employed on the extensive Government works in that country.

ALL true lovers of science will be glad to hear of the approaching visit to this country of Prof. Henry, the Secretary of the Smithsonian Institution at Washington, United States. It is well known how much our own celebrated electricians are indebted to Prof. Henry for his valuable researches in magnetism and electricity, the results arrived at being freely placed at the disposal of all whom they might interest. We understand he is daily expected.

THE celebrated photographer, M. Niépce de St. Victor, having died in very straitened circumstances, leaving a wife and two children totally unprovided for, a committee of French photographers has been formed to collect a fund for their relief. Subscriptions may be forwarded to MM. Blacque and D'Eichthal, bankers, 19, Rue de Grammont; or to the president of the French Photographic Society, 9, Rue Cadet, Paris.

IN the Rev. William Hincks's address as President of the Canadian Institute, we find the following sentence:—"If we may implicitly believe a statement in the new periodical devoted to natural science, NATURE, whilst the English are still discussing the possibility of Darwinianism being true, the Germans have so thoroughly adopted it that it has become the foundation for new systems—the starting-point for fresh inquiries. This may appear to most of us to be going somewhat too fast; but then NATURE may be presumed to be the special organ of the extreme Darwinians, and might be thought to see facts through a somewhat coloured medium." It will hardly be necessary to point out to our readers that we are the organ of no party, extreme or otherwise. Free play has been given in these pages to the expression of opinion by competent men of every section or party. In so far as "Darwinianism" implies rigid accuracy of observation and a candid consideration of all the varied phenomena of natural science, we trust we shall always be Darwinian as we hope to be Newtonian.

A. H. GARROD was elected on the 13th inst. a Foundation Scholar of St. John's College, Cambridge, for proficiency in the Natural Sciences; at the same time H. Blunt and H. N. Read were elected, for the same cause, to exhibitions of 20*l*. and 10*l*. respectively. Mr. Garrod obtained in 1868 the Natural Science exhibition of 50*l*. per annum, open to students commencing their residence.

THE committee who organised courses of lectures for women at Cambridge last year have issued the following scheme of lectures for the academical year 1870-71. They will be delivered, generally speaking, twice a week within the period of University residence:—English History, by Professor Seeley. English Literature, by W. G. Clark, M.A., and others. It is proposed to give a series of short courses on different departments of English literature. They will be delivered once a week during the October Term, certainly, and, if possible, during the whole academical year, commencing with a course on "Shakespeare and the Elizabethan Dramatists" by Mr. Clark. English Language and Literature, by W. W. Skeat, M.A.; Latin, by J. E. B. Mayor, M.A., and A. Holmes, M.A. (Elementary Lectures); Greek, by J. Peile, M.A.; German, by W. C. Green, M.A.; French, by M. Boquel; Algebra and the Principles of Arithmetic, by Professor Cayley; Practical Arithmetic, by J. F. Moulton, B.A.; Geometry and Elements of Physics treated Historically, by W. K. Clifford, B.A.; Logic, by J. Venn, M.A.; Political Economy, by A. Marshall, M.A.; Geology, by T. G. Bonney, B.D. (Botany, by Professor Babington, will be substituted for Geology in the Easter Term); Chemistry, by P. T. Main, M.A.; Harmony and Thorough Bass, by G. M. Garrett, Mus. D.; Theory of Sound in its application to Music popularly and experimentally treated, by S. Taylor, M.A. All persons wishing to attend any of these lectures in the next October Term are requested to apply to the Rev. G. F. Browne, St. Catherine's College, on or before the 1st of October. The committee announce the following Exhibitions:—One of 40*l*. per annum for two years, to be given to one of the senior candidates in the Cambridge Local Examinations, January, 1871, according to the report of the examiners. Attendance at two courses of lectures in every Term will be required as a condition of receiving the Exhibition in each year. Given by Mr. Mill and Miss Taylor. One of 20*l*. for one year together with free admission to three courses of lectures in each Term; and one of 10*l*. for one year. Given by Mrs. Adams. These two will be given by two of the candidates in the Cambridge Examination for Women, July, 1870, according to the report of the examiners. Attendance at two courses of lectures during each of two Terms at least will be required as a condition of receiving either of these Exhibitions.

THE following elections to scholarships have been made at Gonville and Caius College, Cambridge: G. Warington, 50*l*., for chemistry; G. J. Romanes, 20*l*., for anatomy and physiology. We hope that this College, which has been peculiarly prolific in eminent medical men, will soon follow the example of some of its neighbours in offering open scholarships for Natural Science.

DR. ALBERT WALSH was elected, at a meeting of the Royal College of Surgeons of Ireland, held on the 13th inst., as president of the college for this year, and Dr. Wharton was elected vice-president. The only alteration in the council was the election of Dr. Butcher, by a small majority, in place of Dr. Mapother.

THE examination for the vacant fellowship in Trinity College, Dublin, commenced on the 30th May, and concluded on the 11th inst. The following numbers represent the highest possible answering in each of the three courses. Mathematics, 1,250 (pure 1,000, experimental science 250), classics 750 (classics 650, Hebrew and cognate languages 100), mental and moral science 500. The election took place on Trinity Monday, when George Cathcart was elected fellow and the Madden Prize was given to W. S. Burnside.

THE *Academy*, referring to Mr. Lockyer's recent observations that the various vapour-currents in the solar envelope, which has been called the chromosphere, and defined as possibly the outer layer of the sun's atmosphere, were so rapid and violent that the position of the hydrogen lines in the spectrum was altered, and that by noting the amount of alteration, the actual velocity of these "solar storms," as Mr. Lockyer has termed them, could be determined without difficulty; states that Father Secchi, of Rome, who a long time ago denied the accuracy of these observations, and the validity of these conclusions, has quite recently returned to the charge. Father Secchi asserts that the alterations observed in the wave-length are really due to the *sun's rotation*, and then he proves by calculation that the velocity of the sun's surface at the equator is 429 kilometres per second. He next calculates what alteration this motion should make in the position of the hydrogen lines in the spectrum; and last of all he points his spectroscope to the sun and sees exactly such alterations as his calculations require. Now this at first appears a final answer to Mr. Lockyer, but Volpicelli and Fizeau have pointed out that Father Secchi's calculation in fact shows him to be ignorant of Mr. Lockyer's reasoning, and that his 429 kilometres per second should really read two kilometres.

THE *Gardener's Chronicle* congratulates us on finding that our friends across the Channel have as characteristic a Circumlocution Office as ourselves. Here is what the French papers say on the matter:—A provincial druggist, desirous of gathering Fox-glove (*Digitalis*) in one of the State forests, applied for permission to the local authority (*garde général local*), offering at the same time to pay an annual sum of six francs for the privilege. The local magnate transmitted the request to his inspector, who forwarded it to the Conservator of the department, who despatched it to Paris to the Director-General of Forests, who caused it to be sent to the Minister of Finance. The Minister referred it "for study" to the Director-General of Domains, who sent it to the Departmental Director of Domains to be examined by the Registrar. The latter, after examination, pronounced a favourable opinion on the request, and sent it back to the Departmental Director, who forwarded it to the General Director, who, in his turn, despatched it to the Minister, through the agency of the General Secretary of Finance, who availed himself of the opportunity to make his comments on the matter. Then the unhappy druggist's request was returned to the Director-General of Forests, who sent it to the Conservator, he to the inspector, and the inspector to the *garde général*, who was the original recipient of the request. The authority "to cull simples," at length reached the successor of the original postulant, and at an age when he was too old to herborise. We hope this style of management will not be engrafted on to our Indian forest department, already well provided in this way.

THE lecture next Sunday evening at St. George's Hall, Langham Place, under the auspices of the Sunday Lecture Society, will be delivered by Mr. D. Forbes, F.R.S. The subject is "Volcanoes."

M. CLAUDE BERNARD commenced yesterday in the amphitheatre of the geological gallery his course of lectures on general physiology at the Museum of Natural History, Paris. He will treat of the general principles of physiology, especially from the point of view of its relation to the other sciences.

DR. MAXWELL T. MASTERS is engaged in editing a new edition of "Henfrey's Elementary Course of Botany," condensed, and in many parts re-written, which will be published in a few days.

DR. R. VIRCHOW has contributed to a recent number of the *Zeitschrift für Ethnologie* an interesting article on "Portrait or

Facial Urns" (*Gesichts-urnen*), which has been printed separately. Funereal urns, containing bones and ashes, have long been known, in museums, from Egypt and Etruria, the lids of which, generally of stone, were fashioned into the shape of a human head, or that of some other animal. The human heads, if not portraits, are supposed to have at least indicated the condition of the deceased. In the course of the last forty years facial urns have been discovered in mounds in various parts of Germany, mostly near the Roman settlements on the Rhine, but more recently in the neighbourhood of the watering-place Zoppot in Pomerania, but confined to a very circumscribed district, and exhibiting, according to Dr. Virchow, points of marked difference from those found in Rhenish Prussia. Many of these have well-developed human faces, others have rough drawings of animals, sometimes difficult to make out, scratched upon them. Similar urns are described as having been found in Mexico and Peru; but Dr. Virchow believes that the peculiar contour and form of ornamentation of these North German urns indicate an Etrurian origin, and possibly point to a colonising of the Baltic from Italy.

AT the last meeting of the Scientific Committee of the Horticultural Society, the Rev. M. J. Berkeley exhibited some specimens of the female form of *Lychnis diurna*, in which the flowers were infested with smut, and the stamens, usually abortive, were developed. Mr. Berkeley believes that "the *Ustilago* penetrates the plant, but as it can only fructify in the stamens, it would appear to be the determining cause of the production of those organs in the normally female flower." It is laid down as a rule in Mr. Darwin's works that no change can take place in a species through the agency of Natural Selection unless the change is for the benefit of the species. It is difficult to see how the abnormal production of stamens for the purpose of finding a home for the smut can benefit the *Lychnis*; and it would be interesting to know whether vegetable physiologists generally will sanction the idea of such a power in the plant to develope organs which assist in its own destruction.

WE have received a letter from Mr. Walenn pointing out that in our report of the meeting of the Chemical Society on May 19, we have omitted to mention that the calico-printing roller which was exhibited was immersed for some days in a bath containing sulphuric acid and cupric sulphate, then coated with brass in an alkaline liquid, and finally coated with copper in an acid bath. A deposit, $\frac{3}{16}$ of an inch thick, weighing 29 lbs., and of compact copper, was thus produced.

THE *Chemical News* describes a new method of heating stoneware vessels, and of obtaining regulated high temperatures, which will be of considerable importance in conducting chemical and pharmaceutical operations for manufacturing purposes. By this new method, which has been patented by Mr. J. A. Coffey, the pharmaceutical engineer, any temperature ranging from 100° to 700° F. can be safely and easily obtained. The principle is to cause heavy paraffin oil to circulate, first through a coil of pipes in a furnace, and then through the jackets of the pans. It moves by its own convection. As contrasted with steam-heat, the inventor claims for his process a saving of 30 per cent. in fuel, the large amount of heat necessary to convert water at 212° F. into steam at 212° being economised.

"THE Autotype Process: being a practical manual of instruction in the art of printing in carbon or other permanent pigment," is a handsomely-printed pamphlet of 48 pp., very clearly written, and describing all the modern improvements in the process, which are considerable. It is divided into five parts, which treat of the nature and history of carbon printing, its general practice, its special practice for non-inverted pictures, its special practice for inverted pictures, and concluding observations

ETHNOLOGY

The Meenas of Central India

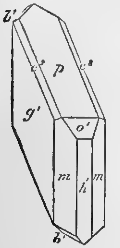
LIEUT-COL. C. L. SHOWERS has communicated to the Proceedings of the Asiatic Society of Bengal, a paper upon the Meenas, a wild tribe of Central India. Lieut.-Col. Showers says, that when the Meenas first fell under his observation in the year 1854, they were in a condition of entire lawlessness. Emboldened by long impunity they carried their audacity so far as to attack and pillage several walled towns in the British district of Ajmur, carrying off not only the entire plunder to their hill fastnesses, but numbers of the inhabitants also, holding them to ransom. At that period it fell to the duty of Lieut.-Col. Showers to take them in hand. He proceeded to Jehazpoor, the centre of the disturbed district, and inaugurated certain measures for the tranquillisation and reclamation of the race. From time immemorial, Jehazpoor, in the State of Adeypoor, had been a notoriously disturbed district. A period of brief tranquillity was accorded to Jehazpoor during the early part of the present century by the appalling severity of the measures of the noted minister Lalim Sing, after Jehazpoor fell into the possession of Kotah in 1806. On the restitution of the district, however, to Meywar, in 1819, it soon relapsed into its former disturbed condition. Jehazpoor was, in truth, a position well chosen for the lawless occupation of professional marauders, being a strong, hilly, and jungly country, where the boundaries of four jurisdictions meet, viz.—Meywar, Boondee, Jeypoor, and Ajmur. There are twelve tribes of Meenas in Central India, but the one under notice is called the Purihars, who were the dominant race in Marwur, till dispossessed of their ancient capital Mundore by the Rhatees towards the close of the fourteenth century. In a generation or two afterwards they are found in the Chronicles lurking on the quadruple boundary already alluded to, a race of outcasts without a common head; and such they have continued ever since, "their hand against every man, and every man's hand against them," plundering in gangs and joining any of the great marauding movements that have from time to time been organised under noted leaders. Thus in 1847, some of the boldest of the outlawed Thakur Javahir Singh's followers were these Meenas. The same indomitable spirit which carried the Purihars forth out of the land of their lost dominion seems to have maintained them in a state of wild independence throughout the long interval since; for though nominally owing allegiance to the States upon the verge of whose territories it has suited their purpose to locate themselves in *fallahs* or gangs, they have never really succumbed to any power, but, hanging together as one man, have always united to repel the frequent futile attempts that have been made from time to time by the rulers of States to coerce any of their Meena subjects, so-called. The aggregate of male adults in the tribe is about 24,000; of this number about 10,000, distributed in 200 villages, are located along these border tracts. Individually the men are brave to desperation, athletic and hardy, many of them tall with fine countenances, denoting their superior origin. The Meena will neither eat, smoke, nor intermarry with the aboriginal Buhl, Mair, Kole, or low-caste Meena of the Aravulla; that is to say, he will not give a daughter in marriage, though he will take to his bed as many daughters of inferior tribes as he can support. Their pride of birth is indeed excessive, fostered by traditions ascending beyond the bounds of history to the regions of myth. The genealogist of the tribe is the honoured guest in every village he visits in his annual round. Each family engages his company for one entire day, which is occupied in recording in the ponderous MS. volume the recent additions to the family tree, whether in the male or female branch; for even the ancestry of the women is duly recorded. About half the tribe are armed with matchlocks of a superior manufacture, about half with the bow, and all with the kattar, or double-hilted dagger, which is a weapon they peculiarly affect. It is a weapon never detached from the person a moment, waking or sleeping. Free from the ordinary prejudices of caste, the Purihars are great eaters of meat, which their cattle-lifting raids furnish in profusion. They are also great drinkers of spirits, which serve to increase their natural ferocity. All are married, and many besides take in keeping the widows of their deceased clansmen to the number of two or three each, or otherwise forcibly domicile women abducted in their raids. Perhaps the most noteworthy fact relating to the tribe was their ignorance, up to the day of Lieut-Col. Showers' arrival among them, of the true character of the British Government as the paramount power.

The raids of the Meenas into British territory brought matters to a crisis, and it became necessary to put them down. But in contrast with the unfortunate contests with savage races, which are going on at the present day in other parts of the world, it may not be unworthy of note that the tranquillisation of Jehazpoor was effected without a shot being fired.

CRYSTALLOGRAPHY

Crystals of Potassic Racemate

ACCORDING to De la Provostaye, potassic racemate crystallises in the rhombic system; but M. des Cloiseaux, in a communication to the Annales de Chimie (xvii. 365), disputes the truth of that statement. All the crystals with which he worked (some being presented to him by Pasteur, some by Lamy and Gernez), are really derived from an oblique rhomboidal prism of 96° 56′. The base of this prism is always highly developed, and its inclined diagonal makes an angle of 92° 28′ with the anterior vertical edge. The plane of the optic axes is perpendicular to the plane of symmetry; the acute bisectrix is negative and normal to g'. The *revolving* dispersion is considerable, as shown by the distance which separates the plane where the red axes are situated from that containing the blue. It is easily found by the polarising microscope across fine sections, normal to the acute bisectrix. The *proper* dispersion of the axes is rather strong, with $\rho < \nu$. For their apparent distance in air (at 19)



$2E = 130\ 2'$ (red rays); $132\ 45'$ (blue rays).

ZOOLOGY

Development of Molgula Tubulosa

IT is well known that the larvæ of the Ascidians generally possess a form resembling, in external appearance at least, that of the tadpoles of our common Frogs, and also resembling these in the agility with which they swim about by means of their tail-like appendage. In fact, as M. Lacaze Duthiers indicates in a paper just communicated to the Academy of Sciences, this peculiarity of development has for a long time been regarded as characteristic of all the Ascidians. According to the celebrated naturalist just mentioned, *Molgula tubulosa* forms an exception to this general rule. Instead of exhibiting the brisk, jerking movements of the embryos of the *Phallusiæ* for example, the embryo of *Molgula* moves very slowly within its egg-capsule, its movements consisting chiefly of alterations in its general form which, however, finally effect the rupture of the capsule. Through this opening the embryo *flows*, like a plastic, amœboid mass, which remains quietly at the bottom of the vessel, merely changing its form slowly by amœboid movements. Soon after exclusion the embryo shows indications of zones in its body, and from the outermost of these processes are given off (for some time only five in number), which seem to fix it to surrounding objects and are analogous to the innumerable filaments of the tunic in the adult.

SCIENTIFIC SERIALS

THE May number of Silliman's *American Journal of Science and Arts* (vol. xlix. No. 147) opens with a paper by E. W. Blake, jun., on a method of producing, by the electric spark, figures similar to those of Lichtenberg. His method consists in throwing the discharge upon the surface of a fusible non-conducting body, of which common pitch, coating a plate of tin, seems to furnish the best results. Figures produced by means of friction and induction sparks are represented by woodcuts. Col. J. J. Woodward publishes an important paper on the application of the magnesium and electric lights to photomicrography, illustrated with figures showing the arrangement of the apparatus. An anonymous writer, using the initials J. H. B. L., describes and figures a new form of mechanical finger for the microscope. The chemical papers are as follows:—Combinations of silicon with alcoholic radicals, by C. Friedal and J. M. Crafts, a long and valuable paper; analyses of meteoric irons, and remarks on the alkalies contained in leucite, by J. Lawrence Smith the first part of a memoir by B. Silliman and H. Wurtz, on flame temperatures in their relations to composition and luminosity; on some double sulphates of the Cerium group, by C. H. Wing; on two peculiar products in the nickel manufacture, by Joseph Wharton; two short notes by O. Loew, on the action of sunlight on sulphurous acid, and on the formation of ozone by rapid combustion; and two notes on methods to be adopted in gas-analyses by Dr. Wolcott Gibbs. Prof. Arthur Wright notices some curious phenomena observed by him in connection with the discharge of an electrical machine. Natural History proper is represented in this number only by two papers, namely, an account, by H. Y. Hind, of the Laurentian and Huronian series in Nova Scotia and New Brunswick, and descriptions of some new corals, by A. E. Verrill. The latter consist of four Alcyonaria and three Madreporaria, one of which is described as forming a new genus. Besides the usual abstracts and extracted articles placed under the head of Scientific Intelligence, the editors publish an extract from a letter describing the movements produced by a strong gale of wind in the great iron dome of the Capitol of Washington. This is illustrated by a tracing of the pencil mark showing the course of the movement of the centre of the dome.

THE *Revue des Cours Scientifiques* for June 4 gives us M. Berthelot's lecture "on the isomeric states of simple bodies," in which he treats especially of the different states of oxygen, and concludes by observing that there exists, at least in certain cases, a relation between the states of a simple free body (element) and the nature of the combination from which it has been separated; that a simple body in a certain condition can enter more easily into one class of combination than into another class; and that a simple body can change its condition even while it exists in certain combinations. We find also a continuation of M. Bernard's lectures on "Suffocation by the fumes of charcoal," and an address by M. C. Robin, entitled, "How the parts of an organism adapt themselves to determinate purposes." The number for June 11 is mainly occupied by the description of a contrivance by means of which a locomotive can be made to act as a drag or arrester of motion in the train, when such is the will of the driver. We have also a report of Prof. Helmholtz's lecture before the Society of Natural and Medical Sciences at Heidelberg, on "the physiological action of electric currents of short duration in the interior of extensive conducting masses."

THE *Journal of Botany, British and Foreign*, for June hardly sustains the interest of the earlier numbers of the new series. The original articles are confined to a continuation of the Hon. J. L. Warren's paper on the British *Rubi*, and of Mr. Worthington Smith's *Clavis Agaricinorum*, and an article of considerable interest to geographical botanists and theorists on the origin of species, "On the World-Distribution of the British *Caryophyllaceæ*," by Mr. J. G. Baker. We have besides a biography (with portrait) of the late Dr. Franz Unger, and some extracts and shorter notes.

THE best article in the May number of the *American Entomologist and Botanist*, is one which is neither entomological nor botanical, "On the Gordius or Hair-worm," by Prof. Jos. Leidy, of Philadelphia, giving an account of the anatomical structure of the animal, its mode of propagation, and of the different species found in the United States. There are several other articles and an abundance of shorter notes, that will be of value to the young naturalist.

THE *Geological Magazine* for June commences with an article by Prof. Harkness, on "Elephant-remains in Ireland," which have now been met with in several localities. The writer mentions incidentally that Ireland has afforded hitherto no traces of either the hyæna or lion; the pleistocene carnivora were confined to the bear, of which, however, there are no historical records, and the wolf, which was finally extirpated about the year 1710. Prof. de Koninck contributes a paper "On some new and remarkable Echinoderms from the British Palæozoic rocks, including a species of *Palæchinus*; a new species and new genus, *Placocystites Forbesianus*, belonging to the family of *Cystideæ*; and a new species of *Haplocrinus*, *H. granatum*." Mr. T. G. Bonney, "On supposed *Pholas*-burrows in Derbyshire," believes these burrows to have been made by a *Helix*. Mr. J. W. Laidlay describes a "Prehistoric Dwelling and Kitchen-midden on the Coast of Haddingtonshire," in which were found a very large quantity of rude pottery in fragments, and, for the most part, roughly moulded by the hands; a number of implements of bone, such as needles, arrowheads, combs, knives, chisels, &c.,

very similar to those found in the Swiss lakes; a vast quantity of bones of oxen (of several varieties, including *Bos longifrons*), sheep, goats, deer, swine, dogs, &c.; of shells a great abundance, especially those of the *Patella* and *Liottrina*; a very rude querne, &c. They are considered to belong to an age as remote as the Roman period, or perhaps extending even beyond it. Mr. J. Croll concludes his account of "The Boulder-clay of Caithness, a product of land-ice," and Mr. C. Lapworth his valuable paper "On the Silurian rocks of Galashiels." Mr. Croll considers that nearly the whole of the North Sea, between Scandinavia and Scotland, was filled with glacier ice at the time of the formation of the boulder-clay of Caithness; and he indicates facts in the glaciation of the Orkney, Shetland, and Faroe Islands which certainly go a long way in support of his views. It is also to this enormous extension of ice that he ascribes that of the Loess, by the daming up of the waters of the Rhine and Elbe. Mr. Croll's paper is illustrated with a map.

Annales de Chimie et de Physique. April, 1870.—This number contains a long and valuable paper by M. Berthelot on the Varieties of Carbon. He commences by pointing out the specific heats of five different kinds of carbon, and comparing them with the specific heat of the element as deduced from that of its gaseous compounds, and shows that the relations between specific heat and atomic weight are not the same as those observed in the case of other elements. He then explains his process for distinguishing and separating the varieties of carbon by treatment with nitric acid and potassic chlorate, and describes the products obtained from carbons of different origin, enumerating no less than thirteen different varieties of the element. The next two sections describe the effects of various agents on carbon, and the carbon obtained from different compounds; concluding with the observation that the kinds of carbon differ so widely in their properties, re-actions, and specific heats as almost to warrant their being considered as different elements. The four following papers are also by M. Berthelot. The first is on the oxidation of hydrocarbons by a strong solution of chromic acid, under which circumstances ethylene produces aldehyde, propylene gives acetone, amylene forms complex bodies, probably derived from an acetone, acetylene produces carbonic, formic, and acetic acids, whilst from camphene camphor is obtained. The second describes a new synthesis of phenol, by treating acetylene with fuming sulphuric acid, thus preparing acetyleno-sulphuric acid and decomposing the potassium salt with fusing potassic hydrate. It appears that at the moment of the liberation of the acetylene from the sulpho-salt three molecules condense and oxidise at the same time. The third paper is on the action of potassic hydrate on the sulphuric derivatives of the hydrocarbons, in which the author describes the products obtained from several of the sulpho-acids. He endeavoured to prepare methylene from sodic formeno-sulphate, but without success, and in consequence of the numerous experiments which he has made he thinks that chemists must give up hopes of its existence. The last paper is on a new synthesis of acetic acid by means of acetylene. The author has discovered several processes which effect this transformation. The last method is by digesting acetylenic dichloride with alcoholic or aqueous solution of potassic hydrate, when potassic acetate and chloride are formed. Acetylenic tetrachloride with alcoholic potash at 100° gives glycolic acid, and with aqueous potash at 230° oxalic acid.

SOCIETIES AND ACADEMIES

London

Royal Society, May 19.—"Spectroscopic Observations of the Sun." No. VI. By J. Norman Lockyer, F.R.S.

The weather has lately been fine enough and the sun high enough, during my available observation-time, to enable me to resume work. The crop of new facts is not very large, not so large as it would have been had I been working with a strip of the sun, say fifty miles or a hundred miles wide, instead of one considerably over 1,000—indeed, nearer 2,000 in width; but in addition to the new facts obtained, I have very largely strengthened my former observations, so that the many hours I have spent in watching phenomena, now perfectly familiar to me, have not been absolutely lost.

The negative results which Dr. Frankland and myself have obtained in our laboratory-work in the matter of the yellow bright line, near D, in the spectrum of the chromosphere being a hydrogen line, led me to make a special series of observations on that line, with a view of differentiating it, if possible, from the line C.

It had been remarked, some time ago, by Prof. Zollner, that the yellow line was often less high in a prominence than the C line; this, however, is no evidence (bearing in mind our results with regard to magnesium). The proofs I have now to lay before the Royal Society are of a different order, and are, I take it, conclusive:—

1. With a tangential slit I have seen the yellow line bright below the chromosphere, while the C line has been dark; the two lines being in the same field of view.

2. In the case of a bright prominence over a spot on the disc, the C and F lines have been seen bright, while the yellow line has been invisible.

3. In a high-pressure injection of hydrogen, the motion indicated by change of wave-length has been less in the case of the yellow line than in the case of C and F.

4. In a similar quiescent injection the pressure indicated has been less.

5. In one case the C line was seen long and unbroken, while the yellow line was equally long, but broken.

The circumstance that this line is so rarely seen dark upon the sun makes me suspect a connection between it and the line at 5015 Angström, which is also a bright line, and often is seen bright in the chromosphere, and then higher than the sodium and magnesium lines, when they are visible at the same time; and the question arises, must we not attribute these lines to a substance which exists at a higher temperature than those mixed with it, and to one of very great levity? for its absorption-line remains invisible, as a rule, in spot-spectra.

I have been able to make a series of observations on the fine spot which was visible when I commenced them on April 10th, not far from the centre of its path over the disc. At this time, the spot, as I judged by the almost entire absence of indications of general absorption in the penumbral regions, was shallow, and this has happened to many of the spots seen lately. A few hours' observation showed that it was getting deeper apparently, and that the umbræ were enlarging and increasing in number, as if a general down-sinking were taking place; but clouds came over, and the observations were interrupted.

By the next day (April 11) the spot had certainly developed, and now there was a magnificently bright prominence, completely over the darkest mass of umbra, the prominence being fed from the penumbra or very close to it, a fact indicated by greater brilliancy than in the bright C and F lines.

April 12. The prominence was persistent.

April 15. Spot nearing the limb, prominence still persistent over spot. At eleven I saw no prominence of importance on the limb, but about an hour afterwards I was absolutely startled by a prominence not, I think, depending upon the spot I have referred to, but certainly near it, more than 2′ high, showing a tremendous motion towards the eye. There were light clouds, which reflected to me the solar spectrum, and I therefore saw the black C line at the same time. The prominence C line (on which changes of wave-length are not so well visible as in the F line) was only coincident with the absorption-line for a few seconds of arc!

Ten minutes afterwards the thickness of the line towards the right was all the indication of motion I got. In another ten minutes the bright and dark lines were coincident.

And shortly afterwards what motion there was was towards the red!

I pointed out to the Royal Society, now more than a year ago,* that the largest prominences, as seen at any one time, are not necessarily those in which either the intensest action or the most rapid change is going on. From the observations made on this and the following day, I think that we may divide prominences into two classes:—

1. Those in which great action is going on, lower vapours being injected; in the majority of cases these are not high, they last only a short time—are throbs, and are often renewed, and are not seen so frequently near the sun's poles as near the equator. They often accompany spots, but are not limited to them. These are the intensely bright prominences of the American photographs.

2. Those which are perfectly tranquil, so far as wave-length evidence goes. They are often high, are persistent, and not very bright. These do not, as a rule, accompany spots. These

* Proc. Roy. Soc. 1869, p. 354, Mar. 17.

are the "radiance" and dull prominences shown in the American photographs.

I now return to my observations of the spot. On the 16th, the last of the many umbræ was close to the limb, and the most violent action was indicated occasionally. I was working with the C line, and certainly never saw such rapid changes of wave-length before. The motion was chiefly horizontal, or nearly so, and this was probably the reason why, in spite of the great action, the prominences, three or four of which were shut out, never rose very high.

I append some drawings, made, at my request, by an artist, Mr. Holiday, who happened to be with me, and who had never seen my instrument or the solar spectrum widely dispersed before. I attach great importance to them, as they are the untrained observations of a keen judge of form.

The appearances were at times extraordinary and new to me. The hydrogen shot out rapidly, scintillating as it went, and suddenly here and there the bright line, broad and badly defined, would be pierced, as it were, by a line of intensely brilliant light parallel to the length of the spectrum, and at times the whole prominence spectrum was built up of bright lines so arranged, indicating that the prominence itself was built up of single discharges, shot out from the region near the limb with a velocity sometimes amounting to 100 miles a second. After this had gone on for a time, the prominence mounted, and the cyclonic motion became evident; for away from the sun, as shown in my sketch, the separate masses were travelling away from the eye; then gradually a background of less luminous hydrogen was formed, moving with various velocities, and on this background the separate "bombs" appeared (I was working with a vertical spectrum) like exquisitely jewelled ear-rings.

It soon became evident that the region of the chromosphere just behind that in which the prominence arose, was being driven back with a velocity something like twenty miles a second, the back-rush being so local that, with the small image I am unfortunately compelled to use, both the moving and rigid portions were included in the thickness of the slit. I saw the two absorption-lines overlap.

These observations were of great importance to me; for the rapid action enabled me to put together several phenomena I was perfectly familiar with separately, and see their connected meaning.

They may be summarised as follows, and it will be seen that they teach us much concerning the nature of prominences. When the air is perfectly tranquil in the neighbourhood of a large spot, or, indeed, generally in any part of the disc, we see absorption-lines running along the whole length of the spectrum, crossing the Frauenhofer lines, and they vary in depth of shade and breadth according as we have pore, corrugation, or spot under the corresponding part of the slit—a pore, in fact, is a spot. Here and there, where the spectrum is brightest (where a bright point of facula is under the slit) we suddenly see this appearance—an interesting bright lozenge of light. This I take to be due to bright hydrogen at a greater pressure than ordinary, and this then is the reason of the intensely bright points seen in ranges of faculæ observed near the limb.

The appearance of this lozenge in the spectroscope, which indicates a diminution of pressure round its central portion, is the signal for some, and often all, of the following phenomena:—

1. A thinning and strange variation in the visibility and thickness of the hydrogen absorption-line under observation.

2. The appearance of other lozenges in the same locality.

3. The more or less decided formation of a bright prominence on the disk.

4. If near the limb, this prominence may extend beyond it, and its motion-form will then become more easy of observation. In such cases the motion is cyclonic in the majority of cases, and generally very rapid, and—another feature of a solar storm—the photospheric vapours are torn up with the intensely bright hydrogen, the number of bright lines visible determining the depth from which the vapours are torn, and varying almost directly with the amount of motion indicated.

Here, then, we have, I think, the chain that connects the prominences with the brighter points of the faculæ.

These lozenge-shaped appearances, which were observed close to the spot on the 16th, were accompanied by the "throbs" of the eruption, to which I have before referred; while Mr. Holiday was with me—a space of two hours—there were two outbursts, separated by a space of almost rest, and each outburst consisting of a series of discharges, as I have shown. I subsequently witnessed a third outburst. The phenomena observed on all three were the same in kind.

On this day I was so anxious to watch the various motion-forms of the hydrogen-lines, that I did not use the tangential slit. This I did the next day (the 17th of April) in the same region, when similar eruptions were visible, though the spot was no longer visible.

Judge of my surprise and delight, when upon sweeping along the spectrum, I found HUNDREDS of the Frauenhofer lines beautifully bright at the base of the prominence!!!

The complication of the chromosphere spectrum was greatest in the regions more refrangible than C, from E to long past *b*, and near F, and high pressure iron vapour was one of the chief causes of the phenomenon.

I have before stated to the Royal Society that I have seen the chromosphere full of lines; but the fulness then was as emptiness compared with the observation to which I now refer.

A more convincing proof of the theory of the solar constitution, put forward by Dr. Frankland and myself, could scarcely have been furnished. This observation not only endorses all my former work in this direction, but it tends to show the shallowness of the region on which many of the more important solar phenomena take place, as well as its exact locality.

The appearance of the F line, with a tangential slit at the base of the prominence, included two of the lozenge-shaped brilliant spots to which I have before referred; they were more elongated than usual—an effect of pressure, I hold, greater pressure and therefore greater complication of the chromosphere spectrum; this complication is almost impossible of observation on the disc.

It is noteworthy that in another prominence, on the same side of the sun, although the action was great, the erupted materials were simple, *i.e.*, only sodium and magnesium, and that a moderate alteration of wave-length in these vapours was obvious. Besides these observations on the 17th, I also availed myself of the pureness of the air to examine telescopically the two spots on the disc, which the spectroscope reported tranquil as to up and down rushes. I saw every cloud-dome in their neighbourhood perfectly, and I saw these domes drawn out, by horizontal currents, doubtless, in the penumbræ, while on the floors of the spots, here and there, were similar cloud-masses, the distribution of which varied from time to time, the spectrum of these masses resembling that of their fellows on the general surface of the sun.

I have before stated that the region of a spot comprised by the penumbræ appears to be shallower in the spots I have observed lately (we are now nearing the maximum period of sun spots); I have further to remark that I have evidence that the chromosphere is also shallower than it was in 1868.

I am now making special observations on these two points, as I consider that many important conclusions may be drawn from them.

Zoological Society, May 26.—G. R. Waterhouse, V.P., in the chair. A letter was read from Mr. W. H. Hudson, C.M.Z.S., containing remarks on birds observed in and around Buenos Ayres, being the fourth communication received from this gentleman on the same subject.—Mr. R. B. Sharpe exhibited and made remarks on a specimen of a rare Asiatic bird, *Podoces panderi*, from the collection of Lord Lilford.—Professor Owen read the sixteenth of a series of memoirs on *Dinornis*, containing an account of the trachea and of some other internal organs of certain species of this genus, together with a description of the brain and some nerves and muscles of the head of *Apteryx australis*.—Dr. J. Murie read a paper on the anatomy of the Prongbuck (*Antilocapra americana*), founded on the examination of the specimen of this animal which had died in the Society's Gardens in the year 1867.—A communication was read from Dr. A. B. Meyer, containing remarks on the poisonous glands of the snakes of the genus *Callophis*, being supplementary to his paper on the same subject published in the *Monatsberichte* of the Academy of Berlin.—A communication was read from Surgeon Francis Day, F.Z.S., containing notes on some fishes from the western coast of India, amongst which were several species described as new to science.—Mr. H. Adams communicated the descriptions of some new species of land and fresh-water shells obtained by Mr. E. Bartlett in Eastern Peru, and by Mr. R. Swinhoe in China and Formosa. Mr. Adams also described two new species of land shells from Africa.

Aeronautical Society, June 3.—Mr. James Glaisher, F.R.S., in the chair. Several papers were read and illustrated by models. An instrument for ascertaining the connection between velocity and pressure, exhibited by Dr. Smyth, of Maidstone, procured the encomiums of the chairman, who stated that it was calculated to supply information that was wanting in the instrument which he had been using. In the course of discussion, Mr. A. Stewart Harrison strongly advocated the necessity of an experimental fund, whereupon the chairman asked Mr. Fred. W. Brearey, the honorary secretary, to read the minutes of a late council meeting at Stafford House, at which were present Mr. Glaisher (in the chair), Sir William Fairbairn, Bart., Mr. Brook, Mr. Wright, Mr. Wenham, Mr. Oheen, and Mr. Brearey; as follows:—Sir William Fairbairn observed that we know but little of the re-action or lifting power of various forms of screw blades in the atmosphere relative to the force employed, though such experiments might be easily tried and the data obtained. Mr. Brook was of opinion that if a successful aerial machine was to be constructed, the most simple and obvious plan was that of inclined surfaces impelled forward horizontally. The most successful experiment that he had ever witnessed was upon this principle, the motive power being a wound-up clock spring, which, as long as the power lasted, sustained the machine, and further that most large birds were capable during long periods of their flight of sustaining themselves exactly in this way. It was further remarked that we were practically ignorant of the correct laws of the sustaining power of inclined surfaces of different forms and areas, and this want of knowledge was a perpetual stumblingblock to those who were wishing to spend time and money on experiments. From the fact that, as the weight and size of birds increased, so did the relative wing area decrease, it would appear that the ratio of sustaining surface to weight or resistance was by no means in equal proportion. The Chairman stated that with respect to plane surfaces of various figures exposed to the direct impact of the wind, he had already been trying some experiments with such instruments as were at his disposal, and that by employing two anemometers at the same time, so as to be sure of comparative results, he found that the indication of force increased with the size of the surface; also in the two instruments, equal surfaces shaped into different contours gave different results. These interesting experiments, so directly bearing upon the question of aerial propulsion and resistance, were still occupying his attention; but at present he could tell us nothing from actual experiment of the resistance of inclined surfaces of various forms. It was then proposed that an experimental fund should be raised by subscription, and that a suitable and well finished anemometer should be constructed, having the means of instantly setting various plane surfaces at any desired angle, and capable of registering both horizontal and vertical force simultaneously for all degrees of inclination. The results to be published for the benefit of the Society." Upon this proposition being put to the meeting, it was carried unanimously.

Ethnological Society, June 1.—Special Sectional Meeting at the Royal United Service Institute. Prof. Huxley, F.R.S. president, in the chair.—A letter was read from Lieut. Oliver, relative to the recent destruction of a cromlech in Jersey.—Col. Lane Fox read communications from Dr. Caulfield, "On the discovery of copper celts near Buttevant, Co. Cork;" and on a supposed Ogham inscription from Rus-glass, Co. Cork.—Mr. C. Spence Bate, F.R.S., then presented an elaborate "Report on the prehistoric antiquities of Dartmoor," forming one of the series of reports being now collected by the Society, with a view to obtaining accurate information on the present condition of the megalithic monuments of this country. After noticing the physical features of the district, the author described in detail the numerous stone circles, avenues, menhirs, cromlechs, cairns, and other prehistoric monuments of Dartmoor. He suggested the idea that the stones in some of the avenues may have been erected in commemoration of the death of persons of distinction, one being added for each burial. The depressions on the summit of some of the cairns were regarded rather as indications of unfinished work than of subsequent disturbance by treasure-seekers. Evidence was adduced of the wanton destruction of the granite blocks in some of the cromlechs; and both in the paper and in the subsequent discussion attention was forcibly directed to the importance of obtaining legislative protection to these prehistoric monuments. A large series of diagrams, plans, coloured sketches, photographs, and specimens illustrated this valuable communication. The discussion was sustained by Mr. W. Morrison, M.P., Mr. Moggridge, Mr. Hyde Clarke, Mr. Black, Rev. G. St.-Clair, Dr. Campbell, and Mr. Lewis.

Chemical Society, June 2.—Prof. Williamson, F.R.S., president, in the chair. Mr. W. B. Tustin was elected a Fellow. —Prof. Odling, F.R.S., delivered a lecture "On the Platinum Ammonia compounds." Platinum combines with chlorine in two proportions, viz., with four atoms of chlorine to form platinic chloride, $Pt\ Cl_4$, and with two atoms of chlorine to form platinous chloride, $Pt\ Cl_2$. All the platinum ammonia compounds are produced in first instance from the plantinous chloride, none of them directly from the platinic chloride. If this is borne in mind, a great simplicity will at once be introduced into the study of the platinum ammonias. Next, the lecturer proceeded to show the manifestations of the atomicities of nitrogen, boron, silicon, and platinum as illustrated in the compounds $NH_3\ HCl$, $BF_3\ KF$, $SiF_4\ K_2\ F_2$, and $Pt\ Cl_4\ K_2Cl_2$, which four compounds are decidedly analogous, and deduced from this as well as from many other evidences the necessity of writing the formula of sal-ammoniac $NH_3\ HCl$, and not $NH_4\ Cl$. Having established this, the metallic derivatives of sal-ammoniac were ranged into two sets—those in which the metal can be detected by the usual tests, and those in which this cannot be done; the first may be typified by the formula NH_3 m Cl, the second by NH_2 m HCl. [The small letter indicates that quantity of a metal which occupies the place of one atom of hydrogen.] The lecturer then extended his comparison of the manifestations of the pentad nature or the pendacity of nitrogen with those of the tetradicity of carbon, and concluded this kind of preface by reminding of the necessity of studying mineral chemistry in the light of organic chemistry. He then gave a short history of the platinum ammonia compounds, beginning with the so-called green salt of Magnus discovered in 1828, mentioning the salts prepared and described by Gros, Reiset, Peyronne, and others, and finally by stating Laurent and Gerhardt's latest arrangements for classifying these compounds. That classification is now by no means satisfactory, and Dr. Odling hopes to bring forward at some future meeting his own views on this subject.

Anthropological Society, May 31.—Dr. R. S. Charnock, vice-president, in the chair. Mr. George Thorne Ricketts, Her Majesty's consul, Manila, was elected a Fellow.

A paper by Dr. John Shortt was read on "The Armenians of Southern India." Early in the sixteenth century a few Armenians found their way into Southern India with the countenance and support of the Hon. East India Company, and under a contract with the company equal privileges with British subjects were conceded to the Armenians. The company further extended favours to them when they reached, in any town, the number of forty, by the provision of a place of worship, and by annual grants of money. For a long time after their arrival in India they avoided mingling with other people, but latterly that rule has been broken through, and alliances in marriage with Europeans are not unfrequent. The Armenians have diminished in numbers, and, it is said, are daily decreasing in influence. The chief causes of their approaching extinction in India appear to be the vice of intemperance, the taint of disease, and the contact with Europeans, more especially the English. The physical and moral characteristics were described; in the former it was stated that the Armenians are strongly allied to the Jewish race, from which they claim descent.

A paper by Mr. John Stirling, M.A., was read on "The Races of Morocco." The inhabitants of that part of Barbary known as Morocco may be arranged under the following heads, viz., Berbers, Al-Ryf, Arabs, Bohára troops, and other Negroes or half-breeds, and Jews. The word *Kabyles* often erroneously used to designate some North African race, is a term applied by the Moors to distinguish villagers or country people engaged in agriculture. The Berbers are probably of Phœnician blood mixed with an old race indigenous to Morocco; at any rate, the remains of dolmens and other monuments would point to that origin. The Al-Ryf are wild descendants of the Ryf pirates, the inhabitants of the northern spurs of the Atlas range which separates Morocco from Algeria. The Al-Ryf are comparatively a fair people, and such of them as follow in-door occupations exhibit a delicate olive complexion. The Bohára troop form a remarkable race; their ancestors were a rebellious Negro tribe living south of the Atlas. They have married with Moorish women, but still retain to a great degree the Negro aspect. The Negroes of Morocco have intermarried with the fair Moors, and produced a mixed race. The true Moors are fair, some individuals having blue eyes and light or red hair.

Sir Duncan Gibb, Bart., M.D., in a paper "On the Paucity of Aboriginal Monuments in Canada," attributed the absence in

Canada of monuments built of stone to the peculiar character of the climate, which would be unfavourable to their preservation. He anticipated the discovery some day of traces of the ancient inhabitants in the great caverns north of Flamborough, and in the island of Anticosti.

Institution of Civil Engineers, May 17.—Charles B. Vignoles, F.R.S., president, in the chair. The paper read was "On recent improvements in regenerative hot-blast stoves for blast furnaces," by Mr. E. A. Cowper, M. Inst. C.E. The author stated that when, in 1828, the late Mr. J. B. Neilson, M. Inst. C.E., introduced the plan of heating the air employed as blast, by means of iron pipes placed in or near a fire, the increase of temperature was at first only from 60° to 100° Fahr. Subsequently, Mr. Neilson obtained a temperature of 600° or 650° and the pipe stoves had since been urged up to 900°, and in a few cases to 1·00°. The wear and tear, however, with such temperatures of blast were considerable; there was great loss of heat by conduction, and the pipe stoves were, as a rule, worked in a leaky condition, necessitating the expenditure of engine power for blowing air uselessly. The improvements described in the paper were based upon Mr. Siemens' regenerative furnace. Each stove of a pair consisted of a wrought-iron cylindrical casing, lined with fire-brick, and provided with a central shaft or flue, which extended to within a few feet of the brick dome forming the top. Around this shaft there were a number of compartments, or boxes, formed of bricks so placed that those in one course were not exactly coincident in position with those in the courses either above or below, though a passage was left open from the bottom to the top of the mass of brickwork. This wrought-iron casing was provided with several valves, three being for the cold blast, of gas, and of air for combustion, and two being for the exit of the hot blast and of the products of combustion. When a stove had been at work heating blast, and it was wanted to reheat it, the first thing to be done was to put another stove in operation, then to shut the hot and the cold blast valves, allowing the air in the stove to be blown out at a small valve to reduce it to atmospheric pressure. The gas, air, and chimney valves were next opened, and the gas, igniting as it entered, gave a large volume of flame right up the central shaft and over and into the regenerator, thus heating the top course of brickwork considerably, the next course rather less, and so on, the products of combustion passing away to the chimney at a temperature of about 300°. In the course of a few hours a large amount of caloric was stored up in the bricks forming the regenerator, a good red heat penetrating nearly to the bottom, when the stove was again ready to heat the blast to a temperature of 1,400° or 1,500°. In these stoves the cost of dust catchers was avoided, and the expense of producing gas was also saved, as the gas was used direct from the top of the blast furnace, and the stoves could be cleaned out with the greatest facility. The construction of the regenerator in compartments or boxes, connected together vertically but not horizontally, gave the power of applying the blast with efficiency (inasmuch as the whole force of the blast was confined to the one passage that was being blown at the time), and admitted of a brush being passed up or down the boxes to remove the dust. The form and proportion of the passages had been found, after numerous experiments, to produce an excellent effect in mixing the air, thereby ensuring a rapid and perfect conduction of heat from the bricks to the air, or *vice versa*, from the products of combustion to the bricks. The results obtained by Messrs. Cochrane from the adoption of these stoves at Ormesby, as regarded the quality of iron, the increased make, and the saving of coke in the blast furnace, had been most satisfactory. Thus there was a saving of 4 cwt. of coke per ton of iron produced, by the use of the regenerative stoves for heating the blast, when compared with good cast-iron pipe stoves, and the saving was still more over ordinary pipe stoves. With a large furnace, producing 475 tons a week, the first cost of these stoves was somewhat less than the cost of pipe stoves, while the expense of working was less, so that the profit, taking everything into account, was estimated to amount to about 4,162*l*. a year.

Cambridge

Philosophical Society, May 16.—Professor W. Cayley, president, in the chair. The following communications were made to the Society:—

(1.) By Mr. Sedley Taylor (Trinity). Mr. Taylor described the nature and classification of musical sounds, their laws and connection, and illustrated experimentally the subject of "beats," and explained at some length, with illustrations, Helmholtz's theory of harmony. The rest of the paper was devoted to a criticism of a theory of consonance given by Professor Tyndall in his published lectures on sound. This theory, he maintained, was, while professing to be that of Helmholtz, a totally different one, in flat contradiction to the facts of experience, and in reality wholly erroneous.

(2.) On a case of asymmetry in the human body, by Professor Humphry. The subject of this paper was a female patient in Addenhook's Hospital, at Cambridge. The asymmetry was very marked, carried out through the whole body; the right arm, for example, being more fully developed, and 2½ inches longer than the left, and extending even to the mammary glands, teeth, tonsils, &c. The subject had always enjoyed good health, was strong, and fully developed. No paralysis had ever been produced by this asymmetry, as had been the case in the instance mentioned by Van der Kolb, a cast of the brain of which was exhibited. Professor Humphry expressed himself wholly unable to account for this instance. As the person was alive and well, of course examination of the internal organs was impossible.

Paris

Academy of Sciences, June 6.—A considerable number of works and memoirs were received in candidature for various prizes in the gift of the Academy.—M. Chasles presented a note by M. Mannheim, "On the determination of the osculatory plane and radius of curvature of the trajectory of some point in a straight line, which is displaced under certain conditions;" and also a note by MM. F. Klein and S. Lie, "On a certain family of curves and surfaces."—A note was read by M. Des Cloiseaux, "On the optical properties of benzile, and of some bodies of the camphor family, in the crystallised state and in solution." The author has found that crystals of benzile rotated the plane of polarisation in different ways, and the right and left crystals, when dissolved and crystallised two or three times, likewise gave a mixture of crystals with opposite rotations. Solution of benzile in ether has no action on polarised light. Benzile thus possesses optical properties similar to those of periodate of soda. Common camphor in solution deviates the plane of polarisation, whilst its crystals have no action upon polarised light. Camphor of patchouli and mint camphor (menthole), both belonging to the hexagonal system, have a negative, uniaxial, double refraction, and their solutions in alcohol deviate the plane of polarisation to the left. Three camphors belonging to the cubic system, namely, Bornean camphor, terecamphene, and monohydrochlorate of turpentine, have no action on polarised light when crystallised, but in solution strongly deviate the plane of polarisation, the first to the right, the other two to the left.—M. Duchemin described a marine galvanic battery, set in action by contact with sea water. It consists of a thick cylinder of zinc, pierced with holes, and containing a porous vessel, enclosing a carbon element surrounded by powdered coke and perchloride of iron.—An extract of a letter from Father Secchi to M. Fizeau, "On the displacement of the lines observed in the solar spectrum," was read. M. Le Verrier communicated an extract from a letter of M. Winnecke announcing his discovery of a new telescopic comet on the night of the 29-30th May, at Carlsruhe.—Mr. H. Sainte-Claire Deville communicated a second memoir on the action of water on iron and of hydrogen on oxide of iron. In this paper he described his experiments with iron at temperatures of 150°, 265°, 440°, 860°, and 1040° C. (= 302°, 509°, 824°, 1580°, and 1904° F.) and with aqueous vapour at constant and varying tensions.—A paper was read by M. E. Frémy on the reduction of nitrous acid by metals, containing the continuation of his researches on this subject brought under the notice of the Academy on the 10th January (see NATURE, No. 12). He identified the body obtained by the reduction of nitrous acid and the nitrites with the oxy-ammonia of M. Lossen, NH^3O^2 (or NH^2O, HO) and stated that it possessed marked basic properties, and is strongly reductive.—M. de Quatrefages presented a note by M. E. Perrier on the circulation of the Oligochæta of the group Naïdæ, which he described from researches made upon *Dero obtusa*. The circulatory apparatus in this worm consists, according to the author, of a dorsal and a ventral vessel, united by a most complex vascular apparatus which varies considerably in its structure in different regions of the body.—A note by M. Feltz, on the phenomena of which the white globules of the blood, and the walls of the capillary vessels, are the seat during inflammation, was communicated by M. C. Robin.—M. R. Wolf suggested that instead of applying the decimal mode of division to the quadrant of the circle and the quarter of the day, as proposed by M. D'Abbadie, the whole circle and the whole day should be taken as the unit of division. M. D'Abbadie remarked that the quadrant is the natural unit which has always been adopted for trigonometrical purposes, and indicated some practical inconveniences which would result from a change. Of the remaining communications only the titles are given.

BOSTON

Natural History Society, *Section of Microscopy*, Feb. 9.—Dr. B. Joy Jeffries in the chair. The following paper was read:—"Notes on Diatomaceæ," by Professor Arthur Mead Edwards. "A few days since (Sept. 1869) I made a gathering in a ditch communicating with the salt water of the Hudson River, opposite the city of New York, at Weehawken, N. J. Of course the water in the ditch was salt, and, in fact, in it last spring I had caught specimens of Stickleback (*Gasterosteus*) which had come up there from the river to spawn, as is their wont to do. The Ten-spined Stickleback (*G. pungitius*) I had found very plentiful, and mixed with it a few individuals of the Three-spined (*G. aculeatus*); in fact these fish occurred in such numbers that when the water became foul, as it did by evaporation, the bottom of the ditch was literally covered with their dead bodies. The gathering, however, I have to speak of at the present time was made for the purpose of procuring Diatomaceæ, and consisted of specimens of an alga belonging to the genus *Enteromorpha*, having attached to it more or less firmly numerous Diatomaceæ and animals. The commonest form of Diatom was a *Cyclotella*, and seemingly fixed in some manner to the *Enteromorpha*, for it was not shaken off by pretty rough usage. How it was fixed I could not detect; most likely by means of a mucous envelope of such tenuity that it is not readily seen. The next most common form is the truly wonderful, inexplicable *Bacillaria paradoxa*, the paradoxical bundle of sticks. Often and often have I spent hours looking at this marvel of nature; the motion without apparent cause or mode, an invisible joint which, as a friend of mine, an engineer, once remarked, would be a fortune to any one who would discover it, for here we have several sticks forming the bundle, moving over each other without separating, and yet the use of the highest powers of the microscope has failed to detect the means of their union into one mass or composite group of individuals. This grouping of individuals together, which we so commonly find among the Diatomaceæ, as in *Schizonema*, *Achnanthes*, *Melosira*, and a host of other genera, appears to me to have its analogue in the animal kingdom in the Polyzoa; which, although generally fixed, yet at certain periods throw off motile forms by means of which the species is distributed. Do not the Diatomaceæ do likewise? I am of opinion that they do, and I shall produce evidence on that point further on. As to the *Bacillaria paradoxa*, the oftener I watch it the more it puzzles me. Not long since I saw one specimen (of course I mean one bundle of individuals) slide out to its utmost limit across the field of view, and then, becoming entangled with two others, which likewise were made up of many individuals, some eight or ten of its frustules (as the complete individuals are called) were twisted around almost off from the rest, so as to lie at right angles to them, and when the group containing the largest number of frustules receded to their former position, which they soon did, the eight or ten seeming by the act of twisting to lose their power of motion among themselves for the time being, were dragged along in a helpless condition, and twisted completely around one revolution, so as thereafter to fall back again into their places, when all went on as usual. That is to say, the regular motion of all the frustules over each other succeeded. Now what kind of a joint can it be that permits of such eccentric movement? As I have already said, I am more puzzled than ever. Along with the *Bacillaria* in the brackish water at Hoboken, I found numerous individuals of an *Amphora*, which I have known in this neighbourhood for many years, and which I considered unnamed as yet. To it I have given the provisional name of *A. lanceolata*, on account of the form of its outline. This genus has always been considered an epiphytaceous one; that is to say, one which grows attached to other plants or submerged substances, yet this form was free and in active motion. In fact I think it was one of the most lively Diatoms I ever saw. So another small species of *Amphora* which is common near here, is always, as far as I have noticed, free. Here we have species appearing both in the free and attached conditions, and this is even more strikingly illustrated in *Schizonema*. *Bacillaria paradoxa* is usually set down as the most rapid in motion of the Diatomaceæ, its velocity being recorded by Smith, as he measured it, at over one two-hundredth of an inch in a second. This is certainly pretty quick when we consider that the length of the frustule is only ·0025 of an inch. But my experience has been that its velocity varies in every degree from that mentioned to perfect rest; at times some individuals will be in rapid movement, while others are motionless; and also I have remarked that from sunrise to noon seems to be the period during which, under ordinary conditions, the movement is most active, while during the afternoon it is very sluggish, and at night almost *nil*. This *Amphora*, as I saw it at the time mentioned, was moving even more rapidly than I ever saw a *Bacillaria* move, and that with a steady onward progression very different from that of most naviculiform diatoms. Many months since I mentioned at one of the meetings of the Lyceum of Natural History in New York, that I had seen two apparently different genera of Diatoms existing within the same investing tube; and now I wish to place that fact upon record, and to state one or two more instances of the same mode of growth. During the month of March 1868, I found in the harbour of New York specimens of *Schizonema Grevillei* in active motion within their investing tubes, but accompanied by a much smaller form possessing a totally different outline from *S. Grevillei*, being blunter at the ends, and with parallel sides. During the same month, and also in April, I found this mode of occurrence very common, and also *Schizonema Grevillei* and a *Homœocladia* in the same tube, and *Schizonema cruciger* and the small form mentioned above, both in the same tube, and *S. cruciger* and *Grevillei* in the same tube. In all these cases the frustules were in lively motion, passing over each other from one end to the other of the tube. In May of the present year, 1869, I found growing in the salt water of the 'Mill pond' at Salem, Mass., *Schizonema cruciger* and *Nitzschia closterium*, W. S. (*Ceratoneis closterium*, C. G. E., and *Nitzschiella closterium*, L. R.), both in the same tube. And here it will be necessary to say something in regard to the form I have called *Nitzschia closterium*, as I shall thereby, I hope, be enabled to clear away a little fog of synonyms. Neither Smith, Kützing, nor Rabenhorst describes or figures any species living within a tube like *Schizonema*, the frustules of which have an outline and markings similar to *Nitzschia closterium*, so that it is not likely that they ever saw anything but the free form or condition of this species. However, Ehrenberg figures and describes, under the designation of *Schizonema? Agardhii* (*Die Infusionsthierchen*, 1838, p. 343, t. xx. fig. xvi.), a form agreeing with this, but the structure of the frustule is that of *Nitzschiella* of Rabenhorst, so that the specific name of this species should be *Agardhii*, whatever its genus be decided to be hereafter."

March 2.—Dr. C. T. Jackson, vice-president, in the chair. The following papers were presented:—"Description of the Larva and Chrysalis of *Papilio Rutulus* Boisd., of California," by Samuel H. Scudder.—"On the Phosphate Beds of South Carolina," by N. S. Shaler. The latter contains a partial account of the observations made upon this district by the author, while in the employ of the United States Coast Survey, and is published with the permission of the Superintendent of the Survey, Prof. Benj. Pierce, of Cambridge. A portion of the conclusions have a certain commercial as well as scientific value, and it was deemed by the Superintendent desirable to place them before the public at the earliest opportunity. The remainder of the description of these beds will be found in the report of the work of the Coast Survey for 1870.

March 16.—Mr. William T. Brigham in the chair. The following papers were presented:—"Note on the Occurrence of *Euleptorhamphus longirostris* on the Coast of Massachusetts," by F. W. Putnam.—"Revision of the Classification of the Mollusca of Massachusetts," by W. H. Dall, Smithsonian Institution.

NEW YORK

Lyceum of Natural History, *Chemical Section*, May 9.—Prof. Charles A. Seely "On the Constitution of Ammoniacal Amalgam." Ammonium amalgam was discovered by Berzelius in 1808; and immediately after by Pontin, Seebeck, and Trommsdorff. Its easy preparation, singular properties, and its relation to current theories, have made it familiar to all chemists. It led to the adoption of Ampere's ammonium theory, gave a great impetus to the theory of organic radicals, and revived the notions of the alchemists of the compound nature of metals. Early in this century it led many to conclude that the base of nitrogen is a metal; and at the present time, without doubt, has assisted in giving currency to the notion that hydrogen is a metal. Except for it, perhaps the crudity, hydrogenium, would not have been inflicted upon us. Of course it has occupied a conspicuous place in chemical literature; scoresof papers, and at least two books, have been printed about it. The name ammonium amalgam expresses the supposed constitution of the substance; the radical ammonium is represented as dissolved in or united with mercury. The ammonium is, moreover, conceded to be a solid or a liquid, and to have a truly metallic character. Thus the latest and best authorities present the case. It is described, in nearly all treatises on chemistry, as if its constitution was as certainly ascertained as that of common salt. There have been from the beginning, however, those who doubted the prevailing ideas, and some (see Daniel's "Chemical Philosophy," p. 520, and Dr. Wetherill on Ammonium Amalgam, in *Silliman's Journal*, vol. x., p. 160) boldly objected to them, but the reasons they alleged had not sufficient weight. Ammonium amalgam has always been a pet with chemists; it has always been ready for the service of one theory or another. The ammonium theory, the radical theory, the nitrogen and hydrogenium theories, have each in their turn been of too much importance to permit any of their props to be withdrawn.

The author considers the so-called ammonium amalgam to be a mechanical or physical mixture of liquid mercury with the gases ammonia and hydrogen, and that its semi-solid consistence is due to the mixture having the nature of a froth. When sodium-amalgam is brought into a solution of sal-ammoniac (the ordinary method of preparing ammonium amalgam) the chlorine combines with the sodium, and the residue ($NH_3 + H$) of the sal-ammoniac is set free all over the surface of the mercury. The particles of the mixed gases adhere to the mercury, and by reason of the movement bringing to the surface fresh mercury, they become enfilmed and carried inward, until the mixture becomes a homogeneous froth. The principal considerations by which this view of the constitution of ammonium amalgam has been reached, are as follows:—

1. The volume of ammonium amalgam is inexplicable in any other way; it is utterly inconsistent with the well-established laws of combinations by volume. There is no case of a liquid or solid chemical compound, or amalgam, which has any analogy to it.

2. Mercury has a mirror-like surface, while ammonium amalgam has comparatively a whiter and more dead surface; it approaches in appearance to matt silver. Such changes are characteristic of froths.

3. If ammonium amalgam be subjected to varying pressure, its volume changes apparently in accordance with Mariotte's law of gaseous volume. To illustrate this important fact, a glass tube one-third inch in diameter, twenty inches long, and fitted with a plunger, was employed. Mercury containing a little sodium was poured into the tube to one-third of an inch in depth, and upon this was poured a strong solution of chloride of ammonium, occupying about two inches in length of the tube. The ammonium amalgam was completely formed in a few minutes, and occupied several inches of the tube. On adjusting and depressing the plunger, the volume of the amalgam progressively diminished till it closely approached the original volume of the amalgam. Also it was notable that the amalgam progressively gained fluidity and the mirror surface, till at the greatest pressure it appeared like mercury. On withdrawing the pressure the original volume and appearance of the compound were resumed, and on reducing the pressure below that of the air, the amalgam still expanded, until it rose above the surface of the liquid in the tube. If the great pressure be maintained, more ammonium amalgam will be formed, the mass expanding progressively, apparently in accordance with the fact that the absorption or adhesion of gases to liquids is favoured by pressure. By means of the simple apparatus used a pressure of ten atmospheres, or a good vacuum, is easily and at once attainable, and the experiments with it are very striking.

The so-called ammonium amalgam is therefore not an amalgam at all; ammonium is not proved to be a metal, and if it be admitted that the monatomic radical really exists in ammonium amalgam, it is neither a solid nor a liquid, but a gas.

The considerations regarding ammonium amalgam are evidently equally applicable to Loew's hydrogenium amalgam; both are only metallic froths. The expansion of palladium observed by Graham, on its absorption of hydrogen, is probably analogous to the case in question. In both cases the gases concerned are condensed by reason of their attraction to the metal; and if the molecules of palladium were made free to move, as those of mercury, it is probable that Graham's hydrogenium alloy would become a palladic froth, more remarkable than the corresponding mercuric froth. Many have erroneously supposed that hydrogen was conspicuous in its capability of being absorbed by metals, and thus have more readily been infused with the hydrogenium theory. Oxygen has an eminence over hydrogen in that property, and yet no one has a theory of oxygenium. Iron absorbs carbonic oxide, but no one is bold enough to suggest that carbonic oxide is a metal.

DIARY

THURSDAY, JUNE 16.

ROYAL SOCIETY, at 8.30.—Papers to be read by Dr. Hofmann, F.R.S.: Dr. H. E. Armstrong, Dr. Alex. Rattray, Prof. Macalister, C. Tomlinson, F.R.S., W. Huggins, F.R.S., Sir Edward Sabine, P.R.S., the Earl of Rosse, F.R.S., Dr. Stenhouse, F.R.S., G. Busk, F.R.S., the Hon. J. W. Strutt, Mr. J. Broughton, Mr. A. Le Sueur, and W. H. L. Russell, F.R.S.

ROYAL SOCIETY OF ANTIQUARIES, at 8.30.—On Heydon Church, Yorkshire: Mr. G. E. Street.

LINNEAN SOCIETY, at 8.—On two Species of Serapias which occasionally present semi-labelliform lateral sepals: Mr. J. T. Moggridge.

CHEMICAL SOCIETY, at 8.

NUMISMATIC SOCIETY, at 7.—Anniversary Meeting.

SUNDAY, JUNE 19.

SUNDAY LECTURE SOCIETY, at 8.—On Volcanoes: Mr. D. Forbes.

MONDAY, JUNE 20.

LONDON INSTITUTION, at 4.—Botany: Prof. Bentley.

TUESDAY, JUNE 21.

STATISTICAL SOCIETY, at 8.—On Free Libraries: Mr. W. E. A. Axon.

ETHNOLOGICAL SOCIETY, at 8.30 (at the Royal United Service Institution, Whitehall Yard).—On the Aymara Indians of Bolivia and Peru: Mr. D. Forbes.

WEDNESDAY, JUNE 22.

GEOLOGICAL SOCIETY, at 8.

THURSDAY, JUNE 23.

ZOOLOGICAL SOCIETY, at 8.30.—On the Walrus: Dr. J. MURIE.—Catalogue of the Mammals of South China and Formosa: Mr. R. SWINHOE.—On a Collection of Birds from the Island of Trinidad: Dr. O. FINSCH.

BOOKS RECEIVED

ENGLISH.—On Diamagnetism and Magne-Crystallic Action: Prof. Tyndall (Longmans and Co.).—Notes on Light: Prof. Tyndall (Longmans and Co.).—Grave-mounds and their Contents: L. Jewitt (Groombridge and Sons).—Gymnastics for Ladies: Madame Brenner.—First Principles of Chemical Philosophy: J. P. Cooke, jun. (Macmillans).

FOREIGN.—(Through Williams and Norgate).—Annales de Chimie et de Physique: Chevreul et Dumas, Tome xx.—Zeitschrift für Ethnologie, 1870, Heft II.—Ueber die Entstehung der Welt: C. S. Cornelius.—Lehrbuch der Chemie: A. Genther.—Histoire des Poissons: Aug Duméril. Tome ii. et Atlas.—Charles Darwin et ses précurseurs francais: A. de Quatrefages.

CONTENTS

THURSDAY, JUNE 23, 1870

THE UNIT OF LENGTH

THE battle of the Standards is over, and we may say the Metre has gained the victory. The need of a new system of weights and measures to amend the strange diversities which disfigure our practice being admitted, the question has once more been started—Should we once for all found our system on a natural basis? The pendulum vibrating seconds in a certain latitude, was long ago proposed as a universal basis of linear measure, and the House of Commons somewhat countenanced it years ago, by prescribing that the length of the yard shall be determined by the length of the second's pendulum. But the action of gravitation on which the terms of the vibration depends, is subject to so many variations and disturbances, that the quantity sought cannot, even on the same spot, be absolutely the same at all times. The real length of a normal pendulum is almost unattainable, so limited is our knowledge of the force of gravity on land and at sea. A more certain basis for a natural unit has been found in the polar axis, the length of which, according to Sir John Herschel, bears a close relation to our imperial inch, and has the advantage of avoiding the many causes of error resulting from the physical peculiarities of the countries through which any measured arc may happen to pass. But are our physicists agreed as to the real length of the polar axis, and would it be worth while to make any alteration in our weights and measures for the sole purpose of attaining some scientific correspondence between the unit in use and a unit founded on nature?

The advocates of the metre rest their arguments on a much broader basis. They do not assert that the metre is absolutely and mathematically the ten millionth part of the quadrant of the earth; they know that the meridians of places differing in longitude are not all precisely of the same length; and they admit that were we now to make a new measurement with our better instruments and more extended information, we might attain much greater accuracy than was arrived at by the French philosophers at the end of the eighteenth century. What commends the metre above any other unit, is the fact, that it is already a cosmopolitan unit, widely recognised, and in general use among many nations; and that whilst other units remain as philosophical abstractions, the metre is the basis of a system, not only perfectly complete, homogeneous, and scientific, but simple and practical in all its parts. Any slight error in the determination of the metre, is more than counterbalanced by the extreme simplicity, symmetry, and convenience of the metric system; and not the least of its recommendations are, that the unit of linear measure applied to matter in its three forms of extension, viz., length, breadth, and thickness, is the standard of all measures of length, surface, and solidity; and that the cubic contents of the linear measure in distilled water at a temperature of great contraction, furnish at once the standard weight and measure of capacity.

When we said that the battle of the Standards is over and that the Metre has gained the victory, it was meant that, for practical purposes, all opposition to the introduction of the metric system has been abandoned, and that Parliament and the Government are now left to introduce it in such a way and at such a time as may be found at once practicable and satisfactory. The use of the metric system has been legalised for the last half-dozen years, but it was not till quite lately that the whole question was submitted to the calm deliberation of a Royal Commission. The Standard Commissioners, who included among their members the Astronomer Royal, the President of the Royal Society, and the late Master of the Mint, considered the question of the introduction of metric weights and measures, in any form, *ab initio*. And after careful examination they gave their verdict in its favour in the following terms:—

"Considering the information which has been laid before the Commission,—

"Of the great increase during late years of internationa communication, especially in relation to trade and commerce;

"Of the general adoption of the metric system of weights and measures in many countries, both in Europe and other parts of the world, and more recently in the North German Confederation and in the United States of America;

"Of the progress of public opinion in this country in favour of the metric system as a uniform international system of weights and measures;

"And of the increasing use of the metric system in scientific researches and in the practice of accurate chemistry and engineering construction;

"We are of opinion that the time has now arrived when the law should provide, and facilities be afforded by the Government, for the introduction and use of metric weights and measures in the United Kingdom."

The Commissioners further recommended that metric standards, accurately verified in relation to the primary metric standards at Paris, should be legalised; that verified copies of the official metric standards should be provided by the local authorities for inspectors of such districts as may require them; and that the French nomenclature, as well as the decimal scale of the metric system, should be introduced in this country. The Commissioners, whatever might have been their predilections, could not resist the fact that the civilised world pronounced itself for the metre, and they sanctioned its legalisation. What is to be regretted is that they stopped there. Since the complete substitution of the metric for the present practice is now practically certain, would it not be much better to prepare for the change and carry it into effect as speedily as possible? No advantage can come from a policy of indecision, and we trust that the Legislature may adopt a more definite course than the one sketched out by the Royal Commissioners. Let it not be imagined that the people will give themselves the trouble of learning the new system, however beautiful and easy, so long as its use is not absolutely necessary. With all the desire of the teachers to introduce it in the schools, they find that they cannot teach the old and the new tables. They cannot afford the time. A compulsory measure is the only method of dealing with the question.

The Warden of the Standards being now employed in procuring Metric Standards, it may be well to add that the mode of constructing them, either from the original Metre at the Archives, or from the copy at the Conservatoire des Arts et Métiers, has been much debated. The

International Statistical Congress, held at Berlin, decided "That the care of preparing and putting into execution the regulations to be followed in the construction of the standards, and of the system itself, should be entrusted to an International Commission, which will also see to the correction of the small scientific defects of the system." The International Geodesical Conference held at Berlin in 1867 decided: "In order to define the common unit of measures for all the countries of Europe, and for all times, with as much exactness as possible, the Conference recommends the construction of a new prototype European Metre. The length of this European Metre should differ as little as possible from that of the Metre of the Archives in Paris, and should in all cases be compared with the greatest exactness. In the construction of the new prototype standard, care should be taken to secure the facility and exactness of the necessary comparisons." And "the construction of the new prototype metre, as well as the preparation of the copies destined for different countries, should be confided to an International Commission, in which the States interested should be represented." Since then, the Imperial Academy of Science of St. Petersburg has taken the matter in hand, and a committee of the Physico-Mathematical class, consisting of MM. Struve, Wild, and Jacobi, has made a report on the subject, observing that the standard metric weights and measures of the various countries of Europe and of the United States differ by sensible though small quantities from one another. They expressed their opinion that the continuation of these errors would be highly prejudicial to science. They believed that the injurious effects could not be guarded against by private labour, however meritorious, and they recommended that an International Commission should be appointed by the countries interested to deal with the matter. This suggestion was approved by the French Government, and consequently the Conference will take place in Paris in August next, when the Astronomer Royal, Professor William H. Miller, and the Warden of the Standards, will represent this country. Everything seems thus tending towards the early realisation of the great scheme of uniformity of weights and measures throughout the world.

MUSEUMS OF NATURAL HISTORY

THERE is no doubt of the popularity of museums of natural history with the lower classes. That it is otherwise with more educated people is perhaps attributable, not so much to indifference to scientific knowledge, as to the fact that hardly any scientific knowledge is to be gained by a cursory inspection of crowded collections, arranged with reference to economy of space rather than to the existing conditions of zoological science. It must not be forgotten that the sentiment of mere wonder, which the stranger forms of animal life are so calculated to excite, was satisfied, or at least blunted, in early childhood, in the case of those of us who have had access to well-illustrated books, and to the zoological gardens of great cities.

Twenty years hence it will hardly be credited that in the year 1869 a competent naturalist, after visiting fifty of the principal museums of Europe, reported on them in the following terms:—"So far as his observations extended, he found no museum where any other purpose than a desire to produce a pleasing and convenient disposition of the specimens was manifested in the general plan of arrangement. In the few cases where there was an evident intention of showing some of the more important general features connected with the distribution of life over the face of the globe, or in the successive geological formations, the imperfection of the means has been too great to afford any great result. Among the fifty museums visited, not one was found in a building especially designed for the purpose of exhibiting collections arranged to show the history of life."* We may fairly hope that the condition of things above described will not endure much longer. The ever-widening interest in the higher problems of zoology awakened by the writings of Darwin and his followers, will no doubt, in time, move even the conservative professors and curators of the great European museums to urge upon their Governments the necessity of providing them with the means of making the collections under their charge visible embodiments of what is now known of the history, distribution, and affinities of animal life, instead of simple gatherings of curiosities, or at best mere storehouses of materials for the professed naturalist. The rearrangement of the great national collections in properly constructed buildings would, even under the most favourable circumstances, be a work of years, and one entailing great and irksome labour upon the distinguished officers of these museums. Meanwhile, there is every prospect that the hopes we have expressed in reference to the European collections will be almost immediately realised in the case of the great museum of Harvard College, under the charge of one of the most eminent of living zoologists, Professor Louis Agassiz. Thanks to the liberality of the State Legislature of Massachusetts, a sum of 75,000 dollars, payable in three annual instalments, is available for this great work, conditionally upon the raising by subscription of like sums. We are glad to learn from the trustees that the first 25,000 dollars have been subscribed, and the corresponding 25,000 dollars received from the State. The spirit which is animating Prof. Agassiz in this matter will be best gathered from his own words in the official report now before us. After paying due tribute to those who, by bringing together the great collections of the old world, laid the foundations of our present knowledge, he says: "We have no longer the right to build museums after this fashion. . . . If I mistake not, the great object of our museums should be to exhibit the whole animal kingdom as a manifestation of the Supreme Intellect. Scientific investigation in our day should be inspired by a purpose as animating to the general sympathy as was the religious zeal which built the cathedral of Cologne or the basilica of St. Peter's. The time is past when men expressed their deepest convictions by these wonderful and beautiful religious edifices; but it is my hope to see, with the progress of intellectual culture, a structure arise among us which may be a temple of the revelations written on the material universe" (p. 6). Prof. Agassiz is able to write in the following encouraging terms of the immediate prospects of his great undertaking:—"With the prospect for

* These words are to be found at p. 41 of a paper entitled, "Annual Report of the Trustees of the Museum of Comparative Zoology at Harvard College, Cambridge, together with the Report of the Director, 1868." Boston: 1869.

the next three years of an income large enough to secure the aid of competent assistants in the different departments, we shall be able to put our immense collections in complete order, and to enlarge the building sufficiently to exhibit all our specimens in their true relations. I hope that in three years any intelligent observer will be able to say that a mere walk through our museum teaches him something of the geographical distribution of animals, of their history in past ages, of the laws controlling their growths as they now exist, and of their mutual affinities—in short, that the whole will be so combined as to illustrate all that science has thus far deciphered of the plan of creation. This is my hope, and it is shared by the efficient corps of assistants on whose co-operation I largely depend for its fulfilment (p. 4)." Prof. Agassiz has set himself a noble task, and we doubt not that when it is completed, the museum of Harvard College will be an intellectual monument worthy alike of its curator and of the science of which he is so distinguished an ornament.

OUTLINES OF HUMAN PHYSIOLOGY

Grundriss der Physiologie des Menschen. Von Prof. Dr. L. Hermann. Dritte gänzlich umgearbeitete Auflage. 1870. (Berlin: Hirschwald. London: Williams and Norgate.)

WHEN this work first appeared, now more than seven years ago, it at once became our favourite handbook of physiology, and it has ever since been our constant companion. Perhaps the chief reason why it so especially commended itself to us was the fact that it served as the clearest and best exponent of what may be called the radical school of physiology. Its general arrangement differed altogether from that of most modern text-books. It entirely threw on one side that division into "functions" (function of respiration, function of digestion, and the like) which, after all, does not lessen much the labour of the author, and certainly leads the student astray, throwing, as it does, into the background, or even completely hiding, the essential oneness and solidarity of the animal body, and bringing the learner to regard the organism as a bundle of "functions," one of which might easily be pulled away without much harm being done. The leading idea of the book was to follow out as closely as possible the doctrine of the conservation of energy. That idea was kept steadily in view throughout the volume, and faithfully adhered to.

Another remarkable feature was the bold attitude taken up towards recent discoveries. These are always great stumbling-blocks to teachers and text-books. Some authors, especially German authors, put in everything that comes up, leaving time and future editions to take out again the things that wither up. Others, again, put in nothing that does not seem to be already fairly established, and the student who trusts to these alone has perhaps to wait several years, till a new edition tells him of results which have had, it may be, a most powerful influence on the progress of the science for as many years back. A third class, and of these Prof. Hermann is one, make up their mind as to what they think will stand and what will not, and so accept the one and reject the other, though both may have been published yesterday. This course being bold is of necessity dangerous; and a new edition is, in the case of such books, a most critical occasion.

An unusual popularity has now brought the present work to a third edition, and the author may certainly congratulate himself on the little which he has had to undo Many new things which in 1863 he boldly accepted have since been ratified by general consent.

The present edition differs from the preceding two in being made much larger and more complete. We trust that it has now reached its full growth, for one of the great merits of the first edition was its small size. Otherwise, beyond the increased filling up of all parts, the book does not differ materially from what it was. We are glad to see that several oversights, such, for instance, as the extraordinary statement of the first edition that the apex of the heart beats between the seventh and eighth ribs, have been corrected, and that the author gives a fair account of matters in reference to which he has been engaged in personal controversies, as, for example, in the physiology of muscle.

The boldness in selection of material to which we have referred, renders the book in some measure unsuitable for a student not sufficiently advanced to have acquired a physiological judgment; but we would urge it upon the notice of all who wish to have a clear and succinct exposition of the physiology of the present day.

M. F.

OUR BOOK SHELF

On the Strength of Beams, Columns, and Arches. By B. Baker, Assoc. Inst. C.E. (London: E. and F. N. Spon, 1870.)

THE subject matter of this little volume is of great importance to Civil Engineers. All structures resolve themselves ultimately into beams, columns, and arches, of some kind. It is therefore of great importance that the engineer should be familiar with the mode of ascertaining their strength or their resistance. We approve, in the main, of Mr. Baker's endeavour to dispense with high mathematics by substituting geometrical solutions for ordinary problems, because, unfortunately, mathematics is not the strong side of English engineers, although England has produced the greatest of mathematicians. But the author seems to labour under serious misapprehensions. He proposes his geometrical solutions, because he thinks that the use of mathematics "involves an unjustifiable waste of time, with the great contingent disadvantage that it checks the growth of sound judgment in the engineer, by giving a fictitious appearance of accuracy to his results which are not susceptible of exact deduction." This is a grievous error. The spirit of mathematics is the expression of most acute and refined reasoning; and how can the practice of intellectual reasoning check the growth of sound judgment in the engineer? The fictitious appearance of accuracy above mentioned, is altogether beside the question, because it is optional; but not so the correctness of our reasoning and arguments. The author makes the above statement in his preface, and we find, unfortunately, that throughout the volume the spirit of mathematics is sadly offended. Let us take for example the author's mode of calculating the strength of beams. He shows us how the strength of a beam may be found geometrically, and derives the formulæ for rectangular and other beams, assuming the neutral axis of the beam to pass through the centre of gravity of the sectional area of the beam.

He then proceeds to compare the calculated resistance of certain simple beams with the observed resistance as ascertained by experiment, and he finds that there is a large discrepancy between the calculated and the experimental ultimate resistances. Adopting Mr. Barlow's notation, he calls f the ultimate resistance to direct tension, F the "apparent" resistance to the same force excited by transverse strain, and ϕ the "resistance due to flexure," then $F = f + \phi$. This will be better understood by reference to figures. Mr. Hodgkinson found in the experiment under consideration, that a square inch of cast iron was ruptured under direct tension by 18,750lb., which in the above equation would be the value of f. When, however, a rectangular bar of the *same* material, one inch square, was tested, a weight of 527lb. applied at the centre of a span of sixty inches, just broke the bar. Applying now the formulæ for rectangular beams to this result, the author finds that the ultimate tensile strength of the sample under consideration must be assumed at 45,630lb. in order that the bar may be able to offer the resistance shown in the experiment; the figure 45,630 he calls the "apparent" tensile strength, and would be the value of F in the above equation; accordingly, $45,630 = 18,750 + \phi$, or $\phi = 26,880$lb., this value of ϕ being termed "resistance due to flexure," a term, we are informed, invented by Mr. W. H. Barlow; * and this new resistance being described as "lateral adhesion of the fibres;" and the author informs us, that the neglect of it may result in an error up to 190 per cent.

We have here a fine confusion of everything referring to the subject. An error is made, which to explain away, a new one must be committed; forces hitherto not suspected by mathematicians are discovered by those whose sound judgment was not checked in its growth by the infinitesimal calculus.

Does the author not know the condition which determines the position of the neutral axis of a beam? The neutral axis passes through the centre of gravity of the sectional area of a beam, provided *the resistance of the material to tension and to compression be alike.* In almost every material these resistances differ from one another, but when only a small fraction of the ultimate resistance of the material need be taken into account—say one fourth—then for practical purposes they may be assumed to be alike.

The author proceeds, however, to breaking strain, using cast iron; its ultimate resistance to tension is about eight tons per square inch, to compression about forty tons. Under these circumstances his original formula no longer holds good; the neutral axis no longer passes through the centre of gravity of the section of the beam, it approaches more and more the side where the greater resistance is offered; and were the resistance to compression infinitely great, the neutral axis would coincide with the position of the extreme fibre of the beam on the compression side, and the whole sectional area would be resisting tension only, and the extreme fibre balanced by compression. The beam would then have just double the resistance without assuming the least increase of tensional resistance of the material. The author's ϕ expresses, therefore, the amount of error into which he and others have fallen, and in case it should be zero, they will find $F = f$ as it should be, and the elaborate fabric of confusion disappears.

In other respects, the work contains much valuable information, and if the unfortunate mistake above referred to, did not crop up throughout the 300 pages, and a natural flow of clear language were substituted for a rather dogmatic and vague style, we should be glad to recommend it to the profession, which ought to have all the aid that modern science can afford. * * * * *

* Mr. Barlow, F.R.S., recently read a paper before the Royal Society on this subject, reviving his theory on the resistance of beams to transverse strain.

Meteorology. By Sir John F. W. Herschel, Bart. From the Encyclopædia Britannica. Second Edition. (Edinburgh; A. & C. Black.)

Introductory Text-book of Physical Geography. By D. Page. Fourth Edition. (Edinburgh: W. Blackwood & Sons.)

WE class these two books together as new editions of standard treatises in their respective departments of science that are among the best that can be used by students or teachers. The term "Meteorology," which has entirely lost its etymological meaning, is defined by Sir John Herschel as "the description and explanation of those phenomena which group themselves under the head of the weather, of the seasons, and of the climate," a branch of natural science of the laws regulating which we are at present almost entirely ignorant, as Dr. Balfour Stewart has shown in these pages. Writers on physical geography content themselves at present with a description of the physical contour of the globe, with some slight reference to its climatology, and the distribution of its animal and vegetable life, Mrs. Somerville's handbook being, as far as we know, the most complete in this respect. The better and more logical mode would seem to us to be, first of all to treat of the earth as a member of the solar system, and thence to deduce the laws which govern its natural phenomena; we believe that in this way such phenomena as those of ocean currents and trade winds, and the variations of climate, would be rendered far more quickly intelligible to the learner than is now the case. From his stand-point, Dr. Page's "Introductory Text-book" discusses the subject in his usual clear, concise, and systematic manner.

Rustic Adornments for Homes of Taste. By Shirley Hibberd. New Edition. (London: Groombridge & Sons.)

THAT two editions of this book have been disposed of in a short time is ample justification for the publication of a third, especially when got up in so handsome a style as the one before us. Works of this kind appeal to a large public, not over-critical as to scientific accuracy, but glad to possess that amount of knowledge which enables them to talk about ferneries and aquaria without committing any egregious blunder. We are far from depreciating the value of this smattering of science where it is all that opportunity permits to be attained. Those who like their homes to be surrounded by beautiful natural objects will here find a large fund of information respecting the aquarium, the fernery, the aviary, the apiary, the conservatory, &c., given in a pleasant style, illustrated with woodcuts and coloured plates. The volume makes altogether a very pretty gift-book, especially for a young lady.

E. Millon, sa Vie, ses Travaux de Chimie, et ses Etudes économiques et agricoles sur l'Algérie. Pp. 327. (London: Williams and Norgate, 1870.)

M. REISET, in the preface, tells us that after the death of Millon, his friends and pupils undertook the publication of a collection of abstracts of the numerous works of this illustrious chemist. The book commences with an interesting biographical notice of Millon, by Dr. Hœfer, in which the political questions which led to his long residence in Algeria are as slightly noticed as possible. The principal portion of the volume was arranged by M. Jules Lefort, with the assistance of MM. Coulier, Commaille, and the late Professor Nicklès. The book contains two hitherto unpublished papers, each extending over forty-three pages, one "on Fermentation" and the other "on the Economic and Agricultural problem of Algeria." The researches on corn also occupy considerable space. The other investigations of Millon are arranged in a very interesting manner, frequently in connected treatises. Opposite the title-page is a good photograph of a bust by M. Clément, and at the end of the book is a chronological list of the scientific works of the author, amounting in number to no less than seventy-nine.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Corona

HAVING been informed that my remarks at the meeting of the Royal Astronomical Society, on Friday last, have been interpreted otherwise than I meant, doubtless in consequence of my having spoken without preparation, I beg to repeat what I intended should have been the purport of my statement.

It seems to me beyond reasonable doubt that we have upon all the photographs, whether of long or of short exposure, a representative of *something which is at the sun*, since the contour of the radiance depicted upon all the photographs exhibits minima, in directions closely approximate to the extremities of the sun's axis of rotation. Furthermore, exterior to all this, and apparently masking it in a very great degree to the ordinary observer, is a much wider and more conspicuous radiance of very irregular outline, to which the name *Corona* has been ordinarily applied. The streamers and irregular projections of this latter corona appeared to me to vary in position during the period of totality in August last. They seem, moreover, to have no connection with the positions of the prominences, nor yet with that of the solar axis, and hence I infer that this phenomenon arises from something *which is not at the sun*.

London, June 15 B. A. GOULD

Euclid as a Text Book

I HAVE been waiting in the hope that Mr. Levett's letter (NATURE, No. 30, pp. 65, 66) would elicit a response from other members of the "rank and file" of mathematical teachers. No one having come forward, I venture to do so, lest the subject should again drop. Mr. Levett's suggestions appear to me at least worthy of some little ventilation, and I hope some leader will be induced to utter a note on the subject. Knowing that many of the leading geometers of this country are favourably disposed to the "reform" movement, I feel sure their silence is not attributable to indifference. In the meantime it is my opinion that no isolated efforts will bring about such a reform as will thrust out Euclid from our schools; united action is what is wanted, and then "a long pull, a strong pull, and a pull all together." I could easily select from the four Universities of Oxford, Cambridge, Dublin, and London, four geometers who could, I believe, if it be possible, bring out in concert a work which should be a fitting rival of the old-world geometry, command the attention which such a work ought to secure to effect the change desiderated, and convince gainsayers. A scheme might be drawn up in concert, the working out of the details committed to one, and the work appear under the united names of the body. Then as to the number of the "rank and file" willing to give their support to such a plan, possibly some mathematical master at one of our public schools (I could in this case also make a selection) could give much valuable information. I hazard the above remarks, not wishing the ball set rolling in the columns of NATURE to come to rest, until the change has been effected or its inexpediency irrefragably demonstrated, *valeant quantum valent*. R. TUCKER

University College School, June 11

The Interior of the Earth

UNDER the signature of Z., I find in your issue of the 16th inst., a short notice of "The Interior of the Earth." A clerical error made during the Epsom or Ascot races, such as F. L. instead of E. I., is excusable, though not comprehensible; a misquotation which he has made may be pardoned, but a misrepresentation purely from neglect of reading is quite unbearable. He tells your readers that I proceed "to explain the earth's heat and volcanic phenomena by a like action on buried vegetable matter." If he had read p. 33, he would have found that to the cause alluded to "I partly assign the changes which have taken place in the strata connected with our coal pits." Z. thinks that a perusal of Lyell's "Principles" would have stopped the writing of my book, I beg to tell him, that this work was the first that convinced me of the great geological error which I have exposed. Z. does not seem to have read my reasons for using Page as my text-book; I hope he is not hurt at being left out himself, but if I had quoted from all the books I have read on the subject, I should have been as unintelligible as some of them.

A sneer is not a review, but if Z., or any one else, can prove to me that an internal and inherent heat within the earth has caused, or does cause, the phenomena alluded to, I will with pleasure renounce my present creed; but, till then, I merely say that as I have controverted the theories of others, I shall be glad to read any controversions of my book, if written in the same spirit of inquiry. H. P. MALET

Prismatic Structure in Ice

THE enclosed letter, which has just reached me from Canada, seems to me so interesting that I venture to hope a place may be found for it in the columns of NATURE. I may, however, state that I still adhere to my conviction that the vertical chains of air bubbles are the consequence of the prismatic structure; since in all the cases I have seen they are too regular to have been formed as my correspondent suggests. Although I believe it is an established fact that, speaking generally, ice contracts with cold, I am not aware that its demeanour at a temperature of about 32° F. is quite so accurately ascertained; it seemed to me, when investigating what had been written on the subject, that further information was needed on this point. The prismatic structure appeared, and still appears, to me inexplicable on any other theory than that of contraction.

St. John's College, Cambridge T. G. BONNEY

"In one of the March numbers of NATURE I see a letter over your signature on the prismatic structure of ice, and as our climate gives us favourable opportunities of observing this and other curious facts respecting ice, I am induced to address a few words to you on the subject.

"The ice on our inland lakes is generally two or three feet thick. As the spring advances, an inch or two may be melted away from the lower surface, and somewhat more from the upper one, but the thickness is not materially reduced until its final disappearance. The first sign of the approaching break up is that the ice becomes dry, from the prismatic structure having commenced to show itself, allowing the surface-water to percolate through the interstices; it is then said to be honey-combed. In this state the lower layers of transparent ice are still solid, though if you cut out a block the prismatic structure is very evident; but the upper portion, which has been formed from a mixture of snow and water, readily breaks up under your feet into little granules of ice. The next stage is that the ice becomes black, showing that it is soaked as it were with water; and if at this time there is any open water, as where a river falls into the lake, and wind enough to create a swell, the whole surface of the ice may be observed to undulate. Even then, sometimes, a single night's frost may make all firm again, and you may even trust horses upon it. If the ice now breaks up prematurely with a high wind, it becomes a mass of spiculæ of ice which have not reached the melting point, and which I have seen accumulate to the depth of six or seven feet against the edge of the ice, which has not yet broken up. But if there is no wind, the whole surface of the lake may appear an unbroken sheet of black ice, still a couple of feet thick, till, in an astonishingly short space of time, sometimes not more than a few minutes, it disappears as if by magic. So sudden is this disappearance, that the ice is popularly believed to sink.

"I once had a very good opportunity of noticing this sudden disappearance. I had built on the ice during the winter a pier of logs filled with stones, and when the spring came, it settled down to the bottom, carrying with it a large cake of the ice. When the lake had opened, I went round the pier in my canoe to see if it had settled evenly. There at the bottom, in six or seven feet of water, lay the cake of ice it had carried down, with the chips still imbedded in it which we had made in building the pier; and, as I looked, blocks would break off of a foot or more in thickness, rise to the surface, break up into spiculæ, and almost instantaneously disappear.

"I quite agree with you that these prisms have no connection with the hexagonal form of ice crystals, but I doubt your explanation that they arise from the contraction of the ice as it approaches the melting-point. Does ice contract under such circumstances? Although water expands in freezing, and, *vice versa*, occupies less bulk when reconverted into water, yet, as long as it remains ice, I conceive that it contracts with cold and expands with heat like other substances. It is certain that, after a night's hard frost, a surface of glare ice will be covered with a multitude of small cracks from the contraction; and larger cracks will sometimes extend through the whole thickness, with a report almost like that of a cannon. I knew a case of a railway bridge, built on piles over a shallow lake, which was entirely destroyed by the alternate expansion and contraction of the ice with variations of temperature. With a spell of cold the ice would be split up with innumerable cracks, into which the water insinuated itself and froze. With warmer weather the whole expanded, shoving the ice up on the shores. On a fresh contraction, the ice could not be withdrawn from the shores, but cracked again in preference. The consequence was, that there was a constant shoving from the middle of the lake towards both shores. The piles in the middle remained upright, but the nearer they were to each shore the more they were driven out of the perpendicular. The damage was great the first winter, but it was repaired. The next winter, by cutting up the ice with ice ploughs, and by making embankments from each shore, the damage was sought to be arrested; but every winter it got worse, till the bridge had to be altogether abandoned; and whilst the piles in the middle are still upright, those near each shore are now laid almost flat. It is to this constant motion in one direction, arising from alternate expansions and contractions, that I have always in a great measure attributed the downward progress of glaciers.

"The true explanation of the prismatic structure appears to me to be the lines of air bubbles, which you yourself notice. These are visible in all ice before any thaw has commenced, and in the process of freezing they seem to be formed in vertical lines. When the thaw occurs, these lines of bubbles form the centres, as it were, from which it penetrates in every direction through the mass. "JOHN LANGTERS

"Ottawa, May 25"

Etna in Winter

THE ascent of a high mountain always gives plenty of opportunities of observation and experiment, but that of the prince of the volcanoes of Europe, and at the same time one of the grandest of its mountains, must always be an undertaking of paramount interest to the student of the phenomena of Nature.

We leave the edge of a plain with a semi-tropical luxuriance of vegetation (the great plain of Catania, where Ceres taught mankind husbandry, and where common belief states wheat to be indigenous), and pass through a country so rich in its profusion of orange, lemon, fig, and olive trees, so-called American aloes, prickly pears, vineyards, peach trees in full bloom,* &c., that it would remind us forcibly of Algiers, did not the irregular surface of the numerous lava currents (on which the prickly pear is cultivated extensively) and the lava-built walls and buildings around bring back Auvergne to our thoughts.

Still higher above the sea level we find karouba trees, with umbrella pines and dwarf oaks in abundance; fields of flax and of wheat (a foot high) and other signs of a more temperate climate; nearer to Nicolosi (the village from which the ascent is made) in traversing the rugged lava current of 1536 we are struck by the peculiar appearance of the bushes of Etna broom which flourish on its surface. Even on the cones above Nicolosi up to a height of about 1,300 metres above the sea level, the vine is still cultivated and excellent wine produced.

After supper and a short nap we start at 10 P.M. with a guide, mules, and a muleteer, and well provided with provisions and wraps (taking with us a few thermometers, &c.); and passing by the Monti Rossi, which were formed during the remarkable and well-described eruption of 1669, we traverse various lava currents and fields of cinders, and find ourselves at last in a straggling wood of stunted oak trees succeeded by one of Italian chestnuts, in which latter we come upon the Casa del Bosco (a solitary house, uninhabited in the winter) after a rather rough ride of nearly three hours. Here we rest for half an hour, warm ourselves at a wood fire which is soon made, and take our supper; then at 1.15 we start for our laborious ascent; we come upon the snow in about an hour, and (having chosen a starlight night after a continuance of fine weather) find it dry and firm; now the work begins; leaving the mules and their driver, we toil on slowly and steadily, not speaking for fear of wasting strength or getting out of breath, and after a long, stiff climb up a very steep surface of almost smooth snow, which scarcely gives a hold to one's feet, we arrive at the Piana del Lago soon after 4 A.M. Here we have a comparative respite, and march merrily up the slight ascent, with the black summit of the cone in front of us, and looking so close that we can hardly believe the guide who says that it will take us nearly two hours to get up to it. At about five o'clock we arrive at the Casa degli Inglesi, where travellers sleep in the summer, but which is now all but buried in snow. On we go (as we have no time to spare before sunrise), the guide pointing out where we might see the Torre del Filosofo were it not entirely covered, and in a quarter of an hour, after crossing several fields of thin, broken ice, and nearly falling into sundry uncomfortable-looking holes, which one doesn't see until one has put one's foot through the crust of ice and snow which conceals them, we reach the foot of the actual cone, and look up its steep sides with some dismay, knowing, as we do by manifold experience, that the ascent of a cone of loose cinders 300 metres high, and at an angle of 40°, is no joke; our work is made somewhat easier by the snow with which the base is covered; but we soon get off this and hurry on to get to the top before the sun rises. Before we are half way up the cone we find the necessity of stopping to take breath so urgent, that we halt and look behind us at the inexpressibly magnificent colours in the sky; this we have to do several times, and were it not for Pietro's "Avanti!" we should go to sleep as we walk; besides which our legs have given way at the hip joints and seem no longer to belong to us; perhaps also some of us may feel a sense of nausea, and those least used to such excursions doubtless have a sharp pain in the knee joints. By screwing up our courage, however, we get gradually higher, and soon the fumes of sulphurous acid that arise around us tell us that we can't have much more to do. Five minutes more and we look down into the immense crater, and instantly turn round and see the sun rise above the horizon and light up the beautiful island at our feet.

Our ascent has been a capital success; we have had a fine starlight night, firm snow, and a good guide; there is not too much vapour, &c., and the brisk north wind drives it away from us. We soon find out that although the soles of our boots are being burnt by the hot cinders, our right hands (which held the alpenstocks) are frozen, and during ten minutes we experience excruciating pain in them. After a hearty breakfast we look around us, and see that the heated state of the ground is due to the continual slow oxidation of the sulphur contained in the ashes or lying about on the surface, and not to any internal action of the volcano. Far down in the crater, where there is less sulphur, the surface is covered with snow, and at the very bottom of it is a huge plug of snow also. With some difficulty we make our way through the suffocating vapour to the highest point of the edge of the crater, and thence look down upon the marvellous scene.

The effect is almost unique, from no other point, except perhaps the Peak of Teneriffe, has one so unbounded a view. We find ourselves at a height of nearly 11,000 feet above the sea level, with no other mountain of considerable height within the horizon at all: the "garden of Europe" stretched out at our feet, beyond its borders the sea, on all sides extending far away into the dim distance, and confusing itself with the sky; to the north of us Calabria, the "toe" of Italy, bounded by mist and looking like an island; we see yonder Monte St. Giuliano (the ancient Eryx), there the hills around Palermo; to the north, the point of Milazzo, very plainly indeed; beyond it, the chain of the Lipari Islands, apparently raised up into the sky (a rather striking phenomenon); farther east, the island seems to touch Calabria, and there are the Straits of Messina; here is Catania, from which we have come: there Syracuse, and in that direction Girgenti, the site of the famous Greek temples; farther west Marsala with its vineyards. All around us, at the base of the mountain, we see the secondary cones, and look down into their craters, while the distant hills look like slight irregularities on the surface of the island, although some of them are so high as to be covered with snow. What with feasting our eyes on the grand view, looking down into the immense crater, the deepest and steepest that we have ever seen, and collecting specimens of the variously-coloured cinders, &c., around us, we have spent two hours on the summit: it is eight o'clock, and our guide warns us that the heat of the sun will fast melt the surface of the snow, and render the descent more difficult and more dangerous. We could not walk round the edge of the craters on account of the vast quantities of stifling vapour that arise from some parts of it, even if we wished to spend another hour in doing so. Taking

* March 4th.

then a last look into the huge abyss, we run easily down the side of the cone, and are in a few minutes at the Casa degli Inglesi again. Here we turn off towards the eastern side of the mountain, and soon come upon the edge of the Valle del Bove, to enjoy perhaps the most remarkable spectacle in Europe. We find ourselves now on the summit of an almost vertical cliff, nearly 4,000 feet high, which constitutes the head of an enormous cleft or valley, about eight or ten miles long by four or five broad; it is as if a piece, constituting about one-sixth part of the mountain, had been cut out of it. On either side it is bounded by cliffs of from one to three thousand feet high, and consisting of layers of lava and ashes traversed by dykes of basalt, trachyte, &c.; several volcanic cones are seen in it, of which Monte Simone, to the northern side, with its lava of 1811, and Centennario, Calanna, and Giannicola to the southern side, with the barren black lava of 1852, are the most noticeable; this immense depression was caused, according to the opinion of Sir Charles Lyell and of Gemmellaro the great Sicilian vulcanologist, by the subsidence of an ancient felspathic volcano, which must, according to calculations made from the inclination of its lava currents, have been much higher than the modern pyroxennic one. (La Vulcanologia dell' Etna, del Professore Carlo Gemmellaro; Catania, 1858.) Such a subsidence is well illustrated on the small scale by the Cisterna, a round hole about 300 yards in diameter, and at least 200 feet deep, which was formed precisely in the manner just mentioned during the eruption of 1792, and which we can see on our way back to the Montagnola; indeed, when we consider how much material is ejected during the various eruptions in the form of lava and of ashes, &c., we see that it would be strange if subsidences, and great ones too, did not happen occasionally.

We now descend quickly, finding our last night's tracks behind the Montagnola, and by 10.30 are off the snow, and find the mules ready for us. In returning to Nicolosi we are able to observe the various lava currents, and to study their sections in the channels of the streams which rush down during the melting of the snow in the summer months, and also to notice the gradual change in the vegetation which the darkness prevented our remarking during the ascent.

We find the heat more and more oppressive, and are afflicted with very severe headaches; on arriving at Catania we find it covered by a dense fog (an extremely unusual occurrence there), and so the congratulations on our safe arrival are mixed with wishes that the weather had been more favourable.

In a future communication some remarks will be made on a few observations taken during this excursion.

W. H. CORFIELD

Paraplegia in Kangaroos

SOME time ago I obtained from Mr. Fairgrieve the bodies of two Kangaroos, male and female, which died during the visit of Wombwell's Menagerie to the West of Scotland. In the female, which I received first, there was extensive ecchymosis in the nuchal region strongly suggestive of bites inflicted by her cage companions. To this I was disposed to refer the softening of the cervical spinal cord, which struck me when removing the brain. On visiting the menagerie, however, I found that her male companion was completely paraplegic, and that he had exhibited the same symptoms. The paraplegia had been progressive, and at the date of my visit, respiration was markedly thoracic. The animal was excited, but I could not satisfy myself whether this indicated cerebral disturbance or arose from the contagion of fear, a younger specimen in the same cage being much alarmed at my approach. The animal died at some distance from Glasgow. I made a careful post-mortem, and found no lesion save in the spinal cord and medulla oblongata. The removal of the cord was difficult, on account of the thickening of the membranes, and their adhesion to the bony walls of the canal. The cord was not merely softened; it was semifluid as far up as the origin of the cervical plexus, and welled out like thick cream from an accidental puncture of the sheath. Dr. Joseph Coats who assisted me in the examination, failed to detect any fatty degeneration of the nervous tissue. Its disintegration was, however, very complete. The other organs were healthy, and the body was well nourished. The disease was manifestly of short duration, and I can only hazard this conjecture as to its cause, that the cage was placed at the angle of the square formed by the cars, and that its inmates were thus exposed to draughts and damp, giving rise to acute meningitis. As, however, an Australian sportsman informs me that something of the same kind has been observed in Kangaroos kept in confinement, and thus deprived, to a large extent, of their customary exercise, I ask space for this abstract of the case, in hope that some of your contributors may be able to throw light on an interesting pathological question.

JOHN YOUNG, M.D.,
Glasgow University Keeper of the Hunterian Museum

Geology and the Chatham Dockyard

BELOW the Alluvial deposit of St. Mary's Island is a very irregular surface of gravel, varying in thickness from 2 to 12 feet, and composed of flints but little rounded, and pebbles of Tertiary Sandstone; beneath the gravel is the Chalk. Now, the success of the Chatham Dockyard Works depends upon the stability of foundations that are built on piles driven into the underlying gravel, through which percolate considerable streams of water; this water must denude the chalk to an appreciable extent and form pot-holes, and the subsidence of the works can but be a matter of time. I can form no idea of the rate at which the Chalk would be denuded under the above conditions, as I am not aware of any experiments having yet been made on the "Action of Water on Chalk."

R. C. HART

Dust and Disease

PERMIT me to add my mite to Mr. Horace Waller's theory respecting the utility of mosquito curtains in warding off fever, generated by the miasma of decaying vegetation.

For the last twenty-five years I have held to this opinion, and acted on it in all my wanderings in the jungles of Ceylon, on the east coast of Africa, and in New Zealand, and I am convinced of its great utility. I have always likened it to Davy's "safety lamp," and I believe that over and above the "sieve-like" property, which a few days' use imparts to it, its value is great as warming the air which passes through its meshes, and keeping the temperature within it more steady and equal.

When the body is relaxed in sleep and the pores of the skin act freely, then is the time that the deadly miasma, cold and damp, even in the tropics, seizes on its victim. What jungle traveller does not know the feeling of the air an hour and a half or two hours before daylight? But the warmth from the body and breath within a well-secured mosquito net, I think effectually protects the sleeper.

This morning I compared the temperature outside and inside my mosquito net, and found it differ 8°, being 62° outside and 70° within, and even this was not a fair trial, for the bed is a large double one (two persons in it), exposing a large surface to the external air; the mosquito curtain being the largest sized *Net* that can be got (and not *Leno*) which I would advise for a travelling curtain in fever latitudes; and moreover, as our mosquito season is past, not tucked in all round as a well-secured curtain should be, yet with all these disadvantages the temperature inside was 8° warmer.

Then, again, who doubts that the body, invigorated by sound sleep, is not more able to resist disease in the day-time? Without a net in mosquito lands I find sleep impossible, and I suppose others do the same.

Let me therefore raise my voice in favour of the mosquito curtain, and advise all travellers in fever countries to look on it as their sheet anchor.

E. L. LAYARD

Cape Town, Cape of Good Hope,
May 3

HEINRICH GUSTAV MAGNUS

IN giving expression to the sympathy generally excited by the loss of Magnus, Professor Tyndall has raised the interest of the British public in a philosopher's life, simple, yet most eminently useful. At the present moment a mere outline of it is all we can venture to offer. Unable to appease, it may yet prove sufficient to keep up the interest in Magnus's life until a fuller biography will do more ample justice to his merits.

Heinrich Gustav Magnus was born on the 2nd of May, in the year 1802. Four years later, Berlin, his native town, had to resign its position as the residence of an independent kingdom. The French war raged with ever-increasing fury, and though Mr. Magnus, the banker, found the means of sheltering his children from the most severe consequences of the national calamity, their youth was naturally a severe one. It appears more than probable that the energy and ambition which raised all of them high above the level of mediocrity, may have originated in the stern impressions of their childhood. Thus one of the brothers, Edward, rose to the highest distinction as a historical painter, and is now one of the ornaments of the Berlin Academy of Arts. A younger brother distinguished himself as a physician; while the eldest, whose death preceded that of Gustav by some months only, continued and extended his father's banking business. A son of the latter became prominent during the last war in Mexico, where, representing Prussia at the Court of Maximilian, he showed great energy in his endeavours to save that misguided monarch's life.

The peculiar talents of Heinrich Gustav showed themselves at an early age. He exhibited a rare proficiency in mathematics when a mere child, and soon expressed a wish to devote himself to the study of nature. At the Berlin University, founded some years before, the Chair of Chemistry had passed from the hands of Klaproth into those of Mitscherlich, who was then at the height of his reputation. Young Magnus was twenty-three years old when he published his first paper on the pyrophoric nature of finely-divided metals. Two years later he received his Doctor's degree, and published a dissertation on tellurium. He subsequently passed twelve months at Stockholm, in the laboratory of Berzelius, who gave to Germany some of its greatest *savants*, of whom Wöhler, in Göttingen, and Gustav Rose, in Berlin, now alone survive. From Sweden he removed to Paris, returning to Berlin in 1831, and began there his university career as a lecturer on technology, a subject which he continued to teach till last summer.

HEINRICH GUSTAV MAGNUS

Nearly all his researches published during this time and up to 1833 were devoted to pure chemistry. A latent interest for natural philosophy can, however, be traced already in his earlier publications. Thus he determined the temperature at which oxide of iron is reduced by hydrogen, and analyses of several minerals were followed by determinations of the decrease of density which vesuvian and granite undergo in fusion. These determinations excited much interest at a time when the doctrine of isomerism was making its first appearance, and every two substances having the same composition and yet exhibiting different properties, were subjects of astonishment and of doubt. He even published papers on capillarity, and on the temperature of the interior of the earth and a thermometer fitted to register the same, as early as 1832. But the researches on which his early fame was founded are of a strictly chemical nature. A paper on the combinations of protochloride of platinum in 1828, contained the description of what is now called Magnus's salt, one of the first known of that important series of metallic ammonium salts, which acquired later an increased importance, as a support of the theory of substitution, and a link between mineral and organic chemistry. Five years later, in 1833, appeared his paper on the decomposition of ethyl-sulphuric acid, and on two new acids, ethionic and isothionic acids. These acids (together with the sulphobenzolic acid discovered by Mitscherlich in 1834) became the starting point of numerous discoveries. They increased our knowledge of isomerism; they prepared the way for the modern views on the constitution of natural bodies, and they enabled Strecker, in 1854, to form, artificially, a constituent substance of bile, taurin.

In the same year (1833) another important discovery concluded, so to speak, Magnus's career as a chemist. Together with Ammermüller, a doctor of medicine and head master of a public school in Würtemberg, he published his researches on periodic acid.

When Magnus appeared again before the scientific world, it was in a new science. He was created Professor of Natural Philosophy in 1834, and no research of his was published during the following three years, evidently spent in qualifying himself for his new position. Researches on steel magnets and on the boiling of liquids reopened the series of his discoveries in 1836; still he had not escaped the sway that chemistry exercised over his mind. Papers on the composition of a fossil resin, ozokerite, on the gases contained in blood, and on the combination of ethylene with sulphuric anhydride (carbyl-sulphate) were published in 1837 and 1838, and even later researches on the formation of tar from ethylene, and on the allotropic modifications of sulphur (1856),* show how much chemistry lost when natural philosophy took possession of Magnus's talents and energies. Later his modesty urged him to disclaim the honours so largely gained through his chemical researches. When after the opening of a university laboratory by Prof. A. W. Hofmann, the expanding scientific interest led to the formation of the German Chemical Society (in 1867), Magnus could only be prevailed upon with difficulty to become one of its vice-presidents, and although he worked on the committee with zeal, offering assistance and advice wherever it was needed, and publishing a paper on the diathermanity of chloride of potassium in the Reports of the Society, he pretended that he had lost all claims to be regarded as a chemist.

We have to revert, therefore, to the second side of his scientific work, his researches in natural philosophy. A determination of the expansion of air, instituted at the same time (1842) and in an analogous manner, by Magnus in Berlin, and by Regnault in Paris, and yielding all but absolutely the same numerical results, proved the exactness of both physicists. The most admirable conformity distinguished likewise researches on the tension of vapours, which both *savants* executed independently of each other in 1844, by entirely different methods. Relating chiefly to the tension of steam, the results thus obtained are as important for practical as for scientific purposes. Researches on the tension of vapours given off by mixtures of different liquids, and a comparison of the mercury thermometer and the air thermometer, preparatory to the great investigations just referred to, were published at the same time.

In 1855 Magnus investigated the form which jets of water assume when issuing from apertures of different shapes, and thereby opened to experimental study the surface-tension of liquids. His inquiry extended to the manner in which the motion of the aperture influences the form of the jet. Two years later he published detailed investigations on electrolysis. The discussion of this complicated question he founded on the theory of chemical substitutions. The temperature of vapours and the conducting power of gases formed the subject of his researches up to 1861. Until then gases had been considered as non-conductors. He proved that hydrogen conducts heat in the same way as do solid bodies, and thereby established a new and striking analogy between this element and metals.

During the last years of his life the radiation of heat formed the chief object of his researches. A paper on the polarisation of the dark rays of heat, the discovery of the diathermanous nature of native chloride of potassium, and lastly a full research on the emission, absorption, and reflexion of heat radiated at low temperatures, were the results of this protracted and fertile investigation. He showed that heat from different sources is refracted under different angles, and absorbed in different proportions by the chlorides of sodium and of potassium, by fluor-spar and other substances. He thus proved, that, if our eyes were able to distinguish different rays of heat, we should see the different substances glowing in the most varied colours at ordinary temperatures, just as we see them emit different rays of light when exposed to heat and observed with the spectroscope.

The receptacle of all his researches is the "Annals of Chemistry and Natural Philosophy" (*Annalen der Chemie und Physik*), published by his friend Poggendorff. He formed a fine collection of scientific apparatus, afterwards bought by the University and put under his control. As a lecturer, Magnus was a pattern of clearness. He loved teaching, and his diction, though plain, showed the high culture of his mind. While in his lectures he aimed at being comprehensible to the large number of students who wished to learn the rudiments of science, he instituted special classes for those who longed to enter into a deeper study of natural philosophy. Graduates and undergraduates assembled around him once a week, to enjoy what he called physical conversations. Here students in turn reported on investigations recently published, the master criticising the report and opening a discussion on those points which appeared to deserve a fuller explanation. Some favoured pupils were instructed in the methods of physical researches in his private laboratory, the master allotting subjects to them, urging them above all to exactness, and warning them against drawing hasty conclusions from their experiments. Many professors of natural philosophy, who have since obtained fame or reputation, have been educated in these classes. From a long list of names we will but mention those of Tyndall, Clausius, Wiedemann, Heusser, Quincke, Palzow, Villari, and Kundt. The laboratory joined his apartment, and he was thus enabled to watch from his sick-bed the investigations that occupied his thoughts. Magnus's health had been impaired for many months. He was suffering when he visited the last meeting of the British Association at Exeter. He was ill when he presided at the banquet given to Professor Hofmann on the 8th of January. Still he continued his work up to the beginning of February, when weakness and excruciating pain forced him to give up his lectures. He foresaw his death, and made the most minute arrangements, order being one of the characteristic properties of his mind. "I have written to you to ask for your advice," he addressed his physician, "but I foresee that my case will give you but little satisfaction."

Magnus married the daughter of M. Humblot, a well-known publisher. His wife, as well as two daughters and one son, survive him. The circle that assembled at his house was very large, and included the leading members of every profession, members of the university, merchants, statesmen, and artists. But he was equally accessible to every unknown youth who wanted his advice and assistance. Ever ready to help, he bestowed his aid, as if he received, not as if he conferred, a favour. Gentle-mannered, conciliatory, and persuasive, he was the mediating element of every society. Nothing can show better the kindness of his disposition than the love which, not his family or his pupils only, but even his domestics, bore for him. A faithful laboratory servant, who took care of his instruments and also nursed him through his last illness, bears witness that he could not endure to see unhappiness or unpleasantness around him. "Why," he would ask sometimes, "will you make life difficult to yourselves? Is it not sufficient your Master should make it difficult for you?"

His death is therefore felt, not only as a severe loss to science, but as a personal pain, by all who had the good fortune of approaching him. Numerous were those who, on the 8th of April, thronged the room where his coffin stood, hidden under palms and flowers. Some parting words were spoken by Dr. Müllensiefen, Professor of Divinity, and a song of Mendelssohn, sad, yet cheering, ascended from his grave.

A. OPPENHEIM

* The latter investigation contained an error which was afterwards corrected by its author, and originated a new discovery. Magnus found that sulphur acquired a deep red or black colour when fused with minute quantities of various organic substances. This change was at first ascribed by him to an allotropic modification of the element.

RECENT ADDITIONS TO THE ZOOLOGICAL SOCIETY'S GARDENS

In April last the total number of additions to the living collection of the Zoological Society of London was 124. Of these, twenty-one were born in the Gardens, forty-four were acquired by presentation and fifty-two by purchase, while one was received in exchange, and six merely "on deposit." The "departures" during the same period, by death and otherwise, were ninety-one. Among the more noticeable additions were :—

1. A female of one of the smaller forms of Rusine deer, purchased by a dealer on the 13th of April, and stated to have been received from the Philippines. This animal is quite distinct in its small size and dark brown fur from any other member of the group now or lately in the Society's collection. If the assigned locality is correct it may probably belong to the Rusa deer of the Philippines, which was first named *Cervus mariannus* by Desmarest, as having been found living by the French naturalists during the voyage of the *Uranie* at the Marianne Islands. Here, however, it was stated to have been introduced from the Philippines. It would be very desirable to increase our knowledge of the deer of the Philippines. Probably there is more than one species that occurs there.

2. A Sooty Crow-Shrike (*Strepera fuliginosa*), purchased on the same day, is one of a peculiar group of Australian birds, of which the Society previously possessed examples belonging to two other species. These are all placed in the cages outside the "Parrot-house," which are devoted to the reception of the hardy species of crows (*Corvidæ*) and their allies, and at the present moment contain examples of several other species of great interest, such as the yellow-billed chough of the Alps (*Pyrrhocorax alpinus*), the chough of our own coasts (*Fregilus graculus*), the Chinese Jay-thrush (*Garrulax sinensis*), and the Australian crow (*Corvus australis*).

THE HUIA BIRD (*Heteralocha gouldi*)

3. A Vulturine Guinea-Fowl (*Numida vulturina*) presented by Dr. John Kirk, C.M.Z.S., H.B.M. Acting Consul at Zanzibar.

For many years this remarkable Guinea-Fowl, which is peculiar for having the head entirely devoid of feathers, and for the long ornamental hackles surrounding the neck, was only known to naturalists from a single specimen, formerly in the United Service Museum. This was figured in Mr. Gould's "Icones Avium," but its exact locality was unknown. More recently, since the eastern coast of Africa has been more thoroughly explored, it has been discovered that this bird is by no means uncommon on the southern part of the Somali coast, and in the adjacent parts of continental Africa. Dr. John Kirk, the well-known companion of Dr. Livingstone in the Zambesi expedition, who has been lately resident at Zanzibar, as acting Consul, has communicated several notices upon this Guinea-fowl to the Zoological Society, of which he is an active correspondent. In one of his letters he says that "it seems to be common at Lamoo, a port situated on the east coast of Africa, in about 2° S. lat. The officers of H.M.S. *Syria*, when lately there, saw several in the market, but used them for the table, not being aware of their great rarity." More recently Dr. Kirk succeeded in securing for the aviaries of the Society the present female example of the species. This fine bird was procured at Brava on the southern part of the Somali coast, just to the north of the equator, and was conveyed, along with a collection of other animals presented to the Zoological Society by Dr. Kirk, in the steamer, *Malta*, through the Suez Canal to Marseilles, under the kind care of Captain Mackenzie.

4. A jackal, stated to have been brought from the River Fernand Vas, south of the Gaboon, and to be the animal referred to in Du Chaillu's well-known "Explo-

rations and Adventures in Equatorial Africa" (p. 243), in the following passage :—

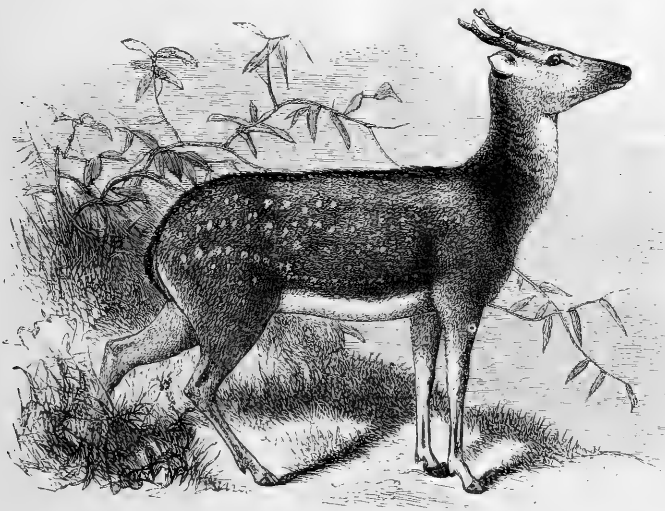
"Before we got to town again, I shot a *mboyo*, a very shy animal of the wolf kind, with long yellowish hair, and straight ears. I have often watched these beasts surrounding and chasing small game for themselves. The drove runs very well together, and as their policy is to run round and round, they soon bewilder, tire out, and capture any animal of moderate endurance."

Such is M. Du Chaillu's fragmentary notice of this animal, of which, however, he does not appear to have sent home specimens. After endeavouring in vain to find a name for this distinctly-marked species of *Canis*, which is readily recognisable by the black and white stripes along the flank, and the long black tail with a black termination, I proposed at the meeting of the Zoological Society, on May 12th, to call it *Canis lateralis*. Dr. Peters, however, has since suggested to me that it may be the *Canis adustus* of Sundeval, and this identification is probably correct, although our example does not agree very well with Sundeval's description. I may also remark that there is no example of *Canis adustus* in the British Museum, and that, without the aid of specimens, the differentiation of the various species of *Canis* is by no means an easy task.

In the month of May the number of additions to the Society's collection of living animals was still more numerous than in April, amounting altogether to 200. Of these fifty-four were received as presents and eighty-three acquired by purchase ; forty-five were bred in the gardens and eighteen were deposited for safe custody. The number of departures in May, by death or otherwise, was seventy-five.

Amongst the 200 acquisitions in May were several of great interest, viz. :—

1. A male deer, forwarded to the Gardens from Singapore by order of H.R.H. the Duke of Edinburgh. This is quite different from any other deer yet obtained living



PRINCE ALFRED'S DEER (*Cervus alfredi*)

by the Zoological Society. It is obviously allied to the axis or spotted deer of continental India, and may be the Malayan form of that species. It differs, however, in its smaller size and smaller ears, and in the dark coffee-brown colour of the fur. I have not been able to find any designation applicable to it, and, at a recent meeting of the Zoological Society, have proposed to call it *Cervus alfredi*, after the Prince, who has transmitted the present individual to us.

2. Three examples of the crested or bladder-nosed seal of the Arctic Seas (*Cystophora cristata*), purchased May 5, out of a whaling vessel, which brought them into Dundee. These are the first examples of this seal that have reached the Society's gardens alive, and are of much interest, as making us acquainted with the external form of a very distinct genus of the *Phocidæ*. The bladder-like excrescence on the forehead, which attains an extraordinary development in the adult male, is but very slightly shown in these young animals, but a look in the mouth at once shows their difference from the typical seals of the genus *Phoca*.

3. A huia-bird (*Heteralocha gouldi*), from New Zealand, purchased May 18th. This bird is remarkable for the great difference in the shape of the bill between the two sexes, as will be readily understood by reference to the accompanying illustration, in which the short-billed individual is the male and the long-billed the female. The figure of the male is taken from the Zoological Society" specimen, the head of the female is copied from Mr. Gould's plate of this species in the "Birds of Australia." Such a divergence in the structure of the beak of the two sexes is very uncommon and scarcely to be paralleled in the class of birds. It is difficult to guess at the reason of it, or to explain it on Darwinian or any other principles. The story is that the male employs his stronger instrument to hew away the wood that covers the grub, and the female her more delicate organ to extract the precious morsel. Unfortunately, we have as yet only one sex of

this bird living in the Society's Gardens, but it is so far certain that our male is evidently very fond of grubs, and will search for them with eagerness in rotten wood. He does not, however, seem obliged to wait for a female of his own species to extract them when discovered, but picks them out for himself. A somewhat parallel case of difference of the bill in the male and female occurs in the humming-birds of the genus *Grypus.**

4. A Tuatera lizard (*Sphenodon punctatum*), purchased May 20th.

This not very attractive-looking lizard is really one of the most extraordinary reptiles now known to exist on the world's surface. In several important particulars of structure it differs from every other known saurian, insomuch that Dr. Günther, who has published an elaborate memoir on its anatomy,† has proposed to constitute it of itself a distinct order of reptilia, equal in systematic rank to the ophidians and saurians. It differs from all the known members of the latter order in having the quadrate bone firmly anchylosed to the skull, and in the entire absence of an intromittent copulatory organ. The vertebra are amphicælian, as in the Geckoid lizards. Dr. Gray first described and figured this reptile under the name *Hatteria* (!) *punctata*, and it has been so generally designated until lately, when it was most fortunately discovered that the generic term *Sphenodon* had been previously applied to a specimen of its skull in the museum of the College of Surgeons. It has thus become possible not to be obliged to employ so vile and barbarous a term as *Hatteria* for the name of this important animal. Dr. Günther, when he wrote his memoir, supposed that this reptile was extinct or nearly so. But one living example has reached England since that date, and more than one, I believe, in spirits. From an article published by Dr. Bennett, of Sydney, in the *Morning Herald* of that city, it appears that so recently as December 1851, this lizard was abundant in one of the islands in the Bay of Plenty, on the north coast of New Zealand. The island in question is one of four small volcanic islands, distant about eight miles from the coast, and situated opposite to the mouth of the Wakatane river. A party of officers, who visited it upon the occasion referred to, are stated to have collected in half an hour nearly forty of these lizards of different sizes, varying from two feet long to three inches. They stated that the island seemed to be swarming with them, and with another lizard called the moko-moko (*Tiliqua zeelandica*). In the day-time these lizards are seen basking themselves in the sun on the bare rocks. Noon is therefore the best time to visit the islands. It is stated that there are four small islands, on two of which Tuateras are found.

I mention this fact in case it should be within the power of any of the Antipodean readers of NATURE to visit these islands, and obtain examples of this reptile. For although the British Museum has a good supply of specimens of it, yet the animal is a great *desideratum* elsewhere, and I believe there are no examples of *Sphenodon* in any of the continental collections.

P. L. S.

NOTES

THE honorary degree of D.C.L. has been conferred, at the recent Commemoration, by the University of Oxford on the following scientific men:—Sir William George Armstrong, C.B.; Sir James Alderson, M.D., President of the Royal College of Physicians; John P. Gassiot, Vice-President of the Royal Society; Charles W. Siemens, F.R.S.; James Fergusson, F.R.S.; Sir J. Kay-Shuttleworth, Bart., the Rev Henry Moseley, M.A., F.R.S., Canon of Bristol; Professor Hermann Helmholtz; George Edward Paget, M.D., President of the General Medical Council; Edward Frankland, F.R.S.; Henry Bence Jones, M.D., F.R.S.; Warren De La Rue, Vice-President of the Royal Society; William Huggins, F.R.S., Secretary to the Royal Astronomical Society. The name of Charles Darwin, F.R.S., would have been included in the foregoing list (as stated in our last number) but he writes that his health is such "that he could not withstand the fatigue and excitement of receiving an honorary degree." We understand that Prof. Helmholtz has also been prevented from attending. There is a rumour that Science would have been even more brilliantly represented if the degree were the simple thing it is often supposed to be. It really stamps, it seems, a judicious mixture of celebrity and orthodoxy; *e.g.*, either much orthodoxy and a little celebrity, or a little orthodoxy and much celebrity, will qualify, but a dash of orthodoxy is *de rigueur*. The *imprimatur*, therefore, is of double value. In the present case, for instance, it is proclaimed to the world that Mr. Darwin, for example, is not only Mr. Darwin, but that Dr. Pusey, and others even more skilled in heresy than he, consider him orthodox. On the whole we should prefer the abolition of tests even here, and one has only to go to Oxford and watch the present scientific activity, the magnificent museums and laboratories which are growing or have grown, to predict that the Oxford of a few years hence will be of the same opinion.

MEETINGS of the Royal Commission on Scientific Instruction and the Advancement of Science have been held at 6, Old Palace Yard, S.W., on the 14th, 15th, 17th, and 21st of this month. Present:—The Duke of Devonshire, K.G.; the Marquis of Lansdowne; Sir John Lubbock, Bart., M.P., F.R.S.; Sir J. P. Kay Shuttleworth, Bart.; Mr. B. Samuelson, M.P.; Dr. Sharpey, Sec., R.S.; Prof. Huxley, F.R.S.; Dr. W. A. Miller, Treas., R.S.; Prof. Stokes, Sec. R.S.; and the Secretary, Mr. J. Norman Lockyer, F.R.S.

MR. E. J. STONE, F.R.S., first assistant at the Greenwich Observatory, has been appointed Astronomer at the Cape of Good Hope. This appointment will be hailed with the liveliest satisfaction by all scientific men, and we may hope that the fine Observatory there may soon take high rank among similar establishments.

THE examiners for honours in the Natural Science School at the University of Oxford, viz., Henry J. S. Smith, Edward Chapman, and Joseph F. Payne, have made the following award:—Class 1. Walter William Fisher, Postmaster of Merton College; Edwin Harding Lendon, Gunsley Exhibitioner, University College; Charles Samuel Taylor, Commoner of Merton College. Class 2. John Fleming Hartley, Commoner of Brasenose College. Class 3. John Richardson Burrow, Thanet Exhibitioner of Queen's College. Class 4. Nil.

WE hear with great satisfaction that the Government of India has ordered the adoption of the metric system of weights and measures.

A NEW Astronomical Observatory has been established by the Government of the Argentine Republic in South America, to be erected at Cordova, about the middle of the continent, on the margin of the Pampas, in lat. $31\frac{1}{2}°$ S. Dr. B. A. Gould has been invited to organise it, and is going out for the special purpose of extending through the southern hemisphere the system of zones, which Bessel and Argelander have already carried from the north pole as far as 30° S. He hopes also to obtain some photometric determinations of the principal southern stars. The undertaking has been instituted and carried out entirely by the Government of the Argentine Republic, at the instance of the President, M. Sarmiento, and of Dr. Avellaneda, the Minister of Public Instruction; but the various scientific institutions of the United States have aided the expedition greatly by loans of important and valuable instruments; and Dr. Gould expresses his obligation to the Coast Survey, the "American Nautical Almanac,"

* See Gould's Monograph of the Trochilidæ, Introduction, p. xxxvi.
† Phil. Trans. 1867, p. 595.

the Washington Observatory, the National Academy of Sciences, and the American Academy of Boston, all of which have afforded valuable assistance in providing him with instruments and equipment. This will be the second public observatory in South America, that at Santiago, in Chile, having been founded in 1851. Efforts are making to provide means for obtaining photographic impressions of some of the more prominent southern clusters of stars, analogous to those taken in the northern hemisphere by Mr. Rutherford; but the success of these efforts is still uncertain. Dr. Gould estimates that three years will suffice to complete the southern zones within the limits which he has assigned to himself. We look forward with the most sanguine hopes to the results of Dr. Gould's labours. In time we may hope to be almost as civilised as the Argentine Republic—almost as anxious to spread the knowledge of Nature.

WE learn from the *Academy* that the Philosophical Faculty of the University of Göttingen has announced for the 11th March, 1873, a prize of 500 thalers in gold, and a second prize of 200 thalers in gold on the Beneke foundation, for the best new determination of the atomic weights of the metals of the earths. The limits of error in the results obtained must be exactly fixed, and the investigation must be accompanied by a complete critical review of the existing scientific material connected with it. In his classical researches in this field, Stas ascertained the combining weights of ten elements, leaving those of five-sixths of the elements more or less unprecisely determined. It has been resolved, therefore, to subject some of the numbers to careful revision, and those attached to the earth-metals have been selected. The dissertation, written in Latin, French, German, or English, and distinguished by a motto, must be deposited with the Dean of the Faculty on or before August 31, 1872.

THE Lecture, next Sunday evening, at St. George's Hall, Langham Place, is on "Cruelty in Relation to Lower Animals," by T. Spencer Cobbold, M.D., F.R.S.

THE Anniversary Meeting of the Society of Arts will be held on Wednesday next, the 29th inst., at four o'clock.

THE indifference of agriculturists to scientific research has been again illustrated by the refusal of the Council of the Royal Agricultural Society to publish an account of the investigations which have established the truth of the old bucolic dogma, that berberries produce rust on wheat growing in their vicinity. There is now no doubt that the berberry-rust and the wheat-rust are two different stages in the genetic cycle of *Puccinia graminis*.

HER Majesty's Commissioners for the International Exhibition of 1871 have resolved to set aside one guinea out of every season ticket sold at three guineas, through the Society of Arts, for the purchase of works of art and industry, out of the exhibition, the same to be circulated throughout the United Kingdom.

THE *Pharmaceutical Journal* announces that a new series will be commenced next month, when it will appear in a new form, and as a weekly publication, the first number to be published on the 2nd of July. The *Pharmaceutical Journal* was originally established by the late Mr. Jacob Bell, as an organ of communication especially devoted to the interests of the Pharmaceutical Society, which was founded at the same time, and it has been published monthly during the last twenty-nine years.

AN industrial and technological museum has been recently formed in Victoria. It is connected with the Gallery of Art and the Public Library at Melbourne, and is governed by the same body of trustees. In the Library there are over 50,000 volumes, and in the Gallery of Art pictures by Goodall, Webb, Graham, Lee, &c., besides several pictures by colonial artists and a very large collection of casts from the antique. Before this museum was formed a Royal Commission had been appointed for promoting industrial instruction, and resulting from this movement we may add, that there are now no less than six schools of design open in Melbourne and the suburbs, with over 600 pupils in attendance.

THE statistics given in M. Bouley's course of lectures on "Madness in Man and Animals" confirm the statement that hot weather is not a cause of *rabies;* out of 302 cases recorded in six years, eighty-nine occurred in the spring from March to May, seventy-four in the summer from June to August, sixty-four in the autumn from September to November, and seventy-five in the winter from December to February. Male animals appear far more subject to the attacks of the disease than female animals. Out of 320 cases of bites from rabid animals, 284 occurred with dogs (male), twenty-six with bitches, five with cats (male and female), and five with wolves (male and female). No instance is recorded of any attack on man by a rabid herbivorous animal. Now that we are approaching the dog-days, we commend these facts to the notice of the chief commissioner of police, and trust we shall have no repetition of the cruel and senseless police regulations as to the muzzling of dogs; to be consistent they should be in force all the year round.

WE learn from the *British Medical Journal* that Miss Garrett has just passed her final examination for the degree of Doctor of Medicine in Paris. Her thesis has been read, and at the same time she received her degree from the Faculty of Medicine. Whatever opinion may be entertained, says our able contemporary, as to the desirability of ladies studying and practising medicine, everyone must admire the indomitable perseverance and pluck which Miss Garrett has shown in overcoming the many obstacles to obtain in the first place the qualification of the Apothecaries' Company in London, and, lastly, the Degree in Medicine of the University of Paris.

MR. CYRUS REDDING, who died recently at a very advanced age will, perhaps, be best known as the author of a "History and Description of Modern Wines," which was first published in 1833, and has passed through several editions, being the standard work on the subject. Among his MSS. he has left a "Wine-book of Europe." Mr. Redding has enjoyed for the last few years a government pension of 75*l.* per annum, which it may be hoped, will be continued to his aged widow.

THE patent for printing photographs by a permanent process known as the Woodbury type, has been purchased by Mr. Vincent Brooks, of Gate Street, Lincoln's Inn Fields, on behalf of a new company.

WE have just seen a bill-head or order to which we think it necessary to call attention. On a scroll at the top is a name which, together with the address, which is on another scroll, we suppress, as we do not wish to assist this person in his advertisements. On other artistic scrolls we find the occupations of the advertiser, dyeing and printing works being set forth on one of them. In the centre is a coat of arms and crest surrounded by a garter, on which is printed *Fellow of the Chemical Society, London.* We have no wish to infer that this gentleman is not a most eminent chemist, but we do most emphatically protest against the membership of a learned society being turned to account for advertising purposes. We hope that the Council will not allow this to continue unnoticed, for nothing could be more damaging to the welfare of a scientific society.

A RECENTLY published part of Baillon's "Histoire des Plantes" contains a monograph of the Papilionaceous section of *Leguminosæ* executed with his usual care and wealth of illustration. We are glad to hear that an English translation of the work is announced.

"MITTHEILUNGEN der Anthropologischen Gesellschaft in Wien," is the title of a periodical which the Anthropological Society of Vienna have begun to publish, with accounts of their own proceedings and papers, of discoveries of ancient remains in tumuli and elsewhere, and papers in which questions in their special pursuit are discussed. Two numbers are out. The first contains an opening address by Rokitansky, and among the contributors we notice the names of F. Müller, Graf Augustus von Breuner, F. von Hauer, and Freiherr von Sacken. Austria is so rich in ethnological varieties and relics that interesting matter sufficient for a monthly periodical must, we should think, be always forthcoming. It will be an acceptable addition to the scientific libraries of this country, and we offer to the society our best wishes for its success.

KARL VON LITTROW'S "Zählung der nördlichen Sterne im Bonner Verzeichnisse nach Grössen" has been reprinted from the Sitzungsberichte der k. Akademie der Wissenchaften. He estimates the number of stars to the sixteenth magnitude (or more exactly to the magnitude 15·8) at 588 millions for the northern hemisphere, and about 1,200 millions for the whole heavens.

"NOTES AND QUERIES for China and Japan," which has just entered on a new series, is a monthly medium of inter-communication for professional and literary men, missionaries, and residents in the East generally, conducted by C. Langdon Davies. In the number just received we find an article on the Fung tree (*Liquidambar formosana* Hance), the leaves of which afford food to a species of caterpillar (termed the wild silkworm), which produces an inferior kind of silk.

THE "Proceedings of the Bath Natural History Society and Antiquarian Field Club" for 1870, are mainly occupied, as they should be, by papers of local interest, which well illustrate the great wealth of the districts to the naturalist, whether geologist, botanist, or antiquarian. Thus we have "The Mammalia and other Remains from Drift Deposits in the Bath Basin," by C. Moore, with copious lists of organic remains found in the pre-historic alluvium and gravel deposits; "Remarks on some of the Fungi met with in the neighbourhood of Bath," by C. E Broome (edible fungus hunting seems to be a favourite pursuit with local field-clubs since the Woolhope naturalists set the example); "Notes on the Chapel and Hospital of St. Mary Magdalene," by Rev. W. Stokes Shaw; and "Notes on a pair of Celtic Spoons found near Weston, Bath, in 1866," by Rev. Preb. Scarth. Of articles of a less local character, we have "Chemical Geology," by Charles Ekin, containing a sketch of recent spectroscopic researches; and a pleasant gossiping paper, by Rev. H. N. Ellacombe, on "The common English names of Plants."

A REPORT from Mr. R. S. J. Ellery, Government Astronomer, on the subject of the Equatorial Telescope at Melbourne, has been received by the Victorian Legislative Council. The telescope arrived in November 1868; its erection at the Observatory was commenced early in July 1869, and the building for its protection was not finished till the 1st June, the final fitting up of the telescope being completed early in July. Observations were attempted as early as April last year, but the telescope was not in working order till the middle of August, since which date observation has progressed more or less satisfactorily. The principal work has consisted in examination and mapping of nebulæ, but the very unfavourable weather throughout almost the whole season has prevented much progress of other work. Positive observations have been made of Winnecke's periodical comet. With respect to spectroscopic observations, Mr. Le Sueur, the astronomer in charge, says—The spectroscope furnished by Mr. Grubb has already proved of much service for nebulæ work. For star work, so far as I can at present judge, it is unsuitable; nevertheless, a very important observation has lately been made therewith, showing that the spectrum of the principal η Argo is crossed into bright lines. In his report to the Board of Visitors in April last, Mr. Ellery referred to the construction of the telescope, but could not at that time speak of its performance or capabilities; but during the several months' use since then Mr. Le Sueur has tested its performance most carefully, and although he has had no prior experience with reflecting telescopes of such dimensions as this, he has been enabled from frequent observations to form a sound opinion of its powers. With the large mirror first used the telescope certainly did not perform so satisfactorily as could be desired, and making all allowances for atmospheric disturbances, the definition was never good; but with the other mirror (supposed B) it became very much better, and Mr. Le Sueur speaks of its performance now as far more satisfactory. The building for the protection of the telescope is in most respects satisfactory. There are several arrangements and appliances yet required, before the telescope can be said to be properly provided, among which are more convenient observing seats, drawing stands, and the erection of a platform outside the telescope room for photographic operations. Some of these are already in progress, and, should sufficient means be available, will soon be completed.

IN the recently issued Colonial Blue Book ("Reports on the state of H.M. Colonial Possessions," part I., West Indies), the Governor of Jamaica reports as follows:—"The cinchona plantation is a most interesting experiment, which may now be pronounced a complete success. Cinchona plants were first received here in 1866. By the close of 1867 the number of young plants had so much increased, that it became necessary to provide land for their final establishment on a planter's scale. Six hundred acres of virgin forest in Blue Mountain were acquired early in the year, and were set apart for the purposes of a cinchona plantation, for which the place is in every way admirably suited. The elevation above the sea ranges from 4,000 to 6,000 feet. It is well watered, has the best aspects, and possesses a soil reported to be admirably suited to the requirements of the cinchona. Fifty acres were cleared, of which forty were filled with cinchonas in the course of the year; about 20,000 plants of five different species were planted. By the latest accounts all of these were in full vigour, and the plantation must by this time be doubled in extent. The plants have stood one of the dryest seasons that has ever been remembered on Blue Mountain, without suffering in the least. There is now no doubt that the cinchona can be successfully reared in Jamaica." The plants for sale, deliverable in the spring of 1869, were applied for to the number of about 2,000 only; but Sir James Grant expresses the hope that with the growth of the plants a spirit of intelligent enterprise will grow amongst the proprietors of mountain wastes, sufficient to induce them to turn some attention to such a highly promising experiment.

THE *Journal of the Franklin Institute* describes a new explosive which has been invented by Mr. Noble, the inventor of nitro-glycerine and dymanite, and which he calls dualine. It consists principally of nitrate of ammonia and very fine saw-dust which has been acted on by nitro-sulphuric acid. It is said not to be decomposed by accidental contact with acids, and will not congeal or lose any of its properties during cold or hot weather. Its explosion does not produce any noxious gases, and it will burn in the open air without exploding.

THE *Architect* states that the North-Eastern Railway Company is forming a new line between Gilling and Helmsley, which passes through the Caulkless spur of the Hambletons, in a deep cutting near Stonegrave. Here, at a depth of nearly thirty feet, a large chamber in the Oolite rock has been discovered. It is as large as an ordinary room, and has three openings from it. The cave is on the same horizon as the famous Kirkdale cave.

MR. MELDRUM ON THE ORIGIN OF STORMS IN THE BAY OF BENGAL *

THE writer commenced by observing that in various papers published during the last ten years, he had stated, as the result of an examination of a large body of observations, that the tropical cyclones of the Indian Ocean, south of the Equator, originated between two contrary streams of air, viz., the N.W. monsoon and the S.E. trade-wind; and, in a paper read on the 10th of November last, he remarked that what had been found to hold good in that part of the ocean might be found to do so generally. As the observations collected by the Society only referred to the Indian Ocean, he could not directly test the matter with regard to the cyclones of other parts of the world. But cyclones also occurred in the Indian Ocean, north of the Equator, and as the Society possessed observations which had been taken there, he proposed to examine the records with a view of ascertaining whether these cyclones were formed, as he believed those south of the Equator were, between two oppositely directed currents of air which had pre-existed. It was to that point alone that he wished to direct attention at present. How the barometric depression in the heart of a cyclone was formed, whether owing to an ascending current, to condensation of vapour, or to other causes, why the air moved more or less round a central area in a particular direction, and why the cyclone had a progressive movement, were subjects upon which he would not then touch; for the question of the existence or non-existence of opposite winds, previously to the formation of the cyclone, had, in his opinion, an important bearing upon all the others, and should therefore be taken up first.

The cyclones of the Indian Ocean, south of the Equator, as was well known, took place during six months of the year, viz., November to May. During that period the N.W. monsoon prevailed from near the Equator to 10° or 15° S., sometimes stretching as far south as the tropic. Still farther south the S.E. trade-wind prevailed. The line or belt separating the two winds often ran obliquely across the ocean from 18° S., near Madagascar, towards the Straits of Sunda. It was in that belt of comparatively low barometer, calms, and variables, that the tropical cyclones of the Indian Ocean, south of the Equator, were formed. The N.W. monsoon was a continuation of the N.E. trade-wind of the northern hemisphere. This might be seen on almost any day from November to April or May, by laying down the directions of the wind at a sufficient number of points; and the daily charts which had been prepared for various periods showed it very clearly. On examining those for February, 1861, for example, which had lately been lithographed, it would be seen that the N.E. trade-wind prevailed over the Bay of Bengal and the Arabian Sea, that as it approached the Equator it became more northerly, and after crossing the Equator into the southern hemisphere it became the N.W. monsoon. The southern limits of the N.W. monsoon, and the northern limits of the S.E. trade, or, in other words, the position of the belt of variables between them, moved backwards and forwards according to the season. It was farthest S. when the southern hemisphere was warmest. As the temperature decreased, towards the end of March, this belt retreated northwards with the sun, came up to the Equator, and crossed it into the northern hemisphere. In whatever part, N. or S. of the Equator, the belt of calms existed, the prevailing winds on either side of it were from opposite directions. When it was S. of the Equator, the prevailing wind to the southward of it was from the S.E. or E. (the S.E. trade), and to the northward of it from N.W. or W. (the N.W. monsoon.) The latter extended at least as far N. as the Equator, and the N.E. trade, of which it was the continuation, prevailed over the Bay of Bengal. The former at the same time prevailed as far south as the parallel of 30° or 40° S. When the belt of calms was N. of the Equator, the prevailing wind to the S. of it was from S.W. or West, and to the N. of it from N.E. or E. The former was the S.W. monsoon, and the latter the N.E. trade-wind. In July and August, when the belt was far N., the S.W. monsoon prevailed over the whole of the Bay of Bengal, and was a continuation of the S.E. trade-wind, just as the N.W. monsoon in February and March was a continuation of the N.E. trade-wind. The belt of calms followed the sun, moving from one tropic to the other, and often passing them. Hence, when it was at its northernmost limit, the S.W. monsoon swept over the Bay of Bengal, and when at its southernmost limit, the N.E. trade-wind did so. But at certain seasons, when the belt of calms stretched across the Bay, the S.W. monsoon blew over one part of it and the N.E. trade over the other.

As, then, observation had shown that the tropical cyclones of the Indian Ocean, south of the Equator, were formed in the belt of calms between the N.W. monsoon and the S.E. trade-wind, and nowhere else, there was at least a presumption that the cyclones of the Bay of Bengal were also formed in that belt, at those seasons when it stretched across the Bay, and separated the N.E. trade wind from the S.W. monsoon; and this presumption was strengthened by the fact that most, if not all, of the cyclones that occurred there, did so at the change of the monsoons; that is, when two contrary winds prevailed in the Bay, and were more or less in conflict.

These general considerations rendered it possible, if not probable, that the cyclones of the Bay of Bengal were formed between two contrary and pre-existing winds. But that was not sufficient. It was necessary to bring the matter to the test of facts; and this could only be done by examining the observations taken in particular storms. He would begin with the destructive storm which visited Calcutta on the 5th October, 1864. On the 12th September in that year, the ship *Furness Abbey*, Capt. Roddock, in 19° 08′ N. and 88° 55′ E., had a fresh breeze from W.S.W. and S.W., and she carried that wind to 4° 44′ N. and 92° 38′ E. on the 21st. The *Victoria Nyanza*, Capt. A. J. Reed, had a strong wind from S.W. on the 21st. Sept., in 18° 17′ N., and 87° 46′ E., and she carried that wind to 0° 45′ S. and 91° 02′ E. on the 25th. The French barque *Leonide*, Capt. Martin, outward bound, approached the Equator with the S.E. trade-wind, which gradually veered to S. and S.S.W., and from 1° 59′ N. and 84 05′ E. on the 6th, to 19° 29′ N. and 88° 27′ E. on the 13th Sept., she had fresh and strong winds from the S.W. Moreover, he had prepared a chart for the 21st September, which showed that on that day eleven vessels, from the Equator to 20° N., in the Bay of Bengal, had the wind from W.S.W. and S.W., in moderate and fresh breezes. These observations proved that up to that date the S.W. monsoon prevailed in the Bay. But a change was at hand. On the 26th September, the wind in the northern part of the Bay was light from the northward, and in the southern part moderate from westward. On the 29th September there was a strong breeze blowing from the W.S.W., with squally rainy weather from near the Equator to at least 10° N., whilst in the northern part of the Bay the wind was light from the N. On the 2nd Oct. there were signs of a cyclone. To the S.W. of the Nicobars a strong breeze was blowing from the W.S.W., with squally rainy weather. In the Gulf of Martaban there was a gale from the S.E., with much rain and lightning. To the S.E. of Coringa the wind was increasing from N.E., with thunder and lightning. On the 3rd and 4th there was strong evidence of the existence of two contrary winds, the one from N.E. and the other from S.W., with a cyclone between them; but the S.W. wind was apparently overcoming the other. On the 5th, when the storm was at Calcutta, the S.W. wind had established itself over the greater part of the Bay. But this was only a temporary victory, for by the 8th the N.E. wind was blowing fresh over the northern portion of the Bay. The S.W. wind, however, still prevailed farther south. By noon on the 15th the N.E. wind prevailed over the whole Bay.

He had not been able to examine the subject farther, but would return to it at next meeting. In the meantime, he thought that the evidence adduced went to show that the storm originated in the belt of calms between the N.E. trade and the S.W. monsoon.

* Paper read before the Meteorological Society of Mauritius, March 24, 1870.

CHEMISTRY

Specific Gravities of Aqueous Solutions

IN Gerlach's *Sammlung der specifischen Gewichte wässeriger Lösungen* is a large amount of information which will prove of great use to manufacturers and others who have to deal with aqueous solutions of acids, alkalies, and salts.

The first table consists of nine columns marked with letters. In column A are placed the formulæ and combining weights, according to the old notation, of the bodies dissolved, both in the anhydrous and hydrated condition. Column B contains the weight of the dissolved body in the hydrated condition, or with water of crystallisation, which is present in 100 parts by weight of the solution. C shows the weight of the dissolved substance in the anhydrous condition. The numbers in this column may be calculated from those in the second column by multiplying by the combining weight of the anhydrous and dividing by that of the hydrated substance. Column D gives the weight of the body in the dry state, which is dissolved in 100 parts of water, and is calculated by multiplying the numbers in column C by 100 and dividing by 100—C :—

$$D = \frac{C \times 100}{100 - C}$$

Column E contains the number of atoms of the anhydrous salt in 100 parts by weight of water. The expression *atom* is here synonymous with *equivalent*. The atom of hydrogen is taken at $\frac{1}{100}$:—

$$E = \frac{D \times 100}{A \text{ (anhydrous)}}$$

F gives the volume of the solution; 100 parts by weight of the water of the solution being taken as 100 volumes :—

$$F = \frac{D \times 100}{\text{spec. grav.}}$$

G indicates the specific gravities of the solutions. H contains the volumeter degrees, according to the scale of Guy Lussac, which correspond to the specific gravities :—

$$H = \frac{100}{G}$$

In column I are found the names of the observers, the temperature, and the references to the sources from which the numbers were obtained.

In this first table we find the various numbers corresponding to solutions of different states of concentration. In some cases the numbers are given for solutions at intervals of 1 per cent. of the salt, in others of 5 per cent., and in others of 10. The table commences with caustic alkalies, including ammonia, potash, and soda. Then follow the potassic and sodic carbonates, the chlorides of ammonium, potassium, sodium, lithium, aluminium, magnesium, calcium, strontium, barium, cadmium, and zinc, and stannous and stannic chlorides. The next section contains the bromides of potassium, sodium, lithium, magnesium, calcium, strontium, barium, cadmium, and zinc; whilst under the iodine compounds we find potassic, sodic, lithic, magnesic, calcic, strontic, baric, cadmic, and zincic iodides. Next comes sodic hyposulphate, and the sulphates of ammonium, potassium, sodium, manganese, and iron, the double sulphate of iron and ammonium, magnesic sulphate, potassia-magnesic sulphate, and the sulphates of zinc and copper. This series is followed by sections containing potassic chromate and bichromate, hydric disodic, and trisodic phosphates; hydric disodic, and trisodic arseniates; nitrates of potassium, sodium, magnesium, strontium, barium, and lead; chlorates of potassium and sodium; bromates of potassium and sodium, iodates of potassium and sodium; potassic ferrocyanide and ferricyanide; plumbic acetate; potassic and sodic tartrate; and Rochelle salt. The remainder of the table is devoted to the acids, and includes the following:—Hydrochloric, sulphuric, sulphurous, phosphoric, arsenic, nitric, acetic, tartaric, and citric.

After the table follows a chapter discussing the relations existing between the specific gravities of equally concentrated solutions; and three others: On the change of volume produced by solution of salts; on the change of volume produced on the dilution of aqueous solutions; and on the change of volume produced by mixing different solutions.

The pamphlet concludes with a table extending over 19 pages, and containing the specific gravities of solutions, in most cases from 1 per cent. to nearly the point of saturation, though in some few instances they are given at every 5 per cent. This table gives, in addition to those of the substances above enumerated, the specific gravities of solutions of sugar and alcohol. Dr. Gerlach deserves the thanks of chemists and chemical manufacturers for undertaking the tedious labour of collecting and arranging in tables the large series of numbers which are found in this pamphlet.

SCIENTIFIC SERIALS

THE *American Journal of Science* for May, 1870, contains a good article "On a simple method of Avoiding Observations of Temperature and Pressure in Gas Analyses," by Wolcott Gibbs, M.D., Professor in Harvard University.

In absolute determinations of nitrogen and other gases, accurate observations of temperature and pressure are, in the ordinary methods of analysis, necessary, and when made require subsequent calculations which, when the analyses are numerous, become rather tedious. By the following simple method these observations may be altogether dispensed with, and the true weight or the reduced volume of the observed gas, obtained at once by a single arithmetical operation.

"A graduated tube, holding about 150 cubic centimetres, is filled with mercury, and inverted into a mercury trough. Two thirds or three fourths of the mercury are then displaced by air, care being taken to allow the walls of the tube to be slightly moist, so as to saturate the air. This tube may be called the companion tube; the volume of air which it contains must be carefully determined in the usual manner by five or six separate observations, taking into account, of course, all the circumstances of temperature and pressure. The mean of the reduced volumes is then to be found, and forms a constant quantity. The gas to be measured is transferred from the receiver in which it is collected, into a (moist) eudiometer tube, which is then suspended by the side of the companion tube, and in the same trough or cistern. Both tubes being supported by cords passing over pulleys, it is easy to bring the level of the mercury in the two tubes to an exact coincidence. The pressure on the gas is then the same in each tube. The temperature is also the same, as the tubes hang side by side in the room set apart for gas analyses, and are equally affected by any thermometric change. It is then only necessary to read off the volumes of the gas in the two tubes to have all the data necessary for calculating the weight of the gas to be measured. . . . . As the observed volume of the air in the companion tube is to the observed volume of the gas in the measuring tube, so is the reduced volume of the air in the first—previously determined as above—to the reduced volume of the gas to be measured. This method of course applies to the reduction of any gaseous mixture whatever to the normal pressure and temperature. . . . . In practice, a companion tube filled with mercury will last with a little care for a very long time. Even when filled with water I have found that excellent results may be obtained, and that the tube will last for some weeks. Williamson and Russell, in their processes for gas analysis, have employed a companion tube for bringing a gas to be measured to a constant pressure, but the application made above is, I believe, wholly new."

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 19.—"A Ninth Memoir on Quantics." By Prof. Cayley.

It was shown not long ago by Prof. Gordan that the number of the irreducible covariants of a binary quantic of any order is finite (see his memoir "Beweis das jede Covariante und Invariante einer binären Form eine ganze Function mit numerischen Coefficienten einer endlichen Anzahl solcher Formen ist," Crelle, t. 69 (1869), memoir dated 8th June 1868), and in particular that for a binary quantic the number of irreducible covariants (including the quantic and the invariants) is = 23, and that for a binary sextic the number is = 26. From the theory given in my "Second Memoir on Quantics," *Phil. Trans.* 1856, I derived the conclusion, which as it now appears was erroneous, that for a binary quintic the number of irreducible covariants was infinite. The theory requires, in fact, a modification, by reason that certain linear relations, which I had assumed to be independent, are really not independent, but, on the contrary, linearly connected together: the interconnection in question does not occur in regard to the quadric, cubic, or quartic; and for these cases respectively the theory is true as it stands; for the quintic the interconnection first presents itself in regard to the degree 8 in the coefficients, and order 14 in the variables; viz., the theory gives correctly the number of covariants of any degree not exceeding 7, and also those of the degree 8, and order less than 14; but for the order 14 the theory as it stands gives a non-existent irreducible covariant

$(a, \ldots)^3(x, y)^{14}$; viz., we have, according to the theory, $5 = (10-6)+1$, that is, of the form in question there are 10 composite covariants connected by 6 syzygies, and therefore equivalent to $10-6=4$ asyzygetic covariants; but the number of asyzygetic covariants being $=5$, there is left, according to the theory, 1 irreducible covariant of the form in question. The fact is that the 6 syzygies being interconnected and equivalent to 5 independent syzygies only, the composite covariants are equivalent to $10-5=5$, the full number of the asyzygetic covariants. And similarly the theory as it stands gives a non-existent irreducible covariant $(a, \ldots)^8(x, y)^{20}$. The theory being thus in error, by reason that it omits to take account of the interconnection of the syzygies, there is no difficulty in conceiving that the effect is the introduction of an infinite series of non-existent irreducible covariants, which, when the error is corrected, will disappear, and there will be left only a finite series of irreducible covariants. Although I am not able to make this correction in a general manner so as to show from the theory that the number of the irreducible covariants is finite, and so to present the theory in a complete form, it nevertheless appears that the theory can be made to accord with the facts; and I reproduce the theory, as well to show that this is so as to exhibit certain new formulæ which appear to me to place the theory in its true light. I remark that although I have in my Second Memoir considered the question of finding the number of irreducible covariants of a given degree θ in the coefficients but of any order whatever in the variables, the better course is to separate these according to their order in the variables, and so consider the question of finding the number of the irreducible covariants of a given degree θ in the coefficients, and of a given order μ in the variables. This is, of course, what has to be done for the enumeration of the irreducible covariants of a given quantic; and what is done completely for the quadric, the cubic, and the quartic, and for the quintic up to the degree 6 in my Eighth Memoir (*Phil. Trans.* 1867.) The new formulæ exhibit this separation; thus (Second Memoir, No. 49), writing a instead of x we have for the quadric the expression $\frac{1}{(1-a)\ (1-a^2)}$, showing that we have irreducible covariants of the degrees 1 and 2 respectively, viz., the quadric itself, and the discriminant: the new expression is $\frac{1}{(1-ax^2)\ (1-a^2)}$, showing that the covariants in question are of the actual forms $(a, \ldots \between x, y)^2$ and $(a, \ldots)^2$ respectively. Similarly for the cubic, instead of the expression No. 55, $\frac{1-a^6}{(1-a)(1-a^2)(1-a^3)(1-a^4)}$, we have $\frac{1-a^6x^6}{(1-ax^3)(1-a^2x^2)(1-a^3x^3)(1-a^4)}$, exhibiting the irreducible covariants of the forms $(a, \ldots \between x, y)^3$, $(a, \ldots)^2 (x, y)^2$, $(a \ldots)^3 (x, y)^3$, and $(a, \ldots)^4$, connected by a syzygy of the form $(a, \ldots)^6 (x, y)^6$; and the like for quantics of a higher order.

In the present Ninth Memoir I give the last-mentioned formulæ; I carry on the theory of the quintic, extending the Table No. 82 of the Eighth Memoir up to the degree 8, calculating all the syzygies, and thus establishing the interconnections, in virtue of which it appears that there are really no irreducible covariants of the forms $(a, \ldots)^8(x, y)^{14}$, and $(a, \ldots^8 \between x, y)^{20}$. I reproduce in part Prof. Gordan's theory so far as it applies to the quintic; and I give the expressions of such of the 23 covariants as are not given in my former memoirs; these last were calculated for me by Mr. W. Barrett Davis, by the aid of a grant from the Donation Fund at the disposal of the Royal Society. The paragraphs of the present memoir are numbered consecutively with those of the former memoirs on Quantics.

"On the Chemical Activity of Nitrates." By Edmund J. Mills, D. Sc.

"On the relative Duration of the component parts of the Radial Sphygmograph Trace in Health." By A. H. Garrod.

Anthropological Society, May 19.—Dr. Berthold Seemann, V.P., in the chair. Mr. Henry F. Chorley read a paper on "Race in Music." The author, after remarking on the vast range of the subject for treatment in the compass of a single paper, proceeded to point out the difficulties that stood in the way of determining what is and what is not truly national music, one great difficulty consisting in the inaccuracies of notation. Notation being comparatively a modern art and the only means by which musical ideas can be transmitted, we are very much in the dark as to the advance made by the ancients in the art of music. Confining himself chiefly to the modern development of music, Mr. Chorley argued that new and original melody is far less common than is generally supposed. By the simple variation of *tempo*, implying some change in accentuation, a melody can be so entirely transformed as to lose its original character. Genuine, fresh, national music is, again, comparatively rare, and its character has always been most marked whenever intercourse has been the most restricted. Passing from the more limited subject of national music to the broader question of race-elements in music, the author illustrated the great distinction which exists between the Asiatic and the European development of the art; in the former it is confined to rhythm, and seldom includes beauty of sound or symmetry of form. In strong contrast to the Oriental ideas of music were cited those of the north of Europe, viz., Norway, Sweden, Denmark, and Russia. In the opinion of the author those people take the highest place as melodists. It should as a fact be noted that with few exceptions those northern airs are in minor keys, which might be taken as an expression of, rather than a protest against, the gloom of the climate and scenery, were it not that the same characteristic largely obtains among inhabitants of the torrid zone. The sense of musical rhythm seems as distinctly marked among different peoples as varieties of physiognomy; for instance, the Peninsular melodies are only characteristic when they are in triple time, the airs in common time being essentially mawkish and pointless, owing such individuality as they have to the sleepy, voluptuous delivery of the executant. On the other hand, the music of France lies essentially in the direction of squared music towards what is piquant as distinct from what is undulating. In treating of the subject of Race in Music the author could not but draw attention to a phenomenon which is of universal recurrence, namely, the demarcation not merely of race but also of sex in the art, be its stages of culture or civilisation ever so primitive, ever so mature. The absence of musical inventive genius in woman is most curious and inexplicable, and offers another signal illustration of the contradictions and inconsistencies which mark music beyond any other art. While women have achieved distinction and often great success in literature, painting, sculpture, architecture, and science, and while they are unsurpassed as interpreters of the drama and of the art of music, not a solitary female composer of originality or even of repute is known to the historical or critical observer. Mr. Dannreuther illustrated the paper by numerous examples on the pianoforte.

Entomological Society, June 6.—Mr. A. R. Wallace, president, in the chair. Mr. F. V. Jacques, of Bristol, was elected a member. A collection of insects sent to the Society by Mr. Henry Ansell, from Kinsembo, S.W. coast of Africa, was exhibited; and another collection from Tugela, Natal, was exhibited by Mr. W. Warwick King. Living specimens of *Ateuchus*, from Venice, were shown by Mr. S. Stevens; and a gynaniromorphous *Brachycentrus*, from Cheshunt, was exhibited by Mr. M'Lachlan. Communications were made by Major Munn, on the Honey-Bee, by Mr. A. G. Butler, on the possible identity of *Argynnis Niobe* with *A. Adippe*, and by Mr. G. R. Crotch on the Genera of *Coleoptera* studied chronologically (part 2, from 1802 to 1821.)

Ethnological Society, June 7.—Dr. A. Campbell, V.P., in the chair. R. H. Tiddeman, B.A., F.G.S., was announced as a new member. Professor Huxley, LL.D., F.RS., President, read a paper "On the chief modifications of mankind, and their geographical distribution." After pointing out those physical characters which are of the greatest value in distinguishing the several modifications—such as colour, character of hair, and form of skull—the author proceeded to describe five distinct types of mankind: 1. The *Australioid*, with slender limbs, dark brown skin, black wavy hair, strong brow-ridges, and long skull; this type is found throughout Australia among the hill tribes of the Dekhan in India, and formerly in the Valley of the Nile. 2. The *Negroid*, with dark skin, black frizzled hair, and long skull; a group which includes the Negroes and Bushmen of Africa, and the Negritos of New Guinea, Tasmania, &c. 3. The *Xanthochroic*, with fair skin and blue eyes, distributed through Iceland, Eastern Britain, Scandinavia, North and Central Germany, and extending through Eastern Europe into Asia, as far as North-west India, and found also in the North of Africa. 4. The *Melanochroic*, a type with dark complexion, occupying an area situated between the Xanthochroic and Australioid peoples; and 5. The *Mongoloid*, a large and somewhat ill-defined group extending throughout Central and Northern Asia, the two Americas, and Polynesia. The paper was illustrated by a large coloured map, showing the distribution of these five groups and their subdivisions. Among those who took part in the subsequent discussion were Mr. George Campbell, Mr. A. R. Wallace, Mr. E. G. Squier, Dr. Ray, Mr. Luke Burke, and Mr. Dardy.—Mr. Squier exhibited a large collection of drawings, plans, and photographs of localities of interest in Peru.

Zoological Society, June 9.—George Busk, V.P., in the chair. The Secretary read some notices of the principal additions to the Society's menagerie during the month of May, and called particular attention to a deer sent home from Singapore by order of H.R.H. the Duke of Edinburgh. This animal appeared to belong to a new species, and was proposed to be called *Cervus alfredi*.—Professor Newton, V.P., exhibited a series of skins of the large falcon found in Alaska and sent to this country for examination by the Smithsonian Institution, and referred them to the *Falco islandicus* of Gmelin.—Mr. Gould exhibited and made remarks upon some skins of British water-ouzels.—Mr. R. Swinhoe read a series of zoological notes made during a journey from Canton to Peking and Kalgan. Mr. Swinhoe's remarks contained descriptions of several new species of mammals and birds, amongst which were a new hedgehog, proposed to be called *Erinaceus dealbatus*, and a new species of dwarf swan, for which the name *Cygnus davidis*, after M. le Père David, its discoverer, was proposed.—Mr. R. Swinhoe also read a paper containing notes on certain reptiles and batrachians collected in various parts of China.—Mr. R. B. Sharpe read a paper on the Ornithology of Madagascar, based upon a collection recently formed by Mr. A. Crossley, in the neighbourhood of Noce Vola, in the north-western portion of the island. Two new species were discriminated, and proposed to be called *Ceblepyris major* and *Corethrura insularis*. Two species—viz., *Bernieria crossleyi* of Grandidier, and *Ellisia madagascariensis* of Hartlaub—were likewise generically separated, under the respective names of *Mystacornis* and *Oxylabes*.—Dr. John Hawkes, F.Z.S., communicated a note on a case of *hernia ventriculi* in a common canary.—Mr. D. G. Elliot exhibited and pointed out the characters of two new species of pheasants from the province of Yarkand, proposed to be called *Phasianus shawii* and *P. insignis*; also a well-marked permanent variety of *P. torquatus* from the Island of Formosa, proposed to be called *P. formosanus*. Mr. Elliot also made remarks on the affinities of the known species of true *Phasianus*, and pointed out their geographical distribution.—A communication was read from Mr. A. Sanders, F.Z.S., containing notes on the myology of a Geckoid lizard, *Platydactylus japonicus*.—A communication was read from Mr. Gerard Krefft, C.M.Z.S., of Sydney, containing a preliminary account of the skeleton of a rare whale, probably identical with *Dioplodon sechellensis*, recently obtained in the Australian seas, near Lord Howe's Island.—Messrs. Sclater and Salvin communicated a synopsis of the birds of the family of *Cracidæ*. The authors proposed to divide this family into three sub-families—the *Cracinæ*, *Penolopinæ*, and *Oreophasidinæ*. Of the first of these twelve species were recognised, and of the second thirty-eight, while the third contained only one representative. One species of Guan was described as new, and proposed to be called *Ortalida erythroptera*.—A communication was read from Professor Barboza du Bocage, F.M.Z.S., containing a description of the young of *Pelecanus sharpii*.—Dr. A. Günther communicated an account of the species of tailless batrachians, recently added to the collection of the British Museum, amongst which was a new diminutive frog, recently discovered by Dr. Cunningham in Fuegia, and proposed to be called *Nannophryne variegata*.—Dr. J. Murie read a paper on the anatomy and osteology of the Saiga (*Saiga tartarica*), founded on examples of this mammal that had lived in the Society's Gardens. The conclusions arrived at by the author as to the systematic position of the Saiga were, that this animal cannot be well included in any of the subdivisions of the ruminants hitherto established, but constitutes a form intermediate between the sheep and the antelope.

Victoria Institute, April 25.—Rev. Dr. Thornton, vice-president, in the chair. A paper was read on "Comparative Psychology," by Mr. E. J. Worshead. The following gentlemen took part in the discussion:—Rev. C. A. Row, Rev. J. B. Ouran, Dr. C. Haughton, Mr. Reddie, Rev. Sir S. Marsh, Rev. C. Graham, and the chairman.

Brighton and Sussex Natural History Society, June 9.—Mr. Sewell, vice-president, in the chair. A paper on Diptera and their Wings, by Mr. Peake, was read, in the absence of that gentleman, by Mr. Wonfor, Hon. Sec. While wings are common to the whole order of insects, the Diptera consist entirely of two-winged flies; but instead of a second or hinder pair, they have little thread-like bodies, terminated by knobs and called *halteres*, originally considered balancers, supposed now by some to be *olfactory* organs and by others organs of *hearing*. From many points of resemblance he thought they were analogous to the hind-wings of insects, and that at present their special use had not been ascertained. Besides the halteres they had also winglets (*alulæ*) which were thought to be only appendages to the fore-wings. Among the Diptera three classes of fliers were found, differing in the form of their bodies and shape of their wings; first, the slender flies, such as gnats, having long bodies, narrow wings, and long legs, but without winglets; secondly, those whose bodies, though slender, are more weighty, as the *Asilidæ*, having larger bodies, shorter legs, and very minute winglets; lastly, those like the house-fly, with short, thick, and often very heavy bodies, furnished with proportionate wings, shorter legs, and conspicuous winglets. From these circumstances it might be inferred that the long legs of the light-bodied flies acted as rudders, while the winglets of the heavier bodied flies assisted the wings in flying. The wings might be described as transparent membranous organs, consisting of two laminæ united by veins or nervures, and upon their arrangement and the form of the antennæ the distinguishing characters of the Diptera were formed. The several parts of the wings and nervures and their differences, as seen in the great groups *Nemocera* and *Brachycera*, were next pointed out, and the paper illustrated by drawings and microscopical preparations of wings.

TRURO

Royal Institution of Cornwall, May 17.—The annual meeting of this society was held in the Council-room, Truro, Mr. J. Henwood, F.R.S., president, in the chair. Mr. H. M. Whitley read a paper contributed by Sir. F. M. Williams, M.P.: "Recent Observations on the Subterranean Temperature of Clifford Amalgamated Mines." This gave the result of various observations made during the past fifty years, and, finally, those made by Captain Gilbert previous to ceasing the deep workings some weeks since. Then at 224 fathom level, being 279 fathoms from the surface, the highest temperature recorded was 123°·5; and at 245 fathom level, or 300 fathoms from the surface, the highest was 125°.

Mr. W. Pengelly read a paper, entitled "Notes on the Archæology and Geology of Devon and Cornwall." A difficulty formerly existed in relation to the Devonian series of rocks, for while in the northern part of the area over which it extended there were found fish, in the south there were only sponges and mollusks. The late Mr. Jonathan Couch discovered in 1843 some fossils which he thought fish, but which Professor McCoy in 1857 pronounced to be sponges. The matter dropped till in 1857 he himself submitted to Mr. W. H. Baily a fossil found at Hannafore point, near Looe, and it, with a number of the same kind collected, were pronounced, on the authority of Mr. Simmons and Professor Huxley, to be ichthyolitic. The next point to which he specially wished to direct attention was the rock joints, of which they had two series, one running east and west, and the other north and south, and of which the one that crossed the other was naturally the more recent. Of the age of these joints few indications had been observed. At the eastern shore of Torbay, however, where both these systems were to be observed, it was evident that they were in existence before the close of the New Red Sandstone era, because their fissures were filled with dykes of that rock. The east and west dykes were faulted and traversed by veins of carbonate of lime, which came up on either side to the north and south dykes, but did not cross them. This showed that a considerable time must have elapsed after the formation of the first system of joints before the second. He would commend this subject to the geologists of the county. Next he would refer to the lower Silurian fossils of the Dodman district. At Budleigh Salterton there was a remarkable collection of quartzite pebbles which contained fossils, of which the analogues were to be found in the rocks of Normandy. However, he objected to being beholden to Normandy if he could trace the pebbles to a source nearer home; and he should be delighted if some energetic young Cornishman would devote himself to the elucidation of these quartzite pebbles. Thirdly, whence were the flints on our western strand derived? There was no deposit of flints to the south of the Teign; the course of the shingle could be traced along the coast from the west to the east; and yet there was an abundance of flints, not only in the present beaches all round Cornwall, but in the raised beaches. It would be a comparatively easy but valuable task to ascertain the relative abundance of flints at various points. It would be of still more value if any one would dredge in the Channel, and ascertain whether there were any submarine outliers of gravel from which the flints could be derived. Lastly, he would suggest an inquiry into the megalithic monuments of Cornwall. The origin of the "Hurlers" at Liskeard and the "Merry Maidens" at Penzance was unknown, and it would be interesting to collect anything relating to them which tradition yet retained.

CAMBRIDGE

Philosophical Society, May 30.—The president (Professor Cayley) in the chair. New Fellow elected, F. S. Barff, M.A., Christ's College. Communications made to the Society.—1. By Professor Miller, F.R.S.: On the Invention of the Camera Lucida by Wollaston. 2. By Mr. H. G. Seeley, F.G.S. (1) On the Frontal Bone in the Ornithosauria; with additional evidence of the structure of the hand in Pterodactyles from the Cambridge Upper Greensand. (2) Note on a new species of Plesiosaurus from the Portland Limestone.

DUBLIN

Natural History Society, June 1.—W. Andrews, M.R.I.A., in the chair. Dr. A. W. Foot read some notes on the different insects captured by him during the present spring. He remarked on the lateness of the season and on the paucity of insects coming to sugar; and in giving a list of insects taken at Kilkenny, expressed the hope that a large number of local collections would be made throughout Ireland, stating that if such were sent to the Museum of the Royal Dublin Society, they would be arranged in the cabinet of Irish insects in that collection, and thus in time material for a catalogue of Irish insects be obtained. Mr. A. Andrews took the chair, while Mr. W. Andrews read a paper on the Crustacea of the West Coast of Ireland. While it was his intention at an early meeting of the next session of the Society to give a complete list of all the Crustacea met with by him on this part of Ireland, including the minute forms, he wished for the present to mention the occurrence there of three species of *Nehalia*, of *Pagurus lævis*, *P. ulidianus*, the latter with ova, of *Pilumnus hirtellus*, *Portunus longipes*, this latter quite new to Ireland, and met with at the entrance to Dingle Bay in fifty fathoms of water, and on a shingly bottom, and of a species of *Galathea* which he regarded rather as a *Munida*. He had taken an immense number of rare minute Crustacea, also many Echinoderms, such as *Echinocyamus pusillus* living, *Spatangus purpureus*, *Ophiura alba*, and young *Echinus lividus*, or at least something very like it, at fifty fathoms depth. Further notices of all these species he would reserve for another occasion. Professor Macalister remarked on the interest of the paper, and said that from the sample of it given now, the paper promised for next session will be of great value, many of the species alluded to being rare. Professor Macalister exhibited a specimen of *Ommastrephes* from the Cape of Good Hope, which if not one of Le Sueur's species, was probably undescribed. A vote of thanks was then passed to the Royal Irish Academy for so kindly accommodating the Society with the use of their house during the past session, after which the chairman adjourned the Society until the first Wednesday in November next.

Royal Geological and Royal Zoological Societies, June 8.—John Barker, M.D., in the chair. The Rev. M. H. Close read a paper "On some Corries and their Rock-basins in Kerry;" the object of which was to demonstrate, if possible, what is still denied by some, the considerable erosive power of a corry glacier. On account of its smallness (its mean diameter being only 550 ft.) the well displayed basin in massive unfaulted rock, in the mountain hollow called Coom Keagh, a well-known glacier site near Dingle, peremptorily disclaims all the explanations hitherto suggested for such physical features, save that of glacial action. The water in the basin is 42 ft. deep, and the mean height of the rock-barrier above the water is 10 ft. The glacier must have removed at least 52 ft. of rock from the site of the basin, and doubtless more, since the rock basin is due only to the difference between the erosion at its middle part and at the rock barrier. The Rev. Professor Haughton and Professor Hull took part in the discussion on this important paper.—Professor R. H. Traquair, M.D., read a paper "On the scales of *Calamoichthys calabaricus*." He described the external hard parts of this fish, including the scales, fin-rays, and those cranial bones which have external "ganoid" surfaces, in this supplementing as well as somewhat correcting the description of Dr. Smith. Especial attention was given to the scales; their whole surface is covered with closely-set punctures, rather irregularly placed towards the centre and anterior superior angle, but assuming a regular arrangement in concentric and also radiating lines on the posterior inferior part of the scale. The microscopical structure of the scale corresponds with what is seen in *Polypterus*. The scales are of an osseous substance, with numerous lacunæ. Vascular canals enter from the attached surface, and then form a complex network which communicates by minute branches with the punctures in the external aspect. The form of the scales on different parts of the body was also alluded to, and some deviations observable on some specimens from the usual regular arrangement of the scales in oblique bands were also described.—Professor Macalister, M.D., then read some letters addressed to the late Dr. Croker respecting a singular mode of reproduction of the Marsupials. The writers were Mr. Williams, M.R.C.S., and Mr. G. T. Lloyd, of Geelong; and after asserting that all the knowledge at present existing on the subject of the reproduction of the kangaroo was erroneous, proclaimed as an important discovery that the young kangaroo was developed from a little mass of mucus which exuded at certain seasons from the nipple of the mother. The Chairman, in calling for a discussion on this correspondence, stated that the actual transit of the young animal from the uterine cavity to the maternal pouch had never yet been proved. [We cannot refrain from expressing our regret that the members of two learned societies could be found to listen to such absolute nonsense as was contained in these letters, and that a professor of zoology in an ancient university, himself a distinguished anatomist, could be persuaded to be the medium of presenting it to such or any audience. As to the remarks of the Chairman, who we think ought to have stopped the reading of the letters, need we say that they are in contradiction to the facts observed by the late Earl Derby's father, or by the present Professor Owen.]—Professor R. Ball read an interesting letter from a friend in Bristol referring to the birth in the Zoological Gardens of that city of a second litter of four tiger cubs. Of the first litter of four, only one survived; and of the present litter all the cubs were more or less malformed and delicate. In Dublin the tigress has never had any offspring, while the lionesses have been most prolific, and their cubs are uniformly perfect and healthy. The societies then adjourned to the next session.

Royal Dublin Society, May 23.—Prof. Dyer in the chair. Prof. R. Ball, M.A., read "An Account of Experiments on the Mechanical Efficiency of the Differential Pulley-block and Epicycloidal Pulley-block."—Dr. T. Emerson Reynolds read a paper "On a Series of Mineralogical Tables."

Royal Irish Academy, May 23.—The Rev. J. H. Jellett, B.D., president, in the chair. Dr. R. M'Donnell read a paper "On a New Theory of Nervous Action, as regards the Propagation of Sensation along Nerves." This memoir will be published in the Transactions of the Academy.—A paper by Mr. Hardinge was read, "On an ancient seal of the diocese of Ossory, and on some coins found near the Boyne."—The following were declared duly elected members of the Academy:—A. FitzGibbon, C.E., E. Hutchins, and John Kelly, of University College, Calcutta.

BERLIN

Royal Prussian Academy of Sciences, March 14.—Prof. Dove read a communication on the diffusion of heat in the Polar Sea, and on the cold of the early part of the present year, the latter accompanied by tables.

March 31.—Prof. Helmholtz communicated a memoir by M. N. Baxt on the velocity of transmission of excitation in the motor nerves of man, containing the results of several series of experiments.—Prof. Hofmann read a paper on substituted melamines, in which he described the following compounds—trimethylmelamine, triamylmelamine, and triphenylmelamine. He confirmed his previous supposition that the substituted melamines are not the direct products of the desulphuration of the sulphur-compounds of urea, but that this formation is preceded by that of the substituted cyanamides. Prof. Hofmann also communicated a memoir by himself and M. Otto Olshausen on the isomers of cyamuric ether, in which the authors describe the cyanætholine of Cloëz, and a series of experiments on the production of the cyanuric ethers of the methylic, ethylic, amylic, and phenylic series.—Prof. W. Peters read a paper on the affinity of the *Ctenodactyli* to the chinchillas and other groups of rodents, in which he stated that the African genera *Ctenodactylus* and *Pectinator* differ in all essential points from the jerboas (*Dipus*), and agree rather with *Chinchilla*, and *Octodon*, or *Echinomys*, whilst they show some tendency towards the *Murinæ*.

April 7.—Prof. Rammelsberg read a paper on the position of thallium in the series of elementary bodies. He described several salts of thallium, such as the iodates and periodates, the chlorides, bromides and iodides of thallium and their double salts, and referred to the isomorphism of the salts of thallium with those of potassium (rubidium and ammonium) as shown especially by the researches of Des Cloiseaux. He stated that although both physically and chemically thallium is a metal, it presents a combination of characters in its compounds which renders its precise location difficult. Prof. Poggendorff noticed a new form of electrical machine, upon which he promised full details at a future meeting of the Academy.

April 28.—A memoir by Dr. P. Groth on the relation between crystalline form and chemical constitution in some organic compounds, was communicated by Prof. G. Rose. The author remarked upon the failure hitherto experienced in all attempts to apply the theory of isomorphism to organic compounds, and stated that he adopted a new method of investigation, which consisted in ascertaining the nature of the change produced in a given crystalline form by the access of a definite atom or atomic group replacing hydrogen. He described a long series of experiments, which lead him to conclude that there are atoms and atomic groups, which, by substitution, alter the crystalline form of a body only in a certain direction. This change he proposes to call "morphotropism," and he indicates the different modes in which the "morphotropic force" may be modified in action.

German Chemical Society, June 13.—Prof. Hofmann has employed his method for taking vapour-densities, to control the formulæ of several organic compounds. Sulphuretted methylic aldehyde (or what has been considered as such) is $C_3 H_6 S_3$. The vapour-density taken in xylidine-vapour was found to be 70·72° instead of 69° required by the theory. The corresponding ethyl compound was found to be $C_6 H_{12} S_3$. It is more volatile than the methylic compound. Chinone (prepared from benzidine instead of aniline, the former giving a better result) has the formula $C_6 H_4 O_2$, and not the double formula which has been lately proposed for it. He then showed a fine specimen of anthrachinone prepared by Messrs. Hopkins and Williams of London.—M. Schlebusch has prepared some derivatives of camphoric acid, and of camphor, notably tetranitro-camphor.—W. Thomsen, who has lately published views on the connection of the basicity of an acid, and the heat developed by its combination with water, draws conclusions from this theory as to the basicity of silicic, hydrofluoric, and fluosilicic acids.—L. Henry described chloroiodides of ethylene, and of allyl.—A. Kekulé described the properties of crotonic acid prepared from aldehyde, from oil of mustard, and from cyanide of allyl; the latter two being identical. The former melts at 72°, boils at 184°·7 C., and crystallises in the monoclinoëdric system.—M. Daube has extracted the colouring principle of the curcuma-root. He gives to it the formula $C_{10} H_{10} O_3$, and the name curcumin.—W. Knop has published a preliminary notice of the action of sulphuric acid and alcohol on albumen.

GÖTTINGEN

Royal Society of Sciences, January 5.—A paper, by M. Max Nöther, on Algebraical Surfaces which may be represented by plane figures, was communicated by M. A. Clebsch.

January 19.—MM. W. Marmé and A. Creite communicated a paper on the Physiological Action of the Alcoholic Extract of *Cynoglossum officinale*. The authors deny that this extract acts in the manner of curare, as stated by some Russian writers; they describe its action as that of a narcotic, and state that it causes death by paralysing the respiratory centre. Dr. Rudolph Fittig communicated some further researches upon the constitution of piperic acid, in which he described several of its derivatives. Professor Wöhler noticed the analysis of the supposed meteoric iron of the Collina di Brianza, by Dr. Haushofer, who had stated that he found in it both nickel and cobalt, the presence of which was denied by previous analysts. The author had analysed a portion of this iron, and had also applied to Professor Rose for an analysis of the fragments in Berlin; no trace of cobalt or nickel was detected by them. Dr. Haushofer's analysis was probably made from a fragment of true meteoric iron.

February 16.—M. Clebsch communicated a memoir, by M. S. Lie, on the relations of reciprocity of Reye's complex. Dr. Rudolph Fittig read a paper on Tetramethylbenzole, in which he described a new solid hydrocarbon, having the formula ascribed to tetramethylbenzole $[C_{10} H_{14} = C_6 H_2 (C H_3)_4]$. For this he proposed the name of *durole*, and he described two of its compounds, namely, *dinitrodurole* and *dibromodurole*.—Professor A. Enneper read a paper on an enlargement of the idea of parallel surfaces.

March 16.—Professor Kohlrausch communicated a memoir, by M. E. Riecke, on the replacement of a system of galvanic currents existing upon a surface by a distribution of magnetic masses.—Professor Henle communicated some zoological observations made at Naples by Dr. Alexander Stuart, of Odessa. The author described the development of new individuals in a colony of *Collozoum inerme*, which takes place by a process of gemmation. He confirmed his former statement that the cilia of *Coscinosphæra* are composed of calcareous crystals. With regard to the medusa-brood of *Velella spirans*, the author stated that all the tissues of the polypary take part in their formation, and that at an early period they possess a body cavity distinct from that of the stomach, which is afterwards filled up with connective tissue, so as to leave only the canals of the water-vascular system. He noticed the occurrence of something which he regards as a digestive passage in the *Gregarinæ* of the earthworms, and briefly described the nervous system of *Creseis acicula*, in which he found a ganglionic œsophageal ring and dorsal and ventral ganglia, each of the latter emitting nervous stems.

DIARY

THURSDAY, June 23.

Zoological Society, at 8.30.—On the Walrus: Dr. J. Murie.—Catalogue of the Mammals of South China and Formosa: Mr. R. Swinhoe.—On a Collection of Birds from the Island of Trinidad: Dr. O. Finsch.

SUNDAY, June 26.

Sunday Lecture Society, at 8.—Cruelty in relation to the Lower Animals: Dr. T. S. Cobbold, F.R.S.

MONDAY, June 27.

Ethnological Society (Extra Meeting), at 8.—On the opening of the Park Cwm Tumulus: Sir John Lubbock, Bart.—On the opening of Grim's Graves, Norfolk: Rev. Canon Greenwell.—On the discovery of Platycnemic Men in Denbighshire: W. B. Dawkins and Prof. Busk.

London Institution, at 4.—Botany: Prof. Bentley, F.R.S.

WEDNESDAY, June 22.

Society of Arts, at 4.—Anniversary Meeting.

BOOKS RECEIVED

English.—Woolhope Naturalists' Field Club: Report for 1869.—Remarks on Synonyms of European Spiders, No. 1: T. Thorell (Williams and Norgate.)

Foreign.—(Through Williams and Norgate).—Bericht über die Fortschritte der Anatomie und Physiologie im Jahre 1869: Henle, Meissner und Grenacher.—Die Ophthalmologische Physik und ihre Anwendung auf die Praxis: Dr. H. Gerold.—Nouveaux éléments de Physique Médicale: Desplats et Gabriel.—Die Bierbrauerei und die Dickmaischbrauerei: P. Heiss.—Etude sur les Diatomacées: Ch. Manoury.—Die Land und Süsswasser Conchylien der Vorwelt: Dr. Sandberger.

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THURSDAY, JUNE 30, 1870

NATURAL SCIENCE AT THE ROYAL ACADEMY

I AM afraid the words of the title of this article will sound like a harsh discord to many ears. Let not the reader suspect that they are in any way akin to that incongruous phrase "art manufacture."

I do not presume to address persons versed in the language of the artistic schools, nor the vaguely vapouring sentimentalists who are leaders of æsthetic cliques.

I know a class of men more worthy of respect who are happily more ignorant: men who are accustomed to attach a defined significance to words: men who, when they hear noises that they do not understand, or when they detect the characteristic haziness of artistic cant, stand meekly on one side, careful and troubled only to pursue the orderly and laborious track of mere matters of fact. It is remarkable that the chasm which separates those groups of men who work in the adjacent fields of art and science is one of the most distinct in the whole system of modern life; as marked as that which divides the official from the commercial mind, for instance, or the sacerdotal from the legal caste. There is no common ground between them. They do not teach their young in the same language; the scientific men generally talking in grammatical and reasonable speech, whilst those who speak on behalf of art generally "gas," if I may be allowed to use an expressive americanism.

A small section of the crowd who throng the Academy galleries at this time of year, persons who habitually associate reason with observation, have been known to complain that the exertions of the painters are to a great extent wasted upon them for want of rudimentary knowledge of the principles upon which fine art is founded, and that the information which probably exists somewhere upon this subject seems to be so mingled with rhetorical flourishes and anecdotes, that if not altogether inaccessible, it demands far more time than most students can afford, to sift it out, so that when they go to look at pictures, they do so in the idlest possible temper, expecting to be merely amused with a pleasant but transient sensation like the smelling of a sweet odour, yet all the while not without misgivings that they might be deriving nobler entertainment or more permanent good. Are pictures mere accidents, or are they produced upon known principles? Are their excellencies estimable by definite methods and referable to known standards, or is it all caprice?

Such questions are very pertinent, and ought to be philosophically answered by able painters, but it happens that such men have not generally cared to spend their energies in speech. If a living painter offers one or two hints concerning the Academy, such as can be written in a weekly journal, they must needs be of a general kind, and with little, if any, reference to particular pictures. In any case, if they do not harmonise with the prevailing tone of dilettantism, they will stand as landmarks of heresy to be kicked against by critics. In the columns of NATURE, however, they will only be read—if at all—by clear-headed and simple-souled naturalists.

The central motive of fine art may be most compactly expressed by the simple term beauty. We will not stop to define the word now. Go to the Academy to seek for it. Do not expect much of it; for amongst the four or five hundred essayists on canvas there represented, a good many, perhaps more than half of them, would repudiate that fundamental principle. Be content to take your beauty in small doses: about in the same proportion as pure gold to the pebbles in the bed of an African river, or sense in a railway novel. When found, enjoy freely in your own way. That is the first stage in artistic culture; some people say the whole—beginning, middle, and end of it. But this will hardly satisfy the scientific student. He will want to find out the relation between beauty and the common world, so as to determine what position this exquisite sensation ought to occupy in the order of his experiences.

Let him then proceed to examine the pictures analytically, and, by way of making a beginning, let him apply a test question all round. Let him ask himself, for instance, whether any example of beauty has been offered to him which depends on a violation of natural laws; whether he has come across anything like a lovely monster. The question may appear to be an idle one, but there is a principle involved in it of the very highest importance, though there is no time to illustrate it here. Let him think it over carefully, and apply it not merely to pictures of monstrous animals and vegetables, but to any abnormal effect of the sun's rays, any deviation from the simple laws by which surfaces govern shadows, and so on; any monstrous clouds, apparently constructed out of featherbeds, and yet beautiful; any monstrous draperies, apparently made of bent metal, or cut in horn, or forced into novel shapes as if by explosive gases, and yet beautiful; any monstrous contortions of the human countenance amongst the large variety of attempts to represent the pleasanter, or more repulsive, or more permanently interesting phases of human life. Let him take note whether he is likely to carry with him to his grave one image or suggestion of loveliness that will have had its origin in a picture produced in ignorance of or defiance of natural law; because, if he find any, the fact will be well worth knowing, and I hope he will not hush it up.

Another hint that I will venture to give the scientific student is, to remember that in these days art, in England, is in its infancy. By the time that the play-going public have sickened themselves with displays of trivial sentimental incident; by the time the new-born professors of art at the universities have taught all the growing boys how life may be treated artistically after it has been earned by doing their day's grinding at the mill; by the time these boys have travelled and studied the art of the ancient world and its relation to the modern; by that time there will have become established a school of original painters, able to represent with ease and accuracy any visible fact, and free to choose out from the procession of physical phenomena such special incidents or moments of visible history, such elements of passing scenes as shall be most profoundly beautiful and delightful to men, and to preserve in comparative permanence their evanescent charms.

I took up a scientific periodical the other day, and lighted upon a letter to the editor concerning the colour of the sky, written of course by a scientific man (one who seems to have read that silly little book by Chevreul on complementary colours), in which he suggests that as the sun looks orange, *therefore* the sky looks blue. He says that upon this theory the planets of Sirius and Vega must have a black sky, those of Betelgeuse a green sky, and so on. But if my readers go to the Royal Academy, some of them will probably be a little surprised to find that they are positively living on a planet which enjoys the advantage of a solid lemon-yellow sky! a sort of information not to be picked up every day. In the same sort of way, ethnological students may inform themselves that the ancient Greek heroines were shaded with streaks of treacle about their "great marbly limbs." But the judicious investigator will take note that the chief end of most of this canvas, and the purpose of most of this paint, is to indulge the artists and their disciples with reminiscences of antecedent art—an exemplification of the cud-chewing tendencies of his species. Many of them have drank deeply at the fountain of second-hand beauty; they have drank perhaps intemperately, and their speech must needs be somewhat sublime to uninstructed ears.

Let not the student carry away the impression that artistic faculties and wanton inaccuracy are necessarily connected. If the scientific writer who offered suggestions to account for the blueness of the sky had acquired the habit, which the practice of painting gives, of comparing the intervals between different colours, and estimating the intensities as well as the extent of the several elements that go to make up any given scene in the out-of-doors world, he could hardly fail to notice that the sun at mid-day would in all cases be the least coloured object in the whole field of view; that the colours of other objects would neither gain nor lose perceptibly where exposed to his direct rays; whereas, the shaded and overshadowed parts of them would suffer various enhancements or modifications from the coloured rays reflected from surrounding surfaces; and that, in the case referred to, in a mid-day cloudless sky, the most widely extended and most purely coloured of all the surrounding spaces being blue, all the shadows and many of the shades would be dyed blue, so to speak, over their own proper colours; and that the whole scene would be steeped in the "blue flood of light" of the poets. The colours of the face of our planet, both native and derived, present a large field for study, and a good deal more can probably be ascertained about them than about many less pleasant yet less neglected subjects; and when the scientific investigators have done with the blue sky, I hope they will let us hear what they have to say of the blue sea.

What is the moral of all this? Simply that the scientific men pay too little attention to the broader aspects of the visible world; while the artists on their part pass by the clear fountain of natural beauty, and content themselves with dreamily sipping lukewarm water from the corroded vessels of their forefathers; the one group of doers standing apart from the other; whereas, if either would go to school with the other, they would, in my opinion, each stimulate and aid the labours of the other, and divide between them a far larger share of the spoils of the world.

JOHN BRETT

ON THE NATURAL LAWS OF MUSCULAR EXERTION

AMONG a multitude of profound and happy suggestions to be found in Mr. Babbage's Economy of Manufactures, are some remarks on the relation between fatigue and the rapidity or degree of muscular exertion. Coulomb, it appears, had previously investigated the most favourable load for a porter, and had ascertained by experiment that a man walking upstairs without any load, and raising his burden by means of his own weight in descending, could do as much work in one day as four men employed in the ordinary way with the most favourable load. Mr. Babbage clearly points out (p. 30) that the exertion necessary to accomplish any kind of work consists partly of that necessary to move a limb of the body, and partly of the force actually utilised in the work. The heavier the work done, the larger the proportion, therefore, of the power utilised. But there is a limit to this mode of increasing the useful effect, because, by the natural constitution of the muscles, they can only develop a limited amount of force in a given time, and the fatigue rapidly increases with the intensity and rapidity of exertion. Hence there is in every kind of work a point of maximum efficiency, which is in practice ascertained more or less exactly by frequent trial.

This subject appeared to me to possess interest for at least two reasons: it might be made to throw some light upon the chemical and physiological conditions of muscular force; it might also point out how we could make some commencement, however humble, of defining the mathematical relations upon which the science of economy is founded. I have therefore attempted to add precision and certainty to the ideas put forth by Coulomb and Babbage, by some experiments of a simple kind.

The first and least interesting series of experiments consisted in ascertaining the comparative distances to which various weights could be thrown upon level ground. The product of the weight and distance was taken as the measure of useful work, and it was the object to ascertain according to what law this varied, and at what point it was a maximum. The weights employed varied from ½ lb. up to 56 lbs., and were thrown as nearly as possible in a uniform manner and at the most advantageous angle. About 57 experiments at different times were made with each weight, or 456 experiments in all; and it was quite obvious that good average results were obtained, the correspondence of different sets being very satisfactory. The results are as below:—

Weight in pounds	56	28	14	7	4	2	1	½
Average distance thrown in feet.	1·84	3·70	6·86	10·56	14·61	18·65	23·05	27·15

A little consideration showed it to be probable that these numbers would agree with an equation of the form—

$$x = \frac{p}{w + q}$$

in which x = distance thrown,
w = weight thrown,
q = constant weight representing about half that of the arm,
p = constant amount of force exerted.

The experiments give us eight distinct equations by which to determine the two unknown quantities p and q; and by the method of least squares we determine their most probable values to be—

$$p = 115{\cdot}7$$
$$q = 3{\cdot}9$$

The formula thus becomes—

$$x = \frac{115{\cdot}7}{w + 3{\cdot}9}$$

And calculating thence the distances for the several weights, they are:—

Weights ...	56	28	14	7	4	2	1	½
Calculated distances	1·93	3·63	6·46	10·61	14·65	19·61	23·61	26·30
Differences from experiment	+·09	−·07	−·40	+·05	+·04	+·96	+·56	−·85

The correspondence is so close as to show that the formula is in all probability the true one, and the quantity 3·9 does not differ much from half the weight of the arm, which might be expected to enter into the question. The fact is that the correspondence is embarrassingly close, and I am inclined to attribute it partly to chance. The experiments could hardly have been expected to give results accurate to an inch or two in some cases, and though the formula must be considered true on the ground of experiment, I do not quite see how to explain it on mechanical principles.

If we regard the useful effect as the moving of the greatest amount of matter, it is $x \times w$, and, theoretically speaking, increases continually with w. For the different weights and calculated distances, it is as follows:—

Weight...	56	28	14	7	4	2	1	½
Useful effect	108·1	101·6	90·4	74·3	58·6	39·2	23·6	13·2

But in reality it was not possible to raise the larger weights without exerting additional force unconsidered in the formula, so that the practical maximum of efficiency is probably about 28lb., in the case of my own right arm. With different people it would, of course, vary somewhat.

The above experiments completely confirmed Mr. Babbage's remarks, but did not seem to lead to any further results. I proceeded, therefore, to other experiments upon the rate of exhaustion of muscular fibre. One mode of trial was to raise and lower various weights by a pulley and cord through the convenient range of the arm, continuing the motion with unrelaxed rapidity until the power of the muscles was entirely exhausted. The results of more than fifty experiments were as follows:—

Weight lifted ...	56	42	28	21	14
Average number of times	5·7	11·9	23·0	37·6	111·0
Useful effect ...	319	500	644	790	1554

These numbers show that the total greatest amount of labour can be done with small rather than large weights in this case; but they fail to give any regular law, owing probably to the weight of the body being brought into use with the larger weights.

The mode ultimately adopted was to hold out various weights in the hand at the full stretch of the arm, and to observe the times during which they could be supported. No two experiments were made with the same arm, without allowing, at least, one hour to elapse, so that the vigour of the muscles might be restored. With the smaller weights there was naturally some uncertainty as to the time, but in the case of the large ones the time was very definite. Altogether 238 experiments were made, an equal number with each arm. Uniting all the experiments for the same weight, the results are:—

Weight ...	18	14	10	7	4	2	1
Times in seconds	14·8	32·5	60·3	87·4	147·9	218·9	321·2

These results are pretty satisfactory averages; thus the probable error, for two cases indifferently chosen, was, for 18lbs. in the left hand about ·5, and for 4lbs. in the right 2·7, and the error of the combined results would be less. With the exception of the results for 10lbs. in the left arm, which appear to be somewhat in excess, these numbers are very regular, and point to a systematic law governing the rate of fatigue. The useful effect, or the product of the weight and time, shows a decided maximum, about 7lbs., as follows:—

Weight	18	14	10	7	4	2	1
Useful effect	266	455	603	612	592	438	321

If the weight held be very small, much power is lost in merely sustaining the arm; if the weight is large, there is comparatively little loss on that account, but the power of the muscles is soon run out, and no sufficient opportunity for restoration is allowed. The weights chosen for dumb-bells and other gymnastic exercises appear to be about those which give the maximum efficiency.

I have made several attempts to explain these numbers by reasonable suppositions as to the conditions of exhaustion and restoration of muscular power. It seemed reasonable to suppose that the supply of new matter from the blood would increase in some proportion to the vacancy or want of it, but all such conditions led to integrals of a logarithmic form, which could not be easily compared with experimental results. No formula that I obtained could be made to agree properly with the figures, and all that can be said is that the curve representing the results has a certain appearance of a logarithmic character, so far agreeing with the formulas obtained. Those who are acquainted with the physiology of the subject might succeed better; I am not sure, for instance, how far the failure of strength is due to the exhaustion of the original substance of the muscle, how far to the inadequacy of the current supply of blood. It is a question again how far in any case of muscular action the supply is promoted by the increased action of the heart, or checked by the possible constriction of the arteries. If these questions have not been or cannot be otherwise decided, they might, perhaps, be indirectly solved by experiments of the kind described.

My own object, however, was not to intrude into the domain of physiology, but to show that definiteness might possibly be given by degrees to some of the principles and laws which form the basis of the science of political economy. In some speculations upon the mathematical theory which must underlie that science (read at the British Association in 1862, and published in the Journal of the Statistical Society for June 1866, p. 282), I endeavoured to show that it was only the excessive difficulty of determining the character of the functions involved, which prevented economy from taking the mathematical form and standing proper to it. There is little doubt as to the principles of the subject; but when we try to put them into figures, the data are found to be so deficient, complicated, variable, and subject to disturbances of all kinds, that any hope of accuracy soon dies away in most cases. In the above experiments I have attempted to determine the exact character of the functions connecting the amount of work done with the intensity and duration of labour in certain simple cases. These cases, however unimportant in themselves, represent principles which have innumerable applications in common life.

W. STANLEY JEVONS

THE NEW ZEALAND INSTITUTE

Transactions and Proceedings of the New Zealand Institute, 1868. Vol 1. Edited and published under the authority of the Board of Governors of the Institute, by James Hector, M.D., F.R.S., Wellington. (London: Trübner and Co.)

OUR brother philosophers at the Antipodes have set us an example that we should do well to follow in this country; those who reside at head-quarters (Wellington) and who form the New Zealand Institute, having affiliated to themselves under a special Act of the Legislative Government, the various other societies engaged in similar pursuits that exist in the New Zealand Islands; and who in consequence transmit their papers, or abstracts of them, to the Institute for incorporation in their Transactions. It is to such an organisation as this that we must look for relief from the overwhelming pressure of miscellaneous scientific literature under which the British naturalist now groans. No matter what branch of science he affects, or in how narrow a groove of it he walks, the number of Transactions, Proceedings, Journals, &c., with which he must keep *au courant*, is the great obstacle to his progress; and if he at all takes a broad view of his science, he must be content that it should be a superficial one, and increasingly so as new societies and journals spring into being.

The rules and statutes of the New Zealand Institute appear to be under parliamentary control, and have little analogy with the charters of our free-born societies:—they were published in the *New Zealand Gazette* of March 9, 1868, and the following is a summary of them:—

Art. 1. Any society desirous of incorporation must consist of twenty-five or more members, and subscribe 50*l.* annually for the promotion of the branch of knowledge it professes.

Art. 2. Incorporation ceases on the failure of these conditions.

Art. 3. Any such society must expend either one-third of its annual revenues in or towards the support of a local library or museum, or one-sixth of its revenues to the extension and maintenance of the museum and library of the Institute.

Art. 4. Failure of this condition is followed by cessation of incorporation.

Art. 5. All papers read at such societies shall be regarded as communications to the Institute, and be published by it, under the following regulations:—

(*a*) The publication shall consist of a current abstract of the proceedings of the incorporated societies, and of papers read before them; and shall be entitled "Transactions of the New Zealand Institute."

(*b*) The Institute has the power to reject papers, but (*c*) must return them. (*d*) A proportional contribution for the cost of publishing the Transactions may be demanded of the societies in respect of the papers they contribute; and (*e*) a proportional number of copies of the Transactions will be sent to each society; which may also (*f*) have as many copies as it pleases, at cost price.

Art. 6. Funds and properties derived from the societies shall be vested in the Institute, and applied by its governors to public uses.

Art. 7. The incorporated societies shall conduct their own affairs, making their own bye-laws, &c.

Art. 8. Certificates of incorporation are granted upon application and compliance with the foregoing conditions, under the seal of the Institute.

The museum and library of the Institute are under the management of the Board of Governors; the laboratory is under the exclusive management of the manager of the Institute.

The governors are nine in number: the official ones are the Governor of the Colony, the Colonial Secretary, and the Superintendent of Wellington; joined to six men, eminent for their scientific attainments or love of science; amongst whom are Dr. Hector, who is likewise manager, and the Hon. Col. Haultain, who is likewise hon. secretary and treasurer. There are four affiliated societies: the Wellington Philosophical Society, the Auckland Institute, the Philosophical Institute of Canterbury, and the Westland Naturalists' and Acclimatisation Society.

With regard to the contents of this first volume, it opens with a capital purpose-like address from the Governor, Sir. G. Bowen, which is followed by a series of papers, for the most part of very great scientific value and interest, and which give a very favourable idea indeed of the spread of scientific knowledge in the Colony, and the number of earnest workers it contains.

Of these, those comprising the Transactions contain no fewer than twenty-two articles on Geology, Physics, Botany, Ornithology, Applied Sciences, Wave and Earthquake phenomena, &c.; these are followed by thirty-nine papers and verbal descriptions of scientific phenomena; and these again by ten essays of great research and merit, respectively entitled:—

"On the Geographical Botany of New Zealand," "On the Leading Features of the Geographical Botany of the Provinces of Nelson and Marlborough," "Remarks on a Comparison of the General Features of the Flora of the Provinces of Nelson and Marlborough, with that of Canterbury," "Sketch of the Botany of Otago," "On the Ornithology of New Zealand," "On the Botany, Geographic and Economic, of the North Island of New Zealand," "On the Cultivation and Acclimatisation of Trees and Plants," "On the Geology of the North Island of New Zealand," "Short sketch of the Maori Races," "On the Maori Races."

The last of these is the result of the life-long labours of one of the most accomplished and industrious of the early missionary settlers in the North Island, Mr. Colenso; whose botanical researches have been as important as his ethnological labours. The typography of the work is very good, but the illustrations are unequal, and often much larger than they need be for all they demonstrate.

From such an excellent beginning who can doubt that important results will quickly follow? To us the experiment of affiliation is a very interesting one, the success of which we hope to witness. That there are in the New Zealand scheme sources of failure is obvious, but we see none which may not be overcome by firmness, judgment, and singleness of purpose, on the part of the head Institute, and by the avoidance of petty jealousies, and a due regard to the interests of Science on that of the affiliated societies. The power vested in the governors of rejecting unfit papers is a most valuable one, if conscientiously exercised; it cannot be too vigilantly watched by the contributors to the Institute, nor too carefully conducted by the governors. In this country the power vested in the councils of our societies, of suppressing papers on the advice of competent referees, works on the whole well, though we could indicate societies, and some of high position, too, which have quite recently published in their Transactions worthless matter, that has evidently never been examined by a council, nor reported on by competent advisers; thus squandering the property of the members, and doing discredit to the science which the members represent.

Is it too much to expect that our powerful and comparatively wealthy societies should affiliate some of their weaker brethren, who devote themselves to the same branches of science? applying their own tests to the admission of contributions to their Transactions, and leaving to lesser societies the freedom to govern themselves as they think best. Of such an affiliation many of the lesser societies would be proud, and the greater could not but benefit by the arrangement.

To one other point only we shall allude in reference to the Transactions of the New Zealand Institute, viz., the advisability of breaking it up, now or at some future time, into series treating of certain branches of science, for the convenience both of workers and purchasers. By so doing, the members would set another example well worth following by some of the oldest, gravest, and most learned of the English, Irish, and Scotch scientific bodies, which publish ponderous quartos of mixed sciences, that are worth neither their shelf-room nor their cost of binding to the mass of the members and purchasers. But we shudder at the thought of suggesting to such august bodies as the Royal Societies of London and Edinburgh, or the Linnean Society, or the Royal Irish Academy, the propriety of breaking up their Transactions into series confined to definite branches of science, and husbanding their funds by giving to their members and correspondents such only as they care to have.

OTHER WORLDS THAN OURS

Other Worlds than Ours. By R. A. Proctor. (Longmans, 1870.)

THAT Mr. Proctor could write, if he were so disposed, an excellent and instructive book on certain branches of Modern Astronomy, we have no manner of doubt; but for the successful accomplishment of such a work, it would be necessary, in the first instance, to settle in his own mind what particular classes of persons the work was intended to suit; and, in the second place, it would be essential for the author to allow himself sufficient time for the completion of his project. It is from a failure in these two particulars that what would otherwise have been a truly remarkable book, is brought down to a level much below Mr. Proctor's actual abilities.

A large portion of the book is devoted to the not very fruitful question of the habitability of the planets or their satellites by beings not generically very different from the human race; but we do not think the question in Mr. Proctor's hands is advanced on either side of the argument, materially beyond where Whewell and Brewster left it. The chief novelty consists in the hypothesis that Jupiter and Saturn may possibly serve as suns to at least some of their satellites; and, by the partial emission of heat (and light?), may compensate for the extreme remoteness of the central orb of the solar system. We can neither follow nor admit the validity or consistency of Mr. Proctor's arguments. The question whether the larger planets have or have not as yet cooled down, by radiation, to a sort of normal temperature, is one of considerable interest, and lies within the scope of astronomical physics; but we cannot say that Mr. Proctor's remarks either remove or elucidate the difficulties of the points at issue. On the contrary he speaks fitfully, and hesitates between doubt and something bordering on dogmatism. For instance, in page 140, he writes thus: "We seem led to the conclusion that Jupiter is still a glowing mass. . . ." Two pages onward he remarks: "He (Jupiter) is not an incandescent body;" and then after he has given his own opinion, that Jupiter is still . . . . "fluid probably throughout, and seething with the intensity of primæval fires, ..." he forthwith falls foul of the late accomplished Dr. Whewell, in terms such as these: (p. 145) . . . . "Surely no astronomer, worthy of the name, can regard this grand orb as the cinder-centered globe of watery matter, so contemptuously dealt with by one who, be it remembered thankfully, was not an astronomer." Our readers will probably feel that such language is to be regretted, and is alike unworthy of Mr. Proctor, and inapplicable to Dr. Whewell.

We have been induced to select the foregoing passages, because they are in fact typical of the style in which Mr. Proctor's volume is written. Laplace, the elder and the younger Herschel, Humboldt, Admiral Smyth, Sir William Thomson, Mr. Tait, Dr. Balfour Stewart, Professor Tyndall, and other honoured names, all in their turn come under Mr. Proctor's adverse criticism with greater or less severity. Mr. Lockyer is the object of our author's especial censure; simply because his opinions do not square with those of our author on the nature of the solar corona visible during a total eclipse, and on which subject wise astronomers confess their profound ignorance, therefore Mr. Lockyer's opinion is condemned as "erroneous," which it may or may not be, and Mr. Lockyer himself is represented as impeding the progress of science! This strong and repeated reference to what our author regards as the scientific peccadillos of an eminent observer, seems to us all the more unnecessary, from the fact, of which Mr. Proctor must be well aware, that one main purpose of the proposed British expedition to view the forthcoming solar eclipse, is to ascertain, if possible, what is the real nature of the mysterious light which forms the corona, and which often streams from the solar photosphere in extended and fantastic forms. We venture to suggest that, for the promotion of that calmness of spirit which is essential to the successful investigation of scientific truth, the refutation of scientific error should in all cases be free from even the appearance of strife and personality. But, passing from the criticisms on Mr. Lockyer's theory of the solar corona, the fate of Mr. Lassell, the present accomplished and experienced president of the Astronomical Society, is not more fortunate. Mr. Lassell, after years of careful observation with a magnificent instrument, unique of its kind, comes to the conclusion that not more than *four* satellites of Uranus have ever been certainly observed. Mr. Proctor, however, thus writes: "I have very little doubt that Uranus has at least eight satellites." And again, although our author admits that Sir William Thomson seems to have abandoned his theory of the probable supply of the sun's heat and light by a battery of meteors, nevertheless he thus writes: "*I am quite certain* * . . . . that at least an important proportion of the sun's heat is supplied from the meteoric streams which circulate in countless millions around him" p. 205). We may fairly ask whence has Mr. Proctor this *certain* knowledge of *countless millions of meteoric streams* impinging on the sun? And how can he, with reference to the satellites of Uranus, venture to set his *opinion* in antagonism with the results of the protracted observations of so accomplished and experienced an astronomer as Mr. Lassell?

We may call Mr. Proctor's attention to the probable inadvertency of his description of the hydrogen spectrum as *white*, which it is not in any sense; and to his inexact description of Mr. Carrington's remarkable observation of what may have been a solar outburst. Mr. Carrington did not use, and he could not have used, as Mr. Proctor assumes, a dark glass in projecting the solar image on a screen, and consequently the alleged *breaking of this dark glass* by the presumed solar outburst, could have occurred in our author's imagination alone. These mis-statements, however, are easily corrigible in a second edition, presuming they are not typical of much else in the volume itself.

We think so highly of Mr. Proctor's astronomical knowledge and general ability, that we have ventured to point out what strike us as blemishes in a work which contains so much that is suggestive and valuable. Among the portions that are most suggestive are Mr. Proctor's remarks on the distribution and motions of the stars *in streams*; we are far from satisfied that our author has as yet made good his case; but whether his theory be correct or not, the suggestions are valuable, and afford ample scope for the astronomy of the future. Overlooking, however, and forgetting the blemishes, we feel no hesitation in recommending this volume to the perusal of all who are interested in the progress of one of the noblest and most fascinating of the sciences. Some of the plates which illustrate the volume are a decided advance upon all their predecessors of a like kind, and augur well as promises of yet further improvements.

C. PRITCHARD

* The italics are ours.

OUR BOOK SHELF

Balfour's Class-book of Botany; being an Introduction to the Study of the Vegetable Kingdom. With upwards of 1,800 illustrations. Third edition. (Edinburgh: A. and C. Black, 1870.)

PROFESSOR BALFOUR'S "Class Book of Botany" is too well and favourably known to botanists, whether teachers or learners, to require any introduction to our readers. It is, as far as we know, the only work which a lecturer can take in his hand as a safe text-book for the whole of such a course as is required to prepare students for our university or medical examinations. Every branch of botany, structural and morphological, physiological, systematic, geographical, and palæontological, is treated in so exhaustive a manner as to leave little to be desired. The illustrations also form, when enlarged, the very best set of diagrams that a lecturer can have. After this, it may seem hypercritical to find any fault with the new edition. We cannot, however, but regret that the opportunity was not taken of rendering the book still more complete by bringing it down to our present state of knowledge. As stated on the title-page, the additions and corrections are entirely confined to the department of organography, and, as far as they go, are valuable. In particular, the treatment of the subjects of carpology, inflorescence, and phyllotaxis, is rendered much more complete. In other departments we have no such additions, and we miss any reference to the recent labours of Hildebrand, Parlatore, and others, following those of Darwin in the department of fertilisation; of King, Strasburger, and many others in the structure of the reproductive organs of Cryptogams, or to the remarkable observations of Prillieux, Rose, and Brongniart on the movements of chlorophyll. In the department of vegetable palæontology in particular, Unger, Schimper, Heer, Ettingshausen, Dawson, and Carruthers have rendered the science of 1854 scarcely recognisable in 1870; and yet we find not a word added, even to the first edition. The fault appears to be that the book was "stereotyped." Scientific works ought never to be stereotyped. The author has evidently been exceedingly cramped in the insertion of new matter, and the correction of errors has been rendered impossible. Thus we find repeated the old account of the mode of fertilisation in *Parnassia*, which has been shown by both English and Continental botanists to be erroneous, and illustrated by a drawing which is quite incorrect. The work is one, however, which is indispensable to the class-room, and should be in the hands of every teacher. A. W. B.

Proceedings of the London Mathematical Society. Vols. I. and II.

THIS Society met for the first time in January, 1865. At that time the only society in London which received mathematical papers was the Royal Society, while the Philosophical Society of Cambridge naturally took its tone from the university examinations, and paid more attention to the proposal and solution of problems than to the working out of new principles. The present society was formed mainly in order that investigations carried on independently over a wide range of subjects might be compared, and that from the comparison there might grow up some new calculus which should bear to the analysis of the present day the same kind of relation that integration bears to Wallis's elaborate investigations of the properties of the centre of gravity. There were other collateral objects of scarcely inferior importance, such as the improvement and extension of the language of mathematics, the simplification of demonstrations of known truths, and the study of the history of mathematics. There is scarcely a branch of pure mathematics in which the society has not advanced the boundaries between the known and the unknown, but the attention of the leading members has been chiefly devoted to the higher curves.

The introduction of a new word or phrase has often marked an epoch in the history of a science; many of the theories systematised by Darwin are to be found in the writings of others before the happy phrase "natural selection" gave them a simple and enduring shape. Of like importance is Mr. Grove's expression, "correlation of forces," and we find in these proceedings several words, the introduction of which appears likely to play an important part in the development of the science.

The word "quantic" for a "rational and integral function" has been for many years in use, but "deficiency" for the number by which the double points of a curve (including cusps) fall short of the maximum $\frac{1}{2}(n-1)(n-2)$, and "unicursal curves" for those curves in which the deficiency = 0, or, in other words, curves in which the co-ordinates (x, y, z) can be expressed as rational and integral functions of a parameter θ, were first used by Prof. Cayley before this society.

The history of mathematics is enriched by an interesting paper by Mr. Merrifield, showing that the Arabs were acquainted with the property of the radical axis.

Prof. Sylvester's proof of Newton's celebrated rule for the discovery of imaginary roots, hitherto undemonstrated, and Prof. DeMorgan's simple proof that every function has a root (which we should be glad to see in a fuller form) are the principal gains in the mere demonstration of known truths. We could wish to see in print the other communications made to the society by Professors Sylvester and DeMorgan, especially those by the former relating to unicursal derivation of successive points on cubic curves, and to residuals.

We heartily congratulate the society on the vitality and enthusiasm evident in every page of its proceedings, and join with them in their hope that they may shortly obtain a tenement of their own worthy of the great, albeit unostentatious, work in which they are engaged.

H. A. N.

Jahrbuch der K.-K. Geologischen Reichsanstalt. Band XIX. Nos. 3 and 4. 1869.

OF this admirable repertory of memoirs on the geology of the Austrian Dominions, the last two numbers for 1869 have lately reached us. This publication contains the cream of the communications made by members of the Imperial Geological Institution, to which the carrying out of the survey of that great and varied tract of country subject to the Austrian sovereign is entrusted; it always includes many papers of great importance to the student of general geology, and the portions now before us present no falling off in this respect. Of strictly geological papers Prof. D. Stur is the principal contributor. He describes the occurrence of brown coal in the district of Budafa, in Hungary; reports at considerable length on the results of the geological survey of the environs of Schmöllnitz and Göllnitz, also in Hungary; and contributes two other papers of more strictly local interest; but the most important of all the geological memoirs is F. von Hauer's notice of the geology of the Western Carpathians, a most interesting district in every respect. Dr. M. Neumayr's contributions to the knowledge of indigenous fossil faunæ contain descriptions of the univalve shells of the freshwater marls of Dalmatia, and of the Congerian strata of Croatia and Western Sclavonia; the species, many of which are new, are well represented on four plates. Five plates are also devoted to the illustration of another palæontological paper, which will probably possess the most interest of all for extra-Austrian geologists—namely, Dr. E. von Mojsisovics's memoir on the Cephalopod-fauna of the Alpine Muschelkalk, some of the species included in which are remarkable for their wide geographical distribution, especially the characteristic species of the zone (*Arcestes* or *Ammonites Studeri*), which ranges to the Himalayas in one direction, and to Spitzbergen in another.

LETTERS TO THE EDITOR

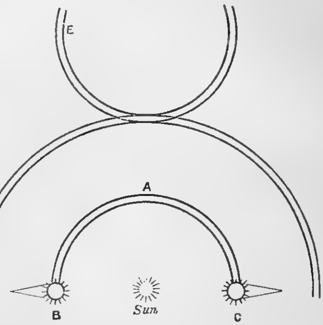
[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Parhelia

(1.) *Seen near Llandudno on 23rd of June, by* JOSEPH PAGET

THIS evening the phenomenon of which a sketch is enclosed was seen here, viz.:—

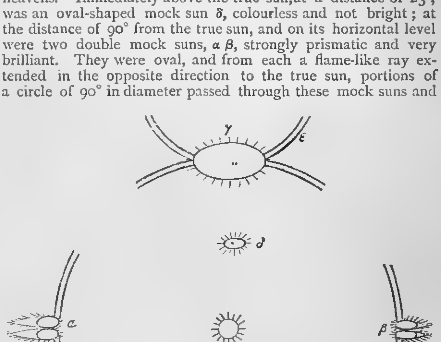
A B C, a portion of a circle of 45° diameter, resting at B and C on mock suns from which a sheaf of light proceeded outwards.



D, a portion of another circle of 90° diameter, at the apex of which was an inverted portion of another circle E of 45° diameter.

(2.) *Seen at Highfield House, near Nottingham, on 23rd of June*

At $7^h\ 36^m$ P.M., there was an extraordinary appearance in the heavens. Immediately above the true sun, at a distance of 23°, was an oval-shaped mock sun δ, colourless and not bright; at the distance of 90° from the true sun, and on its horizontal level were two double mock suns, $\alpha\ \beta$, strongly prismatic and very brilliant. They were oval, and from each a flame-like ray extended in the opposite direction to the true sun, portions of a circle of 90° in diameter passed through these mock suns and



also through an unusually large mock sun γ, situated 45° immediately above the true sun, and which was prismatic and almost too brilliant to look at; from this mock sun there was also a portion of a circle of 45° in diameter.

The phenomenon faded away at $7^h\ 53^m$ P.M. The weather had been hot from the 13th inst., reaching 86·8° in the shade on the 21st, and 86·6° on the 22nd; whilst on the 23rd (the day of this occurrence) it was only 72°, and on the morning of the 24th, the minimum temperature had fallen to 42·9° at four feet, and to 38·2° on the grass, or a fall of 43·8° in temperature in

36 hours. During daylight on the 23rd there was an Aurora Borealis which continued till 1 A.M. of the 24th; at 7 A.M. rain commenced.

Highfield House Observatory, June 24 E. J. LOWE

(3.) *Seen near Burton-on-Trent, June 23.*

The rare and beautiful phenomenon of parhelia was seen by many observers in this neighbourhood at about seven o'clock on the evening of the 23rd inst., and it continued to be visible for more than a quarter of an hour.

The horizontal bar of light, the coloured halo, and the intensified light at the intersections, as also a portion of the upper bow, were all seen very distinctly. The temperature of the atmosphere at the time was rapidly lowering, and during the night a considerable quantity of rain fell.

I presume that the phenomenon was very local, as I have seen no notice of its occurrence in any of the daily papers to which I have had access.

Burton-on-Trent, June 25 EDWIN BROWN

Natural History of Celebes

It will be, perhaps, not without interest to the readers of your periodical to receive the information that I am leaving for Celebes, on a natural history expedition, at the beginning of July, to stay there for a considerable time with the purpose of exploring first this interesting and little known island as far as possible, and this done, the islands in the neighbourhood.

I shall be happy to learn the wishes and supply the desiderata of any naturalist who takes a special interest in the fauna or flora of the island. Letters will reach me Poste Restante, Menado, Celebes.

I have just finished the translation and publication of Mr. Wallace's "Contribution to the Theory of Natural Selection."

ADOLF BERNHARD MEYER

12, Victoria Road, Kensington, London, W., June 28

Fertilisation of the Barberry

C. K. SPRENGEL, in his *Entdeckte Geheimniss der Natur im Bau und in der Befruchtung der Blumen*, gives an excellent account of the structure of the Common Barberry, *Berberis vulgaris*, and points out how it is visited by insects, and how, upon the touch of an insect's limb or proboscis, the irritable filaments move inwards, and press the opened anthers against the stigma. It is needless to recapitulate the details of structure and movements of a plant so well known, but I venture to think that there is a function and a purpose beyond those which Sprengel's ingenuity has pointed out. Sprengel's great object was to show how insects and flowers mutually help one another, and, consequently, when he had shown that the anther could not shed its pollen on the stigma until the filament was touched by an insect, and that, when so touched, the open anther became pressed against the stigma, he was satisfied. He does not seem to have been fully alive to the wider generalisation made by Mr. Darwin, that this relation of insects to flowers serves the purpose of crossing by fertilising the stigma of one flower with pollen taken from another. At any rate, in this case he has been content with the ingenuity of the apparatus for self-fertilisation through the instrumentality of an insect.

But there are one or two circumstances which seem to show that there is something further in this curious motion of the stamens of the Barberry.

In the first place, each filament only moves when touched at a particular spot near its base, where a very slight touch—*e.g.*, with a human hair—will make it start up. The other stamens remain unmoved, and a bee will frequently visit the flowers and carry off the abundant nectar without moving any stamen at all.

In the second place, after the stamen has moved inwards, which it does rapidly and with a sort of jerk, it soon begins again to move slowly outwards and backwards, and in a short time recovers its original position. Pollen still remains in the anther cells, and the stamen is ready again to jump up towards the stigma on the visit of another insect.

Now these two facts are not explained, if the sole object of the movement of the stamens is self-fertilisation. Why in that case should each stamen move separately when gently touched on its inner side, and why should it return to its original place, instead of remaining with its whole mass of pollen firmly pressed against its own stigma? Why, too, should it move inwards rapidly, and retreat slowly?

But if the object be to enable the insect to carry pollen to another flower, these facts, as well as the remarkable points of structure and function noticed by Sprengel and others, are all explained.

The separate stamen moves when touched on its inner side by the thin proboscis or limb of an insect, in such a way as to leave its pollen on that limb or proboscis, or on the body of the insect, which will then generally be interposed between the anther and the stigma. And the stamen returns to its place after the insect has departed laden with pollen to other flowers, in such a way as that, when more nectar has been deposited and another insect comes and touches it, it may again spring up and deposit some of its remaining pollen on that insect, to be again carried by it to fresh flowers.

If this be so, the function of the whole apparatus is not to cause self-fertilisation, but to enable insects to carry pollen from one flower to another. The case is curious, because at first sight the very remarkable movement of the stamens in this plant certainly looks like an ingenious device for *self-fertilisation*, and as such, an argument against the crossing theory.

Mr. Darwin, to whom I have mentioned this, tells me in confirmation, that the North American Barberries (*Mahonia*) have become so much crossed that it is almost impossible in this country to procure a true specimen of the two or three forms originally introduced. T. H. FARRER

The Corona

I DO not think Dr. Gould's lucid account of his own views could have been misunderstood. I, at least, who replied to him, understood him in the sense in which he has written to you.

The chief question at issue has been, whether there is or not *anything at the sun* (to use Dr. Gould's expression, as, on account of its very vagueness, most suitable), *outside the prominences*. Mr. Lockyer has said *no*; I and others have said *yes*; and Dr. Gould has helped to prove we were right. It has always seemed to me, however, that the photographs taken by Mr. De La Rue and Fr. Secchi, in 1860, had settled the question.

The question relating to matter outside this brilliant appendage, is of less moment. We know certainly that during totality there is *some* light in our atmosphere, and the question where this begins or ends is more interesting to the meteorologist than to the astronomer. But I would invite Dr. Gould's attention to the fact that in the March number of the Astronomical Society's *Monthly Notices*, I have given a simple mathematical proof of the fact that no atmospheric light *can* come in any considerable total eclipse from any region of the sky within seven or eight degrees of the eclipsed sun.

So far as this proof is concerned, I cannot admit that the matter is one of theory at all. But my views as to the *nature of the material* producing this coronal light are not founded on absolutely certain evidence, though the evidence in their favour is very strong indeed. Moreover, I should expect that precisely those appearances would be seen which Dr. Gould regards as tending to show that the faint coronal light arises from something *which is not at the sun*.

But really where a mathematical demonstration of a fact is extant, the consideration of arguments derived from admittedly doubtful evidence seems a mere waste of time. It may as readily be shown that the three angles of a triangle are not equal to two right angles, as that in the Indian or American eclipses atmospheric glare could have been visible within seven or eight degrees of the eclipsed sun.

In the supplementary number of the *Notices* of the Astronomical Society, I hope to give a further explanation of the extremely simple proof on which my views depend; and (because all questions involving considerations of tri-dimensional space are perplexing to most non-mathematicians) I am having a mechanical figure constructed to make the matter clearer, in illustration of papers I hope to read at the next meeting of the British Association, and next November before the Astronomical Society.

RICHARD A. PROCTOR

South Lambeth, June 24

Euclid as a Text-book

THE suggestion of Mr. Levett with regard to the formation of an Association for securing a general reform in the teaching of geometry, is worthy of being at once carried out. Such an association would show how general amongst teachers is the dissatisfaction with the ancient methods, and might lead to more uniformity of practice by securing a free public discussion of the best methods and most suitable terms. There are abundant materials available as the basis of discussion, and no doubt the aid of the most distinguished geometers would be easily secured by the Association, so as to bring about a decision on controvertible points. Several English text-books are already in existence, and there are many good features as well as many defects in all of them. Excellent series of lectures have lately been given on the subject in London and Cambridge, and some of the lecturers have printed very full notes for the use of their students. The syllabuses of Mr. Clifford, and Professor Hirst, are very suggestive.

After an exhaustive discussion, the Association would doubtless be able to secure the publication of a text-book which would have the approval and patronage of all its members. If the readers of NATURE who approve of the plan, would send their names to Mr. Levett, with contributions, if possible, towards expenses of printing, postage, and advertisement, some practical result would soon ensue.

Brixton RICHARD WORMELL

Storms and Fishes

CAN any of your readers give me any information on the following subjects?

1. What is Le Verrier's law of storms?

I asked this question some months ago, but no one replied to it.

2. Have any articles or special works on Poisonous Fishes appeared since Dumeril's memoir on the subject?

I should also be obliged to anyone who can give me information regarding fresh-water fishes that are in the habit of attacking bathers. Don Paez describes such fishes in some of the South American rivers. M.D.

The Scientific Education of Women.

WILL you kindly allow me to add some information to an article which I have just seen in NATURE of June 16, on the Scientific Education of Women? First, however, allow me to correct an error which was made in an article on Lectures to Ladies, which I observed in NATURE some months ago, in which it was stated that the first series of educational lectures to women given under the auspices of any society for such lectures, was given under the direction of the Edinburgh Ladies' Educational Association. The fact is, that the first series of lectures of that kind, in recent years, was given in Liverpool, Manchester, Leeds, and Sheffield, under the auspices of the "North of England Council for the higher Education of Women." These lectures were given in the Autumn of 1867. The subject was Physical Astronomy. Under the auspices of that society, as well as of other societies, many sets of lectures have been given since that time on subjects connected with Physical Science. But I have always regretted, as the writer of your article regrets, that a greater number of the lectures have not been on such subjects, for I have always found that women exhibit a peculiar aptitude for the study of Physical Science. I have also found in my own experience a considerable desire on the part of women for such studies; and I believe that the fewness of such courses of lectures to them is to be put down to the scarcity of people at once competent and willing to teach them such subjects. From all that I have seen there is in my mind no doubt that the desire for true scientific instruction throughout the country, both among women and among men, exceeds the present possibility of supplying that teaching. I called your attention to this fact some months ago, and I believe that we cannot over-estimate its significance.

With respect to the remarks made in your article as to the medical education of women, it may interest your readers to know that Cambridge, which has moved so much in the matter of women's education, has not been behind-hand in this; but that a few weeks ago a petition went up to parliament praying that, in the ensuing legislation for the medical profession, provision might be made to prevent the exclusion of women from that profession. This petition was from resident graduates, and one hundred names were attached, among which were those of two heads of houses, nine University professors, and thirty-eight tutors or assistant-tutors of Colleges.

In your notice of the exhibitions in connection with the lectures to women at Cambridge, you speak of the Cambridge higher examinations for women. These examinations, which were instituted last year for women over eighteen years of age, were suggested to the University by a memorial presented by the North of England Council for the Higher Education of Women, a body to whose exertions the whole of this cause is deeply indebted. JAMES STUART

June 22

ILLUMINATION OF THE SEA

THE following is derived from the *Kölnische Zeitung* of June 19:—

"Gulf of Siam, April 11

"Last night, between two and three o'clock, I had the opportunity of witnessing an illumination of the sea of the most peculiar kind. It had become quite calm, after a sharp breeze which had sprung up from the N.N.W., caused by a passing storm in the distance. Heat-lightning was still very frequent in the west horizon, and the sky was covered with light clouds, through which the moon shone rather brightly. We took in sail and set the engines going. I then noticed in the water large white flakes which I had at first taken to be reflections of the moon; they were about a fathom in diameter, apparently lustreless, and of no particular shape, like objects seen lying deep in the water. By the rising and falling of the sea's surface these flakes floated off to a short distance from the ship without imparting any noticeable increase of brightness to the water illuminated by the moon's rays. After steaming further forward for six or seven knots, a most wonderful spectacle presented itself. On both sides obliquely in front of us, long white waves of light were seen floating towards the ship, increasing in brightness and rapidity till at last they almost disappeared, and nothing was observed but a white lustreless, whirling (*schwirrendes*) light upon the water. After gazing for some time it was impossible to distinguish between water, sky, and atmosphere, all which were but just now clearly distinguishable, and a thick fog in long streaks appeared to be driving upon the ship with furious swiftness. The phenomenon of light was somewhat similar to that which would be produced by the whirling round of a ball striped black and white so rapidly that the white stripes seem to be lost and blended with the dark ones. The light was just as if we were enveloped in a thick white fog. The direction of the waves of light upon the ship was always on both sides obliquely from the front. The phenomenon lasted about five minutes, and repeated itself once more afterwards for about two minutes. Without doubt, therefore, shoals of small creatures in the water were the cause of this luminosity, and the waves of light find their cause, according to my conviction, in the white flakes above described. Yet their moderate velocity of $1\frac{1}{2}$ geog. mile per hour, and the weak light at first emitted by each flake, so weak as not to influence the tint of the surface-water, does not seem calculated to call forth a phenomenon of such magical effect as the one described. The luminous appearance commonly seen in the wake of a ship, or in water disturbed by oars or rudder, is not to be compared with such a phenomenon as the above. In the former the light is lustrous, glaring green and blue, like phosphorus, often very splendid in deep clear water, mingled with a reddish white foam. We saw a beautiful instance of this kind one night, in perfectly still and smooth water, in a lonely bay of Nipon. It was pitch dark and perfectly quiet, when a heavy shower of rain came on, in large but not dense drops. Every drop as it struck the water became illuminated, little drops of fire sprang up in the air, and a little luminous circle formed itself. It seemed as if the bay was suddenly filled with little flowers of fire. This phenomenon was almost immediately dissipated by a puff of wind."

FLIGHT.—FIGURE OF 8 WAVE THEORY OF WING MOVEMENTS

IN the Proceedings of the Royal Institution of Great Britain for March 1867, Dr. J. Bell Pettigrew, F.R.S., the distinguished curator of the museum of the Royal College of Surgeons of Edinburgh, announced the startling discovery that all wings whatever—those of the insect, bat, or bird—were twisted upon themselves structurally, and that they twisted and untwisted during their action—that in short they formed *mobile helices* or *screws*. In June of the same year (1867), Dr. Pettigrew, following up his admirable researches, read an elaborate memoir "On the Mechanism of Flight" before the Linnean Society of London, wherein he conclusively proves, by a large number of dissections and experiments, in which he greatly excels, that not only is the wing a screw structurally and physiologically, but further that it is a reciprocating screw. He shows, in fact, that the wing, during its oscillations, describes a figure of 8 track similar in some respects to those described by an oar in sculling. This holds true of the vibrating wing of the insect, bat, and bird, when the bodies of these animals are artificially fixed.

When, however, the creatures are liberated, and flying at a high horizontal speed, the figure of 8, as he points out, is curiously enough converted into a wave track, from the wing being carried forward by the body, and from its consequently never being permitted to complete more than a single curve of the 8. This is an entirely new view of the structure and functions of the wing, and one fraught with the deepest possible interest to the aeronautical world. It promises to solve everything. Dr. Pettigrew's remarkable discovery has received an unlooked-for confirmation within the last few months at the hands of Professor Marey, of the College of France, Paris. This gentleman, whose skill in applying the graphic method to physiological inquiry is unequalled, has succeeded in causing the wing of the insect and bird to register their own movements, and has established, by an actual *experimentum crucis*, the absolute correctness of Dr. Pettigrew's views. Professor Marey's mode of registering displays much ingenuity, and is briefly as follows:—"A cylinder revolving at a given speed is enveloped by a sheet of thin paper smeared with lamp black, and to this the tip of the rapidly vibrating wing of the insect is applied in such a manner as to cause it to brush out its track on the blackened paper, which it readily does. A similar result is obtained in the bird by fixing a registering apparatus to the wing and causing the bird to fly in a chamber. In this case the registering apparatus is connected with the cylinder by means of delicate wires, and the registering is effected by means of electricity. In both cases the figure of 8 and wave movements, originally described and figured by Dr. Pettigrew, are faithfully reproduced. The way of a wing in the air has hitherto been regarded as a physiological puzzle of great magnitude; and well it might be, since some insects (the common fly for example) vibrate their wings at the almost inconceivable speed of 300 strokes per second, that is, 18,000 times in a minute!

It should be added that though Professor Marey endorses Dr. Pettigrew's view as to a figure of 8 movement, and has recently admitted his priority in that observation, he is yet by no means of the same opinion as Pettigrew as to the explanation of the mechanical effect of the movements and the influence of the bird's weight. Pettigrew maintains that the wings act as inclined planes in such a way that the bird actually rises by its own weight. Dr. Marey will not admit this at all, and is at issue with the Scotch anatomist on some other matters of moment, as he recently informed the writer. The beautiful and ingenious experiments which Dr. Marey is now carrying on will place these matters beyond conjecture by the light of experiment.

A FALL OF YELLOW RAIN

ON the 14th of February a remarkable yellow rain fell at Gênes. The following details respecting it are given in a letter addressed to M. Ad. Quetelet by M. G. Boccardo, director of the Technical Institute of Gênes, who examined it in concert with Dr. Castellani, professor of chemistry. The quantitative analysis gave the following results:—

Water	6·490	per cent.
Nitrogenous organic substances	6·611	,,
Sand and clay	65·618	,,
Oxide of iron	14·692	,,
Carbonate of lime	8·589	,,

Observed narrowly under the microscope, the presence was revealed of a number of spherical or irregular ovoid substances of a cobalt blue colour; corpuscles similar to the spores of *Peziza or Permospora;* spores of *Demaziaceæ* or *Spheriaceæ;* a fragment of a *Torulacea* (?); corpuscles of a pearly colour, concentrically zoned, probably small grains of fecula; gonidia of lichens; very scarce fragments of *Diatomaceæ;* spores of an olive brown colour; a few fragments of filaments of *Oscillaria*, *Ulothrix*, and *Melosira varians;* a fragment of *Synedra;* a peltate hair from an olive leaf. If, instead of collecting the earth on the morning of the 11th, when it had already been subjected to the action of rain falling for several hours, I had been able (writes M. Boccardo) to observe the phenomenon during the night, at the moment when it was produced, it is very probable that the microscope would have shown the existence of several kinds of Infusoria, as has been the case in several similar instances.

The author notes that the direction of the wind at Gênes during the night of the 13th and 14th was from the south-east, and without being exactly a hurricane as on the preceding few days, was still very strong. The temperature, previously exceptionally low, had risen, and probably did not fall during the night below $+4°$ R. ($5°$ C. or $41°$ F.) The journals state that on that date a tempest devastated the coasts of Sicily. M. Boccardo, following P. Denza, proposes the theory that the dust came from the coast of Africa. "We ought not to forget," he writes, "that according to Maury's theory of the circulation of the atmosphere, these clouds of dust may well have travelled a long distance before touching the soil of Italy, coming from beyond the Atlantic, like those which in 1846 spread from Guiana to the Azores, over the south of France and the whole of Italy."

RELICS OF NON-HISTORIC TIMES IN JERSEY

CONSERVATION *v.* DESTRUCTION

ON the 18th of May information was received from Jersey of the partial demolition of some tumuli, hitherto undescribed in that island; and, accordingly, two gentlemen, interested in the conservation of all ancient monuments, resolved to make a tour of inspection of the pre-historic remains in Jersey without delay, and the following is the result of the inspection:—The time was necessarily brief, occupying only two days, the party arriving at Jersey at 11 A.M. on the 19th, and leaving the island at 6.45 A.M. on the 21st. A summary of the route taken may be useful to tourists and others who may wish to visit all the pre-historic stone monuments in Jersey, as far as they are known at present. Leaving St. Helier's by the St. Aubin's road, the first attraction is the Ville Nouaux Cromlech, not far from the first martello tower. This structure was examined last year by leave of M. de Quetteville, the proprietor, and described at the time.

As now exposed to view, this cromlech appears to be an elongated *allée couverte*, nearly due east and west, measuring 35 feet in length. Its sides, about four feet apart, are as nearly as possible parallel, although there are indications of the avenue being narrowed towards its eastern extremity, as we should expect to find. The side blocks of stone average from 4 to 5 feet in height, and number eleven on the northern and seven on the southern side, the western end being closed by a fine single slab. The interstices between these blocks are roughly filled up with irregularly shaped smaller stones, evidently built in to prevent the exterior earth and soil of the superimposed tumulus from falling into the sepulchral grotto.

There must have been formerly at least nine cap-stones; of these, two have been removed, as observed above, whilst the whole fabric appears to have been tilted, with an inclination to the south, probably caused either by the unequal pressure of the accumulated sand-drift on the northern side, or by the removal of the ballast from its southern supports. It is difficult to determine whether all the cap-stones are in their original positions, or whether some of them have not slipped between the side blocks from their summits.

Several urns, tulip shaped, &c., with a cylindrical stone muller, were brought back to Guernsey from this cromlech, and are now deposited in Mr. Lukis's museum.

Not far to the north of this spot is a semicircle of stones, which presents a suspicious appearance.

It is much to be wished that the tenant of this field would prevent the causes of the filthy state in which this cromlech is at present. The stones themselves have not been disturbed since the last exploration.

Entering St. Laurence's parish, to the right of the road, on the hill above the vineries, is La Blanche Pierre, a menhir which is fortunately still preserved. The route is next taken to the north-west, through St. Peter's parish to St. Ouen's, and the small hamlet of Trodais. Here the party visited M. Lefeuvre, who in the course of agricultural operations, has removed a large portion of tumuli on his property, and who, six years ago, found within one of them, and has in his possession, a remarkable cinerary urn with four handles, evidently for suspension. The upper portion of this urn, which is hand-made, not turned, is likewise decorated with an ornamental border, consisting of horizontal lines, so arranged as to form three triangles between each handle. It is of a different and later type than the urns discovered in the cromlechs of these islands. M. Lefeuvre accompanied his visitors to the sites of the tumuli. These curious mounds are in two groups, one group being called *Les Hougues de Millais*, on one spur of the hills overlooking L'Etac and St. Ouen's bay, to N.W. of *La Robeline*, and the other on a similar spur to the S.E. of the former, about 800 yards distant, in a line with St. Ouen's windmill; these last are called *Les Monts de Grantez*. A portion only of one of the *Hougues* remains, and exhibits a series of cap-stones, five in number, of which four remain, supported by a dry-walling of smaller slabs, forming a tunnel about 18 feet long, which lies east and west, and was blocked at either end with a broad stone, of which the west one alone is *in situ*. It presents an exact parallel to the *Creux des Faias*, which existed till lately in St. Peter's parish, Guernsey, a few hundred yards west of the menhir at Les Paysans; but which has been swept entirely away. A granite muller was picked up here by the visitors, which also resembled, in a remarkable manner, a similar one picked up but a day or two before by the same gentleman, on the site of the *Creux des Faias*, in Guernsey, showing an identity of manufacture and a contemporaneity of construction of the tumuli in both islands.

Leaving Les Hougues, and after visiting Les Monts de Grantez, St. Ouen's church, which is being magnificently restored, was examined, and a worked stone of the Neolithic period picked up in the churchyard. Pursuing the circuit of ancient remains, the route descends towards the sea by St. Ouen's pond, where, in the Val de la Mare, St. Peter's parish, are *Les Trois Roches*, in all probability a portion of a cyclolith. Two only are upright, the third lying prone at a little distance and not visible at first sight, until one approaches close to it. The ground being marshy it has formed a pit for itself. The upright stones have been apparently worked into shape on their summits, whilst their sides are almost polished from cattle rubbing against them. The new road is now traversed through the western portion of St. Brelade's parish, between the dreary *dunes* of *Les Quenvais*, and ascending by *La Pulente* on to the hills of La Moye; a kitchen-midden is to be found at the summit of the ascent, where the soil has been scarped in the formation of the road, a mass of limpet, ormer, mussel, and other shells, at some feet in depth below the surface of the original soil.

The famous menhir of Le Quesnel, which stood so picturesquely to the south of this spot this time last year, has fallen a victim, and in its place a large quarry yawns; but in Le Marais, close by, is a portion of a circle, and an alignement, in connection with the former menhir, is still to be traced. Close to Moye signal staff a natural cropping up of the rock presents a striking resemblance to a cromlech and circle round it. About half a mile from Le Quesnel, and directly above La Corbière Point, is a fine single stone Dolmen called La Table des Marthes, beneath which were found some bronzed implements many years since.

Over the granite quarries of La Fosse Vaurin, is a curious natural work which, aided by the hand of man, presents the appearance of two basins with a channel for emptying one, whilst the fissures to the east resemble a cross, the work, perhaps, of some hermit in mediæval times. Several mullers, worked stones, &c., were found in the locality during this brief visit, and brought back to Mr. Lukis's collection. A seven miles' drive brings one back to St. Helier's. This day's visitation occupied from 11 A.M., until 9 P.M., but much time was spent in sketching and measuring, searching for stone implements, &c. The find for the day was tolerably good, viz.:—Ville Nouaux Cromlech, 1; Les Hougues de Millais, 1; St. Ouen's churchyard, 1; Les Trois Roches, 3; La Moye, 5 stone implements.

S. P. OLIVER, Lieut. R.A.

40, Hauteville, Guernsey

SOUNDINGS AND DREDGINGS BY THE UNITED STATES COAST SURVEY

IN the office of the Coast Survey at Washington there are about 9,000 specimens of various kinds of marine animals which were brought up by the sounding lead from the sea-bottom, in the region between the shore of Florida and adjacent States and the outer edge of the Gulf Stream, and descending to a depth of 1,500 fathoms nearly. The dredge has been but comparatively little used along the coast of the United States, and that so many specimens were collected by the lead alone is due to the persevering care of the late superintendent of the survey, Prof. A. D. Bache, and to the instructions which he gave to the hydrographical officers. Of course, specimens brought up by the lead can include the smaller animals only, such as Foraminiferæ, Diatomaceæ, and such like; for the larger animals, the dredge must be employed.

The work thus begun has been resumed by the present superintendent of the Coast Survey, Prof. B. Peirce. The surveying parties are instructed to take observations of the depth, velocity, and direction of the Gulf Stream, the temperature and density of the water at different depths, and of the Fauna from the surface down to the bottom. By these researches we may hope that our knowledge of the phenomena of the Gulf Stream will be increased, particularly as regards its powers of transportation from shallow to deeper water, or along its bed, besides its action in forming deposits in particular localities, and its possible influence on the growth of coral reefs on its shores.

The first operations under the new direction were carried on between Key West and Havana, along the route now occupied by a telegraph cable. Dredgings were made at depths varying from 90 to 300 fathoms, and yielded Crustaceans, Annelids, Mollusks, Radiata, Foraminiferæ, Sponges, a single vegetable specimen, being a minute alga, *Centroceras clavulatum*, and "a number of nodules of a very porous limestone, similar in colour and texture to the limestone forming the range of low hills along the shore of Cuba, but composed apparently of the remains of the same animals which were found living." Among these Deltocyathus, Caryophyllia, and Pteropods were recognised in the stone, and found in various stages of fossilisation.

At the end of a descriptive list of the specimens collected during the cruise, M. de Pourtales remarks :—"It would be premature to compare this deep-sea Fauna with the animals inhabiting the regions of lesser depth on the coast of Cuba or Florida. In the first place, many of the smaller forms—such, for instance, as the Bryozoa or the Hydroid polyps of those shores—are not yet sufficiently known to enable us to say if any of the species dredged exist in any other than the abyssal region. Then, a very different value must be assigned to the different classes of animals under examination. Thus, the dead shells must be left out of the question, at least the smaller ones, for they may have dropped with the excrement of fishes, or, in the case of Pteropods, have sunk from the surface after the death of the animal. The Crustaceans and Annelids being abundant and generally sedentary, will, when better known, afford good characteristics of the regions of unequal depth. The same remark applies to the sponges and the Foraminiferæ; the great abundance of the latter and the ease with which they can be brought up by the sounding lead render them particularly useful."

From this it will be understood that the United States Coast Survey is in good hands, and may be expected, when the time comes, to take part in the suggested dredging expedition all across the Atlantic, when England and the States, after accomplishing each a half, are to meet and shake hands in mid-ocean.

NOTES

PROFESSOR HENRY, the President of the American Academy of Sciences, and Director of the Smithsonian Institution, is now in this country *en route* to the Continent to attend the meetings of the International Commission on Standards.

WHEN presiding over the distribution of prizes for the Faculty of Arts and Laws at University College, London, on Friday last, the Bishop of Exeter made some admirable remarks on the nature of a true system of education, and of the places which ought to be occupied by classics, mathematics, and natural science, and the proper method of teaching them. In all true teaching a scientific method is indispensable; it is because this scientific method has been applied to instruction in Greek and Latin, that such good results have been obtained in this department of education. The introduction of scientific teaching has not hitherto met with the same success because it has not been carried out in the same spirit. In very many instances, those who are endeavouring to promote the study of natural science as a part of education, have made the great mistake of omitting altogether that which is essential to true study, namely, scientific method. The reason why the teaching of the natural sciences still hangs back in our public schools is, in great measure, the unscientific method in which science has been taught by many. To form a part of real education, the study of science must be pursued in the same rigorous manner as that of classics or mathematics; it will then prove as hard work to the learner, and the result of its introduction must be most beneficial. While the exclusive study of mathematics must fail as a complete discipline for the understanding, and the great mathematician may be uncultivated as a man, it is very rarely that you see such a result in the student of external nature; therefore, this study must rank by the side of the other, and must hold a place in no way inferior to it. The practical importance given to these remarks by the experience of Dr. Temple at Rugby, ought to make them carry great weight with all teachers of science.

DR. HENRICI has been elected by the Council of University College, London, Professor of Mathematics, in the place of Dr. Hirst, who resigned the professorship, on his appointment to the Assistant-Registrarship of the University of London. Dr. Henrici had acted as Prof. Hirst's assistant during the whole of the session just ended. He had pursued his mathematical studies at Carlsruhe under Professor Clebsch, and subsequently at Heidelberg, where he attended Prof. Hesse's lectures on mathematics, and those by Prof. Kirchhoff on theoretical physics. While at Heidelberg he took his degree of Doctor of Philosophy in the highest grade, and the Philosophical Faculty of the University considered the dissertation which he wrote on that occasion to have so high a scientific value, that they recommended the government of Baden to recognise its importance by conferring upon Dr. Henrici a special public distinction. Dr. Henrici subsequently prosecuted his studies at Berlin and Kiel, and then came to England, where he has resided nearly five years.

THE completion of the deep-sea cable between Falmouth and Bombay was celebrated last Thursday evening by an entertainment given by Mr. Pender, chairman of the British-Indian Submarine Telegraph Company, at which royalty largely assisted. Complimentary messages were exchanged between the Viceroy of India and the President of the United States, the distance of 8,442 miles being accomplished in forty minutes; between the Prince of Wales and the Khédive, the Prince of Wales and the King of Portugal, the Prince of Wales and the President of the United States, and the Prince of Wales and the Viceroy of India. This is the first instance of direct telegraphic communication between India and America. The comic side of telegraphic communication was presented by the message between the Prince of Wales and the Viceroy, which, though despatched soon after twelve at night, and only nine minutes on its way, reached Lord Mayo at five in the morning, when his lordship was, naturally enough, fast asleep. What will be the result when the earth is completely girded with a telegraphic cable, and a message is sent to the antipodes? The question between night and day will be expanded to one between to-day and tomorrow, to say nothing of yesterday.

THE Royal Commission on Scientific Instruction and the Adancement of Science has held two meetings since our last issue.

THERE will be an election at Magdalen College, Oxford, in October next, to six Demyships and one Exhibition. Of the Demyships, one will be mathematical, one in natural science, four classical. The Exhibition will be in natural science. It is necessary that candidates for the exhibition should prove to the satisfaction of the electors that they cannot be supported at college without such assistance. Evidence on this point will be considered as confidential. No person will be eligible for the Demyships who shall have attained the age of twenty years, and (in the case of candidates in mathematics and natural science) who is not sufficiently instructed in other subjects to matriculate as a member of the college. The stipend of the above Demyships and Exhibition is 75*l.* per annum, inclusive of all allowances; but there are tenable with the Demyships certain College Exhibitions, which raise their annual value, on an average, to about 83*l*. They are tenable for five years. Testimonials of good conduct will be required, and a certificate of birth and baptism, which must be presented to the President on Monday, the third day of October, between the hours of three and six, or eight and nine P.M. The examination will commence on the following day. Particulars relating to the examinations in the various subjects may be obtained by applying to the senior tutor. No entrance fees or caution money are required by the college. The University fees payable on matriculation amount to 2*l*. 10*s*.

IT is refreshing to learn that that reverend and tory institution, St. John's College, Oxford, has just elected to a fellowship a devoted physical investigator, in the person of Mr. Bosanquet. This election is important, not only as a recognition of natural knowledge, but also of the principle of research as against that of mere education.

CONSEQUENT on the death of the Rev. Dr. Luby, one of the Senior Fellows of Trinity College, Dublin, the Rev. Mr. Jellett, B.D., President of the Royal Irish Academy, has been co-opted a Senior Fellow. It is understood that the Professorship of Natural Philosophy held by Mr. Jellett will be given to the Rev. R. Townsend, A.M., author of the "Modern Geometry of the Point, Line, and Circle."

THE Minister of Public Instruction in Italy has promised a grant of 1,600 *lire* towards the expenses of instituting a laboratory of cryptogamic botany in Pavia; and it is hoped that a contribution will also be received from the Minister of Agriculture.

THE death of Sir James Simpson has been followed by another heavy loss to medical science in that of Professor Syme, who died on Sunday last, at the age of seventy. Mr. Syme was for a short time Professor of Surgery in University College, London; for a much longer period he occupied the chair of Clinical Surgery in the University of Edinburgh, which he resigned only quite recently to his son-in-law, Mr. Joseph Lister. He was a voluminous writer on surgical subjects, many of his works being held in very high reputation.

THE *Field* suggests that the drought of the summers of 1868 and 1870 is connected with the rapid increase of drainage in this country, the average summer rainfall having been greatly reduced from 1860 to 1870. Our contemporary also expresses an opinion, in which we cordially concur, that a needless alarm has been raised as to the prospects of the corn harvest, as shown in the rise of 10*s*. to 12*s*. a quarter in wheat at Mark Lane. The harvest of 1868 is the finest on record; and we hope to see the prediction of our contemporary fulfilled, that "the cereal crops in this country will, on the whole, turn out favourably."

NOW that so many, both Londoners and their country friends, are flocking to the national botanic garden at Kew, we may call attention to Prof. Oliver's "Guide to the Royal Botanic Gardens and Pleasure Grounds, Kew," which has now reached its twenty-fifth edition. It is a *multum in parvo* of value and interest far beyond the purpose for which it is designed; indeed we do not know where else, in so small a compass and at so low a price, to meet with so much and varied information respecting the vegetable products of different countries, their economic purposes, and their geographical distribution, illustrated with exceedingly well-drawn woodcuts.

IN a paper in the *Bulletins de la Société Vaudoise*, No. 62, Dr. C. Nicati gives a *résumé* of various researches respecting the peculiar red snow which occasionally falls in the Grisons. Some of this snow fell, mingled with common snow and rain, during a violent storm from the south-west on the morning of January 15th, 1867, in various places. The chemical analysis of the melted snow demonstrated the presence of minute quantities of sulphate of lime or gypsum, sulphate of magnesia, organic matters, chlorine, and iron; and microscopic examination detected vegetable fibre, pollen, spores, with here and there diatoms and small crystals. The colour varies from brick red to a pale yellow. This snow is quite distinct from the red snow of the upper Alpine regions, which owes its colour to the presence of the minute plant, *Protococcus nivalis*. After discussing various theories respecting its origin, Dr. Killias expressed his opinion that it is the dust of the desert of Sahara, transported by a sirocco, which gives the colour to the snow of the Grisons. Dr. Nicati gives many interesting particulars, with analyses, of the Algerian sirocco dust, and of the mud-rain in Naples and Sicily; and Professor C. Cramer states that he has discovered, both in the sand of the Sahara and in the red snow of the Grisons, particles of vegetable organisms (especially polythalamia) and minute fragments of animal origin, such as wool, hair, &c. He considers the presence of gypsum in the red snow an incontestable proof of its containing matter conveyed from the desert of Sahara

PROFESSOR GERARDIN has recently communicated to the Council of the "*Société d'encouragement pour l'industrie nationale*" the results of his efforts to purify the waters of the Croult, a small river which, rising at Louvres, passes through Gonesse, Arnouville, and flows through St. Denis, ultimately falling into the Seine. The stream was poisoned by the drainage from the starch manufactories. The principle on which M. Gerardin proceeded was that water in which weeds and shell-fish cannot live was infected and poisoned. The potatoes from which the starch was made contain 75 per cent. of juice, which itself contains 7 per cent of albumen. The water issuing from the factory is clear, reddish, and inodorous; in motion, it forms coherent masses of coagulated albumen. The river deposited in its course masses of whitish, pitch-like substance, without consistence. The surface was covered with white froth; the mud black and stinking, while the water had a strong odour of sulphuretted hydrogen. M. Gerardin recognised the white masses deposited by the waters as the "baregine" characteristic of the sulphurous waters of the Pyrenees. When the works stop, this "baregine" putrefies, and infusoria are developed in abundance. M. Gerardin thought the best method of remedying the unwholesome state of the water would be to destroy the albumen by the simultaneous action of the air, clay, and the organic fermenting agents always contained in cultivated ground, and he determined to make the waters pass over a soil well drained. The factory of M. Boisseau, at Gonesse, consumed in a day 400 hectolitres (sacks) of potatoes, weighing 28,000 kilog. (27½ tons), and containing 21,000 kilog. of juice, carried off by 130,000 litres of water (say 28,500 gallons); these waters are spread over a field whose area is 500 yards square, in which are placed drains at 6 feet distance from each other, and 2 feet deep. The arrangement has perfectly succeeded. Weeds grow in the Croult from Louvres to St. Denis: *Limnæus* and *Planorbis* find their abode in these weeds, the "baregine" has disappeared, the sulphuretted hydrogen odour is entirely gone, and the water is sweet and limpid.

A GOOD tabular arrangement of the Natural Orders of plants is very much wanted by botanical teachers, where the alient diagnostic characters of allied orders are presented to the eye at a glance. That this desideratum is entirely supplied by Dr. Griffith's "System of botanical analysis applied to the diagnosis of British natural orders," we cannot altogether affirm, as each teacher will probably find it vary in some respect or other from his own ideas of the best mode of classification. We can, however, safely recommend it as a useful help to beginners in getting over the difficulties of systematic botany.

FACTS AND REASONINGS CONCERNING THE HETEROGENOUS EVOLUTION OF LIVING THINGS*

IN all ages it has been believed by many that Living things of various kinds could come into being *de novo*, and without ordinary parentage. Much difference of opinion has, however, always prevailed as to the kinds of organisms which might so arise. And, although received as an article of faith by many biologists—perhaps by most—in the earlier ages, this doctrine or belief has, in more recent times, been rejected by a very large section of them. Definitely to prove or disprove the doctrine in some of its aspects is a matter of the utmost difficulty, and there are reasons enough to account for the wave of scepticism on this subject, which has been so powerful in its influence during the last century. The notions of the ancients were altogether crude, and founded upon insufficient proofs. It was not in their power to settle such a question; and when the inadequacy of the evidence on which they had relied became known, then much doubt was thrown also on the truth of the conclusion at which they had arrived. All this was natural enough. When, therefore, about a century ago, the rude microscopes of the time began to reveal a multitude of minute organisms whose existence had been hitherto unsuspected; when more facts became known concerning the various modes of reproduction amongst living things; and, above all, when the philosophical creeds of the day were supposed to be irreconcilable with such a doctrine, then a growing scepticism in the minds of many gradually developed into an utter disbelief in the possibility of the occurrence of what was called "spontaneous generation."

This was the state of things anterior to and during the time of the celebrated controversy between the Abbé Spallanzani and John Needham. Then it was that the former of these two champions, with the view of accounting for phenomena which would otherwise have necessitated his admission of the doctrine which he rejected, recklessly launched upon the world the *hypothesis* that multitudinous, minute, and almost metaphysical "germs" existed everywhere—ready to burst into active Life and development whenever they came under the influence of suitable conditions. Armed with this all-powerful *Panspermic* hypothesis, Spallanzani argued against the conclusions of Needham. His views on this subject were supported by the still more extravagant theories of Bonnet. The doctrine of "*L'Emboîtement des germes*" was the production of an unbridled fancy, and might, perhaps, never have been elaborated, had not the Leibnitzian doctrine concerning "Monads," as centres of force and activity, been already in existence, and at the time all-powerful in the philosophical world.

The controversy which was initiated by these two pioneers in microscopical research they were unable to terminate—the enigma which they sought to solve has, since their time, still pressed for solution, and still the tendency has been to solve it after one or other of the modes by which they attempted to account for the occurrence of the phenomena in question. It is and has been contended, on the one hand, that Living things can originate *de novo*, and without ordinary parentage; it is contended, on the other, that this is impossible—that every Living thing is the product or off-cast of a pre-existing Living thing, and that those which appear to arise *de novo* have, in reality, been produced by the development of some of the myriads of visible or invisible "germs" which pervade the atmosphere.

Now it is obvious, that of these two opposing doctrines, the one must be true and the other false: either Living beings can originate *de novo*, or they cannot. So long as any doubt remains upon this subject, we have to confess our ignorance concerning one of the very first principles of Biology. In the whole domain of Science, moreover, it is scarcely possible to propose a question which is more replete with interest than that which asks whether Living things can be evolved *de novo*. If settled in the affirmative, what light will be thrown upon the past and present history of our globe! How must our notions concerning Life, health, and disease be influenced in one way or the other by its solution!

Without entering into the history of the long controversy which has taken place upon this subject—details of which may be found in the works of Pouchet* and Pennetier,† and in the writings of Pasteur‡—I shall, before describing my own experiments and their results, merely relate as briefly as possible what conclusions have been come to concerning the degree of heat to which inferior organisms may be subjected with impunity, and what temperature, on the other hand, has been invariably found to be fatal to them. Fortunately there is at present much unanimity of opinion on this subject. As a result of numerous investigations which have been communicated to the French Academy, and to the Société de Biologie during the last ten years, we find that both the advocates and the opponents of heterogeny are, within certain limits, pretty well agreed on this most important aspect of the question. The many disbelievers and opponents of heterogeny who took part in these investigations, naturally desired that the power possessed by inferior organisms, both animal and vegetable, of withstanding the destructive influence of high temperatures, should be shown to be as high as possible. We may, therefore, with much safety, assume that the limits of vital resistance could not then be shown to be higher than that which these experimenters were compelled, after frequently repeated investigations, to ascribe to such inferior organisms.

In *dry air* or in a vacuum, organisms are capable of withstanding a notably higher temperature than when they are immersed in fluid. According to the direct observations of M. Pasteur, the spores of certain fungi belonging to the family *Mucedineæ*, seem to possess this tenacity of life to a very great extent; but even these, he says, though they still remain capable of germinating after having been raised, for a few minutes in dry air or *in vacuo*, to a temperature of from 120° to 125° C., lose this power absolutely and entirely after an exposure for half an hour, under similar conditions, to a temperature varying from 127° to 130°C. And the labours of the commission (consisting of the following members—MM. Balbiani, Berthelot, Broca, Brown-Séquard, Dareste, Guillemin, and Ch. Robin) appointed in 1869 by the Société de Biologie, to inquire into the subject, led them to the conclusion that the lower animals which were the most tenacious of life§—the rotifers, the "sloths," and the anguillules of tufts of moss or lichen—succumbed at even a much lower temperature than this. In dry air or *in vacuo*, therefore, we may look upon the temperature of 130°C. for thirty minutes, as marking the extreme limit of vital endurance under such conditions, so far at it has been hitherto possible to fix such a limit. There is, at present, no evidence forthcoming to upset this conclusion. When immersed in *fluids*, however, the power possessed by the inferior organisms of resisting the destructive influence of heat is not nearly so great. Comparatively few, whether animal or vegetable, have been found capable of resisting a temperature of 75°C.; and with regard to that of 100°C., it has been admitted, by MM. Claude Bernard and Milne-Edwards, by M. Pasteur, and by all the other most influential opponents of the doctrines of heterogeny, that such a temperature, even for one minute, has always proved destructive to all the lower organisms met with in infusions‖—so far as these had been made the subjects of special and direct experimentation. And, amongst all the diversity of form presented by the lowest Living things, there is so much of uniformity in property—living matter, as we know it, agrees in so many of its fundamental characters—that biologists and chemists alike may feel a reasonable assurance as to the probable universality of any such rule which has been proved to hold good for a very large number of organisms, more especially when, amongst this large number of cases, no exceptions have been encountered.

Practically, however, it will be found that, in order to appreciate the bearings of the experiments which I shall have to relate, it will be necessary for us more especially to know what are the limits of vital resistance to high temperatures, possessed by *spores* of Fungi on the one hand, and by *bacteria* and *vibrios* on the other.

I am not aware of any experiments tending to show that *spores* of Fungi can survive after exposure for even a few seconds in fluids raised to a boiling temperature (100° C.); whilst, on the

* This paper was originally intended for presentation to the Royal Society, but it was finally not presented, when I understood that, owing to the accumulation of many papers and other causes, no evening could be allotted on which it might be read and discussed. Its appearance *in extenso*, and at once, was thought preferable to the reading of its mere title before the Royal Society, with the probability of a very considerable delay in its publication. —H. C. B.

* Hétérogenie, Paris, 1859. † L'Origine de la Vie, Paris, 1868.

‡ Annales de Chimie et de Physique, 1862.

§ This extreme tenacity of life is perhaps due in part to the chitinous integument with which such animals are provided.

‖ It is quite fair to make this limitation, since we are only concerned with the origin of such organisms. Seeds of higher plants, provided with a hard coat, may—especially after prolonged periods of desiccation—germinate even after they have been boiled for a long time in water.

other hand, there is the concurrent testimony of many observers to the fact that, after such exposure, germination would never take place, because the spores were no longer living. This was the result obtained in many experiments made by Builiard, and related in his "Histoire des Champignons." Mere contact with boiling water was found sufficient to prevent germination; and H. Hoffmann* similarly ascertained that an exposure for from four to ten seconds to the influence of boiling water sufficed to prevent the germination of all the fungoid spores with which he experimented. The experience of other observers has been similar to that above quoted, and amongst these we may cite M. Pasteur himself. Speaking of his experiments with boiled milk in Schwann's apparatus, M Pasteur says:—"Je n'ai jamais vu se former, dans le lait ainsi traité autre chose que des Vibrions et des Bacteriums, *aucune Mucédinée, aucune Torulacée, aucun ferment végétal.* Il n'y a pas de doute que cela tient à ce que les germes de ces dernières productions ne peuvent résister à 100° au sein de l'eau, ce que j'ai d'ailleurs constaté par des expériences directes."†

The evidence which we at present possess concerning the tenacity of life displayed by *bacteria* and *vibrios* in fluids whose temperature has been raised, is just as decisive as that concerning the spores of fungi. M. Pouchet's observations have lead him to believe that vibrios, in common with all the kinds of ciliated Infusoria, are killed by raising the temperature of the fluid which contains them to 55° C. M. Victor Meunier, also, never found any of these organisms alive after they had been similarly subjected to a temperature of 60° C. I have myself invariably found that vibrios were not only killed, but were broken up and more or less disintegrated, after the fluid had been boiled for even one minute. There is every reason also to believe that an exposure to similar conditions kills their less developed representatives—the primordial monads and bacteria. With reference to these organisms, however, one caution is necessary to be borne in mind by the experimenter. The movements of monads and bacteria may be and frequently are of two kinds. The one variety does not differ in the least from the mere molecular or Brownian movement, which may be witnessed in similarly minute non-living particles immersed in fluids; whilst the other seems to be purely vital—dependent, that is, upon their properties as living things. These vital movements are altogether different from the mere dancing oscillations which non-living particles display, as may be seen when the monad or bacterium darts about over comparatively large areas, so as frequently to disappear from the field of the microscope. After an infusion has been exposed for a second or two to the boiling temperature, these vital movements no longer occur, though almost all the monads and bacteria may be seen to display the Brownian movement in a well-marked degree. They seem to be reduced by the shortest exposure to a temperature of 100°C., to the condition of mere non-living particles, and then they become subjected to the unimpaired influence of the physical conditions which occasion these molecular movements.

Such is the evidence existing as to the power of resisting the destructive influence of heat, manifested by the organisms about which we are at present most interested. It is certainly harmonious enough with our ordinary experience, and is, therefore, not difficult for us to believe. Eggs of higher animals containing an embryo may fairly enough be compared with the lower organisms of which we have been speaking, so far as the matter of which they are composed is concerned; and knowing the profoundly modifying influence of water at a temperature of 100° C. upon the comparatively undifferentiated matter of the embryo and of the egg—and also, we may add, even upon the differentiated tissues of the parent fowl—need we wonder much that the same temperature should have been found hitherto to be destructive to the simple and naked living matter entering into the composition of fungus-spores, and of bacteria and vibrios? If any other result had been ascertained, would there not have been much more reason for surprise?

We must therefore be very cautious how we attempt to set aside the conclusions which have been arrived at on this subject, based as they have been upon direct evidence of a most positive character, on account of other evidence which is indirect and more or less ambiguous. Concerning the legitimacy of such an attempt which has been made by M. Pasteur, I shall have more to say hereafter.

Passing on, then, to the more immediate consideration of our

* Etudes mycologiques sur les fermentations.
† Annales de Chimie et de Physique, 1862, p. 60.

subject, it should be distinctly understood that in all the discussions which have hitherto taken place on the possibility of the evolution of Living things, pre-existing *organic matter* has always been supposed to furnish the materials entering into the composition of the new organisms. New combinations and re-arrangements have been supposed to take place amongst the molecules of this pre-existing organic matter, under the agency of some mysterious force or forces—which new combinations of previously uncombined or differently combined molecules have been supposed to result in the production of such primordial living specks as monads and bacteria. The observations of preceding inquirers have also been conducted for the most part on infusions containing organic matter *in solution;* and since the molecules of such matter are then invisible, observers have, of course, been quite unable to follow, by any magnifying power at present attainable, variations in the modes of collocation of such invisible molecules. The minutest specks of living matter—the germs of monads and bacteria, and of the spores of fungi, less than $\frac{1}{50000}''$ in diameter—may be seen gradually appearing under the microscope in previously homogeneous solutions containing none of them.* But although microscopical investigation enables us to adduce evidence of just the same kind in elucidation of the mode of origin of certain low organisms, as we possess in explanation of the mode of origin of crystals,† this evidence is not deemed adequate in the case of organisms. A living thing has been supposed to be a something altogether different, incapable of arising out of a mere collocation of matter and of motion; and, therefore, under the influence of this theoretical assumption, whilst chemists and physicists have thought that they could in a measure account for the genesis of crystals by reference to the affinities and atomic polarities of the ultimate constituents of such crystals, they have, for the most part, declined to adopt a similar mode of reasoning in order to account for the appearance of the minutest living specks in solutions containing organic matter. The same reservation is likewise made by the major part of the biologists of the present day. Whilst it is not an article of faith—whilst such a surmise scarcely crosses our minds—that crystals always proceed from pre-existing germs, in the case of Living things, on the contrary, the doctrine *omne vivum ex vivo* has become almost one of the "forms of thought." Principally owing, therefore, to certain theoretical views concerning Life, and in order to account for facts which would otherwise be adverse to these, biologists and others have been accustomed to make the most extensive postulations concerning the supposed universal distribution of "germs" of all the lower kinds of living things; whilst they have recourse to no parallel hypothesis to account for the appearance of crystals, although we know no more—can drive our knowledge back no further into the phenomena attendant upon the birth of crystals than we can into the phenomena which usher in the appearance of organisms. In each case, under suitable conditions, they appear at first as minutest visible specks, in solutions which were previously homogeneous. In the one

* A more complete account of this part of the subject will be given in a work on *The Beginnings of Life*, shortly to be published.

† Or, better still, concerning the mode of origin of those modified crystals which appear on mixing solutions of gum and carbonate of potash, as described by Mr. Rainey (*On the Mode of Formation of the Shells of Animals, &c.*, 1858). The malate of lime contained in the gum is decomposed, but owing to the slow mixing of the solutions in the presence of gum, the insoluble carbonate of lime does not appear in its usual crystalline condition, but in globular modifications, resembling calculi. When portions of the two solutions are mixed under the microscope, Mr. Rainey thus describes what takes place:—"The appearance which is first visible is a faint nebulosity at the line of union of the two solutions, showing that the particles of carbonate of lime, when they first come into existence, are too minute to admit of being distinguished individually by the highest powers of the microscope. In a few hours exquisitely minute spherules, too small to allow of accurate measurement, can be seen in the nebulous part, a portion of which has disappeared, and is replaced by these spherical particles. Examined at a later period, dumb-bell-like bodies will have made their appearance, and with them elliptical particles of different degrees of excentricity" (p. 9). Mr. Rainey made use of one of Ross's ⅛" object glasses. These modified crystals are produced with no more rapidity than the lowest living things seem to be in other solutions, during hot weather; and the shapes of the products in the two cases are remarkably similar, judging from Mr. Rainey's figures. The protraction of the process, brought about by the presence of gum, serves to bring out more clearly the real relationship existing between the formation of crystals and that of the lowest organisms, in homogeneous solutions.

That there are very strong reasons accounting for this belief I do not attempt to deny. There is, however, much evidence to show that the very same organisms which do propagate their kind after this acknowledged method, may themselves originate *de novo.* Whilst allowing, therefore, the widest generality to any given rule, we may well hesitate before, on this account, we reject certain other alleged facts which are complementary rather than contradictory.

case we have to do with *crystallisable* matter, in solution, and in the other with those big-atomed, unstable compounds which constitute the so-called *colloidal* states of matter. And it is well to call attention to the fact that, concerning these latter states, the late Professor Graham, one of the most cautious and philosophical of chemists, wrote:*—"Another and eminently characteristic quality of Colloids is their mutability. Their existence is a continual metastasis. . . . . . The Colloidal is, in fact, a dynamical state of matter, the crystalloid being the statical condition. The colloid possesses ENERGIA. *It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality.* To the gradual manner in which colloidal changes take place (for they always demand time as an element) may the characteristic protraction of chemico-organic changes also be referred."

Granting, then, that microscopical evidence alone may not be quite satisfactory for settling the mode of origin of such primordial living things as monads and bacteria, it becomes obvious that we must endeavour to throw light upon this evidence by other methods of investigation. Still it should also be kept steadily in mind that microscopical evidence is equally powerless to throw light upon those primordial collocations which initiate the formation of crystals. The problem concerning the primordial formation of crystals and living things is essentially similar in kind. Any difference in degree between our present knowledge on these two subjects must not blind us as to their essential similarity. Monads and bacteria are produced as constantly in solutions of colloidal matter as crystals are produced in solutions containing crystallisable matter. Crystallisable substances are definite in composition, and give rise to definite statical aggregations; whilst colloidal substances, much more complex and unstable, give rise on the contrary to dynamical aggregations. These dynamical aggregations, though they at first make their appearance in the form of monads and bacteria, are, by virtue of the properties of their constituent molecules, endowed with the potentiality of undergoing the most various changes, in accordance with the different sets of influences to which they are submitted. They are dynamical aggregates, in fact, in a condition of unstable equilibrium, and are capable of being diverted into new modes of current and reciprocal molecular activity in response to changes in their medium or environment. These differences between the products met with in solutions containing crystallisable and colloidal matter respectively, may, however, be due simply to the original difference in nature between such kinds of matter. Respecting the origin of the first visible forms which appear in either kind of solution, the evidence which we possess is precisely similar in nature. If such microscopical evidence does not enable us to get rid of the doubt that the smallest visible specks of living matter may have originated from *invisible* "germs" of such organisms, neither does it any more enable us to dispense with the supposition that the smallest visible crystals may have originated from pre-existing *invisible* "germs" of crystals. There is, in fact, so far as actual scientific evidence goes, almost as good reason for a belief in the universal distribution of invisible "germs" of crystals, as there is for our belief in the universal distribution of invisible "germs" of monads and bacteria. The very existence of the one set of invisible "germs" is, in fact, just as hypothetical as the existence of the other. Monads and bacteria we do know; but concerning the existence of invisible "germs" of monads and bacteria we know just as little as we do concerning the existence of invisible "germs" of crystals.

And yet almost all the difficulties in finally settling the question of the truth or falsity of the doctrines of "spontaneous generation" are centered in this question as to the mode of origin of monads, bacteria, and such fungus-spores as similarly originate in homogeneous solutions in the form of the most minute specks of living matter.† Given the existence of such primordial living particles, and we can easily watch changes taking place in aggregations of them, which lead to the production of much larger and altogether different organisms. We can then trace out with the microscope various kinds of evolution—processes of so-called "spontaneous generation," in fact—the establishment of the reality of which is just as much in opposition to generally received biological notions, as is the supposition that the primordial units themselves are able to come into being *de novo* after particular modes of collocation of colloidal molecules hitherto invisible. The amount of difference between such invisible organic molecule and the speck-like organism less than $\frac{1}{50000}$" in diameter, which appears in the previously homogeneous solution, may be no more real or striking than is the difference between some of the visible monads or bacteria and the much larger and higher kinds of living things, whose mode of origin I am about to describe, and which may be seen to arise after particular sets of changes have taken place in aggregations of such monads and bacteria. Of two things previously deemed alike improbable, the one which can come within the range of our vision may be shown to take place—the other being, unfortunately, beyond our ken, admits of no such proof. The unmistakeable upsetting of our preconceptions on the one subject should, however, make us cautious how, on theoretical grounds, we pronounce that to be impossible in the case of organisms which we, nevertheless, believe to be possible and actual in the case of crystals: especially when, in these two sets of cases, the amount of actual evidence which we possess is almost equal and similar.

Waiving, then, for the present the consideration of additional evidence as to the mode of origin of the primordial living particles, the monads and bacteria, and of the apparently similarly originating fungus-spores, I will describe some of the evolutional changes by which higher organisms may be seen to arise in a pellicle formed by an aggregation of the simpler kinds of living particles.

I.

The Mode of Origin of Unicellular Organisms and of Spores of Fungi in the "proligerous pellicle" of organic solutions.

What Burdach named the *proligerous pellicle* of organic solutions is made up of an aggregation of monads and bacteria in a transparent jelly-like stratum, on the surface of the fluid. It constitutes at first a thin scum-like layer, and although the monads and bacteria entering into its composition are motionless, M. Pouchet and others were not warranted in assuming from this fact alone that they were dead. There is, indeed, good reason for believing to the contrary, since, as pointed out by Cohn, when any of these particles are set free from the broken edge of a pellicle they always resume their movements. Motion, therefore, may simply be prevented by the presence of the transparent jelly-like material in which they are imbedded, although the particles may be undoubtedly living.

My observations on this subject have been principally carried on throughout the winter months; and this is a time not favourable for the appearance of ciliated Infusoria in organic infusions. Hence it is, perhaps, that I have not been able to witness any of those changes in the pellicle which have been described by Pouchet, as resulting in the evolution of *Paramœcia*, *Kolpodæ*, and other ciliated Infusoria. The changes which I have observed, however, have been so indubitable in nature, have been seen so frequently, and have had such a close general resemblance to those which have been described as leading to the evolution of *Paramœcia*, that I am quite disposed to believe in the correctness of the observations that have been made by Pouchet and others on this part of the subject.

My own observations have been conducted principally on the pellicle of hay infusions, and one of the commonest processes of what may be termed *secondary organisation* takes place in the following manner. In a pellicle which previously presented a uniform appearance, certain areas, altogether irregular in size and shape—though always presenting outlines bounded by curved lines—gradually make their appearance. These are, at first, distinguishable from the general ground-work of the pellicle only by their somewhat lighter aspect. On careful microscopical examination with high powers, it may be seen that the boundary of such an area—measuring it may be as much, or more than $\frac{1}{300}$" in diameter—is pretty sharply defined from the surrounding unaltered granular stratum. The immediately contiguous granules of this are occasionally somewhat more tightly packed, though at other times no such change is observable. In either case the unaltered portion of the pellicle is quite different from the included lighter area, because in this an increase has, apparently, taken place in the amount of jelly-like material between the granules, and, as well, there is a certain alteration in the refractive index, and occasionally in the size of the granules (monads and bacteria) themselves. The next change observable is, that the included area shows lines crossing it here and there, which at first tend to map it out into certain larger divisions. These intersecting lines gradually increase in number, till at last

* Philosophical Transactions, 1862. Capitals and italics are employed as we give them in the memoir itself.

† Such, for instance, is one of the modes of origin of *Torula* cells.

the mass becomes divided into an aggregation of rounded or ovoid bodies each about $\frac{1}{5000}''$ in diameter. As these subdivisions are taking place, the mass as a whole separates from the unaltered pellicle by which it is surrounded. Occasionally there is

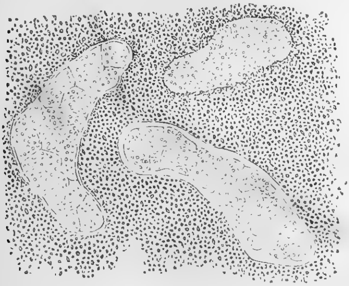


FIG. 1.—Development of Unicellular Organisms: three areas of differentiation showing different stages.

the most distinct interval, at a certain stage, between the parent pellicle and this differentiating mass, whose subdivisions also gradually separate from one another. These subdivisions now appear as independent unicellular organisms, bounded by a delicate membrane, and containing, perhaps, from four to eight of the altered monads and bacteria in their interior.

Throughout the winter months, such areas of differentiation and such resulting unicellular organisms were frequently met with. The unicellular organisms seem during such weather to persist for a very long time in this condition, merely, perhaps, increasing somewhat in size, and most of them ultimately become disintegrated without undergoing further development. They were always seen in a completely motionless condition, and presented no trace of a cilium, so that they were altogether different from the creature known as *Monas lens*. In one solution of hay in which such organisms had been present for some time, after a few days of warmer weather, several of them were found to have become spherical, and to have undergone a considerable increase in size. Some of these were as much as $\frac{1}{2000}''$ in diameter, and on one occasion a stage in the actual transition of one of these unicellular organisms into an *Amœba* was seen with the most perfect distinctness. One

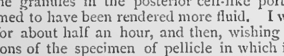


FIG. 2.—Reţresenting gradual enlargement of Unicellular Organisms, and conversion of one of them into an *Amœba*.

half of the organism was distinctly amœboid in character, whilst t'ie other half was almost unchanged, containing large granules like those in the unaltered cells. As slow alteration in shape, of a slug-like character, took place in the anterior diaphanous protoplasmic portion, slow rolling movements occurred amongst the granules in the posterior cell-like portion, whose matrix seemed to have been rendered more fluid. I watched this organism for about half an hour, and then, wishing to examine other portions of the specimen of pellicle in which it had been contained, I moved the glass and was afterwards unable to find this particular specimen again. Unfortunately, I could discover no other Amœbæ or transition states.*

In other cases the areas of differentiation, commencing in a somewhat similar way, terminate in the production of spores of fungi, and I will now describe the mode of evolution of such s,ores as I observed it taking place in portions of a pellicle having a brownish colour, from an old infusion of hay. The development of this brownish tinge in the earlier stages made it more easy to unravel the nature of the earlier changes. The areas which began to differentiate were generally not very large. They were at first quite colourless, and the granules were separated from one another by a notable amount of transparent jelly-like material. The granules themselves were mostly shaped like the figure 8, and each half was about $\frac{1}{30000}''$ in diameter. A later stage was seen, apparently, in other areas which had assumed a very faint brownish tinge, and which presented evidences that

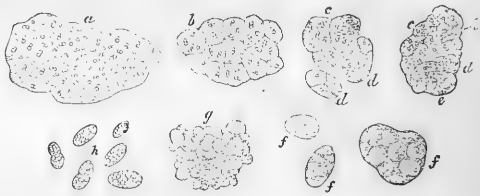


FIG 3.—Mo:le of origin of Spores of Fungi out of differentiating portions of a Pellicle formed on an infusion of hay.

subdivision was taking place. As the process of subdivision progressed, so the brown tinge became gradually deeper. Ovoid masses were frequently seen about $\frac{1}{2000}''$ or $\frac{1}{1000}''$ in diameter, of a decidedly brown colour, with from 8 to 12 or more ovoid subdivisions within the common envelope. As multiplication advanced, the individual products lost all trace of their original granular condition. They became quite homogeneous and highly refractive masses of a brown colour, looking almost like large brown fat globules. At last, multiplication still proceeding, the distended, and always thin, cyst-like, general envelope becomes ruptured and disappears, leaving only an irregular mass of spherical or ovoidal bodies of various sizes. The individual segments, as soon as this process of multiplication has ceased, increase in size, and then gradually become less refractive and lighter in colour. A slight differentiation of their contents also again takes place, marked by the appearance of faint lines within, as they assume the appearance of ovoid bodies about $\frac{1}{4000}''$ in diameter.* Even when they have attained this stage of development, they may again undergo a process of division; though generally, after a time, they give origin to ordinary mycelial filaments.

Similar changes in the refractive index have been frequently noticed in other cases when a protoplasmic mass is at the same time differentiating and undergoing a process of multiplication,† whilst the mode and frequency of the sub-division is exactly comparable with what so frequently happens to the gonidia of lichens.

The changes which I have described represent, I think, only two extreme types of a mode of metamorphosis which is apt to take place in portions of the pellicle. In the one case a certain area of the pellicle, after undergoing some changes, resolves itself into a number of ovoid bodies, which collectively are about equal in bulk to the altered area itself; whilst, in the other case, at different stages, the segments of the altered area undergo a process of growth and sub-division, so that ultimately the mass of spores which results far exceeds in bulk that of the original area when it began to undergo change.

At other times, intermediate processes are met with, and then fungus-spores are produced after a fashion more closely resembling that which leads to the production of the unicellular organisms above described. The areas of change are then larger than those last described, and colourless throughout, whilst the processes of growth and multiplication are less marked at the different stages. Where fungus-spores result after this fashion, the changes in the refractive index, and the homogeneous appearance previously alluded to, still generally manifest themselves at the ultimate stage of division, though nothing of this kind shows itself in the more simple process leading to the production of the unicellular organisms.

Now, however mysterious the nature of these changes may be, which take place, as it seems to us, "spontaneously" in the pellicle on the surface of a solution of organic matter, they are exactly comparable with other changes occurring within the terminal dissepiments of a kind of submerged mucor, named *Achlya prolifera*, similar to those which occur within the thecæ of certain other fungi and of certain lichens, and altogether analogous to that which, as Prof. Haeckel says, takes place in

* Prof. Hartig has, however, described a similar mode of origin of Amœbæ from unicellular organisms, in his observations on the phytozoa of *Marchantiæ*. See *Journal of the Microscopical Society*, 1855, p. 51.

* The markings of these spores are more obscure and less regular than they are represented (*h*) in the woodcut.

† See Nicolet, in Thompson's *Arcana Naturæ*, 1859.

some of the simplest *Amœbæ*, after they have encysted themselves. In all these cases, formless and apparently homogeneous or merely granular living matter, resolves itself more or less rapidly into a number of individualised segments, which are

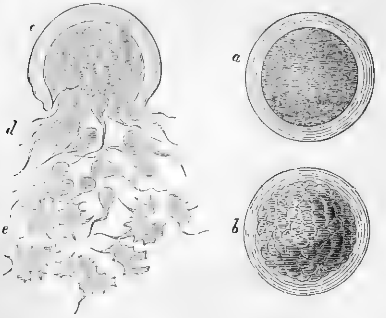


FIG. 4.—Representing subdivision of formless living matter within encysted *Protomyxa*, and exit of products from cyst as active tailed Zoospores, ch subsequently become converted into reptant Amœbæ (Haeckel).

capable of existing as independent living things.* These changes occur in the formless matter of definite organisms, and the products of subdivision tend to reproduce organisms of a similar kind; but the changes which take place in portions of the pellicle are changes occurring in fortuitously aggregated living matter, and the resulting products are, as might have been expected, more variable in kind. There is every reason to believe that the changes which take place in the homogeneous living matter of the encysted *Protomyxa* occur by reason of the molecular properties of this living matter, and are not occasioned by any occult influence exercised by the mere inert cyst-wall, which is but a product of the living matter that it

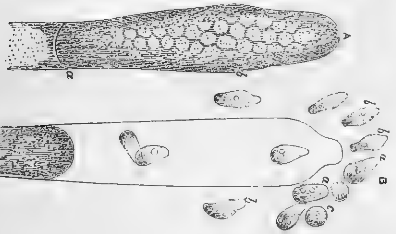


FIG. 5.—Showing the mode of origin of motile Zoospores within the terminal dissepiment of *Achlya* (Tulasne).

encloses. And so we have good reason for supposing that the changes which take place in the mere granular mucilage of the rapidly-formed terminal segment of an *Achlya*, by which this in the space of less than two hours resolves itself into free-swimming zoospores, is to be ascribed to the molecular properties of the mucilage itself which undergoes the change. In the pellicle, on the other hand, we have an aggregation of granular living matter also, and the observations which I have adduced simply go to show that those molecular properties of living matter which lead to its differentiation and further organisation, are not limited to the living matter that is contained within organisms of a definite type. Just as the changes which take place in the structureless living matter of these organisms seem to be due to the forces acting upon, and to the reactions amongst, the several molecules of which it is composed, so do the changes which occur in given areas of the pellicle seem referrible to the influence of physical forces upon the living molecules of which it is composed, and upon the mutual inter-action of these upon one another when under the influence of such incidence. In both cases the changes take place in living matter; in both cases they are the results of molecular activity: in the one set of cases they take place in a fortuitous aggregation of living matter, and the products are accordingly very variable in nature, whilst, in the other set of cases, just as they take place in living matter which constitutes part of a definite organism, so are the products more definite in kind.

But this process, which most certainly occurs in the living matter of a pellicle, is of a kind not hitherto generally recognised as one of the modes by which unicellular organisms, or spores of fungi, may originate. These are, in fact, instances of what has been called "spontaneous generation," or what we may better term heterogenous evolution. The majority of biologists would be as much inclined to believe that these processes did not take place as they are inclined to disbelieve that a monad or a bacterium may be born *de novo* in a solution of organic matter. The occurrence of the one process has been thought to be about as improbable as that of the other. Yet the one can be seen undoubtedly taking place with the aid of the microscope alone. Unfortunately, however, the organic molecules, which are supposed to coalesce in the solution of organic matter, in order to form the smallest visible Living particle, are themselves invisible. We cannot, therefore, trace the genesis of one of these particles with the aid of the microscope, any more than we are able to trace the genesis of a crystal beyond its *minimum visible* stage. In the one case physicists and biologists willingly assume that such ultimately visible particles are the products of a "spontaneous" coalescence of molecules, which are themselves invisible, whilst in the case of organisms they will grant no such assumption. They require us to prove, in fact, that such organisms have not been produced from pre-existing though perhaps invisible "germs," before they will grant for organisms that probability which they at once concede in the case of crystals. This difference which is made between the two cases seems due, in great part, to some theoretical views which are held concerning the nature of Life. And yet it would not be difficult to show that the metaphysical or vitalistic theories in question, to which they commit themselves, are directly opposed to some of the most accredited scientific doctrines of the day.* The doctrine of the Conservation of Energy and of the Correlation existing between the Vital and the Physical forces do, if pushed to their ultimate issues, inevitably bring us to the conclusion that the forces acting within all Living bodies are molecular forces, and that such forces are derived from the physical forces of the outside world just as surely as the matter of the organism formerly existed outside itself. The most careful interpretation of scientific evidence, moreover, would lead us to the conclusion that what is called the Life—or in other words the aggregate set of phenomena displayed by one of the simplest bodies which we call a living thing—is as much the essential and inseparable attribute of the particular molecular collocation which displays it, as the properties of the crystal are essential to the kinds and modes of aggregation of the molecules which enter into its composition. It may be maintained, therefore, that all *à priori* presumptions, based upon the best scientific evidence, would lead us to disbelieve the "vitalistic" theories which are still held by many at the present day. It is the vitalist, however, who alone has any logical reason for insisting that what may be a good and valid mode of accounting for the origin of crystals cannot be considered to hold good in the case of organisms. Those who believe that the forces acting in Living things manifest themselves in the individual molecules of which these are composed, and that such forces are convertible with the ordinary physical forces, have, on the other hand, strong *à priori* reasons for believing that Life will manifest itself wherever particular collocations of complex organic molecules occur. It rests, then, in reality, with the vitalist, who assumes the truth of a mere theory, in favour of which he can adduce no scientific evidence, to show why a different rule should be presumed to

* All this part of the subject will be much more fully treated in the work on *The Beginnings of Life*. The possession or not of the property of motility seems to be an altogether unessential characteristic. The products of subdivision of such an encysted *Amœba* as *Protomyxa aurantiaca* are motile zoospores, and so are those of the fungoid *Achlya prolifera*; but the reproductive products arising from the subdivision of the formless matter within the spore-cases of *Pezizæ* and other fungi are motionless, and this is the case also with the gonidia of the *Saprolegniæ*, which are fungoid organisms, otherwise almost undistinguishable from *Achlya*.

* This I shall attempt to show fully elsewhere.

hold good in the birth of crystals and of organisms respectively.* Those who hold opposite opinions need only suppose that molecules of "organic" matter, or some such complex molecules, may aggregate and arrange themselves after certain modes to produce a Living thing—just as a crystal, endowed with its particular properties, is producible by other modes of aggregation —because with them Life is considered to be a product of molecular collocation and of molecular change. And if the "vitalist" wishes to establish the existence of a more fundamental difference between crystals and organisms than we are prepared to grant, seeing that the scientific evidence seems to be against him, it remains for him at least to endeavour to show good grounds for the establishment of such difference.

It should be remembered, then, that in the present state of science all theoretical considerations seem favourable to the views of the evolutionists, and that the only thing which can be opposed to them is the *assumption* that those processes of reproduction which take place amongst all known varieties of living things are the *only* processes by which such living things can arise. But now, already, by means of microscopical evidence alone, it has been shown that Living things may arise by a process of heterogenesis—not as products of a pre-existing organism like themselves, but by a process altogether different from those which have been hitherto supposed to be general. And it is worth remembering, as we have before pointed out, that the supposed coalescence of invisible molecules and the changes which lead to the production of the minutest living monad in organic solutions, if they could be shown to be true, would not be one whit more startling than those very changes which, before disbelieved in, can now be easily shown to take place in the "proligerous pellicle." Have we not seen that out of a mere fortuitous aggregation of living particles, and the subsequent metamorphoses taking place therein, organisms appear which are much larger, and of a much higher type than those which preceded them, although such a mode of origin was formerly regarded almost as "impossible"?

* I will here make two quotations in order to show that the opinions of two of our leading scientific men (many others might have been quoted) are not at all opposed to the comparisons I have been instituting. They both, in fact, declare emphatically that the phenomena of Life are phenomena of molecular physics.

In his address to the Mathematical and Physical Section of the British Association in 1868, Prof. Tyndall, as president, speaking of a grain of corn, said:—"But what has built together the molecules of the corn? I have already said, concerning crystalline architecture, that you may, if you please, consider the atoms and molecules to be placed in position by a power external to themselves. The same hypothesis is open to you now. But if in the case of crystals you have rejected this notion of an external architect, I think you are bound to reject it now, and to conclude that the molecules of the corn are self-posited by the forces with which they act upon each other. It would be poor philosophy to invoke an external agent in the one case and to reject it in the other. . . . . But I must go still further, and affirm that in the eye of science the animal body is just as much the product of molecular force as the stalk and ear of corn, or as the crystal of salt or of sugar. . . Every particle that enters into the composition of a muscle, a nerve, or a bone, has been placed in its position by molecular force. And unless the existence of law in these matters be denied, and the element of caprice introduced, we must conclude that, given the relation of any molecule of the body to its environment, its position in the body might be predicted. Our difficulty is not with the *quality* of the problem, but with its *complexity*." (Pp. 4 and 5.)

Prof. Huxley, again, in his article on "Protoplasm," in the *Fortnightly Review* for February 1869, says:—"Carbon, hydrogen, oxygen, and nitrogen are all lifeless bodies. Of these, carbon and oxygen unite in certain proportions and under certain conditions to give rise to carbonic acid; hydrogen and oxygen produce water; nitrogen and hydrogen give rise to ammonia. These new compounds, like the elementary bodies of which they are composed, are lifeless. But when they are brought together under certain conditions they give rise to the still more complex body, protoplasm; and this protoplasm exhibits the phenomena of life. I see no break in this series of steps in molecular complication, and I am unable to understand why the language which is applicable to any one term of the series may not be used to any of the others. We think fit to call different kinds of matter carbon, oxygen, hydrogen, and nitrogen, and to speak of the various powers and activities of these substances as the properties of the matter of which they are composed. . . . Is the case in any way changed when carbonic acid, water, and ammonia disappear, and in their place, *under the influence of pre-existing protoplasm*, an equivalent weight of the matter of life makes its appearance? . . . What justification is there, then, for the assumption of the existence in the living matter of a something which has no representative or correlative in the not living matter which gave rise to it?" (The passage I have marked by italics indicates the extent to which Prof. Huxley stops short of the views I have been endeavouring to support.)

Now I maintain that the logical outcome of such doctrines as these is that Life *may* manifest itself whenever certain particular collocations of complex molecules occur, just as surely as crystalline properties will be manifested by chloride of sodium whenever the molecules of this substance combine to form crystals. The *à priori* presumptions being rather in favour of (certainly not opposed to) the occurrence of the new evolution of Living things, we have to show, as well as we are able, that there *is* a tendency to the occurrence of such clusterings as will lead to the formation of bacteria or of fungus-spores, just as we feel compelled to believe that there is a tendency to the occurrence of those particular clusterings of molecules which result in the formation of crystals.

II.

On the probable Evolution of Living Things in Organic and Saline Solutions, which have been previously exposed to high Temperatures, in airless and hermetically sealed vessels.

We must now come to the consideration of all the experimental evidence which can be adduced in support of what the microscope teaches us as to the mode of origin of the lower kinds of Living things.

The method of experimentation which has been principally relied upon, has, since 1837, always been that introduced by Schwann. Sometimes the correspondence has been exact, and sometimes his experiments have been repeated with some slight modification. In this method, the solution of organic matter is first boiled in a flask, the neck of which is securely connected with a tube closely packed with portions of red-hot pumice-stone, or other incombustible substance: after the solution has been boiled for some time, and all the air of the flask has been expelled, the flask itself is allowed to cool whilst the tube containing the closely-packed red-hot materials is still maintained at the same temperature, in order that whatever air enters into the flask may be subjected to a calcining heat as it passes through the tube. When the flask has become cool it will then contain only the previously boiled solution in contact with air at ordinary atmospheric pressure, which has been calcined. Since it has been hitherto settled that the lower kinds of organisms which may be contained in the solutions, are destroyed when these fluids are raised to a temperature of 100° C., and that no organisms have been known to survive after having remained for thirty minutes in air raised to a temperature of 130° C., the boiling of the fluid for a time and the calcination of the air has generally been supposed to be a sufficient precaution to ensure the destruction of all organisms in the experimental media. Experiments conducted in this way have been said to yield negative results by some, whilst others have maintained that in spite of all such precautions, destined to destroy pre-existing Living things, they do, nevertheless, obtain low kinds of organisms, after two or three months, if not before, in their experimental fluids.

Negative results in these experiments can of course prove little or nothing; they may be explained equally well by either party: either no organisms have been found, because they or all the germs which could give rise to them have been killed; or it is just as fair for the evolutionists to explain the absence of organisms on the supposition that the particular fluids employed have not yielded them because of the severity of the destructive conditions to which the particular organic matter in the previously boiled fluids had been subjected. When organisms *are* found, however, in fluids which have been legitimately subjected to the conditions involved in Schwann's experiments, then one of two things is proven: either the amount of heat which hitherto was deemed adequate to destroy all pre-existing organisms is in reality not sufficient, or else the organisms found must have been evolved *de novo* as the evolutionists suppose. Unless, therefore, the standard of vital resistance to heat can be shown to be higher than it was formerly supposed to be, any single positive result when Schwann's experiment has been legitimately performed, is of far more importance towards the settlement of the question in dispute than five hundred negative results. It would tend to show that in the particular fluid employed organisms might be evolved *de novo*. And yet positive results in the performance of these experiments have been obtained again and again by Schwann himself, by Ingenhousz, by Mantegazza, Pouchet, MM. Joly, and Musset, Jeffries Wyman, Dr. Child, and, not to mention any others, even by M. Pasteur himself.*

But even this is not all; organisms have been found in fluids which had been contained in closed vessels, after exposure to conditions still more severe. Prof. Jeffries Wyman, of Cambridge, U.S., published an account (which I am sorry to say I have been unable to obtain from any of our libraries) in 1862 of experiments in which he had boiled fluids containing organic matter for a period of two hours under a pressure of two atmospheres, that

* We have the testimony of M. Pasteur to the fact that organisms may almost always be met with when milk or some other alkaline fluid is made use of in Schwann's experiment. He says he has always met with negative results, however, if such fluids have been raised to a temperature of 110° C. rather than 100° C. Concerning his inferences from these experiments I shall have more to say hereafter.

is to say, at a temperature of 120·6° C. To the fluids so treated no air was allowed access, except what had passed through the capillary bores of white-hot iron tubes. And yet, when the flasks were broken, after a certain time, organisms were found in the fluids which had been submitted to these conditions. Prof. Mantegazza has obtained organisms from the fluids of hermetically closed flasks, after these, containing the putrescible fluids and common air at ordinary atmospheric pressure, had been subjected for some time to a temperature of 140° C.; and Prof. Cantoni, of Pavia, has obtained bacteria and vibrios in the fluids of similarly prepared closed flasks, after these had been exposed in a Papin's digester to a temperature of 142° C. for four hours. And still, though no positive evidence has been forthcoming to show that the standard of vital resistance can be raised—though nobody has shown that any Living thing which has been made the subject of experimentation has been found alive after an exposure for a minute or two in a fluid raised to 100° C., or after an exposure for thirty minutes to a temperature of 130° C. in dry air or *in vacuo*, many scientific men are as much disinclined as ever to admit that the organisms found by the above-mentioned observers could have been evolved *de novo*.

For all those, however, who form their opinions on such matters in accordance with scientific evidence, rather than in obedience to theoretical preconceptions, it must be admitted that the balance of evidence is at present altogether in favour of the supposition that organisms can arise *de novo*. And it only remains for those who are opposed to the notion from an *à priori* point of view to bring forward positive evidence tending to show that the standard of vital resistance, for the organisms in question, is much higher than it has been hitherto shown to be.

Some of the additional evidence I have now to bring forward, therefore, only tends to strengthen the validity of the conclusion which was already deducible from the experiments of Pouchet, Wymann, Mantegazza, Cantoni, and others.

Hitherto, in speaking of the experiments of Schwann, I have only incidentally referred to the destructive influence of heat upon the organic matter contained in the solutions, though very strong evidence could be adduced to show that such organic matter is notably altered after these solutions have been raised to the temperature of 100° C. The disruptive agency of heat is fairly enough supposed by the evolutionists to destroy some of the more mobile combinations in each solution—to break up, more or less completely, in fact, those very complex organic products, whose molecular instability is looked upon as one of the conditions essential to the evolutional changes which are supposed to take place. I shall postpone for the present the consideration of the question as to how far this destructive agency of heat is affected by the alkalinity, neutrality, or acidity of the fluids, though I shall towards the close of this paper bring forward evidence tending to show that organic matter in acid solutions is more damaged by the temperature of 100° C. than is that which is contained in neutral or slightly akaline solutions, when these are heated to the same extent.

To any one looking boldly at the problem, the question which now seems to suggest itself is, whether any other substances can be employed in place of the organic matter, such as would not be injured by a temperature of 100° C., and which, whilst containing the necessary elements for the formation of an organism, might also permit the occurrence of those peculiar molecular re-arrangements which result in the formation of Living things? Postponing, however, the consideration of this question for the present, I will first refer to the influence of another of the detrimental conditions involved in Schwann's experiments.

In those instances where the results were positive, and in which calcined air and previously heated organic solutions were shut up in hermetically-sealed vessels, nothing but the lowest forms of living things ever appeared—mere monads, bacteria, and vibrios—and these generally not till after the expiration of two, three, or more months. There seems reason to believe that the delay, and the very low forms of the organisms met with, are attributable, in great part, to the increased tension which almost invariably occurs within the closed vessels. In some cases the tension becomes so great that it ultimately bursts the flasks. This took place several times in the course of Dr. Child's experiments. After reflection upon these facts, it seemed to me that there was not much room for surprise, looking at it from the evolutionist's point of view, that the results should have been so unsatisfactory. The small amount of space above the level of the fluid, is already occupied, at the time that the flask is hermetically sealed, with air under ordinary atmospheric pressure. But, when putrefactive or evolutional changes begin to take place in the fluids containing organic matter, such changes are almost sure to be attended by the liberation of gases either simple or compound. And in direct proportion to the extent of the liberation, so does the tension and consequent pressure upon the fluid increase within the flask. This pressure upon the solutions might and probably would tend to prevent, in proportion to its extent, those life-giving re-arrangements which are presumed to take place amongst the molecules of the organic matter contained therein. Having come to this conclusion, and as there also seemed to be good reason for the belief that atmospheric air was not needed for the development of bacteria and vibrios, it occurred to me that it would be possible so to modify the experiment of Schwann that it might be repeated under conditions more satisfactory to the evolutionists, and at the same time in a way which would be not less in accordance with the views of the panspermatists. The withdrawal of all air from the flasks in which the boiled solutions were contained, rather than the admission of calcined air, seemed to be the kind of modification which was desirable.* Then the contamination of the boiled fluids with possible atmospheric germs would be as effectually provided against as if air had been only allowed to enter after it had been calcined, and the seemingly obvious advantage would be attained that there would be even greater freedom than usual for the commencement of evolutional changes, on account of the diminished pressure upon the fluids contained *in vacuo*. It was presumed, also, that changes might go on for a certain extent before the evolution of gases had been sufficient to exercise such a repressive influence as to prevent their continuance.

The results of experiments conducted upon these principles have been most uniform, and have been of such a nature as to tend to support the truth of the reasonings which dictated them.

The flasks employed have generally been of small size, capable of holding about two ounces of fluid. These have proved to be quite large enough, and their small size made it easy for me to manage the whole process with a very slight amount of assistance. The method adopted was as follows:—After each flask had been thoroughly cleaned with boiling water, three-fourths of it was filled with the fluid which was to be made the subject of experiment. With the aid of a small hand blow-pipe and the spirit-lamp flame, the neck of the flask, about three inches from its bulb, was then drawn out till it was less than a line in diameter. Having been cut across in this situation, the fluid within the flask was boiled continuously for a period of from ten to twenty minutes. At first ebullition was allowed to take place rapidly (till some of the fluid itself frothed over) so as to procure the more thorough expulsion of the air; then the boiling was maintained for a time at medium violence over the flame of my reading-lamp, whilst the greatly attenuated neck of the flask was heated in the flame of a spirit-lamp placed at a corresponding level. The steam for a time poured out violently into the flame of the spirit-lamp; and whilst my assistant (my wife) turned down the flame of the reading-lamp so as to diminish still further the violence of the ebullition, I directed the blow-pipe flame upon the narrow orifice of the neck of the flask, and so sealed it hermetically. Immediately that the orifice was closed, the heat was withdrawn from the body of the flask.

After a little practice I soon became able to procure in this way an almost perfect vacuum. Even though the vessels were

* I was actually led to adopt this important modification, perhaps, by a mere chance. In the spring of last year Mr. Temple Orme, of University College, had kindly undertaken to perform some experiments with me bearing upon the subject of "Spontaneous Generation." We at first proposed to repeat, with some very desirable variations which he suggested, Schultze's experiments. One day, however, he told me he had boiled an infusion of hay for four hours, and had then hermetically sealed the neck of the flask whilst ebullition continued. In this way a more or less perfect vacuum was procured. This he did as a sort of tentative experiment, and it was then, on thinking over the subject, that I resolved to give the plan a thorough trial, as it appeared to me that by so doing I should be working under conditions which were most in accordance with the theory of evolution. I performed four experiments at that time in concert with Mr. Temple Orme, with hay infusions which had been boiled for four hours and had then been sealed up *in vacuo*. In each of these fluids organisms were found after a comparatively short time. These were the first experiments performed under such conditions. In my subsequent work I have not had the benefit of Mr. Orme's personal assistance, although I have frequently profited by suggestions which he has made.

so small, momentary ebullition could generally be renewed again and again for the space of five minutes after they had been hermetically sealed, by the mere application of one of my fingers, which had been dipped into cold water, to a portion of the glass above the level of the fluid. The water-hammer effect was also very obvious in several which were tested in this fashion.

I believe that an almost perfect vacuum can be produced in this way; in the first violent ebullition the air is driven out of the flask by the fluid, and as ebullition is continuously kept up after this till the flask is hermetically sealed, there is always an outpouring of heated vapour, and no opportunity for a re-ingress of air. But, even if in any given case the vacuum should not prove to be absolute, it does not seem to me that there would be any material abatement from the severity of the conditions which the panspermatists have a right to demand. If, on the one hand, absolutely the whole of the air had not been expelled from the flasks during the process of ebullition, what remained would necessarily be mixed up with a very much larger quantity of continually renewed aqueous vapour, and the effect would probably be that any living things would be just as effectually and destructively heated as if they were lodged in the boiling solution itself; whilst if, on the other hand, the boiling had been arrested for one or two seconds before the complete closure of the almost capillary orifice at the mouth of the flask, even if any air entered, it would have had first to pass through the blow-pipe flame, and then through the white-hot capillary orifice—it would in fact have been calcined as in Schwann's experiment. The conditions of the experiment would then have been no less severe, and the only effect would be that the vacuum with which I prefer to work would have been rendered by so much the less complete. Although I make these remarks with the view of meeting criticisms, I am inclined to think that the vacuum in my experiments has been complete; and it should be remembered that M. Pasteur always adopted this method when he wished to preserve solutions for a time *in vacuo*. Whenever he desired to make comparative trials with the air of different localities, the solutions which had been prepared in this way were assumed by him to be contained *in vacuo*, so that the flasks could then be taken to the localities, with the air of which he wished to experiment. There the necks of the flasks were broken, in order that they might become filled with the air of the respective localities. After this had been done the flasks were resealed, and kept for future observation of their contained fluids. M. Pasteur, M. Pouchet, and others who adopted this method, carried away their experimental fluids *in vacuo*, during a two or three days' journey to the Alps or to the Pyrenees, and it never seemed to have occurred to either of them that evolutional changes might be taking place during their journey. M. Pasteur, in fact, habitually shut his eyes to all such possibilities, they did not come within the range of what he considered possible; such thoughts might, however, have suggested themselves to M. Pouchet and others, although this does not seem to have been the case.

After the flasks had been prepared in the way above mentioned, they were suspended beneath the mantelpiece in my study. During the day there was always a fire in the room, and at night I put my reading lamp underneath them with the flame properly turned down. So far as I have been able to ascertain, the temperature to which they have been subjected has mostly ranged between 23°—29° C. (75°—86° F.) Sometimes they have been exposed to the lower temperature and sometimes to the higher, and I suspect that a variation of this kind may perhaps be more favourable for the production of evolutional changes than maintenance at a constant temperature.

In detailing the results of the following experiments, I shall not enter into any minute description of the organisms found. My main object throughout has been to obtain evidence on the subject as to whether a *de novo* evolution of Living things could or could not take place. The demands upon my time have been so serious in the carrying on of these investigations, that occasionally it has only been small portions of the experimental fluids which have been examined. If, for instance, what I found in the first few drops of the fluid left no doubt in my mind as to the nature and abundance of some Living things contained therein, the remaining portions of the fluid were frequently not examined. Other bodies, therefore, may have been contained in the solutions, which were never seen at all.

H. CHARLTON BASTIAN

(*To be continued.*)

SCIENTIFIC SERIALS

THE *American Naturalist* for June contains several excellent articles. The first is by Prof. J. S. Newberry, "On the Surface Geology of the Basin of the great Lakes and the Valley of the Mississippi." In the northern half of this area down to the parallel of 38° to 40° N. lat., are found, not everywhere, but in most localities where the nature of the underlying rocks is such as to retain inscriptions made upon them, the unmistakeable indications of glacial action. Some of the valleys and channels which bear the marks of glacial action, evidently formed or modified by ice, and dating from the ice period of an earlier epoch, are excavated far below the present lakes and water-courses which occupy them. These valleys form a connected system of drainage at a lower level than the present river system, and lower than could be produced without a continental elevation of several hundred feet. Upon the glacial surface are found a series of unconsolidated materials, generally stratified, called the drift deposits. These consist in the lowest stratum of the Erie clays of Sir William Logan, above which are sands containing beds of gravel; and near the surface elephants' teeth have been found, water-worn and rounded. Upon these stratified clays, sands, and gravel of the drift, are scattered boulders and blocks of all sizes, of granite, greenstone, siliceous and mica slates, and various other metamorphic and eruptive rocks, generally traceable to some locality in the Eozoic area of the lakes. Among these boulders many balls of native copper have been found, which could have come from nowhere else than the copper district of Lake Superior. Above all these drift deposits, and more recent than any of them, are the "lake ridges," corresponding to our raised sea-beaches, embankments of sand, gravel, sticks, leaves, &c., which run imperfectly parallel with the present outlines of the lake margins, where highlands lie in the rear of such margins. The general conclusions drawn are the existence of a glacial epoch over the northern half of the continent of North America, probably contemporaneous with that of Europe, and with a climate comparable to that of Greenland; that the courses of these ancient glaciers correspond in a general way with the present channels of drainage; and that at this period the continent must have been several hundred feet higher than now.—"A Winter's Day in the Yukon Territory," by W. H. Dall, refutes the prevalent idea, perpetuated even by "official" reports, that the island of St. Paul is surrounded in winter by immense masses of ice, on which the polar bears and arctic foxes sail down from the north and engage in pitched battles with the wretched inhabitants. The fact is that there is no solid and very little floating ice near St. Paul in winter; the arctic foxes found there as well as on most of the islands were purposely introduced by the Russians for propagation, a certain number of skins being taken annually; and there is no authentic evidence that the polar bear has ever been found south of Behring's Straits. The country of Alaska comprises two climatic regions, which differ as widely as Labrador and South Carolina in their winter temperature. One contains the mainland north of the peninsula of Alaska and the islands north of the St. Matthew group; the other includes the coast and islands south and east of Kadiak, while the Aleutian Islands, with the group of St. Paul and St. George, are somewhat intermediate. A day's excursion during the winter season in the northern and more inhospitable of these two regions yielded a considerable number of interesting animals.—Articles of a popular character are "Our native Trees and Shrubs," by Rev. J. W. Chickering, Jun.; and "A Few Words about Moths," by A. S. Packard, Jun. A review of Principal Dawson's article in the *Canadian Naturalist* on "Modern Ideas of Derivation," criticises, favourably on the whole, that writer's strictures on the Darwinian theory of Natural Selection.—The Natural History Miscellany contains many interesting notes, either original or culled from English scientific journals.

The fourth part of the *Jenaische Zeitschrift für Medicin und Naturwissenschaft*, June 1870, contains the following important articles. 1. Gegenbauer on the skeleton of the limbs of Vertebrata, and of the Selachia and Chimæra in particular. 2. Abbe on a spectrum apparatus for the microscope. 3. Dr. Dohrn: Further researches on the structure and development of the Arthropoda, especially bearing on the Zoea stage of Crustacea; and lastly, a long and interesting paper by Ernst Haeckel on the "Plastiden-theorie," in which he treats fully of the deep-sea life brought to light by the dredgings of Drs. Wallich, Carpenter, Wyville Thomson, Huxley, and others, describing the Bathybius, Coccoliths, Globigerina, &c. He confesses himself unable to solve the problem of the origin of the immense quantities of protoplasm that form a bottom to the sea, but is disinclined to regard it as consisting of the mycelium of sponges, an opinion advanced by Wyville Thomson. He finds the well-known yellow cells of Radiolaria to contain starch, the reactions of which are not distinguishable from those characteristic of starch derived from vegetables. These starch granules make up more than half of the entire mass of the Radiolaria.

The *Bulletin de la Société Imperiale des Naturalistes de Moscow*, 1869, No. 2 (received June 15, 1870), contains, amongst other valuable papers, a carefully-worked-up description of the anatomy and development of the *Pedicellina*, by B. Uljanin, which is accompanied by two plates illustrating the changes undergone as far as he had an opportunity of observing them.

In the last number (Heft iv. Band lx.) of the *Sitzungsberichte der K. Akad. der Wissenchaften zu Wien* is a long paper by Dr. A. Polotebnow on the origin and mode of increase of Bacteria. These, as most of our readers are aware, consist of very small rods, which present a kind of tranverse striation at tolerably regular intervals, like an extremely diminutive sugar-cane of from two to six or seven joints, and which exhibit irregular vibratory movements. They have been, like other lowly organised forms, sometimes considered, as by Dujardin, to belong to the animal kingdom; sometimes, as by Cohn, to represent a form of vegetable life; and sometimes, as by Pertz, to occupy an intermediate position on the confines of the two kingdoms. Dr. Polotebnow finds that an unbroken series of forms can be observed between the minute round cells which form the mycelium of *Penicillium*, and probably other fungi, and fully-developed Bacteria. In regard to their multiplication, he thinks this can only occur from the cells above mentioned, and that when they have once become fully formed Bacteria they are no longer capable of further multiplication.

SOCIETIES AND ACADEMIES

London

Geological Society, June 8.—Mr. Joseph Prestwich, F.R.S. president, in the chair.

Mr. Henry G. Vennor, of the Geological Survey of Canada, Montreal; Alexander Kendall Mackinnon, Memb. Inst. C. E., Director-General of Public Works, Montevideo, South America; and Mr. Arthur Roope Hunt, Quintella, Torquay, were elected Fellows of the Society.

1. "On the Superficial Deposits of the South of Hampshire, and the Isle of Wight." By Thomas Codrington, F.G.S. This paper treated of the gravel deposits covering the tertiary strata of the country between Portsmouth and Poole, and of the Isle of Wight. The strikingly tabular character of the surface is best seen on the east of the Avon, where from the coast for more than twenty miles inland a gravel-covered plain can be followed, rising gradually from 80 feet to 420 feet above the sea, at the rate of about 20 feet per mile. The high plains of the New Forest, to the eye perfectly level, and indented by deep valleys, are portions of this table-land. The plateau between the Bournemouth Cliffs and the Valley of the Stour, and detached gravel-capped hills further inland, are the remnants of a similar table-land on the west of the Avon, while eastwards the same character prevails up to Southampton Water. Sections parallel with the coast show the level nature of the country, broken only by well-defined river-valleys. On the east of Southampton water a similar tabular surface, sloping at a steeper angle towards the shore-line, and cut through by the valleys of the Itchen, Hamble, and Titchfield rivers, remains; and in the Isle of Wight the gravels capping the flat-topped tertiary hills coincide with a corresponding plain sloping northwards. The gravel covering these table-lands is composed chiefly of subangular chalk-flints, with a varying proportion of tertiary pebbles. Sarsen stone blocks are found everywhere, and on Poole Heath granitic pebbles; and in the gravel of Portsea large boulders of granitic and palæozoic rocks are met with. In the Isle of Wight, chert from the Upper Greensand, and materials from the Lower Cretaceous beds also occur. The colour of the gravel is generally red; and the origin of the white gravel, which often overlies the red, is to be ascribed to the bleaching action of vegetable matter. Brick-earth is generally associated with the gravel at all levels but the highest; but the contorted appearances attributed to glacial action only occur at low levels. No organic remains have been found in the gravel covering the plains, while the valley-gravels of the district have afforded mammalian bones and teeth of the usual species. Flint implements have been found at Bournemouth at 120 feet above the sea; at Lymington, near Southampton, at 80 and 150 feet; and also along the shore between Southampton Water and Gosport, at 35 feet above the sea, from gravel forming part of the covering of the tabular surface, and unconnected with the river valleys. The gravel capping the cliffs of the south coast of the Isle of Wight, in which the remains of *Elephas primigenius* have been found near Brook and Grange, was probably deposited in the same river-basin as the mammaliferous gravel of Freshwater; and the cutting back of the coast-line by the sea has given the tributaries of a river which flowed by Freshwater northwards to the Solent, a direct outfall to the sea; and the streams thus intercepted at a high level, under the changed condition of flow, have originated the *Chines*. The gravel cliff of the Foreland, at the eastern end of the Isle of Wight, consists principally of raised shingle, which towards the south thins out, and is overlain by a thick deposit of brick-earth, a continuation of which caps the cliffs up to the chalk, and in which a flint implement was found by the author at 85 feet above the sea.

General Considerations.—The marine gravel, with granite boulders covering the south of Sussex, is continued westward by the gravel with similar boulders covering Portsea Island; and this again by the Hill-head gravels, with large blocks of Sarsen stone, these lower gravels being bordered on the south by the raised shingle deposits of the Isle of Wight, and on the north by the higher marine gravels of Avisford, Waterbeach, and Bourne, from which the lower gravel is divided by a well-marked step, extending beyond Portsdown Hill to Titchfield, and traceable on the west of Southampton Water. The Hill-head gravels are considered to be an estuarine deposit, of the same age as the marine gravels of Sussex, and the low-level gravels of the river-valleys; they are supposed to have been formed when the Isle of Wight was still joined to the main land, and all the rivers now reaching the sea by Poole Harbour, Christchurch Harbour, Southampton Water, &c., were affluents of a river communicating with an estuary opening to the sea in the direction of Spithead. The gravels lying above the step, such as those of Avisford and Waterbeach, Titchfield Common, Beaulieu Heath, and Bournemouth, are looked upon as equivalent in position and age to the high-level valley gravels. The level of the gravels on the highest parts of the table-lands is such as to indicate an age far greater than that of the highest gravels of the river-valleys; but the uniform surface from the 400-feet level downwards points to a long continuance of similar conditions, during which the gravel from the highest levels to that of the Bournemouth Cliffs was deposited. The area that can with any probability be assigned to the catchment basin of a river such as that which has been before alluded to, is only three-quarters of the basin of the Thames above Hampton, within which it is difficult to imagine that such an extent of gravel could have been spread out; and the inclination of the flattest of the table-lands is for a river such as only mountain-streams have, and quite incompatible with the spreading out of large even surfaces more than twenty miles across. It is considered more probable that the materials of the gravel were brought down from the chalk country on all sides by rivers, and spread out in an inlet of the sea shut in on the south, and opening out eastwards. This view is not without difficulties; it involves a gradual upheaval of the land, which, when the highest gravels now remaining were being spread out at or near the sea-level, must have stood more than 400 feet lower; and a considerable part of this upheaval must have taken place since the formation of the gravel in which implements fashioned by man are imbedded.

2. "On the relative position of the Forest-bed and the Chillesford Clay in Norfolk and Suffolk, and on the real position of the Forest-bed." By the Rev. John Gunn, M.A., F.G.S.

The author commenced by stating that both at Easton Bavent and at Kessingland the Forest-bed is to be seen forming part of the beach, or of the foot of the cliff, and underlying the Chillesford Clay. He considered that the soil of the Forest-bed had been deposited in an estuary, and that after its elevation the trees, of which the stools are now visible along the coast, grew upon it, and the true Forest-bed was formed. After the submergence of this first freshwater, then fluvio-marine, and finally marine deposits were formed upon it; and the author proposed to give the whole of these deposits the name of the "Forest-bed series." The author suggested that the Forest-bed itself is represented inland by the stony bed which lies immediately upon the chalk and between it and the Fluvio-marine and Marine Crags, his theory being that the surface of the chalk, after supporting a forest-bed fauna, was gradually covered up by successive crag deposits.

3. "On *Proterosaurus speneri*, von Meyer, and a new species, *Proterosaurus huxleyi*, from the Marl-slate of Midderidge, Durham." By Albany Hancock, F.L.S., and Richard Howse. Communicated by Prof. Huxley, F.R.S., F.G.S.

In this paper the authors described a specimen which they referred to *Proterosaurus speneri*, von Meyer; and one of a smaller form, which they regarded as new, and described as *Proterosaurus huxleyi*. Both were from the same part of the marl-slate of Midderidge, Durham. The two species agree in having the limbs and tail long and the neck long, and composed of seven vertebræ, in the number of dorsal vertebræ, in the number and character of the bones of the hand, and in some other particulars, sufficient, with these, in the opinion of the authors, to justify the reference of both to the genus *Proterosaurus*. In *P. huxleyi* the ribs are flattened instead of rounded at the proximal extremity, and less widened and grooved at the distal extremity than in *P. speneri*; the hind limb is considerably longer in proportion to the fore limb: and the distal extremity of the humerus is only twice as wide as the constricted part, instead of three times, as in the old species.

Chemical Society, June 16.—Prof. Williamson, F.R.S., president, in the chair. L. A. Lucas and A. W. Bickerton were elected Fellows. Mr. James Bell read a paper on "Fermentation." The author has instituted a series of experiments to determine: 1. The forms of natural ferment which various albuminous bodies will give rise to in solutions of cane sugar, and of cane sugar and glucose. 2. The relative fermentative powers of various ferments, especially of those occurring in malt extract and in the grape juice. 3. The influence of change of soil upon the fermentative organisms. From among the manifold results obtained in these experiments, the following may be mentioned: (*a*) Addition of glucose to fermenting liquids, especially to the juice of grape, is advantageous, inasmuch as it assists to exhaust the juice of its fermentative element, and thus imparts to the wine a greater keeping power. (*b*) Each ferment has its favourite soil. The President, in proposing a vote of thanks to the author, took occasion to give a brief *résumé* of the present state of knowledge of the yeast plant. Though called a "plant," the yeast organism appears in all its functions rather animal than vegetable; the products of its secretion are less complicated than those it takes in; it does not, like plants, require light for its vital process, neither does it absorb heat, but on the contrary gives it off. Alluding, then, to Liebig's recent memoir on fermentation, Prof. Williamson observed that that distinguished chemist had entirely dropped his former notions regarding the process of fermentation.—Dr. Heisch communicated a paper "On organic matter in water." The author was some time ago called on to assist a large manufacturer of lemonade, who suddenly found it impossible to make lemonade that would keep. After a day or two it became turbid, and its odour anything but agreeable. On investigating the liquid under the microscope it was found full of small spherical cells with, in most cases, a very bright nucleus. After examining all the materials employed, the fault was detected to be with the water. On putting a few grains of pure crystalline sugar into some of the water, it became turbid in a few hours, and contained the cells above mentioned. On inquiry it turned out that the well from which the water used in the preparation of the lemonade was obtained, had been slightly contaminated with sewage. This led the experimenter to mix a minute quantity of sewage with a sugar solution; the cells very soon made their appearance. Filtration through the finest Swedish paper does not remove the germs. Boiling for half an hour in no way destroys their vitality. Filtration through a good bed of animal charcoal seems to be the only effectual mode of removing them, but it is necessary to air the charcoal from time to time, else it loses its purifying property.—Mr. Perkins read a letter from Prof. Strecker, wherein the latter claims the priority of having published the true formula of alizarin as early as 1866, a fact which was not mentioned by Mr. Perkin in his recent lecture on alizarin. Mr. Perkin said that this omission was due to an oversight on his part, certainly not to any intention to deprive Professor Strecker of his merits.—Mr. Herman read a paper "On the methods for the determination of carbon in steel." Several samples of steel were analysed according to different methods, with the view of ascertaining which of the usual processes for determining the carbon in iron is the most advantageous. A large number of careful experiments led to the conclusion that the direct burning of the iron filings in a stream of oxygen (Wöhler's process) is the most expeditious and accurate method. In the following table the means of the results obtained by the different methods are given, the quantities of ferric oxide obtained by combustion in oxygen are almost identical with those required by theory.

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.
By Eggertz's method	1·319	·789	·701	·587	·486	·349	·283	
By combustion in oxygen	1·1656	·7602	·635			·3594	·273	·9215
By Elliott's method	1·248	·8065	·724	·6701	·5025	·4772	·349	·9427

PARIS

Academy of Sciences, June 13.—The following mathematical papers were read:—Demonstration of Jacobi's method for the formation of the period of a primitive root, by M. V. A. Le Besque; on the construction of the axis of curvature of the developable surface, enveloping a plane of which the displacement is subjected to certain conditions, by M. A. Mannheim, communicated by M. Chasles; and on a certain family of curves and surfaces, by MM. F. Klein and S. Lie, also presented by M. Chasles.—MM. Jamin and Amaury presented a note on the specific heat of mixtures of alcohol and water, in which they show by the examination of numerous mixtures that the specific heat of such fluids is not only higher than the mean specific heat of their constituents, but that in certain proportions it even exceeds that of water.—M. Bussy remarked that M. Buignet and himself had previously ascertained that a mixture of equivalent parts of alcohol and water had a specific heat greater than the mean of its elements.—MM. C. Martins and G. Chancel presented a second note on the physical phenomena which accompany the rupture of hollow projectiles of various calibres by the congelation of water, in reply to observations made by General Morin and MM. Dumas and Elie de Beaumont on their former communication.—A note by M. J. Violle, on the mechanical equivalent of heat, was presented by M. H. Sainte-Claire Deville.—M. A. Houzeau communicated some experiments on the electrolisation of the air or of oxygen as a means of producing ozone. He stated that the production of ozone is greater at the negative than at the positive pole, that it increases with the electrical intensity but only to a certain extent with the duration of the experiment, that its formation is not prevented by the envelopment of the poles in glass tubes, and increases considerably with diminution of temperature, and that the ozone produced in air contains small quantities of nitrous compounds, which do not occur in that furnished by pure oxygen.—M. de Saint-Venant presented a note by M. J. Boussinesq on the theory of the flow of a liquid through an orifice in a thin wall.—The ephemeris of the newly-observed comet was communicated by M. Le Verrier from a letter of M. Winnecke's.—An extract from a letter from M. Bourgogne, giving an account of a storm which burst on the 29th of May in the neighbourhood of Alais, was communicated by M. Dumas. The hailstones were as large as small walnuts, and the damage done to the vegetation was immense.—A note on the spirting (*rochage*) of the carburets of iron, and on the sparks produced by these metals, with remarks on some new properties of iron, was read by M. H. Caron.—Some researches on platinum, by M. P. Schützenberger, were presented by M. H. Sainte-Claire Deville. This paper related to two compounds previously described by the author, namely, chloro-platinite of carbonyle, and chloro-platinite of dicarbonyle, $C\,O\,Pt\,Cl^2$ and $C^2O^2\,Pt\,Cl^2$, which he stated may be regarded as the chlorides of two diatomic compound radicals, platoso-carbonyle (C O, Pt) and platoso-dicarbonyle $\left(\begin{smallmatrix}CO\\CO\end{smallmatrix} > Pt\right)$. He described the behaviour of these compounds when treated with ammonia and ethylene, and also the action of protochloride of phosphorus upon subchloride of platinum, and of perchloride of phosphorus upon platinum.—A long and elaborate paper on tribromhydrine, by M. L. Henry, was read, and a note by M. Gobley, on the action of ammonia upon lecithine, was presented by M. A. Wurtz. He has found that lecithine in presence of ammonia gives origin to margaramide, to phosphoglyceric acid, and to choline.—M. C. Robin presented a note by MM. Lebert and Cohn, upon a new species of *Peronospora*, parasitic on cacti. The species, which the authors named *P. cactorum*, attacked several specimens of *Melocactus* and *Cereus giganteus*, in General

Jacobi's collection, and affected them with a putrefactive disease analogous to that of the potato, in which a *Peronospora* also takes part.—A note by M. E. F. Marey, "On the mechanism of the flight of birds," was communicated by M. H. Sainte-Claire Deville, to which we have adverted in another column.—A note by M. E. Perrin, presented by M. de Quatrefages, contained observations on the scissiparous reproduction of the Naïdea, as evinced in the genus *Dero*.—Other papers communicated were a note by M. Yvon Villarceau, "On the decimal division of angles and of time;" a note by M. Morache, "On the use of creosote in the treatment of typhoid fever;" one by M. Pegrani, "On the relation of the sympathetic nerve to the secretion of urine;" and one by M. Duboux, "On a new sign of death."

BOSTON

Natural History Society, March 16.—The secretary read the following observations of Mr. L. Trouvelot, upon the tendency of trees to bend toward the east. In the *Scientific American*, of March 5th, 1870, is inserted a paragraph headed "The Growth of Tree Trunks." It is there stated that a French naturalist had been measuring the tree trunks in a forest, and had found them all broader in the east-west than in the north-south direction, while another arborist of Toulouse, similarly gauging the trees, found the greatest swelling of their trunks towards the east-south-east; the former attributing this want of symmetry to the rotation of the earth, while the latter thinks that it is due to the early action of the sun upon the sap. As this paragraph reminded me of some observations which I made some five or six years ago, and which bear closely upon the same subject, I will present them to the society, thinking they may have some value in a scientific as well as in a practical point of view. While in the country, if we observe attentively the tree tops, we shall soon perceive that many species seem affected by a steady wind, though there is not the least breeze to be felt. Soon we notice that the branches of a great many trees have a general tendency to obey an unknown force which bends their extremities towards the east, or perhaps more correctly, in a direction perpendicular to the magnetic meridian. This bending of small branches cannot be observed so plainly upon all kinds of trees; some species having it well marked in every instance, while other species have it less visible, and even some others not at all noticeable. Most prominent for this peculiarity is the cherry tree, sometimes bending its branches towards the east, from head to foot. Next to this come the maple, the button wood tree (*Platanus*), then the pear tree, then the oak, etc. In the last named it is not always noticeable, though if the tree is isolated from others it is very plain in every instance. With the cherry tree it is so certain, that one could almost invariably determine the cardinal points by looking at the direction of its branches. At first I thought this might be due to the action of the prevailing winds, but this hypothesis was somewhat shaken, when I saw in many instances cherry trees sheltered entirely from the west winds by high blocks of houses within a few feet of them, exhibiting the same phenomenon. Whether this direction of the branches of trees is to be attributed to the prevailing winds, or to the rotation of the earth upon its axis, or to the heat or light of the sun, or again, to terrestrial magnetism, I shall not inquire at present, not having sufficient data to establish any theory. It would be of value, I venture to say, if observers would direct attention to this subject, and see if the direction is the same all over the globe, or if it is a local phenomenon, and also ascertain what species of trees obey this unknown force. It is not only in a theoretical point of view that this observation has some value; there is in it a practical lesson for the cultivators of shade and fruit trees. Soon after my observations, it struck me that something practical could be derived from this truth. All country people know by experience—sometimes dearly bought—that the transplantation of trees does not always succeed, and especially when the transplanted trees have arrived at a certain age. Fruit growers tell us that the cherry tree is one of those least likely to live when transplanted, while the apple tree will almost invariably succeed. My observations on many thousand cherry trees have shown me that this tree is very sensitive to the unknown force, while the apple tree is a great deal less so, and it is very seldom that an indication of bending will be seen. Has not this anything to do with success in transplanting? If, without regard to the direction of the branches of a cherry tree, we set this tree in a position contrary to the one it occupied before, its branches now bending towards the west, then it is plain that the force which gave it the bend is acting in an opposite direction, in consequence of which the tree is suffering. But with the apple tree it is different, as this is far less sensitive; therefore it will not suffer much. Ten years ago I bought a fine cherry tree and transplanted it to my garden, of course without regard to direction; the tree is now living; it has not grown a particle: there has not been one inch of new wood added to the length of its twigs since it was put there; the branches have no bend. Five years ago another cherry tree from the same place was also transplanted in my garden; the tree is now treble the size of the other, its branches are strongly bent east. Why this difference? Was the one set in a suitable position, and the other not? I could not tell. But here is something more positive. Three years ago I saw in Malden twenty beautiful pear trees transplanted with the greatest care; all these trees were of pretty good size, being some years old, and they all bent very strongly. They were set without regard to direction; five or six of these trees happened to be placed in about the position which they must have had when growing, the remainder were set in all directions. I went many times that way to watch the success of this small orchard. The very first year about one half were completely dead. The second year took five more, which had been languishing all the summer, and now five out of the twenty are living and in good condition, and strange to say, these five are those which were set with their branches dipping east. Do we owe their life to the fact that after being transplanted they occupied the same relative position with regard to the points of the compass as before, or is it only a curious coincidence? It is more than I can tell. My experience is not sufficient to allow an opinion in this matter; time will throw light upon the subject.

DIARY

FRIDAY, July 1.

Geologists' Association, at 8.

SUNDAY, July 3.

Sunday Lecture Society, at 8.—On Man's Cruelty to Man: Rev. Allen D. Graham.

MONDAY, July 4.

Entomological Society, at 7.
London Institution, at 4.—Botany: Prof. Balfour.
Royal Institution, at 2.—General Monthly Meeting of Members.

BOOKS RECEIVED

English.—Guide to the Western Alps: J. Ball (Longmans).—Treatise on the Astrolabe of Chaucer: A. E. Brae (J. R. Smith).—On the Manufacture of Beet root Sugar: W. Crookes (Longmans).—A Glance at some of the Principles of Comparative Philology: Lord Neaves (Blackwoods).—Technological Dictionary (English-German-French), edited by E. Althaus (Williams and Norgate).—Astronomical Observations taken during the years 1865-1869 at the Private Observatory of J. G. Barclay (Williams and Norgate).—Westward by Rail: W. F. Rae (Longmans).

Foreign.—(Through Williams and Norgate).—Petit traité de physique; 1re fascicule: M. J Jamin.—Bryozoi fossili Italiani; 3za Contribuzione: D. A. Manzoni.—Reactions Schema für die qualitative analyse zum Gebrauche in chemischen Laboratorium zu Berlin.—Jahresbericht über die Fortschritte der Chemie: A. Strecker.—Archiv für mikroskopische Anatomie: M. Schultze.—Vorweltliche Pflanzen aus den Steinkohlengebirge der preussischen Rheinlande und Westphalien: Dr. C. J. Andrea.—Phanologische Beobachtungen aus dem Pflanzen und Thier-reiche: Karl Fritsch.—Annales del Museo Publico de Buenos Aires: F. Savy ed Autor.

CONTENTS

THURSDAY, JULY 7, 1870

PASTEUR'S RESEARCHES ON THE DISEASES OF SILKWORMS

I HAVE recently received from M. Pasteur a copy of his new work, "Sur la Maladie des vers à soie," a notice of which, however brief and incomplete, will, I am persuaded, interest a large class of the readers of NATURE. The book is the record of a very remarkable piece of scientific work, which has been attended with very remarkable practical results. For fifteen years a plague had raged among the silkworms of France. They had sickened and died in multitudes, while those that succeeded in spinning their cocoons furnished only a fraction of the normal quantity of silk. In 1853 the silk culture of France produced a revenue of one hundred and thirty millions of francs. During the twenty previous years the revenue had doubled itself, and no doubt was entertained as to its future augmentation. "Unhappily, at the moment when the plantations were most flourishing, the prosperity was annihilated by a terrible scourge." The weight of the cocoons produced in France in 1853 was twenty-six millions of kilogrammes; in 1865 it had fallen to four millions, the fall entailing in the single year last mentioned a loss of one hundred millions of francs.

The country chiefly smitten by this calamity happened to be that of the celebrated chemist Dumas, now perpetual secretary of the French Academy of Sciences. He turned to his friend, colleague, and pupil, Pasteur, and besought him with an earnestness which the circumstances rendered almost personal, to undertake the investigation of the malady. Pasteur at this time had never seen a silkworm, and he urged his inexperience in reply to his friend. But Dumas knew too well the qualities needed for such an inquiry to accept Pasteur's reason for declining it. "Je met," said he, "un prix extrème à voir votre attention fixée sur la question qui interesse mon pauvre pays; la misère surpasse tout ce que vous pouvez imaginer." Pamphlets about the plague had been showered upon the public, the monotony of waste paper being broken at rare intervals by a more or less useful publication. "The Pharmacopœia of the Silkworm," wrote M. Cornalia in 1860, "is now as complicated as that of man. Gases, liquids, and solids have been laid under contribution. From chlorine to sulphurous acid, from nitric acid to rum, from sugar to sulphate of quinine,—all has been invoked in behalf of this unhappy insect." The helpless cultivators, moreover, welcomed with ready trustfulness every new remedy, if only pressed upon them with sufficient hardihood. It seemed impossible to diminish their blind confidence in their blind guides. In 1863 the French Minister of Agriculture himself signed an agreement to pay 500,000 francs for the use of a remedy which its promoter declared to be infallible. It was tried in twelve different departments of France and found perfectly useless. In no single instance was it successful. It was under these circumstances that M. Pasteur, yielding to the entreaties of his friend, betook himself to Alais in the beginning of June 1865. As regards silk husbandry, this was the most important department in France, and it was also that which had been most sorely smitten by the epidemic.

The silkworm had been previously attacked by *muscardine;* a disease proved by Bassi to be caused by a vegetable parasite. Muscardine, though not hereditary, was propagated annually by the parasitic spores, which, wafted by winds, often sowed the disease in places far removed from the centre of infection. According to Pasteur, muscardine is now very rare; but for the last fifteen or twenty years a deadlier malady has taken its place. A frequent outward sign of this disease are the black spots which cover the silkworms, hence the name pébrine, first applied to the plague by M. de Quatrefages, and adopted by Pasteur. Pébrine also declares itself in the stunted and unequal growth of the worms, in the languor of their movements, in their fastidiousness as regards food, and in their premature death. The discovery of the inner workings of the epidemic may be thus traced. In 1849 Guerin Méneville noticed in the blood of certain silkworms vibratory corpuscles which he supposed to be endowed with independent life, and to which he gave a distinctive name. As regards the motion of the particles, Filippi proved him wrong; their motion was the well-known Brownian motion. But Filippi himself committed the error of supposing the corpuscles to be normal to the life of the insect. They are really the cause of its mortality—the form and substance of its disease. This was studied and well described by Cornalia; while Lebert and Frey subsequently found the corpuscles not only in the blood, but in all the tissues of the silkworm. Osimo, in 1857, discovered the corpuscles in the eggs, and on this observation Vittadiani founded, in 1859, a practical method of distinguishing healthy from diseased eggs. The test often proved fallacious, and it was never extensively applied.

The number of these corpuscles is sometimes enormous. They take possession of the intestinal canal, and spread thence throughout the body of the worm. They fill the silk cavities, the stricken insect often going through the motions of spinning without any material to answer to the act. Its organs, instead of being filled with the clear viscous liquid of the silk, are packed to distension by these corpuscles. On this feature of the plague Pasteur fixed his attention. He pursued it with the skill which appertains to his genius, and with the thoroughness that belongs to his character. The cycle of the silkworm's life is briefly this:—From the fertile egg comes the little worm, which grows, and after some time casts its skin. This process of moulting is repeated two or three times at subsequent intervals during the life of the insect. After the last moulting the worm climbs the brambles placed to receive it, and spins among them its cocoon. It passes thus into a chrysalis; the chrysalis becomes a moth, and the moth when liberated lays the eggs which form the starting-point of a new cycle. Now Pasteur proved that the plague-corpuscles might be incipient in the egg, and escape detection; they might also be germinal in the worm, and still baffle the microscope. But as the worm grows, the corpuscles grow also, becoming larger and more defined. In the aged chrysalis they are more pronounced than in the worm; while in the moth, if either the egg or the worm from which it comes should have been at all stricken, the corpuscles infallibly appear, offering no difficulty of detection. This was the first great point made out in 1865 by Pasteur. The Italian naturalists, as aforesaid, recommended the examination of the eggs before risking their incubation. Pasteur showed that both eggs and worms might be smitten and still pass muster, the culture of such eggs or such worms being sure to entail disaster. He made the moth his starting-point in seeking to regenerate the race.

And here is to be noted a point of immense practical importance. The worms issuing from the eggs of perfectly healthy moths may afterwards become themselves infected through contact with diseased worms, or through germs mixed with the dust of the rooms in which the worms are fed. But though the moths derived from the worms thus infected may be so charged with corpuscles as to be totally unable to produce eggs fit for incubation, still Pasteur shows that the worms themselves, in which the disease is not hereditary, never perish before spinning their cocoons. This, as I have said, is a point of capital importance; because it shows that the moth-test, if acted upon, even though the worms during their "education" should contract infection, secures, at all events, the next subsequent crop.

Pasteur made his first communication on this subject to the Academy of Sciences in September 1865. It raised a cloud of criticism. Here forsooth was a chemist rashly quitting his proper *métier* and presuming to lay down the law for the physician and biologist on a subject which was eminently theirs. "On trouva étrange que je fusse si peu au courant de la question; on m' opposa des travaux qui avaient paru depuis longtemps en Italie, dont les resultats montraient l'inutilité de mes efforts, et l'impossibilité d'arriver à un resultat pratique dans la direction que je m'étais engagé. Que mon ignorance fut grande au sujet des recherches sans nombre qui avaient paru depuis quinze années." Pasteur heard the buzz, but he continued his work. In choosing the eggs intended for incubation, the cultivators selected those produced in the successful "educations" of the year. But they could not understand the frequent and often disastrous failures of their selected eggs; for they did not know, and nobody prior to Pasteur was competent to tell them, that the finest cocoons may envelop doomed corpusculous moths. It was not, however, easy to make the cultivators accept new guidance. To strike their imagination and if possible determine their practice, Pasteur hit upon the expedient of prophecy. In 1865 he inspected at St. Hippolyte-du-Fort fourteen different parcels of eggs intended for incubation. Having examined a sufficient number of the moths which produced these eggs, he wrote out the prediction of what would occur in 1867, and placed the prophecy as a sealed letter in the hands of the Mayor of St. Hippolyte.

In 1867 the cultivators communicated to the mayor their results. The letter of Pasteur was then opened and read, and it was found that in twelve out of fourteen cases, there was absolute conformity between his prediction and the observed facts. Many of the educations had perished totally; the others had perished almost totally; and this was the prediction of Pasteur. In two out of the fourteen cases, instead of the prophesied destruction, half an average crop was obtained. Now, the parcels of eggs here referred to were considered healthy by their owners. They had been hatched and tended in the firm hope that the labour expended on them would prove remunerative. The application of the moth-test for a few minutes in 1866 would have saved the labour and averted the disappointment. Two additional parcels of eggs were at the same time submitted to Pasteur. He pronounced them healthy; and his words were verified by the production of an excellent crop. Other cases of prophecy still more remarkable, because more circumstantial, are recorded in the work before us.

These deadly corpuscles were found by Leydig in other insects than the silkworm moth. He considers them to belong to the class of psososperms founded by J. Müller. "This," says Pasteur, "is to regard the corpuscular organism as a kind of parasite, which propagates itself after the manner of parasites of its class." Pasteur subjected the development of the corpuscles to a searching examination. With admirable skill and completeness he also examined the various modes by which the plague is propagated. He obtained perfectly healthy worms from moths perfectly free from corpuscles, and selecting from them 10, 20, 30, 50, as the case might be, he introduced into the worms the corpusculous matter. It was first permitted to accompany the food. Let us take a single example out of many. Rubbing up a small corpusculous worm in water, he smeared the mixture over the mulberry leaves. Assuring himself that the leaves had been eaten, he watched the consequences from day to day. Side by side with the infected worms he reared their fellows, keeping them as much as possible out of the way of infection. These constituted his "lot temoign," his standard of comparison. On the 16th of April, 1868, he thus infected thirty worms. Up to the 23rd they remained quite well. On the 25th they seemed well, but on that day corpuscles were found in the intestines of two of the worms subjected to microscopic examination. The corpuscles begin to be formed in the tunic of the intestine. On the 27th, or eleven days after the infected repast, two fresh worms were examined, and not only was the intestinal canal found in each case invaded, but the silk organ itself was found charged with the corpuscles. On the 28th the twenty-six remaining worms were covered by the black spots of pébrine. On the 30th the difference of size between the infected and non-infected worms was very striking, the sick worms being not more than two-thirds of the size of the healthy ones. On the 2nd of May a worm which had just finished its fourth moulting was examined. Its whole body was so filled with corpuscles as to excite astonishment that it could live. The disease advanced, the worms died and were examined, and on the 11th of May only six out of the thirty remained. They were the strongest of the lot, but on being searched they also were found charged with corpuscles. Not one of the thirty worms had escaped; a single corpusculous meal had poisoned them all. The standard lot, on the contrary, spun their fine cocoons, and two only of their moths were found to contain any trace of corpuscles. These had doubtless been introduced during the rearing of the worms.

As his acquaintance with the subject increased, Pasteur's desire for precision augmented, and he finally gives the growing number of corpuscles seen in the field of his microscope from day to day. After a contagious repast the number of worms containing the parasite gradually augmented until finally it became cent. per cent. The number of corpuscles would at the same time rise from 0 to 1, to 10, to 100, and sometimes even to 1,000 or 1,500 for a single field of his microscope. He then varied the mode of infection. He inoculated healthy worms with the corpusculous matter, and watched the consequent growth of the disease. He showed how the worms inoculate each other by the infliction of visible wounds with their "crochets." In various cases he washed the "crochets," and found corpuscles in the water. He demonstrated the spread of infection by the simple association of healthy and diseased worms. In fact, the diseased worms sullied the leaves by their dejections, they also used their crochets, and spread infection in both ways. It was no hypothetical infected medium that killed the worms, but a definitely-organised and isolated thing. He examined the question of contagion at a distance, and demonstrated its existence. In fact, as might be expected from Pasteur's antecedents, the investigation was exhaustive, the skill and beauty of his manipulation finding fitting correlatives in the strength and clearness of his thought.

Pébrine was an enigma prior to the experiments of Pasteur. "Place," he says, "the most skilful educator, even the most expert microscopist, in presence of large educations which present the symptoms described in our experiments; his judgment will necessarily be erroneous if he confines himself to the knowledge which preceded my researches. The worms will not present to him the slightest spot of pébrine; the microscope will not reveal the existence of corpuscles; the mortality of the worms will be null or insignificant; and the cocoons leave nothing to be desired. Our observer would, therefore, conclude without hesitation that the eggs produced will be good for incubation. The truth is, on the contrary, that all the worms of these fine crops have been poisoned; that from the beginning they carried in them the germ of the malady; ready to multiply itself beyond measure in the chrysalides and the moths, thence to pass into the eggs and smite with sterility the next generation. And what is the first cause of the evil concealed under so deceitful an exterior? In our experiments we can, so to speak, touch it with our fingers. It is entirely the effect of a single corpusculous repast; an effect more or less prompt according to the epoch of life of the worm that has eaten the poisoned food."

It was work like this that I had in view when, in a lecture which has brought me much well-meant chastisement from a certain class of medical men, and much gratifying encouragement from a different class, I dwelt on the necessity of experiments of physical exactitude in testing medical theories. It is work like this which might be offered as a model to the physicians of England, many indeed of whom are pursuing with characteristic skill and energy the course marked out for them by this distinguished master. Prior to Pasteur, the most diverse and contradictory opinions were entertained as to the contagious character of pébrine; some stoutly affirmed it, others as stoutly denied it. But on one point all were agreed. "They believed in the existence of a deleterious medium, rendered epidemic by some occult and mysterious influence, to which was attributed the cause of the malady." Between such notions and the work of Pasteur, no physically-minded man will, I apprehend, hesitate in his choice.

Pasteur describes in detail his method of securing healthy eggs, which is nothing less than a mode of restoring to France her ancient prosperity in silk husbandry. And the justification of his work is to be found in the reports which reached him of the application, and the unparalleled success of his method, at the time he was putting his researches together for final publication. In France and Italy his method has been pursued with the most surprising results. It was an up-hill fight which led to this triumph, but it is consoling to think that even the stupidities of men may be converted into elements of growth and progress. Opposition stimulated Pasteur, and thus, without meaning it, did good service. "Ever," he says, "since the commencement of these researches, I have been exposed to the most obstinate and unjust contradictions; but I have made it a duty to leave no trace of these contests in this book." I have met with only a single allusion to the question of spontaneous generation in M. Pasteur's work. In reference to the advantage of rearing worms in an isolated island like Corsica, he says:—"Rien ne serait plus facile que d'éloigner, pour ainsi dire, d'une manière absolue la maladie des corpuscles. Il est au pouvoir de l'homme de faire disparaître de la surface du globe les maladies parasitaires, si, comme c'est ma conviction, la doctrine des générations spontanées est une chimère." It is much to be desired that some really competent person in England should rescue the public mind from the confusion now prevalent regarding this question.

M. Pasteur has investigated a second disease, called in France *flacherie*, which has co-existed with pébrine, but which is quite distinct from it. Enough, I trust, has been said to send the reader interested in these questions to the original volumes for further information. I report with deep regret the serious illness of M. Pasteur; an illness brought on by the labours of which I have tried to give some account. The letter which accompanied his volumes ends thus:—"Permettez-moi de terminer ces quelques lignes que je dois dicter, vaincu que je suis par la maladie, en vous faisant observer que vous rendiez service aux Colonies de la Grande Bretagne en repandant la connaissance de ce livre, et des principes que j'établis touchant la maladie des vers à soie. Beaucoup de ces colonies pourraient cultiver le mûrier avec succès, et en jetant les yeux sur mon ouvrage vous vous convaincrez aisement qu'il est facile aujourdhui, non seulement d'éloigner la maladie régnante, mais en outre de donner aux récoltes de la soie une prospérité qu'elles n'ont jamais eue."

Royal Institution, 30th June JOHN TYNDALL

WHAT IS ENERGY?

III.

THE CONSERVATION OF ENERGY

IT is well-known that certain organisms of the animal world do not confine themselves to one state of being or to one order of existence, and the most familiar instance of this roving habit of life is the caterpillar, which passes first into the chrysalis state, and after that into the butterfly. This habit is not, however, peculiar to the organic world, for energy delights in similar transmutations, and we have just seen how the eminently silent and invisible electrical current may occasionally be transmuted into the vivid, instantaneous, awe-inspiring flash of lightning. Nor is this element of change confined to our peculiar corner of the universe, but it extends itself to remote starry systems, in some of which there is a total extinction of luminosity for a while, to be succeeded by a most brilliant luminous outburst, presenting all the appearance of a world on fire.

We shall not enter here into great detail regarding the various changes of energy from one form into another; suffice it to say, that amid all these changes of form, and sometimes of quality, the element of *quantity* remains the same. Those of our readers who are mathematicians know what is meant by variable quantities, for instance, in the equation $x+y+z=A$, if x, y, & z are variable and A constant, you may change x into y and into z, and y into x and into z, and in fact perform any changes you choose upon the left hand side of your equation, *provided that* you keep their sum always constant and equal to A. It is precisely thus in the world of energy; and the invariability of the sum of all the energies of the universe forms the doctrine known as the "conservation of energy." This doctrine is nothing else than an intelligent and scientific denial of the chimera of perpetual motion.

Recognising the great importance of work, it was natural enough at an early stage of our knowledge that enthusiasts should endeavour to create energy or the power of doing work, that is to say, endeavour to construct a machine that should go on working for ever without needing to be supplied with fuel in any way, and accordingly inventors became possessed with the idea that some elaborate system of machinery would, no doubt, give us this grand desideratum, and men of science have been continually annoyed with these projects, until in a moment of inspiration they conceived the doctrine of the conservation of energy!

It flows from this doctrine that a machine is merely an instrument which is supplied with energy in one form, and which converts it into another and more convenient form according to the law of the machine.

We shall now proceed to trace the progress of energy through some of its most important transformations. To begin with that one to which we have already alluded, what becomes of the energy of a falling body after it strikes the earth? This question may be varied in a great number of ways. We may ask, for instance, what becomes of the energy of a railway train when it is stopped? what becomes of the energy of a hammer after it has struck the anvil? of a cannon ball after it has struck the target? and so on.

In all these varieties we see that either percussion or friction is at work; thus it is friction that stops a railway-train, and it is percussion that stops the motion of a falling stone or of a falling hammer, so that our question is in reality, what becomes of the energy of visible motion when it has been stopped by percussion or friction?

Rumford and Davy were the pioneers in replying to this important question. Rumford found that in the process of boring cannon the heat generated was sometimes so great as to boil water, and he supposed that work was changed into heat in the process of boring. Davy again melted two pieces of ice by causing them to rub against each other, and he likewise concluded that the work spent on this process had been converted into heat.

We see now why by hammering a coin on an anvil we can heat it very greatly, or why on a dark night the sparks are seen to fly out from the break-wheel which stops the motion of the railway train, or why by rubbing a metal button violently backwards and forwards against a piece of wood we can render it so hot as to scorch our hand, for in all these cases it is the energy of visible motion which is being converted into heat.

But although this was known nearly a century ago, it was reserved for Joule, an English philosopher of the present day, to point out the numerical relation subsisting between that species of energy which we call visible motion and that which we call heat.

The result of his numerous and laborious experiments was, that if a pound of water be dropped from a height of 772 feet under the influence of gravity, and if the velocity which it attains be suddenly stopped and converted into heat, this heat will be sufficient to raise the whole mass 1° Fahr. in temperature.

From this he concluded that when a pound of water is heated 1° Fahr. in temperature, an amount of molecular energy enters into the water which is equivalent to 772 foot-pounds, that is to say, to one pound raised 772 feet high against the influence of gravity, or allowed to fall 772 feet under the same influence.

He found again that if a pound of water were to fall twice this distance, or 1,544 feet under gravity, the velocity if stopped would raise its temperature 2° Fahr., and in fact that the rise of temperature under such circumstances is proportional to the height from which the pound of water is supposed to fall. By this means an exact relation is established between heat and work. Grove was the first to point out the probability of a connection between the various species of molecular energy; and the researches of Joule, Thomson, and others, have established these relations with numerical accuracy. No better example of the correlation of the various kinds of energy can be given than what takes place in a galvanic battery. Let us suppose that zinc is the metal used. Here the source of energy is the burning or chemical combination of the zinc with oxygen, &c., in order to form a salt of zinc. The source of energy is in fact much the same as when coal is burned; it is the energy produced by chemical combination. Now, as we have said, the zinc combines with the oxygen, and sulphate of zinc is produced, but the result of this combination does not at first exhibit itself in the form of heat, but rather in that of an electric current. No doubt a great portion of the energy of this electric current is ultimately spent in heat, but we may, if we choose, spend part in promoting chemical decomposition; for instance, we may decompose water. In this case part of the energy of the battery, derived as has been stated from the burning of the zinc, is spent in heat and part in decomposing the water, and hence we shall have less heat than if there were no water to decompose. But if when we have decomposed the water, we mix together the two gases hydrogen and oxygen which are the results of this decomposition, and explode them, we shall recover the precise deficiency of heat. Without the decomposition, let us say that the burning in the battery of a certain weight of zinc gives us heat equal to 100, but with the decomposition only 80, twenty units of energy have therefore become spent in the decomposition, but if we explode the mixture of gases procured from the decomposition we shall get back heat equal to 20, and thus make the whole result of the burning of the zinc 100 units of energy as before.

In like manner, if our electric battery is made to do work, thus forming a kind of engine, we shall have the heat produced by the current diminished by the exact equivalent of the mechanical effect which we have obtained from this engine.

There is nothing for nothing in the universe of energy.

B. STEWART

ROUMEGUERE ON FUNGI

Cryptogamie Illustrée, ou Histoire des Familles naturelles des Plantes Acotyledonées d'Europe. Famille des Champignons. Par Casimir Roumeguère. (Paris: J. B. Baillière. 1870.) 4to., pp. 164, figures 1700.

THE numerous introductions to the study of fungi, whether as articles of food, objects of physiological and botanical interest, or as the cause or aggravator of disease both in the animal and vegetable world, which are constantly issuing from the press, or whose speedy appearance is announced, are a certain proof of the daily increasing appreciation of the importance of a tribe which has often been considered as the mere offscourings of the earth, and worthy only of the title of "abominations." These publications of course are of very different value, and the glowing terms in which they are announced sometimes lead only to disappointment after an inconvenient outlay. As a striking instance, Valenti-Serini's work on doubtful or poisonous fungi of the neighbourhood of Turin may be mentioned, which was characterised in the "Annals of Natural History" as "this important work," its true characters being admirably exposed by Mr. Worthington Smith in "Seemann's Journal of Botany;" and unsparing as the remarks are, I consider that they are completely justified. It is simply a disgrace to the Academy under whose auspices it is published.*

This is not, however, the case with the publication whose title is given above; for though it is far from being free from faults, and the illustrations, though selected with considerable skill, are in some cases so coarse as almost to render them useless; still there is such a mass of information as may make it acceptable even to those who are well versed in the subject; and though unfortunately the several matters which come under review are seldom thoroughly worked out, yet they indicate the proper line of research and the best sources of information, in such a manner as to ensure it a hearty welcome. Every possible nook and corner of the mycological library seems to have been thoroughly ransacked, and that without any national prepossession such as occasionally detracts from the credit even of highly approved authorities. Indeed I was not a little surprised to find how diligently English works on the subject had been sifted, and not the less to recognise an allusion even to a sectional address at Norwich, though the remarks of its author have not been quite correctly interpreted.

It is not likely that there should be much novelty in so unpretentious a work, and perhaps it may be as well that no new views should be propounded, founded on imperfect data. It is a great matter to find no glaring errors likely to mislead; though here and there the drift of what has been written may have been misunderstood.

* I need only refer to Tab. 30 to justify this remark; and this instance is not a solitary one.

It is scarcely possible to overrate the importance of the study of fungi in any of the points of view which were enumerated. The Society of Arts and the Horticultural Society of London have very properly called attention to the great importance of fungi as articles of food, by encouraging inquiry or offering rewards for the best collections of esculent, doubtful, and poisonous species. The South Kensington Museum has also done its part. The very faithful set of drawings by Mr. W. G. Smith, exhibited on its walls, and the admirably prepared specimens by Mr. English—which retain their form perfectly, and, to a great extent, their proper colours—must eventually facilitate the due discrimination, which, as in the case of other vegetable esculents, must be matter of experience. It is quite lamentable to reflect what a vast quantity of wholesome food, and food which, from its chemical composition, may profitably replace the consumption of meat in the labourer's family, is utterly neglected, either from ignorance or prejudice.

In the second point of view as regards their physiological and botanical interest, it need only be mentioned with respect to the former, that, with the exception of the true algæ, the phenomena of impregnation cannot be studied more profitably than in those wonderful plants which occur on dead animals or decaying vegetables in water, and which are, undoubtedly, aquatic forms of various moulds, though in some respects they approach the algæ. Then as respects a biological point of view, the question of the origin of atmospheric germs, one of the most difficult of solution which can engage the attention of the microscopist, and which, in my opinion, has never been carried out so as to trace *accurately*, and free from all doubt, the development of the minute bodies which occur in fluids, whether of organic or inorganic composition, into higher forms; while the botanist will find a variety of form and structure which is scarcely surpassed in the higher branches of the science.

As regards the third point. If we consider fungi as the causes or aggravators of disease, it may be remarked, that, notwithstanding all that has been written on the subject, a great deal still remains to be discovered. The dreadful forms of Erysipelas and Hospital Gangrene, which occur so fatally in London hospitals, are, in all probability, dependent somehow on fungi, though the matter has not, hitherto, been found capable of proof, and whatever may be thought of Dr. Tyndall's views, the medical world cannot be too thankful to him for bringing the matter so prominently before the public.* The same also may be said with respect to Dr. Hallier's speculations, though, as I believe, they have been justly challenged both here and on the Continent.

A great deal is known about the influence of fungi in the production of disease in plants, but much more remains to be discovered. It may, eventually, prove that

* That the reproductive bodies of the larger fungi and moulds are widely carried about by the air, will be very evident to any one who has seen the clouds of spores which, in some cases, arise like smoke on the least agitation. Some years ago two flakes of snow were sent to me from Hampstead, prepared as microscopic objects, with the intention of exhibiting the organic matters which they might carry down with them in their course, and both, undoubtedly, contained perfect spores of fungi. Much more then may we expect that organisms which do not exceed a thirtieth or a fiftieth part of their diameter, and which are quite invisible except under very high magnifying powers, should be present everywhere to perform their functions as putrefactive ferments.

some of the more obscure cases of vegetable pathology depend on the minute fermentative bodies which, it should seem, play such an important part in animals. Certain it is that yeast globules and bacteria occur in vegetables where there is, apparently, no immediate communication with the atmosphere, or where, at least, it is as obscure as in some cases which engage the attention of the students of animal biology. Matters which have been long since ascertained to be facts, are still challenged by incompetent and uninstructed observers, and every one who can remove any portion of the prejudices which so materially retard the progress of science, will be doing a good work.

It is to be regretted that the quarto form of the work before us makes it very inconvenient for students, and it is to be hoped that a revised edition in octavo will secure a wider circulation. M. J. BERKELEY

AMERICAN NATURAL HISTORY

The American Naturalist. A popular illustrated Magazine of Natural History. Vols. 1, 2, and 3, from March 1867, to February 1870. (Salem, Mass. Peabody Academy of Sciences. London: Trübner.)

WE have several distinct reasons for bringing this useful periodical before the notice of our readers. In the first place, American Naturalists and writers on science generally complain, and not altogether without reason, that many of the most important works that issue from the Transatlantic press are much less known in this country than their merits deserve. Our personal experience leads us to believe that this complaint is well founded. We lately applied in vain to the Libraries of the Royal Society, of the University of Cambridge, and of a Scottish University, for the *American Naturalist* and for Clark's "Mind in Nature;" and we suspect that very few copies of such books as the following are to be found in English libraries, namely, a complete set of the works of Agassiz since he went to America, Binney's "Terrestrial Mollusks of the United States," Gould and Binney's "Report on the Invertebrata of Massachusetts," Tooney and Holmes's "Fossils of South Carolina," Samuel's "Birds of New England," Dekay's "Fishes and Reptiles of New York," the reports on the Pacific and other Railway Surveys, and the numerous contributions to science published during the last few years, by Marsh, Lea, Leidy, Hall, Wynam Baird, Coues, Packard, Scudder, Le Conte, Stimpson, Verrill, and a host of other writers.

Our second and chief reason for noticing the *American Naturalist*, is on account of its intrinsic value. The three volumes now completed contain a series of important original contributions, by Professors Bailey, Cope, Edwards, Hayden, Henrichs, Orton, and Verrill, Drs. Brewer, Cooper, Coues, Hunt, Joseph Jones, Le Conte, Lincecum, Norton, A. S. Packard, Perkins, Wood, &c. Messrs. Brigham, Dall, Hartt, Hyatt, Lockwood, Morse, Russell, Scudder, and many others. Botany, Geology, Physical Geography, and Zoology, receive their due shares of attention, and in a scientific point of view, the articles occupy an intermediate position between those which we find in "The Annals of Natural History" and Hardwicke's "Science Gossip." Each number contains (1) Original Articles; (2) Reviews of Works on Natural History; (3) Natural History Miscellany, including recent discoveries in Geology, Botany, Zoology, and Microscopy, and terminating with answers to correspondents; (4) Proceedings of Scientific Societies; the whole concluding with a List of Books Received, and a glossary of all the scientific terms occurring in the current number.

If our readers require any further evidence of the value of this periodical, we may add that after one year's independent existence, it has been issued as a publication of the Peabody Academy of Sciences. The trustees "consider it one of the legitimate objects of their trust to assist in the publication of the *Naturalist* by advancing funds sufficient to enable the editors to continue its publication in an improved condition."

Our third and last reason for now urging the claims of this periodical is, that being informed by Messrs. Trübner and Co., to whom we are indebted for the loan of these volumes, that new subscribers can for a time obtain the parts already published at a reduced rate; this seems to be an excellent opportunity for natural history clubs and libraries, both to obtain the back volumes and to order the future numbers. G. E. D.

OUR BOOK SHELF

The Science and Art of Arithmetic, for the Use of Schools. Part I., Integral. By A. Sonnenschein and H. A. Nesbitt, M.A., University College, London. (London: Whittaker and Co.)

FORTY years have elapsed since the appearance of Prof. De Morgan's "Elements of Arithmetic," at a time when perhaps few teachers, as they submitted the rules of the science to their pupils, cared to establish them upon reason and demonstration. The effect of this work was that a rational arithmetic began to be taught generally, and the mere committing of rules to memory took its due subordinate position in the course of instruction. Such a method of treatment will go far to develop and exercise the reasoning powers, and in the case of many pupils, there is hardly any other subject which can so well be made a groundwork for the exercise of the reasoning faculty. The book before us is avowedly drawn up in agreement with the principles of Mr. De Morgan's work, and the aim of the authors is to lead the student "to the discovery of the several rules by some path such as an original discoverer might have travelled." In this first part, which treats of Integral Arithmetic, we consider that they have carried out their principles successfully, and hope they will succeed as well with the remaining two parts, which are to embrace respectively Vulgar Fractions and Approximate Calculations. The rules enunciated are few and tersely given; there is a great store of illustration; elementary difficulties are well stated and honestly grappled with, and cleared up in a way that brings the subject to the level of the capacities of junior students; at the same time advanced as well as young teachers may gather much that is useful from the book. A reader who has carefully gone through the work, can hardly fail to master the early details of the science; if he fail, it will not be the fault of the authors. The subjects treated of are numeration, modes of computation, the so-called first four rules, contracted operations, scales of notation, and properties of numbers. Under this last division we have much valuable matter grouped under the several heads of Divisibility of Numbers, Casting out Nines, Resolution into Prime Factors, Greatest Common Measure, and Least Common Multiple. Throughout and at the end of the work occur numerous examples, very varied, all of which are carefully arranged, and many fully worked out in two or more ways. With this short analysis of the contents, we heartily commend the work to teachers generally, assuming, of course, that they will regulate their use of it in proportion to the requirements of age and ability of their pupils. The work is neatly got up, and we have detected hardly any errata. On page 51, ex. 2, we have "How many petals are there in 376 forget-me-nots?" Here there is an omission and a slight technical error. In botanical language the "forget-me-not" (*Myosotis*) is monopetalous, the number of *lobes* of the corolla being *five*. R. T.

Transactions of the Woolhope Naturalists' Field Club for 1869.

NO slight service has been rendered to the cause of natural science by the numerous naturalists' field clubs scattered here and there through the country, not only in the exploration of the natural products of their respective districts during their summer excursions, but in infusing a love of such pursuits among dwellers in the country. When the transactions of the year are published in so attractive a form as the volume before us, an additional benefit is conferred. The Woolhope Club is one which has been for some years favourably known, chiefly through the labours of one or two genuine naturalists among its members, as having furnished some real contributions to science by its researches among the pleasant woodland county of Herefordshire. The volume consists mainly of lively accounts of the various excursions made by the club during the summer of 1869, with lists of the rarities, zoological and botanical, met with, and reports of the papers read by its members. Among the more important of the latter we may mention Dr. Bull's history of "The Ancient Forest of Deerfold;" and papers on the occurrence and identification of rare birds in Herefordshire and Radnorshire, by Mr. Armitage, Rev. Clement Ley, and Mr. James W. Lloyd, including the peregrine falcon (*Falco peregrinus*), the hobby (*F. subbuteo*), the little merlin (*F. æsalon*), the grasshopper warbler (*Sylvia locustella*), the fire-crested wren (*Regulus ignicapillus*), and the great and little bittern (*Ardea stellaris* and *minuta*). Dr. Bull has given a celebrity to the Woolhope Club for its enthusiasm in favour of edible fungi; there are several papers on the subject, to which is appended Mr. W. G. Smith's *Clavis Agaricinorum*. Several very pretty illustrations ornament the book, among which may be mentioned photographs of some of the remarkable trees of Herefordshire, and a drawing of the famous Deerfold mistletoe-oak.

Contributions to Botany, Iconographic and Descriptive. By John Miers, F.R.S., F.L.S. Vol. II. (Williams and Norgate, 1869.)

THIS volume will be welcomed as an addition to Mr. Miers' contributions to systematic and structural botany, all of which possess the value of the labours of a careful and accurate observer, and one especially conversant practically with South American botany. We find in this volume carefully worked papers on the *Calyceraceæ*, a small order closely allied to *Compositæ*, on the carpological structure of *Bignoniaceæ*, on the history of the maté plant, and the different species of ilex used in the preparation of Paraguay tea, a monograph of the *Tricuspidarieæ*, an essay on the genus *Goupia*, one on the structure of *Heliotropiaceæ*, and a paper on the South American forms of *Ehretiaceæ*. But the most important article is one on the genus *Ephedra*, which Mr. Miers considers has been improperly placed among gymnosperms, maintaining that it has neither naked ovules nor naked seeds, and believing that it is more allied to *Urticaceæ* than to *Cycadaceæ* or *Coniferæ*, presenting a far higher order of structure than these latter orders. The third volume, devoted entirely to *Menispermaceæ*, is promised shortly. A. W. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Life in the Deep Sea

THE interest which attaches to every fact which bears upon the phenomena of life at great depths in the ocean, will, I hope, excuse me for especially directing the attention of the readers of NATURE to the "Beiträge zur Plastiden Theorie" (published in the fifth volume of the *Jenaische Zeitschrift*), with a separate copy of which my friend Prof. Haeckel has just favoured me.

The longest of the papers which constitute the "Beiträge," is devoted to a careful study of *Bathybius*, and the associated Coccoliths and Coccospheres; and it is a matter of great satisfaction to me that Prof. Haeckel has arrived at conclusions which, in all the main points, agree with my own respecting these remarkable organisms.

In a second paper Prof. Haeckel describes a wonderful Radiolarian, *Myxobrachia*, observed during his stay at the Canary Islands, the further study of which promises to throw a new light upon the nature of the Coccoliths and Coccospheres; inasmuch as bodies of the same character were found accumulated, and apparently developed, in masses at the extremities of certain prolongations of the protoplasm of *Myxobrachia*. As *Myxobrachia* attains a length of half an inch, and seems to be abundant in the harbour of Lanzerote, it is to be hoped that Prof. Haeckel, and other naturalists, will not long remain deprived of the opportunity of submitting it to re-examination.

Another important discovery made public in the "Beiträge," is the existence of starch in the well-known "yellow cells" of the *Radiolaria*. In connection with this fact, it is interesting to remark that all the *Radiolaria* are floating organisms, and, consequently, that they are fully exposed to the light of the sun.

T. H. HUXLEY

Jermyn Street, June 23

The "English Cyclopædia"

YOUR issue of June 2 contains a long letter from "Nemo," to which a short reply seems desirable. Most of his statements are incorrect, and, as an illustration of the trustworthiness of his facts, or supposed facts, allusion may be made to his remark that all he can find in the Cyclopædia about *Arvicolæ*, *Crociduræ*, *Crossopi*, *Hypudæi*, and *Sorices* is that *Hypudæus* is sometimes spelt *Hipudæus*; whereas all the species mentioned in the Close Time Report to which he refers are described or noticed in the Cyclopædia. The species of the sub-genera *Crocidura* and *Crossopus* are referred to under their generic heading *Sorex* in the article *Sorecidæ*, E. C. Some of the terms which he says are omitted properly belong to another division of the Cyclopædia. Thus *Acclimatisation* is noticed in the Arts and Sciences division, and something additional will probably be given in the supplement to that division. Again, *Deep Sea Dredging* had scarcely become a subject of general interest when the Natural History Supplement was being written, while the character of the principal results, and the probability of great additions to the subject, rendered it advisable, as was thought, to postpone its consideration until the Arts and Sciences division was supplemented. Some of the results are, however, given under *Alcyonaria* and elsewhere in the Natural History Supplement. As regards the other subjects said to be omitted, most of them do occur. *Darwinism* is noticed under *Species*, E. C. S., and also under *Palæontology*, *Crustacea*, &c. *Dimorphism in Animals* will be found under *Annelida*, *Hydrozoa*, *Generations* (*Alternation of*), *Crustacea*, &c., in E. C. S. *Eophyton* is noticed; and *Eozoon* is repeatedly mentioned, while its systematic position is described under *Foraminifera*. The article *Entophyta* in E. C. is devoted to the fungi connected with skin diseases, while those which are associated with ague and other diseases would be most appropriately noticed in connection with those diseases, which do not belong to the Natural History division. A whole column is given to *Hyalonema* under *Alcyonaria*, E. C. S., in which the contradictory views of Drs. Bowerbank, Gray, Wright, and others, are distinctly referred to. Something is said about *Hybridity* under *Primula*. *Ornithoscelida* is not in; the term was first proposed in a paper read Nov. 24, 1869, which paper was not published in the printed form until after the Supplement had been finished. The general views on *Protoplasm* are given under *Cells*, *Amœba*, and so far as it is identical with Sarcode under *Actinophrys*, *Sarcode*, and other headings in E.C.S. *Rhizocrinus* is referred to under *London Clay*, and its occurrence in the living state mentioned. *Aerolites*: The latest reference is said to be 1861, implying, as it seems, that none of the information is of later date. Falls subsequent to 1861 are mentioned, and many of the facts are of later date; as, for example, those relating to the Alais and Orgeuil aerolites; Sorby's conclusions published in 1865; and Daubrée's experiments, of which accounts were given in 1866 and 1868. The article itself appeared early in 1869. As to the bibliography, the principal authors are mentioned, and a list of the works consulted was written, but was inadvertently omitted. It is also said that the latest reference under *Alca* is 1861, but this again is not correct. The writer of the article *Annelida* was not aware of Claparède's strictures at the time he wrote it; but, after all, they do not seem to affect materially the general statements given in the supplementary volume. Prof. Huxley's views respecting the systematic position of *Archæopteryx* are given under *Birds*, E.C.S. No reference is made to *Protagon* under *Blood*, E.C.S., nor is mention made of Day's colour tests, nor Dr. Richardson's renunciation. Of the last, all that was found in the Reports of the British Association is the title of his paper, which runs thus, "On Coagulation of the Blood; a correction of the Ammonia theory," and of which nothing more is said. Hence it was thought best to say nothing about the matter. Of the long string of terms which "Nemo" has culled from Prof. Huxley's last address to the Geological Society, and which are said to be omitted, the majority are given in the Supplement. For instance, to cite one or two cases: *Anthracosaurus* occurs under *Carboniferous* system, E.C.S.; *Evolution* under *Palæontology*; *Microlestes* under *Rhætic Beds*; and so on. As to the other remarks which have not been specially alluded to, it may be admitted that some of the articles might have been improved. *Foraminifera* would have been all the better if Haeckel's volume had been consulted, only Haeckel's work could not be got. It would have been very desirable if subjects which have been omitted had been inserted, and if cross references had been more numerous; but there were restrictions as to space which rendered it necessary to make a selection. Thus, *Meloe* was inserted and *Sphegidæ* rejected, because there was no room for both. What a Cyclopædia ought or ought not to contain is an open question. It cannot give information upon everything; and probably very few persons not specially interested in the subject want to know about *Hyænictis* or *Ictitherium*. If regard was had to the theoretical view of the matter, and not to the cost and other practical drawbacks, a full account of all that has been done in the last sixteen years would fill several volumes as large as the Supplement to the "Natural History Division of the English Cyclopædia." I beg to sign myself

THE EDITOR

Cuckows' Eggs

WHAT is the drift of this discussion on the eggs of the cuckow? Is it "natural selection," "mimetic analogy," or what? Are we to understand that by some process of "natural selection" the European cuckow can change the appearance of her egg to that of the selected foster parent? or that one set of cuckows lays eggs like titlarks, let us say, another like hedge-sparrows, and so on; and always select each its particular nest in which to deposit its particular coloured egg?

If this is it, *cui bono?* Of course to deceive the foster parent. Is this needed? I doubt it. I do not think the foster parent cares what coloured eggs she sits on, so long as they are about the size of her own, so as not to inconvenience her.

Let us see what cuckows do in other countries, and let me select Africa as my field. If deception is necessary in one country, why not in another? Le Vaillant is so inaccurate that one must take all his statements *cum grano*, but he is right in some things, where, I suppose, he had no temptation to go wrong. He says of *Cuculus gularis* "that its egg is olive grey, dotted with red" (gris olivacé, piqueté de rouge), and that it is laid in the nests of—1, the Jean Frédérick (*Bessonornis phœnicurus*); 2, Coryphée (*Bradypterus coriphæus*); 3, Traquet-patre (*Pratincola pastor*); 4, Pie-grièche fiscal (*Lanius collaris*); and 5, Bacbakiri (*Telophonus bacbakiri*). Now the eggs of No. 1 are of a dirty white or buff ground, more or less spotted with pale rufous; 2, a lovely verditer, irregularly blotched with brown; 3, also verditer, indistinctly clouded with brown; 4, pale grey, blotched at the obtuse end with greenish and reddish spots; 5, light blue, profusely spotted with brown.

Cuculus solitarius, he says, lays its pink egg, dotted with clear brown spots, in the nests of—1, *Bessonornis phœnicurus*; 2, *Bradypterus coriphæus*; 3, Le Capocier (*Drymoica capensis*); 4, Le Réclameur (*Bessonornis vociferans*); and 5, Le fauvette à tête rousse. The eggs of the last two I do not know; those of the first two are described above; those of No. 3 are blue, with brown blotches.

Oxylophus edolius and *O. melanoleucus* he confounds together, but it matters little, as the eggs are alike—pure white—and deposited in nests of—1, Bergeronette brun (*Motacilla capensis*); 2, *B. coryphæus*; 3, Gobemouche mantelé (*Tchitrea cyanomelas*); and others, whose eggs I do not know. Of 1, the eggs are greyish white, or rather nankin, minutely freckled with brown; of 3, they are cream-coloured, profusely spotted with red, brown, and purple spots, in a band at the obtuse end. One of my correspondents finds eggs of *O. edolius* in the nests of *Pycnonotus capensis*, whose eggs are rather deep lake, profusely spotted with dark markings! They also, I know, lay in the nests of *Pycnonotus nigricans*—eggs as of the last. I found Mud-birds (*Malacircus bengalensis*) in Ceylon, feeding a young *O. melanoleucos*, and their eggs are of a uniform deep verditer.

Chalcites auratus lays white eggs also, and some of my correspondents have sent what I *believe* to be their eggs taken from the nests of *Hyphantornis capitalis*, whose eggs are green, profusely speckled with brown, and dark salmon-colour profusely speckled and spotted with dark brown and black.

Now, will any one say, after comparing these different cuckows' eggs with those of the nests in which they have been found, that there is any attempt at imitation, and if not in so many cases, why in that of *C. canorus?*

I used to think and so I wrote ("Birds of South Africa," p. 252) that the eggs of parasitic birds "usually resembled those of the foster parent." This was my idea founded on statements concerning the European cuckow taken from books; but a valued correspondent, taking exception to my position, set me to investigate the subject for myself, and to collect together and analyse my own observations and those of my collectors in this country. She writes as follows:—"The eggs of all the cuckows that I have met with in this country (South Africa) are white, and moreover they are nearly always larger than the eggs of the bird in whose nest they are deposited. With regard to distinguishing eggs, birds of all kinds are exceedingly short-sighted. We used to amuse ourselves by changing the eggs in all the birds' nests we knew of. The owners seldom left them, but took to the strange eggs; and unless their habits were remarkably different, they would blindly rear each other's young, just as they do the young cuckows. It is not necessary, therefore, for nature to make this provision. My second son once filled a Cape canary's nest with so many eggs that when the young were hatched they were more than the poor birds could manage to provide for, and having repented of his mischief, he was obliged to help them bring up their young." (Cf. *Ibis*, 1868, p. 247.)

Since this was written, I have had the advantage of visiting my correspondent, who is well known throughout this colony for her talents, love of natural history, and powers of observation. We often discussed this subject. She and her sons assured me they never cared to select eggs like those of the foster-parent, but simply eggs of those whose food they knew to be similar. They said the confusion they caused was most amusing, but only after the young were hatched. The eggs were incubated without any demur on the part of the foster-mother. After this, surely I may ask *cui bono* the *C. canorus* imitation?

E. L. LAYARD

Cape Town, Cape of Good Hope, May 3

The Chromatic Octave

I HAVE to thank "M. A." for his letter in NATURE of June 9th, suggesting that the wave-frequency to which the complementary of any tint is due, may be, not the geometric mean between that tint and its octave, as I suggested in a letter in NATURE of 28th April, but the harmonic mean. I can scarcely doubt that there must be some simple arithmetical relation between the wave frequencies of any tint and its complementary, but I see no *à priori* reason for expecting to find one such law rather than another; we must try which assumed law will most nearly coincide with fact, and the hypothesis of a harmonic mean does so coincide pretty nearly. The following table (see my previous letter) gives the ratios of the wave-frequencies of red, orange, and yellow as observed, of their complementaries as observed, and of the same as calculated on the hypothesis of the harmonic mean:—

			Observed.	*Calculated.*
Red	36·40	Bluish Green	48·30	48·53
Orange	39·80	Blue	51·80	53·07
Yellow	41·40	Indigo	54·70	55·20

The discrepancies between the observed and the calculated outlines are much less on this hypothesis than in that of the geometric mean; but they are on the same side, and, as I explained in my former letter, I think it likely they may be due to the solar spectrum not being of a truly white colour, owing to the absorption lines toward the violet end. They are on the side which this way of accounting for the fact requires. It would be desirable to make a set of comparative experiments with solar light and the electric light, as I suggested before, in order to clear up this question.

Old Forge, Dunmurry, Co. Antrim, June 13 — JOSEPH JOHN MURPHY

On the reported Current in the Suez Canal

IT is stated on excellent authority that a constant current runs through the central portion of the Suez Canal, from the side of the Mediterranean to that of the Red Sea, and a good deal of surprise has been excited by this apparently anomalous phenomenon. A little consideration will, however, suffice to establish a theory, that constant currents are almost necessary conditions of inter-oceanic canals, and that their absence, not their presence, would be contrary to just expectation. My reason is based on the improbability that a long canal, A B, could be constructed across strata that are almost necessarily inclined in one direction more than another, which should not resist the flow of tidal water from, say, A towards B, more than from B towards A. Wherever this differential aspect is established, a quasi-valvular action is called into existence, and a current along the middle of the canal, in a constant direction, is the necessary consequence.

Let A B be the canal, and *a b* the extreme limits of tidal influence. After each successive rise and fall of the tide on either side, more water will have passed from A towards *a*, than

will have returned from the side of *a* to A, and more water will be able to travel from the side of *b* to B, than can get up the canal from B towards *b*. Consequently there will be a constant current in the ultra-tidal portion, *a b*, of the canal, from the side of A to that of B.

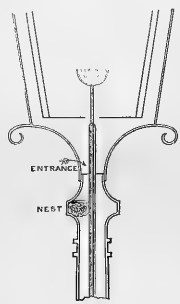
I have made some inquiries, but am unable to learn what notchings, indentations, or sweeps of the sides of a canal, would exercise the greatest differential effect, at low velocities, of the kind of which I am speaking. However, I hear it is a fact well known to sailors, that a spar cannot be towed behind a boat, unless with the greatest difficulty, if its small end be foremost, whereas, it is moved easily enough if its thick end be in front. I argue from this that if a number of spars were moored against the sides of the canal, with their large ends towards A, much less strain would be exerted on the ropes by which they were secured when the current ran from A to B, than when it ran from B to A, and consequently that the current itself would be much less resisted in the former than in the latter case. A succession of very long notches in the sides of the canal would produce identically the same effect, and might call into existence a considerable aggregate of differential resistances. I constructed a model for the purpose of experiment, but found it much too small to give satisfactory results; nevertheless, I will describe it, in hopes it may save trouble to others in designing a suitable arrangement, for the same purpose, on a larger scale. A notched trough was cut, running up and down in long zigzags, and its two ends were brought together into the same reservoir. By alternately allowing water to run into the reservoir, and then drawing it off, the effect of the rise and fall of the tide was simulated. I scattered lycopodium on the water, in the middle part of the channel, to show the direction of the current.

I venture to suggest to those engineers who are connected with inter-oceanic canals, the importance of making experiments on this problem, because it may prove to be quite within their means to produce and to regulate a current within such canals, in the direction and of a velocity most convenient to keep its bed clean and serviceable. FRANCIS GALTON

Birds' Nests

BIRDS, though almost always adapting themselves to circumstances in the use of materials, are frequently, even in the country, very eccentric in their choice of a place for their nests. I have seen a blackcap's nest built of the ordinary materials, in an open flower-pot standing on the top of a garden wall. Apparently there was no possible reason for this, there being plenty of hedges and banks hard by. But in the neighbourhood of London birds may be allowed an excuse for their eccentricities.

In a quiet street in one of the southern suburbs there is now a pair of tom-tits who have taken possession of a cast-iron lamp pillar, wherein they have built their nest and reared their young for two or three years past. It is curious to think what business they could have had there, to have found out that it was a suitable residence. The nest is placed in the bulb or swelling out of the column, just below the lamp, and the birds creep through



the space between the gas-pipe and the iron rim at the top of the column. This space is not three-quarters of an inch in width. The nest is on one side of the pipe, and cannot be more than two inches across. The lamp is lighted every evening, and on one occasion the pillar was actually taken down for some repairs with the nest inside, containing seven or eight eggs, which were, I believe, destroyed; but the birds, concluding I suppose that this was not done with *malice prepense*, but that it was only a necessary domestic difficulty, wisely returned to their home, and continued to occupy the lamp pillar for the remainder of the season, rearing another brood that same year. The accompanying sketch shows the position of the nest. Under the eaves of the adjacent house, two pairs of house martins have built this year. They came flitting about on the 1st June, and the weather being very dry, and no mud to be got at, the "gudeman" of the house kept a little spot in the road well watered, from whence the birds obtained all their necessary mud. The sparrows would plump down after the martins, thinking there was food there, and stand watching the martins at this little wet spot, and wondering apparently why they kept on flying down here, where there was no grub to be got. They tried hard to obtain possession of the martin's nests when half built, but were constantly driven away by the gentleman of the house, and now the nests are finished, and the entrance too small for the sparrows to get in; so that they dwell in comparative security. Two of these martins seem to be the sole occupants of their nest, but the other nest appears to be visited, at least, if not owned by, more than one pair of birds, three or four birds being often seen there at one time. I have often noticed this in the country, but never saw any remarks about it recorded by any one.

C. W. W.

"Other Worlds than Ours"

MR. PRITCHARD, in reviewing my book on the plurality of worlds, says that I represent Mr. Lockyer as impeding the progress of science; on the contrary, I regard Mr. Lockyer as one who has, in a most marked and important manner, advanced the cause of science, and I confidently anticipate admirable work from him in the future. It is surely not wrong in me to express openly my opinion that Mr. Lockyer's theory of the corona * is erroneous, or that (precisely *because* an expedition will set forth next December to observe the corona) the arguments against it cannot advantageously be neglected. But to assert, in the face of the fact that I give *reasons* for objecting to it, that I object "simply because" Mr. Lockyer's "opinions do not square with mine," is to make a misstatement which one can scarcely imagine to result from mere negligence.

Mr. Pritchard quotes my words, "I have very little doubt that Uranus has at least eight satellites," and asks how I venture to set my *opinion* in antagonism with Mr. Lassell's *observations.* How strange he should not have quoted the next sentence also, which would have shown that, as a matter of fact, I set the *observations* of Sir W. Herschel against the *opinion* of the esteemed and eminent astronomer who is President of the Astronomical Society. One can scarcely imagine this omission to result from mere negligence.

Mr. Pritchard makes me say, in the face of Sir William Thomson's abandonment of the theory, that the sun's heat is derived from a battery of meteors, "*I am quite certain* . . . that at least an important proportion of the sun's heat" is so supplied. And he adds, "We may fairly ask whence has Mr. Proctor this *certain* knowledge?" How strange that he should have omitted the remainder of the sentence! What I actually wrote was, "I am quite certain there is no flaw in the evidence I have adduced from the laws of probability, and that we are bound to accept as a legitimate conclusion from that evidence the theory that at least an important proportion," &c. This reference to the evidence, and to the laws of probability, would have spoiled Professor Pritchard's reasoning. Here, again, we can scarcely imagine that the omission results from mere negligence.

There are other points of the same kind in Mr. Pritchard's review, which space prevents my dwelling on. Suffice it to say, that *every criticism it contains* is vitiated by misstatements or omissions, which one can scarcely imagine to result from mere negligence.

RICHARD A. PROCTOR

Pinkish Colour of the Sun

IN reference to the "pinkish colour of the sun," noticed by several of your correspondents, it may interest them to learn that in one of the last numbers of *Cosmos* an account is given of this very same appearance, observed on the 23rd of May, at Rohrbach, on the Moselle, by a M. Hamant. He states, "that up to about two o'clock the day had been very warm, without a breath of wind. At twenty minutes past two the horizon became charged with mist, and a storm seemed imminent. About three the sun lost its brilliancy, assumed a pale yellow hue, and might have been taken for the moon had it not been for its diameter. The mist now began to rise, a north-west wind began to blow very hard; at half-past four the sun became rose-coloured, and at a quarter-past five it turned scarlet."

The exact coincidence to be observed between this account and that given by Mr. A. S. Herschel (NATURE, June 16), is worthy of notice.

Mr. Herschel similarly observed this "very unusual pinkish colour," between five o'clock on the 23rd of May, at Hawkhurst in Kent. He notices the "thick haze of apparently low cirrostratus or, perhaps, rain cloud." This phenomenon is so rare that it is mentioned in old chronicles as a sign of Divine wrath. Of late years the most remarkable case was that observed in South America by M. Emdiais, alluded to in *Cosmos.*

It is, however, especially to be noticed that whereas the two accounts referred to above state distinctly that the phenomenon occurred on "the 23rd of May," your other corespondents state that it also occurred "on Sunday, the 22nd," at about the same time, five o'clock. (See NATURE, May 26, June 2).

It is most remarkable that such a rare phenomenon should have occurred on two consecutive days; visible on the first day at Dunmurry and Dublin and Tynemouth, and on the second in Kent and Gloucestershire and on the Moselle. The hazy nature of the atmosphere on both days seems to have been permanent, and is, without doubt, the cause of the phenomenon.

JOHN P. EARWAKER

Merton College, Oxford, June 28

Monographs of M. Michel Chasles

A FEW years ago I read ten or a dozen papers of a masterly history of geometry by M. Chasles. It was in French, in some quarto transactions of a learned society.

I am desirous of recovering the title and reference, and ask for assistance in the columns of NATURE.

No such a paper as that I refer to is in the Royal Society's admirable catalogue.

Was Chasles' *Aperçu historique* contributed to a learned society? It was published at Brussels, in 1837, but it is scarce, and I have not seen a copy.

C. M. INGLEBY

Ilford, E., June 11

Geographical Prizes

IN reading the report in a recent number on the Prize Medals of the Royal Geographical Society, doubtless many of your readers will have thought very reasonable the wish of Sir Roderick Murchison, "that Eton, Harrow, and Rugby, and other great schools might in future years send candidates for these medals."

It may be well therefore if I explain very briefly the grounds on which the masters of Rugby were almost unanimous in wishing to decline the invitation of the Royal Geographical Society.

The examination is in fact a competition between schools in a subordinate branch of education. Hence the advantage lies not with the best school, but with the one which allows the greatest liberty of choice of special studies. A school like Rugby, whose curriculum, though not narrow, is strongly defined, is at a positive disadvantage in such a competition with a school whose general curriculum is narrower, but its organisation looser; one which allows free specialisation, and prepares for particular examinations. We cannot feel that the school that wins is likely to be the best school.

Further, we agreed that the proposal would not really encourage the study of geography in the school, but would attract only a few individuals. There are in every school certain accumulative prize-acquisitive boys who would learn Chinese or Crystallography or Indian Finance if a prize were offered for such subjects; and it would be these boys who would compete for the geographical medals—such boys would gain little by learning, and the school would gain nothing.

On these and some other grounds the proposal was declined by the Rugby masters; and when it is recollected that it was when Dr. Temple was head-master, most of your readers will be sure that it was not from indifference to real progress, nor from stupidity, nor from fear of novelty, nor to avoid honourable competition with other schools, that we did so decline it.

Rugby, June 13 J. M. WILSON

* For an accurate though incomplete statement of Dr. Frankland's and Mr. Lockyer's theory of the Corona, we refer our readers to the first number of NATURE Many of them will not be surprised to find that it is *not* what Mr. Proctor states it to be. Dr. Frankland and Mr. Lockyer, from their laboratory experiments, have shown that the pressure at the base of the chromosphere is small, and they have therefore stated that it is scarcely possible that a very extensive atmosphere lies outside the chromosphere. Mr. Lockyer has shown, moreover, that the height of the chromosphere as seen by the new method probably falls far short of its real height as seen during an eclipse as it was seen by Dr. Gould. A reference to the same number of this journal will also show that Mr. Proctor has misrepresented Dr. Gould's statements, which endorse the idea put forward by Dr. Frankland and Mr. Lockyer. Dr. Gould has expressly stated "that there were many phenomena which would almost lead to the belief that it was an atmospheric rather than a cosmical phenomenon." This is an opinion held by Faye and other distinguished astronomers, and Mr. Lockyer has simply shown that should this turn out to be the case, the continuous spectrum observed may be explained. Astronomers did not require Mr. Proctor to tell them what he has recently been enforcing; but, more modest than he, they have been waiting for facts, and Mr. Proctor surely is old enough to see that by attempting to evolve the secrets o the universe, about which the workers speak doubtfully, out of the depths of his moral consciousness, he simply makes himself ridiculous, and spoil much of the good work he is doing in popularising the science.—ED.

NOTES

It is with very great pleasure we state that an English friend has just received a letter from Baron Liebig, in his own handwriting, dated Munich, the 1st inst. Although still very weak, he is now able to get into his garden for some little time daily. If he continues to progress as he is now doing he proposes going to Switzerland, to the Engadine, in three weeks, where it is hoped he may soon become quite strong.

Death has been busy lately among the more eminent members of the medical profession. We have this week to record the decease of Sir James Clark, Bart., M.D., F.R.S., chief physician to Her Majesty, which took place on the 29th ult., in the 82nd year of his age. Sir James had held the appointment of physician to several members of the Royal Family, was the author of several works on climate and on consumption, and was a member of the Senate of the University of London. He attended the poet, John Keats, during his last illness at Rome in 1821.

The Radcliffe Observer at Oxford, the Rev. R. Main, has just issued a second Radcliffe catalogue, containing 2,386 stars deduced from observations extending from 1854 to 1861. The well-known care taken by the observer in the reduction of observations, and the admirable instruments in the observatory under his charge, render this volume a very valuable addition to our astronomical libraries.

We have received from Mr. Van Voorst a most beautiful book, which every scientific man in this and other lands will be glad to possess. Its contents are sixteen portraits of eminent scientific men, photographed by Dr. Wallich, the well-known naturalist, and with a skill quite unsurpassed, so far as we know, in any previous attempts. Dr. Wallich states that his aim has been to supply likenesses that shall be both characteristic, free from some errors in taste but too frequently manifest in photographic portraiture, and trustworthy as records of individual feature and expression; and in all these points the likenesses are admirable. We could hope that the success of this volume would be such as to induce Dr. Wallich to enlarge his scientific series. We may add that the present portraits are those of General Sir E. Sabine, Sir R. I. Murchison, Profs. Owen, Bentham, Huxley, Tyndall, Stokes, Ramsay, and Williamson, Dr. Hooker, Sir C. Lyell, Sir W. Logan, Viscount Walden, the Rev. G. B. Reade, and Messrs. Lassell and Prestwich.

At the meeting of the French Academy of Sciences on the 27th ult. the section of Anatomy and Zoology presented the following list of candidates for the place of correspondent vacant by the death of Prof. Carus. In the first rank, Prof. Brandt of St. Petersburg; in the second rank and in alphabetical order MM. Bischoff (Munich), Darwin, Huxley, Hyrtl (Vienna), Leuckart (Leipzig), Lovén (Stockholm), Steenstrup (Copenhagen), and Vogt (Geneva). The election was to take place on Monday last.

A Committee of the House of Commons having reported in favour of the establishment of the national natural history museums on the Thames Embankment, Mr. Beresford Hope inquired, on Monday evening last, whether it was the intention of the Government to carry out this recommendation. The Chancellor of the Exchequer replied that such an appropriation would deprive the ratepayers of a large portion of the land which was to be laid out as public gardens, and therefore Her Majesty's Government were not prepared to ask Parliament to carry out the recommendations of the committee. We are very glad to hear that the Government does intend to insist on so large a portion of the Embankment being rescued from the builders. But have the interests of the ratepayers been so jealously guarded when other claims than those of science were placed in competition with them?

At the D.Sc. examination of the University of London, just held, candidates passed in the following branches:—Branch I.—Mathematics, John Hopkinson, Trinity College, Cambridge, and Owens. Branch IV.—Inorganic and Organic Chemistry or Mineralogy: James Bottomley, B.A., Owens College; David Watson, Royal School of Mines; John Watts, private study. Branch V.—Organic and Inorganic Chemistry: James Campbell Brown, Royal College of Chemistry and private study; Charles Romley Alder Wright, Owens College. Branch VIII.—Physical Optics; Heat; Acoustics: John Hopkinson, Trinity College, Cambridge, and Owens. The D.Sc. examination is intended by the University to be the highest possible test of proficiency in some one particular branch of science, and can be taken in any one of sixteen different branches. It is encouraging to find that not only have a larger number passed the examination than in any previous year, but that it has been taken in branches in which there has hitherto been no candidate; and moreover, that one gentleman has passed in two distinct branches. Owens College may well be proud of the number of successful candidates it has sent up.

At a meeting of the Council of the Royal School of Mines, held on Saturday, July 2nd, the following awards were made:—Two Royal Scholarships of 15*l.* each for first year's students, to W. H. Greenwood and F. C. Milford; H.R.H. the Duke of Cornwall's Scholarship to P. C. Gillchrist; the Royal Scholarship of 25*l.* to R. R. Atkinson; the De la Beche medal and prize of books to W. Gowland; and the Director's medal and prize of books to P. C. Gillchrist. The Edward Forbes medal and prize of books were not competed for this year. The title of Associate of the Royal School of Mines was conferred upon the undermentioned gentlemen:—In the Division of Mining—Messrs. William Gowland, Archibald Liversidge, and H. J. Renwicke. In the Division of Metallurgy—William Gowland, Dillon, Liversidge, A. W. Bickerton, F. W. Bayley, and T. Jones. In the Division of Geology—W. Johnson Sollas.

The examination for women above eighteen years of age, conducted by the University of Cambridge, has been proceeding during the present week, at three centres, London, Manchester, and Rugby. The number of entries last year was thirty-six, this year they have increased to eighty-four, and we are glad to observe that there are candidates in several branches of natural science which were not touched last year, among them Zoology, Botany, and some of the higher branches of Mathematics.

The *Athenæum* states that the University of Vienna has decided to admit women to all the advantages of its medical school, and that two French students have already availed themselves of the privilege.

A telegram from Athens speaks of an earthquake which has taken place in the island of Santorin. The town is a mass of ruins, and several small islets have been submerged.

Dr. Petermann announces in the *Cologne Gazette* that the *Warag*, one of the best steamers of the Russian fleet, left Helsingfors on the 7th of June, under the command of Captain Krämer, on a scientific expedition to Nova Zembla and Spitzbergen. The *Warag* is to proceed *viâ* Bergen, Hammerfest, and Vardö. It will be accompanied by two or three steamers from Archangel, with the Grand-duke Alexis and several eminent scientific men on board. This expedition is to be the precursor to a more important one which is being prepared by the Russian Geographical Society, and will proceed next year to the North Pole.

THE *Moniteur Scientifique* for June 15th, edited by Dr. Quesneville, contains three articles taken from our columns, the source of two only of which are acknowledged—"The Science of Explosives applied to the Art of War," and "Scientific Experimental Research," by Mr. George Gore (or, as the French rendering has it, Mr. George Core). The third, Dr. Lankester's article on "The Extract of Meat," appears as an original paper.

THE annual meeting of the Royal Archæological Institute of Great Britain and Ireland will be held at Leicester at the end of the present month. The meeting is under the patronage of Her Majesty and the Prince of Wales, and Lord Talbot de Malahide, F.S.A., is the president of the year. Leicester itself possesses many features of archæological interest. These include its mediæval churches, the Norman hall of its ancient castle, the "newarke" of the castle, numerous Roman pavements, one being *in situ*, and the noted mass of Roman masonry called the "Jewry Wall," the ancient hall of Corpus Christi Guild, the Chapel of Trinity Hospital, and the now forsaken hospital of William de Wyggelston. With these may be mentioned the site of Leicester Abbey, surrounded by its original walls, where Cardinal Wolsey breathed his last. Among the features of interest which will be included in the proceedings of the week will be the inspection of Kirby, Muxloe Castle, Ashby-de-la-Zouch Castle, the ruins in Bradgate Park, so intimately connected with Lady Jane Grey, the remains of Ulverscroft Priory, the curious stained glass of Woodhouse Chapel, the house known as Latimer's at Thurcaston, the noble church at Melton Mowbray, the fine Norman hall at Oakham, the interesting hall and church at Exton, and the remarkable encampment at Barrow-on-the-Hill. A temporary museum will be established as usual during the week, and will include a collection of portraits of worthies connected with the counties of Leicester and Rutland.

THE general monthly meeting of the Royal Institution of Great Britain was held on Monday, July 4, Sir R. I. Murchison, K.C.B., F.R.S., in the chair. The secretary announced the receipt of 2,000*l.*, a legacy from the late Mr. Alfred Davis, M.R.I., for the promotion of experimental researches.

BABOO RADANAUTH SICKDAR, for many years chief computer to the Trigonometrical Survey of India, at one time in charge of the Calcutta Observatory, and a mathematician of some attainments, died in May last at Calcutta.

THE Governor of Madras has presented to the Horticultural Gardens there four young date palms, which he has procured from Egypt.

THERE has been for some time in India a discussion on the subject of pearl oyster banks on the Tinnevelly coast. Those who have maintained the existence of the oysters are now fully confirmed by the discovery of young oysters in abundance. One bank is four miles in length and about two in width, and in another two years will yield a good harvest of pearls.

A MINERAL spring has been discovered in the Gheiveh hills, near Broussa, in Asia Minor.

THE foundation-stone of a new Observatory was laid at Port Louis, Mauritius, on the 30th of May, by H.R.H. the Duke of Edinburgh, to be called the Royal Alfred Observatory. In addition to astronomical observations, it is intended to make the Observatory a centre for researches for the advancement of meteorology and terrestrial magnetism. Full particulars of the ceremony will be found in another column. In Sir Henry Barkly, who is about to leave Mauritius for the Cape, the colony will lose a Governor who has always had at heart the promotion of everything connected with physical and natural science.

THE temperature of the Cranial Cavity has lately been investigated by Mendel, of Pankow near Berlin. He states that Fick had already found the normal temperature of the cranial cavity to be lower than that of the body generally. Jacobson and Bernhardt had similarly noticed the inferior temperature of the blood arriving at the heart by the superior vena cava, and the depression produced by it in the right cavities. M. Mendel corroborates these results, and finds constantly that in health there is a difference of from seven-tenths to one degree centigrade between the temperature of the cranial cavity and the rectum in the rabbit, and that in the dog the difference is almost as well marked. Duméril and Demarquay have shown that the temperature of the body is lowered by the action of chloroform. Bouisson arrived at the same results, as have also Sulzynski and Scheinnesson; the latter experimenting upon man. The difference observed by Mendel between the cranial and rectal temperature is much more pronounced when the animal is under the influence of chloroform than when in health. Chloroform lowers the temperature generally, but especially that of the cranial cavity. The effects produced by chloral on the general temperature have been already studied by Demarquay. This author has found that the temperature of the body falls several tenths of a degree. Mendel arrives at the same results in regard to the temperature of the cranial cavity, except that it falls to a still greater degree than the general temperature. Dequix, Dupuy, Leuret, and Gscheidlen have found that after a medicinal dose of morphia the temperature of the body rises, though when given in a poisonous dose it falls. Mendel again arrives at a similar conclusion, viz., that the depression of temperature is more rapid and more marked in the cranial cavity than in the rest of the body. In poisoning by alcohol the temperature of the cranial cavity rises to such a point that it surpasses the temperature of the rectum.

THE hygrometrical balance compensating for the severe drought under which Western Europe has been suffering for the past three months, appears to have been maintained by an unusually heavy rainfall, reported at the Paris Observatory as having been experienced in Egypt, Lybia, Asia Minor, and Russia. The Paris correspondent of the *Daily News* states that two theories to account for the drought have been broached among French *savants*. One of these attributes to the cutting of the Suez Canal a displacement of the atmospheric currents which regulate the weather of Europe and North Africa! The other theory is grounded on the statement that the spring rains in France are caused by atmospheric commotions resulting from the breaking up of the ice fields to the east of Newfoundland, and that for this reason a long and severe winter in the Arctic regions is followed by a dry spring and severe and destructive summer storms in Central and Western Europe. The author of this theory suggests that science should be employed in breaking up the ice-fields at the end of a severe winter by nitro-glycerine and picrate of potassium. This scheme is advocated in the pages of *Cosmos*, by a civil engineer of the name of V. Prou, who proposes forming an "Insurance Company against Drought," for the purpose of assisting nature in the disintegration of the Polar ice, and thus forming a more equable temperature.

WE are glad to see that the *savants* of Haarlem continue to bring out the "Archives du Musée Teyler." The new fasciculus just received in London contains the following papers:—"Sur les insectes fossiles du calcaire lithographique de la Bavière," by H. Weyenbergh, jun.; "Description d'un crinoïde et d'un poisson du système heersien," by Dr. T. C. Winkler, and a further instalment "Sur la Dispersion," by Van der Willigen. The plates representing the fossil insects and the crinoïd are beautifully executed, and for purposes of study are almost as good as the originals. By way of explanation we mention that the "Système Heersien," takes its name from a district in the province of Limburg, in which two villages, Upper and Lower Heers, are situate.

FACTS AND REASONNIGS CONCERNING THE HETEROGENOUS EVOLUTION OF LIVING THINGS *

II.

A.—*Experiments in which the fluids employed were raised to a temperature of* 100° *C. for from* 10 *to* 20 *minutes before the flask was hermetically sealed.*

a. Fluids employed being filtered infusions containing organic matter in solution.

1. *Infusions either slightly alkaline or neutral.*

Experiment 1.—A flask containing filtered beef-juice *in vacuo*, which had been hermetically sealed twelve days previously, after the fluid had been boiled for 15 minutes, was opened on January 7, 1870.

The solution itself was clear, and there was no pellicle on its surface, although there was a slight flocculent deposit at the bottom of the flask. The end of the flask having been broken off and its contents shaken, the first drop which was examined with a magnifying power of about 600 diameters † showed the most unmistakeable living organisms. There were mere moving

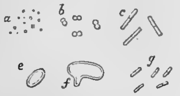


FIG. 6.—*Monads, Bacteria*, and other organisms met with in first experiment.

specks or monads (*a*); several active organisms made up of two minute sphericles (*b*); a few medium sized, actively moving bacteria (*c*); an actively moving spheroidal body $\frac{1}{10000}''$ in diameter (*d*); and several simple cellular bodies with a moderately thick cell wall, but apparently containing only fluid contents. These latter bodies varied in size from $\frac{1}{7500}''$ to $\frac{1}{3300}''$ in diameter. Most of them were simply ovoidal (*e*), one or two of them were seen to be more irregular in shape, either having projections (*f*), or even, as in one case, presenting projections something like cilia.

In the second drop of fluid examined, with the exception of a little granular matter, there were only seen several small but very active bacteria (*g*).

Experiment 2.—A flask, containing a mixed infusion (rather weak) of beef, carrot, and turnip, *in vacuo*, which had been hermetically sealed fourteen days previously, after the fluid had been boiled, was opened on December 23, 1869. The first two or three drops showed a large number of actively moving monads; a number of moving particles having an altogether irregular shape; bacteria distinct and moving, though less numerous; a number of spherical *Torula*-like cells of different sizes, containing a central dot (*a*); and a number of bodies mostly in the form of

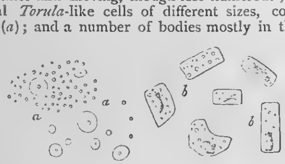


FIG. 7.—*Torulæ* and other organisms met with in second experiment.

parallelopipeds with rounded angles (*b*), though some were quite irregular in shape—these containing granules of various sizes, with sometimes large and altogether irregular protoplasmic-looking masses.

Experiment 3.—A flask containing a weak decoction of beef and carrot *in vacuo*, which had been hermetically sealed thirty-nine days before, after the fluid had been boiled, was opened on January 18, 1870.

This flask had been allowed to remain so long before it was opened because the fluid within showed no noticeable signs of change. There was no pellicle, and only a very small amount of granular material at the bottom of the flask.

On microscopical examination no distinct living things were found. Only a small quantity of motionless granular matter was seen.

Experiment 4.—A flask containing an infusion of hay, together with a few grains of phosphate of soda, *in vacuo*, which had been hermetically sealed seventeen days previously, after the fluid had been boiled, was opened on January 25, 1870.

The fluid itself was not turbid or cloudy, though it had become darker in colour. The bottom of the flask was irregularly lined with granular and slightly flocculent material.

On microscopical examination of two or three drops, there were seen many actively-moving monads; some bacteria of medium size; many quite irregularly-shaped particles in active movement; many flattened bits of protoplasmic-looking material with irregular and slightly curled edges, slowly moving, and ranging in size from $\frac{1}{10000}''$ to $\frac{1}{5000}''$ in diameter (other masses of this kind were distinctly hollow though mostly irregular in shape); and lastly there were several large irregular masses of fibres, the nature of which could not be determined.

2. *Infusions having an acid reaction.*

Experiment 5.—A flask containing a filtered infusion of turnip *in vacuo*, which had been hermetically sealed only *five* days previously, after the fluid had been boiled, was opened on December 15, 1869.

On the second day after the flask had been sealed, the previously clear solution began to exhibit a cloudy appearance. The next day a reticulated scum was seen on the surface of the fluid, which gradually became more manifest on the two following days. When the neck of the flask was opened, its contents were found to emit a most fœtid, sickly odour.

Microscopical examination revealed a very large number of bacteria and vibrionic-looking rods, some straight and others bent, some motionless and others exhibiting languid movements. These, mixed up with a thickly interlaced network

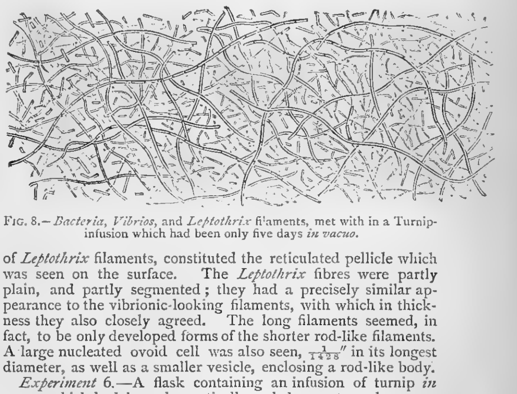


FIG. 8.—*Bacteria, Vibrios*, and *Leptothrix* filaments, met with in a Turnip-infusion which had been only five days *in vacuo*.

of *Leptothrix* filaments, constituted the reticulated pellicle which was seen on the surface. The *Leptothrix* fibres were partly plain, and partly segmented; they had a precisely similar appearance to the vibrionic-looking filaments, with which in thickness they also closely agreed. The long filaments seemed, in fact, to be only developed forms of the shorter rod-like filaments. A large nucleated ovoid cell was also seen, $\frac{1}{1428}''$ in its longest diameter, as well as a smaller vesicle, enclosing a rod-like body.

Experiment 6.—A flask containing an infusion of turnip *in vacuo*, which had been hermetically sealed seventeen days previously, after the fluid had been boiled for 20 minutes, was opened on January 25, 1870. The fluid never exhibited any distinct turbidity, and no pellicle formed on the surface; there was, however, an irregular covering of the bottom of the flask by fine granular matter, with here and there a small patch of filamentous-looking substance. No bad odour was perceived on opening the flask.

Unfortunately just as I was proceeding to examine the contents microscopically, nearly all the fluid was lost, including the filamentous-looking masses. Examination of a few drops of the fluid which remained showed a very large number of monads and bacteria; only a few of them were isolated and displayed movements; they were for the most part aggregated, either into spherical masses or else in the form of portions of a broken layer which presented traces of a *secondary organisation* similar to that represented in Fig. 1.

Experiment 7.—A flask containing an infusion of turnip *in vacuo*, which had been hermetically sealed seven days previously, after the fluid had been boiled for 20 minutes, was opened on February 4, 1870.

* Continued from p. 177.

† An objective of this power was used throughout these investigations; it was an "immersion" glass (No. 9) of Nachet's make. All the objects were drawn, therefore, as seen with a magnifying power of about 600 diameters.

The solution itself was much clouded, and its surface was covered by a thick gelatinous pellicle.

On microscopical examination of the fluid it was found to contain a multitude of very active monads and bacteria. The thick gelatinous pellicle was also made up of an aggregation of these in the usual transparent mucoid material. In very many situations this uniform pellicle was undergoing a process of *secondary organisation*, such as I have already fully described as leading to the production of unicellular organisms, and such as was seen to a less extent in the last experiment.

Experiment 8.—A flask containing a very strong infusion of turnip *in vacuo*, which had been hermetically sealed fifteen days previously, after the fluid had been boiled for 15 minutes, was opened on February 28, 1870.

The solution itself was very cloudy, and there was on its surface a thick coriaceous sort of pellicle distinguished by closely set aggregations or islets of denser growth.

On microscopical examination the fluid was found to contain a multitude of very active monads and bacteria. The bacteria were almost more active than any I had before seen, and there were many different kinds. Some exhibited rapid serpentine movements, accompanied by flexions of the two segments of which they were composed; whilst the movements of others were rapidly progressive in straight or curved lines.

The pellicle was made up mainly of simple *Leptothrix* filaments (mostly without joints or evidences of segmentation); and the thicker islets were found to be produced by a more luxuriant growth in these situations of densely interwoven filaments.

The pellicle was found to be so tough and elastic that some of it could only be mounted as a microscopical specimen after it had been compressed for an hour or two, by placing a small weight on the covering glass.

On the same day that this solution was hermetically sealed *in vacuo*, two other portions of the same infusion were treated in a different manner for the sake of comparison. One (A) was enclosed, and sealed, in a flask with ordinary air and without the infusion having been boiled; another portion (B) was boiled for fifteen minutes, and when the solution was cool, so that the flask was filled with ordinary air as before, its neck was hermetically closed by the blow-pipe flame. The third portion (C), as above mentioned, after it had been boiled for fifteen minutes was sealed up *in vacuo*.

The changes which took place were as follows. Towards the close of the *second* day, solution A became cloudy, and twenty-four hours later that in B was in a similar condition. It was not till two days later that solution C became cloudy. Afterwards, till the fifteenth day, when they were all opened, solutions A and B underwent comparatively little change, only becoming rather more opaque, though no distinct pellicle was formed on either; in solution C, however, the pellicle continued to grow thicker and more distinct throughout the whole period. When opened the reaction of all three was still found to be slightly acid. In other respects their characters were as follows:

Odour.		*Nature of Contents.*
Somewhat sickly, with smell of baked turnip.	Solution A	A multitude of monads and bacteria of medium size, all very active.
Fragrant, like that of fresh turnip.	,, B	Small monad-like granules most numerous; bacteria scarce, and movements not very active.
Decidedly fœtid...	,, C	Monads and bacteria of many kinds, whose movements were extremely active. An enormous quantity of *Leptothrix* filaments.

The results of these comparative experiments are most interesting. The changes commenced first in the unboiled solution, as might have been imagined; then, in the two boiled solutions. We find them commencing in the solution in contact with air before they did in that which was contained *in vacuo*—the reason of this being not quite so obvious. But the changes in A and B were only able to advance to a certain extent, because, apparently, the space above the fluid being already filled with air, these changes, necessitating the evolution of gases as residual products, were only able to go on so long as the increased tension occasioned did not reach such a stage as to stop these molecular rearrangements altogether. As might have been expected, the changes which took place in A were different from those which occurred in B. Those of A seemed to have been more thorough, giving rise to a much larger quantity of bacteria, and the somewhat sickly odour of this was also intermediate between the comparatively unchanged odour in A and the decidedly fœtid odour of B, in which such an enormous development of organisms had taken place. Here, then, the advantages of the vacuum seem to be most clearly shown.

After having read M. Pasteur's account, concerning the growth and development of fungoid organisms which had been placed in saline solutions,* it occurred to me that it would be a subject of much interest to determine whether any evidence could be obtained tending to show that organisms might even be evolved *de novo* in certain saline solutions. This, in fact, seemed to be a problem of very great importance; for, if otherwise suitable, the employment of such saline solutions would be attended by certain advantages. In the first place it was likely that the saline materials in solution would be far less injured by the high temperature of 100° C. than organic substances. And since, in working with a vacuum, we are able to get rid of the air altogether as a possible "germ" containing medium, so, with the view of simplifying our experiments, it becomes desirable that everything in our power should be done to diminish the number of possible pre-existing germs in the solutions employed. But this end, also, seemed likely to be best carried out by the employment of certain simple saline solutions. We should thus be able to get rid, as it were, of Buffon's "molecules organiques," which he supposed to pervade all organic matter that had been fashioned in a Living organism—and also of those ordinary organic molecules whose presence is supposed, even by M. Pouchet and other heterogenists,† to be absolutely necessary in order that "spontaneous generation" may occur. We should thus, indeed, best emerge from the circle of the organic, thus as far as possible elude the company of all those embarrassing molecules which the vitalist may choose gratuitously to endow with "vital" properties, and to regard as the chosen seat of a special "vital force," uncorrelatable with the ordinary physical forces. We may thus be best brought face to face with the problem—Whether the pre-existence of organic matter, which has been elaborated in pre-existing organisms, is, at present, absolutely necessary for the *de novo* origination of Living things; or whether, in fact, these may arise, more or less directly, by changes taking place in an aggregation of new formed molecules of an organic type, which have been themselves produced by the combination of some of the dissociated elements of the saline substances employed.

Just as nitrate of ammonia, carbonate of ammonia, free carbonic acid, and water, with a few saline substances, constitute the materials which—under the influence of the modified physical forces operating in the Living plant—are convertible into similar living vegetable protoplasm, so the problem which now presented itself was, whether under the influence of purely physical forces—acting upon solutions containing some such ingredients—re-arrangements might not be brought about amongst the elements of the substances dissolved and of the aqueous medium itself, resulting primarily in the formation of certain new complex molecules, which, secondarily, under the continued influence of similar physical forces‡, are capable of permitting the occurrence of new modes of collocation resulting in the evolution of the minutest specks of Living Matter §.

It resolved itself, in fact, into an inquiry whether Living things can now originate on the surface of our globe in that fashion after which alone (in accordance with the evolution hypothesis) they could have originated in those far remote geologic ages when Life first began to dawn upon the still heated surface of that rotating and revolving spheroid which now constitutes our planet. Before organic matter of the ordinary kind could exist organisms must have been present to produce it. Organisible compounds of a certain kind must, nevertheless, have preceded organisms. And just as chemists are now able to build up a great number of organic compounds in their laboratories, so it seems quite possible that some such mobile compounds may have been evolved by the agency of natural forces alone acting on the heated surface of the earth, at a period anterior to the

* Annales de Chimie et de Physique, 1862, p. 106

† Who at the same time that they are heterogenists, are firm believers in a special "vital" force.

‡ Those who wish to understand how incident physical forces are capable of bringing about such re-arrangement, should consult vol. i. chap. 2, of Mr. Herbert Spencer's "Principles of Biology."

§ For the genesis of Life, we need only look after the origin of such a speck of Living Matter. This is all that would be required by the greater number of the most advanced physiologists of the day. Given a mere speck of living matter less than $\frac{1}{50000}$" in diameter, and a Living thing may soon appear in the form of a *Vibrio*, a *Torula* cell, or a *Leptothrix* filament. By all such biologists the "vital" forces are held to be *molecular* forces, and such molecular forces are nought else than the resultant attributes of the particular advent of Living things.* We at present may, therefore, well wish to know, whether what is presumed to have taken place then may still take place now, since the affirmative solution of this problem would suffice to throw a halo of reflected light back through the ages, and would thus make that which is now a mere hypothesis, approximate as much as possible to the rank of one of the best established probabilities concerning the life-history of the globe on which we live.

It was with a feeble hope of throwing light upon the above-mentioned subject that I commenced the series of experiments which are now to be detailed. These were at first somewhat tentative, but the success obtained at each step emboldened me to proceed in my endeavours to obtain Living things under more and more arduous conditions. In all cases the saline substances were carefully selected; one of the first requisites being that they should, at all events, contain the four fundamental ingredients of Living things: nitrogen, carbon, hydrogen, and oxygen. Therefore it was that in almost all cases an ammoniacal salt was one of the substances used, on account of its capability of supplying nitrogen. And, with the view of keeping up a sort of uniformity, phosphate of soda was employed as the second saline ingredient in as many of the solutions as possible. This was considered quite as suitable as any other salt, and as a phosphate it presented certain advantages.† It was deemed necessary, moreover, that a certain mixture of substances should exist, in order that there might be sufficient diversity amongst the elements coexisting in the solutions. These elements being then affected differently by the play of incident physical forces upon them, under their new modes of vibration thus excited, new and altogether different mutual affinities might become dominant, after the fashion so clearly indicated by Mr. Herbert Spencer‡ in reference to other molecular re-arrangements. And these affinities *might* also be such as would tend to a colloidal rather than to a crystalloidal mode of aggregation. Concerning such probabilities, we doubtless have much to learn. Amongst saline substances, colloidal modes of aggregation seem to be favoured under certain sets of conditions, and crystalloidal modes of aggregation under certain other sets of conditions. Prof. Graham showed that this was the case with such mineral compounds as silica, the sesqui-oxides of iron, chromium, and other bodies. Then, again, this possibility of an isomeric modification is admirably exemplified by the now well-known tendency of cyanate of ammonia to become converted into the organic compound called urea. We are told in Watts's *Dictionary of Chemistry* § that after an aqueous solution of cyanate of ammonia has been prepared, the "liquid exhibits the reactions of a cyanate, but when *heated* or left to evaporate spontaneously, it is converted into urea." This would seem to show that the passage from the crystalloid to the colloid mode of molecular collocation is by no means a difficult one—that it may be brought about, in fact, by very slight determining causes.

That transitions of a reverse order may be effected from the one to the other state or mode of aggregation—and this, too, even with more complex substances—seems indicated by the fact that certain protein compounds may exist at one time in their usual colloidal state, and at another time as statical crystalline aggregates. The possibility of the assumption of the crystalline form by a certain number of these protein or colloidal substances is, indeed, now placed beyond all doubt.‖ As the most familiar instance of this I may mention the now well-known oblique rhombic *hematoidin* crystals and other crystalline forms obtainable from blood. And amongst these latter are to be included certain tetrahedral crystals discovered¶ by Reichert, in connection with the placenta of the guinea-pig, the behaviour of which to reagents rendered it certain that they were of an albuminous or protein nature.* Chlorophyll, also, has been observed in a crystalline state by M. Trécul.† In these and in many other cases which might be cited, we have, apparently, to do with some mere alteration in the arrangement of molecules, and the crystalline form is to be regarded as possible when certain isomeric modifications are brought about in substances which ordinarily are non-crystallisable. No less an authority than M. Trécul, also, tells‡ us that he has actually seen and watched a tetrahedral crystalline mass of this kind, which had been produced within the cells of the bark of the common elder (*Sambucus nigra*), gradually undergoing a modification in form at a certain part of a most startling nature. Whilst altering in shape, the part so altering was seen to become converted into a short fungoid filament which grew at the expense of the crystal. This change in form, then, could only be taken as the external sign of a much more profound molecular rearrangement taking place within the mass, whereby it was changed from a non-living albuminoid crystal into a Living and growing organism.§ In this remarkable case there must have been not only a relapse into the colloidal mode of molecular aggregation, but a secondary assumption of a still more unstable mode of collocation, by means of which the mass was gradually converted into a living embryo fungus.

It is well, then, for us to bear these facts and statements in mind, whilst we enter into the kindred though somewhat different inquiry, whether, under the influence of suitable conditions, there is any disposition for the ultimate constituents of different saline substances, existing intermixed in a state of aqueous solution, to fall into new groupings or modes of collocation of a non-saline or colloidal nature. If this can take place, we should then have a new kind of decomposition with an almost simultaneous recomposition—a re-arrangement, in fact—giving birth to substances allied to those of the protein group. And it would be only rational for us to suppose that such new-formed protein substances would be as prone to undergo change as these substances are generally found to be. If ordinary protein substances, therefore, which have been built up as parts of Living things, are capable of going through certain Life-giving changes, it would be quite possible that the differently evolved protein—that which comes into existence "spontaneously," or without the influence of pre-existing living things—may go through similar changes. The molecular constitution of these two kinds of matter may be closely allied, and wherever Life-giving changes occur, we are entitled to look upon these as actions resulting from the influence of physical forces upon material collocations whose molecular constitution is of such a nature as to render them prone to undergo current re-arrangements. A series of actions and reactions occur between such material collocations and their environment, and as a result Living things appear and grow. This tendency to undergo change is inherent in colloidal substances. As Prof. Graham told us:—"Their existence is a continual metastatis. . . . The Colloidal is, in fact, a dynamical state of matter, the crystalloid being the statical condition. The colloid possesses ENERGIA. *It may be looked upon as the probable primary source of the force appearing in the phenomena of vitality.*"

B.—Fluids employed being Solutions of Saline Substances in Distilled Water.

Experiment 9.—A flask containing a solution (neutral) of crystallised white sugar, tartrate of ammonia, phosphate of ammonia, and phosphate of soda‖ *in vacuo*, which had been hermetically sealed nine days previously, after the fluid had been boiled for 20 minutes, was opened on January 4, 1870.

atomic collocation which displays them. The speck of living matter is held to contain no newly created force—just as no one believes it to contain newly created material units. Such a germ or embryo Living thing, is only a new mode of collocation of pre-existing matter, and of pre-existing force or motion. Its properties—the "vital" phenomena which it manifests—are the results of the particular material collocation which exists, and of the "conditions" by which this is surrounded. A Living thing is a dynamic aggregate, and between such aggregate and its environment, there is continuous action and reaction so long as Life exists.

I have given the reasons for such views a little more fully in an article entitled "Protoplasm" (NATURE, Feb. 24, 1870), and I would again refer the reader to the views announced by Prof. Huxley and Prof. Tyndall (Note, p. 175).

* See *Appendix* to Mr. Herbert Spencer's "Principles of Biology."

† The presence of phosphates was found by M. Pasteur greatly to favour the growth of fungi in certain saline solutions. (Annales de Chimie et de Physique, 1862, p. 108.)

‡ Principles of Biology, Vol. I. chap. ii., entitled, "The Action of Forces on Organic Matter." § Vol. ii. p. 193.

‖ See an article on "Albuminous Crystallisation," in *British and Foreign Medical and Chirurgical Review*, Oct. 1853.

¶ Beobachtungen über eine eiweissartige Substanz in Crystal-form:—Müller's Archives, 1849, p. 197.

* For an account of these reactions see *Brit. and For. Rev.*, loc. cit., p. 354.

† Comptes Rendus, t. lxi., p. 436. ‡ *Ibid.*, t. lxi. (1865), p. 435.

§ M. Trécul's own words are as follows:—"Lors de mes observations en 1860, j'avais reconnu que des corpuscles colorables en violet par l'iode remplacent frequemment les tétraèdres après la putréfaction, mais je ne vis pas à cette époque la transition des uns aux autres. Je fus plus heureux cette année; j'ai vu les tétraèdres s'allonger par un de leurs angles, et passe graduellement à nos singulières plantules en produisant une tigelle cylindrique. Dans ce cas le tétraèdre, arrondi ou encore anguleux, représente la bulbe. Le tétraèdre peut même s'effacer complétement, et ne laisser après lui qu'une plantule fusiforme ou cylindrique."

‖ The several ingredients were contained in this solution in the following proportions:—To 80 parts of water there were added 16 parts of sugar, 1 part of tartrate of ammonia, and ½ a part each of phosphate of ammonia and phosphate of soda. This solution, therefore, did not contain saline substances alone; the presence of sugar made it form a connecting link, as it were, between the solutions containing organic matter only, and those in which saline substances alone were contained.

Before the flask was opened the solution itself was clear and without the least trace of a pellicle on its surface, though for the last three or four days a very fine deposit was seen on certain parts of the bottom and sides of the flask.

When examined microscopically, a very few monads and bacteria were found in the first few drops of the fluid, which had been poured out before the whole was shaken. The remainder was then poured into a conical glass, and after having been allowed to stand for a time, the supernatant fluid was removed, and the last few drops containing the sediment were examined. In this were seen many bacteroid particles (*a*) and monads of different sizes, exhibiting the most active movements, as well as some irregular-

FIG. 9.—Organisms met with in Experiment 9

shaped particles, also active. Many of the mere monad-like particles had the appearance of being very small *Torula* globules (*b*), and one cell of this kind was seen $\frac{1}{7500}''$ in diameter, and containing a nuclear particle in its interior. Many groups made up of three ovoid particles (*c*) were also seen, and also a dumb-bell-like body, all exhibiting slow movements. There were, moreover, motionless protoplasmic-looking masses—some cuboidal or more or less spherical and hollow, and others altogether irregular in shape, though appearing like masses of formless protoplasm; also peculiar branched fibres with knob-like swellings at the termination of the branchlets, and lastly a small mass made up of a spirally-twisted fibre, similar to that (represented in Fig. 13) which was found more abundantly in subsequent experiments* where tartrate of ammonia and phosphate of soda were employed.

Experiment 10.—A flask containing a solution† of acetate of ammonia and phosphate of soda, *in vacuo*, which had been hermetically sealed forty-two days previously, after the fluid had been boiled for 20 minutes, was opened on March 10, 1870.

The solution during this time had shown no signs of deposit, turbidity, or pellicle, and on microscopical examination of the fluid, no organisms of any kind were discovered.

Experiment 11.—A flask containing a solution‡ of oxalate of ammonia and phosphate of soda, *in vacuo*, which had been hermetically sealed sixty-one days previously, after the fluid had been boiled for 20 minutes, was opened on March 27, 1870. Its reaction was then found to be slightly acid.

The solution itself continued clear throughout, and presented no scum on its surface; but, after a time, a very small amount of deposit made its appearance at the bottom of the flask, and latterly this had appeared as a small, irregular, and opaque whitish aggregation, with branch-like ramifications. It was very friable and easily broke up into a white granular material. Almost immediately after the neck of the flask had been broken, small acicular crystals appeared in abundance on and in the solution, varying from about $\frac{1}{2}$ to 1 line in length.§

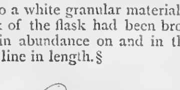


FIG. 10.—Fungus-spores found in Solution of Oxalate of Ammonia.

On microscopical examination, monads and altogether irregularly-shaped particles, showing tolerably active movements, were abundant. A few hyaline spherical vesicles were seen having no solid contents, and some of them were flattened at one pole. Three distinct fungus-spores were seen, and also another otherwise similar body in which there were small granules rather than a single nucleus. There was also a nucleated organism slightly larger, one of whose extremities was rectangular, whilst the other was rounded. In addition, there was a considerable quantity of white granular material which had resulted from the crumbling of large, though opaque white, masses of the same substance; and also some semi-crystalline, opaque rhomboid plates.

Experiment 12.—A flask containing a solution* of tartrate of ammonia and phosphate of soda, *in vacuo*, which had been hermetically sealed eleven days previously, after the fluid had been boiled for 20 minutes, was opened on January 21, 1870. The reaction was then found to be still slightly acid.

No turbidity of the fluid was observed, and no scum formed on its surface, though a very slight deposit gradually collected at the sides and bottom of the vessel, and amongst this there was seen, during the last six or seven days, a small white, flocculent mass, which gradually increased in size.

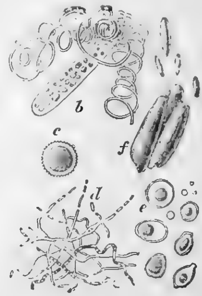


FIG. 11.—Spiral and Confervoid-looking Filaments, with Fungus-spores and Crystals, from a Tartrate of Ammonia Solution.

On microscopical examination of the white mass, this was found to be made up for the most part of a densely interlaced and spirally-twisted fibre, almost precisely similar to what was alluded to in Experiment 9 and to what is represented in Fig. 13, This was colourless, highly refractive, quite homogeneous, and apparently solid. In one place there was seen emerging from amongst the spiral fibres an elongated chamber (*b*) resembling a spore case, and containing many separate protoplasmic-looking masses. At one border of this spirally-twisted fibre-organism, there were a number of other filaments (*d*) about $\frac{1}{20000}''$ in diameter, and containing irregular masses of protoplasm in their interior. These were colourless confervoid-looking filaments.

A few moving monad-like particles were seen, and in the sediment there were many motionless aggregations of such particles imbedded in an almost invisible and scanty jelly-like matrix. In these masses rounded or ovoid fungus-spores (mostly with large nuclei in their interior, in various stages of formation) existed pretty abundantly (*e*). Some of the spores showed a slight neck-like projection and flattening at one extremity, as though they were about to germinate. In addition another much larger spore (*c*) was found. Some of the rounded crystals which were met with in this solution are also represented (*f*).

Experiment 13.—A flask containing a solution† of tartrate of ammonia and phosphate of soda *in vacuo*, which had been hermetically sealed twenty days previously, after the fluid had been boiled for 20 minutes, was opened on February 15, 1870. The reaction of the fluid was then decidedly acid.

The fluid itself showed no signs of turbidity, and there was no trace of scum on its surface. Small whitish flocculent shreds had, however, been seen at the bottom of the flask for the last twelve or fourteen days, during which time they seemed very slowly to increase in size. Some smaller sedimentary particles were also seen at the bottom.

On microscopical examination some of the white shreds were found to be composed of comparatively large masses of the small algoid filaments which were met with in the last experiment; whilst others were made up of an aggregation of fungoid spores with an abundant mycelium which had been developed from them. The spores were precisely similar to those which

* This fibre growth was twisted around a paper-fibre, but there is little doubt that it had simply grown round this as a mere foreign body. Every part of the paper fibre showed the most distinct colour reactions with the polariscope, though the spirally-twisted fibre did not show a trace of such reaction.

† Reaction neutral. ‡ Reaction neutral.

§ Two days before this flask was opened, it was removed for a few hours into another room, where the temperature was lower. The agitation of the fluid, and perhaps in part the lower temperature, soon resulted in the formation throughout the fluid of a number of large acicular crystals from $\frac{1}{2}$ to 1 inch in length. These disappeared in the evening as soon as the temperature of the solution was again raised to about 80° F. The greatly superior size of the crystals produced *in vacuo* is worthy of note.

* Reaction slightly acid. † Reaction slightly acid.

were found in the last solution. There they were about to germinate, and here they had germinated into a fungus of the *Penicillium* type. In one mass the mycelium had produced four or five much larger filaments, terminating in artichoke-like heads of different sizes, bearing naked spores. Though two of the organisms met with in the last experiment were here reproduced, this was not the case with the spirally-twisted fibre organism.

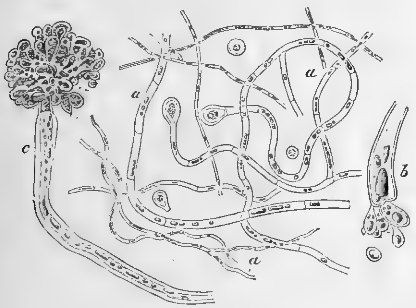


FIG. 12.—Fungus found in the Solution containing Tartrate of Ammonia and Phosphate of Soda.

A few actively moving granules and particles of various shapes were seen, though, as in the last solution, there was nothing resembling a bacterium. Spherules which seemed to represent different stages in the development of the fungus-spores were met with, varying in size from that of an almost inappreciable speck up to that of the perfect spore, which itself varied considerably in size at the time that it began to germinate. In one of these fungus-spores which was about half grown, the nuclear particle within was seen actively moving from end to end of the cell.*

Experiment 14.—A saturated solution of tartrate of ammonia and phosphate of soda *in vacuo*, prepared in same manner as the last solution and at the same time, though opened on the thirty-fifth day, yielded no organisms of any kind.

Experiment 14*a*.—(In this and in the following experiment the solutions were not contained *in vacuo*, but were intended rather to throw light upon the question as to whether the vacuum was favourable or prejudicial to the appearance and growth of organisms in these saline solutions.)

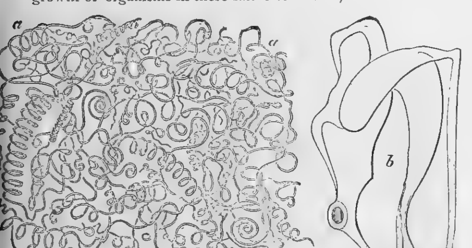


FIG. 13.—Spirally Twisted Fibre Organism.

A solution of tartrate of ammonia and phosphate of soda in distilled water was boiled for 20 minutes, and was then allowed to cool, so as to become filled with air at ordinary atmospheric pressure (which might have contained living germs, since no precaution was taken to destroy them). The neck of the flask was then hermetically sealed, and this was not opened till the thirty-eighth day, when the fluid was found to have a decidedly acid reaction.

The fluid never showed any signs of turbidity, and no pellicle formed on its surface, though a small white flocculent mass had been seen at the bottom of the flask for three weeks, which very slowly increased in size.

The white mass (Fig. 13, *a*) was picked out, and on microscopical examination it was found to be made up for the most part of a spirally-twisted fibre organism, very similar to that which was produced in a similar solution *in vacuo*. The spiral twisting was, however, even more marked, and the fibre in many places was somewhat thicker.*

In addition, two or three small fungus-spores were seen, very similar to those met with in Experiment 12; and there were also some confervoid-looking filaments with irregular masses of protoplasm in the interior, occurring sparingly here and there, similar to those which were met with in much larger quantity in the former experiment. Four or five larger cells were seen with very thick walls, closely resembling the sporangia of some fungi, and in one of them, measuring $\frac{1}{1000}''$ in diameter, many granules were present exhibiting the most active molecular movements. The contents of the other cells could not be made out, as they were so much obscured by the fibre organism in the midst of which they were imbedded.

Experiment 14*b*.—A solution of tartrate of ammonia and phosphate of soda in distilled water, without having been boiled, was exposed to the air † in a flask, and was then examined microscopically on the thirtieth day. The re-action of the fluid at this time was neutral.

There was no pellicle on, or opacity of the fluid, but a small whitish mass of matter had been seen at the bottom of the flask for the last fortnight, which slowly increased in size, till at last it formed a mass bigger than a mustard seed.

On microscopical examination this whitish flocculent matter was found to be made up principally of an enormous aggregation of *Sarcina*-like material, the divisions of which were rather more sharply defined though they were about equal in size to the *Sarcina* which is occasionally met with in urine.

Several fungus-spores were also seen (Fig. 13, *c*) larger than, though otherwise similar to, those met with in *Experiments* 12, 13, and 14*a*. There were none of the confervoid-looking filaments or of the spirally-twisted organisms.

* In Experiments 12 and 13, the actual strength of the saline solutions was not known, but in these as well as in Experiments 14*a* and 14*b*, there may have been about 15 grains of tartrate of ammonia, with about 5 grains of phosphate of soda to an ounce of water.

* It has been suggested to me by a friend whose opinion carries great weight with it, that this is not an organism which had grown in the solutions, but rather some non-living accidental product which had gained access to the solutions. This was a suggestion which deserved a most attentive consideration, more especially as in two other experiments, which will not be cited, foreign bodies were found in the solutions. Are these spiral-fibre masses, then, mere non-living *débris* of some kind? It was suggested that they were spiral ducts of some plant broken up and modified by the boiling process which they had undergone.

With reference to this question it seems most desirable to state, in the first place, that such a spiral-fibre mass has been met with four times in solutions containing a mixture of tartrate of ammonia and phosphate of soda, though never in solutions containing tartrate of ammonia alone, and in fact it has never been found in any other kind of solution except in one whose chemical constitution was almost precisely similar to the mixture above named. This seemed to indicate that it might have been contained within or upon the crystals of phosphate of soda. Successive solutions, however, of many of these in a watch-glass have shown no trace of such fibre-masses (and it may be well to add, perhaps, that a similar statement holds good for the crystals of tartrate of ammonia). These masses of spiral fibre, found only in solutions of a given chemical constitution, have been seen to increase in size from week to week. The Rev. M. J. Berkeley, who was kind enough to examine my specimens, could not identify them with any known cryptogamic organisms.

If they were really altered spiral ducts, then, seeing that they have only been met with in solutions of a certain chemical constitution, they ought to have been contained in the saline substances employed. But an examination of such saline substances as above stated, does not show a trace of these fibre-masses. All the spiral ducts which I have examined, moreover—even after boiling—polarise beautifully, whilst these spirally twisted fibre-masses do not give the least trace of colour reactions with polarised light. And lastly, in certain parts the apparently solid spiral fibre seems to become decidedly tubular, and in other places it widens out into flat expansions of a peculiar character. In one of these expansions (Fig. 13, *b*) differentiation had apparently taken place, which had led to the production of a spore-like body.

A somewhat similar twisted fibre-organism was met with in Experiment 16, though here it was evidently in an embryonic condition. (Fig. 15.) It seemed to be then arising by a process of differentiation taking place in a pellicular mass.

† A piece of paper merely, was loosely inserted into the mouth of the flask with the view of keeping out as much dirt as possible.

Experiment 15.—A flask containing a solution of a potash-and-ammonia alum, and of tartar emetic, *in vacuo*, which had been hermetically sealed twenty-eight days previously, after the fluid had been boiled for fifteen minutes, was opened on March 17, 1870. The fluid then had a decidedly acid reaction.*

The solution continued clear throughout; there was no trace of a pellicle and no deposit at the sides, though a whitish flocculent mass was seen at the bottom of the flask after the first fortnight, which gradually increased in amount, and at last formed a mass about $\frac{1}{8}''$ in diameter.

On microscopical examination the white mass was found to be made up of aggregations of variously sized and irregularly shaped protein-looking particles which were imbedded (*b*) in a most distinct hyaline jelly-like material. The granules were highly refractive, altogether irregular in shape, and they varied in size from $\frac{1}{3000}''$ up to $\frac{1}{5000}''$ in diameter. Though most of the particles

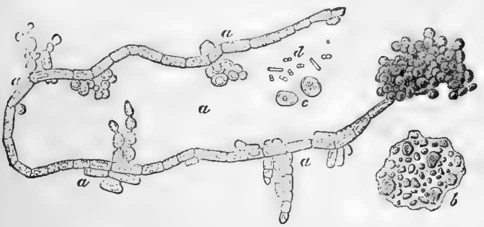


FIG. 14.—Fungus met with in a solution containing Potash-and-Ammonia Alum, with Tartar Emetic.

were motionless and imbedded in the jelly, very many were seen exhibiting active and independent movements; some of these were in the form of little double spherules (*d*), and a very few others resembled bacteria about $\frac{1}{8000}''$ in diameter, though they did not possess the accustomed joint.

Three fungus-spores with thick double walls were seen. Each these was about $\frac{1}{3300}''$ in diameter. Within one of them there were only a number of granular particles (*c*), but within each of the other two there was a large and somewhat irregular nuclear mass.

In addition there was found the complete fungus which is represented in the figure (*a*), with all its spores. In a portion of one of the granular aggregations, a mass of about thirty spores seemed to be undergoing evolution in a part of the mucoid material through which some fine granules were disseminated.

Experiment 16.—A flask containing a solution of carbonate of ammonia and phospate of soda *in vacuo*, which had been hermetically sealed thirty days previously after the fluid had been boiled for twenty minutes, was opened on March 1, 1870. The fluid was then found to have a very slightly alkaline reaction.

The fluid had continued clear and no pellicle had formed on its surface, though a light granular deposit had slowly collected in small quantities on the bottom and sides of the flask.

On microscopical examination, bright highly refractive moving particles, very similar in appearance to those of milk, were met with, of all sizes varying between $\frac{1}{30000}''$ and $\frac{1}{5000}''$ in diameter. There were also numerous crust-like aggregations of such particles. Small *Torula* cells, the smallest being about $\frac{1}{15000}''$ in diameter, were very abundant. They were either single or double, and each cell contained a distinct nuclear particle; some of the larger ones, indeed, contained two. All these cells exhibited very slight oscillating movements. Two cellular looking bodies, each about $\frac{1}{2000}''$ in diameter, and having granular contents, were also seen; and, in addition, there were two or three patches of a peculiar spirally twisted fibre-like organism, growing in portions of the granular crust, and apparently representing an embryonic condition of a spiral fibre, closely resem-

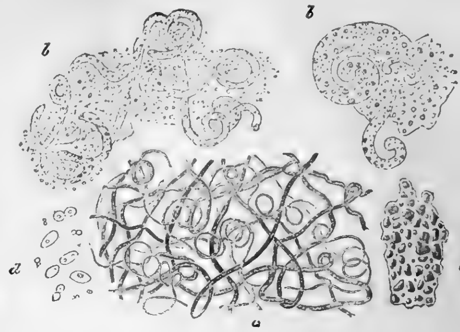


FIG. 15.—Embryonic condition, and also more mature stage, of a Spiral-fibre Organism met with in a solution of Carbonate of Ammonia and Phosphate of Soda.

bling those met within previous experiments. Neither of these gave the smallest trace of a colour reaction with the polariscope.

B.—*Experiments in which the fluids employed were raised to a temperature of from 146° to 153° C. for four hours, after all air had been exhausted and the flasks had been hermetically sealed.*

A temperature of 100° C. has been the degree of heat to which all the fluids in the experiments hitherto related have been subjected. It has been previously found that none of the lower organisms so treated, and which had been afterwards examined, were able to survive an exposure for a few seconds to such a degree of heat. They had nearly all been destroyed, in fact, at a temperature many degrees short of this. Many different kinds of organisms have been submitted to this test, and without the occurrence of any exceptions* such a degree of heat has always proved fatal to them. Looking, therefore, on the one hand, at the uniformity in the experimental evidence, which has itself extended over a wide basis, and, on the other, at the comparative uniformity in fundamental nature and property existing between all the lowest kinds of Living things, which are almost wholly made up of a more or less naked living matter or protoplasm, it is only reasonable for us to conclude, until direct evidence can be adduced to the contrary, that that which holds good for the many without exception may prove to be a rule of universal application. Therefore it was that the Societé de Biologie (and M. Pasteur himself for a long time) assumed that none of the lower kind of organism could survive in a fluid which was raised to a temperature of 100° C.

No evidence has as yet been adduced which is capable of shaking the validity of this conclusion, and the experiments just related are much stronger in favour of the view that the organisms found in my experimental fluids were there evolved *de novo* than were all the negative results in the experiments of Schwann (upon which so much stress was formerly laid) in proving the impossibibility of such a mode of evolution. And yet Schwann's experiments were deemed by many so conclusive that they were thought to have upset the doctrine of heterogeny. The fluids with which he experimented were only exposed to a temperature of 100° C., and, working under a particular set of conditions which are considered to be adverse to the occurrence of evolutional changes, he found no organisms therein when his flasks were opened. I, on the contrary, subjecting my experimental fluids to the same temperature, though exposing them subsequently to quite different conditions, which I suppose to be more favourable for the occurrence of evolutional changes, do find organisms in the fluids when the flasks are opened. His negative results *may* be only applicable to the particular fluids and the particular conditions under which he worked; but my

* This solution was prepared one evening when I had been busy in devising several other mixtures with which I deemed it desirable to experiment. At the time I thought I possessed a pure potash alum, and by some strange oversight I had failed to recognise that, if this had been the case, I should have been employing a solution which contained no nitrogen. Having once planned what mixtures I would use, I did not further think of each of them analytically. It was not, therefore, till my attention was called by Dr. Sharpey to the assumed absence of nitrogen in the above solution, that I became aware of this. It seemed very incredible that an organism should have been produced in a solution containing no nitrogen. I then had some of my supposed potash alum carefully tested, and it was found to contain a considerable quantity of ammonia. I then also learned that as ammonia-alum is now the alum of commerce, it is very difficult to get a pure potash alum.

* No exceptions, that is, amongst such organisms as are met with in infusions. The only known exceptions to that rule being met with in the case of seeds naturally provided with a hard *testa*, and after these had undergone an extreme amount of desiccation.

positive results, and those of many other experimenters, may be considered to have a most important bearing upon the settlement of the general doctrine. It becomes simply a question as to whether the conditions which were formerly believed sufficiently stringent to destroy all pre-existing Living things within the experimental flasks are in reality adequate to effect this. Can the standard of vital resistance be raised? If the old landmarks cannot be shown to be false, then there is very far more evidence of all kinds in favour of, than there is against, the occurrence of "spontaneous generation."

But in order, still further, to put this question beyond the region of doubt, I gladly availed myself of Prof. Frankland's proffered aid. He very kindly offered to procure a perfect vacuum in my flasks, and then, after these had been hermetically sealed, to expose them and the fluids which they contained for some time to a temperature of about 150° C. This temperature for a fluid is so very far in advance of that which is at present believed to be destructive to all the lower organisms, that it is perfectly fair for us to presume, till evidence be adduced to the contrary, that Living things would be completely destroyed by an exposure to such a temperature even for a single minute.

It seemed desirable to try different kinds of solutions in this way, and I therefore asked Dr. Frankland to be kind enough to submit four flasks and their contents to this most stringent test. In order that they might be representative of the solutions with which I had already been working, one was to contain a freshly prepared and filtered infusion of turnip; another a solution of sugar, tartrate of ammonia, and phosphate of soda in distilled water; another a solution of tartrate of ammonia and phosphate of soda alone in distilled water; whilst the last was to contain carbonate of ammonia and phosphate of soda in distilled water.

The second of these solutions was submitted to these conditions by Dr. Frankland, on Feb. 11, 1870, together with two others, one containing turnip juice, and the other carbonate of ammonia and phosphate of soda. On the following day I received a letter from Dr. Frankland, in which he had written as follows:—"Yesterday, I exposed the three liquids to a temperature of 150° C. for four hours. On taking them out of the digester this morning two were broken; one had probably burst with the pressure of carbonate of ammonia vapour, the other had received some slight shock which had broken off the extreme point of glass where it was drawn off before the blowpipe. The third, containing the tartrate of ammonia, sugar, and phosphate of soda, I send along with this, but fear you will not consider its contents favourable for your operations, part of the sugar having apparently been converted into caramel."

A few days afterwards (on Feb. 15) Dr. Frankland submitted the three other solutions to a similar treatment, and he has kindly furnished me with the following statement of the conditions to which they were subjected:—

"Each liquid was placed in a glass tube about $\frac{3}{4}$ inch in diameter, 9 inches long, and closed at one end by fusion of the glass. The open end of the tube was then drawn out so as to form a thick capillary tube, which was afterwards connected with a Sprengel's mercurial pump. The action of the pump soon produced a tolerably good vacuum, when on gently warming the liquid, the latter began to boil, its vapour expelling the last traces of air from the apparatus. After the boiling had been continued for several minutes, the tube was hermetically sealed at the capillary part.

"Three tubes were prepared in this way, containing respectively—

"1. Turnip juice;

"2. Solution of ammonic tartrate and sodic phosphate;

"3. Solution of ammonic carbonate and sodic phosphate.

"The vacuum in tubes Nos. 1 and 2 was so perfect as to render them good water-hammers. In the third tube the vacuum was not so good, owing, doubtless, to the evolution of carbonic anhydride from the ammonic carbonate.

"The three tubes were now placed in the wrought iron digester, described by me in the Philosophical Transactions for 1854, page 260. It consists essentially of a cylindrical iron vessel, with a tightly fitting cover, which can be securely screwed on to it. Through the centre of the cover passes an iron tube, which descends half way down the centre of the cylinder. This tube is closed at bottom, and contains a column of mercury about an inch long, and a thermometer plunged into the mercury shows the temperature of the liquid inside the digester.

"Water being now poured into the digester until it covered the tubes, and the cover having been screwed on, heat was applied by means of a gas stove.

"The temperature was allowed to rise to about 150° C., and was maintained between 146° and 153° C. for four hours, and it is almost needless to say that every part of the sealed tubes and their contents was exposed to this temperature during the whole time. The glass tubes, though of moderately thick glass only, ran no risk of fracture, because the pressure inside them was approximately counter-balanced by the pressure of steam outside.

"After cooling, the tubes were removed from the digester, when it was found that the turnip juice had become discoloured, and the liquid no longer acted as a water-hammer, showing that the vacuum in the tube had been impaired. The contents of the two remaining tubes were apparently unaltered, the vacuum in No 2. being as perfect as ever."

Experiment 17.—The tube containing the infusion of turnip was opened at the end of the twelfth day.

When received from Dr. Frankland the fluid had been changed to a decided, but light brown colour, and there was some quantity of a blackish brown granular sediment at the bottom, though the solution was free from all deposit when placed in the digester. After this tube was suspended in the warm place, as the others had been, it remained in the same position till it was taken down to be opened. A slight scum or pellicle was observed on the surface—covering this partially—on the sixth day. During the succeeding days it did not increase much in extent, though it became somewhat thicker. Although very great care was taken, still the slight movement of the flask, occasioned in knocking off its top, caused this pellicle to break up and sink to the bottom.*

The contents of the flask emitted a somewhat fragrant odour of baked turnip, and the reaction of the fluid was still slightly acid. On microscopical examination, there was found very much mere granular *débris* of a brownish colour, which probably represented the brownish sediment seen when the tube was removed from the digester. There were, also, a very large number of dark apparently homogeneous reddish brown spherules, mostly varying in size from $\frac{1}{7500}''$ to $\frac{1}{20000}''$ in diameter, partly single and partly variously grouped; the nature of these was doubtful though they were probably concretions of some kind. There were also other indeterminate flat and irregular masses, which seemed more to resemble protoplasmic substance in its microscopical characters.

In addition, many irregular and monad-like particles were seen in active movement, though there were no distinct bacteria.

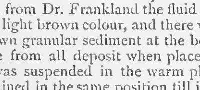


FIG. 16.—*Tailed Monad* and *Torula* Cell from a Turnip Solution which had been exposed to a temperature of 146°—153° C. for four hours.

Several rod-shaped bodies $\frac{1}{2000}''$ in length (*d*) were seen, however, resembling ordinary bacteria, except that they were unjointed and motionless.† In one of the drops examined there was a delicate tailed monad in active movement—a specimen of *Monas lens*, in fact, $\frac{1}{7500}''$ in diameter, having a distinct vacuole in the midst of the granular contents of the cell. Another ovoid body was seen, about the same size, without a tail and motionless, though it contained two nuclear particles within.‡

* It was owing to the appearance of the pellicle and the seeming likelihood of its breaking up and sinking to the bottom if allowed to remain, as I had known others do, that I was induced to open this tube so early. I thought it possible that nothing else might form afterwards. I felt anxious to be able to examine this pellicle before it got mixed with the granular matter at the bottom.

† The bacteria which appear in a simple solution of tartrate of ammonia, however, frequently remain motionless for a very long time, and the *bacteridia* of Davaine are both unjointed and motionless.

‡ Although these were the only bodies of this kind actually seen, it is worth noting that only five or six drops of about the whole ounce and a half of fluid were submitted to examination. This solution was undoubtedly examined rather prematurely. In another turnip solution which has been subsequently exposed in Dr. Frankland's digester to a temperatnre of 146° C. for a shorter time, and which was opened on the 67th day (the details of which I shall

Experiment 18.—The tube containing white sugar, tartrate of ammonia, and phosphate of soda in distilled water, was opened on the fortieth day after it had been heated in the digester.

When received from Dr. Frankland, this solution, instead of being colourless, was of a dark brownish-black colour, not unlike that of porter, this change of colour being apparently due to the conversion of some of the sugar into caramel; though it was free from any notable deposit. After about the fortieth day a thick iridescent scum gradually formed over the whole surface‡, and continued visible for more than a week. It afterwards disappeared, and then I noticed for the first time a large quantity of a brownish-black sediment at the bottom of the flask. No scum again formed on the surface, and no other change was seen to take place in the fluid.

When opened there was no very appreciable odour, though the reaction of the fluid was strongly acid.

On microscopical examination, the deposit was found to be made up in great part of dark, reddish-brown, or ruby-red coloured globules of various sizes, partly single, and partly in various kinds of aggregation. They varied in size from a minute speck $\frac{1}{20000}$" in diameter up to large spherule $\frac{1}{1428}$" in diameter. They presented no trace of structure, and were apparently quite homogeneous. Although larger, they resembled in other respects those which had been met with in the turnip solution. Other bodies, however, were seen, which presented much more obvious evidence of being concretions. They were mostly light brown in colour, some of the smaller somewhat resembling cells, with granular contents, whilst some of the larger, about $\frac{1}{2000}$" in diameter, showed concentric markings, with or without the presence of a darker central nucleus of varying size. Some again were composite structures—each spheroidal as a whole, though made up of a close radiating aggregation of ovoidal bodies around such a central nucleus. None of these spherules showed any colour reactions with the polariscope. I regarded them all as non-living concretions of a doubtful nature

Two fresh looking fungus-spores were seen, however, of the most unmistakeable character. One, apparently just about to sprout, was $\frac{1}{7600}$" in diameter; it had thick walls and a large central nucleus $\frac{1}{12000}$" in diameter. The other was a smaller and more delicate-looking body, having more the appearance of a *Torula* cell. There were also some figure-of-eight-shaped bodies about $\frac{1}{20000}$" in diameter, which were moving about most actively.

In addition, there were a multitude of particles altogether irregular in shape, in most active movement, having a protoplasmic appearance. Some were altogether irregular particles others were larger and more elaborate masses; whilst others—still exhibiting slow movements—were lozenge-shaped, or more or less cuboidal.

Experiment 19.—The tube containing tartrate of ammonia and phosphate of soda in distilled water was opened on the sixty-fifth day.

When received from Dr. Frankland, the solution in this tube was quite colourless, clear, and free from visible deposit. About the fifth or sixth day, however, after it had been suspended in a warm place, a number of small, pale, bluish-white flocculi made their appearance throughout the solution, and continued always in the same situation except when the fluid was shaken,—owing apparently to their specific weight being the same as that of the fluid itself. The contents of the flask were repeatedly scanned with the greatest care with the aid of a lens, though nothing else could be seen until about the expiration of a month. Then there was observed, attached to one of the flocculi about ¼" from the bottom of the vessel, a small, opaque, whitish speck, little bigger than a pin's point. This increased very slowly in size for the next three or four weeks, and then another smaller mass also made its appearance. At the expiration of this time the larger mass was more than ⅛" in diameter, and both could be and were seen by several people with the naked eye. In the last three weeks previous to the opening of the flask it was often remarked that the mass did not appear to have at all increased in size.

Before the flask was opened it was found that it only acted as a water-hammer to a trifling extent, though when the narrow end of the flask was broken off there was a slight dull report, and a quantity of small particles of glass were swept by the in-rush of air into the fluid. There had still, then, been a partial vacuum in the flask. The reaction of the fluid was found to be slightly acid.

This flask was opened in Dr. Sharpey's presence. He had examined the white masses previously with a pocket-len , and when the flask was broken the larger white mass issued with some of the first portions of the fluid, which were poured into a large watch-glass. This was at once taken up on the point of a penknife and transferred to a clean glass slip, when it was immersed in a drop of the experimental fluid and then protected by a thin glass cover. On microscopical examination we at once saw that the whitish mass was composed of a number of rounded and ovoidal spores, with mycelial filaments issuing from them in all

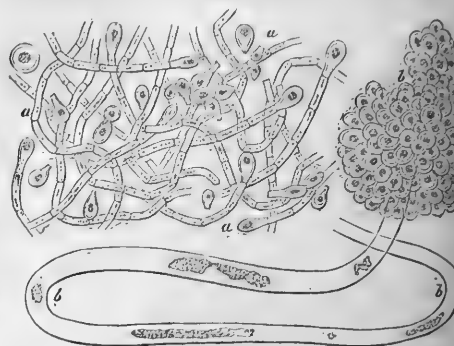


Fig. 17.—Fungus obtained from a solution of Tartrate of Ammonia and Phosphate of Soda which had been exposed to a temperature varying between 146° and 153° C. for four hours.

stages of development. The spores varied much in shape and dimensions; the prevalent size being about $\frac{1}{3500}$" in diameter, though one was seen as much as $\frac{1}{2000}$" in diameter. They all possessed a single rather large nucleus, which was mostly made up of an aggregation of granular particles. Some were just beginning to develope mycelial filaments, and others had already given origin to such filaments about $\frac{1}{4800}$" in diameter, in which there were scattered some colourless protoplasmic granules, but no vacuoles. Contiguous to these fresh and evidently Living portions of the plant, there were other parts in all stages of decay, in which the remains of the filaments were seen in the form of more or less irregular rows of brownish granules representing the altered protoplasmic contents of a previous filament whose walls were now often scarcely visible. Subsequently the smaller white mass was picked out, and this was found to contain some living mycelium and spores, and also a considerable patch of decaying filaments, in connection with which ther was a long and broader filament bearing at its distal extremity a large aggregation of more than 100 spores, quite naked, and very similar in character to those from which the mycelial threads arose. This plant was evidently a *Penicillium* quite similar to what had been obtained from a tartrate of ammonia and phosphate of soda solution on a previous occasion after twenty-one days (*Experiment* 13), and its spores were also similiar to those which had appeared in another solution (*Experiment* 11) of the same kind after eleven days.

The delicate flocculi which first made their appearance in the solution, and which persisted throughout, seemed to be made up of aggregations of the finest granules, which, however, became almost invisible when the flocculi were mounted in glycerine and carbolic acid.

Experiment 20.—The tube containing carbonate of ammonia and phosphate of soda in distilled water, was opened on the thirtieth day in the presence of Prof. Huxley.

When this flask was received from Dr. Frankland, the fluid was somewhat whitish and clouded. During the last ten days a thin pellicle had been seen gradually accumulating on its surface, and in the latter four or five days this increased much in thickness, and gradually assumed a distinct mucoid appearance. The fluid itself was tolerably clear, though an apparent turbidity was given by the presence of a fine whitish deposit on the sides of the glass.

afterwards publish), *Torula* cells, and fungus-spores with developing filaments, were formed in very large numbers, in company with other living organisms.

‡ This was much larger than that exposed in the other three vessels, owing to the fluid being contained in a small flask instead of in a tube.

When the flask was opened, the reaction of the fluid was found to be neutral.

Portions of the pellicle were at once transferred to a glass microscope slip, and then, protected by a covering glass, were submitted to a prolonged microscopical examination.* A number of little figure-of-eight particles, each half of which was $\frac{1}{20000}$" in diameter, were seen in active movement, even in situations where they could not have been influenced by currents. The portions of the pellicle were made up of large, irregular, and highly refractive protein-looking particles imbedded in a transparent jelly-like material. The particles were most varied in size and shape, being often variously branched and knobbed. There were also seen several very delicate, perfectly hyaline, vesicles about $\frac{1}{2000}$" in diameter, these being altogether free from solid contents.

A subsequent and most careful examination of a considerable quantity of the granular matter of the pellicle which had been mounted on two microscope-slips,† and at once protected by surrounding the covering glasses with cement, revealed five spherical or ovoid spores the average size of which was about $\frac{1}{3300}$" in diameter. They all possessed a more or less perfectly-formed

FIG. 18.—Fungus-spores found in a Carbonate of Ammonia and Phosphate of Soda Solution which had been exposed to a temperature varying from 146° to 153° C. for four hours.

nucleus, and all showed a most distinct doubly-contoured wall. One of the smaller of them showed that it had reached a stage where it was about to germinate. In addition, one small mass of *Sarcina* was seen, not very distinctly defined, owing to its being still in a somewhat embryonic stage.

H. CHARLTON BASTIAN

(*To be concluded.*)

NEW OBSERVATORY AT THE MAURITIUS

(*Communicated.*)

HIS Royal Highness the Duke of Edinburgh and his Excellency Sir Henry Barkly, attended by their respective suites, arrived at the site of the New Observatory at half past ten on Monday morning, May 30th, and were received by the President and Council of the Meteorological Society.

The architect of the building, Mr. Bowdler, of the Surveyor General's Department, and Mr. Horne, of the Royal Botanical Gardens, had elegantly decorated the grounds and the enclosed space around the massive stone.

The President, Captain Russell, R.N., read and presented the following address :—

"*To Captain His Royal Highness* ALFRED ERNEST ALBERT, *Duke of Edinburgh, Earl of Ulster, Earl of Kent, K.G., K.T., G.C.M.G., G.C.S.I., R.N., &c.*

"May it please your Royal Highness,

"We, the President and Council of the Meteorological Society of Mauritius, beg leave to approach your Royal Highness with the assurance of our loyalty to the Crown, and of our devoted attachment to Her Majesty the Queen and the Royal Family.

"We desire to offer to your Royal Highness a cordial welcome to these shores.

"Mauritius being admirably situated for meteorological and magnetical observation, it has long been the ardent wish of the Society that an Observatory, fully equipped wtth instruments of the most approved construction, should be permanently established for that purpose.

"Through the wise and liberal policy of the late Sir William Stevenson, and more immediately that of our present Governor and Patron, his Excellency Sir Henry Barkly, the moment for commencing the principal building has now arrived.

"We therefore hail with joy the auspicious occasion which allows us to request that your Royal Highness will be pleased to lay the Foundation Stone.

"Meteorological and magnetical researches, having for one of their main objects the safety and welfare of Navigation, touch closely the commercial prosperity of the world, and link together men of every nation by the cords of a common interest. Your Royal Highness, therefore, being not only an Officer in the Naval Service of a Queen whose ships traverse every sea, and whose Dominions extend to the remotest shores, but also the Son of an Illustrious Prince, who was ever foremost in encouraging Science, there could not, we venture to to think, be a more fitting Monument of the visit of your Royal Highness to Mauritius than a building to be specially devoted to objects with which your Royal Highness's experience cannot fail to prompt a generous sympathy.

"With our warmest wishes for your Royal Highness's welfare and happiness, and with renewed assurances of our devotion to our beloved Sovereign's Person and Family,—We have the honour to be, with the highest respect, your Royal Highness's most humble and most dutiful Servants,

"(Signed) James Tho. Russell, R.N., *President;* H. J. Jourdain, *Vice-President;* A. J. W. Arnott. *Treasurer;* J. C. Browne, C. Bruce, A. B. Commins, M. Connal, J. H. Finniss, Robt. Leech, George McIrvine, *Council;* C. Meldrum, *Secretary.*"

His Royal Highness replied in the following terms :—

"*To the President and Council of the Meteorological Society.*

"Gentlemen,

"I beg to thank you most heartily for your cordial welcome. It will afford me sincere pleasure to lay before Her Majesty the assurance of loyalty and devotion conveyed in your address.

"As an officer in the navy, I fully appreciate the advantageous position of this island for meteorological and magnetical observation, and it is with great satisfaction that I proceed to comply with your invitation of laying the foundation stone of a building to be specially devoted to objects of such importance.

"Your expressions of affectionate interest in all that concerns my future welfare and usefulness in the career which, by God's will, may be before me, are such as to demand a no less cordial response on my part.

"May this ceremony be auspicious to all concerned, and I sincerely trust that the Observatory, when completed, may realise all the expectations of its promoters, and continue throughout the future, not only a source of utility to the colony itself, but an aid to the cause of science, and a blessing to navigators throughout the world.

"(Signed) ALFRED."

The ceremony of laying the foundation stone was then proceeded with. His Royal Highness first deposited in a recess in the lower stone a bottle containing coins of the realm, a copy of the address, newspapers of the colony, and a paper on which was engrossed the following inscription :—

"This foundation stone of the Mauritius New Observatory was laid by Captain H.R.H. the Duke of Edinburgh, in presence of H. E. Sir Henry Barkly, K.C.B., F.R.S., Governor and Commander-in-Chief of Mauritius, at the request of the Meteorological Society, on Monday, the 30th of May, 1870."

His Royal Highness then spread the lower stone with mortar, and the upper stone was lowered into its bed. The Prince afterwards tried the stone with plumb-rule, level, and gavel in right masonic style, and declared it well and truly laid.

The trowel used by his Royal Highness, and presented to him by the society, was of solid silver, elegantly chased and of beautiful form. It was manufactured by Mr. G. Lewison, of Royal Street, and had engraved on it the following inscription :—

"Presented to His Royal Highness Alfred Ernest Albert, Duke of Edinburgh, K.G., K.T., G.C.S.I., R.N., by the Meteorological Society of Mauritius, at the laying of the Foundation Stone of a new Meteorological and Magnetical Observatory. Mauritius, 30th May, 1870."

The Secretary, Mr. Meldrum, then addressed his Royal Highness as follows :—

* Professor Huxley examined portions of the pellicle as soon as they were removed. He at once admitted the general resemblance as regards microscopical characters between this and the pellicle which forms on solutions containing organic matter. The particles were, however, mostly indefinite in shape rather than definite, though they were obviously imbedded in a clear jelly-like material. They became deeply tinted, also, when tested with a weak magenta solution, in the same way that the monads and bacteria of an ordinary pellicle are tinted by such a fluid, whilst the jelly-like material only assumed the faintest tinge of colour.

† These were the two specimens which were mounted in Prof Huxley's presence, and which were examined by him. After he left I spent more than two hours in thoroughly searching over these specimens with a $\frac{1}{12}$" objective. And the result was that five fungus-spores were found in the two specimens as above stated. These are still in my possession.

"May it please your Royal Highness.—On behalf of the Meteorological Society, and as Government Observer, I have the honour to thank your Royal Highness for having been graciously pleased to lay the Foundation Stone of the Mauritius New Observatory.

"I am sure I only give expression to the feelings with which the Society is animated, when I say that it will ever retain a most grateful sense of the generous sympathy and consideration which have induced your Royal Highness to come here, to-day, at personal inconvenience, to perform the interesting and important ceremony which the Society has had the extreme gratification to witness.

"If anything could enhance the pleasure which the Society now feels, it is the presence, on this auspicious occasion, of his Excellency, Sir Henry Barkly, who, during a long and an arduous administration, has not ceased to take a warm interest in the Society's objects, and to whom will be mainly due the merit of establishing in this distant Colony an Observatory which, I hope, will be second to none of the same nature in other portions of Her Majesty's dominions.

"Engaged for the most part in agricultural and commercial pursuits, but yet dependent for the necessaries of life on importations from other countries, and surrounded by a tempestuous ocean, the people of Mauritius, deeply interested in the progress of meteorological science, and many of them actively occupied in promoting it, will, I have no doubt, long preserve a fond recollection of the part which your Royal Highness has been kind enough to take in this day's proceedings.

"But the labours to be carried on here will be not merely of local utility. I trust they will also contribute to the advancement of meteorology and of terrestrial magnetism generally, as well as of certain branches of physical astronomy. In this respect their chief practical aim will be to render service to the noble profession of which your Royal Highness is so distinguished a member; and next to the pleasure of contemplating the works of the great Author of Nature, I know no stronger incentive to perseverance than the circumstance that the building about to be raised on this solitary spot, in the heart of the Indian Ocean, for the special object of conducting researches calculated to be of use to the maritime nations of the world, will in all future time be associated with the cherished name of England's sailor Prince."

His Excellency the Governor briefly addressed the audience. His Excellency said that he had always, as Mr. Meldrum remarked, taken much interest in the Society, and had done all in his power to promote its objects. It gave him great pleasure that the foundation stone of a new observatory had been laid before his departure from the colony. The ceremony could not have been more appropriately or more gracefully performed than by his Royal Highness, who was not only the second son of Her Majesty the Queen, but also a distinguished naval officer. His Excellency concluded amid general applause, by heartily wishing every possible success to what he would propose to call the Royal Alfred Observatory. Thus terminated these interesting proceedings.

SCIENTIFIC SERIALS

Journal of the Chemical Society, June, 1870.—Messrs. T. Bolas and C. E. Groves give a description of the mode of preparation and the properties of tetrabromide of carbon, the discovery of which they had announced in the preceding number of the Journal. Bisulphide of carbon was digested with an excess of bromine in the presence of terbromide of antimony or of bromide of iodine in a sealed tube of 150° for 48 hours. The bromide can also be obtained from bromopicrin and bromoform by treatment with the same reagents. Tetrabromide of carbon crystallises in white lustrous plates, fusible at 91°, nearly insoluble in water, soluble in alcohol, ether, benzol, and bisulphide of carbon, decomposed by aqueous solutions of potassa and soda at 150°. With care it may be sublimed without decomposition. By reduction by nascent hydrogen it produces bromoform and dibromide of methylene. The authors intend to study the action of argentic oxalate, cyanide, &c., on this interesting compound. Prof. A. H. Church continues his researches on new and rare Cornish minerals, giving the analysis of restormelite, which appears to be kaolinite $Al_2 O_3 2SiO_2$, 2Aq, in which some of the hydrogen is replaced by potassium and sodium and a portion of the aluminium by iron. The specific gravity is 2·58, and the hardness about 2. Chalcophillite contains 8 CuO, $Al_2 O_3$, $As_2 O_5$, 24 or 25 Aq., it loses 13·79 per cent. of water *in vacuo*, corresponding to 11 H_2O; the specific gravity is 2·44. This number also contains the commencement of a very long and elaborate paper on the combinations of carbonic anhydride with ammonia and water, by Dr. E. Divers. The author gives a history of the different compounds which he has examined, and describes no less than nine processes for the preparation of normal ammonium carbonate $CO_2 (OH_2)_2 (NH_3)_2$. Its properties and reactions are also fully given. In the second section, only a portion of which appears in this number, the preparations and properties of the half acid ammonium carbonate are detailed.

THE *Revue des Cours Scientifiques* for June 18, is almost entirely occupied with a translation of Prof. Huxley's address before the Geological Society on the course of palæontology during the last eight years. M. Bernard also proceeds with his course of lectures on suffocation by the fumes of charcoal, which is again continued in the following number, where we find also a paper read before the Congress of German Naturalists and Physicians at Innsbrück by Prof. Kékulé, on chemical work, and a review by Prof. Duclaux, of Pasteur's Researches on the Silkworm Disease. In the number for July 2, is a very important article by Prof. Agassiz on the Gulf Stream, being a report of the dredgings from the bottom of the Gulf Stream, made during the third expedition of the U.S. steamer *Bibb*. Prof. Agassiz believes that in the cretaceous period a current set in in the Atlantic from the north-east to the south-west, North and South America being then distinct continents, and that it was only at a subsequent period that communication between the Atlantic and Pacific became interrupted, and that the Gulf Stream set in in the opposite direction. In the same number is a continuation of M. Berthelot's paper on isomeric states of simple substances, treating especially of sulphur.

SOCIETIES AND ACADEMIES

LONDON

London Mathematical Society, June 9.—Prof. Cayley President, in the chair. The Hon. Sir James Cockle was elected a member. Mr. Spottiswoode, V.P., having taken the chair, the president communicated the following "Note on the Cartesian, with two imaginary axial foci." Let A, A′, B, B′, be a pair of points and antipoints; viz., A, A′, the two imaginary points, co-ords $(\pm \beta i, 0)$; B, B′, the two real points, co-ords $(0, \pm \beta)$, and write $\rho, \rho', \sigma, \sigma'$, the distances of a point (x, y) from the four points respectively, say

$$\rho = \sqrt{(x + \beta i)^2 + y^2} \qquad \rho' = \sqrt{(x - \beta i)^2 + y^2}$$

$$\sigma = \sqrt{x^2 + (y + \beta)^2} \qquad \sigma' = \sqrt{x^2 + (y - \beta)^2}$$

we have

$$\rho^2 + \rho'^2 = 2 (x^2 + y^2) - 2 \beta^2$$
$$= \sigma^2 + \sigma'^2 - 4 \beta^2$$

$$\rho\rho' = \sqrt{(x + \beta i + yi)(x + \beta i - yi)(x - \beta i + yi)(x - \beta i - yi)}$$
$$= \sigma \sigma'$$

and thence

$$(\rho + \rho')^2 = (\sigma \times \sigma')^2 - 4 \beta^2$$
$$(\rho - \rho')^2 = (\sigma - \sigma')^2 - 4 \beta^2$$

or say

$$\rho + \rho' = \sqrt{(\sigma + \sigma')^2 - 4 \beta^2}$$
$$i(\rho - \rho') = \sqrt{4 \beta^2 - (\sigma - \sigma')^2}$$

The equation of a Cartesian having the two imaginary axial foci, A, A′, is

$$(p + qi) \rho + (p - qi) \rho' + 2 k^2 = 0$$

viz., this is,

$$p (\rho + \rho') + q i (\rho - \rho') + 2 k^2 = 0$$

or what is the same thing, it is

$$p \sqrt{(\sigma + \sigma')^2 - 4 \beta^2} + q \sqrt{4 \beta^2 - (\sigma - \sigma')^2} + 2 k^2 = 0$$

which is the equation expressed in terms of the distances σ, σ', from the non-axial real foci, B, B′. The radicals are to be taken with the signs ±. This equation gives, however, the Cartesian in combination with an equal curve situate symmetrically therewith in regard to the axis of y.

The distance σ, σ' may conveniently be expressed in terms of a single variable parameter θ; in fact, we may write

$$\pm p \sqrt{(\sigma + \sigma')^2 - 4 \beta^2} = - k^2 - k\theta$$
$$\pm q \sqrt{4 \beta^2 - (\sigma - \sigma')^2} = - k^2 + k\theta$$

that is

$$\sigma + \sigma' = \sqrt{4\beta^2 + \frac{k^2}{p^2}(k + \theta)^2}$$

$$\sigma - \sigma' = \pm\sqrt{4\beta^2 - \frac{k^2}{q^2}(k - \theta)^2}$$

So that assigning to θ any given value, we have σ, σ', and thence the position of the point on the curve. We may draw the hyperbola $y^2 = 4\beta^2 + \frac{k^2}{p^2}x^2$ and the ellipse $y^2 = 4\beta^2 - \frac{k^2}{q^2}x^2$, and then measuring off in these two curves respectively the ordinates which belong to the abscissæ, $k + \theta$ for the hyperbola, $k - \theta$ for the ellipse, we have the values $\sigma + \sigma'$ and $\sigma - \sigma'$, which determine the point on the curve. Considering k, p, q, β as disposable quantities, the conics may be any ellipse and hyperbola whatever, having a pair of vertices in common. The author then proceeded to draw the curve and point out some of its peculiarities.

Mr. T. Cotterill then gave an abstract of his paper "On the intersection of curves, and a collinear correspondence in certain réseaus," going into the discussion of the following theorem:—If the assemblage of all the points of intersection of two plane curves be said to form an "intersect," and a series of groups of points, called the first, second, third, &c., be taken on a plane curve of an order > 2, such that the points in every two successive groups form an *Intersect*; then the points in any odd and any even group, or in any number of odd and an equal number of even groups, also form an *Intersect*. Amongst other applications of this theorem, certain exceptions were pointed out in the laws laid down concerning the intersection of curves. Thus a system of 13 points can be found such that a quartic through 12 of the points must pass through the 13th, whilst the quartics through the 13 points do not intersect again in three fixed points, but any such quartic is cut by any other quartic of the system in three points collinear with a fixed point on the first quartic. Hence arises a correspondence of three collinear points similar to Geiser's correspondence in cubics through seven given points. The details of the correspondence, including certain envelopes given in the paper, refer to curves of any order.

Syro-Egyptian Society, June 7.—Mr. W. H. Black in the chair. Mr. Robert Hay, of Limplum, exhibited a large and unusually fine collection of Egyptian Antiquities, the property of his late father, author of "Views in Cairo," by whom they were collected between the years 1828-33. They consist chiefly of five classes of objects. Bronzes, terra cottas, vases, funereal remains, and amulets in precious stones, and porcelain, besides six large mummies with their outer cases more or less complete. Mr. Bonomi, who gave an explanatory lecture on the various antiquities, pointed out the following objects as being noteworthy either from their rarity or beauty.

1. A large and fine bronze figure of Khonso, twenty-six inches high (No. 32), partly overlaid with antimony, and enamelled with a white opaque pigment, to represent the mysterious garment of that divinity. The head, hands, and collar had been gilt, and the eyes, staff, and mens inserted, but these latter had been lost before it was obtained by its late owner. The workmanship of the statue is of the best Theban art, and is an admirable illustration of the great chemical knowledge possessed by the ancient Egyptians.

2. A small terra cotta statue of a Royal Scribe (706), habited in the costume of the nineteenth dynasty. The seed bag hangs over the right shoulder instead of the left as usual, and the crossed hands clasp on his breast a figure of a hawk with human head and expanded wings. This bird symbolised the soul in Egyptian theroglyphy, and a like figure is used in Psalm xi. 1. Unfortunately this statue has been much injured. A second and more perfect example in limestone (No. 159) was also exhibited. Both rare specimens.

3. Four statues, one in bronze, one in stone, and two in porcelain (Nos. 12, 116, 846), of the Great Theban Divinity, Amun Ra. These figures are of rare occurrence in any material, as about the reign of Amenophis III., the worship of Amun Ra was overthrown, and his name sedulously erased from all inscriptions throughout Egypt, even upon the top of the loftiest obelisks. In a later period, that of Ramesis II., however, the worship of this divinity was resumed, and his name again appears on the monuments.

4. A porcelain amulet (894), representing a human-headed scorpion, having the tail turned up over the back. The head of this figure was originally surmounted by a crown or head-dress. Exquisite finish, probably unique.

5. A wooden tablet (539) of the kind described by some writers as astronomical, but more probably a votive stete dedicated to Homs, and intended as a charm or mystical talisman against accidents, the evil eye, and malign influences generally.

6. Several staves (572) in hard wood having a projecting spur or branch at the top. These staves were carried by the priests in funeral processions, and for some unknown reason it was necessary that they should be furnished with a short projection at a certain angle near the upper extremity, for if this were wanting in this natural stick it was supplied by art; such a specimen so furnished (573) was also exhibited.

7. A small bronze figure of the Goddess Pasht holding a shield in her right hand, with the left extended.—The preceding formed only a part of the objects successively noticed by the lecturer. Many domestic articles, models, lucernæ and personal trinkets remained, which from the lateness of the hour could not further be commented upon. The explanations of Mr. Bonomi were afterwards supplemented by various remarks from Messrs. Cooper, Cull, Hewlett Ross, Williams, Mills, Christie, Drach, W. H. Black, and other gentlemen, and it was further announced by the chairman (Mr. W. H. Black) that a special meeting would be convened for the first Tuesday in July, when the drawings, MSS., and papyri of the late Mr. Hay would be exhibited, and also a collection of photographs lately sent from Syria by Dr. Cull, in illustration of the same. It may be gratifying to our antiquarian friends to know that by special permission of the Council of the S.-E.S., the collection may be viewed during this present month at their rooms, No. 22, Hart Street, W.C., between the hours of eleven and four, where all particulars respecting the destination of the antiquities may be obtained.

DUBLIN

Royal Irish Academy, June 13.—Rev. Professor Jellett, F.T.C.D., president, in the chair. The minutes of the previous meeting were read and confirmed. Dr. Sigerson, F.L.S., read a paper entitled "Further Researches on the Atmosphere." He showed that his former discoveries had been confirmed by later investigations, and proceeded to detail the results of his examination of "special atmospheres." His lecture was illustrated with large diagrams representing some of the objects discovered. In "iron factory" air he found carbon, ash, and iron. The iron was discovered to be hollow balls averaging one two-thousandth part of an inch in diameter, their shell being only one thirty-thousandth, and the iron was then found to be translucid. In "shirt factory" air there were found to be filaments of linen and cotton with minute eggs. The air of thrashing and oat-mills had fibres, fragments, starch grains, and spores. Scutch mills, from the character and quantity of the spongy, spiky dust and its effects, he declared to be human slaughter-houses. By a suggested alteration, the works could be kept free of the dust, and many lives saved. In the air of printing offices antimony was expected, and Dr Sullivan's analysis confirmed the expectation. It is, probably, present to an injurious extent in type-foundries. Stable air was shown to contain scales and hairs, and hair-dressers had a similar atmosphere. Smoke being microscopically examined, the tobacco-smoker's air was shown to contain globules of nicotine. He concluded by stating conclusions arrived at with regard to lung-functions and contagion. The results of Dr. Sigerson's investigations proved, as might be expected, that the air in special workshops contained floating in it particles of the *débris* of the different materials operated on in these particular localities. Some discussion followed, at the conclusion of which, on the motion of Prof. Sullivan, the paper was referred to the Council for publication. John P. Keane, C.E., Calcutta, and Hugh Leonard, Kylemore, were elected members of the Academy.

PARIS

Academy of Sciences, June 20.—M. R. Clausius presented a note on a quantity analogous to the potential and on a theorem relating to it. M. Bertrand communicated a note by M. G. Darroux on the surface of the centres of curvature of an algebraic surface.—A memoir was read by M. Maurice Lévy on the general equations of the internal movements of ductile solid bodies beyond the limits at which elasticity can restore them to their original state.—The following papers on subjects connected with physics were presented to the meeting:—On the variations of temperature produced by the mixture of two liquids, by M. Jamin, upon which M. Bussey made some remarks.—On the electrical effects produced by the contact of inoxidisable metals with acids and neutral and saturated saline solutions, and on capillary affinities, by M. Becquerel. The author describes the results obtained with wires of pure platinum and gold, treated as described in a former communication, but immersed in acid or saline fluids.—Determination of the terrestrial-magnetic intensity in absolute value, by MM. A. Cornu and J. Baille, presented by M. E. Becquerel, in which the authors describe a series of experiments made in accordance with the methods of Gauss and Weber.—Experimental researches upon the duration of the electric spark, by MM. Lucas and Cazin, also presented by M. E. Becquerel.—On the law of the points of congelation of saline solutions, by M. Güldberg.—A note by M. G. Rayet on the reversal of the two sodium lines in the spectrum of the light of a solar protuberance was presented by M. Delaunay.—M. Periseux communicated a note on the transit of Venus in 1874, containing a numerical correction of his former paper on this subject.—M. Delaunay presented a note by M. H. Tarry on the so-called showers of dust and blood. The author notices the storm phenomena preceding the showers of sand which fell in the south of Europe on the 10th and 24th March, 1869, and the 14th February, 1870, indicating that in each case a great barometric depression, accompanied by a violent wind storm, travelled from the north to the south of Europe, crossing into Africa, and returning thence laden with sand from the Sahara. He identifies the material deposited in these storms in all cases with the Sahara sand.—M. C. Dufour read a note relating to magnetic perturbations observed by Saussure at the Col du Géant before the great storm of 1788. M. H. Sainte Claire Deville communicated a note by M. C. Schlœsing on the precipitation of mud by very dilute alkaline solutions. The author stated that a very small quantity of a saline solution facilitates the deposition by water of earthy matter held in suspension by it, and that a deficiency of saline constituents is often the cause of water remaining muddy when standing in clearing tanks.—A note by M. Scheurer Kestner on the composition of crude soda and the loss of sodium caused by the adoption of Le Blanc's process was presented by M. Balard. The author noticed the chief compounds with which crude soda is contaminated, and stated as the result of his researches upon Le Blanc's method that in the fusion of crude soda there is no reduction of soda-salts into metallic sodium, and that the greater part of the loss experienced is due to the formation of insoluble compounds, and averages about 5 per cent.—M. Bertrand communicated a note by M. Rabuteau, on a new, simple, and rapid mode of quantitative determination of the ammoniacal salts, and on the reason why these salts can exist normally in the organism only in infinitesimal quantities. The author stated that chloride of soda prepared by pouring a solution of two parts of carbonate of soda into one part of chloride of lime contains an excess of carbonate and free soda, and that, by the aid of heat, this solution decomposes ammoniacal salts with evolution of nitrogen. From the amount of nitrogen evolved, the quantity of ammonia may be calculated. He considered that the alkalinity of the blood would enable it in like manner to decompose any ammoniacal salts contained in it.—A note on the tribromhydrines, by M. Berthelot, was presented by M. Bertrand, in which the author maintained the isomerism of those compounds in opposition to the opinion of M. Henry.—M. H. Sainte-Claire Deville communicated a note by M. Fontaine on the preparation of bibrominated ethylene, in which the author described a new method of obtaining that compound.—M. Wurtz presented a note on an aromatic glycol, by M. E. Grimaux.—An extract from a letter from M. Pasteur to Marshal Vaillant, describing the results obtained in breeding French races of silkworms, at Villa-Vicentina, was read.—M. C. Robin presented a note by M. Picot containing the results of some experimental researches upon suppurative inflammation and the passage of leucocytes through the walls of vessels.

June 27.—M. Serret presented a note by M. R. Hoppe on a corollary to a theorem of Mr. Crofton's; and a note by M. F. Lucas on some new properties of the potential function was communicated by M. Delaunay.—M. d'Abbadie presented a note by M. J. Hoüel on the selection of the angular unit, containing a further discussion of the question of the decimal division of the quadrant or the circle, and supporting the former as the unit. M. Yvon Villarceau remarked upon this paper, and maintained that the decimal division of the circle is preferable to that of the quadrant.—M. H. Sainte-Claire Deville read a paper entitled "Observations with regard to a Note by M. Jamin on the Variations of Temperature produced by the mixture of two Liquids."—A note by M. J. Chautard on the direction of the currents induced by means of electrical discharges was read.—A letter from Mr. C. K. Akin, claiming priority in the method of calorimetry employed by M. Jamin and ascribed to M. Pfaundler, was read; and M. Neyreneuf presented a note on the phenomena of electrical condensation.—A paper by M. Martin de Brettes on the determination of the thickness of iron casing that can be traversed by a projectile of which the weight, the calibre, and the striking velocity are known, was read, indicating the formulas to be employed, and giving a table of experimental results contrasted with those obtained by calculation.—A paper by Mr. J. N. Lockyer on the last eclipse of the sun observed in the United States was read.—The following papers on subjects connected with chemistry was communicated:—Investigations on some new derivations of triæthylphosphine, by MM. A. Cahours and H. Gal; letters by M. H. Bouilhet and Klein on the deposition of nickel by galvanism, presented by M. Dumas; on a new method of preparation of the chlorobrominated organic compounds by M. Henry; on silicopropionic acid by MM. Friedel and Ladenburg, upon which MM. Dumas and Thenard made some remarks; and a note on phospho-platinic compounds by M. Schützenberger.—A note by M. Montagna was read noticing the occurrence of organic remains in rocks regarded as of igneous origin; and M. de Verneuil made some remarks on a memoir by M. Dieulafait on the zone of *Avicula contorta*, and the Infralias in the south and south-east of France.—M. de Clos communicated a note on the germination (or twin growth) of the whorls of floral axes in the Alismaceæ.—The remaining papers read need no notice, except one, of which unfortunately the title only is given us, in which M. Tremblay suggested *a means of terminating the present drought.*

BOOKS RECEIVED

ENGLISH.—The Revival of Philosophy at Cambridge: C. M. Ingleby (Hall and Son).

FOREIGN.—Annales de Chimie et de Physique, No. XX.—Annalen der Physique und Chemie, No. 5, Repertorium, Heft iv.—(Through Williams and Norgate)—Traité d' Histoire naturelle: M. A. T. Nogués.—Jahresbericht über die Leistungen der chemischen Technologie für 1869: J. A. Wagner.—Prodromus Floræ Hispanicæ, Vol. I.—Etudes sur les Diatomacées: Ch. Manourry.

CONTENTS

THURSDAY, JULY 14, 1870

THE UNION OF THE ELEMENTARY TEACHING OF SCIENCE AND MATHEMATICS

ATTENTION is being more and more given to the teaching of science as a means of education. The object of education may be regarded as being to help people to think for themselves; and our duty in practically educating people is two-fold; we must supply them both with the materials for thought and with the method of thinking. It is in this latter respect that the scientific education which has as yet chiefly been given appears to us to have been somewhat deficient. Yet this is the most important part; for materials for thought are supplied by nature itself, whereas the method of scientific thought and reasoning is the result of the world's progress, and to inculcate that method ought to be our chief aim. The excellence of science as a means of teaching people how to think, consists in two things: first, that the facts with which it has to deal are real tangible things, and secondly that the method of reasoning which it applies to these facts is accurate; for if, in any part of science, the accuracy of mathematical reasoning is not attained, at least we can always put down our finger distinctly on the places where it is and where it is not attained. Now the error which has been made, and which is constantly being made, in the teaching of science throughout the country, is that these accurate methods of reasoning, and these tangible facts, have been separated from one another. In our boys' schools and elsewhere (with very few exceptions), mathematics are taught wholly as applied to hypothetical cases, if even so much as that. They are thus the driest bones of method, or like a mill grinding without any corn in it. A man who learns method in this way is like a man who learns anatomy from diagrams and not from the human body itself. On the other hand the facts with which science has to deal are, with very few exceptions, almost everywhere brought forward as isolated facts, or their connection is treated in such a way that the true scientific accuracy of reasoning, by which that connection is demonstrated, is either omitted, or receives an altogether unimportant place. In the more advanced walks of science and mathematics the University of Cambridge is perhaps primarily to blame for this separation between the scientific method and the facts of science. But that University is rapidly making amends for its previous errors, and is, perhaps, pursuing as direct a course as possible towards the reunion of these two. Perhaps it may be that our teachers of schools and others, coming mostly from the University, have carried this unfair dichotomy into their own teaching. The fact is that we are only just beginning to awake all through the country to the immense benefit to be gained from scientific teaching, and we could not expect that, until this awakening had occurred, science should have been properly taught. At the University there are certainly the most unbounded facilities for the true teaching of science, in the maturer minds and more advanced mathematical acquirements of our students; but at the same time, the problem of uniting at schools the study of mathematics with that of the facts of nature is exactly the same problem. It is often objected, both by those engaged in teaching and by others, that an extensive knowledge of mathematics is required before we can apply it to Physical Science. This belief is perhaps one of the things which stand most in the way of the true teaching of science; but the belief is entirely erroneous. Of course a person must first acquire a knowledge of the technical forms of mathematical expressions which are to be used; but it has been found that at the very earliest stages of the learning of any mathematical subject, the application of it to the facts of nature may be taught. In this way a new life is given to the whole study, and a comprehension of it is attained, which is, at least in most cases, otherwise quite unattainable. Let us take an instance.

The subject called "Variation" in Algebra is exceedingly uninteresting as it is usually taught; a sharp boy or girl regards it as, for the most part, but a poor and unnecessary substitute for the rules of proportion, and the applications of the rules for variation are learned from such cases and exercises as have no connection with any subject of interest. Yet, as every one knows, in every part of physical science the facts which are eventually to take the form of equations present themselves as problems in variation; as, for instance, "Ohm's laws." It has been observed that where the subject of electricity is taught (and we are happy to say that it is now sometimes taught to boys and girls) such a matter as the establishment of Ohm's laws is left out; if taught they are taught as results which have been arrived at, as of immense importance of course, but, at the same time, the train of reasoning whereby they are demonstrated is omitted, just *because* it involves the use of the rules of variation. And so indeed it will always be, so long as the plan is adopted of teaching people these and similar rules as abstract things. For when rules so taught come to be applied, the mind has to fit itself first into quite a new way of looking at things, and waste of both time and trouble is the result. On the contrary, it has been found that a child whose knowledge of algebra extends only to the elementary rules, may be successfully introduced to the subject of variation by an experimental demonstration of Ohm's laws. By this means both subjects are much better understood, and the child feels himself in possession of quite a new power, whereby not only the intellectual, but also the moral benefits resulting from education are intensified. This is only an individual example, but in all cases it seems clear that the teaching of the rules of mathematics and of the facts of nature should be, and can be, even in the most elementary instances, so adapted as to go hand in hand. An opportune and careful assistance enables the pupil to elicit for himself these rules as the most distinct form of expression of the facts of nature, and these rules enable him to trace the connection between these facts. The elementary parts of physical science teem with opportunities for the elucidation and application of the elementary rules of mathematics, the full force of which is not rightly understood until they are applied.

A question which continually arises in the mind of an intelligent pupil is: "How did this or that piece of mathematics come to be used?" This is a question the need of which ought not to exist; for the pupil should be introduced to each piece of mathematics by the process of, as it were, himself discovering that it is the proper mode of dealing with some natural fact. By this method of teaching, that best of all faculties, originality, is fostered, whereby we mean not the making of new discoveries, but the habit of taking our own view of everything; in other words, the habit of independent thought, which habit is the nurse of Freedom. It is precisely because of its immense power of fostering this habit that we believe the teaching of science properly conducted is such a very desirable part of education. The difficulty of carrying out the joint teaching of the facts of science and the methods of mathematics, lies chiefly in the difficulty of getting men who are able to do it, that is to say, men who have a sufficient acquaintance with both subjects. This is the great temporary drawback to the spread of true scientific education. But as soon as the true character of that education is recognised, this drawback will only be temporary. Here it is, however, that the progress of these ideas at the University is so much to be desired. And here it is that the University is, let us hope, hastening to confer a great benefit on the country, by providing, for the teaching of others, men whose education has itself been carried out on this principle. This is one of the reasons for which we hope that there will be no long delay in the establishment of lectures there of an experimental nature on Physical Science. The benefit conferred by these, however, will not be complete, until it is arranged that the taking of a mathematical degree shall have ensured the knowledge of such subjects; and the possession of a certificate of attendance on some such course of lectures might well be imposed as a necessary preliminary to taking a degree with mathematical honours. Indeed we may hope that the day may not be far distant when some experimental knowledge of Physical Science will be demanded from all Cambridge students at the "little-go," along with the present Latin, Greek, and Mathematics, for this, more than anything else, would conduce to that which is now so eminently desirable—the existence of a body of persons able to carry out this joint teaching.

FORMS OF ANIMAL LIFE

Forms of Animal Life; being Outlines of Zoological Classification, based upon Anatomical Investigation, and illustrated by Descriptions of Specimens and of Figures. By George Rolleston, D.M., F.R.S., Linacre Professor of Anatomy and Physiology in the University of Oxford. (Oxford: Macmillan and Co., 1870; Clarendon Press Series.) II.

THE second part of this work consists of elaborate descriptions of fifty preparations in the New Museum at Oxford, designed to illustrate some of the typical specimens of the several animal classes. Thus, among Vertebrata, we have a dissection of the common rat, the skeleton of the same, separate vertebræ of the rabbit, the dissection and the skeleton of a pigeon, the bones of the head and trunk of a fowl, a dissection of the common English snake, vertebræ of a python, dissections and skeletons of a frog and a perch, and vertebræ of a cod.

These descriptions will no doubt be exceedingly useful to the author's pupils, but for others statements that "a black bristle has been passed under the aorta," or "a slip of blue paper under a fascicle of one of these muscles" do not afford much help. Moreover, there is a singular absence of directions how the student is to make these preparations for himself; directions which no one could give better than Professor Rolleston. It would surely have been a more desirable course to print this second part separately in the Museum catalogue; and, instead of mere descriptions of plates, drawn from ready-made dissections, to have given a full account of how first to catch, then to kill, and then to dissect and preserve the several animals mentioned in the third part of the book. The satisfactory way in which careful methods of dissection will preserve the whole of an animal for demonstration was lately well shown by the Curator of the Hunterian Museum, who prepared from a single very poor specimen of *Proteles cristatus* the complete skeleton (articulated so as to allow of each bone being removed without disturbing the rest), the stuffed skin, and all the important viscera. Now, methods of dissection of the so-called lower animals, are just what students of comparative anatomy want; and monographs of the anatomy of a single species like those of Bojanus, or of Krause, are rare even in Germany. Therefore knowing the admirable way in which practical zootomy is taught by Professor Rolleston, we had hoped that, following out his motto παντὸς προσθεῖναι τὸ ἐλλεῖπον, he would have described the steps of the several dissections so that other students might profit by his experience. The plates in the present work would serve very well to illustrate such a manual of dissection, especially if aided by such rough diagrams of relations of parts as every lecturer makes for himself on the black board.

Most of the plates in the third part are copied from actual preparations, and are evidently done with great pains; but it would have been well if some notion of the scale on which they are drawn had been added. The bibliography at the end of each description in Parts II. and III. is very valuable; indeed references are fully given throughout the book.

In the account of the dissection of *Helix pomatia* (pp. 48–54), we look with interest for any new facts as to the existence of a capillary systemic as well as pulmonary circulation in the Gasteropoda; since it has been stated that Mr. Robertson (who prepared all the specimens described) succeeded in demonstrating by injections that the supposed systemic lacunæ are only due to extravasation. This, however, appears not to be the case. We may particularly recommend the account given both of the shell and the soft parts of *Anodonta cygnea* (pp. 54-66, and also the description of Pl. V.), and the comparison with it of *Ascidia affinis* which follows. But perhaps the most valuable description is that of *Astacus fluviatilis* (pp. 90–119, and 205–210*), and especially three tables, of which the first compares the post-oral ganglia in *Astacus*, *Scorpio* and *Sphinx*, both at an early and at the adult period; while the second makes a similar comparison between the same ganglia in the *Amphipoda*, *Isopoda*, and *Orthoptera* (an order of insects which Prof. Rolleston regards as the least differentiated, and approaching nearest to Crustacea); and the third gives a view of the homologies of all the post-antennary segments and appendages in the four Arthropod classes. The views of Prof. Huxley are followed where he differs from M. Milne-Edwards, and the grounds of the several comparisons are clearly stated.

* In Pl. VII. it ought to have been noted that the longitudinal division of the body is not quite complete, so that the *left* eye, antenna and antennule, are seen—all the other appendages belonging to the *right* side.

The Entozoa are illustrated by the *Coenurus* of the rabbit's muscle, which is regarded as probably of "the same species as the one individuals from which are, when in the cystic state, lodged usually in the brain of the sheep, and are the cause of the disease commonly known as the 'sturdy,' 'gid,' 'staggers,' or 'turnsick'" (p. 136). So that *C. cuniculi* is identical with *C. cerebralis.* There are also some diagrams of parts of Tænia in the cestoid state (pp. 246-252), from Leuckart and Van Beneden, with descriptions.

Several new terms are introduced in this work, and most of them are likely to be useful additions to nomenclature. Among them are the words "proctuchous" and "aproctous," which we have only seen before used to distinguish the *Turbellaria* with an anus from those without one. Would it not be possible to substitute *Brachionopoda* for the barbarous word "Brachiopoda," which is moreover too near to "Branchiopoda?" Several terms in common use are given by Dr. Rolleston in an improved form; and we think him quite justified in substituting *Myriopodia* for Myriapoda, *Annulata* for Annelida (which is only a pseudo-classical form of "Les Annelides," the name invented by Lamarck), and *Hedriophthalmata* for Edriophthalmata —though if this last change be made, it will be as well to write "*Hedræophthalma*," since the root must be ἑδραῖος, not the doubtful diminutive ἕδριον, and the analogy of μονόφθαλμος, μυριόφθαλμος points out the true form of the termination. In the same way the word *Echinodermata* (which was first correctly applied by Stein to the *shells* of Echini) and *Pachydermata* ought to be written Echinoderma and Pachyderma, especially as the latter form is actually used by Aristotle. But there are probably too many words of this termination for the change to be easily accomplished. Useless synonyms, however, ought undoubtedly to be abandoned; such troublesome words, for instance, as Actinozoa, Ascidioidea, and Bryozoa, ought to yield to *Anthozoa*, *Tunicata*, and *Polyzoa*, for reasons of convenience, euphony, and priority.

The student will find it useful to note the following errata, in addition to those indicated in the book itself.

P. xiv. *monophydont* for *monophyodont* and *Montremata* for *Monotremata;* p. li., *furculum* for *furcula*, repeated p. 21 *bis* and p. 22; p. ciii., *classes Brachiopoda* for *class Brachiopoda;* p. 21, *obtusator* for *obturator;* p. 25, *Wirbel thiere* for *Wirbelthiere;* p. 35, *Körperban* for *Körperbau;* p. 78, *differs from the imago* for *differs from the larva;* p. 136, and again pp. 241 and 251, *Caenurus* for *Coenurus;* p. 160, *coenosarc* for *coenosarc;* p. 224, *two sets of them and two rays respectively* for *two sets of three, &c.;* p. 252, *Thudichen* for *Thudichum;* and, in the index, *Vesicular* should read *Vesiculae*, while the reference to Prof. Turner will be found at p. 1. of the second part instead of p. i. of the Introduction. Lastly, at p. 251, line 10 from the bottom of the page, the figure 6 is misprinted for the letter *b*.

It is much to be hoped that a second edition of this thorough and painstaking work will soon be called for; since nothing would better prove the increased number of serious zoologists in this country. Should its learned author then see fit to unite the second and third parts together, and to add somewhat full directions for dissection, I venture to think that the book will be made even more useful than it already is.

P. H. PYE-SMITH

NEW ATLASES

The Complete Atlas of Modern, Classical, and Celestial Maps, together with Plans of the principal Cities of the World, constructed and engraved on steel under the superintendence of the Society for the Diffusion of Useful Knowledge, and including all the recent geographical discoveries, compiled from the latest and most authentic sources. Accompanied by alphabetical indexes to the modern and classical maps. 218 maps and plans.

The Family Atlas, containing 80 Maps, constructed by eminent Geographers, and engraved on steel under the superintendence of the Society for the Diffusion of Useful Knowledge, including the Geological Map of England and Wales, by Sir R. I. Murchison, F.R.S.; the Star Maps, by Sir John Lubbock, Bart., F.R.S.; and the Plans of London and Paris, with the new discoveries and other improvements to the latest date, and an Alphabetical Index.

The Cyclopædian, or Atlas of General Maps, with an Index of the Principal Places in the World. 39 maps.

(London: Edward Stanford, 6 & 7, Charing Cross. 1870.)

AT the present time not a week passes which does not more and more enforce the necessity of everyone of us having an atlas of some sort or another to refer to, and, in fact, it may be said that in these days of rapid locomotion and intercommunication with every part of the planet, an atlas is the corner-stone of a library, even if that library otherwise consist merely of a Dictionary and a Blue Book, Imperial Calendar, or Post-office Directory.

Take the last six months. We have all of us been burning to know the ins and outs of that part of Africa, so far as they have been mapped, which we hope that Livingstone is still exploring for us. The Suez Canal has not only, to the amazement of school boys, made Africa an island as well as a continent; but children of a larger growth have had to talk about Port Saïd, the Bitter Lakes, and the Sweet Water Canal, thereby opening up a new region of minute geography; while the Pacific Railroad, now a *fait accompli*, has carried us at once into a part of the world about which the majority of us had thought but little; and we might easily go on multiplying other instances.

Geography, in fact, is now not only "one of the eyes of history," but it is the "eye" of every-day life among all Anglo-Saxon communities; and from this point of view it is satisfactory to see our foremost geographers and purveyors of maps keeping pace with the times, including "all the recent discoveries" in their publications, and bringing this very important information fairly within the reach of all. Let us add, that it is also satisfactory to see them little by little approaching the German standard of map-making, which, to speak candidly, they have not all reached.

We shall clear the ground for what we have to say of the atlases now under notice, if we state that the maps in all of them belong to the series brought out some time ago by the Society for the Diffusion of Useful Knowledge; that the two first on our list are of the size of the maps, and are for the library, while the third contains the maps folded, and will suit either a school-boy or a family of small geographical requirements. The Complete Atlas differs from the Family one in having classical maps and a large number of plans of cities; the modern maps being pretty much the same in both; both also contain maps of the stars. Sir Roderick Murchison's geological map of England and Wales, however, is to be found in the Family Atlas only. All of them contain a most valuable index of places, so that we have on the whole a very practical gradation to suit all requirements, the *quality* being the same but the *quantity* varying.

There is one very admirable point in the arrangement of the Complete Atlas which at the same time reminds us that it is not so complete as we are sure Mr. Stanford will make some edition of it in the more or less remote future. Side by side with the modern (politically divided) map we have the ancient (politically divided) map of the same area, and in this point the Complete Atlas will commend itself to all scholars; but we miss very much indeed the physical maps of the larger areas, and in the interests of physical geography we feel bound to insist strongly on this point, because we are convinced that the importance of such maps to those who want a large atlas is becoming so great that it will not be borne that they shall be relegated to a separate volume.

By many, and those especially who are content with the modern world, the Family Atlas will commend itself by its index-like arrangement, by which the names of all the maps are visible down the side, and the sides of the foremost maps being cut away, any map may be at once turned to.

This much premised, we may state that we have examined the maps and plans very carefully, and find them as a rule as good as any English maps extant, and honestly brought down to date. Mr. Stanford deserves great credit for the admirable and careful way in which this has been done, and we say this the more strongly because we know the immense labour and expense involved in altering map plates from time to time. Of course, in some cases, it has been simply impossible to alter the plates, the alterations have been too great. Take for instance the plans of New York, extending to Forty-second Street only, and Boston, in which the waste space shown in the map, west of the public garden, is now covered with houses. In other cases all the care has been displayed in the detail map, the general map having escaped revision, or *vice versa; e.g.*, in the map of British North America, Russian America is retained, while in the general map it is correctly omitted; in the general map of Canada and the United States the chief town of Iowa is shown as Iowa City, while, in the detail map, Des Moines is correctly given. We could have wished too to see Patagonia, a time-honoured name, divided, as it really is, between the Argentine Republic and Chili; and we have an idea, too, that by an Order in Council, or some such terrible enactment, the "improper" name of Van Dieman's Land has been altered to Tasmania! We notice these points, not as blemishes by any means, but as indications of a more or less minute revision which we are sure Mr. Stanford would have otherwise undertaken, of a collection of maps of which English geography may be proud.

OUR BOOK SHELF

Birds of Marlborough, being a Contribution to the Ornithology of the District. By Everard F. im Thurn. (12mo. pp. 117. Marlborough and London, 1870.)

THIS unpretending little book affords an additional piece of evidence, if more were needed, that science in some form or other is making its way into our schools. A few years ago it was well remarked by one who had given no small attention to the matter, that the relations of the universities and public schools, as regarded science, formed a "vicious circle"—on the one hand the public schools demurred to its encouragement because it did not "pay" their pupils when they reached the university, and on the other the universities hesitated about rewarding scientific studies because they were pursued by intellects comparatively inferior to those which were devoted to the older branches of learning. This state of things clearly admitted of a remedy; either great power of itself could make the first step; but it was certainly the duty of the universities to take the lead in moving. It must depend on them, and on them alone, to alter and improve the whole higher education of our countrymen, for the curriculum of any public school is almost exclusively prepared with reference to the requirements of the universities and the rewards for proficiency that they offer.* They have but to declare that their emoluments and privileges are accessible to excellence in every branch of human knowledge, instead of confining these encouragements to some very few alone, and leave the public schools to respond to the call. With skilful gardeners these nurseries will speedily grow the plants required; the germs are already there, and under the sunny smiles of pedagogic favour and the golden rain of prizes, vigorous saplings will be transplanted to the Groves of Academe, there to hold their heads as high as their rivals from the primæval forests of classics and mathematics, and (may we say?) to be finally of greater utility.

If Mr. im Thurn's book, as might be expected from the performance of so youthful an author, does not contain any addition to science, it will, of course, be interesting to Marlburians as the work of one who has just ceased from being a schoolboy; but its chief value lies in the fact of its indicating the presence of the promising germs we have mentioned above, of the excellent forcing pit found in the Marlborough College Natural History Society, and of the skilful gardener, Mr. T. A. Preston.

Gymnastics for Ladies. Madame Brenner.

ALTHOUGH many of our large towns are now provided with gymnasiums at which ladies' classes have been established, the subject is but little appreciated, especially, in some more important cases, among the ladies themselves. There can be no doubt that for growing girls a large airy room, provided with suitable apparatus, and where a loose easy dress is a necessary condition, must be advantageous, if the exercises performed are such as to induce emulation without over-exertion. When we consider at how much earlier an age "romping" is prohibited to girls than to boys, and how little there is in the routine of a girl's life to correspond to the cricket and rowing which form the best part of her brother's recreations, we think the fact offers a very probable explanation of the increasing languor and delicacy of the ladies of the period. Breadmaking and other manual duties are being superseded by reading and preparing for examinations, and we must, therefore, look to artificial means to preserve a just balance between mental and physical development.

Madame Brenner's book is little more than an advertisement of her class in Bruton Street, being a description of those exercises which she teaches, enlivened by rather severe criticisms of those which others teach. Still we hope her book will find many readers, as the graceful

* See Rep. Brit. Assoc. Dundee, p. xliv.

illustrations, the strains of lively music which we are told accompany every movement, and, above all, the repeated assurance that the ladies need do no more than they like, will all tend to persuade parents and daughters that gymnastics are very pleasant and desirable.

On Eozoön Canadense. By Professors King and Rowney. 8vo. (Dublin, 1870.)

THIS reprint from the Proceedings of the Royal Irish Academy, treats of a controverted subject of considerable interest to geologists and zoologists, namely, the nature of certain Canadian and other serpentinous limestones in which Logan, Dawson, Sterry Hunt, Carpenter, Jones, Gümbel, and others believe they find definite traces of a foraminifer known as *Eozoön*. Great difference of opinion on the subject under notice has been expressed during discussions before learned societies and in memoirs written by geologists, some seeing under the microscope good proofs of the presence of foraminiferal structure; and these observers are mainly rhizopodists well acquainted with the peculiar structures of shelled protozoa, others finding nothing but inorganic fibres, globules, flocculi, &c., of mineral matter in both the Canadian and any other similar serpentinous marbles. Among the latter disputants are Doctors King and Rowney; and in the paper before us there are some new descriptions and figures of specimens illustrative of the structure of certain ophitic rocks from different countries, and likely to be of use to "eozoönal" students, enlarging their field of observation, and aiding them, perhaps, in arriving at definite conclusions. The figures, however, are little better than diagrams, and cannot help the student much. The paper is largely composed of criticisms on the researches and remarks of others, in a highly disputatious form, and not enriched with anything new to those who have thoroughly studied the matter, either mineralogically or from a zoological point of view. The following important facts do not appear to be recognised by the authors: first, that ophites, on the one hand, may not be really "eozoönal" and yet have mineral structure resembling in one point or another what occurs in *Eozoön*; secondly, that true eozoönal rock is often so greatly crumpled up in its metamorphic state, that patches only of the organic structure are found here and there amongst the somewhat similar ophitic mass of granules and fibres.

Die Ophthalmologische Physik, und ihre Anwendung auf die Praxis. Von Dr. Hugo Gerold, of Giessen. Part II. (Vienna, 1870. London: Williams and Norgate.)

THE advances in the department of Ophthalmology have of late years been so rapid and important, that either thoroughly-revised editions of the standard works or altogether new books have become a sheer necessity. The volume before us comes under the latter category, and is the work of a gentleman well known as an able physicist. The present part is occupied with the Dioptrics of the Eye; the defects in it that are due to spherical and chromatic aberration; the terminology employed to indicate the different functional relations of the several parts to one another and to light, as æquatorial, median, and sagittal planes, axes, visual lines, field of vision, angle of elevation, &c.; the principles of perspective and of the construction of the microscope, the ophthalmoscopic investigation of the eye, and the adaptation of convex and concave lenses for hypermetropia or myopia, and lastly, a section on light and colour. The parts we have read appear to be clearly and intelligibly given, and with something like French method and order. The mathematical formulæ introduced are not beyond the comprehension of an ordinary well-instructed reader, and the diagrams are numerous (123 in number) and instructive.

H. P.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Prof. Pritchard and Mr. Proctor

IT has been pointed out to me that Prof. Pritchard, engaged as he is in many important avocations, may quite unwittingly have misjudged my treatise on the Plurality of Worlds. I readily (eagerly) admit this, and also that, in this case, I owe the esteemed Savilian professor an apology for suggesting that he has intentionally wronged me.

The matter is now reduced to a simple issue. I have submitted considerations which are sufficient to convince Prof. Pritchard that his critique is not just. If he withdraws his unfavourable comments, as resulting from accidental misconception, I shall be bound to apologise for too hastily charging him with deliberate unfairness. If he will not, I cannot truthfully withdraw my objections. I will not endure to be represented as speaking severely (and by inference unfairly) of men for whom I have (and have expressed) a most sincere and unqualified admiration—of such men, to wit, as the Herschels, Tyndall, Lassell, Balfour Stewart, and Sir W. Thomson.

RICHARD A. PROCTOR

Whence Come Meteorites?

I HAVE read, with great interest, in the number of June 2nd of your journal the article which Mr. N. S. Maskelyne has devoted to the examination of my theory on the Origin of Meteorites. I request permission to offer some observations on the criticisms of that learned mineralogist.

Although Mr. Maskelyne concludes by saying that, in his opinion, I have not attained the end which I had proposed to myself, I will attempt to show that my system has, in fact, perfectly resisted his attacks.

In truth, the views I have been led to take on the subject of meteorites are not by any means a simple fruit of my imagination. I have been led to them by the observations of material facts easy of verification; and it is only in the background, so to speak, that I have brought under consideration different consequences, which may certainly be matter for discussion. Now, in Mr. Maskelyne's argument, he has given the place of honour to these secondary considerations, whilst he has left the real substance of the question completely in the shade. A few lines will suffice to justify my assertion.

The chemical and mineralogical study of the specimens which compose the rich collections of meteorites at the Museum of the Jardin des Plantes has made me acquainted with *polygenic* masses—that is to say, masses formed of angular fragments soldered together, but possessing each one such decidedly separate characters that it is impossible to suppose that they were originally produced in the forms and in the relative positions which they present at the present day. These clastic meteorites had been previously studied; but not, as far as I am aware, from the point of view at which I have placed myself.

From the studies and experiments I have made on this subject results the indubitable fact that the fragments, the union of which constitutes various clastic meteorites are, each one, completely identical with well-known monogenic meteorites. It is thus, that the clastic meteorite of St. Mesmin (May 30, 1866) contains angular fragments rigorously the same in every respect as those which would be produced by breaking up the meteorite of Lucé (Sept. 30, 1768); fragments soldered together by a dark coloured cement exactly similar to the substance which forms the principal mass of the stone of Limerick (Sept. 30, 1813). It is thus also that in the same cement, the meteorite of Canellas (May 14, 1861), contains fragments of a rock impossible to distinguish from that of which the mass of Montrejeau (Dec. 9, 1858) is a specimen.

How is it possible to understand these positive facts without having recourse to the explanation, so evidently true of terrestrial fragments? For fragments of two distinct rocks to be found associated in one clastic mass, it is absolutely necessary that these two rocks should come from a region where they were in connection. Thus, on one hand, the rocks of Lucé and of Limerick were in connection; thus, on the other hand, the rocks of Montrejeau and Limerick were in connection; then, in conclusion, the rocks of Lucé and of Montrejeau were in connection.

By the side of this first assemblage of facts, of which the meaning seems to me not doubtful, I find another of at least equal importance—that of meteoric rocks evidently eruptive.

The meteoric iron recently discovered in the cordillera of Deesa, in Chili, having been submitted by me to a careful analysis, both chemical and mineralogical, appeared to me clearly to be formed from the mixture of two meteoric rocks, known, each of them, by masses of which they are entirely constituted. The one, stony and black, fell at Sétif, Algeria (June 9, 1867); the other, metallic, constitutes the mass of iron found in 1828 at Caille, in the south of France. Besides this, the metallic portion of the iron of Deesa, in which the black angular fragments are encrusted, has manifestly preserved the character assumed by the iron of Caille when it is subjected to fusion, so that the mode of formation of the Chilian mass cannot be considered doubtful. We must believe that on a globe, large enough to have been the seat of considerable pressure, masses of iron from Caille, still melted, were injected into superposed layers of Sétif rock so as to give birth to dykes, identical, except in their mineralogical nature, with those which the crust of the earth everywhere presents to our view.

These two orders of facts, which seem to me indisputable, being admitted, there remains to explain how fragments of polygenic conglomerates, or of dykes, can wander through space, and here only it is that the hypothetic part of my work begins.

From what precedes the meteorites in question are, by definition, planetary fragments. It remains to learn how the rupture of the planet whence they come can have taken place. On this it is evidently impossible to argue with any certainty.

Nevertheless, it appears to me that several considerations may greatly facilitate a choice among the different explanations which present themselves to the mind.

In the first place the *unity of composition* of the solar system, mentioned by Mr. Maskelyne, is evident.

Secondly, it is manifest that in the same system there exists a perfect *unity of geological phenomena*.

Lastly, but this, perhaps, has less weight, it appears to me that we should have recourse to accidental causes to explain natural phenomena only when every other means is forbidden.

This said, I observe that without making any other hypothesis than that of Laplace, we arrive at the conclusion that the stars tend of themselves to become broken. The earth is cracked in all directions; these fissures, designated as *faults*, are known to everyone. Little by little, as they form, they become reunited by the injection of an internal melted cement. But if the supply of this cement failed, the molecular operation which has opened the faults would still continue its action to enlarge them; we observe this in the moon, which, far more advanced in refrigeration, manifests by its fissures a phenomenon hitherto unknown in our earth. Evidently if we suppose to have been formed at the same time as the moon, a much smaller globe, that globe will have arrived actually at a state of cold far more advanced than that of the moon; and the fissures, excessively multiplied, and increased in depth and in width, may have finished by reducing the globe into separate fragments.

We have no positive proofs that such events have really happened, but is it not a very simple hypothesis to admit that meteorites, which bear so evidently the impress of a detritic character, may have had such an origin?

It is very probable that once parted from one another, the fragments are scattered along the orbit, and it is evident that they will tend progressively to approach the central star, so as to finish by falling on its surface under the form of meteorites.

Now, whether these fragments have been sorted or whether they have not, whether this sorting, if it exists, be or be not in accordance with that which the facts of observation have seemed to point out to me; I consider the question as entirely secondary as regards the general theory, and I request permission, in order to keep within the limits of the present discussion, to lay it absolutely aside for the present. I will simply repeat, in concluding this note, already somewhat long, that positive facts alone have served as the basis of my theory, and that the different circumstances on which my opponent has so learnedly insisted, possess for me but a secondary importance.

At the same time, I sincerely congratulate myself in the fact that my work has had the good fortune to fix the attention of a scientific observer so well placed as Mr. Maskelyne for submitting the mineralogical and lithological part of it to a severe verification.

DR. STANISLAS MEUNIER, Aide Naturaliste au Muséum
23, rue de Vaugirard, à Paris

Monographs of M. Michel Chasles

PAR une lettre inséréee dans le No. 36 de NATURE, page 190, M. C. Ingleby fait appel aux lecteurs de votre Revue pour obtenir quelques renseignements au sujet de "l'Aperçu historique" de M. Chasles, imprimé à Bruxelles en 1837. Le travail, qui porte pour titre exact : "Aperçu historique sur l'origine et le développement des méthodes en géométrie, particulièrement de celles qui se rapportent a la géométrie moderne," a été publié par l'Académie royale des sciences de Belgique dans le tome xi. de ses "Mémoires couronnés et des savants étrangers" (in 4to.), et il est très-difficile aujourd'hui de s'en procurer des exemplaires. Toutefois, M. Ingleby pourra s'adresser, pour consulter ce mémoire, à la Société royale de Londres, qui doit certainement le posséder dans sa Bibliothéque. Voici d'ailleurs la liste des établissements scientifiques de Londres qui ont reçu cet ouvrage a l'époque de sa publication : Société royale, Société astronomique, Société royale de littérature, et Société linnéenne.

J'espère que ces details pourront être utiles à votre honorable correspondant.

Bruxelles, le 8 Juillet A. LANCASTER,
Attaché au Secrétariat de l'Académie royale des Sciences de Belgique

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IN reply to Dr. Ingleby's note I may state that many papers by M. Chasles on various subjects in the history of Mathematics, are to be found in the volumes of the *Comptes Rendus* for 1837, onwards. His "Aperçu Historique" &c., originally appeared as a special volume of the Transactions of the Brussels Academy, but was sold as an independent work. It appeared in quarto, and was published in 1837. Like his "Traité de Géométrie Supérieure," it is very rare, and fetches an enormous price. Mr. Quaritch is, perhaps, the most likely bookseller in London to be able to procure it. The German translation by Sohncke is comparatively cheap, and may be readily obtained through Messrs. Williams and Norgate.

Torquay, July 9 G. E. DAY

The Specific Heat of Mixtures of Alcohol and Water

IN the report of the papers read at the Academy of Sciences, Paris, June 13, which appears in NATURE for June 30, it is stated that MM. Jamin and Amaury presented a note on the above subject, in which they point out, apparently as if it were something new, that the specific heat of some of these mixtures rises even above that of water.

Now, more than two years ago, March 26, 1868, we communicated a paper to the Royal Society giving the specific heat of various mixtures of alcohol and water, and drawing special attention to the remarkable fact that the specific heat of these mixtures is not only above the calculated mean specific heat, but that in all those of less strength than 36 per cent. of alcohol, it is higher than the specific heat of water itself. A knowledge of this fact should therefore be old by this time.

An abstract of our paper is printed in Proc. R. S., vol. xvi., p. 337. Subsequently we examined this and various other properties of similar mixtures more in detail, and communicated our results to the Royal Society in a second paper, an abstract of which is printed in Proc. R.S., vol. xvii., p. 333, and the paper in full in Phil. Trans. for 1869, Part II., p. 591.

The insertion of the above in the next number of your valuable journal will greatly oblige A. DUPRE & F. T. M. PAGE
Westminster Hospital, July 2

Geographical Prizes

HAVING been chiefly instrumental in causing prize medals to be offered by the Geographical Society for competition among the chief public schools, I do not like Mr. Wilson's letter in your last number to pass without comment.

Geography may be, to use his words, a subordinate branch of education, but I maintain that it is so only in the sense that it underlies a large part of liberal knowledge. It underlies the study of history. For example, I do not see how a boy could thoroughly understand Bible history without having acquired a very vivid conception of the geography of Palestine, and the same is true for all other histories, ancient and modern. It follows, as a matter of fact, that geography is incidentally taught to a considerable extent in schools, and I am sorry to say it is sometimes very ill-taught, as we learn from the reports of our examiners, but through some omission, not easily to be explained, if it be not the effect of a mere accident, geographical proficiency has never hitherto been adequately encouraged. Consequently, the Geographical Society has thought it right to step in to supply the needful encouragement. There is another good reason for the interference of the Society, in the fact that facilities of travel have rendered our interests much more cosmopolitan than formerly, while the public schools of the old-established type, have made no corresponding change in their curriculum. Mere youths now-a-days have exhausted the grand tour of two generations back, and a year or two of early manhood is often spent in America, Australia, and India, while books of travel load our library tables. It seems monstrous that a so-called liberal education should not qualify men to journey themselves, or to read the journeys of others, in an intelligent manner.

Mr. Wilson remarks, and his remark deserves respect, that the masters of Rugby were almost unanimous in rejecting the invitation of the Geographical Society, but I can fairly retort that other scholars no less practised in education and no less competent to decide, pronounced our system of prizes to be a valuable and much-needed institution.

It would be easy to write at great length in support of what we have done, and I might perhaps be expected to say something on the respective objects of the political and physical geography prizes, but I do not wish to provoke a discussion in your pages, because I am on the point of going abroad and should be unable to take further part in it. FRANCIS GALTON

"Kinetic" and "Transmutation"

1. WHEN, in 1864, I wrote for the *Reader* the history of the Baconian Philosophy of Heat, I found in use, in connection with the subject, the term "dynamical theory of heat," in English, which was employed as an equivalent for the expression "mechanische Wärmetheorie," current in German. The word "dynamical," already so vague from frequent abuse, corresponded but little, when used in its proper meaning, to the real intent of the theory in question; and the same remark applies, with at least equal force, to the word "mechanisch," even wider in its scope and as often misused. I was thus led to adopt the word "Kinetic," to supersede the above; and that in preference to the current word, "cinematic," which, in conjunction with "theory," would imply a tautology.

I am glad to see that Sir W. Thomson and Professor Tait, in their treatises on Natural Philosophy and on Heat, as well as in some remarkable papers on Atoms which have appeared in NATURE, frequently make use of the same word, "Kinetic," in connection with the theory of heat and of gases, as also in conjunction with "energy." Instead of the expression, "actual energy," originally introduced, I believe, by Mr. Rankine, Sir W. Thomson and Mr. Tait employ the term "Kinetic energy;" and from various motives, linguistic as well as strictly scientific, I venture to think that the original wording of Mr. Rankine in the case of "potential energy," should be likewise superseded, viz., by "dynamic energy."

2. In the *Philosophical Magazine*, I have been rated, indirectly, by Professor Challis, (for no mention is made of my name in connection with the subject), for having applied the word "transmutation" to rays, without recalling the fact of his having done so before me. I considered the expression "transmutation of rays" as the abbreviated and thoroughly English rendering of the words, "change of the refrangibility of rays, or light," used by Professor Stokes; and as such, requiring no authority but the precedent furnished by the existence of the analogous expression of "transmutation of matter." If, however, an authority had to be cited, it would have been Euler, in whose "Nova theoria lucis et colorum" (Opusc. var. argum.) the following passage occurs:—"Cum igitur a corporibus rubris radii tantum rubri, et a violaceis violacei ad nos pertingant, etiamsi radii albi in ea incidissent, manifestum est istam transmutationem a sola reflectione proficisci non posse."

As I have returned to this subject, I may be permitted to express my astonishment that Professor Challis, who thought it due to him that his name should be mentioned for being the author of the expression "transmutation of rays," should have on his part omitted, in speaking of the transmutation of Herschelic rays into Newtonic, a reference to my own share in the *res gestæ*. When I see the same thing being done in so widely circulated a treatise as that of Mr. Brooke on Natural Philosophy, and in one intended for even more popular reading, reproducing the teaching of the Polytechnic, I might think of entering a protest, if experience had not convinced me of its uselessness.

C. K. AKIN

Parturition of the Kangaroo

I BEG leave to call your attention to certain comments in your issue of the 23rd of June on the proceedings of the last meeting of the Royal Geological and Zoological Societies of Ireland. It is usual when parenthetical observations are made in any journal without the customary affix "Ed." to ascribe them to the printer's devil. Now, your devil, in commenting on an *imperfect* report of your Dublin correspondent, would lead your readers erroneously to infer that I had adopted the ideas which he has been pleased to call "absolute nonsense," and takes me to task for saying "that the actual passage of the fœtal kangaroo from the uterus to the pouch was not yet proved;" he himself stating that my remarks were "in contradiction to the facts observed by the late Earl of Derby's father or by the present Professor Owen." Now, a critic calling in question the words of others should be careful of his own. No facts on the subject were observed by the late Earl of Derby's father, and Professor Owen, after elaborate arrangements for the observation, states that "as parturition took place in the night, the mode of transmission to the pouch was not observed." (Phil. Trans. for 1834, p. 344.) There have been four observers in this matter especially worthy of being noticed:—(1) the keeper at the Zoological Gardens, Knowsley, who, according to Lord Derby's statement, saw the young kangaroo born, and that it was placed in the pouch by the paws of the mother (Proceedings of Zoological Society for 1833, p. 132); (2) Professor Owen, as referred to above; (3) Mr. E. G. Hill, who, at thirty yards' distance, saw the kangaroo with her mouth take up what he thought was a stone, open the pouch with her paws, and place it in the marsupium, and that he shot the animal and found a newly-born fœtus in the pouch (Proceedings of Zoological Society for 1867, p. 476); (4) M. Jules Verreaux, who is mentioned by M. E. Alix as having seen the kangaroo remove the fœtus from the vulva with her mouth, and place it in the pouch (Annals of Natural History for 1866, p. 316). These all differ as to the actual facts observed, and would seem sufficient to justify me in the statement I had made. That Professor Owen does not consider the question settled, may be inferred from his concluding observations on the subject, "whether the circumstance of the parturition is constant, viz, the dropping on the ground, or whether the fœtus may occasionally be received by the mouth from the vulva, I am disposed to regard as a matter for further observation; but the main fact of the conveyance of the fœtus to the pouch by means of the mouth may now be held as the more probable (at least the more usual, if not the constant) way in the genus Macropus" (Proceedings of Zoological Society for 1866, page 382). I refrain from any comments, but I thought it right to remonstrate against statements which I felt were injurious to me, to the Society to which I have the honour to belong, and to the advancement of science. JOHN BARKER, M.D.

Dublin, July 1

The Extinction of Stars

IF you will kindly permit an amateur to rush in where astronomers fear to tread, I shall be glad to offer a few remarks on the above subject.

The progress of science enables us to trace, with a probability almost amounting to certainty, the career of a star from its birth; from the most diffused condition of its parent nebula; through the stage of primary agglomeration when it shines as our sun; through the process of cooling into a dim and cloudy spheroid, such as Jupiter or our earth; until cold rules supreme, and the once glowing orb rolls on, barren as our moon.

But when we have reached this stage, we have by no means done with the star. It must continue on its course, and, though in obscurity, it must retain its momentum and its attractive force. Our sun will thus one day travel in darkness, attended by a cohort of funereal planets, and perpetual night will reign over the solar system. This result appears to be but a question of time, and we are, therefore, led to the consideration that many systems must, in all probability, be already extinct, and wandering unnoticed. But as extinction is a gradual process, there will be multitudes of stars in various stages of dimness, and the brilliancy of any orb, its "magnitude" in fact, will therefore depend on its age, quite as much as on its size or distance. On this view, Sir W. Herschel's method of "star gauging" cannot be relied on for a correct determination of the actual shape of the cluster called the "Milky Way," as instead of taking the average of brightness only, as an indication of the average of distance, we have to superadd the average of age. Now, the smaller the star, the more quickly will its light die out, and, therefore, the necessary extent of our galaxy is immensely reduced; in other words, it appears that while the space separating us from the nearer stars, for which parallax has been obtained, remains of course unchanged, the computed distances of those hitherto considered to be farthest off, will be much lessened, as there appears to be no reason for concluding that telescopic stars are necessarily more distant than bright ones for which we cannot obtain parallax, but simply that they are older, or smaller, or both, and therefore dimmer.

Mr. Proctor, in "Other Worlds than Ours," argues that as telescopes barely reach the outermost stars of our own cluster, therefore it is impossible that they should reach to and resolve clusters constituting other systems and lying at distances enormously greater, and therefore that the resolvable nebulæ must lie within our galaxy. If my idea that the stars of our cluster which the telescope shows with difficulty, are not distant but dim, be correct, Mr. Proctor's argument appears to lose its force.

It will be readily allowed that if the light of the stars be fading away, a vast number may have already become extinct, and that it is indeed possible that the orbs now visible may be but a small surviving remnant of far greater multitudes which once illumined the heavens. If our cluster then be much reduced in extent, and its constituents be largely increased in number, it would follow, I imagine, that the chances against collision would be much reduced, and it then becomes less difficult to conceive the possibility of such an event having occurred in the case of the recent outbreak in τ Coronæ Borealis, especially if it were caused by the unobserved approach of an extinct body. This outbreak is usually ascribed to a sudden conflagration of hydrogen, the star being, as Prof. Roscoe says, "on fire." But a star self-luminous surely must be always on fire, and if it contain hydrogen, that gas must be in a state of constant conflagration. The temporary brilliancy of the star seems rather such as would be occasioned by a collision with some comparatively small body, whose impact was yet sufficient to generate heat enough to accomplish its own disintegration and ignition. Let us suppose that collisions are possible, and that their frequency is merely a question of the chances. What would be the consequences of such an event? I imagine that they would depend chiefly on the relative momenta of the colliding bodies; that if one were very much larger than the other, and the velocities high, the temperature would be raised sufficiently to dissipate the smaller into gas, while merely heating, or possibly liquefying the larger. If the bodies were nearly of a size, and their momenta were great, possibly both would be reduced to a gaseous condition; in either case their tendency would be to form ultimately a body equal in weight to the sum of its two constituents. Either the larger body would annex the smaller, or, if both became nebulous, the fervid gases would radiate their heat and contract anew into a system possibly containing a sun and planets.

Again, supposing that two bodies approach each other in such a manner as to avoid a collision, that is, so that their mutual gravity causes them to leave their paths and revolve round each other, we should have the explanation of the existence of double, treble, multiple stars; we should also understand how it happens that some stars (Sirius, for instance,) are accompanied by non-luminous orbs. Also, it would seem that if extinct stars are really far more numerous than is generally supposed, the theory which regards the revolution of attendant dark bodies as one cause of the variability of certain stars, receives fresh support.

Thus, in the course of time, nebulæ would form suns, suns would grow cold, or, while yet glowing, would come into contact and combine with other suns, till gradually space would be peopled with suns, larger and larger, but less and less thickly strewn. Pursuing the idea, we arrive at a period when all the stars of each galaxy shall become agglomerated into one mighty globe—nay, when all these vast galactic suns shall come together and form one solitary orb, in which all the matter once scattered through space shall be collected, accomplishing its successive fates as a sun without a system—a world without a sun—a cold and naked ball.

EARDLEY MAITLAND

Why is the Horse Chestnut Tree so called?

DURING the spring this tree is the ornament and pride of our public and private parks. In "Woodland Gleanings" it is stated to be a native of the north of India, and is supposed to have been introduced into England about 1575.

Our observant forefathers have given it the very significant name of *Horse* Chestnut (*Castanea Caballina*),* to distinguish it from all other species of chestnuts. The *reason* for so doing I have never seen stated in print; but from the three specimens of cuttings from a branch of this tree which I enclose, it will be very manifest. All over its branches, at every bud, can be seen what at a glance will be taken for an exact conformation of the *foot of a horse*, exhibiting the hoof, the nails of the shoeing, the fetlock-joint, &c., in marvellous miniature, some, of course, better developed than others. This curious freak in nature's vegetable kingdom, has, no doubt, been the origin of our nomenclature of this tree; and it would be an interesting point of philological inquiry to ascertain *whether or not its native Asiatic name has incorporated or associated with it that of the horse?*

I write with the view of eliciting information on this point, and with the hope, too, that some of your botanical contributors will throw further light on this peculiarity.

EUGENE A. CONNELL

Fall of an Aerolite

A LETTER of the year 1628, "sent by Mr. John Hoskins, dwelling at Wantage, in Berkshire, to his son-in-law, Mr. Dawson, a gunsmith dwelling in the Minories without Aldgate," and preserved among Nehemiah Wallington's Historical Notices (i. 13) contains the following narration:—

"On Wednesday before Easter, being the ninth of April, about six of the clock, in the afternoon, there was such a noise in the air, and after such a strange manner, as the oldest man alive never heard the like. And it began as followeth:—First, as it were, one piece of ordnance went off alone. Then after that, a little distance, two more, and then they went as thick as ever I heard a volley of shot in all my life; and after that, as if it were the sound of a drum, to the amazement of me, your mother, and a hundred more besides; yet this was not all, but, as it is reported, there fell divers stones, but two is certain, in our knowledge. The one fell at Chalows, half a mile off, and the other at Barking, five miles off. Your mother was at the place where one of them fell knee deep, till it came at the very rock, and when it came at the hard rock it broke, and being weighed, all the pieces together, they weighed six-and-twenty pound. The other that was taken up in the other place weighed half a tod, 14 pound."

I do not know whether there may be any other record of this remarkable aerolite, so simply but graphically described. Is it not just possible that some of the fragments may yet be preserved in the neighbourhood of its fall? At any rate a search would involve but little trouble.

T. W. WEBB

ANDERSON'S UNIVERSITY

WE extract the following from the *Evening Citizen* (Glasgow) of June 22:—

"The annual meeting of the trustees of Anderson's University was held this afternoon within the institution. Mr. William Ewing, in the absence of the president, was called to the chair. In the annual report which was submitted, reference was made to the death of Dr. Penny in November last, and the appointment of a successor. Mr. Young, of Kelly, who had arranged to set aside 10,000 guineas for the endowment of a Chair of Technical Chemistry in connection with the University, had, it was stated, no further proposal to make, he leaving it to the trustees to make what alteration in the deed of trust they may think proper. Under these circumstances, the managers recommended that advertisements be issued for a successor to Dr. Penny; that the chair should in future be styled the Chair of Scientific Chemistry; and that in electing the professor power should be reserved to the trustees to create such other chair or chairs of Chemistry in connection with the University, and elect such additional professor or professors to fill said chairs as the trustees may see fit; and also to arrange and define, from time to time, the respective departments of the subject to which each professor, including the Professor of Scientific Chemistry, should devote himself. Regarding the mode of electing professors, the

* The scientific name of the horse-chestnut is *Æsculus hippocastanum* it has no relationship to the *Castanea*, or sweet chestnut.—[ED.]

managers recommended that no change should be made. The earlier practice was to appoint the professor only for a course of lectures, and upwards of fifty years ago a bye-law was passed that the election of a professor should be only for one session. With a few exceptions, the election of professors has been annual from that date down to the present time, and the power of not re-electing has been of great service, says the report, in the management of the University. Dr. Steven introduced a motion to abolish the annual election of professors. He spoke of the present system as most degrading. It struck at the root of the institution's claim to be a university; and while it was evidently contrary to the will of the founder, Dr. Anderson, he had heard of no case in which it had been of benefit. Mr. Kidston, secretary, remarked that many years ago it had been the means of causing the professors to pay up their rent. Dr. Steven said he had heard it remarked that the trustees might come to look upon security for rent as a qualification in their professors of more consequence than educational ability. The professors, he contended, should be elected *aut vitam aut culpam.*—Dr. Pirrie seconded the motion. An amendment was moved by Mr. M'Lelland for having no alteration in the present system. Dr. Adams supported the motion in a speech of some length, in which he characterised the annual election of professors as somewhat disreputable. The chairman recommended that no alteration should take place. All the other officials, he argued, were elected annually, and why not the professors? Dr. Weir asked an explanation of the paragraph in the report which stated that the system had been found to be of great service. Mr. Kidston, by way of reply, again instanced the refusal of the professors to pay rent. On a division, the amendment was carried by 35 as against 6. The meeting was proceeding to some routine business when our reporter left."

We had hoped that the trustees of Anderson's University would have made good use of the opportunity which exists at the present time, in consequence of the vacancy in the chair of Chemistry and of Mr. Young's munificent offer to endow a professorship of Practical Chemistry, to make some alterations in the status of the professors of the institution; but we seem doomed to disappointment. The professors are still to be appointed yearly, to give one course of lectures, and to have the privilege of paying rent for their laboratories and class-rooms in the meantime. The consequence of this will be that no chemist of eminence will be induced to undertake the duties of a post in which he will find himself on the same footing with *other officials*, doorkeepers, and laboratory man, we suppose; and we shall be much surprised if anyone will be found to apply himself solely to the duties of the appointment if he is to be liable to find himself turned out at the end of a year. The principal portion of his time must necessarily be devoted to commercial work and other means of obtaining a living, to the great detriment of scientific research, and certainly not to the credit of an institution which claims to be a university.

THE MICROSCOPE

Choice of a Microscope.—Medical and other students are at this time of the year purchasing a microscope with which to begin the investigation of animal and vegetable structures. Others who would wish to invest in an instrument are deterred by the expense on the one hand and by the fear of obtaining a worthless thing on the other. Too strong a protest cannot be made against the notions prevalent with regard to microscopes, and encouraged by most of the makers in this country. The handsome-looking instrument of great size, with its long tube and innumerable wheels, is not to be recommended to the would-be observer, even should he feel justified in the expenditure. The microscopes which are used in most of the German laboratories where so much thorough work is done (to the writer's knowledge in Prof. Stricker's and Prof. Rokitansky's laboratories at Vienna, in Prof. Schweigger Seidel's at Leipzig, and in Prof. Claude Bernard's at Paris), are the little instruments of Hartnack, which do not stand above ten inches high, with a simple but large stage without any movement, no rackwork to the tube, but a sliding motion and a fine adjustment. The instrument is used in the vertical position with complete comfort, and when liquid is on the stage, this position being necessary, it is of considerable advantage to have a small microscope over which one can easily bend the head. Large microscopes, with their complicated machinery, are made to suit the optician who sells them, and not for the convenience of the observer. Those who wish to get a microscope should insist either on having one of these small and handy instruments made, or order one from M. Verick or M. Hartnack in Paris. Such a body having been purchased at a very minimum of cost, a larger sum may be expended on the really essential part of the apparatus, namely, the lenses. And here it will be found of great advantage to have the tube of the microscope not more than three-and-a-half or four inches in length, for then the objectives of the continental makers can be used with the greatest advantage, though, with proper care as to the ocular or eye-piece, they may be used on our ordinary long-tubed awkward English microscope. It is almost incredible that the English makers of object-glasses continue to demand three, or even four, times the price for their lenses which foreign makers do for lenses in every respect as good. For two pounds an object-glass may be obtained of M. Verick or M. Hartnack, of Paris, No. 8, which is quite as good a glass and in some respects more pleasant to use than the one-eighth, for which English opticians demand eight guineas. Many persons anxious to work with the microscope are deterred by the price of really first-rate instruments in this country. What we urge upon them most earnestly is to purchase such a body with eye-piece as that described above—simple but strong and steady—for between two and three pounds, and to equip the instrument with the objectives of MM. Verick or Hartnack, say No. 2, No. 5, and No. 8, which can be obtained for another four pounds. We shall have occasion again to speak of the merits of English and foreign objectives, especially of the immersion object-glasses. At present we speak from personal experience, and desire to point out the convenience and cheapness of the small microscope-body, and the thorough excellence and immensely diminished cost of the French makers' object-glasses.

Cutting Sections of Tissues.—The method of "embedding" first practised by Stricker and Klebs is now extensively used in Germany, and is of very great assistance to the practical histologist. It consists simply in surrounding the object from which sections are desired, with either paraffin, stearine, or a mixture of wax and oil. This latter is preferred at Vienna by Prof. Stricker and Dr. Klein, his assistant, and can be obtained of the exact consistency which may be desired; usually equal parts are to be used. A little tray of paper is made, and some of the wax composition in a melted state is poured in. The object to be cut is then placed in the tray, and more composition added, till the object is thoroughly enclosed. When hard, sections of the mass can be cut, the advantage being in the case of thin laminæ or processes, that a complete support is offered by the surrounding composition, and a uniformly thin cutting may be obtained. For some purposes the microtome of Dr. Ranvier, of Paris, is very useful: it is similar to one recently brought out by Mr. Stirling, of the Anatomical Museum, Edinburgh. In this little instrument we have a flat piece of brass with a hole in the centre, leading into a cylindrical chamber, at the bottom of which a screw works. A piece of elder-pith is excavated, so as to hold the tissue to be cut; and when this has been well fixed in it, the pith is squeezed into the cylindrical box through the hole in the brass plate. A razor drawn along the surface of the brass plate cuts through the pith and the tissue it embraces, leaving a surface perfectly smooth and continuous with that of the plate. A turn of the screw, which works into the cylindrical box, now causes a certain very small thickness of the pith and tissue to project above the plate, and the razor again drawn across and pressed on to the surface of the brass plate, cuts a fine section, the exact thickness of which may be nicely regulated by the screw which pushes up the pith. This little instrument may be obtained at a small cost from M. Verick, 2, Rue de la Parcheminnerie, Rue St. Jacques, Paris. It is not unlike an instrument described in English books on the microscope for cutting sections of wood, but its application with the use of pith, previously much in use for making sandwiches with delicate tissues which had to be cut, increases its value greatly. As to knives to be used in making sections, though some large knives are made on purpose, there is nothing better than a first-rate broad-bladed razor. Dr. Meynert has cut his immense collection of brain preparations with a common razor.

Staining and Mounting Tissues.—The method which is now very extensively used in German histological laboratories for the study and preservation of all kinds of delicate tissues, such as sections of the developing hen's egg, morbid growths, fine injections, nerve tissues, &c., is as follows: The section, either from a fresh specimen or from one preserved in alcohol, is placed in a solution of carmine in ammonia, from which all excess of ammonia has been allowed to evaporate, as tested by the smell. The solution is also carefully filtered before use, and diluted to a small extent. After from three to ten minutes or more in the carmine solution, the section is placed in distilled water and thoroughly washed for some time by blowing into the water with a small pipette. From this the section is removed momentarily to a watchglass containing distilled water and two drops of acetic acid, and then is placed in absolute alcohol. The water is thus removed, and in five or ten minutes the section may be placed in oil of cloves, which renders it very transparent. From this it is removed to the glass slip, and is mounted in a solution of gum damara in turpentine, such as is sold by artist's colourmen. At any stage in this process we can proceed back again by the same steps, ammonia being used in place of acetic acid, and re-stain, re-wash, or re-acidify as the case may be. If the staining is carefully managed and the subsequent washing a thorough one, most cellular structures are very beautifully and clearly brought out. Where rapidity is desired, and for the purpose of inspecting a specimen, it may be simply mounted in glycerine after the staining. The process above described is that of Gerlach and Stieda, and is preferred to any other by some observers of great experience. Thus Dr. Meynert, of the lunatic asylum at Vienna, who is throughout Germany regarded as the great authority on the histology of the brain, uses this method for mounting his sections of cerebrum, cerebellum, &c. It is very convenient to have little glass dishes with covers for each of the above-mentioned re-agents, so that the sections may be passed from one to the other and left covered up, if desired, for a day or two—the waste of re-agents involved in filling watch-glasses each time they are required being also avoided. If preparations have been preserved in chromic acid, they must be very well washed before staining, and very often cannot be made to stain well at all. Various methods are useful in various cases, but, as one of great general use, the carmine staining and oil of cloves clearing may be strongly recommended. Staining tissues with nitrate of silver, chloride of gold, and with bile-pigment are most important aids to the histologist, the merits of which have been recently much discussed, and of which we shall have a word to say from experience.

Glycerine Jelly.—This composition, which has been lately introduced, melts at a lower temperature than Deane's medium, and has a greater clearing action on the objects mounted in it. A small piece of the jelly put on a glass slip and warmed, soon liquefies, and is ready to receive any object, after which the cover is directly applied. For objects which do not require any great amount of "clearing," it is a most useful medium. Insects, worms, small crustacea, &c., may be mounted in this way excellently. E. RAY LANKESTER

METEOROLOGY OF JUNE 1870

I BEG to send you a few particulars of the weather of the past month (which was characterised by unusual atmospheric phenomena), deduced from daily observations with standard instruments, the place of observation being in latitude 51° 27′ N., longitude 0° 18′ W., height above sea level 64 feet.

The barometrical readings have been corrected for capillarity, index error determined by comparison at the Royal Observatory, Greenwich, and certified by James Glaisher, Esq., F.R.S., and reduced to 32° Fahr. and mean sea level.

The thermometrical readings have been corrected for index error determined by comparison at the Kew Observatory of the British Association.

Time of observation, thermometer $7^h\ 45^m$ A.M., barometer $8^h\ 0^m$ A.M., wind direction $8^h\ 30^m$ A.M., daily (approximate).

The following are the calculated monthly means, &c.

Mean height of the barometer (corrected)	30·135 in.
Highest observed reading	30·551 in.
Lowest observed reading	29·747 in.
Monthly range	0·804 in.
Mean temp. air ($7^h\ 45^m$ A.M.)	60·8°
,, ,, of evaporation	55·3°
,, ,, of dew point	50·6°
Relative humidity (dry air = 0, saturation = 100)	70
Mean of the maxima	75·1°
Mean of the minima	51·2°
Mean diurnal range of temperature	23·9°
Extremes: Highest reading (June 22)	91·4°
Extremes: Lowest reading (June 6)	41·6°
Monthly range of temperature	49·8°
Mean estimated force of wind (0 to 6)	1·5
Total rainfall	0·597 in.
Days on which rain fell	5
Evaporation on 22 days	3·652 in.
Mean intensity of ozone (24h)	2·5

*** Sun at greatest meridional altitude (year) or greatest N.D. June 21st.

A lunar halo (or portion of a circle) was observed on June 9 shortly after 10^h P.M. (or 10^h astronomical time). Its estimated extent was 270° of a circle whose diameter was 60°. Estimated altitude of the moon at time of observation, 35°.

A thunderstorm occurred on the 16th, with very vivid lightning, yielding 0·355 inch of rain, which was equivalent to 7987·5 gallons, 1288·65 cubic feet, or 35·9 tons per acre, assuming the rainfall to be equally distributed, which may be done with some degree of truth, as the amount measured at the Kew Observatory, one mile distant, agrees with mine to the second decimal.

The atmosphere was moderately charged with moisture during the month, which must have been an assistance to vegetation in spite of the excessive drought.

The rainfall during this month was 0·558 inch less than that registered during the corresponding period last year.

Wind directions in the lower regions of the atmosphere were observed on 12 out of 16 points, the prevailing directions being between W. and S.W. points.

Richmond, Surrey, July 7 JOHN J. HALL

THE ROTUNDITY OF THE EARTH

"PARALLAX" is not dead yet. His backer, Mr. John Hampden, has again brought his sophisms and his misstatements before the public in the form of a periodical called the *Armourer*, which has already had one period of existence, having been discontinued about four years since, "amidst the regrets of hundreds of its readers," as the editor asserts. When Mr. Hampden speaks of the recent experiment by which the falsity of "Parallax's" views was exposed, as "the Bedford Canal swindle," of Mr. Wallace's victory as having been obtained by "Scotch knavery and cunning," and of the conduct of the editor of the *Field* as umpire as having been "false, unfair, and fraudulent," we may well leave these charges to be replied to by these gentlemen themselves, or by the law. As, however, "Parallax" repeats unblushingly his assertion that he has for years propounded his views by lectures in various parts of the country without their having been once refuted, we may call to his remembrance a circumstance which he has probably found it convenient to forget. During the recent experiments at the Bedford Level, "Parallax" carefully concealed the fact that the very same test had been previously applied. In the year 1856, however, after a lecture by "Parallax," at Norwich, two gentlemen challenged him to an experimental proof of his views. He accepted the challenge and was invited to witness the experiment, which invitation, however, he did not respond to, but prudently left the town in the interim. The nature and result of the experiment are detailed in a printed slip which was inserted at the time in the local papers, and a copy of which we append:—

COPY OF AGREEMENT.—We, the undersigned, "Parallax," of No. 61, Upper North Place, Gray's Inn Road, London, on the one side, and John Weir, of No. 14, Suffolk Street, Union Place, Norwich, and Charles William Millard, of Prince's Street, Norwich, on the other side, having different opinions as to whether the Earth be a Plane or a Globe, agree to test the accuracy of our respective opinions in the following manner, that is to say, to place four flags in a straight line, intersecting the River Yare between Strumpshaw or Bradestone and Norton, for a space of not less than four miles, or six miles if possible. The flags to be at the same height above the water except the last or fourth flag, which is to be placed close behind the third flag, at a height of three feet above it; if we can see the fourth or furthest flag above the tops of the other three flags, the Earth is a plane, or if the second flag from the telescope be above a line joining the tops of the first and third flags, the Earth is a globe.—(*Signed*)—"PARALLAX;"—JOHN WEIR, C. W. MILLARD, Engineers and Surveyors.

Dated November 24, 1856 *Witness*—R. F. HINDE

COPY OF CERTIFICATE.—We, the undersigned, hereby certify and declare, that on the eleventh day of December, one thousand eight hundred and fifty-six, we accompanied Messrs. Weir and Millard, and assisted in placing the flags in the manner above mentioned, and that upon looking at the flags with a powerful telescope, the top of the second flag was fifteen inches and one half of an inch above a line joining the tops of the first and fourth flags, and twenty-four inches and one quarter of an inch above a line joining the tops of the first and third flags, thereby proving that the earth is a globe, and that from the results of this experiment, "Parallax" is bound, by the before-mentioned agreement, to renounce, for ever, his theory of the earth being a plane.—(*Signed*)—R. F. HINDE, Sussex-street, Norwich, manufacturer; ALEX. SANDERSON, Magdalen-street, Fye-bridge, tobacconist; W. H. DAKIN, Davey-place, Norwich; JAMES NEWBEGIN, St. Andrew's, tobacco manufacturer.

Will nothing stop "Parallax's" mouth?

TEA

THE word "Tea" is applied to the leaves of numerous plants from which infusions are made in their several native countries. Thus in Paraguay they use a species of Holly, in Abyssinia and Arabia the leaves of *Catha edulis*, and in Labrador those of *Ledum latifolium*.

We propose, however, in this paper, to say a few words about that article which is generally and popularly known as tea, and which forms such an important commercial commodity between China, India, and our own country, How long tea had been used in China before its introduction into Europe early in the seventeenth century no one can venture to say, but it appears to have been first known in England about the year 1660, and no article of commerce, perhaps, presents a parallel history of such rapid development. In 1678 the East India Company imported into England 4,713lb. Tea, however, continued to be a rarity for many years after that date, fetching a high price, and consequently remaining beyond the reach of all but the more wealthy. The demand for it increased so rapidly that in 1725 the consumption in the United Kingdom reached 370,323lb. Since then tea has been more and more in demand, until we find the returns for last year show as much as 139,223,298lb. imported, and 111,889,113lb. entered for home consumption, the computed real value of the tea imported during eleven months of 1869 being 9,115,823*l*.

The plant from which this large source of wealth is obtained is a shrub, the native country of which is still not definitely known. Although it has been cultivated for many hundreds of years in China, and its use alluded to in ancient Chinese legends, it has not been discovered in that country in a wild state, but truly native tea occurs in the jungles of North-eastern India.

At one time botanists were inclined to the opinion that black and green teas were furnished by two distinct species, the former by *Thea bohea* and the latter by *T. viridis*. So little difference exists between them that there seems no doubt as to their being mere varieties, and both are now usually referred to one species, the *Thea chinensis* of Linnæus. Though tea is now largely grown in Assam and some also in Japan, the plants cultivated in both countries are varieties introduced from China. The black and green teas of commerce may be prepared from either form of the plant according to the pleasure of the tea farmer, the colour in a great measure depending upon the rapidity of the artificial drying of the leaf, and also upon the length of time the freshly gathered leaves are exposed to the air before heating. There are, however, districts in China called respectively the Black and Green tea districts, in which the plants are grown specially for each purpose. For the preparation of either sort the leaves are gathered by hand, and the younger ones should alone be taken. If they are intended for the manufacture of black tea they are exposed to the air for a short time, after which they are placed in iron pans and submitted to a gentle heat for a few minutes. By this process much moisture is thrown off, and the leaves are rendered pliable, so that they are easily pressed or rolled between the hands, by which the characteristic twist or curl is given to them. Before, however, they are fit for market, they are exposed to the air for two or three days, and finally dried in iron pans over a slow fire. The chief difference in the preparation of genuine green tea is, that it has to be more quickly dried after undergoing the curling or twisting process in the hands, black tea being allowed to remain in heaps in a flaccid state, before the final drying or roasting, which, in itself, is much slower. A great deal, however, of the green tea consumed in this country, is artificially coloured by the Chinese, chiefly with Prussian blue, gypsum, and turmeric. Of course it is only inferior teas that are so treated, a good face being thus given to them. They can mostly be detected by placing a handful of the tea on a sheet of white paper; a thick, greenish dust will not only be left on the paper, but will rise every time the tea is shaken. By breaking a few leaves also with the finger nails this coloured tea will show a brownish fracture, while genuine uncoloured tea is more or less green throughout, and consequently little or no dust is deposited from it. As the leaves of true tea vary very much in size and form, adulteration with the leaves of some other plants is not so easily detected. The nearest approach, however, to the form of the true tea leaves are those of *Camellia sasangua*. This plant itself is a near botanical ally to the tea, and the leaves are moreover used by the Chinese for scenting many of their teas. Most other leaves which have been found as adulterants may be detected by their forms.

We give a figure of a leaf of true tea.

If a leaf of black tea be soaked in cold water, spread out, and inspected through a microscope of ordinary power, it will present the appearance shown in the cut, the older and larger leaves will be of a dullish green, and the younger ones of a light semi-transparent green. It will not serve us to examine the internal structure of the leaf, as it has many points in common with other leaves, and would moreover require minute examination. The best black tea, then, should present the appearances above indicated, and the same may be said of green tea, with this exception, that after being soaked it is of a paler green colour than the former.

Amongst the commercial varieties of tea the following are the best known:—Congou: this constitutes the bulk of black tea from China. It is that which is usually sold as black tea, and of course varies much in price according to its purity; a really good tea of this description ought to be had at the present time at 2*s*. 6*d*. per lb.

Souchong and Pekoe are both finer kinds of black, and fetch higher prices. Another kind of black called Orange Pekoe, may be known by its long, wiry leaves, which are mostly genuine; it is artificially scented, and is generally used by grocers for mixing with inferior kinds. A fine Pekoe, however, ought to be obtained for about 4*s*. per lb.

Caper is a common black tea, artificially scented; the leaf as we see it in commerce has the form of the Gunpowder leaf, but these are made up of tea-dust and other matters agglutinated.

Amongst green teas, genuine Gunpowder is the finest; the qualities and prices however vary very much; the leaves of the best are in fine, close curls, and are the younger ones gathered from the tops of the plants. The lower qualities of this tea are almost all coloured artificially, and many contain no perfect or whole leaf at all, but are made up of broken tea-leaves; 4*s.* 6*d.* per lb. may be considered a fair price for a good quality Gunpowder tea. In Hyson the leaf is longer than Gunpowder; it is mostly composed of the true leaf, but is very frequently artificially coloured.

Oolong is really a green tea, but with so black an appearance that its colour is only developed by putting it in hot water. It is artificially scented, and is used for mixing with other kinds of tea.

The cultivation of tea in Assam has sent several good kinds into our markets, the Congou, Souchong, and Flowery Pekoe of these plantations being, as hitherto imported, all genuine teas. We regret, however, to see that in the course of the past few weeks a quantity of artificially coloured green tea has been imported from the Indian plantations. Many of the Assam teas have a fine malty flavour, which is so much esteemed that it is frequently imitated and imparted to other teas in London.

A great deal that has been said and written for many years past on the subject of adulteration of food we are bound to admit as truth, but, on the other hand, there has

FIG. 1.—Tea (*Thea chinensis*, L.)

been some exaggeration. With regard to tea, the great demand amongst all classes has led to a very keen competition, not only amongst retail dealers, but also amongst importers themselves. The system of mixing inferior articles with those of better quality must not be wholly laid to the charge of the British tradesman or merchant, for the natives of the several countries producing the various commercial products, practise a great amount of deception. The importation of several chests of such rubbish as the "fine Moning Congou," about which so much talk was made a few weeks since, as well as the numerous cargoes of "tea-dust," a sample of which is now before us, composed of small fragments of various kinds of vegetable matter and other substances, with little or no tea, are proofs that others than the retail dealers are the most culpable. We are ashamed to own that in many instances this system of deception has been taught the natives by our own countrymen; but such is not always the case, and other articles besides tea, as we shall have occasion to show in the course of these papers, are equally subject to native adulteration. A system of manufacture of spurious tea, called "Lie Tea," is openly known to exist in China, and was at one time profitably carried on in England. It consisted in converting the leaves of numerous plants into imitation tea for the purposes of adulteration. Though teas of varied qualities are imported from China, those of the very finest kinds seldom leave the country, except a small quantity which is carried overland to Russia, where they sell for as much as 50*s.* per lb., and the same price is even paid by the princes and mandarins of China in the very country where the tea is produced. It is said that these fine teas would deteriorate in quality in such a journey as that from China to England. A fine variety of Assam tea called Flowery Pekoe, is now chiefly imported for the Russian trade, very little of it being sold in this country. It is worth about 7*s.* 6*d.* per lb., consequently there is little demand for it. Though the Russians boast, and with good reason, of the quality of their tea, a vast quantity of rubbish is sent to that country from China for consumption by the poorer classes. This is known as Brick Tea, and is frequently made up of the sweepings of the manufactories and warehouses mixed with bullock's blood and other refuse, and compressed into hard cakes or bricks; for use it has to be boiled. In some parts of India the natives use a similar kind of brick tea, making, instead of a clear infusion, a thick kind of drink more like soup.

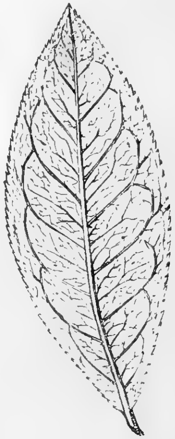

FIG. 2.—Leaf of the Tea Plant—natural size of a full-grown leaf.

Tea contains an active principle called "theine" and a volatile oil, it also contains about fifteen per cent. of gluten or nutritive matter, and about twenty-five per cent. of tannin or astringent matter. The effect of theine upon the human system is to excite the brain to greater activity, but whether or not it soothes the vascular system by preventing the rapid waste of the body, is a point upon which physiologists are not quite agreed. Theine, however, if taken in excessive quantities produces tremblings, irritability, and wandering thoughts, it has been recommended that when these symptoms show themselves, cocoa should be used as a beverage for a few days. The volatile oil is narcotic and intoxicating; it is to this oil that the flavour and odour of tea are due, it is of course present in larger quantities in new teas than in old, therefore the fresher the teas are the fuller is their flavour and odour, consequently no kind of tea improves by being kept exposed to the air or even in paper, so that tea weighed at the time of purchase should be preferred to that sold in packets, the buyers of such tea having to risk the length of time it has been packed; and, moreover, the teas themselves are usually of an inferior description.

Since writing the above, I have had two samples of green tea sent me which have been offered for sale in London during the past week. One sample is composed of nearly or quite half its weight of the young fruits of the tea-plant about the size of small peas, the remainder being made up of broken tea-leaves agglutinated and rolled together, and enclosing fragments of various matters, mineral as well as vegetable, which, of course, are included for the purpose of increasing its weight and bulk. The other sample consists principally of leaf stalks, a few leaves, rice husks, and the pappus fruits of some Compositæ. Truth compels me to say that all the leaves I have examined out of these samples have been leaves of the true tea-plant, or rather fragments of such, but all artificially coloured, and so superficial is the colouring that it can be easily wiped off with the dry finger. These teas have been offered for sale, one at $1\frac{1}{4}$*d.* and the other at $1\frac{1}{2}$*d.* per lb., the duty paid on them being equal to that charged on the best teas—namely, 6*d.* per lb.

This class of tea can, of course, only find a sale amongst unscrupulous tradesmen, who buy it to mix with good teas, and where a comparatively small proportion of this rubbish is mixed with a large quantity of good tea, but yet in sufficient bulk to increase the tradesman's profits, it is difficult for the purchaser to detect a few hundred or more such leaves in the thousands which go to form a pound of tea. It is high time there was some regular system of examination of such articles directly they come into port.

J. R. JACKSON

NOTES

PROFESSOR HELMHOLTZ has left Heidelberg for Berlin, to occupy the position left vacant by the death of Magnus, but with the title of Professor of Physiology.

AT the meeting of the French Academy on the 4th inst., Professor Brandt was elected a correspondent of the section of Anatomy and Zoology. In the final election he received twenty-two votes out of thirty-eight, the remaining sixteen being in favour of Mr. Darwin. In the first ballot Professor Huxley received three votes, and M. Loven one.

ONE of the improvements in the management of the Hunterian Museum of the Royal College of Surgeons, introduced by the present Conservator, has been the publication of an annual report of the progress and condition of the collection, and the exhibition in the theatre of the College, of the specimens that have been added to the Museum during each twelvemonth. As the College year ends at Midsummer, this exhibition has just taken place, and has enabled those interested in the Museum to judge of the nature and value of the additions, and the mode in which they have been prepared. The new specimens include fifty-five specimens of pathological anatomy, one hundred and eleven of normal human and comparative anatomy; the latter chiefly prepared from animals which have died in the Zoological Society's Garden, and a considerable series of skeletons and skulls. We propose to refer more fully to Professor Flower's Report on a future occasion.

THE naturalists of Switzerland have decided to form a scientific congress devoted to the study of the natural phenomena of the Swiss Alps, to include the geologists and palæontologists of France, Germany, and Italy, who have paid special attention to this subject, to be held in Geneva on the 31st of August and 1st and 2nd of September, and to be called the Congress of Alpine Geologists. Among the promoters of the congress are Prof. Studer, of Berne; Prof. Mérian, of Bâle; Prof. Escher de la Linth, of Zürich; Prof. Desor, of Neuchâtel; Prof. Favre, of Geneva; Profs. de Loriol, Heer, and Mousson, of Zürich; Prof. Rütimeyer, of Bâle; Prof. Renevier, of Lausanne; Profs. Vogt and Pictet, of Geneva. A committee for the organisation of the congress has been formed at Geneva, with M. Pictet as president, M. Alphonse Favre as vice-president, and MM. Ernest Favre and E. Sarazin as secretaries. All geologists interested in the subject are invited to be at the president's reception on the evening of August 30th, and anyone wishing to communicate any address or paper is requested to write to M. Ernest Favre, 6, Rue des Granges, Geneva.

WE are pleased to hear that the Government of Demerara has re-considered its resolution for discontinuing the geological survey of that colony, and has now resolved to complete it, under the direction of Mr. Charles B. Brown, an associate of the Royal School of Mines.

A SPECIAL extra meeting of the Syro-Egyptian Society of London will be held on Tuesday, July 19, at half-past seven P.M., for the exhibition of a collection of drawings of Egyptian antiquities, by the late R. Hay, F. Arundale, and C. Laver, Esqs. Messrs. Simpson and Bonomi will give explanations.

AT a meeting of the trustees of Owens College, Manchester, held on Thursday, the 7th inst., the vacancy caused by the resignation by Professor W. Jack, M.A. of the Natural Philosophy Professorship, was filled up by the appointment of Dr. Balfour Stewart, F.R.S., superintendent of the Kew Observatory, to the Senior Professorship, and of Mr. James Thomson Bottomley, M.A., F.C.S., Demonstrator and Lecturer in Natural Philosophy in King's College, London, to the Junior Professorship of Natural Philosophy. Dr. Stewart was also appointed Director of the Physical Laboratory which is about to be established in the college. We are informed that Mr. Bottomley has since withdrawn.

M. CLAUDE BERNARD has been elected a member of the Imperial Council of Public Instruction in France for the year 1869-70; M. Briot has been appointed Professor of Mathematical Physics and the calculus of probabilities in the Faculty of Sciences at Paris; and M. Emery, Professor of Geology, Mineralogy, and Botany in the Faculty of Sciences at Dijon.

THE annual public meeting of the Paris Academy of Sciences for the distribution of prizes and rewards, which should have been held in December last, was postponed till the present month, and is now again put off for some unexplained cause.

IT is with great pleasure we hear that the London Institution, in Finsbury Circus, has appointed Mr. John Cargill Brough to the post of Librarian. The library of this Institution is so valuable that it is fitting it should be under the care of a man who combines literary and scientific qualifications in so eminent a degree. It was right that an office once filled by such men as Maltby and Brayley, should have fallen into good hands.

WE understand that Dr. B. H. Paul has been appointed editor of the *Pharmaceutical Journal*, the new series of which we recently announced.

THE list of members of the Institution of Civil Engineers, corrected to July 1, 1870, contains the names and addresses of 16 honorary members, 702 members, 999 associates, and 177 students, making together 1,894 of all classes.

THE House of Commons decided on Friday last that the land belonging to the Thames Embankment shall be kept entirely free from building. The Natural History Museum will therefore occupy the ground already indicated by us on the space adjoining the Royal Horticultural Gardens.

Apropos of this subject, one of our daily contemporaries (and by no means the worst informed on scientific subjects) makes ludicrous blundering. While generously affirming that "any back street will do for a museum," it maintains that "Englishmen cannot fairly be charged with undervaluing Natural History; witness the crowds that throng the great Grecian temple in Bloomsbury, to pause at the cases of stuffed monkeys, and to marvel at the *horns of the megatherium!!*" as indeed well they may. Whether Englishmen can be charged with undervaluing natural history or not, it is clear that newspaper leader writers can be charged with expressing a confident opinion on subjects in which they are unacquainted with the most elementary facts. Clearly there is room for more science teaching.

AT a recent meeting of the Franklin Institute of Philadelphia Professor Morton resigned the office of Resident Secretary, he having received an appointment in a distant city. The resignation was accepted, and the President stated that Professor Morton had consented to continue his charge of the *Journal of the Franklin Institute*, which had achieved so desirable a position under his management. The new position accepted by Professor Morton is that of President of a College of Mechanical Engineering, to be established in Hoboken, opposite New York. He is succeeded at the Institute by Dr. W. H. Wahl.

THE Council of the Institution of Civil Engineers has awarded the following premiums:—1. A Telford Medal and a Telford Premium, in books, to Edward Dobson, Assoc. Inst. C.E., for his Paper on "The Public Works of the Province of Canterbury, New Zealand." 2. A Watt Medal and a Telford Premium, in books, to R. Price Williams, M. Inst. C.E., for his Paper on "The Maintenance and Renewal of Railway Rolling Stock." 3. A Watt Medal and a Telford Premium, in books, to John Thornhill Harrison, M. Inst. C.E., for his Paper on "The Statistics of Railway Income and Expenditure, and their bearing on future Railway Policy and Management." 4. A Telford Medal and a Telford Premium, in books, to John Sopwith, jun., M. Inst. C.E., for his Paper on "The Dressing of Lead Ores." 5. A Telford Medal and a Telford Premium, in books, to James Nicholas Douglass, M. Inst. C.E., for his Paper on "The Wolf Rock Lighthouse." 6. A Watt Medal and a Telford Premium, in books, to George Berkley, M. Inst. C.E., for his "Observations on the Strength of Iron and Steel, and on the Design of parts of Structures which consist of those Materials." 7. A Watt Medal and a Telford Premium, in books (to consist of the second series of the Minutes of Proceedings, vols. xxi. to xxx. inclusive), to Robert Briggs, of Philadelphia, U.S., for his Paper "On the Conditions and the Limits which govern the proportions of Rotary Fans." 8. A Watt Medal and a Telford Premium, in books, to Edward Alfred Cowper, M. Inst. C.E., for his Paper on "Recent Improvements in Regenerative Hot Blast Stoves for Blast Furnaces." 9. A Telford Premium, in books, to John Grantham, M. Inst. C.E., for his Paper "On Ocean Steam Navigation, with a view to its further development." 10. A Telford Premium, in books, to Daniel Makinson Fox, M. Inst. C.E., for his "Description of the Line and Works of the Sao Paulo Railway, in the Empire of Brazil." 11. The Manby Premium, in books, to Emerson Bainbridge, Stud. Inst. C.E., for his Paper on "Coal Mining in Deep Workings." The Council have likewise awarded the following prizes to students of the Institution:—1. A Miller Prize to Robert William Peregrine Birch, Stud. Inst. C.E., for his Paper on "The Disposal of Sewage." 2. A Miller Prize to Henry Thomas Munday, Stud. Inst. C.E., for his Paper on "The Present and the Future of Civil Engineering." 3. A Miller Prize to William Walton Williams, jun., Stud. Inst. C.E., for his Paper on "Roads and Steam Rollers." 4. A Miller Prize to Sidney Preston, Stud. Inst. C.E., for his Paper on "The Manufacture and the Uses of Portland Cement." 5. A Miller Prize to Edward Bazalgette, Stud. Inst. C.E., for his Paper "On Underpinning and making good the Foundations of the Irongate Steam Wharf, St. Katherine's, London." 6. A Miller Prize to Josiah Harding, Stud. Inst. C.E., for his Paper on "The Widening of the Liverpool and Manchester Railway between Liverpool and Huyton, and on the Construction of a Branch Line to St. Helen's." 7. A Miller Prize to the Hon. Philip James Stanhope, Stud. Inst. C.E., for his Paper on "The Metropolitan District Railway."

THE fabled alligator captured in the Thames some months ago has been surpassed by a hippopotamus disporting itself in the Seine. The scarcity of water has been so great in the Jardin des Plantes, that his majesty had been taken by his keepers to the river for his daily bath, securely held, as was thought, by his chain. One day, however, he snapped his chain during his gambols, to the no small dismay of the *blanchisseuses* and steamboat passengers, one lot of whom he threatened to demolish at a mouthful. Several keepers who attempted to board him were treated to a playful ducking, but after the upsetting of a good number of small boats, he was at length captured and hauled ashore. The poor brute must have thought that the good old times of the pre-glacial epoch had returned.

A SPECIAL general meeting of the Fellows of the Royal Geographical Society was held on Monday last at the Royal Institution, Albemarle-street, to consider the question of purchasing a large freehold house for the society's map rooms, library, and offices. The president, Sir Roderick Murchison, explained the necessity for further accommodation, and stated that the Council had at length concluded to purchase the large house No. 1, Savile Row. The meetings, however, would still be held in the great hall of the University of London. The resolution to purchase the property in Savile-row for 14,400*l.* was carried without opposition.

THE Second Report of the King's School Natural History and Natural Science Society, Sherborne, shows the activity with which the study of natural science is followed in the school. As might be expected in so rich a neighbourhood, geology appears to be the branch of science most zealously cultivated.

WE have received the First Annual Report of the American Museum of Natural History. Until 1869 New York was behind Boston, Philadelphia, Washington, and Chicago in possessing no Natural History Museum, a position which a number of gentlemen then determined their city should no longer occupy. Rooms were therefore secured in a building in the Central Park, and large subscriptions were at once collected, and employed in purchasing collections in Europe. Baron Osten-Sacken and other gentlemen also made large donations of specimens and books; and thus, without any assistance from the State except a grant from the duplicate specimens of Natural History belonging to it, the New York Museum of Natural History is in a fair way to become one of the most important on the continent.

THE Natural History Association of Natal has issued its second report. Papers were read during the past session by the Rev. Dr. Callaway, on phenomena occurring among the natives akin to mesmerism, the basis of their faith in divination, in which many interesting and striking facts were brought forward. Mr. Windham's paper on "The Gamebirds of Natal," was illustrated by a collection of mounted specimens, chiefly shot by himself, and generally identified by Mr. Layard, whose work on the birds of South Africa forms the recognised authority on the subject. In entomology, valuable papers were read by Mr. H. C. Harford, on the larvæ and pupæ of some Lepidoptera of Natal; by Mr. Morant, Notes of a collecting trip in the Transvaal, during which several new and interesting species were captured; and by Dr. Seaman, on protective resemblances in some local forms of insect life. In botany there are records of a new climbing Scrophulariaceous plant, *Buttonia natalensis*, discovered by Mr. E. Button, and of a new date-palm, detected by Mr. M'Ken, curator of the Natal Botanic Gardens. The colony may be congratulated on possessing so energetic a society.

THE Portuguese Consul-General at Bangkok, a hale man and an excellent swimmer, while bathing in the River Menam, suddenly sank from having come into collision with an electric eel, and was drowned. The Siamese say such deaths are not uncommon.

WE have received from Mr. M. J. Barrington-Ward a syllabus of a course of botanical lectures recently delivered at Clifton College, accompanied by the following gratifying remarks :—"My lectures were attended, I should say, by 100 ladies, and I can well bear out what was said in NATURE a few weeks ago as to the ability which women display at such classes. I never met with so much diligence and real skill in pupils before, and I only wish I could send you some of the papers written by these ladies to show how well and logically they handled a scientific subject."

MR. W. CROOKES has reprinted for private circulation his article in the current number of the *Quarterly Journal of Science* on "Spiritualism viewed by the Light of Modern Science." While admitting that phenomena have come under his notice which seem inexplicable on any known physical laws, the main part of Mr. Crookes's paper is occupied by a statement of the tests to which "Spiritualists" should subject their manifestations, which they have at present failed to do.

"DER rationelle Wiesenbau, dessen Theorie und Praxis," by L. Vincent, an exhaustive treatise on arable agriculture, has now reached its third enlarged edition.

*FACTS AND REASONINGS CONCERNING THE HETEROGENOUS EVOLUTION OF LIVING THINGS**

III.

THE results at which we have arrived now require to be looked at from two or three different points of view.

In the first place, with regard to these latter experiments, in which, with the help of Dr. Frankland, a perfect vacuum was procured in the experimental flasks previous to their being hermetically sealed, and before the exposure of them and their contained fluids to the temperature of 146 to 153°C. for four hours, it is desirable to know what the influence of such a temperature would be upon fungus-spores and filaments purposely exposed thereto. It is certain that, so far as all experimental observations have gone at present, no fungus-spore has been known to germinate after it has been exposed in a fluid to a temperature of 100°C. for even a few seconds.† What then would be the effect of a temperature of 150°C. for four hours? Is it possible that a fungus-spore or a fungus-filament at all similar to those which were met with in the preceding experiments could remain as such—could retain its morphological characters, in fact, after an exposure in fluid to a temperature of 150°C. for four hours? With the view of answering this question, I placed a quantity of a small fungus, consisting of mycelial filaments and multitudes of spores, closely resembling although not quite so delicate as those which were met with in the saline mixtures, into a solution (of the same strength as that which had been previously employed) of tartrate of ammonia and phosphate of soda in distilled water, and then handed it over to Dr. Frankland with the request that he would kindly treat this in the same way as he had done the other four solutions. Accordingly, on May 11, a vacuum having been produced within the flask before it was hermetically sealed, the solution was submitted in the same digester to a temperature of 146° to 153°C. for four hours When taken out from the digester, the previously whitish mass of fungoid filaments and spores had assumed a decidedly brownish colour, and it was in great part converted into mere *débris*. On the following morning the flask was broken, and some of the remains of the fungus and its spores were examined microscopically. *The plant was completely disorganised: not a single entire spore could be found;* they were all broken up into small more or less irregular particles, and the filaments were more or less empty, containing no definite contents, and being only represented by torn tubular fragments of various sizes. This utter disorganisation was in striking contrast with other specimens of the fungus, as it existed before exposure in the digester, which I had mounted in order to retain for purposes of comparison. And from the amount of destructive influence which was exercised upon the microscopic fungus in question, we may fairly imagine that the destructive influence of a similar temperature for four hours upon the still more delicate fungus represented in Fig. 17 would have been by no means less in extent. It would seem, at all events, well-nigh impossible that such a fungus could have pre-existed in the solution before its exposure in the digester, and could afterwards have retained all its morphological characters unimpaired, as they may be seen in the specimen now in my possession, from which the above-mentioned drawing was made. The plant must have been developed, therefore, within the flask itself subsequently to its exposure in the digester. What then could its origin have been? No fungus-spore has hitherto been known to germinate—no previously Living thing has been known to live—after the fluid containing it has been raised to a temperature of 100° C. for a few seconds. The fluid in Experiment 19 had however been raised to a temperature of 146° to 153° C. for four hours. We have even seen, in addition, that such a temperature completely disorganises certain closely allied fungus-spores, so that there is good reason for presuming that it would be similarly destructive to such spores as are represented in Fig. 17 if they had pre-existed in the solution. All that I have just said applies equally to the fungus-spores found in Experiments 18 and 20, and to the Ciliated Monad found in the turnip solution.

For the present, therefore, all presumptions, based upon the best available scientific evidence, are strongly in favour of the *de novo* evolution of these organisms within their respective flasks.

Whilst it seems, however, that the Living things which were found in these four experiments must have been evolved *de novo*, it does not follow necessarily that they were evolved in Experiments 19 and 20 out of the re-arranged elements of the saline substances themselves, because no proof has been offered that these substances were chemically pure. That such *may* have been the origin of these Living things seems, however, to be possible from what I have already said, and will, I think, appear even probable, after a due consideration of some of the facts which are now about to be related.

"Germs" are supposed by many to be universally diffused, more especially in the air and within organic substances. It seemed possible, however, and only reasonable, to suppose that they might exist much less abundantly in saline materials than within organic substances, and this was one reason why such materials were made use of in my later experiments. In order to ascertain whether any visible organisms or spores were to be found in the saline materials employed, portions of these have been repeatedly dissolved by distilled water in a watch-glass, and the fluid has afterwards been submitted to the most careful microscopical examination. Moreover, after sufficient time has been allowed for subsidence, the bottom of the watch-glass has then been most carefully scrutinised by a powerful immersion lens. The saline materials employed in the preceding experiments have been potash-and-ammonia-alum, tartar emetic, phosphate of soda, phosphate of ammonia, oxalate of ammonia, acetate of ammonia, carbonate of ammonia, and tartrate of ammonia. The result of repeated examinations of these substances in the manner above stated, has been that not a trace of anything like an organism—no fungus-spore, germ, or egg of any kind—has been found in solutions of any of the substances employed, except in one. This one in which such bodies have been found is that which I have named last—the neutral tartrate of ammonia.

Several of these salts—the oxalate, the acetate, the carbonate, and the tartrate of ammonia—contain within themselves all the ele-

* (Concluded from p. 201.)

† Such a temperature, also, very frequently suffices to produce a considerable amount of disintegration in fungus filaments which are submitted to its influence. It is almost impossible that a perfect organism with a mass of loose spores around it could have braved such a temperature for fifteen minutes, and could then have presented an appearance such as is represented in Fig. 14, the original of which is still in my possession. If not the result of a new evolution, therefore, this fungus must have been developed from a spore which was able to germinate after having been boiled for fifteen minutes; and if so it would be an exception to a rule which has hitherto been found to be general.

ments necessary for the building up of organic substances. Nitrogen, carbon, hydrogen, and oxygen are there, and only require to fall into other modes of collocation in order to give birth to an organisable material. The crystals of the oxalate are very small, those of the acetate are very deliquescent, and carbonate of ammonia exists generally in the form of non-crystalline cakes.* The neutral tartrate, however, exists in the form of large distinct prismatic crystals. Solutions of the first three substances showed no trace of Living things; though organisms were frequently discovered when crystals of tartrate of ammonia were examined.

Before describing these organisms more particularly, it will be well to glance for a moment at the origin or mode of preparation of this salt. The tartaric acid entering into its composition is obtained from *argol*—the crude bitartrate of potash derived from the grape. And although this latter salt is derived from the tissues of a Living plant, the processes to which it is submitted, in order to obtain the tartaric acid in an uncombined state, would most certainly destroy all living "germs" that might have been contained therein. After a solution of the bitartrate of potash has been boiled for a time, tartrate of lime is gradually precipitated by the addition of chalk and chloride of calcium. The insoluble tartrate of lime, after having been washed several times, is then brought into contact with *strong sulphuric acid, diluted with only about four times its bulk of water*, and this mixture is boiled for half an hour.† All this is necessary before a filtrate can be obtained from which the first crystals of tartaric acid are procurable. Ammonia, the other constituent of the neutral tartrate, being a product of the destructive distillation of coal tar, and itself exercising such a destructive influence upon organic matter when existing in the form of strong *liquor ammoniæ*, would not seem to be a very promising nidus for organic "germs." The neutral tartrate of ammonia is, however, prepared by mixing a solution of tartaric acid, procured as above mentioned, with an adequate quantity of liquor ammoniæ, and then evaporating the mixture at a gentle heat. Thus prepared, the crystals contain a notable quantity of water of crystallisation, and are not specially liable to contain organic impurities.

In the stock of crystals that I obtained from Messrs. Hopkin and Williams,‡ and which had been made about six months previously, some were well formed, and almost perfectly transparent, whilst others were less regular in shape, and presented an opaque appearance with more or less of striation within. When a crystal of moderate size was taken, about $\frac{1}{3}''$ in diameter, or a portion of a larger one, and was placed in a large watch-glass with some distilled water, it was frequently found that at first a certain number of opaque-white scales, having a granular aspect under a high magnifying power, dropped from the surface of the crystal to the bottom of the watch-glass. This material, which seemed to have been produced by some superficial alteration of the substance of the salt, dissolved with much more difficulty than the unaltered matter of the crystal. It remained for a long time at the bottom of the glass, and only very slowly disappeared. As the substance of the crystal slowly dissolved away, a number of large and small gaseous bubbles gradually escaped from it. When the crystal was examined with a one-inch object-glass whilst solution was taking place, these air bubbles could be seen at first within cavities, from which they were afterwards liberated by a solution of their walls. Occasionally, from the very centre of a crystal, from which bubbles of gas had been escaping, there floated out a very small and almost invisible filamentary mass, more or less thickly studded with minute air bubbles. Such masses were just visible with an ordinary pocket-lens, and when transferred on the point of a needle to a slip of glass, and examined with a magnifying power of about 600 diameters, they were found to contain more or less of the following constituents:—(1) a minute fragment of cotton or paper fibre; (2) a variable quantity of an almost transparent, insoluble plate-like substance, homogeneous, though broken up in all directions by intersecting cracks; (3) more rarely a small quantity of a tenacious mucoid matter, containing refractive protein-looking granules of various sizes; (4) a quantity of a colourless, confervoid-looking mass, some of whose smaller filaments, $\frac{1}{20000}''$ in diameter, looked like a mere linear aggregation of irregular masses of protoplasm, though in certain larger filaments continuous with these it became obvious that the irregular protoplasm masses were contained within a delicate hyaline cylinder across which dissepiments were sometimes to be seen, as in very minute fungus-filaments; (5) and lastly, certain fungus-spores in almost all respects similar to those which have been met with in so many of the saline experimental fluids. Although four or five of these were frequently interspersed amongst the confervoid-looking filaments, they did not seem to be in organic connection with them. The confervoid-looking, though really abortive fungus-filaments, were also almost precisely similar to the filaments containing irregular masses of protoplasm which were met with (in Experiments 12 and 13), in solutions containing tartrate of ammonia.

Repeated examination of crystals during their dissolution convinced me that such organic bodies invariably came from the interior of the crystal, often from its very centre, and that they were not to be met with on its surface. Seeing, however, that minute shreds of cotton or paper fibre also as frequently came from the interior of the crystal,* it was obviously possible that the organisms met with might have been engaged mechanically during the process of crystallisation, just as it must have happened with the shreds above mentioned. From what has previously been stated concerning the mode of preparation of the neutral tartrate of ammonia and the origin of its constituents, it may be considered almost certain that these organisms could not have pre-existed in the strong *liquor ammoniæ*, and that all living organisms which might by chance have been associated with the bitartrate of potash must have been hopelessly destroyed by the boiling with sulphuric acid, which occurred at one stage in the process employed for the separation of the tartaric acid from its base. During the subsequent process of crystallisation of the tartaric acid from its mother liquor, it is of course possible that any spores existing in the adjacent atmosphere might have dropped into the fluid, and have then become mechanically enclosed within the crystals; and the same chance of such a contamination with spores would exist during the process of crystallisation of the tartrate of ammonia itself. If this, however, had been the real source of the fungus-spores and masses of confervoid-looking filaments, such bodies *might* be found in freshly prepared crystals just as well as in those which had existed for six months.† I therefore asked Messrs. Hopkin and Williams to prepare for me a fresh batch of crystals of neutral tartrate of ammonia. This they were kind enough to do; obtaining them in the same place by the same process, and exposing the mother liquor in a precisely similar way.

An examination of some of these crystals, whilst they were being dissolved by distilled water in a watch-glass, showed that (unlike the older crystals) they were not at all coated on the surface by the comparatively insoluble granular plates; and that only a few very small air bubbles emerged from their interior. And at the bottom of the watch-glass, neither during dissolution nor afterwards, was there seen any trace of the confervoid-looking filaments or of the fungus-spores, though minute shreds of cotton and paper fibres were seen similar to those which were found in the older crystals. An examination of a large number of the new crystals was attended with similar results to those just mentioned.

This absence of the *confervoid-looking filaments* and of the *large fungus-spores* from the recently prepared crystals may be accounted for by either one of two suppositions:—

First. It may be supposed that in the case of the older crystals, the spores and filaments had dropped as such into the solutions in which the tartaric acid alone, or the tartrate of ammonia was crystallising; that they were mechanically engaged in the crystals, and were subsequently liberated unchanged (without having undergone any growth or development) on the dissolution of the crystal.‡ Whilst, on the other hand, in the case of the recent crystals, it may have happened that no such filaments or spores were floating in the atmosphere at the time of *their* formation, and that, consequently, none could have dropped into the solutions. Hence none of these could have been enclosed within the crystals.

* Obtained by a process of sublimation at high temperatures.

† The boiling point of such a solution would be several degrees above 100° C. Heat and acid combined exercise a most powerfully destructive influence upon organic matter, though even *very* dilute sulphuric acid, at *ordinary* temperatures, has been found to be peculiarly destructive to all Living things.

‡ Of New Cavendish Street.

* I had often been surprised at finding such shreds when I submitted some of my experimental fluids to microscopical examination, knowing that I had frequently used freshly prepared distilled water, and had taken every precaution thoroughly to cleanse the flasks which were employed.

† I have been unable to obtain crystals of the neutral tartrate of ammonia of an older date than this, and I should feel much obliged to any one who could send me such specimens, or who could furnish me with a few *crystals* of carbonate of ammonia.

‡ If they had been engaged within the crystals of tartaric acid, they must have been liberated from these during the preparation of the neutral tartrate, only to be re-entangled whilst the crystals of this salt were forming.

This supposition is, I think, unlikely to be the real explanation of the difference between the two sets of crystals. My reasons for so thinking will, however, appear more fully during the discussion of the other supposition.

Second. It may be supposed, on the other hand, that the confervoid-looking *filaments* and the *spores* are organisms which have assumed their existing forms and dimensions by a process of growth and development within the crystal, and that *the starting-point of each alike was a mere speck of Living Matter.*

By this supposition we give the panspermatists the full benefit of our microscopical researches, and so narrow their real requirements in the matter of pre-existing spores. It becomes a much simpler case for them, if instead of being compelled to calculate upon the pre-existence of fully formed fungus-spores, and of confervoid-looking filaments, they need only presume upon the pre-existence of a mere speck of Living matter less than $\frac{1}{60000}$″ in diameter. I most candidly confess, however, that the pre-existence of such specks of living matter is all that is really necessary for them.* Most of those who have worked much at the microscopic investigation of the organisms met with in organic infusions, must have come to the conclusion that there is no break in the continuity of that developmental series which commences with the mere speck of living matter—the primordial *Monad*—and thence proceeds through such forms as the *Bacterium*, the *Vibrio*, the *Leptothrix* filament, and the mycelial filament of a microscopic fungus. I do not mean to say that this is a necessary order of development, which invariably occurs—far from it, but rather that, as *Bacteria* commence their visible existence in the form of Monads, so *Vibrios* are but the developed representatives of certain *Bacteria*, just as the various kinds of *Leptothrix* filaments grow from certain pre-existing *Vibrios*, and just as certain of these *Leptothrix* filaments themselves may perchance become modified into larger segmented fungus-filaments, which, under favourable conditions, may fructify and produce spores, each of which is capable of developing into a plant like its parent in its latest phase of evolution. Originating, then, in the form of the minutest visible Living speck, we may find an organism passing more or less rapidly through the *Bacterium* and the *Vibrio* phase in order to grow into a *Leptothrix* thread, which, in its turn, by further growth and development, may give rise to a microscopic fungus producing large and definite spores. These fungus-spores, under similar influences, are capable of developing at once into a mycelium similar to that from which they have been produced. They do not again go through the lower terms of the series, but are veritable spores, serving only immediately to reproduce a fungus. It is an undoubted fact, on the other hand, which although often stated, is not generally known or admitted, that *Torula* cells and other fungus-spores may also originate as minutest visible *Living specks, which grow and develope at once into fungus-spores,* instead of passing through the intermediate stages of *Bacterium, Vibrio, Leptothrix*, and fungus-mycelium.

* Although this supposition is so far favourable to the views of the panspermatists, since it makes their real requirements so much more simple, still I am afraid they will find it a most troublesome and unorthodox supposition, unless they are disposed at the same time to become out-and-out developmentalists. Their position would be a much more easy one than it is at present if they chose to maintain that such specks of living matter—whatever their precise origin may have been—are practically mere specks of indifferent living matter, having no inherent tendencies, but plastic to the full, and capable of growing into such forms as their environing conditions may determine. (But having thus "swallowed a camel," why should they "strain at a gnat"? Why should they not also believe that the speck of indifferent living matter itself was formable by concurrence of necessary matter and conditions?) Unless the panspermatists were to adopt some such thorough-going developmental views as that which I have just indicated, they will gain comparatively little from the concessions which science compels us to make to them. They will better be able to reconcile their position with the comparative paucity of definite spores and germs which are actually detectable in the atmosphere; but they will find it as difficult as ever to account for the fact *that the right spores or germs should always be in the right place at the right time.* Very little short of a belief that each cubic inch of air contains the germs of myriads of organisms which are known, or which may hereafter be found under previously unknown sets of conditions, would be adequate to account for all the known and observable correspondences between the organisms found, and the precise nature of the fluids employed. And although the wildness and extreme improbability of this supposition must seem patent to all who have a knowledge of such subjects, strange to say, there are very many scientific men who would rather harbour such a belief—who would even, in spite of all laws of evidence, think it more probable—than another supposition, which is, on the contrary, in thorough harmony with all the main principles of their scientific creed. That a "vitalist" should reject this other supposition I can understand; but that all those scientific men—and they are happily numerous—who have discarded the notion of a special "vital principle," should still reject the notion that Living matter is capable of being evolved under suitable conditions and yet should accept this Panspermic hypothesis seeing the nature of the evidence which is respectively adducible in favour of the two views—seems to me almost inexplicable.

There is, indeed, strong reason for believing that the spores and confervoid-looking filaments in question have not dropped as such from the atmosphere, but that they are, rather, organisms which have developed within the crystal. It is almost impossible not to be struck with the improbability of the former of these alternatives, on account of the number of such large spores and filaments which this supposition would require to have been present in the atmosphere over the pans containing the crystallising materials, as compared with the extremely limited number of such large organisms which have ever been obtainable when experimental observations have been made upon the nature of the solid particles existing in the air of all ordinary localities.* The best evidence, however, in proof of the view that they are products of a development which has taken place within the crystal would be, if it could be shown that in a given batch of recently prepared crystals no such organisms were to be found, whilst in many other crystals belonging to the same batch, after an interval of weeks or months, the spores and filaments were to be discovered. Sufficient time has not yet elapsed to enable me to speak definitely on this subject. This much, however, I can say. Certain of the crystals of the batch prepared for me by Messrs. Hopkin and Williams, when examined two days after preparation, were found to contain scarcely a trace of air within. Now, however, after an interval of three weeks, through which they have been kept during the day-time at a temperature of about 80° Fahr., certain other of these crystals do, when dissolved, give exit to a notable quantity of air bubbles. This seems to indicate pretty clearly that a change of some kind has been taking place in the material of the crystal, which has led to the liberation of some of its constituents in a gaseous condition, and also, perhaps, to a liberation of some of its water of crystallisation. Whilst this has been taking place, its other elements may have been grouping themselves anew. Although, at present, there is still no certain trace of the spores or filaments, I am strongly disposed to expect that such organisms will manifest themselves in the course of a few weeks more.

[Two weeks after writing the above paragraph, and whilst these proofs were going through the press, on June 9 I examined three more specimens from the recent batch of crystals which had been set aside for observation. The quantity of gaseous bubbles which escaped from within the crystal seemed almost equal to those which had been met with within the older crystals. One or two small fragments of cotton also emerged, and in addition several very small masses of a transparent mucoid material, containing refractive protein-looking granules of various sizes and shapes. These were almost precisely similar to masses which had been met with in the older crystal. Here and there an early stage, or short portion, of a filament was seen amongst the granules, though none of these were sufficiently long to make me certain as to their nature and affinities. Although nothing else was found, the occurrence of the very small masses of mucoid material seemed to represent a stage in advance of what was met with at the last examination. One of these small mucoid masses I saw within an elongated cavity (near the surface of a half dissolved crystal), two-thirds of which was occupied by a large bubble of gas. Whilst the crystal was still under the microscope, I saw the bubble and the small mucoid mass emerge from the cavity.]

Assuming, then, the view which seems most probable, that the spores and filaments have grown within the crystal—that they are the developed representatives of certain specks of Living matter—two views may still be taken as to the origin of such Living specks. Either (1) these are some of the pre-existing "germs" of the panspermatists which have become mechanically enclosed within the crystal, or (2) these Living specks have been therein evolved by virtue of certain changes and re-arrangements which have taken place amongst the non-living constituents of the crystalline matter.

Of these two alternative views I am, after reflection on the following considerations and evidence, strongly inclined to believe that the latter is most probably the true one:—

(*a*.) It must be remembered that however strange and unlikely a situation the interior of a crystal may appear for the evolution of organisms, there is the strongest reason for believing that cavities are formed within crystals of tartrate of ammonia,†

* In all my investigations I have never met with spores *similar* to these except in one or other of the ammoniacal solutions.

† The gases which appear in bubbles increase in quantity with the age of the crystal, and these gases have been seen to be lodged in cavities within the crystal. These cavities are, perhaps, more especially liable to

and there is almost as much reason for believing that the confervoid-looking filaments and the fungus-spores have undergone a process of *growth* and *development* within such cavities. Other facts, which seem to lend an increased probability to this supposition, will shortly be detailed. But if "the conditions" are favourable enough to permit, or even to stimulate the molecular activity of certain Living particles, and if such molecular activity, whereby the Living speck grows and developes, is but the modified manifestation of the physical forces acting thereupon, I see no theoretical reason why the self-same physical forces acting upon the self-same materials should not have been able, in the same place, to *initiate* a molecular collocation similar to that which they now help to build up from moment to moment. We have been, perhaps, only too much in the habit of looking upon this as impossible. But let us sweep away this habit of mind for the moment, let us look at the facts as they are, and will it be at all easier for us, who believe in no special "vital principle," to understand how from moment to moment non-living matter is converted into matter which lives? However little we may understand it, this process is continually taking place in all growing representatives of the vegetable kingdom, and no one ever thinks of doubting that it does take place because he is unable to understand *how* it occurs. If it were once conceded that a *de novo* evolution of specks of Living matter were possible, then I think most physiologists would at once admit that where specks of Living matter are able to grow and develope, there also they may be quite capable of originating.

(*b.*) The matter of the crystals of tartrate of ammonia is, by a re-arrangement of its atoms, quite capable of giving origin to organisable compounds. If a small quantity of tartrate of ammonia is dissolved in a watch-glass with distilled water, and is protected as much as possible from dust and evaporation by being covered with a wine-glass from which the stem has been broken, and then again with a tumbler, it will be found during warm weather, that in the course of two or three days the bottom of the watch-glass is covered by a number of minute microscopic crystals, interspersed amongst a mixed layer composed of monads, bacteria, and minute *Torula* cells.* These organisms form, in fact, almost as freely (though more slowly) in the ammoniacal solution, as they do in an ordinary infusion containing organic matter. There can be little doubt that the amount of ammonia and of tartaric acid actually diminishes, and that the elements of these enter into new combinations.†

It may be said, however, that such changes do not take place by the mere action of physical forces upon the unstable molecules of the dissolved tartrate of ammonia, and that *Living ferments* are necessary for the initiation of such molecular re-arrangements. In answer to this I can only call attention to the fact that similar changes must have taken place in the fluids within the experimental tubes which were submitted by Dr. Frankland to a temperature varying from 146° to 153° C. for four hours, and that there is not one tittle of evidence at present existing to show that any Living thing could live through such an exposure, whilst there are very strong reasons indeed which should incline us to believe that no Living thing could be subjected to such a temperature without being hopelessly destroyed. Therefore in these cases it would appear that such molecular re-arrangements must have been initiated without the intervention of Living ferments, and thus, too, they would appear to be comparable with those that are known to take place in a solution of cyanate of ammonia. Here "spontaneously," or with the aid of a little heat only, a molecular re-arrangement occurs, and the saline cyanate of ammonia is replaced by a colloidal compound, urea. In order to effect this transformation, no Living ferments are necessary—none have been even supposed to exist, and there is, really, no more reason why we should imagine their presence to be necessary in order that tartrate of ammonia may undergo a more or less similar isomeric transformation.

A careful examination of the mode in which bacteria and *Torula* cells appear at the bottom of a watch-glass containing tartrate of ammonia in solution is also rather valuable on account of its bearing upon this question. What is true of the *Torula* cells is also true concerning the mode of origin of bacteria; the facts, however, can be ascertained rather more satisfactorily concerning the *Torula* cells, and for the sake of brevity I shall now speak only of them. These *Torula* cells, like the bacteria in their earlier stages, are motionless; although, therefore, they increase rapidly after one or more have been formed by a process of pullulation and growth, the numerous quite distinct patches which may be seen scattered over the bottom of the watch-glass, often at well marked distances from one another, represent so many distinct centres of origin. In these several patches there may be seen delicate ovoid *Torula* cells of almost any size beneath $\frac{1}{8000}''$ in diameter. The larger cells exist united in little groups of twos and threes, and budding from them may be seen pullulating projections of different sizes. Separate cells, also, may be seen, smaller and smaller in size, till at last they cease to be cellular in form, and we see only peculiarly refractive dots or specks less than $\frac{1}{60000}''$ in diameter. In other places a colony of *Torula* cells seems to be about to grow up. Here there may be seen merely one or two of the smallest bodies which distinctly display the cellular form interspersed amongst a variable number of the refractive specks of all sizes down to the *minimum visible* stage.* Beyond this, of course, all is darkness. We must be guided by other evidence in forming an opinion as to the probable source or mode of origination of these specks of Living matter, which are so extremely minute that they only just come within the range of our aided vision.

Another remarkable observation made upon a simple solution of carbonate of ammonia, in a watch-glass, makes still clearer the fact of the disseminated origin of organisms in such solutions. It throws light also upon the previous question as to whether the fungus-spores were developed within the crystals of tartrate of ammonia from specks of Living matter, or whether they were mechanically enclosed in their developed form; and it is sufficiently suggestive as to the possible influence of electrical conditions in promoting evolutional changes. Referring to notes made at the time, I extract the following particulars. About eleven P.M. on the 14th of the present month (June) a small quantity of ordinary sesquicarbonate of ammonia was dissolved in some apparently pure (though not distilled) water, in a watch-glass. After solution, and in about an hour's time, the fluid was carefully examined with different microscopic powers, and lastly the bottom of the watch-glass was scrutinised in very many situations with an immersion $\frac{1}{12}''$ object-glass. No Living thing of any kind was seen, though scattered over the bottom of the glass were a large number of tiny crystals, some larger and some smaller than $\frac{1}{3000}''$ in diameter. Under the polariscope they gave the most beautiful and varied colour reactions. The watch-glass was then placed on a mantel-piece with a soft surface (covered with velvet), a wine-glass, with its stem broken off, was inverted over it, and this again was covered by a tumbler, in order, as much as possible, to prevent evaporation and keep out dust. After twenty-four hours the bottom of the watch-glass was again carefully examined, with the $\frac{1}{12}''$ object-glass, and no change was observable. There were the same minute crystals, perhaps rather more numerous than before, but no recognisable specks of protoplasm or other trace of living things. The watch-glass was then replaced as before. The next day (June 16) the weather was hot and extremely sultry. The temperature was about 85° F. in the shade, and the thunder-storm, which seemed imminent during the whole of the day, began about 7 P.M., and continued till the early hours of the morning of the following day. At about 11.30 P.M. of this 16th of June, I again examined the solution in the watch-glass—forty-eight hours after it had been prepared. Then, scattered over the whole of the bottom of the glass, fungus-spores were seen in all stages of development intermixed with the small crystals. They were quite motionless, and mostly separate, rather than in distinct groups. They varied in size from the minutest visible speck up to a spherical nucleated body $\frac{1}{3300}''$ in diameter. No moving particles or bacteria were seen. Probably more than a thousand of these bodies were developing in the one watch-glass—each growing in its own place, and showing no evidence of multiplication by division or pullulation. Until they attained the

form in those crystals which are not perfect in shape, and which present a more or less opaque appearance in their interior. These less perfect types are probably for that reason more prone to undergo molecular changes under the influence of incident forces, especially in the neighbourhood of and around some fibre-fragment which has been enclosed.

* In saline solutions I have generally seen the organisms first, and have found them accumulated principally at the *bottom* of the watch-glass or other vessel in which the solution may have been contained.

† Saline solutions in which spores of fungi were placed, having been analysed previously by M. Pasteur, were again analysed by him after the plants had grown for a time. The proportion of ammonia and of other ingredients was found to have undergone a diminution correlative with the growth of the plants.

* When such a patch is marked, and watched at different intervals, a crop of perfect *Torula* cells is soon seen to occupy this same situation. And it may be well to state here that *Sarcina* also makes its appearance after a fashion which is essentially similar.

size of about $\frac{1}{10000}''$ in diameter no nucleus was visible, though they had by this time assumed a distinctly vesicular appearance. As the spores increased in size, the thick wall gradually became more manifest—though it had a rather rough granular appearance—and a nucleus gradually showed itself within, which was also granular.* The next morning, after twelve hours, the spores seemed to be much in the same condition, though numerous small colonies (30 to 50 in each) of motionless bacteria were now visible. During the day the air was clear, and the temperature lower (76° F.); and after twelve hours more (in

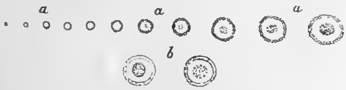

FIG 19.—Representing different stages in the development of Fungus-spores in a solution of Carbonate of Ammonia.

the evening) the bacteria were found to have considerably increased in number, and several of the fungus-spores were seen in a more devoloped condition—their thick walls being wholly or partially consolidated, and the nucleus was also more distinctly defined. In this condition they perfectly resembled the spores which were found in *Experiment* 20, and very closely resembled those which are to be met with in some of the old tartrate of ammonia crystals. The great majority of the spores were, however, still in the granular condition, and they seemed to have made no advance whatever. On the following day these spores were not quite so distinct—some of them seemed to be disintegrating, whilst none of them had undergone any further development. The bacteria, on the contrary, had decidedly increased in quantity. After two days more, minute *Torula* cells began to appear. These did not rapidly multiply, but soon began to develope into mycelial filaments.

The thick-walled spores had possibly come into existence under the influence of the high temperature and the disturbed electrical condition of the atmosphere †; and they seemed to be so much the creatures of these conditions that they were unable to survive under others which were different.

The mode, then, in which fungus-spores make their appearance in a solution of carbonate or tartrate of ammonia, seems to show that they must have originated in all parts of the solution, either by a coalescence and re-arrangement of the invisible molecules of a *pre-existing* colloidal compound,‡ or else through the development of innumerable but invisible "germs," which were disseminated through the liquid. That such invisible "germs" may have existed in the form of colloidal molecules, I am quite disposed to believe—though I am as strongly inclined to disbelieve that these fluids were saturated with "germs" of veritable fungus-spores, which had emanated from some pre-existing fungus of the same kind. We may grant that germs were there *in posse*, though not *in esse*. What warrant have we, indeed, for talking of actual though *invisible* fungus "germs"? No one can know more concerning their existence or formation than I know concerning the coalescence of colloidal molecules into minutest specks of Living matter. The necessity for the postulation of such "germs" must, therefore, seem different to different people, in accordance with the particular views which they may hold concerning Life. Those who believe in a special "vital principle" may naturally enough cling to the notion of a pre-existing germ, which may be the direct recipient of this peculiar power from some pre-existing organism; whilst those who are believers, rather, in the physical doctrines of Life will, I think, gradually find themselves contented with the pre-existence of potential "germs" in the form of colloidal molecules.

* This appearance I had not unfrequently seen before, where such spores had been developing in saline solutions, and it had always strongly suggested the notion to me that these spores were formed by a coalescence of granular particles. Here, however, there were no granules or moving particles present, the spores themselves were the only Living things, and it seemed quite certain that they could not have originated after this fashion. They obviously commenced as minute specks, and the granular appearance manifested itself so long as they were still increasing in size. When growth stopped consolidation began to take place, and an even double-contoured wall soon replaced that which was before irregular and granular.

† We may, perhaps, connect this possibility with the well-known fact that milk, beer, and other fluids are so very prone to turn sour during a thunder-storm, or whilst it is threatening.

‡ One which had existed before the organisms made their appearance, but which was the product of an isomeric modification of the carbonate of ammonia itself.

(*c*.) We find, also, associated with different sets of conditions, different kinds of Living things. In none of the crystals of tartrate of ammonia have I ever found a single distinct bacterium, and there has been the same complete absence of organisms of this kind in all my experimental fluids containing tartrate of ammonia and phosphate of soda, which have been sealed up *in vacuo*. This agreement is very striking, seeing that whenever a similar fluid, or a solution of tartrate of ammonia alone, is exposed to the air, then bacteria appear in abundance.* There is a marked accordance then between the organisms which are produced in the experimental tubes *in vacuo*, and those which come from the cavities within the crystals. There is the strongest reason for believing that the organisms which were met with within one of these experimental tubes must have been evolved *de novo*, since the existing state of our knowledge does not entitle us to believe that any such pre-existing Living thing could continue to live after it had been exposed to a temperature of from to 146° to 153°C. for four hours; and so we derive an additional presumption in favour of the *de novo* origination within the crystals, of those minutest specks of Living matter, which, as we have seen, are capable of developing into such fungus-spores as are there to be found.

In the face of this much more severe test (*Experiment* 19) it is needless to insist upon the results of other experiments in which the solutions were merely exposed to a temperature of 100°C. The fungus-spores which exist within the crystals of tartrate of ammonia do not differ, however, from all other fungus-spores that have been made the objects of experimentation. They too will not germinate after they have been exposed for one minute to a temperature of 100°C. I have taken spores and filaments from a crystal, and one half of them I have boiled for about a minute whilst the others have not been heated at all. The two patches have then been placed, at some little distance from one another, in the same growing box, with a few drops of a solution of tartrate of ammonia. The spores which had been boiled did not germinate, but those which had not been heated soon began to develope filaments. The pre-existing confervoid-looking organism, also, in the one case underwent no change, whilst in the other it grew into a distinct fungus—its filaments widening out till they became about four times as broad as they were originally. These unmistakeable fungus-filaments showed dissepiments at intervals dividing them into chambers, within which were contained large irregular blocks of protoplasm. Occasionally a filament larger than the others, might be seen terminating with a broad convex extremity, and afterwards there gradually appeared on the surface of this the minutest dot-like projections, which slowly increased in size and number. The larger of them soon became vesicular, and after a time within the vesicle granules began to cluster so as to constitute a nucleus. Thus were watched the early stages of the development of a head of fructification similar to, although much smaller than that which is represented in Fig. 17. The rate of growth was generally very slow, and after a time development ceased in my growing box, apparently because the conditions were not suitable for the evolution of such an organism as did grow luxuriantly enough within my experimental flasks. These observations were, however, extremely interesting, because I was thus able to trace all the stages in development, on one and the same plant, from mere granular abortive-looking *Leptothrix* threads, only $\frac{1}{20000}''$ in diameter, which gradually grew into a distinct confervoid-looking tube, having broken masses of protoplasm within, into slowly widening and dissepimented fungus-filaments, that were capable

* There is another difference also which deserves to be pointed out. The crystals of tartrate of ammonia or of phosphate of soda have never shown a trace of the Spiral-fibre organisms or of *Sarcina* (Fig. 13 *d*), and yet when the two have been mixed, in several of the fluids which have been kept *in vacuo*, the Spiral-fibre organism has appeared, and, similarly, on two out of three occasions when this mixture has been exposed to the air *Sarcina* has made its appearance. In one of the solutions *in vacuo* containing carbonate of ammonia and phosphate of soda, a somewhat similar Spiral-fibre has been found, and in the other *Sarcina* was met with. Both these organisms therefore seem dependent upon the presence of phosphates, and it is worthy of note that hitherto *Sarcina* has, so far as I am aware, never been known to exist except in one of the fluids of the animal body where phosphates naturally or unnaturally are present. At first *Sarcina* was discovered by Goodsir in the contents of the stomach, then it was found in the urine, and afterwards within the ventricles of the brain by Sir Wm. Jenner. And now I meet wth it in solutions containing an ammoniacal salt and a phosphate. M. Pasteur has (Ann. de Chim. et de Phys., 1862, Pl. II., fig. 27, K., and p. 80) figured, and alludes to an "Algue formée de cellules quaternaires, déposée sous forme de précipitate," upon the walls of a flask which had contained "l'eau de levûre non sucrée," and which certainly, if not *Sarcina*, must be very closely allied thereto.

of producing a head of fructification of the *Penicillium* type. Thus, in fact, there appeared to be a strong tendency in the *Leptothrix* filaments and in the loose spores found within the crystal, to develope into the same kind of organism when either of these was placed under the influence of other and more suitable conditions. In the crystal itself, apparently, just as the conditions were not suitable for the germination of the spores, so they were not favourable for the developmental conversion of the confervoid-looking filaments into a fungus.

Whether there was any genetic relationship existing between these confervoid-looking filaments which commenced life as *Leptothrix* threads, and the few scattered spores which were frequently found with them within the crystal, is not quite certain. If any relationship did exist, however, it could only have been of one kind: the spores may have been descendants from the matter of the filaments, but the filaments were most certainly not developments from the spores. The spores existed singly or in groups of twos and threes. They were never seen in organic connection with the filaments, so that I am inclined to believe they were not even formed by a process of budding. They must, then, either have derived their origin from a minute speck of the matter of the filament which subsequently grew into a spore,* or they must have been evolved *de novo* where they were found, just as we are compelled to imagine that the similar spores must have been evolved *de novo* within the flask used in *Experiment* 19, at first by a coalescence and re-arrangement of colloidal molecules, and subsequently by a process of development similar to what is represented in Fig. 19. And, if the fungus-spore and the confervoid-looking filament both tend towards the same ultimate developmental form, we can only attribute this to the fact of the existence of a harmony between the "conditions" and such an organism. The confervoid filament and the fungus-spore are both produced within the same crystal: they seem to be but different products of what appears to us to be the same matter and the same "conditions," and if minute differences may have existed at first tending to make the initial modes of development different, the main intrinsic similarity manifests itself at last by leading them both along a line of development which terminates in a common organic form.†

For these various concurrent reasons, therefore, I deem it much more probable that the filaments and spores found within the crystals of tartrate of ammonia have been developed from specks of Living matter there evolved *de novo*, rather than that they have originated from germs of similar pre-existing organisms which had accidentally been enclosed within the crystals.

Before closing this paper, it will be necessary that I should refer more particularly to a certain part of M. Pasteur's researches, seeing that these have so strongly influenced the opinions of very many scientific men on the question of the truth or falsity of the doctrines of the heterogenists. As an experimental chemist, M. Pasteur takes a most honourable position in the foremost rank of workers, and all his investigations on this subject appear to have been conducted with the most scrupulous care. His reasonings, also, may seem at first sight to be all convincing, so that most people might be inclined to admit that he had "mathématiquement demontré," as he so frequently claims to have done, all that he had set himself to prove. The case may seem at first a poor one indeed for the heterogenists; but as soon as one gets over the first impressions produced by the various experiments, and begins to inquire whether the reasonings concerning them have been in all cases fair and logical, then it may be seen that the evidence against the occurrence of heterogenesis is very far from being so strong as it, at first sight, appeared.

On two or three occasions, when it was very important that results should be looked at from different points of view, M. Pasteur has altogether failed to do this, and has wished to interpret them only in accordance with the views of the panspermatists, *quietly ignoring the equally legitimate interpretation of the same results which might have been given by the heterogenists.* At present I shall confine myself to one instance of this kind, because I think that on this particular point the reasonings of M. Pasteur are as mischievous as they are illogical. If others were to follow his example, then certainly we could never hope to get rid of the clouds of controversy which at present obscure this subject.

The experiments of Schwann were for some time erroneously believed by very many to have upset the doctrines of the heterogenists. No organisms, it was said, were ever developed in hermetically sealed vessels when the solutions containing the organic matter had been boiled, and when all the air which was allowed access to them had been previously calcined. Schwann's experiments did yield uniformly negative results when solutions of meat were employed; though his experiments concerning alcoholic fermentation yielded results which were sometimes positive and sometimes negative. M. Pasteur also, for a time, obtained only negative results in repeating the experiments of Schwann. In these experiments, however, he had generally made use of "l'eau de levûre sucrée," of urine, or of some other fluid which was naturally unfitted to undergo evolutional changes of a high order, or even to produce lower organisms in great abundance.* But there came a time when M. Pasteur chanced to repeat his experiments, using precisely the same precautions as before, and yet the results were quite different—organisms were now found in his solutions. There was one important difference, it is true. In these latter experiments, M. Pasteur had made use of milk. Now the quantity of organic matter contained in milk is, of course, very great; it is a highly nutritive and complex fluid. It might, therefore, and ought, perhaps, to have suggested itself to M. Pasteur that the different results of his later experiments were possibly explicable on the supposition that the restrictive conditions—the boiling of the solution and the closed vessel already containing air—were too potent to be overcome by the organic matter in the one solution, whilst they were not too potent and could not prevent evolutional changes taking place in that of the other. For if, in accordance with the belief of the evolutionists, different organic fluids have different initial tendencies to undergo the changes of evolution, it may be easily understood that as the conditions favourable to evolution are more and more restricted, certain of these fluids may altogether cease to undergo such changes, others may manifest them to a meagre extent, and others still, only a little more fully. Therefore, if under the conditions peculiar to Schwann's experiments, certain fluids with low evolutional tendencies have given rise to no organisms, there is nothing whatever contradictory in the fact if it is subsequently ascertained that other fluids, with greater inherent capacities of undergoing change, will, notwithstanding all the restrictive conditions, pass through certain Life-producing changes. When subjected to a pressure of one atmosphere, water boils at 212° F., alcohol at 173° F., and ether at 96° F. The restrictive condition, or atmospheric pressure, is here in each case the same, only, having to do with differently constituted fluids, it is natural enough to look for different results under the influence of like incident forces. Ether raised to a temperature of 100° F. would rapidly disappear in the form of vapour, though no such result would follow the heating of water to a similar extent. And similarly, whilst milk might be capable of yielding organisms in Schwann's apparatus, another fluid less rich in organic matter might fail to do so. It seems almost incredible that such considerations should not have suggested themselves to M. Pasteur; but yet we have no evidence that they did occur to him.† On

* A mode of origin of spores which is, I believe, quite familiar to fungologists.

† This form of fungus-spore seems to be most prone to occur where different ammoniacal salts are employed. It has been met with not only in the tartrate of ammonia solutions, but also in those containing oxalate of ammonia and carbonate of ammonia respectively. And it has been found in no other of my experimental fluids.

* In order to avoid circumlocution in this note, I speak from the evolutionist's point of view. And whether the organisms found in a given fluid have been actually produced therein, or have only there undergone development, we may, for the sake of argument, measure the evolutional capacity of a fluid by the amount and kinds of organisms which are produced in a given quantity of it, in a definite time, and at a given temperature. We must not, however, judge of the evolutional qualities of a fluid by its mere tendency to emit a bad odour in a short space of time. A certain fluid—urine for instance—judged by these qualities, may be disagreeably putrescible, though its evolutional tendencies may be quite low. By many experimenters this difference has not been appreciated, and they seem to imagine that in employing urine they make use of a fluid which is very favourable for such experiments. But they forget that urine is an effete product containing comparatively stable compounds, which have already done their work in the body. It may after a short time swarm with bacteria, and these may be followed by fungi; but there is no comparison between the actual quantities even of these organisms, which will be developed in equal amounts of milk and urine respectively, when they are both exposed to the air for the same time in similarly-shaped vessels, and under the same bell-jar. The milk soon becomes actually solid with fungus growths. M. Pasteur's "l'eau de levûre sucrée," by his own confession (*loc. cit.* note, p. 58) is never found to contain any of the higher ciliated infusoria, and in all probability, though it produces fungi, these are met with in much smaller quantity than they would have been in an equal bulk of milk under the same conditions.

† The experiments and reasonings to which I am now alluding are detailed in pp. 58—66 of M. Pasteur's Memoir (*Ann. de Chim. et de Phys.* 1862).

the contrary, he explains the discrepancy between his earlier and his later experiments by another supposition altogether. As on other occasions, he does not even suggest to the reader that any different explanation is possible from that which he adduces. He deliberately assumes that the bacteria and vibrios which were subsequently found in the milk used in these experiments had been derived from "germs" of such organisms which either pre-existed in or had obtained access to this fluid before it had been heated, and also (contrary to the general rule which had been previously admitted) he assumed that such supposed pre-existing germs were capable of resisting the influence of the boiling temperature in milk. No direct proof of the latter assumption was ever attempted, though M. Pasteur did afterwards endeavour to bring these exceptional cases under a general law by supposing that the results obtained were due to the absence of acidity in the fluids employed. Neutral or slightly alkaline fluids might, he thought, yield positive results in Schwann's experiments, because the germs of bacteria and vibrios were not destroyed, by the boiling temperature in such fluids.

Such was the very definite statement made by M. Pasteur on the faith of a chain of evidence almost every link of which is ambiguous. The most direct observations, however, which can be made upon this subject (and to the desirability of making which he does not even allude) lend not the least support to his assumption. On the contrary, they go to confirm the rule which had hitherto been generally admitted as to the inability of any of these lower organisms to live after an exposure for even a few seconds in a fluid raised to a temperature of 100°C. I have again and again boiled neutral and alkaline infusions containing very active bacteria and vibrios, and the result has always been a more or less complete disruption of the vibrios, and the disappearance of all signs of life in the bacteria. All their peculiarly vital movements have at once ceased, and they have henceforth displayed nothing but mere Brownian movements.*

M. Pasteur approaches the solution of the discrepancy in this way. His attention was arrested by the fact that milk was an alkaline fluid, because he afterwards ascertained that other alkaline fluids also yielded positive results when submitted to the conditions involved in Schwann's experiments. Thus he himself helped to overturn the strongest evidence which had hitherto been brought to bear against the heterogenists. But, this being done, it was necessary for M. Pasteur to explain such an occurrence, if he was not prepared to yield his assent to the doctrine which he had formerly rejected. He now found, truly enough, that the mere alkalinity or acidity of the solution was a matter of great importance in these experiments; he found, for instance, that his "l'eau de levûre sucrée," naturally a faintly acid fluid, was always unproductive when submitted to Schwann's conditions unaltered, though it was, on the contrary, always productive if it had previously been rendered neutral or slightly alkaline by the addition of a little carbonate of lime. Facts of this kind were observed so frequently as to make him come to the conclusion that whilst acid solutions were never productive in Schwann's apparatus, any neutral or alkaline fluids might be, if it were otherwise suitable for such experiments. Then came the question as to how this was to be explained. It should be remembered that M. Pasteur was engaged in investigating the problem of the mode of origin of certain low organisms in organic fluids, concerning which so much controversy had taken place. In this controversy, hitherto, on the one hand, it had been contended that the Living things met with derived their origin from pre-existing "germs" that had survived all the destructive conditions to which the media supposed to contain them had been subjected; whilst, on the other hand, it was contended that if the media had been subjected to conditions which (by evidence the most direct and positive) had been shown to be destructive to the lowest Living things, then such Living things as were subsequently discovered in these fluids must have been evolved *de novo*. It was a question, therefore, on the one hand, as to the degree of vitality or capability of resisting adverse conditions peculiar to the lowest Living things; and, on the other, as to the strength of the tendency to undergo changes of an evolutional character in the organic matter existing in the solutions, and on the degree to which this molecular mobility could persist, in spite of the disruptive agency of the heat to which the organic matter might be subjected. When, therefore, after having been exposed to a given set of conditions, organisms are not subsequently found in the fluids employed, this is explicable in one of two ways—that is, in accordance with either of the two opposing views. Either the heat has proved destructive to all Living things in the solutions; or else the restrictive conditions to which the organic matter in the solutions has been exposed have been too severe to permit the occurrence of evolutional changes therein. Any person seriously wishing to ascertain the truth, and competent to argue, of course would not fail to see that he was bound to give equal attention to each of these possibilities. He had no right to *assume* that the probabilities were greater in favour of the one mode of explanation than they were in favour of the other; this was the very subject in dispute—this, it was, which had to be proved. When, therefore, it was definitely ascertained by M. Pasteur that acid solutions employed in Schwann's experiments yielded negative results as far as organisms were concerned, the establishment of this fact was in reality no more favourable to the one view than to the other. It is what the Panspermatists might have expected, it is true, because—regarding it only as a question of the destruction or non-destruction of germs—even they had convinced themselves that calcining the air and boiling the fluids were adequate to destroy all Living things contained in these media; but, on the other hand, it was equally open to the Evolutionists to say that—the restrictive conditions employed being so severe—they also were not surprised at the probable stoppage of evolutional changes and at the consequent non-appearance of organisms in the solutions. When positive results were obtained, however, the case became altogether different. The rule being absolute, so far as it had gone—and founded on good evidence, to which M. Pasteur and others had assented—with regard to the inability of Living things to survive in solutions after these had been raised to the boiling temperature for a few minutes; no one should have attempted to set aside this rule, except upon evidence equally direct and equally positive, though more extensive, than that upon which the rule had been originally founded. Certainly, no one should have attempted to set it aside on the strength of *indirect evidence, which, though equally capable of explanation in accordance with either one of the two opposing views, was tacitly represented to be explicable only in accordance with one of them.* Such, however, has been the conduct of M. Pasteur. It will, perhaps, scarcely be credited by many that the investigations of M. Pasteur, which have had so much influence, and which have been looked upon by many as models of scientific method, should really contain such fallacies. On other important occasions, however, his reasoning has been similarly defective, though he himself claimed and was believed by many to have "mathematically demonstrated" what he had so plausibly appeared to prove.*

In the present case, after his experiments with milk in Schwann's apparatus, M. Pasteur ascertained that in other alkaline or in neutral fluids, even when they had been subjected to all the conditions above mentioned, inferior organisms might be found more or less quickly. But he also discovered that even such solutions no longer yielded organisms if, instead of subjecting them to a heat of 100° C. they had been exposed for a few minutes to a temperature of 110° C. And it was on the strength of two or three other links of such evidence as this that M. Pasteur sought to upset the rule with regard to the inability of inferior organisms to resist the destructive influence of a moist temperature of 100° C. On such evidence as this he attempted to raise the possible limit of vital resistance by 10° C., and sought to establish the rule that Living organisms might survive in neutral or alkaline solutions if these had not been raised to a temperature of 110° C. He did not seem to see how utterly inconclusive his conclusions were, and that he had not so much right to assume that the organisms met with in his neutral or alkaline fluids had been derived from "germs" which had resisted the boiling temperature, as he or his opponents would have had at once to fall back upon the counter assumption that the evolutional tendencies of neutral or alkaline fluids exposed to high temperatures were greater than those of similar fluids when in an acid state—and that such neutral or alkaline fluids were, as was now seen, capable of overcoming the restrictive conditions in Schwann's experiments and of giving birth to organisms, by permitting the occurrence of Life-evolving changes amongst the colloidal molecules contained therein. He had less right to explain the facts as he did, than the evolutionist would have had to explain them as above mentioned, because he was thus attempting to upset

* See what has been previously said on this subject (p. 171).

* The space at my disposal does not permit of my alluding to these other occasions at present, though I shall do so in my forthcoming work.

previously admitted facts on insufficient evidence, whilst the reasonings of the evolutionist would have been in every way legitimate. And yet M. Pasteur left his readers to imagine that the explanation which he had adduced was that which was alone admissible; he did not refer to the existence of any other mode of explanation, but at once attempted to set aside the old rule. And similarly, when he ascertained that such alkaline or neutral fluids were no longer found to contain organisms if they had been previously submitted to a temperature of 110° C. he was entitled to draw no conclusion from such facts. Nevertheless, M. Pasteur did assume that such indirect evidence entitled him to come to the conclusion that the hypothetical "germs" contained in these solutions—those which were not killed, as he supposed by a temperature of 100° C, were destroyed by a temperature of 110° C. Such two-faced evidence is, however, worthless for raising the standard of vital resistance; and to ignore the possible differences which may exist, from the evolutionist's point of view, between acid and alkaline solutions, as M. Pasteur did, is about as reasonable as if he had imagined that because water does not boil at the temperature of 100° F. the same rule must necessarily hold good for ether.

Much evidence, indeed, can be brought forward to show that even at ordinary temperatures, and under conditions in which there is a moderately free exposure to the air (and, therefore, with every facility for the entrance of "germs"), a neutral or slightly alkaline solution is not only found to contain organisms more quickly, but these are found to exist therein in much greater variety than in solutions in other respects similar, save for the fact of their being slightly acid rather than alkaline or neutral. Any of the higher forms of ciliated Infusoria *may* appear in different neutral or slightly alkaline solutions, though they never present themselves in those having an acid reaction, and *neither are their undeveloped ova or their dead bodies to be found therein.* The amount of difference capable of being produced by the mere acidity of a solution was well seen by me a few months ago. Having prepared * a mixture of white sugar and tartrate of ammonia, with small quantities of phosphate of ammonia and phosphate of soda in distilled water, whose reaction was found to be neutral, two similar wide-mouthed bottles of about three ounces capacity were filled with the fluid. Both were kept side by side in a tolerably warm place, the mouths of the bottles being merely covered in each case by a piece of glass, after glycerine had been smeared over the rim on which the cover rested. Although not hermetically sealed, these solutions were thus sufficiently protected to prevent the access of much dust from the neighbouring fire. The fluid in the one bottle was allowed to remain neutral, whilst to that of the other four or five drops of acetic acid were added, so as to make it yield a faintly acid reaction to test paper. The results were quite different in the two cases. Towards the end of the fourth day the originally unaltered neutral solution began to assume a cloudy appearance; this increased in amount during the next day, and at the close of the sixth day a thin pellicle was found on the surface, and beneath it there were some irregular, flocculent, whitish masses buoyed up by small air bubbles. Examined microscopically, the pellicles and also the flocculent masses beneath were found to be made up of medium-sized monads and bacteria, mixed with crystals of triple phosphate. There were also many scattered cells of a *Torula*, varying from $\frac{1}{5000}''$ to $\frac{1}{10000}''$ in diameter. By this time (close of the sixth day), however, the companion solution which had been slightly acidified, had undergone scarcely any appreciable change. It was still quite clear and transparent, and there was no pellicle on the surface, though there was a very slight whitish flocculent stratum at the bottom of the bottle. Even on the twenty-first day this solution continued in much the same condition—still showing no trace of a pellicle. The fluid itself was clear, and there had been only a very slight increase in the thickness of the white flocculent layer at the bottom of the bottle, which, on microscopical examination, was found to be made up mainly of a granular matter having no definite character—though mixed with this there were a small number of minute but well-formed bacteria. This acid solution had remained throughout in the same warm place, but the bottle containing the neutral fluid had not (after the examination on the sixth day) been replaced in its original place near the fire; it had continued since this time in a part of the room altogether away from the fire, and yet when this also was examined on the twenty-first day, it was found to present a very cloudy, whitish appearance throughout, there was a thick flocculent stratum at the bottom, and also a very consistent, well-marked pellicle on the surface of the fluid, made up almost entirely of large and well-formed *Torula* cells.

Although the results here detailed, as occurring in the neutral and the acidified solutions respectively, are so strikingly different, still they are by no means singular or peculiar to the particular kind of solution which was employed in this experiment. Phenomena essentially similar in kind may be observed when almost any neutral or slightly alkaline organic infusion is employed. Thus, to quote one only out of many experiments bearing upon this point. A short time ago, having prepared a pretty strong infusion of mutton, about an ounce and a half was put, after filtration, into each of two similar flasks. The one portion of the infusion was allowed to remain neutral, whilst to the other were added three drops of strong acetic acid, so as to make the whole yield a faintly acid reaction to test paper. The two flasks were then exposed side by side to a temperature of 75° to 80° F. during the day. In twenty-four hours time the neutral solution was clouded and more or less opaque, whilst the portion which was acid appeared perfectly unchanged. It was as clear as ever; and so it continued even to the end of forty-eight hours, although by this time the neutral solution was quite opaque, muddy-looking, with a pellicle on its surface, and also some flocculent deposit at the bottom of the flask. A microscopical examination of two or three drops of this fluid showed that it was teeming with most actively moving monads, bacteria, and vibrios, whilst a similar examination of the acid fluid showed not a trace of these or of any other kinds of organisms.*

The difference between the results in these two sets of cases was thus extremely well marked, and the results themselves are well worth our serious attention. We had to do with equal bulks of fluid, placed under similar conditions and similarly constituted, with the exception that in each set a few drops of acid had been added to the one fluid, whilst the other was allowed to remain neutral. And it must be confessed that the difference encountered was very similar in kind to that which was observed by M. Pasteur when he made use of acid, or of neutral or alkaline solutions respectively, in repeating the experiments of Schwann. Only here we have had nothing to do with the destructive agency of heat, and germs were as free to enter into the one solution as they were into the other, so that the differences actually observed would seem now, at all events, due simply to the different qualities of the fluids themselves. Of course, such results cannot be adduced as evidence that the evolutional property of the neutral solution was higher than that of the acid solution. It may be not a case of evolution at all, but simply one of growth and development. The results, however, do show plainly enough that the neutral solution was the one most favourable to the growth and development of Living things. And if, starting from this fact, which cannot be denied, the evolutionists see reasons which induce them to assume the possibility that, in addition to mere growth and development, an actual origination of Living things may have taken place *de novo*, they would also be likely to suppose that the neutral fluid was more favourable to such evolution than that which had been acidified.† That solution which was found favourable for the processes of growth and development would also, in all probability, be favourable for evolution. A process would be most likely to be *initiated* where the conditions were suitable for its continuance. And surely the same factors would be at work in the initiation of a Living thing as would be called into play during its contiuance as a growing Living thing. The presumption, therefore, is a fair one, that solutions which are favourable to the growth and development of certain organisms would also be favourable to the evolutional changes which more especially lead to the initiation of such Living things. Seeing, then, that the question of the occurrence or non-occurrence of such initiations is the very matter in dispute, it is certainly most imperative that no one engaged in investigations bearing on the subject should fail to appreciate this that these are possibilities whose probability ought to be assumed as equal. We may well be amazed, then, at the utter one-sidedness of M.

* Dec. 23, 1869. The weather being very cold and frosty. The mixture employed was another portion of the same solution as was used in *Experiment* 9.

* The reverse results, which may be produced by neutralising the acidity of a naturally acid fluid, will be exemplified further on.

† Taking it only for what it is worth, it is, at least, deserving of mention that no reason seems assignable for the presence of *Torulæ* in the one saline solution and not in the other. They were both equally exposed to the advent of "germs." It can scarcely be imagined that the *Torula* germs did obtain access to the both solutions, but that they perished in that which was faintly acid, for, as a matter of fact, *Torulæ* are much more frequently met with in acid solutions than in those which are alkaline.

Pasteur, when we find him completely ignoring one of these points of view, interpreting all his experiments by the light of a foregone conclusion, and looking upon the different solutions employed solely as fluids which are destructive or not destructive to hypothetical "germs" at a given temperature.

It should not be understood that I regard all acid solutions as having a low evolutional tendency. On the contrary, I believe I have helped to show in this paper that *some* acid solutions are most prone to undergo evolutional changes of a certain kind. These do not result in the production of Living things of a high type, but rather in an abundance of organisms of a comparatively low type. It seems to me, however, after careful observation and experiment, that a neutral or slightly alkaline solution to which a few drops of acid have been added is always found, after a given time, to contain a notably smaller number of organisms than an equal bulk of the unaltered solution. And conversely, having an acid solution whose productiveness is known, the number of organisms found in equal bulks under similar conditions, can almost always be notably increased in either one of them by the mere addition of a few drops of *liquor potassæ*, so as to render it neutral or slightly alkaline. This, as I previously pointed out, *may* be interpreted as an indication that alkalinity, or neutrality of the fluids, is more favourable than their acidity for the occurrence of evolutional changes. And thus the fact that organisms were never met with when an acid "eau de levûre sucrée" was used in repeating the experiments of Schwann, though they were met with, on the contrary, in other experiments where portions of this same fluid had been used which had been rendered slightly alkaline by the addition of chalk, might be explained without the aid of that supposition which alone seems to have occurred to M. Pasteur.

But, after reflection on this subject, it seemed to me quite within the range of probability, that the difference between acid and alkaline solutions in respect of the number of organisms which are to be found therein, when these have been simply exposed to ordinary atmospheric conditions, might be exaggerated after they had been exposed to the temperature at which water boils. It seemed quite possible that high temperatures might be more destructive to organic matter when this was contained in acid solutions than when it existed in alkaline solutions. Just as the acid seems to exercise a certain noxious influence even at ordinary temperatures, it may be conceived that this influence, whatever its nature, may be increased in intensity with the rise of temperature, and with the consequent greater facility for the display of chemical affinities. Hot acids will frequently dissolve metals which would remain unaffected by them at ordinary temperatures; and chemical affinities generally are notably exalted by an increased amount of heat. Just as the addition of an acid, therefore, to a previously neutral or slightly alkaline fluid containing organic matter in solution, appears to alter its character in some mysterious way, so may we assume that its action upon the unstable organic molecules goes on increasing in intensity as the fluid becomes hotter. So that, when two portions of a solution containing organic matter —the one neutral and the other acid—have been raised to a temperature of 100° C., whilst the organic matter of the one has been injured only by the mere action of heat; that of the other solution, which has been acidified, has not only had to submit to the deleterious influence of the high temperature, but also to the increased activity of the acid at this temperature. Thus the result would be that the amount of difference between the two solutions which existed before they had been heated, would be found more or less increased after they had been exposed to the high temperature, in direct proportion to the increase in intensity of the action of the acid produced by such high temperature. What we know concerning the precipitation of albumen in urine is quite in harmony with this view. When albumen is present, and the fluid has an alkaline reaction, mere boiling does not cause its precipitation, though, if the reaction had been acid,* the albumen present would have been precipitated, when, or even before, the fluid was raised to the boiling temperature. Or, the same result might have been brought about by the addition of a small quantity of acid to a portion of a neutral or alkaline albuminous specimen which had just been boiled without having brought about a precipitation of the albumen. Thus, the addition or presence of a small quantity of acid, in conjunction with an elevated temperature, is seen to be capable of producing results which cannot be produced by the mere elevated temperature alone. But the fact that an isomeric transformation of albumen can be brought about in this way—that albumen can be transformed so as to be no longer capable of remaining in solution—shows that a molecular change has been brought about by the influence of the acid working at high temperatures, which neither the acid nor the heat, working alone, are capable of effecting.

* Provided this was not due to the presence of a mere trace of nitric acid.

With the view of throwing further light on this subject, on March 27 of the present year I made the following experiments:—A tolerably strong infusion of white turnip was prepared and subsequently filtered.* This had a decidedly *acid* reaction. It was then divided into two portions, one of which was allowed to remain unaltered, whilst to the other a few drops of *liquor potassæ* were added, so as to give the fluid a very faintly alkaline reaction. This addition produced a slight alteration also in the naked eye appearance of the fluid; the faintly whitish opalesence which formerly existed disappeared, and was replaced by an equally faint brownish tinge. About an ounce of each of the two fluids was then placed separately in two small flasks. The fluids were not heated at all, but a piece of paper having been placed loosely in the neck of each so as to exclude dirt, they were exposed side by side to a temperature varying from 75° to 85° F. After twenty-four hours,† the unaltered acid infusion merely showed a more decided opalesence approaching to cloudiness; though that which had been rendered faintly alkaline, had a distinctly opaque whitish colour, and there was also a distinct pellicle covering more than one half of the surface of the fluid. In the three or four succeeding days the amount of opacity, of pellicle, and of deposit increased in both the fluids, though each of these continued to be more manifest in the alkaline than in the acid solution. After a week, however, the difference was scarcely appreciable, though on the whole, for about two weeks afterwards, the quantity of new matter seemed to be greater in the alkaline than in the acid solution.

But, on the same morning that these two portions of the acid and alkaline infusions had been set aside for observation, I had placed with them vessels containing two other specimens of the same fluids. These had been previously treated in the following manner: The acid fluid and the alkaline fluid, after they had been placed in their respective flasks, and the necks of these had been drawn out, were then boiled for ten minutes, and at the expiration of this time—whilst ebullition was still continuing—the drawn-out necks of the flasks were hermetically sealed in the blow-pipe flame. These vessels, therefore, were intended to show, by comparison with the other two, whether the difference produced by mere acidity or alkalinity of the solutions at low temperatures was or was not intensified by the action of heat. The flasks were all suspended in a group at the same time, and were, thenceforward, subjected to the same temperature. The results were as follows: After twenty-four hours the slightly alkaline fluid which had been boiled showed a slight though decided opalescence; it was, in fact, very similar in appearance to the acid solution which had not been boiled. The boiled acid solution was, however, as clear as when the flask was first suspended, and so it remained, apparently quite unaltered, after it had been suspended a week, though the boiled alkaline solution had by this time become decidedly opaque, and also showed some flocculent matter lying at the bottom of the vessel. And now‡, after they have been suspended more than three weeks, the acid solution still remains almost transparent, presenting only the faintest cloudiness, though with no pellicle or deposit at the bottom.§ The boiled alkaline fluid, however, presents a totally different appearance; it is whitish and quite opaque, there is a very thick pellicle covering part of its surface, and also some whitish sediment at the botton of the flask.

The difference which already exists between alkaline and acid solutions at ordinary temperatures is, then, seen to be most notably intensified after similar alkaline and acid solutions have been raised to a temperature of 100° C. And whilst these differences tend to substantiate the reality of the other mode of explanation (which I have suggested) of the discrepancies observed by M. Pasteur when he repeated Schwann's experiments with acid and with alkaline organic infusions respectively, they may also

* The turnip at this season of the year was however very poor and dry as compared with that which was employed in some of my earlier experiments (*Experiments* 4 to 9) during the winter months.

† During the whole of this time the heat only varied between the limits mentioned.

‡ April 19, 1870.

§ This solution was, therefore, much more backward in exhibiting signs of change than were the others which had been used in *Experiments* 4 to 8—a difference probably explicable by the poorer quality of the turnip used in this last experiment.

be considered to strengthen the probabilities in favour of my assumption that an acid fluid is less prone to undergo those molecular changes which lead to the evolution of Living things, than an otherwise similar fluid whose reaction is neutral or faintly alkaline. And yet this explanation was utterly ignored by M. Pasteur; he wrongly assumed that the before-mentioned discrepancies were explicable only in one way; and he moreover illogically attempted to set aside a rule to which he had previously assented, on the strength of evidence which was most ambiguous, and, therefore, inconclusive—in nature. M. Pasteur engages himself in a controversy concerning one of the most important questions in the whole range of biological science, and yet he assumes the attitude of a man who is so convinced beforehand of the error of those who are of the opposite opinion, that he will not abide by ordinary rules of fairness, he will not even, at first, assume the possibility of the truth of the opinions which are opposed to his own. Ambiguous evidence is explained as though it were not ambiguous; conclusions based upon good evidence are attempted to be set aside in favour of conclusions based upon evidence which is comparatively worthless; and, by such illogical methods, M. Pasteur proclaims that he has "mathematically demonstrated" the truth of his own views. Unfortunately for the cause of Truth, people have been so blinded by his skill and precision as a mere experimenter, that only too many have failed to discover his shortcomings as a reasoner.

But it will already have been perceived by the attentive reader, that it was not necessary for me—in my endeavour to establish as a Truth the great doctrine which M. Pasteur has striven to repudiate—to show the inconclusiveness of his reasonings on that branch of the subject to which I have just been alluding. I have striven rather to show in their true light the real nature of such modes of reasoning, which are I fear only too likely to be repeated by others. So long as people are unable readily to appreciate the worthlessness of arguments like these, they will never be likely to penetrate through the clouds of controversy which envelope this subject. Their mental vision will be blinded, and the truth will remain hidden from them. But, lured on by the success of reasonings such as these, others would have grown bolder still, and precisely as the exigencies of the case required, so would the standard of vital resistance to heat have been raised. What object can there be in laboriously ascertaining by direct experiment and observation at what temperature the lower kinds of organisms cease to live, if the information so obtained is to be studiously ignored just when it ought to be used as a kind of touchstone, or as a lamp to illumine phenomena whose explanation would otherwise be doubtful? It is a very easy process, certainly, first to start with the assumption that it is "impossible" for Living things to be evolved *de novo*, and then, every time that Living things are found under conditions where they ought not to occur (if the assumption were true, and if the generally received notions concerning vital resistance were correct), to assume that the very fact of their having been found under these conditions, of and by itself, shows that the previous notions concerning vital resistance were entirely wrong, and that the organisms which were formerly admitted to have been destroyed by a temperature of 100° C., must now be considered to be able to brave for four hours a temperature of 150° C., simply because they have been found in fluids which had been submitted to this temperature. The reasoning by which Truth is sought to be ascertained is, in fact, this:—No matter what the temperature to which the solutions and the hermetically sealed flasks have been exposed—be it even 500° C.—if Living organisms are subsequently found in the solutions, then they or their "germs" *must* have been able to resist the destructive influence of such a temperature, simply because Living things have been found, and because it is *assumed* that they cannot be evolved *de novo*. It is to be hoped that this is not the kind of reasoning which will find favour with those who are seeking for the advancement of Biological Science!

My principal objects in this paper have been to show:—

1. That there is a strong *a priori* probability in favour of the possibility of the occurrence of the heterogenous evolution of Living things, and that the most reliable scientific data which we possess do, in fact, fully entitle us to believe in this as a possibility.

2. That microscopical investigation, whilst it teaches us as much concerning the mode of origination of the lowest Organisms as it does concerning the mode of origin of Crystals, enables us to watch all the steps of various processes of heterogenous Evolution of slightly higher Organisms, such as may be seen taking place in a pellicle on a fluid containing organic matter in solution.

3. That the kinds of organisms which have been shown to be destroyed by a temperature of 100° C. may be obtained in organic fluids, either acid or alkaline, which, whilst enclosed within hermetically sealed and airless flasks, had been submitted not only to such a temperature but even to one varying between 146° and 153° C. for four hours.

4. That a new and direct evolution of organisable compounds may, in all probability,* be capable of arising, sometimes by isomeric transformation of the atomic constituents of a single saline substance such as tartrate of ammonia, and sometimes by a re-arrangement of certain of the atomic constituents belonging to two or more saline substances existing together in solution. It is not only supposed that this may occur, but that even Living things may subsequently be evolved therefrom, when the solutions have been exposed, as before, in airless and hermetically sealed flasks to a temperature of 146° to 153° C. for four hours.

On account of this *a priori* probability, and in the face of this evidence, I am, therefore, content, and as I think justified, in believing that Living things may and do arise *de novo*. Such a belief necessarily carries with it a rejection of M. Pasteur's Theory of Putrefaction, and of the so-called "Germ Theory of Disease."

H. CHARLTON BASTIAN

* It is not pretended that this is proved. The aid of the chemist and physicist must be much more extensively resorted to before such a point could be proved. I hope soon, however, to be able to bring forward additional evidence bearing upon this part of the subject.

BOOKS RECEIVED

ENGLISH.—Travels of a Naturalist in Japan and Manchoria: A. Adams (Hurst and Blackett).—Hydrostatics and Sound; R. Wormell (Groombridge).

FOREIGN.—Théorie mécanique de la chaleur: E. Verdet (Paris: Masson et fils).—(Through Williams and Norgate)—Vierteljahresschrift der Astronomischen Gesellschaft, Nos. 1 and 2: Anwers and Winnecke.—Studien über das centrale Nervensystem der Wirbelthiere: Dr. L. Stieda.—Lehrbuch der Botanik: Dr. J. Sachs.—Resultate aus Beobachtungen auf der Leipziger Sternwarte, pt. 1: Dr. R. Engelmann.

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THURSDAY, JULY 21, 1870

WAR

THE dogs of war are again let loose, and in the two most highly civilised countries of Europe, where, a week ago, science, education, and commerce were in full sway, all the arts of peace are already neglected, and in prospect have gone back a quarter of a century. We can hardly yet realise that at the present moment railways are being torn up, lighthouses dismantled, lightships towed into harbour, and monuments of engineering skill, such as the bridge over the Rhine at Kiel, undermined, so that they may be destroyed at a moment's notice. But these, after all, are calamities of the second order; education is stopped; science schools are broken up; while we write both professor and pupil are forsaking the laboratory and the class-room, and the whole machinery of progress has come to a stand-still.

This journal, of course, has nothing to do with Politics: the function of Science is to unite the whole human family, whereas the function of Politics seems to be, both in the case of the human family and of each nation, to create parties and to emphasise them as much as possible, the object in each case being place for the partisans—whether that place be an income of a few thousands a year in one case or increased territory in the other. But although we cannot discuss politics, we may point out that as Science advances such policies will be overridden—that when Science and Education have taken their proper position—when the sword has given place to brain—when more of the best men of each nation take part in each nation's counsels, the dreadful thirst after blood will give way to something better; monarchs will see the folly of being surrounded merely with empty helmets and uniforms, or at all events if they do not, others will; and much will have been done when the pampering of armed men shall cease.

There is one point however in connection with the coming war which we cannot point out too strongly—one duty which England owes to herself, and which, if it be well done, may make her after all a gainer from the dreadful strife. We have already stated, and the statement is not an exaggeration, that the war will throw the countries engaged in it back a quarter of a century. Now, England at the present moment, be the cause what it may, is in many things a quarter of a century behind France and Prussia, notably in education of all kinds, and especially in scientific education.

The following extract from the Report of Mr. Samuelson's Committee on Scientific Education—a report which we believe has not even yet been taken into consideration by our Legislature—is so much to the point that we give it here:—

"Nearly every witness speaks of the extraordinarily rapid progress of Continental nations in manufactures, and attributes that rapidity, not to the model workshops which are met with in some foreign countries, and are but an indifferent substitute for our own great factories, and for those which are rising up in every part of the Continent; but, besides other causes, to the scientific training of the proprietors and managers in France, Switzerland, Belgium, and Germany, and to the elementary instruction which is universal amongst the working population of Germany and Switzerland. There can be no doubt, from the evidence of Mr. Mundella, of Prof. Fleeming Jenkin, of Mr. Kitson, and others, and from the numerous reports of competent observers, that the facilities for acquiring a knowledge of theoretical and applied science are incomparably greater on the Continent than in this country, and that such knowledge is based on an advanced state of secondary education.

"All the witnesses concur in desiring similar advantages of education for this country, and are satisfied that nothing more is required, and that nothing less will suffice, in order that we may retain the position which we now hold in the van of all industrial nations. All are of opinion that it is of incalculable importance economically that our manufacturers and managers should be thoroughly instructed in the principles of their arts.

"They are convinced that a knowledge of the principles of science on the part of those who occupy the higher industrial ranks, and the possession of elementary instruction by those who hold subordinate positions, would tend to promote industrial progress by stimulating improvement, preventing costly and unphilosophical attempts at impossible inventions, diminishing waste, and obviating in a great measure ignorant opposition to salutary changes.

"Whilst all the witnesses concurred in believing that the economical necessity for general and scientific education is not yet fully realised by the country, some of them consider it essential that the Government should interfere much more actively than it has done hitherto, to promote the establishment of scientific schools and colleges in our great industrial centres."

It is impossible that we can say anything stronger than this in favour of taking the fullest advantage of the opportunity of regaining our intellectual and therefore our commercial prestige.

If England is to prepare for war, the abnormal condition, so let it be; but surely, *a fortiori* she should *prepare for peace*, the normal one, as well. This has never struck her ministers, and the reason is not far to seek.

But this is not all; the same disregard for science, arising from the ignorance of science among our rulers, has probably placed us in another position of disadvantage. While France and Prussia have been organising elaborate systems of scientific training for their armies, a recent Commission has destroyed what little chance there was of our officers being scientifically educated at all. As there is little doubt that a scientific training for the young officer means large capabilities for combination and administration when that officer comes to command, we must not be surprised if the organisation of our army, if it is to do its work with the minimum of science, will, at some future time, again break down as effectually as it did in the Crimea, or that our troops will find themselves over-matched should the time ever come when they will be matched with a foe who knows how to profit to the utmost from scientific aids.

While, therefore, the Continent is being deluged with blood, let us *prepare for peace* as well as for war; let us prepare ourselves for victories in the arts, conquests over nature; let us, by means of a greater educational effort, more Science Schools, a truer idea of the mode in which a nation can really progress, fit ourselves to take our place among the nations when peace returns. Surely if there be statesmen among us, such a clear line of policy will not be overlooked.

Education and Science at the present moment are England's greatest needs.

HEIGHT AND WEIGHT

WITHIN the last few years public attention has been drawn to the question of what individuals weigh, by the facilities afforded for weighing by the construction of weighing-chairs. These chairs are not only to be seen at the Crystal Palace, where diminutive boys tout for custom, offering to tell your "correct weight," but they are also seen at the stations of the Metropolitan Railway and many other places. The practice, therefore, of getting weighed is obviously on the increase, and we want to utilise the knowledge thus gained by showing how it may be turned to most advantage. It will be easily seen that to know the weight of a person without reference to some other standard, such as height, would be of little advantage. But if by taking the height of a person we can say what he ought to weigh, then we have a means of ascertaining what persons ought or ought not to weigh. The difficulty on this subject has been to determine what a man of a certain height really ought to weigh. If this can be determined, then we can say whether a man of a certain height exceeds or falls short of the average weight of men of his stature.

One of the earliest efforts made to obtain anything like a fixed relation between height and weight was that of Dr. Boyd, who weighed a certain number of inmates in St. Marylebone Workhouse. He took the height and weight of 108 persons labouring under consumption, and found they measured 5ft. 7in., and weighed ninety pounds. He then measured and weighed 141 paupers who were not consumptive, and found that their average height was 5ft. 3in., and that they weighed 134lb. This subject attracted the attention of the late Dr. John Hutchinson, and he determined to take the height and weight of all classes of persons in the community. In this way he collected the height and weight of upwards of 5,000 persons. This list, however, included persons who exhibited themselves as giants and dwarfs, and other exceptional cases. He therefore reduced his instances to 2,650 persons, all of whom were men in the vigour and prime of life, and included sailors, firemen, policemen, soldiers, cricketers, draymen, gentlemen, paupers, and pugilists. This group of cases was intended to make one class as a set-off against another, so as to get a fair average. The following is the result of Dr. Hutchinson's observations :—

Height.	Weight.	Height.	Weight.
ft. in.	st. lb.	ft. in.	st. lb.
5 1	8 8	5 7	10 8
5 2	9 0	5 8	11 1
5 3	9 7	5 9	11 8
5 4	9 13	5 10	12 1
5 5	10 2	5 11	12 6
5 6	10 5	6 0	12 10

Of course the result of these investigations of Dr. Hutchinson can only be considered as approximative, and he himself thought that a larger number of observations would lead to a more perfect law. The fact is, his observations are quite sufficient to establish all that we need, and to show that amongst a certain set of healthy men his estimate of weight and height may be regarded as an approach to a healthy standard. It is only where considerable departures from the estimates given by Dr. Hutchinson take place that any particular case demands attention. If this table is examined, it will be seen that the increase in weight for every inch of height is a little more than five pounds. In fact, allowing for any error in observation, we may say that Dr. Hutchinson's table is reducible to the law that for every inch of stature beyond 5ft. 1in., or sixty-one inches, a healthy man increases five pounds for every inch in height. If this deduction be accepted, we may very much simplify Dr. Hutchinson's table, and say that as a rule, a man's weight increases at the rate of five pounds for every inch of height, and this rule holds good for all practical purposes. Starting then with a person 5ft. in height, who, according to the assumed law, should weigh 8st. 3lb., we obtain the following results :—

Height in inches.	Height in feet.	Weight in pounds.	Weight in stones.
in.	ft. in.	lb.	st. lb.
60	5 0	115	8 3
61	5 1	120	8 8
62	5 2	125	8 13
63	5 3	130	9 4
64	5 4	135	9 9
65	5 5	140	10 0
66	5 6	145	10 5
67	5 7	150	10 10
68	5 8	155	11 1
69	5 9	160	11 6
70	5 10	165	11 11
71	5 11	170	12 2
72	6 0	175	12 7
73	6 1	180	12 12
74	6 2	185	13 3
75	6 3	190	13 8
76	6 4	195	13 13

Although this law is approximately good for a certain number of cases, even above and below this table; it is practically found, and especially in the case of children and growing persons, that there is a wide difference of weight at heights below 5ft.

Attention may also be drawn here to the fact that there will constantly occur in the community instances of persons where either the muscular or bony systems are excessively developed, and who consequently weigh more or less than their height. Dr. T. K. Chambers, in his admirable essay on corpulence, published in 1859, calls especial attention to the researches of Mr. Brent on the assumed weights of the statues of antiquity. In order to get at this, Mr. Brent immersed in water accurate copies of these statues, and by ascertaining the quantity of water they displaced he calculated their weights. Dr. Chambers has taken the pains to reduce the absolute weights of these statues to assumed heights, and thus compared the heights and weights of these statues of antiquity with Dr. Hutchinson's modern man. Without giving the whole of the heights and weights, we present the series at the assumed height of 6ft. Thus :—

	Height.	Weight.
	in.	st. lb.
Bronze Tumbler .	60	11 4
Hutchinson's Man	60	12 10
Dying Gladiator. .	60	14 0
Theseus, Brit. Mus.	60	15 0
Hercules, ,, ,,	60	16 10
Farnese Hercules.	60	18 7

On this table Dr. Chambers remarks: "Of the statues here selected, the Bronze Tumbler may be taken as the type of extreme lightness and activity, the Dying Gladiator of robust strength. In Theseus and the smaller Hercules the sculptor's idea of a hero where the bodily strength must be equal to that of any possible man. The Farnese Hercules exhibits a development of muscle greater than is ever known to exist in the human species."

Dr. Chambers also gives the height and weight of certain celebrated prizefighters, the result of Mr. Brent's observations, which makes it very obvious that in certain cases the great weight depends on muscular and osseous development.

	Height.		Weight.	
	ft.	in.	st.	lb.
Perrins	6	2	17	0
Caunt	6	2	14	7
Spring	5	11	13	3
Jackson	5	11	14	0
Bendigo	5	9	12	0
Johnson	5	8	13	5
Slack	5	8	13	10
Mendoza	5	7	12	4

The conclusion we come to with regard to these weighings and measurings is that all ordinary departures from the average height and weight of the body deduced from Dr. Hutchinson's tables are due either to an increase or decrease of the fatty matter or of the adipose tissue in the body. Thus, taking the composition of a human body weighing 154lb. and measuring 5ft. 8in., it will be found that it contains 12lb. of fat.* It is then mainly due to the diminution or increase of this substance that human beings weigh more or less than the standard weights given in the above table. It will be therefore here worth while to inquire what is the use of fat in the system, and what indications are afforded by the height and weight of the human body for caution in diet and regimen.

The exact way in which fat is produced in the tissue of plants and animals is not known, but there is evidence to show that it is found very generally in the tissues of plants and especially in the seeds. Oil when used for commercial purposes is mostly obtained from the seeds of plants, as seen in castor oil, rape oil, linseed oil, cocoa-nut oil, palm oil, and a hundred others. As it is found in the seeds of plants, so it is found in the eggs of animals. The embryo of all animals is developed in contact with oil, of which we have a familiar instance in the yelk of the egg of birds. It appears also that the muscular and other tissues grow under the fostering influence of the adipose tissue.

Besides this primary influence on the growth of the body, fat subserves many other purposes. In the first place it seems to be a reserve of material for producing muscular force when needed. Animals grow fat in summer, but as the supply of this material becomes scanty in winter they lose their fat and get thin. Man himself gets fat in summer and grows thin in winter from the demand on this store for heating purposes. Hybernating animals go to their winter sleep sleek and fat, but wake up in the spring lean and meagre, from the loss of fat in maintaining the animal heat necessary for life. Fat is thus seen to be an essential of animal life. Where there is too little deposited for the purposes of life, then serious disease has already commenced or may set in; whilst on the other hand a redundancy of this deposit may seriously interfere with the functions necessary to life.

It is from this point of view that the value practically of a knowledge of the height and weight of individuals becomes apparent. When the weight of a person is much below his height, then it may be suspected that some disease has set in, which may go on to the destruction of life. One of the earliest symptoms of consumption, the most fatal disease of the civilised inhabitants of Europe, is a tendency to loss of weight. Long before any symptoms are present of tuberculous deposits in the lungs, this loss of weight is observable in persons afflicted with consumption. And at this stage a large amount of evidence renders it probable that the fatal advance of this disease may be prevented. Within the last thirty years a practice has been resorted to with great success of administering to persons losing weight and threatened with consumption, cod-liver oil, pancreatic emulsion, and fatty substances, as articles of food, for the purpose of preventing or arresting the tendency to loss of fat, which obviously results in the production of fatal disease. In fact, it may be stated generally, not without exceptions, that wherever the weight is much below the height, there the commencement of dangerous disease may be suspected, and precautions taken to prevent the loss of fat. That this treatment has been successful in really preventing disease, and loss of life as the consequence, is the conviction of a host of intelligent practitioners of medicine. At the same time, it should be remembered that it is not only necessary in these cases to administer cod-liver oil or pancreatic emulsion as medicines, but that the consumptive should have recourse to a fatty diet, and should eat butter, cream, cream-cheese, fat, and fatty articles of diet.

On the other hand, this knowledge of the true relations of height and weight presents us with individuals who weigh a great deal more than the standard presented by the above tables. In certain individuals, and, in fact, in particular families, there is a tendency to develope adipose tissue. However free from fat may be the food, what little it contains is arrested in the tissues of these individuals, and they become "fat;" that is, they weigh more than their height. The consequences of this fatness are very various. The fat may be so deposited all over the system as not to be an obvious obstruction to the functions of life; but every one can understand that, in the case of two men of equal stature, say 5ft. 8in., one having to carry eleven stone and the other twelve, the latter will be at a disadvantage. This arises from two causes. The heavier man carries, in the first place, a greater weight, and in the second place, his heart has to project into the tissues of the body a larger amount of blood in order to keep him alive. For every pound a man weighs above his height, his system is at a disadvantage, and he suffers in various ways. When fat is equally distributed about the body then no immediate disadvantage is felt. But when fat is accumulated in particular parts of the body, interfering with the functions of particular organs, then its evil influences become speedily apparent. The most accurate account of the effects of the accumulation of fat in the viscera of the chest, will be found in a pamph-

* See Guide to the Food Collection, South Kensington. Third Edition.

let by Mr. Banting, who, although not at all what we should call a fat man, nevertheless, so suffered from fat in the chest that he could not walk forwards downstairs, or stoop to buckle his shoe. There is no doubt that in his case there was a necessity for immediate relief, and he obtained it by abstaining from articles of food which supply fat to the system.

When persons weigh much above their height, it is obviously a matter of importance that they should as much as possible relieve the tax put upon their muscular and circulating system by diminishing their weight. Fortunately, this is not a very difficult thing to do, but it should be done with caution. "To Bant" with success requires caution. The immediate withdrawal of all fatty food, and the substances, such as starch and sugar, which produce fat, is frequently attended with dangerous results. Mr. Banting's diet, although so beneficial in his case, was not altogether a judicious one, and we have no doubt that many of our "stout" friends have found an early grave by their determination to reduce themselves to the standard of weight for their height. With regard to stout people, or those who weigh more than their height, it should be recollected that if they have suffered no inconvenience from their weight, it is better to leave well alone. There are few people living in the scientific circles of London who are not well acquainted with the portly forms and genial faces of well-known men from seventy to eighty years of age. It would be folly on the part of the men who have thus achieved the normal age of threescore years and ten to commence any system of artificial diet, when their natural instincts have guided them, in spite of their weight, to their present green old age.

When studied from a judicious point of view there is no doubt that an estimate of the height and weight of an individual ought to enter into every estimate of the possible chances of life. In medical practice it may become the deciding point of the treatment of disease; whilst in those estimates which Assurance offices are obliged to make of the prospective value of life, it is of the utmost importance. Whenever the weight is below the height there is a fair suspicion of scrofulous or tuberculous disease, which no Insurance office is justified in overlooking. Whilst, on the other hand, when the weight is greatly in excess of the height, there is a tendency to those sudden impairments of muscular and especially circulating powers, which may lead to premature and unexpected death. E. LANKESTER

FOSSIL MAMMALS OF NORTH AMERICA

The Extinct Mammalian Fauna of Dakota and Nebraska; together with a Synopsis of the Mammalian Remains of North America. By Dr. Leidy. With an Introduction on the Geology of the Tertiary Formations of Dakota and Nebraska; with a map. By Dr. Hayden. (Philadelphia, 1869. London: Trübner and Co.)

II.

IN the preceding article the Miocene portion of Dr. Leidy's great work has been reviewed. That part of it relating to the Pliocene and the Quaternary still remains for analysis. That we are able to classify the American Mammalia as the Miocene, Pliocene, and Quaternary, we owe to Dr. Leidy; and his definitions of the two former of these are amply supported by the results arrived at by the Geological Survey of the district, under the direction of Dr. Hayden. The Pliocene strata on the Niobrara River, and in the valleys of the Platte and Loup Fork Rivers, rest on the Miocene beds, which furnished the Mammalia treated of in the first essay. And thus there is evidence that the one series is of later age than the other. Palæontologically, also there is a most remarkable break. Not one species and only one or two genera, namely, Rhinoceros and Castor (Aceratherium ?), are common to the two. With this exception, all the Miocene Mammalia had disappeared during the time that intervened between the formation of the two lacustrine deposits in that region. This fact implies that the one formation is separated from the other by a shorter interval than their European analogues; for in the latter many genera, such as the Mastodon, Hipparion, Hyæna, Elephant, and others, pass from Miocene into Pliocene in such a way as to cause one group of life gradually to shade off into the other, and to render it sometimes impossible to define the last stage of the one from the first of the other.

In the American Pliocenes a Ruminant, the *Merycohyus*, possessed of a full complement of teeth, represented the family to which the Oreodon of the preceding epoch belonged. One species, *M. elegans*, was about the size of a sheep, while a second, *M. medius*, was rather larger. Since the latter is founded only on one upper and three lower molars, it is rather hard to follow Dr. Leidy when he defines the animal as being "one half larger in diameter than *M. elegans*, and intermediate in size between the lama and camel." From such a slender premiss, any exact estimate of the size of the animal must be worthless. The camel tribe were represented by three distinct genera. The Procamelus is distinguished from the camel by the presence of an additional premolar in the upper, and two in the lower jaw. One species, *P. robustus*, was about the size of the living camel. The Homocamelus is remarkable for its large canine, and for the isolated position of the first upper premolar; while the *Merychodus necatus* had molars without an accessory column between the lobes, as in the sheep. The deer is represented by one small species, *Cervus Warreni*, with antlers small and bifurcating, like those of the *C. trigonalis*, figured by M. Gervais from the French Pliocenes, and the *C. dicranoceros* from the Suffolk Crag, and the Miocene of Eppelsheim. One small bifurcating antler, or horncore, may possibly be the solitary evidence of the presence of the antelope in America, but it more probably belongs to a species of deer. In none of the living antelopes is the horncore prolonged into the branch of the horn. *Cosoryx furcatus* therefore cannot fairly be quoted in proof of the range of those herbivores as far as North America.

The Pliocene American Rhinoceros (Acerathere ?) *R. crassus*, belongs to the brachydont division, characteristic of the Pliocenes and Miocenes in Europe. The Mastodon, *M. mirificus*, was devoid of tusks in the lower jaw, and belonged to the tetralophodont section of Dr. Falconer. A species of elephant (*E. imperator* Leidy) was also living during the American Pliocenes. So far as the fragmentary condition of the molar will admit of decision, it belongs to the coarse-plaited variety *E. columbi* of Dr. Falconer. There were also at least three species of Hipparion, and two ancient forms of horse (*Merychippus* and *Protohippus*), which recall to mind the simplicity of pattern represented by equine milk molars. The true horse was represented by *Equus excelsus*; the beaver by the *Castor tortus*, which was half its size. There was also a porcupine (*Hystrix venustus*) which, strange to say, is more closely allied to the crested European than to the living American species. The wolves and foxes of the American Pliocenes are scarcely to be distinguished from those still living in the United States. One (*Canis sævus* Leidy) "is a near relative, if not progenitor, of the existing American wolf (*C. occidentalis*)." *C. temerarius* is intermediate between the prairie wolf and the red fox, while Dr. Leidy admits that the third (*C. vafer*) "may reasonably be supposed to belong to the existing swift fox." The fourth (*C. Haydeni*) probably belonged to a large variety of the western wolf. The larger felines are represented by the Pseudælurus (*Felis quadridentatus*), which was intermediate in size between the panther and the American lynx; and the bear-tribe by the *Leptarctus primus*, a creature allied to the coati.

The evidence afforded by this group of animals, of the physical geography of North America during Pliocene times, is extremely valuable. The absence of the Edentata and of the Rodents of South American type implies that North America at the time was insulated from the south by a barrier which could not be overcome by those animals. The absence of the opossums also points in the same direction. The Isthmus of Panama had not yet risen above the waves to sever the Atlantic from the Pacific Ocean, and to be a bridge for animals migrating from the one continent to the other. On the other hand, the Pliocene genera which dwelt in the basin of the Mississippi point indisputably to an influx of animal life from some other area. The deer, the mastodon, and the elephant, the hipparion, and the true horse, and among the carnivores the genus *Canis* and the Pseudælurus, are perhaps the most significant of the new forms which characterise the American Pliocene. From what region did they come? A glance at the Miocene and Pliocene Mammals of Europe affords a satisfactory answer to this question. All these genera sprang into being during the European Miocene, and with the exception of the last lived also in the succeeding period. It is therefore only reasonable to infer that they found their way into North America from the Euro-Asiatic area, and the fact that they are Miocene genera in Europe renders it possible that their migration began at that time. Whether this reasoning be accepted or not, the lapse of time required, not merely by one or by two genera, but by a group of herbivores with their accompanying carnivores, to fight their way from Europe or Asia into the Southern States—a region which was already occupied by the Oreodonts and other forms—must have been enormous, and probably sufficient to account for the difference between the American and European Pliocenes. If the migration be not admitted, then we must fall back on the now exploded hypothesis of different centres of creation, for there is no other mode of accounting for the presence of the same genera or species in now widely separated regions. It may therefore be inferred from the study of American Mammals, that North America was isolated from the great southern mainland, and that it was connected with Euro-Asia during the Pliocene, as well as during the preceding Miocene epoch.

The Quaternary group of Mammals, in Dr. Leidy's synopsis, points to a considerable geographical change having taken place in the interval that separates the epoch of their existence from the Pliocene. The strange extinct South America Edentates, the Megatherium, Megalonyx, and Mylodon spread over the Southern States, and testify that the barrier, which had confined them to their ancient home in South America, had been removed. In other words they poured northwards over the bridge offered by the Isthmus of Panamá, and occupied the valley of the lower Mississippi. They were accompanied also by extinct forms of the Chinchilla and Cavy. In this way the date of the elevation of Panama above the waves may be satisfactorily ascertained. The Northern extension, therefore, of the South American genera, which at the present time range as far as Mexico and occupy the province of "Austro-Columbia," dates from the Quaternary or Post-Pliocene epoch.

There was, however, no similar geographical change in the relation of North America to the old world. The bisons, horses, and elks, if not identical specifically, are merely varieties or representative species. The Mammoth is common to the whole of North America, and to the vast area in the old world north of a line drawn through the Altai mountains, the Caspian, and the Mediterranean Seas. The musk sheep, so widely spread through Europe in the Quaternary, ranged through Asia along with the Mammoth to the Arctic Ocean, and was found by Captain Beechy in the frozen loam on the American side of Bheerings Straits. In the basin of the Mississippi it is represented by a cognate species, *Ovibos bombifrons* (female), and *O. cavifrons* (male). That these remains really belong to one species of Ovibos, is rendered almost certain by the concurrent and independent testimony of Dr. Rütimeyer and myself. The Cave-lion of Europe was also represented by the *F. atrox* of Natchez. Many other cases might be quoted to show that in those days the connection by land between North America and Asia must have been maintained. It therefore follows, as one might have expected from the soundings in Bheerings Straits, that Northern Asia has been separated from North America in comparatively recent times.

There is one striking fact to be noted in the Post-Pliocene America fauna. Just as certain European Miocene genera appear in the American Pliocene, so two forms which in Europe died out in the Pliocene, the Mastodon and Hipparion, lived on in America into the Post-Pliocene age. It would almost seem as if America was the refuge of forms which had been driven from the old world. That the musk sheep of the Post-Pliocenes of Europe and Asia still lingers only in the high northern latitudes of North America, is a parallel case. These inferences are based on facts accumulated with very great care by Dr. Leidy. His work is an admirable attempt to grapple with most difficult problems; it would have been of far greater value had its author abstained from founding species and genera in some cases from very scanty materials, and in others on the slightest variations from the ideal type.

W. BOYD DAWKINS

OUR BOOK SHELF

OF the *Bulletin de la Société Impériale des Naturalistes de Moscow*, we have just received the first, second, and third parts for the year 1869. The greater part of the important papers in the second part are on Botanical subjects. They include a monographic revision, with tables of species, of the Heliotropes of the eastern Mediterranean region, in which seventy species are cited, and twenty-two of them described as new; a notice of the occurrence of the white Truffle (*Rhizopogon albus*) in the neighbourhood of Moscow; a note on *Empusa muscæ;* and a revision of the species of Characium found in the vicinity of Charkow. Colonel Motschoulsky continues his seemingly interminable "enumeration" of the new species of Coleoptera collected by him during his travels, leading us to wonder how any one man could have collected so many beetles, and, having got them, how he can write so much about them. This, however, is but a small instalment. Another entomological paper of more consequence is a monograph of the genus *Abacetus*, by Baron Chandoir; M. Solsky has a notice of some beetles from Eastern Russia, and M. E. Ballion another on two species of sawflies. The most interesting zoological paper is on the anatomy and development of *Pedicellina* (Sars), by M. B. Uljanin, illustrated with two plates. In this paper our countryman, Mr. Gosse, is absurdly quoted as "Goosc." The remaining papers are by Dr. Ferd. Müller on the determination of the magnetic inclination, and by M. R. Hermann on the composition of Fergusonite. The last part consists of a series of *éloges* on Alexander Von Humboldt, read at a centenary celebration of the great German philosopher. These papers, by different authors, treat of Humboldt from various points of views: as a man and as a naturalist; in his relations to Russia; as an investigator in the domain of electro-physiology; as a botanist; as an investigator of physical geography and climatology. As they are all in Russian we fear these memoirs will find few readers in this country. A German translation of the first of these will, however, be found in the first part of the *Bulletin*, which includes a variety of papers on Natural History subjects. An important geological paper is one by Prof. Trautschold, on secular elevations and sinkings of the earth's surface. Dr. D. Zernoff's memoir on the olfactory organs in the Cephalopoda, which is illustrated with two plates, is a valuable contribution to the anatomy of the Mollusca. In an important memoir, also accompanied by two plates, M. J. Borsenkow describes the developmental history of the ovary and egg in the common fowl. For the botanist we have a continuation of M. L. Gruner's catalogue of plants collected on the Dnieper and the lower part of the Kouka; and a notice, by M. Alexander Becker, of Sarepta, of a journey to Debent, which also, as usual, includes entomological notices. For the entomologist finally we have a catalogue of the Coleoptera of the vicinity of Jaroslaw, by M. von Bell; remarks upon some species as cited in Harold's great catalogue of Beetles, by M. E. Ballion; and, from the inexhaustible Motschulsky, a further instalment of descriptions of new species of Coleoptera.

Für Baum und Wald. Eine Schutzschrift an Fachmänner und Laien gerichtet. Von Dr. M. J. Schleiden. (Leipzig: Engelmann. London: Williams and Norgate.)

A TREATISE on arboriculture, in which, mingled with some extravagant sentimentalism, are many useful hints as to the growth of trees and forests. There can be no doubt that within the last 2,000 years a great amelioration has taken place in the climate of Central and Northern Europe. Varro speaks of the climate of the south of France as unfavourable to the growth of the vine or olive. Virgil describes the Crimea as subject to the rigours of an eight-months' winter; Diodorus Siculus narrates how whole armies crossed the frozen Rhine, Rhone, and Danube; and there is unanimity of testimony among other writers to the same effect. Dr. Schleiden attributes this change in the climate, to a great extent, to the destruction of forests when the country became more thickly peopled, combined, no doubt, with improved drainage. He points out, however, that disforesting may be carried too far, until it becomes positively injurious instead of beneficial. The judicious mean he believes to have been arrived at in England and Ireland, while in some parts of Continental Europe, especially Switzerland and the Tyrol, the almost entire destruction of the timber has caused a diminution of the rainfall to an extent very prejudicial to the crops. Another result in mountainous countries has been the constant accumulation of rain-clouds around the mountain-peaks, and consequent destructive floods and devastating avalanches. In confirmation of Dr. Schleiden's views, it may be stated that in some parts of India the drought has been so severe for several successive years since the destruction of the forests, that the Government has ordered the planting of an enormous number of trees.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Twelve-wired Bird of Paradise

IT may be interesting to many of your readers to know that a specimen of the rare and beautiful twelve-wired Paradise Bird (*Seleucides alba*) is now alive in the Royal Zoological Gardens at Florence. Signor G. E. Cerruti, who has recently returned from an official tour in the Moluccas and New Guinea, writes me that he obtained it from the Rajah of Salwatty, and that although very wild at first, it soon became tame and quiet; and that he had very little trouble in bringing it home. Here is another proof that these wonderful birds can be brought to Europe without difficulty, and once here, with proper care and ample space, there is little doubt they would be long-lived.

ALFRED R. WALLACE

Spontaneous Generation

DR. H. C. BASTIAN, who has recently called attention to the nature of the evidence before scientific men in favour of the theory of so-called spontaneous generation, has supplemented it by fresh experiments of his own. The dilemma in which the opponents of this doctrine are now placed is that they must either admit it, or else allow that a temperature of 150° C. maintained for four hours, and applied by means of liquid, is incapable of killing the germs of infusoria. Many, doubtless, of these opponents will courageously mount this horn of the dilemma, and make the requisite enlargement of their ideas on the subject of vital resistance to change. There are, however, other difficulties in the way. For instance, great difficulties are involved in the assumption that the atmosphere constitutes a storehouse of germs of all kinds ready to burst out into life on the occurrence of suitable conditions.

However small these germs may be, still they must weigh something. And there must be very many of them, seeing that there must be an immense number of kinds of germs, if a volume of air is to supply to any given infusion precisely the right kinds of germs suitable to the conditions provided by the infusion.

Now chemists are in possession of data showing that the possible amount of organic nitrogenous matter in common clear water and common good air is remarkably small—so small, indeed, that the question may fairly be asked—Is it large enough to admit of the requisite number of germs, the existence of which the vitalists assume in water and air?

By the employment of our ammonia method, Chapman, Smith, and myself have shown that the organic ammonia from a kilogramme of good filtered water often falls as low as 0·05 milligramme, and Dr. Angus Smith has shown that a kilogramme of good air sometimes contains as little as 0·085 milligramme of organic ammonia.

A gramme of air—that is about 700 cubic centimetres—contains only 0·00085 milligramme of organic ammonia. Expressing the organic ammonia in its equivalent of dry albumen we have in 700 cubic centimetres of air 0·00085 milligramme of dry albumen. Translated into volume this 0·00085 milligramme of dry albumen will fall short of a cube, the face of which is $\frac{1}{10}$th millimetre in diameter.

Expressed in English measures, the result is, that rather more than one pint of average atmospheric air does not contain so much organic nitrogenous matter as corresponds to a cube of dry albumen of the $\frac{1}{250}$th part of an inch in diameter.

Now is this quantity adequate to admit of the existence of the immense multitudes of germs, the existence of which in atmospheric air is assumed by the vitalists?

J. ALFRED WANKLYN

Colour of the Sky

WITH reference to Mr. Brett's observations on the colour of sea and sky, I have one or two remarks to offer which I think may be of interest. Smokers have all noticed that the smoke from the end of a pipe or cigar is bluer than that which they puff from the mouth, and many may have wondered, as I did for a long time, what the reason of this could be. The contrast may be well seen on a bright sunny day. This is, in fact, the simplest form of the experiment of the condensation of vapours causing them to pass through a fine blue to a white condition, which Professor Tyndall exhibited about two years ago, and which he employed to explain the blue colour of the sky, and the remarkable polarisation of its light. The finer state of division in the freshly-formed smoke gives it its bright blue colour, as does the finely divided aqueous vapour give to the blue sky; the smoke which has passed through the pipe-stem and mouth has become more condensed, and consequently gives a whiter cloud.

The colour of water is, it appears, to a great degree dependent on the same cause as that of the sky. The investigations which Mr. Brett asks for have been already commenced. M. Soret, of Geneva, soon after Professor Tyndall's researches on the cause of the blueness of the sky were published, made similar researches on the waters of the lake of Geneva, and found that the light from the water, when blue, was polarised as the light from the sky, and, so far, there was the probability of the cause of the colour being similar in the two cases. (See *Comptes Rendus* (Paris), April, 1869.) That particles in a fine state of division are the cause of the blueness of water as well as of sky is also made evident from a comparison of the waters of different lakes, seas, and rivers. There are two popular theories as to the cause of the colour of masses of water, which have very deep root, and yet must, it seems, be abandoned. One is that seas or lakes are blue by reflecting the blue sky. On this ground I have heard Mr. Brett's picture in the Academy this year of a deep blue sea, severely criticised, because the sky, which he has painted with it, is not correspondingly blue, and could not furnish the sea's tint by reflection. Mr. Brett is, however, quite right in his fact, as many people know well enough; and the criticism was misplaced, if the blue colour of a mass of water is dependent on the reflection of light from within water containing finely-divided particles—not from the surface only—as explained above. The second popular theory which seems to be ill-founded is that the green colour of lakes, rivers, and seas is due to plants growing on the bottoms and giving their colour by reflection. The green colour is produced in the same way as the blue in all probability, and may be due to a yellowness of the water in some cases, but it is less easily accounted for than the blue colour. M. Sainte-Claire Deville is quoted by M. Soret as stating that waters which give a white residue on evaporation are *blue*, whilst those which give a yellow residue are *green*. Reflection of the colour of the sky, and of the plant colour from the bottom, does no doubt produce colour of water in some cases, but it is only in shallow pools that the latter can have any effect, or through perfectly smooth surfaces that the former can be effective. Some cases of water-coloration which I have noted will be not out of place here:—1. Intensely blue on a bright day, with pale sky and large cumulous clouds, was the colour of water in reservoirs twenty feet deep at Plumstead, depositing chalk (by means of which the water is softened according to a patent process). 2. Intensely blue (the bluest here noted)—Mediterranean at Marseilles. 3. Bright blue—Lake of Geneva. 4. Darker blue, tending to Indigo—sea near Guernsey; also the Laacher See, in the Eifel. 5. Pale blue—sea near chalk cliffs, being at a little distance from the coast green or greyish. 6. Pale blue or greyish blue—the Rhone, the Mosel, glacier streams, &c. 7. Green—the Rhine, the Scheldt (very markedly so at Antwerp, as testified in Belgian pictures), the Seine, Thames Estuary, &c. 8. Intense green—in patches on the Lake of Geneva; in the evening, when the sun was just below the mountains, more frequently on the Lakes of Thun and Lucerne. 9. Bright green—the sea, on a windy day, with bright sun, off the Isle of Man. 10. The sea round the coral reefs of Florida is said to be intensely green; when away from the coast it is deep blue. 11. On a heavy, clouded day, with rain, gleams of sunshine out at sea give patches of green colour and reddish brown. 12. Water standing in an old copper mine at Killarney was intensely green, whilst the water in the lake at the side was black in the mass. 13. Red colour is produced in some seas by algæ, in others and in some rivers by the breaking up of soil coloured red by iron. 14. Opaque green colour is produced in ponds (Serpentine and ornamental waters) by unicellular organisms, which sometimes swarm in these waters. They may similarly become red. Perhaps the most remarkable instance of blue colour, due to the optical properties of water, is the blue grotto of Caprera, where, at any rate, the reflection of the sky is eliminated. A similar phenomenon is the glorious blue and green of the glacier fissures.

Leaving the question of surface reflection aside, which can only come into play in the case of road-side pools and such mirror-like waters, and also leaving aside the appearance of vegetation in clear shallow streams and ponds, it seems that at the present time we may ascribe the blue colour of masses of water to a peculiar reflection of the light from within the water, accompanied with polarisation, and depending on suspended particles. Blackish, brownish, and yellow colour is due to vegetable matter in solution; reddish brown to iron, sometimes; green, sometimes, to copper or algæ, but the green commonly seen on seas, lakes, and rivers, like that of glacier-fissures, probably admits of a similar explanation to that of the blue. I trust some physicist may be induced to enter into the subject in these pages. Has not the production of a series of tints at sunset an origin which may tend to explain the various tints of blue and green waters? I find that Mr. Sorby in the *Philosophical Magazine*, November, 1867, ascribed the blue colour of the sky and the successive yellow orange and red tints of the setting sun to the absorption of the red rays more than the blue, by the fine aqueous vapour of the higher regions of the atmosphere, and of the blue rays more than the red by the coarser vapours near the earth's surface—as *e.g.* a fog.

The foregoing notes may suggest to others similar observations of greater importance, which it would be interesting to collect. It would be very satisfactory, and of interest to many readers, if some one who could speak with authority on the physics of light, would discuss these phenomena, however suggestively, in your pages.

E. RAY LANKESTER

Poisonous Fishes

IN answer to your correspondent M.D.'s second question in your issue of June 30th, I beg leave to refer him to Dr. Günther's article in this Society's "Proceedings," on a Poison Organ in a genus of Batrachoid Fishes. (P. Z. S., 1864, p. 155.)

Zoological Society of London, P. L. SCLATER
11, Hanover Square, London, W., July 10

Fall of an Aerolite, 1628

YOUR correspondent T. W. Webb may be glad to know that a graphic account of the Aerolite he refers to (NATURE, July 14) as having fallen in Berkshire in 1628, will be found in Vol. II. of Chambers' Papers for the People, published 1850, in an article entitled "Memorabilia of the 17th Century," p. 10. This article also contains many other very extraordinary and well-described accounts of earthquakes, floods, mirages, and various startling atmospheric phenomena which occurred during the 17th century. Amongst the latter the accounts of parhelia, or mock suns, and haloes, and the falls of two or three aerolites, are worth noticing.

Unfortunately, throughout the article the sources whence the various notices have been taken is uniformly omitted.

Alderley Edge, Manchester, July 17 J. P. EARWAKER

Are Jupiter's Cloud-belts due to Solar Heat?

A CIRCUMSTANCE which, so far as I know, has not yet been noticed, seems to me to afford very strong evidence in favour of my theory that the cloud-belts of Jupiter are caused by heat existing in the planet itself. If the cloud-belts were caused by solar heat, they should exhibit a characteristic corresponding to what is observed in the case of the earth's equatorial cloud-zone. "At the equator," Kämtz remarks, "the sun nearly always rises in a clear sky; towards mid-day the heavens are clouded; towards evening the clouds disperse." Now it follows that to an observer regarding the earth as we see Jupiter, there would appear at all times only a fragment of an equatorial belt, near the middle of the disc. But the belts of Jupiter present no such fragmentary appearance; there is a change in their aspect close by the edge of the disc, but the change is one obviously arising from the foreshortening.

Another circumstance also is worth noting. If the cloud-belts of the outer planets were sun-raised, the great tropical belt of Saturn ought to follow the sun as the tropical calm zone of the earth does. The fact that the Saturnian belt remains persistently equatorial is very significant. RICHD. A. PROCTOR

The Rotundity of the Earth

IN your number for July 14th, after publishing an *ex parte* statement, you ask, "Will nothing stop Parallax's mouth?" I hope you will allow me to say, in your next issue, that only one thing can operate in doing so! That one thing is a practical experiment fairly conducted, honestly reported, and logically applied. Your readers will notice that although my signature (Parallax) is appended to the "copy of agreement," it does not appear in the "copy of certificate," and it is proper they should know the reason. It had been agreed that the flags should be fixed in my presence, and that the spirit-level employed should be in good working order and to my satisfaction in every respect. On arriving at the "scene of operations" at the appointed time, I found that the flags had been fixed, and that the spirit-level had been adjusted for some time before my arrival. I immediately protested, and demanded that I and the friends with me should go and measure the altitude of each flag, but an obstinate resistance was offered to this, and also to any interference with the adjustment of the "level." I therefore at once declined to be longer present, and returned to Norwich, where the whole matter was exposed at a public meeting. Notwithstanding the manifest injustice of the attempt, and my refusal to have anything to do with it the moment I discovered its unfairness, these wise and clever and very just Newtonians would have the world believe that they had once and for ever settled the question of the earth's true form and magnitude. Such tricks are unworthy of the cause, and the men who can condescend to deal in them can do no real service to the school to which they belong.

I beg to give the friends of the Newtonian system the following simple challenge: To select six miles of still water, place a boat at one end, with a flag say six feet above the surface of the water. Now, at the other end, let a good telescope be fixed at an elevation of eighteen inches; I affirm that the boat and its flag *will be distinctly visible;* whereas, if the earth is a globe of 8,000 miles diameter, the top of the flag would be more than seven feet below the intervening arc of water. This is my challenge, and let the Newtonians decide that it shall be accepted; saying with me, "let us stand or fall by the result!"

1, Hawley Villas, Chalk Farm Road, PARALLAX
London, July 18

[We print "Parallax's" letter, but we warn everybody against accepting his challenge. Mr. Wallace's treatment at the hands of these gentry shows us what to expect. Let "Parallax" take a good telescope and a return ticket to some seaside place and watch the ships travelling to and fro over the horizon. We offer him space in NATURE to detail his observations, and to explain them, if he can, on any other theory than the received one.—ED.]

Eclipse of the Moon

THE eclipse of Tuesday evening (July 12) exhibited some interesting variations in tint and degree of illumination in different parts of the shadow. There seems to have been a dark spot, perhaps half the moon's diameter, about the centre of the umbra, nearly, if not quite, free from refracted light; outside this a ruddy zone; and beyond this again, to the edge of the shadow, a region strongly illuminated, comparatively speaking, with yellowish pink, as it appeared when projected upon the entering, or yellowish green upon the emerging moon. It is true that just after the commencement, and some little time before the end of the eclipse, the part just within the shadow appeared darker than the eclipsed limb; but this, I think, must have been an optical effect caused by the overpowering light of the uneclipsed portion.

When, however, about two-thirds or three-quarters of the moon's surface was covered, both before and after totality, the illumination of the umbra near its boundary was very conspicuous. The "seas" reflected less light than the other portions, and the regions along the south limb, and between the north limb and Mare Imbrium, were specially bright. These gave almost the effect of the illuminated cusps of the moon being distorted, and prolonged into the shadow. When the eclipse became total, the eastern hemisphere, about the region of Oceanus Procellarnus, was covered by a dark shade, so dark that the extreme eastern limb was scarcely if at all visible. A short time after totality the moon was quite hidden by clouds, and remained so, excepting occasional momentary glimpses, during which nothing could be seen, excepting that she was visible as a dull, pinkish light, till just before the end of the total phase, when the clouds broke away from her neighbourhood in a marvellously dramatic way. The emerging was wonderfully beautiful; first a strong, greenish light appeared along the eastern limb; this changed to silvery; then bright, full yellow. The eclipsed part was moderately bright, shining with a reddish light, excepting a deeper shade over the western hemisphere. In fact, the appearance was exactly analogous to that at the commencement of totality. I regretted extremely that I had not the opportunity of watching whether the dark spot really traversed the disc from side to side, but I have little doubt that at the time of central eclipse the moon presented a well-marked annular phase.

As the gathering of clouds, and their sudden dispersion at the end of the total phase, seems a striking instance of the supposed influence of moonlight in dissipating vapour, it is perhaps well to mention that after the end of the eclipse, clouds appeared to be again gathering round the moon.

The telescope I used was a $3\frac{1}{2}$-inch refractor, armed with a power of 35. GEO. C. THOMPSON

Cardiff, July 17

Wave-lengths of Complementary Colours

IN applying Mr. Maxwell's observations to the verification of a hypothesis (see NATURE for July 7), Mr. Murphy identifies complementary colours by their *names*, overlooking the fact that the observations themselves afford pretty accurate determinations of several pairs. Separate results are given for two observers in Tables VI. and IX. of the memoir formerly cited by Mr. Murphy (NATURE, April 28), and are laid down graphically in figures 4 and 5. From the latter form of the data, as the more convenient, though no doubt the less accurate, the following formula was obtained by interpolation, for the relation between complementary wave-lengths :—

$$(\lambda - L)(L' - \lambda') = M^2;$$

where λ is a wave-length between 2350 and 2100 (in Fraunhofer's measure); λ' is its complementary between 1820 and 1700; and L, L′, M are quantities differing for different eyes, and having the values

2076	1842	77·9	for observer "K;"
2132·5	1859·4	51·2	for observer "J."

Mr. Murphy's complementaries are not in general really complementary for either observer. Independently of this, Mr. Murphy's probable error is about ·53, that is, over one in 100: the probable error of the formula is certainly under one in 500, between the limits stated. C. J. MONRO

THE APPLICATION OF PHOTOGRAPHY TO MILITARY PURPOSES

MODERN warfare may in many respects be considered as so many applications of science. Not only is war *matériel* designed and manufactured nowadays upon the most approved data, and according to theories worked out with mathematical accuracy, but a large section of our soldiers are educated in such a manner as fully to appreciate the value of their resources, and so to overcome difficulties which years ago would have been regarded as impossibilities. No instance demonstrates this more satisfactorily than the recent Abyssinian expedition, which, whatever may be said of it as a campaign, cannot but be regarded as one of the most wonderful feats of engineering accomplished in modern times. The nearer warfare approaches perfection, the more decisive, and therefore less cruel it necessarily becomes, as witness the brief duration of the wars of late years on the Continent; and for this reason the improvements in warfare effected by science cannot by any means be regarded as a misapplication of knowledge.

Our present remarks bear reference to the applications made of a very modest branch of science, if science, indeed, it can be called, our object being to demonstrate the many uses made by the War Department of Photography. In the special application of this art-science to military matters, our government is certainly in advance of others, if we except, perhaps, that of France. No less than three establishments have been organised in connection with the army in which photography is extensively practised, the most important of them being the General Establishment at Woolwich; but besides these, there are again many Royal Engineer Stations, both at home and abroad, which are furnished with photographic requisites and employ the camera for divers purposes. At Chatham, the photographic establishment assumes the character of a school of instruction, at Southampton it forms an adjunct to the Ordnance Survey Office, while at Woolwich, of which department we desire more particularly to speak, the duties performed by aid of the camera are as various as they are numerous. For registering patterns, recording experimental results, imparting military instruction, and for other purposes too multifarious to enumerate, photography is extensively used, the faithful accuracy of sun pictures, as likewise the facility with which they are produced, causing the art to be eagerly employed in any way where it can be made available.

As an example of the value of photography in instruction, we would cite an interesting series of pictures taken to illustrate ordnance drill. This series comprises upwards of one hundred views, and demonstrates the practical working of the various kinds of guns, mortars, rockets, &c., in the service. One picture, for instance, will illustrate the command "Prepare for action"; a gun will be shown surrounded by a group of artillerymen in the positions they have been instructed to occupy on the issue of that order, each man having his respective number attached to his cap as a distinguishing mark. The next illustration in the series is probably that of "Load," and the next again "Fire," both of which will represent the change in position of the men, as one operation succeeds another, and the various duties performed in turn by each gunner or number, for it must be remembered that in gun-drill every man is told off to a particular number and entrusted with a separate and distinct duty. Thus, on the promulgation of any new system of drill, or of any modification in the method of working, it is merely necessary for the military authorities to forward pictures of this kind to the different instructors, who cannot fail at once thoroughly to understand the new exercise; and even the rawest recruit who had assigned to him a certain number at a gun would see at a glance the exact position he is to occupy by a reference to the photographs.

Another not less striking instance of the importance of photography in this connection may be given. At the outset of the Abyssinian campaign it will be remembered that several thousands of packsaddles were required for transporting war *matériel* into the interior. These packsaddles were made in and sent direct from England to Annesley Bay, so that the troops coming from Bombay knew nothing of their construction, nor of the method in which they were to be packed. This ignorance in the hurry of affairs would have been of serious consequence (for a military packsaddle of the present pattern is a somewhat complicated contrivance) had not the authorities at home been fully alive to the subject and foreseen the threatening difficulty. A mule at Woolwich was harnessed and packed, after some experience had been acquired in the matter, in the most suitable and approved manner, and the animal then carefully depicted by the aid of the camera; the disposal of the harness and trappings and the correct way in which the packages were to be carried, were thus clearly shown in a photograph, numerous copies of which were immediately sent out to Annesley Bay and distributed among the officers of the Quarter-Master-General's department.

In recording experimental results photography again fulfils a duty which could not be discharged so rapidly and impartially by any other means. The stout iron-cased shields and armoured targets built up of metal plates of different thicknesses, and then fired at with shot and shell of all descriptions, are carefully photographed after each decisive experiment, and a record of indisputable accuracy thus obtained. With a picture before us of a target, constructed to represent the side of an armour-plated vessel which has been experimented on, we can at once form an accurate estimate of the impressions made upon the iron wall by shot of different calibres, while rear and side views of the structure will show plainly the amount of damage which the backing or skin of the shield has suffered. As may be imagined these prints form important illustrations to the written reports made from time to time to the War Office authorities.

The photographing of newly adopted government patterns, whether in the shape of guns, carriages, waggons, mantelets, tents, &c., is also an important section of the work undertaken at Woolwich, as likewise that of producing pictures relating to army equipment, such, for instance, as demonstrate the setting up of cooking apparatus, disposal of ambulances, refitting of ordnance in the field, &c. There is, moreover, the pursuit of photo-lithography to be mentioned, by means of which designs and sketches are copied and transferred to stone for printing off in the ordinary manner.

The subject of working photography in the field is a matter to which much attention has been given at the general establishment, for it will be readily conceived that the simplest and most effective methods of working, as likewise the different uses to which the camera may be put during warfare are questions for very serious study.

The photographic copies, many thousands of which are annually produced and distributed over all parts of Her Majesty's dominions, are not now printed upon silver paper in the ordinary way, but by the so-called carbon or autotype process, a method which produces prints of an absolutely permanent character. Ordinary silver prints are always liable to become faded and stained after the lapse of a few years, owing to the presence in the paper itself, or in the atmosphere with which it comes into contact, of sulphur compounds which attack the metallic silver composing the image. In the carbon pictures, however, no silver at all is present, the composition of the image being a mineral pigment in combination with an insoluble chromium.

Our description of the General Photographic Establishment at Woolwich has been very brief indeed, but enough has been said to show to what an important extent the art is employed in connection with the War Office; the department which we have described is a branch of the chemical establishment of the War department, which was first organised, in 1854, by Mr. Abel, and has gradually become intimately and indispensably connected with every branch of the military service.

THE PHYSIOLOGY OF DIGESTION

I. MASTICATION

I PROPOSE in this and the following papers to give an account of the physiology of digestion, or, in other words, to describe the operations to which food is submitted, and the alterations it undergoes, before it is absorbed into the system and becomes adapted to the nourishment of the body.

With this end in view, it will be expedient to divide the alimentary or digestive tract into a series of sections, each comprising a portion in which certain definite and, in general, well-ascertained processes are effected. The first of these is clearly the mouth, where the food, provided it be not already of a fluid nature, is ground to a pulp and mingled with saliva, or, in other words, where it undergoes mastication and insalivation.

In order to prevent the ingestion of substances that, from their temperature, hardness, acridity, putrefaction, or other chemical or physical properties, are inappropriate as articles of food, the mouth, or vestibule of the alimentary canal, is guarded by three sentinels that, were due attention paid to the impressions derived from them, would rarely be found to mislead: *Touch*, possessed to an exquisite degree by the ruddy lips and tongue; *Taste*, possessed by the tongue and palate; and *Smell*, which, though comparatively neglected by man, is constantly

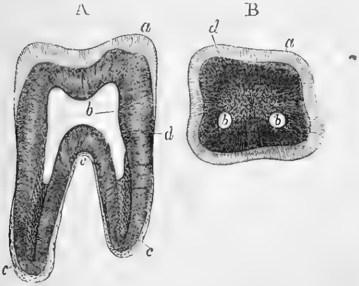


Fig. 1.—A, vertical; B, horizontal section of a bicuspid tooth; *a*, enamel of the crown; *b*, pulp cavity; *c*, cement of the fangs; *d*, dentine; magnified three diameters.

employed by animals as a means of discriminating suitable from unsuitable substances.

The act of mastication is designed to comminute the food, and thus to present a larger surface to the action of the several digestive fluids, saliva, gastric and intestinal juices, &c., as well as to render it more readily capable of incorporation with them. It is more important that it should be thoroughly performed in the case of vegetable than of animal food, since the latter is usually of a softer and more succulent nature, besides being already analogous in composition to the body; whilst the nutritive material contained in the former is enclosed in firm cell-walls that are slowly dissolved in the act of digestion, and requires the action of the several fluids to be long continued before it is fitted for nutrition; and it is accordingly found that the means for effecting such comminution is far more complete in the vegetable than in the animal feeders. In a teleological point of view it is interesting to notice that in the infant living on milk, which does not require mastication, no teeth exist till the fourth or fifth month.

Mastication is accomplished by the movements of the jaws, the margins of which are very generally armed with teeth. Amongst Mammalia the teeth are only absent in the whalebone whales, the anteater, manis, and echidna—many rows of small, sharp, hard, epidermal spines situated on the palate and base of the tongue, however, supplying their place in the last-named animal, whilst the two former may be said to live on animal food that is already, in proportion to the bulk of their bodies, extremely comminuted. Teeth are indeed absent in the whole group of Birds, and in the Chelonia, doubtless in the former case on account of their weight, which would interfere with flight, but their place is supplied in both by the cutting beak and in the latter also by the powerful gizzard. They are absent in the toad amongst Amphibia, and in the sturgeon, paddlefish, pipefishes, ammocete and amphioxus amongst Fishes, but are elsewhere constantly found amongst the Vertebrata; they are very frequent also amongst the Invertebrata, though many live by sucking the juices of the animals on which they subsist.

In regard to the teeth of man, it need only here be mentioned that they are twenty in number in the child, and thirty-two in the adult; that the six front ones, namely, the four incisors and two canines in each jaw, are chiefly employed for cutting and tearing the food; and the remaining back teeth, including the four premolars and the six molars, for pounding and bruising it. The teeth are the hardest parts of the skeleton. Four parts may be distinguished in them—(1) the pulp, which, occupying a cavity (*b*) in their centre, is extremely small in quantity, and contains an artery vein and nerve; (2) the dentine (*d*), which forms the greater portion of the tooth and confers upon it its general configuration; (3) the enamel (*a*), which caps the

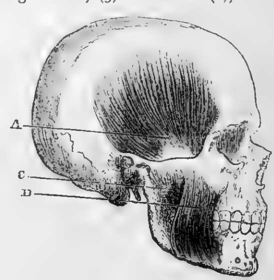


FIG. 2.—View of the external muscles of mastication. A, temporal muscle; D, superficial; and C, deep portion of the masseter muscle.

free or exposed surface of the tooth, or that portion which projects beyond the gum; and (4) the cementum (*c*), which invests the fang.

The Dentine (*d*) is composed of a series of tubes traversing a homogeneous matrix, and extending from the pulp cavity to the outer limit of the dentine. The course of these tubes is undulatory, and they give off numerous branches as they pass outwards. It is difficult to trace their minutest ramifications even with high powers of the microscope. Their interior is occupied by delicate solid threads called dentinal fibres, which, however, it has recently been shown by Neumann are not in immediate contact with the matrix, but are separated from it by a resisting membrane or dentinal sheath. The presence of these fibres accounts for the sensitiveness which it is well known the dentine when exposed possesses, whilst there can be no doubt they are subservient to the slow processes of nutrition which are continuously taking place in even the most superficial parts of these hard organs.

The Enamel (*a*) is composed of a series of six-sided, solid prisms, which contain scarcely more than 2 per cent. of organic matter, but consist almost exclusively of phosphate and carbonate of lime. In the rabbit, rat, squirrel, and all Rodentia, it forms the cutting edge of their chisel-shaped incisor teeth.

The Cement (*c*) is a peculiar kind of nonvascular bone, and though small in quantity and comparatively unimportant in man, enters largely into the composition of the teeth in many of the lower animals. The statement advanced by Jean Jacques Rousseau, on the one hand, that man is a herbivorous animal, and by Helvetius on the other, to the effect that he is carnivorous, both seem to be refuted by the general characters of the teeth, so far as any evidence can be derived from such a source; for in this point of structure man occupies an intermediate position between the Carnivora and the Herbivora, and appears to be adapted for the consumption of both animal and vegetable substances, though doubtless life can be well preserved with a due selection and sufficient supply of either, as we shall subsequently see.

The lining of the mouth is a mucous membrane of considerable thickness, containing numerous glands, and covered with many tiers of tesselated epithelial cells, into the deep surface of which sensitive and vascular papillæ from the membrane itself project.

The movements of the lower upon the stationary upper jaw in man are effected by the muscles, exhibited in Figs. 2 and 3.

They comprise vertical, lateral, and backward and forward movements. The *depression* of the lower jaw is accomplished with considerable rapidity by the contraction of three or four small muscles—the digastric, stylo-genio, and mylo-hyoid—forming the floor of the mouth, the first-named being the principal agent in the Carnivora.

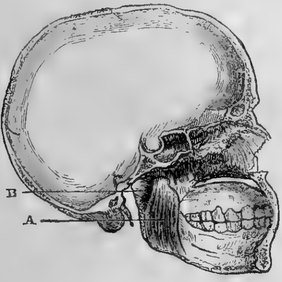


FIG. 3.—View of the internal muscles of mastication. A, *internal* pterygoid muscle; B, *external* pterygoid muscle.

From the position of the joint or pivot on which the lower jaw moves, the front teeth can be nearly twice as widely separated from one another as the back. The difference in the gape of the jaws varies in different people, and to a greater extent than might at first sight be supposed. In some measurements that I have made, I find that whilst the distance between the free borders of the front teeth does not, in some instances, exceed one inch, in others it amounts to upwards of two inches. The segment of a circle formed by the upper is usually somewhat larger than that of the lower jaw. Its *elevation*, which is a far more energetic movement, is performed by the temporal (A, Fig. 2), masseter (C, D, Fig. 2), and internal pterygoid muscles (A, Fig. 3). All these muscles are attached to the jaw between the fulcrum or condyle of the jaw, shown in Fig. 2, just in front of the external opening of the ear, and the weight to be moved or fore-part of the jaw. They therefore constitute levers of the third order; and hence, though acting promptly, are placed at a comparative disadvantage for exerting their fullest force. Still their power is immense even in man, who is again surpassed by many of the Carnivora. It is not easy to estimate it. I have endeavoured to do so, however, by ascertaining the pressure required to crush a Brazil-nut, which seems to be about the limit of the power possessed by young people with good teeth. At least very few can crush the stone of the peach. I find that a weight of about 84lb., but varying from 56lb. to 100lb. is requisite to break a Brazil-nut when placed on one of its flat sides, and this probably is not a very unfair representation of the average power of the three above-mentioned muscles. The lateral movements of the lower jaw are effected by the alternate contraction of the external pterygoids (B, Fig. 3), which are consequently seen in their highest state of development in the Ruminants. They are feeble in man. The forward and backward movements, also feeble, are due, the former to the external pterygoids, the latter to the deep portion of the masseter (C, Fig. 2), the posterior fibres of the temporal and the internal pterygoid.

Another muscle deserves to be noticed—the buccinator—which forms a considerable proportion of the thickness of the cheek, and which is an important agent in preventing the accumulation of food between the teeth and the cheek, the occurrence of which is so troublesome in some cases of paralysis. It is remarkable that all these muscles are supplied by one and the same nerve, the fifth; the last-named, however, receiving some additional motor filaments from the seventh or portiodura.

The movements of the tongue, which are under the influence of the hypoglossal nerve, are of very great importance also in aiding mastication, since its wonderful tactile sensibility enables us to feel for and thrust back between the teeth portions of food which have escaped their action. The centre for co-ordinating the various movements required for mastication appears to be situated in the medulla oblongata.

Having thus considered the mechanical process to which the food is subjected in the mouth, we shall proceed in another article to consider the chemical changes effected in it by the act of insalivation. H. POWER

NOTES

WE have more than once had to notice the liveliness recently exhibited by science at the Antipodes. This has now found expression in the issue of a monthly journal in Australia called "The Scientific Australian," a journal of industry and instruction, specially devoted to those engaged in scientific, artistic, and industrial pursuits, and to the promotion of technological education amongst the operative classes. The editor, Mr. J. S. Knight, Assoc. Inst. C.E., F.R.I.B.A., appears to have enlisted the services of the most eminent scientific men, not only in Victoria, but in the sister colonies of New South Wales, Queensland, South Australia, Western Australia, Tasmania, and New Zealand, and the articles, as a general rule, are to be signed. It is gratifying to find these signs of life in our colonies; we shall watch with interest the career of our contemporary, the first number of which was to be published at Melbourne on the 1st of the current month, and wish it every success.

AT the public sitting of the Paris Academy of Sciences, held on the 11th inst., the following prizes were awarded:—"The Astronomical Prize, Lalande foundation, to Mr. J. Watson, for the discovery of eight new asteroids in one year. The Mechanical Prize, Monthyon foundation, to M. Arson, for his experimental researches on the flow of gases in long conduits. Statistical Prize, Monthyon foundation, to M. Chenu, for his medico-chirurgical statistics of the Italian campaign of 1859-60. Prize established by the Marchioness Laplace, to M. F. A. Voisin. Trémont Prize to M. Le Roux, to aid and encourage him in the pursuit of researches on the index of refraction for certain vapours, and on the measurement of the heat developed by electric currents. Poncelet Prize to M. J. R. Mayer, of Heilbronn, for his memoirs on the mechanical theory of heat; Prize for Medicine and Surgery: a medal of the value of 3,000 francs, to MM. Legros and Onimus, for their works on the application of electricity to therapeutics; a medal of the value of 2,000 francs to M. Cyon, for a similar work. Prize for Experimental Physiology: to M. Famitzin for his researches on the influence of light on the nutrition of plants; a prize of 600 francs to MM. Tripier and Arloing for their discoveries respecting the cutaneous sensitive nerves. Prize for Medicine and Surgery, Monthyon foundation: a prize of 3,000 francs to M. Junod for his MS. work, entitled *Des médications hémospasique et aérothérapique;* two prizes of 2,000 francs to M. H. V. Luschka for his works on anatomy and to MM. Paulet and Sarazin for their treatise on topographic anatomy. Prize for the Unhealthy Arts: two prizes of 2,500 francs to M. Pimont for his *Calorifuge plastique* and to M. Charrière for his life-preservers. Bréant Prize: a prize of 5,000 francs to M. Fauvel for his works on the etiology and the prophylaxis of cholera. Cuvier Prize to Professor Ehrenberg, of Berlin. Bordin Prize, for a monograph of an invertebrate marine animal, divided between M. Marion, author of "Zoological and Anatomical Researches on the non-parasitic imarine Nematoïda," and M. A. Wagner, author of a monograph of the *Ancei* of the Gulf of Naples. Jecker Prize to M. Friedel, for his researches on the compounds of silicon corresponding to the compounds of organic origin. Barbier Prize, divided between M. Mirault on the surgical occlusion of the eyelids in the treatment of double ectropion, and M. B. Stilling, for the perfecting of the operation of ovariotomy. Godard Prize to M. Hyrtl, for his researches on the genito-urinal organs of fish. Desmazières Prize, divided between M. L. Rabenhorst for his European flora of fresh-water and marine algæ, and M. A. Hofmann, for his memoir on Bacteria. Thor Prize to M. Bonnet, for his work on the edible truffle. The Mathematical Prize of 3,000 francs, the Damoiseau Prize for the theory of the satellites of Jupiter, and the Medical and Surgical Prize for the application of electricity to therapeutics have not been awarded.

THE monument to Kepler, to which we alluded some weeks since, was inaugurated on the 24th June, with great *éclat*, at his native place, the little town of Weildiestadt, in Wurtemburg. The statue is of bronze, ten feet high, in a sitting posture. In his left hand, supported on a celestial globe, he holds a parchment on which is drawn an ellipse; in his right hand is an open compass; he is looking towards the heavens. The four corners of the pedestal are adorned with statues, five feet in height, representing M. Mastling, the Professor at Tübingen who taught Kepler mathematics, Copernicus, Tycho Brahé, and Jobst Byrg, the mechanician who assisted in the construction of his optical and astronomical instruments. In the centre of the pedestal is the single word, KEPLER; on each side are bas-reliefs, representing different circumstances in his life:—Kepler, at the age of seventeen, entering the lecture-room of Professor Mastling, at Tübingen, who holds the young pupil by the hand, and explains to him the Copernican system; the discussion between Tycho Brahé and Kepler on the system of the world, in the presence of the Emperor Rudolph and of Wallenstein; and Kepler and Byrg in their workshop at Prague, making their first use of the telescope for astronomical observations. The monument is the work of the sculptor, Kreling, director of the School of Fine Arts at Nüremberg; the statues and bas-reliefs were cast and chiselled in the workshops of MM. Lenz and Hérold in the same town; the pedestal of red sandstone, from a quarry in the neighbourhood of Weildiestadt, was constructed by the architect Egle, of Stuttgart.

AT the Working Men's International Exhibition now being held at the Agricultural Hall at Islington, one of the most interesting objects is a reflecting telescope, exhibited by Mr. T. W. Bush, a baker and grocer in one of the more humble streets of Nottingham. It has specula about thirteen inches in diameter, is equatorially mounted, and presents several novel features of construction that are claimed as improvements. Mr. Bush is a self-taught astronomer, mathematician, and mechanic. He has made, without assistance, the whole of the calculations necessary for the construction of the instrument, and has constructed the models for all its parts. With the exception of the prism and some of the more bulky parts, the whole of the telescope, the brass and iron work, the cutting of the sections, the graduation of the circles and verniers, the casting, grinding, and polishing of the metal speculum, the grinding and polishing of the glass speculum, and even the making of the tools and machinery required, have been the work of his own hands. The specula have been tested by Purkiss's process and proved correct, and the telescope has been found to divide satisfactorily such double stars as η Coronæ, ζ Boötis, and ζ Herculis. Its performance on the moon and on nebulæ is also very fine, and it has been used with a magnifying power of 1,400. Unfortunately, Mr. Bush lives in a low situation close to the River Lene, to a canal, and to smoke-yielding factories, and his observations have been hindered by the state of the atmosphere in his vicinity.

ANOTHER object of interest at the same Exhibition is a display of iron, obtained by a process invented by Sir Antonio Brady, from some of that dockyard refuse irreverently described as "Seely's pigs," and which has been the subject of discussion both in Parliament and by the Press. These pigs were of different qualities, but were all largely contaminated with phosphorus and sulphur, and were supposed to be of little or no value. The presence of phosphorus renders iron brittle when it is hot, the presence of sulphur renders it brittle when it is cold. The pigs containing both were worth in the market about 2*l*. 5*s*. per ton. By Sir Antonio's process the sulphur and the phosphorus can be extracted at a cost of about 35*s*. per ton, and the residual iron is superb. It bears any and every test. One of the pieces exhibited had been beaten cold to the thinness of writing paper at one end, drawn to a point at the other, and then twisted by hand eight turns in an inch at a single heating. Massive bars had been beaten cold until the surfaces on each side of the bend came into perfect contact, and a plate six inches wide and half-an-inch thick had been beaten till its edges were in contact, the flat surface remaining horizontal. In neither case were there any traces of a flaw either at the convexity of the curve, where the metal was stretched, or at the concavity, where it was compressed. Holes in a thick plate had been enlarged by driving cones into them, and, in a word, the iron had been knocked about in every possible way. At a very low estimate it is worth 14*l*. per ton, and as there is plenty of the raw material to be had, the profit of the invention seems likely to be great.

WE regret to have to announce the death, at the Villa Pisani, near Lucca, of A. H. Haliday, A.M., of Carnmoney, Co. Antrim, which event occurred on the morning of the 13th July. Mr. Haliday entered Trinity College, Dublin, in 1822, at the age of 15, and graduated five years afterwards as a gold medalist. Shortly after he was called to the bar; but he never practised. He was High Sheriff of Antrim in 1843. As an entomologist his name will always rank with those of his friends, Curtis, Sichel, Westwood, Dohrn, and Walker, while his quiet and retiring manners, and his great scholarship endeared him to a large circle of friends.

WE regret to have to announce the death of the distinguished Anglo-Saxon scholar, Mr. Benjamin Thorpe, which took place on Tuesday last, the 19th inst. Mr. Thorpe was in his eighty-eighth year.

A REPORT which has recently appeared in the papers about Dr. Livingstone, is referred to by Sir Roderick Murchison in a letter to the *Times*. The statement (communicated by Sir Thomas Maclear, late Astronomer Royal at the Cape of Good Hope) was that one Mr. Anderson, commanding a vessel bound from Zanzibar to the United States, had informed Sir Thomas, on or before the 14th of March, that Dr. Kirk, the consular agent at Zanzibar, had informed him that he had had a recent letter from Dr. Livingstone, who was quite well and about to proceed northwards. Sir Thomas Maclear had indeed (the president of the Geographical Society writes) "communicated the same to me; but I at once saw that the story was false, for I had in my possession a letter from Dr. Kirk, of two months' later date than the 14th of March, in which he could give me no information whatever respecting the great traveller. Since then I have again heard from Dr. Kirk at Zanzibar. I have for some time, and in concluding the geographical session, announced to all concerned that for at least eight months we did not expect to hear any definite news respecting Dr. Livingstone, inasmuch as the supplies and carriers to be sent to him and the answers to letters could not be obtained for a long period. I allude to those supplies which were obtained from Her Majesty's Government, and which only left England a few weeks ago."

WE cull this note from the *Pall Mall* and commend it to Mr. Lowe:—"To scientific English ears still tingling from Mr. Lowe's famous declaration as to Government aid to science, part of the proceedings in the Corps Législatif on Thursday last must have been very tantalising. Chapter 29 of the Budget contained an item of 800*l*. to assist Captain Gustave Lambert's expedition for the discovery of the North Pole. M. Stephen Liégeard moved that the grant should be 4,000*l*., and with the help of a few words from M. Picard his amendment was carried. To be sure it was the day before war was declared. M. Liégeard pressed a good many points into a brief speech. Since 1818 it appears there have been forty-two expeditions in search of this famous mathematical entity, all of which have failed. The three modes of search now urged are—that of Captain Sherard Osborn, by sledges over the frozen ocean (but, says M. Liégeard, what was thought to be a crust of ice is an open sea); the proposal of Augustus Petermann, of Gotha, to follow the route between Spitzbergen and Nova Zembla; and Captain Lambert's, to try the passage through Behring's Straits, which Cook would have done, "had he not fallen under the hatchet of the savages of the Sandwich Islands." It appears further that in the polar region, so fatal to explorers, "there are rich products to conquer—whale oil, fossil ivory, gold, copper, coal, and everything else that 800,000,000 of still unexplored polar hectares can conceal." Captain Lambert has held 200 meetings on this subject, and collected 16,000*l*., besides subscriptions from chambers of commerce, learned societies, the Emperor (2,000*l*.), and others. The exploring vessel, the *Boréal*, is ready; Captain Lambert, "an intelligent man, full of faith and courage, with a constitution of iron," will leave Paris on the 1st of February next, and in the month of August, 1871, so M. Liégeard says, he will—perhaps to the cry of Ohe! Lambert—"plant the French flag on the prolongation of the terrestrial axis." As for Captain Lambert, we do not wish to damp his courage or rust his constitution, but Voltaire wrote a curious prophetic sentence on the 30th of June, 1760, in a letter to Thiriot;—"Il vaut mieux attendre tout du temps en France que d'aller chercher l'ennui et le malheur sous le pôle."

AT a meeting of the Council of the Society of Arts, held on the 8th inst., M. de Lesseps received from the Prince of Wales the Albert gold medal for "services rendered to arts, manufactures, and commerce by the realisation of the Suez Canal."

THE testimonial to Professor Morris was presented on Thursday last by Sir R. I. Murchison in the rooms of the Geological Society, Somerset House.

L'ABBE MOIGNO makes an appeal in *Les Mondes* on behalf of the Niépce de Saint-Victor subscription, which at present hardly exceeds 400 francs; the widow is in great distress and wants the means for educating her two sons. As his inventions were of equal importance to French and English photographers, it seems fitting that England should bear her share in recognising his services. We shall be happy to receive subscriptions.

THE editorial chair of the *British Medical Journal* is about to become vacant. Candidates are to forward their application to the President of the Council, W. D. Husband, Esq., York, on or before the 30th inst.

IN reference to the recent appointment of Dr. A. R. Simpson to his late uncle's chair of midwifery in the University of Edinburgh, we have received a card with the following statements:—(1) That he had never given a course of lectures on midwifery, or on any other subject; (2) That he had never had a pupil (in the proper sense of the word); (3) That he had never held an hospital appointment; (4) That he had never sent in a single testimonial of his fitness for the chair to which he had been appointed; (5) That he had never written or published any paper that attracted considerable notice. The correctness of these facts being assumed, we can well understand the indignation of the faculty in London at the appointment, especially as one of the most eminent practitioners of this branch of surgery, Dr. Matthews Duncan, was a candidate. The time seems to have arrived when the appointment to Professorships in Scotch Universities should be placed in more competent hands than the Town Council. At the recent election the appointment rested with seven electors, four of them members of the Town Council, three men of distinguished literary and scientific position. The three latter voted for Dr. Duncan, the four baillies for Dr. Simpson, thus carrying the election by a majority of one.

M. CH. LE MAOUT calls attention in two recent numbers of *Cosmos*, to the popular belief that constant firing of guns has a tendency to bring down a heavy fall of rain, a belief which appears to be supported by the experience of the last Austro-Prussian war, the siege of Antwerp, and the battle of Solferino. M. Le Maout suggests that a series of experiments to test the soundness of the theory should be tried at the fortifications of Cherbourg, and at Brest, under opposite hygrometrical conditions, in the one case during a moist south, in the other during a dry east wind.

THE Acclimatisation Gardens in the Bois de Boulogne have just received from Java a fine female Orang, three years old, and larger than any that has hitherto been brought to Europe. It is described as extremely docile, and possessed of such a strong sense of propriety as to wash its bust, arms, and shoulders with soap and water whenever it feels to need it. It is allowed a walk every morning in the gardens, when it displays great activity in climbing to the top of the tallest trees. Its height when standing upright is about that of a child five years old.

THE Government of Nicaragua is trying to develop the waters of the Lake of Nejapa, which are reported to be efficacious in syphilitic affections of the system. The official report on the specimens sent to Paris states that the water is turbid, and contains a quantity of matter in suspension, having the smell of rotten eggs, due to sulphuretted hydrogen. The taste is alkaline. Such is the condition of the specimens in Paris, but in its natural situation the water is inoffensive in odour. The present analysis is therefore considered as capable of modification. The qualitative analysis gives us *bases*, magnesia, soda, potass, lime, iron, *acids*, sulphuric, hydrochloric, carbonic. The quantitative analysis states that 500 grains evaporated gave a residuum of 4·15 grains, which in treatment left, as the contents of each 500 grains of water

Bicarbonate of soda	1·90
,, potass	2·40
Sulphate of magnesia	0·55
Chloride of sodium	0·05
Sulphurets of calcium, iron, &c.	1·25

Although the analysis admits of correction with better specimens, it places the Nejapa water in the alkaline class with those of Vichy and Vals.

MR. G. H. HURLBUT, an American, has been appointed engineer to the Government of the United States of Columbia at a salary of 480*l*. per annum.

A LARGE public swimming-bath is proposed at Calcutta, and meets with support.

IN Bolivia there is great excitement in consequence of the discovery of rich silver mines in the Sierra del Limon Verde, fifteen miles from the small settlement of Calama, and seventy-five miles from the shore, in the maritime prefecture of Cobija. In a short time 150 mining licenses had been taken out at the prefecture, and there was a great rush from Cobija.

THE Queensland Acclimatisation Society have sent a parcel of seeds to the Peruvian Government. These have been placed in the Lima Botanic Garden, but are said tobe in bad condition. The Peruvian Government has communicated its thanks to H. B. M. Minister, and has directed a corresponding gift to be sent to the Queensland Society. The transaction has had a very good effect.

ANOTHER coal district has been discovered in India by Mr. W. T. Blanford, F.G.S., in the bed of the Hasdo river, just below the village of Korba, in the Bilaspore district. Mr. Blanford is of opinion that the seam is favourable for working, and that it surpasses the Chanda coal, and is in portions equal to that of Raneegunge.

IRON has been rediscovered in Gwalior in large quantities. It was formerly worked, but abandoned on account of the scarcity of fuel.

AT Chicholi in the Central Provinces of India, a vein of silver has been discovered yielding on assay 90z. 19dwts. 6grs. of silver to the ton of ore.

THE Observing Astronomical Society entered upon the second year of its existence on July 1st. The recent election of officers for the ensuing year has resulted tn the re-election of the former president, treasurer, and secretary and committee. The Rev. R. E. Hooppell, M.A., L.L.D., F.R.A.S., is the president; Mr. William F. Denning, the treasurer and secretary; and the following are the members of the committee: Messrs. S. P. Barkas, F.G.S.,; James Cook, A. W. Blacklock, M.B., H. Michell, Whitley, and Albert P. Holden. The society numbers forty-six members, and was formed for the purpose of aiding the spread of practical astronomy.

THE most recently published part of Martius's "Flora Brasiliensis" is an important one, comprising the ferns of Brazil (orders *Cyatheaceæ* and *Polypodiaceæ*) by Mr. J. G. Baker of the Kew Herbarium. Mr. Baker makes about 250 species; it is to be regretted that at the same time M. Fée had been working at Brazilian ferns from the very same materials in his *Cryptogamie vasculaire du Brésil*, which we have just received; and, following out the practice too much in vogue among Continental botanists, had made a distinct species of almost every slightly different form, thus enormously multiplying their number; his names will claim priority of publication over Mr. Baker's by a few months. The part is illustrated by fifty splendid plates, twenty of them nature-printed by Ettingshausen of Vienna, the remainder representing details of structure and fructification of every sub-genus, large plates of fifteen new and interesting species, and sections of trunks of the arborescent kinds. The *Hymenophyllaceæ*, *Gleicheniaceæ*, and other small orders by Sturm have been published some years; the *Isoetaceæ* and *Equisetaceæ*, by A. Braun, to be published very shortly, comprising only a small number of species, will complete the volume.

THE preparations for the New York Industrial Exhibition are making rapid progress, but it is not expected that it will be pened before the spring of 1872.

THE HARVEIAN ORATION

THE following extracts from Dr. Gull's Harveian oration, delivered at the Royal College of Physicians, on June 24th, form a fitting sequel to the researches of Dr. H. C. Bastian, which we reccently published, on the Spontaneous Generation of Living Things :—

"If it is ascertained beyond all doubt that, in respect of its materials, a living body contains no more than it has received; that, however strange and mysterious its organs and their functions, the warp and woof are of substances with which we are acquainted under simpler conditions, cannot the same be maintained of the forces it exhibits? . . . . It may be objected that there is lurking a kind of *petitio principii* in the supposed relations of simpler forces to their higher forms; that for the conversion of the former into the latter it is necessary to postulate material conditions of a certain kind, and that for the organic conversion we must begin with a living body or its germ; that the boast of the physiologist is like the boast of Archimedes. If he wanted a ποῦ στῶ, they require germs or ova and a living body. But it is clear that such an objection has no weight as in favour of a vital force which is not material, since it is abundantly proved that, whatever be the conditions required, they do not generate any power, but only vary the form of it. They who maintain the hypothesis of a separate vital force, independent of the ordinary forces of nature, and which has no essential relation to them, do, by the very terms of the hypothesis, assume that the phenomena in living things are out of the proper range of science, and they consign us to a perpetual mental inactivity and ignorance in that region of knowledge in which above all others man is interested. . . . . An hypothesis, like that of a separate vital principle, which demands so much, which stops inquiry at once, making progress impossible, by removing the steps by which it could ascend, should at least have the highest sanction of our intellect. . . . The dogma 'omne vivum ex ovo,' for the truth of which Harvey so justly contended against the fanciful notions of his age, cannot perhaps be now maintained in its integrity. Whether, to use an expression of that day, living things are ever produced automatically—that is, *de novo*—through putrefaction or otherwise, is, like the question of the limitation or universality of the germ power, still a matter upon which opinion is divided; and as it is my duty on this occasion to exhort you to investigate nature by way of experiment, I must ask you not readily to accept negative conclusions which impose limits where none may really exist. . . . . The time is passing in which the human mind can remain satisfied to rest under the fetters it has imposed upon itself, or to cherish its own phantasms, as if its very existence depended upon them. 'Man knows only what he has observed of the course of nature' is the notorious dictum of science, showing the limit and the mode of the acquirement of our knowledge: the limit as wide as nature itself; and the mode is but readiness to be taught. Notwithstanding, therefore, the adverse decision of schools and dogmas, science still occupies itself with the possibilities of occasional automatic generation. And that it should be so, let it not raise antagonism in the minds of those whose pursuits (inquiries) lie in another direction, since the infinity of nature may well include facts which at first seem to be antagonistic. . . . . We have lately been rather blamed for not gratefully accepting the germ theory of disease; but to this college the theory is not new, and, I think I may add, has not been proved to be true. It will be in the remembrance of many present that in the year 1849 a theory was put forth that epidemic cholera was due to fungi and their germs. Peculiar bodies, it was said, had been found in the rice-water evacuations, and also in the air and drinking waters of the infected localities. It was confidently asserted that we had substantial facts in support of the theory, and that it fulfilled the conditions required of being both true and sufficient. This college thought the subject of such moment that a sub-committee was formed from the Cholera Committee of that day for its investigation. The drinking water of infected places was examined, the air of rooms in which cholera patients were dying was condensed, that it might afford whatever floated in it for examination; dust was collected from cobwebs, window-frames, books, surfaces of exposed food, and every imaginable place, to try it for cholera germs. . . . The supposed germs, when really germs (for many shapes had been included in the supposed direful growth), were found to be spores of known harmless fungi and confervæ, of which, if even the startling number of thirty-seven and a half millions should be contained in about two drachms of water, as quoted by Tyndall, from Mr. Dancer's examination,* it is probable that the whole or repeated units of such millions might be harmlessly swallowed. But for the most part the supposed germs were not germs of any kind, but broken scraps of vegetable and animal tissues, spiral vessels from dried horse-dung, hairs, wings, and legs of insects, detrita of dress, and the like. The results were, in fact, entirely negative of any peculiar bodies to which the epidemic disease could be referred. One general result arrived at at that time, however, agrees with the observation of Tyndall in his recent investigation of dust by a beam of light—viz., that the floating particles in the air are chiefly of an organic nature. This we might have been prepared for, from the specific weight of dried organic material, enabling such dust to float, when the heavier inorganic substances would be deposited. That the infectious diseases spread by emanations from the sick, must have been long known, and that such emanations are of a solid nature, we may infer from the fact that they may be dried and conveyed from place to place; but in what state, whether as amorphous material or as germs, we know no more to-day than was known a thousand years past. No new fact bearing upon the propagation of contagious disease has been reached by the recent investigations on dust; nor can we infer the nature of summer catarrh because the nasal mucus under such circumstances and at no other time, was found peopled by vibriones, since decomposing mucus is always populous with this common race of infusoria. The phenomena of fermentation and putrefaction in dead and decomposing substances afford no explanation of the changes observed in a living body in a fever process. The purulent matter produced in small-pox, is not, as we know, in any way comparable to the yeast formed in fermenting fluids. On the contrary, the microscope demonstrates that the forms, as for instance in variolous pus, are not different from those contained in other purulent and innocuous exudations. Nor have we any reason to conclude that any forms which are observed are germs which convey the disease. It is to be regretted that a confusion in terms has been made. Instead of dust and disease it ought rather to have been dust and putrefaction, or dust and fermentation, since the relation of dust to disease has not been revealed anywhere in the inquiry. That the air conveys the material causes of the infectious diseases from the sick to the healthy, is a notorious fact, which had equal force before these inquiries were instituted, though, owing to the exigencies of social intercourse, a fact more neglected than in times of comparative ignorance. It is difficult to vindicate exactness in progress without seeming to be at the same time a hinderer of it. The onward and the regulating forces of a machine, though not incompatible, but necessary, require the nicest balance. This reflection suggests itself by the way the spread of infectious diseases has been handled. The theories it has given rise to have been so easily put forward as to thereby create distrust. But the spirit of science is no favourer of negations. 'Der Geist der stets verneint' finds no greater friend in medicine than in theology. Still it will be admitted that no progress can be made by the ready acceptance of every proposition, however distinguished the source from which it emanates. The parasitic origin and nature of epidemics *may* be true, but it has yet to be proved. As an hypothesis, it admits of proof or disproof, and so has further claim upon the industry of those who have put it forward as a suggestion. Without going to the length which this hypothesis demands, we must admit, however, that we know enough to uide us much further than we have yet gone in the practice of prevention."

* Proceedings of the Royal Institution of Great Britain, Jan. 21, 1870.

PROFESSOR TYNDALL'S LECTURES AT THE ROYAL INSTITUTION, ON ELECTRICAL PHENOMENA AND THEORIES

PROF. Tyndall completed a short course of seven lectures at the Royal Institution, on Thursday, June 9th, upon "Electrical Phenomena and Theories," which were made as interesting as all his lectures are, by the ingenuity and completeness of the experimental illustrations; in this particular case the apparatus of his distinguished predecessor, Faraday, being largely drawn up, in addition to considerable accessions of more recent date, many of them derived from the kind help of individuals who have made themselves high reputations in the various branches of Electrical Science. The scope of the Professor's demonstrations covered the entire range of Electrical and Magnetic Science, commencing with the phenomena of voltaic electricity, and passing through the various leading manifestations and peculiarities of electro-magnetism, magnetic force, frictional electricity, electro-chemistry, magneto-electricity, and, of course, electric telegraphy, and the relations of electric motive force to heat.

One remarkable peculiarity in these lectures of Professor Tyndall is, the effective way in which several of the more subtle effects of electrical change and power are made manifest to a large audience by the instrumentality of beams of electric light, manipulated in various ways. Thus, for instance, the elongation of a solid bar of iron, when it is thrown into the magnetic state, by being encircled in the folds of a voltaic current, conveyed by a helix, is shown by the starting of a spot of light, some six or eight inches upon a screen, when the molecular condition of magnetism is excited by the passage of the current. A beam of light falls upon a small mirror, carried at the extremities of the arm of a lever, so resting upon the end of the iron bar, that when the lever is lifted by the magnetic elongation of the bar, the beam of light is shot off from the mirror as a long weightless index. The change in the position of the molecules of iron by the action of magnetism is also proved by throwing the beam through a vertical cell of glass, containing magnetic oxide of iron suspended in water. When the cell is exposed to the influence of the poles of a strong electro-magnetic, the light passing through the cell and contained liquid to a screen beyond, brightens, in consequence of the metallic molecules turning themselves "end on" to the incidence of the beam. The lines of magnetic force assumed, when iron filings are sprinkled over the poles of a magnet, are portrayed by the intervention of a system of lenses, which depicts the image upon the screen. The formation of the "tree of lead" upon the negative electrode of a voltaic current, when a salt of lead is decomposed by the current, is shown in the same way; the arborescent crystals glowing and dissolving alternately on the opposite poles, immersed in the solution as the direction of the current is reversed. The very beautiful colours and patterns of Nohili's rings, formed when lead is thrown down by voltaic decomposition upon a polished plate of steel, are exhibited by a similar intervention of lenses, and the illumination from the electric beam. An artificial telegraph cable, whose resistance to the transmission of the electric current is made identical with 14,000 miles of an actual marine cable, is formed by introducing into the path of the current gaps, consisting of feebly conducting liquids and condensers, so distributed as to represent the respective distances by telegraphic route of Gibraltar, Malta, Suez, Aden, Bombay, Calcutta, Rangoon, Singapore, Java, and Australia. A mirror, belonging to each gap, lies in the path of the currents, carried by a galvanometer, constrained to deflect its needle from the position of both on the instant that the passage of the current is felt. Before the current is sent through the apparatus, ten dots of light, cast from the mirrors by the instrumentality of electric illumination, lie upon the screen, in a straight vertical range. When the current is passed through the apparatus, dot after dot starts aside upon the screen, as the current fills the condenser immediately before each mirror, and then flows beyond to deflect the galvanometer immediately in advance. The deflection of the successive galvanometers, and the corresponding traverse of the beam of light upon the screen, is seen, under this arrangement, to take place at successive steps or intervals, which exactly express the intervals of time which the electric current would require to reach the several stations named, in the actual progress of telegraphy. The starting aside of spot after spot upon the screen when the current is sent through the apparatus, and the subsequent return of spot after spot to the position of original test in inverse order, forms a very striking illustration of the fact that the resistance of an electric cable is in some degree dependent upon its length, and that time is consumed in overcoming this resistance. The most interesting and telling of all these beam-of-light illustrations, however, is certainly the one which is employed to indicate the excitement of diamagnetic force in a tube of copper, when it is suspended between the poles of an electro-magnetic. The tube is carried by a string of silk, and rotates rapidly under the influence of a twist given to the string. The string also carries above the tube a series of small mirrors, which reflect the light of an electric beam, so that a continuous elliptical band of illumination is formed on the screen whilst the twisting is continued. The instant the electro-magnet is made active by the transmission of the current through its helix, the copper tube acquires diamagnetic polarity by induction, and under the influence of this polarity the rotation is arrested, and the band of lights upon the screen is changed into a small stationary spot of illumination. When the electro-magnet is unmade by the arrest of the voltaic current, the spot of light again becomes an elliptical band, under the resumption of the twisting of the silk string with its mirrors and copper tube.

Of the numerous other very pleasing and telling illustrations exhibited in these lectures, space only permits allusion to be made to a very few which have been selected from the series, as being worthy of especial mention. The sound produced by the molecular vibration in iron when its mass is transiently magnetised by the voltaic current, is made audible by suspending an iron poker upon two sounding boards, and making it the core of a helix, conveying an electric current. An assistant is converted into an extemporised electrophorus, by flapping his black coat with fur while he stands upon a glass-legged stool. Small fish of gold leaf are made to float in the air current given off from the knob of a charged Leyden jar. A thick drinking glass is shattered by the expansion of the water contained in it, when sparks formed under the intensifying power of fifty condensers joined "in cascade," and primarily charged by a voltaic battery of one thousand cells, are passed through the liquid. To demonstrate the relation of resistance to heating power, a long line of wire is arranged in alternate links of platinum and silver, and when a voltaic current of due intensity is passed through the length, each stretch of the platinum wire is seen to glow with brilliant red heat, while the stretches of silver wire between remain still invisible. A beautiful series of Geissler's vacuum tubes were brought into successive operation, in which the auroral discharge was broken into stratified leaves, in which the glow was extinguished by the approximation of the poles of an electro-magnet, in which a feeble glow was converted into bright stratified light by the influence of a magnet; and beautiful beyond all the rest, the light from the enclosed negative terminal of the voltaic battery was arranged into the well-known lines of magnetic force, when subjected to the influence of the poles of a magnet.

It would be unnecessary in alluding to these very admirable lectures, to say one word of Prof. Tyndall's clearness and power as an expositor of the phenomena of Physical Science. These are now well known to the hundreds who are attracted to Albemarle Street on the frequently renewed occasions when the Professor performs this portion of his functions as a lecturer of the Royal Institution. It is, however, well worth while to draw attention to a device which the Professor adopts, with the happiest effect, to render his lectures as complete in their instruction as they can be made. He prints a series of well-digested "Notes" of the entire range of the subjects he passes over in each lecture, has them placed in the hands of each individual as he enters the lecture room, and then refers from time to time to the systematic outline, as occasion suggests the expediency of doing so. By this procedure the Professor is able to give full attention and time to each step of his illustrative demonstration, without being hampered with the need of telling everything that he has marked out beforehand—an extremely difficult thing to accomplish in a brief unextensible interval, where *viva voce* teaching has to be employed. Under this management any broken or omitted link in the full argument is readily recovered by glancing the eye over the range of printed notes after the conclusion of the lecture. This plan is well worthy of adoption, wherever popular lectures upon science are delivered to large audiences, with a view to instruction as well as amusement.

ZOOLOGY

Plateau on the Flight of Coleoptera

M. Felix Plateau has supplemented the recent labours of Marey and others upon the flight of insects by examining the movements of the wings of certain Coleoptera. Specimens of the common May-beetle and *Oryctes nasicornis* were selected for experiment. The apparatus used consisted of two pulleys, fastened one above the other, at a distance of two centimetres, on a vertical support; the upper pulley made twelve turns for each one made by the lower, and could be caused to rotate twenty-four times in a second. The insects were killed by ether vapour immediately before each experiment; and the wings could be fastened, by a simple contrivance, to the front prolongation of the axis of the upper pulley.

A wing, in its folded state, was fixed on the instrument in such a manner that its plane made, with the plane of rotation, an angle of 45°, as in the living animal. On turning the pulleys, it struck the air obliquely by its upper surface and front margin; but the small diameter of the apparently continuous revolving disc (as indicated by a graduated scale) proved that the wing was still folded, and that centrifugal force had not affected it. When rotation was produced in an opposite direction, so that the wing struck the air both by its posterior membranous margin and inferior surface, the increasing diameter of the disc gave proof of the expansion of the wing, which, indeed, continued to be much extended when motion was arrested. When the plane of a wing was perpendicular to the plane of rotation, and the revolution of the wheel was such that the wing struck the air by its dorsal or upper surface, no extension ensued; when it struck by its lower surface, only partial extension followed. Now the oblique, not the perpendicular plane is that chosen by nature, and is, as has been seen, much more favourable for flight.

On fixing an open wing on the axis so as to make an angle with the plane of rotation, and turning in one direction, the wing remained open; on reversing the direction (*i.e.* acting on the upper surface) it became partially closed.

SCIENTIFIC SERIALS

In the *Revue des Cours Scientifiques* for July 9, we have an important article on the Axioms of Geometry, by Prof. Helmholtz, which has, however, already appeared in an English form in the *Academy*, and the translation of an article by Mr. E. J. Reed, on *Navires blindés*; while M. Bernard concludes his course on Suffocation by the Fumes of Charcoal. In the number for July 16 there is an address by M. Dumas, delivered before the Academy of Sciences in honour of M. Pelouze, which occupies nearly the whole of the number, leaving room for only a short abstract of M. Bienaymés paper read before the Academy, on the military mortality in the Italian campaign of 1859-60.

Annales de Chimie et de Physique, May, 1870.—"Researches on the Gaseous Products of the Combustion of Coal," by M. A. Scheurer-Kestner. This important paper commences with an historical notice of experiments on this subject by Péclet, Ebelmen, Debette, Commines de Marsilly, Ebelmen and Sauvage, Foucou and Amiguis, and Cailleet, pointing out several causes of inaccuracy which are to be traced in their researches. The author then describes the process employed by him in collecting and analysing the gases from the flue of a steam boiler, Through the brickwork of the furnace a hole was bored, and in it was placed a platinum tube about 700 millimetres long and ten in diameter. To one end of this tube a copper pipe surrounded by a Liebig's condenser is soldered, the other end being closed with a plug. A narrow slit extends the whole length of the platinum tube, so that the air drawn through it is an average specimen of the gas in the flue. It is found also that the gas must be slowly aspirated through the slit in order to obtain a fair average of the gas passing through the flue during a considerable space of time. The author connected the apparatus with a water aspirator, by which he drew $\frac{1}{1000}$ of the total gas which passed up the chimney through the platinum tube; at the same time, from $\frac{1}{250}$ to $\frac{1}{500}$ of the aspirated gas was removed by a branch and collected in a bottle of three litres capacity, from which mercury was allowed to flow very slowly. Thus about $\frac{1}{500000}$ of the gas in the chimney was collected over mercury, and with this the analyses were performed. For the analytical processes we must refer the reader to the original paper, merely pointing out the conclusions at which the author has arrived. The gases of the chimneys were almost always found to contain carbonic oxide and hydrocarbons, even in the presence of oxygen arising from excess of atmospheric air. It was also found that the quantity of carbon lost in the form of smoke in the presence of sufficient air was about $\frac{1}{2}$ per cent., and that the loss of carbon as combustible gases does not exceed 2 or 3 per cent. when the excess of air amounts to 30 per cent. or more. The paper concludes with a section on the theory of the formation of smoke in the presence of an insufficient quantity of air, in which the author discusses the observations of Sainte-Claire Deville on dissociation, and of Berthelot on the action of heat on hydrocarbons, and points out their application to this subject.

To the number for June, 1870, M. P. P. Dehérain contributes an extremely interesting paper "On the Evaporation of Water and the Decomposition of Carbonic Acid by the Leaves of Vegetables." After mentioning the works of previous investigators, he describes the process adopted and the results obtained in his experiments. The leaves were surrounded by a very light flask with a wide and short neck, or by an ordinary test tube in the case of long and narrow leaves, like those of graminiferous plants. The difference between the weight of the globe or tube before and after the experiment gave the quantity of water which was condensed from the leaves. The author finds that the evaporation takes place quite readily, even when the atmosphere surrounding the leaves is saturated with moisture. Different species of plants evolve very different quantities of water under similar conditions, and the proportion of water seems to increase as the size of the leaf diminishes. Thus large leaves of colza evolved between 1 and 2 per cent. of their weight of water in an hour; smaller leaves 11 or 12 per cent., leaves of wheat between 70 and 90 per cent., and of rye between 90 and 100 per cent. In direct sunlight the water evaporated very much exceeds that emitted in diffuse light and in perfect darkness. Thus barley evolved 74·2 per cent. in direct sunlight, 18·0 per cent. in diffuse light, and only 2·3 per cent. in the dark. Numerous experiments are cited to show that this is not caused by heat alone, for when the tube was surrounded with cold water, or even with melting ice, the quantity of water collected was increased, doubtless owing to the more complete condensation of the water. Another series of experiments was made to determine if light of different colours had any influence on the amount of water excreted; and it was observed that yellow light, which also produces more rapid decomposition of carbonic anhydride under the influence of green leaves, is the most favourable to the evolution of water. It has recently been asserted by M. Prillieux that the variations observed when experimenting on the decomposition of carbonic anhydride by green leaves under the influence of light are influenced more by the intensity of the light than by its colour. M. Dehérain has performed numerous experiments with liquids of different colours, but of the same transparency, and shows that the yellow light is the most energetic, then follow red, blue, and finally green.—"On the Determination of Graphite in Cast Iron and Steel." By M. Boussingault. The author treats the iron with corrosive sublimate mixed with water, and heats the residue to volatilise the mercurous chloride. The black substance which is left consists of graphite, amorphous carbon, and silica. This is then heated in the air, the amorphous carbon burns off, and the loss of weight indicates the quantity of carbon in combination with the iron; the residue is next heated in oxygen, so as to cause the combustion of the graphite, which is likewise determined by loss. When the metal is dissolved in hydrochloric acid, the combined carbon is evolved in combination with hydrogen, and part of the silicon present passes into solution.

SOCIETIES AND ACADEMIES

Geological Society, June 22.—Mr. Joseph Prestwich, F.R.S., president, in the chair. Mr. Horace Pearce, 21, Hagley Road, Stourbridge, and Mr. Samuel Spruce, of Tamworth, were elected Fellows of the Society.

1. "Notes on the Lower portion of the Green-slates and Porphyries of the Lake District between Ulleswater and Keswick." By Dr. H. Alleyne Nicholson, F.R.S.E., F.G.S., lecturer on Natural History in the Medical School of Edinburgh. The author describes the characters presented by the lower part of that series of rocks, named by Professor Sedgwick the "Green-slates and Porphyries," which overlie the Skiddaw Slates in the Lake District. He notices the sections of this series in Borrowdale, on the east side of Derwentwater, between Keswick and the Vale of St. John, in the Vale of St. John, in Matterdale, in Eycott Hill, between Ulleswater and Haweswater, and in the neighbourhood of Shap. In the Borrowdale section the sequence of the rocks is given by the author as follows:—Resting on the Skiddaw slates there are (1) a felspathic trap; (2) a great series of ashes, breccias, and amygdaloids, often showing slaty cleavage and worked as slates, but with several intercalated bands of trap; and (3) a second trap. This appears to be a normal section, and is repeated, but diversified by the results of folding and faults in the other localities described by the author, except that in the Vale of St. John the true slaty series seems to be entirely wanting.

2. "Observations on some Vegetable Fossils from Victoria." By Dr. Ferdinand von Müller and Mr. R. Brough Smyth, F.G.S. Mr. Smyth stated that the fossils, of which specimens were forwarded by him, were obtained in one of the deep leads at Haddon, near Smythesdale. No leaves have been obtained from the bed, which consists of a greyish-black clay; the fruits and seed-vessels were obtained about 180 feet from the surface, and represent a flora not very dissimilar to that now characterising some parts of Queensland. The specimens sent include the fruits of a supposed new genus of Coniferæ, described by Dr. von Müller under the name of *Spondylostrobus*. It is most nearly allied to *Solenostrobus*, Bowerbank, but its five valves are not keeled. The columella forms the main body of the fruit; and the seeds are apparently solitary. The species was named *Spondylostrobus smythii*. The remaining specimens consisted of a solitary fruit of a genus of Verbenaceæ; an indehiscent compressed fruit, probably belonging to the Proteaceous genus *Helicia*; a nut nearly allied to the preceding; a large, spherical, unilocular, 3-seeded nut with a thick pericarp, perhaps from a Capparideous plant; a 5-valved capsule of an unknown genus; and fruit-valves of three other plants, probably belonging to the Sapindaceæ, and perhaps allied to *Cupania*. One of the last may belong to the Meliciaceous genus *Dysoxylon*. Dr. Müller considered that these remains indicate a former flora analogous to that of the existing forests-belt of Eastern Australia.

3. "Note on some Plesiosaurian Remains obtained by Mr. J. C. Mansel, F.G.S., in Kimmeridge Bay, Dorset." By Mr. J. W. Hulke, F.R.S., F.G.S. The remains described in this note represent two new species of *Plesiosaurus*. The dorsal vertebræ of the first species are distinguished by extremely short centra, with hollow articular faces. The antero-posterior diameter of 4 centra ranges between 1 and 1·3 inch, the transverse horizontal diameter between 4 and 4·6, and the vertical between 3·8 and 4 inches. For this Plesiosaur the author proposes the specific name of *P. brachistospondylus*. The other species, of which the greater part of the spinal column and portions of the breast and pelvic girdles and limbs are preserved, is a long slender-necked Plesiosaur exceeding 16 feet in length. Its limbs are much larger in proportion to the whole length than in the typical Liassic forms of this genus; but what particularly distinguishes it from these are the massiveness of the humerus and femur, the longer size of the wing-like expansion of the postaxial border, a well-developed trochanter, and especially three articular facets at the distal end of the femur, corresponding to which the second segment of the paddle, representing the leg, contains three coequal bones. The author noticed the impression of a third bone in this segment in the matrix, in which a paddle of *Pliosarurus portlandicus* is imbedded, and the ossicle on the postaxial border of the fibula in *Plesiosaurus rugosus*. He compared the paddle-bones of the Kimmeridge *Plesiosaurus* with those of *Ichthyosauri* and of the Liassic Plesiosaurs and of *Pliosaurus*, he drew attention to the very close resemblance of the humerus and femur to type specimens of the femora of *Pliosaurus brachydeirus* and *P. trochanterius* in the British Museum, and traced a similar resemblance between the elements of the cnemion and tarsus, and those of the Dorchester and Portland Pliosaurian paddles. For this creature, combining a long truly Plesiosaurian neck with Pliosaurian-like limbs, the author proposed the name of *Plesiosaurus manselii*.

4. "Notes on the Geology of the Lofoten Islands." By the Rev. T. G. Bonney, M.A., F.G.S., Tutor of St. John's College, Cambridge. The author described the general appearance of the Lofoten islands, which have commonly been described as composed of granite, but which he stated really consist of gneissic rocks. The scenery of some of the islands, on which he did not land, resembled that of the Cambrian and Cambro-Silurian districts of Wales and Cumberland; and the interior of Hassel showed dark rounded fells, resembling in outline some of the softer Welsh slates. At Stokmarknæs and at Melbo there is a granitoid rock of pinkish-grey colour, consisting of felspar and platy hornblende, with some mica and quartz. The Svolvaer Fjeld in Ost Vaagö shows a distinctly bedded structure in the cliffs near Svolvaer, the *débris* at the foot of which consist of a rock resembling syenite, and a quartzite containing a little hornblende and felspar. Bedding was also observed towards the Oxnæs Fjord. The islets near this coast consisted chiefly of a granitoid rock resembling a syenite, showing traces of bedding to the west of Svolvaer. Seams and veins of quartz, hornblende,

&c., occurred in some of the islets, and these were sometimes too regular to be explained by deposition in fissures. Near the Svolvaer post-office there was gneiss coarsely foliated, containing hornblende and mica, with pink orthoclase felspar. The author concluded, from his observations, that, with few exceptions, the so-called granites of the Lofoten islands are stratified, highly metamorphosed rocks, quartzites, and gneiss, generally with much felspar in the latter, and with more or less hornblende in both, and that they are inferior in position to the gneiss and schists of the mainland, and to the more slaty rocks of the southern and western parts of the same islands. He compares them with some gneiss from Dalbeg on the west coast of the island of Lewis.

5. "On *Dorypterus Hofmanni*, Germar, from the Marl-slate of Midderidge, Durham." By Mr. Albany Hancock, F.L.S., and Mr. Richard Howse. Communicated by Prof. Huxley, F.R.S., F.G.S. The material for this paper consisted of four specimens of *Dorypterus Hofmanni*, which have been discovered by Mr. Joseph Duff, in the marl-slate of Midderidge, and are believed to be the first examples of this fish which have been obtained in this country. The stratum from which they were procured is the same as that described by Prof. Sedgwick in his paper published in the Transactions of this Society (2nd series, vol. iii. pp. 76, 77). The specimens showed that the "ribbon-shaped" process mentioned by Germar is part of a peculiar exoskeleton, and that *Dorypterus* possessed ventral fins, which were situated in front of the pectorals, or "jugular." Hitherto no fishes with ventral fins other than "abdominal" in position have been known to occur earlier than the Cretaceous epoch. The tail is heterocercal, not homocercal, as Germar supposed. The dentition is not displayed in any of the specimens, and the teeth were probably small and inconspicuous; but the general structure of the fish shows it to be most nearly allied to the Pycnodonts.

6. "Observations on Ice-marks in Newfoundland." By Staff Commander J. H. Kerr, R.N., F.R.G.S. Communicated by the Royal Geographical Society. The author describes and tabulates the grooves and scratches observed by him on rock-surfaces in various parts of Newfoundland, especially Conception Bay, the neighbourhood of St. John's, and the north of Bonavista Bay. From the diversity of the direction of the markings and other considerations, he considers that they must have been produced by glaciers, and he believes that the main features of the country were much the same as at present before the glaciation took place. The author thinks that the land has not been submerged since it was freed from its coating of ice.

7. "On the Glacial Phenomena of Western Lancashire and Cheshire." By Mr. C. E. De Rance, F.G.S. The author described the general form of the ground and the preglacial condition and glacial deposits of the districts of Wirral and Western Lancashire, and draws from his observations the following general conclusions. That before and at the commencement of the glacial epoch the north-west of England was more elevated above the sea-level than at present, but afterwards gradually subsided, during which process marine denudation produced the plains of Wirral and Western Lancashire. Part of the latter has since been covered with glacial deposits 200 feet thick. The valleys running in the strike of the Triassic strata appear to have been formed by subaërial agencies. It is probable that when the glacial epoch commenced the hilly country was covered with immense glaciers, or with an ice-sheet, which, as the land sunk, reached the sea. The *High-level lower Boulder-clay* was probably produced by this land-ice. The land continued subsiding until it stood 100 feet lower than at present, submerging the lowlands of Lancashire and Cheshire to a depth of rather less than 25 fathoms, the coast-line being surrounded by an ice-foot, which received on its surface quantities of pebbles and boulders from the lake-district. These, on the breaking up of the ice-foot, were spread over the lowlands, forming the *Low-level Lower Boulder-clay*. The climate then improved, although subsidence still continued, and the sandy and gravelly deposits of the middle drift were produced; these deposits, at whatever elevation they occur, having been found in shallow water during the constant subsidence of the coast-line. The surface of the middle Drift shows traces of what seems to have been subaërial erosion, leading to the supposition that the land must have risen and suffered denudation before that depression during which the Upper Boulder-clay was deposited, at which period the climate again became extremely cold, and fresh glaciers were formed. Before the elevation of the Upper Boulder-clay the climate was greatly ameliorated.

8. "On the Preglacial Deposits of Western Lancashire and Cheshire." By Mr. C. E. De Rance, F.G.S. The author believed that after the deposition of the Esker Drift the country rose to from 200 to 300 feet higher than at present; but in the course of this elevation there was a pause, during which denudation took place, and the low plains, now covered with peat-moss, came into existence. From the consideration of the present depths of the channel between Great Britain and Ireland, the author inferred that an elevation of 200 feet would have caused the coast-line to run from the Mull of Galloway to St. David's Head; and Ireland would have been so connected with Wales as to render possible the migration of mammals, plants, and of man himself. Glaciers probably still persisted in the lake-district during the whole of this period of elevation. During a subsequent subsidence drainage became greatly obstructed, peat was formed, the sea encroached upon the land and worked its way eastward over the sea-bottom of postglacial times, a movement yet in progress. Here and there sand has begun to blow, forming dunes.

9. "Observations on Modern Glacial Action in Canada." By the Rev. W. Bleasdell, M.A., Rector of Trenton. Communicated by Principal Dawson, F.R.S., F.G.S. The author described some phenomena of ice-transport observed in Canada, especially those produced by the flood, anchor, or pack-ice produced in the rapids of the Canadian rivers. To this he attributed the entire disappearance of Crab Island in the River St. Lawrence, near Cornwall. This island occupied about an acre and a half within the memory of men now living; it has now entirely disappeared, and the water above it is gradually deepening. The island, according to the author, has been carried away piecemeal by the action of miniature icebergs, floated off by a rise in the water produced by a dam of anchor-ice below.

10. "On an altered Clay-bed and Sections in Tideswell Dale, Derbyshire." By the Rev. J. M. Mello, M.A., F.G.S. The author describes the sequence of the rocks seen in a quarry in Tideswell Dale as follows:—Beneath a thin layer of surface-soil is a bed of Toadstone, containing concretionary balls, and much decomposed above; beneath this is Toadstone in large blocks of indefinite shape, very hard, dark-green, and apparently doleritic, nine or ten feet thick, passing downwards into a coarse and much decomposed bed, partly amygdaloid, partly vesicular, about one foot thick. Beneath the Toadstone rocks, and without any sharp line of demarcation, is a thick bed of indurated red clay, three yards in thickness, presenting a regularly prismatic-columnar structure, resting on a thin bed of greenish-yellow clay, containing fragments of limestone, which covers beds of good Derbyshire marbles containing corals. The author suggests that the columnar clay-bed may perhaps be a local development of that which forms partings in the limestone near Litton Tunnel.

BRIGHTON

Brighton and Sussex Natural History Society, *Microscopical Section*. June 23.—Mr. Glaisyer, vice-president, in the chair. The subject for the evening was *Infusoria*, by Mr. Wonfor. Every one is aware that if any vegetable or animal substance is placed in water, in a few days the water will be found full of minute organisms, to which the name *Infusoria* or infusion animalcules has been given; many forms, though at first figured and described as distinct species, are now proved to be the early stages of other animals; others have been classed among another group of animals, and a larger number arranged among plants. The class *Infusoria* is much more limited than at one time supposed to be; and further, an increased knowledge might prove that many more were only the early stages of other and higher types of life. Mr. Wonfor then proceeded to point out the nature of their substance, their modes of development, increase, and propagation. So widely were they distributed that scarcely anywhere could water be found which did not contain some *Infusoria*. Many would live only in fresh water, others in salt or brackish water, while others were only to be met with in water containing decomposing vegetable or animal substances. Hence, water contaminated by sewage matter always showed certain types. While some were only to be found in particular infusions, others were common to several. Their appearance, under certain conditions, had led to theories on spontaneous generation, a much debated and debateable point; but as the atmosphere, according to Tyndall and others, appeared to be full of germs, their sudden appearance under favourable conditions was not surprising. The water in which cut flowers were kept was sure to yield some sorts; in fact, he had obtained an abundant supply of one kind from some water in which mignonette had been only three days. The water in bird fountains and in water bottles, if not looked after and frequently changed, would be sure to contain *Infusoria*. The rest of the evening was spent in examining the different forms of *Infusoria* brought for exhibition. Before separating it was announced that the subject for the next meeting in July, would be the "eggs of *Articulata*," *i.e.* of insects, &c.

PARIS

Academy of Sciences, July 4.—M. Serret presented a report upon a memoir by M. Bouquet, on the theory of ultra-elliptical integrals.—M. E. Catalan presented some remarks upon M. Darboux's note on the surface of the centres of curvature of an algebraic surface.—M. Janin read a reply to the observations of M. H. Sainte-Claire Deville, upon the variation of temperature produced by the mixing of two liquids, in which he discussed the theory proposed by M. Deville, and maintained the correctness of his own theory, according to which, he stated, the elevation of temperature of mixtures of liquids may be explained and calculated.—M. H. Sainte-Claire Deville made a few remarks upon M. Janin's paper, and also presented a third memoir on the action of water upon iron and of hydrogen upon oxide of iron, the results of which he sums up as follows :—The increase of tension of the hydrogen formed by the contact of iron and aqueous vapour is a continuous phenomenon when the tension of the aqueous vapour is caused to vary progressively without any change in the temperature of the iron ; the tension of the hydrogen corresponding to an invariable tension of the aqueous vapour decreases continuously when the temperature is gradually increased ; and the same laws are followed in the inverse phenomenon of the reduction of oxide of iron by hydrogen.—A note on a property of Volta's condenser, by M. P. Volpicelli, was read.—Some magnetic observations made at Makerstown, and at Trevandrum, near Cape Comorin, by M. Broun, were communicated.—An extract from a letter from M. Legrand to M. Jamin, on Deluc's thermometers, was read, in which the author stated that the difference between the temperature of the blood given by Deluc, and that now admitted, was due to the difference of atmospheric pressure at which the thermometers were graduated.—A note by M. Amagat, on the compressibility and dilatation of gases, was communicated by M. Balard.—M. Delaunay presented a note by MM. Wolf and Rayet on the light of Winnecke's Comet (Comet I. 1870) in which the authors describe their observations on the very feeble spectrum of that comet.—M. Delaunay also presented a note on the pyramids of Villejuif and Juvisy, the extremities of the geodetic base of Picard and Cassini.—M. Chapelas communicated a note on the spring of 1870, in which he noticed the phenomena of temperature, the direction of the winds, and the amount of rain observed at Paris during the months of April, May, and June of the present year.—M. Daudin communicated a memoir relating chiefly to the drought of the present year, which he ascribed to the prevalence of north-west and north-east winds, caused by some phenomena occurring in the Arctic regions.—M. Janin presented, in the name of M. Fonvielle, a notice of solar halos ; and M. C. Sainte-Claire Deville a note by M. Grad on the climate of Alsace and the Vosges.—M. A. W. Hofmann presented a note on the isomers of the cyanuricæthers in reply to M. S. Cloëz.—A paper on the phosphoplatinic compounds, by M. P. Schützenberger was read, in which the author announced that he had separated the radicals from the chlorine compounds described by him in his former paper.—M. A. Béchamp communicated a paper on the carbonic and alcoholic fermentation of acetate of soda and oxalate of ammonia, in which he described the growth of microscopic vegetation in solutions of those salts, and the production of alcohol thereby, from which he inferred that the synthesis of alcohol is effected by the vegetation, although the constituents of alcohol may not be present in solution. He went further, and stated that the same vegetation produced the same effect even in distilled water !—M. Elie de Beaumont presented a note on the rocks traversed in forming the tunnel between Modane and Bardonnèche in the Western Alps, a distance of 12,220 metres (or nearly 8 miles). The paper includes a long catalogue of the rocks observed, with their depths, which will prove of great value to the geologist.—M. Descloizeaux presented a note by M. C. Velain on the position of the *Terebratula janitor* limestones in the Basses-Alpes ; he referred them to the Neocomian stage, of which he regarded them as the lowest portion.—M. Duchartre communicated a note by M. E. Prillieux, containing an account of some experiments upon the withering of plants, also a note by M. Cave on the generatory zone of the appendages of plants.—M. Chantran presented some interesting observations on the natural history of the Cray-fish, in which he described the mode of copulation of those crustaceans, their oviposition and their changes of skin. The last-mentioned phenomenon takes place fifteen times in the course of the three years during which the animals grow to their adult state.—A note by M. J. B. Noulet contained a statement that in the neighbourhood of Toulouse the house martins all build their nests in accordance with what M. Pouchet calls the old fashion, that is to say, with a small round entrance notched in the upper margin of the nest. The swallows (*H. rustica*), on the contrary, according to the author, build nests resembling those described by M. Pouchet, and M. Noulet evidently considers that the latter naturalist has mistaken the nests of one bird for those of the other. Two physiological papers were communicated ; one on the vitality of the vaccine virus, by M. Melsens ; the other on an unequal production and difference of composition of the milk in the two breasts of the same woman.

PHILADELPHIA

Academy of Natural Sciences, February 1.—Dr. Ruschenberger, president, in the chair. The following paper was presented for publication : "Note on the relations of Synocladia, King (1849), to the proposed genus of Septopora, Prout (1858)." By F. B. Meek and A. H. Worthen.

March 1.—Dr. Ruschenberger, president, in the chair. The following paper was presented for publication : "Descriptions of new species and genera of fossils from the Palæozoic Rocks of the Western States." By F. B. Meek and A. H. Worthen. Prof. Leidy directed attention to a specimen received from the Smithsonian Institution for examination, which he said was the upper two-thirds of the right humerus of one of the extinct giant sloths, and was obtained in Central America by Capt. J. M. Dow. It agrees so nearly in form, proportions, and size, with the corresponding portion of the arm-bone of the *Mylodon robustus* of Buenos Ayres, as described and figured by Prof. Owen, as to render it probable it may belong to the same species. The specimen is unworn, black, not petrified, has no adherent rock matrix, and looks as if it had been obtained from alluvial mud. The interior of the shaft presents a long wide cavity, which might be viewed as the medullary cavity were it not that all the known extinct giant sloths have the limb bones solid. There would perhaps have been less hesitation in deciding as to the character of the cavity, were it not that comparatively recently a reverse condition was observed in a bone where it would not have been anticipated. A short time ago Mr. James Orton, of Rochester, N.Y., submitted for examination a collection of bones from the valley of Quito, Ecuador, S.A. The specimens were obtained at an altitude of 10,000 feet, and from Mr. Orton's account, were imbedded in a cliff of unstratified silt 400 feet in height. Among the bones, besides those of horses, lamas, &c., there was the femur apparently of a Mastodon, but solid or devoid of a medullary cavity. If the hollow interior be the natural condition of the *Mylodon*-like humerus under inspection, it would not belong to *Mylodon robustus*. Independently of the cavity indicated, the bone is sufficiently different in size and form to indicate a different species from the *Mylodon Harlani* of North America. The humerus from Oregon, described by Perkins (Am. Jour. Sci. 1841, xlii. 136), and referred to the latter by Prof. Owen, is not only much larger, but it is of greater breadth in relation with its antero-posterior diameter. The fragment of a humerus from Big-Bone-Lick, Kentucky, represented in fig. 3, plate xiv. of my "Memoir on the Extinct Sloth Tribe," is somewhat smaller than the corresponding part of the Oregon specimen, and is more compressed or wider in comparison with the antero-posterior diameter. Prof. Leidy further observed that there appeared to be a point of some significance in the anatomy of the mandible of *Dromatherium sylvestre* worthy of attention, though the appearance may turn out to be a deceptive one. Prof. Emmons had discovered three isolated rami of mandibles of this most ancient of American mammals in the triassic coal of North Carolina. Of the specimens, one is represented in fig. 66 of Emmon's American Geology, repeated in outline in fig. 650 of Dana's Geology. Another specimen Prof. Emmons presented to the Academy, and is contained in our museum. The point of interest to which reference is made is the apparent absence of a condyle. This process may have been lost, but in the two specimens seen by Prof. Leidy—that figured by Prof. Emmons, and that preserved in our museum—a separation of the process is not obvious.

March 8.—Dr. Carson, vice-president, in the chair. Prof. Leidy made the following remarks:—The reptilian remains from the cretaceous formation near Fort Wallace, Kansas, presented to the Academy by Dr. T. H. Turner, and described by Prof. Cope under the name of *Elasmosaurus platyurus*, belong to an Enaliosaurian, as originally suggested by Prof. Cope. The anatomical characters of the different regions of the vertebral column, those of the shoulder and pelvic girdles, and of the preserved portions of the skull and teeth, are decidedly Plesiosaurian.

March 15.—Dr. Ruschenberger, President, in the chair. The following paper was presented for publication:—"Cross Fertilisation and Law of Sex in Euphorbia." By Thomas Meehan. Mr. Charles Darwin's interesting observations on cross fertilisation have opened a new world for original discovery. The list of plants which seem to avoid self-fertilisation is already very large. I think *Euphorbia* may be added to the number. Certainly this is the case with *E. fulgens*, Karw. (*E. jacquinaflora*, Hook) which I have watched very closely in my greenhouse this winter. Several days before the stamens burst through the involucre, which closely invests them, the pistil with its ovarium on the long pedicel has protruded itself beyond, exposed its stigmatic surfaces, and received the pollen from the neighbouring flowers. The way in which the pollen scatters itself is curious. In most flowers a slight jar or a breath of wind will waft the pollen to the stigmas, but I have not been able to notice any to leave the flowers in this way; for as soon as the anther cells burst, the whole stamen falls from its filament-like pedicel and either drops at once on the pistils of other flowers or scatters its pollen grains by the force of the fall. This *Euphorbia* also furnishes another contribution to the theory of sex which I have advanced. The plan on which the male and female organs are formed is evidently a common one; and the only reason why some flower-heads have a pistil in the centre, and others are wholly staminate, is, that there is greater axial vigour when the female flower is formed. Whenever the common peduncle (below the scarlet involucre) is weak, a pistil never appears in that head of flowers. A few which seem strong neither have them, but the great majority of the strong peduncles are those which bear the female blossoms. Another interesting fact is that the number of male flowers is less in those heads which also bear a female, than in those which are wholly staminate. This seems to add to the point I made in my paper on *Ambrosia*, that after the flowers have been partially formed in embryo, and before the sex has been finally determined, the female flower, being primordially the stronger, has the power of absorbing the males or their partially formed elements into its system. It is certainly remarkable that in both these instances the number of male flowers should decrease in proportion to the existence or vigour of the central female one. The male and female flowers of *Euphorbia fulgens* are formed much alike. the female occupies the centre, and seems really but a prolongation of the main stem, on the top of which is an articulation from which the ovarium springs. The capsula readily falls from this articulation when mature. From the base of the female central peduncle spring weaker peduncles, colourless, appearing indeed almost like filaments, articulated at about the same height as the female, only above the point bearing a short filament and anther—the caduceous part before referred to. No one can fail to see the correspondence of plan in these different parts, and I think that nothing but the favourable position in the direct line of axial vigour made the central flower a female one. Cases occasionally occur in which a tolerably strong head of wholly male flowers will develop the central axis into a pedicel almost as long and vigorous as those which bear female flowers. But the flow of vital force—if I am correct in using this term —not being quite sufficient, the final goal of natural perfection in the female form was not reached. These cases do not occur often, but are well worth looking for, as they show so clearly the dividing line between the forces which govern the male or female sex.

March 22.—Dr. Carson, vice-president, in the chair. The following paper was presented for publication: "Descriptions of Fossils collected during the U.S. Geological Survey under the charge of Clarence King." By F. B. Meek.

April 5.—Dr. Carson, vice-president, in the chair. Prof. Leidy made the following remarks on "Discosaurus and its Allies." The body of the last vertebra in the series of caudals belonging to the Kansas saurian, described by Prof. Cope under the name of *Elasmosaurus*, has the length less than the depth or breadth, which latter is the greater diameter. It is moderately contracted towards the middle, the sides below the neural arch and the surface below the costal articulations being fore and aft concave, and bounded in front and behind by an acute margin from the articular ends. A ridge extends fore and aft between the chevron articulations, and the included surface is concave, and exhibits a single lateral venous foramen. The costal articular processes project from the middle of the side of the body, reaching nearer the fore than the back end of the latter. They are transversely oval, about three-fourths the length of the body, and the height about half. They form a deep concavity, with acute margins extending peripherally. The articular ends of the body are transversely oval and defined from the intermediate portion of the latter by an acute everted margin. A short distance within the position of the latter the surface is marked by a narrow groove, and within the circle of this groove the surface projects in such a manner as to appear like a distinct disc or epiphysial plate applied to and coössified with the body. The surface of the disc is convex at the periphery and moderately concave towards the centre. The articular surface beyond the groove defining the disc appears as an everted ledge, and the triangular articular facets for the chevrons appear as deflections of the ledge. The extension of the latter inferiorly is greater at the posterior extremity of the body than at the anterior extremity, thus producing a larger provision of surface in that position for the articulation of the chevron. The neural arch in the specimen has apparently been so much laterally compressed, that its original condition cannot be ascertained.

BOOKS RECEIVED

ENGLISH.—Lay Sermons, Addresses, and Reviews: By T. H. Huxley. (Macmillan and Co.)

FOREIGN.—(Through Williams and Norgate)—Essai de Philosophie Positive au xix[me] siécle: A. d'Assier.—Ueber die Chimie des Weines: Dr. C. Neubauer.—L'ancienneté de l'homme: Le Marquis de Nadaillac.—Description physique et naturelle de l'île de Crête, Vols. 1 and 2, with Atlas, Tome i. and ii.: V. Raulin.—Cryptogames vasculaires du Brésil: A. L. A. Fée. Mémoires de l'Académie impériale des Sciences de St. Petersburg, vii[me] série, Tome xv., No. 2, Flora Caucasi, part 1: F. J. Ruprecht.

CONTENTS

THURSDAY, JULY 28, 1870

NATURAL HISTORY IN SCHOOLS

AMONG the many indications of the taste for natural science which is spreading throughout the country, there is none more striking than the rapid increase, during the last ten years, of local Field Clubs and Natural History Societies. Those who reside in our larger towns and can avail themselves of every facility for prosecuting their favourite branch of study, are apt to under-estimate the value of these bodies, and to look upon them as merely accessories to the bringing together, under the name of science, of men of congenial tastes, rather than as performing any definite or actual work; but this is an error which can only arise in the minds of those who have had no opportunity of seeing the working of such societies, and who have not therefore felt the advantages by which membership is attended in a remote country district. No doubt there are, here and there, Field Clubs which fall short of the perfection they ought to attain; but that these are the exception, and not the rule, no one who has investigated the matter can reasonably doubt.

It is not, however, to such societies that we would now direct attention, so much as to the rise and progress in our public and private schools, of bodies whose aim and object is the same; a progress which is the more gratifying when we remember that it mainly originates with the boys themselves. As to the value of such societies there cannot be two opinions. We would not for a moment depreciate cricket and other manly sports; but we would supply for the mind a rational substitute for such manias as that of postage-stamp collecting, which a short time ago reigned in our schools.

It may not be known to many who are aware of the existence of such societies in our larger schools, that their origin dates back nearly forty years. It is to the York School, under the superintendence of the Society of Friends, that we must look for the first step in this direction. In 1834—three years after the formation of the Berwickshire Field Club, then the only one of its kind in the kingdom—a Natural History Society was formed in the York School, which is in operation at the present time. Quiet and unpretending, few even of those who take an interest in such societies are aware of its existence, or have seen the modest report which it issues year by year. Had Mr. Carlyle, in his schoolboy days, shared such advantages, we should not have him now lamenting that no one ever taught him the constellations, or "made him at home in the starry heavens!" Looking through the names of members, formerly pupils at the school, we note among them men now known to science; and we shall scarcely be wrong in supposing that science is in a great measure indebted to the York School for the work which they have done, and are still doing.

Turning now to our public schools, the first which claims our notice is Marlborough College. In 1863, the Rev. T. A. Preston, one of the masters, assisted by some of the pupils, published a Flora of Marlborough, "with a hope that, by placing before the members of the college a proof of the botanical riches of our neighbourhood," he might induce some of them to take an interest in the study of botany, and perhaps indirectly through this, of some other branch of Natural History. How far this hope was realised may be gathered from the fact that, in a few months "a spontaneous movement arose among certain members of the school towards the pursuit of Natural History with some degree of system and organisation." The *Marlborough College Natural History Society* was speedily formed, Mr. Preston being elected president, and to him we are indebted for a complete set of the reports issued by the Society, containing the rules, the papers read, and the calendars—ornithological, botanical, and entomological, for the various years. As it is mainly upon the model set by this Society that other similar ones have been formed, a few details may be interesting. The rules strike us as being particularly good, and might be adopted with advantage by Field Clubs generally, although some of the best of them, such as that providing for the exclusion of members who do not evince sufficient energy in the working of the club, could only be satisfactorily carried out in school societies. Glancing through the papers, their general excellence, as well as the variety of subjects, is worthy of note. Not that all are of equal value; this indeed it would not be reasonable to expect; but taking them as a whole, they would be creditable to members of "grown-up" societies. Due prominence is given to those bearing upon matters of local interest, and new discoveries in science are brought before the members. The various calendars are most carefully compiled, and are so printed as to indicate each year's additions to the flora and fauna of the district; the number of members cited as contributing gives us a good idea of the actual work done by the Society. Besides these transactions, a museum has been established, and judging from the numerous contributions acknowledged in the reports, it is assuming very satisfactory proportions. The formation of a museum is a work peculiarly suitable to a school; a common centre, so to speak, is provided, upon which to work; the objects collected are sure to be carefully preserved, and the collection is in a position where it may be of real service to those interested in it. A library has also been formed, and we may suggest to any of our readers who may happen to have on their shelves duplicate copies of any scientific work, that they cannot do better than forward them for the use of such a body.

The *Harrow School Scientific Society* was founded in 1865, but here, as at Marlborough, considerable interest had been manifested in Natural Science for some time previously, as was evidenced by the publication, in 1864, of a Harrow Flora, with chapters on the birds and insects, all by Harrovians. The printed reports give selections from the papers read, which, if not equal to those of the Marlborough Society, are still creditable; we have, however, no calendars, and the additions to the fauna and flora are comparatively few.

The *Rugby School Natural History Society* dates from March 1867, and has issued two reports. The most noticeable feature in the published papers is the prominence given to subjects bearing upon the Darwinian theory of the origin of species, and some extremely thoughtful essays on this, as well as on the more recent facts which support the theory, are printed. Some papers on the "Protective Resemblances," noticeable among British, as well as foreign insects, seem to us particularly suggestive; the natural history of the district receives a due share of attention, and in the two reports before us we have lists of the birds, Lepidoptera, and flowering plants, with dates of appearance, record of new species, &c., besides a copious list of local Lias fossils. A museum and library are in course of formation.

The *Wellington College Natural History Society* held its first meeting on May 13, 1868, and its rules are based upon those of the Rugby Society. One report only has as yet been published, in which we find selections from the papers, including one by Professor Kingsley. Botanical and ornithological lists are given, but the comparatively recent formation of the Society renders any detailed criticism unnecessary.

Last comes the *Winchester College Natural History Society*, established in March last, a notice of which has already appeared in NATURE. Of course little has been done, and nothing published at present; but we learn from the secretary that the plan of working by sections has been adopted, and that the meetings are well attended. To this and to all similar bodies we heartily wish success.

We are sorry to see that Eton is "conspicuous by its absence" from our list. The publication, two years since, of "The Birds of Berkshire and Buckinghamshire," by Mr. Clark-Kennedy, "an Eton boy," induced us to hope that a school which in position is second to none, would not be left behind in the onward march; but as far as we can ascertain, nothing in the way of a society has been established at present. We shall be glad to learn that we are mistaken in this matter, and trust that, even should no such body at present exist, it may not be long before we hear of its formation.

THE RELATIVE VALUE OF CLASSICAL AND SCIENTIFIC TRAINING

Wodurch die humanistischen Gymnasien für die Universität vorbereiten? Rede an die Studirenden der Ludwig-Maximilian's Universität zu München gehalten am 4 Dezember, 1869, von Dr. Med. Max v. Pettenkofer, Professor der Hygiène, z. z. Rector. (München, 1869.)*

THE German-reading public can possess itself at a very trifling cost of a very weighty opinion as to the relative value of classical and of scientific training, by the purchase of an address delivered last December in Munich by Professor Max v. Pettenkofer, in his capacity of Rector or Chancellor of the University for the time being. There is in existence an English document (we fear we cannot speak of it as a *publication*) in the shape of a report, laid before the authorities of Owens College, Manchester, which has appended to it a name nearly, or quite, as familiar to the student and readers of NATURE as Pettenkofer's—viz., that of Professor Roscoe, and in which the same process of "ponderation" is applied to the classical "Gymnasia" and the modern "Real-Gymnasien" severally. V. Pettenkofer, who is not referred to in that report, shall here speak for himself, and we may say at once, that after stating more or less fully the objections which are ordinarily urged against the classical system, he declares himself an adherent of the party which stands *super antiquas vias*. The two delegates of Owens College appear to incline in the same direction somewhat, but are more eclectic and more careful in balancing their utterances as to the possibility of combining the two systems than either v. Pettenkofer, whom we shall forthwith cite on the one, or than Helmholtz, whom they cite on the other side.

The argument from authority has a legitimate place in questions concerning such matters as the genesis of culture and as the existence of capacity and capabilities; for in such questions neither the facts themselves nor the mode of their origination can be always looked upon as beyond the region of probability. But as we are writing in a scientific periodical, we will begin at least with something which admits of being quantitatively estimated; and we will do this by giving the time-tables of the classical (*Humanistischen*) and of the modern (*Real-Gymnasien*) schools in Bavaria, as we find them in v. Pettenkofer's address (pp. 5 and 18).

In classical schools, out of 99 hours per week:—

8 hours per week are given to			German.
26	,,	,,	Latin.
22	,,	,,	Greek.
8	,,	,,	French.

(*i.e.*, 64 hours, or 65 per cent., are given to languages, three-fourths being Latin and Greek, and one-fourth German and French.)

17 hours per week are given to			Mathematics.
10	,,	,,	History.
8	,,	,,	Religious Instruction.

In "Real-Gymnasien," out of 112 hours per week:—

9 hours per week are given to			German.
14	,,	,,	Latin.
13	,,	,,	French.
4	,,	,,	English.

(*i.e.*, 40 hours, or 33 per cent., are given to languages, of which time only one-third is given to one ancient language, one-third to French, the other two-thirds to German)

27 hours per week are given to Mathematics.

(*i.e.*, algebra, elementary geometry, trigonometry, descriptive and analytical geometry and higher analysis, taking 22 per cent. of the whole number)

4 hours per week are given to			History.
19	,,	,,	Natural Science and Geography.
24	,,	,,	Drawing and Modelling.
8	,,	,,	Religious Instruction.

The "Real-Gymnasien" are thus seen to exact 25 per cent. more hours than the classical schools; and it is by this increase on the one hand, coupled with a curtailment of the quota assigned to languages on the other, that time is found for mathematics and for natural science, with the drawing and modelling so indispensable to it. V. Pettenkofer deprecates the making of any material increase in the number of hours to be spent in the gymnasien, on the undeniable ground that the day is no longer, and man no stronger now than were the days and the men of 2,000 years ago; and space for such additamenta as must be made to the curriculum must be found by

* "In what way do classical schools give students a preparation for the University?" An address delivered to the students of the Ludwig-Maximilian University in Munich, on the 4th of December, 1869, by Max. v. Pettenkofer, M.D., Professor of Hygiène, and at the time Rector of the University. (Munich, 1869.)

bettering the methods and means for communicating instruction, and effecting thus an economy of time.*

The Bavarian chemist and hygienist does not himself suggest any ways and means whereby this economy may be effected, and presumptuous though it be, we will attempt to supplement this deficiency by saying that such an economy might be effected in England and English schools by applying one or other or all three of the following lines of treatment to the classical curriculum, even without cutting its Greek adrift. Latin and Greek, to put the boldest suggestion first, might be studied in certain, and those not a few, cases, as literatures and not as philologies; or, as a second alternative, when some training in philology is to be retained at whatever cost, such training might be made more intelligible and so less distasteful and wasteful of time, by making the study of it comparative, as recommended by Professor Max Müller in his evidence before the Commission just referred to; or thirdly, synthetical scholarship, in the way of verse-making, should be considered as a luxury and refinement to be reserved for the delectation and cultivation of those few who, in any age, show any aptitude for it, and synthetical scholarship in the way even of writing Latin prose might, due precautions having been taken, be dispensed with in the cases of youths who, whilst wholly incapable in that, had shown some capacity in some other line. Our "due precautions" should consist in the multiplying the practice of synthetical scholarship in the way of translation from Latin into English. We know the horror which these suggestions will excite in the breasts of schoolmasters of the type represented by the gentleman who told the Commissioners already referred to, that if he were set to teach History in set lessons, he "should not know how to do it." But we believe that by the adoption of any one of the three lines of action just glanced at, space and time might be found for the introduction of the natural sciences into the curriculum of any public school, and that at once without injury to the dignity of either the one or the other of the two sets of studies, and without injury to the physical or mental health of the learners.

But it is time, perhaps, that we should let v. Pettenkofer speak for himself; and this he does (at p. 12, *l. c.*) to the following effect:—"I am convinced that philology and mathematics furnish precisely the material for teaching and intellectual discipline which is essential for our gymnasia, and I look upon the material furnished by other sciences as mere accessories. I know that in putting forward this view, which I do not do now for the first time, I put myself into opposition with the tide of opinion which is prevalent just at present, and which anticipates great advantages from the introduction of additional subjects of instruction, and especially from the introduction of instruction in natural science into 'Latin schools (Lateinschulen) and gymnasia.'" Further on (p. 16) he proceeds as follows:—"The results of actual experience appear to me to favour my views. In other parts of Germany, experiments have now, for a long while, been made with gymnasia and similar institutions, in which much natural science is taught. But I cannot as yet discover that any remarkable number of persons who have subsequently distinguished themselves in natural science have come from these schools. In this matter reliable statistics of the pupils leaving (*der Abiturienten*) a Berlin gymnasium, the so-called 'Old Cologne Gymnasium,' in which natural science has for a long while formed part of the curriculum, would be very instructive. Distinguished men come, from time to time, from this gymnasium, but certainly not in greater numbers than from any other classical (*Humanistischen*) gymnasium where no natural science at all is taught. It would long ago have been a notorious fact if a disproportionate number of the younger professors of natural science in the Prussian Universities could have been shown to have been formerly students in the Cologne Gymnasium."

We imagine that this "Old Cologne Gymnasium," thus referred to by v. Pettenkofer, is none other than the "mixed" (*simultan*) school described by Mr. Matthew Arnold under the name of the *Friedrich Wilhelm's Gymnasium* at Cologne, in his "Schools and Universities of the Continent," pp. 218-221; and but that more antagonism and less familiarity subsisted between North and South Germany six months ago than, we are happy to think, subsists now, we apprehend that more would have been made of the history of this institution by the Munich Professor. For Dr. Jaeger, the director of this mixed school, who, as he had been refused a nomination to another school, the Bielefeld Gymnasium, by the Education Minister, on account of his politics, cannot be suspected of reactionary leanings, spoke to Mr. Arnold in the following sense (see p. 221, *l. c.*): "It was the universal conviction with those competent to form an opinion, that the *Realschulen* were not at present successful institutions. He declared that the boys in the corresponding forms of the classical school beat the Realschule boys in matters which both do alike, such as history, geography, the mother tongue, and even French, though to French the Realschule boys devote far more time than their comrades of the classical school. The reason for this, Dr. Jaeger affirms, is that the classical training strengthens a boy's mind so much more. This is what, as I have already said, the chief school authorities everywhere in France and Germany testify. In Switzerland you do no hear the same story."

With regard to Switzerland, we learn from the Owens College Report above mentioned that Professor Zeuner of the Polytechnic School at Zurich, holds that the establishment of "Real Gymnasia," or High Schools of Science, to take equal rank with the old classical gymnasia, and to put pure and applied science on the same footing for educational purposes as that which the classics enjoy in these schools, is a desirable thing, but that he allows that by the introduction of a "bifurcation" system into the older schools they might be made equal to meeting all modern requirements. Helmholtz, on the other hand, may in the

* It may be well here to lay an English programme of school-work alongside of the two above given schedules of German school-work. A scheme to the following effect may be found as suggested by the Public School Commissioners, in their report, vol. i., 1864, pp. 34, 35:—

In an English public school of 39 hours per week—

11	hours	are to be assigned to	Classics, History, and Divinity (lessons)
10	,,	,,	,, ,, (preparation).
3	,,	,,	Arithmetic and Mathematics, *i.e.*, not less.
2	,,	,,	French and German (lessons).
	,,	,,	,, ,, (preparation).
	,,	,,	natural science (lessons).
	,,	,,	,, ,, (preparation).
	,,	,,	music and drawing.
	,,	,,	composition.

same report be found pleading strongly for "the foundation on equal terms of complete academic institutions for science" as a "counteraction of the tendency of classical men to lean on authority alone."

"Philological culture," says the eminent physiologist of Heidelberg, "has an ill effect on those who are to devote themselves to science; the philologist is too much dependent on authority and books, he cannot observe for himself, or rely upon his own conclusions, and having only been accustomed to consider the laws of grammar, all of which have their exceptions, he cannot understand the invariable character of physical laws." Granting with all respect the premises laid down by Professor Helmholtz, we should demur to the conclusion which he would base upon them, and profess ourselves unable to see that, because particular institutions had a tendency to dwarf and stunt particular faculties, they should therefore be left undisturbed to do this evil work uncounteracted. And still leaving the premises unimpugned, we should set up a cross-indictment to the effect that if classical studies left the student of them unacquainted with the invariability of natural laws, physical studies leave the student unacquainted with the variability of men's minds. But, so far as the business of life consists in having to do business and hold intercourse with our fellow-men, this acquaintance with the variability of men's minds is simply the particular kind of knowledge which is not only the most practically useful and marketable of all kinds of knowledge, but is precisely the kind which, by common consent, is allowed to characterise if not to constitute "culture."

Lord Lyttelton, however, and the Endowed Schools Commissioners would appear to be in favour of the establishment of locally distinct schools for the two sets of studies and of students, and herein to be at one with Helmholtz. The Owens College Delegates, on the other hand, are, like ourselves, in favour of a system of bifurcation, which would not necessarily keep apart persons of different mental conformation who might be much benefited by mutual contact. They have come to this conclusion mainly for reasons based on observations and testimony given in Germany. Our peculiar social organisation makes the question more complex for us; but we, too, have our experience as well as the Germans; and time has shown that an Englishman, whose reputation as an educationalist is equal to that of Helmholtz as a physicist, may, in this very matter, be as far wrong as we believe that great physicist to be. In 1864 Dr. Temple told the Public Commissioners (see Report, vol. ii., p. 312) that he should "not consider it wise to follow the Cheltenham and Marlborough examples by attaching to the public schools modern departments. The classical work would lose, the other work would not gain!" In 1867 we find a distinguished Rugby master, the Rev. J. M. Wilson, speaking to the following effect of the results produced by the changes set on foot in accordance with the proposals of the Public Schools Commissioners, and earnestly and honestly carried out. "Lastly, what are the general results of the introduction of scientific teaching in the opinion of the body of the masters? In brief it is this: that the school, as a whole, is better for it, and that the scholarship is not worse. . . . This is the testimony of classical masters, by no means specially favourable to science, who are in a position which enables them to judge. . . . It is believed that no master in Rugby School would wish to give up natural science and recur to the old curriculum."

G. ROLLESTON

PAMPHLETS ON METEOROLOGY AND MAGNETISM

Journal of the Scottish Meteorological Society. (Blackwood and Sons.)

The Normal Winds of Bombay. By C. Chambers, F.R.S

WE have received the *Journals* of the Scottish Meteorological Society from the beginning of 1867 to the end of last year, and we find them, on inspection, to be full of a variety of interesting and valuable matter.

The Scottish Society does not confine its attention to one particular branch of meteorology, but is broad in its sympathies as well as energetic in the development of its objects, and it is no doubt owing to this that so much is done with comparatively small means, and so much ground occupied with advantage. Among the numerous papers which constitute these journals we observe an address by that veteran agriculturist, the Marquis of Tweeddale, "On the effects of solar radiation in relation to crops." Anything on this subject is interesting from one who has himself grown wheat on the fields of India, and baked it into loaves which were duly distributed to his various sceptical friends.

We note with pleasure a proposal by the noble author for two experiments on the physiological branch of meteorology, firstly, What portion of the value of the sun's direct rays is due to heat, and what to light? and secondly, Whether the heat is of value as applied to the roots in the soil, or as regards its stimulating effects on the plant above ground?

The indefatigable secretary of the society, Mr. Alex. Buchan, contributes many interesting papers, and among them a series on the well-known interruptions in the regular rise and fall of temperature in the course of the year. Six cold and three warm periods are discussed and the author arrives at the following conclusion:—

"The unusually cold or warm periods which occur with considerable regularity at certain times of the year have, so far as we have examined them, been proved to depend on the relations of the polar and equatorial currents to each other. And the circumstance that one of these great atmospheric currents and not the other prevails over this portion of the earth's surface at stated seasons, is a valuable fact in meteorology, particularly in the light it seems to cast on the periodicity of weather changes."

In another memoir, Mr. Buchan discusses the cold weather of March 1867, which he attributes to the unprecedentedly high atmospheric pressure which prevailed in the north and north-west of Europe from the beginning to the 24th of the month.

Mr. Thomas Stevenson, in another very valuable and original paper, introduces the method of Barometric gradients as a means of ascertaining the intensity of storms. Very probably it may ultimately be found that we can measure a storm better by the Barometric differences which cause it than by the violence of the wind which constitutes it a storm, but the first step is surely to measure directly and accurately the intensity of storms considered as independent phenomena, and the second to trace the connection which subsists between storms and Barometric differences.

We have also in these journals papers on Ozone, from Dr. Mitchell and Prof. Crum Brown; and in the physiological branch of the science we have observations on crops, trees, birds, &c., besides an interesting paper by Dr. Mitchell, on the cause of some of the pernicious effects of polar winds.

Besides the *Journals* of the Scottish Meteorological Society, we have received two pamphlets written by Mr. Charles Chambers, Director of the Bombay Observatory, which are of much value to men of science as thoroughly scientific discussions of phenomena observed with instruments of precision. In one of these, entitled "The Normal Winds of Bombay," we have a full analysis of the climate of that part of India as far as the element of wind is concerned.

In another pamphlet by the same author, forming part of the *Transactions* of the Royal Society of London, we have the magnetic phenomena of Bombay discussed in a very able manner after the method first introduced by General Sir E. Sabine and followed now by most magneticians. If we have yet made little advance in assigning the causes of magnetic variations, we have at least in such pages as these a solid foundation upon which to build, and our progress in terrestrial magnetism in this respect contrasts favourably with what we have achieved in former years. BALFOUR STEWART

DONKIN'S ACOUSTICS

Acoustics: Theoretical. Part I. By W. F. Donkin, M.A., F.R.S., &c., Savilian Professor of Astronomy, Oxford. Pp. 202. (Clarendon Press, 1870.)

IF the Delegates of the Clarendon Press are able to carry out their programme, it will be possible before long for English students to learn Physics in their native language. Besides Thomson and Tait's "Natural Philosophy," which, if completed in the same way as it has been begun, will be a book for a nation to be proud of, they promise us a series of separate educational treatises on the several branches of Physics. Of these there are already published Dr. Balfour Stewart's excellent treatise on Heat, and the work mentioned at the head of this notice. This, unhappily, is only a fragment, forming part of the general theoretical introduction to a treatise on Sound and the principles of Music which the author did not live to write. But, although its quality is such as to make us keenly sensible of the loss which English science has suffered by the author's removal before he had completed the work, the part that is published treats of subjects of so fundamental a nature, and so little dependent on what would have followed them, that its intrinsic value is probably not much lessened by the absence of the remainder.

The first chapter begins with a general description of the mechanism of the ear; this is followed by an explanation of the mode of representing vibratory movements by periodic curves, and a discussion of the nature of pitch, and of the principle of the superposition of vibrations, as bearing upon the distinction between noises and musical sounds, and upon the general mode of perception of sounds by the ear. The second chapter is headed "Miscellaneous Definitions and Propositions," and is chiefly occupied with the mode of defining musical intervals, and with the statement of their most important relations. The third chapter treats of the analytical representation of simple harmonic vibrations, and of the composition of vibrations at right angles to each other. The fourth chapter treats of the properties of the *harmonic curve*, of the composition of harmonic curves, and of Fourier's Theorem. The fifth, sixth, and seventh chapters are devoted to the vibrations of elastic strings, the greater part of the sixth being occupied with the description and experimental treatment of the important subject of *forced oscillations*, and an appendix to the same chapter with the mathematical theory of them. The eighth chapter treats of the longitudinal vibrations; and the ninth and last, of the transverse vibrations of elastic rods. The greater part of the book is addressed to mathematical readers, but much of it may be read with profit by students whose mathematical acquirements are very moderate. The evidence throughout the work of the author's mastery of his subject gives to it a freshness and individuality which are in strong contrast to the characteristics of the undigested compilations which form so large a part of the literature of physics. C. FOSTER

OUR BOOK-SHELF

Daily Readings in Natural Science. By Rev. J. Robertson. (London: C. Bean. 1870.)

THIS book has been prepared for beginners in the study of Natural Science; it is clearly and pleasantly written; each day in the week during a term has its subject allotted to it, either on Natural History, Physics, Botany, Astronomy, Natural Phenomena, Chemistry, Geology, Manufactures, Animal Physiology, or Applied Chemistry. The chapters are short, with questions at the end of each for the use of teachers. Mr. Robertson has written several other similar books, which he uses in his own school, and his success as a teacher lends great weight to the following extract from the preface:—"It is hardly necessary to descant lengthily upon the advantage of introducing science teaching into schools. The author, however, may be pardoned for giving his experience of adding science to the usual course of studies among his own boys. The science classes in his school had not long been established before he found that those boys who took no interest in their ordinary work soon manifested a quickness and brightness in dealing with natural objects that was quite remarkable, so much so, that after the first three months he doubled the time devoted to science by the upper form, and commenced new classes for the benefit of the middle and lower forms. Speaking generally, the study of Natural Science quickens a boy's powers of observation and comparison; he learns to express his thoughts in proper logical order, his judgment is developed, and the tendency that all boys have to form hasty conclusions is checked and tempered." To these we may join the following practical hints on conveying scientific instruction to schoolboys, in a notice to teachers at the end of the book:—"To be of real use science must be taught practically. Experiment and deduction should go hand in hand, and a boy ought never to be called upon to commit a fact to memory the truth of which he has not previously seen demonstrated. This of course presupposes perfection in the way of apparatus and specimens, a state of things that may possibly exist some day at such magnificent centres of learning as Eton, Harrow, or Rugby, but the boy who by circumstances is obliged to pass his days at smaller establishments, must take for granted a large number of the facts with which he stores his memory. But the ingenious teacher will see a thousand ways of demonstrating facts to his pupils with the outlay of very little money or time. The short course of

Natural History given here should be supplemented by visits to the Zoological Gardens and the British Museum. Having gone through the first three lessons, the cleverest boy in the class, or the teacher himself, should note down the names of the different animals described, with their peculiarities; the notes might then be copied by each boy, or at any rate read out to them. The visit to the Gardens or Museum should next be paid, not with the idea of wandering about in a desultory manner, but with the object of testing the truth of the statements contained in the lessons. Half an hour with a rabbit's or sheep's head, the examination of the teeth of the cat or dog, will help wonderfully to develop a boy's love for Natural History. The lessons should if possible be illustrated by the skulls or skeletons of some of the smaller members of each order or class. Such specimens may be obtained at a cheap rate from Mr. Cutter, 35, Great Russell Street, Bloomsbury. The same principle may be followed with Astronomy." These extracts show that the book is the work of a practical man, and as such we commend it.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

Spontaneous Generation

PROF. WANKLYN says the opponents of spontaneous generation are placed in a dilemma by some new facts and arguments recently advanced in favour of the theory by Dr. C. Bastian, whose observations were only concluded in NATURE for July 14, 1870.

Prof. Wanklyn might have allowed the "opponents" a little more breathing time, and if his cause is really so strong, why so soon call attention to "other difficulties" under which they labour? These tactics compel me to remark that the spontaneous generation theory will gain nothing by being forced upon the attention. Like other doctrines it must stand or fall according as the facts on which it is based are confirmed or refuted. It matters not whether A or B is an advocate for or against. The question can be determined by observation and experiment only. It is of little importance though the theory be supported by the press or be believed in by a large section of the public. If it can be proved to be true it will be accepted, but the utmost notoriety it may be possible to gain for it cannot create conviction of its truth.

I confess to being an "opponent" of the doctrine but simply because I cannot admit that the evidence yet adduced is at all convincing; while it seems to me the very way in which the doctrine has been thrust forward is calculated to excite undeserved distrust. The attempts made to prepare our minds, as it were, for its reception, the frequent announcements that the proof is *coming*, the bolstering up, if I may use such a term, this doctrine appears to require—instead of promoting its reception, is only calculated to excite suspicion, which may be entirely undeserved. I, for one, am quite prepared to receive the facts adduced in favour of the doctrine, but am by no means disposed to accept immediately or unconditionally the inferences drawn therefrom.

Before I proceed to discuss the "dilemma" in which, according to Prof. Wanklyn, as an opponent of the doctrine, I am placed, I would suggest that figures of the different forms of organisms, supposed to be *spontaneously generated*, be published in NATURE, side by side, with a short description, reference to authority, and magnifying power. In this way we should see at a glance the different kinds of organisms which had been formed *de novo*, according to different authorities.

Some of the figures of Dr. Bastian, I confess, astonished me. To judge from the drawings and statements it would appear that a solution of tartrate of ammonia and phosphate of soda *in vacuo*, was a much more potent generator of life than a solution of boiled turnip or boiled hay. I was certainly very much surprised when I saw Figs. 11, 12, 13, 14, and 15. If the organisms represented in those drawings have been produced, as Dr. Bastian states, in saline solutions destitute of living germs *in vacuo*, the fact is, indeed, so very important and of such very great interest, that it would have been better, in my opinion, to have directed attention to it alone without mixing up the question of fact with Mr. Herbert Spencer's probabilities. The experiments are either sound or not. If the former they do not require the support or assistance of any *à priori* reasoning; if the latter, all the philosophical arguments that can be adduced will not procure their acceptance. Dr. Bastian could not have published his drawings without feeling that those at all familiar with such inquiries would be surprised—and those who support him must therefore not feel hurt at the reserve of "opponents," and because they do not write to the very next number of NATURE to declare themselves *convinced*, and ready to subscribe to the spontaneous generation doctrine and give it all the support in their power.

But indisposition on the part of many of us to accept immediately the inferences does not detract in the slightest degree from the acknowledgment of their importance. I for one, as I said before, am ready to accept them, but not yet, because, as far as I know, Dr. Bastian's results are exceptional, and the experiments may require repetition. Nor could I thus early make the inquiries that would be necessary concerning the details and the many little precautionary measures that are requisite, and which, for aught that appears, have been taken, without seeming to be offensive. But I cannot help saying here that many of the preliminary remarks, and many observations in the notes, and much of the argument, are calculated rather to prejudice the mind against the conclusions sought to be established by experiment.

All the arguments hitherto adduced in favour of spontaneous generation fail to convince. Neither Mr. Herbert Spencer nor Mr. J. S. Mill himself could convince any man acquainted with the facts of the case, that heterogenesis really occurs in these days. At the same time a very strong case—nay, a case certain to convince anyone who was not practically acquainted with the matter—might be made. Such a case has indeed many times been made, and has produced a vast number of converts, some of whom have afterwards fallen away from the faith, and have been led to believe in another view. Many things *seem* to be proved by irrefragable reasoning which are *not proved*, and cannot be proved in the present state of our knowledge. Dr. Bastian of course admits all this, or he would never have tried *new experiments*. It is therefore only these new experiments which, by adding to our knowledge, can alter the question as it stands. All the arguments concerning invisible germs of crystals, the mode of "building together the molecules of corn," "collocations," "*à priori* presumptions in favour of new modes of evolution," &c., &c., merely increase the difficulty experienced by ordinary mortals of grasping the real question at issue and discussing the theory within any reasonable limits. The introduction of these preliminary *à priori* considerations is calculated to confuse. They certainly interfere with the due concentration of the attention upon the results of the experiments. They will not excite persons who are at all conversant with the inquiry to put more trust in the new experimental results than they would be inclined to do without them. On me, I regret to say, they have a contrary effect. The fact of *à priori* arguments having been so very much dwelt upon, makes me think that the mind of the experimenter may have been to some extent prejudiced (prepossessed) in favour of the doctrine he seeks to support by new facts, and in this way they are calculated to excite in my mind, however much I may resist, a doubt whether the inferences which have been arrived at really have been deduced from facts of observation and experiment *only*.

Prof. Wanklyn, whose observations have called forth these remarks, is rather damaging the cause he hopes to serve in another way. He states that "rather more than one pint of average atmospheric air does not contain so much organic nitrogenous matter as corresponds to a cube of dry albumen of the $\frac{1}{250}$th part of an inch in diameter," and affirms that this quantity is altogether inadequate to account for the "immense multitudes of germs, the existence of which in atmospheric air is assumed by the vitalists." Now these "vitalists" hardly require "immense" multitudes, and instead of being "assumed," the presence of quite a sufficient number has been *proved*, and moreover, they can be seen by anyone who chooses to take the trouble to look for them.

But I thank Prof. Wanklyn for his fact and calculation. The evidence he adduces is very interesting, and conclusively in favour of the presence of living germs in the air; for those who accept the views he objects to, do not require a tithe or even a hundredth part of the albuminous matter he finds actually exists. The space occupied by a moist blood corpuscle $\frac{1}{3000}$ of an inch in diameter, which only contained even less than a *tenth of its weight* of dry albuminous matter, would be large enough to hold so many living germs that if uniformly diffused through the pint of air, not a thimbleful could be taken without containing several. These germs, placed under favourable circumstances, would soon grow large enough to be detected without any difficulty.

LIONEL S. BEALE

The Source of Solar Energy

IT is, I think, rather unfortunate that Mr. Proctor, in his recent work entitled "More Worlds than One," should have re-advocated the earlier and now discarded views of Sir W. Thomson concerning the source of solar heat or energy by *meteoric percussion*. That theory, however ingenious as advanced by the physicist, is surely hardly one to be admitted by the astronomer. Nothing less than an intense desire or necessity for finding some solution to the problem, whence or how the solar heat is maintained, could have encouraged scientific men seriously to advance or support so plausible and unsatisfactory a doctrine, or one, when examined, so little supported by what we really know either of meteors or of nature's laws. Having given much attention to *meteoric* astronomy, may I be permitted briefly to state what I hold are serious and practical objections to the validity of the meteoric or dynamical theory as applied to the conservation of solar heat and energy.

1. Because meteors and aërolites are known to impinge and strike the earth in her orbit, *ergo*, as I understand Mr. Proctor, numbers infinitely greater must no doubt be constantly rushing into the sun, as a body at once far larger, and much nearer to myriads of such bodies than the earth herself; but which, at a much smaller distance, are more likely to be drawn into the sun. Now, all that we really do know about meteors amounts to this, that by far the greater number of shooting stars visible in our atmosphere, in size no larger than a bean, and really separated from each other by thousands of miles, belong to fixed and definite systems or rings, having fixed radiant points for certain epochs or periods, showing clearly that these bodies are revolving round the sun, in courses as true and regular as the planets themselves, and are no more eddying or rushing into the sun, merely because they are so insignificant, than is the earth herself. Having projected upon celestial charts the apparent courses or tracks of nearly 5,000 meteors, observed during every part of the year, I feel I am justified in stating that not more than seven or eight per cent. of the shooting stars observed on any clear night throughout the year, are *sporadic*, or do not belong to meteor systems at present known to us. More than one hundred meteor systems are now recognised, several of which appear most certainly to be connected with known comets; and from a paper I have just received from Professor Schiaparelli, of Milan, it would appear that the approximate average *perihelion* distance for 44 of these meteor systems is not less than 0·7, the earth's distance from the sun being 1·0; whilst of these 44 systems, only 4, or about 10 per cent. have their *perihelion* distance under 0·1, that is, approach the sun nearer than nine millions of miles! Now, it is pretty well admitted that meteors are intimately connected with comet systems, yet out of some 200 comets, the elements of whose orbits have been calculated with tolerable precision, only 5 per cent. have their *perihelion* distance under 0·1. The same argument holds good also for planets, whose numbers also diminish after a certain considerable mean distance from the sun. Are these facts, then, in accordance with the notion that meteoric bodies either increase in number as we approach the sun, or that meteors are so constantly losing their senses, or sense of gravity, as to be ever rushing into or against the sun? I might almost ask, do *any* meteors rush or fall into the sun? Is it probable that the *mass* of all "the countless myriads of meteors" in the solar system exceeds that of a single planet? whether that of Mercury or Jupiter does not much signify. When we take into consideration the gigantic amount of meteoric deposits required to maintain the solar heat for hundreds of millions of years, in the meteoric theory, surely the supply of meteors would long since have been exhausted, were the supply at least confined merely to the meteors under a mean distance of 0·1 belonging to our own solar system! The argument, to begin with, is in a great degree fallacious, *e.g.*, because meteors frequently strike the earth, they must, it is argued, strike the sun in vastly greater numbers, and with far greater velocities. But it is forgotten that the meteors themselves, like the earth, are revolving round the sun as a common centre, in regular orbits, and only by accident, as it were, come into mutual collision, just as the tail of a comet might pass through the system of Jupiter and his satellites; while to the end of time neither the earth nor the meteors need necessarily come into contact with the sun.

2. But it is not merely meteors belonging to the solar system which are taxed to provide fuel for our sun; *space* itself may be filled with meteors ready to impinge upon the sun. The arguments against this are: (1) judging from analogy as well as from facts, comparatively few meteors are *sporadic*, consequently the majority cannot belong to stellar space, but to our own system; (2) granting that space itself is really more or less filled with meteors, these would not necessarily rush straight into the sun, unless, as would very unlikely be the case, they had no proper motion of their own. They might be drawn into or enter our system, it is true, but, according to Schiaparelli, only to circulate like comets in definite orbits.

3. The *zodiacal light* is another victim to the emergencies of the *meteoric* theory of solar energy. Whethre composed of myriads of small meteors, or merely a nebulous appendage, or atmospheric emanation belonging to the sun, is it credible that for hundreds of millions of years there could, physically speaking, be sufficient material in the zodiacal light to maintain the sun's heat and supply all the fuel required? Has it ever yet been proved that the entire mass of matter constituting the zodiacal light, is either composed of matter in a solid state, or, if it were, that its mass would be equal to that of our own earth? If composed of separate meteors, are they not each individually revolving round the sun, rather than occupied in being gradually drawn into it as a vortex? *

Of course I do not say that meteors are *never* drawn into the sun, or that they may not occasionally and by accident enter the solar atmosphere; I have merely endeavoured to show that, from what we really do know about meteors and the laws of nature, it is highly improbable that our sun could derive, in sufficient quantity, a needful supply of fuel from meteoric sources. The comet of 1843, which approached the sun within 550,000 miles, was not sensibly deflected from its course; it is just possible that so small a thing as an aërolite might at that distance have been drawn into the sun; but is it not also possible, from what we know of comet and meteor systems, it may be wisely ordained that the smaller bodies of our solar system, such as meteors, do not as a rule approach the sun too closely; and they probably do not, if their *perihelia* distances are rarely under 10,000,000 of miles?

Aërolites are doubtless of larger size and weight than shooting stars, and, as far as is yet known, not so regular in their appearance as shooting stars; but even with that class of phenomena, we notice a certain degree of periodicity in *maxima* and *minima* for certain times of the year, tending to show that they also may be subject to regular laws, and not fall so frequently or promiscuously upon the sun's surface as has been sometimes supposed. If they do not fall in vastly greater numbers, area for area, upon the sun than they do upon our earth, certainly the dynamical effect would be very minute! I may here also observe that even these bodies generally fall to the earth without being consumed, and with a very moderate velocity; their original cosmical velocity having been lost before reaching the surface of the earth. In the case of an aërolite falling upon the sun's surface, its original velocity may similarly have been gradually checked in its passage through the solar atmosphere, and a considerable amount therefore of the calculated mechanical effect lost. Small meteors would probably be consumed thousands of miles from the real body of the sun, seeing that the sun's inflamed atmosphere is now known to extend at times some 50,000 miles. It might almost be a question whether the sun's proper heat may not even be greater than that caused by the simple friction of a meteor through the solar atmosphere!

I merely allude to these minor matters, however, in order to point out some of the numerous uncertainties and difficulties connected with this meteoric or mechanical theory of the origin and conservation of solar heat, in addition to those already alluded to, bearing more especially upon the astronomical bearings of the question. For the present it must still remain a mystery, whence or how the solar heat is maintained, or to what extent really wasted.

Prestwich, Manchester, July 11 ROBERT P. GREG

Choice of a Microscope

WITH all respect to the judgment of my friend Mr. Ray Lankester, I should like to be allowed to oppose a few of the statements made by him in his remarks on the Choice of a

* We beg to refer our readers to Jones' and Liais' observations of the Zodiacal Light. They certainly have not received the attention in this country that they deserve.—ED.

Microscope, in NATURE, No. 37. It is, I believe, quite a mistake to say that you cannot get a cheap working English instrument. The model of Crouch, of London Wall, for instance, is not very much dearer than Hartnack's small model, and yet, at the same time, is in every way better and more comfortable to work with. Crouch's rackwork is so good that for $\frac{1}{4}$ths and $\frac{1}{8}$ths there is no need to resort to the fine adjustment, except on special occasions, whereas, with the sliding tube, the fine adjustment is so continually used that it is very soon thrown wrong, to say nothing of the trouble which a beginner has in working the sliding tube successfully. I once had some of Nachet's instruments, to which Hartnack's are very like, in use in my class at University College, and the sliding movement was very successful in smashing my best specimens and injuring the front surfaces of my lenses. Then again, the English length of tube is undoubtedly an advantage for a slanting position of the microscope, and I suppose that is by far the most common position in which an instrument is used. In addition, Crouch's instrument affords an admirably effective but simple stage movement, which may be entirely removed at pleasure, and has a simple sub-stage tube, into which, if required (and in London it often becomes a necessity), a condenser might be fitted. Crouch's instrument is like Hartnack's, simple but strong, steady, and cheap; it differs in being about three times as convenient, and will, probably, last twice as long.

Nor can I agree with Mr. Lankester in recommending the general purchase of Hartnack's glasses for student's use. His No. 8, for instance, in many respects an admirable glass, is terribly close, and for this, and apparently for other reasons, very soon gets spoilt when used by students. I have been using lately for my classes at University College, a $\frac{1}{5}$th of Crouch's, which, as far as ordinary histological work is concerned, performs in the most satisfactory manner, and yet is quite a cheap glass. I suppose the question of the price of labour prevents our English manufacturers from bringing down their prices to quite the French level, but I believe we get quite an equivalent for the slight excess in the form of greater convenience and better workmanship.

M. FOSTER

Colour-Blindness

THE nature of colour-blindness has never, I think, been satisfactorily ascertained. The usual explanation appears to be that the eye of the colour-blind is insensible, or nearly insensible to light of some particular colour. This I regard as in many respects unsatisfactory, and as I am not aware that the theory which I now suggest has been advocated before, I venture to lay it before the public.

There are no doubt some cases in which the eye seems partially insensible to particular colours or to colours in general. In such cases, however, I believe there is usually defective vision, and not proper colour-blindness. Those only are to be regarded as truly colour-blind who can perceive figures distinctly, but confound colours which other persons distinguish. Such was the case, for example, with Dugald Stewart, who could not distinguish between the colour of the leaves and the fruit of a Siberian crab; but he saw both, and therefore could distinguish the colour of both from that of the sky or cloud which lay beyond them. I mention this case more especially because Stewart was a psychologist, and maintained in opposition to Reid that variety of colour is the means by which we perceive visible figure. This is at least conclusive as to the perception of variety of colours by the colour-blind, which all observations made upon them point to.

Many philosophers have attempted to explain the phenomenon by assuming an insensibility to some colour, red for example. My reasons for rejecting this explanation are: (1) that in some cases where the experiment was tried (see Prof. Wartmann's paper in the Scientific Memoirs for November 1844) the colour-blind person saw the whole of the visible spectrum; and (2) that if red (for example) were seen as black, there would be no danger of confounding it with green, which would, on this hypothesis, be the colour seen most distinctly; but, in fact, confusion in regard to one colour almost always extends to the complementary tint.

The explanation I would offer is that derived from seeing accidental or complementary colours. It is a known fact that the eye has, in general (whether natural or acquired), a peculiar aptitude for white light, and thus if I gaze on a bright surface of any other colour, and look away rapidly towards a dark ground, the complementary hue becomes immediately visible. Nor can it, I think, be doubted that the complementary hue is not produced by the act of looking away. Green, for example, is produced by the red light falling on the eye, and the effect of looking away towards the dark ground is merely to make the green separately visible by cutting off the supply of red. It previously coexisted with and modified the red, but in ordinary eyes only to such an extent as not to prevent the red from strongly predominating in the total perception. This coexistence of the complementary colour with that actually visible is, I believe, known to persons accustomed to make delicate experiments in optics. I recollect in some lectures on the subject which I attended two or three years ago (where the equality of two lights of slightly different colours had to be determined), the professor cautioned us against looking too long at the lights, as he always found in his own case that there was a change of shade and a consequent impairment of the accuracy of his determination if he did so. This I have no doubt arose from the cause I have intimated. When we bear in mind the mutual excitation of sound-vibrations, the fact will create no astonishment. Now I apprehend that in most instances true colour-blindness arises from these complementary colours being excited more rapidly and with greater intensity than in ordinary eyes. If, for example, on looking at a red object the complementary green was excited almost at once, and with such intensity as materially to modify the red; and if, on the other hand, on looking at a green object, the excitation of the complementary red took place with equal readiness and intensity, it is clear that such an eye could not distinguish red from green. Both colours would, in fact, be seen after the first instant as a white or grey. In confirmation of this view I may remark that, according to Seebeck, all the colour-blind persons whom he examined confounded the colours with grey. Another argument in its favour is, that a confusion in regard to one colour seems (according to Wartmann) always to extend to the complementary tint. Again, it is natural to suppose that the production of complementary colours will take place rapidly and with considerable intensity when the eye is unusually sensitive to the incident light. Now, I find this unusual sensitiveness noticed in several of Wartmann's examples. One young woman could read for nearly a quarter of an hour (in the evening) after any one else could. In the cases mentioned by Goethe, the sight of the young men was "very good," and they "appreciated with great delicacy the gradations of light and dark." "Many Daltonians," says Wartmann, "see better in a demi-obscurity than other persons whose sight is more piercing by day than theirs," which he goes on to say was the case with three whom he himself had examined. Lastly, from the same paper it would appear that the colour-blind are either insensible to the phenomenon of accidental (complemental) colours, or see it with great difficulty. This, of course, is just as it should be on my theory. The colour-blind man sees both colours while looking at the coloured object, and he will again see both on looking away from it at the dark ground. If, for example, the colour looked at be red, the accidental green is seen while looking at the red, and it is also vivid enough to produce a secondary accidental red on looking away. The change produced by looking away will, therefore, be very slight, and hardly, if at all, perceptible.

I do not, however, put this theory forward as a complete explanation even of true colour-blindness. In addition to accidental or complemental colours, I believe there is often another phenomenon which may be called subjective colours, which modifies the total perception. In jaundice, it is well known, that black objects will appear yellow, and Dr. Wartmann records one case in which black appeared to the eye of the patient as green or crimson. I may have something more to say on this point hereafter. In such eyes, in fact, the adaptation is not for white light, but for light of some other colour, and the whole phenomenon of accidental colours is altered accordingly.

If these views be correct, it is evident that the colour-blind man will be best able to discriminate colours when he merely takes one glance at the coloured object, and then looks away towards a dark surface. This is worth trying. The fact that form is most easily discerned by taking a pretty long look at the object, makes a man follow the same course when he wishes to discriminate colours, but the advisability of doing so may be doubted. Another consequence is, that the colour-blind man would probably discriminate colours more readily in a faint light than in a bright one. These two observations can be easily made, and if my prediction should prove correct, the result will be of practical advantage to the colour-blind as well as a confirmation of the theory.

Trinity College, Dublin, July 9 W. H. S. MONCK

THE GUATTARI ATMOSPHERIC TELEGRAPH

THIS new invention is stated to consist of certain arrangements and combinations of apparatus whereby ordinary air compressed and passed through a tube, is utilised as a means of communicating intelligence from one given point to another, effecting the same object as the electric telegraph.

The principal portion of the apparatus consists of a reservoir or air-vessel which is charged or filled with air compressed to any desired degree according to the initial velocity or force which it is requisite the movements of the air employed should possess. A double action compression pump, or any other suitable mechanism, may be employed to charge the reservoir or air-vessel, and to sustain the pressure to the required degree. The reservoir or air-vessel is connected by means of a tube or pipe with a writing apparatus of any suitable description, and such as are well known and understood, especially in connection with electro-telegraphy; the tube or pipe being provided with a cock by which more or less force may be given to the current of air whereby the writing mechanism is actuated. In order to regulate the signals, a governor or piston, actuated by hand, is employed, by which pulsations or movements of the air in the tube or pipe are transmitted through a valve which is arranged therein, the currents actuating a lever connected with the writing apparatus. For the purpose of giving or receiving signals, the before-mentioned tube or pipe is connected with a conducting tube or pipe constructed of any suitable material, and which is so arranged that communication can be established between the air reservoir, or vessel, and the writing engine which is placed at the receiving station, or *vice versa*, by means of stop-cocks which are worked by hand. An indicator is employed to show the force of the current of air passing through the transmitting tube or pipe. Similar arrangements are, of course, placed and employed at each end of communication. By means of this invention it is stated that intelligence and signals can be transmitted to any distance; any of the known receiving and recording instruments capable of being used in connection therewith being employed. It is obvious that any number of conducting tubes may be employed, the requisite currents or pulsations of air therein being produced as before mentioned. The Guattari system claims to be more simple than the electric system, both in point of construction and continuous use, for whereas in the latter case it is necessary to use the electric battery and all its accessories, by the former ordinary atmospheric air compressed will perform similar functions. It is also claimed for it that it is free from atmospheric influences, which it is well known materially disturb the electric telegraph on the occasion of storms; and that the tubes employed as the medium for conducting the air would not be subjected to accidents like the ordinary wires, and would therefore necessarily last longer, and thus prove much more economical. We understand also that it is so simple that any person may learn in a few hours how to use and work it with the greatest ease, as compared with the electric system; it is calculated that the machinery necessary to work this system could be produced at about one-half the producing and annual working cost of the electric system.

The Royal Scientific Institute of Naples has already awarded to Signor Guattari a gold medal in recognition of what they consider an important invention, adding a graceful tribute on its presentation to the effect that it was the only gold medal which the Institute had ever awarded. The following experiments were made on Monday, 11th July, 1870:—

1. Transmission by atmospheric compression by means of the large machine, obtaining answers by impulsion and repulsion, Signor Guattari having at present but one machine.

2. System of impulsion and repulsion by a naval apparatus, which may be used with five different derivations or branches.

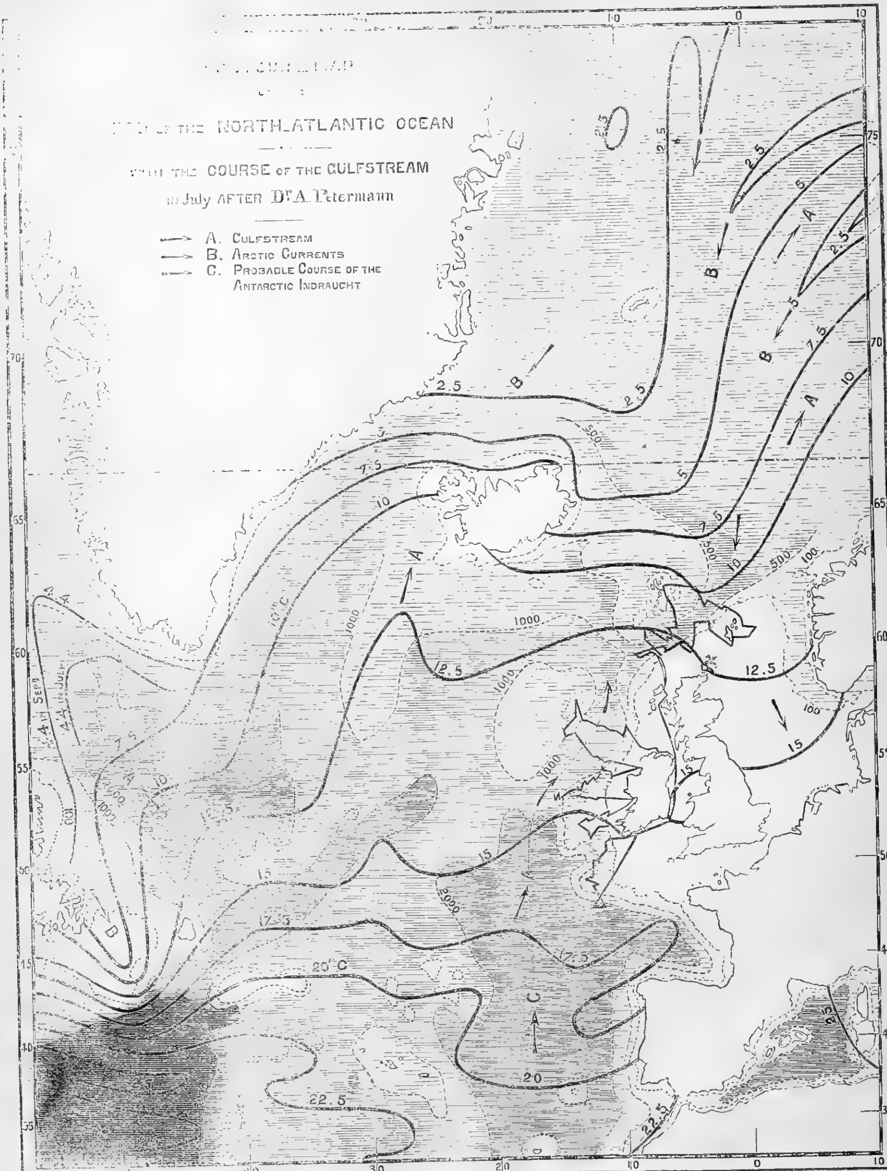
3. Universal telegraphy, namely, despatching telegrams to one or more stations at the same time without the aid of the transmitting machine or the necessity of the sender remaining fixed to any one point.

ON DEEP-SEA CLIMATES*

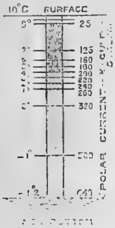
RECENT investigations have certainly tended to confirm the view originally advocated by my colleague, Dr. Carpenter, and myself, that a large portion of the bottom of the present sea has been under water and continuously accumulating sediment, at all events since the commencement of the "Cretaceous period," and possibly much earlier. The marked parallelism which, setting aside all local dislocations and denudations, evidently exists between the Jurassic, the Cretaceous, the Tertiary formations, and the present sea-board, and the evident relation of that parallelism to the older rock axes, would seem indeed to indicate that the main features of the present physical geography may date from a period even anterior to the deposition of the older Mesozoic rocks. With many minor and temporary oscillations, of which we have ample geological evidence, the borders of the Oolitic, the Cretaceous, and the Tertiary seas, have apparently been successively and permanently raised, and the ocean over an area, the long axis of which may probably correspond with that of the Atlantic, proportionally contracted. The question simply is, whether, since the elevation of the Jurassic beds, any oscillation has at any time raised into dry land the whole of the trough of the Atlantic, so as to arrest the deposit of sediment abruptly over the area, and to extinguish all animal life, thus defining what seems to be popularly understood as the close of a geological period, and requiring the complete repeopling of the succeeding sea by immigration, or, according to another view, by the creation of an entirely new fauna. It seemed to us on the whole more probable that the successive elevations of the borders of the Mesozoic sea were accompanied by compensating depression and deepening of the centre of the trough, which may thus have been inhabited throughout by a continuous succession of animal forms; at all events, the onus of proof appeared to rest with those who maintained any breach of continuity.

The deep-sea dredgings on both sides of the Atlantic have brought to light a very large number of hitherto unknown animal forms, and undoubtedly the assemblage bears a decided resemblance to the fauna of the chalk—a resemblance which increases as the investigation proceeds. Probably the most striking point is the apparent identity of the material of the chalk with the chalk-mud of the Atlantic; the globigerinæ and coccoliths by whose accumulation the beds have been, and are now, being produced, seem to be the same; though, of course, it is difficult to determine with certainty the specific identity of such simple and variable forms. Sponges are abundant in both, and the recent chalk-mud has yielded a large number of the examples of the group *porifera vitrea*, which find their nearest representatives among the ventriculites of the white chalk. From Prof. Martin Duncan's report it would appear that the corals, which are chiefly confined to water of moderate depth, are most nearly allied to those of the later Tertiaries. The echinoderm fauna of the deeper parts of the Atlantic basin is very characteristic, and yields an assemblage of forms which represents in a remarkable degree the corresponding group in the white chalk. Species of the genus *Cidaris* are numerous; some remarkable flexible forms of the Diademidæ seem to approach *Echinothuria*. M.

* The substance of a Lecture delivered to the Natural Science Class in Queen's College, Belfast, at the close of the summer session, July 15, 1870.

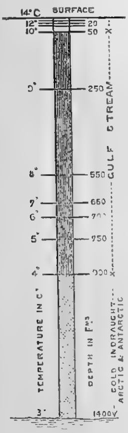

NORTH ATLANTIC OCEAN
COURSE OF THE GULFSTREAM
in July AFTER Dr A. Petermann
A. Gulfstream
B. Arctic Currents
C. Probable Course of the Antarctic Indraught

de Pourtales dredging in the Gulf Stream in the Strait of Florida has found a true *Salenia*, several representatives of the chalk forms of *Cassidulidæ*; and M. de Pourtales on the American side, and we off the west coast of Ireland and off the Shetlands, have dredged a remarkable form, appropriately named by Lyman *Pourtalesia miranda*, which is most nearly related to the Jurassic and Cretaceous genus, *Dysaster.*



Between the Faröe and Shetland Islands. 61° 21′ N. Lat., 3° 44′ W.L., Gr.

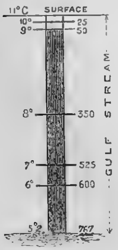
The Crustaceans of the chalk are, as yet, very imperfectly known, so that little can be founded upon them. The Mollusca have not yet been worked up, but the large number of smooth terebratulæ, and of species of the genera *Aporrhais*, *Dentalium*, *Pecten*, *Lima*, &c., from the deeper water, are highly suggestive of older times. Mollusca are, of course, most abundant in comparatively shallow water, and we were prepared to hear from Mr. Gwyn Jeffreys that out of about 120 species new to the British area, dredged in the *Porcupine* expedition, a large number date back to the newer Tertiaries.



Between Rockall & N.W. Coast of Ireland.

With these facts before us, it can scarcely be a matter of surprise that the point of view of those who are carrying on these investigations is insensibly changing, and that when the dredge comes up from a depth of one or two thousand fathoms the number of new species which it may contain is not now so much the question as the relation which these new forms may bear to their ancestors of an earlier epoch, and the light which they may be expected to throw upon types hitherto supposed to be entirely extinct.

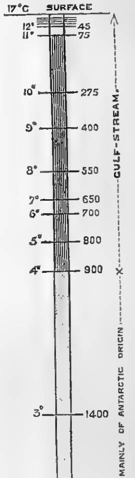
Although there is so striking a resemblance in general character between the fauna of the European chalk and that of the deeper portion of the bed of the Atlantic, especially of a band extending from a depth of 400 fathoms to 900 fathoms in the Gulf Stream area, none of the animals, with the possible exception of some of the Foraminifera, are absolutely identical. The species of *Sympagella*, *Holtenia*, and *Farrea* approach the siphonias and ventriculites very nearly, but they form a distinct subsection of the order. *Rhizocrinus* and its allies resemble *Bourgetticrinus*, and undoubtedly represent it, but there are important differences. The Saleniæ, the Cassidulidæ, and the Dysasters, &c., of the chalk mud approach their Cretaceous antetypes more closely than they do any known living forms, but they are generally dwarfed, and otherwise diverge so far as to require in most cases the establishment of new genera for their accommodation. Now, if we admit the continuous accumulation of sediment of the same character, and the persistence of the same general conditions over a large portion of the area of the present ocean from Mesozoic times, it seems at first sight more difficult to account for this great amount of modification than for the perpetual recurrence in deep dredgings of forms suggestive of close relationship to, and lineal descent from, extinct species.



South-West of the Faröe Islands. 59° 35′ N. Lat., 9° 11′ W. L., Gr.

At the bottom of the ocean, where other conditions are comparatively uniform, we may probably regard successive changes of temperature as the main cause of successive alterations in the fauna of a region, by the modification, extinction, emigration, and immigration of species. It is my object, in the present lecture, to show that in the vertical oscillations which are known to have occurred since the close of the Mesozoic period, we have a *vera causa* of alternations of temperature fully adequate to the entire result.

In order to understand this point thoroughly it will be necessary, in the first place, to pass in review the present conditions of distribution of temperature in the North Atlantic.*



Bay of Biscay. 47° 38′ N Lat., 12° 8′ W. L., Gr.

There seems to be little room for reasonable doubt that the present temperature of the basin of the North Atlantic depends, it may be said, *entirely*—for other modifying causes, such as the drift of the variable winds and the surface-heating and consequent expansion of equatorial water, are comparatively trifling—upon the Gulf Stream and the general indraught of cold water from the Arctic and Antarctic basins to supply the place of the constant warm current streaming north-eastwards from the Strait of Florida. Means of summer temperature which indicate roughly, not quite exactly, for higher temperatures, the mean amount of heat derived from the sun by direct radiation: the heat derived from all other sources, have been reduced from many thousands of isolated observations, and their results incorporated in an admirable and careful paper by Dr. A. Petermann (Geographische Mittheilungen, 1870).

The curves on the accompanying map, copied from Petermann, explain at a glance the distribution of abnormal temperature along the coasts of Western Europe, and indicate unmistakeably the source and direction of the warm current. One point only remained in doubt, namely, the depth to which the temperature of the ocean is affected by the Gulf Stream water. Now that there has been time to correlate and compare the large series of invaluable observations made with consummate skill and care by Captain Calver, R.N., during the *Porcupine* Expedition, this question may be considered solved over a considerable area, and the depth of the Gulf Stream off the west coast of Great Britain and France determined at about 800 fathoms (4,800 feet). This is so very im-

* My colleague, Dr. Carpenter, in many interesting communications on the temperature results of the *Porcupine* expedition (NATURE, Vol. i., p. 490, &c., &c.), denies that the Gulf Stream exercises any influence upon the temperature of the basin of the North Atlantic, and doubts whether it reaches the coast of Europe at all. He attributes the differences of temperature between different zones of depth to "a great general movement of equatorial water towards the polar area, of which movement the Gulf Stream contributes a peculiar case, modified by local conditions." And if I understand him aright, he supposes that this general movement is produced by some cause analogous to that which produces the general circulation in the atmosphere. I am sorry to be obliged to dissent so completely from his view on this point.

portant a matter in connection with the distribution of animal life and the other conditions of the problem at present more especially before us, and there are still so many wide differences of opinion with regard to it, even among competent authorities, that, at the risk of repeating a good deal that you know already, I will explain to you as simply as I can what appears to me to be the present state of knowledge with regard to it. This must be considered, however, merely an outline sketch, and when a phenomenon is represented as a sole cause or a sole result, I mean simply to convey that it is a cause or result so paramount as to reduce all accessories to insignificance.

The ultimate source of the Gulf Stream is undoubtedly, as has been specially insisted upon by Sir John Herschel, the equatorial current of the Atlantic, the drift of the trade winds. The path of that portion which trends north-eastwards is determined by the great initial velocity of the equatorial water which escapes from the Strait of Florida. The glory of the Gulf Stream, as it issues from the Strait, has been the theme of every physical geographer; and Mr. James Croll, in a valuable paper in the February number of the *Philosophical Magazine* on Ocean Currents, has entered into a careful examination of the actual amount of heat conveyed by the Gulf Stream from the Tropics into Temperate and Arctic regions. Mr. Croll calculates the Gulf Stream as equal to a stream of water fifty miles broad and 1,000 feet deep flowing at a rate of four miles an hour, consequently conveying 5,575,680,000,000 cubic feet of water per hour, or 133,816,320,000,000 cubic feet per day.

This mass of water has a mean temperature of 18° C. as it passes out of the Gulf, and on its northern journey it is cooled down to 4·5°, thus losing heat to the amount of 13·5° C. The total quantity of heat, therefore, transferred from the equatorial regions per day amounts to something like 154,959,300,000,000,000,000 foot-pounds.

This is nearly equal to the whole of the heat received from the sun by the Arctic regions, and reduced by a half to avoid all possibility of exaggeration, it is still equal to one-fifth of the whole amount of heat received from the sun by the entire area of the North Atlantic.

The basin of the North Atlantic forms a kind of *cul-de-sac*, and while a large portion of the Gulf Stream water, finding no free outlet towards the north-east, turns southwards at the Azores, the remainder, instead of thinning off, has rather a tendency to accumulate in the northern portions of the trough. We accordingly find that it has a depth off the west coast of Ireland of at least 4,800 feet, with an unknown lateral extension. There are no data as yet to determine the rate of the branch of the Gulf Stream which sweeps round the coast of Western Europe and into the Arctic Sea, but it must be very slow, for even so far south as at lat. 42° N. it has lost all effect upon navigation, its character as a constant current being entirely masked on the surface by the drift of the anti-trades, which has nearly the same direction.

The Gulf Stream is thus a constant "river" of hot water, forced into a particular direction by the rotation of the earth, by the constant winds, and by the configuration of the land; and accumulated and modelled by the confined basin of the North Atlantic and Arctic Sea. The cold water which replaces it is supplied under very different conditions.

Sea water increases steadily in density as the temperature falls till it reaches its freezing point, about 3° C.; the coldest water, therefore, lies at the bottom, and if over any region warm water be removed by any cause from the surface, as for instance in the case of the equatorial current and the Gulf Stream, its place will be supplied by a general indraught beneath of water from the coldest and heaviest, and consequently usually from the deepest sources from which it can be brought in by gravitation. The cold water is, however, merely drawing in to supply a vacancy, and there is no special reason why it should follow one ingress rather than another. From the low initial velocity of polar water it will tend to flow westwards in passing into lower latitudes, but that tendency will probably be entirely subordinate to specific weight in determining the course of the cold influx and the distribution of layers of water of different temperatures.

As cold water can gravitate into the deeper parts of the ocean from all directions, it is only under peculiar circumstances that any movement having the character of a current will be induced; these circumstances occur, however, in the confined and contracted communication between the North Atlantic and the Arctic Sea. Between Cape Farewell and North Cape there are only two channels of any considerable depth, one very narrow along the east coast of Iceland, and the other along the east coast of Greenland. The shallow part of the sea is entirely occupied, at all events during summer, by the warm water of the Gulf Stream, except at one point, where a rapid current of cold water, very restricted and very shallow, sweeps round the south of Spitzbergen and then dips under the Gulf Stream water at the northern entrance of the German Ocean.

This cold flow, at first a current, finally a mere indraught, affects greatly the temperature of the North Sea; but it is entirely lost, for the slight current which is again produced by the great contraction at the Straits of Dover has a summer temperature of 7·5° C. The path of this cold indraught from Spitzbergen may be readily traced on the map by the depression in the surface isothermal lines; and in dredging by the abundance of gigantic amphipodous and isopodous Crustaceans and other well-known Arctic animal forms. The other two Arctic currents along the coasts of Iceland and Greenland are likewise very apparent, taking a slightly western direction from their low initial velocity.

But while the communication between the North Atlantic and the Arctic Sea itself, a second *cul-de-sac*, is so restricted, limiting the interchange of warm and cold water in the normal direction of the flow of the Gulf Stream, and causing the diversion of a large part of the stream to the southwards, the communication with the Antarctic basin is as open as the day, a continuous and wide valley of upwards of 2,000 fathoms in depth stretching northwards along the western coast of Africa and Europe.

That the southern cold water wells up into this valley there could be little doubt from the form of the ground, but here again we have curious corroborative evidence on the map in the remarkable reversal of the curves of the surface isotherms. The temperature of the bottom water at 1,230 fathoms off Rockall is 3·22° C., exactly the same as that of water at the same depth in the serial sounding, lat. 47° 38′ N., long. 12° 08′ W., in the Bay of Biscay, which affords a strong presumption that the water in both cases is derived from the same source; and the bottom water off Rockall is warmer than the bottom water in the Bay of Biscay (2° 5′ C), while a cordon of temperature soundings drawn from the north-west of Scotland to a point on the Iceland shallow, gives no temperature lower than 6·5° C. This entirely precludes the idea that the low temperature of the bottom water of the Bay of Biscay is due to any portion of the Spitzbergen current passing down the west coast of Scotland; and as the cold current of the east of Iceland passes southwards considerably farther to the westward, as indicated on the map by the successive depressions in the surface isotherms, the balance of probability seems to be in favour of the view that the conditions of temperature and the slow movement of this vast mass of moderately cold water, nearly two statute miles in depth, are to be referred to an Antarctic rather than to an Arctic origin.

The water of the North Atlantic thus consists first of a great sheet of warm water, the general northerly reflux of the equatorial current, the most marked portion of it passing through the Strait of Florida, and the whole generally called the Gulf Stream, of varying depth, but attaining off the west coast of Ireland and Spain a depth of 800 to 900 fathoms. Secondly, of a general indraught of Antarctic water compensating at all events that part of the Gulf Stream which is deflected southwards; and thirdly, of a comparatively small quantity of Arctic water which, flowing through two or three narrow channels, replaces that portion of the Gulf Stream which makes its way into the Arctic Sea. As I have already said, the Gulf Stream loses an enormous amount of heat in its northern tour. At, say a point 200 miles west of Ushant, where the observations at the greatest depth were made on board the *Porcupine*, a section of the water of the Atlantic shows three surfaces, at which interchange of temperature is taking place. 1. The surface of the sea, that is to say, upper surface of the Gulf Stream layer, is losing heat rapidly, *a* by radiation, *b* by contact with a layer of air, which is in constant motion, and perpetually being cooled by convection; and *c* by the conversion of water into vapour. As the cooling of the Gulf Stream layer takes place principally at the surface, the temperature of the mass is kept pretty uniform by convection. 2. The band of contact of the lower surface of the Gulf Stream water with the upper surface of the water of the cold indraught. Here the interchange of temperature must be very slow, though that it does take place is shown by the slight depression of the surface isotherms over the principal paths of the indraught. The cold water being below, convection in the ordinary sense cannot occur, and interchange of temperature must depend upon conduction and diffusion, causes which in the case of masses of water of such depth must be almost secular in their action, and probably to a much greater extent upon mixture produced by local currents and by the tides. 3. The third surface is that of contact between the cold indraught and the bottom of the sea. The temperature of the surface of the earth is calculated at about 11°C., but it would be completely cooled down by anything like a movement and constant renewal of cold water; all we can say, therefore, is that contact with the bottom can never be a source of depression of temperature. As a general result, the Gulf Stream water is nearly uniform in temperature throughout the greater part of its depth; there is a marked zone of intermixture at the junction between the warm water and the cold, and the water of the cold indraught is regularly stratified by gravitation; so that in deep water the contour lines of the sea bottom are, speaking generally, lines of equal temperature. Keeping in view the enormous influence which ocean currents exercise in the distribution of climate at the present time, I think it is scarcely going too far to suppose that such currents, movement communicated to the water by constant winds, existed at all geological periods as the great means, I had almost said the sole means, of distributing heat in the ocean, and thus producing general oceanic circulation; they must have existed, in fact, wherever equatorial land interrupted the path of the drift of the trade winds. Wherever a warm current was deflected to north or south from the equatorial belt, a polar indraught crept in beneath to supply its place; and the ocean consequently consisted, as in the Atlantic and doubtless in the Pacific at the present day, of an upper warm stratum and a lower layer of cold water, becoming gradually colder with increasing depth. Wherever such conditions existed it is plain that mere vertical oscillations must have produced very decided changes of climate, through only a small number of degrees, but still very marked if the oscillation affected merely a portion of the cold underlying water, but enormous if it were sufficient to raise or depress the bottom of the sea, the principal theatre of animal life, so as to shift it from the cold layer into the warm, or from the warm layer into the cold.

One of the most striking phenomena connected with the distribution of heat in the North Atlantic is the case of the Shallow including the Hebrides, Orkney and Shetland Islands, and the Faroes, stretching westwards and northwards nearly to Iceland. The average depth is about 500 fathoms, and the Gulf Stream, which has a depth in these latitudes in summer of from 600 to 700 fathoms, occupies the whole of it, giving an abnormal temperature of something like 7° C. Owing to the peculiar conformation of the basin of the German Ocean, a tongue of cold water, with a bottom temperature of – 1° C. creeps into the valley between Scotland and Faroe, where it is overlaid by a stratum of Gulf Stream water, 150 fathoms thick. At the western mouth of the valley the cold water is banked in and retained by the water of the Gulf Stream, which is slowly passing the entrance of the gorge, giving a repetition, on a small scale, of the curious phenomenon described by Prof. Bache, off the coast of Massachusetts, as the "cold wall." My colleague, Dr. Carpenter, has conveniently called these two neighbouring districts, where the thermometer indicates 7° C. and – 1° C. respectively, the warm and cold areas. A depression, affecting that region of 250 fathoms equal to that which admitted of the accumulation of post-tertiary shells on Moel Tryfaen, would produce an extraordinary effect on its climate. In the first place, by mere subsidence, the Gulf Stream not reaching the bottom but flowing over a band of cold water, the temperature of the warm area would be reduced to, say, 3° C., and that of the cold, by an indraught of deeper water from the north, to – 2° C., but the Gulf Stream would no longer bank out the cold indraught from the north-east; which, in that case, passing down a deep open channel from the deep soundings to the west of the Loffotens, would spread along the bottom on the west coast of Scotland and Iceland, and greatly reduce its temperature, and probably entirely alter its fauna.

WYVILLE THOMSON

NOTES

WE have reason to believe that Professor Sir Wm. Thomson will be the next President of the British Association.

WE learn that the Royal Commission on Scientific Instruction and the Advancement of Science, which has met regularly two days a week, have now adjourned over the recess.

THE Royal Astronomical Society has issued a list of the members of the various learned Societies who propose to take part in the observations of the approaching total eclipse of the sun.

WE learn from the last number of the *Revue des Cours Scientifiques* which has reached us—that for the 24th inst.—that on Monday last week the Paris Academy of Sciences continued, in *comité secret*, the discussion of Mr. Darwin's nomination to fill the vacancy in the Zoological Section caused by the death of Purkinge. M. Milne-Edwards first spoke in his favour. While insisting on his own absolute opposition to evolutional doctrines, he rendered homage to the value of the special works of Mr. Darwin, especially the theory of the formation of coral islands. M. Elie de Beaumont also attested the value of this theory, and remarked that Mr. Darwin had done good work which he had spoiled by dangerous and unfounded speculations. He thought he should not be elected until he had renounced them. M. Emile Blanchard, who spoke for more than an hour, was very severe upon Mr. Darwin, styling him an "amateur intelligent," a remark capped by M. Elie de Beaumont (it is stated), who cried out, to the great indignation of M. de Quatrefages, "*C'est de la science mousseuse.*" M. de Quatrefages promised to answer M. Blanchard point by point on Monday last.

THE Archæological Congress is now in full swing at Leicester, one of the most interesting towns and localities which it is possible for such a Congress to visit. The proceedings commenced on Tuesday with an address presented by the Mayor and Corporation, followed by visits to some of the objects of interest in the town. On Wednesday the section met at the usual time, and the Abbey was subsequently visited. To-day (Thursday) there are excursions to Ashby de-la-Zouch, Tutbury, Tamworth, and Polesworth, and a *conversazione* in the evening. The future work is as follows:—Friday—Meeting of members for business; meetings of sections; excursion to Kirby Muxloe Castle; *conversazione* in the evening. Saturday—Excursion to Groby; Bradgate Park, Ulverscroft Priory, Woodhouse Chapel, Beaumanor Park, and Grace Dieu. Monday—Meetings of sections; excursion to Melton Mowbray and Oakham; *conversazione* in the evening. Tuesday—Meetings of sections; general concluding meeting.

PROFESSOR HELMHOLTZ will take up his new duties at Berlin in April, 1871.

THE Geological Survey of India, supported by the Government, have recently issued the completing part of the sixth volume of its "Memoirs." It contains a valuable paper by Mr. W. T. Blanford on the geology of the Taptee and Lower Nerbudda valleys in Western India, illustrated by maps, plates, and numerous woodcuts. Mr. Blanford describes the geology and physical geography of the country generally, giving first an account of the results obtained by previous observers, with references to their works, followed by more precise details of later observations, thereby furnishing an exceedingly valuable monograph. The view of a dyke at Koteda, near Goojree, exhibiting the trap in curved columnar masses, is a charming little picture. Mr. A. B. Wynne gives a description of some frog-beds exposed in Bombay Island in 1867. Numerous well-preserved skeletons of Oxyglossus pusillus (*Rana pusillarius*) were found associated with ribbed fragments of plants, and large, shapeless, structureless pieces of carbonised vegetable organisms. The skeletons were found in all kinds of postures: the hind legs extended, crossed, contracted, or twisted. Dr. Ferdinand Stoliczka, the palæontologist of the survey, gives a brief sketch of the osteology of the frogs discovered, with engravings of several specimens. The same gentleman, in two new parts of the "Palæontologia Indica," just issued, has given engravings and descriptions of some of the Gasteropoda of the Cretaceous rocks of southern India. This work, published at the expense of the Indian Government, is intended to comprise figures and descriptions of the organic remains found during the progress of the survey. The new parts complete the second volume of the Cretaceous fauna of India, with useful tables and an index of species. Mr. Thomas Oldham, the indefatigable superintendent of the survey, has commenced the publication of "Records" of the proceedings of the officers in various parts of India, of which the first four numbers are before us, containing much interesting geological information.

DEATH is hard on the medical profession just now. James Copland, M.D., F.R.S., died on the 17th inst., at the age of seventy-eight. From an obituary notice in the *British Medical Journal*, we learn that his principal claim to fame rests mainly on his performance of that gigantic work "The Medical Dictionary"—which he undertook, and carried to completion after a labour of thirty years. On finishing this work, he wrote in the preface that his labours, which had been incessant for many years, had been persisted in under circumstances and contingencies which few could have endured. Every line in it was written by his own hand, and all the proofs were carefully read and corrected by himself. The Dictionary is a cyclopædic *résumé* of all that has been written on the various subjects treated in it, from the earliest days of medicine down to modern times, with copious references to all the sources of information; and with all this are given the opinions which the author's observation and experience had led him to form. That one man should have undertaken and, labouring single-handed or nearly so, completed such a work, is indeed a remarkable fact. The work is a monument of calm energy and self-reliance, such as is but rarely met with.

NATURAL phenomena must be regarded by the engineer in the tropics. Here the boring worm will teach him salutary caution. In the East we have seen a railway train stopped on an incline by locusts. The locusts have a fancy for sitting on the rails, and when the engine-wheel touches them they are crushed, leaving the rails so oily that the engine slips. On one line, in the locust season, sand-boxes are used with the locomotive. Oysters are, however, a newly recorded enemy to the engineer. Some gourmand suggested the harbour of Tuticorin as a suitable place for oyster beds, and the Madras Government, doubtless appreciative of the value of oysters either for eating or for pearls, turned a deaf ear to remonstrance. Time has, however, justified the remonstrants, for, though the projectors have got an abundant supply of oysters, the harbour of Tuticorin is now said to be in danger of total destruction by the growth of the oyster beds, and the attention of the Government is seriously directed to cross the love of the oysters. The Madras coast is so ill-provided that harbours are more valuable than oysters, and a campaign will be directed against the latter, although the revenue authorities hanker after the taxes on the pearl fishery.

AN experiment, performed by M. J. M. Philipeaux, in which he transplanted one of the incisor teeth of a guinea-pig into the comb of a cock, has been referred to by M. P. Bert in his "Thesis on Animal Grafting," but M. Philipeaux thought that the examination of the specimen with an account of the experiment would be interesting to the members of the Society of Biology, and he accordingly submitted it to their inspection, with the following observations:—On January 13, 1853, M. Philipeaux, after having made an incision into the head of a young cock, introduced into it the incisor tooth of a guinea-pig that had been born a few hours previously. The tooth, very complete and furnished with its bulb, was so placed that the bulb was at the bottom of the wound and the extremity of the tooth turned outwards. On the day the experiment was made the tooth was eight millimetres long and two millimetres thick. The cock was killed ten months after the operation. The tooth, which, on the day of operation, was entirely concealed and covered in the wound, projected, when the animal was killed, five millimetres from the surface. M. Philipeaux had dissected out in the specimen the whole length of the tooth, and found that it measured no less than thirteen millimetres; it had consequently grown five millimetres. The interest of this experiment, which in other respects resembles those of Hunter and Sir A. Cooper, in which the spur of the cock was transplanted to the comb, consists in the circumstance that here a graft was accomplished in one animal of a part belonging to another, belonging to an entirely different zoological class.

WE are sorry to announce the death of Von Graefe, the most distinguished oculist in Europe. Iridectomy, the contribution to ophthalmic surgery with which his name is chiefly associated, was but one out of a multitude of operations which made his clinique at Berlin the resort of persons labouring under eye-diseases from all parts of the world.

WE have received from the Government of Victoria a most valuable collection of the Mineral Statistics of that Colony. Among the appendices is an important illustrated paper by Mr. Ulrich, F.G.S., entitled "Contributions to the Mineralogy of Victoria." We shall return, if space permits, to this interesting State paper. In the meantime, we may state that the total value of the gold raised in the Colony to the end of last year has been 152,706, 120*l*.; and that diamonds, rubies, sapphires, topaz, &c., are now being found.

WE learn from the *British Medical Journal* that M. Duruy (late Minister of Public Instruction), M. Nélaton (the great surgical celebrity of Paris), M. Husson (commonly called Sa Majesté l'Assistance Publique), and M. Milne-Edwards (the well-known semi-English Frenchman and philosopher), are at the head of a movement to found an *Ecole de Médecine* specially or women. The programme has been submitted to the Empress, accompanied by a request that she should become patron of the projected school. In France it is argued that there really is scope or something of this kind. It is alleged that the practice of medicine, from the present dull level of all being competent practitioners, is in a bad state; that it is necessary to institute a new body of practitioners, competent for drudgery and ordinary emergencies, but not competent for consulting practice. It is said that the mass of the profession are far too well educated, and that it is only by having a dash of the charlatan that even a good man can rise above the ranks. This surely is throwing a new light on the question.

THE Abbé Moigno announces in the last number of *Les Mondes* that the stone knives with which Joshua performed the rite of circumcision have been found in his tomb by the Abbé Richard. The account, which concludes as follows, we commend to the special attention of Mr. John Evans and Mr. Sorby:—"Voici donc qu'un des faits historiques les plus singuliers de la Bible a reçu la confirmation la plus éclatante, et que nous entrons en possession de silex taillés il y a 3,550 ans, plus vieux bien certainement, nous le prouverons jusqu'à l'évidence, (!) que les silex taillés de la vallée de la Somme ou des grottes d'Aurignac. Qui sait même si le spectroscope manié par des mains aussi habiles que celles de M. Sorby ne nous démontrera pas la présence, sur quelques-uns de ces silex, du sang de la circoncision." (!!)

OUR readers will recollect the important part played by the telegraph in the Seven Days' War, the introduction of this new arm, so to speak, enabling von Moltke to control all the strategical combinations with unerring accuracy, from a small room in Berlin. To the telegraph the Prussians have now added the balloon, and already we hear that the French army encamped in the environs of Metz have been surveyed with the greatest care. Surely if strategy is to play the part it did in former times in future battles, given two armies, one of which is, by means of a balloon, in electric communication with the Head-quarter Staff, in perfect knowledge of the numbers, at any one point, and the movements of the other, its success must be assured.

PERSONS wanting chemical and philosophical materials and apparatus from Germany, already experience a delay from the interrupted communications even in this beginning of hostilities.

THE heat has been so great at Dowlashéram, in the Madras Presidency, in June, that the Indian papers report many birds have died of sun-stroke.

AMONG the scientific labours of the Government in India in the departments of agriculture and horticulture, which have given such good results in tea and cinchona, may be recorded a garden at Raneekhet, the new hill station above Simla, for supplying vegetables to the troops, and an experimental cotton garden in the Boolundshur district. One chief object of the latter is to test the application of irrigation to various descriptions of cotton seed. Some appear to require water, and some to be injured, but there is no scientific record.

IN the progress of public opinion the public of Madras have come to the conclusion that the monkeys of that city, formerly held sacred, are a nuisance, and the municipality has taken measures to deport them. This requires—first, that they shall be caught. When caught they are to be tenderly treated; but for fear of their early return the aid of modern science is to be called in, and they are to be conveyed by railway trains to Tiruputty. In the distribution of animals the naturalist has thought fit to make little account of the railway, which may effect a displacement of the monkeys of India.

THE necessary operations for the construction of the sea-wall of the New Brighton Aquarium are progressing. It is expected that the aquarium will be completed and furnished by the spring of next year.

"SPONTANEOUS Generation and the Hypothesis of Physiological Units," by Herbert Spencer, is a reprint of a paper intended for the *North American Review*, in reply to an article that appeared in that journal entitled "Philosophical Biology," in which Mr. Spencer thinks that his views on the origin of life are not correctly represented.

THE *Scotsman* describes a Mirage in the Firth of Forth, the most extraordinary instance which can be remembered, which occurred on Friday afternoon. The day was very hot and sultry, and there was a peculiarity about the atmosphere which is seldom observed in this country. About midday a thin, clear, and transparent kind of vapour, through which the surrounding objects began to make their appearance in the most fantastic and grotesque shapes imaginable, settled over the sea. The phantasmagoria were principally confined to the mouth of the firth; but at one time they embraced the whole of the Fife coast as far as the eye could reach, town, village, and hamlet being depicted high up on the horizon with remarkable distinctness. Though the whole coast seemed at least half-way up the horizon, the appearances presented by the towns were very different, some of them having the houses inverted, while others appeared in the natural position. The Bass Rock, the Isle of May, and the rocks around Dunbar harbour, however, attracted most attention, both from their proximity and from the extraordinary forms which they assumed. The Bass, which at one time seemed to lie flat upon the sea, suddenly shot up into a tall spiral column, apparently ten times its usual height, surrounded by battlements rising tier on tier, and presenting a most imposing spectacle. As usual, however, the most fantastic appearances were presented by the May, which, in the course of the afternoon, underwent an almost innumerable series of phantasmagoric transformations. At one time it was apparently as round as a circle, at another seemingly draw out for miles against the horizon; now flat upon the water, then rising to ten times its usual height; occasionally portions appeared to break off and sail away, then to return and unite again—all within the space of a few minutes. Vessels in the offing appeared double—one on the water, and another inverted in the air; and in one instance three figures of one vessel were distinctly visible—one inverted, another on the sea, and a third in its natural position between the two. The fishing boats proceeding to sea in the evening underwent the same transformations when only a few yards off the shore, the double appearance being distinctly visible within a certain distance. The rocks at the harbour also seemed to play fantastic tricks, opening and shutting, rising and falling, with apparent regularity. These extraordinary illusions lasted from midday till night-fall, and excited great interest among the inhabitants of Dunbar, numbers of whom collected in the Castle Park and at the harbour for the purpose of witnessing the phenomena.

WE have received from M. J. L. Soubeiran an interesting account which he has communicated to the Société Impériale d'Acclimatation, of the progress of pisciculture in the Neilgherries, based on Mr. Day's paper in the Proceedings of the Zoological Society.

HOPKINS versus DELAUNAY

WE have received from Archdeacon Pratt a copy of a paper communicated to the *Philosophical Magazine* on Delaunay's objection to Hopkins's method of determining the thickness of the earth's crust by the precession and nutation of the earth's axis.

The archdeacon, on this most important question, states :—

"I am ready to allow, and so would Mr. Hopkins have allowed, that if the crust of the earth revolved round a steady axis, always parallel to itself in space, and if at some particular epoch a difference existed between the rate of movement of the crust and of the fluid within it, the resulting friction would gradually destroy this difference and bring about a conformity in the motion of both parts. I will even go further, and allow that the effect of the internal friction and viscidity of the fluid may be such that the resulting rotary motion may be the same as that which the whole mass would have had at the epoch if it had suddenly become one solid body and thereby suddenly retarded the rotation."

He thus illustrates his position :—

"Suppose a spherical shell or crust of mass C to have within it a solid spherical nucleus, of radius b and mass N, fitting it exactly; and the crust to receive an angular velocity of rotation around an axis fixed in the crust, the nucleus at that moment having no angular velocity; but suppose that a slight force of friction between the surfaces gradually generates a rotary motion in the nucleus; and suppose this force to vary as the difference between the angular velocities of the crust and nucleus—that is, of the surfaces in contact. Let ω and ω' be the angular velocities at the time t, k and k' the radii of gyration of the two bodies, F $(\omega - \omega')$ the force at the equator of the nucleus which represents the friction between it and the crust. Then the equations of motion are

$$\frac{d\omega}{dt} = -\frac{Fb}{Ck^2}(\omega - \omega'), \quad \frac{d\omega'}{dt} = \frac{Fb}{Nk'^2}(\omega - \omega') \quad . \quad . \quad . \quad (1)$$

Suppose also that β would have been the angular velocity, when the primitive impulse was given, on the hypothesis of the crust and nucleus being rigidly connected so as to be one mass. Then

$$\beta(Ck^2 + Nk'^2) = \alpha Ck^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Subtracting the second of equations (1) from the first, putting

$$Fb\left(\frac{1}{Ck^2} + \frac{1}{Nk'^2}\right) = c, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and integrating, we have

$$\omega - \omega' = \text{const.} \times e^{-ct}.$$

When $t = 0$, $\omega = \alpha$ and $\omega' = 0$;

$$\therefore \omega - \omega' = \alpha e^{-ct}.$$

Hence, by the first of equations (1),

$$\frac{d\omega}{dt} = -\frac{Fb\alpha}{Ck^2}e^{-ct} = -\frac{Nk'^2 c\alpha}{Ck^2 + Nk'^2}e^{-ct}, \text{ by (3)};$$

$$\therefore \omega = \alpha - \frac{Nk'^2}{Ck^2 + Nk'^2}\alpha(1 - e^{-ct});$$

and also

$$= \beta\left(1 + \frac{Nk'^2}{Ck^2}e^{-ct}\right), \text{ by (2)}"$$

The paper then continues :—"The first of these expressions shows that the angular velocity of the crust begins with α; and when ct becomes very large indeed, it is reduced to β. Hence the effect of the constant friction of the nucleus against the inner surface of the crust is *at last* to reduce the velocity of the crust to what it would have been at first if the crust and nucleus had been one solid mass.

"We may conclude perhaps that the same effect would be produced, though in a much longer time, if the interior were not a solid sphere, but a fluid mass.

"The above reasoning shows that if the disturbing force producing precession and nutation did not exist, and the interior of the earth were fluid (whatever the thickness of the crust), it may be fairly assumed that the motion of rotation of the crust would now, the earth having existed so many ages, be exactly what it would have been had the earth been one solid mass, all difference of motion having been long ago annihilated by the internal friction and viscidity.

"But the disturbing force producing precession and nutation does exist. It consists of two parts, one constant and the other variable and periodical. The constant part is that which produces the steady precession of the axis (and which I will call for convenience the precessional force); the other produces the nutation. I will consider the precession first. Suppose now, for the sake of argument, that at the present moment, as M. Delaunay imagines, the crust and the fluid are revolving precisely as one mass, all previous differences of motion, even under the action of the disturbing force which produces precession and nutation, having been annihilated by friction and viscidity. I ask—What will be the action of the precessional force from this moment? It tends to draw the pole of the crust towards the pole of the ecliptic: and this tendency, as mathematical physicists well understand, combined with the rotary motion of the crust, produces this singular result, viz., the pole does not move towards the pole of the ecliptic, but shifts in a direction at right angles to the line joining the poles towards the west; so that the inclination of the axis to the ecliptic remains constant, but the axis shifts towards the west. The space through which it shifts in an infinitesimal portion of time varies as the length of the time and the force directly, and as the inertia of the mass to be moved inversely. The inertia of the mass depends upon the thickness of the crust only; for the friction of the fluid against the inner surface of the crust (which might, as I have shown, in the course of years, produce a sensible effect) cannot do so during the infinitesimal portion of time I am considering before the precession is actually produced. The precessional force has its full effect in producing the precession of the solid crust, the fluid not having time to diminish that effect before the axis has assumed a new position; and in this new position of the axis the precessional force is precisely the same in amount as before, to go on causing the precession as before. The precessional force is, in fact, ever alive and active, and shows this in incessantly producing the effect I have described; and the precession goes on steadily, the amount of it depending upon the mass of the crust thus moved, which the fluid has not time to retard or lessen. M. Delaunay says that 'the additional motion due to the above-mentioned causes (the disturbing forces which give rise to precession and nutation) is of such slowness, that the fluid mass which constitutes the interior of the globe must follow along with the crust which confines it, exactly as if the whole formed one solid mass throughout." In reply to this, I say that it is not the slowness of the motion, but the want of solid connection between the crust and the fluid in contact with it that affects the problem. The motion, whatever its amount, is incessantly being generated by the disturbing force, and owing to this want of solid connection, the friction of the fluid has not time during the successive moments during which the precession is generated, to stop or even sensibly to check it.

"It will thus be seen that at every instant the precessional force proceeding from the action of the sun and moon on the protuberant part of the earth's mass will, if the earth be a solid mass, have to move the whole mass; and if the earth have a solid crust only with a fluid interior, the force will have to move only the crust against the evanescent resistance of the fluid within during so short a space of time as it takes to produce precession. The resulting precessional motion will be different in the two cases; and therefore the actual amount of the precession which the earth's axis has (and which is a matter of observation) is a good test of the solidity or fluidity of the interior. This is Mr. Hopkins's method.

"The force producing nutation is much smaller, even at its maximum, than the precessional force. Its effect, however, is precisely the same in this respect—that it depends upon the mass of the solid crust, and in no respect upon the friction of the fluid within it, which has not time to influence the nutation before the nutation is actually produced.

"I do not here undertake to go into Mr. Hopkins's numerical calculations; I simply vindicate his method. I do not here consider what modification the elasticity of the solid material of the earth may have upon his numerical results. I conceive that it would have *no* effect, if the disturbing force were constant and there were no nutation. For, under the dragging influence (if I may so call it) of the constant precessional force, the solid material would be under a steady strain, and would communicate the effect of the force, continuously acting, from particle to particle of the solid part as if it were really rigid; and the resulting precessional motion would be greater or less as the mass of the solid part may be smaller or larger—that is, the solid crust thinner or thicker. But as the disturbing force is not constant, but variable, and there is constantly nutation of the axis as well as precession, the action above described will be somewhat modified;

and the elasticity of the solid material may be expected to have some influence on the result. This influence, however, will be minute, as the part of the disturbing force which is variable and produces nutation is very much smaller, even at its maximum, than the precessional force. The consideration of this matter, however, has no bearing upon the validity or not of Mr. Hopkins's method, but simply upon the numerical value of his final result, not upon the question of the fluidity or solidity of the earth's mass."

The Archdeacon is of opinion "that the strictures of M. Delaunay upon this method, which the genius of Mr. Hopkins devised, betray an oversight of the real point upon which the success of his method depends, and that this method stands unimpaired."

SCIENTIFIC SERIALS

THE *Geological Magazine* for July (No. 73) contains rather ewer original articles than usual, but what there are will be found interesting. The series of notices of eminent living geologists is continued in a notice of one of the most accomplished of the number, Professor John Phillips, of whom we have a good biography, but a very unsatisfactory portrait. Mr. Carruthers gives a notice of the so-called fossil forest near Cairo; he distinguishes a new species of *Nicolia* (*N. owenii*), and illustrates its microscopic structure as compared with that of the old species *N. ægyptiaca* Unger. — Mr. Kinahan communicates a paper containing a comparison of the geological features of Devon, Cornwall, and Galway, with a discussion of the means by which they have been produced; and Miss E. Hodgson a long disquisition on the origin and distribution of the granite-drift of the Furness district. The longest article in the journal is a report of Mr. David Forbes' lecture on Volcanoes, which will be read with much interest.

The *Journal of the Asiatic Society* for April, contains the following Natural History papers—Observations on some Indian and Malayan Amphibia and Reptilia, by Dr. F. Stoliczka. The species described in this paper have been partially collected by the author along the Burmese and Malayan coast, in Penang and Singapore, partially at the Nicobar and Andaman islands, only a few species are noticed from Java, and a few also from the N. W. Himalayas. Short notes on the geographical distribution, and on the general character of the amphibian and reptilian fauna of the Andamans and Nicobars, form a brief preface to the detailed descriptions. Complete lists of all the known species occurring on the two last-named groups of islands are appended. Dr. Stoliczka gave a short sketch of the relations existing between the Andaman and Nicobar reptilian fauna and that of Burma on the one, and that of Java, Sumatra, and the Philippine islands on the other hand. All these islands, he said, have many species in common. He also specially notices the very great number of viperine snakes (*Trimeresurus*) which are to be met with at the Nicobars, but fortunately these species appear to be less dangerous than continental forms usually are. The Nicobarese say that not a single fatal case has resulted from the bite of these *Trimeresurus*, and certainly all the specimens examined had a comparatively small poison-gland. The result of the bite is said to be only a swelling of the wounded part. Dr. Stoliczka also exhibited a specimen of the rare *Callophis intestinalis* obtained from Upper Burma. The species has the poison glands extending from the head to about one-third of the total length of the body, lying free in the cavity of the anterior part, and causing the heart to be much further removed backward than is generally the case in other species of snakes. The President thought there were one or two remarkable features in Dr. Stoliczka's interesting paper. One to which he particularly referred was the relative inefficiency of the poison in certain snakes of Penang and the Nicobars in comparison with the poison of the cognate species found in this country. He did not know whether the circumstances which rendered the possession of an invariably fatal weapon necessary to particular classes of snakes in the struggle for life, while others could maintain themselves without it, had yet received much attention. *A priori*, he thought, one would be disposed to expect that a poison which would disable without causing immediate death, would be more deterrent in its effects, and, therefore, more widely useful to its possessor, than one which killed instantly. At any rate it was curious to find some of the insular species of snakes, though provided with a perfect poison apparatus, much less fatal in the effect of their bite than other closely allied species in Bengal were. The investigation of the causes which had led to this difference ought to be attractive. A short discussion on the effects of snake poisoning ensued. Mr. Waldie desired to know what the symptoms resulted from the bite of the Nicobar vipers, and whether they are the same as are usually known to originate from the bite of other poisonous snakes. Dr. Stoliczka said that the Nicobarese only speak of a swelling of the bitten part, and that they exhibit very little fear of these snakes. Dr. Stoliczka also observed that the poison gland in the species of *Trimeresurus* which he had examined, has a simple glandular form without any appendages, but the skin forming it is very tough, and internally partitioned by numerous irregular lamellæ. The poison of the fresh snake was always present in a comparatively small quantity, and appeared less viscose than the Cobra poison. The differences between the effects of poisoning of the cobra and daboia had been pointed out by Dr. Fayrer.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, June 22.—II. "On the Physics of Arctic Ice as explanatory of the Glacial Remains in Scotland." By Dr. Robert Brown, F.R.G.S., &c. In this paper the author entered into an extended inquiry how far the formation of the boulder-clays and other glacial remains in Scotland and the North of England can be accounted for, on the theory of a great ice-covering having at one time overlain the country in much the same manner as it does now Greenland and other extreme Arctic countries. Taking the hypothesis of Agassiz as his groundwork, Dr. Brown entered into a minute description of the present glacier-system of Greenland, and the nature of Arctic ice-action; and into an inquiry how far glacial remains in Britain correspond with those at present in course of formation in Greenland and at the bottom of Baffins Bay, Davis Straits, and the fjords and bays adjoining these seas. These inquiries were commenced in the year 1861, and have been continued at intervals ever since up to the present summer in various portions of the Arctic regions, the Continent of Europe, in Great Britain, and in North America across to the Pacific. The results of these extended researches have led him to conclude—1. That the subazoic boulder-clay corresponds with the *moraine profonde* which underlies glaciers, and in all likelihood is the immediate base on which the ice-cap of Greenland rests. 2. That the fossiliferous, laminated, or brick-clays find their counterpart in the thick impalpable mud which the subglacial streams are pouring into the sea, filling up the fjords, even shoaling the sea far out, and absolutely in some cases turning the glaciers from their course into other valleys. Allowing the very moderate computation that this impalpable mud accumulates at the rate of only six inches per annum, a deposit of fifty feet in a century must form. If Scotland was at one time covered with an ice-cap, or had glaciers of any extent (as cannot be doubted), then this deposit must have been equally forming, and as a geological formation must be accounted for. No difference could be detected between this glacial mud and the present brick-clays, and every fact went to show that it was to this that we must look for the formation of these laminated fossiliferous clays. The amount of earth deposited on the bottom by icebergs was very insignificant indeed, and could in no degree account for the *boulder-clay*, though it was shown that much of the *boulder-drift* in *some places* could be so accounted for. It was, however, demonstrated that there was a great distinction between the boulders which belonged to the *moraine profonde* and those which were carried off on icebergs as part of the ordinary lateral moraines. The fjords, as already partially advocated in a paper in the Journal of the Royal Geographical Society (vol. xxxix.), he considered due to glacial action, the glaciers having taken possession of these fjords when they were mere valleys, when the coast was higher than now. He further showed that the American explorers are in error when they describe the coast of Greenland as rising to the north of 73°, and subsiding to the south of that parallel. There had been a former rise of the coast, and a fall was now in course of progress through the whole extent. Whether these had previously alternated with other rises and falls is not clearly evidenced by remains, but no doubt exists that a rise preceded the present subsidence. Numerous facts were adduced in support of this assertion. The remainder of Dr. Brown's paper was occupied in an attempt to apply the doctrines regarding the physical action of Arctic ice-action to account for the Scottish glacial remains, and to deduce therefrom evidence regarding the changes Scotland underwent during, and subsequent to, the glacial period.

Aeronautical Society, June 3.—Mr. James Glaisher, F.R.S., in the chair. The following extract from the minutes of a late meeting at Stafford House, was read by the Secretary. It was remarked how little had been done in this country either to demonstrate the possibility of navigating the air, or to prove its impracticability. Sir William Fairbairn observed that we know but little of the reaction or lifting power of various forms of screw blades in the atmosphere, relative to the force employed, though such experiments might be easily tried, and the data obtained. Mr. Brooke was of opinion that if a successful aërial machine were to be constructed, the most simple and obvious plan would be that of inclined surfaces, impelled forward horizontally. The most successful experiment that he had ever witnessed was upon this principle, the motive power being a wound-up watch spring, which, as long as the power lasted, sustained the machine; and further, that most large birds were capable, during long periods of their flight, of sustaining themselves exactly in this way. It was also remarked that we were practically ignorant of the correct laws of the sustaining power of inclined surfaces of different forms and areas, and this want of knowledge was a perpetual stumbling-block to those who were willing to spend time and money in experiments. From the fact that as the weight and size of birds increased, so did the relative wing area decrease, it would appear that the ratio of sustaining surface to weight or resistance was by no means in equal proportions. The Chairman stated that with respect to plane surfaces of various figures exposed to the direct impact of the wind, he had already been trying some experiments with such instruments as were at his disposal, and that by employing two anemometers at the same time, so as to be sure of comparative results, he found that the indication of force increased with the size of the surface; also in the two instruments, equal surfaces shaped into different contours, gave different results. These interesting experiments, so directly bearing upon the question of aërial propulsion and resistance, were still occupying his attention, but at present he could say nothing from actual experiment of the resistance of inclined surfaces of various forms. It was then proposed that an experimental fund should be raised by subscription, and that a suitable and well-finished anemometer should be constructed, having the means of instantly setting various plane surfaces at any desired angle, and capable of registering both horizontal and vertical force simultaneously for all degrees of inclination. The results to be published for the benefit of the Society. Upon this proposition being put to the meeting it was carried unanimously.

Ethnological Society, June 21.—Prof. Huxley, F.R.S., president, in the chair. Colonel Lane Fox made some remarks on the Dorchester dykes and Sinodun Hill, to which attention has recently been directed, and showed that the works are British, and not Roman. He stated that the demolition of these works had been arrested for the present.—Mr. David Forbes, F.R.S., read a paper on the Aymara Indians of Bolivia and Peru. He described them as a small, massive, thick-set race, with large heads and short limbs. The trunk is disproportionately large, and the capacity of the thorax is enormous, being adapted to meet the requirements of respiration in a rarefied atmosphere, as the Aymara lives at an elevation of from 8,000 to 16,000 feet above the sea-level. The proportions of the lower limbs are curious, the thigh being shorter than the leg; the heel is inconspicuous. In colour, the Aymara varies from copper-red to yellowish brown and blackish brown, according to the altitude at which he lives. Many of the customs of the Aymaras depend on their peculiar conditions of life. In consequence of the low boiling-point of water at such great altitudes, beans are rarely used, and the food consists chiefly of potatoes peculiarly prepared. Clay is added to the food, not for any nutritious matter in it, but apparently only to increase the bulk of the meal. In religion, the Aymaras are nominally Christians. They appear to have no system of writing. The discussion on this communication was supported by the President, Mr. E. G. Squier, Mr. Cull, Mr. Dendy, Mr. Bollaert, Mr. Harrison, and Mr. C. Markham. At the same meeting Dr. A. Campbell exhibited tracings of certain rock-inscriptions from British Guiana, and the Hon. E. G. Squier displayed a large collection of drawings, photographs, &c., from Peru.

Anthropological Society of London, June 14.—John Beddoe, M.D., president, in the chair.—Logan D. H. Russell, M.D., of Wilmington, Delaware, was elected a local secretary.—A paper, by Dr. Henry Hudson, was read "On the Irish Celt," in which the author depicted the mental and moral characteristics of that type, and drew conclusions as to the kind of government most suitable to such a people.—Mr. G. H. Kinahan contributed a paper "On the Race Elements of the Irish People." That paper entered largely into the pedigree of the chief families of Connaught and Munster, and treated of the effects of the Cromwellian and other confiscations.—The President (Dr. Beddoe) then read a paper "On the Kelts of Ireland." The principal points proved or indicated in it were the following:—That the Kelts known to the Greek and Latin authors, though they were a light-haired race as compared with the Italians, were darker than the Teutonic tribes, and that their physical type differed in other respects. That the Irish are, generally speaking, a dark-haired but light-eyed race, and that wherever there is much light hair it may be accounted for by a Danish or English cross. That the dark hair of the Irish may be, partly at least, attributed to the Gaelic Kelts. That there is less resemblance between the Irish, taken as a whole, and the Basques, who are generally considered to be the purest Iberians extant, than between the South Welsh and the Basques. That any Basque or Iberian element in Ireland is probably small, and can have only partially contributed to the prevalence of dark hair among the Western Irish. That Ugrian or Ligurian elements may also be present there. The paper was illustrated by minute details respecting the physical types in various parts of modern Ireland, including extensive observations on the colour of the eyes and hair; and the author exhibited a number of photographic and other portraits of Basques and of Bretons, Welshmen, Walloons, and other supposed descendants of the Keltic race.

Meteorological Society, June 15.—Ordinary meeting, Charles V. Walker, F.R.S., president, in the chair. Messrs. W. C. Ellis and Francis Nunes were elected Fellows, and Padre Prof. F. Denza was elected an Honorary Fellow of the society.—The following communications were made: "On the path of the large fireball of November 6th, 1869," by Prof. A. S. Herschel; "On the temperature of the air in Natal, South Africa," by R. J. Mann, M.D., F.R.A.S., &c.; "On the atmospheric pressure with relation to wind and rain," by R. Strachan, F.M.S., and "On the November meteors of 1869, as seen from the Mauritius," by Charles Meldrum, F.R.A.S.—The anniversary meeting was then held, and the report of the council on the present state of meteorological science both at home and abroad, also their report on the present state of the society, which now numbers 343 Ordinary, Life, and Honorary Fellows, and the treasurer's report were then read and adopted. The following is the result of the ballot for the officers and council for the ensuing year.—President, Charles T. Walker, F.R.S., F.R.A.S.; Vice-Presidents, Nathaniel Beardmore, C.E., F.R.S.; C. O. F. Cator; Robert J. Mann, M.D., F.R.A.S.; John W. Tripe, M.D. Treasurer, Henry Perigal, F.R.A.S. Trustees, Sir Antonio Brady, F.G.S., and S. W. Silver. Secretaries, Charles Brooke, F.R.S., F.R.C.S., and James Glaisher, F.R.S., F.R.A.S. Foreign secretaries, Lieut.-Colonel Alex. Strange, F.R.S., F.R.A.S. Council, Arthur Brewin, F.R.A.S., George Dives, F. W. Doggett, Henry S. Eaton, F. Gaster, Charles M. Gibson, Rev. Joseph B. Reade, M.A., F.R.S., W. Wilson Saunders, F.R.S., F.L.S., Thomas Sopwith, F.R.S., George J. Symons, S. C. Whitbread, F.R.S., F.R.A.S., E. O. W. Whitehouse, F.S.A., &c.

Statistical Society, June 23.—William Newmarch, F.R.S., president, in the chair. The following is the list of president, council, and officers, elected to serve for the ensuing year:—President, William Newmarch, F.R.S. Council, Major-General Balfour, C.B., Dr. Thomas Graham Balfour, F.R.S., R. Dudley Baxter, Samuel Brown, Hyde Clarke, D.C.L., L. H. Courtney, Sir C. Wentworth Dilke, Bart., M.P., Dr. W. Farr, F.R.S., W. Fowler, M.P., F. Galton, F.R.S., Right Hon. W. E. Gladstone, M.P., J. Glover, W. A. Guy, M.B., F.R.S., J. T. Hammick, F. Hendriks, J. Heywood, F.R.S., W. Barwick Hodge, F. Jourdan, Prof. Leone Levi, Sir Massey Lopes, Bart., M.P., W. G. Lumley, Q.C., LL.M., J. MacClelland, F. Purdy, Bernhard Samuelson, M.P., Col. W. H. Sykes, M.P., F.R.S., Ernest Seyd, W. Tayler, W. Pollard-Urquhart, M.P., Prof. Jacob Waley, J. Walter, M.P. Treasurer, J. T.

Hammick. Honorary Secretaries, W. Golden Lumley, Q.C., LL.M., F. Purdy, Prof. Jacob Waley.

BRISTOL

The Observing Astronomical Society.—Report of observations made by the members during the period from May 7 to July 6, 1870, inclusive. *Solar Phenomena*—Mr. John Birmingham, of Tuam, writes: "A remarkable obscuration of the sun was observed here on May 22. It lasted from sunrise to sunset, with a short interval in the afternoon of returning brightness. The sun was of a beautiful pink colour, though there was no fog whatever, and its light was so reduced as to permit a long observation of it through the telescope without the aid of a dark glass. I am informed that the same phenomenon was noticed in the South of England on the next day (the 23rd), and on that day also, but late in the afternoon, it was observed at Rohrbach (Moselle), and described by M. Hamant in a letter to the Scientific Association; so that the cause of the obscuration, whatever it was, seems to have been moving eastward and southward." Mr. T. W. Backhouse, of Sunderland, reports that in May "there was a remarkable case of a solar spot making a revolution round another. It occurred with respect to the two largest spots of a group that was half way across the northern zone on May 9. The smaller spot was south of the larger on the 7th at 3^h, but preceded it on the 12th at 21^h, the line joining the two spots having rotated through an angle of 80° or 90° in $5\frac{3}{4}$ days. This movement continued to the 15th, but this would be partly apparent owing to the group approaching the limb. By that time the larger spot was reduced to the size of the other. I cannot say whether the motion was a curve or a straight line, though it was probably the former; nor can I say which of the spots moved or whether both did. They were about 22,000 miles apart on the 9th, at 3^h; but on the 13th, at 20^h, they were 32,000 miles apart. One spot must therefore have moved, relatively to the other, about 34,000 miles in $4\frac{3}{4}$ days, or at the rate of 300 miles per hour." Mr. T. G. E. Elger, of Bedford, says: "The sun spots observed during June were, with the exception of one group, small and devoid of interest when compared with those seen in April and May. The largest spots were confined to the sun's northern hemisphere. Between the 8th and 15th the spots were all small; on the latter date there were only two groups on the disc, and these were insignificant. On the 19th a very remarkable spot was observed, it formed the preceding member of a large scattered group 2′ 52″ in length; its penumbra measured about 1′ 10″ in greatest diameter. At 10 A.M. an isolated mass of light, intensely bright, was remarked on the nucleus. This, at 2 P.M., formed a 'bridge' connecting adjacent sides of the umbra. The nucleus of this spot was very irregular in colour. At $5^h\ 15^m$ on the 19th the central portion was noted as brown and the border as black, and subsequently the variety of tint was still more marked. At 7 A.M. on the 21st, when the penumbra showed evident signs of cyclonic action, not more than half the area of the nucleus was black, the remainder was made up of patches of various shades of brown. The group disappeared at the limb on the 27th." The Rev. S. J. Johnson, of Crediton, observed numerous spots on the sun on May 13. There were then four groups with penumbræ close together. Mr. H. Michell Whitley, of Penarth, says: "June 21—I noted on the sun's disc one very large, round, and well-defined spot; on one side, however, the penumbra was invaded by two tongues of faculæ for a short distance, and in the centre of the umbra was a bright patch."

The Planet Saturn.—Mr. H. Michell Whitley repeatedly observed this object with his $6\frac{1}{4}$in. reflector. He says—"June 21. Air very unsteady, but after midnight better. The Ball—duller yellow than rings, equatorial zone yellow, north of this a pale red belt, and another farther north again, towards Pole much fainter and about midway. Pole of planet bluish grey, edges of ball slightly shaded; no other spots or markings. Ring A—Inferior in brightness to B; colour, pale yellow; no subdivisions or markings on it. Ball's division—Traced all round; widest and darkest, if at all, in Wansa; sharply defined. In colour it was not so black as the sky, but deeper than the crape ring across the ball; colour, dusky. Ring B—This ring was very bright for a short distance from its outer edge, which was verp sharply defined; colour gradually deepens and light fades towards inner edge; outer edge lemon yellow, duller and deeper inwards, strongly suspected to be streaky, but no actual subdivision seen. No line of light on inner edge of ring, which was not sharply defined. Ring C, or Crape Ring—Very delicate colour, dusky purple. I could with care, as a very fine object, trace the edge of the globe through it up to ring B, equally distinct in E and Wansa; no markings of any kind upon it.—June 28, 10^h to $11^h\ 15^m$. Power, 250; definition very fluttering. N. equatorial ruddy belt very distinct; equatorial yellow band the brightest part of the planet. Between the N. equatorial ruddy belt and N. Pole lay one or more very faint ruddy bands. Pole, pale bluish grey; no other markings. The Crape Ring very dark and distinct across the Ball.—July 2, 10^h. Definition very sharp; power 250. A glimpse observation. The two belts before mentioned very much plainer and darker than on June 21—28, and not of such a ruddy hue."

Lunar Observations.—Mr. John Birmingham, of Tuam, Ireland, reports that on June 6 he saw "A very marked central depression in the white spot of Linné though the terminator was so far away as the boundary between the Mare and the Palus Putridinus. The depression was rather east of the exact centre of the white spot, so that the western exterior slope was longer than the eastern." Mr. H. Michell Whitley has observed with great care many interesting and difficult lunar objects, and the results of his observations have been sent to Mr. W. R. Birt, F.R.A.S.

Winnecke's Comet.—Mr. George J. Walker, of Teignmouth, observed this body on June 5, 6, and 7. He says that "It looked like a tolerably bright nebula;" on the 6th, at $14^h\ 13^m$ the comet looked faint owing to the strong twilight.

Meteors.—Mr. G. J. Walker saw "a splendid meteor" on June 24. It traversed the greater part of the sky, and was much larger and brighter than Venus. It was of a blue colour. Mr. Walker adds, "I think it appeared a little to the right of Altair, and passed over Vega and on to the Pointers in Ursa Major; it had a magnificent train, and I think must have traversed an arc of about 120°. The time of its appearance, as well as I could make out from my watch, was $11^h\ 13^m$ G.M.T., and it may have been seven or eight seconds making its sweep over the heavens. I did not hear any sound with it." Mr. H. M. Whitley observed a brilliant meteor on June 29 at $11^h\ 30^m$. It was of the second magnitude. "Pale yellow; velocity very great."

A New Red Star.—Mr. John Birmingham has "frequently observed a red star in Cygnus, not, I believe, previously noticed; at least, it is not in Schjelterup's catalogue (Ast. Nach., No. 1,591), which gives a list of all the red stars known up to 1866. It is of a deep red, of about the 8th Mag., and is near a blue star of the same size. Its approximate position, compared with 32 Cygni, is about

R.A. $20^h\ 15^m\ 37^s$; Declin. + 47° 27′ 28″.

Occultation.—Mr. Walker witnessed the occultation of θ Libræ on June 11, and found that the exact time of disappearance was $9^h\ 27^m\ 55{\cdot}6^s$ G.M.T.

EDINBURGH

Botanical Society, May 12.—Sir Walter Elliot, president, in the chair.—The following communications were read:—Botanical Notes of a Journey through Spain and Portugal, by Mr. T. C. Archer; Botanical Notes on the Garden of Montserrat, Portugal, by Mr. T. C. Archer; Botanical Notes taken on the Rock of Gibraltar, by Mr. T. C. Archer; Report on the open-air Vegetation at the Royal Botanic Garden, by Mr. M'Nab.

June 9.—Sir Walter Elliot, president, in the chair. The following communications were read:—Notes on the Ipecacuanha Plant. By Dr. Gunning, Rio Janeiro. Dr. Gunning states that the Ipecacuanha plant is exceedingly scarce in the province of Rio Janeiro from having been pulled up, and no attention paid to its cultivation. It is exported from Sao Paulo, the province south of Rio, but chiefly from Matto Grosso, a thousand miles up the River Plate. At present Dr. Gunning is rearing a number of cuttings for transmission to India, where it is proposed to cultivate it extensively.—New and rare Mosses from Ben Lawers. By Dr. J. Stirton, Glasgow. In this paper the author reviewed the progress of discovery of mosses on Ben Lawers within the ten years, indicated in general terms the habitats of the rarer species, as well as their tendencies towards increased luxuriance, or gradual decay and extinction, and noticed the affinities between the Cryptogamic Flora of the mountain (Ben Lawers) and that of Scandinavia, more especially of the Dovrefield.—Notice of Grimmias, collected on Arthur Seat, near Edinburgh, by Mr. William Bell and Mr. Sadler. The authors described twelve species and several varieties of the genus Grimmia, as growing on Arthur Seat; noticed their distribution over the hill, and the kind of rocks on which they occurred.—Notes on some British Mosses. By Mr. Wm. Wilson. Mr. Wilson referred to the British species of *Andræa*, which he had revised for the second edition of his "Bryologia Britannica," and especially to *Didymodon jenneri*, a moss recently described and figured in the Society's Transactions. The latter he believed to be in no way specifically different from *Cynodontium polycarpon*.—On the Ferns found in the Valley of the Derwent. By Mr. T. W. Mawson. Mr. Mawson enumerated twenty-eight species and varieties of ferns as indigenous to the Valley of Derwentwater, including *Asplenium germanicum*, *A. septentrionale*, *Hymenophyllum wilsoni*, *Osmunda regalis*, *Ophioglossum vulgatum*, *Allosorus crispus*, &c.

PARIS

Academy of Sciences, July 18.—M. Bertrand communicated a paper by M. L. Painvin on the determination of the elements of the angle of inflexion of a developable surface defined by its tangential equations.—Several papers on physical subjects were presented, namely—an extract of a letter from M. De la Reve to M. Dumas on the magnetic rotatory powers of liquids; further researches upon electro-capillary action, and on the formation of crystallised oxychloride of copper and other analogous compounds by M. Becquerel; a memoir on the variations of temperature produced by the mixing of two liquids by M. H. Sainte-Claire Deville, in reply to the last communication by M. Jamin, and a reply by the same author to the criticisms of M. Jamin upon a memoir published in 1860; thermical researches upon the metallic character of hydrogen associated with palladium, and on a voltaic couple, in which hydrogen is the active metal by M. P. A. Favre; and a note by M. F. Lucas, communicated by M. E. Becquerel, on the possibility of obtaining fire signals visible at a great disrance, for which purpose the author proposes to employ an electric spark generated by an apparatus described by him.—M. de Saint-Venant presented a memoir on the elementary demonstration of the formula of propagation of a wave or intumescence in a prismatic canal, with remarks on the propagation of sound and light, on ressaults, and on the distinction of rivers and torrents.—A note was read by M. Sonrel on the photographic investigation of the sun at the Imperial Observatory of Paris.—MM. Becquerel and E. Becquerel presented a note on the observations of temperature made beneath the soil at the Garden of Plants from 1864-1870, by means of thermo-electric cables, with tables of results.—The following chemical papers were read:—Investigations upon the action of the chlorides of platinum, palladium, and gold upon the phosphines and arsines, by MM. A. Cahours and H. Gall; a note on the decomposition of oxalic acid by M. P. Carles, communicated by M. Bussy; and a note by M. J. Personne on the conversion of chloral into aldehyde, also presented by M. Bussy.—M. Combes presented a note by M. Flajolot on some crystallised compounds of the oxides of lead and antimony, and of oxide of lead with antimonic acid from the province of Constantine, in Algeria.—A report was read from M. Pasteur on the results of the rearing of silkworms from eggs prepared by processes of selection at Villa Vicentina.—M. C. Robin communicated a note by M. A. Sanson on the influence of the rapid development of the bones upon their density; and M. P. Balestra presented an account of his researches and experiments upon the nature and origin of marsh miasmata, from which he is inclined to believe that the miasmata of marshy places are due to the spores of algæ floating in the air.

VIENNA

Imperial Academy of Sciences, June 17.—Dr. K. Exner communicated a memoir on the sensation of light.—M. J. Schubert communicated drawings and descriptions of a lamp and of an electrical apparatus for producing sound.—M. Tschermak presented a report on the recent fall of a meteorite near Murzuk, in Fezzan.—M. K. Puschl presented a memoir on the amount of heat and the temperature of bodies.—A memoir on reflex action of the nasal mucous membrane upon respiration and the circulation of the blood, by Dr. Kratschmer, was communicated by Prof. E. Hering.—Prof. A. Winckler presented a memoir on the relations between the perfect Abelian integrals of different kinds.—M. von Littrow remarked upon the elements of Winnecke's comet, as calculated by Dr. von Oppolzer.—Prof. Hlasiwetz communicated the results of a long series of experiments made by Dr. Weselsky on the formation of the chinones; and Prof. A. Bauer noticed a compound of platinum and lead having the formula Pt + Pb.

June 23.—The following memoirs were communicated by the Secretary:—On the path of Hind's comet (1847, I.), by Dr. K. Hornstein; on similar conic sections, by M. E. Weyr; and two theories of the movement of free resting masses, by Dr. Recht.—Dr. L. J. Fitzinger communicated the third part of his critical revision of the family of the bats, including the genera *Nyctinomus*, *Thyroptera*, *Exochurus*, *Cnephaiophilus*, and *Vesperus*.—A memoir by Prof. G. Hinrichs (of Iowa), on the statistics of crystalline symmetry, was read, as also a note on the annual course of the temperature at Klagenfurt, Trieste, and Arvavaralja, by Dr. K. Jelinek.

BERLIN

Royal Prussian Academy of Sciences, May 5.—Professor Ehrenberg read a communication on the increasing knowledge of invisible life in the rock-forming *Bacillariæ* of California.

May 12.—Professor Poggendorff read a memoir upon some new and remarkable properties of the diametrical conductors of the electrical machine and on a double machine founded upon these. This paper, which is of considerable length, is illustrated with a figure of the new double machine.—Professor W. Peters read a description, illustrated with figures, of *Platemys tuberosa*, a new species of tortoise from British Guiana.

May 19.—Professor Rammelsberg read a paper on the composition of the meteorites of Shalka and Hainholz.

May 23.—Professor Ewald read a paper on some questions relating to the geology of the Andes.

GÖTTINGEN

Royal Society of Science, April 6.—A paper by M. W. Krauss on the anterior epithelium of the cornea was read.

April 27.—M. A. Clebsch communicated a paper by Prof. C. Schweigger on the size of the ophthalmoscopic picture, and M. L. Meyer read a note on the occurrence of granular cells in the nervous centres.

May 7.—Prof. Sartorius von Waltershausen read a memoir on the isomorphism of the sulphates of lead, baryta, strontian, lime, potash, soda, and ammonia.—Dr. M. A. Stern presented a simple proof of the law of quadratic reciprocity, and some propositions connected therewith.—M. A. Clebsch read a paper on certain problems of the theory of algebraic surfaces.—M. W. Klinkerfues presented a note of some investigations on the movement of the earth and sun in the æther.—Prof. Enneper read a paper on a problem of mathematical geometry, and Prof. Kohlrausch a notice of the influence of temperature on the coëfficients of elasticity of certain metals.

BOSTON

Natural History Society, *Section of Entomology*, March 23.—Mr. S. H. Scudder in the chair. "Synopsis *Pseudoscorpionidum synonymica*," by Dr. H. Hagen.—Dr. Hagen stated that Dr. A. S. Packard, jun., had recently discovered in Brunswick, Maine, and in Salem, a species of *Amphientomum*, a genus of Neuroptera, whose body is covered with scales, and heretofore known only from Ceylon.—The following paper was read:—"On the Synonymy of *Thecla Calanus*," by Samuel H. Scudder.

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ERRATUM.—Page 235, second column, line 24, for "Caprera" read "Capri."

THURSDAY, AUGUST 4, 1870

THE SCIENCE AND ART DEPARTMENT

THE Report of the Science and Art Department just issued is a document of such vast importance to all interested in Science or Education, that we take the first opportunity of saying something upon it. The work which has been done in Science by this Department is so little known, however, that it is necessary to preface our account of this year's report by a brief history of what has been attempted, and accomplished, in former years.

In 1853 the Board of Trade proposed to extend a system of encouragement, similar to that already commenced in the Department of Practical Art, to local institutions of Practical Science, and the Treasury at once wrote one of their classical minutes, in which they expressed the concurrence of "My Lords" generally, in the plans proposed by the President of the Board of Trade, "as the most effectual means of giving effect to the recommendation of Her Majesty at the opening of the Session, with a view to the advancement of Practical Science."

Experiments were tried in the way of Science—or Trade and Navigation—Schools at Aberdeen, Birmingham, Bristol, Leeds, Newcastle-on-Tyne, Poplar, Green's Sailor's Home, Stoke, Truro, Wigan, Wandsworth, and other places. Most of these experiments failed after a short time.

In June 1859 the minute which is the foundation of the present system of aid passed, but this minute has been greatly modified from time to time and greatly enlarged.

The plan pursued before 1859 followed, more or less, the analogy of the elementary school system. That is to say, a trained teacher was sent where a committee was formed, a certain salary guaranteed for a year or two, and so on. This kind of encouragement, however, failed. The requirement of the country was not only teachers to teach but people who wished to learn. After a short time teachers exhausted the guarantee and the schools were broken up.

The state of the country as respects science instruction for artisans at this time (eleven years ago) is well described by Dr. Hudson in a letter then written to the Education Department. In Lancashire and Cheshire, he says, there was no instruction in chemistry, except in "small classes chiefly for mutual improvement in elementary chemistry, and conducted without the aid of efficient teachers, which are in operation at Ballington, Hyde, Staleybridge, Stockport, and Burnley. In Yorkshire, the only night schools affording instruction in chemistry are at Bradford, Halifax, Huddersfield, Leeds, and Selby."

"No instruction whatever is afforded at any Mechanics' Institute in Yorkshire, Lancashire, or Cheshire; and mineralogy, as applied to mining, has only been recently added to the programme of class instruction in one single society—the Wigan Mechanics' Institution."

"In the whole district of Lancashire and Yorkshire, extending from sea to sea, there is no adult school or mechanics' institute in which theoretical mechanics is taught, ... or experimental physics." "The three Ridings of Yorkshire, with their 150 mechanics' and kindred institutions, only possess two societies, and these mere village institutes, in which instruction—Manston in Physical Science, and Shipley in Natural Philosophy—is afforded, and this to an infinitesimal amount. With two exceptions, there are no mechanics' institutions or mutual improvement societies in Lancashire in which any elementary instruction in Physics (Natural Philosophy or Mechanics) is given, and the county of Cheshire does not present one instance in which these matters receive attention in similar societies."

The essential point of the system which the department has organised to reach this terrible state of things, is that it pays simply for results with a preliminary test examination of teachers,—the Honours examination enabling teachers to show high qualifications if they possess them. And the aim has been to enlist all kinds of persons resident in different localities—sometimes the teachers of the ordinary day schools, at other times workmen who had an aptitude for teaching—to commence science instruction, and it looks very much as if this plan has met the difficulty. It has permitted small beginnings by persons conversant with a locality when outsiders could have had neither chance of doing anything, nor sufficient work to support them. It has, in fact, been a missionary effort, and as such has succeeded, and has been paid for.

So much for a general historical sketch of the *modus operandi* of the Department; let us now come to the description, and give evidence of a power at work, which, with proper encouragement, will in time do wonders. The "no schools" of 1859 were represented by 120 schools with 5,479 pupils in 1865, and by 779 schools and 34,283 pupils in the present year. Here are the general results in tabular form:—

	1868.	1869.		1870.
Number of Schools under Teachers examined	300	523		799
Number of Classes in the same	856	1,489		2,200
Number of individuals under instruction in Classes under Certificated Teachers	15,010	24,865		34,283
Number of the above who came up for examination	6,875	12,988	(about)	17,000
Number examined in addition to the above who were not in Schools under Certificated Teachers	217	246	(about)	700
Number of Papers worked in:— Subject.				
1. Practical, Plane, and Solid Geometry	1,337	2,638		3,359
2. Machine Construction and Drawing	1,671	2,987		3,656
3. Building Construction	1,185	1,998		2,631
3 (alternative). Naval Architecture	21	12		39
4. Elementary Mathematics	1,390	2,329	Stage I. II. & III.	3,837
			,, IV. & V.	108
5. Higher Mathematics	33	84	,, VI. & VII.	50
6. Theoretical Mechanics	353	629		830
7. Applied Mechanics	167	294		551
8. Acoustics, Light and Heat	769	1,352		2,021
9. Magnetism and Electricity	1,038	2,509		2,613
10. Inorganic Chemistry	964	2,173		2,694
11. Organic Chemistry	123	210		235
12. Geology	309	619		1,069
13. Mineralogy	38	67		63
14. Animal Physiology	1,182	2,227		3,705
15. Zoology	298	303		114
16. Vegetable Anatomy and Physiology	112	144		400
17. Systematic and Economic Botany	73	90		140
18. Principles of Mining	41	48		64
19. Metallurgy	81	126		160
20. Navigation	219	303		260
21. Nautical Astronomy	86	107		68
22. Steam	106	149		311
23. Physical Geography	1,516	2,687		5,435
Total number of Papers worked	13,112	24,085		89,395

From the above table we gather that there were in May, 1869, 523 schools and 24,865 pupils; in May, 1870, 799 schools with 34,283 pupils, the number of teachers at present giving instruction in connection with the Department being nearly 1,000. Now, how is this work being done? In the first place let us say that it is going on among classes of the community which all our other educational means do not touch, and, as may be imagined, much of the work is night work; in some cases the working men taught have commenced their education by building their schoolrooms; and those who know the delights of a laboratory would think that the chemistry was acquired by apparatus and appliances which made even the simplest experiment an impossibility.

Secondly, let us say that all the year's work is brought to a focus by examinations held on the same night throughout the length and breadth of the land, the papers being sent from South Kensington to the local committees with infinite precautions, and the answers being returned sealed the same night to London. They then are handed over, with no indication as to name of candidate or place of examination, to the Government examiners, and when we state that these examiners include the names of Huxley, Frankland, Ramsay, Tyndall, and others of like calibre, we need say no more as to the rigour and fairness of the examination. Here is a table showing how this ordeal is passed :—

Year.	No. examined.	No. of Papers worked.	No. of Papers passed.
1867	4,520	8,213	6,013
1868	7,092	13,112	8,649
1869	13,234	24,085	14,550

It is impossible within the limits of an article to dwell upon the various points of inquiry and interest which lie around the working of the system: we shall be content if we have shown what it is doing, and how the teaching is being conducted. When these points are known there can be no doubt as to the importance of the work done, and, although many improvements may be required, it is clear that *in essentials* the Department is now on the right track and is doing great good. What is most required is systematising and formulating the instruction. Hitherto the teaching has been rather desultory. It is very desirable that regular systematic courses of instruction, adapted to the local requirements, should be imposed as soon as this can be done without checking the spread of instruction. Some examiners complain of "cram," but this is not limited to the South Kensington system; and the teachers complain of poor pay. This should certainly be corrected; the results they are accomplishing are too important to be ignored; and it would seem that the time had almost come for a complete inquiry into the whole system in order that this important national engine should work with the least possible friction and waste of power.

WHAT IS ENERGY?

IV.—THE DISSIPATION OF ENERGY

At this point we can imagine some champion of perpetual motion coming forward and proposing conditions of truce. "I acknowledge," he will say, "that perpetual motion, as you have defined it, is quite impossible, for no machine can *create* energy, but yet I do not see from your own stand-point that a machine might not be constructed that would produce work for ever. You tell me, and I believe you, that heat is a species of molecular motion, and hence that the walls of the room in which we now sit are full of a kind of invisible energy, all the particles being in rapid motion." Now, may we not suppose a machine to exist which converts this molecular motion into ordinary work, drawing first of all the heat from the walls, then from the adjacent air; cooling down, in fact, the surrounding universe, and transforming the energy of heat so abstracted into good substantial work? There is no doubt work can be converted into heat—as, for instance, by the blow of a hammer on an anvil—why, therefore, cannot this heat be converted back again into work?

We reply by quoting the laws discovered by Carnot, Clausius, Thomson, and Rankine, who have all from different points of view been led to the same conclusion, which, alas! is fatal to all hopes of perpetual motion. We may, they tell us, with the greatest ease convert mechanical work into heat, but we cannot by any means convert all the energy of heat back again into mechanical work. In the steam-engine we do what can be done in this way; but it is a very small proportion of the whole energy of the heat that is there converted into work, for a large portion is dissipated, and will continue to be dissipated, however perfect our engine may become. Let the greatest care be taken in the construction and working of a steam-engine, yet shall we not succeed in converting one-fourth of the whole energy of the heat of the coals into mechanical effect.

In fact, the process by which work can be converted into heat is not a completely reversible process, and Sir W. Thomson has worked out the consequences of this fact in his beautiful theory of the dissipation of energy.

As far as human convenience is concerned, the different kinds of energy do not stand on the same footing, for we can make great use of a head of water, or of the wind, or of mechanical motion of any kind, but we can make no use whatever of the energy represented by equally diffused heat. If one body is hotter than another, as the boiler of a steam-engine is hotter than its condenser, then we can make use of this difference of temperature to convert some of the heat into work, but if two substances are equally hot, even although their particles contain an enormous amount of molecular energy, they will not yield us a single foot-pound of work.

Energy is thus of different *qualities*, mechanical energy being the best, and universal heat the worst; in fact, this latter description of energy may be likened to the dreary waste heap of the universe, in which the effete forms of energy are suffered to accumulate, and, alas! this desolate waste heap is always continuing to increase. But before attempting to discuss the probable effect of this process of deterioration upon the present system of things, let us look around us and endeavour to estimate the various sources of energy that have been placed at our disposal.

To begin with our own frames, we all of us possess a certain amount of energy in our systems, a certain capacity for doing work. By an effort of his muscles the blacksmith imparts a formidable velocity to the massive hammer which he wields: now what is consumed in order to produce this? We reply, the tissues of his body are consumed. If he continues working for a long time he will wear out these tissues and nature will call for food and rest; for the former in order to procure the materials out of which new and energetic tissues may be constructed; for the latter, in order to furnish time and leisure for repairing the waste. Ultimately, therefore, the energy of the man is derived from the food which he eats, and if he works much, that is to say, spends a great deal of energy, he will require to eat more than if he hardly works at all. Hence it is well understood that the diet of a man sentenced to imprisonment with hard labour must be more generous than that of one who is merely imprisoned, and that the allowance of food to a soldier in time of war must be greater than in time of peace.

In fact, food is to the animal what fuel is to the engine, only an animal is a much more economical producer of work than an engine. Rumford justly observed that we shall get more work out of a ton of hay if we give it as food to a horse than if we burn it as fuel in an engine. It is in truth the combustion of our food that furnishes our frames with energy, and there is no food capable of nourishing our bodies which, if well dried, is not also capable of being burned in the fire. Having thus traced the energy of our frames to the food which we eat, we next ask whence does this food derive its energy. If we are vegetarians we need not trouble ourselves to go further back, but if we have eaten animal food and have transferred part of the energy of an ox or of a sheep into our own systems, we ask whence has the ox or the sheep derived its energy, and answer, undoubtedly, from the food which it consumes, this food being a vegetable. Ultimately, then, we are led to look to the vegetable kingdom as the source of that great energy which our frames possess in common with those of the inferior animals, and we have now only to go back one more step and ask whence vegetables derive the energy which they possess.

In answering this question, let us endeavour to ascertain what really takes place in the leaves of vegetables. A leaf is, in fact, a laboratory in which the active agent is the sun's rays. A certain species of the solar ray enters this laboratory, and immediately commences to decompose carbonic acid into its constituents oxygen and carbon, allowing the oxygen to escape into the air while the carbon is, in some shape, worked up and assimilated. First of all, then, in this wondrous laboratory of Nature, we have a quantity of carbonic acid drawn in from the air: this is the raw material. Next, we have the source of energy, the active agent: this is light. Thirdly, we have the useful product: that is, the assimilated carbon. Fourthly, we have the product dismissed into the air, and that is oxygen.

We thus perceive that the action which takes place in a leaf is the very reverse of that which takes place in an ordinary fire. In a fire, we burn carbon, and make it unite with oxygen in order to form carbonic acid, and in so doing we change the energy of position derived from the separation of two substances having so great an attraction for each other as oxygen and carbon, into the energy of heat. In a leaf, on the other hand, these two strongly attractive substances are forced asunder, the powerful agent which accomplishes this being the sun's rays, so that it is the energy of these rays which is transformed into the potential energy or energy of position represented by the chemical separation of this oxygen and carbon. The carbon, or rather the woody fibre into which the carbon enters, is thus a source of potential energy, and when made to combine again with oxygen, either by direct combustion or otherwise, it will in the process give out a deal of energy. When we burn wood in our fires we convert this energy into heat, and when we eat vegetables we assimilate this energy into our systems, where it ultimately produces both heat and work. We are thus enabled to trace the energy of the sun's rays through every step of this most wonderful process: first of all building up vegetable food, in the next place feeding the ox or sheep, and lastly through the shape of the very prosaic but essential joint of beef or mutton entering into and sustaining these frames of ours.

We are not, however, quite done yet with vegetable fibre, for that part of it which does not enter into our frames may, notwithstanding, serve as fuel for our engines, and by this means be converted into useful work. And has not Nature, as if anticipating the wants of our age, provided an almost limitless store of such fuel in the vast deposits of coal, by means of which so large a portion of the useful work of the world is done? In geological ages this coal was the fibre of a species of plant, and it has been stored up as if for the benefit of generations like the present.

But there are other products of the sun's rays besides food and fuel. The miller who makes use of water-power or of wind power to grind his corn, the navigator who spreads his sail to catch the breeze, are indebted to our luminary equally with the man who eats meat or who drives an engine. For it is owing to the sun's rays that water is carried up into the atmosphere to be again precipitated so as to form what is called a head of water, and it is also owing to the sun's heat that winds agitate the air. With the trivial exception of tidal energy all the work done in the world is due to the sun, so that we must look to our luminary as the great source of all our energy.

Intimately linked as we are to the sun, it is natural to ask the question, Will the sun last for ever, or will he also die out? There is no apparent reason why the sun should form an exception to the fate of all fires, the only difference being one of size and time. It is larger and hotter, and will last longer than the lamp of an hour, but it is nevertheless a lamp. The principle of degradation would appear to hold throughout, and if we regard not mere matter but useful energy, we are driven to contemplate the death of the universe. Who would live for ever even if he had the elixir of life? or who would purchase, if he might, the dreary privilege to preside at the end of all things—to be "twins in death" with the sun, and to fill up in his own experience the melancholy dream of the poet,—

> The sun's eye had a sickly glare
> The stars with age were wan,
> The skeletons of nations were
> Around that lonely man,
> Some died in war, the iron brands
> Lay rusting in their bony hands,
> In peace and famine some.
> Earth's cities had no sound nor tread,
> And ships lay drifting with their dead
> To shores where all were dumb.

B. STEWART

POPULAR PHYSIOLOGY

What shall we Teach? or, Physiology in Schools. By Edwin Lankester, M.D., F.R.S., &c., &c.

A School Manual of Health. By Edwin Lankester, M.D., F.R.S., &c., &c. (London: Groombridge and Son.)

THERE is an old saying, "that every man when he gets to be forty is his own doctor unless he happens to be a fool;" by which is meant that the pains and discomforts of ill health will, in the long run, convince most men that some knowledge of the facts of physiology and of the laws which govern the human body, is, after all, a desirable thing for the comfortable conduct of life. The main object of Dr. Lankester's pamphlet is to urge the question, "Why leave these lessons to chance and the fourth decade? Why not steal a march on bitter experience, and by making physiology a branch of general education, forewarn and forearm everyone against bodily indiscretions and against transgressions of sanitary laws?" Leaving on one side altogether the value of physiology in its scientific aspect as a means of training the mind, and taking his stand on the ground simply of the importance of it as mere information, the author works out his plea with unflagging zeal and energy. Indeed, all the pages bear tokens of almost the enthusiasm of a crusade. Into town and country, into girls' schools, boys' schools, infants' schools and universities, into corporations, vestries, and town councils, into the functions of clergymen, householders, lawyers, and domestic servants, the flag of physiology is most gallantly carried; and we can hardly imagine an impressionable general reader finishing the little work without at once rushing off to order "Huxley's Elementary Lessons" and the "School Manual of Health."

For ourselves we are free to confess, that while thoroughly sympathising with Dr. Lankester in his laments over the contemptible ignorance, and worse than ignorance, of mankind in all that relates to their bodies, we are not so sanguine as he seems to be touching the results of even general and extensive physiological teaching. We quite feel with him that it is perfectly outrageous that men and women should be so profoundly ignorant, as they are, of the nature of that prison-house from which they can never escape so long as life lasts, that our youth should, under the pretence of training, be taught things which they can never see or touch in after-life, should be made wise in phantoms and myths, and encouraged to put aside all curiosity about the things which they carry about with them always everywhere. Is it not monstrous that many a lad of eighteen should have so vivid a picture in his mind's eye, of, say, Syracuse during the Peleponnesian war, as to make people think he must have lived long years in Sicily, while the inside of his own body is to him a dim mystery, of which he can call up no clear image, but fancies it is some how or other more or less like a pig's? Some day or other men will have difficulty in believing that such a state of things could possibly have existed, and certainly the longest chapter in that great book, *De Hominum Erroribus*, will be the one which deals with the teaching of the young. At the same time, we fear that the millennium will not be very much nearer when every schoolboy knows the properties of gastric juice and even vestrymen believe in respiration. We have seen too many professors of physiology lecture on "pepsin" in the morning and rush violently into heavy dinners and indigestion in the evening, and besides, have had already too much general experience in the "*meliora probo deteriora sequor*," to feel much confidence in the reforming virtues of even the widest and most exact information, especially in everything relating to eating, drinking, and building houses. Nursemaids will continue to choke children, schoolboys to eat green gooseberries, and artizans to block up ventilators, in spite of each and all of them bearing certificates of proficiency in the knowledge of the laws of life.

Dr. Lankester's strongest point is perhaps the negative and destructive, rather than the positive and constructive, value of sound biological knowledge. Mankind suffer not so much from ignorance as from error, not so much from lack of knowledge as from the prevalence of false notions. The thing which the doctor and the sanitary reformer has to struggle against above all other things is the pertinacity with which the general public stick to false and pernicious theories, and the avidity with which they swallow everything which is absurd and ridiculous. Sometimes the attitude of the public mind towards questions of biological science is one of wholesale scepticism, sometimes of blind superstition; in all cases they appear as if they would rather be guided by any spirit than by that of patient inquiry, and of trust in conscientious and careful observation and experiment. Their minds are always readily tickled by any theory if it be extravagant enough: they run rapidly after any sign that is striking enough; but they have no taste for the sober results of sound biology. It is not enough to offer them lessons in physiology. The teacher may, perhaps, by diligence and patience at last get them to accept a part of what he teaches, but not until he uses his science as an instrument of training as well as a source of information.

And this brings us to the point in which apparently we feel obliged to break away altogether from Dr. Lankester. We quite agree with him, as we have said, in the immense value of physiology as viewed as mere information and compared with other kinds of information. But we hold very strongly to the opinion that it is training that is wanted far more than information. It is a change in the eye rather than in the picture towards which we look with hope. Beat into the general run of men some little scientific spirit, teach them how to look at the world around them in a scientific manner, how to arrive at scientific conclusions, how to approach scientific questions; put them in a proper mood, and they will then perhaps begin to become earnest physiologists and sanitary reformers. It is a right state of mind, and not a schoolboy's lesson in oxygen, that will tear down the paper pasted over the ventilator and otherwise help to lessen the labours of the coroner for Middlesex.

MÜLLER'S PHYSICS AND METEOROLOGY

Grundriss der Physik und Meteorologie. Von Dr. John Müller. Zehnte Vermehrte und Verbesserte Auflage. Mit einem Anhange, Physikalische Aufgaben enthaltend. (Erste Abtheilung. Braunschweig, 1869.)

IT is impossible to disguise or repress the feeling of covetousness with which this book of "Elements of

Physics and Meteorology" fills an English reader. In a volume which, when completed, is to contain something less than 600 pages, we have an account of the fundamental phenomena of natural philosophy, which is at once readable and scientific. It is published at 6*s*., is illustrated with 600 admirable engravings, and is to be accompanied by a collection of examples which, with the chapter on heat, will make up the remainder of the book. It is just such another treatise—as copious and accurate, and at the same time as clear and concise—that is wanted in teaching the elements of natural philosophy in England. There are a hundred schools which are compelled to put up with books twice as big as boys care to read or carry, which would introduce such a book as this at once.

The great difficulty which has to be faced and overcome in an elementary treatise of the kind is well stated by Professor Müller in the preface. "The facts of physical science ought never to be presented to the pupil in a mere dogmatic fashion, as acquired results. It is essential that he should comprehend the mode in which they have been deduced, and grasp the connection between the facts themselves and their systematic presentation in a logical system, which exhibits their mutual relations. Even in an elementary treatise like the present the reader ought to find an introduction to the processes of thinking and reasoning which are employed in physics, and should see everywhere examples and applications of the inductive method."

It is of course impossible for the author within his limits to give more than the briefest account of the main facts of the physical sciences. We turn, for instance, to the subject of thermo-electricity, the article on which, we are told in the preface, has been entirely re-written for this tenth edition. It contains just two pages. Nevertheless there is a good account of the fundamental law, and illustrations which enable the reader to comprehend in a very satisfactory way the use of the thermo-pile. There follow two pages more on animal electricity, the bulk of which is devoted to an account of the familiar electric actions exercised by certain fish, with illustrations showing the nature of the organs to which that action is due. The article closes with a few lines indicating the results obtained by Nobili and Du Bois Raymond in confirmation of the ideas of Galvani. These brief summaries of subjects could scarcely fail to be obscure were it not for the abundant diagrams which serve as texts for them.

The book before us is the condensed quintessence of Dr. Müller's well-known larger book on the same subject, which travels over the same ground, giving about four times as much space, and nearly four times as many illustrations. It is obvious, of course, what an enormous advantage it must be, in a task which is, of necessity, one of great difficulty and discretion—that of saying the very least which is requisite for clearness—to have previously arranged the subjects treated in a manner which allows a distinct perception of their relative importance. Without the larger book, this little text-book could scarcely have been what it is. We hope to see some day—the sooner the better—a comprehensive English treatise on Natural Philosophy which will take the place of Müller's large book in Germany, appealing to mathematics as little as it is possible to do without wearisome circumlocution, sufficiently simple to be accessible to any student who has a serious purpose, and at the same time scientifically accurate. From such a treatise it would be easy to condense one which should be for English schools what Prof. Müller's book is for schools in Germany. It would be extremely difficult, in any other way, to put into forty-five pages an account of the theory of sound, and its applications, so full and so simple as that which we find here. Everything is preserved in its proper proportions, and the reader rises from its perusal, not of course imagining that he knows the subject fully, but with a clear apprehension of the fundamental ideas involved, of the main questions of difficulty in the inquiry, and of the more recent discoveries which have enlarged the borders of the science. He is left, at the end of the book, with his curiosity stimulated, and not destroyed. If his after occupations give him sufficient leisure, the boy who has mastered this text-book at school will be certain to ask for more.

WILLIAM JACK

OUR BOOK SHELF

Microscopical Manipulation. By W. T. Suffolk, F.R.M.S. (Gillman, 1870.)

THIS little book is the substance of a course of instruction given by Mr. Suffolk in the spring to members of the Quekett Club. It will be useful to those persons who amuse themselves with microscopes, and do not care to purchase the scientific treatises of Dr. Carpenter or Dr. Beale. There is a chapter for the very youngest beginner on the various parts of an English compound microscope and their uses; then we have hints about the cutting of glass and the old directions as to making cells; mounting objects in balsam and in fluid is next dealt with—the old, old routine methods being detailed once again, with an allusion to Dr. Bastian's process with benzine. It is a pity that Mr. Suffolk has not made himself acquainted with some of the many methods of mounting and preparing objects in use on the Continent, which he might have picked up from Stricker's handbook, Frey's work, or other similar treatises. The best chapter in the book is that on polarised light, because it deals with a subject rather slighted in other works of this kind, in a clear and intelligent manner. We were not, however, prepared for the following in a work on microscopical manipulation:—"The undulatory motion of light would seem to be expressed with considerable clearness in the 1st chapter of Genesis, when read in the original Hebrew, which, in common with the other languages of the same family, is remarkable for the numerous inflexions of its verb, which gives it a delicacy and precision of expression unattainable in Western languages." Mr. Suffolk is quite right in considering that more attention should be paid to the use of polarised light as demonstrating structure, than has been done hitherto. A necessary step towards this is that microscopists should properly understand what are the conditions of production of colour with the polariscope, and not be content with the mere sight of a pretty display. This little book of Mr. Suffolk's will not do much, we fear, to convert what we may call microscopical play into microscopical science. Its receipts and directions are such as will be useful to the man who cares merely to make a series of pretty slides for exhibition to his friends, but do not help the student wishing to add to the storehouse of science. Nothing is said of the manner of studying living objects, living cells, living cilia, living protoplasm; nor do we find an allusion to the use of chromic acid, section instruments, methods of embedding, of gold and silver staining, or other processes important to a working microscopist. The gold and silver-staining methods might have been given if only for the benefit of those who like to make gorgeous preparations.

A small book on "Microscopical Manipulation," well up to the time, would be useful to students. We are sure Mr. Suffolk does not wish to claim this position for his digest of the older handbooks. His excuse for its publication must be that in this country there are many people who indulge in the expensive peepshows sold by our English opticians, to whom it will really be acceptable.

It must not be imagined that we for one moment object to such amusements; on the contrary, they are altogether to be commended where more serious work cannot be undertaken—and only then. E. R. L.

Notes of a Season at St. Moritz in the Upper Engadine, and of a Visit to the Baths of Tarasp. By J. Burney Yeo, M.B. (London: Longmans, 1870.)

WE commend this sensibly-written and interesting little book to the notice of our readers, many of whom, notwithstanding the outbreak of hostilities between our friends across the Channel, may yet seek health and enjoyment in these remote valleys, where it is in the highest degree improbable the tide of war will ever roll. Dr. Yeo's little brochure contains all that it is necessary the intending tourist need know, and much that the invalid ought to know before starting for the Upper Engadine. To the latter class of travellers in particular it is of no slight importance to know the nature of the lodging and food they can obtain, and the advantages to be gained from a residence in a new and untried region; and upon these points Dr. Yeo's experience enables him to speak with much confidence. St. Moritz, it must be remembered, is 6,000 or 7,000 feet above the level of the sea, and the air, though bright and clear, is by no means warm. The waters contain a small proportion of iron, and are strongly charged with carbonic acid, which may perhaps act as a stimulant both to the skin and the stomach in tolerably healthy patients; but Dr. Yeo makes some judicious remarks on their effects on those who are debilitated and exhausted, and the advantages resulting from leaving off the prescribed cold bath, and glass or glasses of cold water. The last chapter contains a capital account of the Fauna and Flora of St. Moritz and Tarasp, the latter embracing between 300 and 400 plants, arranged according to their natural orders.

Reactions-Schema für die qualitative Analyse, zum Gebrauche im chemischen Laboratorium zu Berlin. (Berlin, 1870. Verlag von August Hirschwald. London: Williams and Norgate.)

THIS is a kind of pictorial analytical table in which the characters of the precipitates obtained are indicated by coloured oblong spaces, which will, doubtless, be found very useful for impressing the appearances of the different precipitates on the mind of the student. The borax bead obtained with a compound of cobalt is represented by a blue oval, and the effect of ammonia on red litmus paper is shown by an oblong half red and half blue. The changes of colour produced by the action of sulphuretted hydrogen on a salt of mercury are indicated by an oblong of four different colours, white, yellow, orange, and black.

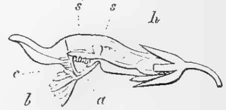
It is unfortunate that this table is not more complete; thus no means of obtaining the solution to be treated is mentioned; the destruction of organic matter before precipitation by ammonia and ammonia sulphide is omitted; the possibility of the precipitate in the third group containing phosphates, and the mode of examining it under such circumstances, is passed over entirely. The spectra of potassium, sodium, and lithium, are indicated by black lines with fine transverse white ones, representing the coloured bands, but unfortunately no means are given to show which is the more refrangible end of the spectrum. Besides these omissions there are some misprints which will no doubt be corrected in a subsequent edition.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

Fertilisation of Polygala

HAS the method of fertilisation of the milkwort, *Polygala vulgaris*, yet been described? It presents one of the most beautiful contrivances with which I have hitherto met for securing a cross through the agency of insects. The corolla consists of five petals united into one piece and folded into the form of a two-lipped tube, the upper lip of which is formed by the over-lapping edges of the corolla; while the lower lip is a somewhat cup-shaped appendage (*c*), furnished with a "beard" of gland-like bodies (*b*), and opening in front by a narrow, vertical slit. The filaments of the stamens are united throughout the greater part of their length with the corolla, but expand within the cup of the lower lip into a two-lobed membrane, crowned by the anthers (*a*). The pistil has two stigmas, one of which (*s*) is



placed at right angles to the upper side of the style and is perfect, while the other (*s*′) is transformed into a spoon-shaped, petaloid prolongation of the pistil, reaching to the opening of the lower lip of the corolla, and dividing the interior of the flower into two chambers, in the lower of which are the stamens, thus completely separated from the true stigma. The entrance to the flower, below the style and in front of the stamens, is closed by hairs pointing outwards from the flower and meeting in front, on the mouse-trap principle; but a narrow passage is left open above the petaloid stigma, and is perhaps capable of a slight distension from the flexibility of the overlapping petals. On each side of the interior of the tube of the corolla, above the style and just behind the true stigma, is a group of strong, white hairs (*h*), pointing down the tube of the corolla, and meeting above the style. If we now suppose a small insect to light upon the "beard" of the flower, it is prevented from immediate entrance by the projecting hairs, but soon finds the narrow passage leading over the stigma into the upper chamber. It is prevented by the hairs in the tube of the corolla from returning by the same path, and is obliged to crawl out through the lower chamber and over the stamens; pollen from which it will, by a repetition of the same process, convey to the stigma of the flower next visited.

In the bud the anthers are in contact with the stigma, and some caution is necessary in dissecting that they may not be crushed, giving the appearance of the pollen having been deposited *en masse* on the spoon-shaped stigma. Naturally, I believe, the pollen is never shed till after the complete expansion of the flower.

I have never actually observed any insect either in the flower or sucking nectar from it, but I have almost invariably found a few small black flies upon the bunches that I have brought in for examination.

The broad and conspicuous "wings" of the calyx having fulfilled their office of "tempting insects to their food," gradually assume the green colour of the ordinary leaves, and closing over the ripening capsule, serve probably to conceal and protect it from the attacks of some enemy.

Kilderry, Co. Donegal W. E. HART

P.S.—I have to record a similar phenomenon with respect to the holly berries of this neighbourhood to that mentioned by Mr. Henry Reeks (NATURE, June 9). I did not remark that any varieties in particular had been rejected; but few that bore fruit (of which there was a much greater quantity than usual) appeared to have lost any of it, so late as the end of May. And yet we had not fewer of the migratory thrushes than in former years, when the holly bushes were generally stripped of their berries before the end of January; and, on the other hand, we had several days of frost, extraordinarily hard for this neighbourhood. On what arguments does Mr. Reeks ground his presumption, so different from Mr. Darwin's own conclusions on the subject, that "berries obnoxious to birds will stand a better chance of propagating and increasing that variety?" If C. W. W. (NATURE, July 7) will turn to Letter 55 of White's "Selborne" he will find the following observation on the House Martin:—"The young of this species do not quit their abodes all together, but the more forward birds get abroad some days before the rest. These approaching the eaves of buildings and playing out before them make people think that several old ones attend one nest."

Our Middle-Class Schools

I WISH to bring before your readers the necessity of immediate action with regard to a branch of education at present not liable to legislative interference. Government is becoming more and more alive to the fact that Education and Science at the present are England's greatest needs; hence the steps taken to extend and enforce primary education. But whilst increased facilities are being afforded to raise the standard of primary education, secondary education is at a stand-still, and upon the whole falls far short of the point it should reach. Thousands of our middle-class schools when compared with what is required, may be placed in the same category as the old dame's school when compared with the modern national school. It requires but the slightest knowledge of the subject to know that our middle-class educational system is as a whole a mere farce, and yet so averse are we to change, that matters are allowed to go on year after year in the same old matter-of-course style without the slightest indication of reform. In order to encroach upon your space as little as possible, I will in a succinct and concise form lay before your readers a scheme which has been lately mooted, which has received the sanction of the highest authorities in these matters, and which is destined ultimately to bring about quite a new system. In speaking thus indiscriminately of our middle-class schools, I do not include many excellent institutions, in which a thorough course of training forms the routine, and which are conducted by gentlemen capable and willing to do the work required. Alas that there should be so few!

Then 1st. It is well known that individual influence is of little service. This fact supports the theory that an association must be formed, consisting of the principals and assistants of middle-class schools, and others interested in the question.

2nd. This society should have certain objects, and its members combined should use their utmost endeavours to assist in carrying out these objects. A few of the aims would be as follows:—

α. The institution of normal colleges for the training of gentlemen who wish to enter the scholastic profession.

β. To recognise some examination, diploma, &c., as sufficient guarantee of the capabilities of gentlemen entering the profession, and insist that such gentlemen shall have this diploma. The evils arising from the incapability of so many of our masters cannot be over-estimated.

γ. The necessity of Government or other central supervision and examination of *every* school. At the present moment the standard of a school is calculated by *nothing*. An advertisement perhaps appears, stating that *all* boys sent to special examinations have passed; and instances are known where *one* boy has been sent up to such examination. It is impossible to decide upon the general tone of a school by the examination of a *few* of the best boys.

δ. The institution of a club-house in London where appointments could be made, business transacted, &c., and attached to it some means by which the incubus of agents could be avoided.

ε. Periodical meetings, &c., &c.

I am afraid this letter is running to an inordinate length, but I just wish to add that invitations have been issued by the editor of the *Quarterly Journal of Education* to a few representative gentlemen for a private preliminary meeting to be held in September next, when the above scheme is to be discussed. Any gentleman wishing to take part in that meeting should address the editor upon the subject. I might have referred to the failure of the College of Preceptors to do the least good. What we must have is an obligatory examination of the whole school, and every school; not leaving it to the whim of the principals. Neither are assistant masters treated as they should be by the College of Preceptors.

X

The Source of Solar Energy

MR. GREG ascribes to me views I do not hold, and then employs my own reasoning to overthrow them. He must have formed his conceptions of my theories from Prof. Pritchard's critique of my "Other Worlds"—a most unreliable source.

To begin with,—I do *not* believe that the solar heat supply is solely derived from the downfall of meteors. I have impressed this very clearly at p. 54 of my "Other Worlds."

I do not believe that *any part whatever* of the solar heat supply is derived from meteoric *percussion*, nor that any meteor ever comes within tens of thousands of miles of the sun's surface in the solid state.

Mr. Greg is very careful to show me that the meteor-systems encountered by the earth cannot fall into the sun. I dwell on this very fact at p. 203 of "Other Worlds"—I say, *totidem verbis*, that no known meteoric system can form a hail of meteors upon the sun. "It is forgotten," says Mr. Greg, "that the meteors themselves revolve round the sun," &c. If *he* has at any time forgotten this, I certainly have not.

"Has it ever been proved," he asks me, "that the entire mass of meteors constituting the zodiacal light, is either composed of matter in a solid state, or, if it were, that its mass would be equal to that of our own earth?" I answer, as Mr. Greg would—"No, it has not been proved, nor is it by any means probable."

There is nothing new to me in Mr. Greg's letter, and little which I have not described myself long ago in the *Intellectual Observer and Student* of 1867, 1868, and 1869. To suppose that I should venture to treat at all of meteoric astronomy, in ignorance of such elementary facts—the very A B C of the science—is not complimentary. Mr. Greg might, at least, have examined what I have written before assigning to me the absurdities he attacks so successfully.

The fact is, this matter of the solar energy only comes in *par parenthèse* in my "Other Worlds." I express no confident opinion whatever about it. I point to some deductions from known facts, and respecting *them* express a certain feeling of confidence. It is not my fault (nor, indeed, can I blame Mr. Greg) if Prof. Pritchard has tacked my words "I am certain" (used with reference to reliable inferences) to a theory respecting which I have distinctly written, that "I should not care positively to assert" its truth. Even that theory is not the absurd one attacked (very properly) by Mr. Greg.

For the rest, most of Mr. Greg's letter is sufficiently accurate, but there are two mistakes in it.

1. We have abundant evidence that the density of the aggregation of cometic perihelia increases rapidly near the sun. For example, whereas between limits of distance 40,000,000 and 60,000,000 miles from the sun this density is represented by the number 1·06, it is represented by the number 1·67 for limits 20,000,000 and 40,000,000 miles, and by the number 8·65 within the distance 20,000,000 miles. The evidence derived from this observed increase of aggregation is not affected by what we know of those cometic or meteoric systems whose orbits nearly intersect the earth's (for they must form but the minutest fraction of the total number) nor by the observed minimum perihelion distance of cometic orbits (for observed comets are but the minutest fraction of the total number).

2. It makes no difference whatever as regards the force-supply of the solar system, whether the substance of a meteor reaches the sun in the solid, fluid, or vaporous state. Given that the substance of a meteor, moving at one time with a certain velocity at a certain distance from the sun, is at another time (after whatever processes) brought to rest upon or within the sun's substance, then either the "force-equivalent" of its motion has been already distributed or the substance of the meteor is in a condition to distribute that "force-equivalent" mediately or directly. In other words, either heat and light have been already distributed, or the central energy has been recruited to the full extent corresponding to the mass, motion, and original distance of the meteor.

I may express here my agreement with the opinion of the Editor of NATURE that the observations made on the zodiacal light by Lieut. Jones and M. Liais ought to be taken into account in any theory of that mysterious object. Taken in conjunction with the other known phenomena of the zodiacal light, they admit of but one interpretation as to the position, dimensions, and general characteristics of the object. Taken alone, we might infer from them that the zodiacal light is a ring of bodies or vapours travelling around the earth (at a considerable distance);

other phenomena suggest that the zodiacal light is a disc of bodies or vapours travelling around the sun; yet others suggest that the zodiacal light is a phenomenon of our own atmosphere. But the only theory which accounts at once for *all* observed phenomena, is that which regards the zodiacal light as simply due to the continual presence in the sun's neighbourhood of bodies or vapours (meteoric or cometic, or both) which come there from very far beyond the earth's orbit, and pass away again on their eccentric orbits. A disc thus formed of continually varying constituents would shift in position, and would wax and wane in extent as well as splendour, precisely as the zodiacal light is observed to do.

RICHARD A. PROCTOR

Spontaneous Generation

THE physical capacity of fungus-spores to throw out mycelium, and from that to be able to reproduce a parent (or, according to Dr. Bastian, to produce a fungus *de novo*), shows a complicated organisation greatly above that of the monad. From a careful examination of Dr. Bastian's experiments and figures, I am led to believe that the majority of the ovoid bodies referred by him to fungus-spores are nothing of the kind, and that if they really belong to the vegetable kingdom at all, they are perfect unicellular plants in themselves reproducing their kind by subdivision; the presumed *mycelium* I should refer to the bursting of the cell-walls, and consequent discharge of the contents, a by no means uncommon occurrence with unicellular organisms. It is, however, impossible to follow the author in his speculations regarding these bodies, as his measurements are so imperfect, and in several instances, where most wanted, omitted altogether. A few of the bodies certainly bear an external appearance to fungus-spores (for instance C, Fig. 11, which might be referred to *Russula* or *Scleroderma*), but as no size is given it is impossible to form an opinion. Perhaps Dr. Bastian will say on what data he refers such objects as are shown on Fig. 3, to fungus-spores, and by what characters he knows the mycelial filaments to be such: the "half-grown" spore (?) described on page 197 has most extraordinary characters for such an object; for, says the author, "the nuclear particle within was seen moving from end to end of the cell."

Had not Dr. Bastian distinctly affirmed that the spores were generated at once from heterogenous materials, I should have assumed his belief in the presence of the perfect plants in the infusions, though the detached spores were all he met with in his experiments; for if any organism originates independently of a parent similar to itself, surely it is reasonable to expect the production to be at once perfect, and not in the egg state. Could an oviparous animal be produced from heterogenous materials, surely one would not expect eggs first to appear. In referring these unicellular bodies to spores, Dr. Bastian appears to me to have defeated the very object he had in view.

That some of the bodies figured are *bona fide* spores of fungi seems very probable, and that they were produced by ordinary parents, seems equally so, for the slight neck-like elongation or spot, which is analogous with the placental scar in animals and flowering plants, is clearly present in the lower right-hand object (Fig. 11) and the lower left-hand object (Fig. 13). If these things were evolved without parents, surely nature would not have given them an umbilicus; it is far more reasonable to suppose that true spores got into the infusions (and perhaps germinated) by some accident similar to the three recorded instances where foreign bodies were undoubtedly found. (Note, p. 197.)

It seems to me rational enough to suppose that unicellular bodies and objects of the lowest possible organisation may be heterogenously produced from the inorganic world, for here the line between one and the other is so fine as scarcely, if at all, to be perceived; indeed, the Brownian motion of monads, some spermatozoids, and the particles forming many inorganic infusions, are scarcely, if at all, to be distinguished from each other. From monads and unicellular organisms, however, spore-producing fungi are greatly removed, and the economy, functions, and structure of most of the latter are now so well known, that it would be simply impossible to convince any botanist that a spore like C (Fig. 11) could be produced from any other quarter than the hymenium of a well-defined parent.

I cannot see that the production of motile zoospores in Achlya (p. 174) has any bearing on the subject, as here we have an already *living* parent; and motile zoospores are by no means uncommon in the vegetable kingdom; their movements, however, so far as I have been able to observe them, do not differ from the Brownian movements seen in the inorganic world.

It is clear that no definite conclusions can be arrived at regarding these bodies and their production till a series of accurate figures is published to an *uniform scale* (with an indication of the colour of the cells, &c.), enlarged at least 2,000 diameters, for even then a monad would be no larger than a pin's-head. It will then be possible to compare the bodies with actual fungus-spores and other bodies well known to botanists and zoologists.

Prof. Wanklyn appears to be unable to estimate the number of germs of fungi known to exist in the atmosphere. That "there must be very many of them" is apparent from the calculation that, if each spore of one species only of one of the higher fungi germinated and reproduced its parent, the children would, in the first generation and in the course of a very few days, form a carpet all over the earth. Now, as fungi abound everywhere in myriads, and the family is almost illimitable, the number of diffused germs is evidently beyond all calculation; their size, too, is often so small that twenty could be conveniently accommodated on the diameter of a single human blood corpuscle. That they are alive is proved by the readiness with which, under favourable conditions, they may be made to germinate. It must also be admitted, in Prof. Wanklyn's favour, that "they must weigh something," though I am not aware of any attempt in that direction at present. Some of their "remarkably small" component parts are, however, made manifest by chemical reaction.

WORTHINGTON G. SMITH

Mildmay Park, London

Super-Saturation

THE following experiments may be found interesting from their bearing on the latest theories advanced on the subjects of super-saturation and the so-called inactive state of bodies. Professor Tomlinson's theory is that a super-saturated solution adheres as a whole to a chemically clean surface, but that a differential adhesion takes place in presence of a chemically unclean surface, because the salt or gas adheres to such a surface while the liquid does not; the former is consequently liberated. The presence or absence of grease is then stated to constitute chemical uncleanness or cleanness. If a greasy surface can be rendered inactive, it is clear that both these propositions cannot be true; either grease is not of itself a cause of uncleanness, or unclean surfaces are not necessarily active ones. The following experiments prove that the fats may be rendered inactive by the same processes which are applied to rods of glass or metal.

1. A bit of composite candle was melted with a very little alcohol; a glass rod was dipped in, allowed to cool, passed through flame of a Bunsen's burner, and a drop of melted fat deposited on surface of supersaturated solution of zincic sulphate. The fat solidified without affecting the solution even on prolonged agitation. A crystal of the salt caused instant solidification.

2. Some solution boiled remained supersaturated for a fortnight with a crust of fat on the top.

3. Ordinary tallow treated as No. 1. Inactive in solution of sodic sulphate solution; touched with finger, crystallised at once.

4. Lard cleaned simply by melting on rod passed through flame, allowed to cool, stirred in solution of sodic sulphate. Inactive. Other end of rod active at once.

5. Same solution boiled; cooled, stirred with rod, treated as the last, and left exposed to the air for 15 minutes. Did not crystallise. This experiment is interesting, as showing that greasy substances are not specially liable to be made active by exposure to ordinary air.

6. Some tallow was melted in a test-tube without precautions of any kind, and while melted was poured to the depth of half an inch on to solutions of ferrous, cupric, and sodic sulphates. In each case it was inactive. This is conclusive on the point in question.

Theoretical objections have been urged against the definition of chemical uncleanness, and I think this might now be surrendered.

Professor Tomlinson may very likely be right in looking to adhesion for an explanation of these phenomena; at all events, far greater probability seems to attach to this view than to that put forth by M. Gernez, adopted by Jamin, and even, I believe, favourably noticed by the Academy itself; which is that only a crystal of the same salt can induce crystallisation. This latter view is open to the theoretical objection that it necessitates our believing that all salts capable of supersaturation are everywhere and at all times present in the atmosphere. Mr. Tomlinson has shown experimentally that a crystal of the salt properly treated can be inactive in a solution of sodic sulphate.*

The following experiments show that an atmosphere presumably saturated with the salt may be inactive without any precautions whatever:—

7. Three solutions of sodic sulphate were prepared; a glass rod was dipped in melted tallow and left to cool for five minutes in a bottle of the salt, without touching the salt or the sides of the bottle. Then the three solutions were successively touched with the rod; inactive in all. Rod replaced in the bottle for ten more minutes; then all three again touched. The third crystallised after a minute or two. Rod replaced for fifteen minutes; active only in the second; replaced for fifteen minutes, active in the last. Thus this greasy rod was inactive in one of the solutions after an exposure of thirty minutes, and after being six times dipped in solutions of the salt.

8. An open test tube, containing a strong solution of sodic sulphate, remained supersaturated for a week suspended in the same large bottle of the salt. The bottle was frequently moved without producing any effect. On removing the cork, the solution crystallised instantly.

On the whole I am afraid we must for the present fall back upon that refuge for the destitute, "Catalytic action."

Birmingham — J. G. Grenfell

Derivation of the Term "Horse-Chestnut"

The explanation of the above name by Mr. E. A. Connell in your last issue, though ingenious, is not, I think, the true one. In a work entitled "Etymons of English Words," by John Thomson, Edinburgh, 1826, the term is explained thus:—"Horse-Chestnut. The *harsh*-chestnut; but the F. and the Swedes have translated it as *horse*." Following this he gives in support of *horse*, being the corruption of harsh, horse-faced, *harsh*-faced, *hard*-featured, horse-radish, *harsh*-radish;" and *harsh, rough, sour*, austere, grating, S. *hærsk*, T. *harsch*, D. *harsk*. So that, accepting this explanation, harsh-chestnut is the more scientific term. — J. Jeremiah

Trehelig, Llangadock, Carmarthenshire, July 16

Ozone and Thunderstorms

In reference to the production of ozone it may interest your readers to know that the quantity developed here has been unusually great during the last few days. On Sunday evening, during the electrical agitation that occurred and ended in a slight discharge, accompanied by heavy rain, we had the highest reading. Mr. Burrows' test paper registered 9, and was almost black. This observation was taken at 10 A.M. During the period of this development the air was very moist. Last evening and this morning have caused the ozonometer to register 7, and this is above the average. To-day, Tuesday, the hygrometer indicated the point of saturation. I may add that old Gilbert White's remark as to activity of swifts during thundery weather has been greatly confirmed. They have kept up an almost incessant screaming during the last few days.

Great Malvern, August 2 — Samuel Barber

The Sun's Corona

A letter of mine, addressed to you a fortnight since, has, I fear, miscarried. It had special claim to admission as complaining of an editorial remark.

I now renew my objection to the editorial note upon my letter referring to Professor Pritchard's critique on my "Other Worlds." As an uncourteous comment upon a passage in which I had paid a high but not undeserved compliment to Mr. Lockyer, I had just reason to be surprised at its appearance.

First, because my account of Mr. Lockyer's views respecting the corona agrees in all essential points with that given in the note, whereas the contrary is implied.

Secondly, because I have not mis-stated Dr. Gould's evidence, though forced to interpret it otherwise than he does.

Thirdly, because my whole reasoning on the corona has been founded on evidence, and is therefore unjustly described in the note as "evolved from the depths of my moral consciousness" (an old witticism, which, however, would have borne repeating had it been to the point.) *

To the personalities in the note, as to the reference to my age, and so on, I make no objection whatever, caring only to notice what seems worthy of notice.

Let me add, however, my protest against a mode of speaking which implies that observers only are to be considered as astronomical *workers*. Not on my own account, but on behalf of a long list of honoured names, I oppose the assumption that the careful study of observations (whether those observations have been made by others or not) is not to be regarded as work. If observers claim with pride such names as Herelius, Galileo, Tycho Brahe, Bradley, and many more, the advocates of thoughtful theorising may point no less confidently to Copernicus, to Kepler, and to Newton, and in our own times to Adams and Leverrier. Those who, like the Herschels, have been able to work successfully in both ways, are few indeed in number.

Observations will never be so little useful as when the attempt to utilise them is discouraged.

Richard A. Proctor

P.S.—Mr. Lockyer seems not to be aware that what he claims to have proved respecting the corona is accepted by me as proved, and forms an essential part of my theory. I am as well satisfied as he can be (and on the same grounds) that the corona is not a solar atmosphere.

* It would seem from the experiments of Dr. L. C. de Coppet (which do not appear to be as well known as they deserve) that the treatment adopted by Mr. Tomlinson for rendering the sodic sulphate inactive really changes the salt. Dr. de Coppet finds that a supersaturated solution of sodic sulphate may be prepared by dissolving the anhydrous salt in cold water; and he writes—"I have arrived at the conclusion that the anhydrous sodic sulphate obtained by the efflorescence of the crystals with ten molecules of water, undergo a change of constitution when heated to temperatures superior to 33° or 34°; for the contact of a particle of the effloresced sulphate always causes the crystallisation of a supersaturated solution of this salt, whereas anhydrous sodic sulphate heated above 33° does not necessarily determine the crystallisation."—Bull. Soc. Vaud. Sc. Nat. X., p. 151.

[These experiments render it probable that the so-called supersaturated solutions really contain the anhydrous salt in a state of unstable equilibrium, only requiring a disturbance to cause it to assimilate water, and thus produce a less soluble compound.—Ed.]

* Still holding to our comments we gladly state that they were not written in the spirit in which Mr. Proctor has read them. He is known to all as an astronomical worker, and our objection to his mathematical result was that it was based upon data among which the principal point at issue was accepted as proved.—Ed.

VON GRAEFE

Speaking of the loss of Von Graefe, whose death, at the age of forty-one, we reported last week, the *Revue des Cours Scientifiques* remarks that Germany has sustained a loss equal at least to the loss of a battle. Von Graefe's death was the sequel of a long consumption, during which he neither diminished his work nor took ordinary precautions. His grand discovery of the cure for Glaucoma was made when he was only twenty-six years old. The *British Medical Journal* thus sums up his professional worth:—

In him the world loses its foremost opthalmologist, one whose brilliant originality was equalled only by his steady industry. Not only was Graefe great in the practice of his profession, but as a teacher his influence was almost unbounded. Although comparatively young himself, he had taught almost all the present school of ophthalmic surgeons. His introduction of iridectomy was, without doubt, the greatest step in the operative surgery of the eye since the introduction of operations for the cure of cataract. Probably, there are now living some thousands in the possession of sight, who but for him would have been in darkness. It is one of those gains which is complete in itself, permanent, and beyond the reach of scepticism. It is priceless. Graefe was an untiring observer, and never allowed his pressing engagements to interfere with the record of his vast experience for the good of others. Although he had done a vast amount of other work, still, however, his discovery of iridectomy shines with such pre-eminent lustre that the inscription,

"He cured Glaucoma,"

would be by no means inappropriate. As a man, Graefe was everything that is admirable, and secured the love of all who knew him. He was open, generous, unostentatious, eager both to give and receive knowledge. His personal appearance was as remarkable as the qualities of his mind. The *Wiener Medizen Wochenschrift*, in announcing Graefe's death, says: "German science loses in him one of her greatest celebrities, and suffering humanity one of its greatest benefactors. With Graefe, a combination of geniality, erudition, self-devotion, energy, and amiability, such as is rarely found in one man, has descended into the grave. His name will ever remain most prominently connected with the history of ophthalmic surgery."

*THE CONTINUITY OF THE GASEOUS AND LIQUID STATES OF MATTER**

IT may be truly affirmed of Physical Science, that its history, for some generations at least, has been one of rapid progress and unceasing change, and that its most earnest promoters have not claimed infallibility for their opinions, nor finality for their results. Its advancing progress has been marked by eras when some long-accepted theory or hypothesis, which had appeared so closely in accordance with all known experiments and observations as to have been received as an obvious truth, has, by further experiments extending into regions previously unexplored, been found to be a faulty or incomplete representation of the phenomena.

Such an era has occurred in the discovery recently announced by Dr. Andrews of the Continuity of the Gaseous and Liquid States of Matter.

We have all been accustomed to consider matter as existing in one or other of three states,—the solid, liquid, and gaseous.

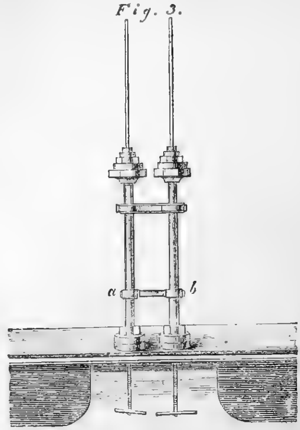
Fig 1.

Fig. 2.

The transition, from any one of these states to another, has hitherto been regarded as necessarily abrupt; at least, if we except the imperfectly understood conditions of softening or plasticity, assumed by such bodies as glass or iron, when gradually passing from the solid to the molten condition. The true state of the case is now found to be very different.

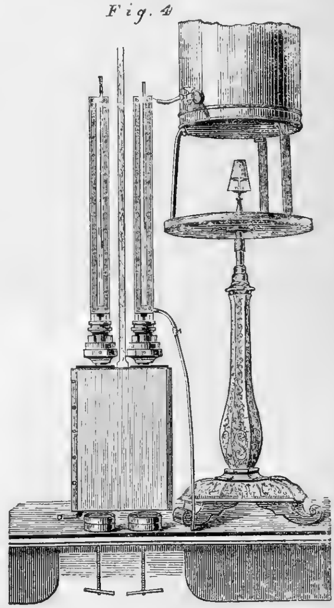
The memoir of Dr. Andrews, of which we propose to give an account in this article, opens with the following historical *résumé* of previous researches bearing more or less in the direction of his investigations:—"In 1822 M. Cagniard de la Tour observed that certain liquids, such as ether, alcohol, and water, when heated in hermetically sealed glass tubes, became apparently reduced to vapour in a space from twice to four times the original volume of the liquid. He also made a few numerical determinations of the pressures exerted in these experiments. In the following year Faraday succeeded in liquefying, by the aid of pressure alone, chlorine and several other bodies known before only in the gaseous form. A few years later Thilorier obtained solid carbonic acid, and observed that the coefficient of expansion of the liquid for heat is greater than that of any aëriform body. A second memoir by Faraday, published in 1845, greatly extended our knowledge of the effects of cold and pressure on gases. Regnault has examined with care the absolute change of volume in a few gases when exposed to a pressure of twenty atmospheres,

Fig. 3.



and Pouillet has made some observations on the same subject. The experiments of Natterer have carried this inquiry to the enormous pressure of 2,790 atmospheres; and although his method is not altogether free from objection, the results he

Fig. 4



obtained are valuable, and deserve more attention than they have hitherto received."

In 1861 a brief notice appeared of some early experiments by Dr. Andrews in this direction. Oxygen, hydrogen, nitrogen,

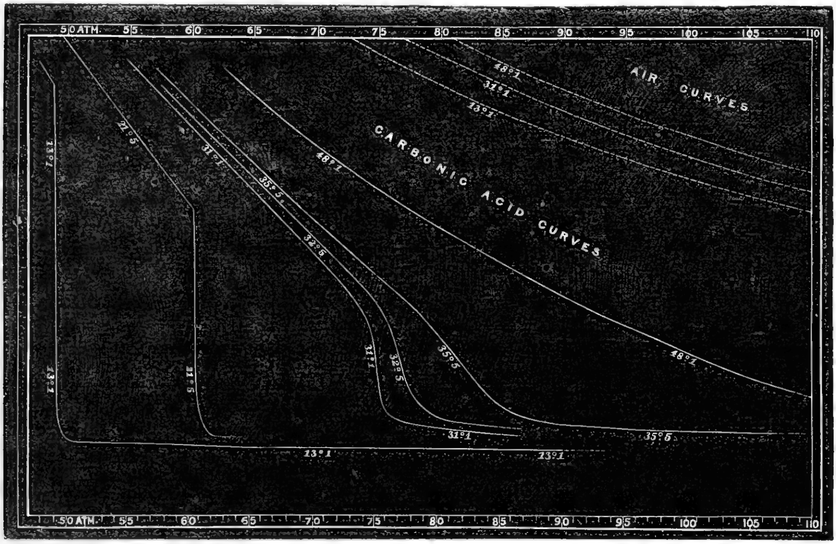
*"The Bakerian Lecture for 1869." By Thomas Andrews, M.D., F.R.S. (Abridged from an Original Essay of Professor James Thomson, LL.D.)

carbonic oxide, and nitric oxide were submitted to greater pressures than had previously been attained in glass tubes, and while under these pressures they were exposed to the cold of the carbonic acid and ether bath. None of these gases exhibited any appearance of liquefaction, although reduced to less than $\frac{1}{500}$ of their ordinary volume by the combined action of cold and pressure. Subsequently, in the third edition of Miller's "Chemical Physics," published in 1863, a short account, communicated by Dr. Andrews, appeared of some further results he had obtained, under certain fixed conditions of pressure and temperature, with carbonic acid. These results constitute the foundation of the researches which form the general subject of the present article, and the following extract from the original communication of Dr. Andrews to Dr. Miller may here be quoted:—"On partially liquefying carbonic acid by pressure alone, and gradually raising at the same time the temperature to 88° Fahr., the surface of demarcation between the liquid and the gas became fainter, lost its curvature, and at last disappeared. The space was then occupied by a homogeneous fluid, which exhibited, when the pressure was suddenly diminished or the temperature slightly lowered, a peculiar appearance of moving or flickering striæ throughout its entire mass. At temperatures above 88°, no apparent liquefaction of carbonic acid or separation into two distinct forms of matter could be effected, even when a pressure of 300 or 400 atmospheres was applied. Nitrous oxide gave analogous results."

For his recent researches Dr. Andrews again selected carbonic acid as the substance for investigation. He devised for his experiments an apparatus, novel in construction, and well suited to exhibit the properties acquired by fluids under very varied conditions of pressure and temperature. The carbonic acid was contained in a glass tube, capillary in the upper and larger part of its length, and for the remainder, of the widest bore in which a column of mercury would remain without displacement when the tube was placed in a vertical position. A movable column or bar of mercury confined the gas to be operated on. This glass tube was secured by careful packing in a massive end-piece of brass, which carried a flange, by means of which a water-tight junction could be made with a corresponding flange, attached to a cold-drawn copper tube of great strength. To the other end of the copper tube a similar end-piece was firmly bolted. The latter carried a fine steel screw, 7 inches long, which was packed with such care that the packing was capable of resisting a pressure of 400 atmospheres or more. A section, exhibiting all the details, is given in Fig. 1. Before commencing an experiment the body of the apparatus was filled with water; the upper end-piece, carrying the glass tube, in which was the gas to be operated on, was firmly secured in its place, and the pressure was obtained by screwing the steel screw into the water chamber. In Fig. 2 the same apparatus is shown with the modifications required when the gas or liquid is exposed to very low temperatures under high pressure. The end of the capillary tube dips into a bath of ether and solid carbonic acid, under a bell jar, from which the air may be exhausted.

In order to estimate the pressure exerted in these experiments, a duplex or compound form of the apparatus was employed, as shown in Fig. 3. The two sides of the apparatus freely communicate through *a b*, so that on turning either of the steel screws the pressure is immediately transmitted through the entire apparatus. In the second tube a known volume of air is confined, and the pressure is approximately estimated by its contraction.

Figure 4 exhibits the complete apparatus with the arrangements for maintaining the capillary tubes and the body of the apparatus itself at fixed temperatures. A rectangular brass case, closed before and behind with plate glass, surrounds each capillary tube, and allows it to be maintained at any required temperature by the flow of a stream of water. In the figure, the arrangement for obtaining a current of heated water in the case of the carbonic-acid tube is shown. The body of the apparatus itself, as is shown in the figure, is enclosed in an external vessel of copper, which is filled with water at the temperature of the apartment. This latter arrangement is essential when accurate observations are made.



The temperature of the water surrounding the air-tube was made to coincide, as closely as possible, with that of the apartment, while the temperature of the water surrounding the carbonic-acid tube varied in different experiments from 13° C. to 48° C. In the experiments as they were performed, the mercury did not come into view in the capillary part of the air-tube till the pressure amounted to about forty atmospheres. The volumes of the air and of the carbonic acid were carefully read by a cathetometer, and the results could be relied on with certainty to less than 1-20 of a millimetre or 1-500 of an inch. We must refer the reader to the original memoir for an account of the details of the experiments, and of the numerous precautions adopted to secure accuracy. The object of the present article is to place as clearly as possible before his mind the main results arrived at, and the general features of the apparatus employed.

In the above diagram, we have a graphical representation of the results of a large number of comparative experiments on air and carbonic acid, under pressures ranging from 48 to 107 atmospheres, and at temperatures for the carbonic acid varying from 13°·1 C. to 48°·1 C. The dotted lines (*Air Curves*) represent a portion of the curves of a perfect gas (assumed to have the same volume originally at 0° C., and under one atmosphere as the carbonic acid), for the temperatures of 13°·1 C., 31°·1 C., and 48°·1 C. The lines designated *Carbonic Acid Curves* show the volumes to which the carbonic acid is reduced at the temperatures marked on each curve, and under the approximate pressures indicated by the numbers at the top and bottom of the figure. Ordinates drawn from the inner horizontal line at the lower part of the figure to meet the curves, will represent the volume of the carbonic acid. These ordinates do not always refer to homogeneous matter, but sometimes to a mixture of gas and liquid.

It will be observed that in the curves for 13°·1 C. there occurs an abrupt, or almost quite abrupt, fall, when a pressure of about 49 atmospheres has been attained. The curve for 21°·5 C. exhibits a corresponding fall, but not till a higher pressure (about 60 atmospheres) has been reached. On close inspection of the figure, a slight deviation from perfect abruptness will be observed in the portion of the curves representing these falls, which Dr. Andrews showed to be due to a trace of air (about $\frac{1}{500}$ part) in the carbonic acid with which the experiments were made. Had the carbonic acid been absolutely pure, there can be no doubt that the fall would have been quite abrupt.

In the curve for 31°·1 C. there is no abrupt fall; but a rapid descent, indicating a corresponding diminution of volume, occurs between the pressures of 73 and 75 atmospheres. As the temperature rises this descent becomes gradually less marked, and when a temperature of 48°·1 C. has been attained, it has almost, if not altogether, disappeared.

At any temperature between −57°C., and 30°·92 C., carbonic acid, under the ordinary pressure of the atmosphere, is unquestionably in the state of a gas or vapour. If within these limits we take a given volume of carbonic acid, and gradually augment the pressure, the volume will steadily diminish, not however uniformly, but according to a more rapid rate than the law for a perfect gas, till we reach the point at which liquefaction begins. A sudden fall or diminution of volume will now take place, and with a little care it will be found easy so to arrange the experiment that part of the carbonic acid shall be in the liquid, and part of it in the gaseous state; the carbonic acid thus coexisting in two distinct physical conditions in the same tube, and under the same external pressure. But if the experiment be made at 30°·92 C., or any higher temperature, the result will be very different. At 30°·92 C., and under a pressure of about 74 atmospheres, the densities of liquid and gaseous carbonic acid, as well as all their other physical properties, become absolutely identical, and the most careful observation fails to discover any heterogeneity at this or higher temperatures in carbonic acid, when its volume is so reduced as to occupy a space in which, at lower temperatures, a mixture of gas and liquid would have been formed. In other words, all distinctions of state have disappeared, and the carbonic acid has become one homogeneous fluid, which cannot by change of pressure be separated into two distinct physical conditions. This temperature of 30°·92 is called by Dr. Andrews the *critical point* of carbonic acid. Other fluids which can be obtained in both the liquid and gaseous states have shown similar phenomena, and have each presented a critical point of temperature. The rapid changes of density which slight changes of temperature or pressure produce, when the gas is reduced at temperatures a little above the critical point, to the volume at which it might be expected to liquefy, account for the flickering movements referred to in the beginning of this article.

The general conclusions arrived at we give in the words of the original memoir. "I have frequently exposed carbonic acid," observes Dr. Andrews, "without making precise measurements, to much higher pressures than any marked in the tables, and have made it pass, without break or interruption, from what is regarded by every one as the gaseous state, to what is, in like manner, universally regarded as the liquid state. Take, for example, a given volume of carbonic acid gas at 50° C., or at a higher temperature, and expose it to increasing pressure till 150 atmospheres have been reached. In this process its volume will steadily diminish as the pressure augments, and no sudden diminution of volume, without the application of external pressure, will occur at any stage of it. When the full pressure has been applied, let the temperature be allowed to fall, till the carbonic acid has reached the ordinary temperature of the atmosphere. During the whole of this operation, no breach of continuity has occurred. It begins with a gas, and by series of gradual changes, presenting nowhere any abrupt alteration of volume or sudden evolution of heat, it ends with a liquid. The closest observation fails to discover anywhere indications of a change of condition in the carbonic acid, or evidence, at any period of the process, of part of it being in one physical state and part in another. That the gas has actually changed into a liquid would, indeed, never have been suspected, had it not shown itself to be so changed by entering into ebullition on the removal of the pressure. For convenience this process has been divided into two stages, the compression of the carbonic acid, and its subsequent cooling; but these operations might have been performed simultaneously, if care were taken so to arrange the application of the pressure and the rate of cooling that the pressure should not be less than 76 atmospheres when the carbonic acid had cooled to 31°.

"We are now prepared for the consideration of the following important question. What is the condition of carbonic acid when it passes, at temperatures above 31°, from the gaseous state down to the volume of the liquid, without giving evidence at any part of the process of liquefaction having occurred? Does it continue in the gaseous state, or does it liquefy, or have we to deal with a new condition of matter? If the experiment were made at 100°, or at a higher temperature, when all indications of a fall had disappeared, the probable answer which would be given to this question is that the gas preserves its gaseous condition during the compression; and few would hesitate to declare this statement to be true, if the pressure, as in Natterer's experiments, were applied to such gases as hydrogen or nitrogen. On the other hand, when the experiment is made with carbonic acid at temperatures a little above 31°, the great fall which occurs at one period of the process would lead to the conjecture that liquefaction had actually taken place, although optical tests carefully applied failed at any time to discover the presence of a liquid in contact with a gas. But against this view it may be urged, with great force, that the fact of additional pressure being always required for a further diminution of volume, is opposed to the known laws which hold in the change of bodies from the gaseous to the liquid state. Besides, the higher the temperature at which the gas is compressed, the less the fall becomes, and at last it disappears.

"The answer to the foregoing question, according to what appears to me to be the true interpretation of the experiments already described, is to be found in the close and intimate relations which subsist between the gaseous and liquid states of matter. The ordinary gaseous and ordinary liquid states are, in short, only widely separated forms of the same condition of matter, and may be made to pass into one another by a series of gradations so gentle that the passage shall nowhere present any interruption or breach of continuity. From carbonic acid as a perfect gas to carbonic acid as a perfect liquid, the transition we have seen, may be accomplished by a continuous process, and the gas and liquid are only distant stages of a long series of continuous physical changes. Under certain conditions of temperature and pressure, carbonic acid finds itself, it is true, in what may be described as a state of instability, and suddenly passes, with the evolution of heat, and without the application of additional pressure or change of temperature, to the volume which by the continuous process can only be reached through a long and circuitous route. In the abrupt change which here occurs, a marked difference is exhibited, while the process is going on, in the optical and other physical properties of the carbonic acid which has collapsed into the smaller volume, and of the carbonic acid not yet altered. There is no difficulty here, therefore, in distinguishing between the liquid and the gas. But in other cases the distinction cannot be made; and under many of the conditions I have described it would be vain to attempt to assign carbonic acid to the liquid rather than the gaseous state. Carbonic acid, at the temperature of 35°·5, and under a pressure of 108 atmospheres, is reduced to $\frac{1}{430}$ of the volume it occupied under a pressure of one atmosphere; but if any one ask whether it is now in the gaseous or liquid state, the question does not, I believe, admit of a positive reply. Carbonic acid at 35°·5, and under 108 atmospheres of pressure, stands nearly midway between the gas and the liquid; and we have no valid grounds for assigning it to the one form of matter any more than to the other. The same observation would apply with even greater force to the state in which carbonic acid exists at higher temperatures and under greater pressures than those just mentioned. In the original experiment of Cagniard de la Tour, that distinguished physicist inferred that the liquid had disappeared, and had changed into a gas. A slight modification of the conditions of his experiment would have led him to the opposite conclusion, that what had been before a gas was changed into a liquid. These conditions are, in short, the intermediate states which matter assumes in passing, without sudden change of volume, or abrupt evolution of heat, from the ordinary liquid to the ordinary gaseous state.

"In the foregoing observations I have avoided all reference to the molecular forces brought into play in these experiments. The resistance of liquids and gases to external pressure tending to produce a diminution of volume proves the existence of an internal force of an expansive or resisting character. On the other hand, the sudden diminution of volume, without the application of additional pressure externally, which occurs when a gas is compressed, at any temperature below the critical point, to the volume at which liquefaction begins, can scarcely be explained without assuming that a molecular force of great attractive power comes here into operation, and overcomes the resistance to diminution of volume, which commonly requires the application of external force. When the passage from the gaseous to the liquid state is effected by the continuous process described in the foregoing pages, these molecular forces are so modified as to be unable at any stage of the process to overcome alone the resistance of the fluid to change of volume.

"The properties described in this communication, as exhibited by carbonic acid, are not peculiar to it, but are generally true of all bodies which can be obtained as gases and liquids. Nitrous oxide, hydrochloric acid, ammonia, sulphuric ether, and sulphuret of carbon, all exhibited, at fixed pressures and temperatures, critical points, and rapid changes of volume with flickering movements, when the temperature or pressure was changed in the neighbourhood of those points. The critical points of some of these bodies were above 100°; and in order to make the observations, it was necessary to bend the capillary tube before the commencement of the experiment, and to heat it in a bath of paraffin or oil of vitriol.

"The distinction between a gas and vapour has hitherto been founded on principles which are altogether arbitrary. Ether in the state of gas is called a vapour, while sulphurous acid in the same state is called a gas, yet they are both vapours, the one derived from a liquid boiling at 35°, the other from a liquid boiling at −10°. The distinction is thus determined by the trivial condition of the boiling-point of the liquid, under the ordinary pressure of the atmosphere, being higher or lower than the ordinary temperature of the atmosphere. Such a distinction may have some advantages for practical reference, but it has no scientific value. The critical point of temperature affords a criterion for distinguishing a vapour from a gas, if it be considered important to maintain the distinction at all. Many of the properties of vapours depend on the gas and liquid being present in contact with one another; and this, we have seen, can only occur at temperatures below the critical point. We may accordingly define a vapour to be a gas at any temperature under its critical point. According to this definition, a vapour may, by pressure alone, be changed into a liquid, and may therefore exist in presence of its own liquid; while a gas cannot be liquefied by pressure, that is, so changed by pressure as to become a visible liquid distinguished by a surface of demarcation from the gas. If this definition be accepted, carbonic acid will be a vapour below 31°, a gas above that temperature; ether, a vapour below 200°, a gas above that temperature.

"We have seen that the gaseous and liquid states are only distant stages of the same condition of matter, and are capable of passing into one another by a process of continuous change. A problem of far greater difficulty yet remains to be solved, the possible continuity of the liquid and solid states of matter. But this must be a subject for future investigation; and for the present I will not venture to go beyond the conclusion I have already drawn from direct experiment, that the gaseous and liquid forms of matter may be transformed into one another by a series of continuous and unbroken changes."

JAMES THOMSON

NOTES

AT last a sum of money has been voted for a new Natural History Museum. In introducing the vote the Chancellor of the Exchequer said the British Museum had long been suffering from repletion, and there were no means of exhibiting the valuable articles which, from time to time, were bought for the national collection. Five years ago the trustees resolved in favour of separating the collections, and it had been determined to separate the natural history department from the books and antiquities. For the natural history collection the typical mode of exhibition had been decided on, and the building required must cover at least four acres. Even the present collection would pretty well fill a building of these dimensions, and provision must be made for further extension. The question was, where should this building be situated? and after referring to possible sites he referred to the locality which we were enabled to state some time ago had been chosen—a plot of ground 16½ acres in extent, which the trustees of the Exhibition of 1851 sold to the Government at 7,000*l.* an acre. It therefore cost 120,000*l.*, but is now worth 100,000*l.* more. The sale was coupled with the condition that any building erected upon the land must be for purposes of science and art. For seven years the land had remained waste, a sort of Potter's field, and a scandal to that part of the metropolis. The Government now proposed to place on that piece of land the museum required for the natural history collection. It would occupy four acres; there would be room for wings, and the outside estimate for the building was 350,000*l.*, not an unreasonable price, considering its extent. For the present, however, the Government merely asked for a small vote to enable them to clear the ground, and in order to take the opinion of the House. Railway communication had now made South Kensington easily accessible, and unless a more eligible, a more accessible, and a cheaper site could be suggested, he hoped the Committee would agree to the proposal. He might add that, if it were hereafter thought desirable to do so, there would be room enough on the same site for the Patent Museum, the necessity of which had been much insisted on. We trust that after the discussion which followed the introduction of the vote the scientific men will speak for themselves, and again let their wishes and opinions be heard.

THE American Association for the Advancement of Science met yesterday (Wednesday) at Troy. Professor W. Chauvenet is president for the year.

IT is gratifying to learn that some of the recommendations of the Royal Commission on Military Education, which were most inimical to the scientific instruction of the army, will not be carried out.

BY Imperial decree the *Association Scientifique de France* has been acknowledged to be an *établissement d'utilité publique.*

THE French observers are making preparations for a combined attack on the 10th of August meteors.

THE list of pensions granted during the year ended the 20th of June, 1870, and charged upon the civil list (presented pursuant to Act 1 Victoria, cap. 2, sec. 6) has been published this week. Among them we note the following:—Mr. Augustus De Morgan, 100*l.*, in consideration of his distinguished merits as a mathematician; Mrs. Charlotte J. Thompson, 40*l.*, in consideration of the labours of her late husband, Mr. Thurston Thompson, as Official Photographer to the Science and Art Department, and of his personal services to the late Princ Consort; Dame Henrietta Grace Baden Powell, 150*l.*, in consideration of the valuable services to science rendered by her husband during the 33 years he held the Savilian Professorship of Geometry and Astronomy at Oxford; Miss Margaret Catherine Ffennell, Miss Elizabeth Mark Ffennell, and Mrs.

Charlotte Carlisle, formerly Ffennell, wife of Captain Thomas Carlisle, jointly, and to the survivors or survivor of them, 30*l.*; Miss Margaret Catherine Ffennell, 10*l.*; Miss Elizabeth Mark Ffennell, 10*l.*; Mrs. Charlotte Carlisle, 10*l.*, in recognition of the labours of their father in connection with the salmon fisheries of the United Kingdom; Mrs. Jane Dargan, 100*l.*, in recognition of the services of her late husband, Mr. William Dargan, in connection with the Dublin Exhibition of 1853, and other works of public importance in Ireland; Mrs. Charlotte Christiana Sturt, 80*l.*, in consideration of the services rendered by her late husband, Captain Charles Sturt, by his geographical researches in Australia; William Henry Emmanuel Bleek, Doctor of Philosophy, 150*l.*, in recognition of his literary services, and in aid of his labours in the department of philology, especially in the study of the South African languages.

THE Radcliffe Observer, the Rev. R. Main, has recently presented his annual report to the Board of Trustees. It concludes as follows:—"An unusually large number of meridional observations have been made, and their reductions kept up to their usual stage; a Second Radcliffe Catalogue of Stars (a work of very considerable labour in its compilation) has been printed and published, in addition to a new ordinary annual volume of the Radcliffe Observations; and, notwithstanding the amount of work of an unusual character which has been performed, none of the ordinary details, either of scientific or of clerk-like work, have in any sensible degree fallen behind or been neglected. I confess that I am well satisfied with what has been accomplished (all of it, in my judgment, being of great utility), and altogether it affords a good specimen of what can be accomplished by a small staff of astronomers well skilled in the making and reducing of observations, and devoting themselves steadily and without intermission to the carrying out of a certain number of definite aims on a plan well prepared and studied beforehand."

MR. JOHN HILTON, F.R.S., late President of the Royal College of Surgeons, who for twenty years has filled the office of surgeon to the above institution, has just been unanimously elected by the governors Consulting Surgeon—an honour which, adds the *Medical Times*, has not been bestowed on any member of the surgical staff since Sir Astley Cooper.

A COMMITTEE has begun operations at the Society of Arts for inquiring into the relations of inventors with the Government departments, and the treatment by these latter of scientific inventions. Considerable zeal is shown on the subject, and the whole question of scientific tribunals will be made matter of discussion. Printed forms for collecting the opinions of those interested will be extensively circulated.

THE Statistical Society has entered on a new epoch of activity. Formerly its presidents were statesmen, but it determined lately, like other learned Societies, to select its president from among its own working members, by choosing Mr. Newmarch, F.R.S. This has been attended with more vigorous action of the Council, and corresponding interest among the Fellows. For the first time the Society, the resources of which have always been limited, gives a premium, having received a donation of fifty guineas from Mr. Wm. Taylor, a Fellow. The Taylor premium is to be devoted to an essay on the subject of Taxation in England, and it appears likely that it will be the means of bringing out many new points on an old and trite subject.

IN order to encourage science our authorities have accorded to graduates of the Lahore University the envied privilege of being seated in Durbar and on other state occasions.

THE first of the Quarterly Weather Reports of the Meteorological Office, with pressure and temperature tables for the year 1869, and notes on Easterly Storms, issued by the Meteorological Committee, embodies the results of observations made at the observatories of Kew, Stonyhurst, Glasgow, Aberdeen, Armagh, Falmouth, and Valencia, during the first three months of last year. The succeeding quarterly numbers for the year will follow as quickly as possible, and the journal will in future appear regularly at intervals of three months. The first number contains complete tables for pressure and temperature, &c., for the year 1869, and the plates exhibiting the continuous registration of these observations for the quarter are arranged to show at one view the instrumental curves at each of the stations for five days, one plate comprising pressure and temperature, while another shows the direction and velocity of the wind, scales of measurement being given at the sides both on the British and French systems. We shall return to this report.

THE *Engineer* states that the new dye known as soluble garnet seems to be coming more largely into use on the Continent, and as the colours produced with it are exceedingly brilliant, similar to those obtained with archil, but much more stable when exposed to light and air, the garnet dye is likely to become a great favourite. The dye was first prepared by Casthelaz of Paris, and is the ammonia salt of isopurpuric acid, which is formed by the action of a metallic cyanide upon picric acid. It is not prepared from the pure crystallised, but from an inferior kind of picric acid, and is probably destined to replace the archil in many cases, in imparting to wool all shades from garnet to chestnut brown. It may be readily combined with other pigments, so that a number of different colours may be obtained. According to Casthelaz, the dyeing of wool and of silk is effected by the addition of an organic acid to the bath, for instance, acetic or tartaric acid, mineral acids being excluded. The dye bath for silk should be cold or tepid in the beginning. Different shades in red and brown are thus obtained that are dependent upon the concentration of the bath, the nature of the mordant, and the time of the operation.

IN the attempts now being made further to utilise the hill regions of India for English residence, the Observatory at Nynee Tal, itself a hill town, is to be removed to Raneekhet, which is said to be chosen by some of its patrons as the future hill capital of India.

A HORTICULTURAL establishment has been opened at Guatemala for the export of the seeds, flowers, and young plants of the country.

THE Peruvian Government is endeavouring to develop the saltpetre district of Tarapaca.

A RICH silver mine has been discovered by Messrs. Lepiani and Steffani near Huamantanga, in Peru, and measures are taken for working it.

THE field of the new silver mines near Cobija, in Bolivia, the discovery of which was reported by us, is stated to be 3,000 to 5,000 marcs per cajou, or from 700*l.* to 1,200*l.* of silver per ton of ore.

ON the 16th June an earthquake was felt all over the state of Nicaragua. At Granada, alarming noises were heard from Momotombo, an extinct volcano in the neighbourhood.

ON the 26th May there was a tremendous earthquake at Lima, the first for a long time. It was also felt at Callao.

AMONG the sums voted last week by the House of Commons in Committee of Supply were the following:—64,721*l.* for Public Education in Great Britain; 164,836*l.* to complete the vote for the Department of Science and Art (being an increase of 11,883*l.* on the vote of last year); 51,255*l.* to complete the vote for the Museum; 6,827*l.* for the University of London; 8,220*l.* for the salaries and expenses of the Endowed Schools Commission; 12,894*l.* for the Scottish Universities; 2,140*l.* for the Queen's University, Ireland; and 2,915*l.* for the Queen's Colleges, Ireland. The total educational estimate for the year was stated by Mr. Forster to be 914,721*l.*, being a net increase of 74,010*l.* over that of last year. The day scholars in average attendance have increased from 1,082,000 to 1,200,000. There are 223 more male and 104 more female teachers than last year. Since 1868 the number of Science schools has increased from 300 to 810, and the number of scholars is now nearly 30,000; the number of scholars in Art has increased since last year from 123,562 to 157,198. The increase in the number of scholars in the regular schools is stated to be in excess of the increase of population. These statistics are interesting, as showing that the increased desire for education in the country at least keeps pace with the advance of opinion among the governing classes in favour of a truly national system of education. The query whether prevention is better than cure is forcibly suggested by three other votes which were passed on the same night:—315,627*l.* for convict establishments in England and the colonies, 203,880*l.* for the maintenance of juvenile prisoners in reformatories, and 643,070*l.* for the constabulary force in Ireland. When shall we arrive at the pitch of civilisation of one of the Swiss Cantons, where the expenditure for educational purposes exceeds that for all other purposes put together?

WE learn from the last report of the Geological Survey of Italy (R. Comitato Geologico) that that body will publish a geological map of Italy on the scale of 1 to 600,000 during the course of next year. The map is that which was compiled by Professor I. Cocchi in 1867 and sent to the Universal Exhibition in Paris. It was a hand-coloured map, the Ordnance map of Upper and Central Italy in six sheets being used as a basis. In compiling this map Professor Cocchi made use of all the published and unpublished materials that he could find. The most southern provinces of the Peninsula and Sicily were not however represented, for although notes and papers on their geology were not wanting, that part of the kingdom had not been mapped geologically. The new map will be divided into four sheets, and new plates will be engraved copying the topography of the Ordnance map, and introducing such modifications and improvements as may be deemed necessary for the new object to which the map is to be applied. The colouring will be done by chromolithography. Accompanying the map there will be a short descriptive memoir and two geological sections, one along the length and the other across the breadth of the country.

M. DIAMILLA-MULLER calls upon all directors of magnetical observatories to observe the declination and inclination every ten minutes from midnight 29th of August (Paris time), to the next midnight, and send the results to him at the bureaux of the Association Scientifique de France. He adds, "On croyait généralement que le soleil agissait indirectement par suite des changements de température qu'il produit à la surface de la terre. J'avais déjà présenté l'hypothèse, basée sur les observations d'Arago, tendant à établir que l'action directe du soleil sur le magnétisme est absolument semblable à l'action d'un aimant sur le fer. Cette théorie est confirmée par les observations faites dans les Colonies anglaises, où l'on remarque l'opposition de signe que le changement de déclinaison du soleil imprime aux courbes qui représentent la variation magnétique dans les pays tropicaux. Il est nécessaire de constater, par une observation directe, que cette loi d'opposition, en rapport avec la déclinaison solaire, s'exerce dans toutes les régions du globe."

PROF. LIONEL BEALE's inaugural lecture to the course of Pathological Anatomy, delivered at King's College, May 5th, 1870, is issued as a separate publication, with the title "On Medical Progress; in memoriam R. B. Todd."

THE third part is published of Dr. Manzoni's "Bryozoi fossili Italiani," accompanied by four plates.

ANOTHER contribution to astronomical literature lies on our table, in the shape of the second volume of "Astronomical Observations taken during the years 1865-69, at the private Observatory of Mr. J. G. Barclay, of Leyton."

MR. KEITH JOHNSTON, jun., publishes, in his usual admirably clear style, a map of the Lake Region of Eastern Africa, showing the sources of the Nile, recently discovered by Dr. Livingstone; with notes on the exploration of this region, its physical features, climate, and population.

*ON VOLCANOES**

VOLCANOES are but so many existing proofs of the activity of internal forces at the present moment, and, as a geologist, I may be almost pardoned if I regret that we do not in our happy isles possess even a single example of an active volcano.

As regards the geographical distribution of recent volcanoes, a glance at the geological map of the world will suffice to show that they are in reality scattered all over its surface, yet, it may be added, more rarely occurring at any great distance from the sea, although exceptional instances are met with inland, in all the four quarters of the globe.

In the North we find the volcanoes of Iceland, Jan Meyen, Kamskatca, Alaska, and others; whilst the Antarctic voyages of Ross proved that the mountains of the land nearest accessible to the South Pole were also active volcanoes.

At the equator, all but innumerable volcanoes are seen in the islands of the Indian and Polynesian Archipelagos, as well as in the Pacific and Atlantic Oceans, and on the main land of South America. Midway between the Equator and the Poles are situated the volcanoes of New Zealand, the Canaries, Cape Verde, Azores, and Sandwich Islands, as also those of Arabia, Eastern Africa, Mexico, Central America, and the volcanoes of the whole range of the Andes down to Terra del Fuego. Nearer home, Vesuvius, Etna, Stromboli, Santorin, and numerous others in the Mediterranean, if not so grand in their dimensions as some of those previously referred to, still present on the large scale all the various aspects of volcanic phenomena, both submarine as well as terrestrial.

If now, however, we take a broader view of volcanic phenomena, and, in addition to the before-mentioned still existing proofs of the general distribution of volcanic centres, as they have been termed, we also take into consideration the occurrence of eruptive rocks of similar origin which are everywhere found disturbing and breaking through the strata of even the oldest rock formations, it will be seen, as least as far as the geology of the earth's surface is at present known to us, that there is scarcely a single area of any magnitude, of either the land or sea, which, at some period or other, has not been broken through or disturbed by what may be termed volcanic forces acting from within the mass of the earth itself; and it is impossible to come to other than the conclusion that these agencies must have played a most important part in determining the main features of the earth's external configuration as well in our times as throughout all periods of its history.

If now the question be asked, what is a volcano? the simplest reply would be "a hole in the ground deep enough to reach such portions of the interior of the earth as are in a molten condition."

In ordinary language, however, the appellation of volcano is usually restricted to those cone-shaped mountains, from the hollow summit of which flames, smoke, and vapours are at times seen to ascend, and which occasionally break out into more imposing activity by vomiting forth showers of ashes and fragments of incandescent rock, or by pouring out torrents of molten stone, to deluge and devastate the unfortunate country in the vicinity.

It having always been admitted that volcanoes owed their origin to forces operating from below, it was suggested by Von Buch, and supported by Humboldt and others, that volcanic cones must be formed by some portion of the surface of the earth, weaker than the rest, being forced out, or, as it were, thrown up like a soap-bubble by the pressure of the vapour and gases confined below, the strata being thereby elevated, fractured, and tilted up on all sides, so as to produce a conical elevation, the central fissure in which became a crater or vent for the escape and passage of the gaseous and liquid emanations from below.

* Outline of a Lecture delivered at St. George's Hall, Langham Place, 9th June, 1870, by David Forbes, F.R.S.

This hypothesis, which accounted for the formation of volcanic cones and craters by a process of upheaval, or, as it was termed, the "crater of elevation," is here alluded to, only because it for a long time was accepted by many eminent men of science, until the subsequent researches, especially of Mr. Scrope and Sir Charles Lyell, demonstrated conclusively that it is not confirmed when their actual structure is studied in the field, and explained their true formation, by what is now termed the "crater of eruption" theory.

If we imagine a volcanic cone cut through its centre, so as to present us with a section of its entire mass, it will be seen that the mineral matter of which it is composed possesses in itself a sort of arrangement in layers, which at first sight somewhat resembles beds of ordinary sedimentary origin broken through and tilted up towards the centre; a closer examination, however, shows that these layers were never at any time horizontal, but that, on the contrary, they had from the very first been deposited in the same inclined position in which they are now seen, and that they must have been formed subsequently, not previous to the opening of the crater itself, since they are entirely composed of matter thrown up from its orifice.

The commencement of an eruption is known in most cases by certain preliminary symptoms indicative of great internal disturbance, such as rumbling noises, and sounds as if of explosions below, which have been likened to subterranean thunder. The surface waters, springs and wells in the vicinity generally acquire an unusually high temperature, diminish in volume or disappear altogether, and repeated earthquake shocks more or less severe are felt, which eventually culminate in a grand convulsion, by which the surface is rent asunder with fearful violence, allowing immense volumes of previously pent up vapour and gases to rush forth from the fissure with such impetuosity as to hurl high into the air huge fragments of the shattered rocks, along with vast quantities of molten lava, in so liquid a condition that during its ascent it is seen to be splashed about in the air like water, and to become separated into particles of all sizes. Vast quantities of these particles, to which the name of volcanic ash or dust has been applied, are instantaneously reduced to so fine a state of division, literally "blown to atoms," as to become converted into an almost impalpable powder, capable of being carried away by the winds prevailing during an eruption to distances of even hundreds of miles from the orifice from which they had been ejected, and ultimately settle down on the land or in the sea to form deposits, whose nature would often be a puzzle to geologists, did not the microscope at once reveal their true mineral character and volcanic origin. Other particles less finely divided become granulated and fall down from the air in the shape of small black grains, known as volcanic sand; whilst still larger portions, owing to the bubbles of vapour or gas entangled in their substance, descend as black porous or spongy stones, from the size of a pea to that of one's head, or larger; and have received the names of Lapilli, scoriæ, or volcanic cinders, from their presenting much the appearance of an ordinary cinder from a coal fire. Although the scoriæ thrown up by volcanoes are in major part of a dark colour, there are also others (called trachytic) much lighter both in colour and weight, which are usually more common at the commencement of an eruption, the ordinary pumice stone which is imported in large quantities from the volcanoes in the Lipari Islands, for the use of the painters, &c., is an example of this variety familiar to you all. A peculiar form of lava is produced by the currents of wind blowing over the surface of the molten matter in the crater, catching up portions of it and drawing them out into long slender filaments like hair or spun glass of all shades of black, brown, or yellow. In the Sandwich Islands, where this variety is very abundant, it is called Pele's hair, from the name of one of their ancient goddesses. In the intervals of an eruption, or after the greatest force of the rush has spent itself, the vapours often rise through the molten lava in the crater, in smart puffs which carry up with them portions of the fluid lava high into the air, whence they descend consolidated as spheres or somewhat elongated bodies consisting of an external shell of solid lava, hollow or only filled with vapour or gas in the centre. From their resemblance to military projectiles, these bodies, which vary from the size of an orange to that of a pumpkin, have received the name of volcanic bombs.

The mineral matter thrown up into the air from a volcanic vent necessarily descends again by virtue of its own weight, a portion drops back into the crater, but the major part falling beyond it, accumulates around its brink to form a mound, which, since the larger and heavier pieces are not projected to so great a distance as the others, keeps, as it increases in size, raising itself more rapidly in height nearest around the vent, then farther off, and thus builds up a hollow cone, the throat or chimney of which is kept open, at least during the continuance of an eruption, by the upward rush of the gases and vapours forced through it by the pressure below. The action of the heat being of course much more intense in the chimney or throat of the crater, now causes, the at first comparatively loose materials which formed its walls, to soften and cement themselves together on the inside into a sort of compact stony tube of communication with the lower regions, much more solid and resistant than the rest of the mass of which as before described the entire cone had been built up. Once this is the case, the molten lava, forced up by the gaseous pressure below, frequently ascends into the crater itself, and overflowing its brim, pours down the outside of the cone, just like water when placed over too rapid a fire is seen to boil over the edge of the pot in which it is heated. These occasional overflows of lava explain how in the section of a volcanic cone layers of more compact lava are so frequently seen alternating with those of the porous scoria and volcanic sand before described. In more rare instances, as for example in the eruption of Mauna Loa, in the Sandwich Islands, in February 1859, the lava is ejected in so wonderfully liquid a condition, and in such enormous volumes, as to present the appearance of a red-hot fountain; the jet of molten lava thrown up from the crater on that occasion is described as about 250 feet in diameter, and as rising some 500 feet above the level of the brim of the crater itself. Occasionally, during an eruption, the rim of the crater, unable to support the weight of the molten lava which fills it, gives way at its weakest point, the lava bursting out and carrying away one side of the cone itself; at other times the lava, after having risen some height up the crater, finds out a point of weakness and breaks through, discharging itself by a fissure some way up the side of the cone, as was the case with the volcano of Sajama, in Bolivia, in 1859, and with Etna in 1865. In many eruptions the lava does not ascend at all into the crater, but breaks out at the very base of the cone, or even at some considerable distance from it, through a subterranean passage. This took place in the eruption of Kilauea, in the Sandwich Islands, in June 1840, when the lava first showed itself at the surface at Arare, some six miles eastward of the crater which supplied it. In fact, most volcanoes will, upon examination, be found at one or other period in their history to have presented examples of more than one, if not of all, these different modes of discharging their molten products.

The eruption of Etna in 1865, which I witnessed, did not proceed from the summit or main crater, but broke out on the side of the mountain, about 5,000 feet above the level of the sea. Along the fissure or rent formed by this convulsion, no less than seven distinct cones rose up at intervals, building themselves up very rapidly from the enormous quantities of scoriæ which were thrown up from their rents; as they became larger the bases of several of these cones extended until they united, and so formed a range of hills, the summits of which in but a few weeks reached the height of several hundred feet, and entirely changed the character of the scenery of this part of the island. The four lowest cones were the most active, but from none of their craters was there any overflow of lava, which, however, poured out from the very base of the cones, forming a fiery river apparently about three miles across, which destroyed all before it, cutting through a large pine forest, and at one place leaping like a cascade of liquid fire over a precipice some 150 feet in height.

The formation of a new or re-opening of an old volcanic vent is usually accompanied by a terrific explosion, often to be heard at immense distances; thus, in 1812, the outburst of the volcano of San Vincent was heard in the north of South America some 700 miles distant. The enormous force developed by the rush of gases and vapours from the fissure may be imagined when it is known that in the eruption of Mount Ararat, in 1840, huge masses of rock weighing as much as 25 tons were thrown out of the crater; Cotopaxi is said to have even hurled a 200-ton rock to a distance of nine miles; whilst the volcano of Antuco, in Chili, in 1828, sent stones flying to a distance of 36 miles.

The issue of gaseous matter from the crater of a volcano is often described as a column of flame; this is incorrect, for although possibly a little burning hydrogen or sulphuretted hydrogen might be present, especially on the outer edge of the column, the appearance of a column or fountain of flame is in reality due to the gaseous matter of which it consists being illuminated by the fragments of red-hot rock and molten lava thrown up along with it (like sparks in fireworks), assisted by the reflection from the red-hot sides of the crater itself, and from the surface of the molten lava below.

The chemical composition of the gasiform emanations from volcanoes proves that they are in greater part incombustible, and therefore does not support the idea that the body of such a column of vapour and gases could be in flames, *i.e.*, actually burning. On the outside of the column, however, innumerable brilliant scintillations of a bluish colour are frequently seen, due to particles of sulphur taking fire as they come in contact with the outer air, and patches of melted sulphur are splashed about, burning brightly as they fall through the air on to the slopes of the cone. The emission or belching forth, as it has been called, of the gaseous matter with its accompanying red-hot ashes and scoriæ, is more an intermittent than a continuous operation. When an eruption is at its height the spasmodic puffs or blasts are jerked out at intervals of but a few seconds, attended by a terrific roaring or bellowing noise difficult to describe in words.

The buried cities of Stabia, Herculaneum, and Pompeii, covered up in parts to the depth of 100 feet by the ashes of Vesuvius, are ocular proofs of the vast quantity which can be sent out of a volcanic vent during an eruption. The volcano of Sangay, in Ecuador, in constant activity since 1728, has buried the country around it to a depth of 400 feet under its ashes, and a French geologist has estimated that in the course of only two days the volcano of Bourbon has thrown out no less than 300,000 tons of volcanic ashes. The immense distances to which these may be transported by the winds is no less surprising; the ashes of Vesuvius, in the eruption which buried Pompeii, darkened the sun at Rome, and were carried as far as Syria and Egypt; those from San Vincent, in 1812, are reported to have made the sky as dark as night in the Barbadoes; and in Iceland, in 1766, the air became so charged with ashes for a distance of 150 miles around Hecla, that even the brightest light could not be distinguished at a few yards.

Amongst the still active volcanoes we meet with some whose craters are several miles in diameter, encircled by precipitous sides rising to even a thousand feet above the bottom of the crater when at rest, which, as in the Sandwich Islands, may contain reservoirs, or rather lakes of liquid lava, two to four miles across, and at times send forth rivers of molten stone several miles in breadth, extending their fiery inundation to a distance of even forty miles from the crater whence they issued. In the eruption of Hualalai, in 1801, a lava current, after reaching the coast, poured out such volumes of melted matter as to fill up a bay some twenty miles deep, and in its place extend a headland some three or four miles farther into the sea. The rate at which these rivers of molten stone flow is a very varying one; in 1805 the lava current from Vesuvius is said to have run down the first three miles in four minutes, yet only completed its total distance of six miles in three hours; and in 1840 that from Mauna-Loa advanced no less than eighteen miles in two hours; whilst on the other hand it is recorded that during the eruption of Etna, which commenced in 1614, and continued many years, the lava stream only completed a distance of six miles in ten years, notwithstanding that all this time it was seen to be in slow but almost imperceptible motion; during the eruption of this volcano in 1865, I found, however, that at the edge of the current the rate of motion varied from 15 to 120 feet per hour according to local circumstances; in the centre of the stream the lava was evidently still more rapid in its movements.

The entire mass of a lava stream often advances, even when to the eye it would appear to have become quite solid; upon my throwing a heavy stone on to the top of a lava current so far consolidated that the stone merely fixed itself into the surface without sinking deeper, it was seen that the stone moved along with the lava which otherwise looked as if stationary. The surface of this lava consolidated and cooled with almost incredible rapidity, so much so that, notwithstanding the protestations of my guides, I walked over lava currents when, at the same time, the fiery stream still flowing below could be distinctly seen through the cracks in the crust over which I passed.

On this occasion also the stems of the pine-trees in the forest which was destroyed by this eruption were converted into charcoal as high as the lava reached, but the upper portions of the trees then toppled over, and remained in an almost unaltered and uncharred condition on the top of the lava current which had so quickly cooled. The crust which forms on the top of lava when cooling, being an excellent non-conductor, acts so efficiently in preventing further escape of heat, that we find streams of lava requiring many years and even ages to become quite cold. Dolomier relates that the lower part of the Ischia lava of 1301 was still hot in the year 1785.

When, owing to the descriptions of the ground around volcanoes, the water from springs, rivers, lakes, or the sea itself, is brought into contact with the heated mineral matter below, we have the production of the so-called mud volcanoes or of fissures sending forth torrents of heated mud and water, and often, to the great surprise of the inhabitants, throwing out numbers of fishes which had lived previously in these sources. The Geysers of Iceland are somewhat similar phenomena, but on the present occasion time will not permit these subjects being treated in detail.

Whilst some volcanoes like Stromboli, the lighthouse of the Mediterranean, as it was called by the ancients, have continued in incessant activity from the oldest historical periods down to the present day, the eruptions of others are only known to have taken place at long intervals. Vesuvius, although imagined by Strabo to have had a volcanic origin, was not known even by tradition to have ever been in eruption until the year 79, when Pompeii was overwhelmed by it. Since that time, however, up to the present date, it has given ample proof of its volcanic activity, yet its history shows several intervals of a century, and one of more than two centuries, in which no eruption took place. No outbreak of the volcano Sangay in Ecuador is recorded before 1728, since which year it has been in continued activity, and Krabla in Iceland also remained at rest for several hundred years before 1724. In fact, it may be safely affirmed that it is quite impossible for us to know whether any volcano at all is entitled to be regarded as really extinct. Even for ages after the last outburst of lava, it is found that smoke and acid vapours continue to be given off from most volcanic rents, and the extraction of the sulphur found in the craters and sublimed into the fissures around dormant volcanoes, forms in many countries an important branch of industry.

Although, as yet, I have confined my remarks altogether to terrestrial volcanoes, it must not be supposed that the depths of the sea are exempt from such visitations, and in the last few years we have had several prominent examples to the contrary in different parts of the world. Submarine volcanoes were well known to the ancients; Pliny and older writers refer to those in the Mediterranean which threw up the islands of Delos, Rhodes, Anappe, Nea, &c. In the Cyclades very curious examples have occurred, both in very ancient and in the most recent times. Of these islands, Therasia is recorded to have been formed in the third century B.C., as also somewhat later in the same century the island of Thera, now called Santorin; subsequently Hiera, 91 B.C., and then Thea, A.D. 19, appeared, which last two were, in 726, united by an eruption, and together formed the present island of Kaimeni. In 1575 a smaller island called Little Kaimeni showed itself, around which, in 1650, numerous other islets were thrown up, which subsequently became united to Little Kaimeni during the eruptions, which continued from 1707 to 1812, when the island, thus increased in size, became known as New Kaimeni. Finally, the last eruption (still going on), which commenced 28th January, 1866, presented us, on the 2nd February, with a new island, now called King George's Island, from the present King of Greece, which, according to the latest accounts, still continues to increase in size. Numerous other examples might be cited, but I shall only mention the island of Johanna Bogoslawa, in Alaska, which, although it only first showed itself above the water in May 1796, had, in 1806, increased so as to be an immense volcanic island, the summit of which was then elevated to no less than 3,000 feet above the level of the sea.

The volcanic products thus forced out under the sea present, as might be expected, a very different aspect from that of the ashes, scoria, and lava from terrestrial volcanoes; the molten lava coming in contact with the water is at once broken up into fragments, coarser or finer, in proportion to the greater or less cooling power of the water in immediate contact with them, and often in great part instantly converted into fine mud, of a greyish colour when formed from prachytic lava, but more commonly of a chocolate or other dark tint, and much denser when produced from the more prevalent pyroxenic lava. Beds of this character, spread out by the action of the sea, often enclosing shells, fish, and other organic remains, become in time consolidated and upheaved, and as they often present an appearance much resembling ordinary volcanic rocks, they have frequently puzzled geologists, who at first found a difficulty in explaining the presence of such fossils in rocks apparently of igneous origin.

Many writers on this subject hold to the belief that volcanoes are mere local phenomena, each one springing from its own comparatively small reservoir of molten matter, supposed to have originated from the softening or fusion of rocks pre-existing on the spot at some depth below the surface. To me, however, this hypothesis appears altogether untenable when it is remembered, amongst other objections which I have elsewhere considered, that volcanic rocks are encountered in all parts of our globe, often continuous or nearly so, over immense areas, and that all these rocks, without reference to the part of the world in which they occur, are invariably alike in character to one another.

Volcanic rocks may be classified under two heads, viz., the dark-coloured, more dense; and the less heavy, light-coloured lavas, termed, respectively, the basic or pyroxenic, and the acid or trachytic lavas. Both these varieties may proceed from the same volcanic vent in succession—for instance, in Vesuvius, where the mineral matter which buried Pompeii is trachytic, but the later lavas are generally pyroxenic in character. This also was the case in the recent eruption of Santorin, as reported upon by the Austrian Scientific Commission.

The examination of volcanic products, no matter how distant the volcanoes may be from one another from which they are taken, prove them to be altogether identical in general, mineral, and chemical constitution.

Taking all these and other data into due consideration, I cannot arrive at any other conclusion than that all volcanoes are connected with one another in depth, and having one common source, not necessarily situated at any enormous depth below the surface, but in which the molten matter—whilst always containing certain general characters—has undergone considerable modifications in composition, mineralogical and chemical, from time to time in the world's history; for under the term volcanic rocks, I would here include all eruptive rocks without exception, whether called granites, syenites, porphyrites, basalts, or lavas, all of which I regard as but so many members of one series, or simply as the products of the volcanic action of different geological epochs.

So much for the molten products of volcanoes. Now a few words on their gasiform emanations, which consist in greater part of the vapour of water, *i.e.* steam, along with volatile chlorides, hydrochloric and sulphurous acids, nitrogen and sulphuretted hydrogen gases. The sulphur, seen to be sublimed in so large quantities, is probably derived from the mutual reactions of the sulphurous acid and sulphurated hydrogen gases, as they come into contact with one another.

Now if it be true that we have a vast accumulation of molten matter at a certain depth below the surface, which observation further informs us must, in major part, consist of the silicates and sulphides of the metallic elements, then, in my opinion, at least, it only requires the assumption that water from the sea should, by some means or other, find its way down into such a reservoir, to account for all the phenomena of volcanoes, both mechanical as well as chemical. The greater part of the water so introduced would be at once converted into steam, which, in its turn, would become still further expanded by a heat so great as that of molten lava, and would develope an enormous power. Calculations have been made which show that water, even when treated to a much less temperature, would exert an "ejection force," as it has been termed, even exceeding that developed in eruptions of the highest volcanoes known. Another portion of the water with the air carried down along with it, acting upon the highly heated sulphides, would become decomposed, and furnish the sulphuretted hydrogen, sulphurous acid, and nitrogen gases given off, whilst the common salt in the sea water, by its action on the hot silicates in presence of steam, would eliminate hydrochloric acid, and account for the appearances of it, as well as of the volatile chlorides found in volcanic fumes. If we accept this explanation, the chemical reactions would be but the effects and not the cause of volcanic phenomena.

The destructive effects attendant on volcanic convulsions are of two different characters, viz., those arising from the earthquakes which accompany and, as a rule, precede outbreaks; and those caused by the products ejected from the volcano itself. The connection of earthquakes with volcanoes has been noted from the oldest times; the earthquakes which commenced A.D. 63, were but the efforts made by Vesuvius to relieve itself, which culminated in the great eruption of 79; the same was the case in Mexico with Jorillo in 1759, and with the great earthquake of 1834 in Chili, which ended in the outbreaks of Osorno and three other volcanoes of the Andes; and lastly, in 1868, the terrible earthquake which visited the coast of Peru and totally destroyed the cities of Arica and Iquique, was followed by the eruption of Isluga, which, according to the latest news, still continues. There seems little reason to doubt that all earthquakes are of purely volcanic origin, and that volcanoes themselves may be regarded as so many safety-valves for blowing off the surplus steam, gases, and molten products from our great internal boiler; for, as a rule, it has been observed that earthquakes either cease altogether or diminish greatly in violence as soon as a neighbouring volcano has cleared its throat.

Although I have resided several years in what are called earthquake countries, and have experienced numerous and severe shocks, amongst others those which resulted in the total destruction of the cities of Copiapo and Mendoza, on which latter occasion some 20,000 inhabitants perished in the ruins, it seems to me quite impossible to convey in words anything like a true picture of such a dreadful catastrophe; the feeble shocks occasionally felt in England cannot give you even the remotest idea of what a severe earthquake is in reality, for not only are cities destroyed and whole villages swallowed up in an instant, as in the case of Arqure during the eruption of Mount Ararat in 1840, but when situated on the coast, even when they have withstood the shock itself, they may be entirely swept away by the great sea wave which follows close upon it, as happened with the cities of Arica and Iquique, in Peru, little more than a year ago. Equally terrible is the destruction caused by the showers of ashes and torrents of molten rock, as in the well-known instances of Pompeii, Herculaneum, and others, too numerous to mention.

The study of volcanic phenomena presents a wide and interesting field for exploration, for as yet our knowledge of the subject is lamentably defective. To follow it up, however, the student should work out a path for himself, taking advantage of every new means of research placed in his hands by the advance made by the collateral sciences, and steering clear of all schools or preconceived notions. Schools in science are what parties are in politics; the "follow my leader" style will not do in this age, for it does not permit of that perfect independence of thought absolutely requisite to ensure success in the pursuit of science. The study of science is the search after truth, but in its study the persevering and conscientious worker, although sure to attain good results in the end, must always bear in mind that his results, even when proved to be *truths*, are still only fragments of *the whole truth*, and that he therefore should guard himself against overrating their value, *i.e.* the extent of their application, since this can only be correctly estimated when these fragments have been found to fit accurately into their true place in the grand plan of nature.

D. FORBES

SCIENTIFIC SERIALS

THE *Journal of Botany* for July commences with a short account by Dr. D. Moore on a form of *Salix arbuscula* in Ireland, which inclines rather towards *S. myrsinites*. Dr. Seemann proceeds with his "Revision of the Natural Order *Bignoniaceæ*," and Mr. Worthington Smith with his valuable "Clavis Agaricinorum," these three articles completing the portion of the paper devoted to original articles, which we regret to see reduced to so small a space. Then follow the second part of Dr. Braithwaite's "Recent Additions to our Moss Flora," and appreciative reviews of Dr. Hooker's "Students' Flora," and Prof. Babington's "Flora of Iceland."

THE *Student and Intellectual Observer* for July contains several good articles, though none of any striking originality. The longest is by Dr. W. B. Carpenter, on the Deep Sea, its Physical Conditions, apparently a report of a lecture delivered during the winter in St. George's Hall. Dr. Henry White's article on Demonism and Convulsionism gives some interesting details of the epidemic which prevailed in Europe during the 17th century. Dr. Wickham Legg, on Zymotics, discusses the theory of the fungus-germ theory of diseases of this class, which he admits explains a good many of the facts, but demands too great a concession in the outset, in the presence in the blood of nearly twenty distinct and separate substances, which exist only to serve as a nidus for the specific ferment, and to be a source of injury to the individual. Mr. Llewellynn Jewitt contributes an article on Celts and other Implements of Bronze, profusely illustrated; Mr. Barff, a third article on Poisons; and Mr. Henry J. Slack, two short papers on the Juniper Fungus (*Podisoma*), and on the Structure of *Pinnulariæ*. The two publications of the quarter selected for separate reviews are Proctor's "Other Worlds than Ours" and Wallace's "Contributions to the Theory of Natural Selection;" and minor papers and reports fill up the number.

SOCIETIES AND ACADEMIES

London

Royal Society of Literature, June 22.—N. E. S. A. Hamilton, librarian, in the chair. Mr. W. R. Cooper exhibited a Greek Tablet from the Hay Collection, found by the late Mr. Robert Hay in the Aasaseef, Thebes, about 1823. Mr. Cooper stated that the relic was one of peculiar interest, as it was a palimpsest tablet, upon which had been written, in the bold uncials peculiar to the fourth century, a list of familiar Grecian names, and among them that of Athanasius. This circumstance, and the fact that it was found near to the ruins of a Christian church, where a long inscription in honour of Athanasius once existed, seemed to warrant a belief that the tablet had some connection with that famous bishop, the more so as the name was not a common one in Grecian history, and the characters are unquestionably of the period in which he lived. Mr. Cooper was supported in his views by Mr. W. S. Vaux and Mr. Hamilton, who examined the antiquity with much interest, and supplemented his short paper by some very able remarks of their own.

Quekett Microscopical Club, July 22.—Annual general meeting, Mr. Peter le Neve Foster, president, in the chair. According to the annual report of the committee, which was read, the Club still maintains its popularity and success. It numbers over 500 members, and meets for the prosecution of miscroscopical inquiry twice a month throughout the year. Mr. Peter le Neve Foster, in vacating the presidential chair which he had so ably filled during the past year, delivered his valedictory address, in which he called attention to various open questions in microscopical science, and which were fields well worth the labour required for their investigation, and which he considered the members might undertake with pleasure to themselves and advantage to the world at large.—Prof. Lionel S. Beale, F.R.S., was elected president for the ensuing year, and Messrs. Henry Lee, F.L.S., Arthur E. Durham, F.R.C.S., Peter le Neve Foster, M.A., and Dr. Robt. Braithwaite, F.L.S., were elected vice-presidents, while Messrs. Allbon, T. W. Burr, F.R.A.S., Witham M. Bywater, and Charles F. White, were elected to fill four vacancies on the Committee.—The proceedings then terminated in a *conversazione*.

Syro-Egyptian Society, July 19.—Messrs. Bonomi and Simpson attended to exhibit and explain a large collection of water-colour and pencil drawings, mostly by the late Mr. Robert Hay, and now the property of his son, Mr. R. J. A. Hay, of Munraw. The sketches represented Egyptian views and antiquities, the most interesting of which were as follows:—A series of coloured views of *Philol* and *Koum Ombos*, taken about 1833, the more valuable as the latter temple having fallen down is now almost completely buried in the Nile alluvium. A series of very elaborately finished drawings of the palace temple of *Medinet Habou*, by the late C. Laver. The original measured plans, sections and details of the Pyramids of Gizeh, by C. Catherwood (to whom and Bonomi we owe the first accurate map of the *Haram es Shereef*). These were accompanied by notes and details of the now famous Sarcophagus in the king's chamber. A large panoramic view of *Thebes* and a folio of sketches near Karnak, in pencil, by F. Arundale. A view of the singular Purple Lake near Thebes, so called from an unexplained phenomenon, viz., that its waters at a certain period annually assume a purple tint. And, lastly, a collection of miscellaneous hieroglyphic inscriptions and mural paintings from the Tombs at Gourna. Many of these, apart from their artistic merit, are deserving notice as being excellent illustrations of the marvellous accuracy obtainable by the use of an almost forgotten instrument, the camera lucida, by means of which, ere the days of photography, the splendid works of Canaletti, Britton, Roberts, and Hay were produced. At the same meeting were also exhibited, by Mr. T. Christy of Fenchurch-street, seven volumes of beautiful photographs from the East, taken in 1869 by M. Felix Bonfils. They represented the present condition of most of the buildings comprised in the Hay drawings, and exemplified in many cases the wanton vandalism of the celebrated Mahomet Ali, who caused many of the then almost perfect temples to be destroyed for the sake of their materials, with which distilleries, cotton factories, and warehouses were erected about the years 1836 and 1840, in fact until the havoc was arrested by a vigorous "Appeal to the Antiquaries of Europe" (1841), by the late G. R. Gleddon, U.S. Consul at Cairo, to whose energy and the united action of the savans of France and England, the present conservation of the monuments of Ancient Egypt are due.

Zoological Society, June 23.—Prof. Flower, F.R.S., V.P., in the chair. An extract was read from a letter received from Dr. J. Anderson, of Calcutta, containing additional remarks on the dolphin of the Irrawaddi.—Two letters were read from Mr. W. H. Hudson, Corresponding Member, containing remarks on birds observed by him in the vicinity of Buenos Ayres.—Mr. Howard Saunders exhibited and made remarks upon some nestlings of the Booted Eagle (*Aquila pennata*) from Southern Spain.—Dr. J. Murie read a memoir on the anatomy of the walrus (*Trichechus rosmarus*), principally founded upon the example of this animal that had lived for some time in the Society's Gardens in November 1867.—Dr. J. Murie also read notes on a species of *Tænia* from the rhinoceros, which he regarded as probably undescribed; and on a case of variation in the horns of the Panolian Deer (*Cervus eldi*). A third communication from Dr. J. Murie contained remarks on *Phoca groenlandica*, its modes of progression and its anatomy.—Mr. R. Swinhoe communicated a catalogue of the mammals of South China and Formosa, with notes upon the various species that he had observed during his numerous travels in those countries. A second communication from Mr. R. Swinhoe contained a list of birds collected by Mr. C. Collingwood during a cruise in the seas of China and Japan, with notes by the collector. The collection was stated to embrace examples of 33 species, amongst which were several of rare occurrence.—A communication was read from Dr. O. Finsch, C.M.Z.S., containing an account of a collection of birds recently obtained in the Island of Trinidad. The collection included 115 species, amongst which were several new to the avi-fauna of the island.—Messrs. H. E. Dresser and R. B. Sharpe read a paper on the Great Grey Shrike (*Lanius excubitor*) and its allies. The differential characters of the various species were pointed out, and special attention was drawn to the Indian Grey Shrikes (*Lanius lahtora*) which was considered to be identical with the Algerian *Lanius pallens* or *dealbatus*.—A communication was read from Mr. J. Brazier, C.M.Z.S., containing notes on the habits of the Grakle of the Solomon Islands, recently described by Mr. Sclater as *Gracula kreffti*.—Mr. J. Brazier also communicated descriptions of ten new species of land shells, collected by Mr. W. F. Petterd in various parts of the Australian region.—Messrs. Sclater and Salvin read an account of several species of birds recently received by M. Boucard of Paris, in collections from Mexico, which were new to the avi-fauna of that country.—Dr. J. E. Gray communicated a paper on some tortoises in the British Museum, with description of some new species.

Ethnological Society, June 27.—Extra meeting, Prof. Busk, F.R.S., in the chair. Sir John Lubbock, Bart., described the opening of the Park Cwm Tumulus, in the peninsula of Gower, South Wales, and exhibited a plan of the structure.—The Rev. Canon Greenwell read a paper on his explorations in Grime's Graves, Norfolk. These so-called graves consist of a large number of pits and galleries in the chalk, excavated in prehistoric times for the working of flint. The explorations led to the discovery of many neolithic flint implements, picks made of the antlers of the red deer, and curiously-sculptured fragments of chalk. Colonel Lane Fox, Mr. Flower, Mr. Fisher, Sir J. Lubbock, and Mr. Dawkins took part in the discussion.—Mr. J. W. Flower exhibited a large collection of specimens from the neighbourhood of Mr. Greenwell's discoveries, including objects of widely different dates, such as palæolithic and neolithic flint implements, a large British urn, and a fine Roman glass bottle. Mr. Boyd Dawkins then gave a verbal abstract of his paper on the discovery of the remains of platycnemic, or flat-shinned people in Denbighshire. Explorations were made in a refuse-heap in a tumulus, and in two bone-caverns, and the human remains thus obtained were exhibited. These proved that platycnemism was manifest in the ancient dwellers in North Wales, as well as in those who buried their dead in the cave of Cro-Magnon in France, and in those whose remains are found in the caves of Gibraltar.—Prof. Busk exhibited and described the peculiarly-formed tibiæ, and distinguished two forms of platycnemism, but attached no value to this peculiarity as a race-character.—Several other papers were taken as read, this being the last meeting of the session.

Paris

Academy of Sciences, July 25.—M. J. Darboux read a reply to some observations by M. Catalan on his note on the centres of curvature of an algebraic surface. M. Bertrand communicated a report on a memoir by M. Massieu on the characteristic functions of various liquids and on the theory of vapours.—A note was read on the mechanical equivalent of heat and on the electro-chemical properties of aluminium, by M. J. Violle.—Father Secchi communicated some further remarks on the spectra furnished by various types of stars.—M. Bertrand presented a note by M. Laussedat on the restoration of a conical sun-dial, from a fragment brought from Phœnicia by M. Renan.—Remarks on the variations of the magnetic needle, by M. Broun, were read; the author cited the decennial differences observed at Munich from 1841 to 1861, and drew from them the conclusion that the western declination increased up to 1855, remained variable in 1856, and then diminished to about 1860, since when there appears to have been a slight increase.—A note by M. H. Gal on the brominated derivatives of anhydrous acetic acid was communicated by M. Cahours. The compound described by the author was bibrominated anhydrous acetic acid, which was obtained by pouring bromide of monobrominated acetyle upon pulverised fused acetate of soda, and distilling the mixture. Its formula is $C^8H^4Br^2O^6$.—A note was read by M. Guyot on the volumetric determination of the soluble fluorides. The author employs a solution of perchloride of iron.—A note by M. Dobroslavine on the fatty matters of the chyle was presented by M. Wurtz; and M. H. Sainte-Claire Deville communicated a note from Professor Cossa, of Udine, giving an account of some experiments made with an amalgam of aluminium, and stating that aluminium acts upon iodide of æthyle in sealed tubes at ordinary temperatures, and that he has prepared aluminium-æthyle by the action of aluminium upon stannæthyle.—M. Faye presented remarks upon some peculiarities of the soil of the Landes of Gascony, in which he noticed especially the characters of the "alios" or impermeable stratum which exists at a depth of one metre from the surface of the soil, and which, in his opinion, was the main cause of the former insalubrity of the Landes. He considered that it was formed by the infiltration of water holding decomposing organic matter in solution during the winter season, and the evaporation of the water in the summer.—M. H. Sainte-Claire Deville presented a note on a schistose rock impregnated with carbonaceous matter, sent by MM. Ravizza and Colomba.—A note was read by M. Dieulafait on the *Terebratula-diphya* limestones of the French Alps, from Grenoble to the Mediterranean. From stratigraphical considerations, the author confirms the results arrived at by M. Hébert upon palæontological evidence.—An extract from a letter by M. Pissis to M. Elie de Beaumont on mountain-systems and on the formation of the desert of Atacama was read.—M. Daubrée presented a note by M. F. Garrigou on the chemical examination of a metamorphosed cement from the Bayen spring at Luchon. A ball of cement which had remained for eighteen years in the hot water (147° F.) of the spring was found to have gained a considerable quantity of silica, some organic matter, and a little fluorine. It contained some "microzymas."—M. de Quatrefages presented a note by MM. F. Garrigou and de Chasteigner on the contemporaneity of man with the cave-bear and the reindeer in the cave of Gargas (Hautes Pyrénées). The remains discovered consisted of a hearth, with flint implements, split bones, &c.—A note by M. Perez on the generation of the Gasteropoda was communicated by M. Milne-Edwards, consisting chiefly of remarks upon the phenomena of copulation in snails.—An extract from a letter of M. de Vallier stated that whilst the general results of the silkworm cultivation in the department of the Basses-Alpes have been deplorably bad, M. Rayband-Lange, following M. Pasteur's rules, has sold cocoons to the value of 64,000 francs.

BERLIN

German Chemical Society, June 27.—F. Sonnenschein has found the oxide of cerium $Ce_3 O_4$ to give characteristic colours with alkaloids. The oxide, when added to strychnine and sulphuric acid, produces a violet colour more stable than that obtained with bichromate of potassium. The oxygen thus developed is in the form of ozone.—C. Graebe and C. Liebermann have discovered that anthrahydrochinone ($C_{14} H_8 O. OH_2$) is formed when the chinone of anthracene is fused with potash. By treating bromide of anthracene, $C_{14} H_8 Br_2$, with sulphuric acid, bisulpho-anthrachinonic acid is formed, which yields alizarin when fused with potash. They have also prepared alizarin-sulphuric acid and tried in vain to transform it by fusing potash into purpurine.—O. Hesse has investigated opium-wax, consisting chiefly of cerotate of ceryl, and of palmitate of ceryl.—W. v. Schneider, by oxidising diamylene, has obtained an acid, $C_7 H_{14} O_2$, and an indifferent oil, $C_{10} H_{20} O$.—E. v. Priwoznik describes tetracetyl bromo-gallic acid and bromacetyl-gallic acid.—P. Wexlsky communicates an easy way of obtaining bichlorinated chinone by treating trichlorinated phenole with nitrous acid: $C_6 Cl_3 H_3 O + O = C_6 Cl_2 H_2 (O_2)'' + HCl$.—A. Ladenburg describes some chlorinated derivatives of stanno-triethyl.—R. Radziscewsky has obtained nitro- and dinitro-derivatives of phenylacetic acid.—Hugo Schiff has observed that phenylcarbamic ether—

$$CO\begin{matrix} O\ C_2\ H_5 \\ N\ H\ C_6\ H_5 \end{matrix}$$

yields, by heating, cyanurate of phenyl, diphenylated urea and triphenylated biuret.—A. W. Hofmann has obtained, in an easy way, cyanate of phenyl by treating phenyl-carbamic ether with phosphoric anhydride. The cyanate is slowly transformed into cyanurate. The transformation takes place suddenly when triethylphosphine is brought into contact with the cyanate. As phenyl-carbamic ether when distilled by itself also yields a certain proportion of cyanate of phenyl, the reaction just now mentioned by Schiff obtains a simple explanation. The cyanates of tolyl, of xylyl, and of naphthyl have been prepared in an analogous manner.—Prof. Hofmann then drew the attention of the society to some new lecture experiments, apologising that an experiment formerly described by him as new was not so. The demonstration of the development of heat through crystallisation, by pouring ether upon a supersaturated solution of acetate of sodium, belongs to Faraday. He then poured fuming nitric acid into hydriodic acid gas in a test-tube, which inflamed the hydrogen. $H_2 S$ and H_2Se show a similar combustion. A second experiment consisted in heating a watch-glass containing a minute quantity of aniline green. According to experiments instituted by Dove the red copper lustre shown by this substance in reflected light is exactly complementary to the green colour shown in transmitted light. When heated a violet is obtained exactly complementary to the yellow metallic lustre which it shows in reflected light. A third experiment showed the colouring power of aniline red. In one hundred million parts of water added to one part of the colouring matter, the colour may be distinguished, supposing the layer of the liquid to have half a metre's thickness. A white silk thread left in this bath for twenty-four hours exhibits a very decided colour. A fourth experiment was arranged to show the formation of nitrous vapour and nitric acid, by burning hydrogen in air. A balloon of 10 litres capacity was provided with three openings, two of which, opposite each other, were provided with platinum tubes soldered to glass-tubes, and serving to introduce the two gases. The red vapours and the acid property of the water can easily be observed. The fifth experiment, an easy way of condensing cyanogen gas, depends upon a simple apparatus, for the description of which we must refer to the society's reports. Lastly, a pretty experiment to show alternate reduction and oxidation was exhibited. A copper crucible was placed on a triangle, so that it could be heated inside with a strong gas burner. A funnel was placed over it, the tube of which was connected with a hydrogen apparatus. By alternately removing and again approaching the funnel the copper became oxidised and reduced.—N. Limpricht described derivations of meconic acid.—R. Rieth has taken the vapour density of metallic chlorides, arriving at the formulæ of Hg Cl and $Hg Cl_2$, $Sn Cl_2$, and $Sn Cl_4$, dissociation being out of the question, because tin is scarcely volatile.

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THURSDAY, AUGUST 11, 1870

SCIENCE SCHOOLS AND MUSEUMS IN AMERICA

AT the present time when we are, as it were, taking stock of our Scientific Institutions, an account of the various schools and colleges in the United States, in which Science is made a chief, if not *the* chief subject, may be welcome to our readers. A paper in the *Canadian Naturalist*, by Prof. Dawson—the result of the Professor's travels through the States, in order to determine by personal visits the practical working of the American Science Schools, and to use the experience so obtained, in the founding of a Canadian School of Science at Montreal—has been largely used.

It was for similar reasons that Prof. Agassiz visited the various museums of the Old World, in order to determine what errors he should avoid, and what precedents he should follow, in founding the magnificent Museum at Cambridge, U.S.

Referring to the various institutions in the various States, we will follow the footsteps of Prof. Dawson, adding whatever has been changed or improved since his visit.

In New York, Science has for some time past been at a very low ebb. Unlike London or Manchester, that busy mercantile community has no time to spend on such apparently trifling matters as the propagation of scientific knowledge, and the acquisition of materials for scientific investigation. The only School of Science in New York is Columbia College; an old-fashioned brick building, in a quaint, old-fashioned square, formerly outside the town, now, by the rapid increase of building, quite enclosed and surrounded. The College-buildings form three sides of a quadrangle, and are long, narrow rooms lighted by windows in the sides. Three rooms are used as laboratories for practical analysis, qualitative and quantitative. Another room is the furnace-room, for assaying purposes; another is used for purposes of drawing, and there are numerous class-rooms and lecture-rooms, but all sadly out of proportion to the size of the town. Two rooms are set apart, one for the mineralogical, the other for the geological and palæontological collections. Among the latter the private cabinet of Prof. Newberry, especially rich in remains from the Carboniferous strata, is the most prominent feature.

The staff of professors, lecturers, and assistants, numbers eighteen in all; and the lectures, practical and theoretical, are purely scientific, embracing mineralogy, metallurgy, chemistry, botany, mechanics, physics, geology and palæontology, assaying, and drawing.

The full course for students is three years, at the end of which they are duly qualified for practical mining work, mineral surveying, and practical chemistry. They have to pass an entrance examination in algebra, geometry, and trigonometry, and at the end of their course most of them attain the degree of "Engineer of Mines," or "Bachelor of Philosophy." The number of students in the last account was from 112 to 120. An important feature of the course is that students are expected in the vacation to visit mines and metallurgical and chemical establishments, and to report on these, and also to make illustrative collections; while, during the session, short excursions are made to the machine shops and the metallurgical and chemical establishments in or near the city.

The practical value of such a course of training cannot be too highly appreciated; it is an example which many of our science schools and colleges would do well to follow. In justice, however, to one of the chief of these in England, it should be mentioned that the engineering and chemical classes in Owens College, Manchester, are in the habit of being taken by the respective professors to the leading scientific and chemical works near Manchester.

Besides Columbia College, New York is now in a fair way soon to possess a very fine public museum. In the beginning of 1869 a bill was carried in the House of Congress to establish in New York a museum, under Government control, similar to the British Museum. This museum is called "The American Museum of Natural History," and it published its first report a little time ago. The fine collection of the Prince of Neuwied, formed chiefly in the Brazils and South America, has been purchased, and communications have been sent to all the United States consuls throughout the world to aid the museum by the collecting and purchasing of valuable natural history specimens in their several localities. We cannot leave the State of New York without noticing how the liberal founder of the Cornell University has made provision for practical and theoretical instruction in natural science. Laboratories, museums, herbariums, libraries of scientific works, have been either presented or bought; and by the scheme by which the students can work out the expenses of their education by their skilled labour, it is now possible for the very poorest artisan or mechanic in America to obtain as valuable a scientific education as any given anywhere in the world.

Proceeding from New York to New Haven, we find here a very important and very fine school of science. The Sheffield Scientific School is a modern outgrowth of the University of Yale College, and was first established in 1847 as a small undertaking, conducted by the elder Silliman, chiefly as a school for applied chemistry and scientific agriculture. In 1860 Mr. Sheffield, a wealthy citizen of New Haven, gave it a building and apparatus valued at 50,000 dollars, supplementing this by a grant of 50,000 dollars to found chairs of engineering, metallurgy, and chemistry. In 1863 it was further enlarged by grants of land from the State of Connecticut in aid of scientific education. It also participates in the benefit which Yale College derives from Mr. Peabody's Museum and his magnificent endowment of 150,000 dollars. The Sheffield School contains some fine natural history collections, one of the most valuable of which is one of Economic Geology, admirably arranged, and which shows the immense mineral wealth of America to great advantage.

The buildings of the school are finer and better arranged than those of Columbia College, and its educational scope is wider. There are six distinct courses of scientific instruction open to the students:—

1. Chemistry and mineralogy.
2. Engineering and mechanics.
3. Mining and metallurgy.
4. Agriculture.
5. Natural history and geology.
6. A select scientific and literary course.

The class-rooms and laboratories are extremely well adapted and arranged; there are small cabinets of specimens of great assistance to the professors in delivering their lectures. It also possesses a fine equatorial telescope, made by Clark, having an object-glass with an aperture of nine inches. For this there is a properly constructed tower, furnished with all the necessary instruments for astronomical observations.

The staff of professors, lecturers, &c., numbers twenty-three, and the number of students is now about 150. Amongst the distinguished men holding appointments here, the names of Dana, Silliman, and Marsh are of European renown.

In Philadelphia we find valuable scientific collections belonging to the Academy of Science, an old and still very vigorous scientific establishment, though sadly wanting more room for its collections. Amongst the treasures of these collections there are several that merit particular notice.

Professor Hawkins has just set up in the museum the skeleton of the Hadrosaurus of the New Jersey Greensand, one of the most gigantic of the immense mesozoic reptiles. Besides this extremely valuable skeleton and other remains, there are portions of the skeleton of a gigantic carnivorous reptile of the same age, with formidable cutting teeth, similar to those of the megalosaurus, and, like it, possessing hooked claws, some of which must have been ten inches long.

Belonging to the Philadelphia Academy of Sciences is a fine scientific library, but both it and the collections suffer from want of space. The museum also contains a fine and very complete collection of American skulls, which Messrs. Morton, Wilson, and Meigs have so elaborately worked out. At Philadelphia two valuable works on palæontology have been undertaken. One of these just published was reviewed in NATURE for June 16, and is an extremely valuable monograph on "The Fossil Mammals of America," by Prof. Leidy. The other work is an equally valuable one, being a monograph on the "Fossil Reptiles of America," by Prof. Cope.

Proceeding from Philadelphia to "the queenly city," Baltimore, we find there an Academy and a band of zealous naturalists, as Tyson, Morris, and Dalrymple. The Peabody Institute here was founded by Mr. Peabody, who was a resident in this city for some time, and who presented to the town the sum of 100,000 dollars for these objects, viz.: (1) to found an extensive library; (2) to provide for the delivery of lectures in science and art; to found (3) an academy of music, and (4) a gallery of art.

The building of the institute was in 1868 being rapidly proceeded with, and is probably now finished. There was then no museum, but the library had already become extremely valuable.

In Washington is located the splendid Smithsonian Institution, of world-wide fame, founded for "the increase and the diffusion of knowledge amongst men," now under the direction of Prof. Henry, the able president of the American Academy of Sciences, and who is now in this country. The chief work hitherto done by the Smithsonian Institution has been in the subjects of geology and natural history, and it already possesses extremely valuable collections, carefully and systematically arranged under the able supervision of Prof. Baird. This institution sets a glorious example to other and much older museums in their treatment of those anxious and willing to make use of the accumulated treasures. Their collections are open to the inspection of any naturalist from any part of the world, who, in some cases, are accommodated with rooms for their work, as well as access to the specimens. Under Prof. Henry's personal superintendence a fine collection of American antiquities is in process of formation, and the number and importance of these objects of early warfare and art make the museum extremely valuable and instructive.

It was in Washington that President Lincoln was so basely assassinated. The building where the event took place, formerly Ford's theatre, has been converted into a museum of a character it is believed perfectly unique. This, the Army Medical Museum, contains a series of excellently mounted preparations of great professional interest, remarkable chiefly for its profuse exhibition of the effects of shot and shell, and other implements of war on the human frame. The materials for this museum were chiefly collected during the American Civil War. It may well be said that the Americans are a wonderful people; there are few other nations which would have been capable of so utilising the results of a protracted internecine war as to make them available in after years towards the advancement of medical science and the alleviation of human pain.

In addition to the purely medical collection, this museum also contains a fine and well-arranged collection of skulls of the various aboriginal American tribes, with a few Mexican and Peruvian. There is also a fine collection of skulls in the Smithsonian Institution.

Before concluding this paper it is necessary to mention two separate munificent gifts, by which the late Mr. Peabody did so much to promote the cause of science in America. The Peabody donation to Yale College has been previously alluded to. One result of this is that Yale College, which was formerly devoted to other subjects, has recently made great progress in science, and bids fair to become one of the leading scientific institutions in America. It still lacks, however, funds towards founding more scientific professorships. Besides this donation, Mr. Peabody in 1867 left 140,000 dollars "for the promotion among the inhabitants of my native county (Essex County, Massachusetts) of the study and knowledge of the natural and physical sciences and their application to the useful arts."

From this gift started the Peabody Academy of Sciences in Salem, Massachusetts, which was inaugurated August 18, 1869. The objects of the foundation are still kept in view by the formation of a museum, which, besides a general collection, shall embrace a complete collection of local specimens from the whole county, and shall keep up and augment the museum of East Indian antiquities collected by the East Indian marine societies, and by the formation of a series of lectures on science, to be yearly delivered.

And, lastly, the nature of the valuable museum which Professor Agassiz is collecting at Harvard College, Cambridge, aided by Government grants and private subscriptions, will have been sufficiently learnt from the article on the Harvard Museum, which appeared in NATURE for June 23.

THE VERTEBRATE SKELETON

SKELETAL archetypes, and "theories of the skull," have of late years gone much out of fashion. The view which made each man a potential Briareus as to limbs, seems itself to be considered as no longer having a leg to stand upon. The fortress of the "Petrosal" has long been carried by assault, and is peaceably and securely occupied; and although we have had lately a brilliant passage of arms apropos of the "auditory ossicles" from which the unlucky Sauropsida retired with broken "hammers" and diminished "anvils;" yet the once widespread interest in skeletal controversies seems to have long subsided. The old war-cries are no longer heard, the question "Is the post-frontal a parapophysis?" falls on indifferent or averted ears, and we fear that even not a few of our anatomists call into daily functional activity a mandible, to the true nature and homologies of which they are comparatively indifferent.

What was the surprise of some, then, who last year witnessed, in the theatre of the Royal College of Surgeons, an unlooked-for resurrection. Some rubbed their eyes—could they have had a long sleep, and was it still the year 1849 instead of 1869? A quasi-vertebrate theory of the skull once more! Again an exposition of cranial hœmal arches!

"Jam redit et Virgo, redeunt Saturnia regna."

But yet in justice it must be said that it was by no means the reproduction of an old or familiar system. The views propounded were in some respects as novel as striking; while in spite of this a careful re-statement of assertions made in the first year of the last Hunterian professorship showed how subordinate after all were the changes made, and how trifling the modifications required as to the statements of that first year. Nor in fact was any new archetypical idea of the whole vertebrate skeleton distinctly proposed for acceptance, though the serial relationship of certain inferior arches was clearly demonstrated, and a striking suggestion made concerning the most anterior of them.

But *some* ideal conception of the vertebrate skeleton as a whole is a necessity for anyone who proposes to extend his osteological labours over several classes, and provided such a conception be a simple "generalised expression of observed facts," no one has a right to complain of its introduction. What best conception then of this kind can be now supplied from the accumulated labours of successive osteologists?

As the points of exit from the skull of the cranial nerves supply the best fixed points for determining special cranial homologies, so probably the arrangement of the nervous system as a whole will supply the handiest key to the explanation of skeletal difficulties.

In the Hunterian Lectures of 1869, the nervous system was treated in a new way, and one by which the sympathetic system lost its isolation, and was called in to take its morphologically important part in the general system of spinal nerves. The embryonic condition being referred to (with ascending dorsal plates and descending ventral-plates—the latter bifurcating to enclose the pleuroperitoneal space between their outer and inner laminæ), each spinal nerve of the trunk was represented as sending one branch upwards into the dorsal plate, another downwards into the outer lamina of the ventral plate (abdominal and intercostal nerves), and another, also downwards, but into the inner lamina of the ventral plate, the collection of these latter internal nerves with their serial homologues forming the sympathetic system. In addition to these, a branch was represented as running directly outward towards the skin, above the external descending branch. Now, such being the condition of the nervous system, what might we *a priori* expect to find in the skeleton? Surely we might expect to find—1st, Parts related to the dorsal laminæ (epaxial); 2nd, Parts related to the external ventral laminæ (paraxial), and 3rd, Parts related to the internal ventral laminæ (hypaxial). To the first category would belong the neural arches, &c.; to the second, the transverse processes, ribs, and steruum; to the third would belong those skeletal structures, if such there are, within the pleuro-peritoneal cavity or medianly situated beneath the vertebral column.

But as to the nerves passing directly outwards above the external descending ones, are there any skeletal structures to answer to them?

Now, fishes present us sometimes with a double series of ribs, whereof the upper strike out towards the skin, while the lower tend to enclose the abdominal cavity. In tailed Batrachians we have two superimposed transverse processes to which a bifurcating Y-shaped rib articulates, and this rib sometimes bifurcates distally also. In mammals we have a rib essentially similar as to its proximal end, but one branch of the Y is diminished into a tubercle which, however, meets a transverse process. Can it be, then, that our own ribs are morphologically double, and that their upper proximal parts together with the fascia ascending from them to bound externally the *erector spinæ*, are homologous with the upper series of the ribs of fishes?

But what are the hypaxial structures, and first, what parts of the skeleton are within the pleuro-peritoneal cavity or are serially homologous with parts so situated? Here an important modification seems necessary in the views given out by Professor Huxley in 1869. He demonstrated unanswerably that the branchial arches are, as Professor Goodsir considered them, thoroughly homologous with the hyoidean and mandibular arches, and not only this, but he also suggested—what was as novel as important—that the *trabeculæ cranii* may be the foremost members of the same group of parts. He considered, however, that all these parts were *costal* in their nature. Now, accepting this view as far as regards the serial homology of the branchial arches with parts more anterior, it is nevertheless here submitted that the branchial arches should be considered parts *within* the pleuroperitoneal cavity, and this because the heart lies *outside* them, and the great vessels (which even in man have reflected on them a continuation of the pericardium) extend along their *outer* sides. It is contended, then, that these arches are hypaxial parts, and, if this is so, then the hyoidean, mandibular, palato-quadrate, and trabecular structures, as they are serially homologous with the branchial arches, must be hypaxial also. If so, the nerves which accompany them (the vagus, &c.) must be serially homologous with the sympathetic nerves of the trunk, and, indeed, this view was put forward by Professor Huxley in the lectures referred to. Are there, then, no true representatives of costal arches in this part of the frame? I think that the external branchial cartilages of sharks and the branchial basket of the Lamprey will be found to be such, and therefore to belong to a quite different category from that to which the branchial arches of osseous fishes pertain.

Again those azygos processes which descend from beneath the vertebral column in the region of the trunk, must be in the line of origin and suspension of the internal lamellæ of the ventral plates of the embryo, and being related to them may be deemed to be hypaxial parts also. Their serial homologues often bifurcate, and are repeated serially in the caudal region by processes or distinct ossicles (chevron bones) protecting the caudal vessels, and which I deem to be hypaxial also. Professor Goodsir has demonstrated that in the crocodile such parts, at the root of the tail, lie within the backward prolongation of the abdominal cavity, and the chevron bones or processes beyond that cavity in the same individual, are clearly the serial homologues of those within it.

According to this view then, the vertebrate axial skeleton in its most generalised expression consists of an antero-posteriorly extended axis, bearing above it (1) a cylinder of *epaxial parts*, for the protection of the cerebro-spinal centres. This cylinder expands anteriorly, and has intercalated three sets of sense capsules, olfactory, optic, and auditory. Everything, whatsoever it be, outside the anterior end of this cylinder (the cranial capsule) is morphologically *outside* the skull, and therefore in such an essentially *external* position is the sella turcica, the anterior communicating artery, &c.

2. From the axis of the skeleton diverge on each side more or less bifid *paraxial parts*, tending to protect or surround the visceral cavity, or homologous with parts which do so tend.

3. From the same axis descend *hypaxial parts*, which parts attain their maximum of size and importance towards the two ends of the skeleton. At the anterior end they by their varied degree of development and coalescence, build up the frame-work of the face, the jaws, and the hyoidean structures.

To this axial skeleton is added, in completely developed forms, two limb-girdles, each consisting of one upwardly and two inwardly and downwardly directed parts on each side. Two limbs, bilaterally symmetrical, are attached to each girdle, and a serial symmetry, bone answering for bone, exists between the anterior and posterior limbs of each side.

Can the skeleton structure of these limbs be expressed in yet simpler terms? Professor Gegenbaur has attempted very ingeniously so to express it, considering the limb bones as differentiations of primitive similar offshoots from a chain of marginal fin bones or cartilages. But much as one would naturally wish to accept so tempting a theory, two obstacles at present oppose themselves. One is the presence of a radial ossicle answering to the pisiforme of the ulnar side. The other is the occasional presence, in fossil forms, of at least one whole chain of such ossicles. So that at present we can hardly in this respect venture upon a more generalised view of the skeleton than the one here adopted.

This conception of the vertebrate skeleton takes little account of the mode of origin of skeletal parts—whether exogenous or autogenous, or of their segmented or unsegmented condition. But such considertaions have been neglected deliberately from a conviction of the completely subordinate importance of such conditions. The views here stated suggested themselves during the study of the skeleton as it exists in tailed batrachians; they have elsewhere been given at length, and their defence attempted, but it has been thought that a brief statement of them here might not be altogether unacceptable to some who are engaged in osteological inquiries.

ST. GEORGE MIVART

HOOKER'S BRITISH FLORA

The Student's Flora of the British Islands. By J. D. Hooker, C.B., M.D., F.R.S., Director of the Royal Gardens, Kew. (London: Macmillan and Co., 1870.)

NOTWITHSTANDING the number of British Floras already in existence, field-botanists have long lamented the want of a text-book combining all the requisites for out-of-door work, unquestionable accuracy, clearly-expressed definitions, a good arrangement, and a portable form. Although the hand-books we have hitherto used have possessed one or other of these features in an eminent degree, no one has yet succeeded in uniting them. For accomplishing this difficult task the best thanks of every British botanist are due to Dr. Hooker. The publication in quick succession of several works with a similar scope, may be taken as an indication of a reviving interest in British botany. Thirty years since, when the Linnean system of classification was still in use, a sufficient acquaintance with plants to enable anyone to give the Latin names of the species of their own districts was a fashionable acquirement, especially with ladies. The knowledge, however, was extremely superficial; it consisted mainly in counting the number of stamens and of pistils, so as to determine the class and order, and of observing the trivial specific characters of the foliage, colour and size of the flowers, &c., and was unaccompanied with the least real acquaintance with structural or physiological botany. An artificial classification like that of Linnæus, must always conduce to this result, and the ease with which plants can be named by such a method, is in itself an evil rather than an advantage. When we advance from an empirical to a natural system, in which the diagnoses of the orders depend on a variety of characters, some of them connected with minute details of structure, the gain, both to the learner and teacher, is immense. The learner is compelled to begin at the root of the matter, and to acquaint himself with the structure and physiological function of every separate organ, and with the different forms it may assume, before he attempts to name a plant; and the teacher can no longer cram his class with that showy surface knowledge which is the bane of popular science teaching. The general adoption of the Natural system of classification was followed by a great falling-off in the ranks of amateurs. The number of real students of botany is now however, we hope, increasing day by day, and the substantial interest and instruction derived from the science are in proportion enormously augmented.

The difficulties of the Natural system must be familiar to all teachers; probably every lecturer has more than once been perplexed by finding that the specimens which have been provided for illustration belong to a species differing in some point of structure from the characters which he has given as belonging to the order under consideration. Teachers will also differ as to the relative importance of certain points of structure, and consequently as to the position of some Natural orders. The uncertainty which still hangs over our classification will be illustrated by the following list of points in which the book before us differs from the fourth edition of Professor Babington's "Manual of British Botany," published as recently as 1856: *Droseraceæ* has been moved from Thalamifloræ to Calycifloræ, the genus *Parnassia* being incorporated with *Saxifrageæ; Acerineæ* undergoes the same change of position; *Balsamineæ* and *Oxalideæ* are abrogated as separate orders, their genera being united to *Geraniaceæ; Portulaceæ, Tamariscineæ*, and *Paronychieæ*, on the other hand, are transferred from Calycifloræ to Thalamifloræ, the tribe *Sperguleæ*, however, of the latter order being relegated to *Caryophylleæ; Grossulariaceæ* is no longer found as a separate order, but is united to *Saxifrageæ; Ilicineæ* or *Aquifoliaceæ* changes its quarters from Corollifloræ to Thalamifloræ; *Loranthaceæ* from Monopetalæ to Apetalæ; *Empetraceæ* from Apetalæ to Thalamifloræ; while the apetalous order of *Callitrichaceæ* disappears, its species being found under Calycifloræ united to *Halorageæ;* the hop, on the other hand, is eliminated from *Urticaceæ*, and appears as a separate order, *Cannabineæ;* and Babington's miscellaneous collection of *Amentiferæ* is divided into the four distinct orders, *Salicineæ, Cupuliferæ, Betulaceæ*, and *Myricaceæ; Trilliaceæ, Colchicaceæ*, and *Asparageæ* are combined with *Liliaceæ.* Although these alterations concern chiefly comparatively small and unimportant orders, there is sufficient change to perplex the student, independently of minor re-arrangements of genera, &c. On the whole it will be seen that the tendency is towards the English practice of "lumping," as contrasted with the Continental practice of "splitting," the number of orders of Flowering Plants being reduced from ninety-seven to ninety-two. We are glad, however, to find that this tendency is not carried to the extent we meet with it in Hooker and Bentham's "Genera Plantarum," where *Papaveraceæ* and *Fumariaceæ* are united; we doubt, indeed, whether the interests of students would not have been better served by keeping apart orders with such clear distinctions of outward structure, as far as British species are concerned, as *Saxifrageæ* and *Grossulariaceæ; Geraniaceæ, Oxalideæ*, and *Balsamineæ; Liliaceæ* and *Colchicaceæ.* The re-arrangements of position are no doubt, in nearly all cases, based on correct botanical principles. The difficulty, however, often experienced in drawing up satisfactory diagnoses of the natural orders may be illustrated by comparing those given by such authorities as Hooker and Oliver. In Hooker's "Student's British Flora," for instance, we find the pistil of *Nymphæaceæ* described as "syncarpous;" in Oliver's "Lessons on Elementary Botany," as "apocarpous." Hooker speaks of the stamens of *Oleineæ* as "epipetalous," Oliver as "hypogynous." In the synopsis of natural orders given at p. xiv. of the book under review, mention of the hypogynous stamens in some genera of *Oleineæ* is omitted.

In his analysis of certain difficult and intricate genera, Dr. Hooker has followed the lead of botanists who have made them their special study, as in the case of *Rubus, Rosa*, and *Hieracium*, where Mr. J. G. Baker's descriptions are adopted. This, no doubt, was a wise course in these instances; we regret, however, to find in the whole work so little of the author's own observations; we are sure that in many cases he could have improved greatly on the method of the "London Catalogue," which has been too implicitly followed. This is especially the case with regard to the plants admitted as "colonists" or "denizens." Why should a place be given, for instance, to *Galinsoga parviflora*, found nowhere, we believe, except within a radius of a few miles from Kew Gardens, from which it has escaped? while, on the ballast hills of the north-east coast and some other localities, many plants have apparently become permanently established, of which no mention is made, or their name is merely given in the Appendix. The time of flowering of plants is also one on which little exact observation appears to have been made; one would judge from our hand-books that the only wild flowers to be gathered in December and January are the groundsel and the daisy; while at least a dozen others could be named that are equally, if not more, perennial. We shall look with eagerness for a work embodying a record of recent physiological and morphological observations on British plants, which Dr. Hooker states, in his preface, it was his original intention to have incorporated with the present volume, and which will possess so great a value from his pen.

The specific descriptions in Dr. Hooker's "Student's Flora" are so admirable, terse, and yet sufficient, the arrangement so excellent, and the size so convenient, that it must rapidly become the work in general use, the companion of every botanist during his summer rambles.

ALFRED W. BENNETT

WATER ANALYSIS

Water Analysis: a Practical Treatise on the Examination of Potable Water. By J. Alfred Wanklyn, M.R.C.S., and E. T. Chapman. Second Edition, edited by E. T. Chapman, Member of the Council of the Chemical Society. Pp. 108. (London, 1870.)

IN the preface to this edition we are told that the whole of the last edition has been transferred almost without alteration, but with slightly different arrangement; and that some new matter has been added, consisting of the tetration of waters; a modification of the process for estimating nitrates; a chapter on volatile organic matter; a method of estimating minute traces of lead; and a chapter on the purification of waters. The tetration of waters is the estimation by standard solutions of the amount of acid present in waters contaminated by the refuse of certain factories or in rain water which has fallen near alkali works. We had thought that the word *tetration*, in the preface, was a misprint for *titration* until we found it so spelt in the text; the latter word (or modification), however, appears on p. 38. The modification of the process for the estimation of nitrates consists in treating the water to which caustic soda has been added with a large excess of aluminium scraps, and pouring off the liquid before distillation, instead of permitting all the aluminium to be dissolved, as in Mr. Chapman's original modification of Schulze's process. In the chapter on volatile organic matter, it is pointed out that on distillation of a water with potassic hydrate, there passes over, together with the ammonia, some combined nitrogen, probably in the form of organic bases, and a process is described for its estimation. The method of estimating minute traces of lead by comparison of the coloration produced by sulphuretted hydrogen water in dilute standard solutions of lead with that obtained by the same reagent in the water under examination is not new, for we remember having seen the experiment illustrated in Dr. Hofmann's lectures at least fifteen years ago. The previous removal of oxidising agents by sulphurous acid is, nevertheless, a useful addition. In the chapter on the purification of water, an ingenious experiment is described to show that separation of suspended matter by filtration through sand and similar substances is really due to subsidence within the interstices of the filter.

As in the first edition, we find no mode of estimating sulphides or the gases dissolved; nor does the treatise contain any indication of the success of the application of the ammonia process in the examination of sea water; it may be answered that the title of the book only refers to potable waters, but then we are at a loss to explain the appearance of some experiments on sewage.

It is much to be regretted that the present editor has thought fit to reprint the preface to the first edition, and also the appendix, which consists of nothing but an attack on Frankland and Armstrong's process. These entail on us the necessity of noticing this treatise at much greater length than the positive information contained in it would justify; for if the statements set forth were passed over without remark, it might seem as if the untrustworthiness of the process were acknowledged by all chemists, and doubt thrown on the accuracy and value of the reports issued by the Registrar General. The authors state in the preface to the first edition that it requires great length of time and great skill to execute it. This may be true, but the vast advantages which it possesses over the old processes amply compensate for the additional trouble. The manipulation, though delicate, is not, we are given to understand, beyond the powers of an average first year's student, at least when he is not influenced by a preconceived distrust in the efficiency of the process, or by a desire to do things with as little trouble as possible. It was not quite fair for the authors to state, two months after the publication of the process, that chemists in general agreed with them as to its invalidity. They should have remembered that "chemists in general" are not such rapid workers as Messrs. Wanklyn and Chapman, and would wish for more time to give a definite decision on the capabilities of a process so entirely new. The appendix consists of a note read by the authors before the Chemical Society. They commence by stating that they do not consider the complete conversion of organic nitrogen into ammonia by their method as being essential to its applicability for determining the relative quality of a water, and that they rely simply on the constancy of the ratio between the amount of albumenoid substance in the water and the quantity of ammonia produced. This is a retractation, though not a very straightforward one; for in their first published account of the process it was stated that all the nitrogen was evolved as ammonia. But it would be well to ascertain if this ratio is really constant, for although this may be the case with white of egg, is it not almost too much to assume that the nitrogenous organic matter present in natural water acts in the same manner as albumen, when we are quite ignorant of its proximate constituents? If the quantity of ammonia always bears a certain relation to the organic matter, and if this ratio is known, the determination of the nitrogenous impurity is merely a matter of calculation; but is it not a fact that some bodies, when treated according to the author's process, evolve more ammonia, in proportion to the amount of nitrogen they contain, than do others? The experiments on strychnine, narcotine, and quinine sulphate, published by Frankland and Armstrong (but not commented on by our authors), show this to be the case. But in their published papers, Messrs. Wanklyn and Chapman admit that the quantities of "albumenoid ammonia" from the following compounds are far from uniformly proportional to the amount they contain, thus:—

Urea and Picric acid gave no albumenoid ammonia

Creatine	gave	$\frac{1}{3}$	of its nitrogen as albumenoid ammonia
Caffeine	,,	$\frac{1}{4}$	,, ,,
Uric Acid	,, about	$\frac{1}{6}$	,, ,,
Albumen	,,	$\frac{2}{3}$	,, ,,
Morphine & 11 other organic substances		$\frac{1}{2}$	,, ,,
Hippuric acid and 7 other organic substances		all	,, ,,

Next follows a list of Frankland and Armstrong's results, in which the differences of the quantities of carbon and nitrogen obtained by experiment and calculations are pointed out. But it should be remembered that in these experiments the substances were weighed instead of being measured in standard solution, and that these solids were first dissolved and the solutions evaporated in order to perform the combustion of the residue. It is afterwards deduced from this same list that the error is inherent in the process, as the results are not better when the amount of organic matter is reduced; but with this reduction the liabilities of error in weighing increase. If this discrepancy were really caused by the imperfection of the process, we should expect to find great variation in the quantities of nitrogen obtained in actual analyses of waters, but in the list of forty nitrogen determinations given in Frankland and Armstrong's paper, the numbers vary from 0·000 to 0·068 per 100,000, or, as our authors would express it, from 0 to 0·68 milligrammes per litre, whereas they accuse the process as being liable to produce an error of no less than 1·29 milligrammes per litre. The following extraordinarily opportune accident has happened during the month of May, and from the results we shall be able to obtain an indication of the concordance of the numbers arrived at by Dr. Frankland's process. In the Registrar General's report it will be seen that the water supplied by the Grand Junction Company was drawn at the cabstand in Woodstock-street, whilst a sample of water was taken at 14, Lancaster Gate, under the impression that it came from the works of the West Middlesex Company. Now, to our certain knowledge, 14, Lancaster Gate is supplied by the Grand Junction Company; thus Dr. Frankland has been analysing two samples of the same water, supposing them to have come from different sources. This mistake could not have been found out till after the report was printed, and the results obtained by the two experiments are as follow :—

	Total solid impurity.	Organic carbon.	Organic nitrogen.	Ammonia.	Nitrogen as nitrates and nitrites.	Total combined nitrogen.	Previous sewage contamination.	Chlorine.	Total hardness.
Grand Junction, collected at the cab stand in Woodstock Street, May 17.	24·7	·135	·020	·000	·184	·204	1520	1·62	19·36
Water collected at 14, Lancaster Gate, supposed to be from the West Middlesex Company; but actually from the Grand Junction, May 15.	24·6	·129	·023	·000	·188	·211	1560	1·60	19·36

It may be objected that even these numbers do not approximate so closely as those of Messrs. Wanklyn and Chapman, but they represent actual quantities obtained from water, and not a theoretical "albumenoid ammonia," which may not be an indication of the quantity of organic impurities.

Since Frankland and Armstrong's paper was published an immense number of analyses have been made with the process, and in his annual report to the Registrar General, Dr. Frankland states that he has seen no reason to be dissatisfied with the results. Probably no one, unconnected with Dr. Frankland's laboratory, knows better than Mr. Chapman that improvements in the details of the manipulations have been made during the last two years; and it is, therefore, with very questionable taste that he has reprinted the appendix to the first edition of the book without a single word of qualification.

It is much to be deplored that two young chemists, with such undoubted abilities as Messrs. Wanklyn and Chapman possess, should have rendered themselves notorious by attacking older workers in scientific investigation. It is, no doubt, very laudable in a young and ardent investigator, when he points out that high authorities may err and frequently have erred, but the manner in which these gentlemen have carried out their corrections has made their matter more distasteful. It would almost seem as if they found an incentive to work in the hope of being able to overthrow the "huge superstructures" which have been raised by men who have been longer in the field of scientific research.

OUR BOOK-SHELF

A Sketch of a Philosophy. Part III. *The Chemistry of Natural Substances.* Illustrated by two folding plates and 150 figurate diagrams of molecules in the text. By John G. Macvicar, LL.D., D.D. (Williams and Norgate, 1870.)

IT is a hard matter to give a just account of this pamphlet. The views propounded by the author are so entirely different from those usually held by chemists, and according to the author's own statement they have been so little studied by others, that it is difficult to know exactly how to treat the subject. We should scarcely be justified in saying that the whole system is mere imagination, though some hold this opinion; but the book, though evidently written with the intensest earnestness, is the work of an enthusiast, which will explain the bitter complaints he makes against modern chemists for not taking more notice of molecular morphology. The author endeavours to explain the formation of all matter by the aggregation of the ethereal element, supposing that all bodies tend to assume a symmetrical, and more or less spherical form. The simplest form of aggregation Dr. Macvicar considers to be the *tetrad*, consisting of four specks of the material element so arranged in space that they form the angles of a tetrahedron, the lines joining them indicating the attracting and repelling forces operating between the units. Two tetrads are also assumed to join base to base producing the *bitetrad*, and from these two forms the tetrad and the bitetrad, all the atoms and molecules of our planet are supposed to be produced. This tetrad by attracting another unit opposite to one of its faces constitutes a group of five units, considered to be the atoms of hydrogen, and with the atomic weight of five. The author proceeds to show the mode of genesis of many other elements and compounds by the juxtaposition of these elemental forms. By calculation he can determine by his system the specific gravities of solids and liquids referred to water as unity, in a manner similar to that by which the densities of gases and vapours may be deduced by the old system. This alone would seem to show that the method deserves more attention from chemists than it has yet received. The non-reception of this molecular morphology may be ascribed to several causes: the diction of the author is peculiar, and he writes in a dogmatic manner, which might be expected in a theological work, but is not usually found in a treatise on natural science; then he pushes his inferences to such an extent (or as some would say, rides his hobby so hard) that his conclusions appear somewhat ludicrous, unsupported as they are by experiment: thus he traces the coincidence between the assumed hexagonal form of the molecule of aqueous vapour and the shape of the minimum snow-flake and ice-flower, and "the inflorescence of plants of the monocotyledonous order, in which an aqueous tissue predominates"; he thinks that one of the forms of aqueous vapour which occupies half the volume of the other, may possibly be converted into the second variety at a high temperature, and thus explain the explosion of steam boilers. Again the dimorphism of water may be the cause of the production of animal heat; for water in the body may be transformed from one of its varieties into the other with evolution of heat; but on escaping from the body as perspiration, the inverse action takes place and cold is produced. But underneath all these extravagances there may be a stratum of truth, and we hope that either the author or some one who understands and accepts his views thoroughly, will so develop them as to ensure their reception by chemists.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Source of Solar Energy

I HAVE not Mr. Proctor's "Other Worlds" by me to refer to, but my impression on reading that book some little time ago, certainly was, that if it did not directly support the *meteoric* theory of solar energy, it at least favoured the idea of innumerable meteors falling into the sun. The principal portion of my letter in last week's NATURE, was not, however, so much addressed against any special views of Mr. Proctor's relative to this *meteoric* theory, as it was against the probability of meteors falling on to or into the sun at all. And whatever be the real value of the arguments I used, I certainly had not before seen them anywhere clearly stated. The question as to meteors being consumed in producing heat by friction in the solar atmosphere, or of their striking the sun's proper surface in a solid form, is of very secondary importance, and would of course be determined chiefly by the size and nature of the meteoric bodies themselves.* The potential or heat result theoretically would be the same either way; though in the former case it might hardly so well explain how whilst heated flames are shot up 60,000 or 70,000 miles from the sun itself, that body is also probably far hotter than the upper regions of this very atmosphere, in which the meteors themselves would be giving out heat.

Prestwich, Manchester, Aug. 5 R. P. GREG

Kant's Transcendental Distinction between Affection and Function

AT page 80 of my "Revival of Philosophy at Cambridge," I ventured to describe a question set by Mr. Mahaffy, at Trinity College, Dublin, as "very oddly worded." I might have said *very improperly worded*, and have justified the sentence; but for the fact that it was the Cambridge examination-papers only that were the subject of my criticism in that work. My boldness in this censure on Mr. Mahaffy has occasioned remark. That gentleman is confessedly a capital metaphysician, perhaps of greater power than the learned professor whose work he translated and annotated. I therefore ask for space in NATURE to assign the reasons on which I asserted that his question was "very oddly worded." Here is the question: "Explain the statement that his [Kant's] doctrine of Space and Time is based on a *transcendental distinction*." I think I cannot be in error in taking this as a reference to the *Kritik der reinen Vernunft*, Transac. Æsth. § 8, *Allgemeine Anmerkungen*, &c., and in particular to the paragraph beginning "Die Leibnitz-Wolfische Philosophie," &c., and that which immediately follows. In the former, and indeed in its immediate precursor, Kant is impugning the view that affection and function (Sense and Intellect) have only a logical difference, as if Sense were only differenced from Understanding by the inferiority of its representations, in precision and clearness. The latter paragraph (the third of those I have referred to) beginning "Wir unterschieden sonst wohl unter Erscheinungen das," &c., may be thus rendered:—

"We otherwise draw a proper distinction, in phenomena, between that which is an essential property of the intuition of it, and is generally valid for every one's sense, on the one hand; and on the other, that which belongs to it accidentally, inasmuch as it is not valid for the faculty of general sensibility, but only for a particular state or organisation of this or that sense."

This, Kant names an *empirical distinction*. To it he opposes what he calls a *transcendental distinction*, viz., that between phenomena and things in themselves, or what is involved therein, that between affection and function. As Kant points out, if we do not make the distinction between affection and function, we cannot explain the transcendental constituents of a phenomenon, and thereby we take it for a thing in itself, *i.e.*, a reality existing independently of our perception of it; and we so lose the distinction between the phenomenon and the thing in itself, of which latter we know nothing whatever.

Now, Mr. Mahaffy's question concerns a certain statement. Whose? Well, probably, it is his own, viz., that which occurs in a footnote to p. 57 of his translation of Fischer's Commentary on Kant's C. p. R. Mr. Mahaffy here says, "We must not confuse the *empirical* distinction between real object and merely subjective appearance with the *transcendental* distinction upon which his (Kant's) doctrine of space and time is based." This I believe to be the topic referred to in the question. It is manifestly open to two objections. Each of the terms "real object," and "merely subjective appearance" is equivocal. "Real object" may be the phenomenal object or the noumenal object. "Merely subjective appearance" may be what Kant calls "blosz Erscheinung" or the impression which the object makes on the particular sense of this or that subject. The actual terms used by Mr. Mahaffy more properly import the distinction between the thing in itself and its phenomenon; yet it is plain he means to follow Kant, and to speak of the distinction between phenomenal object, and particular subjective impression. It is the other matter of the extract with which the question deals; and it is this which I have no hesitation in pronouncing a very improper and misleading statement.

Kant's doctrine of Space and Time is *not* based on this distinction. On the contrary, the distinction is an outcome of that doctrine, and does not, cannot, emerge till that doctrine is established. The fruit of the doctrine cannot be its root; nor can that which is the basis of the distinction be itself based on that distinction. The truth is, that Kant's doctrine of Space and Time is one that concerns Sense only; it is Æsthetic; and as discovering the *a priori* sense-elements of experience, it is called Transcendental Æsthetic. Accordingly it touches but one pole of the distinction, and only so far helps it out! How can such a doctrine be properly said to be based on a distinction between the two poles? The very notion is preposterous, and derives, of course, no countenance whatever from the *Critic of pure Reason*.

The extremely curt and concise manner in which I have dealt with the actual subjects of examination in my book was essential to its brevity and corresponding lowness of price. The above remarks will show how insecure will be any inference of the poverty of my reasons from the paucity of my words.

Ilford, E., Aug. 1 C. M. INGLEBY

Spontaneous Generation

IF there is one thing more curious than another in the "Spontaneous Generation" theory, it is the way in which so-called matters of fact, as proved by careful experiment, are brought forward by the one side to be disproved by the other; one need only instance Pasteur's famous flask experiments, which were thought to be so overwhelming at the time, but which were afterwards refuted, I think by Frémy and others.

I notice with surprise the letters of Prof. Wanklyn and Dr. Lionel Beale in NATURE, with regard to the presence of germs in the air; there is an experiment of Pasteur's, given in his "Memoirs upon the organised Corpuscles which exist in the Atmosphere, 1862," and which I have never seen disproved, and if not disproved it must surely settle at least this part of the question. He passed a quantity of air, taking various precautions to eliminate error, which I need not here detail, by means of an aspirator through a plug of gun-cotton; he then dissolved the gun-cotton in ether, and on examining the sediment which subsided in the course of an hour or two, he found abundant evidence of the presence of organised corpuscles.

Bath CHARLES EKIN

Mirage

I HAVE just returned to England from H.M.S. *Porcupine*, having accompanied the dredging expedition as far as Lisbon. In reading the back numbers of NATURE, I notice in that for July 28 an account of an extraordinary mirage in the Firth of Forth on July 22. A reference to my journal shows me that on the same day we were dredging on the Portuguese coast, within sight of the Ferilhoe and Berlinga Islands, about forty miles north of Lisbon. The bearings of these islands and their exact distance, calculated by the aid of the known height of the lighthouse, gave us, of course, an exact position, which our "dead reckoning" also confirmed. Several solar observations, both for latitude and longitude, were taken by two of the officers during the day, both of whom always arrived at the same result, but this was so widely different from our position as previously determined by two other methods, that we were forced to the conclusion that there was a very false horizon. It was the only instance of the kind during the month I was at sea.

Clifton, Aug. 8 WM. LANT CARPENTER

The Sun's Corona

IF "my mathematical result was based upon data among which the principal point at issue was accepted as proved," it will be easy for you to state what that point is,* and to quote one passage at least in which Mr. Lockyer has associated it with his theory. In this way alone can you justify the assertion in your last editorial note.

So much you are bound in common justice to do. But further, it would be satisfactory if a distinct statement of Mr. Lockyer's

* Suppose the case of a large mass of meteoric iron falling through the solar atmosphere, the first result would of course be a development of heat and light; but if the original or cosmical velocity of the meteoric body was lost through the resistance of the atmosphere (as in similar cases terrestrially happens) *before* the entire mass was consumed, there might possibly result an actual loss of solar energy, caused by the subsequent and necessary melting of the residuum.

* A possible action at the moon's limb suggested by Faye, Gould and others.—ED.

opinion respecting the corona could be made public. At present all that is generally known is, that he regards the corona as "an effect due to the passage of sunlight through our own atmosphere near the moon's place." Those are the words he used (see NATURE, vol. i., p. 14). I imagined that I had understood them. It seems I had not. Will he explain them, and perhaps indicate how the sunlight gets there? I only need to learn how one ray of sunlight can reach the atmosphere near the moon's place, during central totality in any considerable eclipse, and why the atmosphere actually *at* the moon's place (that is, all that cone of air which lies between the eye and the moon) is left free of this sunlight. This being satisfactorily explained, I should waive all other objections and accept the atmospheric glare theory without further question. RICHARD A. PROCTOR

[Mr. Proctor should have quoted the context, in which Mr. Lockyer carefully refers to Dr. Frankland's and his own conclusion (or theory) "against any such extensive atmosphere as the corona had been imagined to indicate." He then states that the "conviction" that the corona is probably an atmospheric effect, "is growing stronger and stronger," if the negative evidence of the new method of observation were alone taken into account; but this is not to elaborate a theory.—ED.]

The Horse-Chestnut

REFERRING to NATURE, No. 37, your correspondent, Eugene A. Connell, has fallen into a mare's nest in the matter of the horse-chestnut.

Country people are well aware of the impression of the horse's foot he has discovered, but the coincidence is quite accidental, and has nothing to do with the name.

"Horse" is a very common prefix, denoting largeness or coarseness, in the same way that the prefix "dog" indicates inferiority and contempt. Thus we have horse-chestnut, horse-bean, horse-radish, horse-mint, horse-parsley, horse-leech, dog-rose, dog-violet, dog-berry (the berry of *Solanum nigrum*), &c.

These prefixes are common to nearly all languages; we have ἱππό-κρημνος, a horse-laugh, "fièvre de cheval," a violent fever, and a host of like terms.

Bath, July 27 CHARLES EKIN

The "English Cyclopædia"

IF the Editor of the "English Cyclopædia," in his letter contained in your issue of July 7, had restricted himself to defending his own handiwork, and had abstained from denying the correctness of my statements, I should not have ventured to ask for space in your columns to reply to him.

In opposition to my statement that I looked in vain for "*Arvicolæ, Crociduræ, Crossopi, Hypudæi, Sorices*, shrews, and voles," the Editor asserts that "all the species mentioned in the Close Time Report are described or noticed in the Cyclopædia." This may be and probably is quite true. I merely asserted that they were not to be found under their respective names. I have stated that I found *Hypudæus* and the voles under the heading *Muridæ*. In return for my information he now tells me that if I wished to become acquainted with *Crocidura* and *Crossopus* I ought to have turned to the article *Sorecidæ*. But how is an unlearned reader like myself to know where to turn? The Editor only confirms the accuracy of my statement as to the great want of cross references. If the Index and the Supplement had contained such references as *Hypudæus* [Muridæ, E.C.], *Crossopus* [Sorecidæ, E.C.], &c., I should probably never have given public utterance to my troubles. In reply to my assertion that I looked in vain for an article on *Rhizocrinus*, I am told, much to my astonishment, that the proper place to find it is under *London Clay*. In my ignorance I had sought for it under its own name, *Apiocrinites*, and *Encrinites*. According to this mysterious system of arrangement, if I had complained that there was no article on *Sparrows*, I should probably have been told that I ought to have looked for a notice of them under the heading *London*.

In my letter I gave a list of twenty-three important subjects on which there were no articles. In defence of these real or apparent omissions the Editor, after making the strange assertion that two of these, *Acclimatisation* and *Deep Sea Dredging*, belong rather to the "Arts and Sciences" than to the "Natural History" division, goes on to say that most of the subjects stated by me to have been omitted "do occur." He must be well aware that I never asserted that they "do not occur." I simply said that there were no special articles on them. He might have had the candour to notice that I unearthed from their hidden recesses the subjects to which he expressly refers in his letter, viz., *Eophyton*, *Eozoon*, *Hyalonema*, and *Protoplasm*.

As I must not trespass further on your space, I will conclude by observing that I fully concur with the Editor in the opinion that "what a Cyclopædia ought or ought not to contain is an open question;" but when an Editor has the moral courage to assert, in illustration of the mode in which he discharged his functions, that "*Meloe* was inserted and *Sphegidæ* rejected, because there was not room for both," and gives no less than twelve columns to the former instead of dividing the space between the two; and when he tells us that *London Clay* is the proper place to seek for information regarding *Rhizocrinus*, the readers of NATURE may draw their own inferences as to what a Cyclopædia, under his superintendence, is likely to be.

I must add that I have not the slightest idea who the Editor of the Supplement is, and that until his letter appeared, I did not believe in his existence.

South Devon, July 8 NEMO

Entomological Inquiries, etc.

I WAS much interested, two nights ago, at finding on the wall of my drawing-room a flattish, dark-grey winged insect, six or seven tenths of an inch in length, which, on being placed in the hand, exhibited two small but brilliant sparks of light towards the extremity of the tail. In the imperfect light in which it was examined, the wings seemed to have elytra and the body to be somewhat like a small caterpillar, with a tapering tail. In size and general aspect it resembled the Italian fire-fly, with which I made acquaintance last summer on the Lake of Como, without, however, a sufficient examination to justify more than the most superficial comparison. My knowledge of entomology is so defective, that I feel unable to form an opinion whether it might be that insect or the male of the common glow-worm (which, however, is not common in my neighbourhood). If so meagre a description may enable any of your readers to give me satisfactory information as to this point, I shall feel much obliged to them.

There is adequate evidence that some kind of fire-fly has been seen during hot seasons in England. Kirby and Spence give a reference, which I have no opportunity of verifying, to Phil. Trans. 1684, as to their appearance in Hertfordshire, and their having been considered the genuine *Lampyris italica*. The following unpublished account may be interesting as having come to me from a perfectly reliable source: "In 1822—the year is pretty certain—during the month of June or July, the weather being very hot, on at least two evenings a number of fire-flies were seen at a village near the Thames, between Reading and Henley; they were flying about the fields and the lawn before a gentleman's house, and some of them came into the house; three or four or more might be seen at a time, like little flying lamps. The insect was brown [reference is then made to a sketch from memory thirty-two years afterwards, from which it must have greatly resembled my specimen], and seems to have had opaque elytra and network wings; the light was in the tail, like that of a glowworm, as bright, but probably not as large. A very intelligent gentleman who was upon the spot, an acquaintance of Dr. Wollaston's, who had been in America and the West Indies, was greatly astonished; he caught some of them, and considered them identical with the West Indian firefly. He said he had heard of their being in England, but never seen them."

A lady, whose experience must be referred to a later date than the foregoing account, has informed me that she once observed them for a single day in Wiltshire.

The newspapers of 1868 or 1869—I am not certain which—spoke of them as abundant in some places; particularly, I think, at Caversham in Kent, where they were even considered "nuisances!" if I recollect right. Some of the readers of NATURE may perhaps be able to furnish information as to this alleged fact.

There is something very remarkable in the occasional appearance of these beautiful insects in our climate. They can hardly be thought to reach us by direct migration. Can it be supposed —as it has been ingeniously suggested to me—that their ova are frequently being imported from warmer countries, but are only fully developed in the temperature of our hottest summers?

While speaking of entomological matters, allow me to mention that in the month of August last year the small blue lobelias in my garden were the favourite resort not only of my hive-bees, but of a species of wild bee so singularly resembling them in every respect (excepting, perhaps, a barely perceptible amount of greater firmness and roundness of form), that they could only be distinguished by the presence of a tuft of lemon-yellow hairs in the front of the head between the eyes. I thought at first that this might have resulted from a lodgment of pollen, but it soon became evident that it was a specific distinction. These pretty insects were very numerous, but I never found any nest. Will some apiarian reader oblige me with an identification?

To turn to a somewhat different subject. In a little book called "Flowers of the Year," published by the Religious Tract Society, is the following passage: "An interesting phenomenon is sometimes exhibited by red and orange-coloured flowers, and also, in a less degree, by yellow-tinted blossoms. It is that of a light of their own colour playing about the plant. This is not the result of an inflammable vapour igniting on the approach of a candle, but seems rather, as Sharon Turner has remarked, 'an actual secretion of light additional to their usual show.' The cause of this phenomenon has not been discovered, but it seems dependent on an electrical state of the atmosphere. It has not been seen during the bright sunshine, but has been observed after sunset in several flowers, as the marigold, the different species of poppy, the scarlet geranium, and even in the heartsease." I have several times met with a similar statement, and much wish to know what trustworthy foundation there may be for it. I have repeatedly tried to verify it by observation, but in vain.

Since penning the above remarks, the yellow-visored bee has again appeared on the blossoms of a Linaria Alpina, the descendant of a plant brought by my wife from Switzerland seven years ago, and now blooming profusely with us. I have seen several specimens of my old friend, but cannot as yet satisfy myself that the yellow tint is due to hair of that colour, as I supposed last year.

Hardwick Vicarage, July 12 T. W. WEBB

The Solar Spots

I HAVE been much interested of late in observing the solar spots, and especially in the greatly-increased numbers which have lately made their appearance. I have counted from 100 to 200 spots, through a six-feet telescope, with a power of 100, quite frequently in the past few months. On the 22nd ult., with a power of 200, I counted 675 sun spots, and on the 27th saw 470 with the 100 eyepiece. In general, I think the number of spots visible is about in proportion to the power used when the atmosphere is favourable for high powers. During the month several spots were visible to the naked eye.

I feel quite desirous to learn what our spectroscopists find about the sun's margin, also if observations indicate a terrestrial magnetic force corresponding with the sun's activity.

Would not many readers of NATURE be much interested with results from Huggins and Lockyer and the Kew observers?

Spiceland, Indiana, July 6 W. DAWSON

DARWIN BEFORE THE FRENCH ACADEMY

THE discussion on the claims of Mr. Darwin for election into the Zoological Section of the Paris Academy was continued at the meeting on August 1 in *comité secret*, and the *Revue des Cours Scientifiques* gives a report, of which the following is an abstract, of M. de Quatrefages' brilliant and able reply to M. Blanchard:—There are two men included in Mr. Darwin, a naturalist observer and a theoretical thinker: the naturalist is exact, sagacious, and patient; the thinker is original and penetrating, often just, sometimes too rash. That the theory with which his name is connected, that of Natural Selection, has in it at least something seductive and plausible, is shown by its having been worked out by such men as Darwin, Wallace, and Naudin, labouring independently and in different paths. If the ideas and the works of Darwin are such as some of his opponents represent, how can they have obtained the support in less than ten years of such men as Lyell, Hooker, Huxley, Karl Vogt, Lubbock, Haeckel, Filippi, and Brandt himself, who has just been elected correspondent in opposition to Mr. Darwin? In Darwin's great work there are certainly some things which are found in Lamarck, the laws of heredity, and the transmission and progressive development of characters. The point of departure of Lamarck is an incessant spontaneous generation, that of Darwin is a unique archetype which he supposes to pre-exist, and the origin of which he does not seek. That which belongs to Darwin alone is the laws of variation which he has established, and the law of correlation of growth. His error has been the confusion between the laws which regulate the foundation and propagation of races and of species; substitute the former for the latter and his theory is incontrovertible. Without defending Mr. Darwin's theories, some of which he has indeed publicly combated, M. de Quatrefages then proceeded to enumerate the various branches of scientific inquiry in which Mr. Darwin has made original observations, and concerning which he has contributed works of great importance to science. In geology we find seven great memoirs—1. On coral islands; 2. Geological observations on volcanic islands; 3. Geological observations in South America; 4. On the connection of the volcanic phenomena in South America; 5. On the distribution of erratic blocks in South America; 6. On the geology of the Falkland Islands; 7. Origin of the saliferous deposits of Patagonia. In botany the speaker invoked the testimony of Dr. Hooker that the most beautiful discoveries made during the last ten years in vegetable physiology belong to Mr. Darwin. Finally, in zoological literature we have the report of the voyage of the *Beagle*; and the monograph of the Cirrhipedes, one of the most important monographs ever published. After speaking of his more popular works on the origin of species and the variation of animals and plants under domestication, M. de Quatrefages referred to his important and laborious investigations of the strange variations in fowls, pigeons, and rabbits; and summed up his eloquent address as follows:—"En résumé, M. Darwin est un naturaliste éminent qui veut écarter de la science l'invocation de la cause première, et chercher l'explication des faits naturels du monde organisé dans les causes secondes, comme on le fait depuis longtemps en géologie, en chimie, en physique. Mais il ne va pas au delà, et il ne faudrait pas juger Darwin sur la parole de quelques disciples qui semblent parfois ne pas avoir lu ses ouvrages. Il y aurait injustice à le rendre responsable des exagérations et des aberrations de ceux qui s'abritent sous son nom."

M. de Quatrefages was followed by M. Ad. Brongniart, who attacked the Darwinian system, denying the existence of variation in plants. The appearance of species is a fact which can only be explained by a supernatural cause, and Darwinism is nothing but a fairy tale. M. Ch. Robin considered that in respect of proved facts which he had introduced into science, there would be a hundred zoologists who should have precedence over Darwin. M. H. Milne-Edwards replied to M. Brongniart, that the sea sometimes discloses fairy tales, and spoke of the very great value of the monograph of the Cirrhipedes. Although himself opposed to Darwinism, he strongly supported his nomination. M. de Quatrefages, in reply, denied the charge against Darwin made by M. Blanchard, that he had declared that man was descended from the apes. In deciding Mr. Darwin's claims, we ought not to be influenced by those points in which we have to combat his views, any more than Lamarck was judged in this manner. In spite of his errors, he will be none the less one of the glories of science and of the Academy. His nomination will not make the Academy Darwinian. Men of science know that the Institute appreciates work independently of doctrine, and men of the world know that the supporters of Darwin in the zoological section, MM. Milne-Edwards, and de Quatrefages, have always professed themselves opposed to his ideas. The discussion was then adjourned to last Monday evening.

TRANSMISSION OF POLARISED LIGHT THROUGH UNIAXAL CRYSTALS

THE appearances presented by the transmission of polarised light through crystals, have long been known as the most magnificent in Optics. It is our intention in this paper to give an account of the more recent observations which have been made respecting the phenomena exhibited by uniaxal crystals, accompanied by such an explanation as will, we hope, render them intelligible to persons very slightly acquainted with science. We shall therefore avoid as much as we can the use of technical terms, and assume as little as possible to be previously known to the reader.

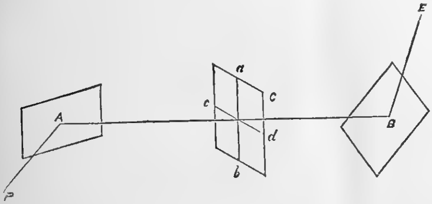


FIG. 1.

Common light consists of undulations of the supposed ætherial medium, in which the vibrations of each particle are perpendicular to the direction of the wave's motion, and in every conceivable direction which this condition admits.

Suppose now a ray of sunlight reflected from a plate of unsilvered glass. The principle of the composition and resolution of forces plainly enables us to regard it as made up of two sets of vibrations, one in the plane of reflection, and the other perpendicular to it. Now for reasons which cannot be explained to a reader unacquainted with mathematics,* it is found that the vibrations of the reflected ray in the plane of reflection, when the angle of

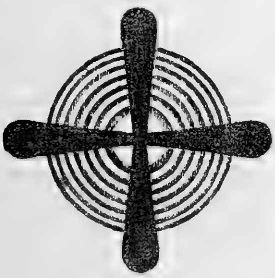


FIG. 2.

reflection is equal to 56° 30′, wholly disappear, so that the reflected ray consists entirely of vibrations perpendicular to the plane of reflection, and is said to be polarised. The reader will carefully remember the distinction between common light and polarised light. Common light is light in which the vibrations occur in every conceivable plane passing through the ray; polarised light is light in which the vibrations occur in only one plane of fixed inclination passing through the ray.

Now let a ray of sunlight fall upon a plate of unsilvered glass at an angle of about 56°, let the reflected ray be received by another glass plate at the same angle, and be thence reflected to the eye. Suppose the position of the mirror to be such that the planes of first and second incidence and reflection are perpendicular to each other. Then remembering what we have just stated, we see that the vibrations of the ray reflected from the first mirror lie altogether in the plane of second incidence. These vibrations, therefore, will all be destroyed by the second reflection, therefore nothing but a dark spot will be perceptible to the eye.

* The mathematical reader is referred to Green's "Memoir on the Reflection of Light," Cambridge Philosophical Transactions, vol. vii.

If now the position of the second mirror be shifted, so that the planes of first and second incidence coincide with each other, while the angle of second incidence remains unaltered, the vibrations of the incident ray will all lie perpendicular to the plane of second incidence, and therefore a bright spot will be visible to the eye.

FIG. 3.

Let now the second mirror be gradually turned round an axis situated in the direction of the ray incident upon it from the first position into the second, then the angle of second incidence will remain unaltered during the revolution, while the plane of second incidence will revolve with the mirror, and, as might be expected, light will begin to be visible to the eye and continually increase, till it attains its maximum in the second position of the mirror. It is usual to call the first mirror the polariser, the second the analyser.

Let a plate cut from a transparent crystal be interposed between the polariser and analyser thus arranged, luminous

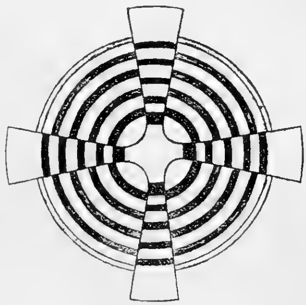


FIG. 4.

curves of varying colours intersected by dark bands will be visible to the eye. Before describing these gorgeous appearances, we must say a few words on the modification light undergoes in passing through a crystal.

When a ray of light passes through a crystal, it is in general divided into two, so that if a plate of crystal be interposed between the eye and a luminous point, the point will appear doubled. But in all crystals there is one, and in some two fixed directions in which no bifurcation of the ray takes place. If we look along one of these lines, called the optic axes, we shall see only one image of the object of vision. Crystals with one axis are called uniaxal crystals; those with two axes are called biaxal crystals, and in this paper I shall confine myself to the former.

We observe that a plane drawn through the direction of the ray or pencil and the axis of the crystal is called a principal plane. It is found that one of the two rays into which the incident ray is divided is refracted according to the ordinary law, and is therefore called the ordinary ray, the other according to a more complicated law, and is called the extraordinary ray. But we especially remark that the vibrations constituting the ordinary ray lie wholly perpendicular to the principal plane of the crystal, those of the extraordinary ray parallel to the principal plane. Consequently both rays will consist of polarised light, according to the definition we have just given of polarisation, and this polarised light will arise from the vibrations of the incident ray being resolved perpendicular and parallel to the principal plane.

Of all uniaxal crystals the best known is Iceland spar, which possesses the power of double refraction in an eminent degree. It occurs in the form of a rhombohedron, and the axis of the crystal corresponds with the shortest diagonal.

Now suppose a plate of this crystal, cut perpendicular to its axis, is interposed between the polarising and analysing plates, when the first and second planes of incidence are perpendicular to each other.

The colour of light depends on the length of the wave. The length of a wave of violet light is ·0000167, and that of a wave of red light ·0000266 of an inch. The wave-lengths of the other colours lie between these. White light is a compound of all colours, and therefore its vibrations are of all lengths lying between these extremes. It will therefore manifestly simplify the explanation we are going to submit to the reader, if we commence by assuming the light to be monochromatic, and its vibrations of the same length. Let a small pencil of monochromatic light, P (Fig. 1), be reflected from a plate of glass, A, at an angle of about 56°, pass through the crystal C, and then be reflected a second time from the plate B at an angle of about 56° in the direction B E. The plane P A B is supposed to be perpendicular to the plane of the paper, the plane A B E to coincide with it. The crystal is supposed to be bounded by parallel surfaces perpendicular to its axis, and in a position perpendicular to the line A B. Let *a b*, *c d*, be two straight lines drawn in the crystal, one in the plane of the paper and the other perpendicular to it. The eye is supposed to be placed at E.

The vibrations of light proceeding from A will all be parallel to the plane of the paper. In general, each ray as it enters the crystal will be resolved into two, and thus will arise two sets of waves which will interfere with each other. Hence a succession of dark and bright curves will be visible to an eye placed at E. Moreover, as the crystal is symmetrical round A B, it may be expected that these curves will be circular. It may also be expected that the diameters of the bright circles will depend on the length of the wave, and therefore on the colour of the light.

We have already defined the principal plane of the crystal for a given ray, to be the plane passing through the ray and through the axis of the crystal, which in this case coincides with A B. As therefore the vibrations of the rays incident on the crystal are all parallel to the plane of the paper, those which enter the crystal along *c d* are all perpendicular to a principal plane of the crystal, and those which enter it along *a b* are all parallel to a principal plane. Therefore every ray which enters the crystal along *a b* and *c d*, will be transmitted to the second plate unresolved, and unchanged, and will therefore be incapable of being reflected to an eye situate at E. Consequently a dark cross will appear to the eye at E, corresponding to the lines *a b*, *c d*, intersecting the system of rings we have just described. Now suppose white light to be substituted for monochromatic light. White light is composed of light of all colours, and we have seen that the diameters of the bright rings are different for each colour. Consequently, instead of dark and bright rings, we shall have a series of coloured rings intersected by a dark cross. This phenomenon is shown in Fig. 2.

If the second plate be turned round an axis in the direction A B, till the first and second planes of reflection coincide, the reader who has followed this investigation will perceive that the dark cross intersecting the rings will be changed into a white cross. This bright cross is shown in Fig. 3.

If the second plate is turned round the same axis into any position intermediate to those we have just described, the rings will be intersected by two crosses, inclined to each other at an angle equal to that between the planes of first and second reflection. There will be coloured arcs between the crosses, but those distances which give a maximum light for the arcs outside the crosses will give a minimum light for the arcs inside the crosses. This phenomenon is shown in Fig. 4. These are the appearances observed when the surfaces of the crystal plate are perpendicular to the axis of the crystal, and to the axis of the incident pencil. Dr. Ohm has investigated the phenomena which occur when the axis of the crystal is inclined at any angle to its surface, and the crystal itself is placed in any position between the polarising and analysing plates. His mathematical investigations will be found in the seventh volume of the "Munich Transactions," and the following are the principal results which he has obtained.

We shall suppose the light monochromatic. There will be a succession of dark and bright curves which are either parabolas, ellipses, or hyperbolas. For a given position of the crystal, the ellipses and hyperbolas are concentric; but the centres are never in the centre of vision, unless the axis of the crystal is either perpendicular or parallel to its surfaces. When the bright curves are parabolas, their vertices are equidistant from one another. When the bright curves are ellipses or hyperbolas, the difference of the squares of the semiaxes of two consecutive curves is a constant quantity. We have seen that when the axis of the crystal is perpendicular to its faces, the bright curves are circles. As we cut the plates so that the axis may be inclined at angles more and more acute to the surfaces, the bright circles become ellipses, which elongate continually till they become parabolas.

Suppose two plates of crystal, with parallel surfaces of equal thickness, and with their axes inclined at the same angle to the parallel surfaces, be placed in contact, so that their axes may lie in the same plane, but not in the same straight line, and then introduced between the polarising and analysing plates, a succession of dark and bright ellipses and hyperbolas, with their centres in the centre of vision, will be seen. It was this experiment which led Dr. Ohm to the results we have endeavoured to lay before the reader.

W. H. L. RUSSELL

NOTES

TWELVE months ago the Erdington Orphanage, founded, built, and endowed solely by Mr. Josiah Mason, at a cost of nearly 250,000*l.*, was opened at Birmingham. Mr. Mason, exactly following the example of Mr. Peabody, has now in contemplation another public work of even greater ultimate importance, namely, a college and schools for scientific and technical instruction, open to all classes, and if the hopes of the founder should be realised, capable of expansion into one of the noblest institutions in the kingdom. As yet the plan is only broadly formed, and some time must elapse before it can be carried into effect; but the *Birmingham Daily Post* states that a beginning has been made, and that for the purpose above mentioned, Mr. Mason has agreed to buy a large block of land in Edmund Street, exactly facing Ratcliff Place, between the Town Hall and the Institute, and reaching back to Great Charles Street. The purchase money, we believe, is more than 20,000*l.*, a magnificent earnest of the ultimate scheme which Mr. Mason has in contemplation. We make this announcement with the greatest pleasure, and ask—Why are citizens who thus consecrate their wealth to such noble and enlightened purposes so rare when their reward is so great?

THE following are the arrangements for the approaching fortieth annual meeting of the British Association for the Advancement of Science, to be held in Liverpool, commencing on Wednesday, Sept. 14. President-elect—Professor Huxley, LL.D., F.R.S. Vice-Presidents-elect—The Right Hon. the Earl of Derby; Right Hon. W. E. Gladstone, M.P., D.C.L.; Sir Philip G. Egerton, Bart., M.P., F.R.S.; Sir Joseph Whitworth, Bart., LL.D., F.R.S.; S. R. Graves, Esq., M.P.; J. P. Joule, Esq., LL.D., F.R.S.; Joseph Mayer, Esq., F.S.A. Chairman of Local Executive Committee—The Mayor of Liverpool (Joseph Hubback, Esq.) Local Treasurer—Henry Duckworth, Esq., F.G.S. The sections are—(A) Mathematics and Physics; (B) Chemical Science; (C) Geology; (D) Biology; (E) Geography and Ethnology; (F) Economic Science and Statistics; (G) Mechanical Science. These will meet each day from eleven to three o'clock, in St. George's Hall, the Town Hall, Free Public Library, and other places to be duly announced. General and Evening Meetings—Wednesday, Sept. 14: The first general meeting will be held in the Philharmonic Hall, at 8 P.M., when Professor Stokes, M.A., D.C.L., will resign the chair, and Professor Huxley, LL.D., F.R.S., will assume the Presidency and deliver an address. Thursday, Sept. 15: The Mayor's First Reception at the Town Hall. Friday, Sept. 16: Lecture in Philharmonic Hall, at 8.30 P.M., by Professor Tyndall, LL.D.; the Mayor's Second Reception at the Town Hall. Saturday, Sept. 17: Address to Working Men, in Concert Hall, Lord Nelson-street, at 8 P.M., by Sir John Lubbock, Bart., M.P., F.R.S. Monday, Sept. 19: Lecture in Philharmonic Hall, at 8.30 P.M., by Professor Rankine, LL.D., F.R.S. Tuesday, Sept. 20: Soirée in St. George's Hall, at 8 P.M. Wednesday, Sept. 21: Concert in St. George's Hall, at 8 P.M. Thursday, Sept. 22: Excursions to several places. Members requiring further information should apply to any of the following Hon. Local Secretaries—Wm. Banister, B.A.; Reg. Harrison, F.R.C.S.; H. H. Higgins, M.A.; A. Hume, D.C.L., LL.D., Municipal-buildings, Dale-street, Liverpool.

PROFESSOR WINLOCK is now engaged in photographing the sun on a plan which, so far as we know, has not before been put into practice. He uses a single lens object-glass, 4½ inches diameter, 40 feet focal length, of crown glass, made by Clark, and corrected for spherical aberration by means of an artificial star of homogeneous (sodium) light in the focus of a 5-inch collimator. The image of the sun is 4¼ inches in diameter. The tube of the telescope points to the North, and the image of the sun is thrown in by means of a reflector of plate glass. This glass is *not* roughened or blackened on one side, because when that was done the heat of the sun distorted the plane surface. The slit is at the object-glass end of the telescope, and that position has the advantage that when it is thrown across no dust is shaken down on to the plate, as is apt to happen in the usual way of working. It is Mr. Winlock's intention to photograph the sun every fair day now. It seems also probable that this mode of photographing might be of advantage for the partial phases of an eclipse.

MR. H. POWER, M.B. (Lond.), and Mr. B. J. Vernon have been appointed ophthalmic surgeons to St. Bartholomew's Hospital.

DR. LAPEYRERE insists in the *France médicale*, on the desirability of disposing of the bodies of the slain during the Franco-German war by incremation rather than sepulture. Although there is a feeling against this mode of obsequy during present times, he points out that it was practised by the most civilised nations of antiquity. The burial of the dead after a battle is always a difficult task; it is probably never done so completely as to destroy the probability of the tainted air giving rise to all kinds of infectious diseases; and when we recollect the enormous masses of men concentrated in a small space in the present conflict, and the season of the year, the matter becomes one of very serious moment.

THE fearful destructiveness of so-called "natural" causes of death, as compared with even the most sanguinary battles, is shown by the fact that during the siege of Sebastopol, the French army lost 20,240 men by death in the field or as the result of their wounds, 75,000 from epidemic and other diseases. During the Italian campaign of two months, the French losses were 3,664 killed or mortally wounded, 5,000 from disease.

THERE was a shock of earthquake on the 12th July at Smyrna. It was not very strong, but lasted a considerable time. The previous shock was on the 24th June, and was felt at different parts of Asia Minor and in Cyprus, Crete, and Egypt. Of the July shock there were only particulars at the last advices of its having been felt at Aivali and some other inland points of Asia Minor. We learn further that the earthquake of the 24th June was felt at Damascus at 6^h 15^m P.M. It was felt also in the town of Zebedani in the Anti-Lebanon, N.W.

THE Geological Survey of Italy will begin its regular definitive work next August. Commencing with Florence, it will first of all complete the study of a portion of that province. As the Ordnance map on the scale of 1 to 50,000 is not yet completed, the Geological Survey will make use of the map made by the Austrian Ordnance surveyors on the scale of 1 to 86,400, enlarged by photography to the scale of 1 to 50,000.

WHATEVER claims Sir Christopher Wren may possess to be considered the originator of the Thames Embankment, it is hardly fair to leave out of sight those which belong to Sir John Kiviet. The latter gentleman was a refugee from Rotterdam who came to England in 1666, and possessed some of the ingenuity of his brother-in-law Admiral Van Tromp. It does not appear how soon after the Fire of London it occurred to Sir John to propose a river embankment, but as early as 2nd December, 1666, we find him examining the soil of the foreshores with a view to discovering whether it was suitable for making clinker-bricks. On the 6th of March following Evelyn definitely proposed to the Lord Chancellor "Monsieur Kiviet's undertaking to warfe the whole river of Thames, or Key, from the Temple to the Tower, as far as the fire destroied, with brick, without piles, both lasting and ornamental." We may presume it was favourably received by Lord Clarendon, as upon the 22nd of the same month Evelyn had audience of the King with reference to building the Quay, and a few days later Sir John Kiviet and the Diarist "went in search for brick earth in order to a greate undertaking." No further mention is made of the scheme, and we may perhaps conclude that it was abandoned ei[illegible] on account of the unpopularity of the inventor (whose Dutch extraction would at that time have been a natural bar to success), or of the fall of Clarendon at the ignominious close of the war with Holland. At any rate, Kiviet has some right to divide the honours with Wren, though, in view of the work just completed, we cannot regret that its execution was reserved for our own times.

THE *Food Journal* for August, which contains some excellent articles, thus sums up the possible influence of War on our food supplies:—"*Free trade in live-stock* is by no means synonymous with *free trade in meat.* France will now get meat whence she cannot get live-stock. Her soldiers and sailors will eat the salt beef and pork of Cincinnati, Chicago, and St. Louis.

Prussia will probably suffer—she will certainly suffer if the war be prolonged—from her native animals being stricken by contagion; and England, that has hoped so long and so much from the resources of the East, will be left to seek for plenty in her own evergreen fields, and trust that some day her pre-eminence over the seas may render her independent of the transportation of a few hundred bullocks and sheep weekly from shores nearest her, by securing the produce—dead, not alive—of healthier, though more distant lands."

THE *Pharmaceutical Journal* (which now appears weekly) states that the plans for the erection of a Pharmaceutical Institute in connection with the University of Marburg have been approved, and the work was to have been commenced at once, but will now probably be delayed by reason of the war.

A COMMITTEE, appointed by the Royal Medical and Chirurgical Society to investigate the hypodermal method of administering injections, has just presented its report, on which the *Medical Times and Gazette* remarks:—"We may safely take, as a broad guide in practice, the rule that the physiological activity of nearly every substance which can thus be used, is three, if not four times greater when it is given by the skin than when it is swallowed." The proper commencing dose of strychnine is $\frac{1}{120}$ grain of the sulphate. The dose of atropine is also $\frac{1}{120}$ grain at first. The dose of morphia is $\frac{1}{10}$ grain to $\frac{1}{6}$ grain.

A TRANSLATION of Professor Huxley's Elementary Lessons in Physiology has just been brought out in Paris, edited by Dr. Dally, who introduces it as "*La Science sans Phrases.*"

THE following parliamentary papers have been recently issued:—Second Report of the Rivers Pollution Commission (discussing the A B C process of treating sewage); report of the Meteorological Committee of the Royal Society for the past year, an important document to which we shall return; and Volume VII. of the Irish Primary Education Commissioners' report.

SOME standard photographs and directions for measurement have been received by the Colonial Office from the Ethnological Society, through Prof. Huxley, the president. These are intended for the measurement and descriptions of aborigines in our Colonies.

THE Statistical Society has under consideration the means of promoting the study of political economy and of the numerical and statistical mode of investigation by granting prizes in our public schools. The first step will be to obtain the opinions of the head masters. The success of the Royal Geographical Society's examinations is an encouragement for this new effort.

OIL seeds having proved such a valuable branch of commerce in India, the Government is trying to promote the gum trade. The plants of the Australian gum tree, the *Eucalyptus*, introduced into the Punjaub, are taking well. It is hoped the gum will be of equal quality to the fossil *Damara Australis*, or New Zealand *Kauri pine.*

COAL containing 68 per cent. of carbon is reported to have been discovered near the town of Darjeeling, in the hill district of the Himalayas. Valuable copper and iron have also been discovered, so that important English settlement has the chance of other resources beyond tea and cinchona. The development of the minerals promises in time to give further employment for English settlers. At present in the hill regions the only metal working is in the iron mines of Kumaon. The hills can supply fuel for working charcoal iron. Our newly-annexed Bhootan territory is supposed to contain iron.

NOTWITHSTANDING the financial raid, the Lieutenant-Governor of Bengal has directed that nothing shall be done in the way of disbanding the establishment assigned for the survey of the Khasi, Naza, and Garrow hills, but, on the contrary, that the survey operations shall be taken up whenever the reduced strength of the staff will admit of it. The Government of India has confirmed this decision.

THE last scientific curiosity is a "Statement of a recently claimed discovery in Natural Science, incentive to mining enterprise," compiled by "Research," who endeavours to prove that geologists are entirely wrong in teaching that the changes in the ocean-line are due to continuous slow elevation or subsidence of the land. It appears that these changes are really due to shiftings of the mass of water in the ocean resulting from "a change of the centre of gravity of the earth's volume;" though how a knowledge that we are at any time liable to a sudden overflow of the waters of the ocean can be "an incentive to mining enterprise" is not clearly shown.

AMONG the novelties in the Exhibition at Paris in 1867 was the American Gatling gun, which, it was asserted, was to revolutionise the art of war. Like the mitrailleuse, the Gatling has as many locks as it has barrels. But whereas in the mitrailleuse the barrels are fixed and the locks have only a horizontal motion, in the Gatling both locks and barrels revolve. The locks have also, when the gun is at work, a reciprocating motion. The cartridges are kept in "feed-cases" side by side, and transferred to a "hopper," or shoot, when they are to be used. A handle is turned, and a process goes on analogous to that of the bullet-making machines, well known to visitors of the Royal Arsenal at Woolwich. When a ten-barrel Gatling is being fired, five of the cartridges are in process of loading and firing, five in process of extraction after discharging their bullets. The locks play backwards and forwards in the cavities where they work, and if one becomes disordered the action of the others is not stopped, provided that everything acts as it ought. Three sizes of Gatlings are generally made, the largest throwing a projectile of about half a pound weight. Case shot are made for the large-sized Gatling as well as solid bullets. It is, however, too heavy, and requires four horses. The chief defect of the Gatling, of all sizes, is that it cannot, like the mitrailleuse, fire volleys, but only single shots, always in the same line.

TWO experiments have recently been tried with a gunpowder invented by M. Pertuiset—the first by the Austrians against iron plates at Pola; the second recently in London. The former were made last September, in continuation of a series commenced some time before. The target was intended to represent the side of a first-class ironclad ship. There were two plates, one of 4·5 inches thick, the other 4·75 inches, English measure. The backing was 10 inches of hard wood, then an iron skin 1·5 inches thick, and behind it 12 inches of oak. The gun was of 8 inches calibre; the charge was 12 kilogrammes of common powder. The projectiles were of cast iron, made in Austria. They weighed 90 kilogrammes, and those intended to explode contained 1,500 grammes of Pertuiset powder. Two solid shot were first fired at the target, and only indented the face of it. The first of M. Pertuiset's shells not only broke the front plate and damaged the backing, but dislodged a mass of iron 22 inches by 15. The second round struck on a sound plate, and not only destroyed the iron, but so smashed the backing as to render the target unfit for further experiments. The opening in the front plates was this time far more considerable than after the first round. The experiments in London were made on horses. The skull was split, and on handling it large pieces of bone came away easily. The surface bones were removed, and the brain beneath was found to be utterly destroyed—a mass of gray and white matter devoid of consistency. When the loose material was lifted out there was a hole like the crater of a mine, 7 inches long by 6 broad. Part of the bullet had been driven up to the back of the head. And this work was done by a weapon that a man can carry in his pocket!

THE FLAGSTAFF OBSERVATORY AT MELBOURNE

ALL earnest workers in science must often have felt the extreme labour attaching to a complete reduction of an extended series of experiments. To derive some portion of truth from such a series may be a comparatively easy task, but to bring out the truth, the whole truth, and nothing but the truth, is a very difficult one. Yet how much more is this difficulty increased, when the experiments are made by Nature herself; so that while on the one hand the scale has become cosmical, we have, on the other hand, lost all control over the apparatus and the experimenter. Thus it happens that our progress in those sciences, which are strictly observational, is more slow than in those which are experimental, and truly we are yet in the infancy of Cosmical Physics.

In Dr. Neumayer, the late director of the Flagstaff Observatory, Melbourne, we have a most earnest observer of the highest type, and those who are aware of the difficulties with which he had to contend, must feel astonished at the very great and valuable work which he has successfully achieved. In the volumes of his results now before us, we have meteorological and magnetical observations of lasting value.* In meteorology we have a very complete set of elements extending throughout the years 1859 to 1863, and including, besides the usual phenomena, observations of the Zodiacal light, meteors, hailstones, atmospheric electricity, and the Aurora.

Not the least interesting of these are the observations of the Zodiacal light, and we may here note one or two remarks rather puzzling to those who assign an exclusively celestial origin to that phenomenon.

1859. Sept. 19.—The upper portion appeared to be much broader than the base.

Sept. 24.—It did not terminate in a point, but in a diffused edge of considerable breadth. The axis of the phenomenon was decidedly not in the ecliptic, but there was a particularly bright line about or in the ecliptic.

1860. March 20.—No regular column of light, but a broad patch of light towards W.; sky clear.

July 18.—A column of light much resembling the Zodiacal light was visible in S., 70° E. The column showed great change.

August 8.—The lower part of the phenomenon appeared of a rosy tint, which at times disappeared and returned.

1861. June 29.—A most extraordinary appearance of light in S.E. It descended in straight, well-defined lines. The light was white, and better defined than the Zodiacal light, and it appeared to be slightly bifurcated. Magnets perfectly quiet during the phenomenon.

The nautical observations form another important division of Dr. Neumayer's labours. These consist of the logs of various vessels which agreed to work in conjunction with the observatory, and from these logs results are derived of much practical benefit to the navigator, and of much interest to the ocean meteorologist. In them the usual method of dividing the ocean into five degree squares has been adopted.

A system of hourly observations in meteorology and terrestrial magnetism was carried on day and night, without interruption, for five years, after which a more simple system of observations was organised.

Another important task undertaken was the magnetic survey of the colony, in pursuing which Dr. Neumayer had frequently to be absent for three or four months at a time. The aggregate number of miles which he travelled over in the survey was 11,000, and the number of stations he examined, 230, situated at all elevations.

But this labour—great and Herculean as it is—represents only a portion of that which Dr. Neumayer has done, and merely denotes the work spent in obtaining raw materials. These have now to be tabulated, reduced, and discussed—a process not unlike that by which the sheaf of the reaper is made into good, wheaten bread. This reduction and discussion have been made by Dr. Neumayer in one of the volumes now before us, and, in obtaining his final results, the most approved scientific methods have been adopted. The magnetic observations are very completely discussed after the method of Sir E. Sabine, and many valuable results have been obtained. In particular, allusion may be made to a connection, traced by Dr. Neumayer, between the magnitude of the lunar-diurnal variation, and the moon's declination, forming a paper which has been published in the Transactions of the Royal Society.

These remarks would be imperfect without alluding to one point for which Dr. Neumayer deserves very great credit. While he has reduced his observations according to the most approved methods, he has, nevertheless, exhibited to his readers, as far as possible, the *actual observations* themselves, so that in future, if other methods of reduction should be followed and other objects sought, we can fall back upon actual facts, capable of being moulded anew into the form required. This is a point which ought surely to be borne in mind in all similar discussions.

B. STEWART

* "Discussion of the Meteorological and Magnetical Observations made at the Flagstaff Observatory, Melbourne."

"Results of the Magnetic Survey of the Colony of Victoria." (Trübner and Co.)

THE MANUFACTURE OF TAR PAVEMENT *

IN most provincial towns there are two important bodies of men, the paving commissioners and the gas directors. The one is pledged to keep the rates low, and the other to keep the price of gas as low as will enable them to provide the statutory dividend. As one means of ensuring a cheap supply of gas is to create a greater demand and obtain a better price for the residual products, it is of advantage to consider a subject the adoption of which would be advantageous to both of these bodies. It is not a new one, but has hitherto been a neglected source of revenue to gas companies, and will also be a great benefit to the public. That subject is tar pavement. In some counties, such as Yorkshire, where stone is as abundant as brain is said to be, tar pavement will receive but little attention; but in the eastern and some other counties where the same conditions do not exist, but where York flag costs 7*s*. per yard laid, tar pavement is a desideratum. In such districts there is a scramble for pavement; and, on account of the high price, unless a paving commissioner reside in the street, it remains unpaved.

Tar pavement may be made of the ordinary cinder-dirt produced in gas-works, of shingle, or of a mixture of both. The material is burnt in heaps like ballast, and when hot is mixed with hot tar. In practice a small fire of coke is made on the ground, and covered with cinder-dirt or shingle. When this layer is hot another is added, and so on in succession until a large enough heap has been provided. The tar is now boiled in an iron copper, and taken when hot and mixed with the hot material from the heap already described, in quantities of two bushels at a time, in about the proportion of one gallon to every bushel of cinder-dirt, and slightly less than a gallon for the gravel. It is turned over and over with the shovel until every part of the material has got a covering of tar. Then the whole is passed through a sieve with $\frac{5}{8}$-in. mesh, and part of it through another with $\frac{1}{4}$-in. mesh, and put in heaps until required. Indeed, it may be kept for months before being laid down.

Before the pavement is laid, an edging should be provided about 2 in. thick, and projecting 2 in. above the surface of the ground to be covered, which should

* Paper lately read before the British Association of Gas Managers, by Mr. T. H. Methven.

be tolerably even. It is advisable to have the ground next the curb well trodden on and rammed before the pavement is laid, otherwise there will be an unseemly hollow next the curb. In laying, the rough stuff is put down first and rolled tolerably firm, then the second quality is put on, then the third, and when the whole has been raked level, a little of the finest material is sifted on through a sieve with ⅛-in. meshes, and a little fine white shingle or Derbyshire spar is sprinkled on the top. The whole must now be well rolled. The best roller is a water ballast roller, which at first is used without ballast, and well wetted to prevent adhesion of the material, and, when the pavement is slightly consolidated, the full weight should be applied.

For heavy cart traffic the material should be made of shingle only, heated and mixed as above, and well rolled. Both descriptions of pavement are laid best and most easily in warm weather, and should be rolled when the sun has warmed it well. Those parts in angles should be well rammed and trimmed off with a light shovel.

Though apparently a simple manufacture, there is a little difficulty in ascertaining the proportion of tar to gravel or cinder-dirt. A little experience will only be necessary in this, as well as in all other manufactures, to enable anyone to carry it out successfully.

This pavement cannot be spoken of too highly, as it is cheap, wears well, and can be easily repaired. The colour, which never can be made to equal York flag, and the smell for some time after it is laid, are the only objections to its use; it can be laid with a good profit in any district at 1*s*. 4*d*. per square yard; and besides being a boon to the public, who must otherwise walk on gravel, is a great advantage to gas companies. To them it provides a remunerative outlet for their tar, which often otherwise must be sold at a low price to distant distillers.

A late paragraph, which appeared in the daily press, states that it is proposed to pave the streets of London with stone laid in asphalte instead of lime grout. This is just a more systematic application of the above-described plan; for the tar, by being boiled and thrown on hot stones, becomes an elastic asphalte.

INDIAN BARRACKS

RECENT discussions regarding the new Indian Barracks have shown the necessity for an adequate knowledge of physical science on the part of all engineers who have to cope with great natural forces.

These costly structures have been described as "sun traps," meaning thereby that the materials and details have been so selected, that instead of the interior of the rooms being as cool as, if not cooler, than the outside, it is cooler for the men to be in the open air under the sun. It is true that this objection has been raised at very few stations, but with adequate knowledge on the part of the architects, it ought never to have been raised at all. The barracks complained of are stated to be brick structures of the usual dark colour, with verandahs supported not on light easily heated and easily cooled columns, but on massive piers and arches like a bridge. The roofs have been constructed almost as they would have been at home, and no adequate means have been adopted for protecting the walls from sun-radiation. Of course the great mass of brickwork becomes heated during the day, and heats the air outside and inside the rooms at night, while the structure of the roof is such that the interior is heated both by direct conduction and radiation. Now surely such mistakes might have been avoided. There are such things as non-conducting materials to be had, and double walls with a ventilating air space between are not an uncommon expedient in this country for keeping out both heat and cold. Again, there is such a thing as white plaster or whitewash for outside walls, which reflects a large portion of the solar heat. Double roofs are not unknown, we believe, in India, and it is possible to construct such a roof as to interpose not only an air current between the outer and inner layer of the roof, but to prevent the radiation of the inner surface of the outer layer passing through the inner layer. These are very elementary applications of physical laws to Indian house construction, and how little they have been attended to may be inferred from a fact which has been stated—viz., that these barracks have been roofed with slate. The ordinary laws of conduction and radiation of heat appear to be at a discount in the Indian works department.

THE PROVINCE OF MINERAL CHEMISTRY

PROFESSOR KOLBE has recently succeeded to the directorship of the *Journal für Praktische Chemie*, rendered vacant by the decease of Erdmann, its original founder; and in his hands this periodical will doubtless become the recognised organ of the modern Leipsic School of Chemistry. Dr. Kolbe, in the first number o the new series of this work, has signalised his succession to the office of editor by an introductory essay, setting forth his opinions upon what he considers must be the future aim of the student of Inorganic Chemistry. Organic Chemistry, once the neglected sister of Inorganic Chemistry (to use the Professor's phraseology), has become so courted and honoured since Liebig introduced her as a young science into the chemical world, that little by little her relative has sunk into comparative obscurity. But the time has now arrived when, in Kolbe's opinion, it is evident that Inorganic Chemistry has not merited this neglect, but that she has it in her power to bestow rewards not less precious than those of Organic Chemistry, upon those who devote themselves to her service.

Much of the attractiveness of Organic Chemistry must, according to Kolbe, be traced to the zeal with which the origin of the almost numberless cases of Isomerism in organic compounds is being searched out; indeed, this zeal has nearly displaced the lively interest in Inorganic Chemistry created by the discovery of Isomorphism. This cause can never actuate the investigator in the domain of Inorganic Chemistry—at least not to the same extent—and for the reason adduced by Kolbe, that Isomerism cannot possibly occur in inorganic compounds to anything like the same degree as among organic substances, owing to the greater simplicity of constitution possessed by the former class. That so few cases of Isomerism have been discovered in Inorganic Chemistry is undoubtedly due to the fact that hitherto we have neglected to investigate mineral substances with special regard to their chemical constitution.

In order to prove the truth of this latter assertion Kolbe asks how can we frame anything like a reply to any question respecting the chemical constitution of the naturally-occurring silicates—felspar, for example? What are the proximate constituents of this compound, and what are their respective functions?

In half a dozen brief sentences Kolbe disposes of all our knowledge upon this subject, beginning with the views of Berzelius, by whom felspar—the typical silicate—was regarded as possessing a constitution analogous to that of dehydrated alum—that is, as a double salt of neutral silicate of alumina and silicate of potash; and ending with Gerhardt, who thought to explain the constitution of this and all bodies by his theory of types, or, in the expressive language of Professor Kolbe—"mit dei mechanischen Handhabung eines todten Schematismus."

The greater portion of the paper is devoted to Kolbe's theoretical views upon the nature of felspar, and he suggests a number of structural formulæ to explain its constitution according to our present opinions upon the quantivalence of the component elements. Whether, however, any of these formulæ represent the actual constitution of felspar, or if, indeed, its true nature can be represented by such formulæ, is a question which Kolbe leaves untouched, for the simple reason that the necessary experimental foundation from which alone valid arguments can be drawn, is entirely wanting.

Professor Kolbe concludes this remarkable paper by defining what should be the future aim of the student in Mineralogical Chemistry. He must not now rest content with a mere quantitative analysis, or with the empirical deduction of rational formulæ from the results of such analysis. Such a process can never fully elucidate the chemical constitution of inorganic compounds. This can only be accomplished by a careful and systematic study of the decomposition, syntheses, and substitutions; in other words, by the application of methods of research similar to those which have yielded such splendid results in Organic Chemistry. T. E. THORPE

NEW OBSERVATORY IN THE SOUTHERN HEMISPHERE

THE following statement with regard to the Cordova Observatory, to the foundation of which we have before referred, is extracted from the last number of Silliman's *American Journal of Science and Arts.*

"The Argentine Congress voted to establish a national observatory at Cordova, at the instance of President Sarmiento, and through the exertions of the present Minister of Public Instruction, Dr. Avelleneda, who invited me to organise and take charge of it, knowing my desire to extend the catalogue of the southern heavens beyond the limit of 30° to which the zones of Argelander extend. Bessel went through the region from 45° N. to 15° S. with systematic zone observations at Königsberg, which have since been reduced and published in two catalogues by Weisse of Cracow. Argelander carried the same systematic scrutiny with the Meridian Circle, from Bessel's Northern limit to the pole, and afterwards from Bessel's Southern limit to 30° S.

"Since then Gilliss has observed a series of zones for 30° around the south pole; but the reduction of these, although very far advanced, was not completed at the time of his death, and the MS. is now stored somewhere in Washington. Let us hope that it may at some time be recovered, the work completed and given to the world.

"My hope and aim is to begin a few degrees north of Argelander's southern limit, say at 26° or 27°, and to carry southward a system of zone observations to some declination beyond Gilliss's northern limit, thus rendering comparisons easy with both these other labours, and permitting the easy determination of the corrections needful for reducing positions of any one of the three series to corresponding ones for the other. It is of course impossible to arrange in advance the details of such an undertaking, but my expectation is to go over the region in question in zones 2° wide (except in the vicinity of the Milky Way where the width would be but one-half as great), up to a declination of about 55°, after which the width would be gradually increased as the declinations became greater. Within these zones all stars seen as bright as the 9th magnitude would be observed, so far as possible, moving the telescope in altitude when no bright star is in the field until some one becomes visible, according to the well known method of zone-observations.

"For reducing the observations, differential methods will probably be employed, inasmuch as the time now assigned for my absence from home would be inadequate for proper discussion of the correction required for nice determinations of an absolute character. Still, it is my present purpose, so far as possible, to make such subsidiary determinations as might hereafter be needed in any attempt at computing the observations absolutely. But as I hardly venture to anticipate any opportunity of making a thorough determination of the constants of refraction, or of the errors of graduation, it seems best to arrange for a differential computation at least at first.

"It is improbable that a sufficient number of well-determined stars will be found available even for this differential reduction, and the necessity may thus be entailed of determining the comparison-stars myself, this determination, however, itself depending upon standard star places. So far as possible I propose employing those heretofore determined by me, and published by the Coast Survey, which form the basis of the star places of the American Nautical Almanac.

"With these observations of position it is my hope to combine others of a physical character to some extent; but in the presence of a plan implying so much labour and effort, it would be unwise to rely upon the possibility of accomplishing much more than the zone-work.

"The meteorological relations of the place are very peculiar, but I dare not undertake any connected series of observations bearing upon these, without self-registering apparatus, which is beyond my means.

"Cordova is one of the oldest cities, and contains the oldest university, of the Western hemisphere. It is situated in $31\frac{1}{2}$° S. latitude, on the boundary of the Pampa, where the land begins to rise toward the group of mountains known as the Sierra de Cordova. It is connected with Rosario, on the Parana, by the Central Argentine Railway, which has probably been already opened to travel through its entire length of about 250 miles, although information to that effect has not yet reached this country.

"The two largest instruments will be a Repsold meridian-circle of 54 inches focal length and $4\frac{1}{2}$ inches aperture, and an equatorial by Alvan Clark and Sons, provided with the 11-inch object-glass, by Fitz, lately in the possession of W. Rutherfurd, who has supplied its place by one of 13 inches. A photometer by Ausfeld of Gotha, according to Zöllner's latest form, has been constructed under the supervision of Prof. Zöllner himself; a spectroscope will be furnished by Merz of Munich, and a clock by Tiede of Berlin.

"The Scientific institutions of the U.S. have afforded the expedition every possible assistance. The Coast Survey lends a circuit-breaking clock, a chronograph, and a portable transit; the Smithsonian Institution lends a zenith telescope; the American Academy of Arts and Sciences of Boston (probably) a photometer and spectroscope; the Washington Observatory and the Nautical Almanac have greatly aided the undertaking by gifts of books and by a manuscript copy of Gilliss's catalogue of Standard Stars; and from the astronomers of England, Germany, and Russia important assistance has been freely and effectively contributed, in the order and supervision of instruments and apparatus, and by the gift of books, as well as by important and valuable suggestions.

"Four assistants will accompany me, Messrs. Miles Rock, John M. Thome, Clarence L. Hathaway, and William M. Davis, jun. We hope to reach Buenos Ayres not later than the middle of August.

"The building is now under construction in Boston. The means available proved inadequate for its construction according to the original plan, which was in the form of a cross, with four square rooms about its centre, and turrets at its four extremities. One half of it will be first erected, and it is hoped that the remaining portion will speedily be added." B. A. GOULD

SCIENTIFIC SERIALS

THE greater part of the *Revue des Cours Scientifiques* for July 23, is occupied by the commencement of a very able paper read before the Anthropological Society of Paris by Prof. Broca, on the Transformation of Species. Commencing with the pre-Darwinian theories of transformation of Blainville and Lamarck, he then proceeds to give a *résumé* of the theory of Darwin, and the arguments in favour of or against the permanence of species, drawn from the observation of living species and from palæontology. Following this we have, *à propos* of the war, an article on field ambulances and hospitals, by Prof. Champouillon. In the number for July 30, we have the rectorial address of von Littrow to the University of Vienna, on the backward state of science among the ancients, and the conclusion of M. Broca's paper on the transformation of species, in which the subject is treated from a philosophical point of view, and the professor sums up strongly against the idea of permanence. The hypothesis of Natural Selection is then discussed, but a much less certain conclusion arrived at. The number for August 6 opens with a report of the discussion on the nomination of Mr. Darwin as corresponding member of the Academy of Sciences, to which we have referred in another column. This is followed by a singularly able and exhaustive review by M. Claparède, of Geneva, of Mr. A. R. Wallace's Essays on Natural Selection, in which he points out that while Mr. Wallace demands the intervention of a superior force to explain the foundation of the human races, and to guide man in the path of civilisation, he altogether denies the existence of such a force as assisting to produce the inferior races of animals and plants, which he attributes entirely to the operation of Natural Selection. In the same remarkably interesting number of the *Revue*, we have also Mr. Marey's extremely important paper on the Flight of Birds.

THE current number (No. XXXIX.) of the *Quarterly Journal of Microscopic Science* is an unusually rich one, containing several papers of great importance. Amongst them we may draw attention to one of considerable length by Dr. Beale, entitled "Bioplasm, and its degradation," with Observations on the Origin of Contagious Disease. He introduces and defends the use of the term Bioplasm, which we think is admissible enough, and serves very well as a distinguishing appellative for actually living matter, as opposed to protoplasm, which has been rather vaguely used to designate organic matter whether dead or alive. The application, however, of the new term is sufficiently wide, since Dr. Beale appears to consider them in a special form for each separate structure in the body originating in the primary mass of bioplasm of the egg. From each subdivision of the latter "in pre-ordained order, and with perfect regularity more are produced, no doubt according to laws, but laws which we know nothing about, except that they are not physical." This last assertion indeed seems open to question, for if we know *nothing* about them, how can it be said with certainty that they are not physical, as it is certain we are not acquainted with all the physical laws of the world. Dr. Beale proceeds to describe the Bioplasm of the Amœba, the principal forms of that of man, and its relations to such morbid products as pus and infectious poisons. An interesting paper follows, by Dr. Macdonald, of H.M.S. *Fisgard*, on the minute anatomy of some of the parts concerned in the function of accommodation to distance. Dr. Caton describes the means he has found best adapted for studying transparent vascular tissues in living animals. To this succeeds a capital *résumé* of Prof. Stricker's "Studien aus der Institut für experimentelle Pathologie in Wien aus dem Yahre 1869," and the part devoted to original communications terminates with two essays, one by Mr. E. Ray Lankester, on the Migration of Cells, and one by A. M. Edwards, on *Diatomaceæ*.

The *Transactions of the Linnean Society*, vol. xxvi., pt. 4, is entirely occupied by two papers on Fossil Cycads. The first, by Prof. W. C. Williamson, is descriptive of the remarkable *Zamia gigas* Lindl. and Hutton, or *Williamsonia gigas* Carr., found in considerable abundance in the Lias at Whitby. He believes it to have borne a strong resemblance to existing Cycads with diœcious flowers. In Mr. Carruthers's valuable monograph of Fossil Cycadean stems from the Secondary rocks of Britain, he shows that these fossils are, as far as is at present known, entirely confined to Secondary strata, the so-called *Cycadeæ* of the coal-measures and other palæozoic strata being rather referable to cryptogamic *Lepidodendra*, and the few specimens reported from Miocene beds being very imperfect and uncertain. In his description which follows, Mr. Carruthers describes four new genera from British rocks, *Yatesia*, *Fittonia*, *Williamsonia*, and *Bennettites*. Both papers are illustrated by excellent plates.—Vol. xxvii., part 2, is of more varied interest. Mr. John Miers contributes two botanical papers; a description of three new genera of *Verbenaceæ*, *Rhaphithamnus*, *Phelloderma*, and *Diostea*, from Chile and the adjacent regions; and a paper on the anomalous genera *Gœtzia* and *Espadea*, the position of which is very unsettled, and which he proposes forming into a new order. Dr. Birdwood describes and figures three new species of *Boswellia*, natives of the Soumali country, all of which yield frankincense, and one of them, he believes, the bulk of the olibanum of commerce. Dr. J. B. Hicks points out a singular resemblance between the genus *Draparnaldia*, and the confervoid filaments of mosses. We have descriptions of new Agarics and Lichens from Ceylon, the former by the Rev. M. J. Berkeley, the latter collected by Mr. Thwaites, and named by the Rev. W. A. Leighton, who also contributes notes on the Lichens of St. Helena, and a description of a new British Fungus, *Sphæria tartaricola*, Nyl. The longest paper in this part is an important monograph by Messrs. Henry Brady, Parker, and T. Rupert Jones, of the genus *Polymorphina*, an attempt to rescue this difficult genus of Foraminifera from the almost inextricable confusion into which it has fallen. Dr. A. Rattray contributes a paper on the anatomy, physiology, and distribution of the *Firolidæ*, forming the section of Heteropoda with a straight and elongated form, and either wholly naked or furnished with a very small shell, and including the genera *Carinaria*, *Carinaroides*, *Firola*, and *Firoloides*. Sir John Lubbock proceeds with his Notes on *Thysanura*, part iv.; and from Dr. Edward Moss we have an account of the genus *Appendicularia*, with its remarkable appendage, or "haus," the object of which in the vital economy has not been ascertained.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, July 4.—Mr. Alfred R. Wallace, president, in the chair. The Rev. F. A. Walker and Mr. Edward Mackenzie Seaton were elected members. Numerous objects of interest were exhibited by or on behalf of Mr. Meek, the Hon. T. De Grey, Mr. F. Moore, Mr. Blackmoor, Mr. Albert Müller, Mr. Jenner Weir, Sir J. C. Jervoise, Bart., Mr. Tegetmeier, and others.—Prof. Westwood made some observations on a group of very minute four-legged *Acari*, and the President mentioned instances of protective mimicry in insects, recently observed by Mr. Everitt in Borneo.—The following papers were read:—"Further observations on the Relation between the Colour and the Edibility of *Lepidoptera* and their Larvæ," by Mr. J. Jenner Weir; "On a Collection of butterflies, sent by Mr. Ansell from South-Western Africa," by Mr. A. G. Butler; "Contributions to the Insect Fauna of the Amazons" (continuation, *Coleoptera longicornia*, Fam. *Cerambycidæ*), by Mr. H. W. Bates; "List of the *Hymenoptera* captured by Mr. J. K. Lord in Egypt and Arabia, with Descriptions of New Species," by Mr. Francis Walker.

EDINBURGH

Scottish Meteorological Society, July 21.—Half-yearly general meeting, Mr. Milne Home in the chair. The following report from the Council was read:—"The Council have to report that the number of the Society's stations is now ninety-one, there being an additition of one since the last general meeting, in consequence of the services of an observer having been obtained for Leith. At the last half-yearly general meeting, reference was made to a renewed application by the Council to Government for pecuniary aid. The application was so far favourably received that the Board of Trade a second time recommended the Council to prefer to the committee of the Royal Society their claim, with a view to an allowance being made from the annual Parliamentary grant of 10,000*l*. for meteorological purposes, of which grant that committee have charge. The Council regret to say that the Royal Society committee have stated that they are unable to make or promise any allowance out of the grant, as the whole of it has been appropriated to other objects; for which objects the grant is, as the committee state, even too small in amount. The Council have in these circumstances been induced to renew their application to Government for a special grant to the Society. The Council have requested Mr. Buchan to prepare a report on the monthly temperature of the British islands, and to state to this meeting a few of the results obtained by him. The subject is one which it is believed has not been thoroughly investigated by any other society, or indeed by any meteorologist except Professor Dove; and Professor Dove's charts, which are now ten years old, were based on observations not only necessarily scanty, but in several cases unavoidably incorrect. The first chart which this Society prepared of the temperature of the British Isles was published in the Society's journal in January 1864, and it has been frequently referred to by meteorologists as the only one existing. That chart was constructed on returns obtained during a period of five years; the new charts which Mr. Buchan is now constructing are founded on observations for thirteen years, ending December 1869. These charts, besides giving the mean temperature of the year for the whole British Islands, give also the mean temperature of each month. The Council anticipate that, when these charts are published, with the tables explanatory of them, they will be found to afford valuable aid in the discussion of many important questions of a practical nature."—Mr. Mohn, Professor of Meteorology in the University of Christiania, and director of the Norwegian Meteorological Institute, who was at the meeting, presented a work entitled the "Storm Atlas," referring to various storms which had passed over the north of Europe. The Swedish returns were from twenty-five stations, where the observations were made three times a day, and were sent gratuitously by the Swedish Institute.

A paper "On the Temperature of the British Islands," by Mr. A. Buchan, was read; thirteen charts illustrating the temperature of the British Islands in each month of the year being exhibited. The author said that the investigation, the results of which were now exhibited on the walls, was one of the most important of all that the Society had undertaken. An early attempt was made to partially solve the problem about seven or eight years ago, and a chart was constructed showing the mean temperature of different parts of Britain in July and in January. These observations had two inherent defects. They were based only on five years, evidently too short a period to yield such results as were quite trustworthy. They were also defective in respect of number of stations, there being some parts of the country very badly represented. Now, however, an investigation had been completed for thirteen years, which must be a very close approximation, indeed, to the solution of the problem. The real temperature of the various months could differ very slightly from what was now exhibited. Further, the number of stations now brought under review was four times what the Society had at first. The observations were upon a mean of thirteen years of 68 Scotch stations, 54 English stations, and 11 Irish stations—Ireland being yet very badly represented. To enable him to draw the lines on the outskirts of the British Isles with greater accuracy than could otherwise have been attempted he had calculated the mean temperature for Faroe. Several Norwegian stations he had obtained from Professor Mohn's publications, and he had also ascertained the mean temperature of places in Belgium and France. The temperatures had been reduced to sea level by the ordinary method of allowing a degree for every 300 feet of elevation, and the lines were drawn upon the charts to show each difference of a degree of Fahrenheit. Among the results brought out, it appeared that in January there was as high a temperature in the north of Shetland as there was in London. As soon as the westerly winds crossed the high mountain ridge that was on the west of the British Isles, they deposited their vapour, radiation took place, and the temperature rapidly fell. In the same way, in Ireland, the lower temperatures were found inside, the higher temperatures outside. Another very marked result was the effect of Ireland upon Britain in increasing the summer temperature and lowering the winter temperature opposite to that island. In regard to the influence of the Irish Sea, Mr. Tennent, a member of Council of the Society, who had for a year or two given a good deal of attention to the direction of winds in different parts of the British Isles, is of opinion that the winds of the Irish Channel were not so much westerly as in Scotland and Ireland, but flowed to a great extent through the centre of the Channel. Now, on looking at the charts, it would be seen that the effect of this current in the winter months was to push up the isothermal lines over the Irish Sea. Through the whole of the months, the observations all showed the marked effect which that open space of water had upon the temperature of the British Islands, as clearly as the effect which Ireland had upon the part of Britain opposite. Questions of temperature had an important bearing upon agriculture. Many agriculturists believed that if the night temperature fell to 40°, there was no growth for twenty-four hours. If the night temperature fell to 40°, the mean temperature might be expected to be about 46°. Thus, then, by observing the charts for the various months, and taking note of those parts of Britain whose temperatures were less than 46°, one would ascertain the places where during certain months there was little growth, a very important question in discussing the crops of the British Islands. The next important temperature for agriculture was that required to ripen cereals. It had been proved by observations made by persons competing for prizes offered by the Marquis of Tweeddale, President of the Society, that for the purpose in question, with the ordinary range of temperature in Scotland, there must be an average of 56°. If it fell below that, there was a deficiency in the crop; if it rose, the crop was so much the better, provided there were rain and other necessary conditions. Accordingly, with the charts for the different months, one could point to those parts of the British Islands where there was some hazard in rearing cereal crops—the places, namely, where the necessary temperature was scarcely to be expected, or where it occurred so seldom that the risk was too great. It was generally supposed that the temperature fell one degree for every 300 feet of elevation, so that supposing at the sea level there was a temperature of 58°, at an elevation of 600 feet the temperature would be 56°, or a temperature sufficient to ripen cereals. In reference to this point, however, it was interesting to compare the station of Braemar on the Dee-side with that of Wanlockhead in the Leadhills, which were among the best equipped stations of the Society. It was well known by experience that on Deeside oats could ripen up to about 1,500 above the sea; but at Wanlockhead, which was only 1,300 feet above the sea, oats were sown only for the straw. Here, then, were very marked differences in the effects of temperature, as shown in the growing crops. Taking the month of June, he found that, adding a degree for every 300 feet of elevation, he got a mean temperature at Wanlockhead of 54·9°, while at Braemar, applying the same correction, he got a temperature of 56·8°. The cause of the difference between the two places he was not prepared to hazard an opinion upon, but it had an important bearing on the produce of the country. He thought that if this question were a little inquired into at some other stations, they might get some general law for guidance in reference to such matters.

Dublin

Royal Irish Academy, June 23.—The Rev. Professor Jellett, B.D., president, in the chair. Mr. Frith read a very interesting paper on arterial drainage in the west of Ireland. Mr. W. Andrews mentioned the second occurrence on the coast of Ireland of the rare Cetacean *Xiphis sowerbii*; it had been found in May last, in Brandon Bay, Co. Kerry. It was seventeen feet in length, the back of head and fins were of a velvety black colour with lines of white. Although sadly mutilated by the fishermen, yet several important parts had been obtained sufficient to enable him to supplement his (Mr. Andrews) provious paper on this very rare whale.—A paper was read by the secretary on the Book of Clonenagh.—The last part of the proceedings was laid on the table, and the receipt of a MS. index to the volume from the Rev. Dr. Reeves was acknowledged with many thanks and ordered to be printed.

Paris

Academy of Sciences, August 1.—A note was read by M. G. Rayet, on the spectrum of the solar atmosphere, in which the author noticed the variability of the bright lines, in confirmation of Mr. Lockyer's observations. M. Berthelot communicated some thermo-chemical investigations upon the sulphurets, in which he described the action of the alkaline sulphurets upon metallic salts in solution, the action of acids upon the alkaline sulphurets, that of sulphuretted hydrogen upon various metallic salts, and of acids upon the metallic sulphurets. A note by M. L. Henry, on the action of pentachloride and pentabromide of phosphorus upon various æthers, was read. M. F. Pisani presented an analysis of nadorite, a new mineral from the province of Constantine. This mineral was supposed to consist of one equivalent of oxide of antimony, and two equivalents of oxide of lead; the author gave as its formula $(Sb^2 O^3, Pb O) + Pb Cl$. A note by Mr. F. C. Calvert, on the evolution of pure nitrogen from nitrogenous organic matters was communicated by M. Chevreul. The author described the evolution of nitrogen from animal matters, by the action of hypochlorite of lime, and gave a tabular view of the quantities produced from gelatine, albumen, calcine, wool, and silk; these amounted in each case, to rather more than one-third of the whole quantity contained in the substance operated upon. A note was read by M. Contejeau, on the maximum of temperature at Poitiers, on the 24th July, 1870. The maximum observed was at $1^h 10^m$ P.M. when the thermometer suspended under the shadow of a tree showed

$41^{\circ} \cdot 2$ C. (= $106^{\circ} \cdot 16$ F.).—A note by M. V. Raulin on the rain-fall of the French Alps was communicated by M. Leverrier. It included tables of mean, annual, seasonal, and monthly rainfalls for sixteen stations.—M. C. Saint-Claire Deville communicated an extract from a letter of M. Chassin on a severe earthquake felt in Mexico on the 11th May of the present year. This earthquake destroyed the town of Pochutta, in the State of Oaxaca, in twelve minutes; it continued until the 19th.—A note was read by M. Marès on the corpuscular disease of the silkworm, and Marshal Vaillant contributed some extracts, showing the advantages which have been obtained in various places by the adoption of the processes of selection of silkworm's eggs recommended by M. Pasteur.—M. Decaisne presented a note by MM. Planchon and Lichtenstein on the specific identity of the *Phylloscera* of the leaves and roots of the vine.—A note by M. E. Roze on some mycological experiments, was communicated by M. Brongniart. The author confirmed the results obtained by M. Oersted as to the production of *Rœstelia penicillata* on the hawthorn by means of *Podisoma clavariæforme* from the juniper, and described some important experiments on the relation of *Claviceps purpurea* (Tul.) to the *ergot* of rye and other grasses.—A note by Mr. F. C. Calvert on the employment of phenic acid as a disinfectant, was presented by M. Chevreul. The author claimed the first application of phenic acid in this way for Dr. D. Davis, of Bristol, in 1867. The perpetual secretary states in a note that it was used on a large scale in Paris in 1865.

BERLIN

German Chemical Society, July 11.—Alexander Müller, having been engaged in analysing the waters of Berlin, stated the principles which he thinks necessary for procuring good analyses, as follows:—The bad property of water depends upon the organic matter contained in it. Water should therefore be evaporated *in vacuo* and submitted to dialysis, the colloid portion to be examined with the microscope. The organic matter should be determined by elementary analysis. Another portion should be evaporated with a weighed quantity of carbonate of soda, to separate silicic and phosphoric acids. The residue should be heated to redness; the loss corresponds to the nitric and nitrous acids and organic matter. The remaining weights, minus the carbonate of soda added, is that of the salts contained in the water, with the exception of phosphates and silicates.—Dr. Schwarz related his experiments instituted to obtain the homologues of isothionic acid with methylic, amylic, and butylic alcohols. Working with large quantities of sulphuric anhydride, he was unable to remark the two modifications which have been lately observed by Schultz Sellac. The boiling point of his anhydride (prepared by distilling Nordhausen acid with phosphoric anhydride) was 26° C.—M. Jaffé in describing an experiment concerning the constitution of rufigallic acid established the mode of the formation of colouring matters in plants. Gallic acid $C_7 H_6 O_5$, a derivation of benzol, when heated with sulphuric acid, yields rufigallic acid, until now considered as $C_7 H_4 O_4$. By treating this compound with zinc powder M. Jaffé has obtained anthracene $C_{14} H_8$. He therefore doubles the formula of rufigallic acid, and thinks it probable that the complicated vegetable colouring matters are derived from the tannic acids which are a constituent ingredient of most plants.—A. Oppenheim described experiments on the action of sulphuric acid on oxygenated organic chlorides. This he found to be analagous to the action of sulphuric acid on the corresponding chlorinated hydrocarbons. The chlorhydrine of glycol yields glycolsulphuric acid, just as chloride of ethyl yields ethylsulphuric acid. Epichlorhydrine, C_3H_5ClO, yields the mixed ether of glycerine, $C_3 H_5 Cl OHSO_4 H$, just as chloride of allyl yields a mixed ether of propylic glycol. Acid chlorides yield sulphuric acids in which the organic acid radical replaces one atom of hydrogen. These compounds are decomposed by water into the acids entering into their composition. Thus we have acetyl-sulphuric acid, phthalyl-sulphuric and benzoyl-sulphuric acid. The latter, however, is gradually transformed into sulphobenzoic acid. By heating, this molecular transposition takes place at once, and thus a good method exists for preparing this acid free from any secondary product. The same chemist, conjointly with M. Ador, established the identity of this acid with that discovered by Mitscherlich. It yields the same salts and is transformed into isophthalic and oxybenzoic acids by fusion with formiate and hydrate of potassium.—L. Carus communicated observations on the temperature necessary for his method of organic analysis in sealed tubes. P. Griess established the formula of benzo-creatine, $C_8 H_9 N_3 O_2$, obtained by the action of potash on the cyanide of amidobenzoic acid.—L. Henry reported on the action of PCl_5 on the ethers of diatomic monobasic acids.—MM. Merz and Mülhauser described the properties of naphthoilic or naphthylcarbonic acid—T. Thomsen described experiments on specific heat. In order to arrive at a standard quantity of heat he heated liquids by burning a certain volume of hydrogen. He arrived at the conclusion that mixtures of sulphuric acid and water have the specific heat of the water entering into the mixture. C. Rammelsberg communicated experiments on the specific weights of the different modifications of tin.

BOOKS RECEIVED

ENGLISH.—A Manual of Zoology, Vol. I.: H. A. Nicholson (Blackwood and Sons).—Heat a Mode of Motion; new edition: Prof. Tyndall (Longmans).—Irregularities and Diseases of the Teeth: H. Sewell (Churchill).—Notes on Electricity: Prof. Tyndall (Longmans).—The Laboratory Guide: A. H. Church (Van Voorst).—Murby's Scripture Manuals, Genesis: Murby. An Elementary Course of Plane Geometry: R. Wormell (Murby).—Cassell's. Book of Birds, Part ix.—Co-operative Agriculture: W. Pare (Longmans).—Henfrey's Elementary Course of Botany, edited by Dr. Masters (Van Voorst) — Mushroom Culture: its Extension and Improvement: W. Robinson (Warnes).—Notes about Aldeburgh: N. F. Hele (J. R. Smith).

FOREIGN.—Archiv für Anthropologie, Vol. IV. (Trübner). —(Through Williams and Norgate) Berichte über die biologisch-geographischen Untersuchungen in den Kaukasus-ländern; with Atlas.

CONTENTS

ERRATUM.—Page 269, second column, last line of table, for 89,395, read 34,413.

THURSDAY, AUGUST 18, 1870

MR. DARWIN AND THE FRENCH INSTITUTE

THE judgment of foreign nations gives the best clue to that of posterity; and it is therefore with peculiar interest that the countrymen of Mr. Darwin have watched the reception of his works in France and Germany. In the latter country his theory of the origin of species has been more or less completely accepted by those best qualified to judge, including men like Gegenbaur and Haeckel; and it has produced a complete literature of arguments and facts "für Darwin," without encountering any very serious opposition. In France, the truth of the theory is far less extensively admitted, and has been lately the subject of prolonged discussion in the Academy of Sciences. The debate on Mr. Darwin's claims has now been adjourned for three months, but so far as it was reported in our last number it furnishes much ground for reflection.

At the present time, Imperial France is, perhaps, the most conservative in science of any country in Europe. It is not, therefore, surprising that, with a few exceptions like M. Claparède, French naturalists refuse to accept the theory of Natural Selection, and do not see (as others, and notably the Germans do) that it has already made a new epoch in human knowledge. Some, like M. Robin, object that it is not "demonstrable," and therefore not scientific at all; as if gravitation or the atomic theory had been, or could ever be, demonstrated like a proposition of Euclid. The Darwinian theory offers an explanation of acknowledged facts by the help of others equally indisputable, and it will only be "disproved" when it ceases to furnish an adequate explanation, or is superseded by a more simple and equally sufficient hypothesis. Meanwhile it fulfils, at any rate, one object of every theory, by stimulating research in all directions, and awakening new interests for the fresh investigations which it suggests. The speeches of MM. Milne-Edwards and de Quatrefages are especially worthy of note; though opposed to the conclusions of "The Origin of Species," both these distinguished men discuss them with the intelligence and clearness of Frenchmen and the tolerance of philosophers: and both accord to the earlier works of Mr. Darwin the hearty praise which they deserve.

Two points come out very clearly in the course of this debate. First, that the wide extent of a scientific man's labours may hinder them from being properly appreciated. Geologists, who know well the value of Mr. Darwin's writings on the formation of coral islands, are ignorant of his admirable series of observations on the fertilisation of orchids; and many who are startled by the boldness of his speculations in zoology would feel more confidence in his judgment, were they acquainted with his work on barnacles—"ouvrage, selon M. Milne-Edwards, qui depuis sa publication suffit à tous les besoins de la science." The field of natural history is now so cut up, that a great dipterist may be almost ignorant of many orders of insects, and quite so of other animals. Few professed geologists have an adequate knowledge of physiology, botany, and geology, and thus we fear science often loses in breadth what is gained in minuteness. One cannot, of course, hope even to touch upon all the provinces which Aristotle "ruled as his demesne," and the extent of Hunter's or of Cuvier's range is probably too great for modern times; but, after all, the theory of natural selection would scarcely have been framed by one who had studied nature in a less comprehensive spirit than Mr. Darwin.

Another remarkable point, which was brought forward in the Academy as a reproach to the English naturalist, is that his researches are those of an amateur. M. Milne-Edwards met the accusation in the best possible way,—by admitting it: "Si l'on entend par amateur un homme qui aime passionnément la science, et qui s'y consacre tout entier sans en attendre aucune rétribution—oui, M. Darwin est un amateur, un grand amateur." But for us in England it is a serious consideration how far it is wise to rely so exclusively as we do upon a continued succession of such amateurs. The result upon the English character of our entire disregard of science as a pursuit of national importance is what might have been expected. When with us a man by his own independent energies comes to the front rank, he is pretty sure to make an epoch in science, be he never so much an amateur in French eyes. Such were Hunter and Faraday. Of such men we can boast at the present moment among the greatest living historians, philosophers, and political economists of Europe.

But, on the other hand, we have never had a continuous "school" in any branch of learning, and as a distinct result the mass of our scientific work is much below the standard of average excellence abroad. Nowhere, probably, are text-books so slovenly and imperfect as in England; nowhere are even good workers in the rank and file of science so narrow and unenlightened; and nowhere does the attainment of equal results so little affect the character of the nation. Whether we shall ever succeed in uniting the discipline and organisation of French science with the freedom and individuality of our own is hard to say, but unless something be done in this direction, we must be content every now and then to see a whole branch like Physiology become almost extinct in this country. The success which has attended the efforts made in Germany (chiefly within this century alone) to establish scientific research in a durable manner, and as a part of the national organisation, may at least encourage us to forward the attempt here. In the meantime, as we have no system of establishing and fostering schools, we must hope for men who cannot be repressed.

Another point introduced by M. Robin was, that considered in respect of demonstrable facts which Darwin had introduced into science, there would be a hundred zoologists who should have precedence over him. "Si des publications de M. Darwin, on élimine les vues dont ni la réalité ni la fausseté ne sont démontrables, et qui dès lors ne sont plus objets de science, il lui reste un ensemble de titres qui est inférieur à celui que représentent les données scientifiques bien démontrées introduites dans la science par M. Bischoff; il lui reste même moins de titres à nos souffrages qu' à quelques-uns des savants qui sont placés *ex æquo* avec lui sur notre liste de présentation. Ce sont là les raisons qui m'ont conduit à ne pas porter M. Darwin au premier rang, et il m' a paru qu'elles ont influé sur le vote des autres membres de la majorité de la section."

THE ICE AGE IN SWITZERLAND*

IF one might venture to use the word "romantic" in reference to the history of any past geological period, it would certainly with most fitness be applied to that time, so recent and yet so remote, which we know as the Ice Age. The story of the old glaciers, like that of the living ones, has a perennial interest. We listen to it over and over again without wearying, much as we used to do with some of the standard tales of childhood. For even though we are now familiar with the evidence which proves that, at no very distant date, the northern parts of Europe and America, including nearly, if not the whole, of our own country, lay buried under a vast sheet of ice, the fact is so strange that every fresh presentation of it comes even yet before us with not a little of the charm of novelty. Hence every description of new facts which tends to elucidate the history of the Ice age in any one locality possesses a more than local interest and importance. More particularly is this the case when the description relates to Switzerland. The Swiss glaciers of to-day have become in a manner the common property and fighting-ground of the geologists of all countries; and all fresh observations which bear on the ancient extension of these glaciers are welcomed as additions to the common fund of geological knowledge.

But there is a peculiar interest attaching to the publication the title of which is given below. It is not only a most meritorious contribution to geological literature, showing in great detail the history of glacial phenomena in one of the Swiss cantons, but it may be taken by all local scientific societies as a model of what enthusiasm and industry, well directed, may accomplish. Its origin and growth may be briefly stated. In December of 1867 the Natural History Society of Aargau determined that the year 1869, being the fiftieth of its existence, should be marked by some fitting celebration. It was suggested that, besides the usual festivities, it would be well if the society would engage in some piece of scientific work, so as to present the result at the anniversary; and it was finally resolved to make an extended and detailed survey of the erratic formations of the canton. General attention had been roused to this subject by the Swiss Naturalists' Society in 1867. That body had issued a circular inviting all authorities and private individuals throughout Switzerland to preserve the erratic blocks which were rapidly disappearing before hammer and gunpowder. MM. Favre and Soret, of Geneva, had likewise pointed out the desirability of having an accurate map made of the distribution of erratics in the country. Accordingly communications were entered into with the federal and cantonal authorities, who cheerfully lent their assistance. A circular was sent out inviting all and sundry to give their aid towards the ascertaining and mapping of the erratic blocks in the canton Aargau. In that document were likewise given directions as to the nature of the observations to be made. Each of the fellow-workers was requested to note in a schedule the erratic blocks (or *foundlings*, as they are happily termed in German) of his neighbourhood, with an exact record of the locality of each block, its local name, height above the sea, proprietor, the nature of the rock of which it consists, the legends, if any, connected with it, such human marks as might have been carved on it, and any other remarks that might seem important. To conduct the whole inquiry a reporter or secretary was appointed. His duties were to receive and reduce into connection and order the reports of the various volunteer surveyors; and that he might the better perform this task, he was authorised at the public cost to visit the more remarkable localities, to mark those blocks which he considered it desirable to preserve in the interests of science, and to preserve two type specimens of all the more important blocks, the one specimen to be deposited in the Aargau Museum, the other to be sent to M. Favre at Geneva. The blocks lying on State property were to be considered thenceforth inviolable, and for the conservation of those resting on other lands the secretary was authorised to treat with the proprietors with a view to the acquisition of the blocks by the State. By thus taking advantage of the general movement the Aargau Society not only aided in a work of national interest, but gained an opportunity of celebrating in a singularly excellent way its own anniversary.

The result of the undertaking is now before us in the form of an octavo volume of upwards of 200 pages, with a map showing the distribution of erratics and the extension of the old glaciers over the canton. A more admirable piece of work has not been done in glacial geology for some time. The society deserves the warmest thanks, and more especially the reporter, Herr Mühlberg, who first suggested the task, and on whom the main share of the labour has fallen. The course of each of the ancient glaciers is traced by the lines of "foundlings" which it has left in its path. The evidence furnished by the Canton Aargau as to the retreat of the ice is carefully collected. The limits of the second glaciers are mapped out, and abundant details are furnished as to their moraines. It has long been familiar knowledge that the great glacier of the Rhone, after abutting against the Jura mountains, pushed an immense body of ice eastwards over the lowlands of Switzerland as far as the district round Soleure. But we now learn that rocky *débris* borne along by the huge glacier has been traced even up to the Rhine at the confluence of the Aar. Swelled by the numerous streams of ice which came down from the Bernese Oberland, the united glacier of the Rhine and Rhone must have poured an enormous mass of ice down the Rhine valley, between the Black Forest and the Jura.

The sequence of events in Switzerland since the close of the Tertiary period is thus epitomised by Herr Mühlberg:—

1. Close of the Tertiary period, elevation of the Alps, &c.
2. Erosion of the valleys in the area of the molasse.
3. Renewed elevation of the Jura, formation of the barriers at Aarburg, Wildegg, &c., and consequent lake-basins.
4. First and greatest extension of the glaciers even to the highest crests of the Jura and over the whole canton. Northern limit of the ice unknown; accumulation of bottom moraines (Grundmoräne, *moraines profondes*) upon the "Stoss-seite" of the hills.
5. Retreat of the glaciers, with deposit of earthy rubbish, angular blocks, scratched stones, sand and gravel; filling up of the lakes behind the above-named barriers (save

* Ueber die erratischen Bildungen im Aargau, &c. Ein Beitrag zur Kenntniss der Eiszeit, von F. Mühlberg. (Aarau: H. R. Sauerländer.)

the still-existing lakes over which the retreat of the ice, if it took place at all, was rapid); erosion of the river terraces.

6. New advance of the glaciers to a line drawn from Dagmersellen to Baden; overlaying of the older glacial deposits with fresh erratic materials; formation of large moraines; origin of the small cross-valleys in the molasse hills outside of the ice-covered region.

7. Periodic retreat of the glaciers (with perhaps an occasional renewed advance) until the ice had finally retired from the canton; newer deposits of moraines, &c., and formation of lakes and marshes where the moraines were laid down across valleys.

8. Changes which are still going on.

There is nothing, indeed, which is novel in this synopsis, but it is satisfactory to find that it is borne out by so large an array of evidence as the author has here brought together. On one or two points we could have wished for information. There is no record given of the direction of the striæ on the rocks. No doubt the molasse is not well adapted for the preservation of such markings, though they would remain even on that rock when protected by superficial clayey deposits. We can hardly doubt that if looked for, striated rock surfaces would be found in Aargau as good as those in the adjoining canton of Solothurn. Herr Mühlberg says nothing of any warmer interglacial epoch, as indicated by lignites lying between the deposits of the first and second glacier periods. Is it that no such evidence exists in Aargau, or that the attention of the observers was not specially directed to this subject?

We cordially commend this little volume to the notice of local scientific societies in this country. Such societies often fail of success, either because on the one hand they are too ambitious and seek to emulate the greater societies in such a manner as to ensure certain failure; or, because, though they possess the will and the ability to work, they lack the strength and the enthusiasm which spring out of well-directed and hearty cooperation. Let them take a lesson from the way in which the Aargau Natural History Society has celebrated its fiftieth anniversary. ARCH. GEIKIE

PRIMITIVE MAN

Primitive Man. By Louis Figuier. Revised Translation. 8vo. (London: Chapman and Hall, 1870.)

M. FIGUIER has been singularly fortunate in the mode in which his books have been received by the French public. "Le Monde avant le Deluge" had a large circulation, and even in England the translation revised by Mr. Bristow met with considerable favour. It is almost needless to remark that it reproduces the wild catastrophic doctrines that were given up in England some thirty years ago, with a grace and elegance which lead the *dilettante* reader to believe that he is learning at the feet of a modern Gamaliel. "L'Homme Primitif" takes up the narrative where it was dropped by the preceding work, and tells the story of the early races of men that have lived in Europe, with that vivacity and idealism which is only to be found in perfection among the people for whom it was written. It is undoubtedly true that M. Figuier's works give a faithful outline of the present state of science in France; but it is none the less true that in England catastrophism is practically extinct, and that many of the inferences of French Archæologists are received by English *savants* with a considerable amount of reserve. We, therefore, hold that Science is not really advanced by these books being put before the non-scientific English reader. Of the two books the latter is by far the best, but it is disfigured by many and grave mistakes. There can be no doubt that it will become almost as popular in England as in France, although the elegance of style cannot be preserved in plain straightforward English.

"Primitive Man," the English editor tells us, is intended to "fill an open place in the literature of Prehistoric Archæology," although it covers precisely the same ground as Sir John Lubbock's "Prehistoric Times," and rivals that work in size. If this means that "the Raffaelesque idealism" of the illustrations is intended to find its way to the hearts of a British public that is given to sensational literature, we think that the editor is right. The frontispiece, representing a family of the Stone age, is admirably adapted to arrest the attention of the mother of a family; and the feasting during the Stone and Bronze ages is fitted to strike gourmands, the pictures of the chase, sportsmen; while the cultivation of gardens during the Bronze epoch is a touching scene of rural happiness. A man is hoeing the ground, while a woman is sowing seed, and hard by stands Phyllis with a basket on her head, and leading a favourite goat that is reaching forward to eat a tempting vegetable (also of the Bronze age), while Corydon up an apple tree is shaking its riches into Galatea's lap, and behind are the sheep tended by a shepherdess, and kine and long-snouted pigs are looking on from the palings. All this is extremely pretty, and will doubtless attract many readers. We doubt, however, whether an appetite for archæological knowledge created by such stimulants would be satisfied with the sober logic of the English standard works on the subject. In the letterpress we have failed to detect any fact of high importance which is not to be found in "Prehistoric Times," or in "The Antiquity of Man." The English edition of "Primitive Man" has been corrected and altered from the French original, and therefore may be treated as a purely English work. In his preface the editor is so anxious to give reasons for translating the book, that he seems conscious that he holds a bad brief. We shall proceed to point out some of the more glaring mistakes.

M. Figuier gives an account of the celebrated controversy about the Moulin-Quignon jaw, in which he states that Dr. Falconer, Dr. Carpenter, Professor Busk, and Mr. Christy "unanimously agreed in recognising the correctness of the conclusions arrived at by the indefatigable geologist of Abbeville," viz., that the flint *hâches* and the human jaw were of the same Quaternary age as the extinct mammalia found in the same gravel pit. So far from this being the case, Dr. Falconer stated in the *procès-verbaux* that "the character which the jaw presents, taken in connection with the conditions under which it lay, are not consistent with the said jaw being of any very great antiquity," while Prof. Busk wrote that "the internal condition of the bone is wholly irreconcileable with an antiquity equal to that assigned to the deposits in which it was found." Mr. Evans also afterwards came to the conclusion that the suspected flint *hâches* are modern fabrications, and that an ingenious and successful fraud had been practised by the workmen. The editor, however, allows M. Figuier's version to stand, feebly qualified by a note in small print, that "it should rather have been said that the ultimate and well-considered judgment of the English geologists was against the authenticity of the Moulin-Quignon jaw." Why, then, let M. Figuier's error be allowed to mislead the English public? The high antiquity of man is amply proved by the genuine flint *hâches*, without involving the apocryphal Moulin-Quignon jaw, which throws a certain degree of doubt on the whole question.

In 1823 Dr. Buckland advocated the theory of a great diluvial flood which swept over the earth, destroyed the extinct mammalia, and conveyed large blocks of stone far from their parent rock. This he shortly after withdrew. From that time down to the present no attempt has been made in England in any scientific work to account for facts which the author could not understand, by the hypothesis of a deluge. The glaciers and ice-bergs are supposed to have transported the blocks of stone, and the presence of the remains of men and animals in caves is accounted for either by the fact of their having lived and died in them, or by their having been swept in by the action of water. Now, however, M. Figuier brings again the old exploded theory to the front. He speaks of the "cataclysm of the European deluge of the quaternary epoch," as if it were an article of geological faith; "the European diluvial inundation was, as we know, posterior to the glacial epoch, a great catastrophe, the tradition of which is preserved in the memory of all nations, marked in Europe the end of the quaternary epoch." In the first place there is not the slightest proof to be adduced of anything of the kind having ever happened in Europe, and in the second there is proof direct that it did not happen at the end of the so-called quaternary epoch. And the proof direct is furnished by the fact that the quaternary mammalia die out one by one, gradually and not all at once. There is an unbroken sequence of animal life in Europe from the Pliocene to the present day. This old diluvial theory runs more or less throughout the first part of the book, and leads the author very frequently astray. There are also minor points which require correction. The little globular sponges, with a hole in the middle, washed out of the chalk, and found in the gravel at Amiens, which are supposed by Dr. Rigollot to have formed necklaces, are figured as "fossil shells." If the author will refer to D'Orbigny he will find that it is figured as *Coscinopora globularis*. That it is a protozoon and not a shell there can be no doubt. It is indeed hard to understand how the mistake could have occurred, for the arrangement of M. Figuier's woodcuts is the same as that given by Sir Charles Lyell ("Antiquity of Man," p. 119) by whom it is rightly described. Again, why does the editor allow M. Figuier to call Professor Huxley "Hippocrates Huxley," Professor Vogt, "Galen Vogt," and Sir Charles Lyell "Celsus Lyell," because the first says that the Engis skull might have belonged to a philosopher, the second, that it belonged to a degraded savage, and the third offers no opinion whatever? It is a piece of wit too subtle for ordinary minds. The latter is certainly not a Doctor, nor has he written, like Celsus, on agriculture, rhetoric, or military affairs. The only possible point of resemblance between the three couples is that each was at the head of his own particular profession in his own time.

The portion of the book relating to the Neolithic Bronze and Iron ages is much better than the earlier part, but it contains little or nothing of importance that has not been published by Sir John Lubbock. We have done our duty by calling attention to the leading blunders. The book is the work of a remarkably skilful compiler, and is written altogether for effect. W. B. D.

OUR BOOK SHELF

An Elementary Course of Plane Geometry and Mensuration. By Richard Wormell, M.A., B.Sc. Second Edition, revised and enlarged. Fcap. 8vo., pp. 276. (London: T. Murby.)

THIS book is the work of a reformer, not so much of geometry, as of the mode of presenting it to the young. Sciences begin in practical applications, and tend by a universal law to become more and more abstract; and the doctrine of the reforming school is that whatever the science may have developed into, it is necessary in teaching it to go back to practical applications, and to seek for a sure foundation for abstract notions in the familiar experience of common objects. Teachers need to be incessantly reminded of this necessity. In teaching physics or chemistry or botany it is perhaps admitted, though not always obeyed; but in mathematics it is generally not admitted; and when admitted, it is rarely followed out to its logical consequences. Geometry, arithmetic, algebra, must alike be presented first in their applications; and then alone, in most cases, can definition and soundness be given. In most cases, we say, because where mathematical talents of a moderately high order exist, as in the generality of mathematical teachers, this necessity is not felt. And for this reason mathematical teachers who are not also observant of mental phrases, may be slow to believe what has just been pointed out as a necessity in their art. Many of them, we suspect, have a secret sympathy with the mathematician who proposed the health of "The prime numbers, the only branch of mathematics that has not been defiled by contact with the concrete."

The aim of Mr. Wormell's book is to teach scientific geometry, the logical dependence of truths on one another being shown, and to make the teaching sound by giving familiar illustrations of all the conceptions involved, and applications of the result attained. The book is interspersed with examples, geometrical and arithmetical. Among the subjects mentioned—we turn to the index, as giving a good idea of the book—are star polygons, axis of symmetry, graduation, land-surveying, spirals and volutes, transmission of motion by cogs, &c. Considering what the aim of the writer seems to have been, the illustrations and applied part of the book have been well done. It is generally clear, moreover, and accurate in style, and is interesting. In form it is adapted for a school book; and for certain schools it may be a success. But it fails as a scientific geometry intended to replace Euclid, in three respects; want of clear statement of axioms and exhibition of the science as rigidly deductive; the avoidance of the difficulty of incommensurables; and a degree of undefinable inelegance of treatment throughout. The book is too long, and is too nearly scientific, to use as an introduction to Euclid or to any of its modern substitutes; and, though it would replace them with advantage if the mathematical education of the student were to end with the reading of this book, it is not easy to see how the student proceeding to higher mathematics could do so without previously mastering another more complete geometry. It seems to me, therefore, a successful contribution to "technical education," and a valuable and suggestive attempt, but not altogether a successful one, to teach scientific geometry on true principles. The book is well adapted for middle-class schools. It is scarcely worth while to notice minor faults, either of the printer, which are very few, or of style. But it is really to be regretted that a degree should have been defined thus:—"Suppose we have a circumference sufficiently large to be divided easily into 360 equal parts, each part is called a degree." A degree is an angle, and this conception ought to be prominently brought forward.

J. M. WILSON

THE *Sitzungs-Berichte* of the Isis Natural History Society of Dresden for the first three months of the present year contains, as usual, a great number of short communications of more or less interest, and among them a few longer notices. Of the latter we may mention a note on the "Occurrence of Precious Stones in Saxon Switzerland," by M. A. Stelzner; a paper on "Diatomaceæ," by Dr. Eulenstein; a notice by Count Pourtalès on the "American Deep Sea Explorations;" a paper on the "Course of the Boomerang," by Professor Schneider; and, especially, a paper (illustrated) by Dr. Geinitz, on some "Fossil Fruits from the Zechstein and Coal Measures." The society has established a section for prehistoric archæology, the first meeting of which is here recorded; its proceedings consisted chiefly in the delivery of a long opening address by the President, Captain Oscar Schuster.

WHETHER the inhabitants of Rhenish Prussia and Westphalia are at the present moment devoting much of their attention to Natural History may fairly be doubted, but hitherto they have shown great activity in this department, and the publications of their Natural History Society generally contain much valuable matter. We have lately received the volume of their transactions for 1869 (*Verhandlungen des Naturhistorischen Vereins*, vol. xxvi.), in which we find several important papers. Kaltenbach contributes the continuation of his valuable "Natural History of the German Phytophagous Insects," consisting of an alphabetical list of the principal plants growing in Germany, either in a wild or cultivated state, with an account of the insects feeding upon each of them. Dr. C. Schlüter gives descriptions of numerous fossil Echinodermata from North Germany, with good illustrations on three large plates; whilst from Mr. F. Winter we have a contribution to the knowledge of the cryptogamic flora of the Saar district, now the scene of military operations. Another important botanical paper is a contribution to the flora of the Rhine by Dr. P. Wirtgen. In the section of the work denominated the "Correspondenzblatt" we find a note by Dr. Mohr on the "Theory of Coal," and in the "Proceedings of the Natural History and Medical Society of the Lower Rhine," a great number of notices upon scientific subjects of all sorts. This latter part is published separately for the present year. We have received the first number for 1870, including the proceedings during January and February.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

School Natural History Societies

IN your notice of the various School Natural History Societies, you have omitted one, which has been established for more than a year, and is now in a very flourishing state. I allude to the Clifton College Scientific Society. Unfortunately, no one is here to give me any statistics, but I understand that it has seventy members, with about twenty more waiting for admission. Mr. Percival has built a museum at a cost of 1,500*l.*, which will, I believe, be opened next September.

T. B. PRESTON

4, Lanesfield Villas, Durdham Down, Bristol
August 1

IN the leading article of NATURE for July 28 (which I have just received), you name some of the chief public schools in which Natural History Societies exist. I am sorry you have omitted to mention Cheltenham College as among that number, and conclude the omission arose from ignorance of the fact. Perhaps it may be interesting to some readers of NATURE to hear that last March, a society consisting of members of that College, was founded under the presidency of the principal (Mr. Jex-Blake) to whom the college is much indebted for the encouragement of Natural Science as a part of the general education of the place.

The Society numbers about fifty members, the department chiefly worked at present being botany, at which several of the boys are becoming tolerably proficient, with a small sprinkling of devotees to geology and zoology.

Our difficulty lies in making meetings sufficiently attractive. Original papers, containing original observations, are scarcely to be expected from the boys themselves till they have been educated to observe; and in our case the number of masters who take an active part in the society is too few to keep interest alive. I cannot, however, doubt, in spite of such difficulties, which have been felt (and in some instances overcome) by others as well as ourselves, that these societies are sowing good seed of which it is not too much to hope the world and science will reap the fruit by-and-by.

L.C.

Boscastle, Cornwall, Aug. 10

Our Dublin Correspondent and the Parturition of the Kangaroo

ON my safe delivery, after a good deal of labour, from the perils of war, and on my arrival in London from Germany, I found your letter, enclosing a copy of Dr. J. Barker's communication as printed in your issue of the 14th ult. Dr. Barker has, apparently, no fault to find with my report, which, as a matter of necessity, could not be otherwise than imperfect. But he somewhat loftily criticises the writer of the comments on my report, who, in spite of the facetious title given to him by Dr. Barker, I believe to be a gentleman of considerable merit, and one whose comments on my correspondence appear to be always most just. Dr. Barker is right when he states that the late Earl of Derby's father did not observe the facts about the Kangaroo which he records; these were observed by the keeper of his collection, but they were placed on record by the Earl and hence the mistake. Dr. Barker seems annoyed that he should be made to appear as if he adopted the views, the absolute nonsense, of the writer whose paper he permitted to be read. Those who know Dr. Barker know what absolute nonsense it would be to believe him capable of adopting them. Yet, ought he not, as chairman, to have repudiated and refuted them? Would it not have been well if he had given the members of the learned societies, on the occasion in question, the information which he now offers to the readers of NATURE, and instead of telling them "that the actual passage of the fœtal kangaroo from the uterus to the pouch was not yet proved," he had told them that the fact of there being such an actual passage had long since been proved; though how the actual transit, whether with the help of the mother's paws or lips, takes place, is still regarded as a matter for further observation; and so, instead of appearing to justify the reading of such a paper as the one referred to, he would from the extent of his knowledge on the subject, have reflected credit on his position, and on the societies to which he belongs, and have made, at least, an effort to advance the science he is so zealous for.

August 13 YOUR DUBLIN CORRESPONDENT

The Horse-Chestnut

I SAW to-day in the last number of NATURE, a letter on the meaning of the word horse-chestnut. As I do not see NATURE regularly, I do not know whether any of your correspondents have called your attention to the similar use of ἵππος in composition. In case this should have escaped them, I send the following extract from Liddell and Scott:—

"ἵππος, vi., in Compos.; it expressed anything *large* or *coarse*, as in our *horse-chestnut, horse-laugh*; v. ἱππό-κρημνος,-μάραθρον, -σέλινον, -τυφια, -πορνος; cf. βου-."

Long Ditton, Kingston, Aug. 5 M. W. MOGGRIDGE

The Rotundity of the Earth

WE have seen the statement signed "Parallax," at page 236 of No. 38 of NATURE, and shall be obliged if you will afford us an opportunity of briefly saying in reply, that when we tried the "flag experiment," the person calling himself "Parallax" was not present.

The experiment was conducted in his absence, as he did not come at the time appointed. He did not come at all that we know of; we did not see him.

J. NEWBEGIN
C. W. MILLARD
W. H. DAKIN

Norwich, August 10

Cuckow's Eggs

A SHORT time ago I addressed you on the subject of Cuckow's eggs, giving you some experiences of my own. I now have much pleasure in forwarding to you a portion of a letter on the same subject from an esteemed and observant correspondent, Mrs. Barber, of Highlands, near Graham's Town, to whom I communicated the substance of my letter to you. Mrs. Barber's name is well known in the botanical world as a most accurate scientific observer; of her ornithological acuteness my work on the Birds of South Africa amply testifies, and you may place full confidence in the statements she has made in this communication.

Cape Town, June 1870 ED. LAYARD

"Your remarks on the eggs of the cuckow tribe are very interesting. I confess that I am a believer in natural selection, and Darwinian in my opinions, but nevertheless in this matter I do not see the necessity for the intervention of natural selection; however, I hope you will bear in mind that I am speaking only with regard to the cuckows of my own country (South Africa), and as far as my observation extends, the eggs of these birds bear no resemblance to those of the birds upon which they are parasitic.

"Many of the different species of the cuckows of this country lay white eggs; the whole of those included in the genus *Chalcites* produce white eggs, the birds upon which they are parasitic are the various species of *Fringillidæ*, they do not, however, confine themselves entirely to this tribe.

"I have frequently seen the eggs of the 'Dedric' (*Chalcites auratus*) and the 'Metje' (*C. klaasii*) in the nest of the Cape canary (*Fringilla canicollis*) and the 'Streep Koppie' (*Fringillaria vittata*), where they were conspicuous not only for their pure white unspotted appearance, but for their size also, which is nearly twice that of the Cape canary, and considerably larger than the eggs of the 'Streep Koppie.'

"I have also found the egg of the 'Dedric' in the nest of the green Sun-bird (*Nectarinia famosa*), where it was also much larger than the grey speckled eggs of the sun-bird, and likewise dissimilar from its pure white colour.

"The egg of *Cuculus solitarius* is of a dark mahogany brown, and this egg I have seen in the nest of the wood robin (*Bessonorius phœnicurus*), when its difference was obvious both in size and colour, my son (F. H. Barber) found one of these dark brown eggs in the nest of the Cape canary! and despite its great dissimilarity compared to the small white speckled eggs of that bird, the work of incubation was quietly going on.

"The birds upon which the 'Honey Guides' are parasitic are *Laimodon leucomelas* (vel *L. unidentatus*). I have frequently seen them at the nests of these birds, where great conflicts occasionally take place between the *Indicators* and *Laimodons*, the latter being fully aware that the 'Honey Guide' is an intruder, the egg of the *Laimodon* is speckled, that of the *Indicator* white.

"The 'October bird' (*Oxylophus edolius*) deposits her white eggs in the nest of the large woodpecker; my brother (Bertram Bowker) once met with *three* of the young of this cuckow in the nest of that bird; it is not a common occurrence, I believe, that so many eggs should have been deposited in a single nest; the large woodpecker is, however, equal in size to the 'October bird;' when the birds upon which they are parasitic are smaller, the cuckow deposits but one egg, as the food and space required will in that case be only sufficient for a single individual.

"In the nest of the green sun-bird (*Nectarinia famosa*) I once observed a young 'Dedric,' which nearly filled the nest. It was not quite full fledged, and its frequent calls for food induced the sun-birds (both male and female) to exert themselves to the utmost, and in fact they had to work hard to satisfy the cravings of this greedy intruder; however they did it with a good will, and apparently without any suspicion that they were being imposed upon. Birds in general have no suspicion on this score, they suspect no trickery, and are therefore willing to incubate any kind of egg, provided it is not too large to fill up the nest. I think I told you how I had occasionally changed the eggs of various species of birds from one nest to another, making fearful confusion in consequence, yet the owners of the nests never suspected that anything was wrong, but proceeded quietly with their work. With regard to eggs, the discriminating power of birds is very obtuse, in fact they have none at all, and therefore in this case the agency of natural selection would not come into play; it would not be required. In nature there is no waste, no failure, no useless expenditure of time and ingenuity, every arrangement is sufficiently perfect to work out its own end without being overstrained or overwrought.—M. E. BARBER."

Special Modification of Colour in the Cushat

IN reading the chapter on "Mimicry," in Mr. Wallace's valuable collection of essays lately published, I was struck by a remark there made in regard to the special modification of the colour of the wood-pigeon. It is stated (p. 53), on the authority of Mr. Lester, that "the wood-dove, when perched amongst the branches of its favourite fir, is scarcely discernible, whereas, were it among some lighter foliage, the blue and purple tints in its plumage would far sooner betray it." This description may be accurate enough in regard to *Columba œnas*, but our experience is against its application to *Columba palumbus*. It was a common pastime of our boyhood to stalk the cushats in a mixed wood of the usual Scotch trees, and while familiar enough with their habit of making their nests in the spruce, unquestionably their favourite perches were on beeches and other hard-wood trees. Even after surmounting the somewhat delicate task of approaching the roosting-place of a cushat, it was no easy matter to detect the bird, except by its note, so closely did its general colours blend with the smooth, lichen-covered boughs of the beech, even where no leaves intercepted the view. The bird appears to build its nest especially in the spruce, not because its general colour agrees therewith (which it does not), but because the thick nature of the foliage and branches gives it, the eggs and young, sufficient privacy. Under all other circumstances it prefers to perch on the beech and other hard-wood trees, where its colours so adapt it for concealment. Of course the casual alighting on the pinnacles of the spruce during the breeding season is of little moment in the present question.

W. C. MCINTOSH

Colour Blindness

ALTHOUGH I have no intention of discussing the theory of colour-blindness propounded by Mr. Monck in NATURE of July 28, it may not be inopportune, while the subject is under the notice of your readers, to call their attention to a peculiarity with respect to the perception of colour, of which I have been able to discover no instance.

Some years ago I was sitting in a chapel opposite to a stained glass window, a portion of which (towards my left) was hidden from me by a pillar, and I observed that, as I moved my head to the right, the window flashed out into brilliancy where it had appeared dull before, while the contrary effect was produced as I moved my head to the left. On examining the conditions of the phenomenon carefully I found that it was due to the fact (which I had not the least suspected before) that my right eye is distinctly less sensitive to colour than my left. This I have since verified in various ways, though the difference is not very easily perceived unless the colours are brilliant, as in stained glass, bright coloured flowers, many of Turner's pictures, &c. The difference consists in this, that all colours appear less bright, or, as I should say, *greyer*, when seen with the right eye, and the more delicate gradations of colour cease to be perceived, while in many cases of even strongly contrasted colours, I should find it difficult to distinguish them with certainty with the right eye, especially if I had not previously seen them with the other eye. I have found too that the central part of the retina of my right eye is more defective as to the perception of colour than the lateral portions, since in looking at an extended surface of a single bright colour, scarlet for instance, a kind of shadow appears to come over any part of the surface to which I direct the eye.

With respect to my absolute power of perception of colour, I believe that though I cannot be said to be colour-blind, my eyes are less sensitive to colour than the average of those who have equally good general sight. For instance, scarlet and green do not appear to present to me the same degree of contrast that they do to most persons with whom I have made the comparison. Close at hand the contrast is sufficiently vivid, but a scarlet uniform seen at some distance in a green field would not attract my attention by contrast of colour, though I could make out the difference under a favourable light when my attention had been called to it; so also the scarlet berries of the mountain ash would at a little distance attract my notice rather by their form than by their colour, especially if seen against the sky or a bright object. Again, I can never pronounce with certainty as to the colour of distant bright lights; the colours of the lights, for instance, used for railway signals, though distinctly enough perceived by me when close at hand, puzzle me much when seen at a distance, while I am quite incapable of assigning with certainty a colour to a star or a meteor.

I should add that my ordinary power of vision is good; though here my right eye has a slight, but unmistakeable, advantage as to distinctness over my left. Hence, in looking at a brilliantly-coloured picture I have found that I could appreciate the drawing best with my right eye, the colour with my left, while in using both eyes each appears to remedy the defect of the other.

I think that the facts which I have here stated cannot fail to be of interest to those who are inclined to theorise on the nature of colour-blindness; but apart from all theory it would be satisfactory if the statement of my case should induce others to examine their own perceptions of colour with each eye separately, and in the event of their observing anything confirmatory of, or contrasting with, my observations, to send an account of them to NATURE. I think it quite possible that such cases may not be very uncommon, since the defect is one which may easily escape the notice of the subject of it.

Harrow, August 1 ROBERT B. HAYWARD

The Source of Solar Energy

MR. GREG still misses my meaning. I do believe that meteors supply a portion of the solar energy, and I also believe that they fall in enormous quantities into the sun; what I do not believe is that the whole solar energy is derived from meteors, or that any meteors fall in a solid state upon the sun (whose surface is also certainly not solid, even if any part of his mass be).

Mr. Greg's reasoning only proves what I have already pointed out, that none of the meteor systems our earth encounters can supply a meteoric downfall on the sun. This is, however, so obvious as to need no enforcing.

The reasoning by which I show that enormous quantities of meteors must fall upon the sun is wholly untouched by Mr. Greg's arguments, and is, so far as I can see, simply incontrovertible.

Surely Mr. Greg is not in earnest in saying that there would be a loss of solar energy if a large mass of iron fell on the sun before it was quite melted (any conceivable mass would, by the way, be vaporised), *because the sun would have to melt the portion which remained solid.* That solar energy would be consumed in the process is true enough; but if Mr. Greg supposes that the total solar energy would be diminished, he altogether misapprehends the whole subject he is dealing with. If the action of the solar energy in changing the condition of matter forming (as the imagined meteorite would) part of the sun's substance had to be counted as loss of energy, the sun would be extinguished in a very short time indeed. Such processes involve exchange, not loss.

If the earth could be placed on the sun's surface, the action of the sun in melting and vaporising the earth, and producing the dissociation of all compound bodies in the earth's substance, would involve an enormous expenditure of energy, yet the solar energy, considered as a whole, would be recruited, even apart from the fact that the earth would serve as fuel. The absolute temperature of the sun would, I grant, be diminished in this imaginary case, though quite inappreciably, but his total heat would be increased by whatever heat exists in the earth's substance.

Apart from this, however, if the minimum velocity with which a meteor or other body can reach the sun, is such as would—if wholly applied to heating the body—completely melt it, then the size of the body makes no difference whatever in the result. The meteor might not be melted if enormously large, but in that case the balance of heat would be communicated to the sun. In reality, of course, the heat correspondining to meteoric motion near the sun is very far greater than is here implied.

But I really must apologise for bringing before your readers considerations depending on the most elementary laws of the conservation of energy. RICHARD A. PROCTOR

Müller's Physics and Meteorology

FROM Prof. Jack's Review of Müller's "Physics and Meteorology," in your issue of August 4, I infer that he is not aware that an earlier edition of that valuable book was translated into English more than than twenty years ago, and formed one of the volumes of Baillière's *Scientific Library.* M.A. Cant.

Aug. 7

Colour of Water

MR. E. R. LANKESTER, in his letter in NATURE on 21st July, does not mention what is certainly one of the most remarkable known instances of a decided colour in water, I mean the Blue Lake near the road from Frutigen to Kandersteg in Switzerland. It is very small, not a stone's throw across. I think it is fed by springs. Its blue tint is so decided as to give the idea of some colouring stuff mixed with the water—not that it can be really so.

The Lakes of Neufchâtel and Bienne are of the same light-green tint as those of Lucerne, Brientz, and Thun, although the latter are fed by glacier streams and the former are fed by the streams of the Jura, where there are no glaciers. This appears to prove that the solid matter which glacier streams contain in suspension can have nothing to do with causing the green tint of most of the Swiss lakes. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Aug. 5

Water Analysis

YOUR article entitled "Water Analysis" consists of a review of a book, a commentary on a paper, and the reviewer's opinions of the character of Mr. Chapman and myself.

I shall not trouble you with any rectification of the statements contained either in the review or in the commentary on the paper; inasmuch as both the book and the original memoirs are accessible to the readers of NATURE, and the entire subject has already been very fully discussed.

In giving his opinion on the character of the authors of the book, the reviewer "deplored that two young chemists, with such undoubted abilities as Messrs Wanklyn and Chapman possess, should have rendered themselves notorious by attacking older workers in scientific investigation."

Perhaps you will allow me to say, that in rendering ourselves notorious in this manner, we have committed no crime, and that I cannot see why it should be deplored.

I believe that a great deal of the work which these older workers have done is unsound, and have endeavoured to sweep away some of that which I believe to be unsound. In this sweeping I have been to some extent successful, successful to an uncomfortable and alarming extent, I suppose your reviewer would say. But, if the rottenness of much that passes current in science is appalling, it is surely matter for congratulation that there are young men who will undertake a crusade against it, even at the risk of incurring the disapprobation of the older men, and of suffering every wrong that the possession of place and power enables these older men to inflict.

London, Aug. 14 J. ALFRED WANKLYN

[Mr. Wanklyn omits the sentence following his quotation:—"It is, no doubt, very laudable in a young and ardent investigator, when he points out that high authorities may err, and frequently have erred, but the manner in which these gentlemen have carried out their corrections has made their matter more distasteful."—ED.]

Suckers from the Apple Tree

MOST of the orchards in the west of Herefordshire have had their herbage injured during the present season by the extraordinary profusion of suckers thrown up by the apple-trees. In many places it looks like a miniature plantation, and is a serious detriment to the pasture. In an equal degree the elms have exhibited the same tendency to throw up an infinite number of shoots, and I am curious to know whether I am right in considering it to be due to the great heat of the surface of the soil.

C. J. ROBINSON

A Natural Fernery

NEAR where I am writing, in this parish (East Woodhay) is a deep hollow lane, with high sloping banks, which are abundantly clothed with the following ferns (nomenclature and arrangement from Dr. Hooker's new "Flora"):—*Pteris aquilina*, *Lomaria spicant* (rare), *Asplenium Adiantum-nigrum*, *A. filix-fœmina*, *Scolopendrium vulgare*, *Aspidium aculeatum*, *A. angulare*, *Nephrodium filix-mas* (with several pretty barren varieties), and *Polypodium vulgare*. Although the ferns are of the most common species, yet from the sloping nature of the ground, and the intermixing of a few other plants, such as *Equisetum sylvaticum*, *Lactuca muralis* (very fine), *Digitalis purpurea*, *Campanula Trachelium*, *Hypericum pulchrum*, *Juncus glaucus* and *compressus*, with a few pretty *Rosæ* and *Rubi*, tend to make it the most charming bit of fern scenery that I have ever fallen in with.

HENRY REEKS

The Science and Art Department

IN your impression of the 4th you have touched upon a point which has of late interested me much, viz., the science teaching of the "Science and Art Department."

At the commencement of the article you say that the work done is so little known that you have ventured its history. If you will allow me a small portion of your valuable space, I will put before your readers a few facts concerning the said "Department."

The teachers in the employ of the Department have no fixed salary; payments are made upon results to those persons who have passed in the first or second class (advanced stage) in the subject or subjects in which they give instruction, or who have passed in "Honours."

Now, a teacher's certificate is by no means difficult to obtain (advanced stage), provided Honours are not tried for, and consequently there is a large number of low-class teachers in the ranks, or those who are only grounded in elements of the subject which they are certificated to teach. For instance, a pupil on entering in 1869 for the examination and taking a second class in the advanced stage is entitled to teach and earn payments on results; such a teacher may only be grounded in the elements of the subject (say chemistry), and when he is applied to for the solution of a problem governed by physical laws he is at a loss and the pupil gets no answer to his query. It is my opinion that the teachers' examinations are much too easy. I think a certificate ought not to be granted unless the candidate has shown that he is familiar with the subject he intends to teach, and also with the cognate sciences. A chemistry teacher should know, at least, light and heat, magnetism and electricity; but at present such knowledge is not required.

I am not speaking specially for chemistry; geometry I may instance. There are many teachers at present engaged in giving instruction who know not a jot of Euclid's elements, nor can they work out the simplest algebraical problem. Should not a *teacher* know the theory as well as the practice, so as to give a definite and true answer to a pupil's inquiries. Machine drawing is the same; the examination consists of mere copying, and I can safely say that there are many teachers of mechanical drawing who would not be able to answer a single question of the paper set by the Society of Arts' examiner.

Science-teaching is daily becoming more and more appreciated, in some districts, however, only but very slowly; and I think it is only right that those teachers who do take an interest in science should endeavour to keep up the standard of the teacher. I should like to see every teacher obliged to pass in Honours—then the chemist would not pass without a thorough knowledge of physics, neither would the teacher of plane and solid geometry escape so easily, nor the pseudo-machinist obtain a certificate in a subject he was not competent to teach. But we must look at science as now taught from another point of view. It is for the encouragement of science among artisans (designated in the *Science Directory* as "the industrial classes") that the payments are made to teachers, the teachers not being able to claim any sum upon those which do not *strictly* come under that denomination.

Now, in the branch of chemistry some important rules have lately been made: candidates for the advanced stage are to be taught qualitative analysis, and each is to be supplied with a 2*l.* set of apparatus, the artisan cannot afford to supply it himself; the institution, or other place where the class meets, has only just enough funds to enable it to keep its head above water—who is then to supply it? The teacher; but often he will not see his way clear, for if he supplies it, 50 per cent. only is allowed him if the candidate obtains a first class. The pecuniary result to a teacher then is—for passing a student in the first class 1*l.*, and in the second a loss of 1*l.*, so therefore it will be advantageous for a teacher to keep his pupils out of the advanced stage altogether. If the authorities continue to let the rule stand, I should think payments would be allowed on middle-class students, as they are the only ones who probably could provide them with the apparatus.

It is all very well to say that an artisan, if he take sufficient interest in the science, *will* provide himself with the apparatus, but it generally happens that the willing ones are those who have the least opportunity of doing what they wish.

The past session has been a trying one for all teachers, for not only have the payments been reduced, but, to make up the list of evils, the standard has been raised, and consequently fewer have passed. The current session seems to be attended by as many drawbacks, which, if not withdrawn or somewhat modified, will, in the opinion of the majority of teachers, produce injurious results.

AN "HONOURS CERTIFICATED SCIENCE TEACHER"

The Intended Engineering College

WILL you allow me a small space in NATURE to call attention to a subject which seems to me to require the serious consideration of everyone who is interested in the progress of science in this country?

In reply to a question put to him in the House of Commons by Mr. Plunket, on the 9th instant, Mr. Grant Duff is reported to have said that it is the intention of Government to establish an engineering college for the Indian service, to be entered by competitive examination. Mr. Grant Duff does not appear to have entered into any detail with regard to the instruction to be given in the intended college, but it is fair to assume that Government would not think it worth while to take the education of engineers for India into its own hands, except with the intention of giving them a thorough and systematic training, at least as complete as what is already supplied by existing institutions. In this case, the instruction to be given in the new college will embrace at least a three years' course of study devoted to pure and applied mathematics, mechanics, physics, chemistry, geology, and the principles of engineering, in addition to the actual practice in the drawing office and workshop. That is to say, the Government school of engineering will be, on this supposition, what the Government School of Mines has become, in the main, a school of pure science.

Now, in view of this probability, the question suggests itself—whether it is fair and just that institutions like University College and King's College in London, and Owens College in Manchester,* which, without Government support or help of any kind, offer precisely the kind of training which we suppose the new college is intended to impart, should have to compete with an institution supported by Government prestige and Government money. The answer to this question affects not merely the interests of private institutions, such as those which have been mentioned, but, so far as the existence of these institutions is a benefit to the general public, it affects the interests of the whole nation; for in the case of colleges which depend for all or much of their income upon the general demand for education, their efficiency and their power to supply teaching of the highest kind, cannot but be seriously interfered with, if they are to be deprived of all share in the training of a class of pupils numerous enough to induce the Government to found a separate college for them alone. The course which Government proposes to adopt would, it seems to me, be justifiable only if the existing colleges were inefficient, and it had a clear prospect of establishing a more successful institution. As I have the honour to be connected with one of the Colleges in question, I do not intend to discuss their merits further than

* The Universities of Glasgow and Edinburgh might also be referred to although they do receive some amount of aid from the public funds.

to say that the quality of the teaching in any institution must depend upon the qualifications of the teachers, and that none of the three Colleges I have named need shrink from a comparison in this respect with any Government school at present in existence.

In answer to these considerations it might perhaps be urged that it is the duty of Government to secure efficient public servants, and that they are not called upon to consider how far private institutions are benefited or injured by the means that appear necessary for this purpose. To this I reply that, even adopting for the sake of argument this point of view, it is the imperative duty of Government not to spend public funds in doing what is already well done by private effort; and that, if anything was required which is not already supplied, public money would be far more productively expended in helping existing institutions to supply the defect, than in founding a new institution which will have to be supported entirely at the national expense. The obviously proper plan is for Government to test the qualifications of candidates for the public service with any degree of strictness they may think proper, but not to burden the country with the expense of educating them.

Perhaps, however, the assumption with which I started may be entirely wrong, and the proposed Engineering College may be intended to supply only a purely professional training. If this be so, there is even less to be said for it than before. In this case it will be merely a "Technical School," attempting to impart the knowledge and experience which can only be really acquired by actual practice under a working engineer.

In any case it seems strange that Government should announce its intention of establishing an Engineering College at the very time when a Royal Commission is making "inquiry with regard to Scientific Instruction, ... and the aid thereto derived from grants voted by Parliament."

University College, London, Aug. 15 G. C. FOSTER

OUR SALAD HERBS

THERE is perhaps no country in the world so rich as England in native materials for salad-making, and none in which ignorance and prejudice have more restricted their employment. At every season of the year the peasant may cull from the field and hedge-row wholesome herbs which would impart a pleasant variety to his monotonous meal, and save his store of potatoes from premature exhaustion; and there can be no question that in hot seasons a judicious admixture of fresh green food is as salutary as it is agreeable. Much has been said lately about the advantage which the labouring man would derive from an accurate acquaintance with the various sorts of fungus, and he has been gravely told that the *Fistulina hepatica* is an admirable substitute for beef-steak, and the *Agaricus gambosus* for the equally unknown veal cutlet. But deep-rooted suspicion is not easily eradicated, and there will always be a certain amount of hazard in dealing with a class of products in which the distinctions between noxious and innocuous are not very clearly marked. There is not this difficulty with regard to salad herbs, and we conceive that the diffusion of a little knowledge as to their properties and value would be an unmixed benefit to our rural population.

The first place must be assigned, on the score of antiquity, to the sorrel plant (*Rumex acetosa*), which in some districts still preserves the name of "green sauce," assigned to it in early times when it formed almost the only dinner vegetable. Its acid is pleasant and wholesome, and more delicate in flavour than that of the wood-sorrel (*Oxalis acetosa*), which, however, is used for table purposes in France and Germany. Chervil (*Anthriscus cerefolium*) is often found in a wild state and is an admirable addition to the salad bowl; and it is unnecessary to enlarge upon the virtues of celery (*Apium graveolens*) when improved by cultivation. John Ray, writing in 1663, says that "The Italians use several herbs for sallets which are not yet, or have not been used lately, but in England, viz., *selleri*, which is nothing else but the sweet smallage; the young shoots whereof, with a little of the head of the root cut off, they eat raw with oil and pepper;" and to this we may add that the alisander (*Smyrnium olusatrum*) is no bad substitute for its better known congener. The dandelion, which in France is blanched for the purpose, affords that *amari aliquid* which the professed salad maker finds in the leaves of the endive, and the same essential ingredient may be supplied by the avens (*Geum urbanum*), the bladder campion (*Silene inflata*), and the tender shoots of the wild hop. Most people are familiar with the properties of the water cress (*Nasturtium officinale*), and the garlic hedge mustard (*Erysimum alliaria*); but it may not be generally known that the common shepherd's purse (*Capsella bursa-pastoris*) and Lady's smock (*Cardamine pratensis*) are pleasant additions, whose merits have long been recognised by our foreign neighbours. In fact there is scarcely a herb that grows which has not some culinary virtue in a French peasant's eyes. Out of the blanched shoots of the wild chicory (*Cichorium intybus*), he forms the well-known *Barbe des Capucins*, and dignifies with the title of *Salade de Chanoine* our own neglected corn salad (*Fedia olitoria*). It would be very easy to extend the dimensions of our list of native salad herbs, for there are, perhaps, some palates to which the strong flavours of the chive (*Allium schœnoprasum*) and stone-crop (*Sedum reflexum*) may commend themselves, but enough has been said to show that Nature has not dealt niggardly with us, and that only knowledge is needful to make the riches she offers available. If the British peasant can be taught to discover hidden virtues in these plants with whose outward forms he has had life-long familiarity, we do not despair of his acquiring the one secret of salad-making, viz., the judicious employment of oil so as to correct the acid juices of the plants and yet preserve their several flavours unimpaired. C. J. ROBINSON

TESTIMONIAL TO PROF. MORRIS, F.G.S.

THE presentation of a testimonial to Prof. Morris on July 14, was the occasion of the meeting of nearly one hundred gentlemen, occupying prominent positions in connection with geology and the allied sciences.

Sir Roderick I. Murchison, Bart., K.C.B., occupied the chair, and, in opening the business of the day, expressed the sincere gratification he experienced in having been requested by the subscribers to this Testimonial to act as Chairman on an occasion when it was sought to do honour to Prof. Morris, whom he highly esteemed as a geologist, and whom he loved as a friend. He then gave a sketch of the career of Prof. Morris as a geologist, showing that in his earlier researches he was among the first to make most valuable communications upon the structure and fossil contents of the Tertiary formation of the South and East of England; and how he next threw much new light upon various members of the Oolitic formation and the Lias, describing the fossil contents with great acumen and ability. These were followed by his *opus magnum*, the "Catalogue of all British Fossils," which had gone through two editions, and was a work which would for ever hold a high place in science, as a truthful record of the succession of all classes of known animals from the earliest traces of life to those of the youngest Tertiary formations connecting ancient with existing nature.

The address was then presented, as follows:—

To John Morris, Esq., F.G.S., Professor of Geology in University College, London.

We, the undersigned, Friends and Cultivators of Geology, taking into consideration the degree in which this science has been advanced by your long and successful labours, are desirous of offering to you a Testimonial of the high estimation in which they are held. Always working in the field, and classifying public and private collections, you have for many years been among the foremost and most diligent of the Students of

Geology and Palæontology; your careful publications have their places among those of the Masters in every geological library; and your thoughtful lectures live, and will live, in the minds of your scholars. We shall be happy if by this Testimonial you may be encouraged to persevere in a course alike honourable to yourself and advantageous to the public, and that it may be pleasant for you in future years to remember that among the names to be submitted to you are many of those who have known and shared and been instructed by your labours.

"*Forsan et hæc olim meminisse juvabit.*"

Mr. Prestwich said: I have known Prof. Morris now some 35 years, and I can truly say that a more earnest geologist, and one more ready to impart to others the great and varied information he himself possesses, there cannot exist. His aim has ever been to spread a knowledge of that science which he has cultivated with so much pleasure and so much success. Often, too, regardless of other considerations, his first object has always been the free, and, if possible, the gratuitous diffusion of geological science—possibly too much so. Still, if he has failed often to secure his reward in *£ s. d.*, he has always secured the respect, esteem, and affection of his friends, and of all with whom he has worked. With most men the prospect of a good fee would be an incitation to work, but with my good friend Morris the surest way, I believe, to get him to work was too often to say that no fee or remuneration would attach to it. His first introduction years ago was by a valued mutual friend, Mr. Lonsdale, who suggested that a work on the Tertiary Geology of the London Basin was needed, and he thought that Prof. Morris might undertake the palæontological and I the stratigraphical part. I am inclined to believe that Mr. Morris must have considered the work as likely possibly to be remunerative, as, though taken up warmly at the time, the first chapter still remains unwritten. This testimonial is, I feel, but a small earnest of our feelings towards Prof. Morris. For the opportunity thus afforded of expressing our good wishes towards our friend, we have to thank the *Mining Journal* for the publicity it has given to the matter, and for an active part in the management we are, I believe, indebted to Mr. Hearn, whilst Mr. Milnes has kindly acted as treasurer for the fund, which for Prof. Morris's sake I only wish had been doubled.

The meeting was then addressed by Mr. Hearn, and Prof. Tennant, and a letter was read from the venerable Prof. Sedgwick; and Prof. Morris made an eloquent reply, in the course of which he said: It is with deep and sincere feeling that I thank you and the members of the Geological Society, and other friends, for the handsome testimonial which I have received at your hands this day. I not only thank you for the kind manner with which you have expressed yourself, but I recall with pleasure the encouragement to pursue my labours you gave me more than 25 years since from that chair when President of the society, in 1842, and still more so when in after years your kind suggestion induced me to become a candidate for, and your strong recommendation placed me in, the position I now hold at University College, the duties of which, during the last 15 years, I have earnestly endeavoured to fulfil, so that I trust you have had no cause to regret the confidence you then placed in me.

Prof. Sedgwick's letter was as follows:—

"The infirmities of old age make it impossible for me to attend the public meeting at the rooms of the Geological Society. This is to me a great sorrow, for no one can value Prof. Morris's most laborious and most useful palæontological works at a higher price then I have done. And I honour him not merely as a man of science but also as a kind personal friend, who has for many years taken his place in the first rank of practical English geologists. Pray present to him my heartfelt congratulations, which the load of 85 years, and a great infirmity of sight and hearing, will prevent me from offering personally."

The meeting then separated.

WHEAT RUST AND BERBERRY RUST

THE theory has long been prevalent among practical agriculturists that the proximity of berberry trees produces rust in wheat. Men of science, unable to trace herein the sequence of cause and effect, long derided the idea, and placed it among the prejudices of the agricultural mind. The facts of the farmer have, however, been too strong for the science of the botanist, and experience has won the day over theory. Let us trace for a moment the history of the inquiry. The first reference to the injurious influence of the berberry on corn appears in Krünitz's Encyclopædia, published in 1774. Marshall, in 1781, speaks of the berberry having been extirpated in Norfolk for this reason, and Schöpf, in 1788, mentions the same idea as prevalent in New England. Other writers of the same period give similar testimony; and in 1806 Sir Joseph Banks writes thus in the *Annals of Botany*:—"It has long been admitted by farmers, though scarcely credited by botanists, that wheat in the neighbourhood of a berberry bush seldom escapes the blight. The village of Rollesby, in Norfolk, where berberries abound, and wheat seldom succeeds, is called by the opprobrious appellation of 'Mildew-Rollesby.' Some observing men have lately attributed this very perplexing effect to the farina (pollen) of the flowers of the berberry, which is in truth yellow, resembling in some degree the appearance of the rust, or what is presumed to be the blight in its early state. It is, however, notorious to all botanical observers that the leaves of the berberry are very subject to the attacks of a yellow parasitic fungus, larger, but otherwise much resembling the rust in corn. Is it not more than possible that the parasitic fungus of the berberry and that of wheat are of the same species, and that the seed is transferred from the berberry to the corn?" The acute suggestion thrown out by Sir Joseph Banks, at a time when so little was accurately known of the structure of fungi, was not followed out for half a century; it was reserved for the German fungologist, De Bary, within the last few years to establish the truth of his theory, and to prove the existence of the

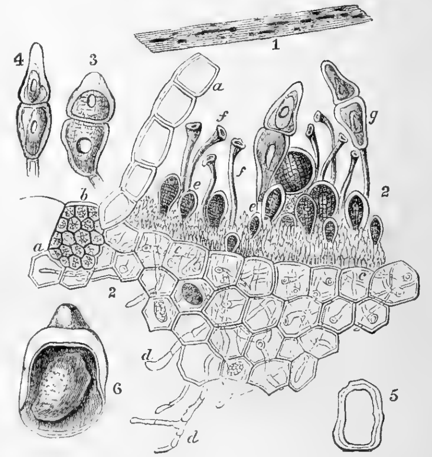


FIG. A.—*Puccinia graminis*, Pers. 1. Leaf of grass, with mildew, natural size. 2. Section of the leaf, with a patch of mildew and rust; *a*, epidermis of the leaf; *b*, bast-cell nerves; *c c*, outer layer of cells of the leaf, on which the parasite rests; *d d*, mycelium; *e e*, young and old spores of *Cæoma lineare*; *f f*, stalks from which the spores have fallen off; *g*, spores of the *Puccinia*. 3, 4 Spores. 5. Section of the wall of the lower spore-cell. 6. Longitudinal section of the upper spore-cell with the spore-nucleus. (2 - 6 magnified.)

phenomenon of Alternation of Generation among Fungi. The researches of Steenstrup and others have made us familiar with this remarkable phenomenon among the lower forms of animal life, but had hardly prepared us to meet with it in the vegetable kingdom. It appears probable, however, that the phenomenon is by no means uncommon here also,—affording another instance of the law that it is in their lowest forms that the animal and vegetable kingdoms approach one another most nearly,—and that whole tribes of fungi hitherto considered distinct are but different phases of one another. This remark applies especially to the two genera of minute parasitic fungi, *Æcidium* and *Puccinia*, to which the rusts in question belong, both belonging to the family *Uredineæ*. The well-known orange-red spots so common on the leaves of the berberry are produced by the *Æcidium berberidis*, while the rust of wheat and other cereal crops, but found equally on some other species of grass, as the common couch-grass or *Triticum repens*, is the *Puccinia graminis*. In the volume for 1865 of the *Monatsberichte der kön. preuss. Akademie der Wissenschaften zu Berlin* is a paper by Dr. De Bary, giving an elaborate account of his experiments on the propagation of these two fungi, in which, if his experiments are reliable, he clearly proves the correctness of Sir Joseph Banks's suggestion that they are one and the same species. The experiment was tried, with due precautions, of inoculating the leaves of the berberry with the spores of the *Puccinia*, the result being the production, not of the same fungus, but of the *Æcidium*, while the sowing of the spores of this latter fungus on the leaves of couch or wheat produced conversely the *Puccinia*. By sowing the spores of either fungus on the plant on which it was itself parasitic, he failed altogether to reproduce the same plant; and this alternation of generation may serve to account for the fact which has often been noticed, that rust is apt to appear not in successive but in alternate years on the same crop.

It is unfortunate to find that in a work bearing a considerable amount of scientific authority among agriculturists, and published in the same year, 1865, Prof. Buckman's "Science and Practice of Farm-cultivation,' the theory which thus appears to have been proved on the Continent was scouted in the following terms: "*Æcidium berberidis* is here referred to, from an opinion prevailing

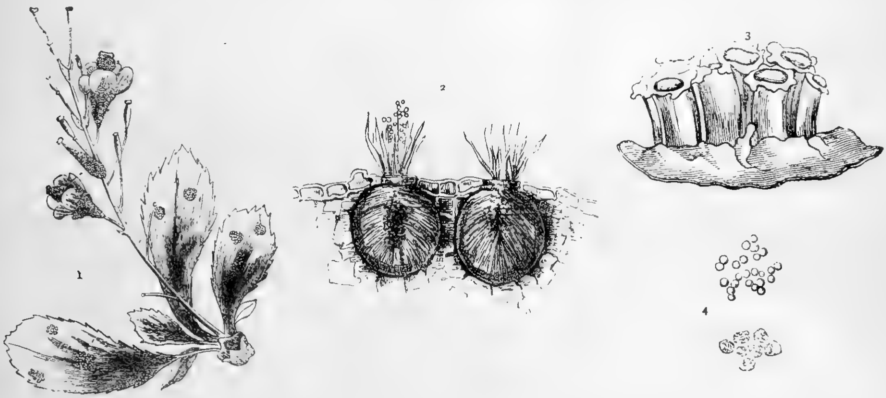


FIG. B.—*Æcidium berberidis*, Gmel. 1. Branch of berberry with spots of rust, natural size. 2. Spermagonia. 3. A group of Peridia with their orifices dentated. 4. Sporidia. (2, 3, and 4 magnified.)

that it is the cause of rust or mildew in wheat. We can no more believe that the berberry rust would produce rust in wheat than the rust of any other plant would do so. Still that wheat growing under a berberry hedge may be more blighted than in the rest of the field is quite true, and so it is with wheat growing under any kind of hedge." Mr. Buckman fails entirely to grasp the argument, which is not that wheat "growing under a berberry hedge" is attacked by rust, but when growing in the proximity of a berberry tree, say at the distance of a field's breadth. Nothing is more certain to weaken the hold of science over practical men than when men of science, in order to support their own theories, set themselves systematically to deny well-known facts. We therefore greatly regret the decision at which it is understood the council of our Royal Agricultural Society has arrived, to refuse a thorough investigation of the subject which has been urged upon them, calling in the assistance of experienced men and the most able fungologists of the day. This is not the way to command the confidence of practical farmers.

We commend to the consideration of the Royal Agricultural Society the conduct of a railway company in the south of France, described in the *Bulletin de la Société botanique de France* for January of this year, to which we have already alluded (see NATURE, vol. i., p. 516). In the commune of Genlis, department of Côte-d'-Or, a berberry hedge was not long since planted on one of the railway embankments; when immediately the crops of wheat, rye, and barley in the neighbourhood became infested with rust. The complaints of the farmers caused the appointment by the company of a commission to investigate the subject, who reported, after a full inquiry, that wherever the berberry was planted the cereals were more or less attacked by rust; where they were absent the crops were free from the disease; and that the planting of a single berberry bush was sufficient to produce the rust where it had never appeared before. The railway company's own commission held that compensation was due from the company to the farmers.

Our illustration of the *Æcidium berberidis* is taken (in part) from Greville's "Scotch Cryptogamic Flora;" that of the *Puccinia graminis* from Corda's "Icones Fungorum."

ALFRED W. BENNETT

In considering the question of the influence of the berberry on the production of rust in wheat, assuming that De Bary's observations are perfectly correct, it is necessary to consider the nature of what is commonly called "rust" in cereals. Presuming that his views are strongly confirmed by the analogous connection of *Rœstelia cancellata*, the pear blight, with the gelatinous parasite of *Juniperus sabina*, it is well to attend to the following facts :—Professor Henslow in an article on the diseases of wheat, in the Journal of the Royal Agricultural Society, proved distinctly that what is commonly called rust is merely a condition of the common mildew, and this at a time when comparatively little was known about these parasites, and when many were inclined to accept the views of Unger that they were mere abnormal developments of tissue or spontaneous growths. The observations of Tulasne and others confirmed to a certain extent Professor Henslow's view, but threw further light upon the matter by showing that many so-called *Uredos* were merely a subsidiary form of so many species of *Puccinia*. Meanwhile, though *Uredo rubigo vera* was the subsidiary form of *Puccinia graminis*, it was recognised that *Uredo linearis* is nothing more than the early stage of the *Puccinia*. Though there is some resemblance between the Uredinoid form of the *Puccinia* and the rust of the berberry, there is none between the perfect condition of the parasite. Our readers will have noticed that at the meeting of the French Academy on August 1st, M. Roze contributed some further illustrations of this interesting subject.

The great difficulty has always been that mildew is most prevalent in countries where not a berberry bush is to be found ; and the same remark applies to the pear rust, which abounds where not a single plant of savine is to be seen, the parasite of the savine being comparatively of rare occurrence. I think, always assuming the fact of the connection between the two parasites, that it may be easily accounted for. It may be true that the berberry plant produces mildew ; but how is this? not probably from the spores of the present year, but from those which fell to the ground the previous season. There is no doubt that these parasites penetrate into the tissues of the young germinating plants, by means not of the original spores, but of minute secondary spores which are produced on them, a circumstance which is fully proved in the case of bunt. This, then, will account for the cereal being mildewed in the neighbourhood of the berberry. But another consideration is necessary to explain the prevalence of mildew where the berberry does not exist, or where it is confined to gardens. The subsidiary spores have no doubt, equally with the *Puccinia* itself, the property of reproducing the mildew, and there are always enough of these blown about, either from previous crops or from the neighbouring grasses, especially in the fens, where every ditch is filled with reeds affected more or less with mildew ; and thus the parasite may be propagated season after season without the Æcidioid form intervening, a circumstance which is not without analogy in other branches of the vegetable kingdom. I may be allowed, perhaps, to recall attention to an article on the development of bunt in the second volume of the Journal of the Horticultural Society of London, which seems entirely to have escaped notice on the Continent, where it is stated in a paper communicated by me on Jan. 18, 1847, with reference to the phenomena described, that " it is quite possible that in plants as well as in the lower animals there may be an alternation of generations."

M. J. BERKELEY

NOTES

A RUMOUR is current that the Government have refused both ships and assistance to the Royal and Royal Astronomical Societies, which have been for some time organising expeditions to observe the approaching total eclipse of the sun. We can hardly believe that the Government will thus venture to brave the opinion of all men of science and culture. It would be a direct acknowledgment that the Government cares as little for a recent position for England in science and the arts of peace as it did a little time ago for her position in the arts of war. Verily we are a nation of Philistines !

OUR readers will hear with great regret that Prof. Wyville Thomson is prevented by illness from taking that share in the scientific exploration of the Mediterranean basin, now about to commence, which has conduced so greatly to the success of the previous expeditions in which he has been one of the workers.

M. OTTO STRUVE, director of the Observatory at Poulkova, M. Wild, director of the Physical Observatory at St. Petersburg, and M. Mohn, director of the Meteorological Institute of Christiania, have just arrived in Paris, for the purpose of taking part in the international conference charged with establishing a universal metre. In consequence, however, of the war, the meeting of the conference is postponed until such time as it may be summoned to meet by the Government.

AMONG the chances of war which have necessitated that Paris should be placed in a state of defence against a besieging enemy, the rasing of the Bois de Boulogne has become one of the first necessary operations. The fine collection of animals belonging to the Société Impériale d'Acclimatation will then have to share the fate of those belonging to the Zoological Society of Cologne, and be dispersed or removed till better times. It is even said that the axe is already at work.

THE Franco-German War is telling heavily on science on the Continent. In the number of the *Revue des Cours Scientifiques* for August 13th, the Editor hints at the possible suspension at an early date of the publication of his journal till the war is over.

To form an idea of the results of a general armament in Germany, it will be sufficient to learn that from the Berlin Chemical Laboratory, besides a great number of students, all the assistants, seven in number, have joined the army, partly as soldiers partly as field-apothecaries. When large masses of troops passed through Berlin, the director of the laboratory placed room for twenty soldiers at the disposal of the military authorities. University lectures have been prematurely closed. The military schools, the agricultural school, and the school of architecture had to close for want of pupils. The upper forms of the grammar schools have also sent many of their pupils into the field ; one of them as many as eighteen out of forty.

THE engineering works of Messrs. Siemens and Halscke, and the ironfoundry of Borsig, are now almost exclusively occupied with the manufactory of torpedoes. It is said that great improvements have been made in these war-engines ; the point chiefly kept in view being to make them moveable from the shore by means of an electro-magnetical rotatory apparatus.

THE season for Congresses Scientific and other has now fairly set in. A Medical and an Engineering Congress are now sitting in the north ; we last week gave some of the arrangements for the forthcoming meeting of the British Association at Liverpool, and it is now announced that the Social Science Congress will meet from the 21st to the 28th of September, at Newcastle-on-Tyne, under the presidency of the Duke of Northumberland. The Social Science Congress has done what the British Association might also do to a certain extent with great advantage. It has stated the questions which press most for solution in the different branches of inquiry with which it deals. With two of these sections, namely, those of Education and Health, we are especially interested, and we willingly acknowledge the high importance of the questions which it is proposed to discuss. They are as follows :—Education. 1. Can better educational results in primary schools be obtained by the amalgamation of such schools? 2. By what means can a direct connection be established between the elementary and secondary schools and the Universities? 3. Is it desirable to teach science in elementary schools, and, if so, what branches of science? 1. Health. What is the best method of disposing of sewage and excreta? 2. What modifications are desirable in the existing sanitary laws and administration? 3. What legislative measures ought to be taken to prevent the adulteration of food, drink, and drugs?

THE British Medical Association will meet next year at Plymouth, under the presidency of Mr. Whipple, consulting surgeon to the Plymouth Infirmary.

WE have received the report of the Scottish Astronomer Royal to the Board of Visitors read at their visitation on the 29th of June. The report, in Mr. Piazzi Smyth's most curious style, refers to so many additional "facilitations" in the observatory that we may soon hope for some work to be done there, and to so many questions which have nothing to do with the observatory that we may possibly return to it.

AT the London Institution, Finsbury Circus, on the 10th inst. Mr. J. P. Gassiot, D.C.L., F.R.S., distributed the prizes awarded, and certificates granted to students who have passed examinations on courses of Educational Lectures delivered during the past session. In the examination connected with Prof. Guthrie's course on Elementary Physics, Edmund Strode gained the first prize, while T. Lyon, Miss Esther Greatbatch, and Miss Annie Piper obtained prizes of the second order. In the examination on Professor Bloxam's chemical lectures, A. J. Richardson obtained the first prize, Isidore Harris and Miss E. M. Hutton taking second prizes. In the examination on Prof. Bentley's botanical course, Miss Emma Ball took the first prize; and A. J. Wallis and Miss Ellen Benham second prizes. Mr. Gassiot stated that the Educational Lectures of the London Institution were commenced in the spring of 1869, by Prof. Huxley, and that the attendance of students from the leading metropolitan schools had fully justified the belief of the managers that increased facilities for scientific education were needed. In distributing the prizes, which consisted of standard scientific books, handsomely bound, Mr. Gassiot referred to the early days of some of the eminent authors and discoverers who had been connected with the London Institution. He stated that arrangements for developing the educational powers of the institution were in progress, and delighted his audience by the announcement that Professors Odling and Huxley would deliver courses of lectures early in the coming session.

WE have had from time to time to record evidences of the increased interest in science felt in the United States. We have just received the first number of the *American Scientific Monthly*, published at Iowa City, and edited by Prof. Gustavus Hinrichs. It contains articles of a popular rather than a deeply scientific character, on various branches of science, chemistry, geology, physics, physiology, &c.

THE Sars subscription fund has now been closed, the total amount received being 12,283 francs.

M. DAVAUNE states, in a recent number of *Cosmos*, that the proposal to raise a subscription on behalf of M. Niepce de St. Victor has met with a warm and hearty response.

REFERRING to the note in our issue of July 28, L'Abbé Moigno makes the following reply in *Les Mondes*:—"Le journal NATURE a émaillé de points d'exclamations notre petit article sur les silex du Sinaï. Il s'étonne que nous nous soyons fait fort de prouver jusqu'à l'évidence que ces silex sont plus vieux que ceux des grottes d'Aurignac et autres. M. Louis Buchner, dont la NATURE devrait bien reproduire les propres paroles, l'a dit avant nous. Il s'étonne plus encore de notre confiance dans le progrès, et rappelle de notre appel à M. Sorby:—attendons; le spectroscope jugera entre nous. En tout cas, c'est déjà quelque chose que des silex ou couteaux en pierre, vieux de 3,350 ans."

MR. ERNEST HART has been unanimously elected Editor of the *British Medical Journal*, in the place of Mr. Jonathan Hutchinson.

THE death is announced, at the age of 75, of Baron Charles von Hügel, one of the founders of the Horticultural Society at Vienna, and a collector, on a large scale, of new Australian plants. He was the author of several botanical works, and held the office of Austrian plenipotentiary at the court of Brussels.

IT is hoped that the Cornell University will be able to form the nucleus of a museum, as Professor C. Fred. Hartt, with a professor of botany and nine students, intends exploring a portion of the valley of the Amazon and the Brazilian coast southward to Bahia, and to collect objects in natural history and geology. Professor Hartt proposes to take stereoscopic views of interest along his route.

THE Royal Academy of Sciences of Belgium offers a prize of 800 francs for an essay on the Affinities of *Lycopodiaceæ*. The essays should be written in Latin, French, or Flemish, and forwarded to M. Ad. Quetelet, the secretary of the Academy, at Brussels, before June 1, 1871.

THE practical examination of workmen and students for the Whitworth Scholarships has been fixed by Sir Joseph Whitworth to take place at his works at Manchester, on the 30th August and 1st September next.

THE *Engineer* gives a plan and section of a proposed tunnel under the Bosphorus, planned by Mr. J. Haddan, one of the chief engineers to the Turkish Government. Unlike Messrs. Bateman and Révy's proposed Channel railway, the idea is to suspend the tunnel at about thirty-five feet below the surface of the water, and fix it to the bottom by means of holding chains.

MR. GEORGE FAWCUS, of North Shields, has contrived an equilateral triangular drawing-board for isometrical drawing. An ordinary T square applied on the edges of an equilateral triangle draws tangents that meet each other at angles of 120°, and other lines drawn parallel to these radiating ones form with them angles of 60° and 120°, which are the exact angles of the apparent squares of isometrical cubes. The inventor believes that the use of this new drawing-board will make the teaching of isometrical drawing both simple and easy. The practice of isometrical drawing is strongly urged in the science and art drawing classes.

A REMEDY has been found for the "borer" that ravages Indian and Ceylon coffee plantations, by applying carbolic acid before the eggs are hatched.

THE curator of the Botanical Exchange Club has just issued his Report for the year 1869. Dr. Boswell-Syme takes the opportunity of recording all the observations which have come under his notice respecting new forms or varieties of British plants, or new localities of the rarer species.

PROF. CHANDLER, chemist to the Metropolitan Board of Health, New York, gives, in the *American Scientific Monthly*, his analysis of fifteen different kinds of fashionable hair-tonics, restoratives, &c., in all of which he finds lead varying in amount from one-ninth of a grain to sixteen grains in the ounce. He states that they owe their action to this metal, and are, consequently, highly dangerous to the health of persons using them. Lotions for the skin he found to be free from lead or other injurious metals, with the exception of one containing corrosive sublimate. The enamels examined consisted of either carbonate of lime, oxide of zinc, or carbonate of lead, suspended in water. The latter kind are highly dangerous; the two former are "as harmless as any other white dirt when plastered over the skin to close the pores and prevent its healthy action." The white powders for the skin are harmless to the same extent.

THE *Western Monthly* (Chicago), in its August number, has a very readable article on Sun Spots and their lessons, in which the author discusses the consequences of the obscuration of one part in one hundred and thirty of the sun's visible surface in the present year, and thinks that he is about to open one of the sealed volumes which contains the principles of prognostic meteorology.

THE "Proceedings of the Birmingham Natural History Society and Microscopical Society" for 1869, is another of those records of natural history researches in the provinces, of which we have so many gratifying instances, the more valuable when coming from the centre of a manufacturing district, where the thoughts of men are naturally turned in such very different channels. We have papers of various lengths on mineralogy, botany, microscopy, physiology, geology, entomology, malacology, and other branches of natural history, showing, if not much originality, much careful observant work. Appended are preliminary lists of the flowering plants, mosses, Lepidoptera, and Mollusca found within a radius of ten miles from Birmingham, a by no means inconsiderable array.

THE astronomical and meteorological observations of the National Observatory of Santiago, in Chile, have now been regularly published since 1853, chiefly by Don José Tomas Vergara. They also include the Valparaiso meteorological observations.

CINCHONA culture has now so far advanced in Madras that the Government is preparing to deal with it as an annual crop.

MR. WORTHINGTON SMITH records in the *Journal of Botany* an instance of a fatal case of poisoning by eating the root of the Water-dropwort, *Œnanthe crocata*, an umbelliferous plant common in ditches and wet places. A carter at Staplehurst, in Kent, ate some of the roots whilst at work, supposing them to be the wild parsnip; in about an hour he became unconscious and convulsed, and death occurred in another half-hour, before medical aid could be obtained. The man had fed his horse with roots of the same plant, and the animal also expired about two hours after eating them. There is no doubt that the *Œnanthe* is a virulent poison, but it seems strange that the horse, as well as the man, should not have rejected a plant of so acrid and suspicious a flavour. Several wild Umbelliferæ are among the most dangerous of British plants, and it is probable that the Greek poison κώνειον was obtained from others besides the hemlock.

SHARKS appear to have recently again made their appearance in the Gulf of Trieste, and the police have issued a notice forbidding people to bathe in the port or on the coast. For each fish destroyed in the waters between Punto Grona and the castle of Duino a reward of 300 florins (about 30*l.*) is given.

THE Indian Government has selected the Khond Hills for cinchona experiments. If they succeed the cultivation will be thrown open to private enterprise, with the view of further promoting employment and cultivation among the Khonds.

THE extent of the Wurdah coal-field in India has been confirmed, and the seams in Berar have been found to be 45 feet in total thickness.

THE total eclipse of July was observed at Constantinople by the Rev. C. Gribble, H.B.M. Chaplain, formerly of the Royal Navy, a local astronomical observer. He contributes an account to the *Levant Herald* of July 20. The dogs of Constantinople continued barking until about the middle of the eclipse.

WE have had occasion to refer to wild-beast legislation and administration, an important matter in India. A curious discussion has arisen in Bombay. Tigers having come to Salsette and killed several people, the magistrates applied to increase the reward, but the Government have refused, thinking that the report of the presence of tigers there will attract English sportsmen from Bombay.

"A CATALOGUE of Maps of the British Possessions in India and other parts of Asia," published by order of Her Majesty's Secretary of State for India in Council, is a very useful publication for those interested in our Asiatic possessions.

THE first and second quarterly publications for 1870 are issued of Auwers' and Winnecke's "Vierteljahrsschrift der Astronomischen Gesellschaft."

DR. SACHS'S "Lehrbuch der Botanik nach dem gegenwärtigen Stand der Wissenschaft" has reached a second enlarged and partly rewritten edition. It is illustrated with 453 woodcuts.

DR. KARL KARMARSCH'S "Technological Dictionary, English, French, German," of which the second edition is just issued, is a most useful compilation, containing the corresponding terms in these three languages employed in Architecture, civil, military, and naval; Civil Engineering, including bridge building, road and railway making; Mechanics, machine and engine making; Ship Building and Navigation; Metallurgy, mining and smelting; Artillery; Mathematics; Physics; Chemistry; Mineralogy; and generally in the Arts and Sciences.

DR. ENGELMANN publishes the first part of his "Results of Observations in the Leipzig Observatory," comprising those made with the meridian-circle.

M. VIDAL'S statement (says the *Photographic News*), that a sensitive plate, if exposed to light in the camera, and then placed behind the yellow glass window of a dark room, becomes attacked by the yellow rays and yields a fogged image, whereas a sensitive plate previously unexposed to light is not affected in the same manner, has been confirmed by results obtained in America, and detailed in the *Philadelphia Photographer*.

PAPERS ON IRON AND STEEL

I.—A VERY COSTLY AND VEXATIOUS FALLACY

I.

"A FRIEND of mine has been converting some common cinder pig-iron into either very fine iron or steel by a very simple process, but does not know who to apply to to learn its value. He is willing to share the profit with anyone who will help him in the matter. I have some small samples of it if you would like to see it, or can tell me who would be likely to interest themselves in the matter. From what I can make out I should think it would make good steel, for it will harden and temper now."

The above, quoted from a letter I have recently received, is a typical sample of a number of others I have had at different times, and it represents the labours of quite a multitude of patient, long-suffering, and miserably deluded investigators. The published specifications of abandoned patents make painful record of wasted money, time, and ingenuity; and suggest dark tragedies of ruined hopes, all arising from the same misunderstanding of the changes which take place in the conversion of *ordinary* pig-iron or cast-iron into *merchantable* steel.

The most humiliating feature of this delusion is that it is not the offspring of popular ignorance, is not prevalent among the beer-drinking class of iron-workers, who sign their names with a ×, but crops out among intelligent self-taught men, who have studied the chemistry of iron and steel as expounded in recognised chemical books. The costly fallacy I allude to is directly traceable to the teachings of our highest scientific authorities. As NATURE is now largely circulating among the class of self-taught and energetic men who supply this ever-recurring crop of victims, and also among those who most unwittingly and unwillingly have deceived them, there can be no better medium through which to effect the demolition of this mischievous error.

By reference to almost any text-book on chemistry, it will be found that cast-iron is described as a compound or mixture of iron and carbon; that steel is another compound or mixture of iron and carbon, but with a less proportion of carbon; and that wrought iron is nearly free from carbon. Further, we are told that the ordinary method of making steel is, first to remove all the carbon from the cast or pig-iron by making it into wrought or bar-iron, and that this bar-iron is afterwards converted into steel by causing it to take up a new dose of carbon in the cementing furnace. The natural inference of a thinking reader is, that this is a clumsy complication, especially if he knows that the process of cementation is slow and costly, that on account of the irregular diffusion of the carbon in the blistered bars, other expensive processes of shearing, tilting, casting, &c., have to follow. Why not at once produce the steel from cast-iron by a process of decarburisation which shall stop at the right point, *i.e.*, when the 3 or 4 per cent. of carbon of the cast-iron is reduced to the one or one and a half per cent. required to produce steel? By doing this, not only the cost of converting wrought-iron into steel, but also the cost of puddling to produce wrought-iron will be saved; and steel, which is but a carburet of iron intermediate between cast and wrought-iron, instead of being so much dearer than either, should be made at an intermediate cost, or cheaper than wrought-iron.

If he dips further into the literature of the subject, and reads the history of the manufacture of iron, he will find further confirmation of such reasoning, as he will learn thereby that the direct production of steel is an ancient art, and that weapons of renowned quality were made from steel thus produced.

By reference to one of the most recent and elaborate English treatises on the subject, Dr. Percy's "Metallurgy," he will find on page 778 that this is described as "the ancient method, which is still extensively practised on the Continent, especially in Styria;" and further down on the same page that "if steel be regarded simply as iron carburised in degrees intermediate between malleable and cast-iron, then it is obvious that the latter during its conversion into the former in the processes of fining and puddling, must pass through the state of steel." On page 805 of the same work he will find further confirmation of his theory in the words, "it is obvious that steel must be produced by melting malleable and cast-iron together in suitable proportions."

I might multiply quotations from this and every other work I have seen in which the chemistry of iron and steel is treated, and show by each of them that the thousand-and-one of unfortunate inventors who have struggled in vain to make steel directly from English pig-iron, have been encouraged in their delusion by the teachings of high chemical authorities.

"If steel be regarded simply as iron carburised in degrees intermediate between malleable and cast-iron," these inventors are perfectly justified in seeking some substance which at the melting heat of cast-iron shall give off a definite quantity of oxygen; and they have logical grounds for believing that by bringing such a substance in contact with the molten cast-iron, and properly regulating its quantity, they may burn out just that surplus carbon which makes all the stated difference between cast-iron and steel. As a multitude of compounds when thus heated do give off oxygen, a vast field of effort is open, and accordingly every available peroxide and decomposable oxygen salt has been administered by strange devices to the melted iron, the same obvious substances used over and over again, and the same failures continually repeated by expectant inventors ignorant of what each other have done or are doing. Gas and vapours have been blown over the surface and under the surface, and through from bottom to top of melted cast-iron, and all (including Mr. Bessemer) have failed to produce merchantable steel from ordinary English cast-iron, without first making it into malleable or wrought-iron.

The reason of this is, that the removal of the surplus carbon is only a small portion of the work which has to be done in order to convert cast-iron into steel of any commercial value. Several other substances have to be removed also; *and no process has yet been discovered by which these impurities can be removed without at the same time removing the carbon in corresponding degree.* I put this in italics because I am convinced by experience of its great practical importance; because I do not find it clearly and distinctly enunciated in any general or special treatise; and further, because I have seen so plainly that the want of clearly understanding it is the rock upon which so many unfortunate inventors have split.

These inventors have not been informed with anything like the necessary degree of distinctness, that the Styrians and others who have made, or are making, steel directly from cast-iron, have started with a very different material to that which bears the same name of cast-iron in England; the difference being sufficiently great to alter totally the conditions of the problem. The cast-iron of the Styrian steel-makers is a nearly pure carburised iron; *our* cast-iron is a carburised, silicised, phosphurised, and sulphurised iron; *their* problem in steel-making is, merely the partial decarburisation of their cast-iron; *ours* is the total desilicisation, the total dephosphurisation, and the total desulphurisation in addition to this. Now, the partial removal of carbon from iron is one of the very easiest problems in practical metallurgy, while the complete removal of silicon, phosphorus, and sulphur, is among the most difficult.

To illustrate the grossness of the fallacy which represents the difference between cast-iron and steel as merely, or "essentially," due to carbon, I may state that on looking down a tabular statement of the analyses I have recently made of thirty brands of ordinary English pig-iron (excluding hæmatite pigs), I find that seven among them contain less than 2 per cent. of carbon, or an average of 1·77 per cent. Now this is below the percentage of carbon which exists in some of the finest and most expensive samples of cast-steel. Therefore, to convert these particular brands of cast-iron into the finest steel, the carbon must neither be increased nor diminished, and if, as Dr. Percy says, the differences between steel, wrought-iron, and cast-iron, "*essentially* depend upon differences in the proportion of carbon," all these brands of pig-iron should be described as steel rather than cast-iron.

Nevertheless they are utterly worthless for any of the purposes for which steel is used, and the common result of the costly experiments of the inventors who endeavour to make steel directly from English pig-iron, is to produce a material very much like them. They usually succeed perfectly in their effort partially to decarburise the pig iron. They take out, say one half of the carbon, and with it a considerable portion of the silicon, and *some* of the phosphorus, sulphur and manganese; but to make a *perfect* steel they must take out *all* of these latter, and leave nothing but pure iron and carbon. Absolute perfection is not, of course, practically attainable in steel-making, but it is approximated in exactly the same degree as the purification of the iron from everything excepting the carbon is effected.

The most notable modern attempt to produce steel directly by the simple decarburisation of English cast-iron was that of Mr. Bessemer. His first idea was to blow air through melted cast-iron, and thereby to oxidise the carbon, and then, when a sufficient degree of decarburisation was effected, to stop the blowing. He supposed that when by this means the proportion of carbon was reduced to about one and a half per cent. the result would be useful steel. He failed entirely in this; he never succeeded in producing merchantable steel from ordinary English cast-iron by this method.

The Bessemer process, as at present conducted, consists in first oxidising simultaneously all or nearly all the carbon and silicon, and then adding to the decarburised iron a new dose of carbon, by means of a known quantity of spiegeleisen of known composition; thus reverting to the old Sheffield principle of first bringing the cast-iron to the state of wrought or decarburised iron, and then adding carbon to convert it into steel.

It is commonly represented that the failure of the early attempts at direct steel-making by the Bessemer process arose simply from the difficulty of determining the right moment at which to stop the blow, and thereby to regulate the proportion of carbon; and that the whole advantage of the spiegeleisen is the means it affords of doing this. Dr. Percy says:—"In attempting to produce steel by the methods specified by Bessemer, it has hitherto been found very difficult, if not impracticable, *at least in this country*, to ascertain with certainty when decarburisation has proceeded to the right extent, and when therefore the blast should be stopped. Accordingly the plan now adopted is to decarburise perfectly, or nearly so, and then add a given proportion of carbon in the state in which it exists in molten spiegeleisen, the precise composition of which should of course be known."* Neither in Dr. Percy's nor any other account of the Bessemer process do I find that the necessity of complete decarburisation as a means of completely separating the silicon is fairly appreciated.

If merchantable steel could be made from English pig-iron by simply stopping the blow before complete decarburisation, Mr. Bessemer would surely have produced some good steel in the course of his long and costly efforts which preceded the idea of introducing the spiegeleisen, for it must be remembered that the quantity of carbon required in steel extends over a very wide range—that steel may contain from 0·40 to 2·00 per cent. of carbon, and that steel with every degree of carburisation within this wide range is in demand in the market at good prices, provided it be free from phosphorus, silicon, &c. Nothing is practically easier than to stop the blow at such a moment as shall ensure a degree of carburisation somewhere between this wide range; and there can be no doubt that, in his early experiments, Mr. Bessemer, like other inventors of direct processes, made an abundance of iron that was duly carburised within the above-stated limits, although he failed to produce useful steel.

Dr. Percy's qualification, "at least in this country," is rather curious. He has probably learned that steel has been directly made in Sweden (though he does not mention it in his work) by the Bessemer process, and he seems to attribute this to the superior ability of the Swedish operators, enabling them "to ascertain with certainty when decarburisation has proceeded to the right extent." I differ entirely from Dr. Percy in this conclusion, being convinced that Mr. George Brown, the manager of the Bessemer Department at the Atlas Works, Sheffield, who was the first to work the Bessemer process with commercial success, is better able (on account of his much greater experience and thorough knowledge of the work) than any of the Swedish manufacturers, to determine when any required degree of decarburisation has been attained. It is not the superior skill of the Swedish operators that has enabled them to make steel directly by the Bessemer process; but the fact that they, like the Styrian workers, used a very superior charcoal-iron to start with; and that the blowing out of all the carbon was not absolutely necessary for the sufficient purification of this quality of iron.

W. MATTIEU WILLIAMS

* "Metallurgy," "Iron and Steel," p. 814. The italics are my own.

ON THE NATURAL LAWS OF MUSCULAR EXERTION

THE experiments published by Mr. W. Stanley Jevons, in NATURE on the 30th June last, illustrate well two laws of muscular exertion which were established by experiments made by myself in 1862 and 1863. These laws may be thus stated:—

Law 1. The work given out by a single group of muscles, in a single contraction, is constant.

Law 2. When the same group of muscles is kept in constant action, the total work done by them until fatigue sets in, multiplied by the rate at which they are compelled to work, is constant.

Mr. Jevons' first series of experiments, in which different weights were thrown by the arm to various distances on level ground, illustrates the first law. In throwing weights in this manner, the arm, after a little practice, instinctively pitches the weight at the angle corresponding to the maximum range, and as the maximum range is proportional to the square of the velocity of projection, it may be used to replace that velocity squared, in estimating the work done by the arm.

The total work done is the same as if the weight used and the weight of the arm were concentrated at the centre of oscillation of the loaded arm, regarded as a compound pendulum.

Let us assume

w = weight held in hand;
x = weight of arm;
v = velocity of centre of oscillation.

By Law 1, the work done is constant and is represented by

$$(w + x)\, v^2 = \text{const.} \qquad (1)$$

Let

V = velocity of hand;
l = radius of oscillation;
a = length of arm.

then

$$v = V\frac{l}{a} \qquad (2)$$

It is easy to show (assuming the arm to be a uniform cylinder) that

$$\frac{l}{a} = \frac{2}{3} \cdot \frac{(3\,w + x)}{(2\,w + x)} \qquad (3)$$

By means of (2) and (3), equation (1) becomes

$$\frac{(w + x)\,(3\,w + x)^2}{(2\,w + x)^2} \times R = A; \qquad (4)$$

where R denotes the range (proportional to V^2) and A denotes a constant, if Law 1 be true.

Mr. Jevons' experiments give the following corresponding values of w and R.

w	R
56 lbs.	1·84 ft.
28 ,,	3·70 ,,
14 ,,	6·86 ,,
7 ,,	10·56 ,,
4 ,,	14·61 ,,
2 ,,	18·65 ,,
1 ,,	23·05 ,,
½ ,,	27·15 ,,

We are required to assign certain values to x and A, which will make equation (4) best coincide with the eight simultaneous values of w and R found by observation.

I find by trial that these values are

$$x = 8{\cdot}1 \text{ lbs.}$$
$$A = 262{\cdot}2.$$

If we solve equation (4) for R, we find

$$R = \frac{A\,(2\,w + x)^2}{(w + x)\,(3\,w + x)^2} \qquad (5)$$

Substituting for A and x in this equation their values above given, we can obtain by calculation the distances to which the weights should be thrown, according to Law 1.

We thus obtain the following comparison between theory and observation.

w	R (observed).	R (calculated).	Difference.
56 lbs.	1·84 ft.	1·90 ft.	−0·06 ft.
28 „	3·70 „	3·51 „	+0·19 „
14 „	6·86 „	6·06 „	+0·80 „
7 „	10·56 „	10·02 „	+0·54 „
4 „	14·61 „	13·90 „	+0·71 „
2 „	18·65 „	19·11 „	−0·46 „
1 „	23·05 „	23·85 „	−0·80 „
½ „	27·15 „	27·39 „	−0·24 „

The agreement here shown between observation and calculation founded on Law 1, is quite as complete as the agreement between observation and the empirical formula used by Mr. Jevons, which may be written, in the notation of the present paper, as follows :—

$$(2w + 7·8)\,R = 231·3. \qquad (6)$$

Mr. Jevons' third series of experiments consisted in holding various weights on the hand extended horizontally, and noting the time during which the weights could be so held. The following are the weights and times observed :—

w	t
18 lbs.	14·8 secs.
14 „	32·5 „
10 „	60·3 „
7 „	87·4 „
4 „	147·9 „
2 „	218·9 „
1 „	321·2 „

Omitting the first of these experiments I find that Law 2 satisfactorily accounts for the remaining six, and gives a constant, which is nearly identical with that obtained from my own experiments made in 1863.

When the arm is extended horizontally, if allowed to fall through an indefinitely small arc, the centre of oscillation falls like a free body under the influence of gravity, and the muscles then lift back the arm through the same arc, and this goes on continuously until the muscles are tired out.

Let us use the following notation :—

w and x are, as before, the weight held in the hand and the weight of the arm.

l = radius of oscillation ;
a = distance of centre of gravity of loaded arm from centre of shoulder joint ;
δs = small space through which the centre of oscillation falls ;
n* = number of such falls during
t = whole time required to fatigue the muscles.

The total work done by the muscles in the time t, is evidently

$$(w+x)\frac{a}{l}\,n\,\delta\,s;$$

but, $n\,\delta\,s$ varies as t, and, therefore, the total work done varies as

$$(w+x)\frac{a}{l}t.$$

The *rate* of work is evidently proportional to

$$(w+x)\frac{a}{l}$$

and since, by Law 2, the total work done before fatigue multiplied by the rate of work is constant, we obtain

$$(w+x)^2\frac{a^2}{l^2}t = \text{Const.} \qquad (7)$$

* I have ascertained the number n from acoustical observations made on the muscular *susurrus*.

And, since

$$\frac{a}{l} = \tfrac{3}{4}.\frac{(2w+x)^2}{(w+x)(3w+x)}, \qquad (8)$$

we find, by substitution,

$$\frac{(2w+x)^4}{(3w+x)^2}t = a. \qquad (9)$$

This equation (9) is the statement of Law 2, as applied to Mr. Jevons' experiments ; and we are required to find values for x and a, which will make equation (9) best correspond with the given observations.

I find, by trial, that the following values will answer best :—

$$x = 7·4 \text{ lb.}$$
$$a = 22{,}050.$$

If we solve equation (9) for t, we find

$$t = A\frac{(3w+x)^2}{(2w+x)^4}. \qquad (10)$$

From this equation, substituting the values of x and A, we obtain the following comparison of observation and theory :

w	t (observed).	t (calculated).	Difference.
14 lbs.	32·5 secs.	34·2 secs.	− 1·7 secs.
10 „	60·3 „	54·7 „	+ 5·6 „
7 „	87·4 „	84·8 „	+ 2·6 „
4 „	147·9 „	147·6 „	+ 0·3 „
2 „	218·9 „	234·4 „	− 15·5 „
1 „	321·2 „	305·5 „	+15·7 „

This comparison is very satisfactory, the differences being much less than possible errors of observations. Mr. Jevons' experiments further show that the *useful effect* has a maximum corresponding to a certain weight. This weight, which gives the maximum of useful effect, may be readily calculated from Law 2.

By equation (10), the useful effect is

$$w\,t = A\,.\frac{w\,(3w+x)^2}{(2w+x)^4}. \qquad (11)$$

This will be a maximum, when

$$(2w+x)\,(9w+x) = 8\,w\,(3w+x)\,;$$

or when

$$6w^2 - 3\,x\,w - x^2 = 0\,;$$

or when

$$w = \frac{3+\sqrt{33}}{12}x; \qquad (12)$$

or,

$$w = 0·73\,x.$$

Substituting for x its value 7·4lb., we find for the weight that gives the maximum useful effect,

$$w = 5·40\text{lb.}$$

The useful effect observed by Mr. Jevons was as follows :

w	Useful effect
18 lbs.	266
14 „	455
10 „	603
7 „	612
4 „	592
2 „	438
1 „	321

The actual maximum corresponds to 5·4lb. lying between 7lb. and 4lb.

I may observe, in conclusion, that the difference of weights x of the arm, found in the two sets of experiments is quite natural.

In the experiments in which the arm was held out horizontally, its weight, 7·4lb., is the weight of the arm below the centre of the shoulder joint.

In the experiments in which the weights are thrown by the arm, a portion of the shoulder blade is in motion, in addition to the simple arm, and the total weight becomes 8·1lb.

SAMUEL HAUGHTON

PROFESSOR ABEL'S CONTRIBUTIONS TO THE HISTORY OF EXPLOSIVE AGENTS *

THE degree of rapidity with which an explosive substance undergoes metamorphosis, as also the nature and results of that metamorphosis, are, in the greater number of instances, susceptible of several modifications by variations of the circumstances under which the conditions essential to chemical change are fulfilled. Gun-cotton furnishes an excellent illustration of the manner in which such modifications may be brought about. If a loose tuft or large mass of gun-cotton-wool be inflamed in open air by contact with, or proximity to, some source of heat, the temperature of which is about 135° C. or upwards, it flashes into flame with a rapidity which appears almost instantaneous, the change being attended by a dull explosion, and resulting in the formation of vapours and gaseous products, of which nitrogen-oxides form important constituents. If the gun-cotton be in the form of yarn, thread, woven fabric, or paper, the rapidity of its inflammation in open air is reduced in proportion to the compactness of structure or arrangement of the twisted, woven, or pulped material; and if it be converted by pressure into compact masses, solid throughout, the rate of its combustion will be still further reduced. If to a limited surface of gun-cotton, when in the form of a fine thread or of a compactly pressed mass, a source of heat is applied, the temperature of which is sufficiently high to establish the metamorphosis of the substance but not adequate to inflame the products of that change (carbonic oxide, hydrogen, &c.), the rate of burning is so greatly reduced that the gun-cotton may be said to smoulder without flame; the reason being that the products of change, which consist of gases and vapours, continue, as they escape into air, to abstract the heat developed by the burning gun-cotton so rapidly that it cannot accumulate to an extent sufficient to develop the usual combustion, with flame, of the material. For similar reasons, if gun-cotton be kindled in a rarefied atmosphere, the change developed will be slow and imperfect in proportion to the degree of rarefaction, so that, even if an incandescent wire be applied, in a highly rarefied atmosphere, to the gun-cotton, it can only be made to undergo the smouldering combustion, until the pressure is sufficiently increased by the accumulating gases to reduce very greatly the rate of abstraction, by these, of the heat necessary for the rapid combustion or explosion of the substance. If, on the contrary, the escape of the gases from burning gun-cotton be retarded, as by enclosing it in an envelope or bag of paper, or in a vessel of which the opening is loosely closed, the escape of heat is impeded until the gases developed can exert sufficient pressure to pass away freely by bursting open the envelope or aperture, and the result of the more or less brief confinement of the gases is a more rapid or violent explosion, and consequently more perfect metamorphosis of the gun-cotton. So, within obvious limits, the explosion of gun-cotton by the application of flame or any highly heated body is more perfect in proportion to the amount of resistance offered in the first instance to the escape of the gases; in other words, in proportion as the strength of the receptacle enclosing the gun-cotton, and the consequent initial pressure developed by the explosion, is increased. Hence, while gun-cotton has been found too rapid or violent in its explosive action when confined in guns, and has proved a most formidable agent of destruction if enclosed in metal shells or other strong receptacles, it has hitherto been found comparatively harmless as an explosive agent if inflamed in open air or only confined in weak receptacles. Modifications, apparently slight, of the manner in which the source of heat is applied to explosive agents, when exposed to air under circumstances in other respects uniform, suffice to modify the character of their explosions in a remarkable manner. Thus a modification of the position in which the source of heat is placed with reference to the body of a charge of gunpowder, which is only partially confined, suffices to alter altogether the character of the explosion produced.

The product of the action of nitric acid upon glycerine, which is known as nitroglycerine or glonoine, appears to be susceptible of only two varieties of decomposition. If a sufficient source of heat be applied to some portion of a mass of this liquid in open air, it will inflame and burn gradually without any explosive effect; and even when nitroglycerine is confined, the development of its explosive force by the simple application of flame or of other sources of heat, by the ordinary modes of operation, is difficult and very uncertain. But if the substance be submitted to a sudden concussion, such as is produced by a smart though not very violent blow from a hammer upon some rigid surface on which the nitroglycerine rests, the latter explodes with a sharp detonation, just as is the case with gun-cotton. Only that portion of the explosive agent detonates which is immediately between the two surfaces brought into sudden collision; the confinement of this portion between the hammer and the support, combined with the instantaneous decomposition of the portion struck, prevent any surrounding freely exposed portions from being similarly exploded by the detonation. A similar result is obtained if any explosive compound or mixture be submitted to a sufficiently sharp and violent blow, but the tendency of surrounding particles to become inflamed by the detonation is in direct proportion to the rapidity of explosive action of the substances. The practical difficulties and uncertainty which attend attempts to develop the explosive force of nitroglycerine by the agency of flame or the simple application of any highly heated body, even when the material is confined in strong receptacles (such as iron shells or firmly tamped blast-holes), appeared fatal to any useful application of the powerful explosive properties of this substance, until M. Alfred Nobel's persevering labours to utilise nitroglycerine, eventually resulted in the discovery of a method by which the explosive power of the liquid could be developed with tolerable certainty. M. Nobel first employed gunpowder as a vehicle for the application of nitroglycerine. By impregnating the grains of gunpowder with that liquid, he added considerably to the destructive force of the powder when exploded in the usual way in closed receptacles. M. Nobel's subsequent endeavours to apply nitroglycerine *per se* were based upon the belief that its explosion might be effected by raising some portion of a quantity of the liquid to the temperature necessary for its violent decomposition, whereupon an initiative explosion would be produced which would determine the explosion of any quantity of the substance.

The circumstance that nitroglycerine, or any preparation of that substance, may be violently exploded when freely exposed to air, by the explosion in contact with it of a small confined charge of gunpowder, or of a detonating substance, while other modes of explosion by the application of heat or flame, which have been described by M. Nobel, only develop explosion under special conditions, points to a decided difference between the action of the two modes of ignition, and appears to indicate that it is not simply the heat developed by the chemical change of the gunpowder or detonating powder which determines the explosion of the nitroglycerine. An experimental investigation of this subject has left no doubt on my mind that the explosion of nitroglycerine through the agency of a small detonation is due, at any rate in part, to the mechanical effect of that detonation, and that this effect may operate in exploding the nitroglycerine, quite independently of any direct action of the heat disengaged.

The readiness and certainty with which gunpowder, gun-cotton, and other explosive substances may be detonated through the agency of a blow from a hammer or a falling body, are regulated by several circumstances; they are in direct proportion to the weight of the falling body, to the height of its fall, or the force with which it is impelled downwards, to the velocity of its motion, to the mass and rigidity, or hardness, of the support or anvil upon which the body falls; to the quantity and mechanical condition of the explosive agent struck, and to the ready explosibility of the latter. Thus a sharp blow from a small hammer upon an iron surface will detonate gunpowder with very much greater certainty than the simple fall of a heavy hammer, or than a comparatively weak blow from the latter. It is very difficult, by repeated blows applied at very brief intervals, to ignite gun-cotton, if placed upon a support of wood or lead, both of which materials yield to the blow, the force set into operation by that blow being transferred through the explosive agent and absorbed in work done upon the material composing the support. If, however, the latter be of iron, which does not yield permanently to the blow of the hammer, the detonation of these substances is readily accomplished. If the quantity of explosive agent employed be so considerable as to form a thick layer between the hammer and support, the force applied appears to be to so great an extent absorbed in the motion imparted to the particles of the comprehensible mass, that its explosion is not readily accomplished; and if the material be in a loose or porous condition (as, for example, in a state of powder or of loose wool), much work has to be accomplished in moving particles of the mass through a comparatively considerable space,

* An abstract of a paper, by Prof. Abel, F.R.S., Chemist to the War Department, in the Philosophical Transactions.

and a second or even third blow is therefore required to determine its explosion.

These circumstances would appear to afford support for the belief that the detonation of an explosive material through the agency of a blow is the result of the development of heat sufficient to establish energetic chemical change, by the expenditure of force in the compression of the material, or by the friction of the particles against each other, consequent upon a motion being momentarily imparted to them. It is conceivable that, from either of these causes, sufficient heat may be accumulated with almost instantaneous rapidity, in some portion of the mass struck, to develop sudden chemical change. The circumstance that the detonation of those portions of an explosive compound (such as gun-cotton or nitroglycerine) which are immediately between the surfaces of the hammer and the support is not communicated to the surrounding portions, may be ascribed to a combination of two causes, the instantaneous nature of the explosion, and the close confinement of the portions struck at the instance of their explosion. The mechanical effect of the detonation is absorbed by the masses of metal between which it occurred, and the gases developed disperse the surrounding portions of the explosive agent, as they rush away from between the two surfaces. It is possible also to detonate gunpowder and other explosive mixtures by a blow in such a manner that only the portions immediately struck are ignited; but those substances may also be exploded, though much less violently, by a less sudden or powerful application of force, in which case they detonate much more feebly; their explosion is accompanied by a larger volume of flame, and by the ignition of those portions which surround the part struck by the hammer. The power of accomplishing the explosion or detonation of gun-cotton or nitroglycerine in open air through the agency of a detonation produced in its vicinity, would therefore appear to be correctly ascribable to the heat suddenly developed in some portion of the mass by the mechanical effect, or blow, exerted by that detonation, and would seem to be regulated by the violence and suddenness (either singly or combined) of the detonation, by the extent to which the explosive material is in a condition to oppose resistance to the force, and by the degree of sensitiveness of the substance to explosion by percussion. There are, however, several well-known facts, and some results of experiments instituted with special reference to this subject, which do not appear to be in harmony with the assumption that the detonation of nitroglycerine and gun-cotton in the manner described is simply due to the *suddenness* of the development and application of physical force.

With the view of ascertaining whether the relative power of different explosive agents to accomplish the detonation of gun-cotton appears to be in direct proportion to the relative mechanical effects of their explosion (*i.e.* to the work performed by them upon a body placed in contact with them), a series of experiments was instituted with the object of comparing this particular action of the several explosive materials. It would appear from these experiments that, when unconfined, the violence of explosion of chloride of nitrogen is less than that of the iodide, and that, if confined under water, it very considerably exceeds that of the exposed iodide, but falls very short of that exerted by unconfined silver-fulminate. It also appears that the mercuric fulminate, which is much less rapidly explosive than either of the other substances, exerts less mechanical force than any of them, if freely open to air, and if inflamed at some portion of the exposed surfaces; if ignited at the lower inner portion of the mass, where the part first inflamed is enclosed by the mass of the material itself, it exerts a destructive force little inferior to that of the chloride of nitrogen enclosed by water; but if confined in a strong envelope (*e.g.* of sheet tin), the mercuric fulminate is greater in violence of action than the unconfined silver-fulminate. These results to a great extent confirm the correctness of the view that the readiness with which the detonation of gun-cotton is accomplished is in proportion to the mechanical force exerted by the initiative detonation to which it is subjected. The force exerted by small quantities of strongly confined silver and mercuric fulminate greatly exceeds that developed by the explosion of comparatively large proportions of the iodide and chloride of nitrogen. This may be accepted as accounting, to some extent, for the fact that the detonation of gun-cotton could not be accomplished by an amount of iodide of nitrogen twenty times greater than that of fulminates required for the purpose, while ten times the quantity of the confined chloride were required to produce the result. That the quantity of mercuric fulminate required to produce detonation is reduced in proportion as means are applied to increase the violence of the force exerted by it at one time, is quite in accordance with the above view.

I venture to offer the following as being the most satisfactory explanation which occurs to me of the remarkable differences exhibited in the behaviour of different explosive agents. The vibrations produced by a particular explosion, if synchronous with those which would result from the explosion of a neighbouring substance which is in a state of high chemical tension, will, by their tendency to develop those vibrations, either determine the explosion of that substance, or at any rate greatly aid the disturbing effect of mechanical force suddenly applied, while, in the case of another explosion which produces vibrations of different character, the mechanical force applied by its agency has to operate with little or no aid; greater force or a more powerful detonation must, therefore, be applied in the latter instance, if the explosion of the same substance is to be accomplished by it.

In conclusion, it may not be out of place to refer briefly to a few illustrations of the important bearings which the new mode of developing the explosive force of gun-cotton has upon the practical uses of the material as a destructive agent. The confinement of a charge of gun-powder or gun-cotton in a blast-hole, by firmly closing up the latter with earth, powdered rock, or other compressible material (by the process known as tamping or stemming) to a depth greater than the line of least resistance opposed to the action of the charge, is essential to the success of a blasting operation; but the great rapidity of explosion, by detonation, of a charge of gun-cotton greatly reduces the value of this operation; the destructive effect of the material, when exploded in a hole which is left open, is not inferior in extent to that obtained by similarly exploding a charge confined in the usual manner. Thus the most dangerous operation in connection with blasting may be entirely dispensed with. In submarine operations, it is no longer necessary to enclose the charge of explosive agent in the strong and therefore cumbersome metal receptacles hitherto required to ensure the full development of its explosive force; the destructive action of a charge of gun-cotton, enclosed in a waterproof bag or thin glass vessel and exploded by detonation, being decidedly greater than that furnished by a corresponding charge confined in a strong iron vessel and exploded by flame. Small charges of gun-cotton simply resting upon the upper surfaces, or loosely inserted into natural cavities, of very large masses of the hardest description of rock or of iron, have broken these up as effectually as if corresponding charges had been firmly imbedded in the centre of the mass and exploded in the usual manner. Lastly, the certainty, facility, and expedition with which certain important military destructive operations may be accomplished by means of gun-cotton exploded by detonation, are not among the least important advantages which are now secured to this interesting and remarkable explosive agent.

SCIENTIFIC SERIALS

POGGENDORFF'S *Annalen der Chemie und Pharmacie*, vol. cxl. part 1.—This number contains (1) the first part of an elaborate paper by E. Ketteler, "On the Influences of Ponderable Molecules on the Dispersion of Light, and on the Signification of the Constants in the Mathematical Formulæ for Dispersion" (pp. 1 to 53). This is a critical examination, based chiefly on Mascart's experimental measurements, of the formulæ by which Cauchy and others have endeavoured to connect the indices of refraction of the various kinds of light with their wave-lengths. The nature and scope of the investigation may be gathered from the four following criteria which the author gives as the tests of a satisfactory formula:—"1. A rational formula must enable us to calculate accurately from their wave-lengths the succession of the several colours and their distribution in space, for the whole measured extent of radiation, for some definite density of the dispersive medium. 2. The constants of the formula must be capable of a distinct physical interpretation, analogous to the interpretation assigned by Cristoffel to the constants in his formula. 3. When the density of the dispersive medium is altered, these constants must participate in the change of molecular constitution in some simple manner, corresponding to what has been ascertained in respect to them in the case of gaseous media. 4. Consequently, as the medium approaches the limit of rarefaction, all the indices must approach unity as their limiting value." The author finds that none of the formulæ hitherto proposed reproduce the experimental results within the limits of error of the measurements, but that this can be done by a formula which he proposes. (2.) "On the Sounds produced by Heated Tubes, and on the Vibrations of Air in Pipes of various Forms," by C. Sondhaus (pp. 53 to 76). Many experimenters must have observed the frequent production of a musical tone when a bulb has been blown at the end of a rather short and narrow glass tube, the sound beginning just as the tube with the still hot bulb is removed from the lips. This phenomenon formed the subject of an investigation by the author twenty years ago, and he now returns to it in a paper which is to be concluded in the next number of the *Annalen*. The principal result which he now publishes is that when the dimensions of bulb and tubes are properly proportioned, similar tones can be obtained with heated glass bulbs from which two open tubes proceed in opposite directions. He also gives an empirical formula which expresses approximately the pitch of the tones obtained in terms of the dimensions of the bulbs and tubes; but as this formula does not seem to be based on any physical explanation of the way in which the sounds are produced, and as it takes no account of temperature, the agreement between its results and those of observation must be considered as at least to some extent accidental. Perhaps the remainder of the paper may give further explanations on these points. (3.) "On Chromates," by C. Freese (pp. 76 to 88), to be concluded in the next part. (4.) "Thermo-chemical Investigations" (continued), by Julius Thomsen (pp. 88 to 114). This section of Professor Thomsen's researches relates to the acids of nitrogen, phosphorus, and arsenic. The thermo-chemical behaviour of these acids, when neutralised with caustic soda, appears to agree in the main with the commonly-received views of their basicity founded upon their chemical properties. (5.) "Further Researches into the Development of Electromotive Force between Liquids," by Jacob Worm-Müller (pp. 114 to 144). Among other results the author arrives at the following remarkable conclusion: "Solutions of acids and alkalis in equivalent proportions (that is such that equal volumes of the solutions neutralise each other) and of the salts formed by mixing equal volumes of these solutions, do not give rise to electric currents when connected so as to form a circuit." This paper also is to be concluded in the next number of the *Annalen*. (6.) "Researches in Electrical Dust-figures," by Wilhelm von Bezold (pp. 145 to 159). (7.) "On the Law of Formation of Kundt's Dust-figure," by Theodore Karrass (pp. 160 to 168). (8.) "On an Electrophorus-machine for Charging Batteries," by Peter Riess (pp. 168 to 172). The author describes a modification of Holtz's electrical machine, which renders it applicable for charging Leyden batteries to a high tension. (9.) "On the Measurement of the Absorption of Light by transparent media by means of the Spectroscope," by C. Vierordt (pp. 172 to 175). The author's method of measurement consists essentially in diminishing the intensity of each part of a normal spectrum, by means of smoked glasses of known absorptive power and the partial closing of the slit of the spectroscope, until it is identical with that of the light transmitted by the medium to be examined. (10.) "An Observation on the Induction-spark," by Dr. A. Weinhold (p. 176).

In the *Journal of Botany* for August, the original articles relate almost entirely to extra-English botany, with the exception of the conclusion of Mr. Worthington Smith's *Clavis Agaricinorum*, which forms an important contribution to the literature of cryptogamic botany.

In the *Proceedings of the Asiatic Society of Bengal* for June are three articles on the Andamanese, the most important of which is by Surgeon Francis Day. He estimates the number now living on the island as probably not much over 1,000, divided into several tribes, which have distinct dialects, so that members of the Little Andamans are scarcely able to understand those of the South Andamans. Their language is very deficient in words; many English and Hindustani words are now beginning to be incorporated in it; numerals are entirely absent. They are anything but prolific, and appear to be gradually dying out from excess of deaths over births. Mr. Day only saw one woman who had as many as three living children; during one year thirty-eight deaths were reported, and only fourteen births among the families living near the European settlements; few appear to live to a greater age than forty, and they are subject to a variety of diseases. We hope to return to this article again. Dr. G. von Martens contributes "Notes on some Javanese Algæ." The remaining articles in the number are philological.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, August 8.—Papers were read on the relation between the specific heats and the coëfficients of dilatation of any body, by M. Phillips, and on the decimal division of the quadrant, by M. A. d'Abbadie, in which he communicated two letters on the subject from M. Nadau and Prof. Airy; and MM. Jamin and Richard contributed some observations on the determination of the relation between the two specific heats of gases.—M. Jamin replied to the two notes by M. Sainte-Claire Deville on July 18th, and entered again at length into the subject of the variations of temperature produced by the mixing of two liquids.—M. Laborde contributed a note on some new experiments on Holtz's electrical machine.—M. Elie de Beaumont presented, on behalf of M. Delesse, a lithological map of the embouchure of the Seine.—A note by MM. Rabuteau and Peyré was presented by M. Ch. Robin, on the poisonous effects of the m'boundou or icaja, a poison used at the Gaboon. The poison used was extracted chiefly from the bark, a small quantity also from the root. The experiments showed that the poison is extremely rapid; but that its fatal effects can be prevented by artificial respiration; the symptoms are in some respects similar to those produced by strychnine.—A letter was read from M. Lichtenstein to M. Dumas, on a means of preventing the irruption of the *Phylloxera vastatrix* in vines not yet attacked. The proposed plan is simply by destroying carefully, from May to August, all the branches on which the winged form of the insect has made its appearance.—A short note was also presented by M. L. Laliman, on a variety of vine (of the American species *V. æstivalis*) not subject to the attacks of the *Phylloxera*.

BOOKS RECEIVED

ENGLISH.—Lectures on Art: J. Ruskin (New York: Wiley and Son).—The Laws of Verse: J. J. Sylvester (Longmans).—The Wind in his Circuits: R. H. Armit (J. D. Porter).—Matter for Materialists: T. Doubleday (Longmans).—The Book of the Roach; G. Fennell (Longmans).

FOREIGN.—(Through Williams and Norgate)—Etudes sur la maladie des vers à soie: L. Pasteur.—Streifzüge (landwirthschaftliche) in Frankreich u. Algerien im Jahre 1862-68: A Petzhold.—Leçons de Chimie, années 1868-69, Dehérain, &c.—Mineralogie der Vulcane: Dr. C. Landgrebe.—De l'enseignement supérieur en Angleterre et en Ecosse: J. Demoycot.—Zonula ciliaris: Dr. F. Merkel.—Deutsche Vierteljahrsschrift für öffentliche Gesundheitspflege: C. Reclam.—Prodromus Floræ Hispanicæ, Vols. 1 and 2: M. Willkom.—Algæ japonicæ Musæi botanici Lugduni-Batavi: W. F. R. Süringer.—Die Osteologie und Myologie von Sciurus vulgaris: C. K. Hoffman.

CONTENTS

ERRATA.—Page 296, first column, lines 10 and 11 from bottom, for "blosz Erscheinung" read "blose Erscheinung."—Page 308, first column, line 14, for "Phylloscerae" read "Phylloxeræ"

THURSDAY, AUGUST 25, 1870

SCIENCE AND MILITARY SURGERY

IT is matter of no small interest at the present time to know something of the scientific position of our Army Medical Service. The question has two aspects—first, the purely professional and technical; and second, the general and scientific. In former times sick and wounded soldiers in all services had inadequate care bestowed on them, but for many years past the advance of humane principles, and improvements in education and in all manner of appliances, have been gradually making way in different European armies; and at this time we are presented with the astonishing spectacle of distinct corps of men and women, many of them of noble and gentle birth, following the example first set by Florence Nightingale, leaving their families and homes to accompany armed hosts to the battle field—their lives considered sacred by both sides—with the single object of conveying away poor wounded men as speedily as possible to shelter and to surgical and nursing care.

It would not be difficult to trace this great movement to the disasters of the British army during the Crimean war; and to the same event we have been indebted for the striking scientific advance in our own army medical service. We have been led into making these remarks by a perusal of the Army Medical Department Report for 1868. This is the tenth volume of an interesting series of official documents, the commencement of which dates from the reforms introduced by the late Lord Herbert.

The present volume maintains the reputation already gained by its predecessors, and even for non-professional readers it affords information of much general interest. A brief sketch of its contents will be sufficient for the object we have in view.

In turning over its pages we find in the first place the medical statistics of the whole British army. Wherever Her Majesty's troops are stationed, there the statistical officer is at work collecting and registering facts, which show not only the state of health at each station, but the diseases incidental to countries and climates the most diverse.

The death rates, which for foreign stations include deaths among invalids, show a large reduction over those which ruled in former years, but in many cases they are considerably too high, and indicate the necessity for increased sanitary precautions. The greatest reductions have been effected in India, where for the first half of the present century the rates averaged no less than 69 per 1000. In 1868 the death-rate was 21·7 per 1000.

The reports from stations are accompanied by sanitary notes, in which the reasons for these death rates are more or less discussed, and there are valuable reduction tables showing the influence of age, locality, &c., on the rates.

The net results of the Abyssinian expedition are given as follows:—The aggregate strength of British troops sent to Abyssinia at various dates was 4,208. There were 42 deaths in Abyssinia, and 12 among invalids in England, making 54 in all; equal to 12·8 per 1000. There were 12 admissions but no deaths from wounds. The total loss arose from disease and accident, the latter cause occasioning 14 deaths.

Among the scientific papers is one by Dr. Wright, giving an account of experiments on the ventilation of a barrack-room, and affording an insight into the nature of solid particles floating in impure air, which particles formed the subject of Prof. Tyndall's lecture on "Dust and Disease," at the Royal Institution, last spring.

The presence of dust particles in the air of the Royal Institution was shown by their reflecting a strong light thrown on them. In Dr. Wright's experiments made at Netley Hospital, 26 cubic feet of air were drawn through an aspirator, and the suspended matters in this air were condensed, and gave the following results under a ⅛th inch object glass:—

Cotton fibres.
Starch granules.
Crystalline substances, sand, or dust.
Vegetable tissues of various sorts.
Pollen.
Amorphous Molecules (? Detritus of epithelium).
Indefinite filaments.
Minute moving particles (? Zoospores).

It is important to point out that these substances were detected in the air of a sleeping-room occupied by 11 men, where each man had 828 cubic feet of air space, the air of which was renewed upwards of four times every hour. The carbonic acid ratio in the outer air was ·393 per 1000 volumes, and in the room air, ·643 per 1000 volumes. The practical result being that for some reason the actual ventilation of the room was very defective, although the quantity of air ought to have been more than sufficient.

Dr. Parkes has supplied an interesting paper on Dr. Hassall's Flour of Meat, giving an experimental account of its effect as a diet on two healthy persons. The object was to ascertain whether it could be used by soldiers, and to what extent. The results arrived at are what might have been expected, viz., that nitrogenous foods of this class are, by themselves, insufficient for purposes of health and nutrition; while, as Dr. Parkes tells us, "the effect on each gentleman of the addition of other articles of diet (vegetables, fat, and a little more starchy food) was described by both as perfectly marvellous." This result gives the clue to the proper manner of using prepared foods of this class.

Dr. Parkes likewise furnishes a report on the progress of hygiène for the year 1869, in which a useful digest is given of the leading contributions made to this important subject in various countries. One important scientific result of painstaking inquiries into the relation of cattle diseases to specific fungi, carried out in America, is stated as follows:—"It will thus be seen that the authors trace all the forms of fungi seen in the blood and fluids in the pleuro-pneumonia, or splenic disease of cattle, to common forms, and that they entirely differ from Hallier on this point."

Hallier has fared no better with his theory of specific cholera fungi at the hands of Drs. Lewis and Cunningham, the two observers appointed by the Indian Government to examine into the question in India. These gentlemen furnish a paper on this subject, in which they state that up to the time of report the fungi supposed to be peculiar to cholera belong to common forms.

There is a curious paper by Dr. Smith on the notorious Delhi boil, in which he goes a long way in connecting this disease with impure water used for ablution purposes by the troops. He shows that dogs who drink this water get boils on their noses, while human beings are affected at the points where the skin is rubbed in the process of ablution. Microscopic preparations of the boil appear to show the presence of ova of distomata similar to those detected long ago in London waters by Dr. Hassal.

Professor Longmore furnishes some valuable remarks on the Prussian arrangements now in actual operation for transport of wounded in time of war. These remarks have a special interest at the present time, and they are illustrated by Professor Longmore's own personal experience. Ordinary railway-carriages and goods-waggons have been selected by the Prussians for the conveyance of wounded by rail. In ordinary fourth-class carriages hooks are screwed into the opposite sides of the carriage, and the field-stretchers with wounded are carried inside and suspended by elastic rings on the hooks. The operation is facilitated by the lower class carriages having a door at each end. When goods-waggons are used, the stretchers are either suspended or, which is far better, they are placed on poles laid on semi-elliptical steel springs inserted in the floor of the waggon. Professor Longmore prefers the arrangements in the fourth-class carriages, as being easier in practice.

There is in the Prussian army a complete organisation of medical officers, bearers, stretchers, field ambulances, &c., for collecting the wounded, dressing them on the spot, and conveying them either to the railway or to hospital, where, as we are glad to learn, they are now meeting with every care and kindness at the hands of humane men and women, whose motto may well be that adopted by one of the societies—"Point d'ennemis pour nous." * After describing the exercises he witnessed, Professor Longmore very properly suggests whether we in this country might not do something in the way of organising a suitable ambulance corps? This was one of Lord Herbert's proposed reforms, but we are afraid little has been done in giving effect to it.

Another subject discussed is the method adopted for identifying the bodies of the killed by means of tickets attached to the clothes or worn round the neck.

There are several other papers, including monthly meteorological abstracts for stations scattered over the whole British Empire, which we should have gladly noticed had our space permitted. But we have said enough to show that, in scientific advantages, the Army Medical Department, with its efficient school at Netley, stands second to none in Europe. It is for those who have the direction of the army to see that there is an organisation provided to give practical effect to it in the field where its services are most required.

* Contributions in aid of this great work may be sent to the National Society for the Aid of Sick and Wounded, 2, St. Martin's Place, Trafalgar Square, S.W.

THE ANNUAL REPORT OF THE ROYAL COLLEGE OF SURGEONS

FOR some years past it has been a custom at the College of Surgeons for the Conservator to collect the various specimens that have been mounted during the preceding twelve months into one room; enabling not only the Museum Committee, but the members of the medical profession, or visitors introduced by them, to see at a glance the additions that have been made during that period. We consider the plan to be an excellent one. It is a powerful incentive to the Conservator to work so that each year's results may surpass the previous one; whilst it calls forth gifts from those who have the opportunity of obtaining rare or valuable specimens, when they see what loving care and diligence are spent on their preparation and exhibition, and to how large a number they afford instruction. We had recently an opportunity of minutely inspecting these additions, and must express our warm admiration at their number and beauty. The Museum, as every naturalist knows, was commenced by the genius of Hunter, who, recognising the value that would attend the comparison of the same organ in the different groups of animals in enabling us to acquire precise knowledge of its function, and to penetrate the mysteries of disease, collected from all quarters typical specimens which he carefully dissected and described; but worker as he was the preparations he left have constituted but landmarks for the direction of succeeding observers. Although neither his time, strength, nor opportunity permitted that he should bring home more than a few examples displaying the wondrous fertility of the new region he had discovered, his success stimulated others to do their utmost. Preparation after preparation of every organised being that could be obtained by purchase or gift was rapidly added, and many times it has been found necessary to enlarge the receptacle for the sake of the new and important preparations that had been obtained, till at length it has attained its present lordly dimensions, and stands without a rival in the world. Nothing, perhaps, could give such an idea of the vast increase it has undergone —which would surely have well pleased its founder, could he have seen how his small though valuable beginnings had increased and multiplied—as the fact that a roomful of preparations that would handsomely furnish forth an entire country museum, is year by year absorbed into it, and scarcely appreciably augments its size.

The additions are divided into six classes—1, The Pathological Collection; 2, The Osteological; 3, The Physiological; 4, The Teratological; 5, The Dermatological; and 6, Anatomical preparations. The first of these has received many additions, and in particular one very important one, in which the carotid and subclavian arteries were tied by Mr. C. Heath for aneurism, and in which life was preserved for four years, and would probably have been considerably prolonged but for the extremely unsteady habits of the patient. The duration of life after the operation has permitted the collateral circulation to be fully established, and all the parts have been beautifully dissected out by Mr. Mosely.

In regard to the osteological collection, a large collection of ancient and modern Italian and Greek skulls has been purchased from the well-known Italian ethnologist, Dr. G. Nicolucci, of Isola di Sora. The number of these skulls was 166, and the entire number in the museum now amounts to 795, the great proportion of them being well authenticated and characteristic examples.

It is one thing, however, to have fine specimens, and another to display them to the best advantage; and often the chief value of a specimen, or even of a collection, is spoiled by the slovenly manner in which the mounting is effected, or the imperfect way in which they can be examined. The difficulties which have hitherto lain in the path of osteological investigation of the skeletons of different animals, have been admirably overcome by the workmen under Mr. Flower's direction. The skeletons are mounted upon very light frames of iron, and the limbs are so articulated with the body as to be removeable on the extraction of a single rivet, and their several segments can be detached with equal facility. The head can be removed, and even its interior be examined, whilst the several vertebræ can be separately taken off without disturbing the position and arrangement of the skeleton generally. The advantages of this mode of mounting for the purposes of comparison and investigation to the real worker are simply incalculable. The mode in which the preparing has been done reflects the highest credit on Mr. Mosely and those who assisted him. We must call attention in particular to a wonderful skeleton of a pike, weighing 32lb., in which every bone has been cleaned and re-attached with wonderful dexterity. The fish was presented by Mr. Petre, of Westwick, Norwich, at the instance of Mr. Frank Buckland. The council of the Zoological Society have given a very fine adult specimen of the recently discovered long-tailed Chinese deer (*Elaphurus davidianus*), with one of the very rare and remarkable South African "Aard wolf," or *Proteles*.

Mr. Flower is gradually performing a great service to all comparative anatomists by carrying out the original idea of Hunter, and placing side by side the same organ as it presents itself in a great variety of animals. By such a method many points are seized in a moment, which it is impossible for the most careful describer to render into words, or for the most diligent reader to grasp, whilst likenesses and correspondences hitherto unrecognised everywhere make themselves apparent. This year we observe that a large number of specimens of the intestinal organs and of the larynx have been mounted, the plan pursued with the latter organ being similar in all; on one side the bones, cartilages, and ligaments being displayed, whilst on the other the muscles are exquisitely dissected.

The Teratological Division, or that treating of malformations and monstrosities, has scarcely received the scientific attention it deserves, whilst the specimens that have accumulated in the College are very numerous, and we are glad to observe that the work of their arrangement has been entrusted to so laborious and intelligent a worker as Mr. B. T. Lowne, whose work on the Blowfly is, we have no doubt, in the hands of many of our readers.

In regard to the Dermatological collection it may be remarked that the past year has been signalised by the institution of what may be termed an entirely new department of the collection; for such illustrations of diseases of the skin as the Museum formerly contained were very limited in number, and were incorporated in the general Pathological series. Moreover, the great majority of the morbid appearances presented by the skin cannot be shown in an anatomical museum by actual specimens, but recourse must be had to models and drawings to perpetuate and illustrate their characters, and no collection of such objects had hitherto been formed in the College.

When the Professorship of Dermatology was founded and endowed last year by Mr. Erasmus Wilson, it appeared necessary that the means of illustrating the lectures should also be provided; and for this purpose, as well as for the general advancement of the study of the subject, Mr. Wilson has presented to the College an extensive collection of drawings, casts, and models of cutaneous diseases, the greater proportion of the latter having been recently executed with great artistic excellence and fidelity by M. Baretta from patients in the Hospital St. Louis at Paris.

In order to provide space for the exhibition of this collection, and for any further additions that may be made to it, the council determined upon the erection of a set of rail-cases around the upper gallery of the western museum, on the same plan as those put up in 1863 in the lower gallery. Their cost will be defrayed out of the proceeds of the Endowment of the Chair of Dermatology, so that the cases as well as the collection must be looked upon as the gift of Mr. Wilson to the College.

Since the completion of the cases, Mr. Wilson has been engaged in arranging the preparations in systematic order, and in preparing a descriptive catalogue of the whole collection, the manuscript of which is now ready for the press.

PSYCHOLOGY IN ENGLAND

La Psychologie Anglaise Contemporaine (*Ecole Expérimentale*). Par Th. Ribot. (Paris: Ladrange, 1870.)

THIS book expounds to French readers the psychological doctrines of Mr. Jas. Mill, Mr. J. S. Mill, Mr. Herbert Spencer, Professor Bain, Mr. G. H. Lewes, and (more briefly) of Mr. S. Bailey, Mr. Morell, and Mr. Murphy. It ends with a short summary of general results won in the course of the great English psychological movement marked by these names, and is prefaced by an introduction giving the author's view of the development of the sciences, and particularly the science of psychology. For the English thinkers, also, of another type (Hamilton, Whewell, Mansel, Ferrier), he seems to promise to do next what he does here for those whom he classes together as making, after the proper tradition of English thought, an experimental school.

The appearance in France of such a work, at the present moment, has a real significance. Taken along with M. Taine's new and weighty contribution to psychological science (*De l'Intelligence*), and with another work or two, it means that the tide of thought is there turning, if it has not already turned. Between the contempt of M. Comte and the airy attentions of M. Cousin, it has fared indifferently with psychology in France for more than a generation. At a time, when in England, a number of active inquirers, continuing the work of last century, have been pushing forward psychological research in a spirit of strict science; when in Germany a number more, reclaimed from high priori roads of speculation to habits of careful introspective search, or starting from a physiological base, have been vying with their English compeers in efforts to resolve the subtle complicacy of psychical states, and thence to explain the most obscure and varied o all growths; the philosophical mind of France has been mostly turned to the history and criticism of opinion, content to retail the cut-and-dried psychology of an earlier day. From this state of things the original scientific inquiry of M. Taine is a refreshing departure, and M. Ribot's work, though in the main expository merely, has its face also set away. The younger French minds are again coming under the influence of English thought, as more than a century ago their forefathers came; and yet far otherwise than then. For, whereas then the triumph of Lockian ideas in France was, in truth, the overthrow of one national system of thought by another, now that mental philosophy in England bears the cosmopolitan character of science, it is but a case of one nation being guided by another into the path of progressive inquiry. Lighting the way in this work, M. Ribot expounds and summarises with all the arts of a Frenchman, and, for the most part, with good insight. Nor, though evidently in accord with the direction of thought which he makes known, is he a slavish expositor: serious gaps in the work both of individuals and of the school he does not fail incidentally to note. His very art, however, may be thought to lead him somewhat astray, when he seeks at the close to strike a general balance of scientific results, presuming upon the idea of a community of thought among so many inquirers. It is no reflection upon the English thinkers, and hardly even upon the scientific value of their labours, that in a matter so great and difficult, their work thus far is more distinguished for the many fruitful lines of inquiry that it has fairly opened, than for the number of questions that it has finally and unanimously closed.

The author's introductory chapter deserves more particular notice, having a lesson not for Frenchmen only. In England, where the cast of philosophical thought has all along been psychological, and well, as even Germans have come to see, where, also, psychology has first and most conformed to the conditions of science, there is yet a certain tendency, and not only in the minds of common people, to think and speak slightingly of mental inquiries. Philosophy (meaning mental philosophy, not, of course, *natural* philosophy in the land of Newton) is opposed to science, chiefly in the sense of being quite unscientific, and "metaphysics," vaguely supposed to be the same thing or the same nothing, is even a time-honoured term of abuse. This is a state of opinion on which M. Ribot's remarks bear so directly, that there may be an advantage in shortly giving the point of them; and perhaps they may suggest an observation or two not unseasonable at the present moment.

Philosophy, according to M. Ribot, was originally the name for science universal; at present, it is a name indefinitely and irregularly applied to an aggregate of several sciences, having relation chiefly to mind; in the future, it will again have the character of universality, but not be science. In other words, it was once the sum of human knowledge, such as that was; it is now, in part at least, a special kind of knowledge, real and scientific, but vaguely defined; it will become (under the name of metaphysics strictly understood) an extra-scientific, but necessary, complement of knowledge. The sciences, such of them as have not grown out of mere arts, have, in fact, detached themselves from philosophy as a great trunk; mathematics as early as the third century B.C., physics as late as the last three centuries, and not, perhaps, completely until the last. Detached, these have a being quite apart from what was called philosophy; they advance, and grow ever more special, untroubled by philosophic questionings about their foundation; indeed they advance because, and from the moment when, they have left all such aside. And this is now not more true of these than of other sciences concerned less with external nature than with man, and has become true even of sciences like psychology, still confusedly spoken of as philosophy. For already in England, and in Germany also, there is an investigation of mind conforming to all the conditions of scientific progress. It deals with actual phenomena, and attempts to explain them by discovering laws; troubling itself no more about an ultimate essence of mind, than mathematics or physics about the ultimate nature of space or motion, and only labouring by every resource of scientific method—observation of individuals, external as well as internal, and of masses through history and statistics, comparative study of the lower animals, investigation of all abnormal mental conditions, and artificial experiments as far as possible—to master the exceptional subtlety and complexity of the phenomena. For the rest, in philosophy proper, or metaphysic, there must ever remain to nobler minds a boundless sphere of consideration and mental endeavour. The underlying questions, which the sciences must ignore in order to advance, are nevertheless there, and will be answered somehow by all but narrow intellects. Speculation on the first principles and last reasons of things, so far from being superseded, is rather more deeply stirred as the sciences become greater, which is to say, more special. Only, verified science such metaphysical speculation can never be. Like poetry (and no more to be got rid of than poetry) it must always have a certain personal and subjective character. In truth, the metaphysician differs from the poet just in this, that in reconstituting the synthesis of the world, he works with abstract ideas instead of concrete pictures. So far, M. Ribot.

There is much that is noteworthy in the view thus roughly sketched. Some things, regarding the origin of the special sciences, could not easily be said better than by M. Ribot; and, in particular, his whole excursus on the question of the conditions, sources, and method of scientific psychology—a question that has perhaps been less considered by English than by German psychologists—may be commended as highly suggestive. In point of actual achievement in psychological science, distinguished from metaphysical speculation, this country is frankly placed first. It seems time, then, that we should cease to be blinded by any mere associations of language to one of our best titles to national fame. Nay, it even becomes our duty, if philosophy is no more than M. Ribot (to say nothing of others) declares, to lift the shadow of the name from off an arduous line of inquiry, sharply enough defined, clearly enough admitting, as it requires, scientific treatment, and already carried to no mean length in spite of popular misapprehension and the graver indisposition of physical inquirers to recognise in it a true branch of science. There is a science of mind, call it by whatever name, that has come into being like other sciences; and it is a natural science too, unless we are prepared to assert that man, on the most characteristic side of him, is an unnatural object.* Still there is likely to be some reason why, to this

* In the newly-constituted Faculty of Science in University College, Psychology, under the name of Philosophy of Mind, is included along with Logic. Already for some years the two subjects have been recognised in the science-programme of the London University; the Bachelor of Science must pass in both, and the Doctorate of Science may be taken for special proficiency in them, along with certain subsidiary subjects, such as Political Economy. A knowledge of them is required also for the degree of M.D.

science of mind in particular, the name of philosophy should have clung, and if the reason be good, philosophy may not be on the road to become the mere poetry of abstracts that M. Ribot foresees. Let us look a little way into this matter.

Psychology is a natural science if it deals with a definite class of phenomena constantly occurring in nature, deals with them on approved principles of scientific method, and marches steadily from old to fresh results: and that it does conform even to the last of these conditions, M. Ribot often in the course of his exposition has occasion, as he always is properly anxious, to show. What, then, is peculiar in the natural science of psychology? The phenomena dealt with are, beyond all others, complex, subtle, and obscure, requiring a finer tact and a larger scientific vision to single them out with any effect; but this, if it explains why psychology has lagged behind the other sciences, gives it no claim to stand apart. The phenomena, however, are quite peculiar, in being, as it were, two-faced, in having not only a side by which, like all other phenomena of nature, they (or, at least, many of them) fall under external observation, but also a side by which, unlike any other natural phenomena, they fall under so-called internal observation. This is, indeed, a peculiarity; and, further, let this go with it, that, as amongst such double-faced phenomena are all facts of human cognition, the subject of psychology extends, in a sense, to all things known or knowable; viewed, namely, as they are or may be known. Nothing to the same degree, nothing of the same kind, can be said of any other science; for though the general sciences, going backwards from biology, have an ever wider and wider sweep, extend, that is, to more and more objects viewed in the special aspects that each considers, psychology in this respect surpasses the most general of them all more than any of them, and in a way that none of them surpasses another. The science of mind does, therefore, stand on a certain level of its own. It has a place among the general sciences, and a place far down, namely after biology, from dealing with phenomena as much more complex than vital phenomena, as these are more complex than chemical phenomena. But it also has a place before all the sciences, from dealing with the growth of consciousness, and specially the laws and limits of those mental processes whereby all knowledge, scientific or other, must come. So that, if as a science it ought to be studied in its place among the sciences, in the light of their results as far as these bear on its subject, and in the spirit of rigid inquiry which they are best fitted to engender, its subject is still such that its results, when thus obtained, are not as theirs. Its results have not only, like theirs, a value in themselves and a forward reference, but they have a backward reference also. From the psychological stage, once reached, all the science that went before is seen over again in a new light. The true sense of the very language of the sciences—of such a word, to take but one instance, as "phenomenon"—is then first understood; and what before was mere practical assumption is turned into intellectual conception. Or, may we not say that there is gained a certain *philosophic* insight? A multitude of questions, M. Ribot truly said, were left unsolved at the root of the sciences, at the root of psychology itself among the rest. But some of them can be solved psychologically, and such of them as cannot are perhaps not all real questions. Where it is possible, the deeper psychological reading of an objective physical fact may not without reason be called *metaphysical*. It would seem, then, that there may be a metaphysic that does not break with science, and that one kind of science still, in some sense, deserves to be called philosophy. Psychology, in short, is Science in its method and Philosophy in its scope.

G. CROOM ROBERTSON

OUR BOOK SHELF

Dictionary of Scientific Terms. By P. Austin Nuttall, LL.D., editor of "The Classical and Archæological Dictionary," "Standard Pronouncing Dictionary," and numerous Educational Works. (London: Strahan and Co., 1869.)

WE are thankful to be able to say that we know nothing more of Dr. Nuttall than we learn from his title-page. We never even heard of him till we read his book, and we most sincerely hope that, as an author of scientific works, we shall know him no more. What the "numerous educational works" that he has published are we cannot tell, but if they are at all like his "Dictionary of Scientific Terms," the sooner they are consigned to the trunk maker and the butter-man, the better will it be for the welfare of the unhappy youths for whose benefit they were composed. These may seem hard words to apply to a writer who hopes to "receive every indulgence from a generous public" (p. xi.); but when that writer outrages all our better feelings by stringing together a series of idiotic absurdities, and calling the result a "Dictionary of Scientific Terms," how are we honestly to deal with him, except by exposing a few of his blunders? We will begin by testing his chemical knowledge. It will hardly be believed that he regards black-lead, brass, magnet, ochre, pewter, and steel as constituting "a few of the principal metals" (p. xviii.); that nitrogen is "unrespirable" (p. 230), although he has previously told us at p. 12 that it forms about 80 parts of atmospheric air); that oxygen "generates acids" (p. 239); that alum is "an earthy chalk, a sulphate of alumina *or* of potash" (p. 18); and that "culinary, rock, or sea salt, is chloride of soda" (p. 277). His natural history is as peculiar as his chemistry. We will merely put his zoology to the test, assuring our readers that in so far as accuracy is concerned, his zoology, botany, and mineralogy are much on a par. "Zoology," he tells us, "embraces an account of all animal creation, the principal classes being the *Mammalia*, *Aves*, *Reptilia*, *Pisces*, *Invertebrata*, and *Insects*." The first class is subdivided into nine orders, of which one is "*Edentata*, or animals wanting some of the teeth of other animals" (p. xiii.) Being anxious to learn more of these covetous, commandment-breaking creatures, we turned to *Edentates*, and found that they are "an extensive order of the class Mammalia, comprehending those unguicolated quadrupeds which have no front teeth, and divided into three tribes, the Tardigrada, the ordinary Edentata, and the Monotremata." We leave our zoological readers to decide how far this description is an accurate definition of the order, according to recent views, such as our author might have learned by consulting the works of Owen, or of Huxley.

"Invertebrate animals are divided into *Mollusca*, *Articulata* and *Radiata*" (p. xiv.) We had hoped that by this time the "Radiata," having done their work, had modestly withdrawn themselves from their old position, and had been replaced by other classes; but Cœlenterata, Hydro-medusæ, &c., are terms unknown to Dr. Nuttall. "The Mollusca is (*sic*) so called from the body being soft and molluscous. It is divided into four classes, the *Mollusca*, *Conchifera*, *Tunicata*, and *Cirripeda*" (p. xiv.) How this marvellous division is accomplished we are unfortunately not told. In the next page we find that the molluscs or soft animals are composed of radiata, starfishes, &c., polyps, corals, &c., naked molluscs and testacea or shell-fish. Need we carry our investigation further?

The terms which the author supposes are used by surgeons and physicians are of the most astounding nature, and many of them, although possibly used a century or two ago, are perfectly new to us. We only confine ourselves to the letter A. Do any of our readers suffer from acatharsia, acratia, acrisy, acropathy, acropy, acroteriasm, acrothymion, adenopharyngitis, aerophobia, agalaxy, agennesia, agrypnocoma, anagogy, anopsy, antispasis, antritis, apagma, apolepsy, apolysis, apotrepis, or arthropuosis? Let them have recourse to such remedial agents as acidulum, acopica, adipson, alborga (a kind of sandal wood made of mat weed), aldehydic acid (which "is a solution of oxide of silver in aldehyde"), alloxan (which our readers will be surprised to learn is "the action of nitric with uric acid"), amylum (which we are told is "a preparation of starch"), anacathartics (which are "any medicines that operate upwards; a cough attended with expectoration;" it is of course the "medicine that operates upwards" and not the cough, that we recommend as a remedial agent), antephiatic medicine, or arrowroot (consisting of starch, albumen, volatile oil, chloride of calcium, and water), which we presume may be obtained at an antidotarium. Should surgical aid be required, an arthrembolum may prove of service. We can only account for this appalling list of medical terms and for the information regarding alloxan, arrowroot, &c., on the supposition that Dr. Nuttall's medical attendant is an incorrigible wag, and that he took a most dishonourable advantage of his position.

Our readers will, we think, by this time be satisfied that our introductory remarks upon this discreditable production were not at all too severe. It is a disgrace to English science that such books should find a respectable publisher.

Das Gesetz der vermiedenen Selbstbefruchtung bei den höheren Pflanzen. Von Dr. O. W. Thome. (Williams and Norgate.)

WE opened this little pamphlet in the hope of finding in it a new contribution to the literature of the self-fertilisation and cross-fertilisation of plants, but were disappointed to discover that it consisted of little besides a *résumé* of the labours of others in this field. The instances in which the self-fertilisation of hermaphrodite flowers is prevented by the fact that the stigma and the stamens ripen at different times, are mostly taken from Prof. Hildebrand's "Die Geschlechter-Vertheilung bei den Pflanzen," and from that botanist's contributions to the "Botanische Zeitung." On the laws of dimorphism and trimorphism we have little but the examples so elaborately worked out by Mr. Darwin in the genera *Linum*, *Primula*, and *Lythrum*. It is singular that from the time that Sprengel first called attention to the provisions which favour cross-fertilisation in plants, now more than seventy years since, so little had been done in this field until the researches of the two eminent botanists above named, and even now they have so few fellow-labourers. There is no department of physiological botany more beneath the eye of every dweller in the country, or of any one who possesses a garden, none which presents so many points of interest even to the casual observer, and so many illustrations for the advocate of the doctrine of "design," and none in which a careful series of observations would be more fertile in results of importance. If country botanists would bestow a portion of the energy which has been wasted in mere collecting, and the eradication of rare plants from their native haunts, on systematic physiological observations, the gain to genuine science would be immense.

A. W. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Gulf-Stream

As a note appended to Prof. Wyville Thomson's lecture "On Deep-Sea Climates," published in your number for July 28, may lead your readers to suppose the divergence of opinion between my colleague and myself upon the subject of the Gulf-Stream to be greater than it is, I shall be obliged by your placing before them a few observations which may put the question really at issue between us in a more definite form.

The term "Gulf-Stream" is continually used in two different senses. By myself it has been employed to designate the current issuing from the Gulf of Mexico through the "Narrows" between Florida and the Bahamas; and this I apprehend to be its true signification. My colleague's definition of it I give in his own words:—"The water of the North Atlantic thus consists first of a great sheet of warm water, the general northerly reflux of the equatorial current, the most marked portion of it passing through the strait of Florida, and the whole generally called the Gulf Stream." He thus distinctly recognises, with myself, the participation of a *Subsidiary Current* with the *Gulf-Stream proper* in the production of the two great temperature-phenomena of the North Atlantic, determined by the *Porcupine* soundings —(1) the elevation of the temperature, not of its surface-layer only, but of a stratum 800 or 900 fathoms deep, by a north-east movement of tropical water; and (2) the depression of the temperature of its deepest portions by a reflux of glacial water from the Arctic and Antarctic basins.

Now I have on no occasion, so far as I can recollect, spoken so disrespectfully of the Gulf-Stream proper as to "deny that it exercises any influence upon the temperature of the basin of the North Atlantic," nor am I aware of having expressed a "doubt whether it reaches the coast of Europe at all." That the Gulf-Stream proper, by raising the temperature of the portion of the Atlantic basin over which it can be distinctly traced, has a most important *indirect* influence upon the temperature of its north-eastern extension, I cannot doubt for a moment; and that its *direct* influence is traceable to the western coast of Europe, as far north as the Bay of Biscay, I accept on the authority of the recently-published Admiralty charts of its course and distribution. But I *have* expressed a doubt as to the extension of the Gulf-Stream proper to the channel between the North of Scotland and the Faroe Islands; and I have ventured to think it an open question whether the super-heating of the surface-water observed on a hot Midsummer day beyond the northern border of the Bay of Biscay was not as probably due to the direct influence of the sun as to the extension of the Gulf-Stream to that locality.

The main questions between my friend and myself are therefore as follows: What are the relative shares of the Gulf-Stream proper, and of the Subsidiary Current, in producing the elevation of temperature in the upper stratum of the North Atlantic to a depth of about 800 fathoms—and what is the motive power of that Subsidiary Current? These questions can only be answered, as it seems to me, by an appeal to certain general probabilities—definite data for their determination being still deficient, for, in the first place, all the calculations which have been made as to the quantity of water which issues from the Narrows, and the amount of heat which it conveys, are based (if I recollect aright, having here no access to books on the subject) upon the assumption that both its temperature and its rate of movement are the same throughout its depth as they are at its surface. Now, until reliable proof shall have been furnished as to both these particulars by our friends of the United States Coast Survey, I must claim a suspension of judgment, many probabilities leading to the suspicion that the bottom-flow may be considerably less rapid than the surface-current, and its temperature considerably lower. Secondly, even admitting in its full force the reputed "glory" of the Gulf-Stream at its exit from the Narrows, I fail to see the evidence that either its heat or its movement is directly concerned in the flow of the warm upper stratum of the north-east extension of the Atlantic towards the Hebrides, the Faroes, and Spitzbergen. For as the stream of superheated water, on its emergence into the open ocean, spreads itself out like a fan, it must necessarily become shallower as it extends instead of deeper, and this (if I remember aright) is what all observation indicates. That a large proportion of it is deflected back towards the Equator is universally admitted, and that the remainder can be gathered together after its initial velocity has been expended, and forced downwards so as to displace colder water to the depth of 800 fathoms whilst still moving north-east, seems to me in the highest degree improbable.

To what then is the north-east movement of the warm upper stratum of the North Atlantic attributable? I have attempted to show that it is part of a *general interchange* between Polar and Equatorial waters, which is quite independent of any such local accidents as those that produce the Gulf-Stream proper, and which gives movement to a much larger and deeper body of water than the latter can affect. The evidence of such an interchange is twofold—that of physical theory and that of actual observation. Such a movement *must* take place, as was long since pointed out by Prof. Buff, whenever an extended body of water is heated at one part and cooled at another; it is made use of in the warming of buildings by the hot-water apparatus, and it was admirably displayed at the Royal Institution a few months since in the following experiment kindly prepared for me by Dr. Odling:—A long but very narrow trough, with plate-glass sides, having been filled with water, a tube into which a steam-jet was conveyed was introduced vertically at one end, whilst a lump of ice was wedged between the sides of the trough at its opposite extremity. Some red colouring matter mixed with gum, of such viscidity as to be carried along by any movement of the liquid mass without mingling with it, was introduced into the water at the end of the trough warmed by the steam-jet, and a like mixture of a blue colour was introduced at the end cooled by the ice. The latter very speedily sank to the bottom along the side of the ice-wedge, and then crept slowly along the floor of the trough, towards its warm end, where it rose along the side of the heated tube until it reached the surface, and then slowly flowed back towards the cold extremity. On the other hand the red liquid passed slowly along the surface in the first instance from the warm to the cold extremity, then sank (as the blue had previously done), crept along the surface of the blue layer covering the bottom of the trough, and then rose (as the blue had previously done) along the side of the heated tube to the surface. Thus a circulation was shown to be maintained in the trough by the application of heat at one of its extremities and of cold at the other, the *heated* water flowing along the *surface* from the warm to the cold end, and the *cooled* water flowing along the *bottom* from the cold to the warm end: just as it is here maintained that Equatorial water streams on the surface towards the Poles, and that Polar water returns along the bottom towards the Equator, if the movement be not interfered with by interposed obstacles, or prevented by antagonistic currents arising from local peculiarities. So far is this from being the case with the general surface-movement in the Atlantic basin, that it will concur with and supplement the motion of the Gulf-Stream proper, which may thus be regarded as a portion of the general Equatorial-polar current, deflected in the first instance by the action of the trade-winds, but subsequently rejoining the great body of water having a north-east motion of its own and imparting to its surface-layer a higher temperature than it would otherwise convey.

Now that this hypothesis is at any rate deserving of consideration, and is not to be dismissed by the *ipse dixit* even of so high an authority as Dr. Petermann, though backed by my excellent colleague, I venture to maintain on the strength of the parallel case afforded by the temperature-phenomena of the Southern Indian Ocean. For, as Capt. Maury has shown, a definite current exists in the midst of it, carrying tropical waters far into the southern temperate zone, and not attributable to any such local peculiarities as those which produce the Gulf-Stream of the Atlantic. Conversely, the Hydra soundings in the Arabian Gulf have given evidence of a northerly reflux of glacial water from the Antarctic basin along the deep-sea bed of the Indian Ocean to replace the more superficial stratum which has moved southwards. This glacial water will in its turn be raised from the depths by the heating action of the tropical sun, and then return as the southerly surface-stream to the Antarctic basin, and would there sink by surface-cooling and again flow northwards along the sea-bed.

The facts of observation, then, being in the case last cited entirely accordant with physical theory, I submit that the same theory may be fairly applied to the explanation of those temperature-phenomena of the North Atlantic, which (as it seems to me) cannot be more than very partially, if at all, accounted for by the agency of the Gulf Stream proper. And I am content to leave it to the judgment of those who are competent to deal with the question whether the north-east movement of the vast breadth of water lying between the coast of Greenland and that of Northern Europe, usually extending to a depth of 5,000 feet, is more likely to depend upon an impetus derived from a portion of the comparatively narrow and shallow current which issues from the Narrows thousands of miles off, or to form part of a general circulation of oceanic water, the cause of which is quite independent of local accidents.

The readers of NATURE will, I am sure, join with me in deeply regretting that Prof. Wyville Thomson is prevented by illness from taking that share in the scientific exploration of the Mediterranean basin, now about to commence, which has conduced so greatly to the success of the two previous expeditions in which we have had the pleasure and advantage of being fellow-workers.

Gibraltar, Aug. 11 WILLIAM B. CARPENTER

Dr. Hooker's "Student's Flora"

ALL the reviews which I have read of the "Student's Flora of the British Islands" are, as they undoubtedly should be, appreciative, but your reviewer has done well in pointing out a few seeming discrepancies, which it would, perhaps, be well if attended to in a second edition; and, if you will permit space, I will endeavour briefly to add to your list of desiderata. In the first place I take it that a good glossary is an essential, if not an absolute necessary, adjunct to a students' manual; yet we look in vain for anything of the kind in Dr. Hooker's new "Flora." Dr. Hooker could scarcely have thought beginners in botany able to interpret many words used by him in his generic and specific descriptions. I am also sorry to see that little word "*sub*" used so extensively; for my own part I do not understand its meaning as applied in zoology and botany. I can quite understand its applied use in sub-contractor, sub-lieutenant, subterranean, &c., but who dares tell us that one tribe, or one family, or genus, or species is subordinate to another? I comprehend a "species" as a form of animal or vegetable life which differs slightly but materially and permanently from its nearest ally; such a form is, in my humble opinion, worthy to hold its own as a good species, not subservient or subordinate to any other form that can be so described, no matter how apparently closely related. Forms of this kind may have (I say it advisedly: we have no proof to the contrary) approached each other through natural selection, without being off-sets in a direct line from a common parent.

Again, why use the term "sub" at all, when a much simpler and less confusing arrangement may be employed in its stead. All naturalists are pretty well agreed that natural arrangements shall consist of orders, tribes, families, genera, species, and lastly, varieties. Beginning with "tribes" of any given order, we have—Tribe 1, consisting of certain families all agreeing in certain recognisable features or parts. Families which cannot be grouped under Tribe 1 would fall under Tribe 2, and so on numerically *ad libitum*, without one being subordinate to the other, which they really are not in nature. The same may be said of genera grouped into families, species into genera, &c.

Dr. Hooker has done well in making varieties (or, as he terms them, "sub-species") of some plants which certainly have no other specific claim. As an instance, I may cite the three almost equally common forms of *Aspidium*, all of which are included under *A. aculeatum*, Sw. This is decidedly a step in the right direction; but should not the forms have retained the name of *angulare* in preference to *aculeatum?* The latter, I believe, claims priority, but the form described under *angulare* is decidedly the most highly developed, having stalked pinnules.

Woodhay, Aug. 12 HENRY REEKS

On Supersaturated Solutions

MY friend Mr. Rodwell was so good as to forward to me a copy of NATURE for the 4th inst., containing an account of some interesting experiments by Mr. Grenfell on the action of fatty bodies on supersaturated saline solutions.

During the last two years I have made a large number of experiments in order to ascertain the function of oils and fatty substances in determining the crystallisation of such solutions. The results of my inquiry are included in a paper, the abstract of which was read before the Royal Society on the evening of the 16th of June last, to which I beg leave to refer.

I may, however, be permitted to make a few remarks arising out of Mr. Grenfell's paper.

According to my view, a *nucleus* is a body that has a stronger attraction for the gas, or the vapour, or the salt, of a supersaturated solution than for the liquid that holds it in solution.

Nuclei, with certain limitations, cease to be such when made *chemically clean*.

A body is chemically clean, the surface of which is entirely free from any substance foreign to its composition.

Thus oils and fatty bodies are chemically clean, if chemically pure, and containing no substance, mixed or dissolved, that is foreign to their composition.

The limitations above referred to are two: (1) the oils, &c., when chemically clean, do not act as nuclei while in the mass, such as a lens or globule; but these oils, &c., whether clean or not, in the form of thin films, act powerfully as nuclei; (2) a liquid, at or near the boiling point, is a supersaturated solution of its own vapour, and a porous body, such as charcoal, pumice, &c., whether clean or not, is a powerful nucleus in separating vapour.

I have on several occasions taken the liberty of opposing M. Gernez's views as to the action of nuclei. He supposes (1) that supersaturated gaseous solutions (soda water, seltzer, &c.) give off their gas to nuclei by virtue of the air that these latter introduce into the solution: in other words, gas must escape into air, and the function of the nucleus is to carry down air; hence rough bodies act better as nuclei than smooth ones. I have shown (Phil. Mag., August 1867) in a series of twelve experiments, that air is not a nucleus, and that rough bodies are inactive, if *catharised* or made *chemically clean*. A rat's-tail file, for example, is a good nucleus, because it holds between its teeth not air, but that filmy kind of matter that is powerfully *nuclear*, and it is not easy to clean a body of this kind; but when clean, it is quite inactive. So, a flint stone that has been exposed to the air, or handled, acts as a powerful nucleus, but when broken, the newly-fractured surfaces are inactive, because chemically clean. And such surfaces are inactive, because the gaseous solution adheres to them as a whole; whereas, if a clean body be handled or exposed to the air, it becomes covered, more or less, with filmy matter, to which the gas adheres more strongly than the liquid does, and hence there is a separation.

There is, I think, abundant proof that air is not a nucleus, its function, if it have any, in this class of phenomena, being that of a *carrier of nuclei*. Proof also is wanting, I imagine, that when a nucleus determines the crystallisation of a supersaturated saline solution, a salt of its own kind is present. When M. Gernez so laboriously prepared his nuclei, so as to free them from salt, he did not perhaps reflect that he was making them chemically clean. Of course I fully admit that, in general, a salt of the same kind as the solution, acts as a powerful nucleus; but in order for it so to act it must adhere more strongly to the saline than to the liquid portion of the solution. It may even happen that a crystal of the same kind, and of the fully hydrated salt, has no nuclear action, because it is in a perfectly catharised condition. And here I must refer to the objection raised by Dr. De Coppet, that in one of my forms of showing this experiment, the hydrated crystals, say of magnesic sulphate, being introduced into the neck of the flask while the solution was boiling, and so left in the covered flask while the solution cooled, such crystals become so changed by the heat as no longer to represent the normal salt, so that when lowered into the solution they formed a different salt, and hence were no test of the point in question, as to whether a salt of the same kind may be rendered inactive as a nucleus. I admit the criticism to be just, but in my original account of the experiment (Phil. Trans., 1868, p. 665) I did not rely upon one form only. Highly supersaturated solutions in clean tubes, plugged with cotton wool, were put, when cold, under the receiver of the air-pump, and left for some time *in vacuo*, over sulphuric acid, the effect of which was to produce crystalline crusts of the normal salt on the surface, and these by shaking fell through the solutions without acting as nuclei; whereas on removing the cotton wool in the presence of air, the solutions crystallised immediately into a solid mass. So also by keeping supersaturated solutions during some months, water escapes through the cotton wool, and a crystalline crust of the normal salt creeps up the air-filled portion of the tube, and this has no nuclear character, because the adhesion between it and the solution is perfect.

So necessary is the action of a nucleus in determining crystallisation in these solutions, that, if care be taken to exclude nuclei, highly supersaturated saline solutions may, by reduction of temperature to 0° F., or from that to − 10° F., be made solid, and by placing the tubes in snow and water at 32° F., the solids rapidly melt into clear bright solutions, without any separation of salt. These effects may be shown any number of times; but whether the solution be solid or liquid, if the cotton wool be removed, crystallisation always sets in, in the case of the solid during the melting, while in that of the liquid the effect is immediate.

With respect to the editorial note that the solutions of hydrated salts contain the anhydrous salt, I have shown in the paper last quoted, and with still greater elaboration in the *Chemical News* for Dec. 10th, 1869, that such is the case with respect to sodic sulphate. I insist on this point, as it is one of first-rate importance in considering the theory of supersaturated saline solutions. I endeavour to prove that it is the anhydrous salt in solution, by showing that at various points of the scale a sudden lowering of temperature produces a shower of the well-known octahedral crystals of the anhydrous salt. I also explain in my original memoir, that it is necessary for these crystals to be deposited before the modified 7-atom salt can be formed, and that even when there is a copious deposit of this salt the liquor above it is not, as Löwel supposed, the mother liquor of the 7-atom salt, but it is still a solution of the anhydrous salt. And more than this, when the sudden change in the curve of solubility takes place at 33° or 34° C., and there is, according to Gay Lussac's supposition, a change in molecular condition, it is still the anhydrous salt that is in solution.

There are several other points that might be enlarged on, but that I fear to trespass further on your valuable space.

Highgate, N. CHARLES TOMLINSON

Astrology

THE belief in astrology which still prevails among the English lower classes to a much larger extent than is supposed, will derive a fresh impulse from the happy guesses which have been made by the editor of "Moore's Almanac" in his issue for the current year. The hieroglyphic with which it is illustrated is less vague than usual, and represents two eagles fighting in the air, and on the plains beneath them hosts of armed men (in decidedly foreign uniforms) engaged in a bloody struggle. Lest the point should be missed, the prophet begins the forecast of the year with the distinct assertion that there will be war between France and Prussia, and that the month of July will be especially disastrous to the Emperor Napoleon. Thus far events have coincided with the voice of the oracle, and seem to confirm the poet's view that

"The warrior's fate is blazoned in the skies."

But we have yet to see whether "in October the King of Prussia (if living) will meet with defeat, and the ex-King of Hanover recover some of his prestige, if not his throne also." M. Comte would have us deal tenderly with astrology, because it was, in his opinion, the first systematic effort to frame a philosophy of history out of the apparently capricious phenomena of human actions. In theory we may do so, but astronomical science is hardly likely, for the sake of sentiment, to treasure up the discarded swaddling clothes which for so many centuries impeded its onward progress.

Norton Canon, Weobley C. J. ROBINSON

On Volcanoes

HAVING only last night returned from Norway, I was not aware before to-day that No. 40 of NATURE (August 4) contained "an outline" of a lecture on volcanoes delivered by me in St. George's Hall, Langham Place, on the 19th June (not 9th as therein stated) last.

Although I cannot but feel highly flattered at the length of this notice, I must regret that the author of this "outline," who, strangely enough, signs himself by my name, has, as will be seen upon reference to the text of my lecture as reported in the *Geological Magazine* for July, omitted every word which could convey to the reader the remotest idea of the object of the lecture itself, or the conclusions arrived at from the evidence brought forward. Just as a man without life is but a corpse, neither can a mere string of facts be called even the "outline of a lecture," when we have only the body without the spirit.

The object of my lecture was to institute a comparison between the relative magnitudes of the operations of internal and external forces in determining the main external features of our globe, and the conclusion arrived at was, to "grant the first rank to the internal, volcanic, or cataclysmic agencies, since, had it not been for their operations, our globe would still have remained a comparatively smooth sphere, surrounded by its external envelope of water, with no visible land for the rivers to traverse or the rain and ice to disintegrate and wear away," &c.

In order that your numerous scientific public may not be led to judge of the lecture by this outline, I trust to your good will in asking you to insert these remarks in your next number.

DAVID FORBES

11, York Place, Portman Square, Aug. 22

A Vivid Mirage

THE illusion known as the mirage is, I believe, not unfrequently observed in the British Isles; but the vividness with which it was displayed on the present occasion will, I trust, be a sufficient apology for troubling you with this letter.

The land bordering the River Nene is protected by banks of from twelve to fifteen feet in height, enclosing a space called the "Wash," which receives the flood waters. It was from one of these banks that the appearance in question was observed, nothing unusual being seen from the level of the fen. I may mention that the Wash at this season is as dry as any other portion of the land.

The day (August 12) was hot, the sky cloudless, and a strong N.E. wind was blowing. About eleven o'clock the phenomenon was first noticed. To the eastward a dark line of trees, some eight miles distant, stood out in bold relief against the clear sky, and in front of this a shining line of silvery brightness was seen, which gradually widened until about twelve o'clock it presented the appearance of a broad expanse of water, ruffled into waves on its near side, but perfectly calm and clear toward the horizon where the line of trees was beautifully reflected on its surface. As I had been approaching the scene all this time, the expanding of the lake appeared perfectly natural, and I could scarcely help thinking the river must have overflowed during the night and drowned the "Wash." This, of course, I knew to be quite out of the question, but the semblance was so perfect that it required an effort to believe that it was but an illusion. Its shores were clearly defined, little bays dimpled it, tiny headlands jutted out from it, and the waves were seen rising and falling with life-like exactitude. The whole appeared quite stationary, and as I approached the spot it gradually faded away, until nothing but a thin blue haze beneath the trees remained, and this at length dissolved.

On looking behind me (*i.e.* westward) another mirage seemed forming, which increased in apparent extent as I went farther from it. In this case the illusion was, if possible, more perfect than in the last, and the comparatively high land of Whittlesea rose like an island from the shining sea. Vehicles passing along the road seemed floating on its surface, their dark drawn-out reflections showing vividly against the sun-lit water, which, in this instance, was quite calm. How long this illusion lasted I know not, but when, about two o'clock, I quitted the bank it was still very distinct.

The dead level of the fen, and the bright sunlight falling upon the parched land, from which the heated air rose tremulous as from a hot plate, render this district peculiarly favourable to the production of such effects. For the accuracy with which the appearance of water was simulated it was quite equal to any mirage I have witnessed on the African deserts.

SYDNEY B. J. SKERTCHLY

Geological Survey, Whittlesea, Cambridge, Aug. 12

Mirages made Easy

THE very interesting account of a mirage in this week's NATURE induces me to send a few observations. The mirage phenomenon is by no means so uncommon in England as many think. Three or four summers ago, on a strip of sand three miles long at Morecambe Bay, I was able to see one almost every hot day, by simply stooping until my eyes were about a yard above the ground. The further part of the sand then appeared as a lake of water, with objects reflected, &c. The nearer edge of this lake receded as the eyes were raised, the whole soon becoming invisible. I saw the same effects last summer off the Holderness Coast, but again only by stooping. At Cambridge I have lately seen a very good lateral mirage, by looking closely along the surface of a wall fifty yards in length, which had been exposed for some hours to a western sun. Objects near the further end of the wall were distinctly reflected.

CHAS. T. WHITMELL

Trin. Coll. Cambridge, Aug. 4

Science and the Government

THE announcement in your last number of a rumour that the Government is about to withdraw its promise of aid to the Total Eclipse Expedition, seems to bring to a climax the relations of the Government towards science. We can hardly forget that one of the prominent members of the present ministry, and the one considered to have the special control over the spending of the public funds, is member for the University of London. Mr. Lowe was sent to Parliament, irrespective of party considerations, as the representative of a body which thinks it has some claim to a leading place among the scientific institutions of our country. Is it not worth while to consider whether the views of the graduates of London University are represented in the present attitude of the ministry, and whether some representation might not be made to the Government, through the Chancellor of the Exchequer, of the manner in which the present relations between the Government and science are regarded by his constituents?

3, Park Village East, Aug. 20 ALFRED W. BENNETT

*AROMATIC GLYCOL**

ALL chemists recollect the profound impression caused by the discovery of glycol in 1855 by M. Wurtz. Up to that time the bodies which were recognised as belonging to the group "alcohol" only included what we now call monatomic alcohols (common alcohol and its analogues), and M. Berthelot hesitated before venturing upon declaring glycerine a triatomic alcohol—an opinion to which Gerhardt never entirely adhered.

M. Wurtz showed that besides ordinary or monatomic alcohols, there are others which, when submitted to certain reagents where ordinary alcohols furnish only one, produce two derivatives. To these substances he gave the name of diatomic alcohols or *glycols*, and recognised that to each monatomic alcohol belonged a corresponding glycol, which only differed from it by the addition of an atom of oxygen. This new view became rapidly extended. It was admitted that belonging to each glycol there was, or might be, a triatomic alcohol or glycerine; that to each glycerine there might be a corresponding tetratomic alcohol, and so on; these alcohols only differing from one another by the number of atoms of oxygen which they contained, the number being always denoted by the atomicity of the alcohol.

Shortly before M. Wurtz's discovery of glycol, Signor Canizzaro, now Professor at the University of Palermo, but then Professor at Genoa, had announced the discovery of a new alcohol, which he called benzylic alcohol, having to benzoic acid the same relation as vinous alcohol has to acetic acid. He had obtained this substance by the action of potash in alcoholic solution, on the essence of bitter almonds.

Some time later the same chemist discovered that this alcohol might be equally obtained by means of toluol (*toluène*). The method he employed was to subject chlorinated toluol to the action of acetate of potash; and finally to decompose the acetate of benzyl thus obtained by means of potash.

This benzylic alcohol was the starting point of a new series of alcohols known as aromatic monatomic alcohols, and in fact soon afterwards cumylic alcohol was obtained, and Signor Canizzaro himself shortly afterwards published his discovery of tolylic alcohol.

It should, however, be observed that the process by means of which Signor Canizzaro had obtained his benzylic alcohol from toluol, succeeded ill with the homo-

* "Sur un Glycol Aromatique." Par M. Edouard Grimaux.

logues of this hydro-carbon, and gave, even with toluol, but small quantities of the alcohol.

Herr Beilstein, however, took up the matter at the point where others had abandoned it. He showed that when chlorine is made to act upon heated aromatic hydrocarbons, there is obtained a chloro-derivative, easily acted upon by acetate of silver or of potassium, and consequently admirably fitted to furnish the corresponding alcohol; whilst on the other hand when used cold, a very stable chloro-substitution compound is formed of the same composition, but furnishing no alcohol.

From this moment no difficulty remained in completing the series of the aromatic monatomic alcohols; but up to this time no diatomic alcohol, or aromatic glycol was known. It was, however, evident that the preparation of such compounds was possible, inasmuch as the knowledge already existed of such acids as formobenzoilic acid, and terephtalic acid, which bore the same relation to these unknown bodies as glycolic and oxalic acids to ordinary glycol.

M. Edouard Grimaux, after much long and careful investigation, has at length filled up the blank by the discovery of tolylenic or xylenic glycol, that is to say a glycol which presents in its composition xylol to which two atoms of oxygen are added without substitution.

To obtain tolyl-glycol M. Grimaux takes the xylol of commerce which he raises to the boiling point in a retort, surmounted by an inverted refrigerator, from which the condensed vapours flow back into the retort, the tubulure of which is closed by a cork, into which a funnel with a tap is inserted. He then introduces drop by drop into the boiling hydro-carbon half its weight of bromine. The bromine is at once absorbed with the production of hydrobromic acid, and on cooling a black crystalline mass is obtained which is first washed with ether, and its purification completed by crystallisation from chloroform. This substance is a bibromo-xylol easily acted on by re-agents.

Treated at a temperature of 150°, or in a closed vessel with an alcoholic solution of acetate of sodium, the bromo-product gives rise by double decomposition to bromide of sodium and to diacetate of tolylene, which takes the form of beautiful colourless crystals.

Subjected in its turn to the action of thirty times its weight of water at a temperature of 170° or 180° in a closed vessel, this acetate of tolylene is separated into acetic acid and tolylenic glycol, which, on the opening of the tube and by the evaporation of the liquid, is deposited in needle-shaped crystals.

The formation of tolyl-glycol may be represented by the following equations:—

$$1.\quad C_6H_4<\begin{matrix}CH_3\\CH_3\end{matrix} + 2\left\{\begin{matrix}Br\\Br\end{matrix}\right\} = 2\left(\begin{matrix}H\\Br\end{matrix}\right\} + C_6H_4<\begin{matrix}CH_2Br\\CH_2Br\end{matrix}$$

Xylol. — Bromine. — Hydrobromic acid. — Bromine of tolylene.

$$2.\quad C_6H_4<\begin{matrix}CH_2\,Br\\CH_2\,Br\end{matrix} + 2(C_2\,H_3\,O_2.\,Na) = 2\,Na\,Br + C_6H_4<\begin{matrix}CH_2.C_2H_3O_2\\CH_2.C_2H_3O_2\end{matrix}$$

Bromine of tolylene. — Acetate of sodium. — Bromide of sodium. — Acetate of tolylene.

$$3.\quad C_6H_4<\begin{matrix}CH_2.\,C_2H_3O_2\\CH_2.\,C_2H_3O_2\end{matrix} + 2\left(\begin{matrix}H\\H\end{matrix}\right\}O\Big) = 2(C_2H_3O_2H) + C_6H_4<\begin{matrix}CH_2\,OH\\CH_2\,OH\end{matrix}$$

Acetate of toluol. — Water. — Acetic acid. — Tolyl glycol.

The path is now open, and nothing will be easier than, by following the method proposed by M. Grimaux, to prepare all the aromatic glycols, of which the hydrocarburets are already known to us.

ALFRED NAQUET

SPECTROSCOPIC NOTES *

SPECTRUM OF A SOLAR SPOT, APRIL 9, 1870, P.M.

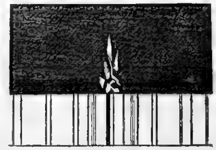
EXAMINED the spectrum of a large group of spots a little north and east of the sun's centre.

The nucleus of the most southerly member of the group reversed the C line finely, turning it into a conspicuous bright line for about 20″ of its length, without any distortion, however, such as is common upon this (dark) line in the neighbourhood of spots.

F was also reversed, but rather faintly.

D_3 could not be made out at all in the nucleus spectrum; neither were 2796 (the numbers refer to Kirchhoff's map) nor *h* reversed. I thought, however, that they were somewhat *thinned*.

The reversal of C and F continued through the whole afternoon.



On the other hand, many of the dark lines were widened and deepened in this nucleus spectrum in the manner which the description and figures of Mr. Lockyer have made familiar. Many also were unaffected. Among these were notably *a*, B, E, 1474, the four lines of *b*, 1691 and G.

The two sodium lines D_1 and D_2, and 850 (Fe) were distinctly, but not greatly, widened.

The effect was most marked upon the following: 864 (Ca), 877 (Fe?), 885 (Ca), 895 (Ca and Li), 1580 (Ti), 1599 (Ti), 1627 (Ca), and 1629 (Ti). I have marked 877 doubtful, because there lies very near it a line whose origin is unknown, and I am not sure to which of the two the thickening was due. The Titanium lines are identified as such by reference to Ångström's Atlas. I was greatly surprised at the prominence they assume in the spot-spectrum, as they are inconspicuous in the normal spectrum; and a similar remark applies to the calcium lines.

I do not intend to convey the idea that the lines mentioned were the only ones that were much deepened; there were many others, mostly faint, affected to nearly the same degree, but I had not time to identify them.

There was at the same time an exceedingly brilliant protuberance on the south-west limb of the sun (position angle 230°), near but not over, a large spot which was just passing off. At the base of this prominence, which was shaped like a double ostrich plume, the C line was intensely brilliant, so that the slit could be opened to its whole width in studying the form above described, but it was not, so far as I could see, in the least distorted. On the other hand, the F line, also very brilliant, was shattered all to pieces, so that at its base it was three or four times as wide as ordinary, and several portions of it were entirely detached from the rest. The figure, without pretending to exact accuracy, and for the sake of distinctness a little exaggerated, gives a fair idea of the nature and extent of the "shattering" alluded to.

Since the C line was not similarly affected it is hardly possible to attribute this breaking up of F to cyclonic motions in the gas from which the light emanates, and it becomes very difficult to imagine a cause that can thus disturb a single line of the spectrum by itself. Possibly this appearance may be the result of local absorptions acting upon a line greatly widened by increase of pressure or temperature.

It continued unchanged for more than half an hour, and until the sun passed out of sight behind a building. The observations were made with the 5 prism spectroscope.

C. A. YOUNG

Hanover, New Hampshire, May 20

* Communicated by the Author.

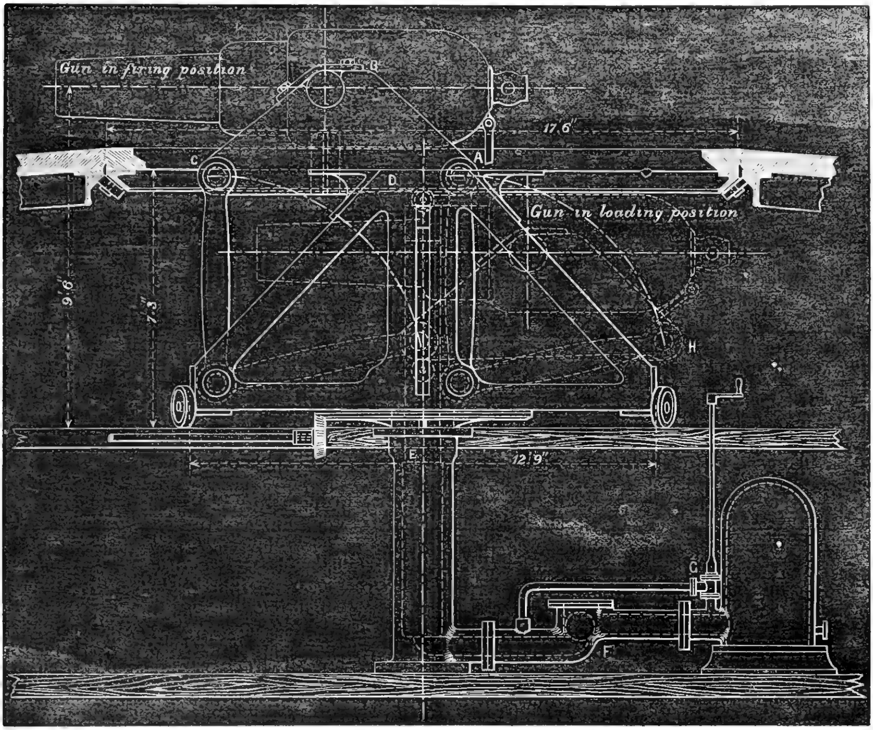
THE SCIENCE OF WAR

I.

CAPTAIN MONCRIEFF'S HYDRO-PNEUMATIC GUN-CARRIAGE FOR SEA SERVICE

SOME apology is needed for venturing to trouble the quiet of NATURE with an account of military engines. The desolation that war is now spreading over some of Nature's fairest scenes, and the waste caused thereby of her bounteous annual gifts, may account for these jottings, which have, perhaps, strangely found their way into her note-book; such topics have been suddenly and violently forced of late upon her attention.

But a further and more substantial reason for finding a place for a subject of this kind within NATURE'S domain is that the action of Nature's forces is in some ways best displayed in warlike engines. In them we use, examine, and experiment with force in sudden and violent action. To produce and also to control and direct the instantaneously created power of explosives has taxed the ingenuity, and also enlarged the knowledge of both the chemist and the mechanician. But for the necessities of war we should know little of Nature's forces in this aspect. The arts of peace only in a very limited form make use of explosive agents. The wonderful progress made of late years in military engines—a progress whose rapidity surpasses the advances of any mechanical inventions for other purposes—has called an amount of attention to those forces and agents which the modern artillerist employs, that has not failed to bring out many special facts in relation to them. Nature's territories are everywhere so rich that he who diligently ploughs any corner, however remote, seldom fails to be rewarded, not only by the crop which he seeks to cultivate, but also by some unsuspected fragments of truth which his plough-share has, as it were, accidentally turned up. For instance, some of the most remarkable examples of conservation of energy are to be found in the action and effects of the mighty projectiles of modern times.



The inventions of the gentleman whose name stands at the head of this paper, afford several important illustrations of the foregoing remarks. It is not more than two years since his Protected Barbette Gun-Carriage for land defence was tried with an instantaneous success that filled the public with astonishment—an exceedingly rare occurrence in inventions so entirely novel in their principles. Having previously existed only in the inventor's mind and in small models, when applied to a 7-ton gun (a monster unknown so recently as the Crimean war), it was found to work with perfect exactness and to realise all that had been promised for it. It may be well to recall briefly to the reader's mind what this carriage effected, and the means by which it acted, as the new carriage is designed to effect a somewhat similar object, though by a wholly different agency.

The purpose which the Protected Barbette Carriage accomplishes is the utilisation of the recoil. This force, which in modern heavy ordnance is a very considerable one, was previously known only as a destructive force. It strained racers and pivots almost beyond the endurance of iron, and shook the granite foundations on which they rested; and artillerists sought to coax it to waste itself as harmlessly as they could in friction. The Protected Barbette Gun-Carriage utilised this force by causing it to raise a counter-weight, while the gun descended below the parapet into safety and concealment from the enemy. This was effected by interposing between the gun and counter-weight a *moving* fulcrum. Any fixed fulcrum, such as a pivot, was found, no matter how strong, incapable of resisting the almost inconceivably rapid action of exploding gunpowder; it was torn from its position before it had time to move. But in the moving fulcrum there is nothing to be broken. The recoil of the gun rolls back the elevators, which raise the counter-weight, and this is retained in its position when the recoil is exhausted, and when released and allowed to fall restores the gun to the firing position again. It should be further observed that the gun commences to move in the direction of the recoil, gradually descending on a cycloidal curve, and thus the force of the recoil is not suddenly checked, but guided. This is one of the conditions essential to its success.*

The invention, which it is the design of this paper to discuss, effects a similar object for naval gun-carriages to that accomplished on land by the Protected Barbette Gun-Carriage. By it the gun is gradually lowered in its recoil below the surface of the deck of the vessel, and the force of the recoil is stored up to raise the gun at pleasure to its firing position above the deck. But the means by which it is accomplished are wholly different. There is no counter-weight and there is no moving fulcrum. It is manifest that at sea the fall and rise of a counter-weight could not be used. The direction of the force of gravitation is fixed and invariable, but the platform of a ship's deck is not always horizontal, and is often rapidly changing its inclination. In the Hydro-pneumatic Carriage, advantage is taken of the elasticity of air to store up and utilise the force of the recoil. A volume of air is compressed by the recoil of the gun, and being retained at pleasure in its compressed state, it is able to lift the gun to the position it occupied before the discharge; and as the moving fulcrum transferred the force of the recoil to the counter-weight without any check or jar, so in this naval carriage a like office is performed by water, which smoothly and effectually conveys the force of the recoil to compress the air in its reservoir. This is accomplished in a very simple manner, which can be easily understood by the accompanying diagram. The lines represent the gun and carriage in the firing position, the dotted lines in the loading position, showing the effect of the recoil. The gun is carried in a small triangular carriage (A, B, C), and this moves down through a quarter circle by the parallel motion of four strong bars, jointed on the carriage and on a platform on the lower deck of the vessel. In the firing position this part of the apparatus is supported by a plunger (D, E) or piston, and by the descent of the carriage this plunger is forced into a cylinder filled with water. This cylinder communicates with an air vessel or reservoir; consequently by the recoil a volume of water equal in bulk to the cubic content of the plunger is forced into the air reservoir; the volume of air is diminished by that amount and the pressure increased. The valve at F is self-acting in one direction only; it allows the water to flow into the air reservoir, but not to return. Consequently, when the recoil has exhausted its force in compressing the air, the gun remains in the loading position. When the valve G is turned, the water is driven from the reservoir by the compressed air, rushes into the cylinder, raises the plunger, and with it the gun and its carriage into the firing position. It should be observed that there is a circular portion of the upper deck, which, with the platform on the lower deck, to which the parallel bars are attached, traverses round on the plunger as a pivot, and so enables the gun to be pointed in any direction. In this rotating circular portion of the upper deck there is a rectangular opening which opens and closes by a pair of self-acting shutters with the rise and fall of the gun. A minute description of these minor arrangements is not here necessary.

One of the most remarkable features of this invention is the extraordinary power of adjusting the force employed to meet the recoil of the gun. By the descent of the plunger the air space is reduced by the cubic content of the plunger; and as the air space can be varied by admitting more or less water into the reservoir, so can the elastic force be varied to meet the recoil. For instance, did the air space in the reservoir equal the volume of the plunger, then the plunger could not possibly descend entirely into the cylinder, as that would compress the air to nothing. On the other hand, were the air space three or four times the volume of the plunger, then a small force of recoil would be sufficient to bring it completely down. The resisting force may thus be adjusted within almost any limits to meet the force of the recoil. It will be further observed that as in the Protected Barbette Carriage with counterweight, the recoil is met by a very slight resistance at the commencement, and is allowed to start in motion in its own direction. The first backward motion of the gun depresses the plunger very slightly, but as the recoil goes on it causes an increasingly rapid descent. For instance, the rollers must travel over two-thirds of the quadrant A H, to send the plunger half way down into the cylinder; the remaining one-third would send it down to the bottom. Thus the resistance to the recoil goes on increasing in a double progression, both from the increased pressure of the air as its volume is diminished, and also from the fact that the motion of the gun produces an increasing diminution of that volume.

Let us go through the process of adjusting the machine into working order. The reservoir and cylinder are both empty, the plunger is at its lowest descent, the gun lies down in the loading position. By a pump communicating with the reservoir, water is driven into the reservoir until the air space is reduced to one-half or one-third, as the case may be, of the volume of the plunger. This will raise but very slightly the plunger and its load. Now air must be pumped in till its pressure in the air space is sufficient to raise the plunger with the gun and its carriage into the firing position. The gun may then be brought down to the loading position.

This entire process need not be literally gone through in every case to adjust the machine. When once (if the expression may be borrowed) the "constants" of the carriage have been determined, *i.e.*, the amounts of air and water required in the reservoir, those quantities can be pumped in, keeping the valve G closed, and the gun remaining quiescent in the loading position. Water should be pumped in so as to leave a fixed air space, and then an amount of air, such that, with the added volume of the plunger, the pressure would still be sufficient to sustain the gun in the firing position. For a twenty-five ton gun the weight to be raised, including that of the plunger and the moveable part of the carriage, would be about thirty tons. Supposing the sectional area of the plunger to be two square feet, this would require a pressure of air of a little over 230lbs. on the square inch, or a little under sixteen atmospheres. When once adjusted, the carriage would remain in complete working order for days or weeks, in fact, until the water was allowed to run off.

While the main object and purpose to be accomplished

* While this article was going through the press, an attempt to make a gun descend an inclined plane by recoil and raise a weight, and so to form a protected Barbette carriage, was tried in the Royal Arsenal, and failed for this reason: The horizontal force of the recoil was not met in its own direction, but at an angle. The gun would not descend the inclined plane beyond a foot or two, and the violent concussion destroyed the elevating gear at the second shot.

by the hydro-pneumatic gun-carriage has been sketched, it is evident that all the applications of the principle of utilising the recoil as embodied in this engine have not been exhausted. The force of the recoil which must be exhausted in bringing the gun down to the loading position is very much greater than the force that would be required simply to raise the gun again to the firing position. The recoil takes place very rapidly, much more rapidly than it is desirable that the gun should rise again. If in the Protected Barbette Carriage it was allowed to return freely to its firing position, it would come up with an inconvenient or even a dangerous violence. The superfluous energy of the recoil stored up in the counter-weight must, therefore, be controlled and exhausted by friction bands. In the hydro-pneumatic carriage the return of the gun can be completely controlled by the valve or stopcock G, so as to bring it to the loading position as gently as desired, and the superfluous energy of the recoil will take the form of heat developed in the compressed air of the reservoir. But this superfluous energy may be seized and utilised. If a second and smaller plunger is attached to the cross head of the main plunger, which supports the gun and its carriage, with a cylinder and reservoir of its own, the power there accumulated may be used for any other purpose, as training the gun and carriage. In this case also, though the pressure produced and the heat generated in the main reservoir will not be so great as if there was no second plunger, still it alone will contain an ample store of force to bring the gun again into its firing position.

To discuss the military advantage of this invention, as a substitute on board ships of war for a turret weighing 300 tons of wrought iron, affording more complete protection to gun and gunners, and not weighing for a twenty-five ton gun more than sixteen tons altogether, would be travelling out of the realm of NATURE.

We have only endeavoured to show in this example with how great efficiency and docility Nature obeys those who understand how to direct her forces, and that all her work is not only efficient but instructive. If we can persuade her to undertake a new task she will teach us a new lesson. In the hydro-pneumatic gun-carriage a means supplies itself for measuring the exact amount of work done by the recoil. The compression of the air in the reservoir, and the heat generated in the process, will give accurate data for measuring the force exerted, and by this a step will be made towards measurements of the power of explosives with a precision hitherto unattainable.

Nature, like the great ancient fabulist, if she is compelled to be our slave, is resolved also to be our teacher.

NOTES

THERE is one part which neutrals may take in the Continental war. With no sympathy for those who have caused the war on either side, our sympathy is all the more due to those who innocently suffer from it on both sides. The following appeal, posted on the walls of every *mairie* in France, will touch other hearts than those of Frenchmen :—"*Appel à la France.*—Au nom de Dieu, au nom de la patrie, au nom de nos fils, de nos frères, de nos braves soldats tombés avec honneur sur le champ de bataille, et toujours héroïques quoique vaincus aujourd'hui, nous faisons un appel à tous les cœurs français. De grâce, donnez-nous de l'argent, du linge, des chemises, des couvertures, des vêtements, de flanelle, etc. Là-bas, sur nos frontières, l'élan des villes, les offrandes touchantes des villages ne suffisent déjà plus à nos chers blessés.—Les besoins sont immenses.—Le temps presse.—Donnez, oh! donnez vite! Envoyez les dons en nature et en argent au siége de la société à Paris, Palais de l'Industrie, porte No. IV." Here is a work in which all may unite—French, Germans, and neutrals, men of science, men of literature, men of business; and above all, our women. Nobly already have English, Irish, and Americans, surgeons, nurses, sisters of charity, come forward in the good work, but still it can only be as a drop in the ocean. To offer succour to the wounded and sufferers on both sides, to assuage as far as we can, the horrors of war, never exhibited on a more fearful scale than within the last few weeks, is now the duty of our more fortunate countrymen and countrywomen.

ANOTHER sacrifice of science to the war! The Congress of Alpine Geologists, the meeting of which we announced to take place on the 31st of this month, is adjourned to a more favourable time. It is probable also that the Congress of Anthropology and Pre-historic Archæology which it was proposed to hold at Bologna, and that of German naturalists to take place at Rostock, will not be held.

THE following sectional arrangements of the British Association are now announced :—A—MATHEMATICAL AND PHYSICAL SCIENCE (in the Crown Court, St. George's Hall) : President—J. Clerk Maxwell, F.R.S. L. and E. ; Secretaries—Prof. W. G. Adams ; W. K. Clifford ; Prof. G. C. Foster, F.R.S. ; Rev. W. Allen Whitworth. B—CHEMICAL SCIENCE (in the Royal Institution, Moore Street) : President—Prof. Henry E. Roscoe, PhD., F.R.S., F.C.S. ; Secretaries—Prof. A. Crum Brown, F.R.S.E., F.C.S. ; A. E. Fletcher, F.C.S. ; Dr. W. J. Russell, F.C.S. C—GEOLOGY (in the Concert Hall, Lord Nelson Street) : President—Sir Philip de Malpas Grey Egerton, Bart., M.P., F.R.S., F.G.S. ; Secretaries—W. Pengelly, F.R.S., F.G.S. ; Rev. H. H. Winwood, F.G.S. ; W. Boyd Dawkins, F.R.S., F.G.S. ; G. H. Morton, F.G.S. D—BIOLOGY (in the Reading Room and Lecture Room of the Free Public Library) : President—Prof. G. Rolleston, M.D., F.R.S., F.L.S. ; Vice-Presidents—John Evans, F.R.S., F.G.S., F.S.A. ; Prof. Michael Foster, M.D., F.L.S. ; Secretaries—Dr. T. S. Cobbold, F.R.S., F.L.S. ; Thos. J. Moore ; H.T. Stainton, F.R.S., F.L.S., F.G.S. ; Rev. H. B. Tristram, LL.D., F.R.S. E—GEOGRAPHY (in the Small Concert Room, St. George's Hall) : President—Sir Roderick I. Murchison, Bart., K.C.B., D.C.L., LL.D., F.R.S., F.G.S. ; Secretaries—H. W. Bates, Assist. Sec. R.G.S. ; Clements R. Markham, F.R.G.S. ; Albert J. Mott ; J. H. Thomas, F.R.G.S. F—ECONOMIC SCIENCE AND STATISTICS (in the Council Chamber, Town Hall) : President—Prof. Jevons ; Secretaries—E. Macrory ; J. Miles Moss. G—MECHANICAL SCIENCE (in the Civil Court, St. George's Hall) : President—Charles Vignoles, C.E., F.R.S., M.R.I.A., F.R.A.S. ; Secretaries—P. Le Neve Foster ; J. T. King.

THE Dutch Society of Sciences, of Haarlem, instituted last year, in addition to its ordinary prizes, two large gold medals, each of the value of 500 florins, one of which bears the name and effigy of Huyghens, the other of Boerhaave. These medals are to be awarded alternately, once in two years, to the *savant*, Dutch or foreigner, who shall have contributed the most, during the previous twenty years, to the progress of one particular branch of mathematical physics or of natural science. The Huyghens medal is to be devoted in 1874 to chemistry, in 1878 to astronomy, in 1882 to meteorology, in 1886 to mathematics, pure and applied. The Boerhaave medal is to be granted in 1872 to mineralogy and geology, in 1876 to botany, in 1882 to zoology, in 1884 to physiology, in 1888 to anthropology. The series will then recur. At their recent annual meeting the society made the first award of the Huyghens medal to M. Clausius for his discoveries in thermo-dynamics.

SIR FREDERICK POLLOCK, late Chief Baron, whose death is announced as having taken place on Tuesday last, in the 87th year of his age, was an amateur photographer of no mean ability, and had been President of the London Photographic Society ; he was an occasional contributor of articles on photography to the Philosophical Proceedings of the Royal Society.

He received his education at St. Paul's School and Trinity College, Cambridge, and was Senior Wrangler and First Smith's Prizeman in 1806.

THE Duke of Argyll writes to the *Times* to say that a remarkable meteor which was visible in the north of England on the 15th inst., was also seen at Inveraray. "It burst," the Duke says, "about 50 degrees above the horizon in the N.N.W., and ts great peculiarity was in the appearance presented by the luminous vapour which was the product of its explosion. This vapour was brighter than the tail of any comet—at first linear in shape—with sharp irregular projections. It was soon, however, curled up, as if by an atmospheric current, into the form of a horse-shoe, and in this form seemed to drift very slowly before the north-east wind in a south-west direction. It gradually, but very slowly, lost its brilliancy, remaining visible for more han a quarter of an hour." Two other correspondents of the *Times* describe a very brilliant meteor which was seen in Cambridgeshire on Saturday night.

A WATERSPOUT was observed off Calais on Saturday evening. From the edge of a thick black thunder-cloud two funnel-shaped projections were seen to depend, until they gradually reached the surface of the sea, on which they created a great disturbance, masses of foam rising up to a considerable height around the foot of each waterspout. It was calculated that the long streamers hanging down from the cloud to the sea were nearly a mile in length. The wind caused them to wave about gently and alter their form slightly from time to time. One of the waterspouts lasted about ten minutes, and the other about a quarter of an hour. During this time they moved rapidly along the sea.

ON Saturday evening, August 20, between 11 P.M. and midnight, a very beautiful aurora borealis was seen near Weobley, in Herefordshire, stretching from N.N.E. to N.W., and visible, with more or less distinctness, for about three hours.

THE *Engineer* states that an exhibition is about to be opened in Tromso, the capital of Finmark. The exhibition will contain the products and appliances used in the several fisheries, those of agriculture, and of mechanical and domestic industry, together with objects and products illustrative of the mode of life and state of civilisation of the inhabitants of those regions.

WE recorded recently the purchase by the Belgian Government of the Martius herbarium as the nucleus of a national collection. The same Government has now also purchased the Brussels Botanic Gardens from the Belgian Horticultural Society. The capital of Belgium has thus laid the foundation of a national establishment comparable to those of Paris and London.

THE Treasurer's report of the British Medical Congress, which has just been sitting for four days at Newcastle-on-Tyne, shows that out of an income of 5,000*l.* per annum it spends just 15*l.* on scientific research. It is stated that the greater part of the funds are required to keep afloat its own journal, and that a proposition from some of the members to publish an annual volume of Transactions found but little favour.

TWO instances are recorded in *Les Mondes* of somnambulism being perfectly cured by the administration of bromide of potassium. In one case, a woman of the age of twenty-four who had been subject to attacks two or three times a week for ten years, was operated upon; the dose given was two grammes of the bromide in seventy-five of water per diem, gradually increased to six grammes; the attacks became at once less and less frequent, and entirely ceased at the end of two months. In the other case, a girl of eight was the subject; one gramme was given morning and evening, and the cure was complete and immediate.

WE have received a specimen of paper manufactured entirely from wood, which is at least equal in colour and texture to the cheaper kinds of ordinary printing paper. There is no doubt that the pulp from the fibre of the fir and some other kinds of wood makes excellent material for paper, which can be prepared at a low price, the only practical difficulty being the high temperature and consequently the high pressure required to decompose the non-fibrous matter. Another material now actually employed for the manufacture of paper, is the husk and seeds of the cotton-pod from which the oil has been expressed, the fibrous pulp resulting from this operation is said to be an excellent paper-making material. The larger portion of the cheaper printing-papers used for newspapers, magazines, &c., is now made entirely from the Spanish Esparto grass, a name given to two distinct species, *Macrochloa tenacissima*, and *Lygeum spartum*, both growing abundantly on the shores of the Mediterranean; but the comparatively high price of this material, more than double what it was a few years since, affords a favourable opening for the introduction of other paper-making fibres.

PROFESSOR ORTON does not give a very encouraging account of the intellectual condition of Ecuador. He says:—"Ecuador boasts one university and eleven colleges, yet the people are not educated. Literature, science, philosophy, law, and medicine, are only names: there is not a single book-store in the city of Quito, and there are only four newspapers published in the whole of the Republic. In the schools the pupils study in concert aloud, Arab fashion." Yet Professor Orton adds that Chile has thought it worth her while lately to sign a convention with Ecuador "for an exchange of literary productions!"

THE Government of Nicaragua has sent an expedition under Mr. Sonnenstern, a civil engineer, to examine whether the River Coco can be made navigable. The report of Mr. Sonnenstern, which is favourable, has been published in the *Gazette* of Nicaragua. The river has hitherto been little known. The Indians are stated to be indolent and docile, and might, by contact with settlers, be civilised.

THERE is a rumour in California that a large quicksilver deposit has been discovered in the coast mountains eastward of San José.

THE Ecuador Government has decreed that in the capital and suburbs no house constructed of cane and straw shall be permitted, and that three months after the date of the decree all those existing shall be demolished. There was a former decree to this effect, which is thus fully enforced.

THE old Palace of Government at Lima, in Peru, is condemned, and a new one, which is to be a stone palace from the designs of M. Zoiles, architect and engineer, is to be built. In preparation, the ministerial departments have been removed from the old building.

THE Federal Government of the United States of Columbia has paid a debt to the state of Panama by the transfer of house property in Panama, which is to be converted into a college for that city.

THE Rev. W. A. Leighton, of Shrewsbury, author of the "Flora of Shropshire," is preparing for publication "A Lichen Flora of Great Britain, Ireland, and the Channel Islands."

WE have to acknowledge the receipt of "Hogg's Secret Code for Letters or Telegrams," with instructions for converting a message into ciphers, and for converting ciphers into a message.

THE Rev. Prof. Haughton reprints from the *Dublin Quarterly Journal of Medical Science* his paper "On the Muscular Forces employed in Parturition, their amount and mode of application."

KARL FRITSCH'S "Phänologische Beobachtungen aus dem Pflanzen und Theirreiche" contains an immense mass of observations taken in the neighbourhood of Vienna and other districts of Austria, in the year 1857 (but now for the first time published), on what may be termed periodical natural history, that is, the period of the first flowering of plants, and the first appearance of migratory birds, insects, and other animals of the summer season.

THE last volume (XIX.) of the Transactions of the Imperial Zoological-Botanical Society of Vienna contains, among its more important articles, contributions to the flora of Greece and Crete, by Dr. E. Weiss; a monograph of the genus *Botrychium*, by Dr. J. Milde (reducing the thirteen species in Moore's Index Filicum to ten); anatomical investigation of *Pleurophyllidia formosa*, by R. Bergh; a second contribution to the flora of Lower Austria, by Dr. A. Neilreich; observations on the metamorphosis of insects in the light of the theory of descent, by F. Brauer (a thoroughly Darwinian article); contributions to Hymenopterology, by Dr. J. Kriechbaumer; descriptions of several Myriapods in the Museum of Vienna, by MM. A. Humbert and H. de Saussure; the Lichens of the Tyrol, by F. Arnold; zoological notes, by G. Ritter von Frauenfeld; contributions to the Fish-fauna of Trans-baikal, by B. N. Dybowski.

IN the "Arbeiten aus der Kiel Institut" we observe that Klünder has been making further investigations into the time occupied in muscular contraction. His experiments have been conducted with a pendulum chronoscope constructed by Hensen. The contraction is traced on a reddened glass plate attached to the arm of a tuning fork, with which it therefore vibrates when this is sounded. The curve described is consequently a sinuous line, its ascending and descending portion decussating. If a vertical median line be drawn on the plate when at rest, the measurements can be examined and compared. These give for the stage of latent excitation a value of $\frac{3}{400}$ths of a second, which, when the muscle is weighted or exhausted, may rise to more than 0·01 sec. Antecedent extension diminishes the duration of this period, as Helmholtz had already remarked. The proper curve of contraction exhibits itself in its middle part as a curved line modified by the elasticity of the muscle. The muscle is quite inactive towards the end of contraction, as shown by the form of the extremities of the curve. The greatest increase in rapidity occurs in the ascending portion of the curve, which corresponds to the greatest development of force in the muscle which is between the 3rd and 4th 1-400 of a sec., the absolute greatest rapidity of the ascent is in the 8th 1-400 sec. The form of the curve is, considerably changed if a heavy weight is appended to the muscle, the period of elevation as well as the fall being both longer. The retardation occurs principally at the commencement of the elevation, at which period the rapidity only slowly increases, as compared with its usual rate.

THE COMING TRANSITS OF VENUS*

TRANSITS of Venus over the disc of the sun have more than any other celestial phenomena occupied the attention and called forth the energies of the astronomical world. In the last century they furnished the only means known of learning the distance of the sun with an approach to accuracy, and were therefore looked for with an interest corresponding to the importance of this element. Although other methods of arriving at this knowledge with equal accuracy are now known, the rarity of the phenomenon in question insures for it an amount of attention which no other system of observation can command. As the rival method, that of observations of Mars at favourable times, requires, equally with this, the general co-operation of astronomers, the power of securing this co-operation does in itself give the Transits of Venus an advantage they would not otherwise possess.

Although the next transit does not occur for four years, the preliminary arrangements for its observation are already being made by the governmental and scientific organisations of Europe. It is not likely that our Government will be backward in furnishing the means to enable its astronomers to take part in this work. The principal dangers are, I apprehend, those of setting out with insufficient preparation, with unmatured plans of observation, and without a good system of co-operation among the several parties. For this reason I beg leave to call the attention of the Academy to a discussion of the measures by which we may hope for an accurate result.

In planning determinations of the solar parallax from the Transits of Venus, it has hitherto been the custom to depend entirely upon the observations of the internal contact of the limbs of the sun and planet proposed by Halley. It is a little remarkable, that while astronomical observations in general have attained a degree of accuracy wholly unthought of in the time of Halley, this particular observation has never been made with a precision at all approaching that which Halley believed that he himself had actually attained. In his paper he states that he was sure of the time of the internal contact of Mercury and the sun within a second. The latest observations of a transit of Mercury, made in November 1868, are, as we shall presently see, uncertain by several seconds. It is also well known that the observations of the last transit of Venus, that of June 1769, failed to fix the solar parallax with the certainty which was looked for, the result of the standard discussion being now known to be erroneous by one-thirtieth of its entire amount. One of the first steps to carry out the object of the present paper will be an inquiry into the causes of this failure, and into the different views which have been held respecting it.

The discrepancies which have always been found in the class of observations referred to, when the results of different observers have been compared, has been generally attributed to the effect of irradiation. The phenomenon of irradiation presents itself in this form: When we view a bright body, projected upon a dark-ground, the apparent contour of the bright body projects beyond its actual contour. The highest phenomenal generalisation of irradiation which I am aware of having been reached is this: A lucid point, however viewed, presents itself to the sense, not as a mathematical point, but as a surface of appreciable extent. A bright body being composed of an infinity of lucid points, its apparent enlargement is an evident result of the law just cited.

[The speaker here drew a number of diagrams for the purpose of illustrating his theory.]

The following diagrams show the effect of this law upon the time of interval contact of a planet with the disc of the sun. The planet being supposed to approach the solar disc, Fig. 1 shows the geometrical form of a portion of the apparent surface of the sun, or the phenomenon as it would be if there were no irradiation immediately before the moment of internal contact. Fig. 2 shows the corresponding appearance immediately after the contact. To indicate the effect of irradiation, or to show the phenomenon as it will actually appear on the theory of irradiation, we have only to draw an infinity of minute circles for each point of the sun's disc visible around the planet to indicate the apparent phenomenon. The effect of this is shown in Figs. 1*a* and 2*a*. The exceedingly thin thread shown in Fig. 1 is thus thickened as in 1*a*, and the sharp cusps of Fig. 2 are rounded off as shown in Fig. 2*a*. The apparent radius of the planet is diminished by an amount equal to the radius of the circle of irradiation, and the radius of the sun is increased by the same amount. Comparing Figs. 1*a* and 2*a*, it will be seen that the moment of internal contact is marked by the formation of a ligament, or "black drop," between the limbs of the sun and the planet. This formation is of so marked a character that it has been generally supposed there could be little doubt of the moment of its occurrence. The remarks of the observers have given colour to this supposition, the black drop being generally described as appearing suddenly at a definite moment.

Examining Fig. 2*a*, it will be seen that the planet still appears entirely within the disc of the sun. The geometrical circle which bounds the latter, and that which bounds the planet, instead of touching, are separated by an amount equal to double the irradiation. And, when they finally do touch, neither of them will be visible at the point of contact. The estimate of the moment of contact must therefore be very rough, the means of estimating being far less accurate than those afforded by a common filar micrometer. In the actual case the eye has to continue the two circles to the point of contact by estimation, through a distance depending on the amount of irradiation, while measures with a micrometer are made by actual contact of a wire with a disc. Such estimates have, therefore, been

* Substance of a paper read before the Thirteenth Annual Session of the American Academy of Sciences, held at Washington, by Prof. Simon Newcomb. (The original paper was illustrated by diagrams.)

generally rejected by investigators, not only from their necessary inaccuracy, but because the time of "apparent contact" depends upon the amount of irradiation, which varies with the observer and the telescope. If there is no irradiation at all, the time of apparent contact and that of true contact will be the same, as shown in Fig. 2, while, when the cusps are enlarged by irradiation, apparent contact will not occur until the planet has moved through a space equal to double the irradiation.

Let us return to the phenomenon at actual contact. According to the theory as it has been presented, the formation or rupture of the black ligament connecting the dark body of Venus with the dark ground of the sky is a well-marked phenomenon, occurring at the moment of true internal contact. This was, I believe, the received theory until Wolf and André made their experiments on artificial transits in the autumn and winter of 1868 and 1869. They announced, as a result of these experiments, that the formation of the ligament was not contemporaneous with the occurrence of internal contact, but followed it at the ingress of the planet, and preceded it at egress. In other words, it appeared while the thread of light was still complete. They furthermore announced that with a good telescope the ligament did not appear at all, but the thread of light between Venus and the sun broke off by becoming indefinitely thin.

The result is not difficult to account for. Irradiation has already been described as a spreading of the light emitted from each point of the surface viewed, so that every such point appears as a small circle. The obvious effect of this spreading is a dilution of the light emitted by a luminous thread, whenever the diameter of the thread is less than that of the circle of irradiation. In consequence of this dilution, the thread may be invisible while it is really of sensible thickness, a given amount of light producing a greater effect on the eye the more it is concentrated. Since the thread of light must seem to break when it becomes invisible at its thinnest point, the formation or rupture of the thread marks, not the moment of actual contact, but the moment at which the thread of light becomes so thick as to be visible, or so thin as to be invisible. The greater the irradiation, and the worse the definition, the thicker will be the thread at this moment.

An interesting observation, illustrative of this point, was made by Liais at Rio Janeiro, during the transit of Mercury of November 1, 1868. He had two telescopes, one much smaller than the other. He watched the planet in the small one till it seemed to touch the disc of the sun, then looking into the large one, he saw a thread of light distinctly between the planet and the sun, and they did not really touch until several seconds later.

Reference to the figures will make it clear that there is no generic difference between the phenomenon commonly called the rupture of the black drop and that of the formation of the thread of light. If the bright cusps are much rounded, as in Fig. 1*a*, the appearance between them is necessarily that of a drop, while if they are seen in their true sharpness, as in Fig. 1, the form of the drop will not appear. It has been shown that with different instruments the phenomenon of contact may exhibit every gradation between these extremes. The only well-defined phenomenon which all can see is the meeting of the bright cusps and the consequent formation of the thread of light at ingress, and the rupture of the thread at egress.

To recapitulate our conclusions—

1. The movement of observed internal contact at ingress is that at which the thread of light between Venus and the sun becomes thick enough to be visible.

2. The least visible thickness varies with the observer and the instrument, and, perhaps, with the state of the atmosphere.

3. The apparent initial thickness of the thread varies with the irradiation of the telescope.

Two questions are now to be discussed. The observed times varying with the observer and the instrument, we must know how wide the variation may be. If it be wide enough to render uncertain the results of observation, we shall inquire how its injurious effects may be obviated.

The first question can be decided only by comparison of the observations of different observers upon one and the same phenomenon. For such comparison I shall select the observations of the egress of Mercury on the occasion of its last transit over the disc of the sun. This selection is made for the reason that this egress was observed by a great number of experienced observers with the best instruments, while former transits, whether of Venus or Mercury, have been observed less extensively or at a time when practical astronomy was far from its present state of perfection, and that the transit in question would therefore furnish much better data of judging what we might expect in future observations. The comparison was made in the following way:—I selected from the "Astronomische Nachrichten," the "Monthly Notices of the Royal Astronomical Society," and the "Comptes Rendus," all the observations of internal contact at egress which there was reason to believe related to the breaking of the thread of light, and which were made at stations of known longitude. Each observation was then reduced to Greenwich time, and to the centre of the earth.

From these comparisons it appears that the contact was first seen by Le Verrier, at Marseilles, at two seconds before nine o'clock, Greenwich time. In one second more it was seen by Rayet at Paris, Oppolzer at Vienna, Lynn at Greenwich, and Kaiser at Leyden. The times, noted by twenty other observers, are scattered very evenly over the following fifteen seconds. Kam and Kaiser, at Leyden, did not see the contact until nineteen and twenty-four seconds past nine.

It thus appears that among the best observers, using the best instruments, there is a difference exceeding twenty seconds between the times of noting contact. This difference corresponds to more than a second of arc, so that really these observations were scarcely made with more accuracy than measures under favourable circumstances with a micrometer, and are not therefore to be relied on. But a great addition to the accuracy of the determination could be made by measures of the distance of the cusps, while the planet was entering upon the disc of the sun. It would tend greatly to the accuracy of the results, if the observers should meet beforehand with the telescopes they were actually to use in observing the transit and make observations in common on artificial transits. It would be a comparatively simple operation to erect an artificial representation of the sun's disc at the distance of a few hundred yards, and to have an artificial planet moved over it by clock-work. The actual time of contact could be determined by electricity, and the relative positions of the planet and the disc by actual measurement. With this apparatus it would be easy to determine the personal errors to which each observer was liable, and these errors would approximately represent those of the observations of actual transit.

Still it would be very unsafe to trust mainly to any determination of internal contact. Understanding the uncertainty of such determinations, the German astronomers have proposed to trust to measures with a heliometer, made while the planet is crossing the disc. The use of a sufficient number of heliometers would be both difficult and expensive, and I think we have an entirely satisfactory substitute in photography. Indeed, Mr. De la Rue has proposed to determine the moment of internal contact by photography. But the result would be subject to the same uncertainty which affects optical observations—the photograph which first shows contact will not be that taken when the thread of light between Venus and the sun's disc was first completed, but the first taken after it became thick enough to affect the plate, and this thickness is more variable and uncertain than the thickness necessary to affect the eye. We know very well that a haziness of the sky which very slightly diminishes the apparent brilliancy of the sun, will very materially cut off the actinic rays, and the photographic plate has not the power of adjustment which the eye has.

But, although we cannot determine contacts by photography, I conceive that we may thereby be able to measure the distance of the centres of Venus and the sun with great accuracy. Having a photograph of the sun with Venus on its disc, we can, with a suitable micrometer, fix the position of the centre of each body with great precision. We can then measure the distance of the centres in inches with corresponding precision. All we then want is the value in arc of an inch on the photograph plate. This determination is not without difficulty. It will not do to trust the measured diameters of the images of the sun, because they are affected by irradiation, just as the optical image is. If the plates were nearly of the same size, and the ratio of the diameters of Venus and the sun the same in both plates, it would be safe to assume that they were equally affected by irradiation. But should any show itself, it would not be safe to assume that the light of the sun encroached equally upon the dark ground of Venus and upon the sky, because it is so much fainter near the border.

If the photographic telescope were furnished with clock-work, it would be advisable to take several photographs of the Pleiades belt, before and after the transit, to furnish an accurate standard of comparison free from the danger of systematic error. There is little doubt that if the telescopes and operators practise together, either before or after the transit, data may be obtained for a satisfactory solution of the problem in question.

To attain the object of the present paper, it is not necessary to enter into details respecting choice of stations and plans of observation. I have endeavoured to show that no valuable result is to be expected from hastily-organised and hurriedly-equipped expeditions; that every step in planning the observations requires careful consideration, and that in all the preparatory arrangements we should make haste very slowly. I make this presentation with the hope that the Academy will take such action in the matter as may seem proper and desirable.

NORTH OF ENGLAND INSTITUTE OF MINING AND MECHANICAL ENGINEERS—MEETING AT GLASGOW

AFTER having had an existence of some fifteen or sixteen years, during which it has done a large amount of scientific and thoroughgoing practical work, this North Country Institute has just deviated from its usual practice of holding its meetings in Newcastle-on-Tyne, and, with the co-operation of the Scottish Engineers and Shipbuilders' Association, has held a very successful meeting in Glasgow, the centre of the great Scottish coal-field, and the head-quarters of the mining and engineering industries of Scotland, and of the shipbuilding industry of the United Kingdom. The meeting began on Tuesday, the 9th of August, and extended over four days. On the opening day the Lord Provost of the City of Glasgow received and formally welcomed the members of the North of England Institute in name of the citizens and the Institution of Engineers and Shipbuilders; and thereafter the chair was taken by Mr. E. F. Boyd, President of the North of England Institute, and the business of the meeting commenced.

There were set down for reading and discussion no fewer than eighteen papers, the subjects treated of being all directly connected with mining and mechanical engineering. Only three papers were overtaken on the first day, when it was found that the time for adjournment had arrived. The afternoon was spent by the members in visiting various engineering and shipbuilding works, and other manufacturing establishments, which were freely thrown open to inspection by their respective proprietors.

We shall here briefly indicate the nature of the papers read and discussed at Tuesday's sitting of the Institute.

1. "On the Geology of the Coal Measures of Scotland," by Mr. James Geikie, district surveyor of the Geological Survey of Scotland. The author described, first, the calciferous sandstone series, which, when typically developed, consists of two groups of strata, the lower pointing to the prevalence of marine conditions during the deposition of the red sandstones and conglomerates, and the upper showing that during its accumulation marine and brackish water conditions alternated with the occasional appearance of land surfaces. Volcanoes were somewhat prevalent during the deposition of both groups. Second: The carboniferous limestone series, consisting of a lower group indicating marine conditions and occasional old land surfaces; a middle group indicating frequent land surfaces, and alternate brackish water and marine conditions; and an upper group pointing chiefly to lower marine conditions, with occasional brackish water deposits and a few old land surfaces: both submarine and subaërial volcanoes very active during the deposition of the whole series. Third: The millstone grit, deposited under almost exclusively marine conditions. Fourth: The coal measures, showing a prevalence of brackish or freshwater conditions, with abundant land surfaces, and speaking also of occasional inroads of the sea. No igneous rocks contemporary with the coal measures or millstone grit. Fifth: Intrusive rocks of three classes, namely, intrusive sheets, referable to close of "coal measures" group; bosses or pipes of tuff or agglomerate, probably of Permian age; and dykes of dolerite of Miocene ages. Sixth: Two systems of faults of different ages; the oldest striking N.E. and S.W., and the other, a double set, striking approximately E. and W. and N. and S. Mr. Geikie, in concluding, referred to the exceeding richness of the variety shown by the phenomena of the Scottish carboniferous formation, and said he had no hesitation in affirming that, when the palæontological and geological history of the carboniferous rocks of Scotland were worked out, there would be prepared one of the most important chapters in the physical history of the country.

2. "On the Magnetic Ironstone of Rosedale Abbey, Cleveland," by Mr. John Marley, M.E. This paper treated of the extraordinary deposit of magnetic ironstone which occurs at Rosedale West, and forms a very peculiar feature in the famous Cleveland ironstone, regarding which various papers have been published since 1857, when Mr. Marley first drew the attention of scientific men to it. The magnetic stone occurs quite isolated in two troughs, one of which is 90 feet deep, and it contains, in the best specimens, from 42 to 50 per cent. of metallic iron. In 1857 its extent was unproved, but it was believed to be very great, but this is now known not to be the case, from the results of recent borings and explorings which Mr. Marley fully detailed. The author explained the curious geological relationship which the magnetic stone bears to the top bed of the lias ironstone of Cleveland. The troughs lie east and west. Icebergs and glacial action were, in his opinion, in no way connected with the induction of the magnetic state, nor yet with the formation of the troughs. The deposits are not two beds of regular strata, nor are they veins, as no fissures have yet been found in the bottom of the troughs, although they have been diligently looked for. Mr. Isaac Lowthian Bell supplemented Mr. Marley's description by giving the results of a visit which he had paid to Rosedale, and stated that the magnetic ore could not be the result of volcanic action, as carbon was always contained in the analysis, as also water of hydration and a notable quantity of carbon.

3. "On the Duty of Cornish and other Pumping Engines," by Mr. J. B. Simpson. This paper was of especial interest on account of the subject treated of in it having a most intimate connection with the economy of fuel and the duration of the coal supply. After describing fully a Cornish engine recently erected in the Newcastle district, the author entered upon an examination of the details of twelve different kinds of engines, and compared their merits with those of the Cornish engine. In conclusion, he said that taking those engines into consideration, their average duty corresponded to a consumption of 14lb. of coal per horse-power per hour. Were a duty of 4lb. obtained, the saving in these engines alone would represent 40,000 tons of coal per annum, or, at 3*s*. per ton, 6,000*l*. The assumed total horse-power of pumping-engines in the Newcastle district is about 10,000, and from this the amount of the possible annual saving may easily be calculated. In many places coal may not be worth 3*s*. per ton at the pit mouth, but in the majority of cases its value is much greater. It is too much the practice to regard coal at the colliery as of little or no value, and that the extra 10lb. or 12lb. per horse-power per hour is not worthy of consideration. But fuel is not the only pecuniary part of the question, as extra consumption of coal means additional water, additional repairs, additional wear and tear, and additional manual labour—and these in the aggregate are very serious items of cost. The time does not seem far off when, in pumping and other colliery engines, the effective duty of 2lb. or 3lb. of coal per horse-power per hour will be considered as important as in the engines of London water-works and ocean steam-ships.

In the evening of Tuesday a *conversazione* was held in the Corporation Galleries. The east and west halls were occupied by numerous collections of objects—geological, palæontological, mineralogical, metallurgical, chemical, mechanical, engineering, mining, &c., together with a magnificent display of photographs, by Annan, of the Old Glasgow College, and various engineering works and Clyde-built ships. This exhibition was of immense scientific and industrial interest, and was at once the most extensive and valuable that has been held in Glasgow for many years. Advantage was taken of this evening's meeting to perform an interesting ceremony, namely, the presentation of a marble bust to Professor W. J. Macquorn Rankine, C.E., F.R.S., first president of the Institution of Engineers in Scotland, as a token of the appreciation and esteem of the members. A duplicate copy of the bust was also presented to the institution as a memorial of the Professor's labours in promoting the success of the institution. In the Large Upper Gallery there was an exceedingly interesting exhibition by means of the oxy-hydrogen light, of sections of fossil corals, by Mr. James Thomson, F.G.S., a gentleman who has of late years gone most extensively and enthusiastically into the study of fossil corals, and made it almost entirely his own; and so fully persuaded are palæontologists of the great value of his investigations, that Mr. Thomson is assisted by a grant from the British Association, at the forthcoming meeting of which he is to present a second report and exhibit his wonderful series of specimens.

On Wednesday morning the reading of papers was resumed, Mr. E. F. Boyd again presiding. The following is a brief notice of the papers overtaken :—

1. "On the Economical Advantages of Mechanical Ventilation," by Mr. D. P. Morison. The author stated that tabulated results of experiments recently made showed that the saving effected in the consumption of fuel varied in most cases from 40 to 80 per cent. in favour of mechanical ventilation as compared with furnace ventilation. The latter had other disadvantages, such as (1) the danger of an open fire in a fiery seam ; (2) in order to avoid that danger, the necessity and serious cost of constructing a dumb drift to convey the return air to the upcast shaft, and the fact that a large amount of fresh air is required to feed the furnaces, while it is of no value in the workings themselves ; (3) the serious fact that the upcast shaft, being usually heated to nearly its practical maximum, cannot, in cases of necessity (such as a sudden fall of the barometer, an unexpected occurrence of a large discharge of fire-damp, or a block in the air-ways), be made at once available for an increased duty ; (4) the inordinate wear and tear upon furnaces, arches, bars, and the shaft lining, whether brick-casing or tubbing, and in case of a coal-drawing upcast shaft, the deterioration of the ropes, guides, cages, and other plant. In no case could the furnace compete successfully with mechanical ventilation. Even in the deepest of English mines the advantage of mechanical ventilation is shown by the economy in fuel being from 35 to 40 per cent. over that required for furnace ventilation. Mr. Morison described various mechanical ventilators, including those of Struve, Nasmyth, Lemielle, Waddle, Guibal, and others. He expressed himself as most in favour of the Guibal ventilating fan, the one most in use both on the Continent and in this country. An interesting discussion followed Mr. Morison's paper, remarks being made by Messrs. Lupton (Leeds), Steavenson (Durham), William Cochrane and Simpson (Newcastle), Barclay (Kilmarnock), Marley (Darlington), Harvey (Glasgow), and others.

2. "On J. Grafton Jones's Coal-getting Machine," by Mr. Arnold Lupton, Leeds. After describing the machine in question, and specially dwelling upon its involving the use of the hydraulic wedge and a drilling apparatus, Mr. Lupton claimed for it the following advantages :—1st, The safety with which mines can be worked by it as a substitute for gunpowder ; 2nd, The superior shape of coal got by the wedge as compared with that got by blowing, and the less amount of slack made ; 3rd, The improvement in the health of the miners likely to ensue on the disuse of gunpowder ; 4th, The saving in labour by using the hydraulic wedge instead of hammer-driven wedges ; 5th, The saving in labour and diminution in the amount of slack made by using the hydraulic machine to push the coal out of the solid, in working those seams whose nature is such as to render it possible. Mr. Lupton stated that Jones's powerful machine is now in use, pushing coal out of the solid without any holing or natural breaks in the seam, at Kiveton Park Colliery, in South Yorkshire. The seam is five feet thick, and the coal is very hard, but by the use of the hydraulic wedge blocks are got four yards long and four feet wide—each about eight tons weight—at one application of the machine. In the course of the discussion which followed various other important and interesting facts were evoked.

3. "On an Expansive Double-cylinder Pumping Machine," by Mr. Andrew Barclay, Kilmarnock.

4. "On an Expansive, High-pressure, Cut-off Slide Valve," by the same.

5. "On a New Coal-getting Machine," by Mr. George Simpson, Glasgow. The author of this paper dwelt at some length on the working of coal on the "long-wall" system by machinery, and then explained the nature of the machinery which he thought most suitable for it. The essential feature of the machine, exhibited and described by Mr. Simpson, is a somewhat saw-like blade which works into the face of the coal seam in a horizontal manner. Mr. Simpson said it was indispensable that the tool to be used should be durable and easily removed and replaced in case of blunting or breakage, and he claimed that his cutter possessed those qualifications. He also showed an application of an endless chain for driving the proposed machinery, and which might be worked by an engine on the drawing road at the face of the coal, or from the bottom of the shaft or other convenient point.

6. "On the Utilisation of Blast Furnace Gases, Coal being used as the Fuel," by Mr. William Ferrie, Monklands, Iron and Steel Works, Lanarkshire. The author stated that it had occurred to him that if raw coal could be coked in the blast furnace as in a common gas retort, the difficulty of withdrawing the furnace gases for use would be overcome, and he immediately commenced experiments with a small blast furnace, one-fifteenth of the capacity of a 50-feet furnace. The upper part was divided into two compartments or retorts into which the coal, ores, and limestone flux were charged ; and the top of the furnaces was closed in by the ordinary bell and cone arrangement, as in the Cleveland district. The gases passed off into a main which communicated by two pipes, one to each side of the furnace, to the entrance of the flues at the bottom of the retorts, and were ignited by the aid of atmospheric air. These flues were spiral, in order that the heat from the burning gases might permeate the materials inside the retorts, while the exhaust gases were thrown off by chimneys at the top of the retorts. This small furnace was worked for about two months with raw coal only as fuel, and the results were highly satisfactory, notwithstanding that the furnace was so small in size. Being convinced that this plan of working a furnace was practicable, Mr. Ferrie had forthwith commenced the alteration of one of the ordinary furnaces at Monkland Works, which he said would be ready for operating with at the end of the month of August.

7. "On Mineral Oil Works," by Mr. David Cowan. The author referred to the manufacture of mineral oils as one of the leading industries of Scotland ; to the nature and extent of the oil yielding materials, namely, bituminous shales and cannel coals, distributed throughout the Scottish coal measures ; and to the quantity and quality of the produce from those materials. He afterwards described the mode of treating the raw materials, referring to the horizontal and the vertical retorts used in Scotland, comparing their advantages and disadvantages, and then described an arrangement of apparatus designed to combine the advantages of both kinds of retorts, and which would at the same time admit of improved facilities of workings. In order to improve the mode of firing, Mr. Cowan suggested that instead of coal the retorts should be heated with gas flame, and further, that the system of first converting the fuel into gas (as successfully worked out by Siemens) should be adopted. He estimated that the mode of heating by gas in this way would effect a saving of from 40 to 50 per cent. of the fuel.

The time allotted for reading and discussing papers having now arrived, the president announced that those papers which had not been overtaken, would become the joint property of the two institutions. They will doubtless be published along with the others in the transactions to be issued by each institution. The afternoon was spent in the same way as that of the preceding day ; and in the evening a grand banquet was held, at which the members of the North of England Institute were the principal guests. Thursday was occupied in visiting collieries, ironworks, engineering, ship-building, chemical, and other manufacturing establishments at a distance from Glasgow ; and on Friday there was an excursion on the saloon steamer Chancellor down the Clyde to Dunoon, and thence up Lochlong to Arrochar, from which the party walked or rode over to Tarbet, on Loch Lomond, a distance of about two miles. The visitors were then conveyed by one of the Loch Lomond Company's steamers to the top of that loch, "The Queen of Scottish lakes," where dinner was served. In the afternoon the whole length of the loch was traversed to Balloch, where the party took train and returned by the Dumbartonshire Railway to Glasgow. All the visitors were greatly delighted with the magnificent scenery along this route, as well as with the kindness, attention, and hospitality shown to them during their four days' sojourn in the "land o' cakes." Altogether, a very great degree of pleasure was experienced both by the members of the North of England Institute and by the members of the Scottish Institution. Not unlikely a return visit will soon be paid to the "Coaly Tyne."

JOHN MAYER

SCIENTIFIC SERIALS

THE *Revue des Cours Scientifiques*, for August 13, commences with a translation of Dr. Carpenter's lecture on the Temperature and Animal Life of the Deep Sea. This is followed by a report of Prof. Milne-Edwards's address in *comité secret* of the Academy in favour of the election of Mr. Darwin as corresponding member, the substance of which we gave last week. The last paper is M. Ed. von Beneden's very important and interesting article on *commensalisme*, or "fellow-boarding," as it has been termed, in the animal kingdom, extracted from the proceedings of the Belgian Academy of Sciences, a translation of which has already been published in this country. The current number for August 20 opens with an article *à propos* of the war, a translation of Prof. Shaw's address to the Military College at Sandhurst on the war establishments of Great Britain. We have then the continuation of M. Marey's paper on the Flight of Birds; and, in conclusion, under the head of "Bibliography," a translation of Mr. Wallace's review of Mr. J. J. Murphy's "Habit and Intelligence," which appeared in our columns.

THE *American Entomologist and Botanist* publishes a double number for July and August, which is occupied by short descriptive articles of interest and value principally to American collectors and students. The article of chief general interest is one on the "Origin of Prairie Vegetation," consisting of an able criticism of Prof. Winchell's theory that the prairies are of lacustrine origin, and that we must look to the source of the prairie vegetation from without,—probably the remains of a pre-glacial flora, the germs of which have remained stored up during subsequent epochs, and come again to life whenever the diluvial surface is again exposed. The writer of the article maintains that there is no need to go so far back as the diluvial period for the origin of the prairie vegetation. Dr. Hale, of Chicago, mentions the interesting fact that the *Ranunculus cymbalaria*, an abundant plant of the eastern sea coast and of the salt springs in the State of New York, is found in great abundance at Chicago, and for several miles along the shores of Lake Michigan, though nowhere else on the Great Lakes. It appears, however, that it also grows on the muddy banks of some of the western rivers.

THE *Geological Magazine* for the present month (No. 74), contains only three original articles, namely, one by Mr. D. Mackintosh on the Dispersion of Shapfell Boulders, and Origin of Boulder Clay; a second by Mr. John Hopkinson on the structure and affinities of *Dicranograptus* (with a plate), including descriptions and figures of the British species of that genus, two of which (*D. formosus* and *D. nicholsoni*) are described as new; and a memoir, with two plates, by Mr. T. Davidson, on Italian Tertiary Brachiopoda, with an important table of the species and their geological distribution. Among the abstracts and notices of memoirs, is a report of an interesting lecture on the Primæval Rivers of Britain, by Professor T. Rupert Jones.

Mittheilungen aus Justus Perthes' Geographischer Anstalt (vol. xvi., No. 8) opens with a remarkably interesting paper —illustrated by a map—by Dr. G. Nachtigal, on his travels in Tibesti. He says that, in spite of Barth's philological investigations, he regards the question as to the nature of the Tibbu as still undecided. They are of middle height, are very well built, and possess elegant yet muscular limbs. The majority of them are of a deep bronze colour, but without a trace of what is usually termed the negro physiognomy. On the whole, their physical and psychical peculiarities, their social and political arrangements, and their manners and customs, resemble those of the Berber infinitely more than those of the Negro. Amongst other things, Dr. Nachtigal records some careful observations of the rivers Zuar and Marmar, the former of which he regards as incomparably the finest river in Tibesti. In M. Lejean's article on his own travels in European Turkey in 1869, he corrects the existing maps in several points, embodying in an elaborate map the results of his investigations. He expresses the greatest contempt for the modern Turks, intimating that those who believe they have recently made real progress are deceived by mere appearances. He says he has gathered full materials for a work or works on the ethnography and archæology of the districts he describes. Professor Pellegrino Strobel describes a journey from the Planchar Pass to Mendoza; and the rest of the number is made up of "Geographical Notices" and translations of extracts from Mr. Robert Brown's "Physical Geography of the Queen Charlotte Islands," and from reports published in the "South Australian Register," on Mr. G. W. Gogden's Measuring Expedition to North Australia.

SOCIETIES AND ACADEMIES

BRISTOL

Observing Astronomical Society.—Report of observations made by the members during the period from July 7th to August 6th, 1870, inclusive:—*Solar Phenomena.*—Mr. Thomas G. E. Elger, of Bedford, reports that the sun spots observed in July exceeded in number those recorded during the previous month, but they were, with a few exceptions, small (less than 30" in diameter), and although pretty equally distributed between the two hemispheres, those to the south of the sun's equator presented a remarkable contrast, both in type and size, to those observed to the north of it; the former, as in June, included some large scattered groups and moderately sized spots of the normal class, while the latter consisted chiefly of solitary specks without penumbræ, and clusters of minute black punctures which frequently assumed very grotesque configurations. A striking feature of the large groups observed during the early part of the month was an evident tendency either to close up or to become dissociated upon reaching a certain position on the disc—about half way between the E. limb and the centre. On the 25th one of the largest groups observed this year appeared at the E. limb; on the 28th it measured nearly 5' in length, and consisted of a large preceding spot 1' 10" in diameter, followed by a straggling train of "wispy" penumbræ enclosing several small spots. This group dwindled away very rapidly after the 28th. Another large spot, about 50" in diameter, was observed from July 13th—25th.

Fresh groups observed in the sun's N. hemisphere during July	9
Fresh groups observed in the sun's S. hemisphere during July	12
Maximum number of groups on disc	12 (July 13, $5^h\ 30^m$)
Minimum number of groups on disc	4 (July 28, $5^h\ 10^m$)

Mr. Albert P. Holden, of London, says, "I observed a very interesting spot on June 21, at 7 A.M. The penumbra was unusually pale, and the umbra of a decided light brown hue. Four darker openings arranged in a square were observed in the umbra, and were readily seen with a very small aperture. A very remarkable circumstance in connection with the sun spots during the last two months has been their extremely light colour. The light brown tints of the umbra have been very marked, and totally different from the dark hues they usually present; while, at times, the penumbræ have been so light as to be scarcely visible. In most of them, however, the nucleus (which is ordinarily so difficult to detect) has been very easily seen, as in the case of the foregoing observation. The fact proves the phenomenon seen to be due to the actual lightening of the spots themselves, because if it were merely an optical or atmospheric effect, the whole spot would be lighter and the nucleus would be quite as difficult to detect as before. It is probable that these appearances may be a necessary result of the maximum of sun-spot activity, and are due (as suggested by Mr. Lockyer) to the thinness of the solar envelope at the present time. This would certainly account for the light hues of the umbræ and penumbræ, and also for the frequency and blackness of the nucleus." Mr. Henry Ormesher, of Manchester, writes, "On the 31st of July, from $2^h\ 15^m$ to $3^h\ 0^m$, while looking at the sun with my 3 in. refractor, I saw a beautiful large cluster of spots occupying an almost central position on the disc. It occurred to me that the umbra in the largest spot appeared more dense on the western side. I therefore determined to examine it with my $5\frac{1}{4}$ in. refractor. I did so, using a power of 181. The result was that it resolved itself into a very fine nucleus of a somewhat oval shape. After making myself sure that the above was the case I examined the cluster generally, and was struck with the beautiful appearance of the brighter part of the sun's atmosphere. A very bright stream ran across the cluster, in a zigzag direction, separating the penumbra. Some parts of this stream, and particularly the upper part, appeared brighter than others, presenting a very mottled appearance."—Mr. William F. Denning, of Bristol, observed the sun, with his 3 in. refractor, on July 14, from $5^h\ 30^m$ to $6^h\ 10^m$. He noticed nine large well-defined *maculæ* on various parts of the disc. A particularly large and interesting group of spots was visible in the N. hemisphere. On July 22, at 8 P.M., a spot was observed in the same hemisphere, which was divided by two bridges of light. He noticed that the penumbra was invaded by numerous minute lines of light, and that the bridges seemed to present the appearance of running matter. This observation was made with power 100 and $10\frac{1}{4}$ in. reflector by Browning.

The Lunar Eclipse of July 12.—The Rev. Ralph Prowde, of Northallerton, Yorkshire, observed this phenomenon, and has forwarded the following:—"I observed the eclipse of the moon on the 12th, but the only thing remarkable was the great contrast of shade between the darker and brighter penumbræ. I say penumbræ, for I suppose the real umbra of the earth's shadow falls within the moon's orbit. The darker interior cone of shadow obscured the edge of the moon and the object on its surface as it passed over them almost entirely, but its own edge did not seem to be nearly so regularly round as the lighter enveloping cone of shade."—The Rev. J. J. Johnson, of Crediton, reports:—"On the evening of the 12th I had a very favourable view of the lunar eclipse. The sky was clear at first, with a small amount of stratus near the horizon. I first caught sight of the moon at 8.41, but it was 8.49 before it got clear of the clouds. I paid particular attention to the degree of distinctness with which the eclipsed portion could be seen. When about four digits were covered I just noticed the copper tin through the telescope. I fancy this would be a little sooner than in the last eclipse I observed (September 1867) but in that of October 4, 1865, which was only of 4 digits, the copper tint was very decided in the telescope at the time of the greatest obscuration. When about six digits, or half the disc, was covered, the copper colour could be clearly seen with the naked eye. I could not make out any particular parts of the moon's surface until 9.35, when I noticed the *Mare Tranquillitatis* and the *Mare Serenitatis* showed with beautiful distinctness through the earth's shadow in the telescope. A few minutes after the total was attained, I was struck with the obscurity of the eastern side of the moon being so much more than I had expected. At 9.55 at least half of its surface was as if blotted out even when seen through the telescope, although I applied two different powers—70 and 150. Three of the *seas* at the western side were all I could make out. Possibly a thin coating of cirrus cloud which covered all the sky about this time might account in some measure for the invisibility of the moon. By 10.30 this had entirely cleared away, and the sky was everywhere covered with stars. The Milky Way very near the moon was about as distinct as it usually appears on a dark clear night. At this period, being the middle of the eclipse, the upper portion of the moon was the invisible part, all those regions lying round the margin of the disc being alone to be seen, except at the vertex, where the margin itself was not discernible. At 11.23 the first streak of light was breaking forth at the eastern edge. At 11.45 the red colour was nearly gone, and the eclipsed part appeared of a grey colour. At 11.58 I noticed there was no trace of the Milky Way; at 12.24 the lunar circle was again complete."—Mr. Oliver J. Lodge, of Hanley, reports that "the colour of the moon during the totality was of a most peculiar copper hue, giving very little light indeed. But during the egress of the shadow it was almost as white and silvery as it usually is, although still under the penumbra." Mr. Edmund Neison, of London, says:—"The colour of the eclipsed disc was during the whole time a dull, yellowish olive-green, both in the telescope and out, but was never dark enough to prevent many of the chief markings and craters being seen. From 10.44, when the lunar disc was fairly above the fog-banks, *Aristarchus* was quite distinct as a bright crater, and even before eleven *Grimaldi* was plainly discernible." At Bristol, Mr. William F. Denning observed the phenomenon, and remarks that even at the time of totality many of the most conspicuous objects on the disc were distinctly visible. The copper tint was also very evident. During a portion of the time the moon was over cast with clouds.

Venus.—Mr. Henry Ormesher, of Manchester, observed this planet with $5\frac{1}{4}$ in. equatorial refractor, on July 23, at 5^h P.M. "The definition was excellent. I observed three dusky spots on the disc, one of which was of very considerable magnitude."

Saturn.—Mr. H. Michell Whitley, of Penarth, writes:—"July 7th, 10^h 11^m, power 208—the ball of the planet dull yellow colour. N. equatorial ruddy belt conspicuous, and another of same colour between it and pole; pole bluish grey; edges of disc fainter than centre; sky in ansæ much blacker than around planet; crape ring across ball nearly as dark as Ball's dension, pale purple; crape ring very easy in ansæ. No line of light between it and B."

Occultation.—Mr. J. C. Lambert, of Sleaford, witnessed the occultation of B.A.C. 5954 on July 10, and found the exact time of disappearance to be 12^h 40^m 41^s mean time.

Meteors.—Mr. J. C. Lambert "observed a very brilliant meteor at 11^h 40^m, July 21. Course from a little below γ Cassiopeiæ to ξ Persei. Nucleus appeared as a star of 1.5 mag.; tail nearly 2° long; colour, yellowish white; duration 2^s. During the time of observation (11^h to 12^h 30^m) observed no less than eleven small meteorites. The course of one of these was from ϵ Bootis to 43 Comæ Berenicis, and immediately afterwards one from a little below 43 Comæ Berenicis to η Bootis. Could this have been one and the same meteor describing an arc?"

Lunar Observations.—Mr. H. Michell Whitley has very carefully examined many interesting lunar objects, and the results he obtained have been forwarded to Mr. W. R. Birt.

PARIS

Academy of Sciences, Aug. 16.—M. Yvon Villarceau communicated some remarks on the decimal division of angles and of time, in reference to M. l'Abbadie's communication of the previous week.—M. Sainte-Claire Deville made a final reply to M. Jamn on the subject of the sepecific heat of mixtures.—M. Wurtz presented a note by MM. Ad. Lieben and A. Rossi on normal amyl alcohol. The same authors recorded last year the manufacture of a new butyl alcohol differing from the alcohol of fermentation, and representing the fourth term in the homologous series of normal alcohols. Taking this alcohol for a point of departure, and applying the same synthetical methods, they succeeded in obtaining a new amyl alcohol, which they call normal, and bearing the same relation to the amyl alcohol already known as the new butyl alcohol does to the butyl alcohol of fermentation. In order to obtain it cyanide of normal butyl is first obtained, and the valeric acid corresponding to it made by the oxidation of ordinary amyl alcohol. The lime-salt of normal valeric acid is mixed with the formiate, and the mixture submitted to dry distillation. Valeric aldehyde is thus obtained, boiling at about 102°, and isomeric with valeral. This aldehyde, treated with nascent hydrogen, yields the alcohol. Normal amyl alcohol bears a strong resemblance to the amyl alcohol of fermentation. It is distinguished by its higher boiling point, 137°. By oxidation it yields valeric acid.—M. Wurtz also presented a note by M. F. Papillon on modifications in the immediate composition of bones, proving that the normal lime contained in the bones of animals may be partially replaced by alumina, magnesia, or strontia, by including these substances in their food.—M. Cave contributed a note on the formative zone of the foliar organs in monocotyledonous vegetables. This he found to occupy the same position as in his previous researches on dicotyledonous plants. In the leaves of endogenous plants the inferior tissue is the older; the layer nearer to the superior epidermis is the younger. In the fruits, also, the author has invariably found the formative zone occupy the same place in those belonging to the two divisions of flowering plants.—M. Jamin presented a note by M. W. de Fonvielle on the astronomical discoveries of the ancients.

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THURSDAY, SEPTEMBER 1, 1870

THE MEDICAL SCHOOLS OF ENGLAND AND GERMANY

I.

IN the region of scientific medicine the Germans enjoy at the present time an undisputed pre-eminence. Their medical books have taken possession of the markets of the world, and their larger schools are themselves like markets in which representatives of all countries appear, in order to exchange gold for the higher culture. Next to German books, those published in Great Britain have the best programme and apparently the widest geographical distribution. But here the enormous territory of the English language plays a very prominent part. To professional men who do not speak the English tongue, English books are but little known and still less read. In like manner, the extent to which foreigners avail themselves of the English schools is exceedingly small. It is very significant that even Americans pass by their natural market, England namely, for the acquisition of higher medical education, and resort annually in troops to Germany, where they have to contend with the disadvantage of a foreign language.

If we now attempt to ascertain the causes of this phenomenon, we must arrive at once at the conclusion that local conditions have nothing to do with it. Great Britain can number nearly as many medical schools as exist in Germany, and their local position is extremely favourable for visits from foreigners. In this respect the larger cities enjoy an enormous advantage over smaller towns, for medical students do not, as a rule, despise good living, and therefore prefer to live in great cities. Hence the medical schools of Vienna and Berlin are rendezvous for travelling physicians, while many a small German town with no less distinguished a teaching power, is visited only by those foreigners who prefer to taste the treasures of a teacher drop by drop, far from the battle of the metropolis. London alone possesses eleven medical schools. Here are also offered to the travelling disciple of science the advantages of the metropolis, and something besides which can only be obtained on the Continent by an expenditure of time and money, and then not altogether, the opportunity, namely, of changing the school, and especially of discovering the one which it will best answer his purpose to visit.

If, in spite of all this, the schools of London form no open market, the cause must be sought for in their quality. It does not, in fact, require much criticism to discover that the construction of the medical schools here is so entirely different from that of the corresponding schools in Germany, that the defectiveness of our language is the only excuse for designating the two by the same name. In the majority of the medical schools of London instruction is only a subsidiary product of the general charity. The hospitals are supported by voluntary contributions, and at one and the same time medical assistance is given to the sick and medical instruction to youths eager for knowledge. The subscribers elect officers for this purpose; and both the electors and the elected are agreed in considering the treatment of the sick as the primary office, instruction as a secondary office. Professional men on the Continent are obliged to bear this relationship constantly in mind, if they would understand how it comes to pass that a nation of such sound judgment in practical life as the English, can act in a manner which, to those who look at such hospitals from the stand-point of the development of science, appears so opposed to every modern theory of work.

In these hospitals, founded and supported by voluntary contributions, the teachers, in the course of their lives, change several times the subject of their lectures. The teacher generally begins with botany, and abandons it after he has acquired a moderate knowledge by several years' instruction, in order to take up another important branch of human knowledge; and perhaps again exchanges this for something else, just at the time when he can say with Faust:—

Und sehe dass wir nichts wissen können.

The final object of such a course is the position of hospital physician or surgeon, with which, as a rule, a profitable practice is also combined. The hospitals attain in this manner the best result which they can attain. They obtain physicians who for several years have given their attention more or less assiduously to reading scientific literature. In so far as these physicians are at the same time in a wider sense useful to the community, this system performs good service in educating a large number of well-read medical men. For the development of these officers into distinguished original investigators, the mosaic-work of their course of study is altogether destructive. From the haste with which they rush through great departments of knowledge, they can give no time or leisure to assist in drawing up the endless chain of causes to the light of day. Such an undertaking, moreover, is entirely outside the aim for which the charitable contributions were given.

Among the numerous London schools, some three or four stand out conspicuously, and one of these is so constituted, from its connection with other non-medical chairs, as well as from the history of its establishment, that one may conclude the fostering of science is not in this case a secondary aim. Let us observe now what assistance is afforded by this school for this purpose; and let us compare these means with those provided by a great German school, for instance, that of Vienna. The writer of this article has chosen this example, chiefly because he is familiar with the interior arrangements of the Vienna school. The consideration also that this is the oldest of the prominent German schools, and that its eventful history can exhibit many points of interest which stand prominently forward, has its influence. Other examples might be brought forward which would equally illustrate the contrast.

It must, in the first place, be borne in mind that the London school just referred to, that of University College, is, like all her sisters, a private institution, while the Vienna Medical School is a State institution; the Government builds or rents the building, directs it, and provides it with officers. The means for this object flow from the provisions of the budget of the Ministry for Public Instruction; and the Government therefore possesses the power of granting the means of diminishing or enlarging them according to circumstances. Since many German schools, which are still State institutions, are yet entirely or partially self-supported, some words of criticism must be devoted to this system. The independent means of the Universities are not only means for the protection of their independence, but at the same time are a bulwark against the attacks of an absolute Government, hostile to science. In those states, however, where a protection against violence is provided by distinct legislation, and especially in those in which the people has a share in the Government, every other bulwark than that which the law offers is only antiquated trumpery; the independent foundations of the Universities are no better than a means for the maintenance of the spirit of caste, and for the fostering of nepotism. In Austria the independent foundations of the Universities have fallen a prey to the insatiability of the State treasury. The freedom which has so rapidly developed itself in Austria during the last few years, found the doors of the Universities open, and forthwith established herself there. Well might their noble spirit be envied by those institutions which have used their independent means for enclosing the school and the church within a common wall!

It will not require many words to prove that the state institution enjoys an advantageous position with respect to the private institution. For while, on the one hand, the State can calculate on future revenues in laying out money for the establishment of scientific institutions, the private institution must regulate itself in accordance with its actual means, and can only reckon upon much narrower materials and temporary factors. This contrast cannot be illustrated in a more striking manner than by comparing the palace which the Saxon Government has built in Leipzig for instruction in physiology, with the one or two rooms which University College, London, is able to devote to the same purpose.

It must, however, not be forgotten that it is only recently that such institutions as that at Leipzig have been established. There are, indeed, at the present moment, only three other institutions in Germany which can be compared to it, viz., the Physiological Institute at Breslau, the splendid Anatomical Institute at Berlin, and the Pathological Institute at Vienna, all of which occupy separate spacious buildings. In these and other universities, establishments of a similar kind, and on a similar scale of completeness, are either projected or are now in course of construction.

S. STRICKER

THE EARLY HISTORY OF MANKIND

Researches into the Early History of Mankind and the Development of Civilisation. By Edward B. Tylor. Second Edition. (London: Murray, 1870.)

MR. TYLOR has devoted himself to a branch of Anthropology of which there are very few students in this country, that namely which treats of the mental development of man as elucidated by his arts and customs, and especially by his myths, his superstitions, and his language. More than a third of this volume is devoted to an elaborate account of the gesture-language used by deaf mutes and savages, and to picture-writing, word-writing, and the influence of names and images, as illustrative of various phases in the development of the human mind. After this we have chapters on the growth and decline of culture, as illustrated by the use of stone implements of various degrees of perfection, by weapons, by modes of procuring fire, and by modifications in various domestic utensils. Then follow accounts of remarkable savage customs, such as the curing of disease by the extraction of foreign substances from the body of the patient, the prohibition of marriage with certain relations or namesakes, tabooing the names, and even avoiding the sight, of certain relations, and the extraordinary custom of the *couvade*. Myths, their origin and geographical distribution, are then discussed; and these varied subjects are all treated from a twofold point of view, either as giving us an insight into the laws of the development of the human mind and the growth of civilisation, or as furnishing, by their similarity over extensive areas and in widely separated countries, an argument for the common origin of the different races of man.

The work is throughout carefully written, and is illustrated by abundance of curious and little-known facts and a critical examination of their bearings. The author is very cautious in drawing any general conclusions, and when he does so carefully indicates all sources of error and uncertainty. The character of such a book cannot be fairly shown by extracts; we shall, therefore, briefly summarise one or two of the more interesting subjects and arguments.

Many persons are, no doubt, under the impression that the deaf and dumb talk to each other by means of the finger alphabet; but the use of this pre-supposes a knowledge of the meaning of words and letters, which the deaf and dumb child can hardly be taught till intelligible communication has been established with it. Alphabetical speech is slow and clumsy, whereas the deaf mute speaks to his comrades as rapidly, if not as precisely, as we do by means of vocal speech. He uses a copious and expressive language of signs, indicating words and ideas by means of simple motions and gestures. This language has the advantage of being natural and universal. English, French, and German children to a great extent understand each other, and even a North American Indian would be able to talk with them all, it being a curious fact that many of the signs used by the Indian tribes are identical with those of the deaf and dumb schools of Europe; and Mr. Tylor states that a Sandwich islander and a Chinese both made themselves understood in an American deaf and dumb institution. The "gesture language" is also connected with spoken language in two remarkable ways. Among low savage tribes there are cases in which speech has to be supplemented by gesture to make it intelligible, and it is, perhaps, reasonable to suppose that at an earlier stage of civilisation the proportion of gestures to words would be greater than it is now. There is also an agreement in some fundamental idioms. In the Aryan languages many substantives have verbal roots descriptive of some of their essential attributes. "Thus, the horse is the *neigher;* stone is what *stands*, is *stable;* water is that which *waves, undulates;* the mouse is the *stealer;* and age is what *goes on;* the oar is what *makes to go;* the serpent is the *creeper;* and so on." Now the deaf and dumb who have no means of communication but by signs, express themselves in the same way. To them the bird is what *flies*, the fish what *swims*, the plant what *sprouts out of the earth*, &c., and the motions of flying, swimming, and sprouting up, are used as the signs for bird, fish, and plant.

Mr. Tylor is usually very cautious in concluding that any art or custom found among distant peoples has had a common origin, or can be used to measure the comparative antiquity of the migrations of races. Yet, in one case, in which he considers that it can be so used, he arrives at conclusions which hardly seem warranted by the facts. The Madagascans smelt iron, as do also the natives of Africa and of the western islands of the Malay Archipelago, but the bellows used in Madagascar is the peculiar Malay form—an upright bamboo, with piston formed of a bunch of feathers, and entirely different rom the inflated skin-bellows of Africa. This curious fact, taken in conjunction with many others, and with the presence of a considerable Malay element in the Malagasi language, as well as some physical resemblance between the Hovas and Malays, conclusively proves that there has been a Malay immigration to Madagascar, and also that it took place subsequent to the introduction of the art of working iron. So far the facts lead us safely; but Mr. Tylor, if we understand him rightly, goes further than this, and holds it to be proved that the art of smelting iron was first introduced from Malaisia rather than from Africa, and also that the Malay migration to Madagascar was a much later event than the Malay migration to Poynesia, where the use of iron was unknown till introduced by Europeans. Now, for all that the facts tell us, iron working may have been known in Madagascar before the Malays came, they merely introducing the bamboo bellows, which would be especially adapted to a country in which bamboos were abundant, but cattle, deer, and all large animals which could furnish suitable skins, *entirely absent.* They certainly might have introduced iron-working also, but the fact of their introducing a more useful form of bellows does not prove it. So, with regard to Polynesia, there are two sufficient reasons why iron-working should not have been introduced there, even if the Malay immigration had been long subsequent to that which invaded Madagascar. The only Malay iron-smelters are certain tribes of Borneo, Sumatra, and the Malay peninsula, while among the Javanese and Coast Malays who are the chief navigators, as well as among the whole of the Moluccan tribes, the art is entirely unknown. But the Malay element in the Polynesian languages is composed of pure Malay and Javanese words, and there is every reason to believe that wandering traders of these nations introduced the Malay language into Polynesia. Added to this the fact that the volcanic and coralline islands of the Pacific contain no iron ore, and we need not wonder at iron workers not being found among them, or that the tribes who still more recently peopled New Zealand should not know how to make use of the iron ore that occurs there. The evidence of language on the other hand would seem to be in favour of the Madagascar migration being the most ancient, because the Malay and Javanese words are generally more changed in the Malagasi than in the Polynesian languages. In the latter, scores of words are slightly modified but intelligible Malay, as *pua* for *bua* (fruit), *ika* for *ikan* (fish), while in the former many equally common words have been greatly altered, as *ravina* for *ron*, Jav. (leaf), *lanitra* for *langit* (sky); and the word *lima* or *rima* (five), which extends almost unchanged over the whole of Polynesia, becomes the hardly recognisable *dimi* in Malagasi. The Hovas are undoubtedly much nearer the true Malays in both physical and mental characteristics, than are the Maories or Tahitians, and this would indicate that a larger and more compact body had reached Madagascar than Polynesia. This is what we might expect, for the chances are so much against a safe canoe voyage across the open Indian Ocean, a distance of nearly 3,000 miles with scarcely an intervening island, that we can hardly suppose it to have occurred more than once; while the numerous islands in every part of the Pacific render it much more probable that canoes accidentally blown out of their course from the Moluccas or New Guinea, might repeatedly reach some islands tenanted by the Polynesian race. But a compact body which ultimately conquered much of the country and established a dominant race, would have a greater tendency to preserve their language unchanged; and the fact that so much change has taken place is an additional argument for the comparative antiquity of the Madagascar immigration. The ignorance of making pottery in Polynesia, an art which has certainly been known to the Malays and Javanese from a very early period, seems at first sight opposed to the theory of a late communication; but this fact may, I think, be easily understood when we consider that the immigrants were most probably traders, and of the male sex, and therefore ignorant of an art which in their native country is almost entirely practised by women. While treating of this subject, Mr. Tylor falls into some confusion by speaking of the "Malayo-Polynesian culture," and "the pure Malayo-Polynesian race," things which can have no existence, if, as I believe, Malays and Polynesians are almost as distinct as Malays and Africans.

The geographical distribution of customs, beliefs, and myths, furnishes our author with materials for some of his most curious and interesting chapters; but, still less than the arts of savage life, do they appear to afford any safe ground for conclusions as to the affinities or early migrations of the races of mankind. We cannot conclude without expressing our admiration of Mr. Tylor's industry and research in so little trodden and comparatively unproductive a field. He has carefully brought together a vast number of interesting phenomena illustrative of the mental condition of savage man, but we cannot help feeling that a satisfactory explanation of them has not yet been arrived at, and that we require researches of a very different nature before we can form any adequate conception of the various causes that have influenced the early mental development of the human race.

A. R. WALLACE

KARL KOCH ON TREE-CULTIVATION

Dendrologie: Bäume, Sträucher und Halbsträucher, welche in Mittel- und Nord-Europa im Freien kultivirt werden. By Prof. Karl Koch. 8vo. Vol. I. 735 pp., without illustrations. (Erlangen: F. Enke, 1869. London: Asher and Co.; Williams and Norgate.)

THE work of Prof. Karl Koch is a valuable addition to the literature of applied Botany; and no doubt throughout German-speaking countries it must early become the volume of all others most redolent of Nicotian essence on the shelves of people who concern themselves about tree-cultivation and general nursery-work. It has a considerable value, too, as a contribution to Scientific Botany, for its author has had long experience, with very favourable opportunities, in connection with everything woody,* hardy enough to bear exposure in the open air in Prussia. He is a good botanist and an enthusiast in his speciality, so that his book includes much useful information, especially of that kind, too seldom put on record, possessed by workers in the open air, or so scattered through periodical botanical and gardening literature, as to be hardly available at a pinch. To English botanists and nursery-folk it offers many interesting features. One amongst many others is the opportunity it affords of comparing the climatic differences between Britain and Central Europe, as deducible from the copious data which the "Dendrology" gives relating to the capacity of the various species to resist the severity of continental winters.

Here is an example of the author's plan of treatment. We take the common Aucuba, or so-called "variegated laurel," of every English shrubbery. *Aukuba*, not *Aucuba* Prof. Koch says it must stand :—

A. Japonica, Thunb. Fl. Japon. 64, tab. 11 and 13 (1784).

THE JAPANESE AUKUBA.

Japan.

Flowers in May and June.

Leaves varying in form, mostly elliptical or elongate-lanceolate, usually sharply toothed above the lower third, glabrous: inflorescence with appressed hairs; petals ovate-lanceolate, dark brown-red; berries coral-red.

One of the most beautiful of evergreen shrubs, unfortunately scarcely hardy in north-eastern Germany, even when protected in winter, though thriving in the open air in France and Rhine-land. In England it is a prime favourite.

Then follows a general account of the habit of the plant.

Until quite recently we have had only the bright-yellow-spotted female shrub in our gardens, but since Von Siebold and Fortune have introduced the unspotted male plant, and a number of new forms, *Aukuba japonica* promises to compete with the Holly in variety. In the last catalogue (1867) of the Siebold-Garden, Witte has published no fewer than twenty-four forms as already in the trade.

Here are enumerated some of the more important of these, arranged according (1) to form of leaf, (2) kind or mode of variegation, and (3) toothing of the leaf-margin.

Altogether more than a page is devoted to this *Aucuba*. To its less-known Himalayan congener, *A. himalaica*, about half as much. The common Barberry extends over nearly five pages, but it is an extreme case of a variable and much-cultivated plant.

Throughout the plan is pretty much the same as is followed in *Aucuba*. After the name follows synonymy so far as may be needed; then frequently some explanation of the origin of the name, whether generic or specific, often including a biographical paragraph in the case of plants commemorating some person. Then follows the geographical distribution of the species. Here we may remark, what this book is especially apt to recall, the curious fact familiar to every botanist, how many well-known species of tree and shrub, even such as are not likely to have undergone material modification under culture, are of doubtful origin,—the horse-chestnut and walnut for example. Persia and the Central Asiatic plateau generally are credited with these trees, and probably the guess is in the right direction, though no wild specimens exist of either species in our herbaria.

There are some things we cannot agree with in the book, of course. For example, we should not undo *Cratægus*, and make all our thorns into medlars (*Mespilus*); nor do we consider the reasons given suffice to justify a renaming of our common lime-trees (*Tilia*); but these are matters too technical for discussion here, and after all of subordinate importance.

The first volume only, including the Polypetalous Natural Orders, is as yet published. Just at present German gardens are left pretty much to take care of themselves, and we fear there is no chance of the second volume until we have this miserable war settled. D. OLIVER

* Down indeed to *Alyssum saxatile* and *Iberis saxatilis*.

OUR BOOK SHELF

Travels of a Naturalist in Japan and Manchuria. By Arthur Adams, F.L.S., Staff-Surgeon R.N. (London: Hurst and Blackett, 1870.)

THOSE who wish to see what a world of pleasure may be opened up to one by an adequate knowledge of some department of Natural Science, ought to read this book. Mr. Adams is an enthusiastic naturalist, with a special "weakness," as he terms it, for insects, and the delight he has experienced in the hunt for specimens, and the close observation of his favourites in different parts of the world, have been simply endless. We should hope that some of his readers will find his eagerness "catching," and be led to feel new interest in objects which they have hitherto regarded with indifference, or perhaps treated as a nuisance. Among other animals observed by Mr. Adams with more or less care, was one of two specimens of the scaly ant-eater (*Manis Javanica*)—a female—which came under his notice. During the day she remained coiled up in a ball, but grew lively as night approached. In walking "she trod gingerly on the bent under-claws of her fore-feet, and more firmly on the palms of her hind-feet." One of her favourite attitudes was that of her gigantic extinct analogue, the Mylodon, as seen in the model of Waterhouse Hawkins in the gardens of the Crystal Palace. Supported on her hind limbs and "the firm, flattened, powerful, muscular tail," she would raise her fore-feet, moving her head and body from side to side, and peering cautiously about with her "little round prominent eyes." On the least alarm she tucked in her head between her fore legs. On one occasion she was coiled up in a strong net and supposed to be properly secured; when night approached, however, she easily burst her trammels, and was discovered by the violent barking of a little dog who was puzzled and alarmed by the apparition of so strange a visitor. Both specimens were fed on raw eggs and chopped raw beef, and seemed to thrive. Besides observations of this sort, Mr. Adams's readers will find scattered throughout his work some pleasant sketches of natural scenery, a few descriptions of amusing personal adventures, and occasional glimpses into the different customs of the countries he has visited.

Echinides du Départemente de la Sarthe considérés au point de Vue zoologique et stratigraphique. Par MM. Cotteau et Triger. Avec 65 planches de fossiles, dessinées et lithographiées d'après nature par MM. Levasseur et Humbert, 10 planches de coupes géologiques, et 2 tableaux. Williams and Norgate, 1855—1869.)

NOTHING can afford better evidence of the zeal and assiduity with which palæontology is now pursued than the fact mentioned by M. Cotteau in the preface to this work, that whereas MM. Agassiz and Desor, in their

Catalogue raisonné of 1845, indicated the number of Echinidæ discovered in the department of Sarthe at forty-one, and D'Orbigny in his Prodrome Stratigraphique at forty-four, the result of the joint labours of the authors, and other naturalists of the district, has raised the number to no less than two hundred and two. The discrimination, description, and illustration of the different species have been performed by M. Cotteau, whilst their stratigraphical arrangement and position have been accomplished by M. Triger. Some of the more remarkable forms discovered by them are the large *Heterocidaris trigeri*, with its peculiar arrangement of tubercles and its singular ambulacral pores, a species of which has recently been obtained by Mr. Wright from the inferior oolite of Yorkshire; the *Metaporhinus sarthacensis*, a curious and exceptional form representing in the Jurassic series the great family of *Spatangidæ*, which only make their appearance at the commencement of the cretaceous period; the *Echinocyphus tenui striatus*, which the authors are inclined to regard as the type of a new genus; the *Cidaris vendocinensis*, which presents such beauty of form and markings, with many others we have no space to particularise. The lithographic drawings are clearly drawn, and comprehend all the species discovered.

Progress of Chemistry. Jahresbericht über die Fortschritte der Chemie und verwandter Theile anderer Wissenschaften. Unter Mitwirkung von Th. Engelbach, Al. Naumann, W. Städel; herausgegeben von A. Strecker. Für 1868. 2tes Heft. (Williams and Norgate, 1870.)

THIS part, like the first, which we noticed a short time since, contains 480 pages; Organic Chemistry, continued from the first part, occupies 354, 13 of which are devoted to Animal Chemistry. Analysis fills 71 pages, the remainder being set apart for Technical Chemistry.

The section on Organic Chemistry contains accounts of Perkin's investigations on the hydrides of sodium and benzyl-salicyl, on butyric coumarin, and butyrocumaric acid, as well as Fittig's criticism of Perkin's views of the constitution of coumarin, which has since given rise to a lively discussion. Notices of Schützenberger's researches on triacetiodol, and of those of Perkin and Duppa on the constitution of glyoxylic acid are given. Stenhouse's experiments on benzol sulphuric acid are described, besides several papers by different chemists on the sulpho acids of the benzol series. Hofmann contributes, as usual, several valuable papers, the most important being those on the cyanide of naphthyl and its derivatives, and on the artificial mustard oils containing the radicals ethyl, methyl, amyl, tolyl, and benzyl, in the place of the allyl existing in the natural essence. The constitution of these compounds is also discussed. Gautier's researches on the carbylamines are continued, and also those of Lossen on hydroxylamine, which are noticed at considerable length. The action on organic bases is concluded by an account of Crum Brown and Fraser's experiments on the physiological effects of the compounds produced by the union of methylic iodide with the poisonous alkaloids.

Under Analysis we find the methods proposed by Frankland and Armstrong for the examination of potable waters, and which have since given rise to some controversy. Russell's apparatus for gas analysis is also described. The section on technical chemistry (only a portion of which appears in this part), contains papers by Rosenstiehl and Kopp, and by Schaffner on the preparation of sulphur from alkali waste, a subject of much importance, especially in this country, where the heaps of residues, which usually evolve sulphuretted hydrogen, and often pollute rivers in their neighbourhood, accumulate in immense quantities; this material, thanks to the study of scientific chemistry, may now be made to yield pure sulphur to such an extent as to make it worth the while of the manufacturer to extract it, while its removal renders the residues innocuous.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Gulf-Stream

I SEND for the perusal of your readers an extract from a note sent to me by Mr. King Groom of Stornoway. The facts he mentioned may be interesting. The beans I have received. When I was in the Hebrides some time since, I was assured that clubs, paddles, and drinking vessels of wood were sometimes found on the shores of the islands, and that these things were supposed to float in the Gulf-Stream from Mexico, but I never saw any. Some of your readers will doubtless be able to say where these beans grow, and to give their opinion as to the probability of their floating from the Mexican Coast, or from some other tropical country or island whose shores are washed by the Gulf-Stream. If these beans are brought over by the Gulf-Stream, it is probable that they may also be found on the west coast of Ireland and on the coasts of Devon and Cornwall. If so it would be interesting to hear from your correspondents in those parts whether any particular virtue is attributed to them by the inhabitants.

Board of Trade, Aug. 25. THOMAS GRAY

"Upon travelling on the shore of Illery I found a, to me, curious bean, known as '*dolichos mens*,' or horse-eye bean. I was told that every year a few are found along the shores of the Outer Hebrides, and they are supposed to be carried by the Gulf-Stream from the Gulf of Mexico. These beans are much sought after, as they are superstitiously supposed by the South Uist and Barra people to be a charm in child-bearing; if at that time the woman has one in her hand she will have little pain in her labour. I was much interested in the story as told me by a Mr. Arbuckle, at Barra, the parish schoolmaster, and confirmed by Dr. M'Donald of North Uist. It is said the curious have a small band of silver wire placed round the bean that has travelled so far of itself, and a silver cross put on the side by a silversmith in the south. The inhabitants state they are sure the bean is brought to the shore of the Gulf-Stream from Mexico, as they have been thus found from time immemorial. Another bean is also brought by the same means—viz., 'Eig autea Mi: Mosa,' a large brown seed."

The British Medical Association

THE reference to the income and expenditure of the British Medical Association last week requires correction. The income of the association, from subscription, is not 5,000*l.* but 3,471*l.*; the subscription annually only one guinea. The association is one which includes an important political and social organisation. For the annual guinea the associates are enabled not only to supply themselves with a journal weekly, which stands on the same footing as those published at a higher price than their total annual subscription, but in addition they keep also an active and important organisation, with branches in every part of the kingdom, which protects medical and public interests and advances medical science. Many think with you that it would be desirable to make larger grants for special researches. But for this purpose it would be necessary to start a special fund. The annual mass of scientific matter published in the journal is treble what could be contained in any volume of Transactions, and has the advantage of being published at the opportune moments of discussion. You would confer a benefit by urging the propriety of a special fund for original research. But the members are so well pleased with the large return for their annual guinea, that their numbers have risen in four years from two to four thousand; and the weekly journal, which now takes the lead in periodical medical literature, is the essential condition of the political and professional authority of the association. To publish in its place an annual volume of Transactions would, it is universally felt, be a suicidal act of retrogression. Among the 4,000 members there are not a dozen who are not aware of this. X.

The Intended Engineering College

"Look not a gift horse in the mouth"

I WAS very sorry to see in NATURE for 18th August a letter from Mr. G. C. Foster complaining of a supposed intention of the Government to aid the teaching of science, and basing this complaint upon what appears to me most narrow and unreasonable grounds. The representatives of science have been tolerably unanimous in demanding of the Government further aid to science, and I think they should with equal unanimity protest against any such grumbling at a promised instalment before its nature and conditions are known.

Mr. Foster says "that it is fair to assume that Government," &c., &c., and then having stated the details of his assumption, proceeds to criticise it. I think (and surely shall not be alone in this opinion) that it is most unfair to assume anything of the kind as the basis of criticism. When the details of the Government scheme appear, will be the proper time to discuss them.

Being as ignorant of these details as Mr. Foster appears to be, I can say nothing about them, but must protest against the principle upon which Mr. Foster's complaint is based. It is this, that Government must initiate no scientific effort, give no special aid or patronage to any college or scientific institution, lest it should assail the vested interests of "institutions like University College and King's College in London, and Owens College in Manchester." According to this shopkeeping view of the interests of these institutions, they themselves should never have come into existence, and all endowments or other extraneous aids to new institutions must be regarded as attacks upon the vested interests of their more venerable competitors.

In the classic period when Munro Primus, Munro Secundus, and Munro Tertius occupied successively the anatomical chair of the University of Edinburgh, its medical school was the most flourishing in Great Britain; students journeyed from London and all parts of England to attend its classes. At about the culminating period of its rising fame the London University was founded, and among its most active promoters were Lord Brougham and other Scotchmen. According to Mr. Foster, these Scotchmen were traitors to their own University, for undoubtedly the medical schools of University College, King's College, and the provincial colleges affiliated to the London University, have, by the competition of their metropolitan *prestige*, patronage, endowments, and local facilities, seriously rivalled their northern predecessor; and if the University of Edinburgh were merely established for the purpose of providing class-fees for its professors, the Scotch promoters of the London University were traitors to their own *alma mater*; but if the objects of the Edinburgh University are the promotion of science and diffusion of general knowledge, then the founders of the London University were co-operating with the Edinburgh University, even though they thinned the attendance in some of its class-rooms.

The other institution whose vested interests Mr. Foster specially pleads to conserve, should rather be suppressed, if his principle were accepted, for by its rich endowment and *prestige*. Owens College competes unfairly with the less-favoured institutions of Manchester and the private science teachers there who have no share in its endowments. Mr. Oliver Mason is about to build and endow with princely munificence a great educational establishment in Birmingham, and has bought the ground within a stone's throw of the Birmingham and Midland Institute. If the council and friends of the Midland Institute accepted Mr. Foster's views, they would denounce Mr. Mason's project as the founding of an opposition shop, which, by its rich endowment, might undersell their own and take away their customers. If they regarded the existing institute as merely established and maintained for the purpose of providing certain professors with their present moderate salaries, for supporting a secretary and his assistant, and for the comfortable maintenance of the porter and his wife, they might consistently do so; but speaking from direct personal knowledge, I can affirm that, on the contrary, they of all others are the most rejoiced by Mr. Mason's munificence, because they are the most deeply interested in the intellectual progress of their town. I recommend Mr. Foster to imbibe some of their spirit, and to rest assured that no revolutionary disturbances are likely to result from excessive endowment or patronage of scientific institutions, either by the present or any other British Government of this generation. Even if Mr. Lowe's next financial surprise should consist in devoting a portion of the national surplus to the elevation of the national intellect, let generous acceptance accompany our amazement.

W. MATTIEU WILLIAMS

Scientific Research

DURING the last century every branch of scientific research has undergone gigantic strides towards perfection. Great credit is due, ay, even in greater proportions than is given, to those talented minds, who, although on every hand impeded by obstacles, have successfully overcome every difficulty, and solved problems which excite the wonder and admiration of the universe. Within the limits in which I am compelled to restrain myself, it will not be necessary to pursue my idea with elaborate detail; but if I generalise with sufficient skill, abler hands can take up the thread and unwind it to the extremest minutiæ.

The means required for the prosecution of scientific research in a systematic manner, have never been at all adequate to the requirements. Various branches of science, such for example as astronomy, chemistry, &c., require an immense outlay in order to enable the philosopher to pursue his investigations with any prospects of a successful result. Others again, such as botany, geology, &c., require the devotion of long periods of time for the collection of specimens and their classification. Our societies certainly have, by their energy and emulative inducements, succeeded in extending scientific research far beyond the point which the most sanguine mind desired a century ago. But might not these societies be made much more useful; would it not be possible to distribute more widely their published transactions? Many gentlemen who take a great interest in scientific questions, but who do not live in London, are prevented from joining the societies by their exclusiveness. To them the valuable libraries, the periodical meetings, are useless, they therefore do not subscribe, and the high price of the publications prevents many from becoming purchasers. It seems to have become a settled opinion amongst scientific bodies, that everybody is able to spend 1*l*., or even more, just when he pleases, upon a luxury.

Unconnected with any society—not because of their own wish, but from their misfortune—hundreds of steadfast, able minds are working, adding, or being forestalled in, as the case may be, their mite here and there, in the onward march of progress. It is by unknown, frequently penniless searches after truth, that the great and complex problems have been solved. These great minds exist among us still. As examples, let me cite one or two instances, before suggesting an idea whereby we might hear more of such people. Whilst residing at Oxford, I became acquainted with a policeman, W. S., whose geological collection and information would have been worthy of one of our greatest and richest savants. A young gentleman, now a dissenting minister, and an undergraduate of that excellent institution, the London University, collected and classified some hundred species of the flora of his native county, Yorkshire. A third gentleman is assiduous in his botanical researches, and has, I believe, collected and classified the whole of the flora of another county. Now, it is such minds as these that, judiciously directed, make the greatest discoveries. Their only incentive to labour is a fixed inherent desire to know more of certain things; they have really no aim but the satisfying of the natural tendency to move in this direction. The published text-books upon various scientific subjects are not adapted for self-taught students, the authors presuppose the aid of a master. There is no meeting together to discuss what this or that one has done, but each laboriously pursues his own path, often wasting valuable time in going over ground already fully explored.

I would suggest that local societies in connection with the central bodies, be formed in *every* county. The nucleus to these bodies already exists in clergymen, doctors, solicitors, and above all, tutors and schoolmasters. There is a yearning amongst many of these men for more information, and, as previously stated, they are debarred from reaping any benefits from the central bodies as conducted at the present moment. The government of these branches should be somewhat similar to that of the older societies; each member ought to contribute an annual subscription, in return for which he would receive—

- α. Free admission to all meetings of the local and of the central society.
- β. The transactions of the society also free.
- γ. Any other benefits as the committee might determine, or which could be obtained.

Thus far the scheme I advocate is simple, but it may be said, this necessitates the formation of as many branches as there are societies. Not so, however; the country members within each district would be too few to allow of this; but they might *all* congregate under one roof, be subject to one government, and reap the advantages of communion. Every member might upon election signify the particular transactions he wished to obtain; his subscription, after deducting a certain per-centage for local expenses, could be sent to the society publishing them, and he should be enrolled upon its books as a *bonâ fide* member. The information to be obtained of one science is generally so closely connected with another, or others, that no difficulty would be found in getting the greater part of the local members together for the purpose of hearing an address upon any scientific subject. The large libraries of the various central societies could be utilised by sending a parcel of books to the local library, such books to be exchanged monthly. It may be asked, would the parent bodies, and science generally, gain by such an arrangement? Are the British Islands too well explored? Is there no more celestial or terrestrial object remaining unknown? Have mathematicians, mechanicians, &c., reached the bounds of their studies? I say to these, and a score of other similar questions, No! Then the watchword should be Onward. By the above means the face of the whole country would be covered by earnest and interested searchers. Botanists might discover new species; astronomers would be joined into an immense circle, closely watching every phenomenon which occurred in the heavens; one statement would be verified by others; geologists would be at the side of every quarry or well, seeking specimens; antiquaries would be at hand to receive "finds," whenever historical ground or old buildings were being moved. Monthly statements of work performed would be forwarded to the general Secretary, to be printed in the Transactions. Lectures would be multiplied a hundredfold. Book-worms would find treasures hitting about in family mansions, and even in village cottages, which would satisfy even their craving appetite. But I am not writing for readers unable to understand. All will admit the feasibility of the plan, if only it were tried. Probably other correspondents may wish to be heard upon the subject, therefore I leave the suggestion in their hands.

Reading C. H. W. BIGGS

Kant's Transcendental Distinction between Sensibility and Understanding

As Dr. Ingleby's letter cannot well be answered, except by me, will you kindly insert the following observations? I am very sorry the form of the controversy compels me to refer to myself; you will see that the point at issue concerns an important question in Kant's philosophy. He said a certain question of mine was badly worded. As a question set out of a prescribed book, he concedes it to have been accurate enough, but he still denies the precision of the statement in that book. I think he is right, and that I was guilty of an error, though by no means so grave an error as he imputes to me. But his imputation is again partly my fault, for I did not write clearly enough. Here are the words which misled him; "we must not confuse the empirical distinction between real object and merely subjective appearance with the transcendental distinction on which Kant's doctrine of Space and Time is based."

In the first place I do not think there is any ambiguity in the term *real object*, when I am speaking of an *empirical distinction*, for then it cannot possibly be a noumenon; and the meaning of *subjective appearance* follows upon its correlative. Dr. Ingleby should, therefore, have found no difficulty in interpreting me rightly so far, and, indeed, he has done so. But he understands the rest of the sentence as if I had written "we must not confuse the empirical distinction between real object and merely subjective appearance with the transcendental distinction *between the same two things* on which Kant's doctrine of Space and Time is based." This I did not say, though I am afraid my words are open to such a construction. He justly adds that Kant's Æsthetic is founded on no such distinction, and he points out the fact that Kant has in the previous page (p. 78 of Hartenstein's Ed.) spoken of his broad distinction in kind between Sensibility and Understanding, as a transcendental distinction.

I perfectly agree with him that this was the point referred to by Kant, and perhaps he is right that the philosopher meant nothing more. But what I had in my head when I wrote the passage, was a special phase or aspect of this same distinction, the aspect which insists, that *it is not merely the ordinary empirical sensibility* (such as tastes and odours), *but the a priori and necessary sensibility which his doctrine contrasts with the understanding*. Of course he has not yet considered, and therefore leaves undetermined, whether the understanding can cognize things, *per se*: but as to sensibility, the most obvious illustration which a superficial teacher would select, in expounding the so-called subjectivity of space and time, would be contingent, as opposed to necessary, data of sense. He would show how colour and taste and warmth were apparently perceived in the object; but were really modifications of the subject, while other qualities (extension, figure, &c.), were really necessary to the object. Kant protests repeatedly against this empirical distinction being used to illustrate his doctrine, which depends on a transcendental distinction—a distinction (I thought) not of mere contingent, but of pure *a priori*, and therefore necessary, sensibility from understanding. The passages which I indicated and translated in the sequel of the note, preach this peculiar aspect of his doctrine, and were cited for this reason alone.

I confess I was led to search them out by overlooking, stupidly enough, his employment of the phrase "transcendental distinction" in the previous page; and the fact, that Professor K. Fischer had omitted to mention so important a point, made me all the more anxious to notice it. But when my language was so ambiguous as to mislead a really competent critic, like Dr. Ingleby, I must only acknowledge my fault, and promise to make amends in my next edition. I trust, however, that in this instance, your readers will absolve me from having blundered in the principles of the Critical Philosophy, even if I gave too much meaning to the *transcendental distinction*. I cannot conclude without thanking your able correspondent for his valuable criticism.

Trinity College, Dublin, Aug. 15 J. P. MAHAFFY

Colour Blindness

To the remarks in Mr. Hayward's letter in NATURE of August 18, may I add my own observations? I have often noticed that my right eye has much greater defining power than my left; as, for instance, in reading print; but when I look at a check pattern of white and black, the white looks much whiter and the black much blacker to my left than to my right eye. Is not this somewhat analogous to Mr. Hayward's case?

St. Peter's, York, Aug. 20 LEONARD MARSHALL

Cross Fertilisation

THERE could perhaps be found no more striking illustration of the law which seems to demand, from all species of living things, frequent crossing as a condition of their continued existence, than is afforded in the structure and development of the flowers of *Lobelia*. A hasty examination of a few specimens of this plant might seem to refute this idea; and I can imagine an anti-Darwinian, unacquainted with the life-history of the flower, pointing triumphantly to it, not only as an instance of pepetual self-fertilisation, but also as an incontrovertible example of an organism specially adapted to the use and convenience of a different species, without itself deriving any advantage from the circumstance. For while the flowers of this genus are furnished with a broad and brilliantly-coloured lip, forming an attractive lure on which insects may alight to feed on the nectar provided for them, the introrse anthers are connected together, so as to form a rigid case completely enclosing the style and imbedding its summit in pollen. In this case, then, insect agency appears to be worse than useless; for though a few grains of pollen may be, and are, shaken out, through a small orifice between the extremities of the anthers, upon the back of every moderate-sized insect which enters the flower; such grains can apparently never be brought into contact with the stigma, and consequently must perish and be wasted. How completely, however, would such a reasoner find the tables turned by more continued observation. *Lobelia* is one of those genera which might be more correctly described as *versisexual* than, as strictly speaking, hermaphrodite. Its flowers are at first entirely male, the female organs not being fully developed till after all the pollen has been removed. Then the style forces its way between the extremities of the anthers, and expands into a broad stigma, so situated as to rub the backs of the bees and other insects that enter the flower, and brush off any pollen that they may bring. Thus, self-fertilisation, instead of being, as it at first seemed, inevitable, is in fact impossible; and insect agency, which appeared at best useless, is absolutely necessary to the survival of the species.

"Versisexuality" seems also to be the rule among the species of *Ranunculaceæ*, *Geraniaceæ*, *Saxifragaceæ*, and probably many other families. It is evident that in such species the pollen of the earliest and the ovules of the latest flowers will be wasted; and since natural selection tends always to prevent any waste, it is conceivable that such species might in the course of many generations give rise to monœcious or diœcious descendants.

Kilderry, Co. Donegal W. E. HART

The "English Cyclopædia"

IN answer to Nemo's letter in your issue of August 11, I do not wish to prolong the correspondence. An index will be added as soon as possible to the Natural History Supplement, in which cross references will be given. I am not the editor of the "English Cyclopædia," but I was

EDITOR NATURAL HISTORY SUPPLEMENT

Holly-berries obnoxious to Birds

ALLOW me to thank Mr. Hart for his remarks on my note, with reference to this subject; and, at the same time endeavour, as briefly as possible, to explain my meaning more fully.

I take it that a holly-tree, standing in a favourable situation for the growth of young plants, and bearing its berries until perfectly ripe (and I noticed a tree loaded with berries on August 1st), would stand a better chance of propagating and increasing that variety than a tree which has been robbed of all its berries by birds during the preceding winter. I am quite aware that the local distribution of some plants is, in a great measure dependent on birds; but, with regard to holly-berries so disseminated by the migratory thrushes, &c., the great majority would be deposited on arable or pasture land, where the young plants would be speedily eradicated by the plough or scythe, and consequently the parent tree would stand a worse chance of propagating itself from seed, than the variety from which the berries had fallen on ground adapted to their growth. Perhaps Mr. Hart will kindly point out where this hypothesis is "so different" from the theory of Messrs. Darwin and Wallace; a theory with which I, as well as most working zoologists, entirely agree.

East Woodhay, Aug. 8 HENRY REEKS

Solar Spots

I EXTRACT the following from NATURE of 28th July, p. 267:—"Mr. T. W. Backhouse, of Sunderland, reports that in May there was a remarkable case of a Solar Spot making a revolution round another. It occurred with respect to the two largest spots of a group which was half way across the northern zone, on May 9th. The smaller spot was south of the larger on the 7th at 3^h, but preceded it on the 12th at 21^h, the line joining the two spots having rotated through an angle of 80° or 90° in $5\frac{3}{4}$ days."

It is interesting to observe that the direction of this rotation, from south to east, is the same as that in which cyclones rotate in the earth's northern hemisphere; in the southern hemisphere they rotate in the opposite direction. This coincidence gives some support to the theory of the solar spots being produced by cyclones. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Aug. 6

Noises Caused by Fish

YOUR issues for May 12 and 19, 1870 contain sundry notices of noises supposed to be produced at sea by various fish. The localities are mostly tropical. But it is not necessary to go so far afield for examples of the noises in question. While on board a steamer at anchor for two or three days in the Tagus off Lisbon in the spring of 1869 (April), I heard noises of the kind referred to, which were attributed by the ship's officers to a fish (whose name I now forget), the sound being produced, it was asserted, only at particular states of the tide. Disposed to consider the explanation a mere sailor's "yarn," or superstition, I did not give to the subject the attention it may have deserved.

Perth W. LAUDER LINDSAY

The Kingfisher's Meal

RETURNING from my morning's round on a pleasant summer's day, I observed a kingfisher perched on a hazel bough close to a pretty little trout-stream; my attention was instantly aroused, for one does not often see these pretty creatures, even during prolonged country excursions, in such a position; and moreover his attitude was peculiar—perfect stillness, with an inclination of the head to the left pinion—just the posture in fact that I have seen a fatally wounded bird take previous to dropping from its resting-place; indeed so close was the resemblance that I expected every moment to see the bird I was watching drop into the water, believing it to have been wounded; guess my astonishment when the supposed invalid was seen to dart with amazing swiftness into the curling stream, rise, and continue its rapid flight without apparent interruption, to the rails surrounding a hay-stack close by, where I saw it making most energetic movements of the head and neck, and first became aware, from observing a silvery, glittering, and writhing little fish in its beak, that, instead of being ill as I supposed, and suddenly determined on trying the effects of a bath, he was actually at dinner. After gorging this lively mouthful, the active and dexterous little fisher-bird returned to his hazel bough looking quite as invalidish as before; but now I was aware of his intentions. "Natura est dux optima." PHILALETHEIAN

Ancient Egyptian Forests

A NOTE in the *Academy* for July speaks of ancient forests now turned to chalcedony, *e.g.* at Cairo, thus indicating a profuse vegetation in former days.

Let it not be forgotten that the hieroglyphs represent Egypt as the "land of trees," Khem having been the god of gardens. On the Rosetta stone Egypt is indicated by "a tree and the sign of land" (*vide* Wilkinson's Ancient Egyptians, ii., 184-7). It seems that the destruction of trees is an unvarying accompaniment of dense population. A. HALL

Poisoning by Œnanthe crocata

IN your comments on the rapidly fatal poisoning case, recorded by me, where a man and cart-horse quickly died after eating a small portion of the roots of this plant, you remark "it seems strange that the horse, as well as the man, should not have rejected a plant of so acrid and suspicious a flavour." Now the flavour of the *root* of this plant is known to be mild and pleasant, and not acrid. I can confirm the truth of its mild taste from experience, as I have twice eaten portions of the root for experiment: the taste is intermediate between that of a turnip and the stalk of celery. The poison did not act as an irritant, but the deaths resulted from paralysis of the heart.

WORTHINGTON G. SMITH

BARON HUGEL

THE death is announced of Baron Charles Hügel, well known as a scientific explorer and a cultivated man of letters. He was born 25th April, 1796, and, after completing his education at Heidelberg, was for some time engaged in the wars in the early part of this century between Prussia and France, and in 1814 he took part in the triumphal entry into Paris. In 1824 he relinquished military pursuits, and returning to Vienna, entered with great earnestness into the study of Natural Science, for which he had always shown a decided taste. For many years he studied assiduously, preparing himself for an expedition he had planned round the world. In 1831, on the 2nd of May, he set sail from Toulon, and was away six years. His ship was fitted out with every appliance for a scientific voyage, and in all the various localities he visited in Asia, Africa, and the then unknown field of Australia, he amassed large and valuable collections. These were, on his return, purchased by the Austrian Government, and to them the Vienna Museum owes its great importance, especially in the botanical treasures he had so lavishly accumulated.

The materials he brought back with him, and the abundant notes he had taken, were utilised in several elaborate scientific publications, such as Endlicher's "Plants of the Swan River District (Australia)," and Heckel's "Fishes of Cashmere."

The baron also delivered two learned and interesting addresses to the meeting of German Naturalists in 1838 and 1843, and besides these he sent many valuable scientific papers, especially on botany, to the Vienna scientific publications.

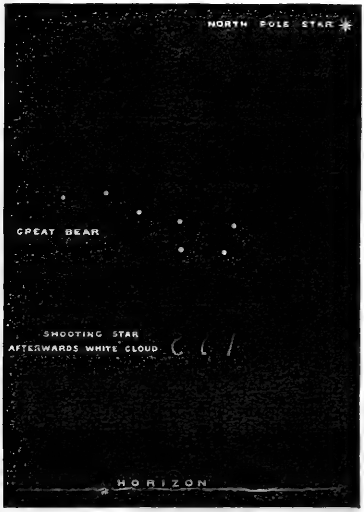
He is also the author of two works in German, "The Basin of Cabul" (Vienna, 1851), and "Cashmere and the Empire of the Sikhs" (Stuttgardt, 1840).

For many years he continued to take a very active part in all the scientific progress of his native country and of Italy. At the time of his death, in his seventy-fifth year, he was Austrian Minister at Brussels.

THE METEOR OF AUGUST 15

We have received descriptions from several correspondents of the remarkable meteor seen on the evening of August 15 over the north of England and Ireland and south of Scotland, to which we referred in our last number.

A correspondent from Portrush sends the following description and sketch:—"At 8.50, on August 15, when stars of first magnitude were only faintly visible, a shooting star was seen in the north-west. I have shown its position in the heavens in the accompanying sketch. It was observed to leave behind it a white thin cloud which

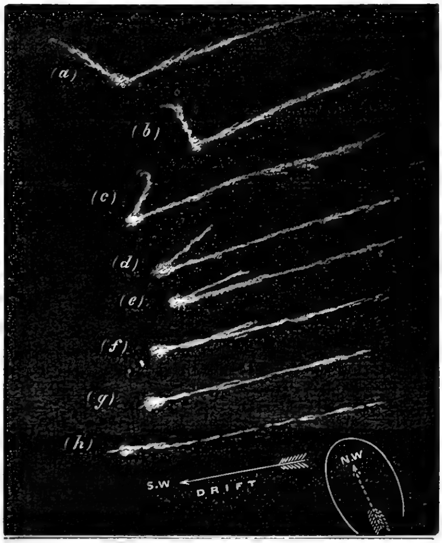


Shooting star, 8.50 P.M., August 15.—N.N.W. 25 deg. above the horizon, left behind it a streak of white cloud, which was clearly visible for ten minutes, drifting with the wind.

drifted a little to the west, and altered its shape from a straight line to a crescent. It was evidently illuminated by the light of the setting sun, and disappeared gradually in ten or fifteen minutes. Was the white thin streak of cloud, vapour, or dust? I observe by the newspaper that this cloud was seen in the neighbourhood of Belfast some forty miles distant, from which I infer that the phenomenon took place at a considerable altitude."

At Dunbar it is described by an observer in the following language:—"A remarkable atmospheric phenomenon was witnessed at Dunbar on Monday night. The phenomenon was first seen about a quarter before nine o'clock, and at that time it was more than half-way up the northern horizon. When first observed it had the appearance of a ball eight or ten inches in diameter, of a bright sparkling white colour tinged with blue, hanging suspended in mid-air. The colour, indeed, throughout was much the same as that of a star of the first magnitude. From the head or ball there issued a tail of the same bright colour, apparently three or four yards in length, and pointing in a north-easterly direction. By-and-by, however, a second tail seemed to branch off from the middle of the first one, at an angle of forty-five degrees, thus giving to the tail of the figure a cleft or forked appearance. This second tail seemed to come and go, being occasionally detached for a few seconds, sometimes indeed being lost sight of altogether, then suddenly coming into view, and appearing to unite again. The phenomenon lasted with little variation for fully twenty minutes, and then proceeded very slowly in a south-westerly direction."

At Kirkbank, near Burntisland, it presented the following appearance:—"A brilliant shooting-star appeared in the north-west on a bright evening sky, and darted out of sight northwards. Its path was precisely that of a body obliquely reflected from an air-cushion. It left a trail like a nebulous haze. At the point of reflection a vivid spot remained, and fainter trails before and behind; corresponding to head or ball and tails noticed at Dunbar. The nucleus drifted towards south-west, and the branches gradually folded together behind, all disappearing as a



faint streak. Duration estimated fully ten minutes, terminating about 9.5 P.M., as Dunbar notice has it." The successive aspects were sketched by the writer and annexed.

And at Arran the appearance presented appears to have been very similar:—"On Monday night about half-past nine o'clock, there was a peculiar manifestation of what appeared to be electrical agency in the sky, at Whiting Bay. At that hour a bright light was seen to flash out from the north-west, near the horizon. It suddenly spread upwards in the form of a long ribbon, the upper half of which afterwards doubled down, when the whole assumed a horse-shoe form, and then gradually faded away. The sky was at the time perfectly clear, and a number of stars were visible, but the brightness of the meteoric appearance completely outshone them."

We should be glad to receive further descriptions of this remarkable meteor from some of our astronomical correspondents.

SCIENCE OF WAR

II.

MACHINE GUNS

THE Machine Gun has for a good many years, in one form or another, excited the attention of the military authorities of Europe and America, and recent events have made it the subject of a great deal of popular interest. The best known guns of this class are the Gatling Battery and the Mitrailleur. The first (of which Figs. 1 and 2, taken from photographs of the original in the possession of the British Government, present a front and rear view) is an American invention, and did good service on many occasions during the late civil war. Three sizes of this gun are constructed. The smallest has ten steel rifled barrels, the calibre being suited to the musket cartridges

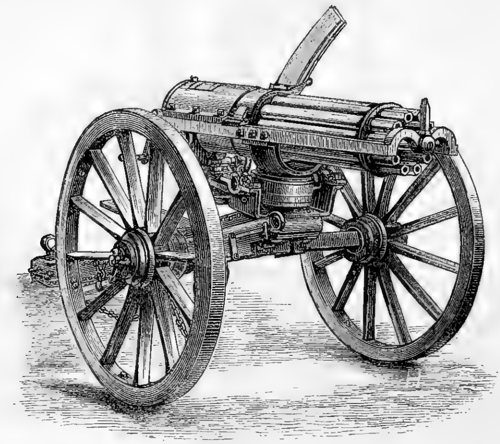


Fig. 1.

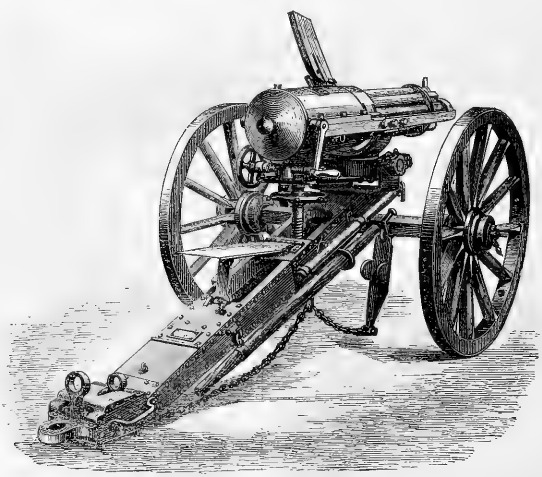


Fig. 2.

used by different Governments. The second-sized gun is constructed with the same number of barrels, but is invariably of three-fourths inch calibre, and discharges solid lead balls of $4\frac{1}{2}$ oz. in weight. The largest sized gun, which is of one inch calibre, has sometimes ten, but generally six barrels. It is provided with solid lead balls half-a-pound in weight, and can also use explosive projectiles. One of the main features of the gun is that it has as many locks as barrels, each barrel and its lock revolving together. Its success, therefore, as a whole, does not depend upon that of each of its parts, for if any of the barrels or their locks are injured, the remaining ones continue to work as well as ever. The weapon is supplied with cartridges by means of "feed-cases," through the hopper—the upward projection rising at the end of the breech-covering nearest the barrels. When it is in operation the cartridges are placed in the rear ends of the barrels, and the breech is closed at the time of each discharge by a forward motion of the locks. A return movement extracts the shells when the cartridges have been fired. In the ten-barrelled gun five cartridges are being loaded and fired whilst as many shells are in different stages of being extracted. The locks are not attached to any part of the gun, and operate on a line with the axes of the barrels. Whilst the gun is revolving, "they play," to quote the words of the manufacturers, "back and forth in the cavities in which they work, like a weaver's shuttle, performing their functions of loading and firing by their impingement on stationary inclined planes or spiral projecting surfaces." The weapon can be loaded and fired only when the barrels, inner breech, locks, &c., are being revolved, all of which operations are set going by a man simply turning the crank. In the most recent guns the covering and back diaphragm in the outer casing are perforated, the apertures being closed by

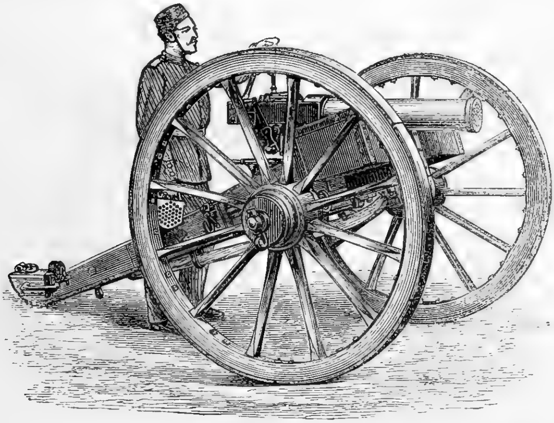


Fig. 3.

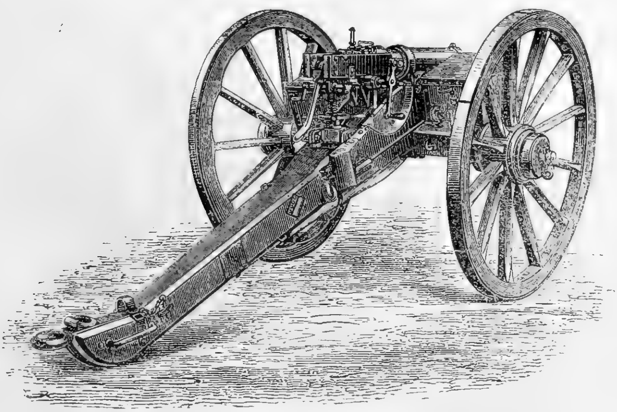


Fig. 4

means of a single removable plug. In this way the locks may be inserted and removed without taking off the caseable plate, obviously a great improvement, as it renders the inspection and repairing of the locks much simpler. The newest guns are also cocked by means of a knob placed at a point on the right side of the weapon. When turned, this knob permits the gun to be revolved without being snapped while not in use. If its position is reversed, the gun can be made to snap or fire at once. The carriage on which the gun is mounted is so constructed that the latter may receive, when being fired, a lateral motion, "so as to sweep the sector of a circle of more than twelve degrees, without moving the wheels or the trail of the carriage." Five hundred yards or more may thus be covered without the continuous fire of the gun being interrupted.

We turn now to the Mitrailleur Gun, of which there are two kinds, the French Mitrailleur and the Belgian, that of Montigny. The gun of which in Figs. 3 and 4 (also taken from photographs of the original) we present views from different points, is that which is at present being made the subject of a great variety of experiments at Shoeburyness. It was constructed under the superintendence of Major Fosbery, and, although improved in detail, in all essential points resembles the Montigny gun. It is the latter that we shall describe in what follows, making free use of the careful report on the subject presented by Major Fosbery about two years ago to the Government.

Like the Gatling gun, the Montigny Mitrailleur is made of three sizes, the smallest containing nineteen guns and the largest thirty-seven. Major Fosbery's gun is of the latter size, is of ·534 inch calibre, and weighs 400 lbs. The barrels are planed exteriorly to an hexagonal form, those of Major Fosbery's gun being rifled on the Metford system. They are fitted and soldered together, and to the wrought-iron tube which surrounds them and forms the barrel of the weapon. To this barrel, it will be seen, are screwed at the breech end two broad plates of wrought-iron. They are placed vertically, and are connected together at the end nearest the barrel by the ring into which the latter is screwed, and near the other end by a transverse bar. These plates form what is called the breech attachment. Placed between them are the breech-block and the lever, of which the long arm forms a handle—in Fig. 3 raised, depressed in Fig. 4. Attached to the short arm of the lever is a cylindrical mass of gun metal, confined to a box or recess of similar shape, in which it can be played freely to and fro. Short tubes are bored in the metal of this cylinder, corresponding in number and position with the barrels of the gun, and in each of these are placed a spiral spring and a small steel piston. On the face of the cylinder a perforated steel plate is screwed, through which the pistons project. They also pass through corresponding holes at the bottom of the recess in which the cylinder moves. In a vacant space which now occurs slides a vertical steel plate or shutter, and beyond we come to a second plate, screwed to the face of the main breech-block, containing a small point or striker, corresponding with each barrel, and therefore with each spring and piston in the movable cylinder. The vertical plate or shutter by which the strikers and pistons are separated, is moved by a transverse axle by means of two ratchet wheels, which take into tooth racks placed on the back of the plate. The axle is kept in position by a coiled spring, and is provided with a handle, which may be seen in both Figs. 3 and 4, on the right of the gun.

Turning back to the lever, we find that it is secured to the breech attachment by trunnions working in brass bearings attached to the side plates, and forming its fulcra. Its short arm is connected with the breech-block by a link formed of two pieces, furnished with left-handed screws, and united by a screwed collar, by turning which the link can be lengthened so as to compensate for any wear and tear in the working parts. By raising the handle of the lever the breech-block is drawn back by means of the link, and by the same means it is pushed forward and forced against the rear end of the barrels when the handle is depressed. As the lever, when the handle is depressed, rests against the bar uniting the plates of the breech attachment, it is obvious that the breech-block could be removed from its place only by a force sufficient to fracture the bar or the trunnions of the lever. No force is ever applied to it greater than that which arises from the explosion of a single cartridge.

The only remaining part of the gun is the cartridge-holder. It is a steel plate about half an inch thick, in which holes are bored, corresponding in position with the strikers and barrels of the gun, and made to fit accurately the heads of the cartridges. On either side of the gun is an ammunition-box, plates being carried ready filled in one, and boxes of cartridges occupying the other. Flanges forming perpendicular grooves are attached to the face of the breech-block, and in these the cartridge holder and extractor is made to slide vertically.

If the gun is to be loaded the lever is raised, when the breech-block is withdrawn to its utmost limit, the lock springs are freed from compression, and the points of the pistons rest lightly against the steel shutter behind which they are placed. A full cartridge-plate is dropped into its place, the strikers being pushed back by its bevelled edges as it descends. Next, the lever is depressed, when the breech-block advances, the cartridges are forced into their barrels, all the springs are compressed, and the pistons are urged against the steel shutter in front of them. When the gun is to be fired the handle to the right is turned. The shutter which is connected with the axle to which the handle is attached at once begins to descend, and as a vacant space is thus left for the pistons, they shoot, one after the other, across it, and come into contact with the strikers. The latter communicate the blow to the cartridges, which are immediately fired. To avoid friction between it and the pistons, the upper edge of the steel shutter is bevelled, and it is so cut into steps that two contiguous barrels are never fired consecutively. The whole thirty-seven are fired by $1\frac{1}{4}$ turns of the handle. At any point the firing may be stopped, and the fired cartridges be replaced, or the whole may be fired by a rapid motion of the handle. When the cartridges are exhausted, the lever is raised, the breech-block is drawn back, and the plate containing the empty cases is taken away. A fresh loaded plate is substituted, when the breech is again closed and the firing renewed.

Neither in the Gatling nor Montigny do the barrels radiate, as is generally supposed; they are arranged perfectly parallel. That such must be the case is indeed evident on slight consideration. For were it desirable to render the tubes in any way divergent it would be necessary, in the first place, to fix upon a specific range at which the arm should be used, as upon the locality of the target would depend the degree of radiation; thus, if the charge were regulated to spread in the most advantageous manner at a hundred yards, its effect would be very insignificant at ten times that distance, by reason of its very scattered nature at a point so remote from the gun. On the other hand a sufficient separation of the bullets is always brought about by the unavoidable discrepancies inherent even to a well-finished arm with parallel barrels; for even if the tubes were all mathematically true—a condition practically impossible to fulfil—very slight variations in the powder charge of the cartridges would always prevent the whole series of projectiles from pursuing a perfectly parallel course and lodging in the target within the same limits as those whence they started. As a matter of experience we may mention that the shooting is considered to be exceedingly correct, when at a fair range the whole thirty-seven bullets from the Montigny are lodged in a target measuring twelve feet square.

We need not here enter into the disputed question how far machine guns are capable of competing with the ordinary field guns. There can be no doubt that if the former be well constructed, they ought to be much more easily worked in the field than the latter. It is in their favour, too, that the effect of their projectiles does not, as is largely the case with field guns, depend upon the proper action of a fuze; and the extraordinary rapidity of their fire (the Gatling gun may be fired, when well manned, from 400 to 500 times per minute, and the Mitrailleur 379 times, or more) is a decided advantage. The absence of recoil by which they are distinguished is also noteworthy. The weak point of the Mitrailleur is its comparatively small range. There is no evidence that it can be fired with effect much over 800 or 900 yards, so that it is comparatively useless at distances which a field gun commands with ease. Moreover, its trajectory at short distances is said to be rather high, and hitherto the cartridges have manifested a tendency to stick after having been fired. These facts alone would be decisive against trusting solely to the Mitrailleur. But it does not follow, because it is not good for all purposes, that it may not be useful for some. There are obviously many positions in which it might inflict great damage on an enemy. Doubtless much light will be thrown on its capabilities by the tests to which it is being submitted at Shoeburyness, and by the manner in which it bears itself among the fearful scenes in which it is at present playing a prominent part on the Continent.

NOTES

DR. C. H. SCHAIBLE has published a little pamphlet of 16 pages, entitled "Self-help on the Battle-field" (*Selbsthülfe auf dem Schlachtfelde*: Trübner), for gratuitous distribution among the German troops, and solicits its reprint in Germany. In spite of the great care now bestowed upon wounded soldiers, it is practically impossible, in great engagements, for all to receive immediate attention; and immense suffering is caused by their lying as much as two days on the battle-field without being removed. In these cases the wounded soldier is thrown entirely on his own resources; presence of mind and quiet judgment are indispensable at such a moment; but both are the result of a knowledge of the proper remedies. This knowledge the author undertakes to teach in a concise and simple way, intelligible to every understanding. He first points out the articles required for immediate dressing, which might be carried by every soldier. He then explains the mode of arresting bleeding; the different steps in dressing the various kinds of wounds; the treatment of fractures; and concludes with some general rules. We earnestly hope it may afford some alleviation of the terrible sufferings caused by the present war.

NOTWITHSTANDING the announcement which was made to the contrary, the International Metric Commission met in Paris from the 8th to the 13th of August, for the purpose of some preliminary business, and then adjourned to a more favourable opportunity. Of the 25 foreign States which accepted the invitation of France, 20 were represented, viz., Austria, Chile, Colombia, Spain, the Roman States, the United States of N. America, Ecuador, Great Britain, Greece, Italy, Nicaragua, Peru, Portugal, Russia, San Salvador, Norway, Sweden, Switzerland, and Turkey. The bureau was constituted as follows:—President, M. Mathieu, of the Institute of France; Vice-presidents, M. Struve, of the Academy of Sciences of St. Petersburg; Prof. W. H. Miller, of the Royal Society of London; Prof. Henry, Secretary of the Smithsonian Institute of Washington; M. Herr, Prof. of Geodesy and Astronomy in the Polytechnic School of Vienna; and General Morin, of the Institute of France; Secretaries, M. Tresca, sub-director of the Conservatoire des Arts et Métiers; and M. Hirsch, director of the Observatory at Neufchâtel. The commission decided that the question to be proposed at a future time should be of two kinds, the first relating to the metre itself, the second to the kilogramme. A committee was appointed to carry out the needful arrangements in the interval before the next meeting, consisting of Prof. Airy assisted by Mr. Chisholm, Baron Wrède, and MM. Wild, Hirsch, Ibanez, Steinheil, Förster, Lang, and Hilgard.

WE regret to learn the death of Dr. Bolley, the celebrated professor of chemistry at the Polytechnic School, Zurich, which took place suddenly on the 3rd of August. He was a native of Heidelberg, where he was born in 1812, and had held positions as assistant and professor in the University of his native town, at the Cantonal School of Aargau, and the Federal Polytechnic School at Zurich. From 1859 to 1866 he was director of this school, and during that time the number of students increased greatly, being attracted from all the civilised countries of the world. He was a commissioner from the Federal Government to the London Exhibition of 1851 and 1862, and to that at Paris in 1867. The works by which he will be best known are his "Manual of Technico-Chemical Research," and his contributions to the most complete and valuable work on chemical technology. To his efforts is greatly due the foremost position which the Polytechnic School at Zurich enjoys among the technical schools of the Continent.

THE Dutch Scientific Society of Haarlem has proposed a series of questions to be answered by the 1st of January, 1872, among which the following are the most important:—1. To define, by anatomical and chemical researches, the mode of origin and the function of wax in living plants. 2. To explain, as much as possible by the aid of original researches, the history of the development of certain malformations and excrescences produced in the oak by different gall-making Hymenoptera. 3. To decide experimentally whether the roots of plants give rise to particular excretions, and in that case to establish the nature of the excreted matters. 4. To study the works of Huyghens, both with reference to the state of knowledge at his time, and with reference to its actual state. 5. The value of the constant of aberration, deduced by Delambre from the eclipse of the first satellite of Jupiter, and that which results from the more recent astronomical measurements, present a difference at present inexplicable. The observations respecting the eclipses of the first satellite of Jupiter are required to be collated, and a new determination of the constant of aberration to be deduced. 6. To make a new series of researches on the influence which the different colours of the spectrum exercise on the respiration of the green parts of plants. 7. To give a monograph of the flora of the sandhills of Holland. 8. To give a systematic description of the marine Phanerogamia.

WE have to announce that Dr. Debus, F.R.S., has been elected Lecturer on Chemistry in the medical school of Guy's Hospital; and that the practical classes of the institution will be under his direction.

DR. T. E. THORPE, of Owens College, Manchester, has been elected by the trustees of Anderson's University, Glasgow, professor of scientific chemistry in the room of the late Dr. Penny.

ON the 1st August severe shocks of earthquake were felt, about two in the morning, in several provinces of Greece, and were attended with disastrous results in the Parnasside and in Livadia. The town of Galaxidi and the villages of Khrusso and Arakhora suffered the most. The latter are nothing more than a mass of ruins; six of the people lost their lives, and all were more or less hurt. At Galaxidi all the houses were injured and some have crumbled to ruins; six children were crushed to death and about 150 adults were injured. Amphissa, the chief town of the province of the Parnasside, also suffered, but in a less degree. There seem to have been additional shocks the next day, but the dates are indistinct.

FROM official statements and from private letters received in London, we learn that Guatemala is suffering from the frequent occurrence and great destructiveness of earthquakes. The chief ravages are in the district of Cuajiniquilapa. The earthquakes have been daily from the 14th April to the last date, the 18th June, with the exception that after the 3rd May there was no shock for three days. The greatest shock was on the 12th June at 3 P.M. The motion was from S.E. to N.E., preceded by hollow rumbling. The church and chapel of the town were nearly destroyed. The principal parochial and municipal buildings, the prisons and custom-houses are in ruins. All the tiled private houses have suffered, especially those built of adobes or sunburnt bricks, only the straw huts escaped. The buildings in the coffee farms are destroyed, as also the fences and ditches. We suppose the fences are built of adobes or tapia. More damage has been inflicted in other parts of the country. The hills of Izquatan, Esclavos, and all about Cuajiniquilapa, exhibit extraordinary effects of the earthquakes. The earth has opened in deep rents, the most prominent of which run from S.E. to N.E. This, it will be seen, was the direction of the great earthquake of the 12th June. This shock lasted from ten to fifteen seconds. Some of the shocks have been tremulous, and others oscillating (*sic* in the Spanish). On the 18th June there were four shocks, between 6 A.M. and 4 P.M. The cause of the commotion is imagined to be in the volcanoes of Trocuamburro and Moyato, as it is known that great rocky precipices covered with trees have been thrown down, rivers dried, and roads blocked.

ON the night of the 12th July, in Salvador, Central America, the sky was dusky and overcast, but cleared off after some electric discharges. Next morning, at 4.50, there was an earthquake. This earthquake seems to have been felt in Guatemala on the same day, causing considerable damage in the departments of Santa Rosa and Jutiapa, particularly to farms, but attended with no loss of life.

IN a recent number of *Les Mondes* Dr. A. Boué calls attention to the fact that a great many scientific publications of the northern and easterly parts of Europe remain almost unknown, except in the countries where the languages (Swedish, Danish, Finnish, Lithuanian, Russian, Czech, Slavonic, Magyar, Polish, Neo-Greek, and Roumanian, and even Dutch) in which they are published are spoken. The author suggests that it would be an advantage if, for each of these publications, either a full translation or an abstract of the papers were simultaneously published in French, English, or German.

WE wish to call attention to a circular respecting the proposed Natural History Museum in connection with Clifton College. It is intended that the museum shall be essentially a British one, and contributions to illustrate the natural history and antiquities of our land are invited. We hope to revert to this admirable effort on an early occasion.

WE have received Sydney papers of May 28, containing a report of an interesting lecture delivered before the Royal Society of New South Wales, by the Rev. W. B. Clarke, on the progress of science during the past year in the Australian colonies.

THE Imperial Academy of Sciences of St. Petersburg has issued the first three parts of Vol. XIV. of their Bulletin; the contents are as follows:—Sur le dégagement d'ammoniaque par les champignons, El. Borscow; Quelques observations faites à 'Observatoire de Pekin—Lettre à M. Wild—M. Fritsche; Sur le genre Dinotherium, réuni à la famille des Elephants, et sur la craniologie comparée des genres de cette famille (Extrait), J. F. Brandt; Sur l'acide urinylique, nouveau produit de l'action de l'acide nitreux sur l'acide urique, N. Sokolof; Manuscrits orientaux de la Bibliothèque Impériale Publique, provenant de la succession de M. le Comte Simonitch, B. Dorn; La houille de Malewka; G. v. Helmersen; Appareil servant à fermer les stigmates chez la Blatte (*Periplaneta orientalis*), O. v. Grimm; Influence de la température sur la conductibilité de la chaleur de quelques métaux (Extrait), R. Lenz; Observations faites à l'Observatoire astronomique de l'Académie en 1868, A. Savitch; Recherches embryologiques sur le *Gyrodactylus*, E. Metchnikof; Recherches anatomiques sur les antennes des insectes, O. v. Grimm; Les canaux sémicirculaires du chat (avec une planche), O. v. Grimm; Une rectification de la table des forces élastiques de la vapeur aqueuse de M. Regnault—Lettre à M. Wild—H. Moritz; Etudes faites à l'aide d'un astro-photomètre de M. Zöllner, P. G. Rosén; Manuscrits orientaux achetés par le Musée asiatique de l'Académie aux hérétiers de M. Graf, B. Dorn; Sur une nouvelle construction de mon Polaristrobomètre (Saccharimètre Diabetomètre) (avec une planche), H. Wild; Sur les aurores boréales du 15-16 Avril et du 13-14 Mai, 1869, H. Wild; Sur l'orage magnétique du 15-16 Avril, 1869, H. Wild; Quelques mots sur les Sturionides européens et asiatiques, J. F. Brandt; Nouvelles recherches embryologiques sur le *Bothriocephalus latus*, Dr. Knoch; Sur deux envois de monnaies, reçus au Musée asiatique, B. Dorn; Sur une méthode d'exprimer les perturbations d'une comète au moyen de séries rapidement convergentes, Dr. H. Gyldén; Propositions concernant la réorganisation du système des observations météorologiques en Russie—Rapport d'une commission, nommée par l'Académie; Réapparition de la comète de Winnecke et découverte de nouvelles nébuleuses, O. Struve; Notice sur l'absorption de l'hydrogène par le fer galvanique, M. H. v. Jacobi; Propriétés générales des polyèdres, qui, sous une étendue superficielle donnée, renferment le plus grand volume, L. Lindelöf; Sur les dérivés chlorés du toluol, F. Beilstein et A. Kuhlberg; La coupole de Mélik-el-Aschraf Abou-l-Nassr-Birsbay, M. Mehren; De quelques versions orientales du conte du trésor de Rhampsinite, A. Schiefner; Histoire de la génération des esturgeons—Communication préalable—A. Kowalewsky, Ph. Owsiannikow, et N. Wagner; Histoire de la génération du *Petromyzon fluviatilis*—Communication préalable—Ph. Owsiannikow; Nouvelles acquisitions de monnaies au Musée Asiatique. B. Dorn. From the same energetic and enterprising society we have the first part of Ruprecht's "Flora Caucasi," carrying down as far as Ampelideæ.

FROM *Cosmos* we obtain the information that M. Böttger has produced a new test-paper which is highly sensitive towards the alkalies and alkaline earths. The reagent is a magnificent colouring matter, obtained from the leaves of an exotic plant (*Coleus verschaffelti*), upon digestion for twenty-four hours with absolute alcohol, to which a few drops of sulphuric acid have been added. The paper is prepared for use by the usual process. The colour is a splendid red, which passes more or less rapidly into a fine shade of green, by the action of the alkalies or the alkaline earths. It is far more sensitive than turmeric; it is unaffected by carbonic acid, and will indicate the presence of the least traces of the carbonates of the alkaline earths in water. A moistened strip of the paper, when held at the opening of a gas jet, immediately assumes a green colour, if ammonia be present.

IT is proposed to erect a statue of Harvey, the discoverer of the circulation of the blood, in the Central Park, New York, and large subscriptions have been received for that purpose. It is to be of bronze, of colossal proportions, "representing Harvey at the moment he felt convinced he had made the great discovery that has immortalised his name." Verily the American sculptors have a pleasant task before them. How does a philosopher usually look under such circumstances?

AT a recent auction sale in New York, the finest known copy of "Elliott's Indian Bible" (Cambridge, 1663), printed in the Indian language, was sold for 1,050 dollars, about 210*l*. There is one copy of this celebrated Bible in the British Museum, one in the Island of Nantucket, and a third on Gardiner's Island, or Long Island Sound.

THE second part of Drs. A. and Th. Husemann's "Pflanzenstoffe," the first part of which we reviewed some time since, is still entirely occupied with the vegetable alkalies or bases, the most important treated of being Coniin, Chinin, Cinchonin, Coffein, Strychnin, Atropin, Nicotin, Hyoscyanin, and Veratrin. The third part, completing the work, is promised in September.

M. AUG. DUMERIL publishes, under the title of "Suites à Buffon," a second volume of his "Natural History of Fishes or General Ichthyology," comprising the *Ganoidei*, *Dipnoi*, and *Lophobranchii*, with an accompanying atlas of plates.

PAPERS ON IRON AND STEEL

I.—A VERY COSTLY AND VEXATIOUS FALLACY

II.

THE greatest enemy to steel is phosphorus; one-tenth per cent. is sufficient to produce serious deterioration, and even to render the harder varieties of steel utterly worthless. As our common English pig-iron is made from clay iron-stones, many of the nodules of which contain, as nuclei or otherwise, the remains of fishes and other animal matter, they are exceptionally rich in phosphorus; and thus all the difficulties of steel-making are greatly increased in this country. There are few results in connection with the progress of British industry of which we have better reason to be proud than our pre-eminence as steel-makers, in spite of the greatest natural disadvantages; and this is the more remarkable from the fact that so great a triumph has been gained by illiterate men who have achieved it by following out with a remarkably sound though unaided sagacity the strict method of true Baconian inductive investigation. Whenever I meet a formulating book-stuffed pedant, I love to tell him of the great unconsidered fact, that while the learned men of the middle ages were muddling their intellects with worthless disputations, the artizans of that period were true inductive philosophers, and that the revival of science only commenced when the men of the universities adopted the method which had always been followed by the men of the workshop.

If the men of the universities have outstripped the men of the workshop in recent times, it is simply due to the fact that science has kept systematic record of its achievements, by means of which each worker has the full benefit of the labours of his predecessors and fellow-workers, and is able to start from the point where these left off; whereas the workshop observers and experimentalists have worked with little or no systematic co-operation. If such co-operation only among one set of investigators has done so much, what may we not expect when it shall not only be extended to the other, but when both sections shall co-operate with each other. This technical and scientific co-operation is the great want of the present age. The artizan needs scientific education, and the professors of science have much to learn from the great mass of facts included in the practical experience of the workshop.

But I must not at present be carried further away into this tempting digression, but return to my main subject by anticipating an objection which will probably be made. The manufacture of puddled steel may be supposed to refute all I have said respecting the impracticability of producing steel directly from English pig-iron. If steel fit for the manufacture of files, chisels, &c., could be made from ordinary English pig-iron by this process, all my statements certainly would be refuted, for puddled steel is simply made by checking the oxidation and arresting it at such a point that some of the carbon in the pig-iron shall remain unburnt.

The facts connected with the manufacture of puddled steel which bear upon the present subject, are as follows: First, puddled steel of merchantable quality cannot be made at all from *common* English pig-iron. Second, the manufacture of puddled steel has been much more successful on the Continent than in England. Third, only *mild* steel and that of an inferior quality is made by this process from English iron.

Referring to the first fact, I may mention that there is a great deal of mystery, and there have been a great many failures and much waste of labour, fuel, and iron in carrying out this process in England. In many forges where it has been tried it is now altogether abandoned, and where it is carried on with any degree of success there is usually much secresy maintained. Now the mystery is not in the puddling, as the necessary modifications in the supply of cinder and the working of the damper are well understood, and have been sufficiently explained in the specifications of abandoned patents and otherwise. The secret part of this process is in the selection of the pig-iron, or rather of the "blend" of pig-irons, for it is found that a mixture of certain brands of pig-iron is better than any single brand used alone.

My own experience in connection with this subject has been very interesting, and is, I think, worthy of record. When engaged as chemist in the works of Sir John Brown and Co. of Sheffield, I made careful analyses of all the numerous brands of pig-iron that are used for various purposes in these works. These I tabulated and kept continually before me, in order to compare their composition with the special uses to which they were applied, and the properties which they, or the material made from them, exhibited. The manager of the iron department was a remarkable example of one of those self-taught, unconscious Baconian philosophers I have above alluded to. He has, during many years, been observing, experimenting, and generalising his inductions consisting of a code of original rules for the manufacture of iron suitable for various purposes. Like the man who had talked prose all his life without knowing it, he has been following strictly the injunctions of the "Novum Organon" in discovering the best "blends" of pig-iron for manufacturing respectively armour-plates, rails, boiler-plates, angle irons, &c., &c.; and among his other mysteries were certain blends for making puddled steel. These he calls his "steel-irons." He selected these, like all the others, without having, or pretending to have, any knowledge of their chemical composition.

By quite a different path, *i.e.* upon purely theoretical chemical grounds, I had determined that certain brands among those I had analysed were the best fitted for making puddled steel, and was anxious to verify my theory. To have asked directly for a revelation of the iron manager's secrets would have been unreasonable, and therefore I simply gave him a statement of the analyses of these particular brands all arranged together, and called them "steel-irons," adding that for the best work I supposed that he mixed with them a proportion of a certain foreign brand. "Hush, don't talk so loud; I don't want these fellows to hear you. Who told you that I use these?" was the substance of Mr. Jevons's reply. My theoretical and his practical selection proved to be exactly the same in result. He had selected just those particular pigs which contained the smallest per-centage of phosphorus, and which relatively to their carbon contained the smallest proportion of silicon.

But this was not all. I had just concluded a number of experiments made for the express purpose of determining the function of manganese in the manufacture of iron and steel, and had come to the conclusion that its usefulness depends upon its readily oxidising, even before all the carbon is oxidised, and thereby affording a base with which the silica could unite and form a liquid and readily fusible silicate. Now this is just what is wanted in making puddled steel, and hence I suggested the addition of the highly manganiferous foreign ore. He had recently discovered that it did just what I expected, and supposed that his discovery was quite new. Such, however, was not the case, for this, like so many other trade mysteries, had been independently discovered by a number of other practical investigators.

The foreign manganiferous metal referred to is Spiegeleisen. Dr. Percy says "Spiegeleisen has been found admirably suited for the production of puddled steel of the best quality, and accordingly it is largely used for this purpose." Now spiegeleisen is remarkably free from those impurities which, as I have stated, cannot be removed from common English pig-iron without also taking out the carbon. I find that the average proportion of silicon to carbon in English pigs is about three-fourths; in spiegeleisen it is below one-fourth, and that the average proportion of phosphorus in the samples of spiegeleisen which I have analysed, is less than one-twentieth of the quantity contained in our Cleveland pigs. Three, four, and five hundredths per cent. is the quantity I ordinarily find in good German or Swedish spiegeleisen. The sulphur seldom exceeds one-tenth per cent., and the large quantity of manganese materially assists in the removal of the silicon. It is, in fact, very similar to the Styrian cast-iron, which, as I have already said, does not present the English difficulty of making steel by the direct process. Both are charcoal-irons, made from remarkably rich and pure ores. The manufacture of cast-iron from such ores, and steel from such cast-iron is mere child's-play compared with our native manufacture.

In reference to the second fact that the manufacture of puddled steel has been carried out more successfully on the Continent than in England, I need only say that this confirms my statements, as the puddlers there are less skilful than ours, and their raw material is a vastly superior charcoal-iron, such as I have already described.

The third fact, viz.: that only *mild* steel of inferior quality is made by this process, is further confirmation of what I have said respecting the necessity of removing the carbon from common pig-iron in order to purify it sufficiently to produce good steel; for even with all this skilful selection of the purest pigs, and the mixing of spiegeleisen with them, it is found in this country impracticable to make puddled steel containing more than one-half per cent. of carbon. Such steel is only fit for rails, tyres, for rubbish cutlery, and other purposes where a very soft steel, or rather steely iron, is used. If the puddling were stopped when the carbon was only reduced to about 1·75, or say 1·5 per cent. (the quantity contained in the best hard cast-steel), the puddled steel would be utterly rotten, it would crush under the hammer whether hot or cold; the reason of this being that even with the best English pigs, the selected "steel-irons," there would, with this amount of carbon, still retain a ruinous proportion of silicon, phosphorus, &c. It is necessary with all available advantages to bring down the carbon to within one half per cent. in order to produce a workable material. Even then it is worth only about one third of the price of good cast-steel.

I might illustrate this subject still further by entering into the details of the chemistry of the Bessemer process and of Bessemer steel, by the history of the nitrate of soda process, and of other attempts to manufacture steel directly from cast-iron; but I think the above is sufficient to expose the fundamental fallacy upon which all such attempts have been founded. I hope to have succeeded more particularly in demonstrating the very great error of those who, in their attempts to make such steel, have, like the friend of my correspondent whose letter opens this paper, deliberately chosen cinder-pig or other inferior iron upon which to make their demonstrative experiments. This was the case with the Heaton Company. They worked for a long time at Langley Mill with one of the worst classes of pig-iron they could have selected for their purpose. I pointed this out to them in a letter printed in the *Chemical News* of February 19, 1869. This effort, the most promising of any of the kind, on account of the action of the residual alkaline soda, was, through this serious mistake, never fairly tested. I witnessed some of their experiments, and analysed and otherwise tested the results. There can be little doubt that with properly selected pigs a material similar to puddled or Bessemer steel, may be made by this process, and by several others that have been tried and have failed; but with the common classes of English pig-irons, all such attempts to make steel directly by the partial oxidation of the carbon must of necessity fail, unless some entirely new, some hitherto utterly unknown method of removing the silicon, phosphorus, and sulphur of the pig-iron is also used. In such a case the novelty, the invention, the triumph, would consist not in the decarburisation of the cast-iron, but in the separation of the other ingredients.

I therefore recommend all inventors who seek to simplify or otherwise improve the manufacture of steel, to direct their attention first to the removal of phosphorus, next to the removal of silicon, thirdly to the removal of sulphur, and last and least of all to mere decarburisation, for that is a problem of the utmost simplicity, and already sufficiently understood.

My next paper will be "On the Chemistry of the Bessemer Process," and will include some original observations, the results of which I believe to be of considerable value to the numerous manufacturers who are now erecting or working Bessemer plant. W. MATTIEU WILLIAMS

SCIENTIFIC SERIALS

POGGENDORFF'S *Annalen der Physik und Chemie*, vol. cxl., part 2.—This number of Poggendorff's *Annalen* contains (1) the conclusion of Ketteler's paper on the theory of Chromatic Dispersion. (2.) The conclusion of Sondhaus's paper on the "tones of heated tubes, and on the vibrations of air in organ-pipes of various shapes." In this part the author compares his formula with Wertheim's experimental results, and shows that in most cases the agreement between them is very close. (3.) The conclusion of Freese's paper on chromates. (4.) "On the work done by gases in motion," by L. Boltzmann. In the first volume of the *Annalen* for 1869 (vol. cxxxvi.), a method of determining the specific heat of air under constant volume is described by F. Kohlrausch, the method consisting essentially in observing the cooling effect indicated by a delicate metallic thermometer enclosed in the receiver of an air-pump when the piston is raised. A few months afterwards (Pogg. *Annalen*, vol. cxxxviii.) Kohlrausch's experiments were criticised by A. Kurz, who objected to them that when the air in the receiver of a pump is expanded by drawing up the piston, it does no work; and that, therefore, theoretically, its temperature ought not to fall. This is of course an obvious blunder; and Boltzmann shows, by a strict mathematical discussion of the experiment, that although the pressure of the air against the piston, and therefore the work done by it, is not quite so great when the piston is raised quickly, as it would be if the movement were indefinitely slow, yet the difference is only a quantity of the same order as the ratio of the velocity of the piston to the velocity of sound, and therefore cannot have perceptibly affected Kohlrausch's results. (5.) "Calculation of the vibrations of a string, taking account of its rigidity," by R. Hoppe. (6.) "On asterism and corrosion-figures in crystals," by H. Baumhauer. (7.) "Comparison of the electrophorus with the electrical machine and the electrophorus-machine," by P. Riess. This paper contains some interesting historical notices of early electrical machines, both frictional and such as acted by induction, but we cannot see that it is of any importance as a contribution to the theory of electrical machines. (8.) "On the velocity of molecular motion and that of sound in gases," by E. Mulder. The author seeks to establish a relation between the velocity of sound in a gas and the mean velocity of translation of its molecules, as deduced by Clausius from the dynamical theory of heat. (9.) "On the production of stationary vibrations and sound-figures in liquidsand gases by solid sounding plates," by A. Kundt. (10.) "On elastic vibrations," by J. J. Müller. By a modification of Kundt's method of measuring the wave-lengths of vibrating columns of air, the author has succeeded in proving that the velocity of propagation of vibrations of great amplitude is perceptibly greater than that of vibrations of small amplitude, both in the case of columns of air and of elastic rods. (11.) "On Leclanché's Manganese battery," by J. Müller. The author finds that polarisation occurs to a considerable extent in galvanic cells of Leclanché's construction when they are employed in a circuit of small resistance, so that under these circumstances he found the electro-motive force of a Leclanché's cell to be only 0·896 of that of a Daniell's cell, whereas, according to Leclanché, it is equal to 1·38 times the electro-motive force of a Daniell's cell. (12.) "On the occurrence of augite-material in meteorites," by C. Rammelsberg. (13.) "On the Lodran meteorite," by G. Tschermak. (14.) "On acoustic attraction and repulsion," by K. H. Schellbach. Additional facts, but as yet no explanation of these curious phenomena. According to the author, the attraction of light bodies by a vibrating tuning-fork was observed and described by Guyot in 1834. (15.) "On the maximum-density and freezing point of mixtures of alcohol and water," by F. Rossetti. This is a short extract from the author's researches on the expansion of water and certain solutions, published in greater detail in the *Annales de Chimie et de Physique* for 1867 and 1869 (vols. x. and xv.). (16.) "A Method of Examining the Structure of Flames," by L. Dufour. The flame is cut across horizontally by a flat lamellar jet of water or of air, and can then be examined at leisure by looking down into it from above. (17.) "Remarks on the colour of iodine," by Carl Schultz-Sellack. The author calls attention to the different colour of iodine in the solid state, or when dissolved in water, alcohol, &c., from that which it shows in the state of vapour, or when dissolved in sulphide of carbon, stannic chloride &c., iodine transmitting in the former case chiefly the extreme red rays of the visible spectrum, and, in the latter case, chiefly the blue and violet rays. He argues that this difference of absorptive character between solid or liquid and gaseous iodine is analogous to the difference which exists, according to Magnus, between the absorptive action of liquid and gaseous water on invisible heat-rays. (18.) Prof. Nordenskiöld announces the discovery of *platinum* as well as gold in the sand of the river Ivalo in North Lapland. (19.) The genuine character of the diamond lately found in Bohemia has been proved by Prof Schafarik by burning a portion of it in oxygen.

Journal of the Chemical Society, July and August, 1870.—The whole of the July number and 22 pages of the August number of this periodical are occupied by the continuation of the very elaborate and exhaustive paper by Dr. Divers on the combinations of carbonic anhydride with water and ammonia. The half acid ammonium carbonate which was previously considered to be represented by the formula $(CO_2)_3\ (OH_2)_5\ (NH_3)_4$ is found by Dr. Divers to contain $(CO_2)_3\ (OH_2)_4\ (NH_3)_4$; when exposed to the air it loses water and ammonia, being transformed into the acid carbonate $CO_2\ OH_2\ NH_3$, of which 8 modes of preparation are described. Another compound obtained in an impure condition by Rose, is considered by the author to have the composition $(CO_2)_5\ (OH_2)_4\ (NH_3)_4$. He also thinks it probable that the orthocarbonate $CO_2\ (OH_2)_2\ (NH_3)_4$ or $CO_4\ (NH_4)_4$ exists. The modes of preparation and properties of ammonium carbamate $CO_2\ (NH_3)_2$ are fully described. The formula attributed to the commercial carbonate of ammonia is $(CO_2)_2\ OH_2\ (NH_3)_3$. The whole of this memoir bears evidence of most careful research and perseverance; the history of the compounds mentioned is traced out; the modes of preparation and properties are carefully described, and very numerous analyses have been made in order to ascertain their composition. Mr. Charles Griffin describes a new gas furnace for chemical operations at a white heat, which does not require a blowing machine. The gas is supplied to this furnace through a bundle of 16 Bunsen's burners, the upper ends of which are surrounded by a metal jacket fitting into a perforated clay plate supported on a tripod. When large crucibles are to be employed, a plumbago cylinder open at both ends and pierced with holes is placed on the clay plate, the conical crucible being supported by the upper end of the cylinder. The crucible and plumbago cylinder are surrounded by a fireclay cylinder resting on three bronze pennies placed on the lower plate. The cylinder is closed at the top by a clay plate, through which a flue is so made that the current of spent gases is bent twice at right angles before escaping into the sheet-iron chimney; the object of this flue is to check the stream of gas which would otherwise be so great as to cool the furnace very considerably. When small crucibles have to be heated, they must be supported on a grate consisting of a clay plate with a cusped aperture. By means of this furnace, ingots of cast iron 4lbs. in weight have been fused in 2½ hours, starting with a cold furnace; when the furnace is hot, 5lbs. could be fused in the same time; the quantity of gas used is 33 cubic feet per hour. The cylindrical body of the furnace may be replaced by one of an oval shape, and containing a muffle in which many operations may be performed, the temperature inside the muffle being sufficiently elevated to fuse silver, gold, and copper. The remainder of the August number is devoted to the proceedings of the society during the session of 1869-70, and a report of the anniversary meeting held on March 30, the latter including the President's address and obituaries of Mr. Brayley, F.R.S., Dr. Graham, F.R.S., Mr. A. B. Northcote, M.A., Dr. Penny, F.R.S.E., and of the late foreign members, Drs. Erdmann and Redtenbacher. These are followed by the list of papers read during the year; the balance-sheet, which shows the society to be in a prosperous condition; and the donations to the library.

SOCIETIES AND ACADEMIES

New Zealand

Wellington Philosophical Society, June 25.—W. B. D. Mantell, F.G.S., president, in his address, directed attention to the large additions made during the past year to the collection of fossil Reptilian remains in the Museum by the officers of the Geological Survey. Several genera are represented, but owing to the nature of the matrix, which exceeds in hardness the most refractory of the "Tilgate" beds, their development will be a work of great difficulty.—Walter Buller, F.L.S., exhibited and described a specimen of the now almost extinct New Zealand Rat or Kiori—in former days a highly valued article of food among the natives, and pointed out its striking resemblance to the ancient black rat of Britain.—T. H. Potts exhibited chicks a few days old of *Anarhynchus frontalis*, showing the characteristic crooked bill, and also the eggs and manner of nidification, along with those of *Charadrius bicinctus*; completely establishing the marked difference between the two birds.—W. T. L. Travers, F.L.S., gave an account of the habits of the Crested Grebe in New Zealand. He has reason to think that they pair for life, and stated that they make additions to the height of their nests, as inundation takes place, but that the eggs will retain their vitality though immersed in water for a considerable time, and inferred that this might have some connection with the coloured mucous layer with which the shell becomes coated during incubation.—Dr. Hector said he had found the nest containing eggs in a tidal lagoon, where it must have risen and fallen with the tide. The eggs were not discoloured. He also exhibited a dusky variety of the bird along with its chick, which is only found on unfrequented inland lakes.—Dr. F. J. Knox exhibited sections of the teeth of a *two-toothed Berardius*, and showed that, from the high state of development, they are not rudimentary as has been surmised, and yet they never appear to protrude through the gum.—John Buchanan exhibited a hybrid between the Australian *Acæna ovina*, which has been lately introduced with imported cattle, and the New Zealand *Acæna sanguisorbæ*.—Dr. Hector, F.R.S., called attention to the practical importance of knowing what fish belonging to the Salmonidæ are found in the New Zealand streams, as the English trout is being rapidly introduced throughout the country. Only one species, *Retropinna richardsonii* (the New Zealand smelt), is mentioned in the latest works as being found in New Zealand; but he exhibited in addition two very distinct forms, one having the same general character as that fish, but with dentition and form of head of an *Osmerus*. The third form is easily distinguished by its small head, forward position of the dorsal fin, small soft mouth devoid of teeth except a feeble row along the upper jaw. It is the common Opokororo of the natives, and grows to a very considerable size in some of the rivers and lakes. He also exhibited drawings of a specimen of *Scopelus* which he had obtained in Milford Sound, as a fourth fish related to the Salmonidæ.

Paris

Academy of Sciences, Aug. 22.—A note was read by General Morin on the first session of the international metric commission, held from the 8th to the 13th of August.—A letter was presented by Professor Newcomb of Washington to M. Delaunay on irregularities in the moon's motion due to the influence of the planets.—A note was read from M. Laranja e Oliveira on a remarkable atmospheric electric shock experienced at Porto-Alegre, Brazil, on June 9th.—M. Chapelas reported his observations on the August shooting-stars. He remarked that the year has been distinguished by a smaller number of shooting-stars than has been known for a long period. The periodic display commenced in the last days of July, but the determination of its maximum presented great difficulty, both from the almost constantly clouded condition of the sky, and from the moon being at her full. During the night of the 10th, in which there was an hour and a half favourable for observation, with an average clear sky amounting to 0·6, forty-six shooting stars were observed, among which were two meteors of the third magnitude. Their mean direction was (as is generally the case, and especially at that hour, $10^h\ 15^m$ to $11^h\ 45^m$) from

N.E. to N.N.E. The point of radiation was very difficult to determine owing to the small number of meteors. The constellations in which most appeared were Perseus, Cassiopeia, Camelopardus, and Aquila. Correcting the number observed for the influence of the moon, we get a mean at midnight of fifty-six shooting-stars per hour in a clear sky, or three more than last year; so that the phenomenon may be considered stationary. At 1 A.M. the sky was completely obscured, and the exact hour of maximum cannot be indicated; but during the time observed the phenomenon proceeded at the rate of ·6 stars per minute.—M. d'Avezac presented, in the name of the author, Don Salvador Clavijo, a general in the Spanish army, resident at the Canaries, a work entitled *Reflexiones sobrè el sistema planetario*, on the rotation on their axis of the bodies comprising the solar system, planets and comets.

Berlin

German Chemical Society, July 25.—M. Küchenmeister has obtained two different naphthoic acids corresponding to the two naphthols, by treating the two naphthylsulphates with cyanide of potassium and the cyanides with potash. One of the acids melts at 217°, the other at 230°.—C. Gräbe has continued his researches on the higher hydrocarbons of coal tar. After treating them with sulphide of carbon, which dissolves chrysen, there remains an impure substance which, when acted on by picric acid, forms a combination crystallising from alcohol in red needles. Decomposed with ammonia, they leave white scales of *pyrrhen*. To this hydrocarbon Laurent allotted the formula $C_{15} H_{12}$. Gräbe's determinations lead to the formula $C_{16}\ H_{10}$, borne out by the picrate $C_{16}\ H_{10}.\ C_6\ H_2\ (NO_2)_3\ OH$. Oxidation with bichromate of potassium and acetic acid yields the chinone $C_{16}\ H_8\ O_2$, from which zinc separates the hydrocarbon $C_{16}\ H_8$. The nitro-compound $C_{16} H_9\ (NO_2)$, and the two bromides in which two and three atoms of bromine take the place of hydrogen, have also been investigated. The author considers his hydrocarbon as phenylen naphthalin, $C_{16}\ H_{10} = C_{10}\ H_8 + C_6\ H_6 - H_4$.—Messrs. Gräbe and Caro have investigated the base contained in impure anthracene, to which, on account of its action on the mucous membrane, they have given the name of acridine. To obtain its sulphate, the impure hydrocarbon is treated with sulphuric acid, and some bichromate of potassium added to the filtrate. It shows a blue fluorescence, and its composition corresponds to the formula $C_{12}\ H_9\ N$, or a multiple of it.—M. Hofmeister publishes researches on phenylic ether and diphenylenic oxide. The former body is obtained by treating sulphate of diazobenzol with phenol, adding potash and distilling the insoluble residue, or by heating together sulphate of aniline, phenol, and nitric acid. It melts at 28°, distils at 240°, is not attacked by zinc, yields with PCl_5 a chloride (not yet analysed), and with sulphuric acid a conjugated acid $(C_6 H\ S_4O_4\ H)_2\ O$.—Lesimple has described a body obtained by distilling phosphate of phenyl, under the name of phenylic ether, ascribing to it the melting point 80° and the boiling point 273°. Hofmeister proves the real formula of this body to be not $(C_6\ H_5)_2\ O$ but $(C_6\ H_4)_2\ O$.—M. Beer has transformed benzophenon $(C_6\ H_5)_2\ CO$ into the chloride $(C_6\ H_5)_2\ C\ Cl_2$, and this by finely divided silver into the hydrocarbon $(C_6\ H_5)_2\ C = C\ (C_6\ H_5)_2$ tetra-phenylated ethylene. Products of substitution have been formed with NO_4, HSO_3 and Br. The bromide has the composition $C_{26}\ H_{17}\ Br_5$.—M. Schöne has obtained a combination of peroxide of barium and peroxide of hydrogen. This compound $Ba \left\{ \begin{array}{l} -O-OH \\ -O-OH \end{array} \right.$ is formed by mixing peroxide of barium with a solution of ½ per cent. of peroxide of hydrogen in water. At 10° it forms colourless monoclinohedric crystals resembling heavy-spar. At ordinary temperatures it becomes yellow, and yields water, oxygen, and peroxide of barium. This explains why alkaline solutions of peroxide of hydrogen are unstable.—V. Meyer communicated his views on the constitution of gallic acid.—Professor Hofmann then rose to report in his name and that of several of his pupils on the following researches partly unfinished, the war having called assistants and pupils from the laboratory into the field.—M. Bischoff, passing chlorine through an alcoholic solution of prussic acid, has obtained two crystalline substances $C_8\ H_{14}\ Cl_2\ N_2\ O_4$, already obtained by Stenhouse, and a new body $C_8\ H_{15}\ Cl\ N_2\ O_4$. Treated with acids or alkalis, they yield carbonic acid, ammonia, hydrochloric acid, and also ethylenic ammonia and a brown substance probably containing very complicated bases.—M. Melens, by treating acrolein with cyanic acid, has transformed it into a kind of trigenic acid.—M. Judson has transformed crotonchloral by oxidation into trichlorocrotonic acid and the salts and ethers of the same. The silver-salt boiled with water yields carbonic and hydrochloric acids, and the chloride $C_3H_2Cl_2$, identical or isomeric with chlorinated allylene. This body can also be obtained by treating crotonic chloral with alkalis. By treating the chloral with perchloride of phosphorus, O is replaced by Cl_2, hydrochloric acid being eliminated at the same time: $C_4\ H_3\ Cl_3\ Cl_2 - H\ Cl = C_4\ H_2\ Cl_4$.—M. Reimer has studied the butylamines. The bromide of tetrabutylamine can not be obtained. It yields at once butylene and hydrobromide of tributylamine.—M. Reiss has prepared butyl-benzol and butyl-anisol.—Professor A. W. Hofmann reported on his protracted researches on the green colouring substance obtained from rosaniline by the action of aldehyde and hyposulphite of sodium. Not forming any combinations and not crystallising, this substance could only be purified by frequent solution and reprecipitation. Numerous analyses lead to the formula $C_{22}\ H_{27}\ N_3\ S_2\ O$ corresponding to one molecule of rosaniline, one of aldehyde, and two of hydrosulphuric acid combined. Rosaniline treated with hyposulphite without the intervention of aldehyde yields a similar product from which acids reproduce rosaniline.—The same chemist, in treating bromide of ethylene with ammonia, has observed the formation of an insoluble white amorphous powder, exhibiting the strange property of absorbing large masses of water and swelling up to a voluminous gelatine. The substance (probably a mixture) corresponds to the formula $\left. \begin{array}{c} (C_2\ H_4)_4 \\ H \end{array} \right\} N_3$ combined with H Br, $H_2\ Br_2$ or $H_3\ Br_3$. But triamines being soluble, it is very likely that the formula should be multiplied.—The same chemist, when preparing cyaniline 25 years ago, observed that the mother-liquor treated with hydrochloric acid, yields dark red crystals. He has at last succeeded in purifying them, and establishing their formula $= C_{21}\ H_{17}\ N_5$. HCl—a triphenylated guanidine $N_3.\ C^{iv}.\ (C_6\ H_5)_3\ H_2$, in which two molecules of cyanogen have combined with two molecules of phenyl. If, however, triphenylated guanidine is treated with cyanogen, a body is formed of exactly the same composition, but of very different properties. The latter is colourless, and is transformed by acids into aniline, carbonic oxide, hydrogen, diphenyl oxamide, and diphenylated parabanic acid; whereas the former, when treated with acids, yields aniline, oxalic acid, and ammonia.—The same chemist has studied the action of triethyl phosphine on cyanate of phenyl, and found the resulting body to be not a cyanurate, but a new substance combining with alcohols, ammonia, &c.—A. Baeyer reported on some further derivatives of mellitic acid.—Mesohydro mellitic acid appears to be the first aldehyde of hydromellitic acid, and derives from it by the action of sulphuric acid and bromine. It is tetrabasic and beautifully crystallised. Only one acid is now wanting to complete the series, beginning with benzoic and ending with mellitic acid. The clue to obtain this acid appears to have been found. The same chemist has obtained a derivative from tetrahydrophthalic acid, which appears to be quinic acid.—Professor Rammelsberg then concluded the meeting by expressing his fervent hopes that a speedy victory of the German arms would allow all members of the society to resume their peaceful pursuits before the reassembling of the society in October.

New York

Lyceum of Natural History, April 4.—The President in the chair.—Dr. J. S. Newberry "On the Earliest Traces of Man found in North America." Dr. Newberry stated that the human relics for which the highest antiquity had been claimed were the Natchez bone and Table Mountain (California) skull. If it could be shown beyond question, that these bones really occurred in the positions to which they have been referred, we should have evidence that man existed on this continent as a contemporary with the mammoth, mastodon, and other extinct mammals, and at a period so remote that all the topographical features of the surface have since changed—in the case of Table Mountain the bottom of a valley having become a mountain summit. As regards the Natchez bone, geological changes have been effected since the date assigned it, which, in Sir Charles Lyell's judgment, must have required a hundred thousand years. In neither of these instances were the human remains actually found by credible persons in strata of high antiquity, and no dependence whatever can be placed upon inferences made from this material in the solution of the question of the antiquity of man. We may to-morrow obtain indubitable evidence of the occurrence of the remains of man in the Table Mountain tertiaries and the Vicksburg bluff; but until such evidence be discovered we must discuss the question leaving these hypothetical cases entirely out of view. No solid and enduring scientific fabric can be reared on doubtful premises. The caves of our country have as yet scarcely been entered upon as ground for archæological research. But one cavern has been examined with any care, that of Carlisle, Pa., by Professor Baird—and this may be said to have been but partially explored. Human remains were found in it, but not of special interest. The fauna represented by the great number of bones collected there, is essentially the same with that which occupied the country on the advent of the whites. This remarkable fact, however, is reported by Professor Baird, that all the species represented in the collections made in the Carlisle cave, have degenerated in size, and this modern degeneracy ranges from ten to twenty-five per cent. The shell mounds on the Atlantic coast, north and south, have been partially investigated by Prof. Wyman, Prof. Baird, and others. In these mounds human remains are constantly met with, but none which can serve as proof of great antiquity. Perhaps the best evidence that these shell mounds are of ancient date, is furnished by the facts reported by Prof. Baird, that those of Maine contain the bones of the great Auk (*Alca impennis*) and those of the walrus. Of these the first is supposed to be entirely extinct, and both in modern times have been confined to higher latitudes. The mounds of the western states, the copper mines of Lake Superior, the old oil wells of Pennsylvania, and the lead mines of Kentucky, really afford us the only traces of human occupation yet found within our territory which have a respectable antiquity, and one which can be measured even negatively in years. All these traces of the ancient semi-civilised people that once inhabited the Mississippi valley, are found overgrown by what we term the "primeval forest," in which are trees five hundred years old; and these trees, in some instances, are growing on the prostrate trunks of individuals of equal size, belonging to a preceding generation. This, then, is the record. We can positively assert that the works of the mound builders were abandoned and overgrown by forests a thousand years ago; how much before that time we have no means of knowing. We may fairly infer that some hundreds of years were consumed in the multiplication of this ancient people, in their spread over and subjugation of the country they occupied, in the substitution of cultivated farms for the pre-existent forest, in the construction of towns so numerous as to thickly dot all the surface, in the thorough exploration and extensive working of the mineral districts and oil fields, in the acquisition of the degree of civilisation they attained, in their gradual reduction in numbers to their total extinction. In New Mexico, Mexico, Central America, and Peru, we have countless monuments of a civilisation generically the same throughout this great area, and a civilisation which was indigenous to America. For the rise, culmination and decline of this civilisation—for it was in its decadence when Columbus first discovered America—we must allow two thousand or three thousand years. Perhaps they occupied much more time than this; but all these changes could hardly have been effected in less than two thousand years. Whether there was any relationship between the ancient Mexicans and the mound-builders is a question yet to be decided. They had this in common, that both were sedentary and agricultural; were miners and builders. But the Mexicans and the Incarial race were famous masons, and built huge structures of dressed stone which scarcely suffer in comparison with our finest architectural monuments. The mound-builders, on the contrary, build in earth and wood, and the structures they raised have little in common, so far as plan is concerned, with those of the southern nations. No geographical connection has been traced between these ancient civilisations. The one seems to have been strictly confined to the Valley of the Mississippi, the other to the high table lands lying between the Rocky Mountains and the Sierra Nevada. In answer to inquiries, Prof. Newberry stated that the inscriptions which covered the monuments of Central America and Peru, like the arrowhead characters of Assyria, and the hieroglyphics of Egypt, were destined to be read. Indeed, it might be said that many of these inscriptions could now be read. But little was to be expected, however, in the way of historical facts, from a perfect translation of all these records. They were apparently, for the most part, local and personal in character, and like the Egyptian and Assyrian records, consisted mostly of religious invocations, laudation of persons, or celebrations of local and temporary political triumphs, which to us have no special significance or value. The mining operations of our ancient Americans were so extensive, that most of the important deposits of copper on Lake Superior had been not only discovered, but worked by them. The working of the oil wells by the mound-builders had not perhaps been noticed by others, but Prof. Newberry asserted it from observations he had himself made. On the bottom lands of Oil Creek, below Titusville, he had, in 1860, noticed that the ground in the primeval forest was pitted in a peculiar way, the pits two or three feet deep, eight or ten feet in diameter, and almost contiguous. These were proved (by excavations made preparatory to boring oil wells) to be the remains of ancient wells or pits sunk in the alluvial clay. One of these, opened to the depth of twenty-seven feet, was cribbed up with timber, and contained a ladder like those found in the ancient mines of Lake Superior, formed from the trunk of a tree on which branches were left projecting six or eight inches. Prof. Newberry had subsequently seen similar pits to these, around the oil springs of Mecca and Grafton, Ohio, and at Enniskillen, Canada West. In the latter locality, a modern oil well cut into the circumference of an ancient one, and this was found to be filled with sticks and rubbish. A pair of deer's horns were taken out thirty-six feet below the surface. The lead vein in Kentucky, to which reference had been made, had been worked by an open cut several hundred yards in length. This was now a ditch some feet in depth, with a ridge of material thrown out on either side, the whole was covered by forest, and trees three feet in diameter were growing upon the ridges of rejected rubbish.

April 11.—The president in the chair. Mr. O. Loew "On Hydrogenium-Amalgam." He showed that when zinc-amalgam is agitated with a weak solution of bi-chloride of platinum, a spongy mass forms upon the surface of the zinc-amalgam, having buttery consistence, and strongly resembling in physical characters, the well-known ammonium-amalgam. This body he considers to be an amalgam of hydrogenium and mercury. To prepare it on a large scale, he shakes thoroughly zinc amalgam, containing three per cent. of zinc, with an equal volume of a solution of bi-chloride of platinum, containing ten per cent. of the salt. The mass becomes warm, and must be cooled from time to time, by plunging the flask, in which the reaction is carried on, into cold water, and also takes on a black colour from the finely divided platinum which is reduced. The mixture is then thrown into moderately dilute hydrochloric acid, by which the excess of zinc and oxychloride formed is dissolved. Unless thus treated, the amalgam is rapidly decomposed with evolution of hydrogen. The platinum is, for the most part, removed with the excess of mercury. The body thus prepared, has the consistency and appearance of ammonium-amalgam as obtained by acting upon an ammonium salt with sodium amalgam. At ordinary temperatures, several days are required for its complete decomposition. It possesses the marked reducing power peculiar to hydrogenium, reducing ferricyanides to ferrocyanides, per salts of iron to proto salts, decolourising permanganate of potassium, &c. This hydrogenium-amalgam also absorbs ammonia, and the resulting body resembles ammonium-amalgam as otherwise obtained. Since Graham compared hydrogenium with the active modification of oxygen, Mr. Loew proposed to consider the following series as parallel:—

Antozone.	Common Oxygen.	Ozone.
Nascent Hydrogen.	Common Hydrogen.	Hydrogenium.

And he further suggests the representing of these three states of hydrogen, by formulæ in the following manner,

[H]	[H H]	[H H] H
Nascent Hydrogen.	Common Hydrogen.	Hydrogenium.

He performed the experiment as described, in a most satisfactory manner, producing a large mass of the supposed hydrogenium-amalgam. The reading of this paper elicited considerable discussion. Dr. I. Walz, spoke in high terms of Mr. Loew's ingenious experiment, but opposed his theoretical views; especially the comparison of nascent hydrogen and antozone, the existence of which he denied. He exhibited the action of bi-chromate of potassium and zinc-amalgam when shaken together, whereby the former is reduced, and apparently a compound of hydrogenium and mercury obtained, which differed in characters somewhat from that exhibited by Mr. Loew. Prof. C. A. Joy referred to the experiments of Schönbein, which conclusively prove, he considered, the existence of Antozone. Schönbein agitated zinc-amalgam with water, and examined the solution obtained. This did not act upon iodide of potassium and starch, until a trace of a proto-salt of iron was added, when immediately the blue colour of iodide of starch appeared. The use of bi-chloride of platinum for assisting the evolution of hydrogen, originated with De la Rive. He thought that Mr. Loew had gone a step farther than either Schönbein or Graham in this most important discovery.

April 18.—The president in the chair. The president made some remarks on the metalliferous deposits of the West, stating it as his opinion, that the production of gold had passed its climax; giving his reasons for so believing. Gold is found disseminated over vast regions in the West; the accumulations in the placers having been worked for ages. He then entered into a description of the manner in which the accumulation of metal had taken place. There is still plenty of gold everywhere, but it is very difficult to separate it from the associated rock in which it is embedded. Those deposits where it could be readily procured, are beginning to be exhausted. The mountain system of the West, cnsidered with respect to the mineral wealth of that portion of the country, he considered divisible into belts, the westernmost or coast range producing mercury, the next eastward or Sierra Nevada Range, is very rich in gold. In the Rocky Mountains, the gold is associated with copper and iron pyrites. In Montana, the gold-bearing veins are extremely rich in that metal, but very difficult to work. Between the Rocky Mountains and the Sierra Nevada, there occurs an argentiferous belt stretching through Idaho, Nevada and Montana; it is in this region that the celebrated Comstock Lode is situated, which, up to the present time, has yielded 75,000,000 dollars. The agricultural portions of California, and the region eastward of the mountains, is of little value except for its mineral wealth, and, if it ever becomes important, it will be by the development of these deposits.

April 25.—The president in the chair. Prof. A. M. Edwards read a paper "On the presence of living insects in the human body," showing that several such cases were on record. In one case a fly had been reared from a larva ejected from the human intestines. As such larva are found mostly in decayed fruit, the plan to be followed for preventing the unpleasant results sometimes, although not always, arising from the introduction of such insects by the mouth, is to avoid eating such fruit or vegetables in a raw condition as are at all decayed.

May 2.—The president in the chair. Prof. A. M. Edwards read a "Report upon a specimen of *Anemone nemorosa* infested by a fungus." This fungus is a species quite common both in this country and Europe, upon the true leaves of the Anemone in early spring, and has been named *Puccinia anemones*. In Ray's "Synopsis" (Third Edition, 1724) it is described in company with true ferns, and it was for a long time supposed that the deeply cleft leaf of the Anemone, with the brown spots upon its under side, was a fern with sori. As Ray says, "this capillary was gathered by the Conjurer of Chalgrave," hence it has come to be known as the Conjurer of Chalgrave's fern. This fungus, like the other microscopic parasitic ones, grows beneath the surface of the plant, throwing out its threads of mycelium among the cells, until it develops the brownish coloured bodies, known as spores (perhaps incorrectly), and it is by the peculiar characters of these that species have been distinguished, although there seem two good reasons for supposing that these plants are not only dimorphous, as has been stated, but polymorphous, assuming different forms according to the habitat in which they are found. In reply to the question as to whether it was true, as was stated by farmers, that barberry bushes infested with fungus or mildew, conveyed that mildew to fields of wheat adjoining, which then showed the presence of "brand," Prof. Edwards remarked that such might very likely be the case, as very little certain is known respecting the life history of these minute plants, and he was now carrying on some experiments, by infesting different plants with fungi taken from others, so as to see if the host which they inhabited modified their characters materially. He described and illustrated, by means of diagrams, the characteristics of the wheat brand, *Puccinia graminis*, and other fungi, and expressed a hope that the botanical members would contribute specimens of such plants as they found to be infested by mildews, brands, and smuts, for the Society's collection.—The President made some remarks on the existence of human remains in caves in this country, in continuation of his communication at a recent meeting. He alluded to the well-known cave at Carlisle, Penn., which has been very carefully searched by Prof. Baird, of the Smithsonian Institution, whose investigations would be, it was hoped, shortly published. Besides the human remains there were found many of various mammals identical specifically with those now or lately living in the vicinity. But one remarkable fact had been developed—viz., that in every case they were at least one quarter larger in dimensions, so that these particular animals at least would seem to have degenerated in size during the lapse of time. Thus, for instance, numerous remains of foxes were found, having characters identical with those now living with the exception of the size. Prof. Shaler's explorations at Big Bone Lick recently, had also brought to light facts of great interest, which showed that the deer and buffalo were comparatively new comers upon this portion of the continent. He (Dr. Newberry) had found the bones of a buffalo on the west side of the Rocky Mountains, although they were not to be seen living there at the present time. The Indians of that district had traditions of the buffalo existing there at a recent period, all of which illustrated the change of fauna which had been for a long time and still was taking place upon this continent.—Prof. O. W. Morris read an "Abstract of the comparative meteorology of the month of March, for the years 1869 and 1870, and of the month of April, 1870," showing that in 1869 the lowest daily mean for March was 13·76°, and for 1870 it was 24·80°, or 11·04° higher. The mean temperature for March 1869 was 34·10°, and for 1870 it was 35·55°, or 1·45° warmer. The mean of the barometer for March 1869 was 29·834 in., and for 1870 it was 29·772 in. The mean humidity for March 1869 was 49·50°, and for 1870 it was 54·85°. The month of March 1870 was thus shown to be warmer than in 1869, the barometric pressure was a little less, and the humidity greater, although there was not so much rain. March 1870 kept up its old reputation as the "windy month." He also quoted from tables, prepared to show some other important facts in meteorology. Thus, examination of the records kept for the last sixteen years (1854-69, both inclusive), shows that the temperature during that time did not vary much, as the mean for the sixteen years is 52·60°, giving eight above and eight below the mean, and a range of 7° only, 1859 having the highest mean (55·66°) and 1868 the lowest (48·67°). The year 1869 was 1·1° below the average; the maximum of 1869 was nearly that of 1855; the minimum was greater than that of any in the series, being 85° above zero, while nine of the years it was below, and in 1866 it was 13° below, and in that year also the thermometer rose to 98·8°, the highest in the series. He also read from abstracts of the temperature, &c., kept by C. Bogert, in New York City, from 1816 to 1853.

BOOKS RECEIVED

English.—A Class book of Inorganic Chemistry: D. Morris (Philip, and Son).—Clavis Agaricinorum: W. G. Smith (L. Reeve and Co.).—The Great Sewage Question: W. Justine (J. R. Day and Co.).

Foreign.—(Through Williams and Norgate)—Berichte über die Fortschritte der Anatomie u. Physiologie im Jahre, 1869; 2tes Heft (Henle Meissner, and Grenacher.)—Die Lehre von den Tonempfindungen: H. Helmholtz; 3te umgearbeitete Ausgabe.—Schmetterlinge Deutschland's und der Schweize: H. v. Heinemann.—Etude sur le Calendrier Copte et ses éphémérides: E. Tissot.—Traité d'histologie et d'histochimie: Frey, Spillmann, et Ranvier.

CONTENTS

THURSDAY, SEPTEMBER 8, 1870

*THE MEDICAL SCHOOLS OF ENGLAND AND GERMANY**

II.

THE German University System, described in our last article, has been recently extended to other parts of the Continent. In Holland, for example, the progress made in this direction of late years has been immense, considering the small extent of the country. Again, in France, at the time that Duruy was Minister of Public Instruction, a measure was adopted which was prompted by the same tendency. One of the most distinguished *savants* of that country (Prof. Wurtz) was sent into Germany to collect information as to the great institutions which exist in that country for the promotion of natural science. The results of this inquiry are embodied in a voluminous report, illustrated with plans of the most important buildings. The favourable impression made on the mind of M. Wurtz by all that he saw in this journey may be judged of by the terms employed in the final paragraphs, in which he sums up his conclusions, and enforces the necessity of introducing German organisation into France.

If we pass from the consideration of the resources of English schools to the functions they perform and the ends they have in view, the contrast between Germany and England becomes still more striking. All German professors are State officials, and hold their posts, with rare exceptions, for life. Consequently, the special work which they have to perform in virtue of their calling is the purpose to which they have voluntarily devoted their existence. In Germany a young man who makes up his mind to a scientific career, dedicates himself from the first to the particular branch of science which he intends to follow, and remains true to it to the end. In this way he acquires a mastery over his speciality which could not be attained in any other way; and if he is a man of mark, he becomes the centre of what in Germany is called a "School," that is to say, he acquires a following of younger men to whom he communicates the precious fruits of his own work; partly orally; partly by demonstration, in such a way that the pupils learn from him in a short time, and in the most advantageous manner, what it would take years to acquire by self-instruction. In England, on the other hand, with the exception of the few scientific men who have studied in Germany, all are what we call "Autodidacten"—*i.e.* self-taught men, who have acquired their knowledge in spite of the want of opportunities. For there are very few investigators by profession, and, in the sense above referred to, no "Schools." The number of those who hold University Professorships is extremely small, and among these even there are few who, in the absence of independent means, are willing to devote themselves exclusively to a pursuit which brings in nothing.

Those who belong to that section of the community which is most fruitful in workers—those, namely, who are without the advantages either of birth or means—cannot, for want of substance, devote their lives to physiological research with that completeness which is necessary if great results are to be obtained. The most that they can do is to give their early years to investigation, with the understood intention of eventually abandoning natural science for those practical duties which are to be the occupation of their mature life, as well as the substantial reward of their previous labours. For the development of science great part of their work is lost, partly because there are no laboratories for instruction, but still more because their time becomes absorbed in other occupations at the very period at which it could be most advantageously devoted to this purpose.

It must, however, be borne in mind that, in Austria at all events, it was not zeal for science that induced the Government to take charge of its interests. The motive is rather to be looked for in the tendency which then existed to keep everything under the management of the central authority, and to maintain its control over all social relations. It is, however, of little consequence how the system originated; for a tree that bears good fruit is none the worse for the foulness which lies about its roots. Some may be inclined to doubt wh ther it is after all advantageous that the sciences should be represented in the Government. It will not be difficult to answer the question. The man who is occupied exclusively in research (and it is only such workers that science really cares for) is very slow in reaping the fruits of his labours. The results which are to be attained in the laboratory, however valuable they may be as materials for the future enrichment and development of the people, are not marketable. A single truth may afford work for a decennium, and often only begins to be productive after the death of its originator. The position of the philosopher is special. No one is more helpless and more completely dependent on the support of the public. With the artist, who stands in a closer relation to the man of science than any one else, it is far otherwise; for the wealthy patrons, whose houses he ornaments with his works, even if they are not always capable of appreciating them, are willing to pay him for them liberally. But in the case of the philosopher, it often happens that the work of many years may be compressed into a few pages.

A nation so well-to-do as the English might perhaps be expected to afford the means for the support of science independently of the Government, especially now that the value of scientific culture is better understood than formerly. But even if the means were forthcoming, there would be no guarantee for their efficient distribution, unless that distribution were placed under the control of competent and responsible persons capable of justly estimating the future worth of a scientific worker from the earliest products of his mind. For this forecasting, endowments are necessary which are not to be met with everywhere and at all times. It is a great gain to any country to possess men fit to be entrusted with this responsibility; but, in order that they may be really useful, it is absolutely necessary for them to be in immediate relation with the central Government.

I have so far entered only into the general features of higher scientific education, without making any very special reference to medical studies. For the purpose of taking a more detailed view of the subject, we must examine more particularly the mutual relations of hospital and school. This will form the subject of a subsequent paper.

S. STRICKER

* Continued from p. 350.

ON AN UNPUBLISHED ITALIAN MS. OF THE SEVENTEENTH CENTURY

NOT long ago I acquired an MS., entitled *Estratto del Libro, segniato A; di Prete Antonio Neri*, which I think is of sufficient interest to merit a short notice in these pages. In the first place, let us make ourselves familiar with the life and writings of the priest Antonio Neri. The greater number of biographical dictionaries do not even mention him, but the "Biographie Universelle," and Hoëfer's "Nouvelle Biographie Générale" are exceptions, both giving a short account of him. As to the time of his birth, I can nowhere find a more definite date than *vers le milieu du seizième siècle*, while Poggendorff alone mentions the year of his death, which occurred in 1614. Antonio Neri was born in Florence, and was educated for a priest; but he appears never to have undertaken priestly duties, preferring to devote his time to chemical and physical studies. For the purpose of extending his knowledge in this direction, he travelled all over Europe, collecting scientific *secrets*, as they were then called, and he succeeded in amassing a large number of these. He visited the principal laboratories of Europe, and resided for some length of time in Antwerp, where he wrote his treatise on the art of colouring glass. He did not hesitate to work as a common assistant, performing the most menial operations of the laboratory, when he found it impossible to gain access to the secrets he sought by other means.

The period in which Antonio Neri lived coincides most nearly with that of his countryman Baptista Porta (born 1537, died 1615). Paracelsus died about the time of the birth of Neri; Jerome Cardan died when he was a boy; Van Helmont was thirty-five years old when Neri wrote his "L'Arte Vetraria;" Galileo and Francis Bacon had not reached the summit of their fame; Robert Fludd was busy with his "Historia Macrocosmi;" Glauber was a boy, Kunckel an infant, Becher was unborn. The Paracelsian iatro-chemistry was making way, Crollius was supporting it, while Libavius was the leader of the opposition; the famous "Tyrocinium Chymicum," of Beguinus, was about to appear; the "Academia Secretorum Naturæ," founded by Baptista Porta, had just been dissolved by Pope Paul V., on the ground that magical and unlawful arts were practised by its members; but the proceedings of this first of the scientific societies remained in the treatise "Magiæ Naturalis," which was the most popular scientific book of the period. Such was the state of the scientific world when Neri laboured and wrote.

The only work ever published by Neri was the treatise on glass-making, to which I have referred. This is a small quarto of 114 pages, and is entitled: "L'Arte Vetraria distinta, in libri sette del R. P. Antonio Neri Fiorentino. In Firenze, 1612. Con licenza di Superiori.'

The order of the ecclesiastical authorities for printing this work is conveyed in no less than seven forms, which are signed, countersigned, attested, and endorsed, and bear dates ranging between March 30 and April 7, 1612. This excessive scrutiny may appear strange at first sight, but let us glance for a moment at collateral facts. The "Index Expurgatorius" had been established by Paul IV. in 1559, and in the very first issue no less than sixty-one printers had been condemned, and the reading of works which issued from their presses forbidden. Now, there was still greater need for caution on the part of the Church; for had not a certain fellow-countryman of Neri, named Gordano Bruno, recently propagated all sorts of heresies in his "Cena de li Ceneri," and had he not suffered death for his temerity? And was there not a contemporary and countryman of Neri, whilom professor of mathematics in the university of Padua, who had shown particular relish for the doctrines of Copernicus, and a particular disrelish for those of Ptolemy and Aristotle, and altogether an insufficiency of respect for the Church? Thus it was that the utterly inoffensive "L'Arte Vetraria" came to undergo so much scrutiny, and after having been certified to contain nothing *contra fidem aut bonos mores*, to be printed with *Con licenza di Superiori* on the title-page.

At the same time, I do not at all mean to assert that scrutiny was unnecessary in regard to the scientific works of this period; for although they did not often contain anything *contra fidem*, they very frequently did contain a good deal *contra bonos mores*, in the form of invocations wherewith to raise a familiar demon, recipes for love philtres, and for ingenious draughts for ridding wives of jealous husbands, while the more philosophical "Elixir Vitæ" sometimes required the blood of a new-born babe. I recently met with an alchemical MS., evidently of some rarity, for it was written on vellum, and the binding showed that it had once been in the library of a Doge of Venice: *Recipe Sanguinis Humani* were the first words that presented themselves to the eye. Again, Beguinus says: "*Recipe quantitatem satis magnam sanguinis virorum sanorum, in flore ætatis constitutorum, pone in vase circulatorio, justæ capacitatis, in B. M. continue bulliens, donec draco propriam caudam devoraverit.*" So that, after all, Alexandre Dumas has not given us such a very exaggerated character in the person of the Alchemist Althotas in his "Mémoires d'un Médecin." As to the matter of remedies, I find the following in an MS. in the Sloane Collection (probably so late as the first half of the 17th century): "Rock-crystal, mixed with sublimed arsenic, is an excellent medecine; in fact, you need not any other medecine . . . . it being taught a witch by a demon, named Rachiel, who was of ye order of Cherubins." The quaintness and *naïveté* of this assertion are quite refreshing; now, whether "you need not any other medecine" because the remedy had the sanction of both a witch and a demon, or because powdered rock-crystal and sublimed arsenic had been found by the asserter to be peculiarly adapted for internal administration, we will not pretend to decide; but, surely, so-called scientific books sometimes required examination in the age of the priest Antonio Neri.

Let us, now that we know something of its author, turn our attention to the MS., *Estratto del Libro, segniato A; di Prete Antonio Neri.* There is good reason to believe that the matter of this MS. was extracted by some seventeenth-century chemist from a larger MS. of Neri, of which he speaks in the preface to "L'Arte Vetraria," and which he had intended to publish had his life only been spared.

The text is Italian, but the work cannot be said to be "writ in choice Italian;" it is rugged, and, of necessity, full of technical terms, and it sometimes passes into a curious kind of Latinized Italian. As to the contents, we have extracts from five of Neri's books: from the book *segniato* A, 155 pages; from B, 78 pages; from D, 5 pages; from E, 13 pages; and from F, 6 pages. Between E and F are inserted 26 pages of "Operationi Copiate da un libro antico qui in Pisa;" also 8 pages about the *Green Lion*, and 10 pages of extremely mystical and unintelligible matter, replete with symbols and Arabic words concerning a certain *Donum Dei.*

An account of the subject-matter of the extracts from the book designated A will, I think, give a fair idea of that of the whole MS. In the first place, we have an account of mercury, to which metal is assigned no less than thirty-five different names, and twenty-two symbols. The Eastern element, then very apparent in chemistry, is noticeable in such names as Chaibach, Azoch, and Baruchet. Various *fisationi* of mercury are described, and the formation of some of its compounds. Gold and silver are next discussed; the latter has fifteen names and ten symbols. The fixation and calcination of gold, the calcination of silver, the solution and tincture of silver, and the conversion of silver into gold are then described. Venus (copper) follows, among the fifteen names of which are Tubalchain, Marchaal, and Cobon, but not Cuprum, or Orichalcum, or Æs Cyprium, which is surprising. Then come iron, lead, and tin; then vitriol, which has seven symbols; sal-ammoniac, which has fourteen symbols; sulphur, which has sixteen; arsenic, antimony, sal-alchali, sal-alembrot, sal-tartaro, sal-anticar, and cinnabar. The extracts from book A are concluded by accounts of the calcination of various metals, of the philosopher's stone, and of the work of transmutation. The short extracts from the other books contain matter of a similar nature; various well-known salts are described, together with new and varied modes of making them; and different solutions of the metals, compounds, and operations.

The ideas suggested by this MS. are manifold. We can but be struck by the excessive complexity of chemistry at this period. When a substance possesses more than thirty distinct names, and more than twenty symbols, and when these are used indiscriminately in one sentence, some idea may be formed of a chemical treatise of two centuries and a half ago. Symbols were used lavishly, not alone to express substances both simple and compound, but for operations and instruments. But the alchemists and old chemists had a special object in preserving the mysticism out of which their science had sprung, and which still, as a thick vapour, shrouded it in obscurity. Their precious *secrets* would otherwise have been at the command of the vulgar, and the result of their years of toil would have been sown broadcast over the world. The true science was but just beginning to loom through the darksome mists which surrounded it. At this time the science was made up of alchemy and iatro-chemistry, with a strong flavour of Kabbalism. As to the matter itself, we find in the works of the period scarcely anything more than had been enunciated by Geber some eight centuries earlier; in fact, there was too much beating about the bush to allow of any real progress. Antonio Neri was a somewhat sensible chemist for the period. His leanings towards alchemy were not excessive; he was not a violent Paracelsian; indeed, he was rather a metallurgical chemist than an iatro-chemist.

Such is a brief sketch of an MS., the matter of which, in a completer form, the priest Antonio Neri had intended to publish had his life been longer spared. Whether the original MS. exists we know not. Perchance it may be hidden in some old monastic library among volumes of Canon Law and countless folios of Middle Age Casuistry; perchance in some dusty nook *in Ædibus Vaticanis*, among the thunderbolts of a past Hierarchy. Who can tell? Oh! if some Sovereign Pontiff would issue a mandate apud S. Petrum sub annulo Piscatoris," to command the cataloguing of the library of the Vatican, how would not Literature, and Science, and Art be benefited by the means! and how would not Italy receive yet greater honour as the focus from which emanated the glorious light of Western civilization!

GEORGE FARRER RODWELL

CATLIN'S AMERICAN GEOLOGY

The Lifted and Subsided Rocks of America, with their Influences on the Oceanic, Atmospheric, and Land Currents, and the Distribution of Races. By George Catlin. (London: Trübner, 1870.)

IN this free-speaking record of what Mr. Catlin has seen of American geology, and of his interpretation thereof, we have the results of strong observational powers and of limited scientific knowledge, stated earnestly and ruggedly, with a faithful adherence to what was first mastered in books, and to the views of nature that early teaching gave. Such works are not rare, but they are not often noticed at large, unless, as in this instance, the author's individuality, sincerity, and earnestness are true and striking. We find in this book on the "Rocks of America" that the author believes, first, in the hypothetical granite of a primæval world; secondly, in a "schistoze zone, quite around the globe, and undoubtedly more or less an open and defined fissure between the two systems" of granite below and "sedimentary formations" above (p. 81); thirdly, "that these vast sedimentary beds, underlying secondary rocks on almost every portion of the globe, have been laid by the agency of water, with the disintegrated particles of granite, and by some (as yet mysterious) process become solidified and crystallised much in the same form, and certainly with the same ingredients, as the granite from which they came" (p. 80). It appears, also, that these low-seated sediments comprise the "azoic and palæozoic rocks" of books; "that the remains of rhizopod and algæ life [*sic*] may be found below gneiss" (p. 140), but not in the limestone in the gneiss; that the acceptation of the Laurentian system of rocks, as worked out and explained by the Geological Surveyors of Canada, is to be deprecated; that the Laurentian limestones have been deposited in "the caverns formed underneath submarine mountains, which are free from all currents of the ocean, by the infiltration of water from the overlying calcareous rocks;" "that in these caverns the first movements of organic life (which could not have existed exposed to the currents of the ocean) began;" and that these limestones were thus "imbedded, and in horizontal strata, beneath azoic rocks, and containing the '*Eozone Canadensis*' [*sic*], and other rhizopod remains which have excited so much attention of late, and been ingeniously used to undermine the established system of geological formations" (p. 141). Fourthly, "that the granite crust, though cracked at various points, from contraction on cooling, has a limit to which those rents descend, below which, from intense heat, and the hasty and unindurated state that the external border of the molten mass must be in, the contraction has not taken place; and being in an arched shape, and resting on a liquid far more buoyant than water, that no fracture of the crust to the surface of the igneous mass has ever taken place, and that no amount of matter could be concentrated on the surface of the earth to produce that effect" (p. 82). Fifthly, that what any geologist would recognise as the *débris* of broken granite decomposed in place, Mr. Catlin refers to eruption from his subterranean granite crust, thus:—"In the Rocky Mountains and the Andes granite is very rarely seen, and when met (at the mountain's base, as it most often is, or on its summit), it is uniformly seen in amorphous masses of various sizes, with shapes plainly telling its history, that it has been shattered and torn from its bed by subterranean explosions or other disturbance, and lifted by (or has followed) the rising mass to the summits of the highest mountains, and flowing out from these, is found at the mountain's base, where it has rolled, while the mountain's top is gneiss" (p. 86). Sixthly, that water, getting to the molten mass below the author's granite, has expanded, and not only erupted the granitic boulders, but blown out great cavities, into which vast areas of surface (as the Mexican Gulf and Caribbean Sea) have fallen, making catastrophes, and leaving "subsided rocks"; whilst elsewhere the cavern-roofs have been held up as "lifted rocks." Seventhly. That the mountain floods rush into and along these cavities, leaving but little to reach the plains (p. 11); that such "submontagne currents" as these, "heated by the volcanic furnaces they have passed" (p. 4), rush out into the Caribbean Sea, and make the Gulf Stream. Eighthly. The Caribbean subsidence deluged the whole Antilles and many Aztec cities, dispersed some of the aborigines among the heights of Mexico and the Rocky Mountains, and sent some Caribs to Guiana, Venezuela, and Honduras, whilst others were transported to Florida, Newfoundland, and Scandinavia, in frail craft on the broad back of the new-made Gulf Stream, that still favours us with such of nature's blessings as it has to give.

Though not always urging his own suppositions without some doubt, Mr. Catlin is dogmatic enough in condemning the present views of ethnologists and geologists when not coincident with his own, as must be too often the case, judging from the foregoing exposition of the main points of his baseless and inconsistent hypothesis.

"I was born in the midst of the Apalachian Ranges, and amongst them spent my hunting and fishing days; and neither there nor in twice crossing both the Rocky Mountains and Andes chains, have I seen anything but the sedimentary and volcanic rocks, excepting here and there beds of shoved-up boulders of granite, raised in the manner already described." Thus the author writes at p. 92; and the mountain-life in youth and hard travelling in middle age, here indicated, are as plainly shown forth in the curious book before us, as a very imperfect geological knowledge is shown by the latter part of the sentence and its explanations as found at other pages.

Mr. Catlin's love of wild scenery, his recognition of the wonderful and mysterious in nature, his limited range in modern geology, his adherence to some old theories, his disapproval of later geological discoveries, and his assumption of hypothetical notions that have sprung up in his own active, wondering, and impressible mind, clearly witness that, "as a reader of geological works, and a spectator of many stupendous orographic structures" (p. 200), he has been self-taught on a very limited basis of natural science. Nevertheless, he conscientiously believes that he offers something towards the explanation of the geographico-geological structure of America, and of the history of the human millions who have inhabited those broad lands, with strange lives and languages, and have either disappeared for ever or have left degraded remnants, still decreasing under the deadly influences of the European. The world knows how warmly and persistently Mr. Catlin has laboured for the benefit of the Indians; not merely preserving their traditions and scraps of history, their languages, religions, customs, and features, but in damming back, if possible, the evils that befall them from their Christian neighbours. He has, indeed, spent the greater part of a long and toilsome life in helping them directly or indirectly; and the remembrance of their unfailing hospitality and kindness is with him in striking contrast with the treachery and cruelty they have suffered, and with the indifferent treatment he has himself received from his own people in the matter of Indian research, as detailed at pages 190 *et seq.* and in the Appendix.

T. RUPERT JONES

THE MODERN BUDDHIST

The Modern Buddhist: being the views of a Siamese Minister of State on his own and other Religions. Translated, with remarks, by Henry Alabaster, Interpreter of H.B.M. Consulate-General in Siam. (London: Trübner and Co., 1870.)

THIS is an extremely interesting little book. The minister whose views it records—Chao Phya Thipakon—conducted the foreign affairs of his country from 1856 till two years ago, when he was stricken with blindness and was obliged to retire into private life. It was then that he published the work—"a book explaining many things"—the more important parts of which are here translated. We need scarcely say that, looked at from our point of view, some of his beliefs are sufficiently strange, and that he sometimes expresses opinions on subjects which are altogether beyond the range of science. At the same time he has in many respects advanced far beyond the great mass of his co-religionists. He will accept nothing merely because it has been handed down by tradition, but demands proofs which will stand the test of rigid examination. In endeavouring to explain such phenomena as rain, epidemic diseases, the tides, &c., he will have nothing to do with spirits, good or bad; he takes his stand on observed facts, and although his explanations may sometimes be inadequate, they are generally quite in the spirit of modern Western investigation. So far as he understands them, he heartily accepts the European doctrines of astronomy. All this strikes a European reader as very incompatible with certain aspects of the

Buddhist religion; but Chao Phya Thipakon is convinced that Buddha knew quite well the truth about the real order of the world, and that he accommodated his language to the prevailing conceptions of his time, only that he might be the more free to proclaim his doctrines on higher subjects. Hence it is proclaimed lawful for a modern Buddhist to open his mind readily to all the results of modern research. Some of the semi-religious customs of his countrymen the ex-minister rationalises in a most amusing way. For instance, the beating of gongs and firing of guns which take place on the occasion of an eclipse, are by no means what they are generally represented—an effort to frighten the dragon who holds the sun in his jaws, so as to make him drop it; they are the expressions of the popular pride and pleasure that the mathematicians of the country are able to predict the time when the eclipse shall occur! With the strictly theological portions of this book, we have, of course, nothing to do here; but we may state that in comparing the different religions of the world with his own, Chao Phya Thipakon is as far removed as possible from a fanatical spirit. He expounds his views calmly, and appears always ready to accept new light from whatever quarter it may come. The objections he raises to the Christian theory of the world betoken a thoughtful and inquiring mind, although, unfortunately, those from whom he derived his ideas of Christianity seem to have been exceptionally poor representatives of their cause. The ethical conceptions of the book are generally of a very noble character. In one point—the proper treatment of the lower animals—the writer, of course, carries his doctrine too far, and he certainly bases it on grounds with which Westerns can have no sympathy; but there can be no doubt that he is practically far nearer the truth than the great mass of Europeans of our own day. On the whole, this book may be accepted as a good omen for the future of the East. It proves that amongst the best minds a genuine spirit of inquiry has been aroused, and that the old cosmogonies and superstitions are already beginning to give way before more scientific conceptions of man and the world.

OUR BOOK SHELF

How Crops Feed: a Treatise on the Atmosphere and the Soil as related to the Nutrition of Agricultural Plants. By Samuel W. Johnson. (New York: O. Judd and Co.)

MR. JOHNSON'S earlier treatise, "How Crops Grow," has been rendered familiar to the English public through Messrs. Church and Dyer's admirable edition, and forms a complete manual of the structure and physiology of the plant, treated in a manner specially adapted for agricultural students. The present work is intended as a companion treatise on everything connected with the nutrition of plants. The subject divides itself naturally into two sections, the first relating to the atmosphere, and the second to the soil, as sources of the food of plants. The question whether plants derive their nitrogen direct from the free element in the atmosphere was long a vexed one among physiologists. Mr. Johnson details the experiments which were thought to favour the argument on both sides, giving the preference, and we think rightly, to the later researches of Boussingault, and those of Lawes, Gilbert, and Pugh, which appear to demonstrate that the enormous quantity of free nitrogen in the air is not available for the food of plants; but that they draw their supply of it from the extremely minute quantities of ammonia and other nitrogenous compounds that form an essential ingredient of the atmosphere. Under the head of water as an element in plant-food, we miss any reference to the recent important researches of Dehérain, which show that the evaporation of water from leaves is determined entirely by light, and by those rays only which are efficacious in the decomposition of carbonic acid, and that it may proceed in a perfectly saturated atmosphere. This omission will probably be supplied should an English edition be published. The second portion of the work relates to the soil as a source of food for plants, and is the one which will be of special interest and value to practical agriculturists. Here we find treated in an exhaustive manner the origin and formation of soils, the kinds of soil, their definition and classification, their physical characters, and the soil as a source of food to crops, including those ingredients whose elements are of atmospheric origin, and those whose elements are derived from rocks. We can do no more than recommend to the notice of those interested in agriculture a work which we believe will be found a reliable handbook to that scientific knowledge in which the bulk of English agriculturists are at present so lamentably deficient. The most scientific of all manual occupations is actually conducted on a system which is a mixture of complete empiricism and unscientific theory.

Reflections, Historical and Critical, on the Revival of Philosophy at Cambridge. By C. M. Ingleby, M.A., LL.D. (Cambridge: Hall and Son, 1870.)

IN this *brochure* of some eighty pages, after tracing the origin and fortunes hitherto of the Moral Science Tripos at Cambridge, Dr. Ingleby seeks to gauge the efficiency of its present constitution by a threefold test; reviewing the selection that has been made of examiners, the prescription that is made of books, and the composition of some examination-papers lately set. It is shown how, more than thirty years ago, when philosophy was all but extinct in Cambridge, Sir William Hamilton, of Edinburgh, in vehemently (as his manner was) attacking Dr. Whewell's views of the prerogative character of mathematics in a liberal education, threw out the idea of a Moral Science Tripos, which sixteen years later it was reserved for his contemptuous opponent himself to carry into effect. So established, how coldly the new Tripos was, and has continued to be, looked upon by the dispensers of College favours, is next brought out with much evidence. In the choice of examiners, the remarkable fact is the preference shown for men not distinguished in the Tripos itself; two only, out of forty-six first-class Moral Science men within the ten years from 1851, having later been entrusted with the examining function! But it is in the list of books prescribed and in the examination-paper that Dr. Ingleby finds most scope for criticism. As regards books he is angry, not without reason, at the omission of certain works, and angrier, for reasons sometimes oddly expressed, at the inclusion of certain others; in the end, he would appear not greatly to mind if, among moderns before Kant, for the sake too of Kant, Hume only were retained. The questions set on modern philosophical systems seem to him "mere *aperçus* of outsiders," seldom showing true intellectual mastery and grasp.

Dr. Ingleby is always lively in his narrative, often forcible in his criticism, and more than once, when he becomes personal, a little violent. To English thinkers all round he administers raps on the knuckles with much impartiality, but the full weight of his cudgel is for the backs of Mr. Mill and Prof. Bain. He may be said to write as a disciple of Kant (with more or less of outlook towards Hegel); and every true student of mental science will be with him in his wish to see the great father of German philosophy really and at first hand acknowledged in Cambridge. When, however, his enthusiasm carries him to say that no philosophy that does not derive from Kant is able to explain the apodeictic judgments of mathematics, and that to Cambridge men other (all other?) thinkers waste their breath, the saying seems strong. Not to say that Cambridge mathematicians do not always seem to Dr. Ingleby an unexceptionable court of appeal in matters of philosophy, one does not see how they should be above listening to other philosophical doctrine any more than their German brethren. The illustrious mathematicians of this century in the country of Kant have not been noted for their readiness to accept his view of the philosophy of their science. G. C. R.

Die Zonula Ciliaris. Habilitations-schrift von Dr. Fr. Merkel. (Leipzig: Engelmann. London: Williams and Norgate.)

IN this little pamphlet the author attempts to disprove the existence of the "Canal of Petit." He describes the Zonula itself as a band, triangular in section, which passing over from the summits of the ciliary processes to the lens capsule embraces the edge of the lens, and becomes attached to both its anterior and posterior surfaces. The fine fibres of which the band itself is composed are in the anterior portion somewhat stouter, and joined together by an imbedding substance so as to form a membrane which offers abundant resistance to post-mortem changes. In the posterior portion the fibres are tender, free from any connecting substance, and very speedily break up after death; they thus readily give way before insufflation or injection, and the cavity or canal thus artificially produced in the posterior portion of the zonula is that which is known to anatomists as the canal of Petit. M. F.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Gulf Stream

NOTWITHSTANDING the mistakes in the botanic names in Mr. Groom's letter, forwarded to you by Mr. Gray, there is no difficulty in identifying the leguminous seeds alluded to. There are several different kinds which are certainly brought by the Gulf Stream from the West Indian Islands, and the countries round the Gulf of Mexico, as the plants producing them are common in Jamaica, Honduras, Guiana, &c. They comprise the horse-eye bean (*Mucuna urens*), the sword-bean (*Entada gigalobium*), the *Mimosa scandens*, Linn., *E. Pursætha* (*M. scandens*, Roxb.), and the antidote cacoon (*Feuillea scandens*). They are sometimes drifted on shore in the seed-pod, but usually the seed alone.

In an article "On some Economic Uses of Nuts and Seeds," in my *Technologist* (vol. iv., p. 339), I alluded to several of the applications of these seeds, but the superstitious one referred to by Mr. Groom is new to me, although the setting the horse-eye beans in silver is very common for bracelets, &c.

P. L. SIMMONDS

The Intended Engineering College

YOU were good enough to publish in NATURE for the 18th of last month, a letter in which I stated what appear to me to be serious objections to the foundation by the Government of a special college for the education of engineers for the Indian Service. Last week's NATURE contains a letter on the same subject from Mr. W. Mattieu Williams, to which I certainly should not think it needful to reply if my personal opinions only were the subject of discussion; but, as the matter on which I ventured to address you is one of public importance, and was treated by me as such, I must ask you to insert a few additional remarks.

The objections to the Government scheme which I have urged in my previous letters were two: first, that the creation of a Government College, which would compete on unequal terms with the principal scientific schools of the country, would be an injury to science; and, secondly, that, leaving the interests of science out of the question, it would be an improper way of spending public money.

Mr. W. Mattieu Williams's way of disposing of these objections is simple, if not very conclusive. It is, to take no notice whatever of the second (which is, perhaps, not surprising, seeing that it is only Indian and not English public money which it is proposed to throw away), and to meet the second with what is, at best, a *petitio principii.* Beginning with the assertion that I complain "on most narrow and unreasonable grounds" of "a supposed intention of the Government to aid the teaching of science"—whereas what I do complain of is the supposed intention of Government to do what I believe will have the exactly opposite effect—he devotes the greater part of his letter, not to showing that my view of the probable effects of the Government scheme is wrong, but to a "protest against the principle upon which Mr. Foster's complaint is based." According to Mr. Williams, this principle is, "that Government must initiate no scientific effort, give no special aid or patronage to any college or scientific institution, lest it should assail the vested interests of 'institutions like University College and King's College in London, and Owens College in Manchester.'"

I will not follow Mr. Williams through his vigorous commentary upon this text, and will only remark, in the first place, that my "complaint" was limited to the particular case of the Government entering into competition with private institutions, to do exactly what they are doing, without reasonable expectation of being able to do it better; and, in the second place, that the ground upon which my complaint of such competition was based was, that it would tend to hinder the spread of scientific education. The fact that my own interests and those of my college may be more or less affected if the Government scheme is carried out, might be a ground of private regret, but it certainly would not have been a reason for asking public attention to the scheme through your columns. On the other hand, I do not consider it as a reason for being silent in regard to a matter which, as I believe, concerns the interests of science and of the public.

One word more. The procedure of the Government in this matter, without previously taking the advice of visible and responsible scientific advisers, is a sample of a mode of conducting public affairs which, it appears to me, is the most serious disadvantage of a public kind under which science in this country has to labour. What we want, even far more than the expenditure of more money upon scientific objects, is some system which should assure us that what is spent in the name of science is spent for the greatest advancement of science, according to the best judgment of the most competent authorities.

G. C. FOSTER

University College, London, Sept. 5

Hollyberries and Birds

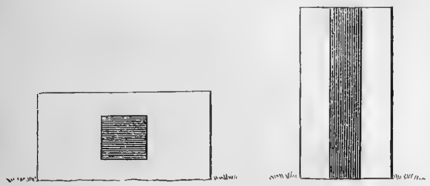
IN reply to Mr. Reeks's question, where his opinion as to the relations between birds and berries seems to differ from Mr. Darwin's, I think it best to allow the latter to speak for himself. At page 240 of "The Origin of Species" (fourth edition), after alluding to the part that insects play in the fertilisation of flowers, by their "unconscious selection" rendering them conspicuous and beautiful, he continues: "A similar line of argument holds good with many kinds of beautiful fruits. That a ripe strawberry or cherry is pleasing to the eye as to the palate, that the gaily-coloured fruit of the spindle-wood tree and the scarlet berries of the holly are beautiful objects, will be admitted by everyone. But this beauty serves merely as a guide to birds and beasts, that the fruit may be devoured and the seeds thus disseminated. I infer that this is the case from having as yet found in every instance that seeds which are imbedded within a fruit of any kind, that is within a fleshy or pulpy envelope, if it be coloured by any brilliant tint, or merely rendered conspicuous by being coloured white or black, are always disseminated by being first devoured." And again ("Variation under Domestication," vol. ii., p. 230): "The white Tartarian cherry, owing either to its colour being so much like that of the leaves, or to the fruit always appearing from a distance unripe, is not so readily attacked by birds as other sorts. The yellow-fruited raspberry, which generally comes nearly true by seed, is little molested by birds, who are evidently not fond of it; so that nets may be dispensed with in places where nothing else will protect the red fruit ('Bull. de la Soc. d'Acclimat.,' tom. vii., 1860, p. 359). This immunity, though a benefit to the gardener, would be a disadvantage in a state of nature both to the cherry and raspberry, as their dissemination depends on birds. I noticed during several winters that some trees of the yellow-berried holly, which were raised from seed from a wild tree found by my father, remained covered with fruit, whilst not scarlet berry could be seen on the adjoining trees of the common kind. A friend informs me that a mountain ash (*Pyrus aucuparia*) growing in his garden bears berries which, though not differently coloured, are always devoured by birds before those on the other trees. This variety of the mountain-ash would thus be more freely disseminated, and the yellow-berried variety of the holly less freely, than the common varieties of these two trees." It appears to me that a hollyberry falling by its own weight from the bush would be borne vertically downwards; and though nourished by a soil impregnated with the decayed leaves of the parent tree, the young plant would be almost entirely deprived of light, and would soon succumb to its more vigorous rivals in "the struggle for existence." Perhaps in a country where but little land is left uncultivated, "the great majority" of seeds transported by birds "would be deposited on arable or pasture land," and thus succeed no better than the others; but so far as my limited experience extends, the most usual positions in which seedlings of the holly naturally spring up, seem to be at the bases of steep rocks or of trees whose branches are not sufficiently low and spreading to exert an unfavourable influence. If Mr. Reeks's speculations be correct, they appear to me to point to the ultimate extinction of the species in a state of nature rather than to its gradual modification.

W. E. HART

Mirages

THE reading of the two letters of your correspondents in the last number of NATURE has called to my mind the fact, which may not be generally known to your readers, that mirages are of frequent occurrence (and I need not add annoyance) to riflemen, especially "small-bore shots."

The most remarkable case of which I have heard was seen at Wimbledon, during the meeting of the National Rifle Association, in July last. The target at the 1,000 yards range is of an oblong form, 12 ft. wide by 6 ft. high, and with a bull's-eye three feet square. On lying down to shoot on the level ground, the target appeared in a reverse position, with the bull's-eye running through the entire height, from top to bottom, thus—



TARGET 12 ft × 6 ft — THE SAME DURING MIRAGE.

the quasi-phantom target continually moving from right to left, backwards and forwards. But this was only visible when lying on the level ground; for in shooting from a mound about four feet high the target appeared quite natural; and what seems stranger still, the lateral motion did not follow the direction of the wind; for it sometimes moved with the wind and sometimes against it. Friends of mine have seen exactly the same phenomenon both at York and at Altcar.

I have several times, on the range here, seen the bull's eye appear to slide up to the top of the target, or down into the ground; and this latter seems the most common and universal form of mirage.

I need not add, that in all these cases the sun-light was intense.

W. PERCY SLADEN

Halifax, Aug. 29

Kant's Transcendental Distinction

HAD I cherished the wish to involve Mr. Mahaffy in a war of words (λογοθηρία), so often degenerating into a war *about* words (λογομαχία), his straightforward and sensible letter, *wreathed* with courtesy and generosity, would have extinguished it. But, with some desire to justify my own censure, I had no such wish; and now that I know exactly what he had in mind, in the examination question, as in the note on page 57 of his work, I will say my say as briefly as possible on his view of Kant's "distinctions."

I thought, and still think (and here the learned translator of Fischer, has misapprehended me), that Kant intended to contrast *general* Sense (not particular sense, as colours, odours, &c.) with Understanding. Otherwise, the repeated reference to "Herr von Leibnitz" would be unintelligible. (See Hartenstein's Ed. of the K. r. V., p. 241 *et seq.*) To suppose, as Mr. Mahaffy suggests, that something more recondite, something quite radical was meant by Kant, seems to me a gratuitous refinement; for *a priori* elements of sense, as those of understanding, are *transcendental*; and the distinction would have only a *logical* difference; or, in Kant's language, it would be a distinction of logical, not of transcendental, reflection. No one, I am sure, knows better than Mr. Mahaffy, that all transcendental *distinction* is the result of transcendental *reflection*; and to this the doctrine of Time and Space is a necessary preliminary; that doctrine, therefore, is not based on the transcendental distinction. I cannot doubt that Kant called the generic distinction between the two faculties (Affection and Function) *transcendental*, not because he was contrasting transcendental elements, but because the distinction was drawn by *transcendental reflection*; *i.e.* reflection which, by the vantage of a transcendental ποῦ στῶ, refers a conception to this or that faculty.

Accordingly, we are not called upon to give a more recondite meaning to the distinction in question in order to explain the use of *transcendental* as applied to it. Rather let us bear in mind what Dr. J. H. Stirling pointed out to me some months ago, that Kant somewhat loosely applies that adjective to other matters besides the *a priori* elements of experience. In fact, he applies it to the said *distinction* and *reflection*, and also to the thing in itself, an object exercising an unknown function, feigned to account for a known state. The unknown function indeed might be called transcendental, but the object is in itself a mere nullity. If Sensation be referred to the *kick* (as Dr. Stirling calls it) received by us from the feigned object, that *kick* is transcendental; but "das Object bleibt uns unbekannt *und transscendente.*" We say, then, that the two forms of sense and the four forms of thought (in apperception) are *transcendental and constitutive of experience*; but the object in itself is *transcendent* and *regulative of thought*. If Kant departs from his own nomenclature in the case of the noumenon, we need not be surprised if he does so in the case of the distinction between sense and understanding.

The bearing of this question on "Kant's View of Space" (which was the topic of controversy between Mr. G. H. Lewes and Dr. J. J. Sylvester in the columns of NATURE) is noteworthy here. The sensibility, according to Kant, is not spontaneous or active, like the understanding. The forms, then (*i.e.* the institutions of Time and Space), are not, cannot be, products of the *activity* of any faculty, and therefore time and space cannot be forms of *Thought* in any legitimate sense of the word. Let it be used in the widest sense possible; let it stand for *the active faculty of mind in general*; and then it can be proved that Kant would have refused to refer to it the forms of general Sense, because he denied to general Sense any activity whatever.

C. M. INGLEBY

Valentines, Ilford, E., Sept. 6

Volcanic Agency *v.* Denudation

MR. DAVID FORBES holds that, in instituting "a comparison between the relative magnitude of the operations of internal and external forces in determining the main external features of our globe, we must grant the first rank to the internal, volcanic, or cataclysmic agencies, since, had it not been for their operations, our globe would have remained without any visible land for the rivers to traverse, or the rain and ice to disintegrate and wear away."

The latter part of the statement cannot, of course, be called in question. But does the conclusion necessarily follow? Suppose I say that a father who died before his son was born, ought, as far as that son's education was concerned, to rank before the schoolmaster who taught him, because but for the father there would have been no boy to teach; or that the quarryman who extracted a block of marble from the quarry ought to rank before the sculptor who shaped it into a statue, because but for the quarryman the sculptor would have had nothing to work upon. In truth, in a case like this, it is hard to attach any definite meaning to the idea of rank. If Mr. Forbes had said that in the task of bringing the earth's surface into its present shape, internal forces have done more work than external

I should have known exactly what he meant, and made bold to differ from him.

I feel sure, however, that those geologists who have endeavoured to revise the almost forgotten teaching of Hutton as to the important part played by subaerial denuding forces in forming the present surface of the ground, are by no means forgetful of the obligations they are under to upheaval for furnishing them with materials to be shaped; and in cases of great mountain chains they have always admitted that the superior elevation of the ground is mainly due to internal action, though they hold that all the sculpturing of the upheaved mass into gorge and peak is due to atmospheric agency. A. H. GREEN

79, Dodworth Road, Barnsley, Sept. 3

Geology of Devonshire

A RAILWAY of eight or ten miles is now in course of construction between Totnes and Ashburton in Devonshire. To a geologist the cuttings near the latter town are most interesting. I am not a geologist, although the science is deeply interesting to me. I returned from Ashburton ten days ago. The rocks there at one part of the line were evidently volcanic. They appear exactly as if they had been melted, and in boiling up a scum or froth had risen on the surface, and in cooling had left air-bubbles, now nearly filled with sometimes yellowish crystals. The rock is very hard, and has a stratum of what was once slate, ten or twelve feet thick, and as the workmen work it out it bears the colour which great fire would give it. As blocks of the other rock are torn out by powder, they are found to contain or enclose fragments several inches square of the superincumbent slate rock, too hard to be melted. This rock is not stratified, but breaks into any form. A few hundred yards off they are working through ironstone as hard as iron itself. The heavy sledge hammer rings on the blocks as on an anvil. At the east end of the town are two pits worked for umber, indeed there are several fields of which the soil a few inches below the surface consists wholly of umber. I do not expect there is any one in the neighbourhood who feels an interest in geology. I saw a letter in NATURE on a geological subject from Mr. Pengelly, of Torquay and I wrote him on the above subject. I have no doubt the line is very interesting in the other parts, as the rocks greatly vary thereabouts.

A learned geologist would have made what I have attempted to describe more interesting. He would find much to employ him in that neighbourhood. W. LUSCOMBE

Hereditary Deformities

IN the lessons in Ethnology in "Cassell's Popular Educator," it is stated, on the authority of Dr. Theodor Waitz, and the Secretary of the Anthropological Society, that "an officer, whose little finger had accidentally been cut across, and had, in consequence, become crooked, transmitted the same defect to his offspring. Another officer wounded at the battle of Eylau, had his scar reproduced on the foreheads of his children." And again, "In Carolina, a dog which had accidentally lost its tail transmitted the defect to its descendants for three or four generations." Do these stories rest on a good foundation? We know that congenital peculiarities of form and disposition are transmitted from parent to offspring, but that an accidental *deformity* should be so transmitted is a very different affair, and if substantiated would introduce *Accidental Distortion* as a co-worker with natural selection in the modification of species.

Faversham, Kent, Aug. 27 WM. FIELD

Poisoning by Œnanthe crocata

PERMIT me to send you the following notes with regard to the case of poisoning by *Œnanthe crocata* which appeared in your issues of 18th August and 1st September.

I. As to the poisonous properties of *Œnanthe*, Prof. Christison found that plants gathered in certain localities were harmless, while others from different places were highly poisonous.

II. As to the mode of death. This seems materially to differ from that observed and recorded with regard to poisoning with hemlock (*Conium maculatum*). In the case of poisoning with hemlock which took place in Edinburgh in 1845 (recorded in the *Edin. Med. and Surg. Journal*, No. 164, and also Prof. Bennett's "Principles and Practices of Medicine") the mind remained clear till the end, and death resulted from asphyxia produced by slow paralysis of the muscles of respiration. The muscular paralysis commenced in the feet.

In the recent case of poisoning by *Œnanthe* there seems to have been coma and convulsion for half an hour previous to death; no paralysis seems to have occurred over the body. From the account of the hemlock case to which I have referred, that plant also seems not to have any particularly acrid taste. The part that seems strange to me is the difference in the mode of death with plants so nearly allied to each other as the *Œnanthe* and the *Conium*. J. W. E., EDIN.

NOTE ON SOME INSTANCES OF PROTECTIVE ADAPTATION IN MARINE ANIMALS

THE various phenomena of mimicry and protective adaptation have recently received much attention, notably from Messrs. Darwin, Bates, and Wallace, and some very interesting facts and reasonings on the subject are contained in the recently published "Contributions to the Theory of Natural Selection" by the last-named author. It can scarcely be needful to explain at much length the nature of the phenomena in question. Well-marked instances of mimicry are not very common; some of the most surprising are those of the leaf and stick insects of the Tropics, which it is almost absolutely impossible, when at rest, to distinguish from dead leaves and twigs. The importance of these resemblances, in conferring protection from attack, will be at once evident. Commoner instances of adaptation, which may indeed be noticed wherever we turn our eyes upon the animal creation, are those of more or less complete resemblance of colour between the animal and its surroundings. The most remarkable instance of this kind which has come under my own observation is perhaps that of the caterpillar of the Emperor moth (*Saturnia pavonia minor*), which, with its green ground and brilliant pink spots, is almost undistinguishable from the heather upon which it frequently feeds.

Numerous instances of this kind amongst terrestrial animals might be brought forward, but less attention has been paid to similar points in the less highly-organised of marine animals. They are, for the most part, much less easily observed in their natural haunts, and their habits and the dangers to which they are exposed are of necessity imperfectly understood. We may note, however, that fishes very commonly assume the colours of surrounding objects; the flounder is almost exactly of the colour of the sand on which it lies, and fishes which bask amongst groves of seaweeds are often of brilliant and variegated colours corresponding very much with the vegetation around them.

The two instances which form the subject of this notice came under my observation while dredging in June last in the Frith of Clyde. In one spot the dredges brought up many plants of *Laminariæ*, with their roots, which consist of a conical mass of contorted and intertwined fibres about a line or two in diameter; amongst these were imbedded quantities of nullipores—a calcareous seaweed of the genus Melobesia—(*M. calcarea*). The larger weed had, in fact, grown in a bed of the nullipore, which came up abundantly in the dredge, and indeed now forms on a closely adjacent part of the coast a raised beach of several feet in thickness. Amongst the nullipore which matted together the laminaria roots were living numerous small starfishes (*Ophiocoma bellis*), which, except when their writhing movements betrayed them, were quite undistinguishable from the calcareous branches of the Alga; their rigid, angularly-twisted rays had all the appearance of the coralline, and exactly assimilated to its deep purple colour, so that though I held in my hand a root in which were half a dozen of the starfishes, I was really unable to detect them until revealed by their movements.

The second instance is that of a shellfish, *Lima hians*. This beautiful mollusc is well known frequently to construct for itself a nest—a long tube lined with byssal fibres and covered externally, after the manner of a caddis-worm, with nullipores, stones, old shells, or probably any material which lies conveniently at hand. We may perhaps account for a habit so different from that of other mollusca by the following considerations:—

The animal is an exceedingly showy one, more so than almost any other British mollusc, having two valves of snowy white, from between which are protruded long tentacular fringes of a brilliant orange or vermilion hue; when alarmed, it darts, or almost, as one might say, flies, in a fitful manner through the water, showing its gorgeous colours very conspicuously—so that indeed in the Channel Islands it has acquired the name of "Angel's Wings." Other mollusca, such as some of the Pectens, are brilliantly coloured, and live without the protection of any nest, but their shells are very strong and close firmly, so that they could not easily be masticated by ordinary fishes. The shell of the Lima, on the contrary, is very fragile, and would easily be dealt with by fishes which are accustomed to devour wholesale crabs and other hard-bodied creatures. It is, therefore, easy to believe that the two characters of tenderness and brilliant colouring would speedily ensure the extinction of the species were it not protected in some extraordinary manner such as that of the concealment afforded by a nest. Mr. Wallace has shown, in a very interesting manner, how birds of brilliant plumage build nests of a character adapted for concealment during incubation, and it seems to me that the similar habit of the Lima may probably be referred to the same cause.

GEORGE S. BRADY

SWALLOWS' NESTS*

A FEW months ago M. Pouchet published an article on the subject of swallows' nests, which seemed destined to modify all our previous ideas of reason and instinct. According to this naturalist, the common swallow had modified his habits, and had made certain progress in the art of nest-building.

Had this theory been correct, we must have renounced our preconceived notions which place an insuperable barrier between reason and instinct. If we assume instinct to be a faculty incapable of development, and not progressive in the animal, it is clear that if any modification or progress were once scientifically proved to have taken place in the arts of which each animal is capable, it would be necessary henceforward to classify such arts as appertaining to the domain of reason.

The communication, therefore, made by M. Pouchet to the Académie des Sciences would have had great weight had the facts which it related been confirmed by subsequent observation.

Such, however, does not seem to be the case, for M. J. B. Noulet has recently pointed out the error contained in M. Pouchet's statement.

According to M. Noulet, there are two distinct varieties of swallows, the window swallow (*Hirundo urbica*, of Linnæus), and the chimney swallow (*Hirundo rustica*).

These two types do not intermingle, and have somewhat different habits; for instance, the chimney swallow is always the first to arrive and the first to leave these countries. Their nests are also different, and the difference is so well marked that there need be no difficulty in deciding as to the builders who have constructed them.

The city swallows (*H. urbica*) always choose the lofty situation, and group their nests in continuous lines, sometimes double and even triple. The *H. rustica*, on the other hand, establishes itself lower down, and constructs its nest apart from its fellows. The nests, too, of the first-named variety are chiefly distinguishable by a greater depth, and by a circular aperture just large enough to allow the bird to go in and out without difficulty.

M. Pouchet, paying no attention to the difference existing between the two varieties to which we have alluded, takes the nests of the city swallow for those of this rustic sister brought to a higher degree of perfection.

This, at least, is M. Noulet's opinion. It remains to be seen if M. Pouchet will accept the explanation, or whether he will be prepared to defend his own theory. In either case the readers of this journal shall be informed.

ALFRED NAQUET

* "Nos deux hirondelles et leurs nids." Par J. B. Noulet.

FACULTY OF SCIENCE IN UNIVERSITY COLLEGE

SOME months ago it was announced in our columns that a Faculty of Science had been formed in University College. In the prospectus now issued for the forthcoming session there appears the following general statement:—

"The Faculty of Science has been instituted to bring into full light the actual extent of the scientific teaching in University College, and to meet, consistently with sound educational principles, the growing demand for instruction in science.

"The Faculty of Arts in University College, instituted to give a general training in literature and science, such as is required for a degree in arts in the University of London, not only has from the first contained chairs in which the scientific instruction has been developed far beyond the needs of arts-students, but by a steady process of growth has come to include others bearing no relation to an arts-curriculum. So considerable is the scientific staff now actively at work in the college, that the authorities believe the time has arrived when the science-teaching carried on within its walls may assume all the character of independence and dignity associated with the academic title of Faculty.

"The demand for instruction in science and the recognition of science in education are facts beyond question. In University College itself the number of students seeking a broad scientific training or pursuing special scientific studies has gone on steadily increasing; while in the University of London and elsewhere, degrees in science, both general and special, have been conferred for some years past. The importance, also, of science as a preparation for industrial pursuits is now generally acknowledged, as appears in the efforts that have been made of late years to supply scientific instruction in so-called technical schools.

"The main principle represented by the new Faculty is, that science should first of all be cultivated for its own sake, and that even where there is a practical object in view, a broad foundation should be laid of general scientific training. It is believed that the habits of thought thus engendered are the first conditions of all true advance, either in scientific discovery or in practical invention. A second principle is, that the pursuit of science should not be divorced from literary culture; and this the Faculty, from its position in University College, is specially enabled to uphold. As regards the interpretation of the word Science, it only remains to add that this is taken in no narrow sense. Certain subjects are included which lie out of the sphere of Natural Science, as commonly understood, but none that do not admit of a strictly scientific treatment."

As at present constituted, the Faculty includes chairs of pure mathematics, applied mathematics and mechanics, physics, chemistry, and practical chemistry, mineralogy and geology, engineering, architecture and construction, botany, comparative anatomy and zoology, physiology, practical physiology and histology, philosophy of mind and logic, political economy.

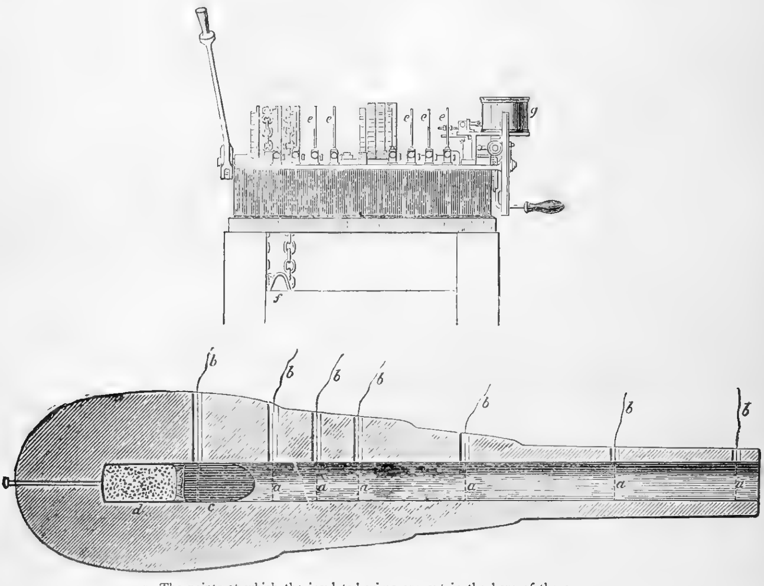
THE SCIENCE OF WAR

III.

GUNS AND GUNPOWDER

WITHIN the past twelve months an invention of considerable ingenuity has been matured which promises to enlarge to a marked degree our knowledge of the science of gunnery. We are all aware of the great perfection to which the cannon in use now-a-days have been brought, and how they represent the results of progress which has been steadily going on during three or four centuries, until at last we have succeeded in producing guns rifled to such a degree of precision that any object visible at a couple of miles may be hit with certainty, while in some cases a distance of ten or even eleven thousand yards is actually reached by the shot. These are improvements at once patent and appreciable, and such indeed as might be anticipated from constant practical and theoretical study of the subject; but while congratulating ourselves upon the advance made in this branch of gunnery science, we cannot, at the same time, but marvel at the slight progress effected towards improving the source of our motive power, and at the fact that no explosive agent has yet been brought forward to supersede the gunpowder now used.

Explosive mixtures have, it is true, been proposed from time to time, whose advent has been heralded with such high-flown praise and promises that one could not but suppose that the days of gunpowder were numbered; in



a The points at which the insulated wires are cut in the bore of the gun.
b The insulated wires leading to the induction coils and disc apparatus.
c The shot.
d The cartridge.
e Revolving discs.
f Weight for setting machinery in motion.
g Stop clock to record the number of revolutions.

one respect or another, however, the new-comer always failed to satisfy its severe military censors, and was thus thrown on one side and forgotten. Even nitro-glycerine and dynamite, of which chemists were at one time so very sanguine, have been entirely rejected for military purposes, and gun-cotton is indeed the only explosive material, besides gunpowder, to which our war authorities are inclined to give ear. The very important qualities which pyroxilin exhibits under various phases of combustion, its immense power, its portable and convenient character, and furthermore its comparative safety when in store and in course of manufacture, render it in some respects equal and even superior to gunpowder for engineering operations.

Nevertheless, at the present moment our knowledge even of gun-cotton is not sufficiently advanced to allow of its being universally employed in the same manner as gunpowder, and it is to the latter, therefore, that we must still look for the principal source of explosive power.

It appears strange that, being as it were continually thrown back upon our elementary explosive, no radical improvements should have been effected in its manufacture; we still employ the proportions of sulphur, charcoal, and saltpetre as they were laid down in the first instance by Bacon and Schwartz, and in fact so stationary has been our position in regard to the matter that it would appear almost as if perfection had long since been attained.

While, however, the chemical composition of powder has remained unchanged, some very important modifications in its rate of burning have been of late years brought about by merely altering its mechanical form and density. Indeed by thus varying its outward character, and without in any way tampering with its chemical composition, an exceedingly rapid, or on the other hand a leisurely, burning product may be secured from precisely the same materials. This means of controlling the explosive character of powder is of especial value in connection with heavy rifled guns, in which an evenly burning product is an absolute necessity; and indeed the most important condition to be fulfilled in gunnery science is always to employ in a gun that powder which is best adapted to the arm. Now it is a well-known fact that the larger the grains of powder are made, the slower does a charge of it burn, supposing always the particles to be uniform in size, and not of a thin chip-like character, or irregular in shape. In rifled guns a slow, regular-burning powder is a *sine quâ non*, for, unlike the case of smooth-bore howitzers and mortars, a steady and increasing push, and not a violent jerk, is necessary to force the projectile through the grooves. For mortars, therefore, a comparatively fine-grain powder is employed, while for big rifled cannon a material of the coarsest description, with grains approaching in size that of a hazel nut, is invariably preferred.

The best charge for a gun, especially if rifled, is moreover that which increases in power as the shot passes through the bore, and which exerts its greatest force as the projectile leaves the muzzle, *but not earlier*; so that, on the one hand, the maximum explosive power is not put forth until the instant the mouth of the gun is reached and the last impetus given, while on the other hand no loss of power is suffered by the charge still burning after the bore is empty. If the charge burns quickly the shot is brought into action too rapidly, and the gun consequently subjected to excessive strain; whereas, if the combustion of the powder is too slow, the shot does not receive the full benefit of the charge, and a large portion of it is lost. Thus, as may easily be imagined, the difficulty has hitherto been to fix upon the precise description of charge necessary to the gun, and it is just this difficulty that has recently been solved by the elaboration of an instrument which accurately informs us of the velocity of any shot during its passage through the gun.

The possibility of discovering what takes place in the bore of a cannon at the time of its discharge, and of ascertaining how fast the shot travels, is a subject that has long attracted the attention of artillerymen, and among others that of a talented officer, Captain Andrew Noble, of the Elswick Ordnance Works. This gentleman's labours have recently been crowned with success; and an apparatus has been devised of which one hardly knows whether to admire more its exceeding delicacy or its wonderful results. With its aid the examination of gunpowder is now being conducted with comparative ease, and what is still more important, with unerring certainty.

A detailed description of the instrument, which has received the name of Chronoscope, would necessitate more space than we have here at our disposal, but its main features, and the principle upon which it is based, are easily explained. The tube or bore of a gun is fitted inside at certain intervals with metal rings (to the number of six or eight) the outside margins of which are sharpened into so many knife-edges. On a shot passing along the bore and through these rings, the edges of the latter are jammed down upon and made to cut through the ends of various insulated wires, one of which is placed under each ring. If we now suppose each of these wires to be in connection with an electric battery, it follows as a matter of course that as one wire after another is cut through, and the insulation consequently removed, an electric current passes; so that if there are six rings and wires fitted at intervals in the tube of the gun, the passage of a shot along it would be instrumental in producing six electric sparks following rapidly one upon the other.

We now understand how the shot is made to tell the tale of its flight; but there remains yet to be explained how the story is written down. This recording of the signals is accomplished by a very simple arrangement. A series of metal discs, one in connection with each wire, is made, by means of a clock-work arrangement, to revolve at a certain rapid velocity, say at the rate of 1,000 inches in a second; the surface of the discs is of polished silver, coated with lamp-black, and as soon as the desired speed has been attained, the gun, which is in electrical communication with the instrument, is fired. As the shot traverses the first ring, No. 1 wire is cut through, and a spark thereupon hops over to the recording disc, removing a little of the lamp-black covering, and thus marking the place by laying bare a minute spot of bright metal. No. 2 wire, when cut by the second ring, leaves a similar record upon another disc precisely in the same manner; and so on with Nos. 3, 4, 5, and 6, the relative position of the six spots on the six discs indicating exactly the velocity with which the shot has passed the six different rings or stations.

A very simple calculation is now necessary to get at the results; if there is a distance of one inch between each point, we know that one-thousandth part of a second has sufficed for the shot to travel from one ring to the other, for we remember that the discs were revolving at the rate of a thousand inches per second. This, however, only by way of example, for as a matter of fact we may mention that a shot usually takes from $\frac{1}{200}$th to $\frac{1}{300}$th of a second to traverse the whole length of the bore, its speed being somewhat slow when passing the first rings, and increasing as it approaches the muzzle of the gun.

A certain amount of correction is of course necessary when reading off the results, but the accuracy of these may at any time be verified. Thus, in order to ascertain whether the electric and mechanical arrangements of the discs are in good order, it would be necessary merely to place the whole number of wires together under a single ring, so as to be cut through at one and the same moment, when the points on the discs should, of course, all coincide.

The great importance of this beautiful invention need not be dilated upon by us, as the value of its aid in experiment is at once apparent to the veriest tyro in gunnery. As a measurer of time and speed of the most perfect character, its delicacy is certainly unsurpassed; for, by merely dividing every inch of the discs into a thousand divisions or degrees, we are at once enabled to calculate with precision to the millionth part of a second.

NOTES

A WEEK or two ago we announced a rumour to the effect that the Government had refused to allow a ship to convey the eclipse observers to Spain and Sicily next December. The rumour was too well founded; the Government has actually refused to tell off a ship for this purpose. This decision in the teeth of the plainest precedents requires no comment on our part; in fact, it is beyond all comment, it is astounding. We are enabled to announce, however, that the American Government, more enlightened than our own, are making extensive preparations; and upon the results of their labours and those of the Continental Governments Englishmen must therefore fall back, in a research which is eminently English. The Americans will send three corps of observation, to be stationed respectively at Malaga, Sicily, and some place in Turkey most available for making the best scientific records and views. One of these corps will be sent from the Naval Observatory, and the other two will be composed of the most scientific men in the country, including the professors from Harvard University. Before the war broke out it was arranged that Rear-Admiral Glisson should extend to the corps at Sicily all the aid and cooperation in his power. But the original plan has been spoiled for the present by the troubles in Europe, Admiral Glisson being obliged to move his squadron to the Baltic for the protection of American commerce in that vicinity.

WE regret to learn that the health of Gen. Sir E. Sabine, the distinguished and venerable president of the Royal Society, is at the present moment such that he is likely soon to demand relief from the pressure of those duties which he has hitherto performed with so much credit.

BEFORE our next number is printed, the annual meeting of English *savants* will have commenced at Liverpool under the presidency of Professor Huxley, whose inaugural address we hope to give in our next issue. The meeting is likely to be one of great interest, and various circumstances will combine to bring together an unusual number of the representatives of every branch of science, including not a few of our foreign *confrères*, who we trust will enjoy the peaceful retreat from the turmoil on the Continent. Atomists and Non-atomists, Cataclysmists and Uniformitarians, Darwinians and Anti-Darwinians, will, for the moment, take the place of Gauls and Teutons—would that *their* differences could be as peacefully discussed! Some useless talk there doubtless will be, but we trust much earnest search after truth for truth's sake, and much evidence of real scientific work accomplished during the past year. We shall endeavour to give a report of the proceedings in the various sections, and to this end we must ask, and confidently expect to receive, the hearty co-operation of the Association itself, not only of the officers of sections, but of every individual member who takes part in the meeting.

THE result of the experiments at Woolwich in reference to war balloons is that it has been found that a height of 100 fathoms at horizontal distance of 600 fathoms from the enemy would enable observers to secure the widest expanse of view. It is ascertained that captive balloons attain stability. The balloon having taken a stationary position, eight cameras and lenses spread round the country at equal distances enable the country to be photographed. The inclination and length of the cord to keep the balloon in the same stratum of air was found to be easily calculable. By the new system of military telegraphy for field service telegraph wires can be carried through the air from *terra firma* to a balloon, and the wire can be paid out as fast as the balloon sails; and two or more balloons can be kept in communication with each other, so that telegraphic operations can be made from the balloon to head-quarters and thence to the base of operations. It is believed that war balloons will be manufactured at the Royal Arsenal, and that officers of Royal Engineers will be trained in their use.

THE *New York Technologist* for September describes a pocket rifle invented by Mr. Stevens. It is said to be capable of doing very accurate work, and the price is moderate. The method of loading is so simple that the weapon can be fired five times in a minute. By simply touching a spring the muzzle of the barrel drops down, leaving the breech exposed; a cartridge is then inserted, the barrel returned to its place, and the weapon is ready for use. For all light game they cannot be surpassed, and their penetration is considerable, driving the ball through three one-inch boards.

MR. W. G. SMITH has recently called attention to the great amount of heat generated by fungi, confirming Dutrochet's observation that it is greater in the case of *Boletus æneus* than of any other plant except the Arum. Mr. Smith believes that it is common to all *Boleti*, especially after decomposition has set in. Three large and beautiful specimens of *Boletus colopus* packed in a box were found by Mr. Smith to raise the temperature of the air from 70° to 75° Fahr., the heat evolved being apparent to the hand.

WE gain some notion, says the *Gardener's Chronicle*, of what a siege means when we learn from Paris that the veteran director of the Jardin des Plantes, the well-known chemist, Chevreul—aptly called, from his researches into the nature of fatty substances, "the king of the fatty acids,"—has placed himself at the head of a brigade composed of *employés* of the Museum, and betaken himself to the fortifications. M. Delaunay, the Director of the Observatory, and M. Milne-Edwards, have marched to the scene of action at the head of nearly the whole of the officers and servants of the Academy of Sciences and the Museum. What should we think here in London if the chief librarian of the British Museum, with Professor Owen and Mr. J. J. Bennett as his *aides*, took the field with their subordinates and occupied Shooter's Hill, or if the director at Kew, with his staff, took upon themselves the defence of Richmond Hill? And yet this is what it has come to in Paris.

A SANITARY council for Bohemia has been formed, consisting of Professors Jaksch, Halla, Kaulich, Town-District Surgeon Dr. Grosse of Prague, and District-Surgeon Dr. Hosier of Karolinenthal.

MR. G. FARRER RODWELL has succeeded Dr. Debus as Lecturer on Natural Science at Clifton College.

THE *Yale College Courant* states that Prof. Silliman has resigned his position as instructor of chemistry in that institution.

WE are authorised to state that there is no foundation for the statement which appeared recently in one of the daily papers, that the London Institution in Finsbury Circus is likely to be removed in order to make room for a railway station.

THE preparations for the Argentine National Exhibition at the city of Cordova are in active progress. The building is approaching completion, and the tramway to connect it with the railway station has been commenced. The tramways in the city have experienced great success, and caused an amount of building speculation in the suburbs beyond the available supply of labour. Several important railway proposals, involving prospective loans of magnitude, are under discussion in Congress.

CORNELL UNIVERSITY (for a notice of which see NATURE, August 11) has recently acquired a fine and valuable collection of fossils from one of its many admirers in England. The educational value of such a collection in America, as illustrating the English geological works, is very great, and Cornell University may well be proud that it has so speedily received such a valuable gift.

IT is proposed to found a Museum of History, Antiquities, and Arts, in the Central Park, New York. This movement is under the auspices of the New York Historical Society, and a grant of land has been given by the Government for that purpose. The plan embraces the erection of suitable buildings where historical relics and treasures of local and national interest may be deposited for preservation and exhibition.

SIG. LARANJA E OLIVEIRA forwarded to a recent meeting of the French Academy of Sciences an account of a very remarkable electric shock experienced at Porto-Alegre, Brazil, on the 9th of June. A thunderstorm was progressing at the time, and large drops of rain falling, with a light south breeze. At 100 metres from his own house, as a flash of lightning without thunder appeared over his head, one of Sig. Oliveira's servants felt a remarkable tingling through the whole of his body, ascending upwards from the soles of his feet, succeeded by a violent trembling; his hair stood on end, so as almost to throw off his hat. At the same time, at the distance of about two metres, white smoke ascended from the ground, accompanied by small consecutive flashes of lightning, but the whole lasting only an instant. A door key, which he had in his pocket, attracted for two days afterwards a needle suspended by a thread.

MM. RABUTEAU and Peyre have been experimenting with the root of a plant in use at the Gaboon as an ordeal poison, and locally known as m'boundou or icaja. It will be remembered that it was from this source that the highly valuable Calabar bean was obtained and utilised in medicine. The authors state that, even in very dilute decoctions, it is very bitter, and appears to contain one or more alkaloids, since the aqueous decoction is largely precipitated by iodide of potassium, and also by phospho-molybdic acid. The poisonous effects of this substance bear some similarity to the effects of brucia; but the authors state that, under certain conditions this poison does not hurt men. Some of the lower animals are readily killed by it; a dose of three milligrammes of the alcoholic extract placed under the skin of a frog kills it; and rabbits and dogs are killed by doses of from 15 to 25 centigrammes of the same extract introduced into the stomach.

M. ROUSILLE, Professor of Chemistry in the Agricultural School of Saulsaie, describes a remarkable phenomenon witnessed by him on Mons Pilatus, at sunrise on July 19th. The air was foggy, the temperature 10·5° C., the barometer 647 mm., the altitude above the sea being 1288 metres. Whitish clouds appeared to form a crest to the mountain, the wind was light from the east, the sun at first very pale, obscured by a light cloud; but suddenly the sky appeared as if on fire, and at the same time the shadow of the mountain was projected on the horizon, at the same time that the image of the sun was reflected in the clouds to the west, surmounted by two rainbows, one above the other, and separated by a grayish band. The image of the sun was orange; the two rainbows which surmounted it consisted of two colours only, red and orange mingled together, and placed symmetrically in each of the rainbows with respect to the separating gray band. The upper bow was paler than the lower one. In proportion as the clouds approached the spectators, the image of the sun increased. At the same time that the rainbows became larger, some black lines became more and more clear, and were soon easily recognised as the shadows of the spectators. As the clouds advanced, the phenomenon disappeared three or four seconds before the spectators were enveloped in them; but the clouds again disappearing, the phenomenon was repeated three times successively in the space of eighteen minutes. The phenomenon appears to have been of the same nature as the well-known Spectre of the Brocken in the Hartz Mountains.

THE second part of Vol. I. of the Natural History Transactions of Northumberland and Durham has just been issued. The following are the most important papers contained in it:—A list of Freshwater Algæ collected in Northumberland and Durham (excluding Diatomaceæ and Desmidiæ) by Mr. G. S. Brady. On a new Labyrinthodont Amphibian from the Northumberland coal field; and on the occurrence in the same locality of *Anthracosaurus Russelli*, by Albany Hancock. The new Labyrinthodont, obtained from the Low Main seam of the Newsham colliery is a small species of Huxley's genus *Urocordylus*, and the name proposed is *U. reticulatus*, expressive of the reticulated structure of the surface of the cranial bones. On some curious fossil fungi from the Black Shale of the Northumberland coal field, by A. Hancock and T. Atthey (illustrated). The same authors contribute two papers (also illustrated) on the genera of fossil fishes *Climaxodus* and *Janassa*. Notes on the Entomostraca of Northumberland and Durham, by Mr. G. S. Brady (illustrated). The Meteorological and Climatological Reports for 1869, by the Rev. R. F. Wheeler, occupy a considerable portion of the volume, and are illustrated by tables and some very curious and interesting drawings of the solid residue left on evaporating rain-water, which fell in London, on the Clyde, at Manchester, and at Newcastle. The volume closes with the annual address to the members of the Tyneside Naturalists' Field Club, by the President, Rev. R. F. Wheeler.

DR. W. F. R. SURINGAR has contributed to the Annals of the Botanical Museum of Leyden a monograph of the Algæ of Japan, chiefly those collected by Von Siebold. The illustrations, both of the Diatomaceæ and the Sea-weeds, are of extreme beauty.

PROFESSOR HALFORD, of the University of Melbourne, in a paper read before the Medical Society of Victoria, has reviewed at length the history of twenty cases of snake-bite treated by his method of injecting liquor ammoniæ into the veins during the last eighteen months. These cases were all in the hands of different practitioners in the colony, who have each reported on them. Recovery followed in seventeen cases. In thirteen of these the practitioners in attendance expressly report that the patients were in a dying condition, and, in their belief, would soon have died, but for the employment of this remedy in the manner prescribed. The method employed was that introduced by Dr. Halford, and first brought to the knowledge of the profession here by him, in the pages of the *British Medical Journal*, through Mr. Paget; viz. by injecting dilute ammonia—say, at the least, thirty minims of the liquor ammoniæ B. P., specific gravity 959—into a superficial vein; the vein being first exposed, and its coats pierced with the nozzle of a hypodermic syringe. Dr. Dempster, Dr. Rae, Dr. Langford, Mr. Dallimore, and Dr. Meyler, each in his own words, and from the observation of separate cases, describe the curative effect as being immediate, and the recovery from collapse to be so rapid and startling as to be "almost magical." This method of treatment, of which such remarkable effects are detailed, has been sharply criticised; but Prof. Halford successfully vindicates the claim of the snakes to be considered highly venomous—almost as much so, he intimates, as some of his London critics. They included the tiger-snake, the brown and black snake of Australia, which are affirmed to be as deadly as the cobra and rattle-snake of India. Strong testimony to the efficacy of the treatment in saving life was borne by Australian practitioners who took part in the discussion, and vindicated Prof. Halford's claim to be considered as the discoverer of a means of rescuing many from an otherwise inevitable death.

A VALUABLE addition to scientific literature has been lately published in Boston, viz.: "Alaska and its Resources," by Mr. W. H. Dall, the director of the Scientific Corps of the late Western Union Telegraph Expedition. This work throws much light upon the flora and fauna of this little visited and comparatively unknown portion of America, lately acquired by the United States Government.

F. MULHAUSEN, civil engineer of Brunswick, has invented, according to the *British Medical Journal*, a new freezing and ventilating machine of remarkable ingenuity. The cold is produced by the mechanical expansion of atmospheric air. It produces, when in operation, any desired degree of cold, freezes water without the use of any chemical agents, and will effectually cool and ventilate any apartment or building, on whatever scale, large or small. In hospitals, especially in tropical climates, where the production of ice and the cooling of the air are often matters of great urgency, and always of great value, in theatres and workshops, and in our new Indian barracks, such a machine will be of infinite value. The London theatres can hardly afford to be without it. The labour of one man, with a small 5-horse motor power machine, will produce 100lb. of ice an hour, and cool 15,000 cubic feet of air from 30° to 50° below Reaumur (*sic* in *B. M. J.*) The production of pure ice, for the purpose of cooling our drinking-water and furnishing a cheap mode of replenishing our domestic refrigerating safes during the hot season, will be a great addition to the sum of comfort in London life. Cheap ice will be especially a great boon to the hospital and sick-room; nothing is so refreshing for the parched lips of the sick man. If it were not so costly as it now is, ice would be very largely used in all hospitals, and would be an infinite boon to the sick.

SCIENTIFIC INSTRUCTION AT THE IOWA STATE UNIVERSITY

THE *American Scientific Monthly* states that the faculty of the State University of Iowa have, with marked unanimity, resolved to recommend to the Board of Regents a new course of study for the first three years, in order to adapt the internal organisation of the institution to the spirit of the new organic law passed by the late General Assembly.

According to this plan, the students have four regular hours of work, lecture, or recitation each school day. This time is equally divided between the *letters* (language, history, &c.), and the *sciences* (mathematics, physical and natural science). The direction of these two classes of studies is to be respectively under the Faculty of Letters and the Faculty of Science, each to have complete control over one-half of the students' time.

Of the two hours spent in the different sciences, one hour is devoted to mathematics, the other, in the two first years to physical, in the third year to natural science. This order might, perhaps, with advantage to the student, be changed so as to have the second year in physical science succeed instead of precede the year in natural science; the student would thereby profit from his greater proficiency in mathematics.

After having completed this three-years' course—which is the same for all students in the sciences, but permits the student, under certain wholesome restrictions, to substitute the different languages one for the other—the student can intelligently decide whether he desires to graduate in the sciences, the letters, or in pedagogics. In the last case, he needs not to have studied any foreign language, but will have spent a great amount of time in the study of his vernacular and its literature; he may then enter the Normal Department, and, at the close of one year, receive the degree of B.D. (Bachelor of Didactics). If, however, he has studied foreign languages, especially Latin and German, he can, after two years of studies, selected according to his own pleasure from among the higher branches taught at the University, obtain either the degree of B.Ph. (Bachelor of Philosophy) or B.A. (Bachelor of Arts). To obtain the former, at least two-thirds of the studies selected by him must have been in the department of Science, while to obtain the latter degree the same fraction should have been studied in the department of Letters.

Facilities for part-graduate courses are also offered, and have, indeed, already been improved by several gentlemen in the past year. We abstain from giving forth details concerning this plan until the Board of Regents shall have taken action thereon. We believe, however, that this plan, in a judicious manner, combines the best features of the American College system with the German University system. It will equally prevent one-sidedness and class-drudgery; while securing familiarity with the *elements* of the principal branches of human culture, it will, at the same time, not only permit each mind to follow its own bend, but also give every individual a fair chance to ascertain correctly in what direction his mind leads him.

This new organisation is especially important to those interested in science. Hitherto, most students commenced the study of science after having spent several years in book studies, especially the dead languages. It seemed they had been imbued with the absurd notion that such studies form a proper preparation for the study of physical and natural science. Experience has, however, abundantly confirmed the common-sense view, that the exclusive pursuit of such literary branches, by permitting the faculties of observation and logical reasoning to slumber, does unfit students for the successful study of nature. For the sake of the student, we are glad that this system, at last, has been thoroughly abandoned. The students will hereafter commence the study of physical science immediately upon entering the University, and, in regular succession, become familiar with the elements and principles of the same. They will take up the natural sciences only after they have become familiar with the elements of physical science. Hereby the professors of physical and natural sciences will, at last, have an opportunity given to teach science in such a manner and at such a time as they deem necessary. We are glad to state that this great reform has the cordial support of each member of the faculty.

NEW CLASSIFICATION OF CLOUDS*

NO one is ignorant that the study of clouds is, from the point of view of our practical needs, one of the most important questions meteorology can present us. Indeed, there is no other meteorological manifestation can so fix the attention of the yeoman in the city, of the agriculturist in the country, of the tourist on the mountain's summit, of the soldier in war, of the sailor in continual strife with the disturbances of atmosphere and sea, and, in fine, of the savant in general.

We everywhere see these different social elements continually watching the diverse appearances which the clouds offer us, and casting upon them a look of interrogation, of disquietude, of desire, of a wish constantly renewed to grasp their forms, in order to predict good or bad weather, according to our social needs.

It is especially when the atmosphere threatens some perturbation, rain, storm, tempest, that the common people examine the character of the clouds. But how often, at every moment of the day, do they ask each other about the temperature, hot, cold, or wet, which sensibly exists, while just as often do they pass by unheedingly the clouds, which exert a no less direct or indirect action upon atmospheric variations, as well in the abnormal state as in the normal.

Moreover each country, according to its geographical position, topography, &c., has its own type of clouds. Here the *Cirrus* predominates, there the *Cumulus*, elsewhere such and such form of clouds which do not exist in other places. All these different appearances of clouds are everywhere intimately connected with some particular condition of climate, and these climatological conditions powerfully influence, in their turn, the health, agriculture, navigation, and the thousand other social concerns of humanity. We may say that the clouds are a great book of nature, constantly open for the perusal of all classes of society. Like a compass, the clouds show us at every instant the direction, the velocity, and the altitude of the superior currents which afterward determine the inferior winds at the surface of the earth. There is, therefore, a permanent weathercock as long as the sky contains a single cloud, however small it may be.

There is necessity, therefore, for undertaking a profound study of clouds in their diverse scientific and social applications; for making researches upon the *nature, form, quantity, direction, velocity*, and *azimuthal rotation* of clouds, corresponding to each stratum perfectly characterised by the origin, intimate constitution, and meteoric products of the vesicular vapours and congealed particles which constitute them. For, in the intimate nature of clouds there is a fundamental condition to be established, which results from the physical force acting immediately after gravitation, upon their constitution. This is the element of *heat*.

Despite this scientific interest, despite this practical need which each feels, and which is so universally acknowledged, despite all this, the study of clouds is yet unhappily in its infancy. It is rarely one sees "clouds" inscribed in the meteorological registers of observatories, and when they are, the registrar has wholly neglected to note their form, their quantity, their direction, their velocity, and their azimuthal rotation. Some say simply, "clouds;" others mention the form or the quantity, may be the direction, or exceptionally these three elements, but assuredly they neglect the velocity, and especially the azimuthal rotation which I have first signalised in clouds, and which is not yet understood. In fine, not a single register gives these five elements for one stratum of cloud, much less for each distinct stratum of those which very often appear superposed in the atmosphere.

* Contributed by Prof. Poey, of Havana, to the Semi-Annual Session of the National Academy of Sciences, held at Washington.

We now proceed to present the basis of a new classification, more in harmony with the actual data of the science, and which is the fruit of twenty years' assiduous study upon the clouds, in the Antilles, as well as in Mexico, the United States, and Europe. From the beginning (at that date) of my meteorological studies at Havana, a city situated in the tropics where the *ensemble* of atmospheric phenomena affects an extreme simplicity in consequence of their surprising regularity, which is effaced as we approach the higher latitudes, ever since, I say, I have more and more felt the necessity of reforming Howard's nomenclature. For a long time I was unable to understand the four cloud formations, which I had come to reject, namely: Stratus, Nimbus, Cumulo-Stratus (different forms of Cumulus), and Strato-Cumulus. It was only later that, having been able to consult Howard's original work, I perceived the errors into which Kaemtz and all the meteorologists had fallen. I have, then, to introduce into Howard's classification the essential modifications which the continued progress of meteorology now requires, in order that the nomenclature may be more in harmony with our new conquests. I acknowledge with pleasure that Howard's classification of clouds, which has ruled without a rival for more than half a century (A.D. 1802), was originally based upon a profound study, directed by great acuteness in the observation of facts. But unhappily this is too plainly stamped with the locality where Howard's studies were alone prosecuted. I am speaking of the gray and cloudy sky of Great Britain, whence are his *Strato-mist*, his imperfect distinction of the two strata *Cirrus* and *Cumulus*, or his *Nimbus* (the rain cloud), the difference which he has established between *Cumulus* and *Cumulo-stratus*, without counting many other details of description, which are faulty, in relation to Cirrus, Cirro-stratus, and Cirro-cumulus.

Here is now the vindication of my three new clouds. When certain clouds are spread out uniformly, cover the whole face of the heavens, take a gray or ash-colour, under which state rain may occur for hours and whole days, what name do we give to these clouds? They are not Howard's *Nimbus*, as we conceive them and as they are generally described. These clouds are neither stormy, nor have they electrical manifestations, there is only a fine and continuous rain. Under this stratum—for it is a true stratum—we see constantly other clouds more or less considerable, but always isolated, come to be lost in it and to increase its thickness. On the contrary, before this stratum begins to break up, and during this operation, we see these same formless fragments detach themselves and fly to other regions. This inferior stratum is not alone, for when its disruption has taken place we see through it another stratum of clouds, whiter and less dense, which is broken up in its turn, and ends by disappearing in an inverse order to that of the first inferior stratum. Have we a name for this variety of cloud, so common in time of rain from the inter-tropical regions to high latitudes, especially in winter, during falls of snow? Does Howard's term Nimbus and his description of it account for this sort of cloud? Certainly not. We name indifferently a *Nimbus*, the single storm cloud, as well as this inferior stratum, or yet the two united strata, and all this without electrical manifestations. This is what I call *Pallium*; that is to say, those strata of which the superior is formed of *Cirrus*, constitute the *Pallio-cirrus* and the inferior of *Cumulus* constitute the *Pallio-cumulus*. The fragments of clouds which differ entirely from the *Cumulus* or *Cumulo-stratus* are the *Fracto-cumulus*.

Hence we see the necessity of distinguishing these two strata by different names; Howard's unique name of Nimbus did not do this, while granting him the greatest descriptive exactitude, which he is far from possessing. This necessity results moreover from the fact that the stratum of *Cirrus* is formed many hours, and even many days, before that of *Cumulus*, especially in the equatorial regions, and in fine that the latter disappears first. Without this distinction we are obliged to call the first stratum *Cirrus* and the second *Cumulus*; but as under this state of strata the form and physical properties of Cirrus and Cumulus change completely, there results the confusion and errors daily committed.

As regards Howard's classification as a whole, while retaining the two types of *Cirrus* and *Cumulus*, with his two derivative clouds, *Cirro-stratus* and *Cirro-cumulus*, I reject entirely his *Stratus*, his *Nimbus*, and his *Cumulo-stratus*, together with the *Strato-cumulus* of Kaemtz, for the following reasons: *Stratus*, because it is not (according to Howard) a cloud properly so-called, but a *mist* or *hoar frost*, or yet by the effect of an optical illusion, a *Cirrus*, a *Cirro-stratus*, or a *Cirro-cumulus*, as seen in perspective at the horizon; *Nimbus*, for the reason that it is an inexact denomination which is moreover applied to an idea as vague as incorrect, from the moment that *Cumulus* is not truly rainy as far as it is found displayed, forming a stratum as dense in appearance and below a second superior stratum of *Cirrus*, equally rainy; *Cumulo-stratus*, because it differs in nothing from Cumulus, according to Howard's own definitions, the three fundamental characters of cloud type and of its derivations being common to these two forms, namely: their horizontal bases, their superior hemispherical basins, and the ascending aggregation of their aqueous particles; in fine, *Strato-cumulus* (Kaemtz's cloud of night), because this modification answers in no manner, no more than Howard's *Stratus* to *clouds of night*, and because, on the contrary, its other characteristics correspond to *Cumulo-stratus*.

On the other hand, I substitute for *Nimbus* the *Pallium*, which I sub-divide into *Pallio-cirrus* and *Pallio-cumulus*, according as its stratum is composed of *Cirrus* or *Cumulus*. This term has the triple advantage of embracing the character, the form, and the effect—that is to say, the *Cirrus* or *Cumulus* forming a rainy stratum. I introduce, in fine, the determination of a second transitorial form, which seems to me can be rigorously distinguished from the preceding in the double relation of cause and effect. This is the *Fracto-cumulus* fragments of clouds which are wandering about without determined form, before their transformation into *Cumulus* (or *Cumulo-stratus*), which are precipitated or detached from the inferior surface of the stratum of *Sallio-cumulus*, and which, in fine, are spread out in horizontal bands at the summit of the *Cumulus* on the approach of gusts of wind. These *Fracto-cumulus* differ from the Cumulus in this: they have neither the horizontal base nor the superior hemispherical basins while they are not very extended; but as soon as they become a little more increased we see at once forming at the centre of the fragment a space more dense and blackish than the rest, which gradually settles until it constitutes the horizontal base of the *Cumulus* (*Cumulo-stratus*), the upper part also becoming rounded by degrees. Thus the *Fracto-cumulus* is the infancy of the *Cumulus*, otherwise called *Cumulo-stratus*, the terms being synonymous.

This new classification is wholly based upon the *nature*, the *form*, the *quantity*, the *direction*, the *velocity*, and the *azimuthal rotation* of the clouds corresponding to each stratum fully characterised by the origin, intimate constitution and meteoric products of the vesicular vapours and congealed particles which constitute them. For, in the intimate nature of clouds there is one fundamental condition to be established depending upon the physical force which first acts upon their constitution; it is the element of *heat*. Clouds are therefore distinguished into *snow clouds* and *ice clouds*, of which the constituent particles are more or less congealed; then into *clouds of aqueous vapour*, of which the vesicles, empty or full, float in a medium above the freezing point.

Under this fundamental aspect there are but two types of clouds properly so called, the *Cirrus* and the *Cumulus*. To the *Cirrus* are attached three transitional forms: the *Cirro-stratus*, *Cirro-cumulus* and *Pallio-cirrus*; and to the *Cumulus*, two other transitional forms; the *Pallio-cumulus* and the *Fracto-cumulus*.

Here is a table of my new classification of clouds compared with that of Howard:

NEW NOMENCLATURE OF POEY

First type.	*Cirrus*	Ice clouds.
Derivative	Cirro-stratus	Ice clouds.
	Cirro-Cumulus...	Snow clouds.
	Pallio-cirrus......	Snow clouds.
Second type.	*Cumulus*............	Vesicular clouds of aqueous vapour.
Derivatives	Pallio-cumulus....	
	Fracto-cumulus...	

OLD NOMENCLATURE OF HOWARD.

First type.	*Cirrus*
Derivatives	Cirro-stratus.
	Cirro-cumulus.
Second type	*Cumulus.*
Derivatives—Cumulo-stratus.	
Third type	*Stratus,*
Derived from the three types—*Nimbus.*	

My nomenclature appears probably more in accordance with the nature of the clouds in this sense than the two types, *Cirrus* and *Cumulus*, are rigorously based upon the constitution of ice and snow-clouds of aqueous vapour. Where there is no proof of the existence of Howard's third type, being that, according to this savant, it is a mist which overspreads the earth at sunset, but which is raised in the morning at the first appearance of that luminary. As to number, my nomenclature offers the same determination of cloud forms, that is to say, seven—the two types and five derivatives.

The order in which the clouds are placed in my table correspond, at the same time, to the order of their appearance, from the highest region of the *Cirrus* down to those nearest the earth, where the *Fracto-cumulus* are produced, according as the vapour of water passes from the state of frozen particles to that of aqueous vesicles, or *vice versa*. However, the *Pallio-cumulus*, which serves as a transition between the two types and their derivatives, is found a little more elevated than the *Cumulus*.

I have thought it suitable to modify Forster's vulgar nomenclature by substituting other names more in harmony with the form and nature of the clouds. I give in continuation the old and new classification :

	Forster's Nomenclature	Poey's Nomenclature
Cirrus	Curl-cloud	Curl-cloud
Cirro-stratus	Wane-Cloud	Thread-cloud
Cirro-cumulus	Sonder-cloud	Curdled-cloud
Pallio-cirrus		Sheet-cloud
Cumulus	Stacken-cloud	Mount-cloud
Pallio-cumulus		Rain-cloud
Fracto-cumulus		Wind-cloud

With the exception of Cirrus, whose name, "Curl-cloud," approaches nearest the form of this species of cloud, all the determinations have been changed. The Pallio-cumulus replaces the Nimbus, also named "Rain-cloud."

I.—CIRRUS (HOWARD).

Cirrus, so named by Howard ("the cat's tail," of sailors), are composed of filaments, whose *ensemble* resembles sometimes a curled hair, a twisted tuft, plumage, the flowing tail of a horse, at other times they are disposed in long, straight bands parallel to each other, or divergent palmated, or like a fish bone or vertebral column, their greater axis being oriented according to the sailing of the cloud and the direction of the wind existing at that altitude, which is not slow in making itself felt on the earth. When they form two or more systems of straight parallel bands, by an effect of perspective, they appear to diverge from their point of departure at the horizon and to converge toward the point of the horizon diametrically opposite, as do the rays of the rising or setting sun.

The *Cirrus* have always a whiteness, sometimes brilliant, sometimes pearly dull. The earliest and the latest reflections of the solar rays upon the clouds colour them with a charming rose tint, more or less intense, according to their density. Their propagation is excessively slow, and their altitude is not less than 10,000 metres (more than six and a quarter miles). These clouds are the highest, slowest, most rarified, most variable in their forms, and the most extended. The appearance and disappearance of *Cirrus* proclaim simultaneously the end and the commencement of good weather. The barometer sinks and then rises, the *ensemble* of meteorological accompanying phenomena pursuing the same course. We quote from Howard :

"They are first indicated by a few threads pencilled, as it were, on the sky. These increase in length, and new ones are in the meantime added to them. Often the first formed threads serve as stems to support numerous branches, which in their turn give rise to others.

"The increase is sometimes perfectly indeterminate ; at others it has a very decided direction. Thus the first few threads being once formed, the remainder shall be propagated in one or more directions laterally, or obliquely upward or downward, the direction being often the same in a great number of clouds, visible at the same time ; for the oblique descending tufts appear to converge toward a point in the horizon, and the long straight streaks to meet in opposite points therein, which is the optical effect of parallel extension. The upward direction of the fibres or tufts of this cloud is found to be a decided indication of the decomposition of vapour preceding *rain* ; the downward as decidedly indicates *evaporation* and fair weather. In each case they point toward the place of the electricity which is evolved at the time.

"Their duration is uncertain, varying from a few minutes after the first appearance to an extent of many hours and even days. It is long when they appear alone and at great heights, and shorter when they are formed lower and in the vicinity of other clouds.

"This modification, although in appearance almost motionless, is intimately connected with the variable motions of the atmosphere. Considering that clouds of this kind have long been deemed a prognostic of wind, it is extraordinary that the nature of this connection should not have been more studied, as the knowledge of it might have been productive of useful results.

"In fair weather, with light, variable breezes, the sky is seldom quite clear of small groups of the oblique *Cirrus*, which frequently come on from the leeward, and the direction of their increase is to windward. Continued wet weather is attended with horizontal sheets of this cloud, which subside quickly and pass into the *Cirro-stratus*.

"Before *storms* they appear lower and darker, and usually in the quarter opposite to that from which the storm arises. Steady high winds are also preceded and attended by streaks running across the sky in the direction they blow in."

II.—CIRRO-STRATUS.

Thread-cloud.—Howard's *Cirro-stratus* is distinguished from the pure *Cirrus* by its filaments being smaller, more compact, more ramified, and, so to say, completely stratified. They are lower, more dense, for often the sun's rays pierce them with difficulty. Their whitish tint is clearer, and it also becomes rose colour in similar circumstances. Their motion is a little more rapid. When at the horizon, we only seeing the vertical projection, they take the appearance of a long and very narrow band. Howard says :

"This cloud appears to result from subsidence of the fibres of the *Cirrus* to a horizontal position, at the same time that they approach each other laterally. The form and relative position, when seen in the distance, frequently give the idea of shoals of fish. Yet in this, as in other instances, the *structure* must be attended to rather than the *form*, which varies much, presenting at times the appearance of parallel bars, or interwoven streaks like the grain of polished wood. It is thick in the middle, and extenuated towards the edge. The distinct appearance of a *Cirrus*, however, does not always precede the production of this and the last modification.

"The *Cirro-stratus* precedes *wind* and *rain*, the near or distant approach of which may sometimes be estimated from its greater or less abundance and permanence. It is almost always to be seen in the intervals of storms. Sometimes this and the *Cirro-cumulus* appear together in the sky, and even alternate with each other in the same cloud, when the different evolutions which ensue are a curious spectacle ; and a judgment may be formed of the weather likely to ensue by observing which modification prevails at last. The *Cirro-stratus* is the modification which most frequently exhibits the phenomena of the Solar and Lunar halo, and (as supposed from a few observations, the Parhelion and Paraselene also. Hence, the reason of the prognostic of foul weather—commonly drawn from the appearance of Halo. The frequent appearance of Halo in this cloud may be attributed to its possessing great extent, at such times, with little perpendicular depth, and the requisite continuity of substance.

"This modification is, on this account, more peculiarly worthy of investigation."

III.—CIRRO-CUMULUS.

Curdled Cloud.—It is sufficient that the *Cirro-stratus* sink a little, or that the temperature of the region they occupy be slightly elevated, in order that the frozen aiglets may be reduced to snow, and give birth in consequence to Howard's *Cirro-cumulus*. In the first place the axes of the *striæ* grow round ; then, by degrees, the entire stratification becomes so, until it forms little balls or corded cotton which we call "frizzled" clouds of "curled" sky (in French, *montonnes* or *pommele*,) when it is completely covered ; in Spanish, *cielo empedrado*. On the contrary, if the *Cirro-cumulus* are elevated a little, or if the temperature is lowered, they return to the superior type of *Cirro-stratus*. The little balls of snow are congealed and crystallised anew into aiglets.

The *Cirro-cumulus* are more dense and lower than the *Cirro-stratus*, from which they are derived, although generally the edges of the small agglomerations or of the entire mass of cloud is transformed into *Cirro-stratus*, whenever, by a greater elevation or a lower temperature, the congelation is more vigorous. Their motion is also more rapid, their colour slightly greyish, and they may, moreover, be tinged rose-colour, or, rather, become reddish.

The *Cirro-stratus*, but more especially the *Cirro-cumulus*, are remarkable by reason of a characteristic of the highest importance, from the point of view of the distribution of congealed aqueous vapour, and one which has escaped the sagacity of Howard and his successors. It consists in the most fantastical combinations, reproducing all the formations, hydrological, and physical, of our Continent and seas. Here a deep bay with promontories, capes, peninsulas, isthmuses, &c.; there, a river, brooks, lakes, &c.! further on, vast continents and open seas. The entire mass and the outlines of each of these accidents are besprinkled with *Cirro-cumulus*, sometimes edged with *Cirro-stratus*, of which the volumes of little balls are seen diminishing and vanishing from centre to circumference, while at the side, in the empty spaces, we perceive the purest azure of the heavens. Should it be a lake, the water will be represented by the blue sky, and *terra firma* by the *Cirro-cumulus* which surrounds it. By carefully studying all these transformations we remark in them the greatest analogy with the phenomena of the precipitation and congelation of dew upon solid bodies. There is, therefore, at this altitude, in the same stratum, and one after the other, so to say, some portions of the atmosphere enjoying different degrees of density and of temperature, in order that the congelation of aqueous vapour may take place in so variable a manner.

The influence of *Cirro-cumulus* upon the lowering of the temperature at the surface of the earth is so considerable that the human body feels it at once. A curdled sky at the new moon of a calm night in the tropics is a sky relatively glacial for these latitudes.

This effect may be due to their greater proximity and to the considerable quantity of balls of snow which constitute this type of cloud. The *Cirrus* being found much elevated and the *Cirro-stratus* much less abundant, although both are formed of glacial aiglets, have not the same influence upon the terrestrial temperature. Howard says:

"The *Cirro-cumulus* is formed from a *Cirrus*, or from a number of small separate *Cirrus*, by the fibres collapsing, as it were, and passing into small, roundish masses, in which the texture of the *Cirrus* is no longer discernible; although they still retain somewhat of their relative arrangement. This change takes place throughout the whole mass at once, or progressively from one extremity to the other. In either case the same effect is produced on a number of adjacent *Cirrus* at the same time and in the same order. It appears in some instances to be accelerated by the approach of other clouds.

"This modification forms a very beautiful sky, sometimes exhibiting numerous distinct beds of these small connected clouds, floating at different altitudes.

"The *Cirro-cumulus* is frequently seen in summer, and is attendant on warm and dry weather. It is also occasionally and more sparingly seen in the intervals of showers and in winter. It may either evaporate or pass to the *Cirrus* or *Cirro-stratus*."

Under the generic name of *Pallium*, I have classed two forms of clouds, which present the appearance of a mantle or veil of considerable extent, of very compact texture, well defined at the edges, of an excessively slow march, and embracing, moreover, the visible vault of the sky. According as the *Pallium* is formed of *Cirrus* or of *Cumulus* it is distinguished into *Pallio-cirrus* and *Pallio-cumulus*. The appearance of these clouds signalises bad weather, and their disappearance good weather.

The stratum of *Pallio-cirrus* is first formed, and some hours or some days afterwards that of *Pallio-cumulus* is formed under it. These two strata remain in view at a certain distance from each other, and by their reciprocal action and reaction produce storms and the heavier rains, accompanied with considerable electric discharges. They are electrified, but with contrary signs; the superior stratum of *Cirrus* is negative, and the inferior one of *Cumulus* is positive, the same as the rain which it disengages; while the electricity of the air, at the surface of the earth, is negative. But when these two strata attract each other a discharge is produced; and the inferior stratum continues to pour out the surplus water it contained without giving any sign of electricity, no more than the air in contact with the earth. This state continues until the inferior stratum opens up, the superior afterward, they then disappear, the one after the other. Fine weather then returns. The *Pallium* chiefly predominate during the rainy season, in inter-tropical regions, and in the higher latitudes during winter, at the time of falls of snow. A part of the *Pallio-cumulus*, which has not been reduced, or which has not been scattered to other regions, gathers at the horizon and is transferred into the *Cumulus*. As to the *Pallio-cirrus*, they disappear entirely if fine weather is maintained.

THE ANCIENT LAKES OF WESTERN AMERICA, THEIR DEPOSITS AND DRAINAGE *

THE wonderful collections of fossil plants and animals, brought by Dr. Hayden from the country bordering the Upper Missouri, are from deposits made in extensive fresh-water lakes which at one time occupied much of the region lying immediately east of the Rocky Mountains. The water of these lakes was first salt or brackish, as the remains of oysters and similar estuary forms show. By continental elevation the whole country west of the Mississippi was raised out of the cretaceous sea, and these estuaries became lakes inclosed by raised dry land. The knowledge of this country from the Mississippi to the Pacific Ocean has been accumulated by various explorers besides the writer, as Dr. Hayden, Mr. George Gibbs, Professors W. P. Blake and Thomas Antisell, and Prof. J. D. Whitney and the State Geological Survey of California, and Baron Richtofen, the lamented Rémond, Drs. Shiel, Wislizenus, and others. Besides Mr. Clarence King has explored a large tract of this country, but his very important contributions have not, as yet, been made public. The general character of the topography of the region west of the Mississippi has been given by these great lines of elevation traversing the country from north to south. There are the Rocky Mountains, the Sierra Nevada, and the Coast Ranges. The last is the most modern, and is composed, for the most part, of Miocene Tertiary rocks. Parallel with this lies a narrow trough, in California traversed by the Sacramento and San Joachin Rivers, encroached on by the mountains at places, but still in Oregon and Washington, traversed by the Willamette and Cowletz Rivers. These two sections are drained through the Golden Gate and Columbia. The mountain barriers formerly caused the valleys to consist of great inland lakes, which are now only represented by the chain of small pieces of water still to be seen in that region of country. East of the Sierra Nevada and between it and the Rocky Mountains is another still larger basin. For a thousand miles it has no openings to the westward, which are less than five thousand feet above the sea, but at three points there are gateways, which may be passed, but little above the sea level. These are the *canons* of the Sacramento (Pit River), the Klamath, and the Columbia. These have been cut through by the drainage of the interior of the continent. The former beds of the lakes have thus been left dry and waste—the only real desert on the North American continent. The Sierra Nevada is older than the Coast Ranges, and projected above the ocean, though not to its present altitude, previous to the Tertiary and even Cretaceous ages. This we learn from the fact that strata belonging to these formations cover its base. The mass of the Sierra Nevada is granitic rocks and metamorphic slates, proved by the California Survey to be triassic and jurassic. These slates are traversed by the gold-bearing quartz. East of the Sierra Nevada is a high and broad plateau five hundred miles wide, and from four to eight hundred feet in altitude, and reaches south into Mexico. This mountain belt was once the margin of the Pacific Ocean. Its crest is crowned by volcanic cones like gigantic towers of a fortification. The central portion of this plateau was called by Fremont "the great basin," as it forms a hydrographic basin drained by the Columbia and Colorado. The former makes its way to the ocean through a gorge in the Cascade Mountains, whilst the latter escapes to the south through a series of *canons*, of which the most important is nearly a thousand miles in length, and from three to six thousand feet deep. In vol. vi. of the Pacific Railroad Reports the country of the Columbia is described and the reasons for concluding that it had cut its way through the Cascade Mountains, and similar facts were observed in the district drained by the Klamath and Pit Rivers. Certain peculiarities are to be seen in the country between the Sierra Nevada and Rocky Mountains. In the northern and middle portions of the great table lands the surface is somewhat thickly set by short and isolated mountain ranges, sometimes called "the lost mountains." These rise like islands above the level of the plain, and are generally com-

* Contributed by Professor J. S. Newberry to the Proceedings of the New York Lyceum of Natural History.

posed of volcanic or metamorphic rocks. The spaces between them are level desert surfaces. Towards the north and west, on the tributaries of the Columbia, Klamath, or Pit Rivers, the plateau is cut by these streams, and the deposit can be examined. The rocks are nearly horizontal, some are coarse volcanic ash, with fragments of pumice and scoriæ. Others denominated "concrete" resemble the old Roman cement. Many are quite white, and are generally known as "chalk-beds," though they contain no lime. The late Prof. J. W. Bailey determined these to consist of the remains of fresh-water species of Diatomaceæ. The stratification and horizontality of these beds show them to have been thrown down from great bodies of water which once covered the greater part of these level plains. From south-western Idaho and eastern Oregon have lately been brought large collections of animal and vegetable fossils, of great variety and interest. The plants were mostly collected by the Rev. Thomas Congdon, of the Dalles, Oregon, at great risk of life and while exposed to great hardships, on the flanks of the Blue Mountains. They are apparently Miocene, forming twenty or thirty species, nearly all new, and which represent a forest growth as varied and luxurious as can be found on any portion of the continent. The animal remains came mostly from the banks of Castle Creek in the Owyhee district, Idaho. These were sent by Mr. J. W. Adams of Ruby City. They consist of bones of the mastodon, rhinoceros, horse, elk, and other large mammals of which the species are probably in some cases new, in others identical with those obtained from the deposits examined by Dr. Hayden. There are also bones of birds and great numbers of the bones and teeth of fish. These last are cyprinoids applied to *Mylopharodon*, *Milocheilus*, &c., some three feet and more in length. Also many fresh-water shells, as *Unio*, *Corbicula*, *Melania*, and *Planorbis*. These illustrate the inhabitants of the extinct lakes, which were of a much larger size and greater depth than the great fresh-water lakes which now lie upon our northern frontier. Between these were areas covered with a luxuriant and beautiful vegetation and inhabited by herds of elephants and other great mammals. In the streams were numbers of fish and mollusks of species now extinct. Gradually these lakes evaporated and at last became dry. In the Klamath lakes and Suisun Bay we have their remanents, whilst on the Columbia the drainage-streams have cut *canons* two thousand feet deep. At times the peace and quiet of this country were disturbed by violent volcanic eruptions from the peaks of the Sierra Nevada, which ejected showers of ashes covering the land and filling the lakes, as is seen in the strata now existing, some ten and twenty feet thick. Sometimes lava was thrown out and covered hundreds of miles of surface, and is now seen as solid basalt. Then quiet reigned, and new fresh-water deposits were formed, only to be succeeded by other volcanic disturbances. Some parts of this plateau have not been drained, and the remains of the ancient lakes now exist as Salt Lake, Pyramid Lake, and others. These are gradually diminishing, as is to be seen by indications all around their borders, where we can trace ancient shore lines. The alkali plains and salt flats mark the places of dried-up lakes; all of these still existing being excessively salt. This is the state of things at the north. In the south, the great Colorado plateau is without mountain barriers or local basins, and there are few traces of extinct lakes. This arid district was once a beautiful and fertile plain, drained by the Colorado, which, on the western margin poured over a precipice five thousand feet or more high, into the Gulf of California, which then reached several thousand miles farther north than it does now. In time the river cut its way farther back through the subjacent rocks, and at last formed that remarkable gorge, nearly a thousand miles long and three to six thousand feet deep. As the channel deepened, the country around became dryer, until it was the arid plain we find it now. Almost no rain falls on this plain, therefore the walls of the *canon* remain sharp-cut precipices unaffected by moisture. On the east of the Rocky Mountains is the great plateau country of the plains, which differs from the country to the west, by not being bordered on its east by a mountain chain, but sloping gradually to the Mississippi. Its surface was also covered by great fresh water lakes, larger, if not more numerous, than those now existing on our northern boundary. From the northern portion of this plateau Dr. Hayden has brought his specimens, and he has there obtained a harvest of scientific truth which will form for him an enduring and enviable monument. He has studied the deposits which accumulated in these lakes, and they are very rich in specimens of both animal and vegetable life. The vertebrate remains have been studied by Dr. Leidy, who has published his investigations in the splendid monograph so well known, and which forms a contribution to palæontology, not second in value or interest to that made by Cuvier, by his illustrations of the fossils from the Paris basin, nor to that of Falconer and Courtly, descriptive of the Sewalik hills of India. The first instalment of the plants have been described by Dr. Newberry, in the report of Colonel W. F. Reynolds, U.S.A., not yet published. The descriptions are published in the Annals of the Lyceum of Natural History of New York, vol. 9, 1868. The general conclusions from these examinations have greatly enlarged the flora of the Tertiary and Cretaceous periods. Since then largely additional material has been collected by Dr. Hayden, Mr. Congdon and Dr. Le Comte, and Dr. Newberry; and in Alaska by Mr. W. H. Dall and Captain Howard, and by others in Greenland. The flora and fauna of the lake deposits on both sides of the Rocky Mountains apparently belong to one and the same geological age, and tell the same story as to topography, climatic conditions, and development of animal and vegetable life. There is a striking difference in one particular between the deposits east and west. In Oregon, Idaho, and Nevada, volcanic material has accumulated in the lake basins to a much greater extent than on the east of the mountains. The deposits of the Upper Missouri regions are shales, marls, and earthy limestones, with immense quantities of lignite and almost no traces of volcanic material. The animals and vegetables of the Tertiary here were in much greater number than now. This existed long enough for thousands of feet to accumulate in the lake basins, and sometimes these deposits are found turned up on edge on the flanks of the mountains, showing that this chain, although existing in embryo from the earliest palæozoic ages, has been subjected to great modifications. The collections made by Dr. Hayden at various points differ among themselves. In every bed are new species, and between some deposits there are no connecting links. In the beginning of the cretaceous the land surface and climate of this continent were similar to the present period, the trees for the most part belonged to the same genera. Then most of the region west of the Mississippi sunk beneath the ocean, and the cretaceous deposits were made containing more tropical species. There were islands in the western sea, and the Gulf Stream had a course north and west from the Gulf of Mexico to the Arctic Sea. In the earlier Tertiary ages the continent here emerged from the ocean and approached the previous and present conditions indicated by the flora. In this category are to be placed the Green River Tertiary beds, those of Mississippi studied by Lesquereux, and those of Brandon, Vermont. In the Miocene the continental surface was broader, the western lakes were fresh, and the vegetation very much like that of the present day. A few palms then grew as far north as the Yellow Stone River, and a flora flourished in Alaska and Greenland as varied and as luxuriant as now grows along the fortieth parallel. At this time land connected Europe, this continent, and China, as the flora in this region was essentially the same, a large number of plants being common to the three continents. The mammals were peculiar; over our western plains rolled herds of great quadrupeds rivalling in number and variety those of southern Africa at the present time. This state of things continued during the Pliocene age and up to the ice period. In the middle Tertiary the climates of Alaska and Greenland were those of New York and St. Louis at present. Then came the Glacial epoch, and the climate of Greenland of the present time is brought down to New York, and all the northern portion of the continent is wrapped in ice. This change of climate was gradual, but the animals and vegetables were driven southward until the glaciers reached the thirty-eighth or fortieth parallel, when a temperate climate prevailed in Mexico, and only on the southern border would the temperature be what it had previously been on our northern border. Thus nearly all the animals were exterminated or forced into very narrow limits in southern Mexico. Plants bore their expatriation better, and as a consequence we find the present flora of our continent much more like that of the Miocene than is our fauna, though most of the forest-trees have become extinct. Of these the Glyptostrobus is an example, which grew all over our continent and northern Europe. In the glacial period it was exterminated except in China, where it now grows. So when we compare the present flora of China and Japan with that of the eastern half of our continent, we find the strongest proofs of their relationship; many species are identical, while others are but slightly changed. Some of the great mammals of the pre-glacial period bade defiance to these changes, as the mastodon and elephant, both of which could endure great changes of climate, and the mammoth, we know, was defended from cold by a thick coat of hair and wool. We find its remains embedded in peat-bogs and marshes, where they were mired and suffocated, and it is even claimed that here, as in Europe, it was contemporaneous with man.

SCIENTIFIC SERIALS

Annales de Chimie et de Physique, July, 1870.—This number contains a paper, by M. A. Müntz, on the Composition of Skin, the modifications it undergoes during the process of tanning, and on the decomposition of tannin in the tan-pits. The experiments detailed in this paper were undertaken by the author to determine, from a theoretical point of view, the changes taking place in the conversion of hides into leather. A piece of ox-hide which was being converted into leather for the soles of boots was selected as the most appropriate for the investigation, and the operations were commenced after cleansing and depilation. In these processes no chemical change would take place, except perhaps in cases in which lime is employed in the depilation, a small quantity of which is deposited in the skin, but afterwards removed by a solution of glucose. The following process of *swelling* consists in steeping the hides in an acid liquid for a time varying from three weeks to two or three months. This acid liquid contains acetic and lactic acids and a small quantity of tannin: its effect is to distend the pores of the epidermis and thus facilitate the subsequent process of tanning. During the swelling so much water and other substances are absorbed that the hide increases in weight to such an extent that it is now as heavy as it was before the cleaning and depilation, the addition of dry matter amounting to nearly 19 per cent.; a small quantity of mineral matter is lost, the augmentation being due to the addition of carbon, hydrogen, and oxygen. After eleven months in the tan-pit an increase of nearly 83 per cent. was observed; a small diminution in the quantity of nitrogen took place, while the mineral constituents and carbon, hydrogen, and oxygen were augmented. The organic materials added had exactly the same composition as those absorbed by the hide during swelling, but they differ much from the composition of tannin. The author believes that the loss of nitrogen during the tanning process is due to a decomposition of part of the leather, for ammonium salts were found in the liquor from the tan-pits. The structure of leather is also very different from that of skin; while the latter is fibrous the former is spongy; skin will absorb three or four times its weight of water, swelling considerably, but leather scarcely absorbs one and a half times its weight, and without increase of volume; 100 parts of skin by treatment with boiling water leave 3·35 of insoluble matter, the rest being converted into gelatine; the residue from leather under the same circumstances is about 48 per cent. The compound obtained by the action of tannic acid on gelatine differs very much in composition and properties from leather. A description is given of a process for the estimation of tannic acid; and M. Müntz announces the observation that the residue, after boiling skins with water, contains a substance dissolved by Schweitzer's cupro-ammoniacal reagent, and thus resembling cellulose, but containing about 15 per cent. of nitrogen. The author next gives an account of the composition of the mineral substances present in skin and in leather, and points out the changes produced during the tanning. He concludes that the tannic acid is partially converted into more oxidised compounds, as gallic acid, glucose, lactic, acetic, formic, carbonic acids, and most probably propionic acid, the remaining less oxidised residue converting the skins into leather. The experiments detailed in this paper were carried out in the laboratory of M. Bossingauult.—M. L. Henry contributes a paper on glyceric tribromhydrine, the object of which is to show that the compound obtained by Berthelot by the action of phosphoric bromide on dibromhydrine or epibromhydrine, and described by him as tribromhydrine, must have been some other compound. The author points out that the saturated compounds of the triatomic radical $(C_3H_5)'''$ obtained either from the allylic group or from glycerine, are always identical, with the single exception of Berthelot's tribromhydrine, which boils at 180°, while the tribromide of allyl obtained by Wurtz by the action of bromine on allylic iodide, boils at 217—218°. He also shows that the analysis given by Berthelot is not as concordant with the theoretical numbers as might be expected, and that the physical properties described by him do not correspond with those that might be looked for in a compound of the composition $C_3H_5Br_3$. Finally, he announces that by the action of phosphoric bromide on pure dibromhydrine, he has obtained the true tribromhydrine, which, in all its physical and chemical properties, is identical with the tribromide of allyl of Wurtz.—The next paper is by M. P. Schützenberger, on a new acid of sulphur. Schönbein observed that when a solution of sulphurous acid is placed in contact with zinc the liquid becomes yellow, and acquires the property of decolorising indigo and litmus, but after a short time sulphur is deposited and the reducing action of the liquid disappears. M. Schützenberger has employed a concentrated solution of sodic bisulphite in the place of the sulphurous acid, and has succeeded in preparing the salt of a new acid. A flask of about half a litre capacity was filled with zinc shavings and a strong solution of sodic bisulphite poured in. The vessel was then closed and placed in cold water; after half an hour the odour of sulphurous acid had disappeared, when the liquid was poured into a flask containing $1\frac{1}{2}$ litres of alcohol and the mixture agitated. A crystalline deposit of the double sulphite of sodium and sodium was produced, and from this the clear liquid was rapidly decanted into bottles, which were filled entirely, closed, and allowed to cool. After a short time a mass of fine colourless needles was deposited, consisting of *sodic hydrosulphite* $\left.\begin{matrix}(SO\,H)'\\ Na\end{matrix}\right\}O + H_2O$. It is rapidly oxidised by exposure to the air into sodic bisulphite, and the solution when placed on filter paper is so rapidly oxidised that sufficient heat is evolved to cause steam to be given off. On adding sulphuric or oxalic acid to the solution an orange red colour is produced, but the liquid is rapidly decolorised with deposition of sulphur. Sodic hydrosulphite may also be obtained by nascent hydrogen evolved by electrolysis. The production of this compound is represented as follows:—

$$\underset{\text{Sodic bisulphite}}{\left.\begin{matrix}(SO)''\\ Na\,H\end{matrix}\right\}O_2} + H_2 = \underset{\text{Sodic hydrosulphite}}{\left.\begin{matrix}(SO\,H)'\\ Na\end{matrix}\right\}O} + H_2O.$$

The oxidation of the hydrosulphite thus:—

$$\left.\begin{matrix}(SO\,H)'\\ Na\end{matrix}\right\}O + O = \left.\begin{matrix}(SO)''\\ Na\,H\end{matrix}\right\}O_2.$$

The decomposition of the acid:—

$$2\left.\begin{matrix}(SO\,H)'\\ H\end{matrix}\right\}O = 2\,H_2O + S + SO_2.$$

The solution of the sodium salt is gradually transformed, out of contact with air, into sodic hyposulphite and water:—

$$2\underset{\text{Sodic hydrosulphite}}{\left.\begin{matrix}(SO\,H)'\\ Na\end{matrix}\right\}O} = H_2O + \underset{\text{Sodic hyposulphite}}{S_2O_3Na_2}.$$

By means of an alkaline solution of sodic hydrosulphite indigo is rapidly converted into white indigo; acetone produces isopropylic alcohol; and oil of bitter almonds is partially transformed into benzylic alcohol. This substance promises to be a useful reducing agent for many operations.

In the *Revue des Cours Scientifiques* for Aug. 27 is a translation of Prof. Hofmann's address in honour of the late Prof. Graham, delivered before the German Chemical Society, which occupies the greater part of the paper. Then follows a report of the sittings of the Anthropological Society of Paris on April 21 and May 5, containing a report on the Ethnology of Lower Brittany, which led to an animated discussion on the origin of species and the theories of transformation and natural selection. A translation is also given of Prof. Sterry Hunt's paper, read before our Royal Society, on the probable seat of volcanic action. In the number for September 3 is a report from M. Giraud-Teulon on the causes of myopy, its relative frequency, and its influence in military efficiency. M. Marey continues his interesting and valuable paper on the flight of birds, which is here discussed from a mechanical point of view. We have the reports of the sittings of the Anthropological Society on May 19, June 2 and 16, and July 7 and 21; the subjects of discussion being the brain of man and of the primates, pathological osteology of the newly-born, acclimatisation of Europeans in Africa, and the conclusion of the discussion on transformation. The *variétés* comprises a paper by Prof. Nélaton, of the Faculty of Medicine of Paris, on wounds produced by fire-arms.

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Aug. 29.—A single paper only was read at this meeting by M. J. Boussinesq, a theoretical essay on the laws, found by experiment by M. Bazin, for the uniform flow of water in open channels. The formulæ deduced by the writer from theoretical considerations he found to agree with those indicated by M. Bazin from experiment, and also not to differ materially from those which M. Darcy has given to represent the rapidity in circular pipes full of liquid, for the axis of the pipe or at one-third or two-thirds of the radius.

NEW YORK

Lyceum of Natural History, May 9.—The President in the chair. Professor Charles A. Seely "On the Constitution of Ammonium-amalgam."—Dr. Isidor Walz's "Notes on the Extinction and Reducing Power of Mercury." "At the last meeting of the Chemical Section of the Lyceum, I drew attention to the conversion of liquid zinc-amalgam to a gray powder, when shaken with a solution of potassium bichromate. Subsequently I became convinced that this phenomenon was solely due to the extinction of the mercury, and have made a number of experiments regarding the phenomenon of which I present the following results. It is hardly necessary to state that the mercury used was absolutely pure. It is very difficult, but essential, to use chemically pure mercury, as even a very small trace of a foreign metal is often sufficient to influence the results materially. The experiments were made in ordinary test-tubes, in which the materials were shaken a length of time varying from a few seconds to ten minutes. I believe that we ought to distinguish between two methods or kinds of extinction, namely the mechanical and the chemical. The former is effected by a very large number of solutions of neutral salts, which exert no chemical action on mercury, and even by pure water, if shaken long enough. The extinction of the mercury in this case is produced simply by the interposition of fine films of the liquid between the globules, into which the mercury is separated by the mechanical agitation, and which are thus prevented from running together again. By mechanical extinction mercury is converted into what appears to be a fine powder, which, however, never loses its white colour and metallic appearance, and under the lens its globular structure is clearly seen. Quite interesting in many cases are the reactions which accompany the chemical extinction of mercury, which takes place when the metal is shaken with a solution of a salt, by which it is chemically affected. In these cases the newly-formed mercury compounds act in the same way as the films of liquid in the former instance, preventing the separate globules from reuniting. A finer division of the metal is obtained in less time than by the mechanical method, and the resulting metallic powder is generally of a dull gray leaden colour. When a solution of potassium bichromate is poured upon mercury, the convexity of the surface is at once destroyed; presently the surface is tarnished and begins to look wrinkled, while at the same time a greenish-black powder commences to deposit itself. This greenish-black powder is a mixture of chromic and mercurous oxide; it is formed abundantly when the two liquids are agitated more or less strongly; the mercury is at the same time completely extinguished, and at the end of the reaction neutral potassium chromate alone remains in solution, which is not acted upon by mercury. Ferric chloride extinguishes mercury; ferrous and mercurous chlorides are formed. Potassium permanganate also acts upon the metal; manganic and mercurous oxides are deposited, while potassic hydrate remains in solution. Mercury shaken with Fehling's solution is simply extinguished mechanically, when all the reagents used are pure; but when a very small quantity of zinc-amalgam is added, Cu_4O is reduced from the solution. A solution of potassium ferricyanide does not affect the fluidity of mercury; but when the two are shaken together a green powder is formed in large quantities, which, if allowed to stand, changes to a dark, and later still to a light blue colour. Potassium ferrocyanide appears to be formed at the same time. I am still engaged in studying this interesting reaction, and will endeavour to determine if this blue powder is Prussian blue or not. Sodium hyposulphite, also, does not affect the mercury physically; on agitation, however, a heavy black powder, mercuric sulphide, is formed. Its amount increases with the lapse of time, and in one of my test-tubes, which has hardly been disturbed for weeks, the original black sulphide has assumed a yellowish red colour. I conclude from these observations that the reducing power of pure mercury is greater than is generally supposed, and I expect to be enabled to obtain some interesting results from an extension of these experiments. I have repeated some of Lowe's experiments, which he described at our last meeting, and can state that similar results are obtained by substituting palladium bichloride for the platinum salt. I cannot, however, yet coincide with him in considering his final product as hydrogenium-amalgam, as by every method by which it has yet been made it contains another metal besides mercury and hydrogen, namely, either platinum, palladium, gold, or silver, in no inconsiderable proportion."

May 16.—The president in the chair. Mr. Frederick Prime, jun., read a paper on the Metallurgy of Argentiferous Galenas, giving details of the several processes at present in use, with their various advantages and disadvantages. He entered into a detailed account of the methods put into practice in Freiberg, and showed how these could, and would, be used in the United States.

BOOKS RECEIVED

AMERICAN.—Alaska and its Resources: W. H. Dall (Boston: Lee and Shephard).—Proceedings of the American Philosophical Society, No. 82.—Transactions of the Chicago Academy of Sciences, 1869. Parts 1 and 2.—Report of the Smithsonian Institute for 1868.—Proceedings (vol. ii. pt. 1) and Bulletin (Nos. 1-12) of the Essex Institute.—The Indians of Cape Flattery: G. G. Swan (Smithsonian Institute).

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THURSDAY, SEPTEMBER 15, 1870

THE MUSEUMS AND SCIENTIFIC INSTITUTIONS OF LIVERPOOL

FOR the third time since the founding of the British Association for the Advancement of Science, its annual meeting is being held in Liverpool. Those who, from former visits, are conversant with the town and its multitudinous attractions, will have every inducement to attract them thither once more; but as there are many who are paying Liverpool a visit for the first time, it may not be superfluous to direct special attention to those points which come more especially under the notice of all who are interested in scientific objects and pursuits, the more especially as during the meeting of the Association time is limited, or fully occupied during the chief portion of the day.

In Liverpool, then, first and foremost stands the Derby Free Public Museum, which is for once to belie its name by being closed to the general public during the meeting of the British Association, being only open to the members. As the reception-room occupies the central hall of this building, it will be of necessity much visited, and we have not the slightest doubt that its fine collections will be duly appreciated and admired. Visited yearly by upwards of half a million people of all classes it may be said without prejudice to other and much older country museums, to be the finest museum out of London. It took its rise in the zoological collection presented to the town by the late Earl of Derby some twenty years ago. The munificence and philanthropy of the late Sir William Brown induced him to erect the handsome building in which the Museum and Free Library are now placed, entirely at his own cost (about 35,000*l.*), on land given by the Corporation for that purpose. Since that time the museum has acquired by special gifts some extremely valuable and large collections, to be hereafter noticed more particularly, and is in daily receipt of specimens and contributions from its many admirers. Owing to the great maritime trade of Liverpool, the Museum is being constantly supplied with valuable specimens, collected by ships' officers and seamen all over the world; and as these gifts are acknowledged in the local papers each week, every inducement is held out for others to follow such good examples. The Museum, which is under the able curatorship of Mr. T. J. Moore, aims at being a Zoological Museum, and, moreover, essentially a practical and working one. For this purpose the specimens are arranged as far as possible in open table-cases, and proceed in order from the lowest animal forms, the Protozoa, through all the Invertebrata, to the Reptilia, Aves, and Mammalia.

Beginning in the room labelled Bird-room No. 1, the table-cases are filled with fine specimens of Protozoa, Corals, &c., and as the general arrangement in the cases throughout the Museum is pretty much the same, its completeness and perfection will be gathered if we describe one case more particularly in detail. We will take the large class of Spongida. In the centre of the case is placed a card about the size of a page of an octavo book, on which are legibly printed the chief characteristics, and an accurate description of the class of animals which are represented in the case. In the Spongida and many others, these descriptions are taken from Professor Huxley's work on the Classification of Animals. Around this card are arranged the mounted specimens, each accurately named and its locality given. British species are marked by distinctive labels, and the case is completed by the introduction of fossil specimens. Thus at a glance there is given an accurate description which can be soon copied out, and around it are placed the recent and fossil specimens of the class of object described. In short, upright, cases above these are arranged any more conspicuous and showy objects of the same class. The simplicity and thorough usefulness of the Museum may be easily imagined when it is understood that this system is pursued throughout, and that, in the large classes of Insecta and Crustacea, all the subclasses and distinctive genera are similarly treated.

Before leaving this room we cannot but notice a magnificent specimen of the rare coral *Isis Hippuris*, from Port Elizabeth (?), and the fine collection of the elegant Venus' flower-baskets (*Euplectella*), in reality a sponge, from the Philippine Islands. In Bird-room No. 2 we proceed, through the classes of Polyzoa and Molluscoida and Ascidia, to the Mollusca proper. There is a fine case of *Terebratulæ*, which deserves notice, as the recent specimens are very numerous, and are contrasted with good fossil specimens. A hasty glance showed us recent specimens from New Zealand, the Moluccas, China, Philippine Islands, South Australia, America, Sandwich Islands, Japan, Sicily, North Britain, Peru, Norway, Panama, Singapore, California, the Mediterranean, &c. &c., —surely not so bad for one case of rare shells in a local museum.

In the next rooms, Bird-rooms Nos. 3 and 4, the Bivalve shells are followed by the Gasteropoda and Cephalopoda, proceeding onward to the Crustacea and Insecta, of which there are—especially in the latter class—very fine and well-arranged collections. In the collection of Mollusca and Gasteropoda is incorporated a valuable typical collection presented to the Museum in December 1869 by Mr. S. Smith. All around the three rooms we have described are arranged large upright wall-cases filled with stuffed specimens, chiefly birds, whilst in Room 5 are arranged the collection of fish and reptilian remains. There is a fine skeleton of the Dodo, from Mauritius, which was presented by Mr. Higginson, and is only excelled by the slightly more perfect ones in the British Museum and the Cambridge Anatomical Museum.

The collection of stuffed animal remains is arranged in the basement of the building, and is chiefly remarkable for the fine specimens of the Gorilla which are there exhibited. It is under process of re-arrangement, and so does not show to much advantage. The British collection is excessively poor, especially in birds, and there is every opportunity to any one who has a collection of British birds to present them to the Museum, where they will be much appreciated, and certainly properly utilised.

In a room below the basement is a small but almost unique collection, which we cannot but think will be largely visited this week. We allude to the collection of aquaria, where many of the rarest and most interesting marine and freshwater animals may be seen to great advantage. It is no slight advantage for the naturalist to study from living specimens the appearance and habits of such rare animals as the Axolotl from Mexico, and the Proteus from the caves of Adelsberg. In other aquaria are specimens of salamanders from Central Europe, the gigantic bull-frog of America, and many curious and rare American fish. There are also examples of the English lump fish, grey mullet, soles, flounders, shanny, and the rare blenny. The beautiful appearance of these fish, and their varied and in some cases gorgeous hues, cannot be imagined by those who have only seen the same fish when dead. There are also several fine tanks full of sea-anemones in very thriving condition.

We have now nearly exhausted the scientific portion of the Museum; it only remains for us to notice the poor collection of geological specimens, which looks particularly bad just at present, as it is under process of arrangement. Still it is not much at the most, and is decidedly inferior in every way to the rest of the contents of the Museum. There is, however, a small case full of fine specimens of ferns and other carboniferous plants, collected by one of the local secretaries, the Rev. H. H. Higgins, from a railway cutting about eight miles from Liverpool. We hope to direct special attention to this new bed in another column, as it is within such easy distance of Liverpool, and the remains there found are so valuable and plentiful that all geologists attending the Association should pay it a visit.

Besides the above, there is still much to be seen in the Derby Museum, for in 1867 Mr. Joseph Mayer, a wealthy Liverpool citizen, presented to the town his exceedingly valuable collection of antiquities, coins, and gems. Its value is shown by the following extract from the report of the Library and Museum Committee :—"This collection, it is no exaggeration to say, is the finest of the kind ever presented to the public. In some of its departments, those of Wedgewood ware and ivory carvings, it is unique. It contains the best collection extant of illustrations of the Liverpool pottery ware, a manufacture for which the town was once celebrated, but which has long been extinct In Egyptian and Assyrian antiquities it is very rich, particularly in gems. The Fausset collection of Anglo-Saxon remains, the finest extant, forms a portion of it, together with a large number of ancient manuscripts and illuminations."

In recognition of this valuable gift a statue has been erected to Mr. Mayer in St. George's Hall. The collection is arranged in three tiers of galleries, round a wide open space in the centre of the Museum, and communicating with each other and the rest of the Museum. We cannot afford space to give any full description of this Museum, but its varied collections of antiquities, ancient armour, ancient weapons, musical instruments, coins, gems, watches, carvings, intaglios, Wedgewood and Majolica ware, rare MSS. and Mexican hieroglyphics, will all command crowds of visitors. In the upper gallery is placed *the Collection of the Lancashire and Cheshire Historic Society*, which holds its meetings in Liverpool, and a collection of valuable antiquities from the sea-coast between Leasowe and Hoylake in Cheshire, which belong to Mr. Ecroyd Smith and others, and concerning which Mr. A. Hume, of Liverpool, recently wrote a long and illustrated description in his "Ancient Meols"—the name of an old Anglo-Saxon and English settlement supposed to have been submerged by the sea.

The next place which will claim the most attention in Liverpool, after the Derby Museum, is the *Bidston Observatory*. This is reached by taking the ferry boat to Seacombe, and thence either omnibus or cab to the Observatory, about two miles distant. It stands in a fine situation, commanding most extensive views, and is about 200 feet above the level of the sea. Its great attraction, beside the fine equatorial telescope it contains, are its ingenious self-registering meteorological instruments. Of these the first to see is King's self-registering barometer, where, by means of an ingenious arrangement, the hourly movements of the barometer are self-recorded. There are only three such instruments in England—this one, one in Birmingham, and one in Mr. Crossley's private observatory in Yorkshire; and they are stated to be superior to the ordinary photographic method employed elsewhere. In an upper part of the building is an instrument whereby, on the same sheet of paper, four different set of observations are self-registered. These are, the velocity of the wind, its pressure and its direction, and the amount of rain-fall. The instrument only requires to be attended to once a day, and then acts twenty-four hours without intermission. When we visited it on Saturday the pressure of the wind had reached as much as 65 lbs. per square foot, and its velocity at that time was about seventy-one miles per hour. The Observatory is open to all members of the Association on production of their tickets, and Mr. Hartnup, its director, will have great pleasure in showing and explaining all the instruments. We must not omit to notice a very ingenious and beautiful chronograph for recording transits by means of a most delicate piece of mechanism acting by electricity, and literally writing its own observations.

Returning to Liverpool, there are one or two more museums which deserve attention. The first of these is the *Museum of the Royal Institution*, in Colquitt-street. Before the opening of the Derby Museum, this was the principal museum in Liverpool, but it is now left comparatively in the shade. It is essentially a natural history museum, but the specimens are not well stuffed, and, together with the rooms, which are badly lighted up, have a very dingy and dirty appearance. There is, however, a fine collection of birds ranged round the walls of the two best rooms, the centre of which is occupied by a large collection of shells, neatly arranged and displayed, and well named, of which the chief specimens came from the collection presented to the museum by Mr. George Green, of Liverpool. The mineralogical collection, which wants re-arranging and classifying, has lately received large additions from the collection of the late Dr. James Stewart Trail, of Edinburgh, lately presented to it. There is a geological collection arranged round an upper gallery, but of which nothing commendable can be said. The materials are there for a good typical collection, and there are a few good American collections which are interesting; but they all sadly want naming and properly arranging, localities given, &c.

Next to this is an Economic Museum, which is worth visiting, arranged somewhat on the system employed at South Kensington. Besides these, there is a herbarium, a fair collection of butterflies and beetles and a collection of fungi from the Liverpool district, and a good collection of zoophytes from Bootle, near Liverpool. There is also a small collection of ethnographical objects, weapons, Indian remains, flint axes, &c., and the usual miscellaneous objects of a local museum. There is a large stuffed specimen of the sea-elephant from the South Shetland Islands, which is valuable, as the species is yearly becoming much rarer. It seems a pity that some arrangement could not be made between the two museums, so that this one could be lightened of some of its heterogeneous contents, and more room given for a better display of some special collections. Besides this, those who are interested in mineralogy should not fail to visit the *Medical Institution*, in Hope-street, where Phillips's mineral collection is well exhibited. There is also the *Museum of the Royal Infirmary School of Medicine*, which is more likely to please those interested in medical pursuits than the general public.

To those botanically inclined, the extensive *Botanical Gardens in Edge Lane* will be very attractive. They cover an extent of about eleven or twelve acres, and are very tastefully laid out, and the large conservatories contain many fine and choice exotics.

Those who are interested in the practical application of science to the requirements of the present day, should not fail to visit *the Liverpool Sewage Utilisation Works at Sandhills*. These are open to all members of the Association, and will be found to afford much practical knowledge on a subject that is yearly attracting more attention.

The Liverpool scientific societies, though they have existed many years, have failed to attract very much attention, or to become prominently known. The most important of these is *the Lancashireand Cheshire Historic Society*, which was started about 1847, as a purely antiquarian society in all matters relating to the two counties. It has published a set of transactions containing much valuable information; but of later years it has enlarged its scope and taken in other branches of scientific knowledge, with a result, we fear, not commensurate with the wishes of its promoters. *The Liverpool Philosophical Society*, and *the Liverpool Geological Society*, unlike the similar societies in Manchester and Leeds, are but little known outside the city in which they hold their meetings.

Soon after Owens College was founded in Manchester, a somewhat similar college, called *Queen's College*, was started in Liverpool; but whether owing to lack of energy or good management on the part of its directors, or from the overpowering influence of the old-established Collegiate Institution, it has certainly failed in approximating in any direction to the national importance and value that the former college has obtained.

In some degree compensating for these failures, Liverpool possesses a very extensive and much-patronised *Field Naturalists' Club*, which does a great deal by weekly excursions to infuse into its members a taste for natural science and out-of-door scientific work, and there is little doubt but that from all its members the British Association will receive a hearty welcome; and let us hope that from this year's meeting may date an increased impetus to Science in the neighbourhood.

J. P. E.

NOTES ON THE GEOLOGY OF THE COUNTRY AROUND LIVERPOOL

THE following brief notes on some of the points of geological interest may perhaps be of use to geologists from the South of England visiting the British Association's Meeting at Liverpool.

The tract of country lying between the rivers Dee and Mersey, known as the peninsula of Wirral, is composed of Triassic rocks, forming a series of undulating ridges and valleys, running parallel with the strike of the rocks. The average elevation of the crest of the hills is about 150 feet, and they, as well as the valleys, are more or less covered with glacial drifts. The northward prolongation of the hills and valleys is abruptly terminated by a broad plain, but little raised above the sea-level, which forms the seaward portion of the Hundred of Wirral. This plain, which is composed of peat partly covered with alluvium, is drained by the River Birket, which flows eastward along its whole length, from a little south of Hoylake, until it falls into Wallasey Pool, an arm of the River Mersey, which separates the tract of comparatively high land known as Wallasey (anciently an island) from the Triassic hills south of the Pool.

Crossing the Mersey, the town of Liverpool is found to rest on a continuation of the Triassic hills south of the river, but more deeply covered with drift, which has in most cases so entirely filled up the valleys that the whole of the Coal-measure and Triassic districts of south-west Lancashire are one vast plain, surrounding a central nucleus formed by the spurs of the Pennine Chain, composed of the Millstone Grit.

The latter formation, on the borders of the Lancashire coal-field, reaches an average thickness of 5,000 feet, and is composed of four great beds of grit, divided from each other by thick beds of shale. These beds are known as the *Rough Rock*, or first grit; the *Haslingden Flags*, or second grit; the third grit; and the *Kinder Scout*, or fourth grit. The first and fourth grits are generally coarse, conglomerative, and massive; the third, as a rule, finer and not conglomeratic—both it and the fourth grit are often divided into two, three, and even more beds of thick seams of shale, which thin, wedge out, and thicken in the most irregular and local manner. In fact, though the Millstone Grit, as a whole, maintains a general average thickness in any special area, its members in *detail* appear to be ever changing places in relative consequence with each other, proving the shallowness of the sea, the proximity of land, and the existence of currents laden with different materials, sand and pebbles washed from those deep-seated quartzites which raised their heads above the sea at the time of the deposition of the grit in the area now occupied by the Pennine Chain.* Workable coal-seams occur in the first and third grits, and thin seams in the fourth; associated with them are shales in which a flora and fauna of about 30 species occur, the same species recurring in the different seams of shale: 18 species occur in the shales of the "Feather-edge" coal† (in the Rough Rock); amongst them are *Calamites Suckowii*, and *Pecopteris arborescens*. The former species I recently found in some shales of the second grit, exposed

* Prof. E. Hull, F.R.S., Quart. Journ. Geol. Soc., August, 1868.
† This coal is described by Mr. E. W. Binney, F.R.S., in the Trans. Geol. Soc. Man. vol. i.; by Prof. E. Hull, F.R.S., in Geol. Surv. Mem. "On the Geology of Oldham," and in the "Geology of Bolton-le-Moors."

in the new excavations for the Liverpool Waterworks' upper reservoir, above Alance Bridge, north of Rivington. A little further west another bed of black shale occurs, apparently on the same horizon as that from which the Geological Survey Map (six-inch map, Lancashire, sheet 78) records the presence of *Goniatites*. These and other fossils also found in the shales forming the roof of two coal-seams, occurring between the second and third grits. The shales dividing the latter grit are also fossiliferous in two instances in Lancashire, and *Goniatites* may be found in the shales between the third and fourth grits, at Old Kates Dingle and Shore Brook, below Noon Hill, east of Rivington (reached from Adlington Station). They reach, I should say, a thickness of 600 feet, and contain two thin coal-seams. The Kinderscout grit, though nearly 1,000 feet thick, appears to be almost devoid of organic remains; but the occasional fragments of *Stigmaria* testify to the existence of land during the period of its deposit, as do also the thin seams of coal.

The Coal Measures.—These are so well known through the various survey memoirs of Professor Hull,* and papers of Mr. E. W. Binney, F.R.S., that it is needless for me to describe them. They are divided into three divisions: the upper is devoid of coal, and is absent in South-west Lancashire; the middle coal-measures contain all the valuable coals, the best being that at the base, known as the Arley Mine, which is perhaps equal to any coal in England. The sixth seam, above the Arley, is the celebrated Wigan "cannel coal:" it is three feet thick at that town, thinning out in every direction, with Wigan as a centre, as shown by Mr. Hull.

The Lower Coal Measures, or Gannister beds, from their containing siliceous concretions, locally called "Gannisters," have five or six workable coals which are known as Mountain Mines. Coal-measures occur at Neston on the west coast of Cheshire, and again at Croxteth Park, near Liverpool, and it is probable that they underlie the whole of the Triassic rocks of Wirral, and of part of Liverpool itself.

Permian.—No geologist should leave Liverpool without visiting the two Permian outliers, discovered by Professor Hull, at Skillaw Clough and in Bentley Brook, Bispham (north-east of Ormskirk); they consist of sandstones, marls, and magnesian limestone.†

Trias, or New Red Sandstone.—This is divided into the following subdivisions:—

Keuper Series	Grey and Red Marls . .	1,000 feet
	Lower Keuper Sandstone .	400 "
Bunter Series	Upper Mottled Sandstone.	500 "
	Pebble Beds	800 "
	Lower Mottled.	100 "

The *Lower Mottled Sandstone* is best seen in the Liverpool district, at Eastham, on the Cheshire side of the Mersey, where it forms a cliff capped by the *Pebble Beds*. The latter are well seen in the quarries at Everton, above Liverpool; they are generally stained a deep brick-red with peroxide of iron, and contain seams of quartz-pebbles running along the lines of current-bedding seams of grey and red marl, which also occurs in small pockets in the rock. The *Upper Mottled Sandstone* is, as a rule, pebbleless, much false-bedded, streaked and mottled in its middle portion, yellow above, and deep bright red below. The latter beds are well seen at the mouth of Bromborough Pool, near Birkenhead; the middle beds at Ormskirk, where the celebrated section, first described by Mr. Hull,* occurs, where nearly level beds of conglomeratic Lower Keuper Sandstone rest on the denuded upturned edges of the variegated beds of the Upper Mottled Sandstone: it is exposed in the railway cutting leading towards St. Helen's, a little east of the town.

Lower Keuper Sandstone.—The base of this sandstone in this as in other districts, is extremely pebbly, and consists of hard grit, not unlike some of the beds of the millstone grits, and like them, and the sands and gravels of the Middle Drift Period, is much false-bedded. The middle portion consists of fine-grained freestones, separated by thin seams of grey marl, supporting water: these are well seen in the railway cutting at Orrel (east of Waterloo),† first described by the writer in the Survey Memoir on the district. The Labyrinthodon bed (3 to 4 feet thick) occurs near the base of this part of the Keuper Sandstone: it is best seen at Storeton, near the windmill (on the Cheshire side of the river): many fine footprints may be seen in the Liverpool museums. In the Orrel Railway cutting, magnesia is found to occur in the shale, as well as pseudomorphous crystals of salt, which also occur in the shales at the top of the sandstone, or at the base of the marls, whichever way they may be taken; for the sandstones, shales and marls, are in reality only one series gradually passing from the one into the other, as the sea grew shallower and shallower, and became supersaturated with salt, until at length the sea became a salt lake. From this sequence there is, however, one exception in this district, which I have not noticed elsewhere. A conglomeratic bed occurs near the very top of the Keuper Sandstone, immediately below the horizon of the shales; the pebbles consist of quartz, and are apparently derived from the same source as those occurring in some of the millstone-grit beds. The existence of round pebbles in a deposit proves either the proximity of a coast line or the shallowness of the water at the period of deposition; for unless the water is shallow, currents, I know by experience, have not the power to move pebbles. The Trias, as a whole, appears to have been formed during a period in which subsidence hardly kept pace with the deposition of sands and clays brought down by rivers from continental lands. This upper pebble bed would appear to have been thrown down at a moment when the movement of subsidence was greater than usual, causing islands of quartzites, or possibly of millstone grit, containing quartz-pebbles.

Keuper Marls.—This division attains an immense thickness in the country between Liverpool and Southport; but is so deeply covered with drift, glacial and post-glacial, that sections are very rare. Much of it, like the northern end of Cheshire, is scarcely above high-water mark, forming a low-level plain covered with peat-moss; between it and the sea intervenes a tract of blown sand, forming dunes or "hoes" as they are locally called, which is traversed by the railway from Liverpool to Southport.

* Mem. Geol. Surv., "Geology of Oldham," "Geology of Wigan," "Geology of Bolton-le-Moors," "Geology of Prescot."
† See "Geology of Wigan," p. 27, and Geol. Surv. Map, 89, S.W.

* Mem. Geol. Surv. "Geology of Wigan." By E. Hull, F.R.S.
† Mem. Geol. Surv., "Description of 90 S.E.," and "Geology of the country between Liverpool and Southport." By C. E. De Rance, F.G.S.

Looking seaward from the train, the eye can descry nothing but range behind range of dunes, the only variety being produced by the irregularity of their heights; the only change from the ceaseless monotony of their yellow slope, the dark green stripes of dwarf willows that serve still more strongly to bring out the sterility o the scene. Leaving the train at Ainsdale, or Hightown, and examining the Lancashire Sahara more closely, it is found to consist of three portions:—a range or series of ranges of sand hills, from one to two miles in breadth. sloping down to the peat-moss,—a central plain,—and a range of sand-hills between the plain and the sea, protecting the former from the latter. Their incoherent masses would, however, be of little avail, were it not for the matted roots of the Sand-reed (*Ammophila arundinacea*), locally called "starr-grass," and woven by the people into mats and other articles, and which grass they were unable, until lately, by an old Act of Parliament, to cut or destroy, under the most severe penalties. In the plain, or rather in the series of small oval plains divided from each other by little ridges, running from the sea to the land, there is, in the summer, a dense carpet of spongy moss, mixed with sedges, and sprinkled with flowers. The great quantity of lime constantly set free by the dissolving of marine shells in the sand, causes many of the plants to be of species generally found on a chalk soil. Here occur various plants belonging to the Gentian tribe, as the Perfoliate Yellow-wort (*Chlora perfoliata*), the Red Centaury (*Erythræa Centaurium, E. pulchella*, and *E. latifolia*). *Gentiana Amarella*, with its purplish-blue flowers, will be also found about this time. On the adjoining moss-land occurs the rare marsh-gentia, *Gentiana Pneumonanthe*, with its large blue bell with five green stripes, With it grows the Buckbean (*Menyanthes trifoliata*). In the "slacks" (the local name for the little oval plains in the sand-hills), the beautiful *Pyrola media* occurs in great abundance, as does also the Grass of Parnassus (*Parnassia palustris.*) Each of these slacks has a distinguishing name, as "Bull-rush," "Long," "Mayflower," "Round," and "Dale Slacks:" these in winter receive the drainage of the sand hills, which, being stopped by the carpet of vegetation, forms a series of large standing pools of water, in the midst of apparently porous sand.

To return to the Keuper Marl: it is a series of red, green, and grey marls, with occasional seams of freestone, much ripple-marked, and beds of shale, generally with pseudo-morphous crystals of salt, and often veins of fibrous gypsum. Through denudation, the top of it is never seen, and it is therefore, with the exception of the Drift, the newest formation in the Liverpool district.

Glacial Drift.— Professor E. Hull, F.R.S., proved (in a paper read at Manchester, in 1862) that the Glacial Drift in the Manchester district was capable of division into an Upper and Lower Boulder Clay, divided by a Middle Sand and Gravel, which he called the "Middle Drift." The writer, in a paper on "Glacial Phenomena of Western Lancashire and Cheshire," read at the last meeting of the Geological Society, attempted to prove that this classification holds good, not only in the whole of Western Lancashire, but from the River Dee to the flanks of the Cumberland and Westmoreland Mountains; and since writing that paper he has found that the terrace of Glacial Drift skirting the mountains of North Wales, lying between them and the sea, is capable of that division, the cliffs of boulder clay east of Llandudno (round the Little Orme's Head) being distinctly divided by a *Middle Sand*, containing the same species of shells as those occurring in the Lancashire Middle Drift.

The *Lower Boulder Clay* (the writer endeavoured to show, in the paper alluded to above) was formed by an ice-sheet, which covered nearly the whole country down to a level of about 150 feet to 200 above the present sea-level; this clay he termed the "High-level Low Boulder Clay." Below a level of 100 feet, the clay appears to have been formed by the summer melting of an "ice-foot," which surrounded the sea-margin—at that level the land, through subsidence, standing that amount lower than at present. An amelioration of climate then took place during which the sands and gravels of the *Middle Drift*, with shells of Celtic type, were deposited round the edges of higher, and still higher, successive coast-lines, as the land gradually sank, until the sand and gravel, at Macclesfield, more than 1,200 feet above the present sea-level, was deposited in water of the *same depth*, and containing the *same shells*, as that in which the middle drift of Blackpool, only fifty feet above the sea was deposited. The phenomena exhibited by the middle drift, of the invariable rise from the sea to the land, in an inclined plane—the undulating surface, now far below, now up to, but never above, that plane—can only be explained by the theory that it was formed as *sand-banks* in shallow water on a gradually subsiding tract; and the *Upper Boulder Clay* is a marine deposit, formed of the detritus brought down by glaciers in the valleys of the Cumberland Lake district, to the ice-foot, which melting carried its spoils over the sea-covered plains. All these divisions are more or less well seen in the Liverpool district, especially on the Cheshire side of the river, in the neighbourhood of Egremont and Eastham. Further south, north of Chester, the Middle Drift is particularly well developed. Most of the boulder clay in North Cheshire belongs to the upper division, the lower clay being absent, having been denuded away. Glacial striæ were discovered by Mr. Morton F.G.S., at Flaybrick Hill, the direction being N. 30° W. at an elevation of 120 feet above the sea-level; and at Toxteth Park, the direction being N. 42° W.; also by Mr. Hull, F.R.S., at Kirkdale, the direction N. 15° W., caused probably by icebergs during the Upper Boulder Clay period.

In the district between Liverpool and Southport a bed of sand occurs, forming a line of old sand hills at the inland edge of the peat-moss plain, and making a sort of step between it and the comparatively high-level (80 to 160 feet) boulder-clay plain above. One of these hillocks is called Shirdley Hill; I therefore called the sand the "Shirdley Hill sand."* It is about 30 feet thick, and underlies the later deposits of the peat plain, but rests itself on a thin deposit of what I called the "Lower Peat." Above this sand, and beneath the great bed of peat (Upper Peat), is a bed of grey clay containing freshwater shells, which I called the "Lower *Cyclus* clay." I have observed it from the Island of Walney, in North Lancashire, to the coast of North Wales, and believe it, as well as the peat above it,

* "Description of Geol. Surg. Map, 90 S.E." and "Post-Glacial Deposits of Western Lancashire and Cheshire," read at last meeting of the Geol. Soc.

occurs at the bottom of the greater part of the Irish Sea. All along the coast of Lancashire, Cheshire, and to a certain extent North Wales, the peat, with a forest at its base and the clay containing the roots of the trees, may be seen, nowhere so well as at Leasowe, in Cheshire, and at the mouth of the Alt, Hightown, Lancashire (eight or nine miles from Liverpool). In both localities the peat, the forest at the base, and the grey clay below, occupy the country inland, run *under* the sand dunes, emerge on the coast, and disappear under the sea sand at low-water mark. In North Cheshire the peat is sometimes split into two, a bed of grey clay, with *Scrobicularia piperata*, being intercalated in the mass. This I consider to have been formed when the Mersey entered the sea, through what is now the gorge of Wallesey Pool. Very near the top of the peat a thin seam of sand occurs, both in Cheshire and Lancashire, containing *Tellina Balthica* and *Cardium edule*. I called it the zone of *T. Balthica*. The grey clay of the Isle of Man, with *Cervus Megaceros*, is no doubt of the same age as the "Cyclus Clay" of Lancashire.

Those who visit Hightown will find the peat, which is there from twelve to twenty feet thick, covered on either side of the River Alt with an alluvium which, near the sea, contains *Scrobiculariæ*, and inland graduates into a fluviatile deposit, with freshwater shells. They will find the base of the sand dunes, where they rest on the upper surface of the peat, to be, as at Leasowe, in North Cheshire, a freshwater deposit, which I called the *Bythinia tentaculata* sand; it contains thin seams of peat up to a height of eight or ten feet from the base.

My notes are already so long that I abstain from saying anything of the marine fauna of the coast and other matters: those that I have written refer to districts in which the maps, &c., of the Geological Survey are already published, and I have of course written as a private geologist, stating my individual opinions.

C. E. DE RANCE

H.M. Geological Survey

THE SECOND PROVINCIAL MEETING OF THE IRON AND STEEL INSTITUTE

THE Institute bearing the above name was originated about two years ago, chiefly by the North of England ironmasters, among whom there are many gentlemen who combine in themselves great practical skill and a large amount of scientific knowledge. It was not formally launched into existence till the month of June, 1869, when the inaugural address was delivered to a meeting of the members, held in the Hall of the Society of Arts, by the president, the Duke of Devonshire, who is intimately and extensively connected with the iron and steel trades through the great works of Barrow-in-Furness, perhaps the greatest Bessemer steel works in the world. The aim of the Institute is to hold two meetings annually—one in London, in the spring, and the other in the country in the autumn. The first provincial meeting was held, about a year ago, in Middlesborough, the capital of the Cleveland district, the greatest and most scientific iron-making district either in this or any other country. In May last the second metropolitan meeting was held; and now the second provincial meeting has just been held at Merthyr Tydvil, in South Wales. At all these three meetings there have been read papers of very great interest and importance, both from a scientific and a practical point of view. Then, taking the experience of the two provincial meetings, the members not only have the benefit of hearing the papers read and of taking part in the discussions to which they give rise, but they also have the opportunity of visiting the numerous works that are thrown open for their inspection, where they can see scientific theories put to practical tests, and where they can compare notes with each other upon the subjects which deeply concern them as practical and professional men. It is well known that "iron sharpeneth iron:" and in these country meetings of the Iron and Steel Institute this wise saw has many apt illustrations. Examples to imitate are seen in abundance; many points are observed that are suggestive and that excite to further improvements in other hands; and in numerous instances things are seen which impart lessons of a totally different sort, inasmuch as they show what errors of commission are to be avoided. Both successes and failures can give instruction to thoughtful minds.

This year's provincial meeting of the Institute, as already indicated, has been held at Merthyr. It opened on the morning of Tuesday, 6th September, in the Temperance Hall of that town, the centre and most important seat of the iron trade of South Wales. After the transaction of some formal business, and the appointment of Mr. Henry Bessemer as the president-elect of the Institute, the business of reading and discussing the papers prepared for the meeting was proceeded with. Altogether there were seven papers set down for reading and discussion on the mornings of Tuesday and Wednesday, the 6th and 7th September, which was all the time that could be devoted to that work, as the afternoons were required for visiting and inspecting the works in and immediately around Merthyr, while Thursday and Friday were required for the inspection of works at a distance. One praiseworthy feature in connection with the meetings in Merthyr was the circumstance of printed copies of the papers being in the hands of the members before they were read by the authors. Owing to this arrangement members were generally enabled to come prepared to enter upon the discussion of the papers with intelligence and with some degree of satisfaction. In two instances the papers supplied to the members were accompanied by engravings of the objects described. Both of these features of the Iron and Steel Institute Meeting might be copied with advantage by other learned societies, as they are calculated to impart additional interest to the meetings.

The Iron and Steel Institute already numbers upwards of 350 members, including several peers of the realm, about a dozen members of Parliament, and almost every person in the kingdom who has of late years added to the stock of our knowledge regarding the manufacture, the manipulation, and the use of iron and steel. It has very soon acquired a national importance, and its future prosperity seems to be almost assured. In order to make the Institute more and more useful to the persons who may be connected with it as members, it is intended to publish a Quarterly Journal of the Institute instead of the Transactions. This will commence on the 1st of January, 1871, and its contents will be as follow:—

First, the proceedings of the Institute, and of the Council from time to time. Second, the papers and discussions at the general meetings of the Institute. Third, communications from members upon matters of special interest to the trade, which are approved by the Council. Fourth, a quarterly epitome of inventions, discoveries, publications, and proceedings bearing upon the British iron and steel trades. Fifth, a comprehensive report on matters connected with the iron and steel trades in foreign countries. It is very satisfactory to know that the foreign department of the journal will be under the special management of Mr. David Forbes, F.R.S., and that the general editorship will be conducted by Mr. John Jones, F.G.S., the Secretary to the Institute and to the North of England Iron Trade.

We now proceed to give a brief notice of the papers read at the recent meeting at Merthyr:—

I. "On the Geological Features of the South Wales Coal-Field." By Mr. William Adams, Cardiff. In this paper, which was of special interest to such of the members as were strangers to the district, the author traced the history of our knowledge regarding the position, nature, and extent of the coal deposits which have given such an industrial importance to South Wales. Attention was directed to this coal-field as far back as the year 1570, but the essay itself did not appear till the year 1796, when it was published in the *Cambrian Register*. Since that time many persons have devoted attention to the subject, including Edward Llwyd (1697, "Philosophical Transactions," 1712); Edward Martin (Royal Society, 22nd May, 1806); Buckland and Conybeare (1822, "Geological Transactions," vol. i.); Robert Bakewell (1833); Sir Henry J. De la Bêche (1846, "Memoirs of the Geological Survey," vol. i.); Mr. Hussey Vivian, M.P. (1860); and Mr. Hull (1861, "Coal Fields of Great Britain"). According to Mr. Adams, the South Wales coal-field extends from Blaenavon and Pont-y-pool, on the east, to St. Bride's Bay, in Pembrokeshire, on the west, a distance of about ninety miles; while its breadth varies from about sixteen miles on the east to about four miles in the extreme west. The superficial area is given as low as 906 square miles (Hull), and as high as 1,200 square miles; but Mr. Adams puts it at 937½ square miles, or 600,000 acres. Taking the average thickness at sixty feet, as given by Mr. Vivian, M.P., one of the Royal Commissioners on the Coal Supply, and deducting one-third for loss in working, for faults, waste, &c., the extent of coal is 36,000,000,000 tons. The author pointed out the qualities of the mineral in different parts of the coal-field. On the east it is bituminous, and makes a very superior coke; it continues so to Rhymney; at Dowlais it becomes free-burning; at Cyfarthfa it begins to take on the characters of anthracite; and further westward the coal becomes more and more distinctly anthracitic, until, in the west, it becomes so pure that it is worked for drying hops and malt and for distilling purposes, &c. Within the field there are obtained the well-known steam coals of Merthyr, and the Aberdare and Rhondda valleys. Argillaceous ironstones are very abundant and good. They are interstratified with the coal, and have an aggregate thickness of from sixty to seventy inches or upwards, and until about thirty years ago they yielded practically all the iron which was smelted in Wales. Since then the deficiency for the iron-works has been drawn from other British and foreign mines. The pig iron made in South Wales in 1854, according to Mr. Hunt's "Mineral Statistics," was 750,000 tons, and the coal raised in the same year was 8,500,000 tons, while the amount raised in 1868 was 13,210,000 tons. Owing to the remarkable denudation in the valleys of the district, the lowest seams of coal can be won by pits of less than 1,000 yards in depth throughout about two-thirds of the basin. A greater depth may be required west of the Vale of Neath and on to Llanelly, where the deepest part of the basin occurs. Throughout the coal-field there are faults, running north-west and south-east, which give a vertical displacement of 250 or 300 yards; and there are others running east and west which give a displacement of from 400 to 500 yards; and one is said to occur in Pembrokeshire which is equal to a displacement of upwards of 666 yards. The deepest pits are those which reach down to the nine-feet seam; their depth varies from 304 to 435 yards. In the coal-field there is found some of the best fire-clay known; there is also a remarkable siliceous stone, which is used to make the well-known refractory fire-bricks called Dinas brick. Alum shales occur, and, near Pont-y-pool, a rich oil shale is found, which yields on distillation from fifty to fifty-five gallons of crude oil, of which twelve to fifteen per cent. may be separated as mineral turpentine. Mr. Adams also referred to the extent and distribution of the mountain limestone—so useful as a flux in smelting the ironstone—and in the fossils which the coal measures of the district contain; and, in concluding his very valuable paper, he also strongly urged the desirability of a new geological survey on the six-inch scale being made.

In the course of a short discussion which followed the reading of the paper, several interesting points were raised, more especially in reference to the effects of the faults in changing the character of the coal.

II. "On Pumping and Winding Machinery." By Mr. G. C. Pearce, Cyfarthfa Iron Works. This paper embraced a short description of the pumping and winding machinery lately erected at the Castle Pit, near Merthyr, the property of Mr. R. T. Crawshay. Notwithstanding the shortness of the paper, it would be difficult to give a satisfactory description of the machinery in the form of an abstract, especially without illustrations. But a few facts may be mentioned. The pit is 333 yards in depth to the bottom of the sump, but the pumps only raise the water 279 yards, where there is an adit. It is of an oval form, 22 feet 8 inches long by 12 feet wide, with a brattice dividing the winding parts from the pumps; and it is all built with brick throughout. The total cost of the machinery and the sinking of the pit was about 30,000*l*., the winding engine alone costing above 4,000*l*.

In the afternoon of Tuesday the Castle Pit was visited by the members of the Institute, many of whom were much struck with the gigantic and ingenious character of the machinery erected by Mr. Pearce.

Mr. E. A. Cowper, C.E., F.R.S., followed with some observations in reference to the working of steam for pumping, winding, and blowing engines; but no discussion ensued.

III. "On the Condition of Carbon and Silicon in Iron and Steel." By Mr. George J. Snelus, Associate of the Royal School of Mines, chemist at Dowlais Iron Works. This paper was of considerable length and very elaborate. It contained the results of a long course of experimental inquiry, instituted with a view to determine the conditions in which the two non-metallic bodies, carbon and silicon, exist in iron and steel. Dr. Percy had said in his celebrated work, "Iron and Steel," that not a trace of graphite could be detached by the point of a penknife from the fractured surface of highly graphitic iron; but Mr. Snelus had proved the incorrectness of this statement by examining some pig iron which had cooled slowly under a mass of slag, and which had in consequence very large crystals. From the surfaces of these crystals the graphite could not only be separated with the point of a penknife, but even with the finger-nail; and when the graphite was removed the iron underneath rapidly rusted in a damp atmosphere. The same thing was afterwards observed with the fractured surface of Bessemer pig-iron, and the scales removed were found on further examination to be pure carbon. By pulverising pig-iron and then using the magnet a considerable amount of graphite was separated. Other mechanical means were employed, and the same results were obtained. In spiegeleisen the carbon was found to be almost wholly combined. The author had never found as much as five per cent. of combined carbon in pig iron, although many analyses had been published in which the carbon was put down at even six per cent. Mr. Snelus was not inclined to believe that there was any definite compound of carbon and iron but, he thought that the carbon was dissolved in the iron, the amount taken up by the molten iron varying according to several circumstances. According to Mr. Snelus there is no pig iron that is destitute of silicon, and he had never met with a case in which either steel or wrought iron was totally free from it. Good Bessemer and tool steel rarely contains more than two or three parts in 10,000. One part of silicon in 1,000 of Bessemer steel renders it hard and brittle when cold. In ordinary Bessemer pig iron it is present in quantities varying from one to four per cent. The author gave it as his opinion, from experimental inquiries, that silicon is dissolved or "occluded" in iron in the same way as carbon is, but that the solvent power of the metal is so much greater for silicon than for carbon, that it is quite a rare thing, even if it ever occurs, for silicon to separate in a free state from the iron; and he considered that the methods of mechanical separation which he had adopted for the investigation of the condition of carbon and silicon would prove effectual aids to the ultimate analysis of iron, and a valuable supplement to the ordinary methods of research.

Mr. Isaac Lowthian Bell complimented Mr. Snelus upon the great value of his paper, and criticised some of the statements made in it. On the presence of silicon in steel, Mr. Bessemer said that a very general opinion prevailed in the trade (and it was correct when they spoke of large quantities) that silicon was deleterious to the make of iron. It was the impression that they should get rid of silicon, and they would then make better iron. That was not the case, in proof of which assertion he called attention to the old steel process as carried on in Sheffield, and adduced instances to show that the presence of silicon improved the quality of the manufacture. He had not had an analysis of the best quality of Sheffield steel that did not contain silicon.

A discussion was then taken upon a paper read at the meeting held in London in May last, the subject of which was "A Method of Designing Rails," and the afternoon was spent in visiting the works of the Plymouth Iron Company, the Castle Pit winding and pumping machinery, and the Cyfarthfa Iron Works, the property of Mr. Crawshay. At the last-named works there was seen an immense stock of puddled iron, upwards of 20,000 tons, stacked in "houses," each containing 300 tons.

The proceedings were resumed on Wednesday morning. The papers read were the following:—

I. "On a New Form of Pyrometer." By Mr. C. W. Siemens, C.E. F.R.S. D.C.L. After describing briefly the Wedgewood and Gauntlett pyrometers, and one previously constructed by himself, Mr. Siemens proceeded to describe a pyrometer of more universal applicability. It is based upon the peculiar properties of the pure metals to offer an increasing resistance to the passage of an electrical current with increase of temperature. A platinum wire of known electrical resistance is wound upon a cylinder of fire-clay, upon which a helical path has previously been cut to prevent contact between the turns of the wire. The coil of wires, so prepared, is enclosed within a cylindrical casing of platinum if the temperatures to be measured exceed the welding heat; or of iron or copper, if lower temperatures only require to be measured. The two ends of the coil of wire are brought out end ways, and are attached within the protecting tube to thicker leading wires of copper, insulated for a short distance by being passed through pipe-clay tubes, and further on by india rubber or gutta percha, terminating at the measuring instrument, which may be placed at any convenient distance. The characteristic feature of this instrument is that the usual calculations necessary to determine electrical resistances by the Wheatstone or other methods are dispensed with, and a reading in degrees of a large scale is at once obtained by so placing the index lever that the electrical current, generated in a small battery and passed through the measuring instrument, including the platinum wire of the pyrometer, produces no deflection of the galvanometer needle. The temperature which these degrees represent is expressed by a table of reference, which accompanies each instrument. The correctness of this instrument depends solely on the ratio of increase of electrical resistance in the platinum wire, with increase of temperature. This rise is considerable, the resistance being increased fourfold by an increase of temperature from the freezing point to about 3,000° Fahr. The ratio of increase is, however, not uniform, but follows a parabolic law which the author embodied in a table. The pyrometer last described is the result of more careful investigation on the part of Mr. Siemens, who has been animated by a desire to fill up a blank in the means at our disposal to carry on metallurgical inquiries with a high degree of certainty, and he therefore does not seek any commercial compensation, through the Patent Office or otherwise, for using this invention.

A discussion followed, in which Mr. Snelus, Mr. I. L. Bell, and Mr. E. A. Cowper took part.

II. "On the Efficiency and Durability of Plain Cylindrical Boilers." By Mr. Jeremiah Head, Middlesborough. The author of this paper aimed at pointing out the many high qualities possessed by the plain cylindrical boiler, to investigate the cause and extent of the defects which they possess, and to point out satisfactory remedies for them. Such boilers are still in large request. Out of 17,825 boilers now on the books of the various boiler insurance companies 4,052, or 22·7 per cent., are of this type. But such boilers are very liable to fracture and break their backs, and Mr. Head gave an explanation of the causes of such occurrences, and explained the method which he had adopted to prevent the recurrence of such a mishap in a boiler that had been repaired about the beginning of March last. His plan is to suspend the boiler upon volute springs. No springs are necessary for boilers of 30 feet long upon two supports; but for lengths of 60 to 75 feet five supports are required, the end ones being furnished with double springs.

The discussion following this paper was so long that no time was left for the consideration of a paper by Mr. Kohn, C.E., on "The Production of Alloys of Iron and Manganese, and on their Application to the Manufacture of Steel." A paper on "A Contribution to the History of the Puddling Process" was also left over for consideration at the next meeting in London. On that occasion the place will be selected for the next autumn meeting.

In the afternoon of Wednesday, the world-famous works of the Dowlais Company were visited by the members of the Institute. Thursday was occupied with a visit to Swansea, where an inspection was made of the Landore Steel Works, where the Siemens-Martin process is carried on; the Hafod Copper Works, belonging to Mr. Vivian, M.P., and the Spelter Works. Friday was devoted to a visit to the Crumlin Viaduct and the Ebbw Vale Company's Iron and Steel Works. These several inspectorial visits were thoroughly enjoyed, and the hospitality with which they were attended was most profuse. Doubtless, many of the practical and professional men who were present at the inspection gained a large fund of mental insight which will, ere long, be applied to practical advantage, or, at all events, be put to a practical test.

JOHN MAYER

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

A Mirage

As several notices of Mirage seen in Britain during the present year have recently appeared in NATURE, I venture to trouble you with the following substance of a few brief notes (made on the spot) of an instance which I witnessed on April 15, 1870.

With a companion I was walking slowly from Chesilton, in the "Isle" of Portland, along the western slope of the famous Chesil Bank, in a north-westerly direction—my friend on one of the numerous narrow terraces into which the slope is broken, I on that next below. The day was bright and warm, and the sea was calm. Having occasion to stoop down, I caught sight of what for the moment I thought a considerable pool of water on the terrace on I which stood, in the direction in which we were moving. A moment's thought reminded me that a pool in such a beach of loose pebbles was out of the question, and convinced me that for the first time in my life I saw a Mirage. The hour proved to be exactly noon, Greenwich time. My friend, on having his attention called to it, saw a similar "pool" on his terrace. On trying a series of experiments, we found that when we were quite erect the phenomenon was barely visible, and would never have commanded attention, but that as the eye was brought nearer and nearer to the surface the "pool" grew larger and larger; that in ordinary cases each observer could see it on his own terrace only, but that when he brought his eye to the outer edge of one terrace the "pools" on it and on that next below were visible at the same time, and were almost blended. As we advanced so did the Mirage. At 12.20, local time, when the sun was, of course, a very few degrees west of south, it bore N. 33° W., magnetic variation being allowed for. The apparent water, like the real water in the adjacent sea, was a dark blue. We left the beach at two o'clock, and noted that the "pools" were as distinct as ever; and, being then some distance from Portland, we observed a similar phenomenon in that, the opposite, direction.

Lamorna, Torquay, Sept. 10th, 1870 W. PENGELLY

External Configuration of the Earth

UNDER the heading of "Volcanic Action *v.* Denudation," I observe in the last number of NATURE (Sept. 8th), a letter from Mr. A. H. Green, in which that gentleman finds fault with the conclusion arrived at in the following quotation, apparently extracted from some, to me unknown, report of my lecture on volcanoes, the original of which is given in the *Geological Magazine* for July 1870; in instituting "a comparison between the relative magnitude of the operations of internal and external forces in determining the main external fractures of our globe, we must grant the first rank to the internal, volcanic, or cataclysmic agencies, since, had it not been for their operations, our globe would have remained without any visible land for the river to traverse, or the rain and ice to disintegrate and wear away;" but as immediately afterwards Mr. Green himself adds, "the latter part of this statement, cannot, of course, be called in question," these very words, alone, seem to me tantamount to a complete admission that he can have no basis whatever for disputing the deduction that the internal agencies must be placed in the first rank; be it remembered, also, that his quotation is merely a summing up of the evidence brought forward in the course of the lecture.

What I maintain—and I imagine every unbiassed person, whether scientific or not, if but endowed with a reasonable amount of common sense, will agree with me,—is, that when comparing the relative importance of the two very opposite forces which have combined to model the external surface of our globe, in all times down to the present, the precedence must be given to that agency which is admittedly, not only the primary one, but also the one which actually called the other into existence.

In answer to Mr. Green's similes, and as he says that, in a case like this "it is hard to attach any definite meaning to the idea of rank," I may assist him by simply asking, for example, whether any reasonable person, after contemplating, say St. Paul's Cathedral, one of the most prominent features of our metropolis, could possibly think of ranking the architect who originated it below the stone-mason, or sculptor who, directed by him, afterwards altered so vastly the external appearance of the rough shell of the edifice as it first rose from the ground.

Every one, whether geologist or not, admits that the most striking features in the world's physical geography are the mountain ranges which rise up and look down upon the plains below, formed from their own *débris*; and when it is remembered that these mountains are but so many grand proofs of the intensity and activity of those internal forces which not only elevated them, but, in most instances also, even supplied the larger portion of the rock substance which builds up their entire masses, does it not seem strange that any question of rank should arise, when comparing them with those external forces (denudation, &c.) which, of very necessity, can only be great in proportion as the anterior internal forces developed the conditions necessary for calling them into existence, and provided materials for them to operate upon?

DAVID FORBES

11, York Place, Portman Sq., W., Sept. 12th, 1870

The Meteor of 15th August

IN NATURE, page 357, you give notices and sketches of the Meteor of 15th August, and invite further description.

The particulars stated by the Duke of Argyll, and specially the very exact location assigned to the Meteor by your Portrush correspondent, render it probable that a tolerable approximation may be made to the exact position as regards height, &c.

To this end, and in supplement of notice accompanying diagram second, NATURE, page 357, the following details are important.

The Meteor's apparent position is recovered by means of its very close proximity to summits of the Binn hill, near and to the north of this place, with assistance of the six-inch Ordnance maps.

Local Time of first appearance	8·50	8·55 P.M. 15 Aug.
Altitude, uncorrected for refraction,	7°35′25″	8°31′30″
Azimuth, west of north,	66°2′3	67°23′10″

Observer's position, exactly in line of High-water level, In Latitude 56°3′51″ North, Longitude 3°12′45″ West.

The apparent size of the Meteor partly explains these pairs of limits, within which the nucleus at least may be located.

GEO. J. P. GRIEVE

Kirkbank, Burntisland, Sept. 12th

GEOLOGICAL DISCOVERY IN LIVERPOOL

ALL geologists visiting Liverpool for the approaching Meeting of the British Association for the advancement of Science will be glad to hear of the recent discovery of some new beds in the coal measures near Liverpool, which are exceedingly prolific in fossil remains. The new line of railway between Liverpool and St. Helen's runs at one place not far from Rainhill, through Thatto Heath, where a long cutting was rendered necessary. This cutting is through beds all belonging to the carboniferous strata—a thick coal seam, and the accompanying beds of shale and fireclay being all cut through. The fireclay contains abundant remains of all the varied plants of this epoch, with a few marine shells, *Anthrocosia* in some places. There is a large collection of these fossils now on exhibition in the Derby Museum, collected by the Rev. H. H. Higgins, who has shown great energy in the matter, and who has most generously presented his collection to the museum. This collection well merits the attention of all geologists, especially those interested in fossil botany. The plants found are in the most perfect state of preservation, and they are by no means difficult to meet with. They are chiefly *Sigillaria*, *Calamites*, *Lepidodendron*, *Neuropteris Loshii*, *N. nervosa*, and *N. gigantea*: some of the species, the rarer *Asterophyllites* and *Sphenophyllum*, together with many others, being not yet accurately determined. Throughout the clay are found scattered nodules of ironstone, which, on being broken up, are found to contain fossil remains, generally ferns or *Equiseta*. There have, however, been two exceedingly valuable finds lately made, two fine specimens of the wings of Neuropterous insects having come to light. One of these is in the possession of the Rev. H. H. Higgins; and the other, measuring some 3 to 3½ in. in length, is in the possession of Mr. E. Cleminshaw, of Merton College, Oxford. Both of these will be exhibited at the approaching meeting. They were both found in the ironstone nodules, and are very interesting, as only one other specimen, we believe, is known from the English coal measures. Not far from this cutting is a small coal pit, from which many interesting fossils are to be obtained. These are chiefly fish-remains, teeth, jaws, scales and bones, and a few rare ferns. The ease with which the blocks split into thin laminæ render these fossils easily found, and they are in a good state of preservation.

This locality is easily reached by train from Lime Street Station to Rainhill, from which place it is distant some twenty minutes' walk. The navvies on the line are very obliging, and have all the finer specimens, with which they are very willing to part for a few coppers or some tobacco.

J. P. EARWAKER

NOTES

We have reason to believe that the scheme for the proposed Indian Government School of Engineering is being warmly combated by the scientific branches of the army, whose counter proposal is that the Royal Military Academy at Woolwich should be made into an equivalent of the *Ecole Polytechnique*, and that young men intended for the engineering service of the Indian Government should be educated there, or better still, officers of the Royal Engineeers should be lent to the Indian Government. On this very important topic we would ask whether the result of the first report of the Royal Commission on Military Education, which is still sitting, has not been to reduce both in quantity and quality, the scientific teaching at Woolwich, which was already so absurdly small, that, mathematics apart, more science might be learned at many commercial academies.

It will be a long time before England forgets the loss of the *Captain*, though probably when a serious naval engagement does come, the going down of the largest ships, with the loss of all men on board, will be almost a matter of course. But as we have so often heard lately, *à la guerre comme à la guerre*, perhaps a more subdued thrill will then run through us. There seems little doubt that the *Captain* was top-heavy. The fearful rapidity with which, first a roll of 10 deg., then of 22 deg., then of 25 deg., and then a terrible catastrophe were recorded, leave no doubt on this point, and the turrets, the six twenty-five-ton guns, heavy armour, and masts high above the centre of gravity, must bear the blame. Fortunately, however, Captain Moncrieff's last achievement, which we described a few weeks ago, comes to our assistance by abolishing the turrets, and puts the weight of the enormous guns precisely where it is wanted.

The balloon is becoming a war engine with a vengeance. M. Nadar has been appointed a member of the Committee under the presidency of M. Berthelot, for the scientific defence of Paris, and several captive balloons are to be employed ; while we learn from Strasburg that captive balloons are to be used for dropping nitro-glycerine bombs on the powder magazines of the town ; the operations are to be conducted by an Englishman named Walter, and Herr Mahler, of Berlin.

The annual excursion of the London and Middlesex Archæological Society took place on Tuesday last. Notwithstanding the inclemency of the weather there was a numerous attendance.

The Warwickshire Agricultural Society's annual gathering was held on Tuesday last at Leamington. The Earl of Warwick presided at the annual dinner, which was attended by the representatives of both divisions of the county. As usual at these agricultural meetings, we fail to discover anything in the proceedings calculated to convey any impression to the agricultural mind of the necessary intimate connection between agriculture and science.

A leading article in the *British Medical Journal* for Sept. 10 is very severe on the insufficiency of the education at present given to our medical students, and the consequent inferiority, comparatively speaking, of the medical profession in this country. "The miserable inferiority in scientific research ; the dearth of original work, the want of exactness, the poverty of physiological investigation, the ignorant impatience of practical detail which we all have to deplore so much in the mass of professional work at this day, are due to the inadequate preliminary cultivation of our students ; to their defective training in scientific method ; the small base on which the pyramid of medical lore is made to stand. The solemn deprecation of excessive devotion to microscopical research ; the empty sneer at chemical physic ; the idle and mischievous disregard of instruments of precision—the sphygmograph, the thermometer, the laryngoscope, the ophthalmoscope—are all the expressions of a Philistine ignorance. . . The inferiority of English to German medicine is due to this inferiority of preliminary training." Judging from the letters we have received, medical men here are very indignant at the strictures on English medical training contained in the two articles by Professor Stricker, recently printed in our columns. This is what one of their own journals says on the subject. Here is food for thought for the members of our Science Commission during the recess.

The editor of the *Gardeners' Magazine* offers a prize of twenty guineas for the best essay on irrigation as applied both to the farm and the garden. There is probably no question of greater material importance to England at the present time. A system of storage and irrigation by which the superfluous rain that falls in autumn and winter, frequently carrying devastation in its course, could be intercepted, stored, and applied to the fertilisation of the soil between May and August, would ensure to the farmer, and consequently to the country, a gain that would be simply incalculable in such summers as the one we have just experienced.

The Museum of Natural History in Madison University, New York, has had its valuable collection lately classified and arranged. Among its most valuable possessions are reckoned the collection of tropical and other rare birds collected by Prof. Bickmore ; a group of gay beautiful plumaged birds brought from the Spice Islands, are especially noticeable. There is also a good collection of North American birds.

Mr. J. J. Bennett, the Curator of the Botanical Department of the British Museum, has just issued his annual report for 1869. The principal business done in the department during the year has been :—The rearrangement of a portion of the presses of the general herbarium ; the rearrangement of certain orders of *Apetalæ* and *Endogens*, and of the lichens, both British and foreign, with numerous additions to each ; the selection of a very large number of specimens from the herbarium of the late Mr. N. B. Ward, and from the collection of Abyssinian plants send by Dr. Schimper through the Foreign Office ; the naming, arranging, and laying into the general herbarium of Berlandier's Mexican collection, of Linden's collection from New Granada, Tate's from Nicaragua, Coulter's from California, Sartwell's *Carices* of North America, Wright's collections from the Neilgherry Hills and from India generally, Jameson's from the Andes of Quito, *Orchideæ* from different countries, ferns from the islands of the South Pacific, and of a large number of miscellaneous specimens of various families and from different countries ; the examination and arrangement of the recent and fossil *Coniferæ* and *Cycadeæ*, and of Mr. Brown's collection of fossil woods ; the arrangement and incorporation in the general herbarium of a large number of European plants ; the rearrangement of various portions of the British herbarium, and of the collection of fruits and seeds ; and the rearrangement of various parts of the collection contained in the Exhibition Rooms, and especially of the cases containing *Coniferæ* and *Cacteæ*, with large additions. The most important additions to the collection during the year have been :—Upwards of 1,000 European plants from the collection of Dr. Rostan and the late Mr. N. B. Ward ; 900 plants of Ingermannland ; 300 from Sicily ; 200 European fungi ; 200 Italian cryptogams ; 3,000 plants of Abyssinia, collected by Dr. Schimper ; more than 3,000 plants of South Africa, from Mr. Ward's collection ; upwards of 500 from Madeira, collected by Lemann and others ; nearly 1,000 from the mountains of Altai ; 1,000 from India, collected by Dr. Wright ; 400 from Malacca, collected by Griffith ; 100 from the Feejee Islands, collected by Harvey ; 1,000 from North America ; 400 fungi of South Carolina ; 300 plants from Nicaragua, collected by Tate ; 700

from the Andes of Quito; a fine set of pine cones from California; numerous specimens of plants and fruits, chiefly from Africa. The number of visits paid to the herbarium during the year for purposes of scientific research was 974.

WE have already signs of the opening of the medical session on the 1st of October next. The number of the *Chemical News* for Sept. 9th is devoted to a very useful summary of the requirements of the various examining bodies in this country in chemistry and physics, and of the courses of lectures and laboratory instruction given at the different colleges and medical schools in London and the provinces. The *British Medical Journal* for Sept. 10th also gives the regulations of the General Medical Council, and Medical licensing bodies, and notes concerning the hospitals and medical schools.

WITH reference to the paragraph in our last number respecting the use of ammonia in Victoria as a cure for snake-bite, we learn that the inhabitants of that colony are so deeply impressed with the great practical value of the discovery, that they are collecting subscriptions for a fitting testimonial to present to Dr Halford, who was the first to suggest and carry out this mode of treatment. Any contributions for this object will be received by Dr. G. E. Day, of Furzewell House, Torquay, who states that it has been the means of saving a number of cases in an apparently hopeless state of collapse.

SOME important experiments are now been conducted with the new description of torpedo submitted to the Government by Mr. Whitehead. The *Oberon* yesterday floated out of dock at Chatham, and will be sent round to Shoeburyness, where the experiments, which are to be carried out under the superintendence of a commission, of which Lieutenant-Colonel Nugent, Royal Engineers, is the president, are to take place. The wooden vessel to be operated upon with the torpedoes, which are fired from the stem of the *Oberon* beneath the water, is *L'Aigle*, which has been placed at the disposal of the committee by the Admiralty for that purpose. These torpedoes are *locomotive*, the motive power being compressed air, and already the results obtained are surprising. The minutes of evidence taken before the select committee on the Abyssinian expedition are published this morning. They fill a blue-book of 600 pages.

A BOTTLE-NOSED whale, measuring eighteen feet in length by eight feet in girth, came ashore near Burntisland, on the Scotch coast, on Thursday afternoon last. It was drawn up on to the beach while still alive. It is to be hoped that some museum will see about securing the skeleton of this whale; it would be a very welcome addition to many museums, and could, we should think, be obtained for a small cost.

THE extreme rarity of well-authenticated examples of the parasitism of the mistletoe on the oak has induced Dr. Bull, of Hereford, to collect the known instances, which he finds to be eight in number, viz., three in Herefordshire, and one each in Gloucestershire, Monmouthshire, Devonshire, Hants, and Surrey. In the most recently-discovered instance, in the Forest of Deerfold in Herefordshire, the mistletoe was found on an oak of the variety *sessiliflora*, some fifty or sixty years old; it is a female plant, growing high up on the main stem, and forming a large spreading branch with a diameter of three-and-a-half feet, and springing from the oak in a single stem nearly four inches in circumference. The mistletoe also grows on a thorn close by, and has probably sprung from a seed dropped by a bird from above.

A NEW process for making steel has been discovered in America, by means of which it is stated that American steel can be made equal to that usually imported into that country. It is manufactured from a peculiar iron ore found in Codorus township, Pennsylvania. The iron is mixed in a reverberatory furnace with middling pig-iron in the proportion of one to six. It is hoped by means of this valuable discovery to manufacture steel rails at a cost of about sixty dollars per ton.

THE BRITISH ASSOCIATION.—LIVERPOOL MEETING, 1870

AS we go to press the great annual scientific meeting has already commenced; and although the President is at the present moment actually delivering his opening address, we are able, through Prof. Huxley's kindness, to give our readers a verbatim report. We believe it will be found to rank in interest and importance along with any of its predecessors. We are also able to give Prof. Roscoe's Address to Section B; the Kew Report lack of space compels us to defer till next week. We have already given the particulars of the places of meeting and officers of the various sections. Not much remains to be added: by the time this is in the hands of our readers the meeting will be in full swing, and those who are attending it will already be at home in all the various arrangements. Among the most interesting occasions will doubtless be Sir John Lubbock's lecture to working men. The Mayor's reception at the Town Hall, continued for two successive evenings, though not open to all who show the ticket of the Association, is virtually so. All who have arrived in time will receive a formal invitation; and any omission, if such occur, will rest with those who should promptly send forward the names. Another entertainment is that to be given in the Philharmonic Hall on Saturday evening, the 17th, by Dr. E. R. Bickersteth. Besides giving a subscription on the largest scale to the local fund, he will entertain about 700 strangers and 300 of our own townspeople. Eight excursions have been arranged for Thursday, September 22nd, in connection with the Association. The first of these is to Cefn Hall, near St. Asaph, where the party will be received at a luncheon by Mrs. Williams Wynne; the excursionists will start from the George's Landing Stage by the railway boat. An excursion party will also leave for Chester by the same boat. An excursion party to Crewe Works will leave the Lime Street Station by an early train. The guests, whose number is limited to one hundred, are invited to a luncheon at Crewe, provided by the London and North Western Railway Company. A fourth excursion will be to Llandudno, and will start from the Prince's Landing Stage in the *Eblana*, kindly lent for the occasion by the City of Dublin Steam Packet Company. Dinner and tea will be provided on board the vessel at hours most convenient to the excursionists. Another party will leave the George's Landing Stage for Llangollen, and have luncheon at the Hand Hotel, Llangollen. A sixth excursion will visit Widnes, where there will be a dinner in the public hall, by invitation of the Widnes committee of reception. There will likewise be an excursion to Wigan, and an excursion up and down the River Mersey. In Liverpool many of the chief works, manufactories, and public institutions will be open to the inspection of the members of the Association all through the week. Among the papers intended to be read, the titles of which have already reached us, the following are among the most interesting:—In Section A, Francis Galton, F.R.S., "Barometric Predictions of Coming Weather."—John J. Hall, "A new Electro-magnetic Electrometer."—A. W. Bickerton, "A new Heat Engine."—W. Rowett, "Ocean Telegraphy."—Henry Hudson, Glenville, Fermoy, Ireland, "On the Wave Theories of Light."—Dr. Joseph Henry, Smithsonian Institution, Washington, U.S.A., who will be present at the meeting, "On the Rainfall of the United States."—R. S. Ball, Royal College of Science, Dublin, "The small Oscillations of a Rigid Body."—S. Hewett, Marlborough, Wilts, "The Earth's Centre of Gravity, Axis of Revolution, and Magnetic Axis or Centre."—W. M. Watts, "The Existence of two Spectra produced by Carbon incandescent at the same Temperature."—In Section B, C. R. Tichborne, F.C.S., "On the Action of Street Dust as a Ferment."—W. H. Perkin, "On Artificial Alizarine."—A. H. Church, "Experiments on the Preservation of Stone," "Contributions to Mineralogical Chemistry."—John G. Macvicar, "On the Structure and Form of an Atom of Moisture" (illustrated by models).—J. H. Lloyd, M.D., Anglesea, "On the Dry System of Sewage."—In Section C, J. Logan Lobley, "On the Stratigraphical Distribution of the British Fossil Gasteropoda."—W. C. Williamson, "On the Organisation and Affinities of the Calamities of the Coal Measures."—G. A. Laborn, "On the Tertiary Coal-field of Southern Chili."—Charles Ricketts, "On a Railway Section across the Prescot Coal-field."—John W. Judd, "On the Age of the Wealden."—Geo. Busk, a paper by Dr. Leith Adams, "On a New Species of Fossil Elephants from Malta."—Charles Jeaks, "On the Norwich Crag."

ADDRESS OF THOMAS HENRY HUXLEY, LL.D., F.R.S., PRESIDENT.

MY LORDS, LADIES, AND GENTLEMEN,—It has long been the custom for the newly installed President of the British Association for the Advancement of Science to take advantage of the elevation of the position in which the suffrages of his colleagues had, for the time, placed him, and, casting his eyes around the horizon of the scientific world, to report to them what could be seen from his watch-tower; in what directions the multitudinous divisions of the noble army of the improvers of natural knowledge were marching; what important strongholds of the great enemy of us all, ignorance, had been recently captured; and, also, with due impartiality, to mark where the advanced posts of science had been driven in, or a long-continued siege had made no progress.

I propose to endeavour to follow this ancient precedent, in a manner suited to the limitations of my knowledge and of my capacity. I shall not presume to attempt a panoramic survey of the world of science, nor even to give a sketch of what is doing in the one great province of biology, with some portions of which my ordinary occupations render me familiar. But I shall endeavour to put before you the history of the rise and progress of a single biological doctrine; and I shall try to give some notion of the fruits, both intellectual and practical, which we owe, directly or indirectly, to the working out, by seven generations of patient and laborious investigators, of the thought which arose, more than two centuries ago, in the mind of a sagacious and observant Italian naturalist.

It is a matter of every-day experience that it is difficult to prevent many articles of food from becoming covered with mould; that fruit, sound enough to all appearance, often contains grubs at the core; that meat, left to itself in the air, is apt to putrefy and swarm with maggots. Even ordinary water, if allowed to stand in an open vessel, sooner or later becomes turbid and full of living matter.

The philosophers of antiquity, interrogated as to the cause of these phenomena, were provided with a ready and a plausible answer. It did not enter their minds even to doubt that these low forms of life were generated in the matters in which they made their appearance. Lucretius, who had drunk deeper of the scientific spirit than any poet of ancient or modern times except Goethe, intends to speak as a philosopher, rather than as a poet, when he writes that "with good reason the earth has gotten the name of mother, since all things are produced out of the earth. And many living creatures, even now, spring out of the earth, taking form by the rains and the heat of the sun." * The axiom of ancient science, "that the corruption of one thing is the birth of another," had its popular embodiment in the notion that a seed dies before the young plant springs from it; a belief so wide spread and so fixed, that Saint Paul appeals to it in one of the most splendid outbursts of his fervid eloquence:—

"Thou fool, that which thou sowest is not quickened, except it die." †

The proposition that life may, and does, proceed from that which has no life, then, was held alike by the philosophers, the poets, and the people, of the most enlightened nations, eighteen hundred years ago; and it remained the accepted doctrine of learned and unlearned Europe, through the middle ages, down even to the seventeenth century.

It is commonly counted among the many merits of our great countryman, Harvey, that he was the first to declare the opposition of fact to venerable authority in this, as in other matters; but I can discover no justification for this wide-spread notion.

* It is thus that Mr. Munro renders

"Linquitur, ut merito maternum nomen adepta
Terra sit, e terra quoniam sunt cuncta creata.
Multaque nunc etiam exsistant animalia terris
Imbribus et calido solis concreta vapore."

De Rerum Natura, lib. v. 793-796.

But would not the meaning of the last line be better rendered "Developed in rain-water and in the warm vapours raised by the sun"?

† 1 Corinthians xv. 36.

After careful search through the "Exercitationes de Generatione," the most that appears clear to me is, that Harvey believed all animals and plants to spring from what he terms a "*primordium vegetale*," a phrase which may nowadays be rendered "a vegetative germ;" and this, he says, is "*oviforme*," or "egg-like;" not, he is careful to add, that it necessarily has the shape of an egg, but because it has the constitution and nature of one. That this "*primordium oviforme*" must needs, in all cases, proceed from a living parent is nowhere expressly maintained by Harvey, though such an opinion may be thought to be implied in one or two passages; while, on the other hand, he does, more than once, use language which is consistent only with a full belief in spontaneous or equivocal generation.* In fact, the main concern of Harvey's wonderful little treatise is not with generation, in the physiological sense, at all, but with development; and his great object is the establishment of the doctrine of epigenesis.

The first distinct enunciation of the hypothesis that all living matter has sprung from pre-existing living matter, came from a contemporary, though a junior, of Harvey, a native of that country, fertile in men great in all departments of human activity, which was to intellectual Europe, in the sixteenth and seventeenth centuries, what Germany is in the nineteenth. It was in Italy, and from Italian teachers, that Harvey received the most important part of his scientific education. And it was a student trained in the same schools, Francesco Redi—a man of the widest knowledge and most versatile abilities, distinguished alike as scholar, poet, physician, and naturalist—who, just two hundred and two years ago, published his "Esperienze intorno alla Generazione degl' Insetti," and gave to the world the idea, the growth of which it is my purpose to trace. Redi's book went through five editions in twenty years; and the extreme simplicity of his experiments, and the clearness of his arguments, gained for his views, and for their consequences, almost universal acceptance.

Redi did not trouble himself much with speculative considerations, but attacked particular cases of what was supposed to be "spontaneous generation" experimentally. Here are dead animals, or pieces of meat, says he; I expose them to the air in hot weather, and in a few days they swarm with maggots. You tell me that these are generated in the dead flesh; but if I put similar bodies, while quite fresh, into a jar, and tie some fine gauze over the top of the jar, not a maggot makes its appearance, while the dead substances, nevertheless, putrefy just in the same way as before. It is obvious, therefore, that the maggots are not generated by the corruption of the meat; and that the cause of their formation must be a something which is kept away by gauze. But gauze will not keep away aëriform bodies, or fluids. This something must, therefore, exist in the form of solid particles too big to get through the gauze. Nor is one long left in doubt what these solid particles are; for the blowflies, attracted by the odour of the meat, swarm round the vessel, and, urged by a powerful but in this case misleading instinct, lay eggs out of which maggots are immediately hatched upon the gauze. The conclusion, therefore, is unavoidable; the maggots are not generated by the meat, but the eggs which give rise to them are brought through the air by the flies.

These experiments seem almost childishly simple, and one wonders how it was that no one ever thought of them before. Simple as they are, however, they are worthy of the most careful study, for every piece of experimental work since done, in regard to this subject, has been shaped upon the model furnished by the Italian philosopher. As the results of his experiments were the same, however varied the nature of the materials he used, it is not wonderful that there arose in Redi's mind a presumption, that in all such cases of the seeming production of life from dead matter, the real explanation was the introduction of living germs from without into that dead matter.† And thus the hypothesis

* See the following passage in Exercitatio I.:—"Item *sponte nascentia* dicuntur; non quod ex *putredine* oriunda sint, sed quod casu, naturæ sponte, et æquivocâ (ut aiunt) generatione, a parentibus sui dissimilibus proveniant." Again, in "De Uteri Membranis":—"In cunctorum viventium generatione (sicut diximus) hoc solenne est, ut ortum ducunt a *primordio* aliquo, quod tum materiam tum efficiendi potestatem in se habet: sitque adeo id, ex quo et a quo quicquid nascitur, ortum suum ducat. Tale primordium in animalibus (*sive ab aliis generantibus proveniant, sive sponte, aut ex putredine nascentur*) est humor in tunicâ aliquâ aut putamine conclusus." Compare also what Redi has to say respecting Harvey's opinions, "Esperienze," p. 11.

† "Pure contentandomi sempre in questa ed in ciascuna altro cosa, da ciascuno più savio, là dove io difettuosamente parlassi, esser corretto; non tacero, che per molte osservazioni molti volti da me fatte, mi sento inclinato a credere che la terra, da quelle prime piante, e da quei primi animali in poi, che ella nei primi giorni del mondo produsse per comandemento del sovrano

that living matter always arises by the agency of pre-existing living matter, took definite shape; and had, henceforward, a right to be considered and a claim to be refuted, in each particular case, before the production of living matter in any other way could be admitted by careful reasoners. It will be necessary for me to refer to this hypothesis so frequently, that, to save circumlocution, I shall call it the hypothesis of *Biogenesis;* and I shall term the contrary doctrine—that living matter may be produced by not living matter—the hypothesis of *Abiogenesis.*

In the seventeenth century, as I have said, the latter was the dominant view, sanctioned alike by antiquity and by authority; and it is interesting to observe that Redi did not escape the customary tax upon a discoverer of having to defend himself against the charge of impugning the authority of the Scriptures;* for his adversaries declared that the generation of bees from the carcase of a dead lion is affirmed, in the Book of Judges, to have been the origin of the famous riddle with which Samson perplexed the Philistines:—

> "Out of the eater came forth meat,
> And out of the strong came forth sweetness."

Against all odds, however, Redi, strong with the strength of demonstrable fact, did splendid battle for Biogenesis; but it is remarkable that he held the doctrine in a sense which, if he had lived in these times, would have infallibly caused him to be classed among the defenders of "spontaneous generation." "Omne vivum ex vivo," "no life without antecedent life," aphoristically sums up Redi's doctrine; but he went no further. It is most remarkable evidence of the philosophic caution and impartiality of his mind, that although he had speculatively anticipated the manner in which grubs really are deposited in fruits and in the galls of plants, he deliberately admits that the evidence is insufficient to bear him out; and he therefore prefers the supposition that they are generated by a modification of the living substance of the plants themselves. Indeed, he regards these vegetable growths as organs, by means of which the plant gives rise to an animal, and looks upon this production of specific animals as the final cause of the galls and of at any rate some fruits. And he proposes to explain the occurrence of parasites within the animal body in the same way.†

It is of great importance to apprehend Redi's position rightly; for the lines of thought he laid down for us are those upon which naturalists have been working ever since. Clearly, he held *Biogenesis* as against *Abiogenesis;* and I shall immediately proceed, in the first place, to inquire how far subsequent investigation has borne him out in so doing.

But Redi also thought that there were two modes of Biogenesis. By the one method, which is that of common and ordinary occurrence, the living parent gives rise to offspring which passes through the same cycle of changes as itself—like gives rise to like; and this has been termed *Homogenesis.* By the other mode the living parent was supposed to give rise to offspring which passed through a totally different series of states from those exhibited by the parent, and did not return into the cycle of the parent; this is what ought to be called *Heterogenesis,* the offspring being altogether, and permanently unlike the parent. The term Heterogenesis, however, has unfortunately been used in a different sense, and M. Milne-Edwards has therefore substituted for it *Xenogenesis,* which means the generation of something foreign. After discussing Redi's hypothesis of universal Biogenesis, then, I shall go on to ask how far the growth of science justifies his other hypothesis of Xenogenesis.

The progress of the hypothesis of Biogenesis was triumphant and unchecked for nearly a century. The application of the microscope to anatomy in the hands of Grew, Leeuwenhoek, Swammerdam, Lyonet, Vallisnieri, Reaumur, and other illustrious investigators of nature of that day, displayed such a complexity of organisation in the lowest and minutest forms, and everywhere revealed such a prodigality of provision for their multiplication by germs of one sort or another, that the hypothesis of Abiogenesis began to appear not only untrue, but absurd; and, in the middle of the eighteenth century, when Needham and Buffon took up the question, it was almost universally discredited.*

But the skill of the microscope-makers of the eighteenth century soon reached its limit. A microscope magnifying 400 diameters was a *chef d'œuvre* of the opticians of that day; and at the same time, by no means trustworthy. But a magnifying power of 400 diameters, even when definition reaches the exquisite perfection of our modern achromatic lenses, hardly suffices for the mere discernment of the smallest forms of life. A speck, only $\frac{1}{25}$th of an inch in diameter, has, at 10 inches from the eye, the same apparent size as an object $\frac{1}{10000}$th of an inch in diameter, when magnified 400 times; but forms of living matter abound, the diameter of which is not more than $\frac{1}{100000}$th of an inch. A filtered infusion of hay, allowed to stand for two days, will swarm with living things, among which, any which reaches the diameter of a human red blood-corpuscle, or about $\frac{1}{3200}$th of an inch, is a giant. It is only by bearing these facts in mind, that we can deal fairly with the remarkable statements and speculations put forward by Buffon and Needham in the middle of the eighteenth century.

When a portion of any animal or vegetable body is infused in water, it gradually softens and disintegrates; and, as it does so, the water is found to swarm with minute active creatures, the so-called Infusorial Animalcules, none of which can be seen, except by the aid of the microscope; while a large proportion belong to the category of smallest things of which I have spoken, and which must have all looked like mere dots and lines under the ordinary microscopes of the eighteenth century.

Led by various theoretical considerations which I cannot now discuss, but which looked promising enough in the lights of that day, Buffon and Needham doubted the applicability of Redi's hypothesis to the infusorial animalcules, and Needham very properly endeavoured to put the question to an experimental test.

ed omnipotente Fattore, non abbia mai più prodotto da se medesima nè erba nè albero, nè animale alcuno perfetto o imperfetto che ci se fosse; e che tutto quello, che ne' tempi trapassati è nato e che ora nascere in lei, o da lei veggiamo, venga tutto dalla semenza reale e vera delle piante, e degli animali stessi, i quali col mezzo del proprio seme la loro spezie conservano. E se bene tutto giorno scorghiamo da' cadaveri degli animali, e da tutte quante le maniere deil' erbe, e de' fiori, e dei frutti imputriditi, e corrotti nascere vermi infiniti—

> 'Nonne vides quæcunque mora, fluidoque calore
> Corpora tabescunt in parva animalia verti'—

Io mi sento, dico, inclinato a credere che tutti quei vermi si generino dal seme paterno; e che le carni, e l'erbe, e l'altre cose tutte putrefatte, o putrefattibili non facciano altra parte, nè abbiano altro ufizio nella generazione degl' insetti, se non d'apprestare un luogo o un nido proporzionato, in cui dagli animali nel tempo della figliatura sieno portati, e partoriti i vermi, o l'uova o l'altre semenze dei vermi, i quali tosto che nati sono, trovano in esso nido un sufficiente alimento abilissimo per nutricarsi: e se in quello non son portate dalle madri queste suddette semenze, niente mai, e replicatamente niente, vi s'ingegneri e nasca."—REDI, *Esperienze,* pp. 14-16.

* "Molti, e molti altri ancora vi potrei annoverare, se non fossi chiamato a rispondere alle rampogne di alcuni, che bruscamente mi rammentano ciò, che si legge nel capitolo quattordicesimo del sacrosanto Libro de' giudici. . . . . ."—REDI, *l.c.* p. 45.

† The passage (Esperienze, p. 129) is worth quoting in full:—

"Se dovessi palesarvi il mio sentimento crederei che i frutti, i legumi, gli alberi e le foglie, in due maniere inverminassero. Una, perchè venendo i bachi per di fuora, e cercando l'alimento, col rodere ci aprono la strada, ed arrivano alla più interna midolla de' frutti e de' legni. L'altra maniera si è, che io per me stimerei, che non fosse gran fatto disdicevole il credere, che quell' anima o quella virtù, la quale genera i fiori ed i frutti nelle piante viventi, sia quella stessa che generi ancora i bachi di esse piante. E chi sà orse, che molti frutti degli alberi non sieno prodotti, non per un fine primario e principale, ma bensì per un uffizio secondario e servile, destinato alla generazione di que' vermi, servendo a loro in vece di matrice, in cui dimorino un prefisso e determinato tempo; il quale arrivato escan fuora a godere il sole.

"Io m' immagino, che questo mio pensiero non vi parrà totalmente un paradosso; mentre farete riflessione a quelle tante sorte di galle, di gallozzole, di coccole, di ricci, di calici, di cornetti e di lappole, che son produtte dalle querce, dalle farnie, da' cerri, da' sugheri, da' lecci e da altri simili alberi da ghianda; imperciocchè in quelle gallozzole, e particolarmente nelle più grosse, che si chiamano coronati, ne' ricci capelluti, che ciuffoli da' nostri contadini son detti; nei ricci legnosi del cerro, ne' ricci stellati della quercia, nelle galluzze della foglia del leccio si vede evidentissimamente, che la prima e principale intenzione della natura è formare dentro di quelle un animale volante; vedendosi nel centro della gallozzola un uovo, che col crescere e col maturarsi di essa gallozzola va crescendo e maturando anch' egli, e cresce altresì a suo tempo quel verme, che nell' uovo si racchiude; il qual verme, quando la gallozzola è finita di maturare e che è venuto il termine destinato al suo nascimento, diventa, di verme che era, una mosca. . . . . Io vi confesso ingenuamente, che prima d'aver fatte queste mie esperienze intorno alla generazione degl' insetti mi dava a credere, o per dir meglio sospettava, che forse la gallozzola nascesse, perchè arrivando la mosca nel tempo della primavera, e facendo una piccolissima fessura ne' rami più teneri della quercia, in quella fessura nascondesse uno de suoi semi, il quale fosse cagione che sbocciasse fuora la gallozzola; e che mai non si vedessero galle o gallozzole o ricci o cornetti o calici o coccole, se non in que' rami, ne' quali le mosche avessero depositate le loro semenze; e mi dava ad intendere, che le gallozzole fossero una malattia cagionata nelle querce dalle punture delle mosche, in quella giusa stessa che dalle punture d'altri animaletti simiglievoli veggiamo crescere de' tumori ne' corpi degli animali."

* Needham, writing in 1750, says:—

"Les naturalistes modernes s'accordent unanimement à établir, comme une vérité certaine, que toute plante vient de sa sèmence spécifique, tout animal d'un œuf ou de quelque chose d'analogue préexistant dans la plante, ou dans l'animal de même espèce qui l'a produit."—*Nouvelles Observations,* p. 169.

"Les naturalistes ont généralement cru que les animaux microscopique étaient engendrés par des œufs transportés dans l'air, ou déposés dans des eaux dormantes par des insectes volans."—*Ibid.* p. 176.

He said to himself, if these infusorial animalcules come from germs, their germs must exist either in the substance infused, or in the water with which the infusion is made, or in the superjacent air. Now the vitality of all germs is destroyed by heat. Therefore, if I boil the infusion, cork it up carefully, cementing the cork over with mastic, and then heat the whole vessel by heaping hot ashes over it, I must needs kill whatever germs are present. Consequently, if Redi's hypothesis hold good, when the infusion is taken away and allowed to cool, no animalcules ought to be developed in it; whereas, if the animalcules are not dependent on pre-existing germs, but are generated from the infused substance, they ought, by-and-by, to make their appearance. Needham found that, under the circumstances in which he made his experiments, animalcules always did arise in the infusions, when a sufficient time had elapsed to allow for their development.

In much of his work Needham was associated with Buffon, and the results of their experiments fitted in admirably with the great French naturalist's hypothesis of "organic molecules," according to which, life is the indefeasible property of certain indestructible molecules of matter, which exist in all living things, and have inherent activities by which they are distinguished from not living matter. Each individual living organism is formed by their temporary combination. They stand to it in the relation of the particles of water to a cascade, or a whirlpool; or to a mould, into which the water is poured. The form of the organism is thus determined by the reaction between external conditions and the inherent activities of the organic molecules of which it is composed; and, as the stoppage of a whirlpool destroys nothing but a form, and leaves the molecules of the water, with all their inherent activities intact, so what we call the death and putrefaction of an animal, or of a plant, is merely the breaking up of the form, or manner of association, of its constituent organic molecules, which are then set free as infusorial animalcules.

It will be perceived that this doctrine is by no means identical with *Abiogenesis*, with which it is often confounded. On this hypothesis, a piece of beef, or a handful of hay, is dead only in a limited sense. The beef is dead ox, and the hay is dead grass; but the "organic molecules" of the beef or the hay are not dead, but are ready to manifest their vitality as soon as the bovine or herbaceous shrouds in which they are imprisoned are rent by the macerating action of water. The hypothesis therefore must be classified under Xenogenesis, rather than under Abiogenesis. Such as it was, I think it will appear, to those who will be just enough to remember that it was propounded before the birth of modern chemistry, and of the modern optical arts, to be a most ingenious and suggestive speculation.

But the great tragedy of Science—the slaying of a beautiful hypothesis by an ugly fact—which is so constantly being enacted under the eyes of philosophers, was played, almost immediately, for the benefit of Buffon and Needham.

Once more, an Italian, the Abbé Spallanzani, a worthy successor and representative of Redi in his acuteness, his ingenuity, and his learning, subjected the experiments and the conclusions of Needham to a searching criticism. It might be true that Needham's experiments yielded results such as he had described, but did they bear out his arguments? Was it not possible, in the first place, that he had not completely excluded the air by his corks and mastic? And was it not possible, in the second place, that he had not sufficiently heated his infusions and the superjacent air? Spallanzani joined issue with the English naturalist on both these pleas, and he showed that if, in the first place, the glass vessels in which the infusions were contained were hermetically sealed by fusing their necks, and if, in the second place, they were exposed to the temperature of boiling water for three-quarters of an hour,* no animalcules ever made their appearance within them. It must be admitted that the experiments and arguments of Spallanzani furnish a complete and a crushing reply to those of Needham. But we all too often forget that it is one thing to refute a proposition, and another to prove the truth of a doctrine which, implicitly or explicitly, contradicts that proposition, and the advance of science soon showed that though Needham might be quite wrong, it did not follow that Spallanzani was quite right.

Modern chemistry, the birth of the latter half of the eighteenth century, grew apace, and soon found herself face to face with the great problems which biology had vainly tried to attack without her help. The discovery of oxygen led to the laying of the foundations of a scientific theory of respiration, and to an examination of the marvellous interactions of organic substances with oxygen. The presence of free oxygen appeared to be one of the conditions of the existence of life, and of those singular changes in organic matters which are known as fermentation and putrefaction. The question of the generation of the infusory animalcules thus passed into a new phase. For what might not have happened to the organic matter of the infusions, or to the oxygen of the air, in Spallanzani's experiments? What security was there that the development of life which ought to have taken place had not been checked or prevented by these changes?

The battle had to be fought again. It was needful to repeat the experiments under conditions which would make sure that neither the oxygen of the air, nor the composition of the organic matter, was altered in such a matter as to interfere with the existence of life.

Schulze and Schwann took up the question from this point of view in 1836 and 1837. The passage of air through red-hot glass tubes, or through strong sulphuric acid, does not alter the proportion of its oxygen, while it must needs arrest or destroy any organic matter which may be contained in the air. These experimenters, therefore, contrived arrangements by which the only air which should come into contact with a boiled infusion should be such as had either passed through red-hot tubes or through strong sulphuric acid. The result which they obtained was that an infusion so treated developed no living things, while if the same infusion was afterwards exposed to the air such things appeared rapidly and abundantly. The accuracy of these experiments has been alternately denied and affirmed. Supposing them to be accepted, however, all that they really proved was that the treatment to which the air was subjected destroyed *something* that was essential to the development of life in the infusion. This "something" might be gaseous, fluid, or solid; that it consisted of germs remained only an hypothesis of greater or less probability.

Contemporaneously with these investigations a remarkable discovery was made by Cagniard de la Tour. He found that common yeast is composed of a vast accumulation of minute plants. The fermentation of must or of wort in the fabrication of wine and of beer is always accompanied by the rapid growth and multiplication of these *Torulæ*. Thus fermentation, in so far as it was accompanied by the development of microscopical organisms in enormous numbers, became assimilated to the decomposition of an infusion of ordinary animal or vegetable matter; and it was an obvious suggestion that the organisms were, in some way or other, the causes both of fermentation and of putrefaction. The chemists, with Berzelius and Liebig at their head, at first laughed this idea to scorn; but in 1843, a man then very young, who has since performed the unexampled feat of attaining to high eminence alike in Mathematics, Physics, and Physiology—I speak of the illustrious Helmholtz—reduced the matter to the test of experiment by a method alike elegant and conclusive. Helmholtz separated a putrefying or a fermenting liquid from one which was simply putrescible or fermentable by a membrane which allowed the fluids to pass through and become intermixed, but stopped the passage of solids. The result was, that while the putrescible or the fermentable liquids became impregnated with the results of the putrescence or fermentation which was going on on the other side of the membrane, they neither putrefied (in the ordinary way) nor fermented; nor were any of the organisms which abounded in the fermenting or putrefying liquid generated in them. Therefore the cause of the development of these organisms must lie in something which cannot pass through membranes; and as Helmholtz's investigations were long antecedent to Graham's researches upon colloids, his natural conclusion was that the agent thus intercepted must be a solid material. In point of fact, Helmholtz's experiments narrowed the issue to this: that which excites fermentation and putrefaction, and at the same time gives rise to living forms in a fermentable or putrescible fluid, is not a gas and is not a diffusible fluid; therefore it is either a colloid, or it is matter divided into very minute solid particles.

The researches of Schroeder and Dusch in 1854, and of Schroeder alone, in 1859, cleared up this point by experiments which are simply refinements upon those of Redi. A lump of cotton-wool is, physically speaking, a pile of many thicknesses of a very fine gauze, the fineness of the meshes of which depends upon the closeness of the compression of the wool. Now, Schroeder and Dusch found, that, in the case of all the putrefiable materials which they used (except milk and yolk of egg), an infusion boiled, and then allowed to come into contact with no air but such as had been filtered through cotton-wool, neither putrefied nor fermented, nor developed living forms. It

* See Spallanzani, "Opere," vi. pp. 42 and 51.

is hard to imagine what the fine sieve formed by the cotton-wool could have stopped except minute solid particles. Still the evidence was incomplete until it had been positively shown, first, that ordinary air does contain such particles; and, secondly, that filtration through cotton-wool arrests these particles and allows only physically pure air to pass. This demonstration has been furnished within the last year by the remarkable experiments of Professor Tyndall. It has been a common objection of Abiogenists that, if the doctrine of Biogeny is true, the air must be thick with germs; and they regard this as the height of absurdity. But Nature occasionally is exceedingly unreasonable, and Professor Tyndall has proved that this particular absurdity may nevertheless be a reality. He has demonstrated that ordinary air is no better than a sort of stir-about of excessively minute solid particles; that these particles are almost wholly destructible by heat; and that they are strained off, and the air rendered optically pure by being passed through cotton-wool.

But it remains yet in the order of logic, though not of history, to show that among these solid destructible particles there really do exist germs capable of giving rise to the development of living forms in suitable menstrua. This piece of work was done by M. Pasteur in those beautiful researches which will ever render his name famous; and which, in spite of all attacks upon them, appear to me now, as they did seven years ago,* to be models of accurate experimentation and logical reasoning. He strained air through cotton-wool, and found, as Schroeder and Dusch had done, that it contained nothing competent to give rise to the development of life in fluids highly fitted for that purpose. But the important further links in the chain of evidence added by Pasteur are three. In the first place he subjected to microscopic examination the cotton-wool which had served as strainer, and found that sundry bodies clearly recognizable as germs, were among the solid particles strained off. Secondly, he proved that these germs were competent to give rise to living forms by simply sowing them in a solution fitted for their development. And, thirdly, he showed that the incapacity of air strained through cotton-wool to give rise to life, was not due to any occult change effected in constituents of the air by the wool, by proving that the cotton-wool might be dispensed with altogether, and perfectly free access left between the exterior air and that in the experimental flask. If the neck of the flask is drawn out into a tube and bent downwards; and if, after the contained fluid has been carefully boiled, the tube is heated sufficiently to destroy any germs which may be present in the air which enters as the fluid cools, the apparatus may be left to itself for any time and no life will appear in the fluid. The reason is plain. Although there is free communication between the atmosphere laden with germs and the germless air in the flask, contact between the two takes place only in the tube; and as the germs cannot fall upwards, and there are no currents, they never reach the interior of the flask. But if the tube be broken short off where it proceeds from the flask, and free access be thus given to germs falling vertically out of the air, the fluid which has remained clear and desert for months, becomes, in a few days turbid and full of life.

These experiments have been repeated over and over again by independent observers with entire success; and there is one very simple mode of seeing the facts for oneself, which I may as well describe.

Prepare a solution (much used by M. Pasteur, and often called "Pasteur's solution") composed of water with tartrate of ammonia, sugar, and yeast-ash dissolved therein.† Divide it into three portions in as many flasks; boil all three for a quarter of an hour; and, while the steam is passing out, stop the neck of one with a large plug of cotton-wool, so that this also may be thoroughly steamed. Now set the flasks aside to cool, and when their contents are cold, add to one of the open ones a drop of filtered infusion of hay which has stood for twenty-four hours, and is consequently full of the active and excessively minute organisms known as *Bacteria*. In a couple of days of ordinary warm weather the contents of this flask will be milky from the enormous multiplication of *Bacteria*. The other flask, open and exposed to the air, will, sooner or later, become milky with *Bacteria*, and patches of mould may appear in it; while the liquid in the flask, the neck of which is plugged with cotton-wool, will remain clear for an indefinite time. I have sought in vain for any explanation of these facts, except the obvious one, that the air contains germs competent to give rise to *Bacteria*, such as those with which the first solution has been knowingly and purposely inoculated, and to the mould-*Fungi*. And I have not yet been able to meet with any advocate of Abiogenesis who seriously maintains that the atoms of sugar, tartrate of ammonia, yeast-ash, and water, under no influence but that of free access of air and the ordinary temperature, rearrange themselves and give rise to the protoplasm of *Bacterium*. But the alternative is to admit that these *Bacteria* arise from germs in the air; and if they are thus propagated, the burden of proof that other like forms are generated in a different manner, must rest with the assertor of that proposition.

To sum up the effect of this long chain of evidence:—

It is demonstrable that a fluid eminently fit for the development of the lowest forms of life, but which contains neither germs, nor any protein compound, gives rise to living things in great abundance if it is exposed to ordinary air, while no such development takes place if the air with which it is in contact is mechanically freed from the solid particles which ordinarily float in it and which may be made visible by appropriate means.

It is demonstrable that the great majority of these particles are destructible by heat, and that some of them are germs or living particles capable of giving rise to the same forms of life as those which appear when the fluid is exposed to unpurified air.

It is demonstrable that inoculation of the experimental fluid with a drop of liquid known to contain living particles gives rise to the same phenomena as exposure to unpurified air.

And it is further certain that these living particles are so minute that the assumption of their suspension in ordinary air presents not the slightest difficulty. On the contrary, considering their lightness and the wide diffusion of the organisms which produce them, it is impossible to conceive that they should not be suspended in the atmosphere in myriads.

Thus the evidence, direct and indirect, in favour of *Biogenesis* for all known forms of life must, I think, be admitted to be of great weight.

On the other side the sole assertions worthy of attention are that hermetically sealed fluids, which have been exposed to great and long-continued heat, have sometimes exhibited living forms of low organization when they have been opened.*

The first reply that suggests itself is the probability that there must be some error about these experiments, because they are performed on an enormous scale every day with quite contrary results. Meat, fruits, vegetables, the very materials of the most fermentable and putrescible infusions are preserved to the extent, I suppose I may say, of thousands of tons every year, by a method which is a mere application of Spallanzani's experiment. The matters to be preserved are well boiled in a tin case provided with a small hole, and this hole is soldered up when all the air in the case has been replaced by steam. By this method they may be kept for years without putrefying, fermenting, or getting mouldy. Now this is not because oxygen is excluded, inasmuch as it is now proved that free oxygen is not necessary for either fermentation or putrefaction. It is not because the tins are exhausted of air, for *Vibriones* and *Bacteria* live, as Pasteur has shown, without air or free oxygen. It is not because the boiled meats or vegetables are not putrescible or fermentable, as those who have had the misfortune to be in a ship supplied with unskilfully closed tins well know. What is it, therefore, but the exclusion of germs? I think that Abiogenists are bound to answer this question before they ask us to consider new experiments of precisely the same order.

And in the next place, if the results of the experiments I refer to are really trustworthy, it by no means follows that Abiogenesis has taken place. The resistance of living matter to heat is known to vary within considerable limits, and to depend, to some extent, upon the chemical and physical qualities of the surrounding medium. But if, in the present state of science, the alternative is offered us, either germs can stand a greater heat than has been supposed, or the molecules of dead matter, for no valid or intelligible reason that is assigned, are able to rearrange themselves into living bodies, exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be, even for a moment, doubtful.

But though I cannot express this conviction of mine too strongly, I must carefully guard myself against the supposition

* "Lectures to Working Men on the Causes of the Phenomena of Organic Nature," 1863.

† Infusion of hay treated in the same way yields similar results; but as it contains organic matter the argument which follows cannot be based upon it.

* For a full account of the most recent series of experiments of this description see Dr. H. C. Bastian's paper in NATURE, No. xxxv., p. 170; No. xxxvi., p. 193; and No. xxxvii., p. 219.—ED.

that I intend to suggest that no such thing as Abiogenesis ever has taken place in the past or ever will take place in the future. With organic chemistry, molecular physics, and physiology yet in their infancy, and every day making prodigious strides, I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call "vital" may not, some day, be artificially brought together. All I feel justified in affirming is that I see no reason for believing that the feat has been performed yet.

And looking back through the prodigious vista of the past, I find no record of the commencement of life, and therefore I am devoid of any means of forming a definite conclusion as to the conditions of its appearance. Belief, in the scientific sense of the word, is a serious matter, and needs strong foundations. To say, therefore, in the admitted absence of evidence, that I have any belief as to the mode in which the existing forms of life have originated, would be using words in a wrong sense. But expectation is permissible where belief is not; and if it were given me to look beyond the abyss of geologically recorded time to the still more remote period when the earth was passing through physical and chemical conditions, which it can no more see again than a man can recall his infancy, I should expect to be a witness of the evolution of living protoplasm from not living matter. I should expect to see it appear under forms of great simplicity, endowed, like existing fungi, with the power of determining the formation of new protoplasm from such matters as ammonium carbonates, oxalates and tartrates, alkaline and earthy phosphates, and water, without the aid of light. That is the expectation to which analogical reasoning leads me; but I beg you once more to recollect that I have no right to call my opinion anything but an act of philosophical faith.

So much for the history of the progress of Redi's great doctrine of Biogenesis, which appears to me, with the limitations I have expressed, to be victorious along the whole line at the present day.

As regards the second problem offered to us by Redi, whether Xenogenesis obtains, side by side with Homogenesis; whether, that is, there exist not only the ordinary living things, giving rise to offspring which run through the same cycle as themselves, but also others, producing offspring which are of a totally different character from themselves, the researches of two centuries have led to a different result. That the grubs found in galls are no product of the plants on which the galls grow, but are the result of the introduction of the eggs of insects into the substance of these plants, was made out by Vallisnieri, Reaumur, and others, before the end of the first half of the eighteenth century. The tapeworms, bladderworms, and flukes continued to be a stronghold of the advocates of Xenogenesis for a much longer period. Indeed, it is only within the last thirty years that the splendid patience of Von Siebold, Van Beneden, Leuckart, Küchenmeister, and other helminthologists, has succeeded in tracing every such parasite, often through the strangest wanderings and metamorphoses, to an egg derived from a parent, actually or potentially like itself; and the tendency of inquiries elsewhere has all been in the same direction. A plant may throw off bulbs, but these, sooner or later, give rise to seeds or spores, which develop into the original form. A polype may give rise to Medusæ, or a pluteus to an Echinoderm, but the Medusa and the Echinoderm give rise to eggs which produce polypes or plutei, and they are therefore only stages in the cycle of life of the species.

But if we turn to pathology it offers us some remarkable approximations to true Xenogenesis.

As I have already mentioned, it has been known since the time of Vallisnieri and of Reaumur, that galls in plants, and tumours in cattle, are caused by insects, which lay their eggs in those parts of the animal or vegetable frame of which these morbid structures are outgrowths. Again, it is a matter of familiar experience to everybody that mere pressure on the skin will give rise to a corn. Now the gall, the tumour, and the corn are parts of the living body, which have become, to a certain degree, independent and distinct organisms. Under the influence of certain external conditions, elements of the body, which should have developed in due subordination to its general plan, set up for themselves and apply the nourishment which they receive to their own purposes.

From such innocent productions as corns and warts, there are all gradations to the serious tumours which, by their mere size and the mechanical obstruction they cause, destroy the organism out of which they are developed; while, finally, in those terrible structures known as cancers, the abnormal growth has acquired powers of reproduction and multiplication, and is only morphologically distinguishable from the parasite worm, the life of which is neither more nor less closely bound up with that of the infested organism.

If there were a kind of diseased structure, the histological elements of which were capable of maintaining a separate and independent existence out of the body, it seems to me that the shadowy boundary between morbid growth and Xenogenesis would be effaced. And I am inclined to think that the progress of discovery has almost brought us to this point already. I have been favoured by Mr. Simon with an early copy of the last published of the valuable "Reports on the Public Health," which, in his capacity of their medical officer, he annually presents to the Lords of the Privy Council. The appendix to this report contains an introductory essay "On the Intimate Pathology of Contagion," by Dr. Burdon Sanderson, which is one of the clearest, most comprehensive, and well-reasoned discussions of a great question which has come under my notice for a long time. I refer you to it for details and for the authorities for the statements I am about to make.

You are familiar with what happens in vaccination. A minute cut is made in the skin, and an infinitesimal quantity of vaccine matter is inserted into the wound. Within a certain time a vesicle appears in the place of the wound, and the fluid which distends this vesicle is vaccine matter, in quantity a hundred or a thousandfold that which was originally inserted. Now what has taken place in the course of this operation? Has the vaccine matter, by its irritative property, produced a mere blister, the fluid of which has the same irritative property? Or does the vaccine matter contain living particles, which have grown and multiplied where they have been planted? The observations of M. Chauveau, extended and confirmed by Dr. Sanderson himself, appear to leave no doubt upon this head. Experiments, similar in principle to those of Helmholtz on fermentation and putrefaction, have proved that the active element in the vaccine lymph is non-diffusible, and consists of minute particles not exceeding $\frac{1}{20000}$ of an inch in diameter, which are made visible in the lymph by the microscope. Similar experiments have proved that two of the most destructive of epizootic diseases, sheep-pox and glanders, are also dependent for their existence and their propagation upon extremely small living solid particles, to which the title of *microzymes* is applied. An animal suffering under either of these terrible diseases is a source of infection and contagion to others, for precisely the same reason as a tub of fermenting beer is capable of propagating its fermentation by "infection," or "contagion," to fresh wort. In both cases it is the solid living particles which are efficient; the liquid in which they float, and at the expense of which they live, being altogether passive.

Now arises the question, are these microzymes the results of *Homogenesis*, or of *Xenogenesis*; are they capable, like the *Torulæ* of yeast, of arising only by the development of pre-existing germs; or may they be, like the constituents of a nutgall, the results of a modification and individualisation of the tissues of the body in which they are found, resulting from the operation of certain conditions? Are they parasites in the zoological sense, or are they merely what Virchow has called "heterologous growths"? It is obvious that this question has the most profound importance, whether we look at it from a practical or from a theoretical point of view. A parasite may be stamped out by destroying its germs, but a pathological product can only be annihilated by removing the conditions which give rise to it.

It appears to me that this great problem will have to be solved for each zymotic disease separately, for analogy cuts two ways. I have dwelt upon the analogy of pathological modification, which is in favour of the xenogenetic origin of microzymes; but I must now speak of the equally strong analogies in favour of the origin of such pestiferous particles by the ordinary process of the generation of like from like.

It is, at present, a well-established fact that certain diseases, both of plants and of animals, which have all the characters of contagious and infectious epidemics, are caused by minute organisms. The smut of wheat is a well-known instance of such a disease, and it cannot be doubted that the grape-disease and the potato-disease fall under the same category. Among animals, insects are wonderfully liable to the ravages of contagious and infectious diseases caused by microscopic *Fungi*.

In autumn, it is not uncommon to see flies, motionless upon a window-pane, with a sort of magic circle, in white, drawn round them. On microscopic examination, the magic circle is found to consist of innumerable spores, which have been thrown off in all directions by a minute fungus called *Empusa muscæ*, the spore-forming filaments of which stand out like a pile of velvet from the body of the fly. These spore-forming filaments are connected with others which fill the interior of the fly's body like so much fine wool, having eaten away and destroyed the creature's viscera. This is the full-grown condition of the *Empusa*. If traced back to its earlier stages, in flies which are still active, and to all appearance healthy, it is found to exist in the form of minute corpuscles which float in the blood of the fly. These multiply and lengthen into filaments, at the expense of the fly's substance; and when they have at last killed the patient, they grow out of its body and give off spores. Healthy flies shut up with diseased ones catch this mortal disease and perish like the others. A most competent observer, M. Cohn, who studied the development of the *Empusa* in the fly very carefully, was utterly unable to discover in what manner the smallest germs of the *Empusa* got into the fly. The spores could not be made to give rise to such germs by cultivation; nor were such germs discoverable in the air, or in the food of the fly. It looked exceedingly like a case of Abiogenesis, or, at any rate, of Xenogenesis; and it is only quite recently that the real course of events has been made out. It has been ascertained, that when one of the spores falls upon the body of a fly, it begins to germinate and sends out a process which bores its way through the fly's skin; this, having reached the interior cavities of its body, gives off the minute floating corpuscles which are the earliest stage of the *Empusa*. The disease is "contagious," because a healthy fly coming in contact with a diseased one, from which the spore-bearing filaments protrude, is pretty sure to carry off a spore or two. It is "infectious" because the spores become scattered about all sorts of matter in the neighbourhood of the slain flies.

The silkworm has long been known to be subject to a very fatal and infectious disease called the *Muscardine*. Audouin transmitted it by inoculation. This disease is entirely due to the development of a fungus, *Botrytis Bassiana*, in the body of the caterpillar; and its contagiousness and infectiousness are accounted for in the same way as those of the fly-disease. But of late years a still more serious epizootic has appeared among the silkworms; and I may mention a few facts which will give you some conception of the gravity of the injury which it has inflicted on France alone.

The production of silk has been for centuries an important branch of industry in Southern France, and in the year 1853 it had attained such a magnitude that the annual produce of the French sericulture was estimated to amount to a tenth of that of the whole world, and represented a money-value of 117,000,000 of francs, or nearly five millions sterling. What may be the sum which would represent the money-value of all the industries connected with the working up of the raw silk thus produced is more than I can pretend to estimate. Suffice it to say that the city of Lyons is built upon French silk as much as Manchester was upon American cotton before the civil war.

Silkworms are liable to many diseases; and even before 1853 a peculiar epizootic, frequently accompanied by the appearance of dark spots upon the skin (whence the name of "Pébrine" which it has received), had been noted for its mortality. But in the years following 1853 this malady broke out with such extreme violence, that, in 1858, the silk-crop was reduced to a third of the amount which it had reached in 1853; and, up till within the last year or two, it has never attained half the yield of 1853. This means not only that the great number of people engaged in silk growing are some thirty millions sterling poorer than they might have been; it means not only that high prices have had to be paid for imported silkworm eggs, and that, after investing his money in them, in paying for mulberry-leaves and for attendance, the cultivator has constantly seen his silkworms perish and himself plunged in ruin; but it means that the looms of Lyons have lacked employment, and that for years enforced idleness and misery have been the portion of a vast population which, in former days, was industrious and well to do.

In 1858 the gravity of the situation caused the French Academy of Sciences to appoint Commissioners, of whom a distinguished naturalist, M. de Quatrefages, was one, to inquire into the nature of this disease, and, if possible, to devise some means of staying the plague. In reading the Report* made by M. de Quatrefages in 1859, it is exceedingly interesting to observe that his elaborate study of the Pébrine forced the conviction upon his mind that, in its mode of occurrence and propagation, the disease of the silkworm is, in every respect, comparable to the cholera among mankind. But it differs from the cholera, and so far is a more formidable disease, in being hereditary, and in being, under some circumstances, contagious as well as infectious.

The Italian naturalist, Filippi, discovered in the blood of the silkworms affected by this strange disease a multitude of cylindrical corpuscles, each about $\frac{1}{6000}$ of an inch long. These have been carefully studied by Lebert, and named by him *Panhistophyton*; for the reason that in subjects in which the disease is strongly developed, the corpuscles swarm in every tissue and organ of the body, and even pass into the undeveloped eggs of the female moth. But are these corpuscles causes, or mere concomitants, of the disease? Some naturalists took one view and some another; and it was not until the French Government, alarmed by the continued ravages of the malady, and the inefficiency of the remedies which had been suggested, dispatched M. Pasteur to study it, that the question received its final settlement; at a great sacrifice, not only of the time and peace of mind of that eminent philosopher, but, I regret to have to add, of his health.*

But the sacrifice has not been in vain. It is now certain that this devastating, cholera-like Pébrine is the effect of the growth and multiplication of the *Panhistophyton* in the silkworm. It is contagious and infectious because the corpuscles of the *Panhistophyton* pass away from the bodies of the diseased caterpillars, directly or indirectly, to the alimentary canal of healthy silkworms in their neighbourhood; it is hereditary, because the corpuscles enter into the eggs while they are being formed, and consequently are carried within them when they are laid; and for this reason, also, it presents the very singular peculiarity of being inherited only on the mother's side. There is not a single one of all the apparently capricious and unaccountable phenomena presented by the Pébrine, but has received its explanation from the fact that the disease is the result of the presence of the microscopic organism, *Panhistophyton*.

Such being the facts with respect to the Pébrine, what are the indications as to the method of preventing it? It is obvious that this depends upon the way in which the *Panhistophyton* is generated. If it may be generated by Abiogenesis, or by Xenogenesis, within the silkworm or its moth, the extirpation of the disease must depend upon the prevention of the occurrence of the conditions under which this generation takes place. But if, on the other hand, the *Panhistophyton* is an independent organism, which is no more generated by the silkworm than the mistletoe is generated by the oak or the apple-tree on which it grows, though it may need the silkworm for its development in the same way as the mistletoe needs the tree, then the indications are totally different. The sole thing to be done is to get rid of and keep away the germs of the *Panhistophyton*. As might be imagined, from the course of his previous investigations, M. Pasteur was led to believe that the latter was the right theory; and, guided by that theory, he has devised a method of extirpating the disease, which has proved to be completely successful wherever it has been properly carried out.

There can be no reason, then, for doubting that, among insects, contagious and infectious diseases, of great malignity, are caused by minute organisms which are produced from pre-existing germs, or by homogenesis; and there is no reason, that I know of, for believing that what happens in insects may not take place in the highest animals. Indeed, there is already strong evidence that some diseases of an extremely malignant and fatal character to which man is subject, are as much the work of minute organisms as is the Pébrine. I refer for this evidence to the very striking facts adduced by Professor Lister in his various well-known publications on the antiseptic method of treatment. It seems to me impossible to rise from the perusal of those publications without a strong conviction that the lamentable mortality which so frequently dogs the footsteps of the most skilful operator, and those deadly consequences of wounds and injuries which seem to haunt the very walls of great hospitals, and are, even now, destroying more men than die of bullet or bayonet, are due to the importation of minute organisms into wounds, and their increase and multiplication; and that the surgeon who saves most lives will be he who best works out the practical consequences of the hypothesis of Redi.

* Etudes sur les Maladies Actuelles des Vers à Soie, p. 53.

* In NATURE, No. xxxvi., p. 181, will be found a *résumé*, by Prof. Tyndall, of Pasteur's investigations of the silkworm disease,—ED.

I commenced this Address by asking you to follow me in an attempt to trace the path which has been followed by a scientific idea, in its long and slow progress from the position of a probable hypothesis to that of an established law of nature. Our survey has not taken us into very attractive regions; it has lain, chiefly, in a land flowing with the abominable, and peopled with mere grubs and mouldiness. And it may be imagined with what smiles and shrugs, practical and serious contemporaries of Redi and of Spallanzani may have commented on the waste of their high abilities in toiling at the solution of problems which, though curious enough in themselves, could be of no conceivable utility to mankind.

Nevertheless you will have observed that before we had travelled very far upon our road there appeared, on the right hand and on the left, fields laden with a harvest of golden grain, immediately convertible into those things which the most sordidly practical of men will admit to have value—viz., money and life.

The direct loss to France caused by the Pébrine in seventeen years cannot be estimated at less than fifty millions sterling; and if we add to this what Redi's idea, in Pasteur's hands, has done for the wine-grower and for the vinegar-maker, and try to capitalise its value, we shall find that it will go a long way towards repairing the money losses caused by the frightful and calamitous war of this autumn. And as to the equivalent of Redi's thought in life, how can we over-estimate the value of that knowledge of the nature of epidemic and epizöotic diseases, and consequently of the means of checking, or eradicating, them, the dawn of which has assuredly commenced?

Looking back no further than ten years, it is possible to select three (1863, 1864, and 1869) in which the total number of deaths from scarlet-fever alone amounted to ninety thousand. That is the return of killed, the maimed and disabled being left out of sight. Why, it is to be hoped that the list of killed in the present bloodiest of all wars will not amount to more than this! But the facts which I have placed before you must leave the least sanguine without a doubt that the nature and the causes of this scourge will, one day, be as well understood as those of the Pébrine are now; and that the long-suffered massacre of our innocents will come to an end.

And thus mankind will have one more admonition that "the people perish for lack of knowledge;" and that the alleviation of the miseries, and the promotion of the welfare, of men must be sought, by those who will not lose their pains, in that diligent, patient, loving study of all the multitudinous aspects of Nature, the results of which constitute exact knowledge, or Science. It is the justification and the glory of this great meeting that it is gathered together for no other object than the advancement of the moiety of science which deals with those phenomena of nature which we call physical. May its endeavours be crowned with a full measure of success!

PROFESSOR H. E. ROSCOE'S OPENING ADDRESS TO SECTION B.

GENTLEMEN,—In the midst of the excitement of the horrible war in which the two most scientific nations of the continent are now plunged, and in which even the Professors of Chemistry and their students take a humane part, let us endeavour to turn our thoughts into channels more congenial to the scientific inquirer, and allow me to recount to you, as far as I am able, the peaceful victories which, since our last meeting in Exeter, have been achieved in our special department of chemistry. And here may I remind you of the cosmopolitan character of science, of the fact that it is mainly to the brotherly intercourse of those interested in science, and in its applications to the arts and manufactures in different countries, that we must look as the small but living fire which in the end will surely serve to melt down national animosities, and to render impossible the breaking out of disasters so fatal to the welfare of humanity as that of which we are now unfortunately the spectators.

With regard to the position of chemical science at the present moment, it will not take a careful observer long to see, that in spite of the numerous important and brilliant discoveries of which every year has to boast, we are really but very imperfectly acquainted with the fundamental laws which regulate chemical actions, and that our knowledge of the ultimate constitution of matter upon which those laws are based is but of the most elementary nature. In proof of this, I need only refer to the different opinions expressed by our leading chemists in a discussion which lately took place at the Chemical Society on the subject of the Atomic Theory. The President (Dr. Williamson) delivered a very interesting lecture in which the existence of atoms was treated as "the very life of chemistry." Dr. Frankland, on the other hand, states, that he cannot understand action at a distance between matter separated by a vacuous space, and, although generally granting that the atomic theory explains chemical facts, yet he is not to be considered as a blind believer in the theory, or as unwilling to renounce it if anything better presented itself. Sir B. C. Brodie and Dr. Odling both agree, that the science of chemistry neither requires nor proves the atomic theory; whilst the former points out that the true basis of this science is to be sought in the investigation of the laws of gaseous combination, or the study of the capacity of bodies for heat, rather than in committing ourselves to assertions incapable of proof by chemical means.

Agreeing in the main myself with the opinions of the last chemists, and believing that we must well distinguish between fact and theory, I would remind you that Dalton's discovery of the laws of multiple and reciprocal proportions (I use Dr. Odling's phraseology), as well as the differences in the power of hydrogen replacement in hydrochloric acid water, ammonia, and marsh gas, *are facts*, whilst the explanation upon the assumption of atoms is, as far as chemistry is as yet advanced, *a theory*.

If, however, the existence of atoms cannot be *proved* by chemical phenomena, we must remember that the assumption of the atomic theory explains chemical facts, as the undulatory theory gives a clear view of the phenomena of light; thus, for instance, one of the most important facts and relations of modern chemistry, which it appears difficult if not impossible to explain without the assumption of atoms, is that of Isomerism. How otherwise than by a different arrangement of the single constituent particles are we to account for several distinct substances in which the proportions of carbon, hydrogen, and oxygen are the same? Why, for instance, should 48 parts, by weight, of carbon, 10 of hydrogen, and 16 of oxygen, united together, be capable of existing as three different chemical substances, unless we presuppose a different statical arrangement of the parts by which these differences in the deportment of the whole are rendered possible. If, then, it be true that chemistry cannot give us positive information as to whether matter is infinitely divisible and therefore continuous, or consists of atoms and is discontinuous, we are in some degree assisted in this inquiry by deductions from physical phenomena which have been recently pointed out by the genius of Sir William Thomson. He argues from four different classes of physical phenomena, and comes to the conclusion not only that matter is discontinuous, and therefore that atoms and molecules do exist, but he even attempts to form an idea of the size of these molecules, and he states that in any ordinary liquid, transparent or seemingly opaque solid, the mean distance between the centres of contiguous molecules is less than the 100 millionth, and greater than the 2,000 millionth of a centimetre. Or, to form a conception of this coarse-grainedness, imagine a raindrop, or globe of glass as large as a pea, to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion, the magnified structure would be coarser grained than a heap of small shot, but probably less coarse grained than a heap of cricket balls. There is, however, another class of physical considerations which renders the existence of indivisible particles more than likely. I refer to the mechanical theory of gases, by means of which, thanks to the labours of eminent English and German philosophers, all the physical properties of gases—their equal expansion by heat, the laws of diffusion, the laws of alteration of volume under pressure—can be shown to follow from the simple laws of mechanical motion. This theory, however, presupposes the existence of molecules, and in this direction, again, we find confirmation of the real existence of Dalton's atoms. Indeed, it has been proved that the average velocity with which the particles of oxygen, nitrogen, or common air are continually projected forwards amounts, at the ordinary atmospheric pressure, to 50,000 centimetres per second, whilst the average number of impacts of each of these molecules is 5,000 million per second.

The mention of the molecular motions of gases will recall to the minds of all present the great loss which English science has this year sustained in the death of the discoverer of the laws of gaseous diffusion. Throughout his life Graham's aim was the advancement of our knowledge in the special subject of the molecular properties of gases. With this intent he unceasingly laboured up to the moment of his death, in spite of failing health and pressure of official business, unfolding for posterity some of the most difficult as well as the most interesting secrets of nature in this branch of our science. "What do you think," he writes to Hoffmann, "of metallic hydrogen, a white magnetic metal?" and yet now, through his labours, the fact of the condensation of hydrogen in the solid state by metallic palladium, and to a less extent by other metals, has become familiar to all of us. Then, again, I would remind you of Graham's recent discovery of the occlusion of hydrogen gas in certain specimens of meteoric iron, whilst earth-manufactured iron contains, not hydrogen, but absorbed carbonic oxide gas, proving that the meteorite had probably been thrown out from an atmosphere of incandescent hydrogen, existing under very considerable pressure, and therefore confirming in a remarkable degree the conclusions to which spectrum analysis had previously led us. The position in the ranks of British science left by Graham's death will not be easily filled up. He accomplished to a certain extent for dynamical chemistry what Dalton did for statical chemistry; and it is upon his experimental researches in molecular chemistry that Graham's permanent fame as one of England's greatest chemists, will rest.

As closely connected with the above subjects I have next to mention a most important research by Dr. Andrews, of Belfast, which, marking an era in the history of gases, shows us how our oldest and most cherished notions must give way before the touchstone of experiment. No opinion would appear to have been more firmly established than that of the existence of three separate states or conditions of matter, viz. the solid, the liquid, and the gaseous. A body capable of existing in two or more of these states was thought to pass suddenly from one to the other by absorption or emission of heat, or by alterations of the superincumbent pressure. Dr. Andrews has shown us how false are our views on this fundamental property of matter, for he has proved that a large number of, and probably all, easily condensible gases or vapours possess a critical point of temperature at and above which no increase of pressure can be made to effect a change into what we call the liquid state, the body remaining as a homogeneous fluid. Whilst below this critical temperature certain increase of pressure always effects a separation into two layers of liquid and gaseous matter. Thus with carbonic acid the point of critical temperature is 30°·92 C., and with each given substance this point is a specific one; each vapour exhibiting rapid changes of volume and flickering movements when the temperature or pressure was changed, but showing no separation into two layers. Under these circumstances it is impossible to say that the body exists either in the state of a gas or of a liquid; it appears to be in a condition intermediate between the two. Thus carbonic acid under the pressure of 108 atmospheres, and at 35° 5′C., is reduced to $\frac{1}{430}$ of the volume which it occupies at one atmosphere: it has undergone a regular and unbroken contraction, and it is a uniform fluid. If we now reduce the temperature below 31° C., the liquid condition is assumed without any sudden change of volume or any abrupt evolution of heat. We can scarcely too highly estimate the value of the researches of Andrews.

As examples of the power which modern methods of research give of grappling with questions which only a few years ago were thought to be insoluble, I may quote the beautiful observations, now well known, by which Lockyer determined the rate of motion on the sun's surface, together with those of Frankland and Lockyer respecting the probable pressure acting in the different layers of the solar atmosphere; and, lastly, the results obtained by Zöllner respecting solar physics, and especially the probable absolute temperature of the sun's atmosphere, as well as that of the internal molten mass. These last results are so interesting and remarkable, as being arrived at by the combination of recent spectroscopic observation with high mathematical analysis, that I may perhaps be permitted shortly to state them. Starting from the fact of the eruptive nature of a certain class of solar protuberances, Zöllner thinks that the extraordinary rapidity with which these red flames shoot forth proves that the hydrogen of which they are mainly composed must have burst out from under great pressure; and, if so, the hydrogen must have been confined by a zone or layer of liquid from which it breaks loose. Assuming the existence of such a layer of incandescent liquid, then applying to the problem the principles and methods of the mechanical theory of gases, and placing in his formulæ the data of pressure and rate of motion as observed by Lockyer on the sun's surface, Zöllner arrives at the conclusion that the difference of pressure needed to produce an explosion capable of projecting a prominence to the height of 3·0 minutes above the sun's surface (a height not unfrequently noticed) is 4,070,000 atmospheres. This enormous pressure is attained at a depth of 139 geographical miles under the sun's surface, or at that of the $\frac{1}{668}$ part of the sun's semi-diameter. In order to produce this gigantic pressure, the difference in temperature between the enclosed hydrogen and that existing in the solar atmosphere amounts to 74,710° C.! In a similar way Zöllner calculates the approximative absolute temperature of the sun's atmosphere, which he finds to be 27,700° C.; a temperature about eight times as high as that given by Bunsen for the oxyhydrogen flame, and one at which iron must exist in a permanently gaseous form.

Passing on to more purely chemical subjects, we find this year signalized by the redetermination of a most important series of chemical constants, viz. that of the heat of chemical combination by Julius Thomsen, of Copenhagen. This conscientious experimentalist asserts that the measurements of the heat evolved by neutralizing acids and bases hitherto considered most correct, viz. those made with a mercury calorimeter by Favre and Silbermann, differ from the truth by 12 per cent.; whilst the determination by these experimenters of the heat of solution of salts is frequently 50 per cent. wrong. As the result of his numerous experiments, Thomsen concludes that when a molecule of acid is neutralized by caustic alkali the heat evolved increases nearly proportionally to the quantity of alkali added until this reaches 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$ of a molecule of alkali, according as the acid is mono-, di-, tri-, or tetra-basic. Exceptions to the law are exhibited by silicic, and also partly by boracic, orthophosphoric, and arsenic acids. In the two latter, the heat of combination is proportional for the two first atoms of replaceable hydrogen, but much less for the third atom. A second unexpected conclusion which Thomsen draws from his calorific determinations is, that sulphuretted hydrogen is a monobasic acid, and that its rational formula is therefore HSH.

Another important addition made to chemistry since our last meeting is a new, very powerful, and very simple form of galvanic battery discovered, though not yet described, by Bunsen. In this second Bunsen's battery only one liquid, a mixture of sulphuric and chromic acids, and therefore no porous cells, are employed. The plates of zinc and carbon can all be lowered at once into the liquid and raised again at will. The electromotive force of this battery is to that of Grove (the most powerful of known forms) as 25 to 18; it evolves no fumes in working, and can be used for a very considerable length of time without serious diminution of the strength of the current, so that Bunsen writes me that no one who has once used the new battery will ever think of again employing the old forms. I had hoped to be able to exhibit to the Section this important improvement in our means of producing a strong current; but war has demanded the use of other batteries, and Bunsen has been unable to send me a set of his new cells.

Amongst the marked points of interest and progress in inorganic chemistry during the past year, we have to notice the preparation of a missing link amongst the oxy-sulphur acids by Schützenberger. It is the lowest known, and may be called Hydro-Sulphurous Acid, H_2SO_2. The sodium salt, $NaHSO_2$, is obtained by the action of zinc on the bisulphite. As might be expected, it possesses very powerful reducing properties, and bleaches indigo rapidly. The metallic vanadates have also been carefully examined, and the existence of three distinct series of salts proved, corresponding to the phosphates, viz. the ortho or tribasic vanadates, the pyro or tetra-basic vanadates, and the meta or monobasic vanadates. Of these, the ortho-salts are most stable at a high temperature, whilst at the ordinary atmospheric temperature the meta-salts are most stable. In the phosphorus series, as is well known, the order of stability is the reverse; and thus the points of analogy and of difference between phosphorus and vanadium become gradually apparent.

As an illustration of the results of modern organic research—for in viewing the year's progress in this ever-widening branch of chemistry it is impossible to do more than give a few illustrations—I may quote Baeyer's remarkable investigations on Mellitic Acid. Originally discovered by Klaproth in honeystone or mellite (a substance which yet remains the only source of the acid), mellitic was supposed to be a four-carbon acid. Baeyer has quite recently shown that the acid contains 12 atoms of carbon, or has a molecular weight three times as great as was originally supposed. He has shown that mellitic acid is benzol-hexacarbonic acid, $C_{12}H_6O_{12}$, or benzol in which the 6 atoms of hydrogen are replaced by the monad radical carboxl (COOH); as benzoic is Benzol Monocarbonic acid, or benzol in which one of hydrogen is replaced by carboxyl. The most interesting portion of Baeyer's research, however, lies in the intermediate acids, partly new and partly acids already prepared, which he has shown lie between mellitic and benzoic acid, and in which from 1 to 6 atoms of hydrogen in benzol are respectively replaced by carboxyl. Nor is this all, for he has proved that, with two exceptions, each of these six acids is capable of existing in three isomeric modifications, thus giving us an insight into the arrangement of the molecule of these aromatic compounds. For the simplest mode of explaining these numerous isomers is that given by Baeyer in the different order in which the several atoms of hydrogen in the benzol molecule are replaced. Thus in the first or ortho series, the hydrogen atoms in benzol being numbered in regular succession, they are replaced in the same regular succession. In the second or meta series, the order is 1, 2, 3, 5, &c., whilst the third or para series take open order as 1, 2, 4, 5.

Thus we have :—

	Ortho Series.	Para Series.	Meta Series.
$C_{12} H_6 O_{12}$ Hexeabasic	Mellitic or Benzolhexacarbonic.	—	—
$C_{11} H_6 O_{10}$ Penta .	Unknown.	—	—
$C_{10} H_6 O_8$ Tetra . .	Pyromellitic or Benzoltetracarbonic.	Isopyromellitic.	Unknown.
$C_9 H_6 O_6$ Tri . . .	Trimesinic, or Benzoltricarbonic.	Hemimellitic.	Trimellitic.
$C_8 H_6 O_4$ Di . . .	Phtalic or Benzoldicarbonic	Isophtalic.	Tetraphtalic.
$C_7 H_6 O_2$ Mono . .	Benzoic or Benzol Monocarbonic.	—	—

Amongst the most interesting series of new organic bodies are those in which tetrad silicon partly replaces carbon. Our knowledge of these substances is gradually becoming more complete; the last new member prepared by Friedel and Ladenburg is silico-propionic acid $C_2 H_5 Si O_2 H$, the first of a series of carbo-silicic acids containing the radical $Si O_2 H$.

The interesting researches of Matthiessen and Wright on Morphine and Codeine have thrown a new light on the constitution of these opium alkaloids. Treated with hydrochloric acid, morphine loses one molecule of water, and gives rise to a new base, called apomorphine. Thus :

$$\underset{\text{Morphine.}}{C_{17} H_{19} NO_3} = H_2 O + \underset{\text{Apomorphine.}}{C_{17} H_{17} NO_2}$$

which differs in a remarkable manner from morphine, both in its chemical and physiological actions, being soluble in alcohol, ether, and chloroform, whereas morphine is nearly insoluble, and acting as the most powerful emetic known, $\frac{1}{10}$th of a grain producing vomiting in less than ten minutes. Codeine, which only differs from morphine by CH_2, also yields apomorphine on treatment at a high temperature with hydrochloric acid, methylchloride being at the same time eliminated.

An important application of the dehydrating and carbon condensing power of zinc chloride, long known in its action on alcohol to produce ether, has been made by Kekulé in the reduplication of aldehyde to form croton-aldehyde with loss of water.

$$2 (C_2 H_4 O) - H_2 O = C_4 H_6 O.$$

This croton-aldehyde is also probably formed as an intermediate product in the manufacture of chloral from aldehyde, and gives rise to the formation of crotonchloral

$$C_4 H_3 Cl_3 O.$$

The discovery of the sedative properties of chloral-hydrate by Liebreich marks an era in medical chemistry second only to the discovery of the anæsthetic properties of chloroform. Chloral not only combines with water to form a solid hydrate, but also forms solid alcoholates; but the se bodies appear to possess quite different medicinal properties from the hydrate, and it is important that no alcoholate should be present in the officinal preparation. The chemistry of colouring matters has lately received an enormous impetus in the working of the brilliant discovery of the production of artificial alizarine, the colouring matter of madder, by Messrs. Graebe and Liebermann. This discovery, announced at our last meeting, is of the highest importance, whether we regard its scientific interest or its practical and commercial value, and it differs from all the former results which have been brought about by the application of science to the production of colouring matters, inasmuch as this has reference to the artificial production of a natural vegetable colouring substance which has been used as a dye from time immemorial, and which is still employed in enormous quantities for the production of the pink, purple, and black colours which are seen everywhere on printed calicoes. During the past year much progress has been made in the practical working of the processes by which this colouring matter is obtained from the hydrocarbon anthracene contained in coal tar, and new and more economical plans for effecting the transformation have been independently proposed by Perkin and Caro, and Schorlemmer and Dale. The theoretical investigation of the reaction, and especially of the nature of some other peculiar products formed in addition to alizarine, which render the artificial colouring matter different from natural alizarine, has been carried out by Mr. Perkin, and especially by Dr. Schunck. As we are promised papers on this subject from both these gentlemen, I need not now enter further into these interesting questions.

The surest proof of perfection in a manufacture is the degree in which the waste products are utilised, and in which the processes are made continuous. One by one the imperfections of the original discovery are made to disappear, and the products which were wasted become sources of profit, whilst in many cases their utilisation alone renders possible the continuance of the manufacture in the midst of a rapidly increasing district. The Section will have the opportunity of inspecting the practical working of at least two of the most valuable of these new processes which have lately been introduced into our most important chemical manufacture, that of alkali. The first of these has been at work for some time; it is that of the recovery of sulphur from the vat waste, that *bête noire* of the alkali makers and of their neighbours. Dr. Mond has now, I believe, satisfactorily solved the difficult problem of *economically* regaining the sulphur by oxidising the insoluble monosulphide of calcium in the lixiviating vat itself to the soluble hyposulphite, and decomposing this by hydrochloric acid, when all the sulphur is deposited as a white powder.

The second of these discoveries relates to the recovery or regeneration of the black oxide of manganese, used for the evolution of chlorine in the manufacture of bleaching powder. This subject has long attracted the attention of chemists, and a feasible though somewat costly process, that of Dunlop, has been at work for some time at Messrs. Tennant's works at St. Rollox.

During the last year a very beautifully simple and economical process, proposed by Mr. Weldon, and first successfully carried out on a practical scale in Messrs. Gamble's works at St. Helen's, has quickly obtained recognition, and is now worked by more than thirty-seven firms throughout the kingdom. The principle upon which this process depends was explained by Mr. Weldon at the Exeter meeting. It depends on the fact that although when alone the lower oxides of manganese cannot be oxidised by air and steam under the ordinary pressure to the state of dioxide, yet that this is possible when one molecule of lime is present to each molecule of oxide of manganese. The manganous oxide is precipitated from the still liquors with the above excess of lime, and by the action of steam and air on this a black powder consisting of a compound of manganese, dioxide and lime, $Mn O_2 C_a O$, or calcium manganite, is formed. This, of course, is capable of again generating chlorine, on addition of hydrochloric acid, and thus the chlorine process is made continuous with a working loss of only $2\frac{1}{2}$ per cent. of manganese. The Section will have the advantage of seeing Mond's process at work at Messrs. Hutchinson and Weldon's process, at Messrs. Gaskell, Deacon, and Co.'s, at Widnes.

A third process, which may possibly still further revolutionise the manufacture of bleaching powder, is the direct production of chlorine from hydrochloric acid without the use of manganese at all. In presence of oxygen and of certain metallic oxides, such as oxide of copper, hydrochloric acid gas parts at a red heat with all its hydrogen, water and chlorine being formed. This interesting reaction is employed by Mr. Deacon for the direct manufacture of bleaching powder from the gases issuing directly from the salt-cake furnace. Air is admitted together with hydrochloric acid gas, and the mixture is passed over red-hot bricks impregnated with copper salt. The oxide of copper acts as by contact and remains unaltered, whilst the chlorine, watery vapour, and excess of air pass at once into the lime-chamber. The difficulty in this process is the large volume of diluting nitrogen which accompanies the chlorine, but I believe we shall hear from Mr. Deacon that, notwithstanding this drawback, he has accomplished his end of making good bleaching powder by this process.

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THURSDAY, SEPTEMBER 22, 1870

THE GOVERNMENT AND THE ECLIPSE EXPEDITION

SEVERAL of the correspondents who have been lately so busily engaged in chronicling the scenes of the present terrible war have wound up their descriptions by the remark that they could scarcely get rid of the impression that they have been all the time the sport of horrid dreams—that the carnage has existed only in their imagination, and that civilised Europe has been uninterruptedly pursuing the arts of peace.

Similarly it is difficult for an English man of science, proud of his nation's past achievements in many of the most noble problems, and among them those which have necessitated voyages to various parts of the world—now to witness a transit of Venus—now to make magnetic observations—now to survey the Polar Regions—and now to observe an eclipse of the sun; to realise the fact that in this present year of grace, 1870, there exists a British Government which in the curtest possible manner has put a stop to an expedition which promised to add as much to our renown in the present as the expeditions to which we have referred, and many others that we might have mentioned, have done in the past.

The proposed expedition now in question had for its object the observation of the approaching Eclipse of the Sun. A brief statement will suffice to show its importance, and how naturally it followed other similar expeditions.

It is now more than a century and a half ago since certain things called *red flames* were seen round the edge of the eclipsed sun; another appearance called the *corona*, sometimes of great irregularity and magnitude, having been noticed from the highest antiquity. Time went on, however, till only as lately as ten years ago a British expedition to Spain determined, to the satisfaction of everybody, that these red flames belonged to the sun, and not to the moon, as had been suggested. Here, then, was one great point gained by the help of the British Government—other European Governments, let us add, assisting. This was in 1860. In 1868 there was another eclipse, this time in India. The Indian side of the Government nobly assisted again by telling off officers, granting money, and affording the use of a ship to a distinguished Frenchman who chose India for his standpoint of observation, the French and German Government expeditions preferring other stations. It is seen, therefore, that the work done at the eclipse of 1868 was Government work, and it was admirable. The nature of the red flames, which had been localised in 1860, was now roughly determined.

There was still another eclipse in 1869, visible in America. Here Government help was not needed at all, as the eclipse swept right over the land; *still it was nobly given*, and one of the most important recent contributions to astronomy is the American Government Report, of the observations made by its officers. The American observers fairly broke ground in another branch of the research, the nature—not of the red flames this time but—of the corona; but they did not settle the question, and it now remained for astronomers to "crown the edifice" of all our eclipse work by settling it at the eclipse next December.

Thanks to the Janssen-Lockyer discovery, we can now study the red flames day by day; and, although the corona resists the new method, the discovery, taken in connection with Frankland and Lockyer's observations of the hydrogen spectrum, has yet thrown much light on its possible nature, so that the new method makes observations next December more precious.

Having shown, then, both the importance of the observations of the next eclipse, and the fact that they are the natural sequel of the work which has been done by Government aid, and which, it is not too much to say, could not have been done without it, let us next state some facts with regard to the proposed Expedition. It was first proposed by the council of the Royal Astronomical Society, who appointed a committee of council to take the necessary steps. The Royal Society followed suit, and next a joint committee was formed. To this joint committee were committed the whole of the arrangements. Here, shortly, is the programme agreed upon:—

1. At least two expeditions would be necessary—one to Spain, the other to Sicily.
2. At least 1,000*l.* would be required for instruments and observatory-building expenses, &c.
3. The presidents of the Royal and Royal Astronomical Societies and the Astronomer Royal were to form a deputation to the Government to ask for two ships and 500*l.*, the remainder being subscribed by the societies themselves.
4. The observers were to be carefully organised, and competent persons placed in charge of each branch of the research in each expedition.

It is clear from this that our scientific bodies have done all in their power.

Unofficial *pourparlers* having led to the belief that the Government would not be found wanting on its part, those interested in the work spared no pains to organise the expedition as minutely as possible, and all the best observers came forward to make it second to none of its predecessors.

Here is the official list of those who were prepared to take part in the observations:—

Spectroscopy.—Mr. Lockyer, Dr. Gladstone, Mr. Buckingham, Lieut. Brown, Mr. T. W. Backhouse, Lieut. Collins, Rev. A. W. Deey, Lieut. Davies, Mr. G. Griffith, Mr. W. B. Gibbs, Rev. H. A. Goodwin, Mr. S. Hunter, Mr. W. A. Harris, Rev. F. Howlett, Mr. W. Ladd, Capt. J. P. Maclear, Rev. S. J. Perry, Mr. A. C. Ranyard, Mr. G. M. Seabroke, Mr. H. Tomlinson, Mr. Pedler.

Polarisation.—Prof. Pritchard, Mr. R. Abbatt, Mr. Bushell, Mr. Chambers, Mr. G. Griffith, Mr. W. B. Gibbs, Rev. F. Howlett, Mr. W. Ladd.

Photography.—Mr. Browning, Mr. Brothers, Lieut. Abney, Mr. Buckingham, Lieut. Brown, Mr. Chambers, Mr. Clodd, Mr. R. Sedgfield.

General Objects.—Mr. Lassell, Col. Strange, Mr. Dallmeyer, Mr. J. Bonomi, Mr. E. J. Lowe, Prof. J. Phillips, Prof. H. E. Roscoe, Mr. G. J. Stoney, Mr. C. G. Talmage, Mr. C. B. Vignoles.

General Assistance.—Mr. R. Abbay, Mr. R. Inwards, Lord Lindsay, Mr. W. J. Lewis, Rev. W. Monk, Mr. R.

S. Newall, Mr. L. B. Phillips, Mr. W. Pole, Mr. F. C. Penrose, Prof. Tyndall, Mr. R. Webster, Mr. J. E. Backhouse, Mr. E. E. Bowen, Col. Drayson, Admiral Ommanney, Mr. Thos. Slater, Mr. P. E. Sewell, Mr. W. Rossiter, Capt. Noble, Mr. W. K. Clifford, Mr. W. H. H. Hudson.

Here was a rich promise of a victorious campaign, and the scientific world already congratulated itself on being able at last to "settle the corona," when suddenly, as a bolt out of the blue, came a letter from the Admiralty *declining even a single ship*, on the ground that such a purpose was entirely foreign to the purpose for which Parliament places funds at the disposal of the Naval Department.

We think we had better leave this astounding statement as it stands. It seems really as if the present Admiralty authorities are in absolute ignorance as to the real facts of the case; as to what England has done before; as to what precedents exist to which men of science can point.

Under these circumstances we trust that an appeal will be made to Mr. Gladstone, whose culture, wider than that of his more prominent colleagues, will at once grasp the huge Philistinism of this proceeding. Should he reverse their decision, which he may fairly do, on the mere ground that it is against all precedent, assuredly the scientific men of Britain will hail it as a happy omen—an indication that the hope experienced by Prof. Huxley at Liverpool the other day will, in time, be realised. If, on the other hand, the decision is to stand, it must be distinctly understood that, both in the judgment of our contemporaries and of posterity, it will, as has been already been pointed out in the daily press, bring shame upon the scientific repute of England, who now, with her forces all ready to achieve another victory over nature, is held back by "My Lords" for the sake of a few pounds sterling. Surely there is little hope for us if in such a campaign as this we are to succumb to a

Lust of gold
And love of a peace that is full of wrongs and shames;
Horrible, monstrous! not to be told.

REPLY TO PROFESSOR HUXLEY'S INAUGURAL ADDRESS AT LIVERPOOL ON THE QUESTION OF THE ORIGIN OF LIFE

I.

SPEAKING with all the authority which years of earnest and successful labour have conferred, and, moreover, "from the elevation upon which the suffrages of his colleagues had for the time placed him," Prof. Huxley has just given us in his Inaugural Address, as President of the British Association for the Advancement of Science, a "history of the rise and progress of a single biological doctrine"—that first proclaimed by Francesco Redi, and to the effect that *Every living thing proceeds from a pre-existing living thing.*

However reluctant to enter a protest against what has been said by an eminent scientific man, for whom I have always entertained the greatest respect and esteem, I feel so strongly that the representations which have been made concerning a subject to which I have directed the most earnest attention for the last eighteen months, are not only inadequate, but altogether incapable of being regarded as an impartial statement of the main points at issue, that I cannot hesitate as to the propriety of publicly expressing this opinion.

Fearful, therefore, lest harm should be done to the cause of science by this address, through the great influence of the speaker, and mindful of the momentous issues which turn upon the proper solution of the question under discussion, I—sinking all personal feelings, risking all imputations, anxious only that the truth should be known—will venture to state what really seems to me to be the true aspect of the problem, and how far the remarks of Prof. Huxley really bear upon this, or have been, in other respects, not sufficiently explicit.

The doctrine, whose history Prof. Huxley professes to trace, and whose probable truth he thinks remains unshaken, has reference to a question which is of more fundamental importance than any other throughout the whole range of Biological science. It is either true that *all* living matter, without exception, comes into being in connection with pre-existing living matter, or else it is true that *some* living matter can arise from non-living materials free from all connection with pre-existing living matter. This alternative is one the full meaning of which may, perhaps, be realised better by putting another, which, though strictly analogous, is somewhat freer from mystery. It may, then, similarly be said, it is either true that *all* crystalline matter, without exception, comes into being in connection with pre-existing crystalline matter, or else it is true that *some* crystalline matter can arise from non-crystalline materials, free from all connection with pre-existing crystalline matter. Matter when it passes into the *crystalline* condition exhibits properties of a certain kind, and when it passes into the *living* condition it exhibits properties of another kind, to which we commonly apply the term "vital." Now the question in each case is, whether by mere concurrence of certain physical conditions, aiding and abetting the inherent properties of the matter itself, some kinds of matter can fall into modes of combination called *crystalline*, whilst other kinds are capable of falling into modes of combination called *living*; or whether, in each case, a pre-existing "germ" of the particular kind of matter is necessary, in order to determine, in suitable media, either of these modes of combination. Are we to believe that crystals can appear in no solution whatsoever without the pre-existence in that solution of certain crystalline germs,* and similarly that living things can arise in no solution whatsoever without the pre-existence in such solution of living germs? To many persons it may at first sight seem that there is no analogy between the two cases; such, however, is not the opinion of very many who are best entitled to speak on the subject.|| It is admitted by them that the analogy is of the closest description; and it is interesting to note that although the actual evidence which can be brought to bear upon these two questions is very similar in kind, and alike conflicting in nature, the generally received opinions as regards the proper answers to be given to these two questions have inclined to the view that, whilst it is possible for crystals to originate *de novo*, it is at present impossible for living things to originate after this fashion.

* It must not be supposed that this is a mere hypothetical case. On the subject of crystallisation generally in supersaturated solutions, I will quote the following passage from Watts' *Dictionary of Chemistry*, Vol. v., p. 349. :—"This sudden crystallisation, if not produced by cold, appears to depend essentially on contact of the solution with small solid, perhaps crystalline particles; for it is not produced by passing air previously purified by oil of vitriol through the solution, or by agitation with a glass rod previously purified from dust by ignition. *According to Violette and De Gernez, the sudden crystallisation is in all cases induced only by contact with a crystal of the same salt*, possessing the same form and degree of hydration as the crystals, which separate out; and in the case of those supersaturated solutions which crystallise suddenly on exposure to the air, it is due to the presence of minute particles of that salt floating in the air. From an experiment of De Gernez it appears that microscopic crystals of sodic sulphate may be obtained by passing air, even in the open country, through pure water, and evaporating the water on a glass plate. Jeannel, however, denies the necessity of contact with the salt actually contained in the solution. He finds, indeed, that a supersaturated solution of sodic acetate may be made to crystallise by contact with any solid substance (a piece of paper for example), and a solution of sodic tartrate by contact with a clean, dry, glass rod" Here, then, we have also a veritable "germ" controversy. I was informed, however, a few weeks ago by Prof. Frankland that even in the case of sodic sulphate it had lately been shown that, *under certain conditions*, crystallisation can certainly take place where no crystalline germ could possibly have existed. The "germ" theory of the origin of crystals in supersaturated solution, has, therefore, been overthrown. This has been possible, however, only because it has been more easy to show that a given set of conditions are inimical to the existence of a crystal, than it has yet been to induce people to believe that any given set of conditions are incompatible with the existence of living matter.

It is worthy of remark, however, that the germ controversy concerning crystals can only be settled in the minds of those who are content to accept the high probability that the properties of any *invisible* portions of crystalline matter would correspond with the properties which similar visible crystalline matter is known to display. It is this reluctance to admit an equally high probability in the case of living matter, which alone causes the sister controversy to continue. Otherwise the question would have been settled long ago.

† The analogy between the supposed possible origins of crystals and organisms in solutions has been rendered much more obvious since the discovery by the late Professor Graham, that when dissolved the saline substance does not remain as such in solution, but that the acid and the base exist separately, and are separable by a process of dialysis. When crystallisation takes place, therefore, we have a combination of materials taking place similar to, though simpler than, what may be presumed to take place in the genesis of a Living thing.

The question is one of much interest, and it may therefore well be asked why such a totally different verdict should have been given in two cases, the analogy of which is so remarkable. The reason is, however, not difficult to find. Mere theoretical considerations have been all-powerful in influencing the verdict, and in inducing those who are informed upon the subject to read the evidence in different ways. Living things manifest such complex properties that the whole notion of Life has been shrouded in mystery. Biologists at first could not bring themselves to believe—some cannot do so now—that the phenomena which living things manifest are absolutely dependent upon the properties of the variously organised matter entering into their composition. They were obliged to have recourse to some metaphysical entity—some "anima," "archæus," or "vital principle"—under whose directing influence the living form was supposed to be built up, and upon whose persisting influence many of the phenomena of Life were supposed to depend. The aid of no similar metaphysical "principle" has, however, been deemed necessary in order to account for crystalline structures and properties. It was in the main conceded by most physicists, and the doctrine remained unquestioned by biologists, that matter of certain kinds might, by virtue of its own inherent properties, aided by certain favouring circumstances—and quite independently of all pre-existing germs—fall into such modes of collocation as to give rise to crystals. But, owing to the influence of the theoretical considerations already mentioned concerning the nature of Life, a similar possibility could not easily be granted in reference to the origin of Living things. Was it not held that the living thing owed its structure or organisation to the active influence of a special and peculiar principle? This "vital principle" was neither ordinary matter nor ordinary force, neither was it in any way derivable from either of these; how then could it be supposed that the coming together of matter of any kind could give rise to a living thing?* The aggregate of properties, which we designate by the word "Life," were not supposed to be dependent upon, to be, in fact, properties of the material aggregate which constituted the Living thing. Life was presumed to be due to the manifestations of a something altogether peculiar—of a "vital principle," which was inseparable from living matter. Doctrines akin to these having been already proclaimed and disseminated by the influential teachings of Paracelsus, Van Helmont, and others, it cannot be a matter for surprise that the brilliant demonstrations of Redi should have had a great influence in their time. Observation after observation appeared now to confirm the existence of a seemingly universal mode of origin of Living things—a mode too which was more in harmony with the philosophical views of the day than that which had hitherto been deemed possible. Doubts, however, soon sprang up. New means of observation opened up new questions for solution. And what has been the result? Many battles have been fought, many victories have been won, and now the biological doctrines of the day have assumed an entirely new form. The ever-increasing strides of Science have wrought the most fundamental changes in our notions concerning Life. Under the influence of the well-established doctrine concerning Persistence of Force—and more especially since the clear recognition of the subordinate doctrine as to the Correlation existing between the Physical and Vital forces—physiologists have now begun to recognise, and most unhesitatingly to express the opinion, that the phenomena manifested by living things are to be ascribed simply to the properties of the matter as it exists in such living things. No one has expressed himself more decidedly on this subject than Prof. Huxley himself, and he may fairly be taken as an exponent of the modern doctrines on this question. He says:†—"Carbon, hydrogen, oxygen, and nitrogen are all lifeless bodies. Of these, carbon and oxygen unite in certain proportions and under certain conditions to give rise to carbonic acid; hydrogen and oxygen produce water; nitrogen and hydrogen give rise to ammonia. These new compounds, like the elementary bodies of which they are composed, are lifeless. But when they are brought together under certain conditions they give rise to the still more complex body, protoplasm; and this protoplasm exhibits the phenomena of life. I see no break in this series of steps in molecular complication, and I am unable to understand why the language, which is applicable to any one term of the series, may not be used to any of the others. We think fit to call different kinds of matter carbon, oxygen, hydrogen, and nitrogen; and to speak of the various powers and activities of these substances as the properties of the matter of which they are composed. . . . Is the case in any way changed when carbonic acid water and ammonia disappear, and in their place, under the influence of pre-existing protoplasm, an equivalent weight of the matter of Life makes its appearance? . . . What justification is there then for the assumption of the existence in the Living matter of a something which has no representative or correlative in the not-living matter which gave rise to it?"

* Buffon, it is true, as Professor Huxley has pointed out, did make an attempt to reconcile two incompatible theories.

† *Fortnightly Review*, Feb. 1869.

For Professor Huxley, then, and for all who hold similar opinions on this subject, the constitution and properties of living things are so far comparable with the constitution and properties of crystals, that both, in each case, are alike supposed to be the products of the combination of ordinary matter of different kinds. And, as might have been expected, nearly all the biologists and physicists who hold these opinions, are now inclined to admit their belief in the *possibility* of the origination of living matter free from the influence, and independently, of all pre-existing living matter. They are quite content to admit that Redi's doctrine may be wrong. Prof. Huxley, indeed, in his recent address, desires us to understand that this is an opinion to which he still adheres; he says:—"I think it would be the height of presumption for any man to say that the conditions under which matter assumes the properties we call 'vital,' may not some day be artificially brought together."

Having reached this stage, having got rid of the supposed necessity for the intervention of a special "vital principle" before living matter can come into existence,* I think it will be seen by all how very important it has become to look into the truth of Redi's doctrine, which has found its best modern expression in the phrase *omne vivum ex vivo*, seeing that that doctrine was born and nourished under the influence of the old, and now well-nigh effete, metaphysical notions concerning Life. Certainly, now that this theoretical barrier has been removed, we ought to inquire more carefully than ever whether there is still a sufficient warrant for the different verdicts which have been given in answer to the questions as to whether crystals on the one hand, or living things on the other, do or do not originate *de novo* in this particular stage of the Earth's history.

Now, at all events, theory inclines no more to the one side than it does to the other; it is quite possible to reconcile this with either view.

Seeing, therefore, that we may now act without fear as impartial judges, let us inquire into the nature of the evidence which alone can be relied upon for the solution of these two questions.

If living things are to come into being *de novo*, they could, or, at all events, are only supposed to originate from the rearrangement of matter which previously existed in a state of solution. And although it is known to be possible for certain kinds of pre-existing solid matter to assume a crystalline form, we will, for the present, confine our attention to the origin of crystals in an apparently homogeneous fluid. Each of these material forms, therefore, would have to commence as a smallest conceivable speck, and each would grow, though differently, by the formation of matter of like kind, under influences generally similar to those which were influential in bringing about the primordial collocation. These primordial collocations, however, are hidden from our view, and will, perhaps for ever, remain so. As a matter of observation, all that we actually know concerning the origin of crystals or of certain living things in solutions is this. In previously homogeneous solutions of crystallisable matter, or in certain apparently homogeneous colloidal solutions, we may, under certain conditions, see the minutest crystals† or living things, respectively, make their appearance. In both cases these are, at first, mere motionless specks, whose *minimum visible* stage may be less than $\frac{1}{100000}$th of an inch in diameter. It must either be presumed, in the case of such embryo living things (as most people do presume in the case of crystals), that these, even then, and however minute, represent stages in the growth of later material collocations which had been initiated under the combined influence of existing matter and "conditions" at a point far beyond the reach of our most aided vision; or, on the other hand, it is equally

* It may perhaps be as well to state here that I have not much expectation of influencing those whose belief in the existence of a special "vital principle" remains still unshaken.

† The appearance of the crystals is best watched in the viscid solutions described by Mr. Rainey; since the rapidity of the process is thereby very much diminished, and the forms themselves are also more akin to those of living things. See his work *On the Modes of Formation of the Shell of Animals*, &c., 1858, p. 9.

open for us to suppose that such minutest visible living things had proceeded from the growth of pre-existing germs which were themselves invisible.

This being, as I conceive, the real state of the case, and Professor Huxley being in the position of a person, admitting* that a crystal can be produced *de novo*, admitting also the *possibility* that a living thing may so arise, but denying that there is any evidence worthy of serious consideration to show that a living thing can at the present time originate *de novo*, let us see on what evidence he has come to this conclusion, and what other evidence he has practically ignored.

In the first place, he does not attempt to deny—he does not even allude to the fact—that *Living things may and do arise as minutest visible specks, in solutions in which, but a few hours before, no such specks were to be seen.* And this is in itself a very remarkable omission. The statement must be true or false, and if true, as I and others affirm, the question, which Professor Huxley has set himself to discuss, is no longer one of such a simple nature as he represents it to be. It is henceforth settled, so far as *visible* germs are concerned, that living things *can* come into being without them. It can now, at all events, be said that *some* Living things do not come from *visible* germs. Who, therefore, in the face of this fact will say that the doctrine *omne vivum ex vivo* remains unshaken? Perhaps, however, this particular case where an exception to the rule is possible, was not known to Professor Huxley. I wish I could bring myself to believe that this was really the case. Certain it is that had he recognised the existence of this apparent exception to the general rule he would then have had to discuss a much more difficult question, and he would have been compelled seriously to inquire into the value of experiments whose existence he has now almost ignored. Again then I affirm that multitudes of minute living things may and do gradually appear in fluids, beneath the microscope, where no *visible* germs previously existed. Here the hypothesis that *every* living thing proceeds from a pre-existing living thing may break down, and those who wish to establish the continuity of this rule are bound to discuss the nature of the existing evidence which is in favour of the notion of the living things in question originating from pre-existing *invisible* germs, as against the opposite *possibility* of their having originated *de novo.* The burden of proof rests as much on the one side as it rests on the other. We cannot safely continue to affirm a rule until the cases in which it seems doubtful have been thoroughly discussed. Analogy is often but a treacherous guide.

And, when we come to the discussion of this hypothesis as to the origin of living things from germs which are *invisible*, all alike are rendered, to a certain extent, helpless. No one, then, can come forward, as Redi is said to have done, "strong with the sense of demonstrable fact," and any one who wishes or calls upon his opponent to demonstrate the truth of his views, when the question is one concerning the presence or absence of *invisible germs*, shows himself to be ignorant as to how the matter in dispute can alone be settled. The subject is one in which direct demonstration must give place to reasoning, although experiment and observation may and must be brought forward in support of this. Let those, however, who wish to proclaim the universality of the rule *omne vivum ex vivo*, recollect that, if they expect to influence reasonable people who are themselves competent to form an opinion on the subject, they are bound to consider the *possible* exception to which their attention is directed, and to weigh the evidence for and against the origin of these minutest visible living specks from germs which are supposed to exist, but which are *invisible*.

The reason, indeed, which seems to induce most people to believe that living things cannot arise *de novo*, is because in 999 cases out of a thousand which come under their actual notice, there cannot be a question that a living thing originates from a pre-existing living thing. A rule, which is of such apparently universal application, they say, is most likely to be the rule which applies to any doubtful case. Much is made out of this argument, which is, of course, a very valid one so far as it goes. But, on the other hand, knowing, as I have pointed out, *that any living things which arise, de novo, from non-living matter, must appear in solutions as minutest visible specks*, it need not be a matter of much surprise that this mode of generation is one which is unfamiliar to the world at large. Have we not seen, indeed, that the most accomplished biologist, provided with the very best microscope hitherto made, though he gets down to a *minimum visible* stage of less than $\frac{1}{100000}$" in diameter is just as powerless in face of the hypothesis of *invisible* germs as those who worked with the rude microscopes which alone were in vogue two centuries ago? And, more especially is this consideration one which presses for earnest attention, when we further consider that some of the minute living things which first appear as tiniest specks in homogeneous solutions grow into *Bacteria*, and that concerning the real origin of these, *in such cases*, we are as ignorant as we were concerning the real origin of crystals, when they appeared in previously homogeneous solutions. The probability that these latter have originated *de novo* has, of late years, had to be established by a process of reasoning similar to what we are obliged to have recourse to, if we wish to throw light on the question of the origin of these specks of Living matter. *Bacteria* grow, and after a time aggregations of them may be converted under our very eyes into *Fungus*-spores * capable of throwing out a filaments and of developing into perfect plants. Nobody pretends to know, however, how, or whether, the *Bacteria* which make their appearance in a homogeneous solution have originated from invisible *Fungus*-emanations: all that we know is, that in suitable solutions, appearing homogeneous to high microscopic powers, in the course of a very short time, a multitude of perfectly motionless specks appear, in situations where previously no specks had existed. Being motionless and diffused their number cannot be accounted for by any supposed rapidity of multiplication—the only possible explanations seem to be, either that the specks have originated from as many pre-existing germs which were invisible, or else that they have proceeded from material collocations, which have been initiated in the fluid itself by virtue of the molecular properties of the substances in solution, and the physical forces or sum total of "conditions," acting thereupon.

And this is really the question which has to be considered. When it is supposed that Living things do appear independently of pre-existing living matter, in certain solutions nothing more than this is supposed to have taken place. New Living matter is presumed to have appeared—independently of germs—in the solutions within these flasks, and to have made *its* appearance as living matter may, in certain other fluids, under our very eyes, in the form of minutest visible specks, which have been exposed to great and long-continued heat in hermetically sealed masses. And similarly such specks, are the only forms of Living matter which are supposed to be capable of arising *de novo.* Once formed, it is true, one of these living specks may develop into a *Bacterium*, and this may develop into a *Vibrio* or a *Leptothrix* filament, whilst another of the living specks may develope at once into a *Fungus*-spore.† It should be clearly understood, however, that *all the Living things which are supposed to arise out of non-living materials, are presumed to appear in fluids, and gradually to emerge from the region of the invisible into that of the visible*; at which latter point they, for us, constitute specks less than $\frac{1}{100000}$" in diameter.

Making no statements whatever upon this subject, however, in support of the doctrine which he considers to remain unshaken, let us see what line of argument Professor Huxley has taken, in order to establish the validity of this belief to the members of the British Association for the Advancement of Science.

The "long chain of evidence" which he considers sufficient to allow us still to place faith in the rule *omne vivum ex vivo*, seems to me, to be, in reality, utterly inadequate for this purpose, and incapable of affecting the real question at issue. Nothing that has been said bears at all upon the problem as to whether it is possible that the minute living specks to which I have referred do or do not originate *de novo*, though, as I have already said, it is these, and such as these only, which are presumed to originate after this fashion. If he had really wished to influence those who are conversant with the subject, it would have been absolutely imperative for Prof. Huxley to have entered fully into the consideration of a subject which I will presently mention, but to which he makes only the most casual allusion. All the facts which he has brought forward—all the references to the investigations of Spallanzani, Schultze and Schwann, Cagniard de la Tour, Helmholtz, Schrœeder and Deutsch, Tyndall and Pasteur—are simply contributions to the "Atmospheric Germ Theory," tending to show that there are germs of living things in the air, and that the living things found in *some* solutions may have been developed therefrom. But although differing

* I suppose this may fairly enough be presumed even in the absence of any specific statement as to his belief on the subject. This is, however, an assumption on my part.

* See NATURE, No. 35, p. 173, Fig. 3.
† See NATURE, No. 37, pp. 221, 223.

from him in my interpretation of the results of some of these investigations,* I am quite content to accept the conclusion which is alone derivable from this long chain of evidence. I am even prepared to grant to Professor Huxley, for the sake of argument, that *Bacteria* may be "suspended in the atmosphere in myriads." The evidence thus referred to, if true in all respects, would have been very valuable if it had been brought against the doctrine that none of the minute living things of infusions derived their origin from atmospheric germs, though it may and does fall utterly powerless before the doctrine which is alone urged, that *some* of the Living things met with in infusions appear to be produced independently of pre-existing living matter. If it could be proved that the air contained five hundred times as many germs as can now be shown to exist therein, this discovery would still be quite compatible with the truth of the other doctrine that under the influence of certain conditions some Living things, appearing as minutest visible specks, do arise *de novo* in solutions.

Whether such an occurrence can or cannot now take place is a question which is not at all dependent upon the prevalence or paucity of germs in the atmosphere. I may also remind Prof. Huxley of a fact which he seems to have forgotten, and that is, that the atmosphere is not the only source of germs. These may be present in the water or in the materials dissolved therein. Seeing, therefore, that in certain experiments which constitute the corner-stones of his edifice of proof, and which are brought forward, I suppose, as being capable of influencing our judgment upon this great question, the materials which were dissolved and the water employed were merely boiled for fifteen minutes, we must look upon this as an admission by Prof. Huxley that in his opinion the exposure of the *solution for such a time to a temperature of* 100° *C. was an adequate precaution to ensure the destruction of all pre-existing living things that may have been contained therein.* This is a most important admission—tacit though it be—in the face of other evidence which can be mentioned, and if Prof. Huxley does not really believe this, how is it possible for us to understand what either his argument or science gains from the citation of the following experiments?

Having boiled portions of "Pasteur's solution" for fifteen minutes, in three separate flasks, he placed in the neck of one of them, whilst ebullition was continuing, a large plug of cotton wool, left another with the mouth of the flask open, whilst into the third, when cool, he placed some *Bacteria* taken from a solution of hay. "In a couple of days of ordinary warm weather," he says, "the contents of this [latter] flask will be milky from the enormous multiplication of *Bacteria.* The other flask open and exposed to the air will, sooner or later, become milky with *Bacteria,* and patches of mould may appear in it; while the liquid in the flask, the neck of which is plugged with cotton wool, will remain clear for an indefinite time." And then Prof. Huxley adds:—"I have sought in vain for any explanation of these facts, except the obvious one, that the *air contains germs* competent to give rise to *Bacteria,*" and similar to those with which one of the solutions was purposely inoculated. Now, with reference to these statements, the possibility at once suggests itself, that had a different solution been used in the case where the neck of the flask was plugged with cotton-wool a very different result might have been obtained. In order to throw light upon this subject I have performed the following experiments:—

Immediately after reading Prof. Huxley's address, I procured a piece of cooked meat, made an effusion of the same, and after filtration put it into a flask. It was then boiled for fifteen minutes, after a large plug of cotton wool, $1\frac{1}{4}''$ in length, had been pushed into its neck. After this time the plug was rendered tighter by pushing in more wool. Another flask was prepared in a similar way, only in this, a strong filtered infusion made from undressed meat was placed. At the expiration of the fourth day (Monday morning, Sept. 17th) the weaker solution, still quite *clear,* was opened, and on microscopical examination of two or three drops of the fluid a multitude of minute motionless particles of various sizes were seen, others in active movement, and two or three *Bacteria* about $\frac{1}{8000}''$ in diameter. The flask containing the stronger solution was opened at the expiration of forty-two hours. The fluid still appeared quite clear, and on microscopical examination of a few drops of the fluid *many* tolerably active Bacteria were found varying between $\frac{1}{6000}''$—$\frac{1}{1000}''$ in length, besides a multitude of particles, some moving and others motionless.

These results seem to me what might have been expected after what I have made known concerning putrefaction *in vacuo.* It could scarcely be expected that mere *filtration* of air should be able to prevent putrefaction when it has been already shown that this will take place in the absence of air.

What conclusion, then, is now deducible from Prof. Huxley's three comparative experiments? Certainly nothing that has any value for the support of his argument.

[A strong point made by Prof. Huxley is the supposed fact that the possibility of preserving meat is a fatal reply to the experiments of myself and others. I shall show next week, that the actual facts strengthen my point of view, and that "perfectly good" cases of meat which I have examined have contained *Bacteria* and *Leptothrix* filaments.]

H. CHARLTON BASTIAN

* Neither time nor space will permit of my mentioning these various points on which I am inclined to differ from him. When Prof. Huxley says, however, after a tragical metaphor, "It must be admitted that the experiments and arguments of Spallanzani furnish a complete and a crushing reply to those of Needham," I will only say that I cannot agree with him, and will remind him that, in this case at least, he is not supported by Pasteur, whose logic is so invincible. Pasteur says (*Ann. de Chim. et de Phys.*, 1861, p. 9):—"Un examen impartiel des observations contradictoires de Spallanzani et de Needham sur le point le plus delicat du sujet, va nous montrer en effet, *contrairement à l'opinion généralément admise* que Needham ne pouvait en toute justice abandonner sa doctrine en presence des travaux de Spallanzani."

I would also call Prof. Huxley's attention, as an impartial historian, to some communications made by M. Victor Meunier to the French Academy (*Comp. Rend.* 1865), from which he will see with reference to the vessels with bent necks, that it is possible to perform these experiments with an "entire success" of a different kind from that to which he alludes. Others besides myself have also performed such experiments with results similar to those of M. Meunier. Much seems to depend upon the nature of the solution employed.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

English Physiology

THE present state of physiology in this country ought to be a matter of regret. Though foremost in many things, Britain is far behind Continental countries in the field of physiological science. We can boast of a few distinguished physiologists, as John Hunter, Sir Charles Bell, and Dr. John Reid; and famous microscopists, as Carpenter and Beale; but a very small number of English names can be cited compared with the host of Continental physiologists, past and present, as Magendie, Müller, Von Beyoid, Von Baer, Béclard, Bernard, Brown-Sequard; Du Bois Reymond, Helmholtz, &c. This discrepancy arises not from want of talent, but from lack of opportunity. The mental qualities required by a physiologist, as observation and memory, are developed separately at different periods of life. Hence there are only a limited number of years during which any such branch of learning can be cultivated with fresh ardour, and during which the power termed originality can be brought into play. The Continental schools make use of these precious years by affording those who are naturally inclined to cultivate any one branch of science, full scope for repeating the observations of their predecessors, and for endeavouring to add to the existing stock of knowledge. By having various laboratories and certain paid appointments connected with their universities, they allow young men to devote their whole time and energy to the study of individual subjects, as physiology. Those who set themselves to work of this kind do not look forward to the practice of the medical profession, but purpose to live and work as physiologists. These young men are known by their labours to be specialists, and are proposed by the senatus with which they are connected for a vacant professorship when it occurs. This is the only method of securing original and extensive work in any one scientific branch, as Physiology. It would be well, therefore, if the approaching Royal Commission of Inquiry into the State of Science in this country would not overlook Physiology, but would make some arrangements whereby Great Britain might no longer be stigmatised by her Continental neighbours as "having no Physiologist."

Dr. Stricker, in the two articles he has already communicated to NATURE on "The Medical Schools of England and Germany," has not referred to the Edinburgh University, which possesses the best furnished Physiological Laboratory to be found in Great Britain, and one equal to most of those met with in Germany. The plan recently introduced into this Scottish University of having salaried assistants to the professors resembles that which exists in the German Universities. This arrangement not only allows opportunity for carrying on original research, but also enables the professors to impart a most beneficial impulse to younger men.

In this country it has hitherto succeeded best, and appears to be most in consistence with our national constitution, to place the pecuniary assistance afforded to scientific men under the control of learned bodies as the Universities and the Royal Society. It is to be hoped, however, that our Government will, ere long, recognise the duty of advancing physiologists by aiding them with grants of money. Every medical school in Britain ought to have a physiological laboratory, well furnished with instruments—electrical, chemical, and physical; and with the view of instituting and supporting these it will be necessary to supply certain sums of money annually from the public exchequer. In addition to money given to Medical Schools for the purpose of buying instruments, two or more grants might be set aside as prizes to be bestowed annually on the schools furnishing the best physiological work, and to be distributed by the governing bodies of the said schools among the workers. The awarding of such prizes might very well be entrusted to the Royal Society.

I trust that this plea on behalf of physiology will not pass unnoticed by you.

Birkenhead P. M. BRAIDWOOD

Mirage

MIRAGE is not, in my experience, an uncommon phenomenon. I saw it this summer on the flats at the mouth of the Dee (Wirral side). It may often be seen, with a bright sun and still air, after heavy rain, on Hartford Bridge Flats, and other level and gravelly heaths in the Bagshot Sand district, where the fir trees may be seen floating in water, or forming promontories jutting out into a lake—a phenomenon similar to, though far less striking than, what I have seen on the Plain of Aan, in Provence.

The most curious mirage-effects I ever saw were on the Wash during hot summer weather. The mirage is there known as the "looming of the land," and when it is about it is impossible at moments to distinguish the sand and weed-banks from the sea, while the distortion, both perpendicular and horizontal, of ship's masts, &c., is ludicrous. In one case I saw a herd of seals on a sand-bank transformed into a row of long-legged monsters, wading in water, or rather rooted by their long legs to the legs of a similar row of monsters below them, which was their distorted reflection in wet mud. I had some difficulty, at first, in making out what they were.

Eversley Vicarage, Sept. 16 C. KINGSLEY

Astronomical Science

IN your number for August 5th, a letter referring to Astrology, signed "C. J. Robinson" ends as follows:—"Astronomical Science is hardly likely for the sake of sentiment to treasure up the discarded swaddling clothes which for so many centuries impeded its onward progress." Surely such language indicates a sad confusion of ideas on the subject, since it is most unquestionable that the belief of antiquity in Astrology—far from retarding—greatly promoted the study of Astronomy. In fact, the names of Ptolemy and Kepler show that the greatest of ancient and the greatest of modern astrologers were at the same time the greatest Astronomers of their era, and the brilliant discoveries of the latter in both sciences suffice to dispose of the "swaddling clothes" theory without citing the instance of Napier, who, it is well known, invented that most admirable scientific expedient and indispensable handmaid to Astronomy, Logarithms, to shorten and facilitate his astrological calculations.

I have not seen Moore's Almanac referred to by Mr. Robinson, but any one by consulting an Ephemeris may verify the following curious facts. War against Prussia was declared by the French Emperor on the 15th July. The preceding lunar change was a total eclipse of the Moon on the 12th, in 20° 15′ of Capricorn, when the Sun and Moon had (substantially) the same declination as Herschel, Saturn, and Jupiter. Between noon on the 14th and noon on the 15th, Mars came to the opposition of Saturn retrogade. On the 15th, Herschel was in conjunction with the Sun, the planet having at the same time the exact parallel of declination of Saturn and Jupiter. So exceptional and extraordinary did these planetary positions and relationships appear to me that more than two years ago I made two crosses at the middle of July in my Ephemeris, and outlined a hand in the margin that I might not omit to note when the time came whether anything unusual occurred. Now the eclipse on the 12th took place on the Ascendant in the Revolutionary Figure of the Emperor Napoleon in square to Mars and opposition to Herschel, and according to the old astrologers "an eclipse of the Moon in Capricorn in evil aspect to Mars causes military disasters," whilst modern astrologers credit aspects of Herschel with producing events of a strange and unexpected character. Again the same figure presents the Moon in conjunction with Saturn retrograde on the place arrived at by Herschel by direction, whilst the Ascendant falls on the place attained by Saturn, the whole presenting, according to the canons of Astrology, a rare combination of evil portents. Probably it is the preceding data taken in conjunction with the primary directions (also of evil import) which have furnished the ground for the predictions of misfortune to the French Emperor to which Mr. Robinson alludes. T. S. PRIDEAUX

7, Eardley Crescent, West Brompton

[We insert this as a specimen of a kind of letter which it should be impossible to write in the nineteenth century.—ED.]

Insects upon a Swallow

DURING the month of August, at Meran, in the Tyrol, a swallow sitting upon a stone at the side of a public thoroughfare let me take it up without showing the least fear, or even moving. The cause of its indifference was immediately apparent; two large insects of a dark slate colour were running about the bird upon the outside of its feathers, their power of adhesion being considerable. While trying to remove them, one got upon my hand and was lost, being thrown some distance by the second of two hasty but vigorous shakes. The other fell to the ground after hanging by a thread, similar to, but much stronger than, a spider's single thread. The form of the insects was quadilateral, the head being at one of the angles, the measurement between the opposite angles being about ¼ inch; the strength of the skin was so great that the insect required three crushing rubs by a lady's foot against the road before its activity was destroyed. The bird seemed conscious of release from its parasites, and struggled to get away, and then was only just able to flutter languidly to a tree about forty yards distant. The toughness of the insect, its activity and power of clinging, fully account for the inability of the bird to free itself.

I have seen an account *somewhere* of a bird, whether a swallow or lark I forget, similarly troubled, and showing the same fearlessness of capture. G. H. H.

Birkenhead, Sept. 8

NOTES

PROFESSOR HUXLEY's presidential address is not his only outcome at Liverpool which it is our duty to chronicle—a duty which we perform with gratitude to him for his plain speaking. At the unveiling of Mr. Gladstone's statue on the 14th inst., Mr. Huxley, after referring to the Compulsory Education measure, which promises in time to rid us of our worse than Eastern degradation, as one of Mr. Gladstone's greatest achievements, added that if he might presume to give advice to a man so eminent as Mr. Gladstone—if he might ask him to raise to a still higher point the lustre which would hereafter surround his name in the annals of the country, it was that he should recollect there was more than one sort of learning, and that the one sort which was more particularly competent to cause the development of the great interests of the country, was that learning which we were in the habit of calling Science. That Mr. Gladstone was profoundly acquainted with literature, that he was an acute and elegant scholar, they all knew, but he suspected that the full importance for the practical interests of the country of developing what was known as Science was not quite so clear to the Prime Minister as it might be But, seeing the great faculty of development which his past career had shown, he had no doubt that such a man would by-and-by see that if this great country was to become what it should be, he must not only put the means of education within the reach of every person in the land, but must take care that the education was of such a nature as to provide those persons with the knowledge which they could apply to their pursuits, and which would tend to make them understand best those laws under which the human family existed.

THERE was still another outcome equally noticeable for its plain speaking, for, addressing the working men of Liverpool, the President of the British Association remarked, and his remarks were responded to by three cheers, that it had been a shock to him, walking through the streets of Liverpool, to see unwashed, unkempt, brutal people side by side with indications of the greatest refinement and the greatest luxury. He remarked upon this to working men because he believed it was the secret of the uneasiness and unrest which betrayed themselves in their political movements. The people who formed what are called the upper strata of society talked of political questions as if they were questions of Whig and Tory, of Conservative and Heaven knows what; but beneath there was the greater question whether that prodigious misery which dogs the footsteps of modern civilisation should be allowed to exist, and whether there should be in those nations which prided themselves most upon being Christian that predominant and increasing savagery of which such abundant specimens were seen in Liverpool. If in the course of history the savagery of nations has been gradually put down by one process only—by learning the laws of nature and the laws of social life, and obeying those laws—he urged upon them that history in this matter did not lie; and if they were to succeed in their great aspiration, they had only one course, and that was to learn the laws of nature and do their best to obey them. To that end all their efforts should be directed; to that end the great educational movement must be directed; and if their efforts were directed wisely and well, he could not doubt they would meet eventually with the most perfect measure of success.

THE war has, unfortunately, much curtailed the Reports of the British Association in the London Press, but this is not all, it is surely a matter for regret that the space given has been devoted to the least important papers. The Liverpool Press, too, has disappointed us, and has fallen far short of what has been done in smaller towns at recent meetings of the Association. We do not blame the editors, they know their public, and this is the real source of the evil: the British public, there is no concealing the fact, have as yet not the least idea of the importance of Science; they do not know what it means, or how it may help them in the daily affairs of life. One of the Liverpool reports has particularly struck us on account of its charming *naïveté*. Speaking of Professor Clerk Maxwell's paper on Hills and Dales, after remarking, with a tone of regret, that in Section A there was only a very small attendance which "consisted exclusively of mathematicians," the report adds, "The whole subject was treated in a very scientific manner, quite unlike what might have been supposed from the plain English title of the paper."

THE five classes of the Institute of France have unanimously resolved to draw up a protest in view of the eventual bombardment of the monuments, libraries, and museums of Paris. The protest will be addressed to every academy in the world, inviting them at the same time to give their adhesion to it. Meanwhile, the Minister of Public Instruction has been given a credit of 50,000f. to enable immediate precautions to be taken.

THE foundation stone of the new building about to be erected for Owens College, Manchester, is to be laid next Friday. His Grace the Duke of Devonshire, K.G., F.R.S., President of the Extension College, the Bishop of Manchester, and the President, and other distinguished members of the British Association are expected to be present and take part in the proceedings.

IT is said to be the intention of the Government to greatly increase the number of medical officers who are employed for the purposes of sanitary supervision under the Privy Council; the whole of the kingdom will be divided into sanitary districts, each under a medical officer with a salary of not less than 600*l.* per annum.

ANOTHER Dutch contribution to Natural History lies on our table in the form of a monograph of the Squirrel, "The Osteology and Myology of *Sciurus vulgaris* L., compared with the Anatomy of the Lemuridæ and the Chiromys, and on the Position of the latter in the Natural System," by Dr. C. K. Hoffmann and H. Weyenbergh, jun. It is a prize essay for the Dutch Academy of Sciences at Haarlem, but is written in German.

PROF. RAULIN'S "Description physique et naturelle de l'île de Crête," is published under the authority of the French Minister of Public Instruction in two thick volumes and an atlas; the first volume including the history, geography, and statistics of the island; the second, its meteorology, geology, botany, and zoology.

THE first annual part of Dr. G. Radde's Report of biological and geographical researches in the Caucasus country includes an account of journeys in the Mingrelian mountains, and the three mountain-valleys Rion, Tskenis-Tsqabi, and Ingur, with maps and plates, the latter beautifully executed and coloured.

A SERIES of experiments has been made at the Government farm in Madras on the applicability of the "gram" plant as fodder for cattle and horses in those districts instead of the seed; it succeeds well on poor soil, and gives four crops in the year. It has been found superior to grass, and can be made into hay.

THERE is good report of the progress of ipecacuanha cultivation in India, where it is found so valuable in that prevalent disease, dysentery. Since Dr. John Murray obtained for it the notice of the Indian Government, it has been successfully planted in the Neilgherries and other of our hill settlements, and in the plains. It has done well even at Calcutta.

A SOLUTION of tannin has been used in the treatment of cotton fabrics, as are hides in the manufacture of leather, and, according to *Cosmos*, the cotton thereby acquires greater strength, and resists moisture and disintegrating effects better. No attempt is made to explain the chemical reaction which produces this important change, but it is believed that the change cannot be great, since it has escaped the notice of practical tanners.

THE Rev. Cyrus Byington's "Grammar of the Choctaw Language," the manuscript of which is in the possession of the American Philosophical Society of Philadelphia, has just been published under the editorial supervision of Dr. D. G. Brinton. Mr. Byington was a native of Stockbridge, Massachusetts, and from 1819 to the close of his life, in 1868, was a missionary among the Choctaws, whose language he studied with so much thoroughness, that when he died he was engaged in revising his grammar for the seventh time; and his family still have a Choctaw dictionary, embracing 15,000 words, which was left, like the grammar, in manuscript. Another work of interest to philologists has been printed at Bogota, in New Granada: a Grammar of the Chibcha Language, by Dr. Ezequiel Uricoechea. The title is:—"Grammatica. Frases, Oraciones, Cathezisms, Confessonario y Boca Bulario de la Lengua Chibcha, 1620. Copiada del Manuscrito Orijinal por E. Uricoechea." The volume fills 64 pages in 8vo.

SALMON, it appears, are found in great abundance on the Pacific coast. The *San Francisco Bulletin* says, "From Mexico to Alaska every clear stream running into the ocean is frequented by salmon. These fish even ascend small streams which one can jump across, and the number which frequent large streams is wonderful. The size, quality, and shape vary considerably in the different streams, the largest being caught in the Sacramento river. While the salmon theoretically must have clear water, it is remarkable that it seems to thrive in the muddy waters of the Sacramento." Here is possibly some news for Mr. Frank Buckland.

THE Government of Honduras, in Central America, has granted to M. de la Roche for ten years the exclusive privilege of planting and exporting Corozo nut, paying two reals each hundred weight as royalty. What Corozo nut may mean, it is not easy to say.

MR. E. G. SQUIER, late commissioner of the United States in Peru, reprints a paper read before the American Geographical Society, containing Observations on the Geography and Archæology of Peru.

PROFESSORS JOHN TORREY and Asa Gray reprint from the Proceedings of the American Academy of Arts and Sciences their "Revision of the Eriogoneæ," a tribe of the order *Polygonaceæ*, first instituted by Mr. Bentham, wholly American, and especially characteristic of the drier western regions of the northern continent. They recognise seven genera, the same number as Mr. Bentham, the number of species being increased from 105 to 115.

THE Quekett Microscopical Club has just issued its fifth Report. The number of members has increased during the last five years from eleven to over 500, "all imbued with a strong desire to seek out the unfathomable stores of interest revealed by the microscope, and all influenced by that insatiable thirst for the observation of the minute and the beautiful that only the microscope can open to view." The club now meets twice a month throughout the year, at University College, Gower Street, and excursions are made during the summer season for the purpose of providing microscopical research.

WE have just received the prospectus of the Ladies' Educational Association, and we are glad to see that the lectures will include two subjects in Science, a chemical course by Professor Williamson, and a course of eighteen lectures on experimental physics, by Professor G. Carey Foster. Both courses will be delivered at University College. By applying to the secretary, a free ticket can be procured for the first lecture of each course ; those requiring class tickets, free tickets for opening lectures, prospectuses, and information, are requested to send to the hon. secretary, J. E. Milne, Esq., 27, Oxford-square, Hyde-park, W.

THE two volumes now published of Willkomm and Lange's "Prodromus Floræ Hispanicæ," include the Ferns, Gymnospermæ, Monocotyledones, Apetalæ, and Gamopetalæ.

EXPERIENCE only too clearly shows that familiarity breeds contempt. Even earthquakes are now quite appreciated in some parts of the world. Thus the *San Francisco Bulletin* of the 11th August says, "Popular prejudice is rather in favour of these lighter demonstrations of subterranean force, as they seem to stave off the heavier shocks." What next?

THE Smithsonian Institute of Washington has appointed a committee of scientific men to make a series of experiments to ascertain the temperature of the earth's crust at a considerable depth below the surface. For this purpose an artesian well at St. Louis is to be utilised, and as this is 3,843 feet deep some interesting results may be looked for.

THE decree of the Committee of the National Defence of Paris announcing that all woods and forests which might endanger the defence of the country will be set on fire on the approach of the enemy, has already been acted upon to a large extent. Independently of the loss in an artistic and æsthetic point of view, we can hardly be aware, probably the Parisians are hardly aware themselves of the amount of self-sacrifice this resolution will entail on themselves, and on their descendants. For some years past the climate of the central regions of France has been rapidly becoming drier, to the serious injury of many of the crops, a result attributed in part to the extensive cutting down of forests. The destruction of the world-famous forests of Fontainebleau, St. Cloud, St. Germain, and the Bois de Boulogne, will involve a material loss to the country, possibly hardly exceeded by the actual expenses of the war itself.

THE BRITISH ASSOCIATION

LIVERPOOL, *Tuesday Morning*

THE Liverpool meeting of the British Association is a great success. Distinguished visitors, a large company, interesting papers, and splendid weather, have all combined in its favour. The hotels are all full to overflowing, and accommodation extremely difficult to get. Almost all our well-known *habitués* are here ; and among foreigners, Henry, Van Beneden, Stricker, Bolzani, and a number of others, lend lustre to the meetings. Professor Huxley's address on Wednesday was listened to by a large and attentive audience, who appeared thoroughly to follow his train of argument. At the general committee some new regulations of considerable importance were proposed, particulars of which will be found in another column. On Thursday a casual visitor to Liverpool would see at once, on emerging from the railway station at Lime-street, that something unusual was stirring. The centre of operations was the space that includes St. George's Hall, where several of the sections are located, and the closely adjoining Derby Museum, where the Biological Section is to be found, and the reception-room, reading-room, and post-office. In this space and the neighbouring streets, the members of the Association may be recognised by the little blue or buff map-cards they carry, as symbolical as Murray in a Continental tour. Taking the various sections in turn, we find three located in St. George's Hall, A, E, and G. The Geographical Section is always a popular one, and attracted the largest audience of any to listen to the well-known and popular president, Sir Roderick Murchison, deliver his opening address. The room selected was the small concert-room—not a very small one, by-the-bye—which was well filled, a large proportion of the audience being ladies, though I am afraid Sir Roderick's failing voice hardly reached the whole of the company. How large a proportion stayed to hear Sir H. Rawlinson's report on the Site of Paradise I did not wait to see. The two other sections in St. George's Hall, the Mathematical and Mechanical, had to be reached by long winding passages, past rooms redolent of the law ; and were, of course, much more thinly attended, but very few ladies being met with here. Crossing over to the Derby Museum, and passing through the Reception Room, we reach the Free Public Library, where Professor Rolleston enchained a large audience by an address of upwards of an hour, which was generally admitted to be *the* address of the day. His commanding presence, measured diction, and his happy hits and classical allusions, exercised a great charm over the meeting, and many were the inquiries where a report of the speech was to be obtained. This section then divided into three sub-sections, under the presidency of Prof. Rolleston, Prof. Michael Foster, and Mr. John Evans, each of which attracted a goodly number of visitors. A very large audience listened to Dr. Brown-Séquard's long but very interesting account of his researches on the nervous system of guinea-pigs, of which little animal it is said, that he has left two thousand behind him in Paris, which, he fears, may fall a prey to the Prussians or to the exigencies of the siege. Those sections which are held at comparatively remote places are at a certain disadvantage, though many of the visitors appeared to avail themselves of cabs or of the luxury of the tramway omnibuses, which London does not yet possess, to visit them. The Geological Section is held in a small and not very convenient room, the Concert Hall, in Lord Nelson Street. Here there was no opening address, and the attendance was small. The most remote of all is the Chemical Section at the Royal Institution in Colquitt Street. At the Section for Economic Science, in the Town Hall, I arrived just in time to hear the venerable Sir John Bowring propose a vote of thanks to the president, Prof. Jevons, for his opening address. The "economic" rule was there laid down, which might have been adopted with advantage in some of the other sections, that, during a discussion, no one was to speak for more than ten minutes. Considering the distance from the central buildings, the attendance here was not small; and the committee of this section appears to contain the largest infusion of the aristocratic element—Lord Derby, Lord Houghton, Sir J. K. Shuttleworth, and Lord Neaves. The first day was closed by a *soirée* given by the Mayor in the Town Hall, where about 1,400 visitors were collected, consisting of members of the Association and the principal visitors in Liverpool. A *soirée* was also held in the Free Public Library, where Mr. Moore and the Rev. H. H. Higgins had got together a very large and interesting collection of scientific objects and works of art, including the well-known and valuable "Mayer collection."

On Friday and Saturday I confined my visits to the several sections comprised under the head of Natural Science. In the Geological Section there have been several papers of great interest, Mr. Judd's having attracted particular attention, and the discussion which followed was thought very interesting. The chemists do not appear to have mustered in force; at all events, that section took a holiday on Saturday, and a good many interested in it have seized the opportunity of visiting some of the numerous chemical works in the neighbourhood. Section D, on the other hand, has suffered from a plethora of papers, notwithstanding its division into three sub-sections. The Geographical Section is always popular. Great disappointment was expressed at the withdrawal of Mr. Hepworth Dixon's paper, which was, however, to some extent compensated by the part he took in the discussion which followed Governor Gilpin's paper on Colorado. But the greatest crush on Friday was in Prof. Rolleston's sub-section; and the interest excited on the subject of Spontaneous Generation, by the president's opening address, was shown by the attention with which the crowded audience listened to a two-hours' discussion, for which purpose the three sub-sections were for a time united. The campaign was opened by a description of some recent experiments by Dr. Child, the well-known advocate of spontaneous generation, on Abiogenesis. This was followed up by an elaborate paper, on the other side, from Mr. Samuelson, which was succeeded by some remarks from Dr. Crace Calvert, who spoke of the extreme difficulty with which the atmosphere can be entirely freed from organic germs. The discussion which followed was sustained by Dr. Hooker, Mr. Bentham, and other distinguished naturalists, entirely on the orthodox side of Biogenesis. In the remarks with which he summed up the debate, Prof. Rolleston complimented Dr. Child on the gallantry with which he had stood in the breach alone against such a consensus of opposition; and expressed his regret at the absence of the great champion of Abiogenesis, Dr. Charlton Bastian—the report of whose experiments in NATURE, he said, it was the bounden duty of every one who had now heard the other side of the question to read. Prof. Huxley was not present during the discussion; and though Mr. Herbert Spencer was, he took no part in it. It is rumoured that the combat is to be renewed to-morrow by the chiefs themselves: if this is the case, probably the whole Association will be there to hear them. In a discussion which took place in the same section on Thursday, Mr. Gwyn Jeffreys spoke of the hindrance which the *Porcupine* dredging expedition in the Mediterranean had experienced from the unfavourable weather. He mentioned the fact, which is but little known to naturalists, and which is not without importance in reference to the theory of glacial epochs, that the species of mollusca dredged up from great depths in the Mediterranean in previous expeditions are identical with Arctic species. It is very gratifying that Prof. Wyville Thomson, who was prevented from taking his share in that expedition through illness, is now completely restored to health. Prof. Tyndall's eloquent discourse on Friday on the scientific uses of the imagination was just of the kind which pleases the audience for whom it was intended, and was rapturously applauded.

At a meeting of the General Committee yesterday afternoon, Prof. Huxley in the chair, invitations were read for meetings of the Association to be held at Edinburgh, Brighton, Bradford, and Belfast. The Scotch metropolis was represented by Prof. Balfour and Sir Walter Elliot, the southern watering-place by Mr. Hallett and Mr. Mayall the photographer, the Yorkshire manufacturing town by the Mayor of Bradford and Mr. Alderman Law, and the Irish city by Dr. M'Gee and Mr. Patterson. In reference to Brighton, it was mentioned that the three south-eastern counties of England have together only enjoyed one meeting of the Association during the last twenty-five years, the Thames appearing to be a kind of Rubicon which the Association has found a difficulty in crossing. It was moved by Sir Roderick Murchison, seconded by Mr. Cowan (who stated that the neighbourhood of Edinburgh manufactured 200 miles of paper per diem), that the next meeting of the Association be held at Edinburgh. Lord Houghton then moved, in accordance with the new resolution passed by the General Committee, that the meeting for 1872 be held at Brighton. This was seconded by Mr. Gassiot, and also carried unanimously. The motion of Prof. Stokes, seconded by Mr. Spottiswoode, that Sir William Thomson be the president-elect, was received with great enthusiasm. Sir Walter Elliot proposed, and Prof. Rolleston seconded, the appointment of the vice-presidents for 1871, viz., the Duke of Buccleuch, the Lord Provost of Edinburgh, the Right Hon. J. Inglis, Sir Alexander Grant, Sir Roderick Murchison, Sir Chas. Lyell, Dr. Lyon Playfair, and Dr. Christison. Prof. Crum Brown, Mr. Ed. Sang, and Mr. T. B. Margaret are to be the local secretaries; and the time fixed was the middle of August, the day to be settled by the Council. The committee then resumed the subject of Vivisection, which had been adjourned from the last meeting. Mr. Johnstone Stoney proposed the resolution of which he had given notice, that "Having regard to the well-known character of the British Association, and to the circumstance that the business of the General Committee is necessarily transacted under pressure of time, it is not expedient, under ordinary circumstances, that it be recommended to this committee to appoint committees or pass votes for investigations to be carried on by the method of vivisection." This resolution Mr. Stoney was anxious to withdraw in favour of two others, but the chairman decided that these could only be brought forward in the form of amendments. The original resolution was therefore seconded, *pro formâ*, by Prof. Stokes. Mr. Samuelson then proposed as an amendment, "That the committee of Section D be requested to draw up a statement of their views upon physiological experiments in their various bearings, and that this document be circulated among the members of the Association, and that the said committee be further requested to consider from time to time whether any steps can be taken by them or by the Association which will tend to reduce to its minimum the sufferings entailed by legitimate physiological inquiries, or which will have the effect of employing the influence of the Association in discouragement of experiments which are not clearly legitimate of living animals." The amendment was seconded by Professor Rolleston, and carried by a large majority. The following appointments were then made :—Council : The President and President elect ; Vice-president and Vice-presidents elect ; General Secretaries and Assistant-secretary ; General Treasurer ; trustees, presidents of former years, and the following gentlemen :—Mr. Bateman, Dr. Beddoe, Mr. G. Busk, Dr. Debus, Mr. Warren Delarue, Mr. J. Evans, Captain Galton, Mr. F. Galton, Mr. Gassiot, Mr. Godwin-Austen, Lord Houghton, Mr. W. Huggins, Sir John Lubbock, Prof. W. A. Miller, Mr. Newmarch, Sir S. Northcote, Prof. Ramsay, Prof. Rankine, Dr. J. Simon, Lieut.-Col. Strange, Col. Sykes, Sir W. Tite, Prof. Tyndall, Mr. A. R. Wallace, Prof. Wheatstone, Prof. A. W. Williamson. General Secretaries, Prof. Hirst and Dr. Thomas Thomson. Assistant Secretary, Mr. Griffiths. Treasurer, Mr. Spottiswoode. Auditors, Mr. G. Busk, Dr. M. Foster, Mr. Gwyn Jeffreys. Mr. J. Evans and Dr. M. Foster were added to the Committee of Recommendations. B.

REPORT OF THE COUNCIL

"The Council have received the usual reports from the General Treasurer and from the Kew Committee. Their reports for the past year will be laid before the General Committee this day.

The Council have to report upon the action they have taken relative to each of the four resolutions referred to them by the General Committee at Exeter.

The first of these resolutions was—

'That the Council be requested to take into their consideration the existing relations between the Kew Committee and the British Association.'

The Council accordingly appointed a Committee of their own body to examine into these relations. This Committee had before them a special report drawn up by the Kew Committee, and, after due deliberation, they recommended—

'That the existing relations between the Kew Observatory and the British Association be continued unaltered until the completion, in 1872, of the magnetic and solar decennial period; but that after that date all connexion between them shall cease.'

The Council adopted this recommendation, and now offer it, as their own, to the General Committee.

The second resolution referred to the Council was as follows:—

'That the full influence of the British Association for the Advancement of Science should at once be exerted to obtain the appointment of a Royal Commission to consider—

First, the character and value of existing institutions and facilities for scientific investigation, and the amount of time and money devoted to such purposes.

Secondly, what modifications or augmentations of the means and facilities that are at present available for the maintenance and extension of science are requisite ; and,

Thirdly, in what manner these can be best supplied.'

By a third resolution the Council was 'requested to ascertain whether the action of Government in relation to the higher scientific education has been in accordance with the principles of impartiality which were understood to guide them in this matter ; and to consider whether that action has been well calculated to utilise and develop the resources of the country for this end, and to favour the free development of the higher scientific education. That the Council be requested to take such measures as may appear to them best calculated to carry out the conclusions to which they may be led by these inquiries and deliberations.'

The Committee of the Council appointed to consider these two resolutions reported their opinion to be favourable to the appointment of a Royal Commission to inquire into the relations of the State to scientific instruction and investigation ; and they added that no such inquiry would, in their opinion, be complete which did not extend itself to the action of the State in relation to scientific education, and the effect of that action upon independent educational institutions.

Your President and Council, acting on the advice of this Committee, constituted themselves a Deputation and waited upon the Lord President of the Council. They are glad to be able to report that their efforts to bring this important subject before Her Majesty's Government have been attended with success. On the 18th of May, Her Majesty issued a Commission "to make inquiry with regard to Scientific Instruction and the Advancement of Science, and to inquire what aid thereto is derived from grants voted by Parliament or from endowments belonging to the several universities in Great Britain and Ireland and the colleges thereof, and whether such aid could be rendered in a manner more effectual for the purpose." The Commissioners appointed by Her Majesty are the Duke of Devonshire, the Marquis of Lansdowne, Sir John Lubbock, Bart., Sir James Phillips Kay Shuttleworth, Bart., Bernhard Samuelson, Esq., M.P., Dr. Sharpey, Professor Huxley, Dr. W. A. Miller, and Professor Stokes. J. Norman Lockyer, Esq., F.R.S., has been appointed Secretary to the Commissioners, who, up to last July, were engaged taking evidence with great assiduity, and have now adjourned their meetings until November. There is every reason to hope that valuable results will follow from their deliberations.

The fourth resolution which the General Committee referred to the Council was—

'That the rules under which members are admitted to the General Committee be reconsidered.'

A Committee of the Council devoted considerable care to a revision of the existing rules. The modified rules approved by the Council are now submitted for adoption to the present General Committee, whose constitution is, of course, not affected thereby. The most important of the proposed changes are that henceforth new claims to membership of the General Committee shall be forwarded to the Assistant General Secretary at least one month before the next ensuing Annual Meeting of the Association ; that these claims shall be submitted to the Council, whose decision upon them is to be final ; and that henceforth it is not the authorship of a paper in the Transactions of a scientific society which is alone to constitute a claim to membership of the General Committee, but the publication of any works or papers which have furthered the advancement of any of the subjects taken into consideration at the Sectional meetings of the Society.

Your Council has, also, had under its consideration the desirability of removing certain administrative inconveniences which arise from the circumstance that the next place of meeting is never decided upon by the General Committee until near the close of the actual meeting. They are of opinion that the arrangements of the General Officers would be greatly facilitated, and at the same time the convenience of those who invite the Association consulted, if the General Committee were to decide upon each place of meeting a year earlier than they do at present. In order to make the transition from the existing practice to the proposed one, your Council recommend that two of the invitations which will be received at the present meetings be accepted, one for 1871, and another for 1872.

It has often been urged that the Association labours under disadvantages in consequence of its not possessing central offices in London, where its Council and numerous committees could hold their meetings, where the books and memoirs which have been accumulating for years could be rendered accessible to Members, and where information concerning the Association's proceedings could be promptly obtained during the interval between annual meetings. The Council have had the subject under consideration, and in the event of the establishment at Kew being discontinued, they are prepared to recommend that suitable rooms, in a central situation, should be procured. The additional annual expenditure which this would involve would probably not exceed 150*l.*

The Council have added the names of Professor H. A. Newton and Professor C. S. Lyman, who were present at the Exeter meeting, to the list of corresponding members."

We append the new rules referred to in the Council's Report.

" *New Rules for Admission to the General Committee*

The General Committee will in future consist of the following classes of members :—

CLASS A.—PERMANENT MEMBERS

1. Members of the Council, presidents of the Association, and presidents of sections for the present and preceding years, with authors of reports in the Transactions of the Association.

2. Members who, by the publication of works or papers, have furthered the advancement of those subjects which are taken into consideration at the sectional meetings of the Association. With a view of submitting new claims under this rule to the decision

of the Council, they must be sent to the assistant general secretary at least one month before the meeting of the Association. The decision of the Council on the claims of any member of the Association to be placed on the list of the General Committee to be final.

CLASS B.—TEMPORARY MEMBERS

3. The president for the time being, or, in his absence, one delegate representing him, of any scientific society publishing transactions. Claims under this rule to be sent to the assistant general secretary before the opening of the meeting.

4. Office-bearers for the time being, or delegates, altogether not exceeding three, from scientific institutions established in the place of meeting. Claims under this rule to be approved by the local secretaries before the opening of the meeting.

5. Foreigners and other individuals whose assistance is desired, and who are specially nominated in writing, for the meeting of the year, by the president and general secretaries.

6. Vice-presidents and secretaries of sections."

SECTIONAL PROCEEDINGS

SECTION A.—*Mathematical and Physical Science.*—President, Prof. J. Clerk Maxwell, F.R.S.

The president delivered the following address:—

AT several of the recent meetings of the British Association the varied and important business of the Mathematical and Physical Section has been introduced by an Address, the subject of which has been left to the selection of the president for the time being. The perplexing duty of choosing a subject has not, however, fallen to me. Professor Sylvester, the president of Section A at the Exeter meeting, gave us a noble vindication of pure mathematics by laying bare, as it were, the very working of the mathematical mind, and setting before us, not the array of symbols and brackets which form the armoury of the mathematician, or the dry results which are only the monuments of his conquests, but the mathematician himself, with all his human faculties directed by his professional sagacity to the pursuit, apprehension, and exhibition of that ideal harmony which he feels to be the root of all knowledge, the fountain of all pleasure, and the condition of all action. The mathematician has, above all things, an eye for symmetry; and Professor Sylvester has not only recognised the symmetry formed by the combination of his own subject with those of the former presidents, but has pointed out the duties of his successor in the following characteristic note:—

"Mr. Spottiswoode favoured the Section, in his opening address, with a combined history of the progress of mathematics and physics; Dr. Tyndall's address was virtually on the limits of physical philosophy; the one here in print," says Professor Sylvester, "is an attempted faint adumbration of the nature of mathematical science in the abstract. What is wanting (like a fourth sphere resting on three others in contact) to build up the ideal pyramid is a discourse on the relation of the two branches (mathematics and physics) to, and their action and reaction upon, one another—a magnificent theme, with which it is to be hoped that some future president of Section A will crown the edifice, and make the tetralogy (symbolisable by $A+A'$, A, A', AA') complete."

The theme thus distinctly laid down for his successor by our late President is indeed a magnificent one, far too magnificent for any efforts of mine to realise. I have endeavoured to follow Mr. Spottiswoode, as with far-reaching vision he distinguishes the systems of science into which phenomena, our knowledge of which is still in the nebulous stage, are growing. I have been carried by the penetrating insight and forcible expression of Dr. Tyndall into that sanctuary of minuteness and of power where molecules obey the laws of their existence, clash together in fierce collision, or grapple in yet more fierce embrace, building up in secret the forms of visible things. I have been guided by Professor Sylvester towards those serene heights

> "Where never creeps a cloud, or moves a wind,
> Nor ever falls the least white star of snow,
> Nor ever lowest roll of thunder moans,
> Nor sound of human sorrow mounts, to mar
> Their sacred everlasting calm."

But who will lead me into that still more hidden and dimmer region where Thought weds Fact; where the mental operation of the mathematician and the physical action of the molecules are seen in their true relation? Does not the way to it pass through the very den of the metaphysician, strewed with the remains of former explorers, and abhorred by every man of science? It would indeed be a foolhardy adventure for me to take up the valuable time of the section by leading you into those speculations which require, as we know, thousands of years even to shape themselves intelligibly.

But we are met as cultivators of mathematics and physics. In our daily work we are led up to questions the same in kind with those of metaphysics; and we approach them, not trusting to the native penetrating power of our own minds, but trained by a long-continued adjustment of our modes of thought to the facts of external nature. As mathematicians, we perform certain mental operations on the symbols of number or of quantity, and, by proceeding step by step from more simple to more complex operations, we are enabled to express the same thing in many different forms. The equivalence of these different forms, though a necessary consequence of self-evident axioms, is not always, to our minds, self-evident; but the mathematician, who, by long practice, has acquired a familiarity with many of these forms, and has become expert in the processes which lead from one to another, can often transform a perplexing expression into another which explains its meaning in more intelligible language.

As students of physics, we observe phenomena under varied circumstances, and endeavour to deduce the laws of their relations. Every natural phenomenon is, to our minds, the result of an infinitely complex system of conditions. What we set ourselves to do is to unravel these conditions, and by viewing the phenomenon in a way which is in itself partial and imperfect, to piece out its features one by one, beginning with that which strikes us first, and thus gradually learning how to look at the whole phenomenon so as to obtain a continually greater degree of clearness and distinctness. In this process, the feature which presents itself most forcibly to the untrained inquirer may not be that which is considered most fundamental by the experienced man of science; for the success of any physical investigation depends on the judicious selection of what is to be observed as of primary importance, combined with a voluntary abstraction of the mind from those features which, however attractive they appear, we are not yet sufficiently advanced in science to investigate with profit.

Intellectual processes of this kind have been going on since the first formation of language, and are going on still. No doubt the feature which strikes us first and most forcibly in any phenomenon, is the pleasure or the pain which accompanies it, and the agreeable or disagreeable results which follow after it. A theory of nature from this point of view is embodied in many of our words and phrases, and is by no means extinct even in our deliberate opinions. It was a great step in science when men became convinced that, in order to understand the nature of things, they must begin by asking, not whether a thing is good or bad, noxious or beneficial, but of what kind is it? and how much is there of it? Quality and quantity were then first recognised as the primary features to be observed in scientific inquiry. As science has been developed, the domain of quantity has everywhere encroached on that of quality, till the process of scientific inquiry seems to have become simply the measurement and registration of quantities, combined with a mathematical discussion of the numbers thus obtained. It is this scientific method of directing our attention to those features of phenomena which may be regarded as quantities which brings physical research under the influence of mathematical reasoning. In the work of the section we shall have abundant examples of the successful application of this method to the most recent conquests of science; but I wish at present to direct your attention to some of the reciprocal effects of the progress of science on those elementary conceptions which are sometimes thought to be beyond the reach of change.

If the skill of the mathematician has enabled the experimentalist to see that the quantities which he has measured are connected by necessary relations, the discoveries of physics have revealed to the mathematician new forms of quantities which he could never have imagined for himself. Of the methods by which the mathematician may make his labours most useful to the student of nature, that which I think is at present most important is the systematic classification of quantities. The quantities which we study in mathematics and physics may be classified in two different ways. The student who wishes to master any particular science must make himself familiar with the various kinds of quantities which belong to that science. When he understands all the relations between these quantities, he regards them as forming a connected system, and he classes the whole system of quantities together as belonging to that particular science. This classification is the most natural from a physical point of view, and it is generally the first in order of time. But when the student has become acquainted with several different sciences, he finds that the mathematical processes and trains of reasoning in one science resemble those in another so much that his knowledge of the one science may be made a most useful help in the study of the other. When he examines into the reason of this, he finds that in the two sciences he has been dealing with systems of quantities, in which the mathematical forms of the relations of the quantities are the same in both systems, though the physical nature of the quantities may be utterly different. He is thus led to recognise a classification of quantities on a new principle, according to which the physical nature of the quantity is subordinated to its mathematical form. This is the point of view which is characteristic of the mathematician; but it stands second to the physical aspect in order of time, because the human mind, in order to conceive of different kinds of quantities, must have them presented to it by nature. I do not here refer to the fact that all quantities, as such, are subject to the rules of arithmetic and algebra, and are therefore capable of being submitted to those dry calculations which represent, to so many minds, their only idea of mathematics. The human mind is seldom satisfied, and is certainly never exercising its highest functions, when it is doing the work of a calculating machine. What the man of science, whether he be a mathematician or a physical inquirer, aims at is, to acquire and develop clear ideas of the things he deals with. For this purpose he is willing to enter on long calculations, and to be for a season a calculating machine, if he can only at last make his ideas clearer. But if he finds that clear ideas are not to be obtained by means of processes, the steps of which he is sure to forget before he has reached the conclusion, it is much better that he should turn to another method, and try to understand the subject by means of well-chosen illustrations derived from subjects with which he is more familiar. We all know how much more popular the illustrative method of exposition is found, than that in which bare processes of reasoning and calculation form the principal subject of discourse. Now a truly scientific illustration is a method to enable the mind to grasp some conception or law in one branch of science, by placing before it a conception or a law in a different branch of science, and directing the mind to lay hold of that mathematical form which is common to the corresponding ideas in the two sciences, leaving out of account for the present the difference between the physical nature of the real phenomena. The correctness of such an illustration depends on whether the two systems of ideas which are compared together are really analogous in form, or whether, in other words, the corresponding physical quantities really belong to the same mathematical class. When this condition is fulfilled, the illustration is not only convenient for teaching science in a pleasant and easy manner, but the recognition of the mathematical analogy between the two systems of ideas leads to a knowledge of both, more profound than could be obtained by studying each system separately.

There are men who, when any relation or law, however complex, is put before them in a symbolical form, can grasp its full meaning as a relation among abstract quantities. Such men sometimes treat with indifference the further statement that quantities actually exist in nature which fulfil this relation. The mental image of the concrete reality seems rather to disturb than to assist their contemplations. But the great majority of mankind are utterly unable, without long training, to retain in their minds the unembodied symbols of the pure mathematician; so that if science is ever to become popular and yet remain scientific, it must be by a profound study and a copious application of those principles of truly scientific illustration which, as we have seen, depend on the mathematical classification of quantities. There are, as I have said, some minds which can go on contemplating with satisfaction pure quantities presented to the eye by symbols, and to the mind in a form which none but mathematicians can conceive. There are others who feel more enjoyment in following geometrical forms, which they draw on paper, or build up in the empty space before them. Others, again, are not content unless they can project their whole physical energies into the scene which they conjure up. They learn at what a rate the planets rush through space, and they experience a delightful feeling of exhilaration. They calculate the forces with which the heavenly bodies pull at one another, and they feel their own muscles straining with the effort.

To such men impetus, energy, mass, are not mere abstract expressions of the results of scientific inquiry. They are words of power which stir their souls like the memories of childhood. For the sake of persons of these different types, scientific truths should be presented in different forms, and should be regarded as equally scientific, whether it appears in the robust form and the vivid colouring of a physical illustration, or in the tenuity and paleness of a symbolical expression. Time would fail me if I were to attempt to illustrate by examples the scientific value of the classification of quantities. I shall only mention the name of that important class of magnitudes having direction in space which Hamilton has called Vectors, and which form the subject-matter of the Calculus of Quaternions—a branch of mathematics which, when it shall have been thoroughly understood by men of the illustrative type, and clothed by them with physical imagery, will become, perhaps under some new name, a most powerful method of communicating truly scientific knowledge to persons apparently devoid of the calculating spirit. The mutual action and reaction between the different departments of human thought is so interesting to the student of scientific progress, that, at the risk of still further encroaching on the valuable time of the Section, I shall say a few words on a branch of science which not very long ago would have been considered rather a branch of metaphysics: I mean the atomic theory, or, as it is now called, the molecular theory of the constitution of bodies. Not many years ago, if we had been asked in what regions of physical science the advance of discovery was least apparent, we should have pointed to the hopelessly distant fixed stars on the one hand, and to the inscrutable delicacy of the texture of material bodies on the other. Indeed, if we are to regard Comte as in any degree representing the scientific opinion of his time, the research into what takes place beyond our own solar system seemed then to be exceedingly unpromising, if not altogether illusory. The opinion that the bodies which we see and handle, which we can set in motion or leave at rest, which we can break in pieces and destroy, are composed of smaller bodies which we cannot see or handle, which are always in motion, and which can neither be stopped nor broken in pieces, nor in any way destroyed or deprived of the least of their properties, was known by the name of the Atomic Theory. It was associated with the names of Democritus and Lucretius, and was commonly supposed to admit the existence only of atoms and void, to the exclusion of any other basis of things from the universe.

In many physical reasonings and mathematical calculations we are accustomed to argue as if such substances as air, water, or metal, which appear to our senses uniform and continuous, were strictly and mathematically uniform and continuous. We know that we can divide a pint of water into many millions of portions, each of which is as fully endowed with all the properties of water as the whole pint was, and it seems only natural to conclude that we might go on subdividing the water for ever, just as we can never come to a limit in subdividing the space in which it is contained. We have heard how Faraday divided a grain of gold into an inconceivable number of separate particles, and we may see Dr. Tyndall produce from a mere suspicion of nitrite of butyle an immense cloud, the minute visible portion of which is still cloud, and therefore must contain many molecules of nitrite of butyle. But evidence from different and independent sources is now crowding in upon us which compels us to admit that if we could push the process of subdivision still further we should come to a limit, because each portion would then contain only one molecule, an individual body, one and indivisible, unalterable by any power in nature. Even in our ordinary experiments on very finely divided matter we find that the substance is beginning to lose the properties which it exhibits when in a large mass, and that effects depending on the individual action of molecules are beginning to become prominent. The study of these phenomena is at present the path which leads to the development of molecular science. That superficial tension of liquids which is called capillary attraction is one of these phenomena. Another important class of phenomena are those which are due to that motion of agitation by which the molecules of a liquid or gas are continually working their way from one place to another, and continually changing their course, like people hustled in a crowd. On this depends the rate of diffusion of gases and liquids through each other, to the study of which, as one of the keys of molecular science, that unwearied inquirer into nature's secrets, the late Prof. Graham, devoted such arduous labour.

The rate of electrolytic conduction is, according to Wiedemann's theory, influenced by the same cause; and the conduction of heat in fluids depends probably on the same kind of action.

In the case of gases, a molecular theory has been developed by Clausius and others, capable of mathematical treatment, and subjected to experimental investigation; and by this theory nearly every known mechanical property of gases has been explained on dynamical principles, so that the properties of individual gaseous molecules are in the fair way to become objects of scientific research. Now Sir William Thomson has shown by several independent lines of argument, drawn from phenomena so different in themselves as the electrification of metals by contact, the tension of soap-bubbles, and the friction of air, that in ordinary solids and liquids the average distance between contiguous molecules is less than the hundred-millionth, and greater than the two-thousand-millionth of a centimètre. This of course is an exceedingly rough estimate, for it is derived from measurements, some of which are still confessedly very rough; but if, at the present time, we can form even a rough plan for arriving at a result of this kind, we may hope that as our means of experimental inquiry become more accurate and more varied, our conception of a molecule will become more definite, so that we may be able at no distant period to estimate its weight. A theory which Sir W. Thomson has founded on Helmholtz's splendid hydrodynamical theorems, seeks for the properties of molecules in the ring-vortices of a uniform, frictionless, incompressible fluid. Such whirling rings may be seen when an experienced smoker sends out a dexterous puff of smoke into the still air, but a more evanescent phenomenon it is difficult to conceive. This evanescence is owing to the viscosity of the air; but Helmholtz has shown that in a perfect fluid such a whirling ring, if once generated, would go on whirling for ever, would always consist of the very same portion of the fluid which was first set whirling, and could never be cut in two by any natural cause. The generation of a ring-vortex is of course equally beyond the power of natural causes, but once generated, it has the properties of individuality, permanence in quantity, and indestructibility. It is also the recipient of impulse and of energy, which is all we can affirm of matter: and these ring-vortices are capable of such varied connections, and knotted self-involutions, that the properties of differently knotted vortices must be as different as those of different kinds of molecules can be.

If a theory of this kind should be found, after conquering the enormous mathematical difficulties of the subject, to represent in any degree the actual properties of molecules, it will stand in a very different scientific position from those theories of molecular action which are formed by investing the molecule with an arbitrary system of central forces invented expressly to account for the observed phenomena. In the vortex theory we have nothing arbitrary, no central forces or occult properties of any other kind. We have nothing but matter and motion, and when the vortex is once started its properties are all determined from the original impetus, and no further assumptions are possible. Even in the present undeveloped state of the theory, the contemplation of the individuality and indestructibility of a ring vortex in a perfect fluid cannot fail to disturb the commonly received opinion that a molecule, in order to be permanent, must be a very hard body. In fact one of the first conditions which a molecule must fulfil is, apparently, inconsistent with its being a single hard body. We know from those spectroscopic researches which have thrown so much light on different branches of science, that a molecule can be set into a state of internal vibration, in which it gives off to the surrounding medium light of definite refrangibility—light, that is, of definite wave-length and definite period of vibration. The fact that all the molecules, say of hydrogen, which we can procure for our experiments, when agitated by heat or by the passage of an electric spark, vibrate precisely in the same periodic time, or, to speak more accurately, that their vibrations are composed of a system of simple vibrations having always the same periods, is a very remarkable fact. I must leave it to others to describe the progress of that splendid series of spectroscopic discoveries by which the chemistry of the heavenly bodies has been brought within the range of human inquiry. I wish rather to direct your attention to the fact that not only has every molecule of terrestrial hydrogen the same system of periods of free vibration, but that the spectroscope examination of the light of the sun and stars shows that in regions the distance of which we can only feebly imagine there are molecules vibrating in as exact unison with the molecules of terrestrial hydrogen as two tuning forks tuned to correct pitch, or two watches regulated to solar time. Now this absolute equality in the magnitude of quantities, occurring in all parts of the universe, is worth our consideration. The dimensions of individual natural bodies are either quite indeterminate, as in the case of planets, stones, trees, &c., or they vary within moderate limit, as in the case of seeds, eggs, &c.; but, even in these cases, small quantitive differences are met with which do not interfere with the essential properties of the body. Even crystals, which are so definite in geometrical form, are variable with respect to their absolute dimensions. Among the works of man we sometimes find a certain degree of uniformity. There is a uniformity among the different bullets which are cast in the same mould, and the different copies of a book printed from the same type. If we examine the coins, or the weights and measures, of a civilised country, we find a uniformity, which is produced by careful adjustment to standards made and provided by the State. The degree of uniformity of these national standards is a measure of that spirit of justice in the nation which has enacted laws to regulate them and appointed officers to test them. This subject is one in which we, as a scientific body, take a warm interest, and you are all aware of the vast amount of scientific work which has been expended, and profitably expended, in providing weights and measures for commercial and scientific purposes. The earth has been measured as a basis for a permanent standard of length, and every property of metals has been investigated to guard against any alteration of the material standards when made. To weigh or measure anything with modern accuracy, requires a course of experiment and calculation in which almost every branch of physics and mathematics is brought into requisition.

Yet, after all, the dimensions of our earth and its time of rotation, though, relatively to our present means of comparison, very permanent, are not so by any physical necessity. The earth might contract by cooling, or it might be enlarged by a layer of meteorites falling on it, or its rate of revolution might slowly slacken, and yet it would continue to be as much a planet as before. But a molecule, say of hydrogen, if either its mass or its time of vibration were to be altered in the least, would no longer be a molecule of hydrogen. If, then, we wish to obtain standards of length, time, and mass which shall be absolutely permanent, we must seek them not in the dimensions, or the motion, or the mass of our planet, but in the wave-length, the period of vibration, and the absolute mass of these imperishable and unalterable and perfectly similar molecules. When we find that here, and in the starry heavens, there are innumerable multitudes of little bodies of exactly the same mass, so many, and no more, to the grain, and vibrating in exactly the same time, so many times, and no more, in a second, and when we reflect that no power in nature can now alter in the least either the mass or the period of any one of them, we seem to have advanced along the path of natural knowledge to one of those points at which we must accept the guidance of that faith by which we understand that "that which is seen was not made of things which do appear." One of the most remarkable results of the progress of molecular science is the light it has thrown on the nature of irreversible processes,—processes, that is, which always tend towards, and never away from, a certain limiting state. Thus if two gases be put into the same vessel they become mixed, and the mixture tends continually to become more uniform. If two unequally heated portions of the same gas are put into the vessel, something of the kind takes place, and the whole tends to become of the same temperature. If two unequally heated solid bodies be placed in contact, a continual approximation of both to an intermediate temperature takes place. In the case of the two gases, a separation may be effected by chemical means; but in the other two cases the former state of things cannot be restored by any natural process. In the case of the conduction or diffusion of heat the process is not only irreversible, but it involves the irreversible diminution of that part of the whole stock of thermal energy which is capable of being converted into mechanical work. This is Thomson's theory of the irreversible dissipation of energy, and it is equivalent to the doctrine of Clausius concerning the growth of what he calls Entropy. The irreversible character of this process is strikingly embodied in Fourier's theory of the conduction of heat, where the formulæ themselves indicate a possible solution of all positive values of the time which continually tends to a uniform diffusion of heat. But if we attempt to ascend the stream of time by giving to its symbol continually diminishing values, we are led up to a state of things in which the formula has what is called a critical value; and if we inquire into the state of things the instant before, we find that the formula becomes absurd. We thus arrive at the conception of a state of things which cannot be conceived as the physical result of a previous state of things, and we find that this critical condition actually existed at an epoch not in the utmost depths of a past eternity, but separated from the present time by a finite interval. This idea of a beginning is one which the physical researches of recent times have brought home to us, more than any observer of the course of scientific thought in former times would have had reason to expect. But the mind of man is not like Fourier's heated body, continually settling down into an ultimate state of quiet uniformity, the character of which we can already predict; it is rather like a tree shooting out branches which adapt themselves to the new aspects of the sky towards which they climb, and roots which contort themselves among the strange strata of the earth into which they delve. To us who breathe only the spirit of our own age, and know only the characteristics of contemporary thought, it is as impossible to predict the general tone of the science of the future as it is to anticipate the particular discoveries which it will make. Physical research is continually revealing to us new features of natural processes, and we are thus compelled to search for new forms of thought appropriate to these features. Hence the importance of a careful study of those relations between mathematics and physics which determine the conditions under which the ideas derived from one department of physics may be safely used in forming ideas to be employed in a new department. The figure of speech or of thought by which we transfer the language and ideas of a familiar science to one with which we are less acquainted may be called scientific metaphor. Thus the words velocity, momentum, force, &c., have acquired certain precise meanings in elementary dynamics. They are also employed in the dynamics of a connected system in a sense which, though perfectly analogous to the elementary sense, is wider and more general. These generalised forms of elementary ideas may be called metaphorical terms in the sense in which every abstract term is metaphorical. The characteristic of a truly scientific system of metaphors is that each term in its metaphorical use retains all the formal relations to the other terms of the system which it had in its original use. The method is then truly scientific, that is, not only a legitimate product of science, but capable of generating science in its turn. There are certain electrical phenomena, again, which are connected together by relations of the same form as those which connect dynamical phenomena. To apply to these the phrases of dynamics with proper distinctions and provisional reservations is an example of a metaphor of a bolder kind; but it is a legitimate metaphor if it conveys a true idea of the electrical relations to those who have been already trained in dynamics. Suppose, then, that we have successfully introduced certain ideas belonging to an elementary science by applying them metaphorically to some new class of phenomena. It becomes an important philosophical question to determine in what degree the applicability of the old ideas to the new subject may be taken as evidence that the new phenomena are physically similar to the old. The best instances for the determination of this question are those in which two different explanations have been given of the same thing. The most celebrated case of this kind is that of the corpuscular and the undulatory theories of light. Up to a certain point the phenomena of light are equally well explained by both; beyond this point one of them fails. To understand the true relation of these theories in that part of the field where they seem equally applicable we must look at them in the light which Hamilton has thrown upon them by his discovery that to every brachystochrone problem there corresponds a problem of free motion, involving different velocities and times, but resulting in the same geometrical path. Professor Tait has written a very interesting paper on this subject. According to a theory of electricity which is making great progress in Germany two electrical particles act on one another directly at a distance, but with a force which, according to Weber, depends on their relative velocity, and according to a theory hinted at by Gauss, and developed by Riemann, Lorenz, and Neumann, acts not instantaneously, but after a time depending on the distance. The power with which this theory, in the hands of these eminent men, explains every kind of electrical phenomena must be studied in order to be appreciated. Another theory of electricity which I prefer denies action at a distance and attributes electric action to tensions and pressures in an all-pervading medium, these stresses being the same in kind with those familiar to engineers, and the medium being identical with that in which light is supposed to be propagated. Both these theories are found to explain not only the phenomena by the aid of which they were originally constructed, but other phenomena which were not thought of, or perhaps not known at the time, and both have independently arrived at the same numerical result which gives the absolute velocity of light in terms of electrical quantities. That theories, apparently so fundamentally opposed, should have so large a field of truth common to both is a fact the philosophical importance of which we cannot fully appreciate till we have reached a scientific altitude from which the true relation between hypotheses so different can be seen.

I shall only make one more remark on the relation between mathematics and physics. In themselves, one is an operation of the mind, the other is a dance of molecules. The molecules have laws of their own, some of which we select as most intelligible to us and most amenable to our calculation. We form a theory from these partial data, and we ascribe any deviation of the actual phenomena from this theory to disturbing causes. At the same time, we confess that what we call disturbing causes are simply those parts of the true circumstances which we do not know or have neglected, and we endeavour in future to take account of them. We thus acknowledge that the so-called disturbance is a mere figment of the mind, not a fact of nature, and that in natural action there is no disturbance. But this is not the only way in which the harmony of the material with the mental operation may be disturbed. The mind of the mathematician is subject to many disturbing causes, such as fatigue, loss of memory, and hasty conclusions; and it is found that from these and other causes mathematicians make mistakes. I am not prepared to deny that, to some mind of a higher order than ours, each of these errors might be traced to the regular operation of the laws of actual thinking; in fact we ourselves often do detect, not only errors of calculation, but the causes of these errors. This, however, by no means alters our conviction that they are errors, and that one process of thought is right and another process wrong. One of the most profound mathematicians and thinkers of our time, the late George Boole, when reflecting on the precise and almost mathematical character of the laws of right thinking as compared with the exceedingly perplexing, though perhaps equally determinate, laws of actual and fallible thinking, was led to another of those points of view from which science seems to look out into a region beyond her own domain. "We must admit," he says, "that there exist laws" (of thought) "which even the rigour of their mathematical forms does not preserve from violation. We must ascribe to them an authority, the essence of which does not consist in power, a supremacy which the analogy of the inviolable order of the natural world in no way assists us to comprehend."

SECTION B.—*Chemical Science.*—President, Professor Roscoe, F.R.S.

Report of the Committee on the Chemical Nature of Cast Iron.—Mr. David Forbes, F.R.S., reported on behalf of Professor Abel, Dr. Matthiessen and himself, that it had not been in their power, as a Committee, to make any important progress in the investigation of the chemical nature of cast iron during the past year. This was partly owing to the dismantled condition of the required apparatus. The Committee asked to be reappointed, so that the experiments might be resumed without much further delay.

On a New Chlorine Process without Manganese.—Mr. Henry Deacon, of Widnes. As the closing paragraph of Professor Roscoe's address briefly and clearly described the essential nature of Mr. Deacon's process, it is not necessary further to refer to it, except to state that hitherto Mr. Deacon has not succeeded in making the process a commercial success, and he prefers in the meantime to employ Mr. Weldon's process, though he is satisfied that his own will yet become practically applicable in the production of chlorine for the manufacture of bleaching powder.

SECTION C.—*Geology.*—President, Sir Philip de Malpas Grey Egerton, Bart., M.P., F.R.S.

The President departed from the practice of giving an introductory address, inasmuch as the time of the Section would be fully occupied with the reading and discussion of the papers to be submitted to it.

On the Glaciated Surface of Triassic Rocks near Liverpool.—Mr. G. H. Morton. The grooves on the rocks were of a uniform direction, 35° W. of N., and were due to the action of land ice.

Mr. Boyd Dawkins and Rev. Mr. Crosskey distinguished between the deposits produced by land ice and the later deposits containing stones dropped from floating ice. Professor Hawkins insisted that the granite boulders of Lancashire and Cheshire were derived from Ravenglass and not from Shapfell as stated; but Professor Williamson and Mr. Dawkins asserted that they had found Shap boulders in the clay.

On Sections of Strata between Huyton and St. Helens, exposed in cuttings in the railway now making between these places.—Dr. Ricketts

Report on Slicing and Photographing of Fossil Corals.—Mr. James Thompson. The continued investigation of these fossils had added greatly to the number of species, and the prepared sections had exhibited indications which supplied characters by which recognised forms could be better distinguished and false species eliminated. The report was accompanied with a large series of slices and exquisite photographs, full of promise for the future students of the group.

Report on the Fossils of Kiltorcan.—Mr. W. H. Bailey. These consisted of specimens of *Cyclopteris hibernica*, the remains of *Sagenaria bayleana*, a freshwater shell, *Anodonta jukesii* a crustacean *Protocaris mchenrici*, and scales of *Coccosteus* and *Glyptolepis*. Mr. R. H. Scott stated that Professor Heer had determined the specific identity of fossils from Bear Island collected by the Swedish Polar expedition in 1869, with those from Kiltorcan. Mr. Carruthers had recently examined the extensive series of Kiltorcan Fossils at Dublin, made by Mr. Bailey. They supplied information regarding all the parts of the *Sagenaria* and *Cyclopteris*, enabling investigators to deal with them almost as satisfactorily as with recent plants.

Fourth Report on the Leaf-beds of the Bagshot Series of Hampshire.—Mr. W. S. Mitchell.

On the Evidences of Recent Changes of Level on the Mediterranean Coast.—Mr. G. Maw. The coast structure, the general absence of sea-cliffs within the straits of Gibraltar, due to the shelving of the contour of the land under the sea were described. The inset current from the Atlantic was considered as indicating a general subsidence of the whole Mediterranean basin. The submarine springs passing through channels of sub-aerial origin occurring in the coast caverns implied a submergence of the coast line. Evidences of upheaval were to be found in the lagoons and flats which abound on the coast of Corsica, and which are covered with long ridges of shingle deposited, it was believed, by streams which debouched on the marsh when it was submerged. Further at Gibraltar a great deposit of stratified sand in Catalan Bay, showing a submergence of 700 feet; and at Cadiz, as well as at Tangiers on the opposite side of the basin, raised sea beaches were found. Sir Charles Lyell thought the out-set current balanced the inset one and destroyed the value of that current as supporting the notion of submergence. Prof. Busk referred to the memoirs of the late Mr. Smith, of Jordanhill, on the Mediterranean basin, and at length gave his own observations on the rock terraces and caverns of the rock of Gibraltar. There were three successive terraces exhibited only on the eastern side of the rock, showing that a barrier extended in recent geological times across the straits confining the Mediterranean to a higher level than the Atlantic. The changes described by Mr. Maw were really the last of a series indicated by the terraces at Gibraltar. Prof. Ansted thought the great power of enormously swollen streams so common in Corsica and neighbouring regions might account for some of the phenomena referred to by the author.

On the Organisation and Affinities of the Calamites of the Coal Measures.—Prof. Williamson. Numerous well preserved and novel forms of stems of *Calamites* were described with great minuteness, and estimates of their systematic position, based on these structures, were suggested. The author described two forms of nodullary rays, and a third set of radiating cellular structure connected with nodes. He considered the phragmas seen on Calamite stems to indicate the attachment of the roots. He suggested that this group of fossils must be separated from the *Equisetaceæ* and placed in an order to which he proposed to join the name *Calamitaceæ*. Mr. Cann thus doubted whether systematic determinations based on stem structure were of value. He preferred the evidence derived from the fruit, and that had been determined to differ very little from the fruit of the living *Equisetum*. The various points of difference pointed out by the author he considered to depend upon the more highly organised vegetative portions of the fossils, and exogenous growth of the stems. Mr. Bentham insisted on the value of fruit characters for determining systematic position, and urged the desirability of employing subgeneric names for imperfectly determined forms.

SECTION D.—*Biological Science*—President, Prof. Rolleston, M.D., F.R.S.

The President delivered the following address:—

AMONGST the duties of the President of a Section the delivery of an Address has in these latter days somehow come to be reckoned, and that I may interpose myself for but as short a time as possible between your attention and the papers announced to you for reading upon your list, I will begin what I have to say without any further preface.

I wish first to make a few observations as to the kind of preparation which is indispensable, as it seems to me as a preliminary to an adequate and intelligent comprehension of the problems of biology; or, in other words, to an adequate and intelligent comprehension of the discussions which will take place in this room and in the two other rooms which will be assigned to, and occupied by, the departments of Ethnology and Anthropology, and that of Physiology Pure and Proper, and Anatomy.

Having made these observations, I propose, in the second place, to enumerate the subjects which appear likely to occupy prominent places in our forthcoming discussions; and thirdly, I will, if your patience allows me, conclude with some remarks as to certain of the benefits which may be expected, as having been constantly observed to flow from a due and full devotion to biological study.

In the first place, then, I wish to say that though the problems of biology have much of what is called general interest; that is to say, of interest for all persons, attaching to them, as indeed how could they fail to have, including as they do the natural history of our own and of all other species of living organisms, whether animal or vegetable; some special preparation must be gone through if that general interest is to be thoroughly and intelligently gratified. I would compare the realm of biology to a vast landscape in a cultivated country of which extensive views may be obtained from an eminence; but for the full and thorough appreciation of which, if necessary, the gazer should himself have cultivated some portion, however small, of the expanse at his feet. It is, of course, a matter of regret to think that persons can be found who look upon an actual landscape without any thought or knowledge as to how the various factors which make up its complex beauty have come to co operate; how the hand of man is recognisable here; how the dip of the strata is visible there; and how this alternation is detectable in another place as the potent agency in giving its distinctive features; but I take it that real and permanent, however imperfect, pleasure may be drawn from the contemplation of scenery by persons who are ignorant of all these things. I do not think this is the case when we here deal with *coup d'œil* views of biology. The amount of the special knowledge, the extent of the special training need not necessarily be great, but some such special knowledge and training there must be if the problems and argumentations familiar to the professed biologist are to be understood and grasped by persons whose whole lives are not devoted to the subject, so as to form for them acquisitions of real and vital knowledge.

The microscope has done very much, indeed I may say it has done almost all that is necessary for enabling all persons to obtain the necessary minimum of practical and personal acquaintance with the arrangements of the natural world of which I am speaking. The Glass trough used in Edinburgh, the invention of John Goodsir, whose genius showed itself, as genius often does show itself, in simple inventions, can be made into a miniature aquarium. I purposely use a word which calls up the idea of an indoors apparatus, wishing thereby to show how the means I recommend are within the reach of all persons; and in it, lying as it does horizontally and underlaid as it is by a condenser, animal and vegetable organisms can be observed at any and at all hours, and continuously, and with tolerably high magnifying powers even whilst undisturbed. Thus is gained an admirable field for the self-discipline in question. The microscope which should be used by preference for exploring and watching such an aquarium should be such an one as is figured in Gaskell's work on the Microscope (p. 58, fig. 36), as consisting of a stem with a stout steadying base, and of a horizontal arm some nine inches long, which can carry indifferently simple lenses or a compound body. I think of the two it is better that the aquarium should be horizontal rather than the microscope; and those who think with me in this matter can nevertheless combine for themselves the advantages of the horizontal position of the instrument with those of the horizontal position of the objects observed by modifying the eye-piece in the way figured by Quekett (p. 381, fig. 266.) It would be a long task to enumerate fully all the scientific lessons which may be gathered, firstly, and all the educational agencies, secondly, which may be set and kept in movement by a person who possesses himself of this simple apparatus. The mutual interdependence of the animal and vegetable kingdoms, their *solidarité* as the French have called it, and as the Germans have called it too, copying herein the French, is one of the first lessons the observer has forced upon him; the influence of physical and chemical agencies upon the growth and development of living beings he soon finds strikingly illustrated; the mysterious process of development itself is readily observable in the eggs of the common water-snails and in those of freshwater fish, so that the way in which the various organs and systems of organs are chiselled out, built up, and finally packed together and stratified can be taken note of in these yet transparent representatives of these great sub-kingdoms which all the while are undisturbed and at peace; and all these points of large interest are but a few of many which these small means will enable anyone to master for himself in the concrete actuality, and thoroughly. The necessity for carefulness and truthfulness in recording what is seen, the necessity for keeping in such records what one observes quite distinct from what one infers, the necessity for patience and punctuality, are lessons which, from having a moral factor as well as a scientific one in their composition, I may specify as belonging to the educational lessons which may be gathered from such a course of study.

I have been speaking of the microscope as an instrument of education, and I wish before leaving the subject to utter one caution as to its use when this particular object of education is in view. If a subject is to act educationally, it must be understood thoroughly; and if a subject is to be understood thoroughly, it must form one segment or stretch in a continuous chain of known facts. Ἀρκτέον ἀπὸ τῶν γνωρίμων, said one of the greatest of educators; you must start from some previously existing basis of knowledge, and keep your communications with it uninterrupted if your knowledge is not to be unreal. And my concrete application of these generalities is contained in the advice that no sudden jump be made from observations carried on with the naked eye to observations carried on with the highest powers of the microscope. I am speaking of the course to be pursued by beginners, and beginners we all were once, and if our places are to be filled, and filled they will be, by better men as we hope than ourselves, they will have to be filled, we also hope, by men who have yet to become beginners. It is in their interest I have been speaking, and I say that a beginner does not ordinarily get an intelligent conception of the revelations of the microscope except in Bacon's words, *Ascendendo continenter et gradatim*, by progressing gradually from observations with the naked eye through observations dependent upon dissecting lenses, doublets and triplets, and the lower powers of the compound microscope, up to observations to be made with the higher and highest magnifying powers.

Unless he ascend by gradations from organs and systems, structures and tissues and cells, his wonder and admiration at the results of the ultimate microscope analysis, of what he had but a moment before knowledge of only in the concrete and by the naked eye is likely to be but unintelligent.

There are three other agencies which can be set into activity with nearly as little trouble and difficulty as the simple apparatus of which I have just been speaking, and which will, like it, secure as a necessary preliminary discipline "*propädeutik*" for their rational comprehension of Biology. These are Local Museums, Local Field Clubs, and Local Natural Histories. Local authorities, persons of local influence, should engage and interest themselves in the starting into life of the two former of these agencies, and if some such person as Gilbert White could be found in each county to write the Natural History of its Selborne, I know not at what cost it could not be well to retain his services. As the world is governed upon each particular area of its surface, there is to be found a certain percentage of the population occupying it who have special calls for particular lines of study. It is the interest of each county to have such means and such institutions in being as will render it possible to detect the existence of persons gifted with such special vocations, to give the talent thus entrusted to them fair scope for development, and to render smaller the risk of their dying mute and inglorious. A young man who is possessed of a talent for Natural Science and Physical Inquiry generally, may have the knowledge of this predisposition made known to himself and to others, for the first time, by his introduction to a well-arranged Loca Museum. In such an institution, either all at once, or gradually, the conviction may spring up within him that this investigation of physical emblems is the line of investigations to which he should be content to devote himself, relinquishing the pursuit of other things; and then, if the museum in question is really a well-arranged one, a recruit may be thereby won for the first and growing army of physical investigators, and one more man saved from the misery of finding, when he has been taken into some other career, that he has somehow or other mistaken his profession, and made of his career one life-long mistake. Here comes the question. What is a well-arranged museum? The answer is, a well-arranged museum for the particular purpose of which we are speaking, is one in which the natural objects which belong to the locality, and which have already struck upon the eye of such a person as the one contemplated, are clearly explained in a well-arranged catalogue. The curiosity which is the mother of science is not awakened for the first time in the museum, but out of doors, in the wood, by the side of the brook, on the hillside, by scarped cliff and quarried stone; it is the function of the museum, by rendering possible the intellectual pleasure, which grows out of the surprise with which a novice first notes the working of his faculty of inspiration, to prevent this curiosity from degenerating into the mere woodman's craft of the gamekeeper, or the rough empiricism of the farmer. The first step to be taken in a course of natural instruction, is the providing of means whereby the faculties of observation and of verification may be called into activity; and the first exercise the student should be set down to is that of recognising in the actual thing itself, the various properties and peculiarities which some good book or some good catalogue tells him are observable in it. This is the first step, and, as in some other matters, *ce n'est que le premier pas qui coute.* And it need not cost much. There is a name familiar to Section D, and indeed not likely for a long while to be forgotten by members of the British Association generally, extrinsic means as well as the intrinsic merits of the well-loved man conspiring to keep his memory fresh among us, and the bearer of that name, Edward Forbes, has left it as his opinion that "It is to the development of the provincial museums that, I believe, we must look in future for the extension of intellectual pursuits throughout the land." (Lecture "On the Educational Uses of Museums," delivered at the Museum of Practical Geology and published in 1853. Cited by Toynbee, "Hints on the Formation of Local Museums," 1863, p. 46.) With the words of Edward Forbes I might do well to end what I have to say, but I should like to say a word as to the policy of confining the contents of a local museum to the natural-history specimens of the particular locality. No doubt the first thing to be done is the collection of the local specimens, and this alike in the interest of the potential Cuviers and Hugh Millers, who may be born in the district, and in the interest of the man of science who may visit the place when on his travels. But so long as a specimen from the antipodes or from whatever corner of our world be really valuable, and be duly catalogued before it is admitted into the museum, so that the lesson it has to teach may be learnable, I do not see my way towards advising that foreign specimens be excluded. It is to my mind more important that all specimens should be catalogued as soon as received, than that any should be rejected when offered.

I must not occupy your time further with this portion of my address. Let me first say that a person who wishes to know what a Field Club can do for its members, and not for them only, but for the world at large, will do well to purchase one, or any number more than one, of the Transactions of the Tyneside Naturalist's Field Club; and that if there be any person who thinks that White's Selborne relates to a time and place so far off that there can be no truth in the book, and who yet would like to try upon himself the working of the fourth disciplinary agencies of which I have spoken; that, namely, of sending some Local Natural History on the spot of which it treats, and comparing it with the things themselves *in situ*, let him repair to Weymouth, and work and walk up and down its cliffs and valleys with Mr. Darwin's book in his hands.

I shall not be suspected in this place and upon this occasion, nor, as I hope, upon any other, of a wish to depreciate the value of scientific instruction as an engine for training the mind. But neither, on the other hand, should I wish to depreciate the value of literary culture, my view of the relations of these two gymnastics of the mind being the very simple, obvious, and natural one that they should be harmoniously combined—

Alterius sic
Altera sic poscit opem vis, et conjuvat amice.

I know it may be said that there are difficulties in the way, and especially practical difficulties, but I have always observed that people who are good at finding out difficulties, and especially practical difficulties, are like people who are good at finding out excuses,—good at finding out very little else. The various ways of getting over these difficulties are obvious enough, and have been hinted at, or fully expressed by several writers of greater or less authority on many occasions. It is, however, of some consequence that I should here say what I believe has not been said before, namely, that a purely and exclusively literary education imperfect and one sided, as it is, is still a better thing than a system of scientific instruction (to abuse the use of the word for a moment) in which there should be no courses of practical familiarising with natural objects, verification, and experimentation. A purely literary training, say, in dialectics, or what we are pleased to call logic, to take a flagrant and glaring instance first, does confer certain lower advantages upon the person who goes through it without any discipline in the practical investigation of actual problems. By going through such a training attentively, a man with a good memory and a little freedom from over-scrupulousness, can convert his mind into an arsenal of quips, quirks, retorts, and epigrams, out of which he can, at his own pleasure, discharge a mitraille of chopped straw and chaff-like arguments, against which no man of ordinary fairness of mind can, for the moment, make head. It is true that such sophists gain this dexterity at the cost of losing, in every case, the power of fairly and fully appreciating or investigating truth; of losing in many cases the faculty of sustaining and maintaining serious attention to any subject; and of losing in some cases even the power of writing. A well-known character in an age happily, though only recently, gone by, who may be taken as a Cæsar worthy of such Antonies, used to speak of a pen as his torpedo. Still they have their reward, they succeed now and then in convincing juries, and they are formidable at dinner tables. It would not be fair, however, not to say that a purely literary training can do much better things than this. By a purely Classical Education a man, from being forced into seeing and feeling that other men could look upon the world, moral, social and physical, with other (even if not with larger) eyes than ours, attains a certain flexibility of mind which enables him to enter into the thoughts of other and living men, and this is a very desirable attainment. And, finally, though I should be sorry to hold with a French writer that the style makes the man, the benefit of being early familiarised with writings which the peculiar social condition of the classical times, so well pointed out by De Tocqueville (*De la Démocratie en Amérique*), conspired and contributed not a little to make models of style, is not to be despised. Such a familiarity may not confer the power of imitating or rivalling such compositions, but it may confer the power of appreciating their excellences, the one power appearing to us to be analogous to the power of the experimenter, and the other to that of the pure observer in Natural Science; and we should undervalue neither.

Masters of Science, it must be confessed, are not always masters of style; let not the single instance of last night tempt you to generalise, it was but a single instance, the writings of the man whom we in this Section are most of us likely to look upon as our master in Science have been spoken of by our President in his recently published volume as "intellectual pemmican;" and if scientific reading and teaching is to be divorced from scientific observation of natural objects and processes, it is better that a man, young or old, should have in his memory something which is perfect of its kind, entire and unmutilated, such as the opening of sentences of the *Brutus* of Cicero, which Tacitus, I think, must have had in his memory when he wrote his obituary of Agricola, or as the opening sentences of the *Republic* of Plato, or the conclusion of the *Ajax* or Sophocles, than that he should have his memory laden with a consignment of scientific phrases which *ex hypothesi* have for him no virtual reality. I have already said that I am strongly of opinion that literary should always be combined with scientific instruction in a perfect educational course; these somewhat lengthy remarks refer therefore only to systems in which it is proposed that we should have not only a bifurcation but a radical separation of studies and students, and the moral of this may be summed up by saying that a purely scientific education must be a thoroughly practical one, familiarising the student with actual things as well as with words and symbols. It was upon the solid ground that Antæus learnt the art of wrestling, it was only when he allowed himself to be lifted from it that he was strangled by Hercules.

Coming now to the second part of my address, I beg to say that the word Biology is at present used in two senses, one wider, the other more restricted. In this latter sense the word becomes equivalent to the older, and till recently more currently used word "Physiology;" it is in the wider sense that the word is used when we speak of this as being the section of Biology; and this wider sense is a very wide one, for it comprehends animal and vegetable Physiology and Anatomy, firstly; Ethnology and Anthropology, secondly; and thirdly, Scientific Zoology and Classificatory Botany, inclusively of the Distribution of Species. It may have been possible in former times for a single individual of great powers of assimilation to keep himself abreast of, and on a level with, the advance of knowledge along all these various lines of investigation; but in those times knowledge was not, and could not, owing to difficulties of intercommunication, the dearness of books, the costliness or the non-existence of instruments, have been increased at the rate at which it is now being, year by year, increased; and the entire mass of actually existing and acquired knowledge was of course much smaller, though man's power of mastering it was no smaller than at present. It would now be an indication of very great ignorance in anybody which should pretend that his own stock of information could furnish him with something in each one of the several departments of knowledge I have just mentioned, which should be worthy of being laid before such an assembly as this. As will have been expected, I shall not presume to do more than glance at the vegetable kingdom, large as is the space in the landscape of life which it makes. What I do propose to do is merely to draw your attention to a very few of the topics of leading interest, which are at the present moment being, or rather will shortly begin to be, discussed by experts in the Department of Physiology and Anatomy; in the Department of Ethnology and Anthropology; and thirdly, in the Department of Scientific Zoology.

Under the head and in the Department of Physiology Proper and Anatomy, our list of papers and, I am happy to add, the circle of faces around us suggests to us the following subjects as being the topics of main interest for the present year: the questions of Spontaneous Generation; that of the influence of organised particles in the production of disease; that of the influence of particular nervous and chemical agencies upon functions; that of the localisation of cerebral functions; that of the production and indeed of the entire *role* in the economy of creation of such substances as fat and albumen; and, finally, that of the cost at which the work of the animal machine is carried on.

The question of Spontaneous Generation touches upon certain susceptibilities which lie outside the realm of science. In this place, however, we have to do only with scientific arguments, and I trust that the Section will support the Committee in their wish to exclude from our discussions all extraneous considerations. Truth is one; all roads which really lead to it will assuredly converge sooner or later; our business is to see that the one we are ourselves concerned with is properly laid out and metalled.

Upon this matter I am glad to be able to fortify myself by two authorities; and first of these I will place an utterance of Archbishop Whately, which may be found in the second volume of his Life, pp. 66–68, æt. 57, an. 1844. "A person possessing real faith will be fully convinced that whatever suppressed physical fact appears to militate against his religion will be proved by physical investigation either to be unreal or else reconcilable with his religion. If I were to found a church, one of my articles would be that it is not allowable to bring forward Scripture or any religious considerations at all to prove or disprove any physical theory or any but religious and moral considerations." My second quotation shall be taken from the great work of one of the first, as I apprehend, of living theologians, John Macleod Campbell, "The Nature of the Atonement," pp. xxxii.–xxxiii. Introd., and it runs thus:—There are "other minds whose habits of pure scientific investigation are to them a temptation to approach the claim of the Kingdom of God on our faith by a wrong path, causing them to ask for a kind of evidence not proper to the subject, and so hindering their weighing fairly what belongs to it. No scientific study of the phenomena which imply a reign of law could ever have issued in the discovery of the kingdom of God. But neither can it issue in any discovery which contradicts the existence of that kingdom; nor can any mind in the light of the kingdom of God hesitate to conclude that if such seeming contradictions arise there is implied the presence of error either as to the facts or as to conclusions from the facts." These are valuable words and weighty testimonies. But in a matter of this importance one must not forbear to point out what may seem to be wanting even in the dicta of such men as the two I have quoted. Neither of them have allowed the possibility of error attaching itself to the utterances of more than one of the two parties in such issues as those contemplated. Neither appears to have thought of the cases in which religious men, if not theologians, have brought war on the world because of the offences they have with ill-considered enunciations created. And, whilst fully sympathising with all that the Archbishop and Mr. Campbell have said, I must say that they appear to me to have left something unsaid, and this something may be wrapped up in the caution that there may be faults on both sides. But at any rate this Section cannot be considered a fit place for the correction of errors save of the physical kind; and all other considerations are for this week and in this place extraneous. In some other week or in some other place it will be, if it has not already been, our duty to give them our best attention.

To come now to the kind of considerations which are the proper business of Section D, let me say that for the discussion of the question of Spontaneous Generation very refined means of observation, and, besides these, very refined means of experimentation, are necessary. And I shall act in the spirit of the advice I have already alluded to as given to the world by one of her greatest teachers, if I put before you a simple but a yet undecided question for the solution of which analogous means of a far less delicate character would appear to be, but as yet have not proved themselves to be, sufficient. Thus shall we come to see very plainly some of the bearings, and a few of the difficulties, of the more difficult of the two questions. What an uneducated person might acquiesce in hearing spoken of as Spontaneous Generation, takes place very constantly under our very eyes, when a plot of ground which has for many years, or even generations, been devoted to carrying some particular vegetable growth, whether grass or trees, has that particular growth removed from it. When such a clearing is effected, we often see a rich or even a rank vegetation of a kind previously not growing on the spot spring up upon it. The like phenomenon is often to be noted on other surfaces newly exposed, as in railway cuttings and other escarpments, and along the beds of canals or streams, which are laid bare by the turning of the water out of its channel. Fumitory, rocket, knotgrass, cowgrass, *Polygonum aviculare*, and other such weeds, must often have been noted by every one of us here in England as coming into and occupying such recently disturbed territories in force; whilst in America the destruction of a forest of one kind of wood, such as the oak or the chestnut, have often been observed to be followed by an upgrowth of young forest trees of quite another kind, such as the white pine, albeit no such tree had been seen for generations growing near enough to the spot to make the transport of its seeds to the spot seem a likely thing. In one case referred to by Mr. Marsh, the hickory, *Carya porcena*, a kind of walnut, was remarked as succeeding a displaced and destroyed plantation of the white pine. Now the advocates of Spontaneous Generation must not suspect me of hinting that there is any question, except in the minds of the grossly ignorant, of the operation of any such agency as spontaneous generation here; no one would suggest that the seeds of the *Polygonum aviculare*, to say nothing of those of the Hickory, were produced spontaneously; but what I do say is, that the question of how those seeds came there is just the very analogue of the one which they and their opponents have to deal with. And it is not definitely settled at this very moment. Let us glance at the instructive historical parallel it offers. For the very gross and palpable facts of which I have just spoken there are two explanations offered in works of considerable authority. The one which has perhaps the greatest currency and commands the largest amount of acceptance is the one which, in the words of De Candolle, regards *la couche de terre végétale d'un pays comme un magazin de graines*, and supposes that in hot summers and autumns, such as the present, the fissures in the ground, which have proved so fatal this year to the young partridges, swallow up a multitude of seeds, which are restored again to life when the deep strata into which they are thus introduced, and in which they are sealed up as the chasms close up, come in any way to be laid open to the unimpeded action of the sun and moisture. Squirrels, again, and some birds resembling herein the rodent mammalia, bury seeds and forget to dig them up again; and it is supposed that they may bury them so deep as to be protected from the two physical agencies just mentioned. Now Germination cannot take place in the absence of oxygen, and I would add that well-sinkers know to their cost how often the superficial strata of the earth are surcharged with carbonic acid. The rival explanation and the less popular—I do not say the less scientific—looks to the agency of transportation as occurring constantly, and sufficing to explain the facts. By accepting this explanation, we save ourselves from running counter to certain experiments, some of which were carried out, if I mistake not, under the auspices of this Section (see British Assoc. Reports), and which appear to curtail considerably the time during which seeds retain their vitality, and to multiply considerably the number of conditions which must be in force to allow of such retention for periods far shorter than those which have to be accounted for. A better instance of the expediency of checking the interpretations based merely upon observations however accurately made by putting into action experiments, cannot be furnished than by recording the fact put on record by Mr. Bentham, when discussing this question in his last year's address to the Linnæan Society.

"Hitherto direct observation has, as far as I am aware, only produced negative results, of which a strong instance has been communicated to me by Dr. Hooker. In deepening the lake in Kew Gardens, they uncovered the bed of an old piece of water, upon which there came up a plentiful crop of *Typha*, a plant not observed in the immediate vicinity; and it was therefore concluded that the seed must have been in the soil. To try the question, Dr. Hooker had six Ward's cases filled with some of the soil remaining uncovered close to that which had produced the *Typha*, and carefully watched; but not a single *Typha* came up in any one of them." (Note in President's address May 24th, 1869, page 72 of Linnæan Society's Proceedings.)

To this I would add that experiments with a positive result, and that positive result in favour of the second hypothesis, if hypothesis it can be called, are being constantly tried in our colonies for us, and on a large scale. I had taken and written here of the *Polygonum aviculare*, the "knot" or "cowgrass"—having learnt on the authority of Dr. Hooker and Mr. Travers (see Natural History Review, January 1864, p. 124, Oct. 1864, p. 619), that it abounds in New Zealand, along the roadside, just as it does in England—as a glaring instance, and one which would illustrate the real value of the second explanation even to an unscientific man and to an unassisted eye. But on Saturday last I received by post one of those evidences which make an Englishman proud in thinking that whithersoever ships can float thither shall the English language, English manners, and English Science be carried, in the shape of the second volume of the Transactions of the New Zealand Institute, full like the first, from beginning to the last page with thoroughly good matter. In that volume, having looked at its table of contents, I turned to a paper by Mr. T. Kirk on the Naturalized Plants of New Zealand, and in this, at p. 142, I find that Mr. T. Kirk prefers to regard the *Polygonum aviculare* of New Zealand as indigenous in New Zealand. Hence that illustration which would have been a good one falls from my hands. And I must in fairness add, that because one agency is proved to be a *vera causa*, it is not thereby proved that no other can by any possibility be competent simultaneously to produce the same effect, whatever the Schoolmen with the law of Parsimony ringing in their ears may have said to the contrary. I have dwelt upon this subject at this length with the purpose of showing how much difficulty may beset the settlement of even a comparatively simple question which involves only the use of the unassisted eye, or at most of a simple lens. The *a fortiori* argument, I leave you to draw for yourselves with the simple remark, that the question of Spontaneous Generation is now at least one to be decided by the microscope, and by the employment of its highest powers in alliance with other apparatus of all but equal complexity.

We come, in the second place, to say a word as to the extent of the influence which organic and living particles, of microscopic minuteness but solid for all that, have been supposed, and in some instances at least have been proved, to exercise upon the genesis and genesiology of disease, and so upon the fortunes of our race, and our means for bettering our condition, and that of our fellows. I need not refer to Dr. Sanderson's valuable Report (just published in the Privy Council's Medical Officer's Blue Book, Twelfth Report, 1870, p. 229, upon those contagion particles which he proposes to call by the convenient name, slightly modified from one invented by Professor Bichamp, of Microzymes; for Dr. Sanderson is here to refer to the matter for himself and for us; and when this meeting is over we shall all do well to lay to heart what he may tell us here and now, and besides this, to study his already printed views upon the matter. It may be perhaps my business to remind you that these views, so far as they are identical with Professor Halliers' as to the importance of those most minute of living organisms, the micrococcus of his nomenclature, the microzymes of Mr. Simon's Blue Book, were passed in review as to their botanical correctness by a predecessor of mine in this honourable office—namely, by the Rev. J. M. Berkeley, at the meeting held two years ago at Norwich; and that some of the bearings of the theory and of the facts, howsoever interpreted, upon the Theory of Evolution, were touched upon by Dr. Child in his interesting volume of Physiological Essays, p. 148, published last year. It would not perhaps be exactly my business to express my dissent from any of these results or views put forward by any of these investigators I have mentioned; but I wish to point out to the general public that none of these inquirers would affirm that the agencies shown by them to be potent in the causation of certain diseases were types and models of the agencies which are, did we but know it, could we but detect them, potent in the causation of all diseases. Many diseases, though possibly enough not the majority of the strictly infectious diseases, are due to material agents quite distinct in nature from any self-multiplying bodies, cytoid or colloid. To say nothing of the effects of certain elements—and elements, it will be recollected, in their singleness and simple atomicity have, as the world happens to be constituted and governed, never been honoured with the office of harbouring life—which when volatised, as mercury, arsenic, and phosphorus may be, or indeed which, when simply dissolved, may be most ruinous to life, there are, I make no doubt, animal poisons produced in and by animals, and acting upon animal bodies which are neither organised nor living, neither cytoid nor colloid. Dr. Charlton Bastian is not likely to underrate the importance of such agents, howsoever produced, in the economy, or rather in the waste, of Nature, yet from his very careful record of his own very closely observed and personal experience we can gather that he would not demur to conceding that non-vitalised, however much animalised, exhalations may be only too powerful in producing attacks, and those sudden and violent and fever-like attacks, of disease. Dr. Bastian tells us (Phil. Trans. for 1866, vol. 196, pt. ii. pp. 583-584) that whensoever he employed himself in the dissection of a particular nematoid worm, the *Ascaris megalociphala*, he found occasion to observe, and that in himself, and very closely, the genesiology of a spasmodic and catarrhal affection, not unlike hay-fever as it seems to me, but under circumstances which appear to preclude the possibility of any living organisms being the cause of it as they have been supposed, and by no less an authority than Helmholtz, to be of the malady just mentioned. For in Dr. Bastian's case this affection was produced, not only when the *Ascaris megalocephala* was dissected when fresh, but "after it had been preserved *in methylated spirit for two years, and even then macerated in a solution of chloride of lime for several hours before it was submitted to examination.*" Could any microzyme or megalozyme have survived such an amount of antizymotic treatment—such a pickling as this? This is not exactly a medical association, and I have entered upon this discussion not altogether without a wish to show how subjects of apparently the most purely scientific and special interest, as Mycology and Helminthology, the natural history, that is to say, and the morphology of the lowest plants and of the lowest vermes, may come when we least expect it, come or be brought to bear upon matters of the most immediate and pressing practical importance. And in this spirit I must say a word upon the way in which the pathology of snake-bites bears upon the matters I have been speaking of, and the extent of the debt which practical men owe to such societies as our Ray Society, and to such publications as their colossal volume on the snakes of India, in which Dr. Günther's views as to the real history of the striking and terrible yet instructive phenomena alluded to, are combined ("Snakes of India," Ray Society, 1864, p. 167). That the snake-poison is an animal poison is plain enough; that it is fatal to men and animals everybody knows; but I rather think that these two facts relative to it are not equally notorious, rich in light though they be, viz., that the potency of this particular animal poison varies in direct ratio to the quantity imbibed or infused, just as though it were so much alcohol, or so much alcoholic tincture of musk or cantharides; or secondly, that its potency varies in direct ratio to another varying standard, viz., the size of the animal producing it. Now, the vaccine matter from the arm of a child is as potent as the vaccine matter from the arm of any giant might be, if such a large creature could in these days escape the operation of the vaccination laws; and whether a grain or a gramme of it be used, will make no difference, so long as it be used rightly. There is a contrast indeed between the *modus operandi* of these two animal poisons. I would add that in the Edinburgh *Monthly Medical Journal* for the present month there is a very valuable paper, one of a series of papers, indeed, of the like character, by Dr. Fayrer, where at page 247, among much of anatomical and other interest, I find the following important statement:—"This poison may be diluted with water, or even ammonia or alcohol, without destroying its deadly properties. It may be kept for months or years, dried between slips of glass, and still retain its virulence. It is capable of absorption through delicate membranes, and therefore it cannot be applied to any mucous surfaces, though no doubt its virulence is much diminished by endosmosis.* It appears to act by a catalytic form; that is it kills by some occult influence on the nerve centres." There is such a thing as an ignorance which is wiser than knowledge, *for the time*, of course, only; such an ignorance is wisely confessed to in these words of Dr. Fayrer's:—An explanation may be true for some, yet not thereby necessarily for all, the facts within even a single sphere of study, even a true explanation may have but a very limited application, as a tangent cannot touch a circle at more than a single point. The memoirs, published in our own reports by Dr. R. W. Richardson, on the action of the nitrites, and those published by Dr. A. Crum Brown and Dr. Fraser, there and elsewhere, on the connection between chemical constitution and physiological action, deserve especial study as bearing on the other side of this discussion; whilst Prof. Lister's papers show how the reference of certain diseases to inhalistic agencies may become o most useful importance in practice. There exists, as is wel known, a tendency to involve all Physiological into Physico-chemical phenomena; undoubtedly many have been, and some more may still remain, to be so ordered, but the public may rest assured that in the kingdom of Biology no desire for a rectification of frontiers will ever be called out by any such attempts at, or successes in the way of, encroachment; and that where physics and chemistry can show that physico-chemical agencies are sufficient to account for the phenomena, there their claim upon the territory will be acceded to, as in the cases we have been glancing at; and where such claims cannot be established and fail to come up to the quantitative requirements of strict science, as in the cases of continuous and of discontinuous development or self-multiplication of a contagious germ, and in some others, they will be disallowed.

(*To be Continued.*)

SECTION E.—*Geographical Science.*—Sir Roderick Murchison, K.C.B., F.R.S., President.

In opening his address, after alluding to the more recent geographical discoveries which had been made, and to the geographical expeditions now in progress in Asia and Africa, and of which a much fuller account is to be found in his recent address to the Geographical Society, the President passed on to the subject of Deep Sea Soundings and their relation to Geology. Here he said he dissented most strongly from the views held by Dr. Carpenter and others, that in a broad sense we may be said to be in the Cretaceous epoch, since so many of the marine forms met with were similar to, if not identical with, those which lived at that time. Thus he says, "May we not indeed by a similar bold hypothesis affirm that we still live in the older Silurian period? for albeit no bony fishes then existed, many globigerinæ and creatures of the lowest organisation have been found in these old rocks associated with terebratulidæ and dingulæ, the *generic* forms of which still live." Surely we need not point out to Sir Roderick Murchison that *generic* forms are one thing and *identical species* another, and that whilst the former are evidence of *similarity of condition*, the latter are evidence of *persistency of conditions.* From this Sir Roderick passed to the subject of the Physical Geography of the Ocean, and paid a great compliment to the valuable work lately published by Mr. J. K. Laughton, on "Physical Geography in its Relation to the Prevailing Winds and

* Diapedesis may account for what virulence remains, and the poison may therefore possibly be a cytoid.

Currents;" and after referring to the various papers about to be read to the Section, he concluded his address by expressing his conviction, founded on all recent information, that Dr. Livingstone was alive and amongst friendly natives, and that Sir Samuel Baker would shortly meet him with fresh stores of provisions, &c.

SECTION F. —*Economic Science.*—President, Prof. W. Stanley Jevons.

In the President's address, which was of some length, after some introductory remarks, tending to show that the Economic Section has to deal with a class of subjects capable of strictly scientific treatment, and that the social are the necessary complement to the physical sciences, Professor Jevons proceeded to state that in this kingdom during the last thirty or forty years we had tried a mighty experiment, and to a great extent had failed. The growth of the arts and manufactures and the establishment of free trade had opened the widest means of employment, and brought an accession of wealth previously unknown; the frequent remission of taxes had left the working classes in fuller enjoyment of their wages; the poor laws had been reformed and administered with care, and the emigration of millions might well have been expected to leave room for those that remained. Nevertheless within the last few years we had seen pauperism almost as prevalent as ever, and the slightest relapse of trade threw whole towns and classes of people into a state of destitution little short of famine. This state of things was exactly what Malthus would have predicted of a population which, while supplied with easily earned wealth, is deprived of education and bribed by the mistaken benevolence of the richer classes into a neglect of the future. We now had an Education Act, but this ought not to withdraw attention from many other causes of evil still existing in full force. Amongst these was the mistaken humanity of charitable people. The amount extended by the upper to the lower classes was almost incredible; but it did more harm than good. The helpless poor were most numerous precisely in those towns where charitable people and institutions most abounded. But far worse than private charity were the innumerable small charities established by the bequests of mistaken testators. It would be well worthy of Mr. Goschen's attention whether all such charities might not be transferred to the care of the guardians of the poor, so as to be brought under the supervision of the Poor Law Board, and distributed in accordance with sound principles. The State, which undertook the ultimate support of the poor, was bound to prevent its own efforts to reduce pauperism from being frustrated, as they are at present. As regards medical charities, Professor Jevons said no one could for a moment propose to abolish hospitals and numerous institutions absolutely necessary for the relief of accidental suffering; but no working man was solvent who did not lay aside so much of his wages as would meet the average amount of sickness falling to the lot of the man and his family. So it was not easy to determine this amount. There were or might be sick clubs which would average the inequalities of life. Hospitals need not be self-supporting, and in cases of severe and unforeseen suffering they might give the most lavish aid; but they ought not to relieve slight and ordinary disease without a contribution from those benefited. With respect to the Poor Law medical service, every one admitted that where medical aid is given it ought to be good and sufficient; but, on the other hand, the better we make that service the more do we tend to increase and perpetuate that want of self-reliance and providence which is the crowning defect of the poorer classes. In this and many other cases we ought to regulate our humane impulses by a stern regard to the real results of our actions. Referring to the financial policy of the kingdom, Professor Jevons pointed out that in Cobden's sense free trade is now actually achieved. For the future the remission of customs duties would be grounded on other motives than it has often been in the past. It was a mistake to suppose that foreign trade ought to be encouraged before everything else. The internal trade and industry of the country were at least equally deserving of attention, and it might be that there were stamp duties, licence duties, rates, and other taxes which, in proportion to the revenue they returned, did far more injury than any customs duties now remaining. The question of local taxation was one which especially required attention. The amount raised by local rates was more than equal to the whole of the customs duties; nevertheless they continued to be levied substantially according to an Act passed in the reign of Queen Elizabeth. There was sure to be a continuous increase of local taxation. Turning to the question of surplus revenue, Professor Jevons said that there probably now existed no grievous pressure of taxation, and no considerable inequality as regards the several classes of the people. He calculated that average families spending 40*l.*, 85*l.*, and 500*l.* a year, consuming moderate quantities of tobacco and spirituous liquors, all paid about 10 per cent. of their income in general or local taxation. Only the taxation of the middle classes was mostly unavoidable, whereas at least half the taxation of the poorer classes depended upon the amount of tobacco and spirituous liquors consumed. The present incidence of taxation, therefore, was such that it seemed inexpedient to proceed further in the reduction of the customs and excise duties. To do so would be to throw the whole cost of Government upon the wealthier classes, and especially those who have tangible property. Besides, when really hurtful taxes were removed, the working classes were not sufficiently temperate and educated to render it certain that the further remission of taxes would lead to the profitable expenditure of income. The true channel for surplus revenue was the reduction of the national debt. The wars at the commencement of this century had secured for us fifty years or more of nearly unbroken peace, and yet at the end of this period of ever-advancing wealth the great debt stood almost at the same figure as at the beginning. We enjoyed the peace and left our descendants to pay its cost. If it was said that this country is now far wealthier and better able to endure the annual charge of the debt than ever before, it must be remembered that the expense of war is also grtealy increased. In a great war we should now have to incur an expenditure of hundreds of millions, or else relinquish our prominent position. In dealing with the subject of the excessive mortality in great towns, Prof. Jevons expressed his surprise that more attention had not been drawn to the probable influence of a poor Irish population in raising the death-rate. According to the census returns of 1861, the unhealthy towns of Liverpool, Manchester, Salop, Glasgow, Dundee, &c., were all distinguished by possessing a large Irish population, whereas the healthy towns of London, Birmingham, Bristol, Hull, Aberdeen, &c., had less than 7½ per cent. of adult Irish residents. Sheffield was the only remarkable exception to this indicated. Prof. Jevons referred to the approaching census of 1871, as likely to afford many data for the investigations of economists, and insisted that it ought to be taken in as nearly as possible a uniform manner in all the three parts of the United Kingdom. He also directed attention to the copious and excellent statistical publications now provided by Government; referred to the efforts which were being made, previous to the present war, to facilitate the adoption of an international currency; and concluded with some remarks on the transference of the telegraphs to Government control. Many people looked forward to the time when the uniform cost of a telegram would be 6*d.*; but such a reduction of the rate, by bringing an increase of work, would greatly augment the expenses of the department, and inflict a loss upon the nation.

CONTENTS

THURSDAY, SEPTEMBER 29, 1870

HOUSE ACCOMMODATION FOR LEARNED SOCIETIES

THE movement which originated with the Statistical Society about three months ago for bringing under the shelter of one roof various learned societies of the metropolis, has already made that progress which gives the best assurance of ultimate success.

The "Learned Societies' Accommodation Committee" is at present constituted by delegates from the under-mentioned bodies:—The Anthropological Society; the British Archæological Association; the East India Association; the Ethnological Society; the Institute of Actuaries; the Iron and Steel Institutes; the Juridical Society; the Meteorological Society; the Photographic Society; the Royal Colonial Institute; the Society of Arts; the Social Science Association and Law Amendment Society; the Statistical Society; and the Victoria Institute.

Each of these Societies is represented on the Committee by one delegate. The President of the Statistical Society, Mr. Newmarch, has been chosen chairman, but as this Society had already a delegate, the Chairman has no vote, otherwise the Society would have two voices at the Board, while the others were restricted to one each.

The Committee reserve to themselves the "power to add representatives from other learned Societies." By this resolution the combining societies may be increased, and probably will be, as the scheme approaches nearer accomplishment.

One body named in the list above has so large a fellowship, and so wide a scope in its objects, that its wants are consequently great and peculiar. The Society of Arts is likely to need house room in, we believe, a year or two. This society, Mr. Le Neve Foster remarks, would require "all the room we have at present and something more." When their extensive premises in the Adelphi are brought to mind, it is at once felt that Mr. Foster's society stands apart by the magnitude of its essential wants from all the others just named. To a certain degree the needs of the Society of Arts do not accord with the humbler demands of the other societies. The latter may be housed in a moderately capacious building, with a common meeting room or theatre, capable of accommodating from 150 to 200 members. Hence, these smaller scientific bodies offer a much easier undertaking to organise and manage. In London the difficulties of obtaining an appropriate site are enormously multiplied by any large increase of required frontage. On the other hand, the union of the Society of Arts with the other learned bodies presents the opportunity of a bolder enterprise. A comprehensive project for lodging all the London societies lacking house-room in one mansion is, doubtless, an attractive idea to many minds. Thus, it would appear at first sight, that, under these circumstances, two courses emerge: a moderate plan, with proximate execution, for the smaller societies; a grander scheme, with, in all probability, indefinitely remote accomplishment.

The Committee have avoided, by the unanimous resolutions of the 1st July, any conflict between these views. They have resolved in effect:

1. That convenient and permanent accommodation should be provided in a distinct building for societies that do not require extensive museums and libraries.
2. That the Committee express their earnest desire to co-operate with societies requiring larger accommodation for libraries and museums, either by "a combined application to Government for a site or building, or by joint action for the purchase of a convenient site."

The Committee think that if the wider co-operation spoken of in the second resolution should be successful, the plan for the smaller societies "may be either treated as a separate block in an associated group of buildings, or as a constituent part of one large building."

The Committee have taken the necessary step of giving instructions for the preparation by a competent architect of sketch plans and the elevation of a building adapted to the requirements of the smaller societies. These plans, it is understood, will be laid before the Committee when it re-assembles in October. Further, the Committee determined that the first subject for consideration at their next meeting "be the appointment of one or more of its members to represent its view and wishes to *The Aid to Science Commission*."

If, eventually, only the smaller scheme be adopted, it is thought the cost of the whole building and the purchase of the site could be compassed by an outlay which would offer no pecuniary impediment. In the absence, however, of working plans any stated sum can only be regarded as roughly approximate. The site itself may prove a business not so easily dealt with. Position is a matter of such precious importance to the utility of the undertaking that it is not likely to be undervalued—success or failure very much hangs upon the local habitation.

The proceedings of the committee seem to have been thus far, prompt and business-like. We have no doubt, therefore, that their efforts to economise the resources, and thereby augment the utility of the scientific societies of the metropolis, will speedily bear good fruit.

THE BERLIN WORKING MEN'S CLUB

FOR some years past there has been carried on at Berlin a Working Men's Club (for so it seems best to translate *Der Berliner Handwerker Verein*), established, we are told in its Reports, by working men and friends of labour, in order to promote general culture, sound technical knowledge, and good manners, among its members. This it attempts to do by means of popular lectures, classes for serious instruction, gymnastics, a library and reading-room well stocked with books and journals, concerts and social gatherings, or pleasure parties in which the wives and children of members take part. In many respects it resembles our own Mechanics' Institutes and Working Men's Clubs; but there are several features deserving special attention.

Not the least noteworthy point about it is its success. The average number of members at any one time is about 4,000, of whom nine-tenths at least are *bonâ fide* working men; but owing to the migratory habits of the German artizans, no less than 10,000 names are placed on their books every year. Well worthy of attention is the essentially democratic nature of the constitution and management. It is founded on the principle of self-help;

every man over 17 years of age, and unconvicted of crime, is eligible for membership, and after election continues to be a member so long as he pays his three silver groschen (somewhat less than 4*d.*) every month. The affairs are not conducted *for* the members by the philanthropic nobility, clergy, and gentry of the place, but *by* the members, through a representative assembly chosen annually, two-thirds at least of whom must be engaged in trade, and through a triennial committee, two-thirds of whom must also be engaged in trade. There is also a president, with vice-presidents, and a teacherhood, chosen either directly or indirectly by the members.

Yet in spite of, or rather perhaps because of, this working of self-help, they are enabled to enrol among their members, and receive active support from, the best and most active minds of Berlin. The leading professors of the University, the energetic young literary and scientific men of the city, the chief men of industry and commerce, all come forward to help them; and in their lists of popular lectures we find many names of European reputation.

The lectures, free to all members, and delivered in the club building, are intended to stir up the minds of the members to what is going on around them; while all those who are alert to the value of knowledge have access, on the payment of a small fee, to classes in which such subjects as reading, writing, arithmetic, geometry, mechanics, book-keeping, &c. &c., are taught in a thoroughly earnest and business-like manner. There are, besides, technical practical classes in construction and architecture; and the managers, having very definite views about play and work, take the greatest care that the musical and social gatherings shall be eminently successful in giving delight, and helpful in building up those "good manners" which are so desirable.

Then again, it is thoroughly catholic in spirit. It belongs to no religious or anti-religious party, advocates no political views, democratic, socialistic, or conservative; and, in fact, has no shibboleth of any kind whatever. It does not try to introduce serious learning and sound knowledge under false pretences. It does not call its members together to hear music, and then give them the stones of science. Every one can do as he likes. If he desire solid knowledge, there it is. If he care only for music or for a promenade, he can listen to the very best of the former, and walk about on fixed days among a crowd of comrades, all bent on pretending to be happy. He may, if he like, take out the whole of his monthly three groschen worth in abundant exercise on the parallel bars. So long as in any wholesome way the heavy burden of the artizan's daily life is lessened, and his dull life lighted up, the club thinks that it has in a measure accomplished its ends.

We imagine that most of our readers have read the article by Mr. James Stuart in No. 3 of NATURE, and they who have any experience of English workmen will, we venture to think, agree with nearly all that is there laid down. Putting together the burden of that article and the Report of this Berlin Working Men's Club, the question naturally suggests itself, Could we not in England do as much and a great deal more than is done in Berlin? It is one of the qualities of science, that he who has any is always anxious to give to others; and there surely could be no difficulty in establishing in London and in other large cities scientific teacherhoods willing to hold out the right hand of fellowship to all working men's clubs which were felt to be really acting on the principle of self-help.

There are many excellent and flourishing working men's clubs in London and elsewhere, and more than one working men's college. But we believe we are not going beyond the truth in stating that they lack the catholicity of the Berlin society. They are not all free from the suspicion of *arrière pensée* of some kind or other. Moreover, as separate institutions they tend to distract efforts, and cannot make a united appeal to the whole body of scientific men; while some of them at least are worked from above, the principle of dependence being more conspicuous than that of self-help. And so it comes to pass that those who are eager for knowledge have to struggle on without the counsel which they might otherwise share, often drawing their truth from wells by no means undefiled, and the dull unawakened multitude but rarely hear the voices which might rouse them from their sleep, and which now are wasted on the listless ears of fashionable audiences.

It is true that our scientific men are burdened far more than their German brethren with work undertaken for the sake of getting their daily bread, and for that alone; and so far unremunerative labour is to them a double task. But we have no doubt that, in spite of this, many would be led to come forward by the strong convictions they have that the welfare of their countrymen and their country is tottering for the lack of knowledge. By means of a little organisation, in which our artizans are pre-eminently skilled, much work might be got out of such teacherhoods, with the least possible wear and tear. They might deliver occasional lectures both for instruction and for rousing attention; but their chief use would be to advise the various clubs in their more serious work of class instruction, directing their studies, and assisting them in the selection of teachers.

Far be it from us to throw the slightest obstacle in the way of imperial secular education; but while waiting for the good time coming—in which the ladder of learning, from the lowest to the topmost rung, shall be free to every man, of every rank—much lies in the hands of the working men themselves. Let them show that they are ready for instruction, and have adopted the principle of self-help, and we believe that they will find scientific men ready to meet them half-way. It will not cost the workmen much to establish schools; it will not cost the teacherhood very much to give counsel; and the machinery for the payment of the actual class teachers is already in large measure provided for by the Science and Art Department of the Government.

The lamentable war that is now desolating some of the fairest provinces of Europe, and the unproductive current in which the energies of all Germans are at present conducted, must necessarily throw back this movement on the Continent. As long as we hear of peasant and professor working together in the field and on the ramparts, we cannot expect that they will also co-operate in the pursuit of scientific knowledge. But when this war-cloud has passed away, when France and Germany alike again turn to the arts of peace, it will be, we trust, with a renewed determination to persevere in that road which can alone lead to true national greatness.

Our own mechanics' institutes and kindred undertakings are just now commencing their autumnal session. We commend to them the consideration whether the principle of self-help cannot be more definitely recognised than has hitherto been the case. We hear with pleasure that the Working Men's College in Great Ormond Street is projecting an extension of its scientific programme in the approaching session. Let men and women be treated, not as artisans, mechanics, or gentlemen, but simply as men and women, standing toward the teacher only in the position of recipients of something which he feels the power and necessity of imparting; let no thought of any other relationship enter into this connection; and we predict for this and other equally admirable institutions a far wider popularity and usefulness than they have hitherto enjoyed.

REPLY TO PROFESSOR HUXLEY'S INAUGURAL ADDRESS AT LIVERPOOL ON THE QUESTION OF THE ORIGIN OF LIFE

II.

THE main argument must now be resumed: this having been only temporarily laid aside in order to inquire how far Prof. Huxley's "long chain of evidence" touched the real point at issue.

Having shown that, in reality, this has no immediate bearing upon the question in dispute, and having endeavoured to show to what extent the burden of proof rests with Prof. Huxley and others who affirm the universality of Redi's doctrine, it has now to be shown what evidence can be brought forward which may influence our judgment in the selection of one or other of the two possible modes by which alone the minute motionless specks of Living matter appearing in certain solutions can be supposed to originate. We must inquire, as much as possible independently of theoretical considerations, towards which of the two modes of origin—the germ or the germless—the evidence should induce us to lean.

It will be well, however, in the first place, to submit the following considerations to those who wish to form an unbiassed opinion upon the subject. Supposing that the minutest visible specks of living matter have originated from the growth of pre-existing *invisible* germs, there is still no reason whatever to induce us to believe that the invisible portions of Living matter would differ from visible portions in their power of resisting the destructive influence of heat. Whether visible or invisible, we are supposed only to have to do with Living matter, and it cannot be supposed that the qualities of this matter would vary simply *because* it existed in a state so minute as to elude our observation. What has been found to hold good, therefore, concerning the inability of visible Living matter to resist the destructive agency of heat may also be presumed to hold good for any invisible portions of Living matter. Invisible germs must be supposed to be amenable to the same influences as those which affect visible germs.* If the latter are destroyed by any given amount of heat, we should have every reason to expect that the former would also be destroyed under similar circumstances.†

It seems to me that *the only means* which we at present possess of throwing light upon this question, as to whether the minute Living things which appear under our eyes, in certain solutions, really derive their origin from pre-existing Living things, or spring into being *de novo*, is to subject other suitable solutions within hermetically sealed flasks, to a degree of heat which, on good evidence, is deemed adequate to kill all pre-existing Living things. If Living things are, notwithstanding the destructive exposure, subsequently to be found in the fluids when the flasks are opened, the evidence would seem to be strongly in favour of the *de novo* origination of such Living things—more especially if the heat employed had been great and long-continued. So far as all direct experiment and observation has hitherto gone, no Living thing whatsoever has been found to survive in a fluid which has been exposed for two or three minutes to a temperature of 110° C. And if we couple this fact with a due consideration of the fundamental unity in Nature of all Living matter, the supposition that any Living things—found in solutions that had been submitted to a *far greater* heat for two, three, or four hours—had braved this heat with impunity, would be an assumption seemingly much more improbable* than the only possible counter supposition, viz., that the Living things had been evolved *de novo*. The former supposition would be less likely to be true, because, instead of being consistent or harmonising with our general knowledge, it would seem to be a mere isolated fact bearing on its face the impress of grave improbability. *Bacteria* and fungus-spores which cannot, when made the subject of direct observation, resist the influence of a lower temperature, *are*, however, to be supposed capable of resisting the influence of a much higher temperature when their behaviour is watched by no human eye, though at a juncture when human prejudice emphatically requires that they should do so.† This extreme improbability—this isolated and otherwise unsupported notion—is cherished, whilst the other supposition, which is consistent with direct observation so far as it can go, and which is thoroughly in harmony with a great mass of scientific truth, is rejected. And why is it rejected? Because it is alleged that a great mass of human experience, having no immediate bearing upon this particular subject, and which is only related thereto by analogy, *seems* to make it improbable. And yet, as a matter of fact,—and although precisely the same reasoning is applicable against the alternative which they adopt—if the probability of a present *de novo* origination of Living things, after the fashion which is alone maintained, were to be admitted by every scientific man to-morrow, the whole body of human experience would remain perfectly undisturbed. A new probability, akin to a fact,‡ and one of the most extreme importance, would, it is true, have been added to the sum-total of human knowledge, and the only loss or contradiction would be, that those who had hitherto cherished the formula *omne vivum ex vivo* as the expression of a fundamental truth, would have to give it up. Like many another dogma, which in the course of time is toppled over, this expression of an over-hasty, though formerly justifiable, generalisation, now that it has been shown to be incompatible with the latest teachings of science, would have to fall into the shade of cold neglect.

* It was suggested to me by a friend that extreme smallness of size might be a protection against the influence of heat; in illustration of which possibility my attention was called to the fact that the water in capillary tubes will not freeze at times when that in larger vessels will become solidified. But although the water in the capillary tube does not freeze, this is due rather to some altered molecular condition of the fluid, and not because its temperature is not lowered just as much as that contained in the larger vessel which does freeze. I cannot see how smallness of size can confer immunity from alterations of temperature—more especially of any particles, however minute, which are contained within hermetically sealed flasks retained at a given heat for *four hours*.

† I have already pointed out (note p. 410) that the problem is utterly impossible to be solved if this be not granted as a probability; and that, similarly, without the concession that *invisible* crystalline matter resembled in its properties *visible* crystalline matter, it would be equally impossible to consider it as *proved* that a crystal can originate in a solution *de novo*, independently of a pre-existing crystalline germ.

* Although "germs," so far as we know them, are incapable of resisting the influence of great and prolonged heat, it was suggested by Prof. Rolleston, in the discussion which took place in the Biological Section on Sept. 21, that some germs *might* exist which were less amenable to the influence of heat, owing to the protein substances entering into their composition being in some peculiar isomeric state. We know for instance that *peptone*, which is a modification of albumen, is not coagulable by heat. All that we should deduce from this fact, however, seems to be this, that whereas ordinary albumen can, under the influence of heat, be made to undergo a certain isomeric modification by which it is rendered *insoluble*; this same albumen may, by a different process, be converted into *peptone*, a modification which is *not* capable of being converted into the *insoluble* isomeric condition by the application of heat. Too much stress must not be laid upon mere coagulability; and we must be, as it seems to me, further careful not to mix up our conception of this property too closely with another which is quite distinct, viz., as to the ability of Living things to withstand the influence of heat.

† Here we are brought face to face with the real difficulty. In order to explain the results of certain experiments, we *must* accept an apparent infraction of one or other of two rules which have hitherto been found to be universal, so far as human experience has gone. A Living thing has no more been known to be capable of surviving a temperatue of 150° C., than another Living thing has been *known* to arise *de novo*. Prof. Huxley, and those who think with him, appear to forget, in their present extreme unwillingness to give up the doctrine *omne vivum ex vivo*, that they can only retain it by abjuring another doctrine which has a similar seeming universality, so far as human experience has gone. We have nothing, then, but *probabilities* to guide us in our choice. Hence much difference of opinion will probably exist, till scientific men in general have come to adopt such physical doctrines concerning Life as those which Prof. Huxley has hitherto so ably taught.

‡ All so-called "facts" are, to the philosopher, only possibilities which vary in their degree of probability. This is inevitable, owing to the "Relativity of Knowledge," so that possibilities, probabilities, and facts, merge insensibly into one another.

What, then, are the facts which have been made known bearing upon the solution of this question?

Before the date of M. Pasteur's researches, it was generally supposed that Living things were incapable of surviving in a fluid which had been raised even for a few minutes to the temperature of 100° C.; but, after the results of his experiments, he claimed* to have a right to conclude therefrom that, whilst Living things were destroyed in acid fluids which had been raised for a few minutes to the temperature of 100° C., they were not certainly killed in alkaline fluids unless these had been raised for a few minutes to a temperature of 110° C.

This, however, is the point at which Prof. Huxley has chosen to close what he considers to be the history of the rise and progress of the doctrine expressed by the phrase *omne vivum ex vivo.* Then, ignoring all † that had been done in the interval between the years 1862 and 1870, he concludes a long but almost irrelevant chain of evidence with an account of three recent (?) experiments of his own, concerning the cogency and worth of which I have already spoken.

But let us briefly glance at the most important work which has been done, in order to throw light upon the subject in dispute, in the interval between the appearance of M. Pasteur's memoir in 1862 and the three experiments made by Prof. Huxley himself—work which he so summarily dismisses from notice.

I will say nothing now concerning the various experiments which have been made similar to those of M. Pasteur, but with contradictory results; I will refer rather to experiments in which the flasks and solutions employed have been exposed to a degree of *heat much higher and much more prolonged* than that which was proclaimed by M. Pasteur to be adequate to prevent the occurrence of all organisms in the solutions, and in which, nevertheless, Living things have been found on opening the flasks. As I have elsewhere mentioned,‡ Prof. Jeffries Wyman,§ of Cambridge, U.S., published an account in 1862 of experiments in which he had boiled fluids containing organic matter for a period of two hours, under a pressure of two atmospheres, that is to say, at a temperature of 120°·6 C. To the fluid so treated, no air was allowed access except what had passed through the capillary bores of white-hot iron tubes. And yet, when, after a certain time, the flasks were broken, Living organisms were found in the fluids contained therein. Prof. Mantegazza,|| of Turin, has obtained Living organisms from the fluids of hermetically closed flasks after these, containing the putrescible fluids and common air at ordinary atmospheric pressure, had been subjected for some time to a temperature of 140° C. Prof. Cantoni,** of Pavia, has found Living bacteria and vibrios in the fluids of similarly-prepared closed flasks, after these had been exposed in a Pepin's digester to a temperature of 142° C. for four hours. And, lastly, I have myself recorded experiments, †† made with the kind assistance of Prof. Frankland, showing that Living organisms almost similar to those which have been ascertained to be incapable of resisting the influence of a fluid raised to the temperature of 100° C. for a few minutes may be met with, after a time, in solutions which had been exposed in hermetically-sealed and airless flasks, to a temperature varying between 146° and 153° C. for a period of four hours. Whilst, by another experiment,‡‡ it was found that a fungus and spores, as nearly as possible similar to that which had been found in a living state in one of the former experiments, were all *completely disintegrated,*§§ after exposure for an equal period, and in a flask containing a similar solution, to the same temperature of 146° to 153° C.

Now, in reference to these results, it should be remarked that there is not one tittle of evidence, so far as I am aware, which can be adduced tending to show that any single Living thing can continue to *live* in a fluid which is exposed even for a few minutes to a temperature of 110° C.—the degree of heat which M. Pasteur thought necessary to ensure the destruction of all pre-existing Living things. And also it has been shown just as definitely that none of the lower Living things which have been submitted to the test, have ever been found to survive an exposure in *dry air** for 30″ to a temperature of 130° C. Still less, therefore, would they be capable of withstanding the influence of an *extremely condensed vapour* at a temperature of 150° C., or even at 140° C., for a period of *four hours.*† There is, at present, no reason whatever for inducing people to believe that the living things met with in the experiments of Professors Wyman, Mantegazza, Cantoni, and those made by myself in concert with Prof. Frankland, had been derived from germs which were capable of living through the fiery ordeal to which the flasks had been submitted, save the extreme reluctance of these people to bring themselves to believe that Livings things can now‡ arise independently of pre-existing Living matter. Moreover, it should be understood, that experiments of this kind seem to be such as are alone capable of aiding us to come to a conclusion on this, the only question in dispute—whether the motionless specks which appear in previously homogeneous solutions, are more likely to have proceeded from the growth of pre-existing *invisible* germs, or to have arisen quite independently of pre-existing Living matter, under the influence of molecular affinities analogous to those which are believed to lead to the formation of similar specks of crystalline matter.

And yet, without one word concerning the limits of vital resistance; with what must be considered as a tacit admission that the very organisms in question are destroyed in a fluid maintained at a temperature of 100° C., for 15 minutes; without a single explicit mention of the experiments to which I have just been referring; with a seeming utter inappreciation of their important bearing upon the great question at issue—Prof. Huxley, closing his historical summary with a notice of the labours of M. Pasteur, ends an almost completely irrelevant statement with the mention of three experiments of his own, which, if they are not to be considered as altogether worthless, are, certainly, of no conceivable value for the establishment of the doctrine which he supports, or for the overthrow of the supposition that Living things can at the present time arise *de novo.*

Surveying the field of science from the elevated "position in which the suffrages of his colleagues had, for the time, placed him," recognising it as one of his privileges and duties, with "due impartiality," to declare "where the advanced posts of science had been driven in, or a long-continued siege had made *no* progress," Prof. Huxley ventures, in the face of the facts above-

* That M. Pasteur's experiments did not warrant him, however, in coming to the conclusion that Living things were capable of living in an alkaline solution when this was exposed to a temperature of 100° C., I have endeavoured to show in NATURE, No. 37, pp. 224—228.

† With the exception of the recent investigations of Prof. Tyndall, which Prof. Huxley considers capable of supporting his own view of the question, although Prof. Tyndall has really done nothing whatever to convince the public that the organic dust which exists in the atmosphere is even in part made up of the "germs," about which he talks so freely.

‡ NATURE, No. 35, p. 175.

§ "*Experiments on the Formation of Infusoria, &c.*" Cambridge, U.S.

|| These experiments were not made in the interval above referred to, but even ten years before the publication of M. Pasteur's memoir. See *Journal de l'Institut, Lombard*, t. iii., 1852.

** See Gaz. Med. Ital. Lombard. Ser. Der. v. t. i, 1868, and two communications made to the Royal Lombard Institute, one in April 1868, and one in November 1869.

†† NATURE, No. 36. ‡‡ Ibid.

§§ See NATURE, No. 37, p. 219. The experiment in which the somewhat similar fungus was met with was No. 19 (NATURE, No. 36, p. 200), and to this I would particularly direct the reader's attention. The mode of appearance of the fungus, its gradual increase in size, as well as its microscopical characters, all point to its having been *a living fungus.* Whilst the partial preservation of the vacuum for 65 days shows pretty plainly that there was no unobserved crack in the glass. The partial destruction of the vacuum was most probably *due to the liberation of gases within the flask*, owing to some decomposition of the tartrate of ammonia during the growth of the fungus. *It is not likely that germs contained in the air could get through a crack, if any such existed, which was impervious to the air itself.*

* NATURE, No. 35, p. 170.

† Prof. Tyndall seemed to have completely forgotten all this during the discussion which took place in the Biological Section of the British Association on Wednesday, September 21. He there alleged as his principal reason why the conclusions which I am inclined to draw from my experiments should not be drawn—after I had pointed out to him that *I had no wish to exclude "germs" or Living things* from the flasks which were hermetically sealed,—that germs might have adhered to the upper portion of the flask, and might never have come into contact with the heated fluid. But this objection was seen to be futile in the face of the work which had been done concerning the influence of *dry heated air* upon lower kinds of Living things—work of whose existence Prof. Tyndall seemed to be in ignorance, or which he had entirely forgotten, until he was reminded of the opinions of M. Pasteur on this subject. Prof. Tyndall, indeed, *seemed* to know very little more of M. Pasteur's views than he did of my own. Until it can be shown, however, that any single minute Living thing *can* withstand the influence of a *condensed vapour* at 150° C., for four hours, the objection which he started so triumphantly remains and exists only as a *highly improbable supposition*, in the face of which I can again fearlessly state my conclusion—that, taking all the *evidence* as it at present exists, I am as much, even more, entitled to believe that the organisms found in my flasks had been evolved *de novo*, than that they had been produced from pre-existing germs of Living matter, seeing how universally destructible this has been shown to be by heat.

‡ Even though some of these are quite willing to admit the *possibility* of such an occurrence, and are ready to accept the notion that in past ages of the earth the first Living matter did so originate from a combination of mere non-living materials.

mentioned, to tell the British Association for the Advancement of Science, and the public generally, that Redi's great doctrine appears to be "victorious along the whole line;" whilst the views and experiments of those who think differently are thus referred to:—"On the other side the sole assertions worthy of attention are that hermetically sealed fluids which have been exposed to long-continued heat have sometimes exhibited Living forms of low organisation when they have been opened."

All comments on such a proceeding seem needless—the facts speak only too plainly for themselves.

I will, however, say a few words concerning the mere empty generalities which Prof. Huxley opposes to the definite results of an honest band of workers.

He commences in this way:—"The first reply that suggests itself is the probability that there must be some error about these experiments, because they are performed on an enormous scale every day with quite contrary results. Meat, fruits, vegetables, the very materials of the most fermentable and putrescible infusions, are preserved to the extent, I suppose I may say, of thousands of tons every year by a method which is a mere application of Spallanzani's experiment. The matters to be preserved are well *boiled* in a tin case provided with a small hole, and this hole is soldered up when all the air in the case has been replaced by steam. By this method they may be kept for years without putrefying, fermenting, or getting mouldy." This is a very plausible statement, certainly; and one apparently tending to confirm Prof. Huxley's views. But what are the real facts of the case? I have made many inquiries and some microscopical examinations during the last three days, the results of which I will now communicate to Prof. Huxley and others.

Having visited one of the largest establishments in London, and seen the whole process to which the meats and vegetables are submitted for preservation, the information I have to convey is of the most authentic description. For this opportunity, and for many particulars communicated in a long conversation, I am much indebted to the courtesy of Mr. McCall, of Houndsditch.*

A number of cases, enclosing the provisions, instead of being simply heated to a temperature of 212° F. as most people would understand from what Prof. Huxley said, are first heated in a large chloride of calcium bath (warmed by steam) to a temperature of 230° to 235° F. for more than an hour and a half. The hole through which the steam has been issuing is then closed with solder, and as soon as the last of the set has been thus hermetically sealed, a higher pressure of steam is turned on, by which the bath is quickly raised to a temperature of from 258° to 260° C.†—at which temperature it is maintained for more than half an hour. Thus it is now learned that the meats are exposed to a heat of 230° to 235° F. for more than one and a half hours, and then to a temperature of 258° to 260° F. for another half hour at least. All this is very different from the simple statement that the provisions are "boiled," for a time not specified. Prof. Huxley, in the next place, mentions the possibility of failure, though he seems to attribute all these to "unskilfully closed tins." Now, on inquiry, it appears that the number of unmistakable failures even in the very best establishments is very appreciable, and although many of these failures may be accounted for by defective closure, Mr. McCall assured me that in a certain number of cases, where not the smallest defect could be detected in the tin, where the mode of preparation was unexceptionable, and the provisions originally of the best description, yet for some inscrutable reason some of these tins did prove utter failures. Gas was found to be evolved within, causing them to bulge at the extremities, and when opened the meats either showed a central decomposition of a most fœtid character without mould, or else mould might be found on some portions of the surface. He further assured me that certain tins which had been thoroughly well prepared, and in which the provisions seemed to remain in a perfect state of preservation ‡ for two or even three years, might more or less suddenly show signs of a considerable evolution of gas within, owing to the provisions having fallen into a state of putrefaction. In other instances provisions would keep for ten years

* And also for the kind permission to make known what he had told me.

† Whilst I was in this establishment one of the baths was seen to have reached a temperature of 263°. F. It was boiling very briskly. The more or less solid contents of the tins would require a longer time to be raised to any given temperature than a fluid; so that, practically, the meats may have been exposed only for a comparatively short period to the higher temperatures mentioned. I may state that 230° and 260° Fahr., correspond to 110° and 126·6° C.

‡ As judged by evidences of a vacuum within.

or more without any appreciable change. I was informed also that turtle, and all the soups which solidified when cold, invariably remained good. Amongst these there were no failures. Mr. McCall was somewhat doubtful as to whether in hot weather, provisions were more prone to fail after severe thunder storms. He had, however, "often thought that electricity" had something to do with the failures. Some of the large retail sellers spoke much more decidedly to me as to the number of failures after thunder. On this question, however, I lay no stress—I merely repeat what I was told.

Wishing to learn what microscopical appearances would be presented by provisions which were sold as being "perfectly good," I procured three specimens from two of the most esteemed retail establishments, informing the original owners that I wished to submit them to a microscopical examination. One of these was a tin of "Julienne Soup," which had been prepared ten months; the second, "Salmon," prepared six months; and the third, "Lobster," only six weeks old. The "Salmon" when opened, had not an altogether pleasant smell; the other two seemed quite fresh. In portions taken from the surface of each, I found the most unmistakable evidences that slight changes had taken place. All presented an abundance of flat granular aggregations,* figure-of-8 bodies, and a very appreciable quantity of *Bacteria* and *Leptothrix* filaments—some of these latter being plain and others jointed. The *Leptothrix* filaments were mostly about $\frac{1}{30000}$" in diameter. Some of the *Bacteria* were $\frac{1}{8000}$" in length, and many were moving pretty actively in the specimens taken from the "Julienne" and the "Lobster" figure-of-8 particles. In the "Salmon," I also found, during my comparatively short examination, two or three portions of *Fungus*-filaments, having dissepiments within, and measuring $\frac{1}{6000}$" in diameter.

Thus, to sum up, it appears that provisions, prepared as above described,† which have been exposed for more than two hours and twenty minutes to a temperature varying from 110° to 126° C. do, not unfrequently, *for no discoverable reason*, fall into a state of decomposition which renders them useless, and that the only specimens which I have examined microscopically, three in number, all presented evidences that Living things had been growing and developing in the hermetically sealed tins.‡ Why, in some cases, the changes should be so small in extent as not to impair the value of the provisions, and in other cases these changes—passing through the more intermediate grades—should render the provisions utterly useless, I, or others holding similar opinions, can scarcely be called upon fully to explain. Certain it is, however, that the facts above mentioned, including the circumstance that the failures sometimes take place after the tins have been hermetically sealed for two or three years, and that gelatinous§ substances are the least prone to change—are all

* Some of these had undoubtedly arisen from a granular degeneration of the meats themselves. Some muscular fibres presented a healthy appearance, while others were more or less completely granular.

† I may state in reply to what was said by Mr. Eddowes in the discussion on Sept. 21, that the provisions examined by me had all been prepared by a process essentially similar to that adopted by Mr. McCall. I took care to ascertain this. The "salmon" was not prepared, as he supposed, in Canada, but by a well-known Scotch house.

Since the above was written, I have (Sept. 26) examined two tins which were prepared by Mr. McCall in 1861. One containing "Lamb and Vegetables" was perfectly good. It contained not a drop of fluid, though some glutinous matter was present. On microscopical examination I could find *no trace* of organisms. The other tin, containing "Veal and Peas," was also perfectly good; the odour was just like that of fresh meat. The contents were very dry, not a drop of fluid could be procured, although the surface was bedewed with a slight moisture. When a small portion, scraped from the surface and mixed with a drop of water, was examined microscopically, hundreds of extremely minute *Bacteria* and monilated chains were seen—all either dead, or else extremely languid. These results are very interesting when compared with what was found in the three other tins, whose contents were much more moist and contained actual fluid.

‡ Without reference to the question whether the *Bacteria* and *Leptothrix* filaments were living when seen by me, the very fact of their having been formed in such a very appreciable quantity, seems to make it more probable that they had been *developed* after the exposure to the heat within the hermetically closed tins, than that they had pre-existed in the fresh provisions in the state in which they were found. There was, however, no reason whatever for supposing that the *Leptothrix* filaments were dead, or that the slow movements of the *Bacteria* were not *languid vital* movements; between which and Brownian movements it is impossible to draw any line of demarcation.

§ It could not be supposed that a gelatinous substance would afford facilities for the molecular rearrangements to take place, without which no new evolution of Living matter would seem possible. On the other hand, if the Living things which are sometimes found in these cases are derived, as many will suppose, from undestroyed germs, it does not seem so easy to understand why they should not germinate on the surface of a gelatinous substance. The "Julienne soup" examined was not gelatinous, it rather resembled a moderately thick solution of gum in consistence.

rather strongly in favour of my view of the case, and will continue to be so, so long as our *knowledge* concerning the inability of Living things to resist the destructive influence of very high temperatures remains in anything like the same condition as it is at the present day.

Prof. Huxley is inclined to believe that there has been some error about the experiments recorded by myself and others. With regard to my own experiments, however, the chances of error were certainly diminished to a minimum. Certain fluids were placed in glass vessels, and were handed over to one of the most accomplished chemists in this country, with the simple request that he would extract most of the atmospheric air from the flasks, would seal them hermetically, and would then expose them to a temperature of 150° C. for four hours. All this is certified by Prof. Frankland to have been faithfully done.* One of the flasks was opened in the presence of Prof. Huxley himself, whilst another of them was opened in the presence of Prof. Sharpey; and although the others were opened when I was alone, I hope the results are none the less reliable. In the face of these facts, and of what has been detailed elsewhere, it seems difficult to imagine that the experiments are not really trustworthy.†

Prof. Huxley then concludes his observations on these experiments by saying :—"But if, in the present state of science, the alternative is offered us, either germs can stand a greater heat than has been supposed, or the molecules of dead matter, *for no valid or intelligible reason that is assigned*, are able to rearrange themselves into living bodies, exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be even for a moment doubtful."

Although this climax is thoroughly consistent with the style of the preceding remarks, I find it very difficult to understand why Prof. Huxley should have so much departed from his usual method of argumentation. I should like to ask him, however, whether he considers it the function of a scientific investigator to believe *only* in such seeming possibilities as he can at the time explain or account for; and also whether he who believes in the analogy between crystals and organisms,‡ can "assign any valid or intelligible reason" which is likely to be satisfactory to himself or to others, why the constituents of common salt, when in solution, should under certain circumstances aggregate into crystals of a cubical form; and why, on the other hand, the constituents of sulphate of soda should aggregate into rhombic crystals. Notwithstanding his inability to explain these facts, I suppose he nevertheless accepts them *as facts*, even although in the case of sulphate of soda, *almost exactly the same kinds of crystals result, whether they have proceeded from pre-existing crystalline germs, or whether they have arisen de novo.*§ Prof. Huxley seems only too much to overlook the fact that what may be perfectly inexplicable from one point of view, may, on the contrary, flow as a necessary consequence from one of an opposite nature. Although, therefore, as a disciple of Redi, the facts to which he has alluded may seem difficult to explain, Prof. Huxley must recollect that two rival doctrines are in question. And having two doctrines of almost equal probability to decide between, it seems to me mere childishness to reject a certain well-supported interpretation simply because it is inexplicable on the one hypothesis, and to think that this inexplicability is an argument against the interpretation given, when, so far from being inexplicable, this, *in the light of the counter hypothesis*, is nothing else than a logical consequence. That some such similarity as that which is alluded to should exist, is only to be expected by those who believe that the lowest living things are but the products of the molecular properties of a complex matter, and the "conditions" acting thereupon.* I entirely agree with Mr. G. H. Lewes, when, in a most valuable essay,† he points out that "similarity in the laws and conditions of Organic Combination must produce similarity in organisms, independently of relationship, just as similarity in the laws and conditions of inorganic combination will produce identity in chemical species." It is the extreme complexity of the materials in the one case, and their corresponding sensitiveness to modifying influences, which make it hopeless for us to think of ever getting the same uniformity of results, which we *are* able to attain when we have to do with simple inorganic materials. The difference, however, is one of degree, not of kind.

I enter a protest, therefore, against the first portion of Prof. Huxley's Inaugural Address, for the following reasons :—

1. Because it does not seem to be characterised by "due impartiality."

2. Because it is calculated to mislead the public; since what is represented as relevant and of first importance, has only an indirect bearing on the subject: *Abundance or paucity of germs in atmosphere.*

3. Because the real issues having already been pointed out by others, Prof. Huxley ignoring these, approaches the problem as though they had never been stated, and as though he himself were not aware of them: *Mode of origin of specks of Living matter in apparently homogeneous solutions.*

4. Because it allows room for the inference, and even suggests it, that evidence which is generally admitted to be of the greatest importance for the solution of the question in dispute, is really of little or no importance: *Limits of vital resistance to heat, and presence of Living organisms in closed vessels which had been previously exposed to great heat.*

5. Because, without any sufficient warrant, it throws doubt upon the "trustworthiness" of certain experiments, of whose real nature his audience and the public are not informed: *Experiments of Wyman, Mantegazza, Cantoni,* &c.

6. Because it opposes the definite results of these experiments by nothing but insufficient statements, and what appear to be crude suppositions: *Statements and assumptions concerning preserved meats.*

The general effect being, I conceive, an entire misrepresentation of the present state of knowledge upon the questions concerning the Origin of Life, which are at present under discussion.

H. CHARLTON BASTIAN

* See his description of the process, NATURE, No. 36, p. 199.

† The possibilities of error, which in a previous discussion (on Sept. 20) in the Biological Section, seem to have been suggested by Prof. Huxley, were two in number. First, that unperceived cracks may have been present in the hermetically sealed flasks, and second, that objects supposed to have been *Living*, may not have been so in reality. I have already spoken of these possibilities with reference to *Exp.* 19, and there is no better ground for either of the suppositions in reference to *Exps.* 17, 18, and 20. (See NATURE, No. 36, pp. 199—201.)

‡ See quotation, NATURE, No. 46, p. 411.

§ There is a very slight difference in the form of the crystals in the two cases, because in order to make sure of the absence of crystalline germs, the new crystals have to form under a different and exceptional set of conditions. But, notwithstanding what Prof. Huxley says, we find even a more striking divergence occasionally, in the case of organisms, which possibly have been evolved from similar materials though under different conditions. I have elsewhere said (NATURE, No. 37, p. 223):—"We find also associated with different sets of conditions, different kinds of Living things. In none of the crystals of tartrate of ammonia have I ever found a single distinct bacterium, and there has been the same complete absence of organisms of this kind in all my experimental fluids containing tartrate of ammonia and phosphate of soda, which have been sealed up *in vacuo*. This agreement is very striking, seeing that whenever a similar fluid, or a solution of tartrate of ammonia alone, is exposed to the air, then bacteria appear in abundance. There is a marked accordance, then, between the organisms which are produced in the experimental tubes *in vacuo*, and those which come from the cavities within the crystals," whilst these differ altogether from those which are met with in a similar solution exposed to the air. (See also what is said in *Note* on same page concerning the occurrence of *Sarcina*.)

* It is difficult, almost impossible, for us to say how far seemingly great differences in conditions, are really very different in respect to the influences which are most potential in leading the not-living to assume Living modes of combination, because we do not know for certain what these *most* potential factors are, and therefore how far these may be present or absent under circumstances apparently dissimilar.

† "Darwin's Hypothesis." *Fortnightly Review*, April 1868, p. 372.

**** *Owing to the great pressure on our space, we are compelled to postpone several articles of real value which are already in type.*

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

University College Lectures for Ladies

IN this week's number of NATURE I see it noted that among the courses of lectures announced for the ensuing winter by the Ladies' Educational Association in connection with University College, are included two upon scientific subjects (chemistry and experimental physics). May I venture to point out that the prospectus makes mention also of a third, namely, on logic, introduced by ten lectures on the psychology of intellect? This course was, certainly, designed as a scientific one, and indeed as a substitute for the course on mathematics suspended temporarily by force of circumstances. My own motive for drawing attention to the point will not, I think, be misunderstood, when so lately I had occasion in your columns to say a word for psychology as a natural science.

University College, Sept. 23 G. CROOM ROBERTSON

Mirage

IN connection with Mr. Kingsley's letter in your number of to-day on Mirage, I may mention that when in a steamer going up the Throndhjem Fiord, in Norway, last July, I saw some remarkable Mirage effects. In one case there appeared to be a large city, which altered as the ship advanced into a long line of very white cliffs of basaltic formation and then disappeared, and nothing was seen but very some low rocks; in other cases there appeared to be rocks suspended in the air at some distance from the surface of the water. It was a fine afternoon, and the sea very calm. W. P. M.

Liverpool, Sept. 22

Meteor

AT 8.30 p.m. on Sunday the 11th, a fine meteor was seen in the zenith traversing from East to West. It had a comet-like tail, and a star-like head; visible altogether for about ten seconds. In passing there was a "hish" sound, as of a rocket.

At 8 p.m. on Thursday the 15th, the Aurora or Northern Lights were very bright—mostly red, divided by rays of whiter light. Many persons, who were upon the pier, thought there was "a fire somewhere!"

Lowestoft, Sept. 16 SEPTIMUS PIESSE

Origin of Species and of Languages

THE extreme brevity of my former letter on this subject seems to have hindered Mr. Ransom, and perhaps other readers, from appreciating the analogical argument I used. Will you, in consideration of the importance of the inquiry, allow me now to illustrate that argument at greater length?

There are two sets of facts that stand out in marked contrast. No irrational animal has ever formed a language. Man alone, in all his varieties, has.

I agree with Mr. Ransom that no language has originated from an *intention* to form a new language; I see no reason to doubt that languages have arisen from the gradual variation, selection, and combination of a few primary sounds; and I think that existing languages are constantly undergoing change through the operation of physical, physiological, and other natural causes, irrespectively of reason. But the fact remains to be accounted for, that no animal unendowed with reason has ever selected and combined sounds into a language.

The cause does not lie in a want of significant sounds to begin with. No one who has ever owned a dog is ignorant how many emotional sounds—sounds, too, that vary greatly in individuals and varieties—*he* makes use of; but he has never even begun to make a language of them. Neither does the cause lie in a want of power to distinguish, and in the case of some animals, to imitate very accurately the natural sounds they hear, so as to have a supply of vocal symbols for things and occurrences ready for adoption if they will. But can any irrational animal be named that has ever begun to use such sounds as symbols denoting things or events, still less to modify them in order to express modified meanings, and far less to combine them into symbols of complex things, or into phrases, propositions, and sentences? The mocking-bird mimics the song of the whip-poor-will, the creaking of the wheelbarrow, the lowing of the ox, and the pattering of the rain; but does it ever, like the Greeks, Romans, and Gaels, speak of the ox by the name of *bo*; or, like us, speak of the rain as *pattering*; or modify that sound, like the Hebrew and the Teutonic races, into a name for the substance that patters (*matar, water, Wasser*), and use it to tell that it wants a drink? Least of all, has any irrational animal ever juxta-placed sounds, as the Chinese do, in different orders to express different relations between the things they denote; or with Aryans, modified sounds into prefixes and terminations to express metaphorically such abstract relations?

Every step in these processes involves an exercise of reason. True, there is no grand intention on the part of one man or nation to form a language, but there are countless intentions of individual men to express individual ideas and thoughts as they emerge, or to express them more accurately than before; and then, when one man by an exercise of reason devises and uses a new symbol or phrase, others imitate and adopt it. And so, while I admit that there are unintentional variations of words, and consequently (by degrees) of languages; and while I admit that there has been no intention to form a language as a whole, I think we must say that it is by countless intentions of rational beings that languages have been gradually formed.

It may be objected that savages possess languages, and that *they* are not rational. "My monkey *Wallady*," writes Sir Samuel Baker, "looks like a civilised being in comparison with the Nuehr savages." And yet, while the Neuhr savages have a language, Wallady has none, any more than my terrier Shag, knowing fellow though he be. Why this contrast, but because the most savage man is differenced from all other animals by the possession of reason?

Now, then, the argument against the theory of the formation of the species, or of their endowment with new organs, by a reasonless process, is this:—The experiment of the possibility of such a thing has been actually tried on the most extensive scale in the analogous matter of language, *and has failed*—failed in every instance except where reason has been at work to prompt and direct. Ought we not then to pause, while our data are so imperfect, and while science is making strides that may soon bring her to a point of view that will show her present logic to have been utterly at fault—to pause before entertaining a thought so revolutionary and perilous as that an eye, a beast, a man has been formed without presiding intelligence or design at all.

The subject is seductive; but I fear I have already encroached too much on your space. WILLIAM TAYLOR

The Cockroach

THE cockroach (*Blatta orientalis, Linn.*) has found an apologist in Dr. Norman Macleod, who asserts his incredulity in the current stories of this insect's bad habits. Cockroaches look, he says, like black priests among the beetles, and, like the priesthood generally, have been made the objects of misrepresentation and slander. Anyhow, the doctor treats as mythical the tradition, constant on ship-board, that cockroaches are in the habit of nibbling the nails of those who sleep with their feet uncovered. Not only are they harmless, but they are absolutely useful, inasmuch as they may be readily trod upon and killed by all who are willing to gratify their feelings of disgust and benefit society. In the history of the cockroach we can trace the origin of the nail-nibbling myth, if myth it be. The insect is indigenous in the warmer parts of America, and, in spite of its Linnean name, is only oriental through having been carried to the East by shipping. It has a natural love for warmth and for sweet things, and can indulge the latter taste by feasting on the feet of natives engaged in sugar manufacture. If Gilbert White is correct in his surmise that the insect was not introduced into England until late in the last century, its powers of reproduction and adaptation must be very large. It is, of course, very difficult to identify with absolute certainty the insects mentioned in classical authors, but there is a good deal to lead one to suppose that the μυλακρὶς mentioned by Aristotle and the *Blatta histrinorum* of Latin writers was the same as our loathsome pest. The English name is curious and worth investigation, but unhappily there is so much guess-work employed in derivations that this branch of philology cannot claim to be recognised as one of the "exact sciences."

Norton Court, Weobley C. J. ROBINSON

On the Dissipation of Energy

THE value of the successive numbers of NATURE is not a little enhanced by the papers of Professor Balfour Stewart on "Energy," which also lead us to long for his forthcoming volume on "Physics." If that work prove equal to that which he has already published on "Heat," it will give us a manual which may well compare with the best of those which have been published abroad, and it will besides possess a freshness of its own.

But is it desirable that the doctrine of the conservation of energy should be represented everywhere as a modern discovery? No doubt *the experimental verification* of the transformability of equivalent quantities of mechanical power of various kinds into equivalent temperatures is a modern discovery. But *the doctrine itself* belongs to the epoch of Leibnitz, and was deliberately set forth by that great philosopher himself. "I do not" (says he) "undertake [in his last correspondence with Dr. S. Clarke] to establish my dynamics or my doctrine of forces. This would not be the proper place for doing so. However, I can very well reply here to the objection that has been made to me. *I have maintained the conservation of active forces in the world.* It was objected to this, that two soft non-elastic bodies on meeting lose some of their force. I answer no. It is true that the masses lose it as to their entire movement, but their particles receive it, being internally agitated by the force of the meeting. Thus the loss is only in appearance. The forces are not lost but only dissipated among the small particles. Now this is not to be lost, but to act as those do who change a piece of money into small coin." * Is it said that in the above words Leibnitz does not state that one of the principal forms of the incident force when dissipated in bodies is heat, which is known now to be the fact, or that perhaps he did not know that heat was a mode of action at all? the answer is, that it was never doubted by any philosopher of that epoch that heat is a mode of motion. And in the *Micrographia* of R. Hooke (see specially Obs. 6, 7, and 8), there is a discussion of phenomena from this point of view as interesting and as accurate as any that is to be found in any modern book.

But a no less important consideration for science is the decision of Sir W. Thomson, Professor B. Stewart, and other physicists of this school, that the doctrine of the dissipation of energy is co-ordinate with its conservation, and that the destiny of the universe is to become its own cemetery! This theory, expressed in its most general terms is to the effect that all rectilinear motions (locomotion) naturally tend to be transformed into intimately reciprocating motions (heat) which are naturally irrecoverable into their first forms, and which tend to be ultimately so distributed that externally universal repose or death must ensue. Now, in view of all the experiments that have as yet been made, and all the results of equivalent transformations between foot-pounds and temperatures which have been obtained, this is surely a very bold generalisation. It is an inference from what man finds in his *work* as to what nature must do in her *play*. But there are differences between the two which have not been duly considered. Thus to us the concrete state is everything, for we are ourselves concretes. We live from moment to moment only by condensing and concreting the aeriform. But Nature delights in the aeriform. And such is the tendency of material elements in general to the aeriform state, as to lead legitimately to the inference that the concrete state is a forced and a defective state of matter which is possible to the material elements only when somewhat of their full complement of virtue has gone out of them. As to heat, has not the production of heat in a concrete body expressly for its function to emancipate the constituent particles more or less, so as to put them in the way of gaining the aeriform state? Are not almost all concretes at almost all known temperatures continually giving off particles into the aeriform state, and that all the more the hotter they are? Even iron and clay are not without a smell in a damp atmosphere. Adopting then, as is now generally done, the aeriform or nebular state as primeval, is not the proper corollary of the doctrine of the conservation of force—not the reduction of the system to a dead mass, but its restoration to a fully aeriform nebular or ætherial state, with new fitness for producing all those phenomena which the nebular hypothesis supposes? Would not such be the issue supposing the cosmical action to be all in one direction? And if that action be not, as we see it is not, all in one direction, but in two reciprocal directions, is not the corollary of the doctrine of the conservation of force to the effect that in its general features the mundane system shall remain as it is?

As to the sun to which we in this planet owe everything, since there is no actual evidence that his action upon us now is less powerful than it ever was, would it not be well, instead of being so much concerned in producing fuel for him (with such indifferent success), rather to take into consideration Dr. Clarke Maxwell's formulæ in physical optics, which lead to the conception that the solar radiation is an electro-magnetic action? If so, then the sun would be always receiving as much as he was giving, and would not be wasting his energy save on the dissimilar bodies (the planets) which circle round him. With respect to the celestial spaces in general he may possibly be insulated in the æther by his own heat like a drop of water in the spheroidal state. Meantime the 22nd of December may bring us some discovery as to his structure. J. G. M.

* See "A Sketch of a Philosophy," part I. p. 2. (Williams and Norgate.)

NOTES

THE foundation stone of the new building for Owens College, Manchester, was laid on the site which has been purchased in Oxford Street on Friday last. The ceremony was performed by the Duke of Devonshire, K.G. The building has been designed by Mr. Waterhouse, the architect of the Manchester Assize Courts and of the new Town-hall. The style of the building is Gothic. It will have accommodation for 600 day students, and for a much larger number of evening students. A sum of 102,000*l*. has been placed at the disposal of the building committee, 67,000*l*. of which is at present available for the erection of the college. The cost of the building is 90,000*l*., so that a sum of from 25,000*l*. to 30,000*l*. is still required for building purposes.

WE greatly regret to have received intelligence of the death at Geneva, on the 18th inst., of Dr. Augustus Waller, F.R.S. Dr. Waller held a high place among those physiologists who have enriched their science by original research. He is best known for his important contributions to the physiology of the nervous system, and especially for the introduction of a new method of investigation applicable to various important objects of neurological inquiry, which, not only as used by himself, but in the hands of others, has tended materially to advance the knowledge of that department. We owe also to Dr. Waller original and valuable observations on various other physiological questions. In acknowledgment of his scientific labours he twice received the Monthyon Prize of the French Academy of Sciences, first in 1852 for a research, in which he was assisted by Prof. Budge, of Bonn, and again in 1856 for experiments, exclusively his own, showing an important relation between the nutrition of nerve-fibres and their connection with nervous centres. For these and other researches in Experimental Physiology the Royal Society awarded him one of the royal medals for 1860. Dr. Waller began professional life as a general practitioner in Kensington, but his growing passion for original inquiry in science led him to devote himself to it entirely, and with the exception of a short time that he was Professor of Physiology in Queen's College, Birmingham, he resided abroad, and enjoyed the intimacy of the most celebrated Continental physiologists, who thoroughly appreciated his merits. Latterly he went to reside at Geneva, and commenced practice as a physician, still, however, continuing his physiological pursuits. He died quite suddenly in a fit of Angina pectoris, to which complaint he had been for some time subject.

WE are able to state that the *Quarterly Journal of Science* has passed into the hands of Mr. W. Crookes, F.R.S., who will from the present time be sole proprietor and editor.

THE American Science Association met at Troy, N.Y., on August the 17th. Professors Agassiz, Dana, and Henry were unavoidably absent. Papers on subjects of scientific interest were read by Professors Stimson of Boston, Winchell of Michigan, Orton of Vassar, Bradley of Jersey City, and many others.

IN addition to the professorial chairs already instituted at the University of Otago, New Zealand, the council of that University have now resolved to institute a chair of natural science, the salary of which will be 600*l*. per annum, besides class fees, &c., commencing to run from the day of embarkation. No religious test will be required of candidates.

THE programme of the lectures of the Birmingham and Midland Institute for the forthcoming session include a large proportion of a scientific character, as will be seen from the following list:—1870. October 3 and 10, "The Movements of Gases," by Professor Odling. October 17 and 24, "Extinct Animals intermediate between Reptiles and Birds," by Professor Huxley. October 31 and November 7, "Erasmus Darwin and Anna Seward, their Works and their Friends," by George Dawson. December 5, "The Lost Tribes of Tasmania," by James Bonwick. 1871. February 13, "Primitive Civilisation," by E. B. Tylor. March 13 and 20, "The Astronomy of Comets," by Professor Robert Grant. March 27, "War Ships and their Guns," by E. J. Reed. March 3 and 10, "Recent Researches in Physical Science," by W. F. Barrett.

AMONG the lectures to be delivered during next session before the Exeter Literary Society are "Recent Researches in Spectrum Analysis," by Rev. R. Kirwan; "Queer Flames, and what they have to say for themselves," by C. Meymott Tidy; "The Orbs of Heaven," by W. F. Quicke; and "Stonehenge compared with some other megalithic monuments," by Rev. R. Kirwan.

WE have received a prospectus of a new American Scientific Journal, to be called the *American Journal of Microscopy*. It will endeavour to create an elevated taste for microscopic science among the people. It will give full descriptions of the construction and ways of using the various kinds of microscopes and other optical instruments, the selection, gathering, and mounting of the most beautiful and interesting microscopic objects, microscopical diagnosis of diseases, reports of microscopical societies, and, in short, everything pertaining to microscopy. This journal will be ready for subscribers and agents about the 1st of October, and will be issued in neat quarto form of sixteen pages, of suitable size for preserving and binding, printed on fine white paper, in clear type, at one dollar per year. It will be published at Chicago.

WITH respect to the "whale of the bottle-nose species" said to have come ashore at Burntisland, Prof. Turner states that the animal is not one of the toothed whales, but a small whalebone whale. The plicated belly and the fatty fin on the back, placed it amongst the *Balænopteridæ* or finner whales. The white baleen, dashed here and there with pink, the form of the skull, and the elongated cruciform breast-bone, proved it to be the *Balænoptera rostrata* or piked whale—the smallest in the baleen whales inhabiting the North Sea. The specific distinction of this cetacean was first established by the late Dr. Robert Knox, from a specimen taken in 1834 in the stake nets at Queensferry, who named it the *Balæna minimus borealis*. Several specimens have since that time been stranded on the coasts of the Firth. Mr. Prentice has, with great liberality, presented the skeleton to the Anatomical Museum of the University of Edinburgh.

MR. W. G. SMITH'S "Clavis Agaricinorum," a paper originally presented to the Woolhope Naturalists' Field Club, and already printed in the *Journal of Botany*, will be very acceptable to mycologists in the separate and more complete form in which it is now published. Mr. Smith follows essentially the system of Fries and Berkeley, in dividing the enormous genus of *Agaricus* (numbering 450 British species alone) into five series, termed partly from the colour of the spores, *Leucospori*, *Hyporhodii*, *Dermini*, *Pratellæ*, and *Coprinarii*. Each of these five series he then divides into ten sub-genera, distinguished by characters of the hymenophorum, each of the five series of ten sub-genera closely corresponding to every other such series. Out of the fifty sub-genera which would thus be derived, only thirty-two are at present known, the remaining eighteen links in the chain remaining to be discovered. A number of very clear and carefully-executed diagram-plates which are appended will be of great service in rendering intelligible to beginners the principles of the arrangement of this exceedingly difficult genus.

PROF. O. C. MARSH has reprinted from the *American Journal of Science and Arts* several papers of considerable interest of American palæontology; Notice of some fossil Birds from the Cretaceous and Tertiary formations of the United States; Notice of some new Mosasauroid Reptiles from the Greensand of New Jersey; and a description of a new and gigantic fossil Serpent (*Dinophis grandis*) from the Tertiary of New Jersey.

WE are informed from American sources that the Western Union Telegraph Company in America propose to co-operate with the War Department, for the rapid transmission of telegraphic reports of "the operation and reports of storms for the benefit of commerce on our Northern lakes and sea-board." The practical value of the Meteorological information that can thus be rapidly sent across the great continent of America, from the Pacific to the Atlantic, cannot but be very great, and there is every hope that the War department will close with the offer.

SIR RODERICK MURCHISON announced to the Geographical Section of the British Association at Liverpool the receipt of the following telegram relating to the safety of the *Germania*—a little vessel which sailed some time ago on an expedition to the Arctic regions, chiefly at the expense of the German geographers and naturalists:—"Freeden to General Sabine. Koldeway—wintered in East Greenland, 70°; sledged to 77°. Arrived at Bremen last night. Extensive results—best health. Hamburgh, Sept. 12."

THE Consul-general of Chile at Bogota, the capital of Columbia, has written to his government to ask for particulars on the "Baldia" plant, reputed to be a great specific against liver complaint.

THE Economic Society of Guatemala is endeavouring to promote agricultural education there by means of books.

ONE of the most disastrous earthquakes we have lately recorded is that in the large city of Batang, on the Kin-sha river in Tibet. It began on the 11th April, and there were various shocks until the 9th May at 10 A.M., when it shook the whole city, causing a universal conflagration. Above 10,000 people were burnt to death. Several neighbouring villages shared the same fate. Many public buildings were destroyed, including those of the French millionaires. After the great shock others came on, and the people remained camped out for five days.

A SLIGHT earthquake is reported on the 26th July at 11 to 5 P.M., at Managua in Nicaragua, produced it is supposed by the volcano of Momstombe, from which subterranean rumblings were heard nearly every night. The people of Leon state that they have noticed flames and smoke proceeding from the crater. This seems to be the same earthquake that is reported from the neighbouring Republic of San Salvador on the 27th July. There it was strong, and lasted some seconds. On the 28th there was another, at 11.30 A.M., of a more violent character, but no serious damage was done.

ON the morning of the 1st August there was a slight shock of earthquake at Calcutta at about 5 minutes to 10.

THE Arabian Nights are in progress of realisation. Deposed sultans and princesses in disguise are not wanting; and the gold diggings furnish nuggets as large as any holder of a magic lamp can want. Aladdin's jewellery is now under realisation. The Diamond diggings in the Vaal River are the scene of successful venture, and one company in six months has got 22,000*l* worth of diamonds. Women and babies are now taken to this scene of fortune, where bands of music, billiard tables, and other accessories of pleasure, have already appeared. Aladdin had not a billiard table when shut up in the cave of diamonds.

THE BRITISH ASSOCIATION

LIVERPOOL, *Wednesday*

THE number of tickets issued has now increased to 2,800, very considerably more than was expected either by the officers of the Association or by the Liverpudlians. The roll of eminent foreigners is also much larger. In addition to those mentioned in my first letter we have now the company of Prof. Tchebichef, of St. Petersburg, one of the most distinguished of living mathematicians, Prof. Plateau of Bruges, Dr. Anton Dohrn of Jena, Prof. Von Baumhauer of Leyden, and others. Some of these have added greatly to the interest of the sections in which they took part, and have been very warmly received. Dr. Dohrn in particular was greeted with much warmth, from the fact that three weeks ago he was summoned to join the German army and had not since been heard of by his English friends. The statement of a local paper that Prof. Hofmann of Berlin is here is incorrect. A letter was, however, read from him in the Chemical section, soliciting contributions in kind from our chemical manufacturers for the needs of the German hospital service. A large audience met to-day in the Geographical section to hear Lord Milton read his paper on "Railway Routes across America;" and the lecture-room in which the Ethnological sub-section holds its meetings was crammed whilst Sir John Lubbock was discoursing on "Stone Implements from Western Africa." The small and inconvenient Crown Court in St. George's Hall devoted to Section A had a much larger complement than usual while the Rev. F. Howlett read his paper on "Solar Spots," and exhibited his elaborate diagrams, showing the great interest now taken by the public in solar phenomena; although, at the same time, Section G offered a counter attraction, especially to the members connected with the town, in Mr. Mackie's and Sir E. Belcher's papers on "The Unprotected State of Liverpool." The heavy rain and thick fog of yesterday morning have both cleared off, and the weather is again everything that could be desired.

The subject of Spontaneous Generation is undoubtedly *the* question of the meeting of the British Association for 1870. The title of the paper by Professor Huxley which headed yesterday's list in the department of Zoology and Botany, did not appear to bear directly upon it, and yet it was generally understood that it would reopen the subject. The President's discourse, for he had scarcely a note before him, was a popular account of the mode of development and form of those minute structures which the microscope reveals in such prodigious numbers in infusions containing organic matter, *Penicillium*, *Torula*, *Bacterium*, and *Vibrio*. He adduced arguments in favour of the theory that these various bodies are not distinct organisms, but are different modes of development of the same substance, and a more admirable and luminous exposition, it was generally admitted, has seldom been delivered. In the course of his remarks, Prof. Huxley took occasion to explain the difference between the "Brownian" motion of the molecules of inorganic matter, and the vital motions of living matter, and expressed his conviction that the motions observed by Dr. Bastian in the infusions which had been subjected to long-continued high temperatures, were referable to the former and not to the latter cause. During the discussion which followed, Dr. Bastian entered the room, but when called on by the president of the section, preferred deferring his reply till the following day. This morning Dr. Bastian gave an account of his experiments on the contents of hermetically sealed cases of preserved meats, with which the readers of NATURE are already familiar, and reiterated his conclusion that the facts he had elicited were such as to throw on the Biogenists the burden of proof that life did not really, as was apparently the case, originate *de novo* from lifeless materials. Professor Huxley was not able to be present at this discussion, but a somewhat sharp passage of arms took place between Dr. Bastian and Professor Tyndall, each maintaining his well-known view respecting the atmospheric germ theory. The reply of Prof. Tyndall, "Prof. Huxley's lieutenant," as he was described by the president of the section, was not generally accepted as conclusive, in consequence of his apparently not having made himself thoroughly acquainted with the facts of the series of experiments performed by Dr. Bastian.

Several of the sections had got through their programme of papers yesterday, but the majority sat this morning, and Section D even intruded into the time fixed for the meeting of the General Committee which concluded the business of the meeting. In opening the business of the committee, Professor Huxley remarked that whatever reports are issued in the name of the committee of recommendations, it should be clearly understood that they are issued without any sanction of the Association, and that the responsibility of these reports, and the conclusions that may be drawn from them, rest entirely with the authors of the reports. The Association does not for one moment endorse views on subjects on which persons of eminence may hold different opinions. All that the Association is responsible for, is to place in a position for making reports gentlemen who are competent to make them; what these gentlemen say is entirely on their own responsibility. Grants were then ordered for various purposes, as recommended by the committee of recommendation, in accordance with the list which will be found in another column. A number of resolutions not involving grants of money, which came up from the various sections, were also passed.* The most important among these were:—A resolution on the subject of Vivisection, and appointment of a committee, from Section D; a resolution, brought up from Sections A and B, requesting the Council to co-operate with the Councils of the Royal and Astronomical Societies urging on the Government the propriety of reconsidering their refusal to aid in the observation of the approaching Total Eclipse of the sun by the grant of a vessel; a resolution from Sections A, B, and G, with reference to the proposed foundation by the Government of an Engineering College, to the effect that it is undesirable that any fresh grants should be made for purposes of scientific instruction until the Royal Science Commission has reported. The only resolution which excited much discussion was one from Section F in favour of the adoption of a compulsory metric system of weights and measures. An amendment was moved by Mr. Hawksley, and seconded by Professor Rankine, that the recommendation should be limited to the adoption of the metric system for international purposes, on the ground that a binary system is now, and always will be, the mode adopted by uneducated persons in all countries for their own purposes, as being the simplest. The amendment was adopted by a considerable majority.

Saturday

Those who were present at the meeting of the British Association which has just concluded its sittings, speak of it as presenting some points of favourable contrast with preceding meetings. There were still a good many papers presented of a purely technical character, which would have been far more suitable for the transactions of one of the learned societies, and which were perfectly unsuited for the miscellaneous audience collected to hear them. On the other hand there were not a few, perhaps a larger number than on previous occasions, which treated purely scientific subjects in a philosophical manner, calculated not only to interest but to enlarge the minds of all who had the pleasure of listening to them. I may illustrate my remark by reference to one division only, Section D. A glance over the titles of the papers read in this section will at once suggest several belonging to the former category. On the other hand, Dr. Brown-Sequard's paper on "Various Alterations of Nutrition due to Nervous Influence," and Prof. Flower's on the "Connection of the Hyoid Arch with the Skull," though treating of subjects belonging to pure science, were discussed in a manner which carried with them a non-scientific audience, who cannot have failed to carry away some ideas altogether new to them. In the same manner Prof. Huxley's

* Owing to these resolutions not having been yet sanctioned by the Council, we are compelled to defer their publication for the present.

"History of the Development of the Lowest Forms of Infusorial Life" was a model of clearness and succinctness; and the President himself paid a compliment to Mr. Bennett's paper, which followed, on the "Theory of Natural Selection looked at from a Mathematical Point of View," as the first attack on the hypothesis conceived in a philosophical spirit, and such a paper as it is the special object of the Association to bring out. The great question of the meeting, that of "Spontaneous Generation," has already occupied sufficient space in our columns; we look to the Liverpool meeting as the starting-point from which the discussion must in future be carried on in a truly philosophical and inductive spirit, free from the dogmatism which has hitherto surrounded it.

The attitude of Liverpool towards the Association has been somewhat of a puzzle to its members. If we were to judge from the remarks heard in the streets and from the ordinary visitors at the hotels, and the "intelligent policemen" who were posted here and there to direct the wandering visitor, we might suppose that a meeting of the British Association was a monthly occurrence in Liverpool, so utterly indifferent did they appear to it. The prevalent opinion appeared to be that we were another Church Congress in some sort of disguise. That there is a public in Liverpool who watched its proceedings with intelligence and interest was evident; but this, at least, is certain, that such a public has no representative in the Liverpool press. Let any one who wishes to see what the papers say about the Association take up the *Liverpool Courier* of this morning, where they will find a leader devoted to a ponderously jocose reply to an innocent remark of ours last week. "Save us from our friends" was the remark with which we laid down the following comment on the Association:—"The philosophers have come and gone, and Liverpool is at peace. They had a week of tremendous talk—tremendous not only as regards volubility, which is a blight we are well accustomed to in Liverpool, but also as regards the technical ponderosity of the themes. However, it is over at last, and though we entertain the most devout admiration for science and scientific people, a sigh of relief escapes involuntarily as we speed the parting guest. It was impossible that we could live long at the high pressure of the past week. It was more than human nature—of course we except scientific human nature, which is on a higher round in the Darwinian ladder than we poor cotton-dealers and traffickers—could bear for another seven days. The philosophy was too exalted for our earthy intellects, and we are bound to confess that the local savans—we have quite a battalion of the genus, such as they are—manifested signs of weariness under the tempest-torrent of imported intelligence. Of course it was right that the personages who hang on to the skirts of philosophy should be members of the British Association; but what a change between the first and the last day! Mathematics and biology were a gay pastime in the initial stage, the wise men and the learned ladies were positively vivacious over the germ-theory, and not a few were prepared to enter the lists against any opponent of abiogenesis. But this enthusiasm could not be sustained, for there was no basis of real intelligent interest, and the animation waned as the stream of science still flowed on."

The reporter of the *Courier* clearly found that he was getting beyond his depth, and wisely absented himself from the sittings during the last few days, or he would have known that the interest showed no signs of abatement. I venture to predict that those Liverpudlians for whom the meeting was not altogether "caviare" have been so well pleased with the success of the meeting that in due time we shall have another invitation to pay the town a visit, when I hope large numbers of those who have now dispersed will again meet.

It will be seen that the annual grant of 600*l.* for the purposes of the Kew Observatory has only two more years to run. We must trust that the additional means thus placed at the disposal of the Council will be applied to purposes directly connected with the real advancement of science, and will not be frittered away in bricks and mortar.

After holding the concluding general meeting which followed that of the general committee, the visitors to the Association rapidly dispersed; a small proportion, however, staying to avail themselves of one or other of the excursions which were arranged for the Thursday. In this respect next year's meeting at Edinburgh will afford a much larger scope for the lovers of the picturesque. B.

Resolutions of the General Committee

Applications for Reports and Researches not involving Grants of Money

That Prof. R. B. Clifton, Mr. Glaisher, Mr. Huggins, Dr. Matthiessen, Prof. W. Hallows Miller, Dr. Balfour Stewart, Mr. G. Johnstone Stoney, Lieut.-Col. Strange, and Sir J. Whitworth, Bart., be a Committee for the purpose of reporting on Metric Standards, in reference to the communication from Prof. Jacobi, appended hereto:—

"The Academy of Sciences of St. Petersburg, observing that the Standard Metric Weights and Measures of the various countries of Europe and of the United States, differ by sensible, though small, quantities from one another, express the opinion that the continuance of these errors would be highly prejudicial to science. They believe that the injurious effects could not be guarded against by private labours, however meritorious, and they have therefore recommended that an international commission be appointed by the countries interested to deal with this matter. They have decided to bring the subject before the Russian Government, and have appointed a Committee of their own body, who have drawn up a careful Report containing valuable suggestions; and they have deputed Prof. Jacobi to lay this Report before the British Association, and to request the Association to take action in reference to it."

That Dr. Anton Dohrn, Prof. Rolleston, and Mr. P. L. Sclater be a committee for the purpose of promoting the foundation of zoological stations in different parts of the world, recognising the foundation of a zoological station at Naples as a decided step in this direction; that Dr. Anton Dohrn be the Secretary.

That the committee of Section D be requested to draw up a statement of their views upon Physiological Experiments in their various bearings, and that this document be circulated among the members of the Association.

That the said committee be further requested to consider from time to time whether any steps can be taken by them or by the Association, which will tend to reduce to its minimum the suffering entailed by legitimate physiological inquiries, or any which will have the effect of employing the influence of this Association in the discouragement of experiments which are not clearly legitimate on live animals.

Resolution passed by the Committee of Section D (Biology):

"That the following gentlemen be appointed a Committee for the purpose of carrying out the suggestion on the question of Physiological Experiments made by the General Committee:—Professor Rolleston, Professor Lawson, Professor Balfour, Dr. Gamgee, Professor M. Foster, Professor Humphry, Professor W. H. Flower, Professor Sanderson, Professor Macalister, and Professor Redfern; that Professor Rolleston be the Secretary, and that they be requested to report to the Committee."

Involving Application to Government

That Sir R. I. Murchison, Bart., Sir Charles Lyell, Bart., Mr. Findlay, and Admiral Sir John D. Hay be a committee for the purpose of bringing to the notice of the Commissioners of the Admiralty the importance of revising the survey of the west coast of South America, with a view to ascertaining what changes have taken place in the levels since the recent great earthquakes on that coast; that Mr. Clements Markham be the secretary.

That Prof. Jevons, Mr. R. Dudley Baxter, Sir John Bowring, Mr. J. T. Danson, Mr. James Heywood, Dr. W. B. Hodgson, and Prof. Waley be a committee for the purpose of urging upon her Majesty's Government the expediency of arranging and tabulating the results of the approaching census in the three several parts of the United Kingdom in such a manner as to admit of ready and effective comparison; that Mr. Edmund Macrory be the secretary.

SYNOPSIS OF GRANTS OF MONEY appropriated to Scientific Purposes by the General Committee at the Liverpool Meeting in September 1870. The names of the members who would be entitled to call on the General Treasurer for the respective Grants are prefixed:

	£	s.	d.
Kew Observatory			
The Council.—Maintaining the Establishment of Kew Observatory	600	0	0
Mathematics and Physics			
*Brooke, Mr.—British Rainfall	50	0	0
*Thomson, Professor Sir W.—Underground Temperature	150	0	0
*Tait, Professor.—Thermal Conductivity of Iron and other Metals	20	0	0
*Thomson, Professor Sir W.—Tidal Observations	100	0	0
*Glaisher, Mr.—Luminous Meteors	30	0	0
Crossley, Mr.—Observations of Lunar Objects	20	0	0
Herschel, Sir J.—Recomputation of the Gaussian Constants for 1839	50	0	0
Stewart, Professor B.—Standard Measures of Electrical Capacity	20	0	0
Hockin, Mr.—Standard Electrodynamometer	20	0	0
Thomson, Professor Sir W.—Standard Potential Gauge	20	0	0
Chemistry			
Williamson, Professor.—Reports of the Progress of Chemistry	100	0	0
Brown, Professor Crum.—Thermal Equivalents of the Oxides of Chlorine	25	0	0
Geology			
*Lyell, Sir C., Bart.—Kent's-Cavern Exploration	150	0	0
*Duncan, Dr. P. M.—British Fossil Corals	25	0	0
*Symonds, Rev. W. S.—Sedimentary Deposits in the River Onny	10	0	0
*Mitchell, Mr. W. S.—Leaf-beds of the Lower Bagshot series	20	0	0
Thomson, Mr. James.—Sections of Fossil Corals	20	0	0
Scott, Mr. R. H.—Mesozoic Deposits of Omenak, North Greenland	50	0	0
Woodward, Mr. H.—British Fossil Crustacea	25	0	0
Busk, Mr.—Fossil Elephants of Malta	25	0	0
Biology			
*Carruthers, Mr.—Fossil Flora of Britain	25	0	0
*Sharpey, Dr.—Physiological Action of Methyl Compounds	25	0	0
*Sclater, Mr.—Record of the Progress of Zoology	100	0	0
*Foster, Professor M.—Heat Generated in the Arterialisation of Blood	15	0	0
Balfour, Professor.—Effect of the Denudation of Timber on the Rainfall in North Britain	20	0	0
Geography			
Murchison, Sir R. I., Bart.—Exploration of the Country of Moab	100	0	0
Statistics and Economic Science.			
*Bowring, Sir J.—Metrical Committee	25	0	0
	£1840	0	0

* Re-appointed.

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE

Abstract of an Investigation of the Mathematical Theory of Combined Streams.—Professor W. J. M. Rankine, F.R.S. The object of the investigation of which this is an abstract is to extend to combinations of any number of streams of fluid, whether liquid, vaporous, or gaseous, the principles which have been applied to combinations of two streams by previous authors, and especially by Professor Zeuner, in his treatise entitled "das Locomotiven-Blasrohr" (Zürich, 1863). Several component streams of fluid, each coming through its own supply-tube and nozzle, are led in directions parallel to each other, into one end of a cylindrical space called the junction-chamber, in which they mingle so as to form a resultant stream; and that resultant stream escapes from the other end of the junction chamber through an orifice called the throat. The dynamical principle upon which the motion depends is that of the equality of impulse and momentum. The aggregate momentum per second of the component streams is found by multiplying the mass of fluid which comes from each nozzle in a second by its velocity, and adding together the products. The momentum of the resultant stream is the product of the mass of fluid discharged from the throat in a second, into the velocity at the throat. The difference of these two momenta is equal to the impulse per second exerted in the junction-chamber, which impulse is found by multiplying the area of the throat by the difference between the intensities of the pressure at the nozzle end and at the throat end of the chamber respectively. If there be a gain of momentum, the pressure at the throat is less than at the nozzles; if there be a loss of momentum, the pressure at the throat is greater than at the nozzles.

There is always a loss of energy, which is expended in producing eddies; unless the velocities of the component and resultant streams are all equal to each other. The amount of that loss can be calculated in any given case, by the help of the principle already stated; and that principle being expressed in the form of an equation, and taken together with another equation expressing the equality of the mass discharged at the throat to the sum of the masses which come through the nozzles, affords the means of solving various problems as to combined streams.

Abstract of a Paper on the Thermo-dynamic Acceleration and Retardation of Streams.—Professor W. J. M. Rankine, F.R.S.* The object of this paper is to state in a more general and comprehensive form than has hitherto been done to my knowledge, a thermo-dynamic and hydro-dynamic principle of which many particular cases are well known and understood. That principle may be stated as follows:—

In a steady stream of any fluid, the abstraction of heat at and near places of minimum pressure, and the addition of heat at and near places of maximum pressure, tend to produce acceleration; the addition of heat at and near places of minimum pressure, and the abstraction of heat at and near places of maximum pressure, tend to produce retardation; in a circulating stream, the quantity of energy of flow gained or lost in each complete circuit is equal to the quantity of energy lost or gained in the form of heat; and in the absence of friction, the ratios borne by that quantity to the heat added and the heat abstracted (of which it is the difference) are regulated by the absolute temperatures at which heat is added and abstracted, agreeably to the second law of thermo-dynamics.

Amongst particular cases of the thermo-dynamic acceleration and retardation of streams, the following may be specified:—

Acceleration by the addition of heat at and near a place of maximum pressure; the draught of a furnace; and the production of disturbances in the atmosphere in regions where the ground is hotter than the air.

Retardation by the abstraction of heat at and near a place of maximum pressure; the dying away of atmospheric disturbances in regions where the ground is colder than the air.

Acceleration by the abstraction of heat at and near a place of minimum pressure; the injector for feeding boilers, in which a jet of stream, being liquefied by the abstraction of heat, is enabled not only to force its way back into the boiler, but to sweep a current of additional water along with it; also, to a certain extent, the ejector-condenser.

The conduction of heat from the parts of a stream where the pressure and temperature are highest, to the parts of the same stream where the pressure and temperature are lowest, produces, according to the foregoing principles, a gradual and permanent retardation of the stream, independently of the agency of friction; and this is accompanied by the production of heat to an amount equivalent to the lost energy of flow.

* Printed in full in the "Philosophical Magazine" for October, 1870.

SECTION B.—CHEMICAL SCIENCE

On the Weldon Process for the Manufacture of Chlorine.—Mr. W. Weldon, F.C.S., the author, said the process was one for the manufacture of chlorine by means of a perpetually-regenerated reagent consisting mainly of a compound containing the elements of peroxide of manganese and lime, and which was previously unknown. He had described the process last year at the Exeter meeting, when it was in operation at only two works. It is now either in operation or on the point of being adopted at almost all the works in this country, and at a number of works in France and Germany. In consideration of the fact that the production of chlorine will probably be completely revolutionised by the Weldon process, and considering, likewise, that chlorine is largely prepared in the neighbourhood of Liverpool and in other parts of Lancashire, the author had agreed to the request of Prof. Roscoe that he should submit to the section a brief account of the practical results, which the process had been found to yield under more extended experience, and of the development which it had undergone during the year. The author first described the apparatus employed, and exhibited a small model of it, and then proceeded to state that the chloride of manganese, which results in the ordinary preparation of chlorine, and which is generally acid, is neutralised by adding to the liquor finely divided carbonate of lime. The liquor then consists of a neutral mixed solution of chloride of manganese and chloride of calcium, and contains, in suspension, a large quantity of sulphate of lime and smaller quantities of oxide of iron and alumina. The clear solution, after settling, is oxidised by passing into it a blast of atmospheric air from a blowing engine, and heated, if necessary, by a current of steam. Milk of lime is then run into the oxidiser until the liquid ceases to give a manganese reaction with solution of bleaching powder. A further quantity of milk of lime is added, and ultimately from eighty to eighty-five per cent. of the manganese is converted into peroxide. The mixture is allowed to settle, the chloride of calcium solution forming the supernatant liquid is run off, and the residual black mud containing the manganese peroxide is used in the stills where hydrochloric acid is decomposed and chlorine gas produced. A residual liquor such as was commenced with, results, and the round of operations is begun again; and so on, time after time indefinitely. After giving an outline of his mode of treating still liquor, Mr. Weldon described at considerable length the details of the process, both as to quantities of materials employed and obtained, and the nature of the chemical compounds formed at different stages of the process. As explained by Prof. Roscoe, the principle upon which the process depends is that, although when alone, the lower oxides of manganese cannot be oxidised by air and steam under the ordinary pressure to the state of dioxide, yet this is possible when one molecule of lime is present to each molecule of oxide of manganese. The manganous oxide is precipitated from the still liquors with the above excess of lime, and by the action of steam and air on this, a black powder, consisting of manganese dioxide and lime, or calcium manganite (MnO_2 Ca O), is formed. This compound is again capable of generating chlorine from hydrochloric acid, and thus the chlorine process is made continuous with a working loss of only 2⅜ per cent. of manganese.

A short discussion followed the reading of the paper, in the course of which Mr. Gossage stated that his experiments on the improvement of the chlorine process had extended over thirty-five years, and he was glad Mr. Weldon's efforts had been attended with such a large measure of success.

Air Pollution from Chemical Works.—Mr. Alfred E. Fletcher, F.C.S., one of the inspectors under the Alkali Act. The author remarked that during the progress of many manufacturing processes gases or vapours were given off, which passing into the surrounding air, polluted it and rendered it more or less unfit for animal or vegetable life. If all noxious vapours were to be suppressed by the summary stoppage of the manufacturing processes causing them, we must dispense with the use of a variety of useful substances; indeed, it might be said that every manufacture was accompanied at its birth by some offensive smell or smoke. Still, the public were right in requiring that noxious vapours should be reduced to a minimum. Those who were not acquainted with manufactures would be surprised at the large amount of noxious vapours discharged from works. He then referred to the direction which he thought future legislation should take, his opinion being, he said, guided by the observations of Dr. Angus Smith, in his reports as chief inspector under the Alkali Act. In places where complaints were made against manufacturers by farmers, as to damage to their crops by corrosive smoke, let the district be called "a manufacturing district," upon the requisition of a certain number of inhabitants. To such district an inspector should then be appointed, who should have power at any time to ascertain the nature and amount of gases escaping from the various works. At the end of each month, or a longer period, the inspector should publish a list of all the works in his district, with a number indicating the average amount of acid vapour he had found upon his visits. There the inspector's duties should terminate, as he should be neither prosecutor nor judge, but merely publish the facts ascertained, which the farmer himself could never have gathered. He contended that such a plan would be beneficial in its operation. It would be universal in its application, and would embrace every description of manufacturing works. Hitherto legislation had been partial. There was an Alkali Act, but it regulated the alkali manufacture only.

In the discussion it was strongly urged that over-legislation on this question should be avoided, and that the crops do not suffer to the extent which was sometimes imagined. The Alkali Act had been so far beneficial as to call the attention of manufacturers to the subject of air pollution, and they had been induced to employ improved apparatus at their works.

On the Phenomena of the Crystallisation of a Double Salt.—Mr. J. Berger Spence. Mr. Spence said that hitherto many scientific chemists had doubted the possibility of producing soda alum; but the results of upwards of fifty experiments which he had made conclusively showed that this salt can be produced under certain circumstances. The principal point of interest in Mr. Spence's paper consisted in the discovery, made by him, that the crystals are produced from an amorphous mass, which is formed when the solution is prepared at high densities. The immediate result of this discovery may be that the large quantity of ammonia which is now used in the production of alum will be displaced in favour of soda, and that the valuable fertiliser, ammonia, which has no intrinsic value in alum, will be given to the soil, which, in an economic point of view, will be of considerable advantage to the country.

SECTION C.—GEOLOGY

Report on British Fossil Corals.—Prof. P. M. Duncan. The distinction between the palæozoic and later coral faunas was shown to be not so exact as was supposed, and that the aporose and perforate corals existed in the palæozoic rocks, as well as rugose and tabulate forms, which latter had closely allied recent analogues. The report contained a new classification of the Tabulata, and entered into the Alcyonarian characters of the *Chætetinæ* and the Hydrozoan characteristics of the *Milleporidæ*.

On the Fossil Elephants from Malta.—Dr. Leith Adams. After referring to his former reports, communicated to the Association in 1865 and 1866, in which the situation and nature of the Maltese ossiferous caves were described, Dr. Leith Adams now submitted further observations on the elephantine remains which has been collected by him in enormous quantities in those localities, and pointed out the important results that might be expected to flow from the comparison of these materials with those which had been brought by Captain Spratt from Zebbug, in which the late Dr. Falconer, in the year 1862, had discerned the existence of a dwarf or pigmy species, together with that of a larger form. Subsequently, Mr. Busk, on proceeding to work out Captain Spratt's collection in detail, found reason to discriminate three distinct forms, one of the average dimensions of the existing African and Asiatic species, and two others, differing from each other not only in size, but, as it would appear, in other osteological characters, but both of comparatively small or dwarf stature. The Zebbug collection, however, afforded but very scanty evidence with respect to the largest form, whilst that made by Dr. Leith Adams abounds with its remains, and will consequently allow of the correct determination of the true relations of that form with the two smaller forms found in association with it, as well as with other existing and extinct species, and especially with *E. antiquus*, whose relations up to the present time have remained obscure. Mr. Busk, by whom the paper was communicated, exhibited specimens selected from Dr. Adams' collection, proving the existence of the three forms above adverted to.

Report on the Exploration of Kent's Cavern.—Mr. W. Pengelly. During the past year the committee had investigated the only portions of the eastern division of the cave which had remained unexplored. Those portions had been called by the Rev. J. McEnery, the North and South Sally Ports in the belief that they led to external openings. The South Sally Port has a south-east direction into the hill, and away from the hill-side; it occupies a space of 80 feet by 40 feet. It was filled with, first, a red cave-earth from 12 to 20 inches thick; second, a stalagmitic floor from 1 to 24 inches thick; and third, a cave-earth of unknown depth, but exceeding 5 feet. The diggings yielded a large number of bones—including several birds and a few fish—portions of antlers, and about 1,400 fragmentary and perfect teeth, some of them still attached to the jaw-bones. The teeth belonged to the following animals:—Horse, hyena, rhinoceros, bear, sheep, badger, fox, rabbit, elephant, deer, lion, ox, hare, and pig. Agglutinated lumps of wings and elitra of beetles. Besides these, twenty-one flint implements and flakes were found. The North Sally Port covers an area of 86 feet by 84 feet, and passes out to an opening in the eastern slope of the hill. The three layers described in the South Sally Port are found in this opening also, and the remains obtained in the excavations were the same as and in like proportions to those found in the South Sally Port. Smerdon's Passage was determined to be the entrance to the North Sally Port, and also to a previously unsuspected passage. Numerous remains were found in this passage.

SECTION D.—BIOLOGICAL SCIENCE

Professor Rolleston's Inaugural Address. (Continued from p. 427.)

Pathology has made a return to Physiology for much service she has received, and this in the following directions. Dr. W. Ogle has thrown much light on the physiology of the cervical sympathetic nervous system by his record of a pathological history to be found in the recently issued volume (vol. lii.) of the "Medico-Chirurgical Transactions." The rough and cruel experimentation of war has had its vivisections utilised for the elucidation of the physiology of nerves, and especially of their trophic function, by the valuable volume issued by the American Sanitary Commission, under the editorship of Dr. Austin Flint. Dr. Broadbent has done something towards elucidating the question of the localisation of functions in particular parts of the cerebral convolutions which was so extensively and so very exhaustively discussed at Norwich by his paper in our most useful and comprehensive Cambridge "Journal of Anatomy and Physiology," May 1870, "On the Cerebral Convolutions of a Deaf and Dumb Woman."

I take this opportunity of mentioning two valuable papers on the very practical question of the influence of the vagus upon the heart's action. One of these is a German paper by a gentleman who is a zoologist and comparative anatomist as well as a physiologist, Dr. A. B. Meyer, "Das Hemmungsnervenssystem des Herzen" is the title of his memoir, a separate publication as I think; the other is an abstract of a paper [I have not seen the paper published *in extenso* as yet] by Dr. Rutherford, "On the Influence of the Vagus upon the Vascular System," published in the Cambridge journal just referred to. Especially do I think Dr. Rutherford's view as to the vagus acting centrifugally as regards the stomach, and carrying stimulus, not thither but thence, to the medulla oblongata, which stimulus is then radiated downwards by a route formed distally by the splanchnic nerve, so as to produce inhibitory vascular dilatation in the neighbourhood of the pepetic cells, as worthy of attention.* A considerable number of the papers which will be read before this Section, indeed a considerable part of the Section itself, will be devoted to the Natural History of Man. Nothing, I apprehend, is more distinctive of the present phase of that "proper study of mankind" than the now accomplished formation of a close alliance between the students of archæology strict and proper and the biologist with the express purpose of jointly occupying and cultivating that vast territory. Literature and art and the products of the arts furnish each their data to the ethnologist and anthropologist in addition to those which it is the business of the anatomist, the physiologist, the palæontologist, and the physical geographer, to be acquainted with; nor can any conclusion attained to by filling up any single one of those lines of investigation be considered as definitely absolved from the condition of the provisional until it has been shown that it can never be put into opposition with any conclusion legitimately arrived at along any other of the routes specified. In political alliances the shortcomings of one party necessarily hamper and check the advance of the other; a failure in the means or in the perseverance of one party may bring the joint enterprise to a premature close; mutual forbearance, not to dwell longer upon extreme cases, may finally be as effectual in slackening progress as even mutual jealousies. No such disadvantages attach themselves to the alliance of literature with science, as the German "Archiv für Anthropologie," issued to the world under the joint management of Ecker the biologist and Lindenschmidt the antiquarian, will show any one who consults its pages, replete with many-sided but not superficial, multifarious but never inaccurate, information.

The antiquary is a little prone, if he will allow me to say so, when left alone, to make himself but a connoisseur; the historian, whilst striving to avoid the Scylla of judicial dulness, slides into the Charybdis of political partizanship; and the biologist not rarely shows himslf a little cold to matters of moral and social interest, whilst absorbed in the enthusiasm of speciality. The combination of minds varying in bent is found efficacious in correcting these aberrations, and by this combination we obtain that white and dry light which is so comforting to the eye of the truth-loving student, to say nothing as to its being so much stronger than the coloured rays which the work of one isolated student has sometimes cast upon it from the work of another. It would be invidious to speculate, and I have forborne from suggesting whether the literary contingent in the conquering though composite army has learnt more from observation of the methods and evolutions of the scientific contingent, or the scientific man from the observation of the literary; it is, however, neither invidious nor superfluous to congratulate the general public upon the necessity which these, like other allies, have been reduced to, of adopting one common code of signals, and discarding the exclusive use of their several and distinctive technicalities. Subjects of an universal interest have thus come to be treated, and that by persons now amongst us, in a language universally understanded of the people. I have been careful to include the Palæontologist amongst the scientific specialists whose peculiar researches have cast a helpful and indeed an indispensable light upon the history of the fates and fortunes of our species. But it is not organic science only which anthropology impresses into its service, and it would be the sheerest ingratitude to forget the help which the Mineralogist gives us in assigning the source whence the jade celt has come or could come, or to omit an acknowledgment of the toil of the analytical chemist, who has given the percentage of the tin in the bronze celt, or in the so-called "leaden" and therefore Roman coffin.

I am very well aware that many persons who have honoured me by listening to the last few sentences, have been thinking that it is at least premature to attempt to harmonise the two classes of evidence in question; and that the best advice that can be given to the two sets of workers severally is, that they should work independently of each other. Craniography is said, and by irrefragable authority, to be a most deceptive guide; works and articles on ethnology tell us stories of skulls being labelled, even in museums of the first order of merit, with such Janus-like tickets as "Etruscan Tyrol *or* Inca Peruvian;" and one of the most celebrated anthropotomists of the day, has been so impressed with the fact that Peruvian as well as Javanese and Ethiopian skulls may be found on living shoulders within the precincts of a single German university town, that he has busied himself with forming a pseudo-typical ethnological series from the source and area just indicated. Great has been the scandal thence accruing to craniography, and the collector of skulls has thence come to be looked upon as a dilettante with singular ghoul-like propensities, which are pardonable only because they relate only to savage races of modern days, or to cemeteries several hundred years old, but which are not to be regarded as being seriously scientific. Now to me the existence of such a way of estimating such a work appears to argue a sad amount of ignorance of the laws of the logic of practical life, or, indeed, of the chapters on "approximate generalisations," which any man, however unpractical, can read in a treatise on logic. A man's features and physiognomy are instinctively and intuitively, or, if you prefer so to put it, as a result of the accumulated social experiences of generations of men, taken as a more or less valuable and trustworthy indication of his character; were this not so, photographers would not, as I apprehend, and hope they do; make fortunes, yet the face is at least as often fallacious as an index of the mind as the skull is fallacious as an index of race. The story of the misconception by a physiognomist of the character of Socrates is familiar to us, as I think, from Lemprière's Dictionary; and it may serve to parallel the story which Blumenbach and Tilesius tell us of the exact correspondence of the proportions of a skull from Nukahiva with those of the Apollo Belvidere. The living faces in a gaol again, to put the same argument upon other grounds, are as dangerous to judge from as are the skulls in the museum; yet every detective is something like a professor of physiognomy, and most of them could

* Since writing as above I have seen, but have not read, a paper by Dr. Coats in Ludwig's "Arbeiten aus der Physiologischen Anstalt zu Leipzig" for the present year. The Wurtzburg Physiological Laboratory Reports for 1867-1868, contain, as is well known, a series of papers on this subject.

write a good commentary on Lavater. The true state of the case may, perhaps, be represented thus:—A person who has had a large series of crania through his hands, of the authenticity of which, as to place and data, he has himself had evidence, might express himself, perhaps, somewhat to the following effect if he were asked whether he had gathered from his examination of such a series any confidence as to his power of referring to, or excluding from, any such series, any skull which he had not seen before. He might say, "the human, like other highly-organised types of life, admits of great variety, aberrant forms arise, even in our own species, under conditions of the greatest uniformity possible to humanity; amongst savages great variety exists (see Bates, 'Naturalist on the Amazons,' ii. p. 129), even though they all of them may live the same 'dull grey life,' and die the same 'apathetic end;' and consequently it may never, except in the case of Australian or Esquimaux, and perhaps a few other crania, be quite safe to pledge oneself as to the nationality of a single skull. Still there is such a thing as craniographical type, and if half-a-dozen sets, consisting of ten crania apiece, each assortment having been taken from the cemeteries of some well-marked nationality, were set before me, I would venture to say, after consultation and comparison, especially with such assistance as that which an assembly such as this might furnish me with, that it might be possible to show that unassisted cranioscopy, if not invariably right, even under such favourable circumstances, was nevertheless not wrong in a very large number of cases." If it is true on the one hand that *in generalibus latet error*, it is true on the other that security is given us by the examination of large numbers for the accuracy and reliability of our averages, a principle which, Gratiolet informs us, is thoroughly recognised in Chinese metaphysics, and which he has formulated in the following words:—"L'invariabilité dans le milieu s'applique à tout. La verité n'est point dans un seul fait mais dans tous les faits; elle est dans les moyennes, c'est-à-dire dans une suite d'obstructions formalées après le plus grand nombre d'observations possibles." (Mémoire sur les Plis cérébraux, p. 93). The natural history sciences do not usually admit of the strictness which says that an exception, so far from proving a rule, proves it to be a bad one; rather are we wise in saying that in them at least the universality of assertion is in an inverse ratio to that of knowledge, and that the sweeping statements dear, as Aristotle long ago remarked (Rhetoric, ii. 21. 9 & 10.; ii. 22. 1.), to a class which he contrasts with the educated, are abhorrent to the mind of organic nature. It is true enough, as is sometimes said, that when opinions and assertions are always hooped in by qualifications, the style becomes embarrassed, and the meaning occasionally hard to be understood; but this difficulty is one which lies in the very nature of the case, and the real excellence of style does not consist in its lulling the attention and relieving the memory by throwing an alliterative ring on to the ear, but in the furnishing a closely fitting dress to thought, and an accurate representation of actual fact.

If we are told that the attempt to harmonise the results, not merely of cranioscopy, but of any and all natural science investigation, with the results of literary and linguistic research, is needless and even futile, this is simply equivalent to saying that one or other of these methods is worthless. For as Truth is one, if two routes, purporting both alike to lead to it, do not sooner or later converge and harmonise, this can only be because one or other of them fails to impinge upon the goal. It is true that by certain lines of investigation light is thrown upon a problem, but at a single point, and that all further prosecution of investigation along that line will but lead us off at a tangent. Still the throwing of even a single ray upon a dark surface is an achievement with a value of its own; and it is a cardinal rule in our sciences never to ignore the existence of seemingly contradictory data, in whatsoever quarter they may show themselves. For what would be said of an investigator of a subject such as physical geography, who should declare that he would pay no attention but to a single set of data, when he was discussing whether a particular archipelago had been formed by upheaval, or should be held to be the fragments and remnants of a disrupted continent; and that if geological evidence was in crying discord with his interpretation of the facts of the distribution of species, it was not his business to reconcile them. He would be held to have neglected his business, as you may see by a reference to Mr. Bentham's Address to the Linnean Society, May, 24, 1869. (Linn. Soc. Proc. for 1869, p. xcii.*)

The argument from identity of customs and practices to identity of race is liable to much the same objections and to much the same fallacies as is the argument from identity of cranial conformation. The case may be found admirably stated in Mr. Tylor's work on the "Early History of Mankind," p. 276, ed. 2, and I may say that the means of bringing the problem home to oneself may be found by a visit to any collection of flint implements. In such a collection as Mr. Tylor has pointed out, p. 205, we are very soon impressed with the marked uniformity which characterises these implements, whether modern or thousands of years old, whether found on this side of the world or the other. For example, a flint arrowhead which came into my hands a short time back, through the kindness of Lord Antrim, after having done duty in these iron times as a charm at the bottom of a water-tub for cattle in Ireland, was pointed out or at to me by a very distinguished Canadian naturalist, who was visiting Oxford the other day, as being closely similar to the weapons manufactured by the Canadian Indians. Now after such an experience one may do well to ask in Mr. Tylor's words ("Early History," p. 206):—

"How, then, is this remarkable uniformity to be explained? The principle that man does the same thing under the same circumstances will account for much, but it is very doubtful whether it can be stretched far enough to account for even the greater proportion of the facts in question. The other side of the argument is, of course, that resemblance is due to connection, and the truth is made up of the two, though in what proportions we do not know. It may be that, though the problem is too obscure to be worked out alone, the uniformity of development in different regions of the Stone age, may some day be successfully brought in with other lines of argument, based on deep-lying agreements in culture which tend to centralise the early history of races of very unlike appearances, and living in widely distant ages and countries."

If the psychological identity of our species may explain the identity of certain customs, its physiological identity may explain certain others. Some of this latter class are of a curious kind, and relate not to matters of social or family, but to matters of purely personal and individual interest, concerning as they do the sensibility, and with it, all the other functions of the living body. Such customs are the wearing of labrets, or lip-rings, nose-rings, and if I may add it without offence, of certain other rings, inserted in the wide region supplied by the fifth or tri-facial nerve. A physiological explanation may lie at the base of these practices, which appear to put at the disposal of the persons who adopt them a perennial means for setting up an irritation, whence reflex consequences in the course of reflex nutrition and reflex secretion, as of gastric juice, may flow. A curious book was written, or at least published, on the subject of these practices, and others akin to them, in 1653, by Dr. John Bulwer, a benevolent doctor, who paid attention to the care of the deaf and dumb previously, I think it is stated, to Dr. Wallis, and who consequently, with proper pride, if this precedence really belongs to him, signs himself "J. B. cognomento Chirosophus." The title of the book is "Anthropometamorphosis; Man Transformed, or the Artificial Changeling." I was made acquainted with its existence by my friend, Mr. Tomlinson, of Worcester College, from the library of which Society I procured a copy for consultation; the book is not rare I think; but I think it is little known, it contains much that is curious, and it is, inasmuch as it was written more than 200 years ago, ὅτ' ακήρατος ἦν ἔτι λείμων, from some, though not from all points of view, the more valuable. It is, I apprehend, to some of these customs, as well as to others, that Zimmerman—not the author of the work on Solitude, but Zimmerman the zoologist—alludes in a rather amusing passage, which may be found in the third volume of his larger work on the Distribution of Species, and on Zoology (see p. 257). I speak of the passage as amusing, it is more than that, or I would not quote it; indeed you will not see that it is particularly amusing unless I tell you that volumes ii. and iii. are of date 1783, and are dedicated to his own father, whilst volume i., of date 1778, is dedicated to "His Most Serene Highness and Lord, Ferdinand Duke of Brunswick, my Most Gracious Lord." Its quality of amusingness depends upon these dates, and the speculations they set us to make as to how the Duke had offended the man of science in the interval between 1778 and 1783. It may be a warning to Serene Dukes how they

* The following references to passages of the kind referred to above as to the unreliability of craniographical evidence may be useful:—Geographisches Jahrbuch, 1866, p. 481. Hyrtl, Topograph. Anatomie, i. p. 13. Henle, System. Anat. i. 198. Krause, Archiv für Anthropologie, ii. 1. Holder, *ibid.* See also His and Rütimeyer, and Eckers in their systematic works severally, the Crania Helvetica, and the Crania Germaniæ meridionalis.

treat men of science. It runs thus :—"If you argue from similarity of customs and ceremonies to identity of origin of two tribes under comparison, you must first show that these customs are not such as would naturally tend to the amelioration of the conditions of the inhabitants in the two countries under consideration, and would probably therefore, or can naturally suggest themselves to each of the races in question. Or there may be customs founded on innate folly and stupidity, and thus, for your argument to be valid, you must show that of two peoples widely separated, each cannot by any chance come into its own country to adopt the like foolish and stupid customs. For whilst two wise heads are to make out, each independently of the other and contemporaneously, a wise discovery or invention, it is much more likely in the calculation of chances, and considering the much greater number of fools and blockheads ('Thoren und Dummköpfen') that in two countries widely apart, closely similar follies should be simultaneously invented. And then, if the inventing fool happens to be a man of influence and consideration, *which is, by the way, an exceedingly frequent coincidence*, both the nations are likely to adopt the same foolish practice, and the historian and antiquarian after the lapse of some centuries, is likely to draw from this coincidence the conclusion that the two nations both sprang from the same stock." Judge and speculate for yourselves how the spirit which breathes in this passage was excited, but note its scientific value too. We must not forget that it is possible, in thought, at least, to dissociate the psychological unity of man from his specific identity even; and that as regards identity of race, it is only reasonable to expect that when similar needs are pressing, similar means for meeting them are not unlikely to be devised independently by members of two tribes who have for ages been separated from their original stocks. The question to be asked is, does the contrivance about which we are speculating, combine or does it not combine in itself so large a number of converging adaptations, as to render it upon the calculation of chances, unlikely that it should have been independently invented? Yet this very obvious principle has been neglected, or Lindenschmidt would not have found it necessary to say, that by laying too much stress upon certain points of national identity in the stones used for the formation of cromlechs or dolmens, the Hünenvolk might be made out to have chosen to settle only in those parts of Germany where erratic blocks of granite or other such large stones could be found! (Archiv für Anthropologie, iii. p. 115, 1868.)

Sir John Lubbock's recently published work on "The Origin of Civilisation," may, I anticipate, cause the history and genealogy of manners and customs to enter largely into the composition of our lists of papers. There is no need for me, as the author of the book is here himself to speak, as announced, for himself, to occupy your time in recommending his work; but I may be allowed to say that the utility of such pursuits as those which Sir John Lubbock's book treats of, receives some little illustration from the fact that, as we learn from him and from Mr. Tylor, the human mind blunders and errs and deceives itself in these subjects in just the same way as it does in the kindred, though more immediately arising, pressing and important matters of social and political life. In these latter spheres of observation we are apt occasionally to mistake one of those intermittent reactions of opinion, produced as eddies are produced in a river, by the deposit of sand and mud at angles in its onward course, for a deliberate giving up of the principles upon which all previous progress has been dependent. The straws which float upon the surface of a backwater may be taken as proofs that the river is about to flow upwards, and a feeble oarsman in a light boat may be deceived for some moments by the backward drifting of his small craft. Now an analogous blunder is often made in matters of purely historical interest; and we may do well to learn from the experience thus cheaply earned. "The history of the human race has," says Sir J. Lubbock, p. 322, *l. c.*, "I feel satisfied, on the whole been one of progress; I do not of course mean to say that every race is necessarily advancing; on the contrary, most of the lower are almost stationary," but Sir John regards these as exceptional instances, and points out that if the past history of man had been one of deterioration, we have but a groundless expectation of future improvement, whilst on the other, if the past has been one of progress, we may fairly hope that the future will be so also.

Mr. Tylor's words are equally to the purpose, though as forming the end of a chapter merely, and that at the end of the book, they are less enthusiastic in tone. (P. 193, Tylor, "Early History of Mankind.") They run thus—

"To judge from experience, it would seem that the world, when it has once got a firmer grasp of new knowledge or a new art, is very loath to lose it altogether, especially when it relates to matters important to man in general, for the conduct of his daily life, and the satisfaction of his daily wants, things that come home to men's 'business and bosoms.' An inspection of the geographical distribution of art and knowledge among mankind seems to give some grounds for the belief that the history of the lower races, as of the higher, is not the history of a course of degeneration or even of equal oscillations to and fro, but of a movement, which, in spite of frequent stops and relapses, has on the whole been forward; that there has been from age to age a growth in man's power over nature, which no degrading influences have been able permanently to check."

I must not trespass into the province of the Botanist, but I should be glad to say that no easier method of learning how the natural history sciences can be made to bear upon the history of man, as a whole, can be devised than that furnished by the perusal of such memoirs as those of Unger's upon the plants used for food by man. The very heading and title of the paper I am specially referring to appears to me to have an ambiguity about it which, in itself, is not a little instructive. In that title, "Botanische Streifzüge auf dem Gebiete der Cultur-Geschichte," the latter word may be taken, I imagine, etymologically at least, to refer either to culture proper or to agriculture. At any rate, the paper itself may be read in the Sitzungsberichte of the Vienna Academy for 1857; it has, I suppose, superseded the interesting chapters in Link's "Urwelt und Alterthum," of date 1821; and it is not unlikely, I apprehend, to be itself, in its turn, superseded also.

Coming, in the third place, to Zoology, I suppose I shall be justified in saying that the largest issue which has been raised in the current year, an issue for the examination of the data for deciding which the two months of July and August which are just past, may have furnished persons now present with opportunities, is the question of the kinship of the Ascidians to the Vertebrata. There is or was nothing better established till the appearance of Kowalewsky's paper, now about four years ago, than the existence of a wide gulph between the two great divisions of the animal kingdom, the Vertebrata and the Invertebrata; nothing could be more revolutionary than the views which would obviously rise out of his facts, and within the present year these facts have been abundantly confirmed by Prof. Kupfer, whose very clearly written and beautifully illustrated paper has just appeared in the current number of Schultze's "Archiv für Microscopische Anatomie." Kupfer's researches have been carried on upon *Ascidia canina*, but they more than confirm the accuracy of what Kowalewsky had stated to take place in *Ascidia mammillata*, and which may be summed up briefly thus: In the larval Ascidian we have in its caudal appendages an axis skeleton clearly analogous, if not essentiallly homologous, to the chorda dorsalis of the vertebrate embryo, as consisting like it of rows of internally-placed cells, and giving insertion by its sheath to muscles. We have further the nervous system and the digestive taking up in such embryos much the same positions relatively to each other, and to this molluscan chorda dorsalis, that are taken up by the confessedly homologous system in the Vertebrata; we have the nervous system originating in the same fashion and closing up like the vertebrate myelen cephalon out of the early form of a lamellar furrow into that of a closed tube; we have finally the respiratory and digestive inlets holding the vertebrate relationship of continuity with, instead of the invertebrate of dislocation and separation from each other. Such are the facts; but I am not convinced that they will bear the interpretation that has been put upon them; though I must say the possession of this chorda dorsalis by the active locomotor larva of the Ascidian which one day settles down into such immobility, lends not a little probability to Mr. Herbert Spencer's view of the genesis of the segmented vertebral column in animals undoubtedly vertebrate. But on this view I should not be inconsistent with myself, inasmuch as, to waive other considerations, the chorda dorsalis in each case would be considered as an adaptive or teleological modification, not a sign of morphological kinship. Much perplexity may or must arise here, and whilst entertaining these views, I felt myself bound to examine myself strictly to find whether in not taking them up, I might not be giving way to that reactionary reluctance to accept new ideas which advancing years so frequently bring with them; but a recent paper, by Lacaze Duthiers, published in the *Comptes Rendus* for May 30, 1870, and translated in the Annals and Magazine of Natural History for July, 1870, would justify me, I think, in calling that reluctance by another name. For in that paper the renowned malacologist just mentioned has brought to light the fact that there is another sessile and solitary Ascidian, the *Molgula tubulosa*, which goes through no such tadpole-like stage, as had been supposed to be gone through by all Ascidians except the Salpæ, which is never active and never puts out the activity which is so remarkable in the other Ascidians, but settles down and remains sedentary immediately after it is set free from the egg capsula, neither enjoying a Wanderjahr *nor possessing a chorda dorsalis*. We are not surprised after this that M. Lacaze Duthiers observes that "although embryology may and must furnish valuable information by itself, it may also, in some cases, lead us into the gravest errors." Mr. Hancock, of Newcastle-upon-Tyne, has sent us a paper upon this subject, which will be read duly, and duly noted by us.

Leaving Malacology, which has not in the United Kingdom obtained the same hold as yet upon the public mind that it has on the Continent, where, like Entomology, there and here, it has a periodical or two devoted to the recording of the discoveries of its votaries, I have much pleasure in directing attention to two short papers by Siebold in the *Zeitschrift für wissenschaftliche Zoologie* (xx. 2, 1870), on Parthenogenesis in *Polistes gallica v. Diadema*, and on Pædogenesis in the *Strepsiptera*. In each of these short papers Siebold informs us that adequate room and time could not be given them in the Innsbruck meeting held just a year ago, or in the report of the meeting. It is to me a matter of difficulty to think what there could have been of greater value than those papers in a section of Wissenschaftliche Zoologie; it will be to all present a matter of congratulation to learn from the venerable professor's papers that he will shortly favour us with a new work on the subject of Parthenogenesis. A fresh instance of Parthenogenesis in Dipten, in *Chironomus*, has just been put upon record in the St. Petersburg Imperial Academy's memoirs (xv. 8, Jan. 13, 1870).

The subject of the Geographical Distribution of the various forms of vegetable and animal life over the surface of the globe, and in the various media, air, earth, water, fresh and salt, whether deep or shallow, has always been, and will always remain, one of the most interesting subsections of biology. It was the contemplation of a simple case of geographical distribution in the Galapagos Archipelago which brought the author of the "Origin of Species" face to face with the problem which the title of his work embodies; and it is impossible that sets of analogous and of more complicated facts,—many of which, be it recollected, such as the combination now being effected between our own Fauna and Flora and those of Australia and New Zealand, are patent to the observations of the least observing,—should not, since the appearance of that book, force the serious consideration of the explanation it offers upon the thoughts of all who think at all. The wonders of the Deep Sea Fauna will, I apprehend, form one, the Commensalism of Professor Van Beneden another, subject of discussion, and furnish an opportunity for receiving instruction to all of us. The one set of observations is a striking exemplification of the way in which organisms have become suited to inorganic environments, the other is an all but equally striking exemplification of the way in which organisms can fit and adapt themselves to each other. The current journals have,* as was their duty, made us acquainted with what has been done in both of these directions, and I am happy to say that in the case of the Deep Sea Explorations as in that of Parthenogenesis and Spontaneous Generation, a new work, giving a connected and general view of the entire subject, is announced for publication.

One instance of the large proportions of the questions which the facts of geographical distribution bear upon, is furnished to us in the address recently delivered before the Geological Society by its president, who is also our president, aud who may have forgotten to refer to his own work (see NATURE, No. 24, 1870). Another may be found in the demonstration which Dr. Günther, contrary to our ordinarily taught doctrines, has given us (Zool. Soc. Trans. Vol. vi., pt. 7, 1868, p. 307) of the partial identity of the fish-faunas of the Atlantic and Pacific coasts of Central America; a third is furnished to us by Mr. Wallace's works *passim*.

It would be superfluous, after introducing even thus hurriedly to your notice so large a series of interesting and important subjects as being subjects with which we shall forthwith begin to deal in this Section, to say anything at length as to the advantages to be expected reasonably from the study of Biology. I may put its claims before you in a rough way by saying that I should be rejoiced indeed if when money comes to be granted by the Association for the following up the various lines of biological research upon which certain of its members are engaged, we could hope to obtain a one-hundredth, or I might say a thousandth part of the amount of money which has in the past year been lost to the State and to individuals through ignorance or disregard of biological laws now well established. I need say nothing of the suffering or death which anti-sanitary conditions entail, as surely as, though less palpably and rapidly than, a fire or a battle; and I might, if there were time for it, take my stand simply upon what is measurable by money. This I will not do, as it is less pleasant to speak of what has been lost than of that which has been or may be gained. And of this latter let me speak in a few words, and under two heads—the intellectual and the moral gains accruing from a study of the Natural History Sciences. As to the intellectual gains, the real psychologist and the true logician know very well that the discourse on method which comes from a man who is an actual investigator is worthy, even though it be but short and packed away in an Introduction or an Appendix, or though it cover but a couple of pages, like the "Regulæ Philosophandi" of Newton, more than whole columns of the "Sophistical Dialectic" of the ancient Schoolman and his modern followers. "If you wish your son to become a logician," said Johnson, "let him study Chillingworth"—meaning thereby that real vital knowledge of the arts and sciences can arise only out of the practice of reasoning; and as to the value of actual experimentation as a qualification for writing about method, Claude Bernard and Berthelot are, and I trust will long remain, living examples of what Descartes and Pascal, their fellow-countrymen, are illustrious departed examples. (See Janet, Revue de deux Mondes, tome lxii. p. 910, 1866.)

I pass on now to say a word on the working of natural science studies upon the faculty of attention, the faculty which has very often and very truly been spoken of as forming the connecting link between the intellectual and the moral elements of our immaterial nature. I am able to illustrate their beneficial working in producing carefulness and in enforcing perseverance, by a strong turning upon the use of, or rather upon the need for, a word. Von Baer, now the Nestor of biologists, after a long argumentation (Mem. Acad. Imp. Sci., St. Petersburg, 1859, p. 340) of the value which characterises his argumentations as to the affinities of certain Oceanic races, proceeds to consider how it is that certain of his predecessors in that sphere, or rather, in that hemisphere, as Mr. Wallace has taught us Oceania is very nearly, had so lamentably failed in attaining or coming anywhere near to the truth. This failure is ascribed to something which he calls "Ungenirtheit," a word which you will not find in a German dictionary, the thing itself not being, Von Baer says, German either. I am happy not to be able to find an exact equivalent for this word in any single English vocable, the opposite quality shows itself in facing conscientiously "the drudgery of details, without which drudgery," Dr. Temple tells us (Nine Schools Commission Report, vol. ii., p. 311), "nothing worth doing was ever yet done." Mr. Mill, I would add, speaks to the same effect, and even more appositely, as far as our purpose and our vocations are concerned, in his wise Inaugural Address at St. Andrews, p. 50. For the utter incompatibility of an ἀταλαίπωρος ζήτησις,—those two words give a Thucydidean rendering of "*Ungenirtheit*,"—with the successful investigation of natural problems, I would refer any man of thought, even though he be not a biologist, to a consideration of the way in which problems, as simple at first sight as the question of the feeding or non-feeding of the salmon in fresh water (see Dr. McIntosh, Linn. Soc. Proc., vii., p. 148), or that of the agencies whereby certain molluscs and annelids bore their way into wood, clay, or rocks must be investigated. It is easy to gather from such a consideration how severe are the requirements made by natural science investigations upon the liveliness and continuousness of our faculty of attention.

I shall speak of but one of the many purely moral benefits which may be reasonably regarded either as the fruit of a devotion to or as a preliminary to success in natural science. Of this I will speak in the words of Helmholtz, taking those words from a report of them as spoken at the meeting of the German Association for the Advancement of Science, which was held last year at Innsbruck. There Professor Helmholtz, in speaking of the distinctive characteristics of German scientific men, and of their truthfulness in particular, is reported to have used the following words:—

* See NATURE, No. 39, July 28, 1870, and "Royal Society's Proceedings," Aug. 1870, for Deep Sea Explorations, and *Academy*, Sept. 10, 1870, for Commensalism.

"Es hat diesen Vorzug auch wesentlich zu verdanken der *Sittenstrenge* und *der uneigennutzige Begeisterung* welche die Männer der Wissenschaft beherrscht und beseelt hat, und welche sie nicht gekehrt hat an aussere Vortheile und gesellschaftliche Meinungen." These words are, I think, to the effect that the characteristics in question are in reality to be ascribed to the *severe simplicity of manners* and *to the absence of a spirit of self-seeking*, which form the guiding and inspiring principles of their men of science, and prevent them from giving themselves up to the pursuit of mere worldly advantages, and from paying undue homage to the prejudices of society. I think *Sittenstrenge* may be considered as more or less adequately rendered by the words *severe simplicity of manners*; at any rate, as things are known by their opposites, let me say that it is the exact contradictory of that "*profound idleness and luxuriousness*" which, we are told by an excellent authority (the Rev. Mark Pattison, "Suggestions on Academic Organisation," p. 241),—for whose accuracy I would vouch in this matter were there any need so to do,—"*have corrupted the nature*" of a large class of young men amongst ourselves, whilst *the absence of a spirit of self-seeking* is, in its turn, the contradictory of a certain character which Mr. Mill (*l.c.*, p. 90), has said to be one of the commonest amongst us adults, and to which Mr. Matthew Arnold has assigned the very convenient epithet of "Philistine." Investigation as to whether these undesirable tendencies are really becoming more rife amongst us, might be carried on with advantage in a place such as this, in the way of inquiries addressed to colonists returning home after a successful sojourn abroad. Such persons are able to note differences without prejudice, and, *ex hypothesi*, with unjaundiced eyes, which we are apt to overlook, as they may have grown up gradually and slowly. But, perhaps, researches of this kind are not quite precisely the particular kind of investigation with which we should busy ourselves; neither would the leaders of fashion, the persons with whom all the responsibility for this illimitable mischief rests, be very likely to listen to any statistics of ours, their ears being filled with very different sounds from any that, as I hope, will ever come from Section D. Whether men of science in England are more or less amenable to blame in this matter than the rest of their countrymen, it does not become us to say; but it does become and concern us to recollect that we have particular and special reasons, and those not far to seek, nor dependent on authority alone, for believing and acting upon the belief that real success in our course of life is incompatible with a spirit of self-seeking and with habits of even refined self-indulgence.

Department of Ethnology and Anthropology.—John Evans, F.R.S., in the chair.

Before commencing the business of the department, Mr. Evans offered a few remarks as to the subjects that properly came within the province of the department—the present condition of knowledge of those subjects, and the methods at command for increasing that knowledge. The subjects which may with propriety be brought under the consideration of the Department may approximately be defined as:—(1) all that relates to the antiquity of man, or the origin of the various races of mankind; (2) all that illustrates the progress and development of human civilisation; and (3) all that concerns the condition of the less civilised portions of the human race, even if not immediately connected with any general question of its origin or progress. The President then proceeded to show what an enormous field these subjects embrace, how much there is still to learn in the means of investigating them, notwithstanding the efforts of the numerous labourers, who have now for many years been employed in this field of research, and concluded a very interesting address by expressing his confidence that nothing would be said calculated to injure the feelings of any who, like ourselves, are in pursuit of truth, and that all will bear in mind how difficult it is to take in the whole of any single truth at one view, and how, of its many sides, two contending parties may each be seeing one only, and that possibly not the most important.

On the Principal Geological Changes which have occurred in Europe since the Appearance of Man.—Professor P. Martin Duncan, F.R.S., F.G.S., &c.

This short communication does not pretend to offer any new facts or opinions; but I trust that it will be found to be a truthful representation of the results of the more important labours of men who have laboured so sedulously and conscientiously on the subject of the antiquity of man. As such, I hope the paper will be of interest to the associates of the Section, although I cannot anticipate that it will yield any satisfaction to my more learned colleagues.

Most of the geological changes which have occurred since man first appeared in Europe are estimated and asserted in consequence of the results of direct observation upon the succession of phenomena which belong to the debatable ground between what is usually termed physical geography and geology. Some, however, are dependent upon the arguments which are brought forward by naturalists respecting the limitation and separation of parts of great natural history provinces.

For instance, if the remains of man or his early works are found in sediments high up on the sides of valleys, the sediments having been produced by the river when it flowed far above its present level, the vertical distance between the remains and the existing water-level affords evidence of geological or physico-geographical changes. And if a vast number of bones belonging to extinct and existing species of large animals, such as elephants and rhinocerides, hyænæ and lions, are found upon islands the area of which could not possibly have sustained and nourished the mammalia during life, the separation of the areas from the nearest continent is inferred, especially if the species are still existing there, or if their remains abound there.

It is necessary to premise that no trace of man has been found associated with any deposits which are formed during the Glacial Period in Northern Europe. The very nature of them would prevent such a discovery.

The loftiest mountains of Europe, such as the Alps, Pyrenees, Ardennes, and Vosges, underwent a great grinding down during the Glacial Period; and when it was over the results of this wear and tear were scattered far and wide all around them in the form of wash-down, gravel, and more or less angular stones. In a general view, this gravel, the result of the first glacialisation, is of the same geological age as the drift gravel of England north of the Thames, and of Northern Germany, which is called glacial drift, and which, as I have just observed, is older than the earliest traces of man in Europe. The earliest remains of man and his works, and of the beasts associated with him and hunted by him, rest upon these deposits, and are therefore later in time.

It would appear that man followed up the retreating ice of the north of Europe, for the remains of his works are found high up in many British valleys, which must then have begun to be formed by the natural drainage out of the deposits of the Glacial Period.

Now, after a time, there was another period of mountain glacialisation; and the glaciers of the Alps and Pyrenees, especially, extended far into the districts below them.

The grinding down of the mountain sides during this second glacialisation produced enormous quantities of mud and gravel; and when the glaciers retreated this detritus was washed far and wide over the plains. This deposit is constantly found to cover the remains of man and his works, and is therefore later in time.

The second glacialisation, and the dispersal of the wash-down, appears to form a rude line of separation between the Palæolithic Period, when man used rude stone weapons, and the Neolithic Period, when smooth or polished implements were made by him; and they also, in a general sense, mark the time when the great mammalia, the early prey of man, disappeared from the northern and western parts of Europe.

The following are the principal geological changes which occurred after the appearance of man in Europe:—

1. The subsidence of an area of land which connected Sicily with Crete and Northern Africa north of the Sahara.

2. The formation of a volcanic tufa on the hills bordering the present valleys of the Tiber and its tributaries; the excavation of those valleys by the river and its streams; the last eruption of the volcanoes of Latium, and their permanent extinction. The space included in the Roman territory has received its contour, and vast tracts near the coast have been worn away.

3. The formation of valleys in the Alpine detritus, which covered up large tracts of Northern Italy, and the re-excavation of old valleys, which had been more or less filled with the detritus. This great gravel was the wash-down of the wear and tear of the first extension of the south Alpine glaciers, and was, in a general sense, contemporaneous with the upper glacial drift of Northern Europe. It was deposited before man, as traced by his relics and works, lived in South Europe.

The dispersal of vast depths of silt and gravel over the plains into the valleys and far up the hill sides of the Sardinian States and Lombardo-Venetia, south, and to a certain extent south-east, of the Alps; the result of the wash-down of the wear and tear of the second extension of the Alpine glaciers; the formation of valleys in this gravel or silt, and the production of such heights as those which bound such plateaux as Rivoli.

4. Considerable local alterations in the relative level of land and sea along the west Neapolitan coast.

5. The formation of the straits of Gibraltar.

6. The excavation of such valleys as that of the Manganores in Central Spain, the formation of gravels containing flint implements and mammalian bones near Madrid, and therefore far beyond the influence of marine action.

7. The wearing down of many of the valleys to the north of the Pyrenees below the level of such lower mammaliferous caves as those in the neighbourhood of Tarascon; the dispersion of the results of the wear and tear of the second extension of the Pyrenean glaciers, and the filling up of the old valleys with it; the re-excavation of the valleys, and the carrying down of their silt or loess to the plains; the formation of streams and water-courses through this deposit.

8. The formation of certain valleys in the Perigord by streams to a certain extent, but principally by the gradual effects of rain, heat, frost, and other meteorological actions.

9. The excavation of the valleys of North and Eastern France, and the denudation and retrogression of their watersheds.

10. The dispersion of Alpine rocks, gravels, and rocks to the north of the Alps, produced by the wear and tear of the great glacialisation, which was, in a general sense, contemporaneous with the first extension of the glaciers south of the Alps, and the first extension of the Pyrenean glaciers, occurred before man appeared in Europe. After his first wanderings and huntings he left his remains above this old Alpine detritus. Then the valleys in the carboniferous limestone of Belgium were worn down ninety yards or more by rain and rivers, and the bones of the extinct mammalia and rude stone implements and bones of man were washed into caves with the gravel.

After the retirement of the glaciers, subsequent to this second extension, the wash-down of the Alps, Vosges, and Ardennes was spread over the older gravel. It filled up the valleys, and extended with a thickness varying from a few yards to a thousand feet and more, all down and over what is now the valley of the Rhine, Holland, and Belgium. The loess thus formed was then washed out of the valleys, was cut into by rivers, and has been worn down ever since.

11. The separation of the coasts of France and England about Dover and Calais.

12. The excavation of nearly all the valleys in the district east of a line drawn from King's Lynn to Portland, the denudation of their watersheds, and retrogression of the river sources.

13. The denudation of the valley of the Weald of Kent.

14. The separation of the Isle of Wight from the main land.

15. The formation of a great part of the Bristol Channel.

16. The upheaval of many sea beaches, and the general destruction of forest land on the south and west of England; the formation of many peats.

17. Enormous destruction of the sea coast.

18. A sufficient upheaval of the Scandinavian peninsula and Denmark, to produce such a restriction of the outlet as has determined a change in the marine fauna of the Baltic.

19. A slow upheaval of large areas appears to have accompanied the excavation of the valleys upon them, and a subsidence of equally large districts appears to have accompanied the recession of the second glaciers; probably another upheaval followed.

20. The uprise of the Desert of Sahara in Africa after the second extension of the Alpine glaciers.

SECTION E.—GEOGRAPHICAL SCIENCE

On Atmospheric Currents.—Mr. J. K. Laughton. In examining into the geographical distribution of winds, we must bear in mind that well-attested and careful observation is the only satisfactory basis, and that descriptions founded on theoretical opinions are of no value whatever. If we refer Hadley's Theory of the Trade Winds to this test, we find, *in the first place*, that the effect of heat in producing wind is not quite such as has been represented. Experimentally, heat does not produce a blast, unless the space between the heat and cold air be very confined, as is roughly shown by holding a newspaper before the fire. Geographically, heat does not cause a wind towards any of the principal areas of greatest temperature; either towards the Great African Desert, the Desert of Arabia, or of Australia, towards the Red Sea or the Persian Gulf, or even, when carefully traced, towards the Great Prairie of North America. *In the second place*, we find that the effect attributed to the rotation of the earth is not consistent with numerous observed facts, such as the S.E. wind in the Gulf of Mexico, the N.W. wind on the coast of North Africa, the N.W. gales in the North Atlantic, the S.W. wind on the south coast of Australia, and very many others; and, indeed, the idea appears to have originated in a temporary forgetfulness of the power of friction, which in the case of air and all fluids is very intense.

Winds, which, in accordance with Hadley's theory, have been very generally divided into *polar* and *equatorial*, seem more naturally to divide themselves into *easterly* and *westerly*; and it is this division which has, from the days of Columbus, been adopted by really practical men, to whom the winds were matters of fact, not of mere theory; but the trade-winds—having attracted early notice by their very great steadiness and regularity—have always been considered as the direct manifestations of the first cause, whatever it might be, of the great atmospheric movements; the westerly winds, which were not discovered till much later, having been considered as secondary and comparatively of little importance. But, as our experience grows larger, we learn that the westerly winds have an extent and a strength and a rude vigour incompatible with the idea of their secondary nature. Whether in the northern or southern hemisphere, they are exceedingly violent and boisterous; and, without considering the Arctic and Antarctic regions, concerning which we have not sufficient evidence, they extend from 60° N. to 60° S., interrupted only by the trade-winds, which blow over an area large indeed, but small *in comparison* with that on which they intrude. The trade-winds are also of very limited height, whilst above them the westerly winds blow as strongly as they do in the temperate zones, where they reach into the upper strata of the atmosphere as far as we have any knowledge. We are thus led towards the conclusion that the westerly winds are really the primary winds, far extending and boisterous; whilst the quiet equable trade-winds—of very limited volume—are reflex streams of air caused by the impact of the great westerly winds on the continental barriers, whether against mountain ranges or the more sluggish air which lies over the land. The Atlantic Ocean affords us the most familiar illustrations of this; where we see the main westerly stream dividing on about the parallel of 45° N., and turning north, as a south-westerly wind on our coasts, and the coast of Norway; or south, as a north-west and northerly wind on the coast of Portugal, and a north-easterly on the coast of Africa; whilst the rest of it forces its way onward, a westerly wind over Northern Europe and Asia, or a northerly deflection in the several basins of the Mediterranean. On the other hand, on the extreme west, the westerly wind continually dragging away the air from the eastern side of the Rocky Mountains, causes such a tendency towards a vacuum, that the air from the south and north is induced towards it, and the wind over Western America rules from the south or north, according as the season throws the axial line of the temperate zone to the north or south of its mean position. Examining at great length into the various local winds and irregularities in the different parts of the world, we arrive at the conclusion that the whole atmosphere has a continued tendency to move from west to east, and does so more when it is not interrupted. The interruptions are of two kinds: one occasional and irregular, being caused by fluctuations in the hydrostatic condition of the air, the other permanent or seasonal and regular, caused by the pressure of lines of coast and mountain ranges.

It is impossible to say definitely why the atmosphere should have this prevailing motion; but if the cause is neither heat nor the influence of the earth's rotation, nor any agency which we can detect at work on the earth, we are driven almost insensibly to the belief that it must be the result of celestial attraction; and the fact that the barometer shows no trace of any noteworthy rise or fall, as of an atmospheric tide, suggests that the atmospheric currents, which must necessarily be formed by the action of such an intense disturbing force, do not in any way clash, but flow uninterruptedly onwards in one certain direction, either towards the east or towards the west. All observation shows us that there is not a permanent current towards the west, but that there is one towards the east; and although we are unable at present to master all the details of the manner of the motion, the evidence of geographical fact, combined with that of astronomical possibility, justifies us in inclining towards the belief that the motive force for which we are seeking is really the disturbing force of the attraction of the heavenly bodies.

SECTION G.—*Mechanical Science.*—President, Mr. Charles B. Vignoles, F.R.S., Pres. C.E.

In his introductory address, bearing chiefly on railway development and the defence of the country, the President remarked that that day, the 15th of September, was the anniversary of an event in which Liverpool played a most important part forty years ago. The opening of the Liverpool and Manchester Railway was entirely owing to the high public spirit shown by the merchants of Liverpool. At that time the ways and means of communication were so completely crippled that the trade of Liverpool would have become paralysed, had not better, speedier, and cheaper means of communication been created, and especially by the opening of the railway connecting Liverpool and Manchester. In this great work Liverpool took the lead; and it was particularly owing to the zeal of one of Liverpool's most distinguished citizens, the late Mr. Henry Booth, who was the original secretary of the railway company, and to the energy and scientific knowledge which he brought to bear upon the question of railways, that attention was paid to the improvements of the steam-engine, which was now performing such wonders both at home and abroad. He (the President) thought the people of Liverpool had not shown themselves sufficiently grateful to the memory of Mr. Henry Booth in allowing his name to lapse, as it were, from public recollection; but he was glad to have been invited to a meeting held in Liverpool a short time ago, at which a subscription was inaugurated for the raising of a statue to that eminent man and successful worker in mechanical science. The subscription list needed only a little addition to complete this most desirable object. Liverpool was peculiarly appropriate for this meeting of the British Association, for that Association and the railway system might be said to have had their birth in the same year, both having originated just forty years ago. He had himself taken part in laying down the Liverpool and Manchester Railway, and felt a special interest in this anniversary. The President then went on to mention the principal subjects which would be discussed in that Section. He next referred to the position of this country in regard to the means and preparedness for military defence, and as to the military service of England. He might, perhaps, be disabusing the minds of many persons who had supposed that the Government of this country was not prepared or was not alive to the necessity of creating the best means of internal communication, in the event of war or invasion of the country, when he mentioned to them that for several years past the military authorities had been in constant communication with the chief engineers of the country, and had formed deliberate arrangements, by which, in the event of such a casualty as a military invasion of England, within forty-eight hours the military forces of the whole country, say 100,000 men, might be brought down upon any given point that might be attacked. Therefore they might feel reassured as to the position of England in case of an invasion. As an old soldier himself, he had, at the request of the Government, treated this question, and had shown completely that within twelve hours of the alarm of invasion at any given point, the rolling stock of the railways of the country could be applied for concentrating all the military resources of the country upon the point threatened. He had stated in Liverpool forty-one years ago that the institution of railways would have this very result; and it had been practically exemplified in the war that was raging on the Continent. On Tuesday, he might mention, papers would be read upon the construction and stability of ships by some of the most distinguished men in the country, bearing upon the most unfortunate accident that had occurred to the ironclad steamship *Captain*, to the causes of which the attention of the whole scientific world was now being directed.

REPORT OF THE COMMITTEE ON BOILER EXPLOSIONS

Mr. Lavington E. Fletcher, C.E., reporter. The other members of the Committee were Sir William Fairbairn, Bart., C.E., F.R.S., LL.D., &c.; Sir Joseph Whitworth, Bart., C.E., F.R.S.; John Penn, C.E., F.R.S.; Frederick J. Bramwell, C.E.; Hugh Mason, Samuel Rigby, Thomas Schofield, Charles F. Beyer, C.E.; and Thomas Webster, Q.C., F.R.S.

In concluding their report, the committee stated that they decidedly incline to the plan of enforcing inspection directly by law rather than indirectly by penalty. They are not without apprehensions that, however ingeniously the principle of joint-stock insurance might be surrounded with a series of checks and counterchecks, yet that it would lead to inspection being cut down to the lowest possible point. On the other hand, were the inspection enforced by law, and nationally administered either by a central steam board or by a series of district ones, they consider that a far more generous system would be secured. The steam boards, uninfluenced either by private or local interests, or by the desire to accumulate profits, would take altogether higher ground, and inspect, not simply in their own interests, and just sufficiently to narrowly escape explosion, but with a view to assist steam users, disseminate practical information on the making and management of boilers, and promote progress. These objects would be altogether foreign to competing joint-stock insurance companies. The committee hold the view that, had coroners' verdicts been as satisfactory as they might have been, boiler explosions would not have been as numerous as they now are. With the additional experience of another year they feel compelled to take one other step in advance, and they have come to the conclusion that the time has arrived when the Government should enforce the periodical inspection of all steam boilers. They are convinced that explosions might be, and ought to be, prevented; that competent inspection is adequate for this purpose, and that any well-organised system of inspection extended throughout the entire country would practically extinguish boiler explosions, and save the greater part of the seventy-five lives now annually sacrificed thereby.

A paper on the same subject was read by Mr. E. B. Marten, C.E., and the discussion was taken upon both communications. The speakers were Sir William Fairbairn, Mr. Siemens, Sir Joseph Whitworth, Messrs. Hawksley, Bramwell, Rigby, Longridge, Gray, Mallet, Sir William Armstrong, and the President. In summing up, the President remarked that many accidents were attributable to the dishonest construction of boilers. English habits seemed to kick against anything like Government interference, but such accidents as had arisen from boiler explosions should be put an end to as forcibly as possible—like stamping out the smallpox or the cattle plague, notwithstanding vulgar prejudices—if necessary, by an iron hand. The Government should pass a law making the inspection of boilers compulsory.

On Mechanical Stoking.—Messrs. James Smith and J. and T. Vicars, Liverpool. The paper is too long to reproduce here, but we may mention, in reference to the method described in it, that Mr. L. E. Fletcher, C.E., of the Steam Users' Association, remarked that he had witnessed some very carefully-conducted trials with the apparatus as against very careful hand-firing, and that he could testify that the furnace was perfectly smokeless, and in every respect attained good results.

CONTENTS

ERRATA.—Page 391, col. 2, the asterisk referring to first foot-note should have been placed after the word "feet," line 33—Page 393, col. 2, line 5 from bottom, for "Cyclus" read "Cyclas"; line 2 from bottom, for "Surg." read "Surv."—Page 408, col. 2, Contents, for "G. D. De Rance" read "C. E. De Rance."—Page 423, col. 2, line 34 from bottom, for "this" read "their"; line 6 from bottom, for "Gaskell's" read "Quekett's"—Page 424, col. 1, line 30 from bottom, for "*gradatium*" read "*gradatim*"; col. 2, line 4, for "Loca" read "Local"; line 6, for "this" read "the"; line 5 from bottom, for "Darwin's" read "Damon's"—Page 425, col. 1, line 5, for "conjuvat" read "conjurat"; line 8 from bottom, for "*Ajax* or" read "*Ajax* of"; col. 2, line 28, for "which" read "who"—Page 426, col. 1. line 13, for "war" read "woe"; line 34 from bottom, for "knotgrass, cowgrass" read "knotgrass *s.* cowgrass"; line 24 from bottom, for "*porcena*" read "*porcina*"—Page 427, col. 1, line 1, for "Bichamp" read "Béchamp"; line 17 from bottom, for "may come when" read "may, when"; col. 2, line 35, for "inhalistic" read "vitalistic"; line 36, for "useful" read "vital"; line 37, for "involve" read "resolve"; line 39, for "ordered" read "resolved."

THURSDAY, OCTOBER 6, 1870

SCIENTIFIC ADMINISTRATION

NO reflecting Englishman can contemplate the great events of the present time without desiring to extract from them such warnings and instruction as may be serviceable to his country in case she should be drawn into war. Accordingly the press teems with discussions on every branch of the military art. We leave these to others. In what respects the constitution, the discipline, the training, and the arming of one army are superior to those of the other, it is scarcely the function of this journal to point out. Taking the broad fact that the Prussian army has, up to the present point, proved itself superior on the whole to that of France, and indeed to any army that has ever existed—a fact that no unprejudiced person will deny—let us ascertain, if we can, whether there may not be recognised some one broad cause to account for so broad a fact.

In this inquiry we have been almost forestalled by the Chancellor of the Exchequer at Elgin. Mr. Lowe recited the lessons which he considered we have to learn from the Prussians. He spoke of their "intelligence," their "organisation and docility," their "extraordinary knowledge, forecast, and diligence." He enumerated nearly all the qualities that command success. But there was one word which that profuse enumeration did not contain—a word which Mr. Lowe no doubt felt unable to utter, and that word is—Science. A Government which refuses aid to astronomers anxious to observe so rare a phenomenon as a total solar eclipse, cannot be expected to vaunt the prowess of science. Mr. Lowe's statement of the causes of Prussian success was therefore incomplete; it was a mere reckoning of the bricks of the building, without a word of its architecture and design.

The Prussians, whatever their other qualities, are emphatically a scientific people, and to that predominating characteristic first and foremost are their recent military triumphs due. We do not mean that because they are great chemists, astronomers, and physicists, therefore are they necessarily great soldiers; so narrow a proposition would hardly be tenable. What we mean is that the spirit of science possesses the entire nation, and shows itself, not only by the encouragement given throughout Germany to physical research, but above all by the scientific method conspicuous in all their arrangements. What does the word Science, used in its wider sense, imply? Simply the employment of means adequate to the attainment of a desired end. Whether that end be the constitution of a government, the organisation of an army or navy, the spread of learning, or the repression of crime, if the means adopted have attained the object, then science has been at work. The method is the same, to whatever purpose applied. The same method is necessary to raise, organise, and equip a battalion, as to perform a chemical experiment. It is this great truth that the Germans, above all other nations, if not alone amongst nations, have thoroughly realised and applied. In all the vast combinations and enterprises with which they have astounded the world, no one has been able to point to a single deficiency in any one essential element. Every post has been adequately filled and every want provided for; from the monarch, the statesman, and the strategist, to the lowest grade in the army—each department complete, each arm of the service, whether cavalry, infantry, or artillery, trained to its own special duties, and efficiently equipped for their performance. This is the method of science, literally the same method which teaches the chemist to prepare his retort, his furnace, and his re-agents, before commencing his experiment.

This, we maintain, is the great lesson, of a material kind, which the war should teach us. Where is our science? At the Admiralty and the War Office, partisan placemen preside over technical administrations. Is that science? Under pressure of the newspapers or of private influence, a ship of war is built by an amateur in spite of the demonstration of our professional adviser that she must be unsafe, and she goes accordingly to the bottom with 500 souls in the first gale of wind. Is that science? One-half of the forces on which we reckon for the defence of the nation is composed of patriotic volunteers, with whom training is optional, and to whom efficient officers and arms are denied. Is that science? The government of London, the greatest metropolis in the world, is parcelled out to scattered knots of ignorant, sordid tradesmen, on whom no ingenuity has ever been able to fix a shadow of responsibility. Is that science? Have we before us the crudest outline of the strategical and military operations with which it is proposed that an invasion of this country should be repelled? Coming political policies always, in England, cast their shadows long before. Have we any indications of a coming military policy? Have we the means of calling together, in a short space of time, properly provided with the necessaries of a campaign, the forces requisite for carrying out a given military policy? Do we know, for instance, how our volunteers, who are reckoned on, man for man, as equivalent to regular troops, are to be employed?

We fear it is a terrible truth that absence of scientific method is as conspicuous with us as its presence is with the Germans. As a nation, we have never realised the necessity for system and completeness in utilising our material resources. The use for the scientifically trained mind has, in our idea, been limited to chemicals and the like.

In courage, energy, intelligence, and wealth, natural and acquired, England need shrink from no comparisons with other nations, but she has yet to awake to the want of that something in her arrangements that shall enable her to turn her enormous advantages to the best account. Science, using the word in its sense of the method applied to things, not to the things themselves, is that something.

OWENS COLLEGE, MANCHESTER

OCCASIONS may sometimes arise, and in fact have already arisen, when it becomes a necessity for a journal like ourselves, devoted exclusively to scientific matters, to direct some attention to what is going on around it in the general world. One of these lately occurred, and caused us to make the remarks we did recently on the apathy displayed by the Government towards scientific research. This week, in a similar manner, we desire to call the attention of our readers to some proceedings which took place on the 23rd ult. in Manchester. This we do because of the great interest these proceedings have to all scientific men, both on account of the ultimate benefit science generally will attain through them, and also on account of some one or two very remarkable speeches made on the occasion.

The laying of the foundation-stone of the new Owens College building by the Duke of Devonshire, its newly elected president, is an event of national importance. For in it we see clearly—and such is the view held by all the professors and governors of that institution—the beginning of the great Scientific University of the North of England. Owens College was founded some twenty years ago by the munificent bequest of Mr. John Owens, a Manchester merchant of the old school, who left his money to found an endowment for the promotion of learning in his native town of Manchester. He was wise enough to stipulate that his money should not be employed for any building purposes, but solely towards educational purposes, leaving it to his fellow-townsmen to provide the house accommodation. This was done at first on a small scale, but Owens College has been from that time gradually increasing in numbers year by year, until a few years ago it was found absolutely necessary that new buildings should be obtained to meet the constantly augmenting number of students. Subscriptions were immediately set on foot, and in a short time something like 130,000*l.* was raised. With this the present buildings have been undertaken. On Friday week the foundation-stone was laid by the Duke of Devonshire, in the presence of the Bishop of Manchester, Professor Huxley, Professor Tyndall, and all the professors and governors, and the chief notabilities of Manchester. In the address presented to the Duke of Devonshire, it is stated that the projected buildings will provide for 600 day students, and a much larger number of evening students, and will include both chemical and physical laboratories. The architect, Mr. Waterhouse, has provided for the permanent accommodation of the Natural History and Geological Museums, presented by the late Manchester Natural History Society and the Manchester Geological Society, and also for the large library and various lecture and examination rooms.

The Duke of Devonshire, in laying the stone, remarked that he looked upon this day as one very celebrated in the annals of Manchester, and one destined to make Manchester more and more renowned at no distant date, as possessing a college second to none in England, and one which would become the centre of the scientific culture of the north of England. No year passes without some considerable benefactions being made to the college, and as fast as funds accumulate, new professorships in all branches of science are being founded, and the college has already become one of quite national importance.

Professor Huxley and Professor Tyndall also spoke, the former congratulating the town on the great results it has attained without any State aid, this State aid having been refused, and showing the great benefits which could not fail to attend the increased college accommodation in the great manufacturing and mining district all round Manchester; and the latter showing that the past work achieved by the eminent and able professors of the college was a sure guarantee of the work which would be done in the future.

The proceedings wound up with a luncheon in the Town Hall, at which the most remarkable speech of the day was delivered by the Bishop of Manchester, Dr. Frazer. In replying to the toast of the Bishop of Manchester and the clergy of all denominations, he said he had no hesitation in replying to this toast, as Owens College had been founded for educational purposes, without any reference to special religious bodies, and he continued—

"I take a very large, broad, and comprehensive view of what is meant by Truth. I believe that everybody who earnestly seeks to propagate the truth, to preach the truth in the largest sense of the word, is doing good to his fellow men. I never believed that true Science is contrary to true Religion, or that true Religion ought to be afraid of any legitimate consequences of true Science. I know well, and the knowledge makes me speak with some tremulousness, that I am in the presence of those who are considered to be, and who have established their right to be considered to be, the ablest interpreters of the laws and phenomena of the physical and material world. I cordially welcome those gentlemen as teachers and propounders of the truth. If scientific men will only believe that we, the clergy of this kingdom, are not sceptics in disguise, or charlatans trying to palm off upon the world something that has been found to fail; if they will only believe that we want to tread calmly, step after step, where we find our remedies have succeeded, I think they will allow that we are searching after truth, the only truth I care to find—practical truth—truth that will elevate man in the scale of being—and I think they will admit that we are trying to follow out truth by strictly scientific methods. I do not care from what source it comes; I will welcome every means which is calculated to settle the disputed boundaries between Religion and Science, and show that both alike, in their legitimate province, minister to, and help to bind up the great temple of Truth."

The fearlessness with which the Bishop thought it his duty to speak out, would, if followed throughout the whole of England, serve to overthrow that unfortunate antagonism there is at present between Religion and Science, founded on an entire misconception of the aims and the value of the latter as compared to the former.

The further proceedings on this interesting occasion were the delivery of some very instructive speeches by Profs. Huxley and Tyndall, and also by Prof. Henry, of the Smithsonian Institution, Washington, which, however, our space prevents us from further noticing. Certain it is, however, that the proceedings of the opening day are such as every scientific man throughout the kingdom will welcome with pleasure, and we cannot doubt that a report of them will be interesting to our readers. We hope that this occasion will inaugurate a new era for science, and will serve to bind together in the strongest bands those untiring workers in the pursuit of truth, whether they be scientific men or the clergy of all denominations.

The present Government has so far entirely refused to assist Owens College by any grant of money similar to that which a few years ago fell to the share of Glasgow University. The Government alleges that since the Manchester merchants have done so much for the college they can easily do more, and so complete the good work they have so well commenced. A stranger and more disheartening reason it would be hard to imagine. Our rulers appear to have yet to learn that there is such a thing as principle in the application of public money to the promotion of the real progress of the nation. We look forward to the report of the Science Commission to define the principles on which these grants should rest; and we trust we may then have a Government both capable of understanding what these principles are, and of firmness in carrying them out into practice.

J. P. E.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Aurora Borealis

THE Aurora Borealis noticed in the *Times* was observed here on Saturday the 24th inst. between 9 and 11 P.M. Another was observed on Sunday the 25th between 2.30 and 3.15 A.M.; and again another on the same day about 8.30 P.M.

I did not see the first, but I did see the two last, and the Aurora of Sunday morning appears to have been the most vivid of the three.

About 2.30 A.M. a strong red glare as of blood appeared above a thick black cloud about 40° eastward of north and 30° elevation. As this faded, the red glare appeared westward of north at the same elevation. The clouds did not extend to the horizon, which was pretty clear, and in half an hour they had passed away.

At 3.15 the sky was clear, and vivid yellowish rays extended nearly to the polar star. The rays had a gradual motion to the eastward. This was well observed by the rays passing in front of the stars of the tail of the Great Bear, which were at that time nearly parallel to the rays. I ceased observing about 3.30.

The Aurora observed at 8.30 P.M. appeared to me very faint in comparison to that at 3.20 A.M. I would hardly have noticed it if a friend had not pointed it out to me.

This same person had observed the Aurora of the 24th, and it was from his observation that I inferred that the Aurora of the morning of the 25th was also much brighter than that of the evening of the 24th.

N. A. STAPLES

Louvain, Sept. 30

Fuel of the Sun

I AM not mathematician enough to form any opinion on the merits of the controversy as to the "fuel of the sun;" that is to say, I am not able to decide whether it is consistent with the conditions of the equilibrium of the solar system that the sun's heat should have been kept up through the ages of geological time by the falling in of meteors. But I wish to state some evidence which proves that meteors are constantly falling in, though it does not touch the question whether this source is sufficient to account for the whole or any large part of the total supply of heat radiated away by the sun.

In the first place, the meteors have been seen. On Sept. 1, 1859, Mr. Carrington and another observer simultaneously observed two meteor-like bodies, of such brightness as to be bright against the sun's disc, suddenly appear, move rapidly across the sun from west to east, and disappear.

The fact that their motion was from west to east is important. If the supply of meteors to the sun is constant and tolerably regular, it is scarcely possible to doubt that the meteors, like the entire solar system, move round the sun from west to east, and occupy a space of the form of a very oblate spheroid, having its equator nearly coincident with the sun's equator.

If this is the case, the meteors ought to fall in greater numbers near the sun's equator than near his poles, making the equator hotter than the poles. Such is the fact. Secchi, without having any theory to support, has ascertained that the sun's equator is sensibly hotter than his poles. The instrument used was an electric thermo-multiplier, and the indications show, not the ratio, but the difference of the heat from the two sources compared.

It can scarcely be doubted that the meteors must enter the sun's atmosphere with a velocity not much less than that of a planet, revolving at the distance at which they enter. We know that the sun's rotatory motion is incomparably less than this, and consequently the meteors, revolving from west to east, ought to make the sun's atmosphere move round his body in the same direction, and with greater velocity in the equatorial regions, where most meteors fall in. This is what is observed. Mr. Carrington, also without any theory to support, has shown that the motion of the solar spots from west to east is most rapid in the latitudes nearest the equator. We cannot compare the motion of the spots with that of the sun's body, as we do not see his body. But the fact that the motion from west to east is most rapid in the equatorial latitudes proves that these motions are not due to any cause like that which produces trade-winds and "counter-trades" of our planet; for, supposing the sun or any planet to rotate from west to east, in any circulation that could be produced in its atmosphere by unequal heating at different latitudes, the relative motions of the atmospheric currents in high and low latitudes would be similar to that of the trade-winds and "counter-trades," and opposite to that which the motions of the spots indicate in the atmosphere of the sun. This will be true at all depths in the atmosphere.

JOSEPH JOHN MURPHY

Suggestions for the Improvement of Meteorological Investigation

THE position of Great Britain at the head of a vast empire encircling the globe, and soon to be at the centre of a network of telegraphs that will feel all the pulses of the world, imposes upon British naturalists and the British Government the duty of leading the way in the important work of meteorological investigation. In the hope of aiding the progress of this work, I venture, through your columns, to call public attention to the following suggestions:—

First:—The increase of the number of meteorological stations on and near the equator is very desirable. For instance, an increase of weather reports from the West Indies and the Atlantic States of North America, especially about latitudes 30° to 32°, would be highly valuable to the people of Great Britain and other portions of Western Europe.

Second:—In meteorological reports, we should recognise both the unity of the atmosphere and its division into areas corresponding with the great divisions of the earth's surface into land and water. As storms are generally confined within these areas, they may be called storm areas, or sections of the atmosphere in which disturbances are very closely connected. For instance, the area within which the greater storms that visit Great Britain begin and end, or circulate with destructive force, is bounded by the equator on the south, and the Rocky Mountains on the west. The northern and eastern boundaries are not yet determined. On September 7, 1869, the first "Northers" of the season visited New Orleans; on September 8th, storms passed over the Northern States; and between September 9th and 23rd, storms passed over Great Britain and Western Europe. Again, on October 1st, 1869, the barometer at Havana indicated the approach of bad weather; on October 2nd, 3rd, and 4th, there were heavy gales and rains at New York; on the night of October 4th, occurred one of the most destructive storms that has ever visited Maine and New Brunswick; on October 6th, there was a heavy gale in England. The destructive gale in England, on October 16th, was preceded by a hurricane in New England on October 11th. These two last-mentioned storms appear to have been closely connected not only with each other, but also with the extraordinary heat which prevailed in England on October 8th, 9th, and 10th, and in France on October 11th. All the storms mentioned, however, are only specimens of the many annual disturbances of the same kind whose connection with the Atlantic Ocean as a centre has been, or may easily be traced. They are referred to here, merely to show that about an eighth of the whole atmosphere constitutes, and may be named, the Atlantic storm area. To make a weather report of much practical value in Great Britain and Western Europe, it should cover the whole of this area. The number of places, however, from which reports are published, need not be so large as at present.

Third:—The records of the atmospheric conditions and changes should be arranged with reference to the latitude and longitude of each station. At present there is no system in tabulating meteorological observations. The weather tables now published in the daily papers are of comparatively little value to the general public, owing to their want of arrangement. A weather-table, to be of any great practical value in the northern hemisphere, should contain—first, a record of observations made at points south of the place for which the table is designed, and if possible on or near the equator and the 30th to the 32nd parallel; next, a record of observations made at western points within the limit of what I have ventured to call the storm area; next, a record of observations at northern stations about latitude 60°; and next, at eastern stations within the storm area. This statement is not to be understood as implying that all storms begin at the equator or at any one point of the compass, or that they are in all cases confined within one section of the atmosphere, or move in one direction.

Fourth:—It is very important to obtain correct and copious data regarding the atmospheric currents between (say) 5,000 feet and five miles above the level of the sea, and especially at various points on and near the equator, and at about 30° to 32° North and South latitudes. Within these limits the rain-bearing currents of the atmosphere move. If self-registering meteorological instruments were placed permanently upon several of the leading mountain ranges of the world, and their records copied at stated intervals, we should obtain valuable data for determining the direction, velocity, and magnitude of the controlling atmospheric currents of the globe. More valuable still would be data obtained by the use of self-registering and self-regulating machines that would ascend to any desired height within the limit mentioned above, remain up for a time determined by clock-work, and then descend, bringing with them complete records of temperature, moisture, direction and velocity of currents, &c. Additional facts regarding the great atmospheric currents within the limits named are required to enable us to interpret correctly the oscillations of barometers near the surface of the sea. G.

New York

Colour Blindness

I HOPE your readers will bear with a few more observations from me on the foregoing subject. It is an undoubted fact that modes of nervous action which have once coexisted tend to excite each other afterwards. This the phenomenon known as Association of Ideas sufficiently proves. It is to this cause that I should be disposed to attribute the phenomenon of accidental or complementary colours. I do not, of course, mean that this latter fact can be resolved into Association of ideas, but that it and association depend on the same organic law. It is probable that in addition to this, however, the mutual excitation of vibrations comes in. Judging from the determination of wave-lengths by diffraction, the ratio of the wave-length of any given ray to that of the complementary one is not very far from 2 to 3 or 3 to 2; and, as the colours excited do not seem to be exactly complementary, it is probable that the vibration referred to is that chiefly excited. For an accurate determination of this question we should require to determine the wave-lengths for each colour in the liquid which surrounds the eye, or rather, perhaps, in the retinal substance. Such are the two causes to which I should be disposed to ascribe the phenomenon of accidental colours. From the former it would follow that the more completely the eye had been accustomed to white light, the more likely the person would be to become colour blind; and that colour-blindness might be remedied, or at least altered in character, by accustoming the eye to look at everything through coloured glasses.

There are, however, subjective causes which determine the eye to exhibit various colours in particular instances. Of these, jaundice, which makes the object appear yellow, is one. Derangement of the stomach, I believe, often invests objects in the dark with a blue tint. Colours of this description constantly mingle with our ordinary perceptions without being noticed, but when a finer distinction is needed it becomes necessary to avoid them. Thus, in some of the delicate optical experiments to which I referred in my last, the experimentalist must not operate after a long fast or a hearty meal, or after taking any alcoholic drink. He sometimes finds, also, that his two eyes differ in their appreciation of colours. Two instances of this subjective colouring recently came under my notice. In one case an old lady for some time saw everything red, which she attributed to looking very much at a red flower (which she greatly admired) in her sitting-room. She was advised to look a good deal at the green fields opposite her window, and soon recovered. In the other case a friend of mine, reading a note-book which he had marked with a blue pencil, was surprised to find that the marks appeared green. He showed me the book, and the marks were quite unaltered to my eye. He was reading hard, and somewhat nervous, but otherwise in good health. In most of the cases I have referred to, the subjective colouring was only temporary; but I have little doubt that there frequently exists a permanent subjective colouring which modifies all the phenomena of vision, and leads to effects, in some respects, similar to those of ordinary colour-blindness.

Some time ago the question was suggested to me whether a blind man (whose retina was not destroyed) could have a perception of extension by sight, or whether, in the uniform darkness which was supposed to surround him, he could distinguish extension at all? I tried the experiment by shutting my eyes, but finding that when the light was good a very perceptible amount made its way through the eyelids, I placed bandages over them in addition. I could never, however, obtain uniform darkness. Points over the field always appeared less dark than the surrounding ones, and the positions of these points were easily distinguishable. On making the experiment for the third or fourth time, I was rather surprised to see part of the visible surface covered by a faint blue. Subsequent observation confirmed this, and I now believe that it was only from habitual inattention to colours when the eye was shut (as well as the difference between colours on a dark and on a bright ground), that I failed to observe it on the first occasion. These colours are by no means confined to blue. In fact, I think I have seen every tint in the spectrum, and I never can close my eyes for three minutes without seeing some of them more or less distinctly. They generally only cover a patch which, however, is not fixed, and the colour first visible sometimes disappears altogether. I may, perhaps, add to this that I often see a collection of small, bright, round moving spots about the centre of the field of vision. These spots I can likewise see in the dusk with my eyes open. This affords a proof of subjective colours in my own eyes, but from their variability, I do not think my eyes have a predilection for any particular tint. I often see more than one of these colours at the same time. If the eye had a predisposition for any particular colour, it could probably be discovered in this way. The observations are worth repeating. A close attention to the sensations is required on account of their faintness.

I have mentioned the expedient of looking through coloured glasses. Professor Wartmann succeeded in this way in making colour-blind persons distinguish colours which they confounded with the naked eye—a fact quite explicable on my principles; but he found it impossible to predict what effect a glass of any given colour would produce. But might we not by repeated experiments hit upon the particular tint which would suit the eye of any particular patient? If, for example, we tried his eye with a solar spectrum, and interposed one or more coloured glasses of different tints, and in this tentative manner at last succeeded in making him see seven colours, each confined within the same limits that they are to the ordinary eye, it is pretty evident that these glasses would enable him to distinguish the colours of all objects in the daylight.

Not having read much on the subject, I cannot say how far these views are original. The subject is at least one which requires further investigation, and if I can induce some of the eminent contributors to your columns to take up the matter, my letters will not have been vain though the result may be to overthrow all that I have advocated. W. H. S. MONCK

Trinity College, Dublin

The Effect of Tannin on Cotton

IN your last number you mention the fact that cotton fabrics are rendered more durable by treatment with tannin, as if it was a new discovery, and state that "it is believed the change cannot be great, since it has escaped the notice of practical tanners."

In our neighbourhood (the coast of Northumberland) the fishermen have, for many years past, been in the habit of tanning their nets and sails with oak-bark or catechu.

At the tanyard with which I am connected, we tan a large number yearly, including many cotton nets. Not only does it render them more durable, but in some cases, where wet nets have heated and become tender, their toughness has been restored by tanning. I cannot attempt to explain the chemical action which takes place, and, indeed, the still more important ones by which leather is produced are very imperfectly understood.

HENRY R. PROCTER

North Shields, Sept. 26

The Intended Engineering College

IN my letter on the above subject, I alluded to Mr. Mason's magnificent foundation of an educational institution in Birmingham, and by a queer inadvertence wrote the Christian name "Oliver"; and shall be much obliged if you will insert this correction of my mistake. It is Mr. *Josiah* Mason, the well-known pen manufacturer and founder of the orphanage and almshouses at Erdington, who is so liberally and judiciously enriching the Midland metropolis

W. MATTIEU WILLIAMS

The Haze Accompanying Auroral Displays

I DOUBT if the haze, seen before and during Aurora, has received sufficient attention. The beautiful displays which we in Canada have so frequently seen of late, have been well adapted to lead us to inquire into their cause; and I think in making observations, the beauty of the luminous portion has led us to overlook other things which are equally important.

On the night of April 15, 1869, we had a grand exhibition of Aurora at Toronto. It spread itself all over the heavens, forming a glorious canopy, filling the south as well as the north.

Previous to any auroral display, however, the atmosphere became thick and hazy; I was viewing the setting moon through a telescope, and though there were no clouds, I found the definition become extremely bad; I thought my breath must have got on the eye-piece, but soon found this was not the case. I then went outside, and on looking round, found the whole atmosphere full of haze. It had not the appearance of fog, but the whole air seemed thick and turbid, and shortly auroral columns commenced forming. This haze was visible for hours. Referring to this feature, Prof. Kingston says (*American Journal of Science*, July 1869, p. 65):—"Throughout the night, a generally diffused luminosity prevailed, such as is commonly seen with a full moon and hazy sky. This was evidently not occasioned by the moon, which was scarcely four days old, and was low in the horizon, but was part of the aurora itself, the brilliancy of whose more active features it greatly impaired." This haze is seldom seen spread in this way all around us; but it is usually seen as a bank in the north, and is surmounted by the auroral arch; stars can be seen through it, but it greatly dims their lustre. Prof. Loomis says:—"The slaty appearance of the sky, which is a common feature of great auroral exhibitions, arises from the condensation of the vapour of the air, and this condensed vapour probably exists in the form of minute spiculæ of ice or flakes of snow. Fine flakes of snow have been repeatedly observed to fall during the exhibition of auroras, and this snow only slightly impairs the transparency of the atmosphere without presenting the appearance of clouds. It produces a turbid appearance of the atmosphere, and causes that dark bank which in the United States rests on the northern horizon. This turbidness is more noticeable near the horizon than it is at great elevations, because near the horizon the line of vision traverses a greater extent of this hazy atmosphere. When the aurora covers the whole heavens, the entire atmosphere is filled with this haze, and a dark segment may be observed resting on the southern horizon."

Whilst approving of the professor's description, I must dissent from his explanation of the cause. This haziness is seen during our hot summer nights, as well as our cold ones; and I have seen it when snow-flakes would be out of the question in such a temperature. Many reasons leads me to regard this haze as cosmical, falling on our earth from without; but at present, I will call attention to the appearance only, reserving my explanation for a future time.

A. ELVINS

NOTES

IT is with very great regret that we have to announce the death, at the age of fifty-two, of Dr. William Allen Miller, Professor of Chemistry in King's College, London, and treasurer and vice-president of the Royal Society. Professor Miller's writings have earned for him a position in the literature of chemistry, from which he will be very greatly missed.

WE understand that Professor Allman has resigned the Chair of Natural History in the University of Edinburgh. It has long been felt that the subjects of Geology and Mineralogy which have hitherto been taught from this Chair are of such vast and growing importance and extent, that they can no longer be properly included in it when a successor is appointed. It gives us the greatest pleasure to state that Sir Roderick Murchison, who has already done so much for the geology of his native country, has munificently come forward with the offer of 6,000*l.* towards the endowment of a separate Chair of Geology and Mineralogy, on the understanding that on this, as on former occasions, the Government will supplement the private grant by an equal sum. Here then we have an admirable occasion for the Government to show itself alive to the importance of fostering the cultivation of those sciences on which especially the future welfare of the country must largely depend.

THE new Faculty of Science in University College, London, was opened on Tuesday last, by an inaugural address by Prof. A. W. Williamson, F.R.S. The discourse was devoted to an exposition of the importance of scientific method, and of the value of a scientific training, as an introduction even to the life of an ordinary man of business.

IN addition to his discourse to the Social Science Congress at Newcastle, Dr. Lyon Playfair delivered, on Thursday evening last, the inaugural address on opening the session of the Birmingham and Midland Institute. The subject of the lecture was announced as "The Inosculation of the Arts and Sciences;" and in its course the lecturer discussed the intimate union between science and labour. It is not science which creates labour or the industries flowing from it. On the contrary, science is the progeny of the industrial arts on the one side, and on the other of the experiences and perceptions which gradually attach themselves to these arts, so that the evolution of science from the arts is the first circumstance of human progress, which, however, quickly receives development and impulse from the science thus evolved. Industrial labour, then, is one of the parents, and science the child; but, as often happens in the world, the son becomes richer than the father, and raises his position. Apologising for the apparently pedantic form of the word, Dr. Playfair said he proposed to treat of the "inosculation" of arts and sciences, their junction with open mouths, as when two arteries join and mingle their contents. It will be seen that science does not depend upon facts alone, but upon the increase of mental conceptions which can be brought to bear upon them; these conceptions increase as slowly as the common knowledge derived from experience—they both descend by inheritance from one generation to another, until science in its progress becomes a prevision of new knowledge by light reflected from the accumulated common knowledge of the past. In the progress of time common knowledge passes into scientific knowledge. The amazing changes which have taken place since 1838 are due to our better conceptions of forces and their mutual relations and conversions. Formerly heat, light, electricity, magnetism, and chemical affinities, were thought to be separate and independent existences, not even related to each other. Now we know that forces are convertible and interchangeable. This knowledge has already given great stimulus to their application, and will do so more in the future. Further, we know that the primary source of nearly all the power on the earth is the sun above.

THE publishing trade shows indications of its usual activity at this time of the year. Among the announcements of forthcoming works from the leading houses bearing more or less directly on science, we note the following. From Messrs. Longmans and Co. :—The Life of Isambard Kingdom Brunel, Civil Engineer, by Isambard Brunel, B.C.L. ; A System of Logic and History of Logical Doctrines, by Dr. F. Ueberweg, 1 vol. ; The Sun : Ruler, Light, Fire, and Life of the Planetary System, by Richard A. Proctor, B.A. ; Spectrum Analysis in its Application to Terrestrial Substances and the Physical Constitution of the Heavenly Bodies, familiarly explained by Dr. H. Schellen, Director der Realschule I. O. Cologne, translated from the German by Jane and Caroline Lassell, edited, with notes, by William Huggins, LL.D., D.C.L., F.R.S , 1 vol. crown 8vo., with coloured plates and other illustrations ; Select Methods in Chemical Analysis and Laboratory Manipulations, by William Crookes, F.R.S., &c., editor of the *Chemical News ;* A Handbook of Dyeing and Calico Printing, by William Crookes, F.R.S., &c., illustrated with numerous specimens of textile fabrics ; Text-Books of Science, a new series of elementary works on Mechanical and Physical Science, forming a series of Text-Books of Science adapted for the use of artisans and of students in public and other schools, in small 8vo., each volume containing about 300 pages. From Messrs. Macmillan and Co. :—The Sun, by Balfour Stewart, LL.D., F.R.S., and J. Norman Lockyer, F.R.S. ; The Beginnings of Life, including an Account of the present State of the Spontaneous Generation Controversy, by H. C. Bastian, M.D., F.R.S.; An Introduction to the Osteology of the Mammalia, by W. H. Flower, F.R.S. ; A Treatise on Magnetism, designed for the use of Students at the University, by G. B. Airy, Astronomer Royal ; New Volumes of the School Class Book Series—Elementary Lessons in Logic, Deductive and Inductive, by Prof. Jevons, and Elementary Lessons in Physics, by Balfour Stewart, LL.D., F.R.S. From Messrs. Rivingtons :—Exercises adapted to Algebra, Part. 1, by J. Hamblin Smith ; A Manual of Logic, or, A Statement and Explanation of the Laws of Formal Thought, by Henry J. Turrell. From Messrs. Blackie and Son :—A translation, by Prof. Everett, of Prof. Deschanel's Elementary Treatise on Natural Philosophy, Part 1—Mechanics, Hydrostatics, and Pneumatics, illustrated with a coloured plate and many woodcuts. From Messrs. Asher and Co :—Man in the Past, Present, and Future, from the German of Dr. L. Buchner, translated by W. S. Dallas, F.L.S. From Messrs. Churchill :—A Manual of Botany, by Robert Bentley, Professor of Botany, King's College, London, and to the Pharmaceutical Society (second edition) ; A Laboratory Text-Book of Practical Chemistry, or Introduction to Qualitative Analysis, a guide to the course of practical instruction given in the laboratories of the Royal College of Chemistry, by W. G. Valentin, F.C.S. ; Handbook of Volumetric Analysis, or the Quantitative Estimation of Chemical Substances by Measure, by Francis Sutton, F.C.S., Norwich (second edition, much enlarged).

THE early publication is announced of "The Year-Book of Pharmacy," containing the proceedings at the yearly meeting of the British Pharmaceutical Conference, and a Report on the Progress of Pharmacy, which will include notices of all Pharmaceutical papers, new processes, preparations, and formulæ published throughout the world. It will be edited by Mr. J. C. Brough.

THE Professor of Chemistry to the University of Cambridge will begin a course of lectures on the Experimental Laws of Heat on Monday the 17th of October. He will also give instruction in Chemical Manipulation on Mondays, Wednesdays, and Fridays, at 1 P.M., beginning on the 10th of October, at the University Laboratory, which will be open daily from 10 till 6 for the use of students.

THE prospectus of the South London Working Men's College for the coming session includes classes in the following departments of Natural Science :—Chemistry, Physics, Animal Physiology, Geology, Metallurgy, and Applied Mechanics, as well as in Political Economy. The presence on the Council of the names of Professors Huxley, Fawcett, and Tyndall, Sir John Lubbock, and Dr. Cobbold, are a sufficient guarantee of the quality of the instruction given.

PROFESSOR DUNCAN, F.R.S., will commence a course of evening lectures on Geology, at King's College, London, on Monday, October 10th.

THE first meeting for the session of the Royal Microscopical Society will be held at King's College, London, on Wednesday, the 12th inst., at 8 o'clock, when the following papers will be read by Dr. G. W. Royston Pigott, M.A. :—"On Aplanatic Illumination," and "On Aplanatic Definition, with Optical Illustrations."

A MEETING of the Leeds Field Naturalists' Society will be held on Tuesday, October 11th, at 8 o'clock, at the Young Men's Christian Association, South Parade, to consider the advisability of holding weekly meetings during the winter session.

OWENS COLLEGE, Manchester, has lately received a very valuable donation to its large geological collection, in the shape of a collection of fossil Marsupials from Australia. This collection was to have been presented to the British Museum, but the donor ultimately decided to bestow it on Manchester instead.

THE Jesuit College at Manilla, in the Philippine Islands, has established a meteorological observatory, with self-recording instruments, where records of earthquakes are made in that region so fertile of them.

A MAGNIFICENT refracting telescope, with an object-glass of 25 inches diameter, is being constructed at the manufactory of Messrs. Clarke and Sons, Cambridgeport, Massachussetts, for the National Observatory at Washington. The money for this valuable instrument was voted by Congress last session, and amounts to 50,000 dollars, about 10,000*l*. It is believed that this telescope, when completed, will be one of the largest in the world ; meanwhile, it will take four years to finish it completely.

THE closing of Paris deprives us of the communication of meteorological observations and the Registrar-General of the records of births and deaths. The next international congress should provide for the requirements of science.

THE recent great summer-heat in America has been attracting great attention there. According to the records of Yale College, it has been the hottest summer for the last 92 years. "From July 10, to Aug. 15, 1870, the mean daily temperature was, at New Haven, 85°, and no season since 1778 has shown so many consecutive hot days. Our highest temperature this summer was (July 17) noted at 98°, and this has only been exceeded four times during the period above indicated at New Haven, the thermometer rising to 100° one day each year in 1784, 1800, and 1845, whilst in 1798 it reached 101°." This will be very interesting to compare with the temperatures ascertained this year in England.

IN the aquarium of the Dublin Zoological Gardens there are several specimens of the blind fish (*Amblyopsis spelæus*) lately brought from the Kentucky caves by Prof. Mapother. The small specimens, being very transparent, show the vertebral column, the heart, and the optic bulbs very distinctly. In the largest there are dark red spots over the optic bulbs, probably due to their having been kept in an iron vessel, which may have given colour for a rudimentary pigment membrane.

In the last part of the *Zeitschrift für Biologie* is an interesting paper by Dr. Camerer, on the locality of the sense of taste, or, as he expresses it, "On the dependence of the sense of taste on the part of the oral cavity irritated." His experiments have been conducted on himself, his wife, a lady friend, and six peasant girls. He employed solution of common salt, quinine, sugar, and sulphuric acid as the exciting agents, and localised their action by pouring a drop or two into a small open tube that was pressed on different parts of the tongue, the determination of the test fluid being required without the subject of the experiment being made acquainted with the nature of the solution used. It is well known that there are three kinds of papillæ on the tongue—the conical, the fungiform, and the circumvallate. Dr. Camerer finds that the sensibility of different parts of the tongue depends essentially on the presence and on the number of the fungiform papillæ. The fungiform papillæ, he finds, are most abundant near the apex of the tongue, they are less numerous at the edges of the tongue, and disappear near the root. There are no papillæ on the under surface of the tongue. The latter part he finds to be destitute of gustatory sensibility, whilst the sensibility is most acute when the papillæ are most closely aggregated together. He states also that when a strong solution has been tasted of any of the above substances, the delicacy of the sensibility is impaired for twenty-four hours. Other direct and carefully conducted experiments showed that the gustatory sensibility resided in the fungiform papillæ themselves and not in the parts adjoining.

According to the *Food Journal*, in Belgium the butchers use laurel-oil on the door-posts and window-frames for the purpose of keeping away flies, with great success. So simple a contrivance would be a great boon to the *habitués* of our eating-houses and confectioners, and would be useful to every housekeeper. The emanation from minced laurel leaves is rapidly fatal to all small insects.

With a view further to promote the cultivation of the Rhea fibre, the Viceroy of India has sanctioned an expenditure temporarily of 8*l*. per month, at Shaharunpore and at Dehra Dhoon, a hill station.

There are now in London specimens of iron manufactured for the first time in Peru by the Peruvian Government Commission from magnetic iron ore, found within twenty miles of Lima, and from fair anthracite taken from a seam sixty feet thick at sixty miles from Lima. The Government is awaiting the development of the railways now in progress. Renewed efforts are being made to enlist English capital in the restoration of the silver mines.

Mr. Adams, the Secretary of our Legation in Japan, has been rendering good service in doing all he can to protect the silkworm in those countries by his influence and advice, which are willingly received. At the last advices he had returned from a lengthened tour in the interior, on which he will report. In Japanese are some illustrated works on silk culture.

*ON CERTAIN PRINCIPLES TO BE OBSERVED IN THE ESTABLISHMENT OF A NATIONAL MUSEUM OF NATURAL HISTORY**

It having been now finally determined that the Natural History collections of the British Museum shall be removed from their present site to South Kensington, to form the nucleus of a National Museum of Natural History,† it appears to me that the principles upon which the proposed new institution are to be established and conducted, are well worthy of the special and most serious attention of the British Association for the Advancement of Science. The inauguration of a National Museum of Natural History by one of the nations that have contributed most largely to the advancement of the natural sciences, is an event that is not likely to recur very often. If the opportunity thus presented be properly taken advantage of, and the new institution started upon sound principles of administration and arrangement, there can be no doubt that a most material impetus will be given to the progress of natural science in this country.

Under these circumstances I think I need hardly apologise for troubling the section with a few remarks upon certain points which appear to me to be most essential to be observed in the establishment of a National Museum of Natural History. These, I trust, will at all events provoke discussion, and induce some of the many distinguished naturalists present at this meeting to turn their attention to this most important subject.*

The energies of our rulers, especially in these troubled times, are too fully occupied with ordinary politics to allow them to bestow much care on such a matter, and unless it be forced on their attention by the British Association, or in some other authoritative manner, the result will be, I fear, that the system of administration now followed in the British Museum as regards the Natural History collections, will be transplanted along with the collections themselves, and the excellent opportunity of a grand reform, which may never again present itself, will be utterly wasted.†

The remarks which I propose to offer to the section on this subject may be divided into three heads. First, I will say a few words concerning what appears to me to be the best mode of government of the proposed National Museum of Natural History. Secondly, I will speak of the form of building which in my opinion ought to be adopted; and lastly, of the arrangement of the collections within that building.

I. *Of the form of Government of the National Museum of Natural History.*

On this part of my subject I shall make but few remarks, having regard to the fact that, in common with many other of my fellow naturalists, I strongly committed myself on this point some years ago, and have in nowise changed my views since that period. In the memorial, of which I hold a copy in my hands, and which was presented to the Chancellor of the Exchequer in 1866 having been signed by 25 leading members of the Royal, Linnean, Geological, and Zoological Societies, it will be found to be stated that in our opinion the chief administration of the National Museum of Natural History should be entrusted to one officer, who shall be immediately responsible to some member of the Government. Those who are acquainted with the present mode of administration of the Natural History collections in the British Museum will, I am sure, readily agree to this proposed reform. It will be recollected that the government of the British Museum is vested by Act of Parliament in a body of fifty trustees, consisting principally of great officers of state, and of nominees of certain families whose ancestors have contributed to the heterogeneous contents of that building. Amongst these fifty trustees there are but two or three that are in any wise interested in Natural History. Their secretary and

* Read before Section D at the meeting of the British Association at Liverpool, on September 16, 1870.

† On the 3rd of August last a vote of 6,000*l*. was proposed in the House of Commons by the Chancellor of the Exchequer to clear the ground "for the erection of a Natural History Museum" on the site of the International Exhibition at South Kensington, and carried, after a division.

* For account of this discussion see p. 465.

† In the "bill to enable the Trustees of the British Museum to remove portions of their collections," prepared and brought in by the Chancellor of the Exchequer in 1862, it was proposed to be enacted that the trustees might remove the natural history collections to South Kensington and certain pictures to the National Gallery. But, in a subsequent clause, it was proposed to be added that "*except in so far as was therein before expressed, nothing therein contained should affect the rights, powers, duties, or obligations of the trustees of the British Museum.*" At that time, therefore, it was clearly intended to continue the rule of the trustees over the natural history collections when removed to South Kensington.

chief executive officer is the present principal librarian, with whose great literary qualifications for his position every one is well acquainted, but who would not, I am sure, claim for himself in any sense the name of a naturalist. It will thus be seen that the actual government of our Natural History collections is at present vested in persons who have no special qualifications for the task. But, it may be said, there is the Superintendent of the Natural History collections, and the keepers of the various departments into which they are divided—have they nothing to do with the administration? To this I reply, very little indeed, unless their advice is asked, or unless they choose to offer it. And, in the latter case, they can only address the trustees through the secretary, who is the only official present at the meetings of the trustees, and in whose hands, therefore, the administration of the Natural History collections is practically vested. This objectionable form of government, we think, ought to be replaced by appointing a director of the proposed new institution, "immediately responsible to one of the Queen's Ministers." This simple form of administration has been most successful in other scientific institutions, such as the Kew Gardens and Herbarium, and the Royal Observatory, and we believe it would be the best in the present case. It might, however, be advisable to give the Director of the National Museum of Natural History a board of advice, composed of the heads of the principal departments into which the Museum is divided. Or another mode of softening the despotism would be to appoint a board of visitors, consisting of distinguished naturalists. These might be delegates from the principal scientific societies of the country, each of whom would be specially bound to see that the particular branch of science, to the advancement of which his society is devoted, received its fair share of attention.

As regards the subordinate appointments in the Natural Museum of Natural History, these ought to be made, if not on the nomination of the director, at least not without his full sanction and approval. The director, being held responsible for the well-doing of the whole establishment, should certainly be allowed to select his own officers more or less directly. It is well known that some of the appointments made by the trustees in the departments of Natural History in the British Museum have been, to say the least of them, in no wise felicitous, and that in one case at least great public scandal has been caused by the notorious incompetence of the person nominated. It is in vain to address remonstrances to a body of irresponsible trustees, but if the director is required to sanction every nomination, we shall know to whom to apply in case of any appointment not being up to the mark.

II. *Of the form of Building of the National Museum of Natural History.*

In discussing the form of building best adapted for a great National Museum of Natural History, let us begin by considering the principal classes of persons for whose accommodation it is or ought to be constructed. These are—

1. The public at large, who go there to get a more or less general notion of the structure of natural objects and of their arrangement in the *Systema Naturæ*.

2. The students who use the Museum for scientific purposes.

3. The officers of the institution, whose business it is to amass and arrange the collections.

In the opinion of most members of parliament apparently especially of those who represent metropolitan constituencies, the first of these three classes is that whose accommodation ought to be first considered in the present case. In my opinion, and probably in that of most of those here present, the National Museum of Natural History ought to be constructed primarily for the accommodation of the third of the three classes. For, unless the officers of the institution have ample space and opportunity to examine and arrange the collection, it is obvious that neither the public nor the special student can be benefited thereby. At the same time I do not think that the public ought to be utterly excluded from their Museum four days in every week, as is now the case, and I therefore put it forward as an axiom that some system of construction of the New Museum should be adopted whereby the public can be admitted all day and every day to view the collections without interfering with the scientific work of the establishment or with the special examination of objects by students. There is, so far as I know, only one plan by which this object can be carried out—namely, by arranging the exhibited objects in large wall-cases, to which access is obtainable from the back by doors opening into work-rooms adjoining the exhibition room. In this way any ordinary object can be removed out of the series into the adjoining work-room, and returned to its place without disturbing the public in front of the cases, just as any article can be taken out of the shop-windows in Regent Street without interfering with those who are looking into them from the pavement outside. This system of exhibition would be attended by the further very great advantage that the glass cases may be hermetically sealed on the side towards the public, and the ingress of dirt and dust thus prevented. Those who are acquainted with the filthy state of the specimens in the public galleries of the British Museum, in spite of frequent cleansings inflicted upon them, will readily appreciate the merit of this plan.*

This collocation of the exhibition galleries and corresponding working-rooms being insisted upon as of primary importance, the general form of the building must depend somewhat upon the site on which it is to be placed. My own belief, however, is that a hollow square, or something nearly approaching that form, will in many ways be most convenient for a National Museum of Natural History, and the sketches which I now exhibit, which have been prepared for me by my accomplished friend Mr. Osbert Salvin, will serve to show the general plan of arrangement which I propose. The building might be of three or four stories, since, in the system of exhibition which I advocate, it would not be necessary to have top-lights. The basement, which might be partly below the surface, would be dedicated to taxidermy and to rooms for unpacking, storage, and mechanical work of all sorts. In the outer galleries running round the whole length of the ground story, I should propose to arrange the entire series of vertebrates from the highest mammal to the lowest fish. The specimens, according to the system already spoken of, would be placed in hermetically sealed glass cases along the inner walls of the galleries. The inner series of rooms surrounding the interior of the hollow square would be the working-rooms for the officers of the museum and the students of natural history, and would communicate with the glass cases on the inner side of the outer galleries. Each set of working-rooms would, of course, be in immediate apposition to the glass cases containing the corresponding series of exhibited objects. The lights to these working-rooms would be furnished from the inner sides of the hollow square.

In the first story of the building I should propose to arrange the series of invertebrate animals in exactly the same way, with the rooms for officers and students immediately adjoining them on the inner side.

The third story might contain the botanical and mineralogical collections, and perhaps certain others which it might not be possible to introduce into the general series, unless room could be found for these collections in the second story.

* In an admirable article on this subject in NATURE, for May 26, 1870, Prof. Flower has attributed the original invention of this mode of exhibition to myself, I having first brought it under his notice. It appears, however, from a subsequent communication to NATURE by Prof. Flower (June 2, 1870), that the same plan had been already proposed by Dr. Hooker in the *Gardener's Chronicle* for 1858, p. 749. I can only, therefore, claim to be *an* (not *the*) original inventor of this method of arrangement.

In a circular building, the centre of the hollow square, I should propose to place the library above and lecture-theatre below. The library might be connected by light iron galleries with the different working-rooms, so that the students of every department would have equally ready access to it.

Such is a slight outline of the kind of building I would propose for a National Museum of Natural History. It is, of course, a mere sketch, and there would be, no doubt, many difficulties in the details to be surmounted, but none, I think, such as an experienced architect would not be able to overcome. The advantages of this plan would be :—

1. The museum might be opened to the public every day without interfering with the scientific work of the establishment or of the students. Under the present arrangement, the collections are only open two or three days in the week, during which scientific work is suspended, as regards all objects in the public galleries.

2. The exhibited specimens would be much better protected from dirt and dust than they are in cases opening in front.

3. The exhibition of the whole series of organic beings in one continuous range of galleries would be much more instructive to the public than any system in which (as in the British Museum) they are dispersed about in different rooms.

4. The library being in the centre, would be equally accessible from any one of the working-rooms surrounding the interior of the hollow square.*

III. *Of the Arrangement of the Collections in the National Museum of Natural History.*

The remarks which I have already made under the previous head will have served to show the section that I am an advocate of what has been called the "typical," but what it would be better, perhaps, to call the "representative" system of arrangement of the Natural History collections. Nor am I able to understand how any reasonable person can seriously maintain that every object in a National Museum of Natural History ought to be exhibited to the indiscriminating public. In accordance with the views of the memorialists of 1858,† who may be considered as having inaugurated the reform in our Natural History collections which I hope to see shortly carried out, the collections should be primarily separated into three series : (*a*), objects for public exhibition ; (*b*), objects for private study. The class *a*, which is to be arranged in the public galleries behind the hermetically sealed glass cases, should embrace a very full and well-selected series of representatives of the principal forms of every class. In some cases it may be necessary to place in this category examples of every species of a group, in others only a selection of each genus or of each family. Every specimen exhibiting the external form in this series should be carefully prepared and mounted in a natural attitude. The representative species of the group having been selected, specimens of both sexes and of all ages should be placed in the series, as likewise examples of variation, if any such are known. The skeleton and other preparations of the internal structure should be added, as also the eggs and nests in the case of birds, and examples of corresponding structures in other classes. In short, the utmost endeavours should be made to illustrate, by preparations, models, and drawings, the life-history of the selected "representative," in as complete a manner as possible. To every exhibited specimen should be attached a printed label, giving its scientific and popular name, locality, and origin, and some short explanation regarding its chief peculiarities and most noticeable points of interest. There can, I think, be no doubt whatever that a small but well-selected series of any branch of the kingdom of nature, arranged after this method, would be of much greater interest and much more instructive to the public at large, than ten times the number of objects arranged according to the present fashion of the British Museum.

On the other hand, the great mass of the collections (*b*) intended only for the private examination of experts should be treated after a very different fashion. In this division of the collections, the object is to arrange specimens in as small a space as possible, and, at the same time, in the most convenient manner for easy examination. The work-rooms immediately adjoining the part of the public galleries appropriated to division *a* of any class, will, of course, be devoted to the reception of division *b* of the same class, so that the whole *a* and *b*, being separated only by the partition-wall at the back of the glazed cases, which will be pierced by frequent doors, will practically form but one collection. In these work-rooms, moreover, should be assembled together the whole of the specimens relating to the particular class to which they are devoted. In the British Museum, according to the present system, the mounted specimens are in one room, the skins in a second, the skeletons in a third, and the spirit-preparations in a fourth. So that, in order to make a complete examination of a small mammal, for instance, it may be necessary to go to four or five different parts of the building, ranging from the galleries to the cellars, and from the extreme north-east corner of the former to the furthest south-west corner of the latter. In the new National Museum of Natural History, it is to be hoped, this inconvenience will be remedied by the entire amalgamation of the various collections of skins, mounted specimens, spirit-specimens, and skeletons, into one uniform series. Besides the greater convenience of this mode of arrangement, another obvious advantage will be that the future student will be induced to devote his attention rather to the whole structure of the organism than to confine it to one particular part. If bird-cabinets were accompanied by skeletons and corresponding specimens in spirits, there can be no doubt that a much more perfect system of ornithology than any that we have yet attained to would be quickly arrived at. Our new national museum must take the lead in this great reform, and set an example to other collections. In the same way, as every naturalist will allow, our conchological brethren will lose nothing by having the soft bodies of the mollusca close at hand to aid them in their investigations on the form of the external shell. There may be, of course, some exceptional cases in which it will be practically impossible to adopt this course, but, as a general rule, the principle should be insisted upon that every specimen, of whatever nature it may be, should be located in the rooms devoted to the reception of the class to which it belongs, and should be placed as nearly as possible in immediate apposition to its nearest natural allies.

To carry out these principles to their legitimate issue, I do not hesitate to support the view put forward by Prof. Flower * and other naturalists, that the palæontological department of the British Museum, as at present constituted, ought to be totally abolished, and its contents distributed amongst the zoological and botanical collections, so that extinct forms may be studied in association with their nearest living representatives. The arguments in favour of this plan are, I think, unassailable, and although some little difficulties may be met with in carrying it out, there are none, in my opinion, that may not be overcome by judicious treatment. There is no doubt, I believe, that the progress of palæontology and palæophy-

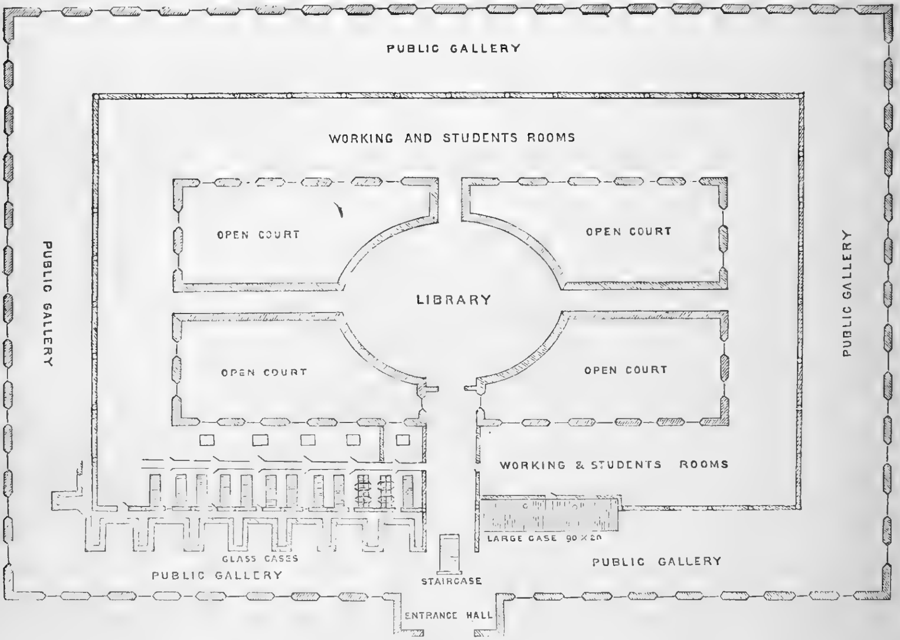
* A great deal has been said by those who have advocated the retention of the Natural History collections in their present site, about the importance of keeping up their conjunction with the National Library. It is, of course, obvious that their removal will necessitate the acquisition of a special Library of Natural History for the new museum. I believe, however, that a library of the kind, sufficiently comprehensive for all practical purposes, can be got together without much difficulty and at a comparatively small cost, and that when formed, it will be of much greater use for those working at the collections than the present overgrown establishment at the British Museum. It must be also recollected that the library of the British Museum is only available for the use of the officers. The books cannot be brought to the specimens nor the specimens to the books by ordinary students.

† See this memorial, as reprinted in NATURE for June 9, 1870.

* See NATURE for May 28, 1870.

tology has been much retarded by the neglect of the students of the extinct forms of animal and vegetable life to make themselves sufficiently acquainted with the structure of the corresponding forms now in existence. So long as fossils were looked upon as the products of numerous successive and independent creations, there might have been some excuse for this mode of dealing with them. But now that we regard animated nature, past, present, and future, as one and indivisible; now that we acknowledge the stream of life, since its first appearance on this planet, to have been unbroken and continuous, let us exhibit its products, whether existing or extinct, in one continuous and unbroken series. The structure of an extinct organism can only be correctly understood after study of the nearest allies at present in existence. The best palæontologist must be he that has deduced his knowledge of extinct beings from comparison of their remains with the corresponding parts of those now alive. Those who appreciate these truths will not fail to allow that the proposed amalgamation of the palæontological collection in the general series in the new Museum of Natural History, will be a decided step in advance, and one imperatively called for in the present state of natural science.

I have now, I think, touched upon some of the principal points on which changes are required in our present system of treatment of the collections of natural history belonging to the nation. It would be easy to go into further particulars in which reforms are needed. Especially I might call attention to the inadequacy in point of numbers of the present staff of officers in some of the Natural History departments of the British Museum, the insufficiency of the yearly sum allowed for acquisitions, the vexatious regulations concerning the examination of specimens, and the miserably insufficient accommodation for private study. But all these things we may well hope to see altered in a new institution, and I will not



take up time by enlarging upon them. In conclusion, however, I will recapitulate the principal topics touched on in the following propositions, which I trust the members of the British Association will agree with me in putting forward as the "platform" of reforming naturalists.

1. The administration of the New Museum of Natural History should be vested in a director, who should be immediately responsible to one of the Queen's Ministers.

2. The collections should be primarily divided into two series: *(a)* those intended for public exhibition; *(b)* those reserved for private study.

3. The collections *a* (for public exhibition) should be arranged in their natural order, in one continuous series of galleries, so as to give the best possible general idea of the principal forms of life, and of their arrangement according to the natural system.

4. The collections *b* (for private study) should be arranged in rooms immediately adjacent to the public galleries, in such a manner that the corresponding portions of *a* and *b* should practically form but one series, and that the private student should have access at all times to objects in the public galleries.

5. A complete Library of Natural History should be furnished for the special use of the institution, and be placed in some central portion of the building, equally accessible to all departments.

6. The collection of osteology, the spirit-preparations, the skins in store, the series of British animals, the collection of "nests and nidamertal structures," and all other subordinate collections, should be amalgamated in the general series.

7. The collections of the Palæontological Department should likewise be amalgamated with the general series.

P. L. SCLATER

SOCIAL SCIENCE CONGRESS, NEWCASTLE

DR. PLAYFAIR'S OPENING ADDRESS TO THE EDUCATIONAL SECTION

In the address delivered by Dr. Lyon Playfair before the National Association for the Promotion of Social Science, he began by referring to the lamentable position of English education at present. Speaking of the Act of last session, he pointed out that it deals with the quantity of education, and not with its quality; and insisted on the absolute necessity of introducing instruction in Science into our primary schools. The following are some of the more important passages of the address on this point:—

The educational principle of continental nations is to link on primary schools to secondary improvement schools. The links are always composed of higher subjects, the three R's being in all cases the mere basis of instruction. Elementary science, and even some of its applications, is uniformly encouraged and generally enforced. I shall not detain you with examples, as they are to be found in any work treating of continental schools. But as we have no schools corresponding to the secondary improvement schools for the working classes, we suppose that we can do without the higher subjects used as links. With what result? Our primary schools, on the whole, do not teach higher instruction than a child of eight years of age may learn. In our class of life, our children acquire such knowledge as a beginning; with the working classes, they get it as an end. What an equipment for the battle of life! No armour-plate of knowledge is given to our future artisan, but a mere thin veneer of the three R's, so thin as to rub off completely in three or four years' wear and tear of life. I am speaking on official record, for we are assured by inspectors, that nothing under Standard IV. suffices for permanent use, and yet the Committee of Council ell us that four-fifths of the children of ages at which they leave school pass only in lower standards. Recently, under Mr. Corry's minute, inducements have been given for subjects higher than the three R's, but for some reason it produces scarcely any result. So, under our present system of elementary teaching, no knowledge whatever bearing on the life-work of the people reaches them by our system of State education. The air they breathe, the water they drink, the tools they use, the plants they grow, the mines they excavate, might all be made subjects of surpassing interest and importance to them during their whole life; and yet of these they learn not one fact. Yet we are surprised at the consequences of their ignorance. A thousand men perish yearly in our coal-mines, but no schoolmaster tells the poor miner the nature of the explosive gas which scorches him, or of the after-damp which chokes him. Boilers of steam-engines blow up so continually that a committee of the House of Commons is now engaged in trying to diminish their alarming frequency; but the poor stokers who are scalded to death or blown to pieces, were never instructed in the nature and properties of steam. In Great Britain alone more than one hundred thousand people perish annually, and at least five times as many sicken grievously, out of pure ignorance of the laws of health, which are never imparted to them at school; they have no chance of learning them afterwards, as they possess no secondary schools. The mere tools of education are put into the hands of children during their school time without any effort being made to teach them how to use the tools for any profitable purpose whatever; so they get rusty or are thrown away altogether. And we fancy that we have educated the people! Our pauperism, our crime, and the misery which hovers on the brink of both, increase terribly, and our panacea for their cure is teaching the three R's up to Standard III. The age of miracles has passed by, and our large faith in our little doings will not remove mountains. It is best to be frank. Our low quality of education is impoverishing the land. It is disgracefully behind the age in which we live, and of the civilisation of which we boast; and until we are convinced of that we cannot be roused to the exertions required for its amendment. In censuring the low condition of knowledge in our primary schools, as represented by the results of the Revised Code, I do not aim to restore them to the position which many of them had before it. That code was, in fact, rendered necessary because their aggregate teaching was not sufficiently large and diffused to justify the increasing expenditure. In imitation of our classical schools, verbalism and memory-cramming had grown up as tares and choked the growth of the wheat. Words had taken the place of conceptions. A child could tell you about the geography of the wanderings of the children of Israel, but had no conception whatever of the ordinary phenomena around it. It was hopeless to put to them the commonest scientific questions. Whence comes the water that fills the Thames? What is the origin of hail, snow, rain, or dew? Why does the sun rise in the east, or set in the west? What produces night and day, summer and winter? In history they could rattle out to you the names and dates of kings and queens, perhaps even the names and ages of all Queen Anne's children as they died in childhood; but, as a true historical conception, apart from memory cramming of words and dry facts, to be vomited forth upon the examiner, it required a very good school under the old system to find it. Words, instead of ideas, were worshipped. Inspection, under the old system, did something to correct this tendency to verbalism and cram; under the new system they had no time, and, if they had, would find fewer of the higher subjects taught in any way. The teaching of science, if properly done, is the reverse of all this, and will go far to remedy its defects. Books in this case ought only to be accessories, not principals. The pupil must be brought in face of the facts through experiment and demonstration. He should pull the plant to pieces and see how it is constructed. He must vex the electric cylinder till it yields him its sparks. He must apply with his own hand the magnet to the needle He must see water broken up into its constituent parts, and witness the violence with which its elements unite. Unless he is brought into actual contact with the facts and taught to observe and bring them into relation with the science evolved from them, it were better that instruction in science should be left alone. For one of the first lessons he must learn from science is not to trust in authority, but to demand proof for each asseveration. All this is true education, for it draws out faculties of observation, connects observed facts with the conceptions deduced from them in the course of ages, gives discipline and courage to thought, and teaches a knowledge of scientific method which will serve a lifetime. Nor can such education be begun too early. The whole yearnings of a child are for the natural phenomena around, until they are smothered by the ignorance of the parent. He is a young Linnæus roaming over the fields in search of flowers. He is a young conchologist or mineralogist gathering shells or pebbles on the sea shore. He is an ornithologist and goes bird-nesting; an ichthyologist and catches fish. Glorious education in nature, all this, if the teacher knew how to direct and utilise it. But as soon as the child comes into the school-room, all natural God-born instincts are to be crushed out of him; he is to be trained out of all natural sympathies and affections. You prune and trim, cramp and bind the young intellect, as gardeners in olden times did trees and shrubs, till they assumed monstrous and grotesque forms, altogether different from the wide-spreading foliage and clustering buds which God himself gave to them, and which man is idiot enough to think he can improve. Do not suppose that I wish the primary school to be a lecture theatre for all or any of the "ologies." All the science which would be necessary to give a boy a taste of the principles involved in his calling, and an incitement to pursue them in his future life, might be given in illustration of other subjects. Instead of mere descriptive geography drearily taught and drearily learned, you might make it illustrative of history, and illustrated by physical geography, which, in the hands of a real master, might be made to embrace most of what we desire to teach. The properties of air and water, illustrations of natural history, varieties of the human race, the properties of the atmosphere as a whole—its life-giving virtues when pure, and its death dealings when fouled by man's impurities—the natural products of different climes, these and such like teachings are what you could introduce with telling and useful effect. Far better this than overlading geography with dry details of sources and mouths of rivers, of isothermal lines, latitudes and longitudes, tracks of ocean currents, and other tendencies towards the old verbalism and memory-cramming. If I have explained myself with clearness, you will see that while I advocate the introduction of higher subjects into our schools, I wish them to be of immediate interest and applicability to the working classes. The main difficulty in education is getting them to stay long enough at school. Teach them, while you have them, subjects of interest and utility. The short time will thus be made productive, and inducement will be offered for its extension. Six months spent in teaching future labourers the geography of the wanderings of the children of Israel, is sheer waste of time, either for their eternal or temporal interests. Think of the few precious hours as the training for a whole lifetime, and let us use them by giving living and intelligent learning, not obsolete and parrot instruction. Those who are believers in the teaching of the great secondary schools of this country will deem my aspirations for the improvement of primary education, low and utilitarian. Frankly I admit the latter. Such a style of education will never realise Lord Brougham's hope that the time may come when every working man in England will read Bacon; but it might contribute to the fulfilment of Cobbett's desire, that the time might come when every man in England could eat bacon. I deny, however, that the utilitarian view of primary education is ignoble. The present system is truly ignoble, for it sends the working man into the world in gross ignorance of everything that he is to do in it. The utilitarian system is noble, in so far as it treats him as an intelligent being, who ought to understand the nature of his occupation, and the principles involved in it. The great advantage of directing education towards the pursuits and occupations of the people, instead of wasting it on dismal verbalism, is that, while it elevates the individual, it at the same time gives security for the future prosperity of the nation. In the industrial battles of peoples, we are content to leave our working classes armed with the old Brown Bess of warfare, while men of other countries are arming themselves with modern weapons of precision. In the competition of nations, the two factors of industry—raw material and intellect, applied to its conversion into utilities—are altering their values. The first is rapidly decreasing, the second quickly augmenting in value. We anchor our hopes on the sand, which the advancing tide of knowledge is washing away, while other nations throw out their anchors on firm ground accumulating around, and enabling their vessels to ride in safety. There are instances of nations, rich in the natural resources of industry, yet poor from want of knowledge how to apply them; and there are opposite examples of nations utterly devoid of industrial advantages, but constituted of an educated people who use their science as a compensation for their lack of raw material. Spain is an example of the first class, and Holland of the second. Having pointed out at some length the contrast between these two countries, in consequence of the difference of their culture, Dr. Playfair proceeded to show the necessity of good physical training, to argue in favour of a compulsory educational system, and of graded education, and to define the true position and qualifications of teachers in primary schools.

THE BRITISH ASSOCIATION

LECTURE ON STREAM LINES IN CONNECTION WITH NAVAL ARCHITECTURE, BY PROF. RANKINE

THE lecturer stated that his object was to give a brief summary of the results of some application of the mathematical theory of hydrodynamics to questions regarding the designing of the forms of ships, and the mutual actions between a ship and the water in which she floats. The art of designing the figures of ships had been gradually developed by processes resembling those called "natural selection," and the "struggle for existence" in the course of thousands of years, and had arrived in skilful hands at a perfection which left little more to be desired, when the object was to design a ship that should answer purposes and fulfil conditions which had previously been accomplished and fulfilled in the course of practical experience. But cases now frequently arose in which new conditions were to be fulfilled; and purposes accomplished beyond the limits of the performance of previous vessels, and in such cases the process of gradual development by practical trials made without the help of science was too slow and too costly, and it became necessary to acquire and to apply scientific knowledge of the laws that regulate the actions of the vessel on the water and of the water on the vessel. Amongst the questions thus arising were the following: What ought to be the form of the immersed surface or skin of a ship in order that the particles of water may glide smoothly over it? And, the form of such a surface being given, how will it affect the motion of particles in its neighbourhood, and what mutual forces will be exerted between the particle of water and that surface? Practical experience, unaided by science, answers the first question by saying that the surface ought to belong to a class called "fair surfaces" (that is, surfaces free from sudden changes of direction and of curvature) of which various forms have in the course of ages been ascertained by trial, and are known to skilful ship-builders. That answer is satisfactory so far as it goes; but in order to solve problems involving the mutual actions of the ship and the water, something more is wanted, and it becomes necessary to be able to construct fair surfaces by geometrical rules based on the laws of the motion of fluids, and to express their forms by algebraic equations. There were many very early attempts to do this, but through not being based on the laws of hydrodynamics, they resulted merely in the finding of empirical rules for reproducing when required forms which had previously been found to answer in practice, and did not lead to any knowledge of the motions of the particles of water or of the forces exerted by and upon them; and they had little or no advantage over the old process of modelling by the eye and hand, and of "fairing" the lines with the help of an elastic rod called a "batten." As regards this process, indeed, the mathematical methods about to be referred to are to be regarded, not as a substitute for it in designing the form of a ship, but as a means of arriving at a knowledge of the mutual actions between her and the water, which the old process is incapable of affording. The earliest method of constructing the figures of ships by mathematical rules, based on hydrodynamical principles, was that proposed by Mr. Scott Russell about twenty-five years ago, and since extensively practised. It consisted in adopting for the longitudinal lines of a ship curves imitated from the outlines of waves in water. The motions which surfaces formed upon this model impress on the water were known to a certain degree of approximations. These "wave-lines," however, although they were fair curves in the sense already mentioned, were by no means the only fair curves, but were only one class out of innumerable classes of curves having the property of gliding smoothly through the water; and it was well known in practice that vessels had proved successful whose lines differed very widely from wave-lines. It was therefore desirable that methods should be devised of constructing by mathematical rules based on the laws of the motions of fluids, a greater variety of curves possessing the requisite property of fairness, and not limited to the wave-line shape. Such had been the object of a series of researches that had been communicated to the Royal Society at different dates since 1862. They related to the construction of what it has been proposed to call stream-lines. A stream-line is the track or path traced by a particle of water in a smoothly and steadily flowing current. If, when a ship is gliding ahead through the water with a certain speed, we imagine the ship to be stationary, and the water to be flowing astern past the ship in a smooth and steady current with an equal average speed, the motions of the ship and of the particles of water relatively to each other are not altered by that supposition; and it becomes evident that if the form of surface of the skin of the ship has the property of fairness, all the tracks of the particles of water, as they glide over that surface, are stream-lines, and the surface itself is one containing an indefinite number of stream-lines, or, as it has been called, a stream-line surface. It is also to be observed, that when we have deduced from the laws of the motion of fluids the relations which exist between the form of the stream-lines in different parts of a current, and between those forms and the velocities of the particles as they glide along different parts of those lines, we know the relations between the form and speed of a ship whose surface coincides with a certain set of these stream-lines, and the motions of the particles of water in various positions in the neighbourhood of that ship. The lecturer then proceeded to explain, and to illustrate by diagrams, the methods of constructing stream-lines. These methods were based upon the application to stream-lines in a current of fluid of a mathematical process which had previously been applied by Mr. Clerk Maxwell to lines of electric and magnetic force. A current of fluid is represented on paper by drawing a set of stream-lines so distributed that between each pair of them there lies an elementary stream of a given constant volume of flow. Thus, while the direction of flow is indicated in any given part of the current by the direction of the stream-lines, the velocity of flow is indicated by their comparative closeness and wideness apart, being evidently greatest where these lines lie closest together, and least where they are most widely spread. If, upon the same sheet of paper, we draw two different sets of stream-lines, these will represent the currents produced in one and the same mass of fluid by two different sets of forces. The two sets of lines form a network, and, if through the angles of the meshes of that network we draw a third set of stream-lines, it can be proved from the principle of the composition of motions, that this third set of lines will represent the current produced in the same mass of fluid by the combination of the forces, which, acting separately, would produce the current represented by the first two sets of stream-lines respectively. The third set may be called the resultant stream-lines. Suppose, now, that a third set of component stream-lines are drawn representing the current produced by a third set of forces : this will form a network with the previously drawn resultant stream-lines, and a set of lines drawn through the angles of the meshes of this new network will represent the resultant current produced by the combination of the three sets of forces ; and so on to combinations of any degree of complexity that may be required. In order to draw a system of stream-lines suited for the longitudinal lines of a ship, three sets at least of component stream-lines must be combined. One of these is a set of parallel straight lines, representing a uniform current, running astern with a speed equal to the actual speed of the vessel. A second set consists of straight lines radiating from a point called a focus in the forepart of the vessel, and they represent the diverging motion that is produced by the ship displacing the water near her bows. The third set of component stream-lines consists of straight lines converging towards a second focus in the afterpart of the vessel; and they represent the motion of the water closing in astern of the ship. The resultant stream-lines thus produced present a great variety of forms, all resembling those of actual ships having all proportions of length to breadth, and all degrees of bluffness and fineness at the ends, ranging from the absolute bluffness of a sort of oval to a bow and stern of any degree of sharpness that may be required. It has been proposed to call stream-lines of this sort Oögenous Neoïds ; that is, ship-like lines generated from an oval, because any given set of them can be generated by the flow of a current of water past an oval solid of suitable dimensions. The properties of these curves were investigated in 1869. They have, however, this defect, that the absolutely bluff ovals are the only curves of the kind that are of finite extent; all the fixed curves extend indefinitely in both directions, ahead and astern ; and in order to imitate the longitudinal lines of a fine-ended vessel, a part only of some indefinitely extended curve must be taken. In 1870 an improvement in the construction of such curves was introduced, by which that defect was overcome ; it consisted in the introduction of one or more additional pairs of foci, involving the combination of at least five sets of component stream-lines. By this device it is possible to imitate longitudinal lines of actual vessels by means of complete closed curves, without rising portions of indefinitely extended curves ; and thus the motion of the particles of water, as shown by the stream-lines that lie outside the closed lines representing the form of the vessel, becomes more definite and accurate. The lecturer mentioned that the idea of employing four foci and upwards had been suggested to him by the experiments of Mr. Froude on the resistance of boats modelled so as to resemble the form of a swimming bird ; for which purpose stream-lines with four foci are specially adapted. It has been proposed to call such lines Cycnogenous Neoïds—that is, ship-like curves of shapes like that of a swan. In such curves the outer foci—that is, the foremost and aftermost—are situated in or near the stem and sternpost of the vessel, which are represented in plan by small horse-shoe curves, as if they were rounded off at the corners instead of being squared, as in ordinary practice. The inner foci are situated respectively in the fore and after body. When the foci of the longitudinal lines of a vessel have been determined, the proportion borne by the aggregate energy of the motion impressed on the particles of water to that of the motion of the vessel herself can be approximately determined. The lecturer next proceeded to explain the bearing of some of the mechanical properties of waves upon the designing of vessels, especially when these properties are taken in combination with those of stream-lines. It had long been known that ships, in moving through the water, were accompanied by trains of waves, whose dimensions and position depended on the speed of the vessel ; but the first discovery of precise and definite laws respecting such waves was due to Mr. Scott Russell, who published it about twenty-five years ago. The lecturer now described in a general way the motions of the particles of water in a series of waves, and illustrated them by means of a machine designed for that purpose. He showed how, while the shape of the wave advances, each individual particle of water describes an orbit of limited extent in a vertical plane. The periodic time of a wave, its length, the depth to which a disturbance bearing a given ratio to the disturbance at the surface of the water extends, and its speed of advance, are all related to each other by laws which the lecturer explained. He then stated that Mr. Scott Russell had shown that when the vessel moved no faster than the natural speed of advance of the waves that she raised, these waves were of moderate height, and added little or nothing to her resistance ; but when that limit of speed was exceeded, the waves and the resistance caused by them increased rapidly in magnitude with increase of speed. His own (Professor Rankine's) opinion regarding these phenomena was, that when the speed of the vessel was less than or equal to the natural speed of the waves raised by her, the resistance of the vessel consisted wholly, or almost wholly, of that arising from the friction of the water gliding over her skin ; and he considered that this opinion was confirmed by the results of practical experience of the performance of vessels. The wave motion, being impressed once for all on the water during the starting of the vessel, was propagated onward like the swell of the ocean, from one mass of water to another, requiring little or no sensible expenditure of power to keep it up. But when the ship was driven at a speed exceeding the natural speed of the waves that she raised, these waves, in order to accompany the ship, were compelled to spread outwards instead of travelling directly ahead ; and it became necessary for the vessel, at the expense of her motive power, to keep continually originating wave-motion afresh in previously undisturbed masses of water ; and hence the waste of power found by experience to occur when a ship was driven at a speed beyond the limit suited to her length. This divergence or spreading sideways of the train of waves had a modifying effect on the stream-lines representing the motions of the particles of water. It caused them, in the first place, to assume a sinuous or serpentine form ; and then, instead of closing in behind the ship to the same distances from her course at which they had been situated when ahead of her, they remained permanently spread outwards. In other words, the particles of water did not return to their original distances from the longitudinal midship plane of the vessel, but were shifted laterally and left there, much as the sods of earth are permanently shifted sideways by the plough. The place of the water which thus fails to close in completely astern of the vessels is supplied by water which rises up from below and forms a mass of eddies rolling in the wake of the ship. This was illustrated by a diagram. Lastly, the lecturer explained the principles according to which the steadiness of a ship at sea is affected by storm-waves, and the difference between the properties of steadiness and stiffness. The mathematical theory of the stability of ships had been known and applied with useful results for nearly a century ; but in the course of the last ten years it had received some important additions, due especially to the researches of Mr. Froude on the manner in which the motions of the waves affect the rolling of the vessel. A stiff ship is one that tends strongly to keep and to recover her position of uprightness to the surface of the water. A steady ship is one that tends to keep a position of absolute uprightness. In smooth water these properties are the same ; and a stiff ship is also a steady ship. Amongst waves, on the other hand, the properties of stiffness and steadiness are often opposed to each other. A stiff ship tends as she rolls, to follow the motions of the waves as they roll. She is a dry ship ; but she may be what is called uneasy, through excessive rolling along with the waves. The property of stiffness is possessed in the highest degree by a raft, and by a ship which, like a raft, is broad and shallow, and whose natural period of rolling in smooth water is very short compared with the periodic time of the waves. In order that a ship may be steady among waves, her natural period of rolling should be considerably longer than that of the waves ; and in order that this property may be obtained without making the vessel crank, the masses on board of her should be spread out sideways as far as practicable from her centre of gravity ; this is called "winging out the weights." A vessel whose natural period of rolling in smooth water is only a little shorter or a little longer than that of the waves, has neither the advantages of stiffness nor those of steadiness, for she rolls to an angle greater than that of the slope of the waves ; and her condition is specially unsafe if her natural period of rolling is a little greater than that of the waves, for then she tends to heel over towards the nearest wave-crest, to the danger of its breaking over her deck. This is called "rolling against the waves." The most dangerous condition is that of a vessel whose period of rolling in smooth water is equal to that of the waves that she encounters, for then every successive wave makes her roll through a greater angle ; and under these circumstances no ship can be safe, how great soever her statical stability. All these principles have been known for some years through Mr. Froude's researches. The lecturer exhibited a machine he had contrived for illustrating them, in which the dynamical conditions of vessels of different degrees of stiffness and steadiness were approximately imitated by means of a peculiarly-constructed pendulum hanging from a pin, whose motions imitate those of a particle of water disturbed by waves.

SECTIONAL PROCEEDINGS

Section A.—Mathematical and Physical Science

On the Mode of Action of Lightning on Telegraphs, and on a New Method of Constructing Telegraph Coils.—Mr. S. Alfred Varley. He remarked that when lightning storms occur in the neighbourhood of telegraph wires, although the wires may not be actually struck, powerful currents are induced in them which may be sufficiently strong to fuse the coils, but which more frequently simply demagnetise, and as often reverse the magnetism of the magnetic needles situated in the coils of needle telegraph instruments. Thus, not only is a considerable amount of damage done annually to telegraph instruments, but telegraphic communication is very liable to serious interruption. Mr. Varley mentioned a number of observations going to prove that an interval of dust separating two metallic conductors opposes practically a decreasing resistance to an increasing electrical tension, and that incandescent particles of carbon oppose about $\frac{1}{60}$th part of the resistance opposed by a needle telegraph coil. Reasoning upon these data, he has constructed an instrument, the main feature of which is what he terms a "lightning bridge." Two thick metal conductors, terminating in points, are inserted usually in a piece of wood. These points approach one another within about $\frac{1}{16}$th of an inch in a chamber cut in the middle of the wood. This bridge is placed in the electric circuit in the most direct course which the lightning can take, and the space separating the two points is filled loosely with powder, which is placed in the chamber, and surrounds and covers the extremities of the pointed conductors. The powder employed consists of carbon (a conductor) and a non-conducting substance in a minute state of division. When this instrument is used, therefore, lightning which strikes a circuit finds in its direct path not a space of air but a bridge of powder, consisting of particles of conducting matter in close proximity to one another. These the lightning connects under the influence of the discharge, and the particles are thrown into a highly incandescent state. The secondary current, developed by the demagnetisation, finds an easier passage across this heated matter than through the coils. These lightning bridges have been in use since January 1866, and at the present moment there are upwards of 1,000 doing duty in this country alone. Yet not a single case has occurred of a coil being fused when protected by them. The reason why a powder consisting entirely or chiefly of conducting matter cannot be safely employed is, that although it can oppose a practically infinite resistance to the passage of electricity of the tension of ordinary working currents, when a high tension discharge occurs, the particles under the influence of the discharge generally arrange themselves so closely as to make a conducting connection between the two points of the lightning bridge. In the course of his exposition, Mr. Varley endeavoured to prove that when telegraphic circuits protected by ordinary protectors are struck by lightning, it is to the secondary current and not to the main discharge that the fusion must be attributed. He also pointed out the defects of the protector, which consists of two silk wires wound side by side upon a bobbin.

Mr. Varley also read a second paper containing *A Description of the Electric Time Signal, at Port Elizabeth, Cape of Good Hope.* After an elaborate account of the Liverpool time ball, he proceeded to say that in the year 1859, Sir Thomas Maclear, the Astronomer Royal of the Cape of Good Hope, inspected the electrical time signals in this country, with a view to erecting time balls in connection with the Royal Observatory at Cape Town. Sir T. Maclear remarked the greater rapidity of action of the Liverpool trigger, and this led to Mr. Varley's afterwards designing and constructing at different times two triggers for use in the Cape. Both these triggers discharged more rapidly than the Liverpool trigger. In September 1864, he was requested to construct a trigger for discharging a time ball to be erected at Port Elizabeth. He considered the intervention of any relay or secondary apparatus to be objectionable. He therefore determined if possible to construct the trigger sensitive enough to be discharged by the batteries in the Cape Town Observatory, and in its construction he adopted a modification of a principle first introduced by Professor Hughes in his printing telegraph (described at the Newcastle meeting).

The trigger was constructed with a soft iron armature, rendered magnetic by induction from a compound bar magnet, and which strongly attracted the soft iron cores of an electro-magnet, but which was prevented from actually touching the poles of the electro-magnet.

A spiral spring attached to this armature was so adjusted that it nearly overcame the magnetic attraction induced by the bar magnets.

The time current polarised the electro-magnet in the opposite direction to that induced by the bar magnets, and as the attraction between the armature and the soft iron cores was already almost overcome by the spiral spring, a very small amount of polarisation in the opposite direction was necessary to release the armature, which was rapidly pulled away by the spiral spring, and the trigger discharged.

There were some other alterations made in the general mechanical construction of this trigger, but they may be regarded as matters of detail.

The rapidity of discharge was very great, $\frac{1}{20}$th part of a second only elapsed between the arrival of the time current and the falling of the ball.

From a report in the Port Elizabeth paper of August 29, 1865, giving an account of the inauguration of this time signal, and forwarded to Mr. Varley by Sir Thomas Maclear, it appears that the time elapsing between the time current leaving the Observatory at Cape Town and the receipt at Cape Town of the signal announcing the falling of the ball, is only $\frac{1}{15}$th of a second.

The time which elapsed between the Greenwich current reaching London and the falling of the ball at Liverpool was $\frac{11}{20}$th of a second. In other words, the Algoa ball is discharged from a distance of 500 miles in less than $\frac{1}{7}$th of the time of that of the Liverpool ball.

What is being daily done in the Cape can, however, be best summed up by a short quotation from a letter received from Sir Thomas Maclear, giving an account of the successful inauguration of this time signal. After detailing the general arrangements, Sir Thomas Maclear goes on to state: "A few tentative signals having proved satisfactory, the 'preface' was issued from the Observatory at ten minutes before one o'clock, and at the instant of one o'clock, the Observatory time-ball clock closed the circuit discharging the Observatory ball, the Simon's Town ball, twenty-four miles distant; the Cape Town time gun, three miles distant; and the Port Elizabeth ball, distant 500 miles."

On the present State of the Question relative to Lunar Activity or Quiescence.—W. R. Birt, F.R.A.S. From the time of Schröter, the question of change on the moon's surface has been more or less agitated. The *Selenotopographische Fragmente* contains numerous instances of what he considered to be changes of a temporary character, and a few of a more permanent nature, as the formation of new craters. It is, however, notorious that he failed to establish the fact of a decided change in any one instance; nor is this to be wondered at when we consider the paucity of the materials he had at his command. Notwithstanding the comparative neglect into which the observations recorded in the "Fragments" have fallen, and the judgments passed upon them by some of the best known selenographers, there can be no question that they embody the results of zealous and persevering attention to the moon's surface, and ought not to be passed over in the examination of any given spot, the history of which we are desirous of becoming acquainted with *during the earliest period of descriptive observational selenography.*

The labours of Schröter's successors, Lohrmann, and Beer and Mädler, have added greatly to the number of objects, either as delineated on their maps or referred to in their letter-press. Lohrmann appears to have carefully studied Schröter's results, as we find him quoting the measures obtained by Schröter in several instances. On examining the results of the two greatest selenographical works of the present century, and comparing the one with the other, we find precisely the same kind of phenomena presenting themselves which in a great measure perplexed Schröter; but as Lohrmann and Mädler worked independently of each other, and Mädler evidently had a very low idea of the value of the preceding labours of Schröter, these phenomena passed unnoticed at the time. Upon consulting the three works for elucidating the history of any given object, such results as these are frequently obtained. An object is found in Schröter designated by a Greek or other character, and its appearance described in his text. This object may be altogether omitted by Lohrmann, but given on Beer and Mädler's map, and objects are by no means rare which may be found on Lohrmann, but omitted by Beer and Mädler, and *vice versa*.

Were the results of the labours of Julius Schmidt during a period of nearly thirty years given to the public, there can be no doubt that our knowledge of selenography would be greatly advanced. His chart must contain a large proportion of the objects previously recorded by Schröter, Lohrmann, and Beer and Mädler; and judging from the instances already alluded to, of *apparent omissions* by one or other of the above-named observers, it is highly probable that the number of such instances would be much increased. The value of his measures (4,000) of the altitudes of lunar mountains for comparison with or addition to those of Schröter and Mädler, cannot admit of a doubt. His published catalogue of rills is very valuable in this respect. It is to Schmidt that we are indebted for one of the most important announcements bearing on the subject of lunar activity, that of a change in the crater *Linné*, "which," says Mädler (Reports British Association, 1868, p. 517) "has hitherto offered the only authentic example of an admitted change." He had previously said (same report): "What has lately been observed in the crater *Linné* proves, at all events, that *there* real changes have taken place, and that, too, under circumstances even visible to us." Further on, however, the great selenographer remarks that on the 10th of May, 1867, his eye having undergone an operation for cataract, he attempted an observation of *Linné* in the heliometer of the Observatory at Bonn, and found it shaped exactly, and with the same throw of shadow as he remembered to have seen it in 1831. "The event," he says, "of whatever nature it might have been, must have passed away without leaving any trace observable by me." The doubt still hanging over this object is well known, and it may be regarded as furnishing, at least, one of the instances of the present state of the question of activity. The uncertainty attaching to the question of change in this particular instance mainly arises from the difficulty of deciding upon the accuracy or otherwise of the delineations of Lohrmann and Beer and Mädler, although both describe it as showing a diameter of five or six English miles. Generally speaking, the observations between October 1866 and July 1870, all agree in its present appearance, differing greatly from that which it must have presented according to the delineations and descriptions of the two selenographers just named, also that no change of a physical character has taken place in it during the 3½ years it has been under observation. It has been supposed that photography would solve all such difficulties, and that photograms of the lunar surface taken under similar angles of illumination and visual ray would agree with each other; but here again precisely the same difficulties present themselves which perplexed Schröter, and which have been met with in comparing Lohrmann's and Beer and Mädler's works. Objects figured by the earlier selenographers occur on some photograms, but not on others, of about the same phase of illumination. There appears to be an agency capable of affecting the visibility of objects, rendering them indistinct or invisible on some occasions; while, on others, they are distinctly seen on the photograms. Whatever operations may have taken place in the crater *Linné*, producing phenomena the recurrence of which is *rare*, in all the examples above mentioned, from Schröter's time to the present, we have phenomena of a different character, exceedingly difficult of explanation, and constituting an important element in the solution of the question of present activity or quiescence; for unless it be fully proved that *all* these instances depend upon changes of visual and illuminating angles, a strong suspicion will exist of their being more immediately connected with the moon itself. To effect such a proof, however, is a matter of no small difficulty. Mädler alludes to the performance of calculations of the most varied kind as necessary for the delineation of lunar forms, and in the case before us the calculation of several elements for *each separate observation*, and they are very numerous, is absolutely essential for the purpose of referring the phenomena observed to changes of illumination and visual ray. Calculations of this kind have not been made to any great extent, and the consequence is, that the entire question remains involved in doubt. During the last seventeen months, as many as 1,227 observations of the spots on *Plato* alone have been made, and although the varying state of the earth's atmosphere affects in no slight degree the visibility of such delicate objects, phenomena are presenting themselves which call for a much more rigorous treatment than has yet been accorded to them. The affirmation of change on, or quiescence of, the moon's surface, must depend, not upon the accumulation of desultory and undiscussed observations, but upon such as are undertaken on a well-arranged system, and discussed with reference to every known agency capable of affecting them. The present state of the question is therefore one of *doubt*, one that calls for observation of the most vigorous character, and discussion of the most rigorous nature to settle it. Observation of late has been tending towards a registration of minute detail detected on the moon's surface, but discussion in various ways is behind the requirements of selenography, and until it can keep pace with observation the doubt alluded to above must remain.

Section B.—Chemical Science

On Artificial Alizarine.—Mr. W. H. Perkin, F.R.S. In introducing Mr. Perkin the President said that that gentleman might be regarded as the representative in England of artificial colouring matters, and that the subject to be treated of was one of great importance, both theoretically and in its practical aspects. The author referred to the use of madder and its preparation called garancine, in the production of Turkey red dye, and then traced the history of the investigations of chemist regarding the chemical nature of the colouring matters contained in madder-root. About thirty-nine years ago those investigations commenced, and ever since that time they have been continued by many eminent chemists, among others by Graebe, Liebermann, Schunck, Strecker, Laurent, Anderson, and others, as also those of the author himself. Two colouring compounds had been obtained from madder known as alizarine and purpurine. The exact composition of alizarine had been the subject of much discussion among chemists. From that compound a hydrocarbon derivative had been obtained which is called anthracene, and then from anthracene, as an ingredient of coal-tar and mineral pitch, alizarine had been produced by the action of various chemical agents. Alizarine, thus artificially produced, yields with mordants the same colours on cotton goods as the natural alizarine from madder-root. Mr. Perkin performed a great variety of experiments in order to demonstrate the chemical identity of the artificial and natural alizarine when absolutely pure. He had also, during upwards of twelve months, been engaged in studying the properties of anthracene and its compounds, all of which are very markedly fluorescent. There were many difficulties in the way of obtaining artificial alizarine in large quantities, but they were gradually disappearing. In the discussion which followed, Dr. Schunck, F.R.S., referred to the alleged differences between the natural and the artificial alizarine, and said he had no doubt whatever that the two were identical, and he thought the confusion had arisen from persons examining impure products. The artificial product was generally supplied impure, but the impurities could be separated. He was quite satisfied as to the importance of alizarine, and that it was the only essential dye product of madder. Some years ago he had shown that the finest madder pinks contained nothing but alizarine.

Section C.—Geology

On the Extension of the Coal-fields beneath the newer Formations of England.—Mr. Edward Hull. Having referred to the paper by Sir R. I. Murchison, on the parts of England and Wales in which coal may or may not be looked for, the author expressed his gratification that his own views coincided in the main with those of his chief, especially as regarded the absence of coal in the eastern and portions of the midland counties, now overspread by mesozoic formations. The author showed that there was evidence for believing that the coal measures were originally deposited in two continuous sheets, one to the north, and the other to the south of a ridge of old land formed of Silurian rocks which stretched eastward from Shropshire to the south of the Dudley coal-field. This ridge, or barrier, had never been altogether submerged beneath the waters in which the coal measures were deposited. Towards the north, the boundaries of the coal formation were formed by the Cambro-Silurian rocks of North Wales, the Lake District, and portions of the southern uplands of Scotland. Over the region north of the barrier, the coal measures were deposited in greatest thickness towards the north-west; while over that south of the barrier they were deposited in greatest force in a westerly direction.

At the close of the coal period, disturbances of the strata, resulting from lateral pressure acting in north or south directions, took place over the whole carboniferous area of the north of England, whereby the strata were thrown into a series of folds, the axis of which ranged along approximately east and west lines. These disturbances were accompanied and followed by enormous denudation, by which the coal measures were swept away over large tracts of the north of England, and the northern limits of the Lancashire and Yorkshire coal-fields were determined. As regards the tract south of the central barrier, it was inferred, on the ground of parallelism of direction with the east and west flexures of the north of England, that the northern and southern limits of the South Wales coal-field, the axis of the Mendip Hills, and the easterly bend of the culm-measures of Devonshire, were all referable to the same geological period, *i.e.*, that which intervened between the deposition of the carboniferous and the Permian rocks.

After the deposition of the Permian beds over the inclined and denuded surfaces of the carboniferous rocks, disturbances (accompanied by denundations) occurred along lines nearly at right angles to those of the preceding period, *i.e.*, along north and south lines (approximately). To this epoch the axis of the Permian chain, and all north and south trendings of the strata, were probably to be referred. Some of the results brought about by these movements were the disseverance of the Lancashire and Cheshire from the Yorkshire and Derbyshire coal-fields, the determination of the western limits of the Flintshire and Denbighshire coal-field, the disseverance of the Forest of Dean coal-field from that of South Wales, and the uptilting of the lower carboniferous rocks along the eastern margin of the Somersetshire coal-field beneath the Jurassic formations.

From these considerations it seemed clear to the author that to the intersection of these two systems of disturbances (*i.e.*, the E. & W. with the N. & S.) and the concomitant denudation, the basin-shaped form of nearly all the British coal-fields (sometimes partially concealed by newer formations) might be attributed.

The author then proceeded to show that over these carboniferous basins, the Permian and Triassic rocks were distributed according to a well-defined plan, the Triassic strata thinning away towards the south-east of England; and concluded by discussing the views of Sir R. I. Murchison, Professor Ramsay, and Mr. Godwin-Austen regarding the absence or presence of coal under the cretaceous and tertiary strata of the south of England.

On the History and Affinities of the British Coniferæ.—Mr. W. Carruthers. Having pointed out the great divisions of this natural order, the author traced their appearance and development in the stratified rocks. The *Araucarieæ*, now represented by fifteen species, all confined to the southern hemisphere, made their appearance in the carboniferous period, where at least eight species determined from the wood structure had been found. In the secondary rocks six species had been found based on the cones, and these showed an affinity to the group of modern Araucarias found in the Pacific Islands. The *Pineæ*, a large group chiefly confined to the northern hemisphere, appeared in the Old Red sandstone, as determined by H. Miller; a single species had been determined from wood in the coal; the species greatly increased in the secondary rocks, where several species of cedars had been detected. The *Taxodieæ*, represented among living plants by fifteen species, chiefly from the northern shores of the Pacific, made their appearance in the secondary rocks, one species being abundant in the Stonesfield slate, and were continued by species of Sequoia in cretaceous and Tertiary rocks. The two species from the Gault are associated with pines having the characters peculiar to the species associated with the existing mammoth trees of California. The *Cupresseæ*, represented by the cypresses, and in our native flora by the juniper alone, are known only in Tertiary strata by a few species of fruits and foliage. The *Taxineæ*, containing nearly 100 species, found all over the world and represented in Britain by the yew, made their appearance in the carboniferous rocks, as determined by a fruit described by Dr. Hooker, and shown by him to be nearly related to the living *Salisburia*. The supposed Taxineous wood from the North American Devonians, to which Principal Dawson gave the name of *Prototaxites*, was a remarkable *Alga* of enormous size. Several Taxineous fruits had been found in the Eocene strata at Sheppey.

Notes on Fossil Crustacea.—Mr. H. Woodward. A considerable number of new species was described which had been met with during the past years belonging to strata from the Silurian to the Tertiary. The author expounded the changes in the larva of the living King Crab, and showed the remarkable resemblances between its early condition and the palæozoic *Trilobites*. The earliest known King Crab occurs in the Upper Silurian, so that the pedigree of these two ancient forms dove-tailed into each other in Silurian times, and these contemporaneous forms approached much nearer to each other than would be expected from a comparison of the living King Crab with the Trilobite.

Report on Earthquakes in Scotland.—Dr. Bryce.

On the Tertiary Coal Fields of Southern Chili.—Mr. G. A. Lebour. This was a detailed description of the beds of coal, and those intercalated with them. The list of fossils appeared to Prof. Harkness and Mr. Carruthers to indicate a Secondary rather than a Tertiary age.

Section D.—Biology

Mr. Edward Atkinson, of Leeds, read a paper on the *Osteology of Chlamydophorus truncatus*—a fine male specimen of which had been presented to the Philosophical and Literary Society of Leeds. First glancing at the bibliography of this little quadruped, the author proceeded to draw attention to some points in the structure of its skeleton. The general conformation of the head is very remarkable, differing from all other Edentates in its relative dimensions, excelling its congeners both in altitude and in breadth as compared with length. He also alluded in detail to the structure of the lower jaw, the ear, the scapula, sternum, and pelvis. With regard to the dentition, his observations were not quite in accord with those of Harlan or Hyrtl. *C. truncatus* is a true monodont with eight grinders on either side of both maxilla and mandible. Those of the lower jaw perforate the whole depth of the bone, dimpling the inferior margin. The first tooth of the lower jaw has no opponent, and therefore no masticatory surface. The eighth upper tooth is also without an antagonist, but its analogue in front has a double facet.

Mr. R. McAndrew, F.R.S., presented a report on the *Marine Mollusca of the Gulf of Suez*. This report gives the general result of a dredging excursion to the Gulf of Suez in February and March 1869. Mr. E. Fielding accompanied the author. Leaving Suez on the 10th February in a boat of about twelve tons burthen, with one of about five tons for dredging, and a small boat for landing, the party reached Tur in about three weeks' time. Their crew consisted of Maltese and Neapolitans, an Arab, who proved an excellent diver, and a native of Tur, who acted as pilot. From Tur they crossed over to the Point of Zeite and the desolate islands situated towards the western side of the Straits of Jubal. After working about a week among these, and finding it a very rich collecting ground, they bore away to Ras Mahommed, where they ended their labours, proceeding from this to Tur, from whence they went by land to Suez. The number of species obtained (not including the Nudibranchiates) was 818. Of these 619 have been identified, the remaining being still undetermined. About 355 have not previously been recorded as from the Red Sea. Of these 53 species, including three genera, are new to Science, and have been described by Messrs. H. and A. Adams. Professor Issel, of Genoa, records 640 species as from the Red Sea, and his list includes 100 new species. Some of these were figured but not described in Savigny's "Description de l'Egypt." Mr. McAndrew dwelt on the extraordinary dissimilarity between the Fauna of the Red Sea and that of the Mediterranean; the number of species common to Japan, the Philippines, Australia, and to the Red Sea, is worthy of further observation In addition to the Mollusca, a collection of Echinoderms, Crustacea, and Corals, was made and divided among the British, Edinburgh, and Liverpool Museums. The sponges collected were sent to Dr. Bowerbank, except one, which had been described by Mr. Carter as a new genus under the name of *Grayella.*

Professor Wyville Thomson, F.R.S., read a report on *Some of the Echinoderms of the Expedition of H.M.S. Porcupine.* The impression was very general that through the exertions of Forbes, McAndrew, Jeffreys, and others, the marine fauna of the British Islands was now pretty well known. It was also thought that below a depth of some 300 or 400 fathoms animal life became extinct. Through the investigations of Dr. Carpenter and the author in H.M.S. *Lightning*, and since then by investigations carried on in H.M.S. *Porcupine*, with the additional help of Mr. Jeffreys, not only had the number of new species found been very great, but animal life had been found abundant to the enormous depth of upwards of a mile. Confining himself now to the Echinoderms, he might say that the fauna became not so much a local fauna as one of depth and temperature. All the well-known Scandinavian forms were met with in the "cold area"—such as *Pteraster*, *Euryale*, &c.; while in the "warm area," such wonderful genera as *Pourtalesia* and *Brissingæ*, having possibly its nearest ally in forms found in the Ludlow rock, but also a new soft-bodied genus belonging to the Diademidæ, were met with. All the new forms, embracing both new genera and species, would be described in full in the forthcoming report.

Dr. M'Intosh, F.L.S., read a preliminary report on *Certain Annelids dredged in the expedition of H.M.S. Porcupine*. The specimens were chiefly procured from water under 500 fathoms off the coast of Ireland. They are on the whole of a northern type, many of the rarer having been previously procured by Mr. Jeffreys off the Shetland Islands, and well known in the northern seas generally. There were several new and most interesting species, including a *Sthenelais*—a form allied to *Leanira Malmgreni*, but probably requiring a new genus for its reception; a *Eunice*, *Nothria* and *Chætozone*, the *Antinoe Sarsi* of Kinberg, and the *Petta pusilla* of Malmgren were, besides, added to our fauna. The author tendered his thanks to Professors Carpenter and Wyville Thomson, and more especially to Mr. Gwyn Jeffreys, for their kindness in securing the collection.

Dr. G. W. Child read a Paper on *Protoplasm and the Germ Theory*. Mr. Samuelson read a Paper *On the Controversy on Spontaneous Generation, with new experiments*. In the interesting discussion which followed, the President, Dr. Hooker, Mr. G. Bentham, and Mr. Crace Calvert took part.

Mr. P. L. Sclater read a Paper on *Certain Principles to be observed in the Establishment of a National Museum of Natural History*. [This Paper will be found *in extenso* in another column, with a woodcut. The following is an epitome of the interesting discussion which followed.]

Mr. Wallace entirely agreed with all the main principles advocated by Dr. Sclater, such as the separate government of the Natural History Museum, the association of Palæontology with Zoology, and the separation of the collections into a "typical and a scientific series," both of which should be at all times available; but he differed from him on a point which he considered to be no less important than any of these, viz., as to the mode of arrangement of the specimens which would be most efficient for all the purposes such a museum should fulfil. In a national institution, if any part of it was set apart for the elevation, instruction, and amusement of the public, these purposes should be carried out in the most efficient manner, and this could not be done by the system of wall-cases advocated by Dr. Sclater, and which he (Mr. Wallace) believed to be radically wrong. The objections to these wall-cases were numerous:—

1. They admit of any object being seen by the smallest number of persons at once, so that any one person studying an object, almost necessarily monopolises it, and prevents others from approaching it, an inconvenience that reaches its maximum in the recessed cases exhibited in Dr. Sclater's plan.

2. Objects in wall-cases can be seen only on *one* side, which, as *all* sides of natural objects require to be seen, would necessitate many specimens to do the duty of one.

3. The observer on the one side, from which alone he can see an object, will generally stand in his own light, and will often have distinct vision further impaired by reflection from the glass.

4. When small objects occur alternately with large ones, a great waste of space occurs, and the attention is distracted from the less conspicuous object.

5. The use of wall-cases on one side of a gallery for an entire museum, is an expensive and wasteful mode of arrangement.

Objections (1) (2) and (3) are of the greatest importance. A public national museum must accommodate the thousands who throng to it on holidays, when alone the working classes can reap its benefits; and they should be invited and induced to examine and study, not merely to gaze and pass on. Teachers and parents should be able to give information as to the groups exhibited without interfering with other visitors, none of which things are possible with a range of wall-cases. The system advocated by Mr. Wallace was that of detached cases on tables or on the floor, of various sizes, and each exhibiting one typical object or group of objects, capable of being seen on *all sides*, and admitting of convenient examination in the best light by the *greatest number* of persons at once. The system had been adopted in a new museum at the India House, and at South Kensington, and was advocated by Dr. Gray, and partially exemplified in the great gorilla case, the groups of birds of paradise, and other detached cases in the British Museum. The numerous and very great advantages of this system should not be lost for the sake of an infinitesimal increase of convenience to scientific men. The great majority of specimens exhibited in the public galleries would consist of *common species*, of which an ample series of specimens would be preserved in the scientific collection for study. Of the few rare species which it might be advisable to exhibit to the public, perhaps not more than one a week would be required for scientific examination, and all such might be so mounted as to be easily brought into the students' room, adjacent to the gallery, when required. The man of science would thus *lose nothing*, while the public would gain incalculably; and so greatly was Mr. Wallace impressed with the educational superiority of one mode of arrangement over the other, that he believed it would be better to have the very rare and unique species represented by drawings or models only in the public department, rather than have the whole collection arranged in wall-cases, for the one purpose of allowing the scientific man to get them out more easily on the rare occasions when he required them.

Prof. Archer, of Edinburgh, said: However some of us may differ from Dr. Sclater in his opinions about the arrangements of the contemplated National Museum of Natural History, none of us will, in the slightest degree, differ from him in his belief that this is a subject of paramount importance. I am compelled to say that I do not agree with him as to his arrangement of wall-cases and back entrances, for some considerable experience has convinced me that unless under some peculiar circumstances, as in narrow galleries where there is too little space for detached cases, wall-cases are entirely a mistake. In this respect my own personal experience perfectly coincides with the opinions of Mr. Wallace, but Mr. Wallace has even underrated the advantages of the system he advocates, for he has only indicated by his diagrammatic illustrations a series of cases similar in size, placed at equal distances. But at South Kensington, where the question of constructing cases best adapted for the display of objects in a Museum, has received a greater amount of intelligent attention than in any other museum, they have shown that you can make cases which will admit of a perfectly symmetrical arrangement, and yet be of various sizes, so that small objects as well as large ones may be so exhibited as to permit of their being examined from all sides, instead of from only one point of view as in wall cases. Wall space is valuable for illustrations, especially pictorial ones, but when you arrange groups of animals in them, it is certain that if they are tolerably suitable for the exhibition of large specimens they cannot be equally fitted for small ones. There is one other point in which I cannot agree with the author of the paper, and that is, in the line he draws between the requirements of a Public Museum and one for the use of students in natural history. My own views are to exhibit as much as you can without injury to the specimens, because you never know what portion of your visitors are earnest students or pleasure-seeking idlers; and still further, you do not know how soon this class may be converted into the former.

Prof. Newton thought that being connected with a museum which was emphatically "national," he should be wanting in his duty if he did not express his general agreement with the principles laid down in Mr. Sclater's paper. What might be called the "structural" part of this very important question had been dwelt upon by previous speakers, but there was another part on which they had scarcely touched. This was the constitution of the governing body and officials of the New Museum. First it had been stated in the paper (and the statement was true) that of the fifty trustees of the British Museum only two or three were scientific men. That the museum was what it was, reflected, then, the greatest credit on the energy of those two or three. But care must be taken that the museum of the future, whether sent to South Kensington or kept in Bloomsbury, should be relieved of the burden of the Trustees; it was essential that their authority should cease, and that scientific authority alone should be supreme. Secondly, with regard to the mode of appointment of the officials—that was a matter for great deliberation. He believed that the system adopted a few years ago had not yet had time to produce all the mischievous results which would follow if it were persevered in; but it was clear to him that in future they should have nothing to do with competitive examinations and Civil Service commissioners, in appointing assistants to the different departments, and he would prefer, as was suggested in the paper, that appointments should be made by the mild despotism of the director or superintendent of the museum.

Department of Zoology and Botany

Dr. B. W. Richardson read the *Report on Methyl Compounds.* He commenced his report by giving a review of some results of his previous reports, describing at length the action of nitrite of amyl and hydrate of chloral, both of which had proved of the greatest service in the treatment of disease. The former had been applied most usefully in the treatment of tetanus; the latter had been so largely applied as a narcotic, that since the discovery of its narcotic properties by Liebreich, more than a million persons had been successfully subjected to its influence. After his review of the past, the author brought forward new matter of research, introducing detailed accounts of the action of ethylate of sodium, ethylate of potassium, sulphur alcohol, sulphide of ethyl, bromide of ethyl, and triethylic ether. The facts respecting the action of these substances were all rich in interest, but two may be named specially, viz., in relation to the ethylates of sodium and potassium, and to triethylic ether. The first, when brought into contact with the surface of the body, acts as the most potent of known caustics, and promises to be rendered painless as well as caustic. The second is a new volatile anæsthetic, the sleep produced by which is deep, gentle, and apparently free from danger. In a final part of his report, Dr. Richardson dwelt on some general physiological observations, which attracted considerable attention. He showed that by the action of some of his anæsthetics, he could induce insensibility to pain without fully destroying consciousness; and he explained that in time this progressive step would be entirely realised. He described the effect produced by repeating applications of volatile agents upon the external nerves' expanses; and on the results of direct experiment, he explained that certain agents, such as nitrite of amyl, act immediately through the nervous system without any absorption of them by the blood. At the close of his report, Dr. Richardson showed how the elementary modification of the bodies of an organic series influences the physiological action of each compound, and expressed a hope that, by continued research, physiologists, moving with the chemists, would speedily bring the subject of the action of medicinal agents into the ranks of positive science.

Dr. Brown-Sequard read two papers on the *Apparent transmission of abnormal conditions due to accidental causes, and on various alterations of Nutrition due to Nervous Influence.*

The President of the Association (Professor Huxley) said: The great theoretical problem they had now to determine was what effect artificial modifications and external conditions had upon living organisms—whether they produced changes which, being transmitted hereditarily, became the basis of new races. Referring to a resolution which had been brought forward at a former meeting, which endeavoured to pledge the Association to abstain from making grants of money to persons engaged in experiments which involved vivisection, he said they had before them that day one of the most experienced physiologists and vivisectors of his day, and he had only to ask the audience to form their own judgment as to whether Dr. Brown-Sequard was likely to inflict one particle of pain upon any creature whatever without having a plain and definite purpose in view. For himself he might say that nothing was more grievous to him than to think of the existence of pain in anything whatever. He hated to see it inflicted upon animals, and he carried his objection to its infliction so far that he disliked even to see a man beating his wife. Neither Dr. Brown-Sequard nor himself were indifferent to pain, and he hoped that in no sense were they cruel. He thought that the gentleman who brought forward the resolution to which he had referred, hardly knew what he was dealing with. If his friend Dr. Brown-Sequard would pardon his referring to a matter personal to him, he would remind the meeting that that great experimental physiologist, and that accomplished vivisector, who had, he supposed, performed as many vivisections as any man in the world, some years ago thought it advisable to turn the vast knowledge of the diagnosis of disease which he had obtained by this means into actual practice, and he (Professor Huxley) could assure them, from what he knew, that before long his wonderful mastery over symptoms caused his consulting rooms to be absolutely crowded by human beings suffering under multiform varieties of nervous disorders, who sought at his hands and from his knowledge that relief which they could not obtain elsewhere. The prevention of cruelty to animals, when understood in its proper sense, was as good an object as men could devote themselves to, but when they confounded the brutal violence of the carter or the wife-beater with an experiment carried out by a man of science, gently and for the purpose of relieving misery, the enthusiasts in that cause should change their name, and convert themselves into a society for the promotion of cruelty to mankind. If that question came before the Association again, and he hoped it would, he trusted they would recollect that the order of nature was such that certain kinds of truth were only attainable by experiments upon living animals, and that when they might result to the welfare of thousands and thousands of untold human beings who might otherwise be suffering unimaginable misery, those experiments were perfectly justifiable.

Dr. R. McDonnell, F.R.S., of Dublin, said that the President of the Association had viewed the admirable communication of Dr. Brown-Sequard from the Darwinian point of view, one of the greatest interest. He, like Professor Humphry of Cambridge, regarded such communications rather in their practical bearings, but first he might be allowed to say, how entirely he concurred with the President in his observations on the subject of experimental researches conducted upon animals. Indeed Dr. Richardson's report was in itself the most unanswerable argument that such experiments are undertaken with the hope of diminishing human suffering, and whosoever would oppose such an important and indeed successful means of attaining this end must be prepared to submit to the imputation of desiring that pain should remain unalleviated. Dr. McDonnell then alluded to the subject referred to both by Dr. Richardson and Dr. Brown-Sequard in speaking of the transmission along the nerves of certain sensations, and their being intercepted. He said that he had long felt some difficulty about adopting the hypothesis of Dr. Brown-Sequard that there existed distinct conductors for various sensations, as those of heat, pain, tickling, contact, &c. In explanation of the remarkable cases sometimes met with in which an individual who felt perfectly the contact of one's hand yet could not distinguish heat or cold, he proposed another hypothesis than that of distinct conductors, and he was indeed happy, on this occasion, to have an opportunity of submitting this hypothesis to the section and to Dr. Brown-Sequard for consideration. His (Dr. McDonnell's) hypothesis was, in fact, an application of the undulatory hypothesis to the propagation of nervous sensation—he supposed that sensations such as those of heat, pain, contact, as well as those of various colours, of form, of sound, were waves of different wave-lengths; and that, under certain circumstances, some waves were absorbed or intercepted while others passed on to the sensorium. He, in fact, drew an analogy or illustration of his hypothesis from Prof. Tyndall's well-known experiments on the absorption of radiant heat by vapours or scents passed into the air filling a glass tube. The glass tube in this experiment represented the nerve tubule, the slight change effected in the air contained within it produced by the introduction of the minutest quantity of scent causes an absorption *in transitu* of some waves of heat, others pass; thus, according to his supposition, might be explained the effect on vision of santonine. The experiment of seeing the complementary colour upon gazing at a white ground after looking upon a coloured disc, might be explained thus: A slight chemical change is effected in the nerve tubule by gazing at the coloured disc; when the white ground is looked upon, all undulations pass through *save* those which are absorbed, viz., those of the colour previously looked at. This, of course, gives the complementary colour. Many phenomena connected with sensation, Dr. McDonnell conceived, would find in this hypothesis a simpler explanation than in that of distinct conductors.

Department of Ethnology and Anthropology

On the Anthropology of Lancashire.—Dr. Beddoe, President of the Anthropological Society of London. The author drew a marked distinction between the inhabitants of North and South Lancashire, both as to their ethnological history and their present physical characteristics. In the former, he believed the Norse element to preponderate, having been introduced, probably, by colonisation from the Isle of

Man and even from Dublin. The people were still tall and fair, and often strikingly Scandinavian in aspect. The remaining British element might be partly Gaelic. In the south of the county, immigration and physical degeneration connected with the great development of the cotton manufacture, had been, and were, effecting changes in the prevailing physical type, which had previously been more Anglian and British, while the Norse element had been comparatively weak. The paper was partly based on numerical data.

On the Ottoman Turks.—Dr. Beddoe. This paper mainly consisted of a minute physical description of the Ottomans of Anatolia, with notices of certain tribes of Yuruks and Turkomans scattered about Asia Minor. The physical type, which for brevity's sake, he called Turanian, was much more prevalent among the former than was generally supposed. It was doubtful whether there was any need for invoking the influence of climate or other *media* to account for the elevation that had occurred in the Ottoman physique. Inter-marriage with the women of subjected races soon after the conquest, and absorption of foreign elements, might sufficiently account for it, and as these had been most prevalent in Rumelia, and in the large towns, it was there that the original Turanian type had been most obscured.

Mr. John S. Phené read a paper on *A recent examination of British Tumuli and Monuments in the Hebrides, and on the western coast of Scotland.*

On the Builders of the Megalithic Monuments in Britain.—Mr. A. S. Lewis. The author divided the inhabitants of Britain into three leading types, the Kymric, long-headed, dark-haired, and light-eyed; the Iberian, dark-eyed and dark-haired; and the Teutonic, round-headed, light-haired, and light-eyed. He controverted the idea now gaining ground that the Iberians represented the aboriginal race, and that they exclusively were the builders of megalithic monuments. He attributed these monuments to Iberians and Kymry indifferently, and believed the latter race to have come to Britain before the former. These views he supported by, among other arguments, a careful consideration of the statistics of the physical characteristics of the inhabitants of Great Britain, collected by Dr. Beddoe, President of the Anthropological Society of London, from which he showed that the Iberians were found in the largest numbers in the southern part of the island, while the monuments were found throughout it, and this distribution of races seemed also to show that the Iberians were a later arrival than the Kymry. Mr. Lewis stated, however, that the statistics were not sufficiently numerous to be absolutely conclusive, and appealed to the members of the Association to assist in collecting further statistics of the physical characteristics of the inhabitants of their own districts.

On the Massagetæ and Sacæ.—Mr. H. H. Howorth. Relying upon the Chinese authorities translated by Stanislas Julien and V. St. Martin, the author identified the Massagetæ with the Ta Yuetchi or Great Yuetchi, and the Sacæ with the Sse or Szu of the Chinese authors. A close criticism of all the information about the Massagetæ and Sacæ furnished by the Greeks, enabled us to say that they were two names for one race, or at most for two branches of one race, Massagetæ being probably the native form, and Sacæ its Persian equivalent. Western writers throw little light on what this race was. The Chinese authors prove that it was a branch of the Thibetan race, called by them the Khiang, which was predominant in Central Asia before the aggrandisement of the Turks in the sixth century. The same authors enable us to connect the Massagetæ and Sacæ with the Indo-Scyths who overthrew Bactria and the Greek civilisation of Asia in the second century, B.C. Sacæ is equivalent to the Sah and Saka of the Indian Epics, and to the more Western Scyth, and in the cuneiform inscriptions is the Aryan substitute for the Semitic Gimiri, the Cimmerii of Herodotus. These facts enable us to destroy the old nonsense about the Sacæ and Saxons, the Massagetæ and Goths, the Cimmerian and Welsh, having been related to one another. Sacæ, Massagetæ, and Cimmerii were all Turanians and in fact Thibetans.

Mr. Dendy read a paper on *Shadows of Genius.*

On the Racial Aspects of Music.—Mr. Kaines. The author drew attention to the settled melancholy which pervaded the music of the north of Europe—a characteristic not observable in the music of the south of Europe, or of the other people of the globe. He endeavoured to account for this physically and practically, and showed there were vast differences in the temperaments in the peoples in the north and south of Europe. Of the one it might be said "melancholy marked it for her own," while cheeriness and brightness marked the other. The first seemed saddened by the mysteries of life, death, God, and immortality. Mr. Kaines noticed briefly the great religious revolution which had taken place in Europe, and how it had (probably) powerfully influenced its music. Protestantism broke the spell under which the human intellect was bound by Roman Catholicism, and enlarged the sphere o man's knowledge only to show him how much there was that he could never know. Catholicism, in engaging to answer all the intellectual and moral needs of man, took from him responsibility, and gave him a restfulness to which Protestantism is a stranger. The change from the old to the new (or rather revised) faith, had not been without its effect on music, and the emotional cravings and wild unrest which characterised the music of our times, might be attributable to this cause.

A long and interesting discussion ensued, in which Mr. James Smith, Dr. O'Callaghan, the Rev. Mr. Owen, Dr. Evans, Dr. Hitchman, and others took part.

Section E.—Geography

Mr. Winwood Reade read a paper on the *Upper Waters of the Niger*, and as we understand that he will shortly read a similar communication before the Geographical Society, a brief abstract of his paper will be sufficient for the present. Last year Mr. Reade made an exploring journey from Sierra Leone to the Niger, and visited the gold mines of Bouré, a country mentioned by many travellers, but which he has been the first to reach. Leaving Sierra Leone on January, he went to Falaba, as Major Laing had done before him fifty years ago, though by a different route. Like Major Laing, he was detained at Falaba, and not permitted to pass that important town. He returned to Sierra Leone, bringing with him messengers from the King of Falaba to the Governor of Sierra Leone, and these, grateful for the kindness and liberality with which they had been treated, promised Mr. Reade that he should be allowed to pass Falaba if ever he should visit them again. He determined to go back with them at once. The promise was kept; Falaba is only fifty miles distant from the Niger or Toliba (great river), and within a month after leaving Sierra Leone he reached that river, which has now, in its western course, been touched by explorers at three distinct points: by Mungo Park, at Segou, in 1796; by Caillié, at Couroussa, in 1828; and in 1869 by Readin, at Farabana, where the river is only a hundred yards broad. The author of the paper claims to have discovered the most direce and the shortest route to the Western Niger. Without presuming to compare himself with such giants in travel as Park and Caillié, he pointed out that while Caillié had not been able to reach the Niger under two months, nor Park (nor subsequently the followers in his footsteps, Dochard in '21 and Mage in '64) under four months, he had reached it in one month. Mr. Reade expressed his thanks to Mr. Swanzy, who had borne the expenses of his two years' African travel; to Mr. Heddle, a merchant at Sierra Leone; and to the Governor-in-Chief, Sir A. Kennedy.

Sir H. Barkly, K.C.B., who was in the chair, having thanked Mr. Reade for his paper, Mr. F. Galton made some interesting remarks on the Niger, and said that the discovery of its being only 250 miles from Sierra Leone would, without doubt, have an important influence on the political future of that colony. Lord Houghton asked why nothing had been done by the Sierra Leone Government during the last fifty years to explore the country lying interior of their colony. Mr. Reade said that he was unable to answer that question, but perhaps the extreme difficulty of getting through the coast tribes had something to do with it; as he had explained in his papers it cost him two journeys to make the insignificant distance of 250 miles. We may explain to those who follow these abstracts with their maps that the position of Falaba is correct; and that Bouré or Buri (which is a country, not a town) is approximately correct, as laid down by Caillié, who passed near it. But the tract of country between Falaba and Caillié's first position on the Niger (Couroussa) must be mapped afresh. Mr. Reade does not intend to alter the position of the Niger's source, as laid down by Major Laing from native information obtained by him at Falaba. He was prevented by the wars constantly prevailing in that region from visiting the source, but the information which he collected respecting its position confirms in all essential particulars that obtained in 1822 by Major Laing.

Section F.—Economic Science and Statistics

On the Aptitude of North American Indians for Agriculture.—James Heywood, M.A., F.R.S. Indian Reservations in Canada are under the control of the Secretary of State at Ottawa. Mr. W. Spragge, Deputy-Superintendent of Indian Affairs, presents annually to the Secretary of State a report on the Canadian settlements of Indians. The Six Nations Indians in the Tuscarora reserve, near Brantford, on Grand River, in the province of Ontario, form the most important settlement of aborigines in Canada. Their reservation comprises 55,000 acres, surrounded on all sides by thriving communities of white settlers. The Indian population of this reserve amounts to about 3,000 persons, including 2,800 of the Six Nations, and about 200 of the Mississaguas, or Ojibbeways, located near the river New Credit, at the southern extremity of the Tuscarora reserve. According to a report of Commissioners, appointed by Sir Edmund Head, Governor-General of Canada, in 1856, the Six Nations Indians were settled in the Tuscarora reserve, by Mr. Thorburn, the Commissioner, in "farm lots, averaging 100 acres each by actual survey." The total clearing of the Tuscarora reserve "amounted in 1856, to 7348 acres, more than half of which had been done by the Indians themselves, the remainder having been chopped by squatters, who had been removed from the land." "Most of these squatters were compensated for their improvements to the amount of more than 8,000*l.*, paid from the funds of the Six Nations Indians." The Commissioners of 1856 report that the Six Nations Indians cultivate on their reserve "separate farms, and each is secure in his possession from the other Indians on the lot he occupies. His heirs inherit his improvements, but the soil belongs to the Six Nations in common. The Indian has no right of transferring his portion of land to another. The revenue of the Six Nations Indians amounts to 39,489 dollars annually." Besides the two Schools in the New Credit district, maintained by the Indian bands of that locality, there are in the portion of the Tuscarora reserve inhabited by the Six Nations, eight Schools, principally supported by the New England Company, a London corporation, formed under the Commonwealth, whose funds are devoted to the extension of civilisation and Christianity among the aborigines in British Colonies, and especially in Canada. Mr. Henry Lister, a member of the New England Company, visited the Tuscarora reserve in 1868, and reported of the Six Nations Indians that their chief crops were "wheat, Indian corn, oats, and hay." Most of the Indian houses in this reserve, Mr. Lister described as "cottages of one or two rooms, built of boards or logs, and usually heated by a stove. There is not a single village," Mr. Lister remarks, "on the reserve; each house stands in its own lot of about 50 acres." An agricultural society was formed in 1868, among the Six Nations Indians of the Grand River, at an annual subscription of one dollar (about four shillings), for each member, and their first show was held on the 15th of October, 1868, on a farm within the reserve. The policy hitherto pursued in Canada, with regard to Indians, has been to induce them by means of small annuities to remain, to a great extent, as residents in the Indian reservations of the Dominion to which their lands or settlements may respectively belong. According to the Rev. Edward R. Roberts, missionary to the New England Company at Chemong, near Peterborough, in Canada, the province of Ontario was "divided into districts, with reference to the Indians. The land of each district was valued at a certain rate per acre, and the interest of the aggregate sum was paid half-yearly to the Indians included in that district, which constituted their annuity. And, in addition, each band of Indians had a reserve of land in a particular locality for their settlement. The aggregate annuity of the several bands," Mr. Roberts observes, "remains the same, whatever changes by death, birth, or emigration may take place. If a band of Indians becomes less in number, those who remain receive proportionably more annuity. While, however, an individual Indian (or family) ceases to receive his annuity from the fund appropriated to the band he leaves, he may be received into another band, by application, and a vote of the people; but as such an accession to their numbers diminishes their individual annuity by allowing others to share it, an application of this sort is seldom acceded to, as might be expected."

Section G.—Mechanical Science

On the Extent to which existing Works and Practice militate against the profitable Utilisation of Sewage.—Mr. J. Bailey Denton, M. Inst. C.E. The author stated that, notwithstanding the great amount of attention devoted by chemists and other scientific persons during the last twenty-five years to the treatment of sewage, the general opinion arrived at now is that the refuse of towns can only be made to give up its fertilising elements by transporting it direct to the land either by the agency of matter or earth. In support of this view he made two quotations from the reports prepared by the Rivers' Pollution Committee of Inquiry. It is generally admitted that wherever people are congregated, and a number of dwellings exist together, it is not possible to provide for the largely increasing use of water, by a population doubling itself within the period of fifty years, without underground conduits for the discharge of liquid sewage. In nearly all our cities and large towns systematic sewage already exists. In the midland and southern towns water-closets are comparatively numerous, though privies with cess-pools still predominate, but in the northern towns water-closets are comparatively few, and the middens nearly universal. After mentioning various instances in which there is infiltration of subsoil water into the sewers, doing mischief in a variety of ways, the author called attention to the evil of indiscriminately admitting a largely disproportionate quantity of water into the sewers, without any power to regulate the time and extent of dilution. Assuming, with the Rivers Pollution Commissioners, that sewage must be utilised upon the land by the process of irrigation, Mr. Denton proceeded at some length to consider the conditions which should be observed in order to obtain the maximum amount of benefit from sewage farms. He concluded by saying, "With a sewage farm naturally or artificially drained, and the surface sloped so as to make the absorption and filtration of sewage certain; intermittent filtration may be practised by itself at any time when it is desirable to resort to it independently of irrigation. At seasons when the sewage may be applied profitably to vegetation, of course the two processes will proceed together; but it will only be by operations admitting alike of combined or separate action that purification and profit may be secured free from all chance of malaria. With the prospect of applying the sewage of towns extensively to land by way of irrigation, it is most desirable that the proper preparation of land to receive it should be indisputably understood and acted upon.

SOCIETIES AND ACADEMIES

Paris

Academy of Sciences, Sept. 12.—M. Faye communicated a note on the mode of observing the approaching transit of Venus, in which, after giving some account of Mr. Newcomb's memoir on the same subject, he suggested an application of photography by means of electrical apparatus. M. Faye also presented a note on the chemical agents to be employed in opposition to miasmatic infection, in which he remarks upon the application of the phenic compounds to this purpose.—M. Dumas and M. Chevreul made some observations on the subject of this paper.—A letter was read from M. Sédillot on the surgical indications and the consequences of amputations in connection with wounds.—M. C. Bernard presented a note by M. Rabuteau, on the means of annulling the effects of insufficient alimentation. The author described the effects produced by Coffee in diminishing the waste of material in vital operations, and maintains that by the free use of coffee life may be supported in full activity with much less than the theoretical amount of nourishment. Cocoa and Tea partake of the same qualities.

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THURSDAY, OCTOBER 13, 1870

NATURAL HISTORY SOCIETIES

I.

EXISTING Natural History Societies, or Field Clubs, may be divided into two classes: those which have already a definite scientific position, and aim especially at working out the flora and fauna of their country or district; and those which have for their object the popularising of the various branches of Natural Science, always with due regard to scientific exactness. The first consist chiefly of professed naturalists; the second of intelligent persons who have some desire to gain a little insight into the wonders with which they are surrounded. Some societies combine the two; and these are, perhaps, the most useful of the three classes. Although taking a somewhat lower tone, our second class is to the full as important as the more advanced one; and if by its means our fellow-countrymen obtain even a slight knowledge of some branch of Natural History, something will have been done to diminish in some measure the mass of ignorance on matters connected with Natural Science which still prevails among educated people. Apropos of this, we may mention one instance which has lately come to our knowledge. Some people, of average education and intellect, who had resided for fifteen years in a country district, had brought to them a full-grown larva of the Privet Hawk-moth. Having no idea of its nature,—although one suggested that it was a "locust!"—but a sort of dread of its mysterious appearance, and of the horn on its tail, a council of war was held, and, it being considered too large to "squash," the unfortunate creature was forthwith placed in a pail of water, where it remained for eight or ten hours. At the end of this time a naturalist intervened, and the caterpillar was rescued from its bath; when, strange to relate, it positively lived for three or four days, but died while passing into the state of pupa.

Having had some practical experience in the working of more than one local society, and somewhat special opportunities for becoming acquainted with the *modus operandi* pursued by others, a few notes on some of the more salient points which they present may be of some interest, and possibly of some use, to those who purpose taking part in establishing such a body. On the present occasion, we will confine ourselves to those societies which, either from the long period for which they have been established, or from other favouring circumstances, have attained a definite position, and are chiefly maintained by experienced naturalists, deferring for a future paper some hints and suggestions on the formation of less pretentious bodies.

One of the most important duties which devolves upon a society or field club possessing a fair proportion of working members, is the investigation of the flora and fauna of the district in which it is placed. Agreeing, as all will do, with Linnæus, that "turpe est in patriâ vivere, et patriam ignorare," it is evident that it is mainly through the agency of such bodies that a knowledge of the Natural History of the country generally is to be obtained. For such purposes there is no plan so satisfactory as that which is termed "working by sections." On this system, the society is divided into a number of smaller bodies, each having for its object the investigation and reporting upon some one branch, and consisting exclusively of those who are able and willing to devote both time and attention to the subject. Each section has its chairman and secretary, and holds its meetings independently of the remainder of the society. This plan has two advantages—only those who really *work* will undertake to join a section, and their meetings are kept free from any hindrance arising from the "drones," who, it must be confessed, are to be found more or less in every society, great or small. Besides, the three or four members forming each section can always conveniently meet at each other's houses, a proceeding which could not be so readily carried out if the whole number were assembled for each meeting; and as the societies which have the advantage of a room specially retained for meetings are but few in number, this consideration is not unimportant.

The thorough and convenient investigation of the Natural History of a district, though an important, is not the only duty of a field club; the same body may be of yet greater service in aiding in the publication of the facts which have been collected by its members. In this manner local societies have already done good service to science; thus, in botany alone, we owe to the Tyneside Naturalists' Club, Mr. J. G. Baker's valuable "Flora of Northumberland and Durham;" to the Holmesdale Naturalists' Club, Mr. Brewer's "Flora of Surrey;" to the Worcestershire Field Club, Mr. Lees' "Botany of Worcestershire;" while of similar works in preparation we may mention the "Flora of Herefordshire," by the Woolhope Club (of which Part 1 is already published); that of East Kent, by the East Kent Natural History Society; of Folkestone, by the Folkestone Society; of Berkshire (a much needed contribution to British botany), by the Newbury District Field Club; and many more. The Tyneside naturalists certainly stand first in the value and importance of their published proceedings, which, especially since their union with the Natural History Society of Northumberland, Durham, and Newcastle, have attained a scientific position which renders them indispensable to those who would obtain a complete knowledge of the Natural History of the country at large. As a proof that local matters are not neglected in these volumes, catalogues of the Lepidoptera, Mollusca, Zoophyta, recent Foraminifera, and Fossils, have been published in them, and also issued separately at a moderate cost; and the last volume contains a paper on the "Crustacean Fauna of the Salt Marshes," and a "Catalogue of the Aculeate Hymenoptera," of the two counties. We have been thus particular in commenting on these transactions, as they appear to us to afford a very good example of what the publications of the higher class of field clubs ought to be: essentially local, yet at the same time of sufficient general interest to be really valuable contributions to the Natural History of England. Second only to them in importance are the "Proceedings of the Berwickshire Naturalists' Club," of which the sixth volume is now in progress. This society is of especial interest as being the first local field club established in the kingdom.

Another advantage attending the publication of local floras by field clubs, is that by their means the expenses attendant upon such works fall less heavily upon those who undertake their compilation, and a fair number of subscribers is more readily obtained. Although much has already been done in publishing such works, there is still much remaining to be done; we could wish, for example, that the Manchester naturalists would publish a new and more complete edition of the "Manchester Flora," and that the Liverpool people would issue in a separate and completed form the flora which appeared in part in their journal. These floras, with that of Birmingham, if carefully worked out, with full references to the older writers, would be of a value second only to that of Trimen and Dyer's "Flora of Middlesex," as showing the influence of cultivation upon the botany of a district.

We must not dismiss the subject of publications without a reference to one or two of those emanating from more recently established societies, which have been favourably received. The Woolhope Club has now issued four volumes, copiously illustrated with photographs and coloured lithographs, the contents of which are of somewhat more general interest than those we have already referred to. This being the case, we can but regret that the volumes are inaccessible to the general public; but a limited number only are printed, which are almost confined in circulation to the members of the club. Noteworthy papers are those on the remarkable trees of Herefordshire (adorned with some exquisite photographs), on the fungi of the county, and on its geology; while the antiquarian will find some prominence given to archæology. The Malvern Naturalists' transactions are similar in general features to those of the Woolhope Club, and we observe that the two societies vie with each other in the attention they bestow upon edible fungi. The Birmingham Society has just issued the first volume of its proceedings—a very creditable one—which has the additional advantage of being obtainable by outsiders at the moderate cost of half-a-crown. We need not remark further upon this, as a notice of it lately appeared in our columns.

The general arrangement of meetings, &c., must depend almost entirely upon local circumstances; and the same remark will apply to rules, which should be as few and simple as possible. As we are now speaking only of firmly established bodies, any hints upon these matters are deferred for a second paper.

A WORD ABOUT YALE

THE following account of the Yale University scheme, by Professor J. D. Dana, is taken from the *Yale College Courant*:—

1. The Classical or Academic and the Scientific departments (ordinarily called Yale College, and the Sheffield School of Science) are distinct colleges for the undergraduate students of the University—distinct in teachers, scholars, buildings, apparatus, and special working libraries. They have in common a general library, and the officers meet for the discussion of University questions in a common University Faculty.

2. In each college the first two years of the four* are years of preparatory study without optional or elective courses, except perhaps in place of the higher mathematics of the second year. After the close of the second year a number of elective courses are before the student.

3. In the Academic College—whose special subjects of study are the classics, modern languages, mathematics, astronomy, history, intellectual and moral philosophy, political economy, general literature, &c.—the principles of natural science, physics, and chemistry are taught so far as is necessary to give depth and breadth to an academic education; a general knowledge of the laws or systems of nature, both organic and inorganic, being essential in these days to a true scholar, whatever his purpose in life.

4. In the Scientific College—whose special subjects of study are the various natural sciences, physics, chemistry, mathematics, and the practical applications of these sciences—literary subjects are added, including modern languages (some knowledge of the ancient languages being required for entering), political, moral, and intellectual science, history, physical and political geography, &c.—in order to give in this branch of the University a thorough and well-grounded education, and make the graduate a man of high culture.

5. In the Academic College, optional or elective courses are confined to its special subjects of study: (1) the classics, (2) modern languages, (3) English language and literature, (4) mathematics. None are allowed in the departments of natural, chemical, or physical science, as these subjects are admitted into this college only so far as is necessary to give that breadth and depth to education which every graduate should have.

6. In the Scientific College also, elective courses are confined to its special objects of study—that is, to the natural sciences, physics, geology, metallurgy, mechanics, engineering, &c.

7. The post-graduate courses of the University comprise many distinct departments in the lines of the Academic and Scientific Colleges. Connected with the former, there are (or may be) courses in Latin, Greek, different Oriental languages, linguistics, English language and literature, history, intellectual philosophy, mathematics, astronomy, &c. &c. Connected with the latter there are (or may be): First, in *pure science*, courses severally in the different physical sciences, general chemistry, organic chemistry, mineralogy, botany, zoology, palæontology, geology, mathematics, astronomy, &c.; Secondly, in *applied science*, courses in civil engineering, mechanical engineering, mining and mining engineering, practical mechanics, metallurgy, agriculture and agricultural chemistry, &c.

8. The students of the Academic College take, on graduating, the degree of Bachelor of Arts; and those of the Scientific College that of Bachelor of Philosophy.

The students of the post-graduate courses, after two years of study, in which high scholarship is attained as tested by a rigid examination, take the degree of Doctor of Philosophy; except in the case of students in Civil Engineering, who may receive that of Civil Engineer after one year of study.

The University includes also the Schools of Law, Medicine, Theology, and the Fine Arts. But of these it is not necessary here to speak. Neither of them has, in any part of its curriculum, an undergraduate department analogous to that of the Academic or Scientific College.

* The three-years' course of the Scientific School will probably be made a four-years' course within a year or two.

In connection with the above brief statement I offer the following remarks:—

1. The ranges of studies in the two colleges, the Academic and Scientific, are so diverse in character, that the interests of the students and of education are better subserved by two distinct faculties working separately, than by one single combined faculty. There is not in the Yale scheme that multiplicity of optionals before the students after they have entered the University, which inconveniently subdivides classes, offers inducements to indolence, and tends to break down thorough discipline and study; for, in the act of entering, the student decides as to the range of his optionals; and if afterwards not satisfied (which would seldom be the case) he can join the other college.

2. It might be supposed that the scheme would require an unnecessary duplication of professors. But this is not so at Yale. In the Academic College there are already four instructors in Greek, four in Latin, five in mathematics, physics, and astronomy; and the professors of rhetoric, history, moral and intellectual philosophy, &c., are more than well occupied with their academic labours. The scientific students, if embraced in the Academic College, would actually require as many additional instructors as are needed under the existing system of the University.

3. In some scientific departments in the Academic College (zoology and botany, for example), in which the instruction occupies but a small part of the college course, there is no objection to employing the services of some of the scientific faculty, if this is feasible; and, where possible, the academic faculty may serve the Scientific College. Moreover, while all lecture-rooms had better be separate, the more costly kinds of apparatus may well be used in common, in order to avoid needless expenditure.

4. It may be added that many scientific students commence their training as scholars by first graduating in the Academic College. For the higher training in science, such a preparatory course in the classics is believed to be eminently desirable. They then enter an advanced class in some one of the departments in the Scientific College, and take the degree of Bachelor of Philosophy, or of Civil Engineer; or by special proficiency, after two years of study, that of Doctor of Philosophy. The Scientific College also admits of partial courses of study which do not lead to any degree.

5. The modification in American colleges which is demanded by the vast development of the sciences of nature within the past century—the era of origination for many of them—and also by the contemporary progress of linguistic and other sciences, is accomplished by the Yale scheme through a method which does not sacrifice, in any degree, classical education, and which at the same time combines thorough literary culture with the widest range and highest development of scientific education. The Classical College stands beside the Scientific, open to all who desire to commence with a classical basis; and the Scientific College offers a thorough and liberal education for all who would pursue a more distinctively scientific course.

6. The Yale scheme contemplates no important change in the Classical or Academic College except in the elevation of the department of modern languages and literature; and its ideal with regard to modern languages cannot be wholly realised until a knowledge of French and German is given (like that of Latin and Greek) in preparatory schools, and required for admission to the college.

7. The great change that has taken place at Yale is in the introduction of its School of Science. This school is not the result of any preconcerted plan on the part of the University. It is a gradual growth of the past twenty years, urged on by the demand in the land for scientific knowledge among lovers of science, those seeking to become its teachers, and others interested in its practical departments; and it has been carried forward to its present organisation mainly through the labours and judgment of the scientific men who have been slowly gathered into its faculty. More than two-thirds of its endowments are due to private munificence, and the remainder to the National Agricultural and Mechanical Fund.

WALLACE ON NATURAL SELECTION

Contributions to the Theory of Natural Selection. A Series of Essays. By Alfred Russel Wallace. (London: Macmillan and Co., 1870.)

IN the discussions of the French Academy, to which we referred in a recent number, M. Elie de Beaumont ventured to describe Mr. Darwin's theory as *La Science Mousseuse.* The phrase is a good one, and expresses very happily the kind of work for which some of the speakers in that debate are distinguished. But although we too in England are not unacquainted with this kind of popular science, scientific works do from time to time appear which are popular without being frothy, and to this class the present book belongs. While strictly accurate in matter, it is easy in style, and is so free from technical language, that it may be understood by educated men who are not professed naturalists; so that we hope it will be read by a large number of those to whom Mr. Wallace's delightful volumes have made the Malay Archipelago familiar.

The arrangement of the essays (most of which have been published separately) does not, perhaps, bring out their mutual connection so well as might be, and there is no attempt to blend them into a continuous series. Four main subjects are discussed, and each has its own peculiar interest.

The first and second chapters are reprinted as originally written in the East Indies, and, with the eighth, form Mr. Wallace's contribution to the theory of natural selection in general. It is remarkable that the same pregnant idea which Mr. Darwin has for ever united with his name should have occurred independently to another English naturalist on the other side of the globe. The public opinion of the scientific world will no doubt assign Mr. Wallace the full credit which the preface to this volume so modestly claims; and the highest respect is due to his varied and fruitful labours in both hemispheres; but a warmer feeling than respect will be paid to the spirit by which the following passage was prompted:—"I have felt all my life, and I still feel, the most sincere satisfaction that Mr. Darwin had been at work long before me, and that it was not left for me to attempt to write 'The

Origin of Species.' I have long ago measured my own strength, and know well that it would be unequal to the task. Far abler men may confess that they have not that untiring patience in accumulating, and that wonderful skill in using, large masses of facts of the most varied kind—that wide and accurate physiological knowledge, that acuteness in devising and skill in carrying out experiments, and that admirable style of composition, at once clear, persuasive, and judicial—qualities which in their harmonious combination mark out Mr. Darwin as the man, perhaps, of all men now living, best fitted for the great work he has undertaken and accomplished."

The third chapter is on so-called Mimicry among Animals, and contains an account of some of the remarkable cases of dimorphism observed by the author and by Mr. Bates, and of those in which one species closely resembles not only leaves and inanimate objects, but other specially protected animal forms. The facts thus established are explained with great ingenuity, and often with equal probability, by the operation of the natural laws of selection. The ways in which even brilliant colouring may become a means of protection are well illustrated, so that this branch of study is made to yield support instead of difficulty to the Darwinian theory. The following chapter, the only technical one in the book, is an application of the same law to explain the various forms and distribution of the group of *Papilionidæ.* It may be compared with Fritz Müller's study of the Crustacea from a similar point of view; and we believe that more solid progress will be made by carefully working out the application of natural selection to restricted and well-known animal groups than by attempting the construction of more comprehensive and imposing phylogenies.

In the seventh chapter Mr. Wallace makes a somewhat similar inquiry into the relation of the colour of birds to the form of their nests, and concludes, from a very wide survey, that when the female is of conspicuous colours, the nest is adapted to conceal her during incubation, while open nests are made by those already sufficiently protected. The exceptions to the rule are candidly stated, and most of them satisfactorily met. That the true law of the habit has been discovered is, perhaps, too much to say; but the evidence is at least enough to lead to further investigation on this interesting subject. Another essay, styled, not very happily, "The Philosophy of Birds' Nests," attempts to explain the building of nests and also the song of birds as the result not of "instinct," but of conscious imitation; gradual improvement being of course brought forward by the survival of the most skilful architects and the constant sexual demand for the best musicians. But not only does Mr. Wallace thus raise nest-making to the rank of an intelligent art, he also shows how much of human construction is simply imitative, and therefore as fairly to be called instinctive as a bird's; while in another passage he shows how the alleged wonderful displays of instinct in savages are really the result of habit and of reason.

This chapter on Instinct in Men and Animals would naturally introduce the last two, in which the working and the limits of the law of natural selection on the human race are considered. This is probably the most difficult, as it is certainly the most generally interesting, of the questions affecting the origin of animal species Those who are not satisfied with the genealogies of Haeckel, and wait for the more cautious and philosophic conclusions expected from the master of the subject, will scarcely, we think, accept the views propounded in this volume by Mr. Wallace. He points out very clearly how most of the human peculiarities of structure may be supposed to have originated by the survival of the forms fittest for their mode of life, and is fully aware of the necessary change going on at the same time in the various functions, to bring them also into harmony with structure. And he shows with great justice how mental and moral qualities must interfere with the absolute carrying out of the law of natural selection—not only in civilised communities, where it is continually and designedly contravened, but among all savages who take, for instance, the least care of the sick and aged of their tribe. But, beside and apart from the operation of the general law of organic life, with these various modifications and restrictions, Mr. Wallace believes that another and independent cause has been at work in the evolution of the human frame, and that this has been a supernatural one. He maintains that the large size of the brain in man, the scantiness of his hairy covering, the great specialisation of his extremities, and some other peculiarly human characters, cannot be explained, except as the result of the direct action of the Creator's will. In fact, he compares man as he at present exists with such products of artificial human selection as the seedless banana or the London dray-horse; so that, if we may thus express Mr. Wallace's theory, man is God's domestic animal.

A great deal of the metaphysical discussion which occupies the last pages of the volume, including the verses quoted from an American poetess, has, we confess, to our mind, the same "double disadvantage" which the author finds in "the law of unconscious intelligence pervading all organic nature put forth by Dr. Laycock, and adopted by Mr. Murphy," that, namely, of being "both unintelligible and incapable of proof;" but the theory of divine artificial selection supplying the deficiencies of natural selection in the formation of man may, we think, be at once met by the following considerations.

The theory of natural selection does not suppose a kind of large female divinity, whose name is Nature, and whose function is to select from animals and plants those fittest for survival. The theory rests, as Mr. Wallace, in another part of his work, is careful to remind the reader, on certain proved facts (enumerated at p. 302), which necessitate the survival of certain forms by virtue of the proved physical laws which we see in daily operation. But these so called laws are, to all who believe in a Creator, simply the manner of His action. To say that our brains were made by God, and our lungs by natural selection, is really to exclude the Creator from half His creation, and natural science from half of nature. All the phenomena we know are of necessity ultimately referable to the First Great Cause: the object of science is to discover their secondary causes; and if the theory of natural selection does not explain how the larynx or the brain of man were developed, then we must try to find another which will. To fall back for explanation upon the primary efficient cause of their existence and the design with which they were framed, is only to confuse two distinct branches of inquiry.

At present, however, we may be content to see how far we can work the Darwinian hypothesis, and can only hope that there may be other "contributions" to the theory as interesting and valuable as these of Mr. Wallace.

P. S.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

Dr. Bastian and Spontaneous Generation

I FIND that the "Address" which it was my duty to deliver at Liverpool, fills thirteen columns of NATURE. The "Reply" with which Dr. Bastian has favoured you occupies fifteen columns, and yet professes to deal with only the first portion of the "Address." Between us, therefore, I should imagine that both you and your readers must have had enough of the subject; and, so far as my own feeling is concerned, I should be disposed to leave both Dr. Bastian and his reply to the benign and Lethean influences of Time.

But I am credibly informed that there are persons upon whom Dr. Bastian's really wonderful effluence of words weighs as much as if it were charged with solid statements and accurate reasonings; and I am further told that it is my duty to the public to state why such distinguished special pleading makes not the least impression on my mind. With your permission, therefore, I will do so in the briefest possible manner.

The first half of Dr. Bastian's "Reply" occupies seven columns of your number for the 22nd of September. In all this wilderness of words there is but one paragraph which appears to me to be worth serious notice. It is this:—

"In the first place, he does not attempt to deny—he does not even allude to the fact—that *living things may and do arise as minutest visible specks, in solutions in which, but a few hours before, no such specks were to be seen.* And this is in itself a very remarkable omission. The statement must be true or false—and if true, as I and others affirm, the question which Professor Huxley has set himself to discuss is no longer one of such a simple nature as he represents it to be. It is henceforth settled that as far as *visible* germs are concerned, living beings can come into being without them."

If I did not allude to the assertion which Dr. Bastian has put in italics—it is because it bears absurdity written upon its face to any one who has seriously considered the conditions of microscopic observation. I have tried over and over again to obtain a drop of a solution which should be optically pure, or absolutely free from distinguishable solid particles, when viewed under a power of 1,200 diameters in the ordinary way. I have never succeeded; and, considering the conditions of observation, I never expect to succeed. And though I hesitate to speak with the air of confident authority which sits so well on Dr. Bastian, I venture to doubt whether he ever has prepared, or ever will prepare, a solution, in a drop of which no "minutest visible specks" are to be seen by a careful searcher. Suppose that the drop, reduced to a thin film by the cover-glass, occupies an area $\frac{1}{4}$ of an inch in diameter; to search this area with a microscope in such a way as to make sure that it does not contain a germ $\frac{1}{10000}$ of an inch in diameter, is comparable to the endeavour to ascertain with the unassisted eye whether the water of a pond, a hundred feet in diameter is or is not absolutely free from a particle of duckweed. But if it is impossible to be sure that there is no germ $\frac{1}{10000}$ of an inch in diameter in a given fluid, what becomes of the proposition so valuable to Dr. Bastian that he has made your printer waste special type upon it?

I now pass to the second part of the "Reply," which, though longer than the first, is really more condensed, inasmuch as it contains two important statements instead of only one.

The first is, that Dr. Bastian has found *Bacterium* and *Leptothrix* in some specimens of preserved meats. I should have been very much surprised if he had not. If Dr. Bastian will boil some hay for an hour or so, and then examine the decoction, he will find it to be full of *Bacteria* in active motion. But the motion is a modification of the well-known Brownian movement, and has not the slightest resemblance to the very rapid motion of translation of active living *Bacteria*. The *Bacteria* are just as dead as those which Dr. Bastian has seen in the preserved meats and vegetables; and which were, I doubt not, as much put in with the meat, as they are with the hay, in the experiment to which I invite his attention.

The second important statement in the second part of the "Reply" is:—

"Professor Huxley is inclined to believe that there has been some error about the experiments recorded by myself and others."

In this I cordially concur. But I do not know why Dr. Bastian should have expressed this my conviction so tenderly and gently as regards his own experiments; inasmuch as I thought it my duty to let him know both orally and by letter, in the plainest terms, six months ago, not only that I conceived him to be altogether in the wrong, but why I thought so.

Any time these six months Dr. Bastian has known perfectly well that I believe that the organisms which he has got out of his tubes are exactly those which he has put into them; that I believe that he has used impure materials, and that what he imagines to have been the gradual development of life and organisation in his solutions, is the very simple result of the settling together of the solid impurities, which he was not sufficiently careful to see, in their scattered condition when the solutions were made.

Any time these six months Dr. Bastian has known why I hold this opinion. He will recollect that he wrote to me asking permission to bring for my examination certain preparations of organic structures, which he declared he had clear and positive evidence to prove to have been developed in his closed and digested tubes. Dr. Bastian will remember that when the first of these wonderful specimens was put under my microscope, I told him at once that it was nothing but a fragment of the leaf of the common Bog Moss (*Sphagnum*); he will recollect that I had to fetch Schacht's book "Die Pflanzenzelle," and show him a figure which fitted very well what we had under the microscope, before I could get him to listen to my suggestion; and that only actual comparison with *Sphagnum*, after he had left my house, forced him to admit the astounding blunder which he had made.

To any person of critical mind, versed in the preliminary studies necessary for dealing with the difficult problem which Dr. Bastian has rashly approached—the appearance of a scarlet geranium, or of a snuff-box, would have appeared to be hardly more startling than this fragment of a leaf, which no one even moderately instructed in vegetable histology could possibly have mistaken for anything but what it was; but to Dr. Bastian, agape with speculative expectation, this miracle was no wonder whatever. Nor does Dr. Bastian's chemical criticality seem to be of a more susceptible kind. He sees no difficulty in the appearance of living things in potash-alum, until Dr. Sharpey puts the not unimportant question, whence did they get their nitrogen? And then it occurs to him to have the alum analysed and he finds ammonia in it.*

And as to the elementary principles of physics—in his last communication to you, Dr. Bastian shows, that he is of opinion that water in a vessel with a hole in it, from which the steam freely issues, may be kept at a temperature of "230° to 235° F. for more than an hour and a half."† I hope that Professor Tyndall, whom Dr. Bastian scolds as authoritatively and as unsparingly as he does me, will take note of this revolutionary thermotic discovery, in the next edition of his work on Heat.

It is no fault of mine if I am compelled to write thus of Dr. Bastian's labours. I have been blamed by some of my friends for remaining silent as long as I have done concerning them. But when, because I have preserved a silence, which was the best kindness I could show to Dr. Bastian, he presumes to accuse me publicly of unfairness, and to tell your readers that my Address "is calculated to mislead" them, I have no alternative left but to give them the means of judging of the competency of my assailant.

Jermyn Street, Oct. 10 T. H. HUXLEY

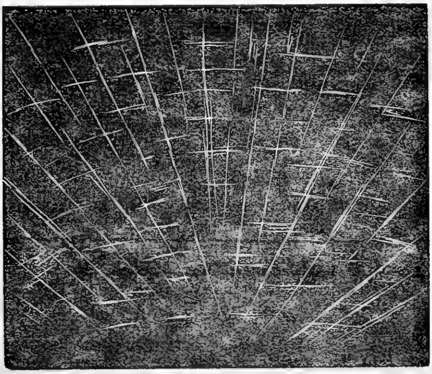
Ozone developed by Humidity and Electricity

IN confirmation of the idea as to the connection of excess of ozone with humidity and electrical action, to which attention was drawn in my previous letter of August 4, I forward you the following observations.

* See NATURE, No. 36, p. 198. † Ibid, No. 48, p. 433.

Between August 19 and September 9, the average of ozone registered was very great, being about eight by Schönbein's test-paper. On the five days ending September 9, the quantity each morning was nine. During the longer period mentioned, humidity was also in excess. Rain fell every day from September 1 to 9.*

The weather during the last month has been very pleasant—we have not had more than one really wet day, and rain has generally fallen in the night. Though there have been no thunderstorms, we have had evidence of their occurrence in other parts by the presence of electrical cumulus. But the most remarkable phenomenon in connection with electricity was the appearance of an Aurora Borealis *in the daytime*, which happened on



the afternoon of September 4. I pointed this out (about 4 P.M.) in the form of thin reddish streaks resembling linear cirrhus, which radiated in a symmetrical manner from the north, at the same time anticipating a luminous appearance after nightfall. This took place: yet the light was not in the northern part of the heavens, but stretched clear across from W. to E. in a broad band of glowing white light. Fainter streaks appeared afterwards in the north. A dark cirrhus mist overspread the sky at the time.

The radiating cirrhus, or Auroral cloud that appeared in the daytime, was crossed by lighter streaks, as in the accompanying sketch.

The day previous this phenomenon was seen at Edinburgh, and also in this neighbourhood, though I did not observe it.

These manifestations, followed each time as they have been lately by change of weather, seem to confirm the theory of Mr. Buchan and M. Silbermann; and I could adduce other observations pointing to the same inference, viz. that the Aurora Borealis is an atmospheric phenomenon, connected with the polarisation of vapour.

Great Malvern, Sept. 12 S. B.

* The above notes were taken by Mr. R. J. Wood.

Aurora Borealis

On going out this evening I saw a very fine Aurora Borealis, which literally covered the whole sky to within 15° of the southern horizon. The streamers were of a yellowish white colour, but at times very decidedly red. They shot up from the east, north, and west strongly, feebly from the south. The radial point was within 5° of Scheat (β Pegasi) A.R. $22^h\ 56^m$., dec. N. 27° 15′±.

This radial point remained constant for about one hour and a half, when it suddenly shifted, at 11.30, to within 5° of the zenith towards the east.

The light was sufficient during the flashes to see my watch, and read the second hand with ease.

Altair (α Aquilæ) was quite blotted out by the glare as it passed over; also the stars in Cygnus and Andromeda.

I used a spectroscope on the light, and found that I could distinguish the bright line near the group of calcium lines (wave-length 5,567) almost anywhere I pointed to in the sky, excepting on the south horizon; while to the north, I not only saw the bright line, but also others apparently near F, one bright, the other very faint.

I hope that there will be other notices in your paper of this very fine Aurora.

My latitude is N. 57° 8′ 56″. LINDSAY

Dunecht, Aberdeen, Sept. 24

A MAGNIFICENT display of Aurora Borealis was observed here this evening, between 9^h and 12^h P.M. During the early part of the morning the suspended magnets were so much disturbed that I considered it useless to continue a series of absolute determinations of the magnetic elements on which I was engaged. The auroral display when first noticed bore a very striking resemblance to the effect produced by a brisk and squally wind passing over an otherwise calm lake. Magnetic clouds overspread the northern half of the sky, and were abruptly terminated by an irregular arch, stretching from the magnetic E. to W., and passing almost through the zenith. This arch was never very well defined, but it served for some time as an apparent barrier to the rapid passage of the waves of magnetic light from the N. towards $9^h\ 45^m$ P.M. The whole N. horizon was brilliantly illuminated, but in some points more so than in others, and from these points broad streamers darted forth, extending often from the horizon to the zenith. Several of these stupendous beams of light, many degrees in breadth, were sometimes seen at once, and occasionally the whole N. horizon shot forth these brilliant streamers. The colour of the beams was often red, but more frequently white, but many changed from white to red, or red to white, before vanishing from sight. The sky at times was partly of a deep red hue.

At $10^h\ 45^m$ some of the beams assumed a more stationary character, and radiated from a point some 70° above the E.S.E. horizon. The whole sky then became covered for a short time with the magnetic clouds, which were rarely dense enough to obscure stars of the second magnitude. At about eleven o'clock the phenomenon again completely changed, returning suddenly to its former aspect, of a lake violently agitated by a gusty wind, which brilliantly lit up the thin clouds as it passed rapidly onwards from the N. Towards midnight the activity of the forces considerably abated.

During the storm the vertical force magnet was completely thrown off balance, and the declination and horizontal force magnets suffered considerable perturbations.

Stonyhurst, Sept. 24 S. J. PERRY

Botanists and the Halfpenny Postage

FOR several years I have been in the habit of sending herbarium specimens by "book post," by merely placing the plants between sheets of white cardboard, which were invariably fastened by string, without wrappers, and the address written on the cardboard itself; so that, in fact, the parcel was open at both ends and sides. On Tuesday last I did up a small packet of green or living plants, and sent them to the village post-office. As the parcel weighed under four ounces, I affixed two halfpenny stamps. On calling at the post-office the next day, imagine my disgust at seeing my parcel of plants stuck up in the window to be sundried! Upon inquiry, I found that the postman had very *wisely* declined taking it to the borough office, as "several similar parcels were lying about there now, and would not be forwarded until the senders had prepaid the postage at letter-rate."

I say the postman acted wisely in refusing to take my parcel, because, on taking it to the Newbury post-office myself on Thursday, I found that unless I paid eightpence, or rather put on seven more penny stamps, *double* that amount would have actually been charged to the recipient, at which I should have felt much grieved, as the specimens were really not worth half the amount.

Another gentleman of my acquaintance sent a parcel to the same office with a fourpenny stamp on; this the clerk kindly *defaced*, and returned the parcel as "not sufficiently paid"!

I have read the new rules, and can see no clause bearing on this subject—either for or against herbarium specimens, or other objects of natural history, being sent between cardboard with open ends at the old rate of four ounces and under for one penny, and should therefore be glad of any correct information from you or the readers of NATURE.

East Woodhay, Oct. 7 HENRY REEKS

Working Men's Colleges

I HAVE only just seen your remarks on Working Men's Colleges, and your suggestion that "men and women should be treated, not as artizans, mechanics, or gentlemen, but simply as men and women." May I, speaking as a member of the College in Great Ormond Street, and also of the younger college here, and having an intimate knowledge of both during the whole of their existences, assure you that both these colleges were opened and have been carried on in the spirit you suggest, and in no other? Certainly, we have never in either college desired to treat *women* as either "artizans, mechanics, or gentlemen."

Secondly, I do not think either college is second, even to the "Berlin Working Men's Club," in *catholicity*. Each college puts forth a programme of what it can give its members, and there is not in either college the slightest effort to induce any member to do anything but what he has a spontaneous desire to do. Neither college "belongs to any religious or anti-religious body." Neither college has any "Shibboleth of any kind whatever."

Thirdly, your suggestion that all the colleges and clubs should be united into one, would have more value in a small town than in London, with its hundreds of miles of streets and its millions of inhabitants.

Lastly, there is an essential difference between a college and a club. The one has a foundation in work, the other in recreation; the chief work of the one is mental, of the other social; in both it is moral. Both are necessary, but they need not necessarily be carried on in the same building.

Another point is that working men *cannot* form a college *for* and *by* themselves, except they scarcely need it. The number of members needful to defray the expenses is so large, and the number of men in London possessing the elementary education requisite to give them an intelligent and persistent desire for such a place is comparatively so small, that I do not think any college in London can be wholly self-supporting for some years to come.

If any of your readers desire to know what kind of work the two colleges are doing, I shall be happy to give them full particulars of the work and of its difficulties, and still more of its need to be done. I think you are in error in speaking of the *extension* of the scientific programme of Great Ormond Street College. I have before me the new programme of that college. In *this*, the younger college, we have put before our students a larger number of science classes than we have hitherto done, because we are beginning to find that the men who have passed through our night schools and elementary classes give us hopes of doing good in this way. Possibly it is to this that you meant to refer.

W. ROSSITER, Hon. Sec.

October 4 South London Working Men's College

[We wrote by the card in speaking of a projected extension of the scientific instruction at the Working Men's College in Great Ormond Street.—ED.]

Lunar Rainbow

I HAVE just witnessed this evening (Monday, 10th, 7.30 P.M.) a magnificent lunar rainbow, distinctly coloured throughout, and with the reflection bright towards the west. The space within the bow appeared lit up by a silvery haze, offering a marked contrast to the inky appearance of the cloud beyond the bow.

The moon was shining brightly as the shower commenced during which the rainbow was seen. It was observed at different times during the evening by several persons.

Cromer, Norfolk J. G. DUTHIE

NOTES

WE have great pleasure in announcing that the American Government have voted 6,000*l*. for the expedition which will be sent to Spain and Sicily to observe the coming eclipse. It will be in the recollection of our readers that our own Government have refused to give either a single ship or a single shilling in aid of our own observations; as we said before, comment is useless.

WE have also a word to add to another instance of American enlightenment which we chronicled last week, namely, the vote of 50,000 dollars towards the construction of a refractor similar to that recently erected by Mr. R. S. Newall at Gateshead. The Superintendent of the U.S. Naval Observatory, Washington, has written to Mr. Newall a letter which we have been permitted to see, in which, after referring to the munificence which has endowed astronomy with such an instrument as the Newall telescope, he requests permission for Prof. Newcombe to inspect it, with a view of judging what devices and mechanical arrangements are best adapted to secure the successful and easy manipulation of the American instrument.

THE death of Prof. Miller has been followed by another heavy loss to Chemical Science in that of Dr. Augustus Matthiessen, one of the rising chemists of greatest promise in this country. The work which he had already done had acquired for him a reputation equalled by few, and exceeded by none of his fellow-workers of his own age; to this we hope to refer more at length next week. He occupied the position of Lecturer on Chemistry at St. Bartholomew's Hospital, and was, at the time of his death, one of the Examiners to the University of London. He was in his 39th year.

THE "fish torpedo" which, as we stated some little time ago, has been for some time subjected to various experimental trials by a committee of naval officers, under Captain Arthur, R.N., was put to a practical proof on Saturday, at Sheerness. The *Oberon*, paddle-wheel steamer, had been specially supplied with a 12-horse power engine and air pumps for filling the torpedo with compressed air, and fitted with a large discharging tube at the bow for launching it under water. The peculiarity of the torpedo is that it will maintain its passage at any particular depth between five and fifteen feet from the surface; the propulsion being entirely submarine and effected at the rate of six or seven miles an hour by the action of the compressed air on a screw propeller. Two torpedoes were run against the *Aigle*, a large hulk lying in the harbour, both from a distance of 140 yards. The first contained a charge of 67lb. of gun-cotton, and hit the hulk, exploding on impact, and making a clean hole, 20 feet by 10 feet in area, and sinking her at once. Nets were placed at 15 feet from the side of the hulk, and the second torpedo run at them, being launched from a framework attached beneath a boat. This torpedo, containing a charge of 18lb. glyoxiline, was caught in the nets and exploded there, doing no damage whatever to the side of the hulk. The machine is the invention of Mr. Whitehead, an English engineer, having works at Fiume, in Hungary.

WE must refer our readers to the October number of the *Astronomical Register* for an account of a discussion on the great Melbourne telescope, at the Royal Society of Victoria. The colonists mistrust their great reflector, and we do not wonder at it, the day for metallic specula is past, and we regret that our Royal Society had anything to do with sending out such an instrument.

THE *Astronomical Register* announces that the post of Government Astronomer at Sydney is vacant by the death of Mr. George K. Smalley.

NOW that the Government are accused by a tribunal appointed by themselves, of having built a top-heavy ship "in deference to public opinion, as expressed in Parliament and in other channels" (*sic*) and "in opposition to the views and opinions" of their scientific adviser, might we be allowed to suggest that the more Government attempts to encourage the advancement of scientific ideas and studies among members of Parliament and other channels, the better? The cost of the *Captain* in hard cash would have helped to disseminate a vast amount of scientific education and interest throughout the land had it been properly spent; and now it is quite lost, because the Government are Philistines, and do not like Science, and build top-heavy ships because ignorant members of Parliament and other "channels" clamour for them.

ARTISTS, manufacturers, and others who have not expressed their desire to be admitted as exhibitors at the International Exhibition of 1871, are requested to do so before the 10th of November next. Her Majesty's Commissioners, as already notified to the public, do not intend to award prizes to exhibitors. They will, however, afford every facility to societies and individuals desirous of offering prizes for the encouragement of Art or industry in connection with the Annual International Exhibitions; and are prepared to receive such offers, and to publish the conditions of competition which the donors may wish to prescribe. The conditions are announced of a competition of prizes for the best fan, the first prize of 40*l*. being offered by Her Majesty the Queen. The painters and decorators are completing their work in the fine art galleries. We understand that it is the intention of Her Majesty's Commissioners to invite artists and exhibitors of all fine art works to inspect these galleries shortly.

THE American Association for the Advancement of Science closed its nineteenth meeting on the 25th of August, at Troy, N.Y. Owing to the illness of President William Chauvenet, of St. Louis, the Vice-President, Dr. T. Sterry Hunt, of Montreal, presided. The meeting was largely attended, there being about 300 names enrolled on the treasurer's books. The next meeting is to be held in Indianapolis, and the meeting in 1872 will probably be held in San Francisco, upon the invitation of the California Academy of Sciences. The president-elect for the next meeting is Prof. Asa Gray, of Cambridge; general secretary, Prof. F. W. Putnam, of Salem, Mass.; treasurer, Mr. William S. Vaux, of Philadelphia.

WE learn that the building for the New York Industrial Exhibition will be commenced on the 15th of December. Twenty-three acres have been purchased between 98th and 102nd Streets, the purchase money amounting to 2,658,000 dollars. It is intended to make this one of the finest, or perhaps we should rather say *the* finest permanent institution of the kind in the world. The building will probably cost not less than 8,000,000 dollars, and it is the intention of the managers to advertise for designs at an early day. The importance of such an enterprise to New York can hardly be over-estimated. The benefits that have been conferred by the Central Park in opening up a place of resort uncontaminated by beer-saloons and other demoralising agencies has been very great, and the present effort to extend facilities for instructive pleasure and innocent amusement deserves well of all who desire the good of the city.

WE understand that the following are candidates for the Regius Professorship of Natural History in Edinburgh, vacant by the resignation of Professor Allman:—William N. McIntosh, M.D. Edin., F.L.S.: H. Alleyne Nicholson, M.D., D.Sc. Edin., F.R.S.E.; and Wyville Thomson, LL.D. St. Andrew's, F.R.S. We believe we are correct in stating that there is no truth in the rumours that either Prof. Flower, of the Royal College of Surgeons, or Prof. E. Perceval Wright, of Dublin, is a candidate for the chair.

THE authorities of King's College, London, have arranged that the duties of the chair of chemistry, vacant by the death of Prof. Miller, shall be performed, pending the appointment of a successor, by Prof. Odling.

THE First Commissioner of Works intends to have distributed this autumn, among the working-classes and the poor inhabitants of London, the surplus bedding-out plants in Battersea, Hyde, the Regent's, and Victoria Parks, and in the Royal Gardens, Kew. If the clergy, school committees, and others interested, will make application to the Superintendents of the Parks nearest to their respective parishes, or to the Director of the Royal Gardens, Kew, in the cases of persons residing in that neighbourhood, they will receive early intimation of the number of plants that can be allotted to each applicant, and of the time and manner of their distribution.

THE Horticultural Society held a Fungus-show on the 5th inst. for competition for prizes offered by Mr. W. W. Saunders, F.R.S., for the best collection of Edible and Poisonous Fungi. Several exceedingly good collections were shown, which attracted a great deal of attention, many of the visitors being evidently astonished at the large number of common fungi which are warranted by experts to be not only innocuous, but wholesome articles of diet.

ON the 4th inst. a large public meeting was held in the Mechanics' Hall, Dewsbury, for the promotion of Technical Education in Yorkshire. Classes for instruction in drawing and elementary science are to be opened under certified teachers. The inaugural address was delivered by Mr. J. Buckmaster, of the Science and Art Department. After explaining the aid rendered by the Government, he pointed out at some length the industrial and moral advantages derived from the acquisition of scientific knowledge.

THERE has just been started in the city of Baltimore, U.S.A., a society of fifty members, called "The Maryland Academy of Sciences." It is intended to pay special attention to microscopy. The principal officers are Philip T. Tyson, President; John G. Morris, Vice-president; Edwin A. Dalrymple, Corresponding Secretary.

THE annual examinations for degrees in the Queen's University, in Ireland, commenced last week, and will be continued during the greater part of the present week. The examining body of the University consists for the most part of the professors in the different faculties in the three Queen's Colleges. We regret to learn that Prof. Wyville Thomson is unable to take his part in these examinations owing to continued ill-health.

PROF. VERRILL, of New Haven, has just returned from an expedition to the Bay of Fundy. The greatest depth encountered in dredging even as far as fifty miles from the coast, was not beyond 120 fathoms. Very large collections were made, many rare and about sixty new species were discovered, the number of species in Prof. Stimpson's list being more than doubled. We hope soon to have a catalogue of the fauna of the bay from Prof. Verrill.

SIR WALTER ELLIOT is compiling a record of what has been done by local societies in Great Britain and Ireland, towards elucidating the natural history of the districts in which the societies meet. Any information, as when such societies were established and by whom, and details generally as to their proceedings, will, we hope, be forwarded to Wolfelee, Hawick, N.B.

MR. JAMES BRITTEN, of the Royal Herbarium, Kew, and Mr. Robert Holland, of Mobberly, Knutsford, Cheshire, have in preparation a work on the folk-lore connected with plants. Any assistance will be gladly received by either of these gentlemen at the addresses given above.

A WORK is announced on the land and fresh water shells of British India, "Testacea indica, terrestria et fluviatilia," by Sylvanus Hanley, F.L.S., and William Theobald, of the Geological Survey of India. It will be issued by Messrs. L. Reeve and Co., in monthly parts, and will contain sixty to eighty 4to plates.

IN another column we give an abstract of Capt. Carmichael's paper read to the Geographical Section of the British Association, relating to the aboriginal Indians of Central America. Since then, the following interesting information has come to hand. A correspondent, writing from Santa Fe, New Mexico, to the *New*

York Tribune, says:—"Governor Arny, the indefatigable special Indian agent for this territory, has lately returned from a point north of the San Juan country, and reports that during his tour to reach the Utah Indians, his party found the Canon de Chelly in the great Sierras, which was explored for more than twenty miles. Among canons towering precipitously to the height of 1,000 to 2,000 feet, they found deserted ruins of Aztec cities, many of which bear the evidences of having been highly populous. In one of these canons, the rocky walls of which rose not less than 2,000 feet from the base, and whose summits on either hand inclined to each other, forming part of an arch, there were found high up, hewn out of the rocks, the ruins of Aztec cities of great extent. In each of these rocky eyries there remained in a state of good preservation a house built of stone, about twenty feet square, containing one bare and gloomy room, in the centre of which were traces of fire, and also a single human skeleton. The only solution of this enigma thus far advanced is that these solitary rooms were the altar places of the Aztec fires; that from some cause the people at a remote period were constrained to abandon their homes, but left one faithful sentinel in each instance to keep alive the flame which, according to the Indian traditions of these regions, was to light the way of Montezuma, their so long hoped for Messiah, again to his people. A close examination of many of the ruins proved that the builder must have been skilled in the manufacture and use of edged tools, masonry, and other mechanical arts." A good idea of these rocky canons or mountain gorges will be obtained by reference to the description and illustrations of the Canons of the Sierra Nevada, given in NATURE, vol. i. p. 434.

IN directing attention to the recent regulations with regard to scientific teaching in force at Yale College, in our present number, we alluded to the exploring party which left Yale College under the charge of Prof. Marsh. We are glad to announce that their endeavours have been crowned with great success. They spent three weeks examining the geology of the country between the north and south branches of the River Platte, and discovered in Northern Colorado an extensive tertiary deposit, abounding in fossil remains. The formation is identical with the "Mauvaises terres" deposit of Dakota, and apparently forms the south-western border of some ancient fresh-water lake (see NATURE, Vol. II., p. 385, "The Ancient Lakes of Western America"). These beds were traced to the north, and along the North Platte River; several thousand specimens were collected, and among them a number of new species of tertiary mammals.

ARRANGEMENTS are being made to light the stations of Rawul Pindee in India with gas from the local deposits of petroleum, being one of the first examples of their utilisation.

FULLER details have now arrived of the great earthquake we reported as having occurred in Thibet in May, and which extended over a wide area of country.

MR. BROUGHTON, the Government quinologist in India, has been called upon to examine the bark of an indigenous Indian tree, *Hymenodictyon excelsum*, supposed to be a powerful febrifuge. He reports that it contains a bitter principle, identical with æsculin, and also found in the horse-chestnut tree, but of little therapeutic value.

THE experiments in growing Carolina rice in our great rice-country of English Burmah are reported as having failed.

IT may be esteemed a benefit that we have a local press in India, which may collect for us more facts as to natural phenomena, but the acceptance of anything from such sources must be received with caution. New facts about snakes are in this class. The Vellore correspondent of the *Madras Standard* asserts on reliable information that a native woman near that town lately gave birth to a child and a snake, and that another has produced twins, and a third child, which looked like a toy elephant. A very respectable and well-educated Mussulman lately reported the exhibition, we think at Benares, of a mermaid from Japan, which he accepted on the evidence of his own eyes and the statements of the highly respectable Mussulman showmen, and duly reported to the paper of which he is the correspondent.

A GOOD example of the value of agricultural shows and their influence on produce, is shown in India by the Vellore shows, which have now been held for twelve years. At the same time the results show the difficulty of organising cattle-shows there, and how it may be overcome. As is well known, the superstition about the sacred cow is strong in India, and the ryots can hardly be got to exhibit, and the first cows shown were scarcely worth a prize. Now the exhibition of the famous Vellore breed has reached 173 cows and heifers, and a great improvement is visible. It is suggested that the exhibition of Vellore bulls will also be attended with advantage. In India the greatest care is requisite in the most trivial undertakings in dealing with the superstitions of the natives.

THE Cinchona cultivation has so well succeeded in the English hill settlement at Darjeeling, in the Himalayas, that last year 5,000lbs. of bark was sent to London from Cinchona trees planted in 1862 on one plantation. Tea produced, in 1869, 1,319,743lbs. from 10,769 acres of hill land formerly said and reputed to be worthless, and unsuited to give a return to Englishmen. We shall now hear of Indian bark as well as Peruvian, as we know Indian tea to hold its own against Chinese.

CONGRESS has granted 30,000 dollars for the erection of a Government Winter Garden, either at New York or Washington, somewhat similar to that at Kew, but on a smaller scale. This will partake partly of the nature of an economic garden, in which useful plants can be raised and then disseminated far and wide throughout the States.

"THE Reign of Law," by the Duke of Argyll, which has been published so long in England, has at last been republished in America. It is announced as the first American from the fifth London edition, published by C. Leat and Co., New York. We also hear of a Canadian reprint of Prof. Huxley's "Lay Sermons."

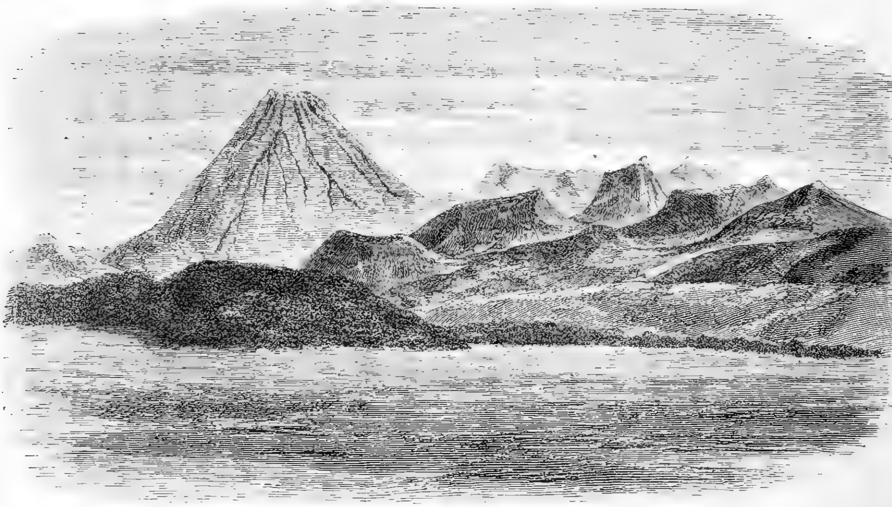
THE cinchona trees are taking well in Jamaica. Experiments on the culture of American tobacco in India are being made by the Maharajah of Burchwan in Midnapore and Cuttack. The last year's experiments with seeds from James River, Virginia were very successful.

ARGENTIFEROUS galena has been discovered in the district of Beerbhoom, in India, by Mr. Ball, of the Geological Survey. The assay of some picked specimens gives 110 ozs. of silver to the ton of lead, and it is considered there is a sufficient quantity of ore to justify working.

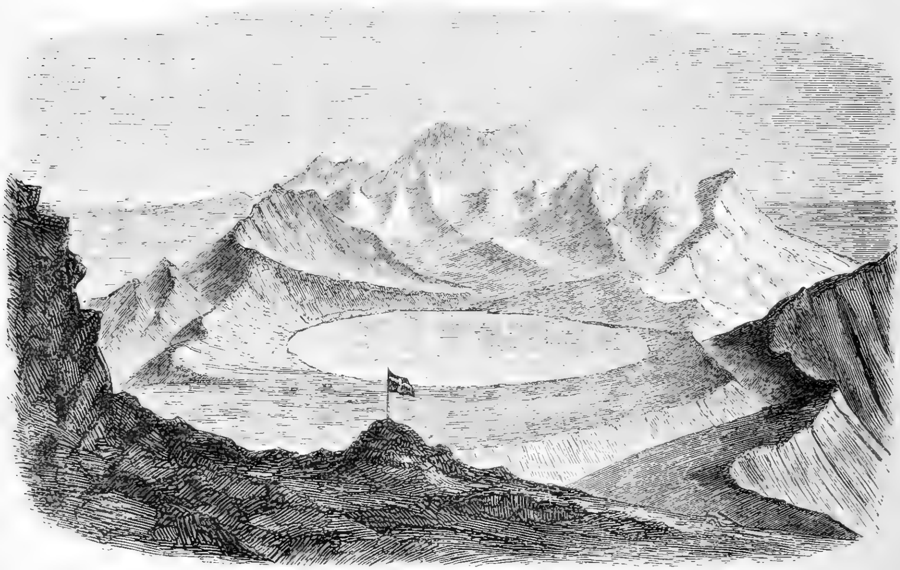
ERUPTION OF THE VOLCANO TONGARIRO, NEW ZEALAND

THROUGH the kindness of Dr. J. D. Hooker, we have received the following important and interesting account of the eruption of this volcano, together with drawings, from the pencil of Dr. Hector, of the most interesting features of the mountain itself, from which we are enabled to copy the accompanying woodcuts:—

Dr. Hector announced, at the meeting of the Wellington Philosophical Society of New Zealand, held July 17th, that Tongariro, the only active volcano in the colony,

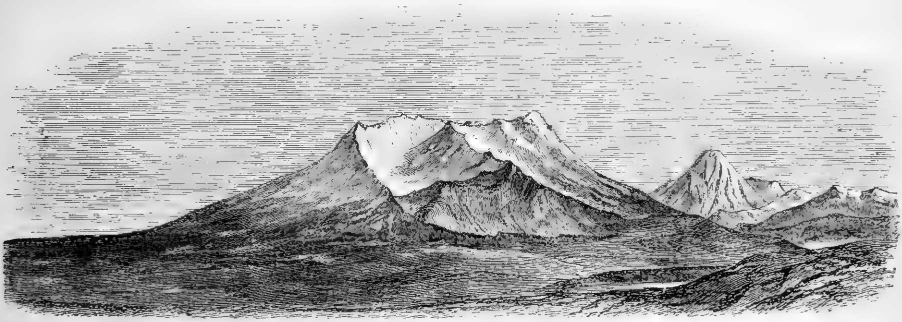


NGAUR HOE CONE OF TONGARIRO, FROM THE EAST



LAKE ON THE TOP OF TONGARIRO, LOOKING SOUTH OVER NGAURUHOE AND RUAPEHU which is situated in the interior of the North Island, had burst into active eruption in the month of April last, and for the first time within the knowledge of the colonists, or even the traditions of the Maories, lava streams have been emitted. On the occasion of previous eruptions, the outbursts have consisted only of ashes and gaseous matters, the former having been spread over a district extending for upwards of thirty miles round the mountain. The volcano is 6,000 feet high, and consists of a group of irregular broken cones, and one very perfect cone, known to the natives as Ngauruhoe. Jets of boiling water and steam continually issue from the north side of the mountain. At an altitude of 3,600 feet, and on the top of Tongariro proper, is a lake 300 yards across, the water of which is of an intense green colour. Ten miles to the south of Tongariro is the ancient trachyte cone of Ruapehu, which is the loftiest mountain in the North Island, having an altitude of 9,600 feet. It is a notable circumstance, that on the 5th of April last, when electrical disturbances were so marked in Europe, and the brilliant displays of Aurora Borealis were generally observed, the corresponding phenomena of the Aurora Australis were extremely well marked in the Southern Hemisphere, attended also by electrical disturbances of unusual character; and on the same evening a well-marked earthquake shock was experienced in the volcanic district of New Zealand, and shortly after the above eruption was reported. The country is very inaccessible at this season, but from the north end of Taupo Lake, where there is now a telegraph station, a distant view is obtained. On the 10th July, the immense volumes of dense black smoke which are being emitted from Tongariro were plainly visible from the hills at Napier, as well as from parts of the surrounding plains. Loud reports were distinctly heard for the previous fortnight, like the boom of heavy artillery, or rather the noise caused by the falling of an immense body of matter, at intervals of five minutes or thereabout. These reports (which are very loud in the vicinity) are sometimes accompanied by a quiver of the earth, and in each case by a great up-burst of flame and red-hot masses like molten rock. A broad stream of red-hot lava is distinctly visible flowing down the side of the mountain in a wavy irregular mass; and in the night the flames issuing from the crater are described as forming a highly interesting and beautiful spectacle.

On the 18th a surveyor reports that he observed, about 1.15 P.M., a sudden column of smoke come out of Tongariro (just as if a steamer was firing up), and soon after it seemed to change to white steam; he stood on a hill about eighty miles distant, and could just see the top of Tongariro to the east of the shoulders of Ruapehu.



RUAPEHU AND TONGARIRO, FROM THE ONETAPU DESERT, ON THE SOUTH-EAST SIDE AT THE SOURCES OF THE RANGITIKEI RIVER

THE BRITISH ASSOCIATION

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE

The Radcliffe Observer (Rev. R. Main) communicated the *Observations of Shooting Stars made at the Observatory during the past year*, from 1869, August 19, to 1870, August 31.

Since the last report to the British Association, nearly 300 shooting stars have been observed by Mr. Luces at the Radcliffe Observatory, Oxford, distributed as follows:—

27 in the remaining part of August, 1869.
11 on the 1st and 3rd of September.
17 in the month of October.
16 on the 4th and 6th of November.
21 on the nights of the 8th, 9th, and 10th.
None on the 11th in a watch at intervals from $7^h\ 30^m$ till $12^h\ 30^m$.

The nights of the 12th and 13th were thickly overcast. On the 14th there was a break from 11^h to 12^h, but no meteors seen; eight were observed on the night of the 15th, between 8^h and $13^h\ 30^m$, in bright moonlight. On the 28th one was seen.

On December 29 a remarkable meteor, about equal to one-fourth of the full moon, was seen through the window of the sitting-room, with a bright gas-light nearly between the observer and the window. Course from the Pleiades to the south horizon.

None were observed in 1870 till March 30, when one was observed by Mr. Main at $8^h\ 20^m$, larger than Jupiter, of a brilliant white, and a period of about 5 seconds from the zenith, vertically downwards a little to the south of the prime vertical.

On April 12th, at $11^h\ 13^m$, one larger than α Lyræ, of a brilliant white, visible 2^s to 3^s from a point near α Lyræ, northward about 10°.

April 19 at 14^h, 3 in quick succession were seen near α Herculis, of about second magnitude, with a rapid downward motion, one nearly vertical, one slightly inclined to the west, and the other to the east. Watch in bright moonlight till $16^h\ 30^m$.

Only 7 were seen in May, and 1 in June. On July 8, at $9^h\ 20^m$, the sky completely overcast and no star visible, a meteor was seen below the clouds at a point near Cassiopeia, with a downward motion of about 6°, and bursting at the end of two seconds of time.

Another meteor was seen below the clouds for about half a second on July 15, at $12^h\ 25^m$, near β Andromedæ, scarcely any stars being visible in that part of the heavens.

About 20 were seen on the night of July 21 by different persons while Mr. Luces was conversing with them, between 9^h and 13^h, but he did not see any till $13^h\ 43^m$, when one equal to α Lyræ left a point near that star, taking its course, with a long train, towards the north horizon.

Only 5 were seen between this and August 8, when 4 were seen in strong moonlight and clouded sky. 11 were observed on August 9 under the same circumstances; one at $13^h 46^m$, equal to Capella, started from δ Ursæ Majoris with a downward course, of about one second, when it burst, leaving a train visible about two seconds.

On August 10, between $9^h 33^m$ and $15^h 30^m$, 36 were observed, nearly all in the northern part of the heavens, with strong moonlight and clouded sky. One at $14^h 10^m$, equal to α Lyræ, of a bright white colour, burst near β Cygni.

Twenty-six were observed on August 11, between 9^h and 15^h, and on the 12th, 7 between 8^h and $10^h 30^m$, the sky becoming overcast at 11^h.

3 were observed on the 13th, from 10^h to 12^h.
3 on the 15th, from 12^h to 13^h.
7 on the 17th, between $9^h 20^m$ and 12^h.
5 on the 22nd, between $10^h 20^m$ and 12^h.

Four on the 24th, 3 on the 26th, 2 on the 29th and 30th, and 3 on the 31st.

The most noteworthy besides those already mentioned were:—

One on August 19, 1869, in strong moonlight, when few stars were visible, from a point near α Pegasi to near γ Pegasi, about equal to α Lyræ, for about two seconds.

On October 6, at $7^h 5^m$, one about equal to Jupiter appeared to burst near the S.W. horizon.

On October 27, one as bright as Jupiter, visible for two or three seconds, from ψ Tauri to the Pleiades.

November 4, at $6^h 5^m$, 1 equal to one-sixth of full moon, yellowish, was seen by Mr. Béchaux for about three seconds, with a train about 2° in length, from α Arietis to a point above ε Pegasi.

November 8, at $7^h 43^m$, one of the 1st magnitude, yellow, visible about 3^s, with a train from δ Aurigæ to α Draconis, and another at $8^h 10^m$ of the 1st magnitude, white, with a train from α Cygni to θ Lyræ, visible for three seconds.

SECTION B.—CHEMICAL SCIENCE

On the Lancashire Alkali Trade.—Mr. William Gossage. This paper chiefly dealt with the scientific and commercial progress of the alkali manufacture in Lancashire during the last nine years. Mr. Gossage regarded as one of the most important facts of the last nine years the passing of the Alkali Act, 1863, an Act which makes it imperative that all manufacturers decomposing common salt for the production of sulphate of soda should condense not less than 95 per cent. of the hydrochloric acid gas evolved by such decomposition; and he said that the means which he devised in the year 1836 for effecting this condensation were now universally adopted by the soda manufacturers, with such success that not only did they comply with the legislative requirements of condensing 95 per cent. of the hydrochloric acid set free, but in many instances the condensation exceeded 99 per cent. Speaking of the benefits arising from the appointment of Dr. Angus Smith to the office of chief inspector under that Act, he considered that it may be fairly expected that when the same amount of care and attention is applied to subduing the bad effects resulting from other noxious vapours, chemical manufactories will be relieved from the charge of occasioning injury to the neighbourhoods in which they are situated. The most important use of hydrochloric acid obtained by this condensation was the manufacture of bleaching powder. At the date of his former paper, the chlorine required for the manufacture of bleaching powder was obtained by the action of hydrochloric acid on native peroxide of manganese; but recently, Mr. Walter Weldon had perfected a process whereby peroxide of manganese is obtained from the chloride of manganese produced by the action of hydrochloric acid on peroxide of manganese. Mr. Weldon's process has been successfully adopted by some of the largest manufacturers of bleaching powder. Mr. Gossage next referred to Mr. Deacon's method of manufacturing chlorine without the use of manganese, and to the circumstance that Mr. James Hargreaves, of Widnes, has also devised means for producing chlorine without the use of oxide of manganese. In his last paper, Mr. Gossage remarked that nearly all the sulphur used in the soda manufacture was re-obtained in combination with calcium, forming what is designated "alkali waste," and he then suggested that this presented a problem worthy of consideration by his juniors. Mr. Ludwig Mond, a German chemist, had now made the nearest approximation to the solution of this problem, with which he (Mr. Gossage) was at present acquainted. Mr. Mond's process had been carried out successfully by various manufacturers, but unfortunately the quantity of sulphur obtained was far short of that in the waste; and he (Mr. Gossage) considered that the problem he had mentioned still remained as an exercise for ingenuity and perseverance. He had also alluded in his paper before the Association in 1861 to the means he had adopted for obtaining copper and silver from the burnt residue of copper pyrites, which had been used for yielding sulphur in the manufacture of sulphuric acid. This mode of working had now been superseded by a process devised and carried out by Mr. Henderson. After contrasting the quantities of soda and other chemicals manufactured respectively in the Lancashire and Newcastle districts in 1852 and 1869, showing the much greater proportionate increase in the former district, he closed his paper by observing that it might reasonably be concluded that Lancashire was now the largest seat of chemical manufactures in this country.

The President, by way of supplementing Mr. Gossage's remarks, said that in the ten years previous to 1861 the increase in the amount of salt decomposed was 300 per cent. The amount consumed in 1852 was 38,600 tons, and in 1861 it had risen to 135,000. Professor Roscoe proposed a vote of thanks to Mr. Gossage for his valuable paper, and said that he was the father of the alkali trade, and that he held a position second only to that of the illustrious Leblanc, the inventor of the early process of making soda. Mr. Spence seconded the proposal, and said that Mr. Gossage was almost the originator of the soda manufacture; he was certainly the originator of some of the most important processes in it.

On the Hydrogenation and Hydriodate of Cyanogen.—Mr. T. Fairley.

On the Distillation of Sulphuric Acid.—Mr. T. Fairley.

On the Time needed for the Completion of Chemical Change.—Dr. Hurter.

On Reciprocal Decomposition viewed with Reference to Time.—Dr. J. H. Gladstone, F.R.S.

On a Method for the Determination of the Amount of Sulphur in Coal Gas.—Mr. A. Vernon Harcourt, F.R.S.

On the Estimation of Sulphur in Coal Gas.—Mr. W. Marriott, F.C.S.

Notes on Thermal Equivalents—(1) *Fermentation.* (2) *Oxide of Chlorine.*—Mr. J. Dewar, F.R.S.E.

On the Discrimination of Fibres in Mixed Fabrics.—Mr. J. Spiller, F.C.S. The author refers to the fact that silk alone, of all the materials ordinarily used in the production of textile fabrics, is soluble in concentrated hydrochloric acid, so that this reagent may be resorted to for testing the purity of silk, and determining the proportion of this substance entering into the composition of a variety of mixed goods. For the purpose of identifying wool in the presence of cotton, flax, jute, &c., it is recommended to immerse the fabric or loosened fibres in a warm aqueous solution of picric acid, which dyes the wool of a bright yellow, without imparting any colour to cotton. Thus, by testing mixed fabrics successively with hydrochloric and picric acids, valuable indications are afforded regarding their constitution. The chemical properties of certain compounds formed in this manner from silk were described, and a photographic application pointed out which turns to account the extraordinary degree of sensitiveness to light exhibited by a modified form of argentic chloride produced by precipitation from such hydrochloric solutions.

The Chemical Section did not sit on Saturday, but a large party of the members, headed by Professor Roscoe, Mr. C. W. Siemen, F.R.S., and Mr. A. E. Fletcher, F.C.S., Alkali Act Inspector, spent the day in visiting various chemical works at St. Helens.

SECTION C.—GEOLOGY

Report on Hæmatite.—Professor Stokes.

On the Green Slates and Porphyries of the Lake District.—Professor Harkness and Dr. Nicholson. The authors pointed out that in the Lake Country there is most commonly the three-fold series of rocks, occurring between the Skiddaw slates, upon which the green slates and porphyries rest, and the Coniston limestone which succeeds them. This series consists of traps, which form the lowest member of the group, and which are very persistent; of ashes, trappian breccias, tuffs, and amygdaloids, which constitute the middle portion of the series, and of hornstone traps, which form the highest portion of the group. The middle and upper portion of the series are by no means so persistent in their mineral characters as the lower member. The upper member was shown to be absent from the series in the Long Sleddale Valley, its place being represented by ashes and trappian breccias, and the middle series, which is usually composed of slaty ashes, breccias, and traps, was shown to be greatly modified in type in the northern portions of the Lake Country. This middle member of the series is well seen in the country south of Keswick in Borrowdale, having its normal mineral character more abounding in ashy beds, here worked for slates. Eastwards from this the slaty ashes become less abundant, and in the districts north of Ulleswater these ashes were almost entirely absent. In the Vale of St. John, porphyry, with comparatively small felspar crystals, makes its appearance in connection with the ashy slates. North-east from the Vale of St. John this porphyry becomes more developed, and the ashes are less abundant. At Eycott Hill, near Troutbeck station, the lower porphyry also occurs very finely exhibited, and containing large crystals of felspar. Near the south-east of Carrickfell, a hill largely composed of syenite rocks is seen, having a highly crystalline character, and exhibiting all the features of Diorite. These rocks the authors regard as highly metamorphic conditions of the porphyry above referred to or resulting from the action of the syenite of Carrickfell. The Caldbeck fells, which form the northern limits of the Lake district, have on their northern side the representatives of the green slates and porphyries well developed. Here there is an entire absence of the ashy slates of the middle series, the place of these being occupied by a porphyry similar to that of Eycott Hill. This porphyry, however, in the neighbourhood of Roughtongill has been so far influenced by the syenite of Carrick, veins of which penetrate it, that it now assumes a crystalline form, and appears as hypersthene rock.

The average thickness of the green slates and porphyries, or their representatives, in the Lake Country, the authors think does not exceed 5,000 or 6,000 feet, a thickness made up of igneous products, which in the north of England represent the upper Llandeilo series, and a large portion of the Caradoc or Bala group.

The authors regard the terms green slates and porphyries as applied to these rocks in Cumberland and Westmoreland, as inappropriate, since they express merely local conditions, and they propose as a substitute the name Borrowdale series.

On the discovery of Upper Silurian Strata in Roxburgh and Dumfries.—Mr. C. Lapworth. South of the great axis of the lower Silurian rocks of southern Scotland, upper Silurians had been determined many years ago by Professor Harkness in the neighbourhood of Kirkcudbright. The author had further detected these beds in a very wide strip of country extending from near Lockerbie to near Hawick, a distance of forty miles, with a maximum width of eight or ten miles. Another large outlier had been detected extending from Ernton Hill to Oxnam near Jedburgh; and a further small outlier high up in the Cheviots at the head of Kale Water. The rocks in all these districts have the facies of the Kirkcudbright series, and the fossils are common to all of them. They consist of *Graptolithus colonus, Priodon, Flemingii* and *Nillsoni, Rhynconella nucula, Orthoceras tracheale,* fragments of *Pterygotus, Ceratiocaris* or *Dictyocaris,* &c.

On the Age of the Wealden.—Mr. E. W. Judd. The Wealden constitutes one great continuous formation with well-defined palæontological characters. As with the "Poikilitic" series, its beds can only be referred to the different members of our established marine classification by violent and arbitrary divisions. It must therefore stand as one of the terms of that new system of terrestrial classification which Prof. Huxley has shown must be founded. The epoch of the English Wealden commenced towards the close of the Oolitic period; it continued during the whole of the Tithonian (a great system of rocks lately discovered on the Continent), and ceased towards the end of the middle or beginning of the Upper Neocomian. The passage of the Upper Oolite into the Wealden, and that of the Wealden into the Upper Neocomian, was each of them gradual. Fresh water deposits were formed continuously, but not contemporaneously over the whole area of the Wealden, so that in the north-west we find only the lower beds represented, and in the south-east only the upper ones, while in the central portion we find the whole series complete. In the little marine band of Punfield, only twenty-one inches thick, we have the representative of a portion of the Middle Neocomian, a formation only elsewhere found in this country in the middle of the Speeton clay and in Lincolnshire. The fauna of the marine band of Punfield has very striking affinities with that of the coal-bearing strata of Eastern Spain (which are more than 1,600 feet thick), and especially with the middle portion of that series. The North German Wealden, which is quite unconnected with that of England, and is probably the product of another river, is not strictly contemporaneous with the latter, for while it appears to have commenced about the same period, its duration was considerably less, it having terminated the close of the Lower Neocomian.

On the Physical Geology of the Bone Caves of the Wye.—Rev. W. S. Symonds. The author had lately received from Sir J. Campbell fossil bones from Arthur's Cave, on the Great Doward, on the right bank of the Wye, among them the teeth of *Equus fossilis,* and a collection had been sent to Prof. Owen, and been determined by him to belong to the mammoth, rhinoceros, hyæna, reindeer, and the great fossil ox. The caves of the Wye were at a considerable height above the bed of the river. The author considered that the district had been submerged below the glacial seas, the clefts of the precipices and the mountain limestone platform being often covered with boulder clay and angular erratic stones.

On Geological Systems and Endemic Diseases.—Dr. Moffat. From continued observation during a long practice, the author has ascertained that in a carboniferous district goitre and anœmia were prevalent, while these diseases were markedly absent in the New Red Sandstone. This he attributed to the presence of iron in the one set of rocks, and its complete absence in the other.

Report of Sedimentary Deposits of River Onny.—Rev. De la Touche. The author gave an estimate of the rate of accumulation of these deposits from a series of observations.

Notes of a Recent Visit to the Great Tunnel through the Alps and of several points of Geological Interest suggested by the Conditions of the Works in the present nearly complete state.—Prof. Ansted. The operations of the tunnel involved a direct cut through a series of rocks on a line at a great depth below the surface. At the middle of the tunnel there is 5,400 feet of rock above it. Observations on physical phenomena have been made throughout the progress of the works. The temperatures at various distances and depths have been recorded. Borings 10 feet into the rock were made at intervals of 500 metres, and the temperature determined at their extremity. The last observation made in his presence was 20,342 feet from the south end of the tunnel, and 5,000 feet from the surface. The temperature shown was 80·5° F., exhibiting an increment of 1° F. to about 100 feet. Before the value of these observations could be ascertained, the mean annual surface temperature and the depth of the permanent temperature lines must be determined. On the 31st of last month (August) there remained 2,000 out of 40,000 feet of rock to pierce; as the operations progress at the rate of 500 feet per month, the tunnel may be expected to be completed by the end of the year.

On some Points in the Geology of Strath, Isle of Skye.—Profs. King and Rowney. The authors entered into a minute description of the rock structure of the district, from which he drew the following conclusions:—That the ophite of Skye is an altered rock of the Liassic period; that it possesses the same microscopic features as the ophites of earlier geological ages, occurring in Canada, Connemara, and elsewhere; and that igneous action, developing a granitic rock, and producing decided metamorphism in an adjacent deposit, has operated at a later geological period in Skye than in any other part of the British Islands. The authors further maintained that all the features of the so-called organism, *Eozoon canadense,* had been detected by him in the Skye ophites, and asserted that the different structures known as chamber casts, canal system, nummuline layer, &c., were chemical and structural changes, induced by metamorphic action.

Section D.—Biology

On a mode of Reproduction by Spontaneous Fission in the Hydroida.—Prof. Allman, F.R.S. The hydroid in which the phenomenon which formed the subject of this communication had been observed is a campanularian, which in the general form of its trophosome comes near to *Obelia dichotoma.* Since however no gonosome was present in any of the specimens collected, it was impossible to assign to it a systematic position otherwise than provisionally. The remarkable physiological act by which it is distinguished, associated as this is with an important morphological character, would seem to indicate a distinct generic rank, and the author accordingly suggests for it the provisional generic name of *Schizocladium.*

Besides the branchlets, which—as in the hydroids generally—support the hydranths (plypites), others are developed in abundance from all parts of the hydrocaulus. These commence just like the ordinary branchlets, as offshoots from the hydrocaulus, and consist as usual of a continuation of the cœnosarc, invested by a chitinous perisarc. Unlike the ordinary branchlets, however, they never carry a hydranth, but are destined for the multiplication of the colony, by a process of spontaneous fission.

After the entire branchlet has attained some length, the contained cœnosarc continues to elongate itself. In doing so it ruptures the delicate pellicle of chitine which closes the extremity of the branchlet, and extends itself quite naked into the surrounding water.

It is now that the process of fission commences. A constriction takes place in the cœnosarc at some distance below its free extremity, and in the part still covered by the chitinous perisarc. The constriction rapidly deepens, and ultimately cuts off a piece which slips entirely out of the perisarcal tube and becomes a free zooid, while the surface of disseveration soon heals over, and the axial cavity of the free frustule becomes here as completely closed as at the opposite end.

The detached fragment strikingly resembles a *planula* in all points except in the total absence of vibratile cilia. It attaches itself by a mucous excretion from its surface to the walls of the vessel, and exhibits slight and very sluggish changes of form.

The further history of the fission-frustule was traced, and the important and unexpected fact was shown that it never directly develops a hydranth. After a time a bud springs from its side, and it is from this bud alone that the first hydranth of the new colony is developed.

The bud which thus becomes developed into the primordial hydranth remains attached to the fission-frustule which forms for it a sort of hydrorhiza, but which would seem ultimately to perish and give place to true hydrorhizal filaments. In the meantime the primary bud emits others, and a complex branching colony is the result.

The author compared the fission-frustule to the free medusoid element of other hydroids with which it agreed in never becoming directly developed into a new trophosome, but from which it differed in the very important fact of taking no part in the true generation of the hydroid and in giving origin to a new colony only by a simple non-sexual multiplication.

Observations on Protandry and Protogyny in British Plants.—Alfred W. Bennett, F.L.S. The arrangement of the reproductive organs in hermaphrodite plants, the presence in the same flower of both pistil and stamens, suggested to the minds of the older botanists no other idea than that of fertilisation. It is, however, now generally admitted that, even in hermaphrodite flowers, cross-fertilisation is the rule, self-fertilisation the exception. Two sets of facts have been especially observed,—in particular by Darwin in this country, Hildebrand in Germany, and Delpino in Italy,—to favour cross-fertilisation in hermaphrodite flowers; the phenomena of dimorphism and trimorphism, and the special arrangements which render it easier for the pollen to be brushed off by insects visiting the flower than to fall on its own stigma. But, besides these, another arrangement exists by which self-fertilisation is hindered, the simple fact that the stamens and pistil belonging to the same flower are frequently not ripe, so to speak, at the same time. The terms *Protandry* and *Protogyny* used by Hildebrand to express, in the one case the development of the stamens before the pistils, in the other case the development of the pistil before the stamens, are so convenient and expressive that they have been adopted in this paper; the term by which he expresses that the two organs are matured simultaneously, "Non-dichogamy," does not seem so happy, and the author proposes to substitute for it *Synacmy*—the phenomena of *Protandry* and *Protogyny* forming together that of *Heteracmy*.

The most frequent arrangement appears to be that the pollen commences to be discharged from the anthers at a longer or shorter interval before the maturing of the stigma. In some cases there still remains a certain quantity of pollen in the anthers when the stigma is ready to receive it; in other cases, the anthers have either withered up or entirely dropped off before fertilisation of the ovules can possibly take place. Synacmy, or the contemporaneous maturing of the reproductive organs, is nearly as frequent as protandry; while protogyny is a phenomenon of far less common occurrence. The two extremes among the species observed may be stated to be *Campanula rotundifolia* and *Scrophularia aquatica*. In some Natural Orders, as *Leguminosæ* and *Labiatæ*, all the species examined, with scarcely an exception, range themselves in one or other of the three classes; while in others, as *Rosaceæ*, they are distributed over all three, and in some instances, even closely allied species of the same genus differ in this respect, as, for instance, *Potentilla* and *Ranunculus*. Careful observations might even, the author thinks, in some cases, derive from this point a useful diagnosis of difficult species.

In those Natural Orders in which the flowers are furnished with two sets of stamens of different lengths, it is most usual for the longer ones to discharge their pollen at an earlier period than the shorter ones, and they probably have different functions to perform. This is commonly the case with *Cruciferæ*, *Carophyl-leæ*, *Geraniaceæ*, and *Onagrariceæ*, but not, apparently, with *Labiatæ* or *Scrophularineæ*. The same phenomenon is found in those orders where the numerous stamens are arranged in different whorls, as *Ranunculaceæ* and *Rosaceæ*. The author then gave an account of a number of observations on British wild plants.

SECTION E.—GEOGRAPHY

The Ruined Indian Cities of Central America.—Captain Carmichael. The author commenced by giving an account of the impression caused on first beholding these ruins, and showed how the question involuntarily suggested itself as to the originality of their architectural designs, and stated that a certain familiarity of trait and outline was invariably recognised; and that in his opinion, formed from personal investigation, the architecture of the Aboriginal Indians of Central America was but a diversified reproduction of that of Eastern countries. He then pointed out a number of similarities in their architecture, designs, customs, &c., to nations of the East; and showed how, as a general rule, it was very difficult to explore these ruins owing to the hostility of the existing tribes of Indians.

As regards their antiquity, he assigned to many of them an earlier foundation than that accorded to them by Stephens and Squier, and adduced some very convincing, if novel, proofs in support of his theory. The picture he drew of the palaces of Quiché in Guatemala fully bore out the statement of Torquemada that they rivalled those of Montezuma; and he showed that if that city—one of some eight hundred years' standing—was in such a perfect state of conservation some fifty years ago, that the padre of a neighbouring Indian village who then walked among its streets and palaces, imagined himself in Spain, what must be the era of those numerous cities compared with which Quiché was modern?

He then pointed out the great length of these ruined cities, and added that in connection with this a remarkable fact had seemingly been overlooked by most Central American writers, viz., that the stone buildings whose ruins we now find extant were used as temples, palaces, and public offices generally, the poorer inhabitants living in huts of a perishable nature; an arrangement which represented an almost incredible amount of population. He then analysed the various elements composing the architecture of the ruined buildings and monuments, and gave an interesting account of the various uses to which the teocali and tumuli were put by the Toltecan and Aztec priests, viz., for sacrificial and burial purposes, to serve as beacons, for warlike defences, &c.; and explained the relations between the temples and alcazars or palaces, and offered a few hints as to the deciphering of the hieroglyphics, a subject to which he has paid much attention, and for which he is specially qualified from his knowledge of the Maya or Indian language, showing that they were chiefly the works of the Indian priesthood; and, above all, were intended to inculcate moral and religious precepts, chronological events being made quite subservient to them. He then referred briefly to the round towers which contained the estufas for the sacred fire of Montezuma, in connection with the worship of the Sun, and passed on to explain the nature and significance of the various hideous and awe-inspiring idols to whom the human sacrifice was offered on the summit of the teocali, and stated it as his belief that these idols, as well as the planed stones, were carved with clay or flint instruments, as he had often found flint and obsidian implements, but in no instance an instrument of metal.

Referring to the state of decay in which they were mostly found, he stated that there were ruins which had never been visited by the Spaniards at the time of the conquest, and expressed it as his opinion that their crumbling and ruinous condition was mainly brought about by the earthquakes so prevalent throughout Central America, in conjunction of course with the action on them of time and the elements. He gave a most interesting account of a ruined city in British Honduras, called Xmul, which he claims to have discovered, and concluded by pointing out the great extent of unknown and untravelled districts in Central America, particularly in Guatemala, as presenting a fine field for future geographers and naturalists, and expressed it as his firm conviction that there existed at the present day an Indian city—yet to be discovered—whose inhabitants occupy the same splendid palaces and temples as in the days of the Spanish Conquest, whose priests inscribe fresh precepts on their tablets, and who would then read to us their now mystical hieroglyphics. He supported the statement by describing an exploration he made in the southern district of British Honduras westwards towards Guatemala, where, after several days' perilous river navigation and further journey on foot, he discovered in the neighbourhood of the Coxcomb Peak the remains of an abandoned maize plantation, and saw smoke ascending from the distant forest; and believes that the tribe of Indians who occupy this part of the country, which was before considered to be uninhabited, have some connection with the mysterious Aztec city he spoke of.

Mr. H. Howorth corroborated the date of the foundation of the city of Quiché, as verified by a Mr. Spencer, who had also read a hieroglyphic on the lintel of a doorway at Palenqué. Mr. Spencer had also discovered great similarity between the names of the signs of the zodiac and the arrangement of the calendar of the Aztecs and those in use in Thibet. Several members present took part in a discussion, to which Captain Carmichael replied, adding that he had recently returned from California, where he had heard a Japanese and a digger Indian of Nevada, then brought together for the first time, converse intelligibly.

Section G.—Mechanical Science

On the Sewage of Liverpool and Neighbourhood.—Mr. J. N. Shoolbred, C.E. In this paper the author said it was calculated that about 900,000*l.* had been expended in drainage and sewage works in the borough of Liverpool. Of this about 600,000*l.* might be set down as requisite for drainage (the primary object); 300,000*l.* would, therefore, represent the amount due to conveyance of water-closet sewage; and setting aside 20 per cent. upon this as interest, together with a large amount for depreciation and repairs, &c., there would be 60,000*l.*, or about 2*s.* 4*d.* per head, as the annual expense due to the removal of the water-closet sewage. Taking the length of sewerage over the whole of the borough of Liverpool, the probable average distance that the sewage has to travel from its entrance into the sewer till its discharge into the river Mersey is about one mile and a half, or perhaps even more, inasmuch as the inhabited portions of the town mainly lie back away from the river. Liverpool and its neighbourhood are favoured by nature with above the average amount of facilities; first, for the collection of its sewage by water carriage, and then in finding, at a comparatively short distance, an outlet in the river Mersey for the larger portion which it is deemed advisable to get rid of in this manner. Again, Liverpool is fortunate in having an outlet which, at least for the present, secures immunity to the town from the unpleasant consequences which sometimes arise from creating an acknowledged sewage nuisance; while, should the town itself at any time prefer to derive some benefit out of this refuse which it now throws away, Liverpool possesses, at no great distance, a most suitable and extensive site for utilising its sewage by irrigation upon the land, with at the same time a certain market in itself for the vegetable produce of that irrigation. The author then detailed the arrangements made in 1866 and since, or in progress, for the concession of the Liverpool sewage to companies whose object is to use it in irrigation upon sewage farms; and he also described the nature of the lands which are, or may be, used for sewage irrigation.

On Sewers in Running Sand.—Messrs. Reade and Goodison, Civil Engineers. This paper formed a very natural sequel to the preceding one, inasmuch as it dealt with the difficulties of constructing sewers in the sands between Liverpool and Southport, the district traversed by the Liverpool, Crosby, and Southport Railway, and the means which the authors had adopted in order to overcome those difficulties. The whole of the district referred to is one mass of sand, resting on a bed of moss and marl, varying from ten to twenty feet deep below the surface. It has no natural drainage, and in wet seasons in the lower portions or slacks, flushes of water form, in consequence of the elevation of the shore line of sand-hills. After enumerating the manifold difficulties which they had to encounter, the authors described at considerable length the appliances which they ultimately resolved upon using. The primary object which they wished to attain was to get a dry subsoil wherein to lay the pipes, that the cement joints might have time to set and become water-tight, and by securing more time for the laying of the pipes, laying a greater length at a time, and the prevention of disturbance or drawing of the pipes while preparing the next excavations, to ensure greater certainty and perfection in the gradients and junctions, and consequently improve the general system of pipe sewerage. From the experiments which they have made with the subsoil drains, they have thoroughly satisfied themselves as to their efficiency. The subsoil drains are in fact a foundation for the pipes of the sewer, and the sewer itself can be as readily constructed upon them as if the ground were perfectly dry. They have begun to use the subsoil drains in the sewerage works which they are carrying out at Birkdale, a length of 10,000 yards.

On the Ashpit System of Manchester.—Alderman Rumney. The authorities of Manchester have at all times objected to the general use of water-closets in cottage dwellings. In the first place, because they believed that in the limited space available in houses occupied by the working classes, they would prove a greater nuisance than the privy and ashpit outside; secondly, because of the loss of valuable manure which would be occasioned; and thirdly, because, looking at the rapid increase of population in the district, and the limited area of the watershed, the time would come when all the water available would be required for domestic and manufacturing purposes, and could not be wasted in water-closets. Adhering, then, to the dry in opposition to the wet system, the corporation has for some time been engaged in the attempt to improve the existing privies and ashpits, and to discover the best form to be adapted in all new property erected within the city. In the construction of ashpits, the object of the Health Committee appointed by the corporation was to prevent as far as possible the decomposition of the excreta, and consequent generation of gases passing off into the surrounding atmosphere; and as decomposition is accelerated by moisture, they determined that all ashpits should be made dry, excluding the rainfall by covering them over, and the drainage from the yard by requiring the floors and walls to be made water-tight; they required also that the ashes from the pit should be placed daily in the ashpit for the purpose of condensing as far as possible the ammoniacal and other gases, and preventing organic matter impregnating the air in the immediate vicinity. In addition to these arrangements, it was foreseen that in summer time, when decomposition is most vigorous, and the supply of neutralising ashes most scanty, a closed ashpit might become a greater nuisance than an open one, and a ventilating shaft or chimney was determined upon, to be carried from the top of the ashpit up to the side of, and a little above, the eaves of the house, for the purpose of carrying off all the gases and light vapours, and allowing them to mix with the surrounding atmosphere at an elevation which would not injuriously affect the inmates of the dwelling. In the Appendix to the Report of the Medical Officer of the Privy Council just printed, Dr. Buchanan and Mr. Radcliffe express themselves in high terms regarding the new system. Already upwards of 1,500 new privies and ashpits have been erected under the supervision of the committee; the occupants of the houses are perfectly satisfied, and are constantly expressing their approval.

When the reading of the papers was concluded, a long and animated discussion followed, the speakers being Professor A. Reynolds, Mr. Brierly (of Blackburn), Professor Ansted, Mr. Rawlinson, C.B., C.E., Mr. Glazebrook, Mr. Hawkesley, C.E., Professor A. W. Williamson, and Mr. Hope, V.C.

The first paper in this Section on Saturday gave rise to a very unpleasant occurrence, in which the President, according to general report, adoped a rather unusual proceeding. His treatment of the author was somewhat unceremonious. The paper was entitled

On a new Heat Engine.—Mr. A. W. Bickerton, F.C.S., Associate of the Royal School of Mines. After the author had well-nigh done reading his paper, the President quite unexpectedly stopped him, and told him that he had been talking absolute nonsense. If he (the President) had seen his paper first, he would not have permitted it to be read, as there was no time to discuss a thing which was radically fallacious. The idea of an engine worked by the expansion of nitrogen under the influence of heat was fallacious in principle and practically impossible. A gentleman in the body of the room said he had listened to the paper with great interest, and regretted that it had not been allowed to be concluded. This remark was received with applause by the audience, and still greater applause followed when the author said, in retiring, that time would show whether the President or the principle was right. It seems clear that, having been accepted by the Committee of the Section, and the title of it placed upon the programme of business, the paper was entitled to be read. We are glad to lay a short abstract of the "burked" paper before our readers, expressing no opinion, further than that opinion should not be stifled.

The principle of the engine is as follows:—Crude nitrogen gas is heated in a serpentine system of tubes until the pressure is double that of the air. It is then admitted into a cylinder in which it presses forward a piston, and is allowed to expand. Next it passes into an apparatus where it is cooled, and consequently diminished to half its bulk. The cooling is effected in a new arrangement, which is so constructed that the whole of the heat above that of the external air is transferred to an equivalent volume of air passing in an opposite direction. This heated air is then used as a blast for the fire, $\frac{1}{10}$ going to the hearth of the furnace through a tuyere, and $\frac{4}{10}$ mixing with the products of combustion immediately above the fire, so as to complete any imperfect combustion, and also to modify the temperature of the whole mass, so that it may not be likely to injure the iron of the gas tubes, and the remaining $\frac{5}{10}$ being introduced into the system at a point further on. The construction of the system of tubes is such that, by the time the products of combustion reach the open air, they shall have parted with nearly all their heat, and transferred it to the nitrogen contained in the tubes, and hence a chimney draught cannot be used, and the blast has to be produced by a blowing engine. The nitrogen, after having been cooled to half the volume it occupied in the first cylinder, is then compressed and forced into the system of tubes at the point furthest from the fire. It is this forcing the gas back again into the system of heating tubes that appears absurd; but it must be remembered that the gas while leaving the heating tubes occupies twice the space it does when being forced back, hence it fills a cylinder of twice the area, and the force that may now be disposed of is equal to half the pressure exerted in the larger cylinder. But the other half of the power is not lost, it is simply conveyed back to the heating tube, and is used again. The only losses that can arise are those which are incidental to all engines, such as radiation, conduction, &c., inasmuch as there is avoided the enormous loss of heat that usually goes up the chimney, together with the still greater loss that is constantly being carried away by the condensed water,—an amount in itself six times as great as that converted into work in the steam-engine. The inventor considers that he does not expect too much if he expects his new heat engine to convert 60 per cent. of the heat of combustion into work, a duty that is fully 500 per cent. above that of well-constructed steam-engines.

Of course, as the author was not permitted to finish the reading of his paper, no discussion was taken upon it.

REPORTS OF COMMITTEES

THIRD REPORT OF UNDERGROUND TEMPERATURE COMMITTEE

Mr. G. J. Symons, whose observations, extending to a depth of 1,100 feet in a well at Kentish Town, were reported at last meeting, has since repeated his observations at several depths.

The first 210 feet of the well (which is eight feet in diameter to the depth of 540 feet) are occupied by air, and in this portion of the well the second series of observations give temperatures exceeding those observed in the first series by from 2° to 5°, the excess diminishing as the depth increases. The second series were taken in July and August, whereas the first series were taken in January. It is evident that, in this portion of the well, in spite of the precautions taken to exclude atmospheric influences, by boarding over the well and erecting a hut over it, the temperature varies with the seasons, the variations being in the same direction as in the external air, but smaller, and diminishing as the depth increases, but still amounting to 2°·2 at the depth of 200 feet.

We can feel no certainty that even the mean annual temperature in this portion of the well represents the temperature in the solid ground. On the contrary, the mean temperature in the well at any depth is probably intermediate between the temperature of the solid ground at that depth and the mean temperature of the external air.

It is well that such observations should have been carefully made and recorded in this one instance, if only for the sake of warning; and they show that we cannot expect to attain the object for which the committee has been appointed by observations in large shafts filled with air.

Mr. Symons has also repeated the observations at 250 feet (which is 40 feet under water), and at the depths of 600 feet, 750 feet, and every fiftieth foot from this to 1,100 feet, which is the lowest point attainable on account of the mud, which extends 300 feet lower. The differences from the results obtained last year are + ·2, − ·3, − ·4, − ·2, − ·2, 0, − ·1, − ·1, 0; which, upon the whole, strongly confirm the correctness of the observations.

The temperature at 1,100 feet is 69·8, which, if we assume the mean temperature of the surface of the ground to be $\left\{\begin{matrix} 50^\circ \\ 49^\circ \end{matrix}\right\}$, gives a mean increase downwards of $\left\{\begin{matrix} \cdot 0180 \\ \cdot 0189 \end{matrix}\right\}$ of a degree Fahrenheit per foot, or 1° for $\left\{\begin{matrix} 55\cdot 5 \\ 52\cdot 9 \end{matrix}\right\}$ feet.

The curve in which temperature is the ordinate and depth the abscissa, exhibits considerable irregularities till we reach the depth of 650 feet, beyond which it is nearly a straight line, and represents an increase of ·0187 of a degree per foot.

The strata penetrated by the well to the depth to which our observations extend, consist of clay, sand, chalk, and marl, besides flints. (See tabular list appended.)

Mr. Symons, in his report, calls attention to the anomalous position of a column of water, increasing in temperature and, consequently, diminishing in specific gravity downwards, and suggests the inquiry why the warmer and lighter portions do not ascend to the top. The proper reply seems to be that the diminution of specific gravity, amounting to less than one part in 50,000 per vertical foot, does not furnish sufficient force to overcome liquid adhesion, and the water is thus able to remain in unstable equilibrium.

Mr. Symons intends during the remainder of the present year, verifying those of his observations which have not yet been repeated, and concludes his report by remarking that it appears desirable to ascertain by observations from year to year, whether the temperature at a given depth (say 1,000 feet) remains constant or is subject to minute changes, periodical or otherwise—a suggestion which appears fully worthy of being carried out.

Mr. Wm. Bryham, manager of Rosebridge Colliery, Ince, near Wigan, has taken very valuable observations during the sinking of that colliery, which is now the deepest excavation in Great Britain. The principal results have already been given in a paper to the Royal Society by Mr. Edward Hull, director of the Geological Survey of Ireland, who had previously published some important contributions to our knowledge of underground temperature, and has now consented to become a member of this committee. Some of the depths have however been re-measured since Mr. Hull's paper was read, and we are now enabled, through the kindness of Mr. Bryham, to furnish a rather more accurate report.

The temperatures observed, and the depths at which they were taken, are as follows:—

Depth in Yards.	Temperature Fahrenheit.
161	(64½)
200	([illegible]6)
558	78
605	80
630	83
663	85
671	86
679	87
7[illegible]4	88½
745	89
761	90½
775	91½
783	92
800	93
806	93½
815	94

All these temperatures, except the two first, were observed during the sinking of the shaft, by drilling a hole with water, to the depth of a yard, in the solid strata at the bottom. A thermometer was then inserted, the hole was tightly plugged with clay so as to be air-tight, and was left undisturbed for half an hour, at the end of which time the thermometer was withdrawn and read—a mode of observation which appears well adapted to give reliable results. With respect to the temperatures at 161 and 200 yards (which are enclosed in brackets to indicate uncertainty), Mr. Bryham says that he has some doubt as to the correctness of the thermometer with which they were taken, and that they were not taken in the shaft at the time it was sunk, but in the seams at the depths named.

Asuming the surface temperature to be 49°, we have, on the whole depth of 815 yards, or 2,445 feet, an increase of 45°, which is at the rate of ·0184 of a degree per foot, or a degree for every 54·3 feet.

On plotting the temperature curve, including the two observations marked as doubtful, we find that it naturally divides itself into four portions, which are approximately straight lines.

The most remarkable of these portions is the second from the top, extending from the depth of 161 yards to that of 605 yards. It embraces 1,332 feet, and shows an increase of only 1° for every 86 feet.

The third portion, extending from the depth of 605 yards to that of 671 yards, covers only 198 feet, and shows an increase of 1° for every 33 feet.

The lowest portion extends from the depth of 671 yards to 815 yards. It covers 432 feet, and shows an increase of 1° in 54 feet.

The topmost portion will be affected by the assumption we make as to surface temperature. Assuming this as 49°, it shows an increase of 1° in 31 feet.

It is interesting to compare the Rosebridge observations with those previously made by Mr. Fairbairn at Astley Pit, Dukinfield, Cheshire, which have been described by Mr. Hull in "The Coalfields of Great Britain," and by Mr. Fairbairn himself in the B. A. Report for 1861. The results have been thus summed up by Mr. Hull:—

1. The first observation gives 51° as the invariable temperature throughout the year at the depth of 17 feet. Between 231 yards and 270 yards the temperature was nearly uniform at 58·0. And the increase from the surface would be at the rate of 1° F. for 88 feet.

2. Between 270 and 309 yards, the increase was at the rate of 1° for 62·4 feet.

3. Between 309 and 419 yards, the increase was at the rate of 1° for 60 feet.

4. Between 419 and 613 yards, the increase was at the rate of 1° for 86·91 feet.

5. Between 613 and 685 yards, the increase was at the rate of 1° for 65·6 feet.

The result of the whole series of observations gives an increase of 1° for every 83·2 feet.

Mr. Fairbairn's own summary is as follows:—"The amount of increase indicated in these experiments is from 51° to 57¾°, as the depth increases from 5⅔ yards to 231 yards, or an increase of 1° in 99 feet. But if we take the results which are more reliable, namely, those between the depths of 231 and 685 yards, we have an increase of temperature from 57¾° to 75½°, or 17¾° Fahrenheit. That is a mean increase of 1° in 76·8 feet."

Mr. Fairbairn here, by implication, throws doubt on the alleged invariable temperature of 51° at the depth of 17 feet, a determination which in itself appears highly improbable, seeing that at Greenwich the thermometer, whose bulb is buried at a depth of 25·6 feet, exhibits an annual range of 3°·2, while that at the depth of 12·8 feet exhibits a range of 9°. But even if we assume the mean surface temperature to be 49°, we have still upon the whole depth an increase at the rate of 1° in 80 feet, as against 1° in 54·3 feet at Rosebridge.

Mr. Fairbairn's paper gives also the results obtained at a second pit at Dukinfield, which agree with those in the first in showing an exceptionally slow rate of increase downwards. The temperatures at the depths of 167½ yards and 467 yards were respectively 58° and 66½°, showing a difference of 8½° in 299½ yards, which is at the rate of 1° in 106 feet. The increase from the surface down to 167½ yards, assuming the surface temperature as 49°, would be 9°, or 1° in 56 feet, and the mean rate of increase from the surface to the bottom would be 1° in 80 feet, the same as in the first pit.

A tabular list of the strata at Rosebridge is appended to this report. A full account of the strata at Dukinfield is given in Mr. Fairbairn's paper (B.A. Report, 1861).

With strata so nearly similar and in two neighbouring counties, we should scarcely have expected so much difference in the mean rates of increase downwards. In this respect, Rosebridge agrees well with the average of results obtained elsewhere. Dukinfield far surpasses all other deep mines or wells, so far as our present records extend, in slowness of increase.

This implies one of two things, either that the strata of Dukinfield afford unusual facilities for the transmission of heat, or that the isothermal surfaces at still greater depths dip down in the vicinity of Dukinfield.

Mr. Hull has called attention to a circumstance which favours the first of these explanations, the steepness of inclination of the Dukinfield strata. He argues, with much appearance of probability, that beds of very various character (sandstones, shales, clays, and coal), alternating with each other, must offer more resistance to the transmission of heat across than parallel to their planes of bedding, as Mr. Hopkins has shown that every sudden change of material is equivalent to an increase of resistance; and it is obvious that highly inclined strata furnish a path by which heat can travel obliquely upwards without being interrupted by these breaches of continuity.

To this suggestion of Mr. Hull's it may be added that inclined strata furnish great facilities for the convection of heat by the flow of water along the planes of junction. It appears likely that surface water, by soaking downwards in this direction, may exercise an important influence in assimilating the temperature at great depths to that which prevails near the surface. Mr. Hull's own statement of his views is given in the foot-note below.*

Mr. McFarlane has been prevented from continuing his observations near Glasgow during the past year by the press of business incident to the removal from the old to the new college.

Mr. F. Amery, Druid House, Ashburton, Devon, has taken some observations with one of the Committee's thermometers in the shaft of a mine which had been unused for a year, and was nearly full of water. The shaft is 12ft. × 7ft., and descends vertically for 350ft., after which it slopes to the south at an angle of 50°, continuing to the depth of 620ft. The water stood at 50ft. from the surface. Mr. Amery observed the temperature at every 50th foot of depth in the vertical portion, and found it to be 53° at all depths, except at 250ft. and 200ft., where it was 53·4 and 53·2 respectively. A copper lode crosses the shaft at the depth of 250ft.; and it appears to be generally the case in the Cornwall and Devonshire mines, that copper lodes exhibit a high temperature, a circumstance which Prof. Phillips explains by the conformation of the strata, which is such as to cause water from greater depths to make its way obliquely upwards by following the course of the copper lodes.

The nearly constant temperature observed from the surface to the bottom of the shaft seems to indicate a large amount of convective circulation. In this respect small bores have a decided advantage.

Mr. G. A. Lebour has taken observations with one of our thermometers in several shafts and bores near Ridsdale, Northumberland, made for working coal and ironstone. Mr. Lebour does not report the temperatures observed, which he characterises as discrepant and utterly valueless, owing, he believes, to the numerous water-bearing beds which they cut through, and the very varying temperature of these waters. Having now, however, found a dry bore, he hopes to make a useful series of observations next winter.

* "Rosebridge Colliery occupies a position in the centre of a gently-sloping trough, where the beds are nearly horizontal; they are terminated both on the west and east by large parallel faults, which throw up the strata on either side. The colliery is placed in what is known as 'the deep belt.'

"Dukinfield Colliery, on the other hand, is planted upon strata which are highly inclined. The beds of sandstone, shale, and coal rise and crop out to the eastward at angles varying from 30° to 35°. Now, I think we may assume that strata consisting of sandstones, shales, clays, and coal alternating with each other, are capable of conducting heat more rapidly along the planes of bedding than across them, different kinds of rock having, as Mr. Hopkins's experiments show, different conducting powers. If this be so, we have an evident reason for the dissimilar results in the case before us. Assuming a constant supply of heat from the interior of the earth, it could only escape, in the case of Rosebridge, across the planes of bedding, meeting in its progress upwards the resistance offered by strata of, in each case, varying conducting powers. On the other hand, in the case of Dukinfield, the internal heat could travel along the steeply-inclined strata themselves, and ultimately escape along the outcrop of the beds.

"I merely offer this as a suggestion explanatory of the results before us, and may be allowed to add that the strata at Monkwearmouth Colliery, the thermometrical observations at which correspond so closely with those obtained at Rosebridge, are also in a position not much removed from the horizontal, which is some evidence in corroboration of the views here offered."—*Proc. Roy. Soc.*, Jan. 27, 1870.

One of the Committee's thermometers has recently been sent to Mr. John Donaldson, C.E., Calcutta, who has expressed his desire to aid in scientific observation, and being now engaged in examining for coal and iron under Government, is likely to render us effective service.

Shortly after the last meeting of the Association, the secretary of this committee addressed a letter to Prof. Henry, secretary of the Smithsonian Institution, U.S., requesting his co-operation in furthering the object which the committee have in view, at the same time forwarding one of our protected thermometers.

In June of the present year an answer was received from Prof. Baird, assistant secretary in charge, to the effect that Prof. Henry's ill-health during the present season had prevented his communicating to us the results of his labours in response to request.

The letter addressed to Prof. Henry made special reference to an artesian well of extraordinary depth which was understood to be in course of sinking at St. Louis, and at the same time a letter was addressed, and a special thermometer sent, to Mr. C. W. Atkeson, the superintendent of the work of boring at St. Louis. No reply has been received from Mr. Atkeson, who appears to have left St. Louis before the letter arrived; but letters have been received through the Smithsonian Institution from Dr. Chas. W. Stevens, superintendent of the County Insane Asylum at St. Louis, this being the institution for whose uses the well was sunk, together with a very interesting newspaper cutting, consisting of Mr. Atkeson's report on the works. The boring of the well was commenced (at the bottom of a dug well 71½ feet deep) on the 31st of March, 1866, and was continued till the 9th of August, 1869, when the work was stopped at the enormous depth of 3,843½ feet, exceeding by more than one-half the depth of Dukinfield Collery. The strata penetrated consisted in the aggregate of 63 feet of clay, 6 feet of coal, 380 feet of shales, 2,725 feet of limestone, and 620 feet of sandstone.

A cast-iron tube of 11½ inches bore was first put down, reaching from the top and secured in the limestone at the bottom. This tube was then lined inside with a wooden tube, reducing the bore to 4½ inches. A 4½-inch drill was put down through this tube on the above-mentioned date. The bore was afterwards enlarged to 6 inches, and subsequently to 11½ inches to a depth of 131½ feet. A sheet-iron tube was then put down, extending from the top to this depth, and the bore below was enlarged, first to 6 and afterwards to 10 inches diamenter, to the depth of 953 feet. A sheet-iron tube, 79 feet long, was then put down, which rests on the offset at the bottom of the 10-inch bore. The 4½-inch bore was then enlarged to 6 inches to the depth of 1,022 feet, and a wrought-iron tube of 5 inches bore, weighing more than six tons, was introduced, reaching from the top and resting on the offset at the bottom of the 6-inch bore, thus securing the work to this depth, and reducing the bore to convenient size to work in. The 4½-inch bore has been continued to the depth of 3,843 feet 6 inches without further tubing.

At the depth of 3,029 feet the first observation of temperature was taken, and the reading of the thermometer was 107° F. This first observation is stated by Dr. Stevens to be specially worthy of confidence, as having been confirmed by several repetitions, or rather, to use Dr. Stevens's own words, "this was the maximum of several trials." It was taken, as well as those that followed it, by means of a registering thermometer (kind not mentioned); but in answer to our inquiries, Dr. Stevens states, upon the authority of the carpenter who attached the thermometer to the pole by which it was lowered, "that no means were taken to defend the bulb from pressure." In the absence of further information (and Mr. Atkeson himself has not yet spoken), we can place no reliance upon the temperature recorded, as the thermometer had to bear a pressure of ⅝ of a mile of water.

The temperatures registered at lower depths, the deepest being 800 feet lower, were all, strange to say, somewhat lower than this, a circumstance which is all the more remarkable because the pressure (which tends to make the reading higher) must have increased with the depth. At the bottom, or rather at 3,837 feet, being 6½ feet from the bottom, the temperature indicated was 105°. Either of these results, taken apart from the other and compared with the surface temperature, would give a result not improbable in itself. The mean temperature of the air at St. Louis appears to be about 53°, but it seems desirable to avoid publishing calculations till the data are better established.

Unfortunately, the apparatus which was employed in boring has all been removed, after the insertion of two wooden plugs, with an iron screw at the upper end of each, one at the offset at a depth of 1,022 feet, and the other at the offset at the depth of 953 feet, for the purpose of separating the fresh from the salt waters. These plugs were driven in with great force, and can only be withdrawn with the aid of a series of poles and other appliances, such as were used in the boring, which will be rather costly. The poles alone are estimated to cost 1,152 dols., or about 200*l.* If the plugs were withdrawn—and, according to Dr. Stevens, there is nothing but the expense to prevent—the whole well would be available for observation. The committee will make every effort to prevent so rare an opportunity from being lost.

The Secretary has also been in correspondence with Messrs. Mather and Platt, of Salford Iron Works, respecting a boring at Moscow, for which they have furnished machinery, and which is to be carried to the depth of 3,000 feet. They refer to General Helmerson, of the Mining College, St. Petersburg, as the best authority to whom application can be made for particulars of the Moscow boring as to temperature, &c. The secretary has accordingly written to General Helmerson, endeavouring to interest him in the objects of the committee, and offering to forward thermometers. No reply has yet been received.

An element which it is necessary to know, with a view to the correct reduction of our observations, but which in many instances it is difficult to obtain by direct observation, is the mean annual temperature of the ground, at or near the surface. Instances frequently occur in which the temperature at the depth of 200, 300, or it may be 500 feet is accurately known, while the temperature in the superincumbent strata can only be guessed at. This is the case at the Kentish Town well, and partially at Rosebridge and Dukinfield collieries.

It is very desirable that in connection with temperatures at great depths there should in each locality be an accurate observation at the depth of from 50 to 100 feet. At such depths in the solid ground before it has been disturbed by mining operations, one observation suffices to give a good approximation to the mean temperature of many years. At depths of two or three feet it is necessary to observe, once a week, or so, throughout a year, in order to get the mean temperature at that depth for that year; and this may differ by a considerable amount from the mean of a series of years.

In the Report of the Scottish Meteorological Society for the quarter ending December 1862, there is a comparison of the mean temperature of the air with that of the soil at the depths of 3, 12, and 22 inches, at four stations, from observations extending over five years; and in the Journal of the same society for the quarter ending December 1865, there is a comparison of the temperature of drained and undrained land from one year's observations, undertaken for this purpose at two stations, and including also a comparison with the temperature of the year. The mean temperature of the air for each day is, in these comparisons, assumed to be the simple arithmetical mean of the maximum and minimum, as indicated by self-registering thermometers 4 feet from the ground. From these observations, it appears that the mean annual temperature of the soil was in every case rather above that of the air, and that the excess was greater for sand than for undrained clay, and was greater for drained land than for the same land undrained.

The greatest excess occurred in the case of the 22-inch thermometer at Nookten (Vale of Leven), where both surface and subsoil are sandy and dry. The five yearly means at this station were:—

Air 46·1; soil at 3 inches 46·3, at 12 inches 47·3, at 22 inches 48·0; giving an excess of 1·9 for the temperature at the depth of 22 inches as compared with air.

The smallest excess, in the case of the 22-inch thermometers, observed for five years, was at Linton (East Lothian), where it amounted to 0·7; but the observations on the effect of drainage gave for the year of observation an excess of only 0·2 at the depth of 30 inches in light sandy but undrained soil under a ryegrass crop, at Otter House near Loch Fyne, the corresponding excess for drained land of the same kind and in the immediate vicinity being 0·9.

The mean temperature at the depth of 3 feet at Professor Forbes' three stations at Edinburgh, from five years' observations, gave an excess of 0·55 above the mean temperature of the air at Edinburgh as determined by Mr. Adie's observations.

Observations on soil temperature in England are much needed, but the Greenwich observations give an excess of soil above air temperature falling within the limits above quoted, the excess at 3 French feet being 1·7, while at 24 French feet it is reduced to 1°. The soil of which the Observatory Hill is composed, and in which the thermometers are sunk, is dry gravel, and the unusual circumstance of decrease of temperature downward observed in the comparison of the 3 feet and 24 feet thermometers, seems to indicate that the surface of the hill is warmer than the surrounding land.

In the present state of our knowledge, then, it appears that when the temperature of the earth has been observed at a depth of some hundreds of feet in any locality in Great Britain, and has not been accurately determined at a less depth, some knowledge of the rate of increase downwards may be obtained, by assuming provisionally that the mean temperature of the surface is about a degree higher than the mean temperature of the air, supposing the latter to be known.

It is to be wished that the Meteorological Society would, from the ample materials in their possession, publish a map of annual isothermals for Great Britain; and the objects of this committee would be greatly furthered by an extensive series of soil temperature observations at the depth of about 3 feet.

The committee are anxious to carry into effect Mr. Hull's proposal (quoted in their last Report) to bore down from the bottom of a deep mine; and as Rosebridge Colliery appears to be an eminently suitable locality for such an operation, the Secretary has consulted Mr. Bryham respecting its practicability and probable cost. Mr. Bryham's reply is that there would be no difficulty in carrying out the proposal at Rosebridge, that to make preparations and bore 300 feet would, on a rough estimate, cost £150, and that the second 300 feet would probably cost about the same sum.

The committee would earnestly appeal to the liberality of the Association to enable them to put this design in execution, and they would remark that the sooner it is carried out, the more valuable the results obtained will be, as the mine has been but recently opened to its present depth, and the influence of atmospheric temperature will every year become more sensible in the strata below.

SCIENTIFIC SERIALS

In the *Annalen der Chemie und Pharmacie* for May 1870, we find several important papers, of which the following are abstracts:—"Investigations on some derivatives of cinnamic acid," by Carl Glaser." In this lengthy and most interesting paper is described the Phenylpropiolic acid $C_9H_6O_2$, a new acid which differs from cinnamic acid by containing H_2 less. It is obtained either by the action of sodium and CO_2 on β bromstyrol, or by the action of alcoholic potash on a bromcinnamic acid. On heating with water to 120°, it splits up into CO_2 and acetenylbenzol $C_2H.C_6H_5$, which latter can also be obtained by the abstraction of 2 HBr from dibromstyrol. Silver, copper, and sodium derivatives of this remarkable hydrocarbon are described, from the latter of which, by the action of CO_2, the author succeeded in regenerating phenylpropiolic acid. The paper concludes with an account of some Cl and Br derivatives of styrol.—"On mercuryditolyl," by E. Dreher and R. Otto. A white crystalline compound obtained by the action of Na_2Hg on bromtoluol. The authors did not succeed in preparing the corresponding mercury compound from the isomeric brombenzyl.—Note on the behaviour of dibenzyl at high temperatures, by the same authors. This compound is split up according to the equation

$$2\left\{\begin{matrix}CH_2C_6H_5\\CH_2C_6H_5\end{matrix}\right. = 2\,C_6H_5CH_3 + \left\{\begin{matrix}CH\,C_6H_5\\CH\,C_6H_5\end{matrix}\right.$$

Toluylen

"Note on the conversion of Thiophenol (Phenylic sulphydrate) into Phenylic disulphide," by the same authors. This is effected by distilling the mercury compound of the former, when it splits up into mercury and the bisulphide, which is probably due to the decomposition taking place at a temperature at which the affinity of Hg for S does not yet come into play, since the homologous toluol and benzyl compounds, which require a much higher temperature, give mercuric sulphide and a monosulphide.—"On two isomeric pentachlorbenzols and bichlorbenzols-chloride," by R. Otto. A short description of the mode of preparation and properties of the above compounds. The existence of two pentachlorbenzols, which now seems to be placed beyond all doubt, is of great interest, as it is one of the very few facts irreconcileable with Kekulé's benzol theory, which does not admit of the existence of more than one.—"On sulphotoluid," by R. Otto and Gruber. Obtained by the action of sulphuric anhydride on toluol.—"On aceto-mercury monomethyl and aceto-mercury monethyl," by R. Otto. These two compounds are prepared by the action of acetic acid on mercuric methide and ethide respectively.

$$\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}Hg + \left\{\begin{matrix}CH_3\\CO\,OH\end{matrix}\right. = \left.\begin{matrix}CH_3\\C_2H_3O_2\end{matrix}\right\}Hg + CH_4$$

—"On the preparation of organic sulphur compounds by means of sodic hyposulphite," by the same author. Alcohol heated with a concentrated solution gives mercaptan; ethylic iodide mercaptan and ethylic sulphide; chlorbenzyl gives benzylic mercaptan and sulphide.—"On diamidonitro phenylic acid, a new derivative of picric acid," by Peter Griess. This acid is prepared by the reduction of picric acid with ammonic sulphide.—"On azobenzol sulphuric acid," by P. Griess. A product of the action of hot fuming sulphuric acid on azobenzol. By fusion with potash, phenoldiazobenzol $C_{12}H_{10}N_2O$ is obtained, which on treatment with ammonic sulphide, is converted into oxybenzidin $C_{12}H_{12}N_2O$.—"On ozone and antozone," by C. Engler and O Nasse. In this lengthy paper are described a long series of experiments, all of which seem to prove conclusively the non-existence of the third modification of oxygen, called by Schönbein and others antozone, and which the authors prove to be hydric peroxide formed by the oxidation of water, which was present in all those cases where the so-called antozone has been observed by ozone.—"On the constitution of arbutin," by Hugo Schiff. The author considers arbutin to be derived from one atom glycose + one atom hydroquinone $- H_2O$, and describes a number of acetyl and benzoyl derivatives in support of his views.—"On the action of hypochlorous acid on allylic chloride," by H. v Geyerfell. The author has obtained the compound $C_3H_6OCl_2$ by direct addition of the elements of hypochlorous acid to allylic chloride. This on reduction with sodium amalgam yields a liquid, probably allylic alcohol. (?)—"On a new method for the estimation of grape sugar," by Karl Knapp. This method is based on the fact that an alkaline solution of mercuric cyanide is entirely reduced to metallic mercury by grape sugar. By direct experiment it was found that on boiling, 400 mgr. $Hg(CN)_2$ are reduced by 100 mgr. sugar. The solution is prepared by dissolving 10 grm. pure dry $Hg(CN)_2$ in water, adding 100 cc. of a sodic hydrate solution of 1·145 sp. gr., and diluting to 1,000 cc. Pure grape sugar is prepared by recrystallising the commercial dried at 100°, from absolute alcohol. The experiment is performed by heating 40 cc. of the mercury solution—this amount corresponds to 100 mgr. sugar—to boiling in a porcelain dish, and then adding sugar solution to complete precipitation of the mercury, the end of the reaction being ascertained by placing a drop of the liquid on to a piece of the finest Swedish filter paper, covering a small beaker containing some very strong ammonic sulphide; a brown spot is observed so long as mercury remains in solution. The advantages of this method over Fehling's are, that being equally accurate, the test solution is exceedingly easy to prepare and perfectly stable, a shorter time is required for the estimation, and that the foreign bodies which mask the pure colour of the cuprous oxide are without influence on the reduction of the mercuric cyanide.—"On some isopropyl compounds," by R. D. Silva. In this notice are described isopropylic succinate, benzoate, nitrite, and nitrate, all prepared by the action of isopropylic iodide on the respective silver salts of the acids.

The *Journal of Botany, British and Foreign*, for September contains a paper by Dr. H. Trimen on Early Icelandic Botany, including an account of Rotböll's observations on the new or little-known but rare plants found in Iceland and Greenland, which appears to have been overlooked by Professor Babington in his "Revision of the Flora of Iceland." It was published in 1770, and adds a few species to the number stated by Professor Babington to be indigenous to Iceland. We have also one of Mr. J. G. Baker's careful and useful contributions to British systematic botany, an account of the British dactyloid saxifrages, which he states to form a complete series of varieties from *S. cæspitosa* to *S. hypnoides*, without any clearly marked gap at any point between the extremes; and the line of progression substantially straight, very little if at all complicated, as in the case of *Rubus*, by cross-relationships. The order of sequence is as follows: 1. *S. cæspitosa*, 2. *S. Sternbergii*, 3. *S. decipiens*,

4. *S. quinquefida*, 5. *S. hypnoides*. A table of their geographical distribution is subjoined. Two or three other short original articles and notes are also included, and the official report for 1869 of the botanical department of the British Museum.

THE *Geological Magazine* for October (No. 76) commences with a long paper, illustrated with two plates, by Mr. D. Mackintosh, on the surface-geology of the Lake district, relating chiefly to the effects of glacial conditions observable among the mountains of Cumberland and Westmoreland.—Mr. T. Davidson contributes a third series of descriptions of Italian tertiary brachiopoda, including numerous species of the genera *Rhynchonella* and *Crania*, which are figured on the accompanying plate.—In a paper on the chalk of Kent, Mr. G. Dowker, following Mr. Whitaker, distinguishes the Margate chalk as constituting the highest section of the chalk observable in that district, and gives a list of the fossils which it contains. The author proposes a division of the Kentish chalk into six sections.—A fourth and last paper is by Mr. H. B. Medlicott, on the mode of occurrence of faults in strata. The remainder of the number is, as usual, occupied by reports, reviews, &c.

SOCIETIES AND ACADEMIES

NORWICH

Norfolk and Norwich Naturalists' Society, Aug. 30.—Mr. Stevenson read a valuable paper communicated by Prof. Newton, of Cambridge, on the method adopted by his brother and himself for registering Natural History phenomena. The Register, a volume of which was on the table, was kept at Elveden, near Thetford, during a period of ten years, and its great value consists in the variety and completeness of the information with regard to each species of bird found in that neighbourhood, and the slight amount of labour required to keep up the daily record. This is effected by the use of signs extremely simple in their construction, but conveying an amount of information never before dreamed of in registers of this description: a life history from day to day of each species is given; all the rare and occasional visitors recorded, and the most striking botanical phenomena are all noted fully and explicitly, but in such a way as to occupy barely five minutes in doing. What a boon this decrease in labour alone is to the conscientious recorder, he who has had to post up his register after a hard day's work in the field will be in a position fully to appreciate. Some of the results obtained from the study of the register are highly valuable, as, for instance, the migratory habits of the song thrush, and we strongly recommend the paper, which will be published in the Transactions of the Society, to the consideration of naturalists, feeling certain that important results would be obtained by the comparison of registers kept on Prof. Newton's plan in different districts of the county. Mr. Stevenson also read a note with regard to a habit of the rook, which appears to have attracted very little attention, viz., that of throwing up the indigestible portions of its food in the form of large pellets, after the manner of hawks and owls. Several of these pellets, or "quids," picked up on the cliffs at Cromer and Sherringham, consisting mainly of the indigestible husks of barley, with a few pebbles and fragments of small beetles, were exhibited, all having been found near the edge of the cliffs, together with rooks' feathers, showing that the birds had been preening themselves during the process of digestion. A number of interesting plants found in the neighbourhood were exhibited by Mr. Bircham. At the suggestion of Mr. Southwell, a subcommittee was formed to take into consideration the formation of a list of the natural productions of the county; and it is hoped that help for this purpose will be rendered by kindred societies and naturalists resident in the county. The President announced that Mr. J. H. Gurney, jun., who has been travelling in Algeria, would read a paper on the birds of that country at the next meeting of the Society, on September 27th.

PARIS

Academy of Sciences, Sept. 5.—M. Cave presented a second note on the generative zone of the appendages in monocotyledonous plants.—M. de Saint-Venant communicated a note by M. Boussinesq, supplementary to his memoir on periodical liquid waves, and showing the general relations between the internal energy of a fluid or solid body and its pressures or elastic forces.—A note was presented by M. W. de Fonvielle on a theory of Mariotte's on barometric oscillations, relating to the rising of the mercury when the wind is from the north and north-east, and its fall when the wind is from the south and south-west.—M. Delaunay communicated a notice of the discovery of a new comet by M. Coggia, at Marseilles, on the 28th August.—A note on the composition of nadorite by M. Flajolot, was presented by M. Combes. The mineral may be regarded as a combination of oxide of lead and oxychloride of antimony, in accordance with the formula $Sb^2\ O^2\ Cl\ 2Pb\ O$.—M. Claude Bernard communicated a memoir on the venom of the scorpion, by M. Jousset. The author has experimented with the poison of *Scorpio occitanus*, by inoculating it upon tree frogs. He found that it acts directly upon the red globules of the blood, depriving them of the power of passing each other freely, and thus causing them to become agglutinated to each other, and obstruct the circulation. The extent of the action of the poison is dependent on its quantity.—M. Zaliwski noticed a battery of zinc and carbon giving a maximum of intensity for twelve hours. The zinc is immersed in a solution of hydrochlorate of ammonia, the carbon in a mixture of nitric and sulphuric acid.

BOOKS RECEIVED

ENGLISH.—Advanced Text-book of Zoology: H. A. Nicholson (Blackwood and Sons).—Thayer Expedition: Scientific Results of a Journey in Brazil: L. Agassiz (Trübner and Co.).—Physical Geography: J. K. Laughton (Potter).—The Food, Use, and Beauty of British Birds: C. O. Groom-Napier (Groombridge and Sons).—Treatment and Utilisation of Sewage: Prof. Corfield (Macmillan and Co.).—Arithmetic, Parts 2 and 3, Sonnenschein and Nesbit (Murby).

FOREIGN.—(Through Williams and Norgate)—Helgoland: Nord-see-studien; Ernst Hallier.—Fauna öfver Sveriges och Norges Ryggradsdjur: Wilh. Lilljeborg.—Der Laacher See und seine vulkanischen Umgebungen: Dr. Jacob Noggerath.—Die Sculptur und die feineren Structur-verhältnisse der Diatomaceen, Heft 1.: Fritsch und Müller.—Iconographia familarum naturalium regni vegetabilis, Heft xx.: Dr. Schnitzlein.—Note sur un Foyer de l'Age de la Pierre polie: E. Perrault.—Mollusques tertiaires, Fasc. i.: F. Bayan.—Erster Nachtrag zum Lehrbuche der Aufbereitungskunde, mit Atlas: von Rittinger.—Natur und Gott: H. Baumgärtner.—Ueber Eis und Schnee: G. Studer.—Texture, Structure, und Zell-leben in den Adnexen des menschlichen Eies: Dr. Winklner.—Grundzüge einer Spongien-fauna des atlantischen Meeres: Dr. O. Schmidt.—Les Houillères en 1869: A. Burat.—Geographisches Jahrbuch III. Band, 1870: E. Behm.—Wissenschaftlich-technisches Handbuch des gesammten Eissengiessereibetriebes: E. F. Dürre.—Ueber die wachsende Kenntniss des unsichtbaren Lebens: Dr. Ehrenberg.—Botanische Untersuchungen über die Alkoholgährungspilze: Dr. M. Reess.—Verhandlungen der schweizerischen naturforschenden Gesellschaft in Solthurn, 1869.—Mittheilungen der naturforschenden Gesellschaft in Berne, 1869.

CONTENTS

THURSDAY, OCTOBER 20, 1870

ON THE COLOUR OF THE LAKE OF GENEVA AND THE MEDITERRANEAN SEA

THROUGH kindness for which I have reason to feel both proud and grateful, I have had placed in my hands two bottles of water taken from the Mediterranean Sea, off the coast of Nice. To my friend M. Soret I am also indebted for two other bottles taken from the Lake of Geneva. The friendly object in each case was to enable me to examine whether the colour of the water could in any way be connected with the scattering of light by minute foreign particles, which is found so entirely competent to produce and explain the colour of the sky. In the open Lake of Geneva, Soret himself had studied this question with considerable success,* and my desire was to apply to it other methods of examination.

The bottles, as they reached me, and with their stoppers unmoved, were placed in succession in the convergent beam of an electric lamp. Water optically homogeneous would have transmitted the beam without revealing its track. In such water the course of the light would be no more seen than in optically pure air. The cone of light, however, which traversed the liquid, was in both cases distinctly blue, the colour from the Lake of Geneva water being especially rich and pure. Something, therefore, existed in the liquid which intercepted and scattered, in excess, the shorter waves of the beam. The longer waves were also scattered, but in proportions too scanty to render the track of the beam white. The action, in fact, was identical with that of the sky. Viewed through a Nicol's prism the light was found polarised, and the polarisation along the perpendicular to the illuminating beam was a maximum. In this direction, indeed, the polarisation was sensibly perfect. A crystal of tourmaline placed with its axis perpendicular to the beam was transparent; with its axis parallel to the beam it was opaque. By shaking the liquid larger particles could be caused to float and sparkle in the beam. The delicate blue light between these particles could be quenched by the Nicol while they were left shining in the darkened field. A concave plate of selenite, placed between the Nicol and the water, showed a system of vividly coloured rings. They were most brilliant when the vision was at right angles to the beam, just as they are most brilliant when the blue sky is regarded at right angles to the rays of the sun. In no respect could I discover that the blue of the water was different from that of the firmament. The colour presented by the Mediterranean water was a good sky-blue, while that presented by the Geneva water matched a sky of exceptional purity.

My interest was long ago excited by the attempts made to account for the colour of the Lake of Geneva, and continued observation in 1857 impressed me more and more with the notion that the blue was mainly that of a turbid medium. Soon afterwards I wrote thus regarding this colour :—

"Is it not probable that this action of finely divided matter may have some influence on the colour of some of the Swiss lakes—on that of Geneva for example? This lake is simply an expansion of the river Rhone, which rushes from the end of the Rhone glacier. Numerous other streams join the Rhone right and left during its downward course, and these feeders being almost wholly derived from glaciers, carry with them the fine matter ground by the ice from the rocks over which it has passed. Particles of all sizes must be thus ground off, and I cannot help thinking that the finest of them must remain suspended in the lake throughout its entire length. Faraday has shown that a precipitate of gold may be so fine as to require a month to sink to the bottom of a bottle five inches high; and in all probability it would require *ages* of calm subsidence to bring all the particles in the Lake of Geneva to its bottom. It seems certainly worthy of examination whether such particles, suspended in the water, do not contribute to that magnificent blue which has excited the admiration of all who have seen it under favourable circumstances." *

Through the observations of Soret, and through those here recorded, the surmise of thirteen years ago has become the verity of to-day.

But though in the action of small particles we have a cause demonstrably sufficient to produce the blueness referred to, it is not the only cause operative. In the Lake of Geneva we have not only the blue of scattering by small particles, but also the blue arising from true molecular absorption. Indeed, were it not for this, the light *transmitted* by a column of the water would be yellow, orange, or red, like the light of sunrise or sunset.† Not only then is the light mainly blue from the first moment of its reflection from the minute particles, but the less refrangible elements which always accompany the blue are still further abstracted during the transmission of the scattered light. Through the action of both these causes, scattering and absorption, the intense and exceptional blueness both of the Lake of Geneva and the Mediterranean Sea I hold completely accounted for.

During the year 1869, M. Lallemand communicated to the Paris Academy of Sciences some interesting papers on the optical phenomena exhibited by certain liquids and solids when illuminated like the actinic clouds in my experiments. I also, in 1868, had examined a great number of liquids in the same manner, and a brief reference to these experiments will be found towards the end of a paper on the blue colour of the sky and the polarisation of its light, published in the Proceedings of the Royal Society for the 16th of December, 1868. M. Lallemand supposed the scattering of the light to be effected not by foreign particles but by the molecules of the liquids with which he experimented. M. Soret, on the other hand, contends against this novel view, maintaining that the scattering of the light is an affair of particles and not of molecules. While admiring the skill and learning displayed by the young French physicist, I am forced to take the side of Soret in this discussion M. Lallemand assumes a purely hypothetical cause while a true cause is at hand. He bases his case mainly on clear glass and distilled water. But the clearness is that observed in ordinary daylight, which is a very deceptive test. Glass exhibits the phenomena

* See his letter to me, Philosophical Magazine, May, 1869.

* Glaciers of the Alps (1860), p. 261.

† In fact, we have a dichroitic action of this kind exerted by glacier water when the subsidence is less complete than in the Lake of Geneva.

of scattering in every degree of intensity. Exceedingly fine examples of dichroitic action on the part of this substance are to be seen in Salviati's window in St. James's Street.* By reflected light the dishes and vases there exposed exhibit a beautiful blue—by transmitted light, a ruddy brownish yellow. The change of colour is very striking when, having seen the blue, a white cloud is regarded through the glass. Where the opalescence is strongest, the transmitted light, as might be expected, is most deeply tinged. From these examples, where the foreign ingredient is intentionally introduced, we may pass by insensible gradations to M. Lallemand's glass. The difference between them is but one of degree. Many of the bottles of our laboratory show substantially the same effect as the glass of Salviati. We can hardly ascribe to molecular action, which is constant, an effect so variable as this. It is also a significant fact that, in the case of pellucid bodies—rock salt, for example—where the powerfully cleansing force of crystallisation has come into play, M. Lallemand himself found the scattering to be *nil*. Under severe examination, rock salt itself would probably be found not altogether devoid of scattering power. I have examined many fine specimens of this substance, and have not succeeded in finding a piece of any size absolutely free from defect. A common form of turbidity exhibited by clear rock salt, when severely tested, resembles on a small scale "a mackerel sky." Nor have the specimens of Iceland spar that I have hitherto examined proved absolutely wanting in this internal scattering power.

In relation to this question, which is one of the first importance, the deportment of ice is exceedingly instructive. As a rule the concentrated beam may be readily tracked through ice, at least at this season of the year, when the substance shows signs of breaking up internally. In some cases the sparkle of motes, which are evidently spots of optical rupture, reveals the track of the beam. In other cases the track appears bluish, though rarely of a uniform blue. By causing a previously sifted beam to traverse lake ice in various directions, we are soon made aware of remarkable variations in the intensity of the scattering, and we find some places where the track of the beam wholly disappears. The convergent beam is sometimes divided by a transverse plane, one half of the cone being visible and the other invisible. In other cases the cone is divided by a plane passing from apex to base, one half shining with scattered light, and the other showing the darkness of true transparency. Now, if the scattering were molecular, it ought to occur everywhere, but it does not so occur, therefore it is not molecular. The scattering is, perhaps, in most cases due to the entanglement in the ice, when the freezing is rapid, of the ultra-microscopic particles abounding in the water. It is only by excessively slow freezing that such particles could be excluded from the ice. Purely optical ruptures of the substance itself, if minute and numerous enough, would also produce the observed effect.

The liquids which I examined in 1868 all showed in a greater or less degree the scattering of light, to which was added in many cases strong fluorescence. In no respect did the deportment of the non-fluorescent liquids which showed a blue track differ from that of the blue actinic clouds with which I was then occupied. I examined water from various sources and found it uniformly charged, not only with particles small enough to scatter blue light, but with far grosser particles. Tested by the concentrated beam, our ordinary drinking water presents a by no means agreeable appearance; some of the water with which London is supplied is exceedingly thick and muddy. Nor does distillation entirely remove the suspended matter. Soret vainly tried to get rid of it; he diminished its effect, but he did not abolish it. I was favoured a few days ago with specimens of distilled water from four of the principal London laboratories. Looked at in ordinary daylight the liquid in each case would, in ordinary parlance, be pronounced "as clear as crystal," but when placed in the concentrated beam of the electric lamp, the notion of purity became simply ludicrous. No one who had not seen it would be prepared for the change produced by the concentrated illumination. There were differences of purity among the specimens, arising, doubtless, from the different modes of distillation, but to an eye capable of seeing in ordinary light what was revealed by the concentrated beam each of the specimens would appear as muddy water. I also examined a specimen of extra purity distilled from the permanganate of potash and liquefied in a glass condenser. It contained a large amount of foreign particles; not of those which scatter blue light, but grosser ones. Such must ever be the case with water distilled in the laden air of cities and collected in vessels contaminated by such air. These facts amply justify the language applied by Mr. Huxley to the statement that solutions without particles can be obtained by the processes hitherto pursued. Such a statement could only be based upon defective observation. In the number of this journal for the 17th of March, an experiment is described in which water was obtained from the combustion of hydrogen in air, the aqueous vapour arising from the combustion being condensed by a silver surface of unimpeachable purity. In this case, though the floating particles of the air were, in the first instance, totally consumed, the water was still well laden with foreign matter. The method of obtaining water here referred to had been resorted to by M. Pouchet with a view of utterly destroying all germs, and my especial object in repeating the experiment was to reveal the dangers incident to the inquiries on which M. Pouchet and others were then engaged. But the warning was unheeded. It is not for the purpose of adding to the weight of calamities, already sufficiently heavy, that I allude to this, but rather to advertise the adventurous neophyte, who may be disposed to rush into inquiries which have taxed the skill of the greatest experimenters, of some of the snares and pitfalls that lie in his way.

JOHN TYNDALL

Royal Institution, Oct. 18

* Mentioned to me by a correspondent.

NICHOLSON'S MANUAL OF ZOOLOGY

A Manual of Zoology. By H. A. Nicholson, D.Sc. Vol. I. Invertebrate Animals. (Blackwood and Sons.)

A BOOK such as this aims at being was wanted—one which should give a little more of systematic detail than is to be found in Professor Huxley's "Outlines" or

Professor Rolleston's introductory chapters of his "Forms." It required, however, a man of considerable knowledge of the subject to write such a book worthily, and we doubt whether Dr. Nicholson, though he deserves credit for enterprise, was quite the man to undertake it. He excuses himself for shortcomings in plan and execution in his preface, on the score of leading a busy life. Now is it, we would ask, for men who lead lives devoted to other objects than the pursuit of zoology, to bring out educational works on that branch of science?

Having once determined not to aim at originality, Dr. Nicholson has really performed a service in reproducing so much of Professor Huxley's lectures, both the later ones and those published in the *Medical Times and Gazette* in 1856-7, as he has here, with due acknowledgment, done; for it is not everyone who can now get at those lectures for themselves. Most of the illustrations in this book appear to come from these lectures also, but this is not acknowledged. Other figures of Turbellarians we recognise as copied from an article in the *Popular Science Review*. All figures not original should be acknowledged in a work of this kind, and if it were done in this case we should find that Dr. Nicholson has seldom gone to an original source—but has copied the copies of other people. We may make exception, however, in respect of the figure of *Rhizocrinus* on page 134 and a few others which have not before made their appearance in any handbook or manual of our acquaintance; figs. 47, 67, 68, 73, which we pick out at random, are as old as the hills. It is not only as to figures that we have to regret that the author does not quote at first-hand. On page 4 the student is told that Professor Huxley has applied the name of "protoplasm" to the physical basis of life. No one would resent more than Professor Huxley this statement, seeing that Mohl's term for vegetable "Schleim" was extended to animal "Schleim," and adopted in the broad sense of the protoplasm theory by Max Schultze and Kuhne more than seven years since. Dr. Bastian's name is quoted (p. 154) in connection with the remarkable history of *Ascaris nigrovenosa*, which we really owe to Leukart and Mecznikow. The history of a science is really of sufficient importance to render it desirable to keep to it as truly as possible, and the compiler of such a work as this should not so entirely confine himself to appropriating the gleanings of others, but should shift for himself, and give us the result of inquiries among the original writers.

The account of the Annelida is not very full, nor is it accurate. The leeches are made to form an order of the group, and the common horse leech and medicinal leech are the only members of it mentioned. Oligochæta is given as a synonym for Terricola, and is divided into Lumbricidæ and Naïdidæ. The latter group is said to include *Tubifex*, and then it is said that nonsexual reproduction characterises it. The fact is that Oligochæta includes Terricola = Lumbricidæ and Enchytrœidæ, and Limicola = Sænuridæ and Naïdidæ; *Tubifex* belonging to Sænuridæ and not to Naïdidæ, and exhibiting no reproduction by budding, which is confined to the genera *Nais*, *Chætogaster*, and *Æolosoma*. The fluid of the pseudo-hæmal system in *Aphrodite* and *Polynoë* is stated to be yellow, but Claparède's researches show these worms to be anangian, with the doubtful exception of *Aphrodite aculeata*, so that they have not this fluid at all, either yellow or of any other colour. Whilst no mention is made of Haeckel's proposal to connect sponges with Cœlenterata—perhaps a judicious omission at present—we find them actually classed as Rhizopoda, which is to us a new step in the opposite direction, and not a justifiable extension of Dujardin's class. In defining the Annuloida, Professor Allman's words, relative to the Echinodermata, only are quoted as though applying to both Echinodermata and Scolecida, into which two groups Dr. Nicholson follows Professor Huxley in dividing the sub-kingdom.

The Mollusca are treated by the aid of the late Dr. Woodward's manual, due prominence being given to the homological views of Professors Huxley and Allman. Mr. Hancock is not, however, mentioned, nor are Duthier's views on *Dentalium*, nor again are the exceedingly important facts of the resemblance of development of the nervous system and chorda dorsalis of the Tunicate larvæ and of Vertebrata, established by Kowalewsky and Kupffer, alluded to.

Perhaps a greater omission is that of all reference to the Monera of Haeckel. Whatever he thinks fit to do with them, the genera *Protamœba*, *Protogenes*, *Protomyxa*, *Myxastrum*, *Protomonas*, and *Vampyrella*, deserve recognition on Dr. Nicholson's part; so also do the Labyrinthuleæ of Cienkowski. Another omission is that of the fresh-water forms of Radiolaria, described of late years by Archer, Focke, and Greef; whilst under Myriapoda, we find no mention of Sir John Lubbock's curious aberrant form *Pauropus*; no mention of *Cecidomyia* larvæ under Insects; no mention of *Rhabdopleura* under Polyzoa, though it is alluded to in connection with Graptolites, the solid axis of the latter being supposed to resemble the chitinous rod of Prof. Allman's new order of Polyzoa (*Quarterly Journal of Microscopical Science*, January 1869)—a resemblance, if admissible, not confirmatory of Sertularian affinities, by the way. Interesting transition forms, such as *Echinoderes*, *Rhabdosoma*, *Myzostomum*, are not spoken of. The curious low forms of Arachnida, belonging to the genus *Pentastomum*, are said to occur in *some* vertebrates. It should be stated definitely that a species infests man, and that one body out of every five has been said to contain specimens.

It is not an agreeable task to point out so many deficiencies; at the same time a book destined for educational purposes can least of all be excused in these matters. For all that it contains of reference to recent zoological literature, this manual might be dated ten years back, when the same condensing process applied to the same educational books would have produced much the same result.

There is a glossary to this book which should be useful; we have only looked at its first page, and did not venture further into what promised to be a new series of mistakes. There is no Greek word "aktin," a ray—though Dr. Nicholson says there is.

As a rule, the definitions of both large and small groups are well given by Dr. Nicholson, and the most original part of his manual appears to be in those paragraphs which give the geological range of the various groups; these are well and carefully done, though there are exceptions—the geographical distribution is not given with equal care.

E. R. Lankester

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

The Evolution of Life: Professor Huxley's Address at Liverpool

BELIEVING that readers of NATURE can feel no interest in the extended personalities with which Prof. Huxley almost fills his letter this week, and believing also that such matters are little worthy of occupying your columns, I shall only allude to that part of his letter which contains statements having a scientific bearing.

The distinct issue raised in my experiments was, were *living* things to be found in the fluids of my flasks? If so, such living things must either have braved a higher degree of heat than had been hitherto thought possible, or else they had been evolved *de novo*.

The effect of the very high temperature upon pre-existing living things, which were purposely exposed thereto, was shown by their complete disorganisation in an experiment which is recorded in NATURE, No. 37, p. 219, and to this I would especially direct Prof. Huxley's attention.

Prof. Huxley advances an explanation of the mode of origin of the distinct fungi, bearing masses of fructification (NATURE, No. 36, figs. 12, 14, and 17) and of the inextricably tangled coils of spiral fibres (figs. 13 and 15) found in my flasks after exposure to temperatures at and beyond Pasteur's standard of destructive heat; his theory is entirely novel, apparently extemporised for the occasion, and is very startling. He says, and in justice to Prof. Huxley I quote the passage in full, "Any time these six months Dr. Bastian has known perfectly well that I believe that the organisms which he got out of his tubes are exactly those which he has put into them; that I believe that he has used impure materials, and that what he imagines to have been the gradual development of life and organisation in his solutions is *the very simple result of the settling together of the solid impurities, which he was not sufficiently careful to see when in their scattered condition when the solutions were made.*"

Now, although it was quite true that minute portions of *Sphagnum* leaf were found in two unpublished experiments, it seems very marvellous that on this slender foundation Prof. Huxley should hazard such a purely imaginative and unprecedented hypothesis as to the mode of production of fungi.

I have, moreover, not been able to see why the occurrence of the incident to which he refers should make him repudiate a number of experiments in which *unmistakeably living things* were found in fluids from hermetically sealed flasks after these and their contents had been exposed to temperatures higher than those which living things are known to be capable of resisting.

Following a precept more honoured in dialectics than in science, Prof. Huxley has attacked his opponent rather than the arguments which he affects to destroy. He objects to only one passage in my "Reply," and this he thinks was not worthy of the special type in which it was printed; and yet, notwithstanding its special type, I can only conclude from his reply that Prof. Huxley has failed to appreciate its meaning. My words were: "Living things may and do arise as minutest visible specks, in solutions in which, but a few hours before, no *such* specks were to be seen." The word which now alone stands in italics was ignored by Prof. Huxley. I had no wish to tell him that certain refractive particles, or foreign bodies, might not be visible in the thin film of fluid to which I referred. I alluded to the gradual and equable development of living specks throughout a fluid containing no apparent germs. His retort that some unobserved visible germs might have become centres of development is a *contre-sens*. It does not apply to the gradual appearance of myriads of *equally diffused* motionless particles in a motionless film of fluid.

The very authoritative tone which Prof. Huxley has lately assumed in his remarks concerning Brownian movements and those of living organisms, fails to impress me very much. His knowledge about these movements, as I have good reason to know, is of quite recent growth. Movements which, in the month of March of the present year, Prof. Huxley did not regard as Brownian, he now does believe to have been of this nature. If he is now right, what value is to be set upon his knowledge of Brownian movements six months ago; and what guarantee have we that in another six months Prof. Huxley may not again take a different view?

Let me assure Prof. Huxley, however, that the duty which he is "credibly informed" he owes to the public remains still undischarged. I protested against his "Address" on scientific grounds which are fully stated, and those who have read my protest will see that Prof. Huxley cannot dispose of the question really at issue by recounting any mistakes of mine, whether real or imaginary. If, as I believe, he has failed to give any worthy or serious view of the question, this could have been in no way necessitated by a disbelief, however strong, in my experiments. The labours of Profs. Wyman, Mantegazza, and Cantoni had already taken the question into regions never attained by M. Pasteur, and therefore they demanded a fair consideration. Is Prof. Huxley, in his capacity of President of the British Association, warranted in ignoring their labours, and therefore in misrepresenting the present state of science on the subject, because, owing to two errors among my many experiments, he declares himself to have altogether lost faith in my skill or capacity as an investigator? The answer cannot be doubtful. Is it, again, consistent with his high responsibilities that he should pervert the real issues, and should do a grave injustice to others, in order that he might preserve a "silence" which should be his "best kindness" to me? Let me tell Prof. Huxley that I repudiate such "kindness," as any honest man would who is simply seeking after truth, and relegate it to the same regions as I would that indescribable air of restrained omniscience whereby he endeavours to crush arguments and facts, to which he altogether fails to reply.

H. CHARLTON BASTIAN

University College, Oct. 17

Aurora Borealis

A BRILLIANT display of the Aurora Borealis was visible here on Sept. 24th. I submit the following statement of my observations, which probably may be acceptable to your readers.

About 8.30 P.M. a broad white streak of light was seen due north, extending from the horizon towards the north star. This streak was soon accompanied by several others, rising and streaming upwards from the horizon between the north-west and north-east points, and converging towards the zenith: at the same time all the northern portion of the sky became illuminated.

At 9 P.M. the coruscations of the Aurora became more brilliant and active, streaming with great rapidity towards the zenith, though none could be seen to reach that point, assuming very beautiful colours, which were generally of light pink and reddish hues.

About 10 P.M. the colouring and brilliancy of the Aurora attained its fullest effect, the coruscations varying with yellow, pink, and almost crimson hues, the intermediate spaces of the sky in the background appearing to be light green. The brightest streamers were confined to the north and north-westerly directions. The coruscations continued in rapid action with surprising grandeur up to 10.30 P.M., when a somewhat indistinct purple arch was visible, with its culminating point north and about thirty-five degrees above the horizon. Shortly after this exhibition, the display of coruscations and luminous streamers gradually ceased action, but the northern portion of the sky remained illuminated, resembling twilight after sunset.

The sky was quite clear at the commencement of the Aurora; it became partially obscure about 10 P.M.; the clouds then, especially those in the north-east, reflected very beautiful roseate tints from the Aurora, which, in combination with the varying colours of the coruscations, produced a most striking and grand spectacle.

Three shooting-stars were observed between 9 and 10 P.M., and several were seen on the previous night.

The barometer was at 30·4, and has not read below 30·3 during the past week. The temperature has been very regular for the same time; the maximum by day was 64°, and the minimum at night 51°. Winds have been constant from the east, and the sky free from cloud.

The northern region of the sky remained illuminated throughout the night, producing a strong reflection on the sea, and rendering the rocks and vegetation around me quite perceptible. Even at 3.30 A.M. the light from the northward produced all the effect of early dawn.

I must add, that during all my Arctic voyaging I never witnessed in any Aurora the same conditions of varied colouring as were displayed on this occasion.

It may also be of interest to state that on the 21st inst. I observed a bright meteor to shoot in a path from near the Pole Star to Capella, vanishing near the latter after discharging a brilliant light-green flash.

From Ilfracombe the view to the north commands an extensive range of the sea horizon.

ERAS. OMMANNEY, Rear-Admiral

Ilfracombe, Devon

Natural History Museums

WHILE admiring and agreeing with the main features of Dr. P. L. Sclater's plan for the exhibition of the National Natural History collections in the new Museum at Kensington, I must beg to be allowed to enter a protest with regard to the geological portion of the scheme, which was all but completely overlooked by those who took part in the discussion of his suggestions and by Dr. Sclater himself.

The only words which Dr. Sclater devoted to the subject were to the effect that the palæontological collections should be merged into the general zoological and botanical ones. This arrangement is, however, one which will not serve the purpose of the geologist. We can fancy the utter misery of the man who wishes to study the fossils of some special formation, when he finds that to do so he will have to rush from case to case, amid stuffed and spirit specimens of all kinds, all over the place. The naturalists who deplore the loss of time incurred by them under the present order of things at the British Museum, have no thought for the geologist whom their proposed arrangement would condemn to far worse wandering in the new building. But in this particular I, as a geologist, sincerely trust that the project will not be carried out without some revision, and that a palæontological collection, quite independent of any other, and arranged *stratigraphically*, will form a by no means inconspicuous part of the Museum. The advantages of this system may be seen at a glance in Jermyn Street, where the British fossils are thus arranged.

In a collection as large as that of the National Museum must be, it may very likely be found impossible to exhibit specimens of every species in the show cases, and indeed it is by no means desirable that this should be done. It would be quite sufficient, and as far as the general public is concerned infinitely better, to limit the *exhibited* specimens to those belonging to species characteristic of each formation, and to keep all others, which would interest students only, in a cabinet drawer, where they would be at once handy to the specialist and not wearying to the sight-seer. Duplicates enough could doubtless be spared to complete the zoological and botanical series in the lower galleries, but unique and rare specimens should, to my mind, most decidedly be kept for the geological series. In conjunction with the latter, it would be highly desirable to establish a lithological collection, the absence of which, with the solitary exception of the small one in the Geological Museum, is every day more and more to be wondered at in such a country as ours.

Not only should appropriate rock-specimens accompany the fossils of each fossiliferous bed, but they should be so arranged that the organisms and the matrix in which they are found embedded, could be examined side by side. Typical examples of unfossiliferous strata should be placed in their regular order of succession, including specimens of contemporaneous rocks from other parts of the world. Clear geological sketch-maps, boldly coloured and not over-burdened with names, showing the distribution (so far as it is known) of the various equivalent groups of beds, should be placed in conspicuous places at intervals in the room or gallery, and thus, or in some closely-resembling manner, could a homogeneous whole be arrived at, combining the greatest amount of instruction to the public and the greatest convenience to the student.

Of course the geological collection shadowed forth above would by no means take the place of another more detailed lithological collection, which should, if possible, be added to the mineralogical one, containing all the igneous rocks, &c., and, like the minerals, arranged *chemically*.

G. A. LEBOUR, F.G.S. &c.,
of the Geological Survey of England.

Changes of Level at Pozzuoli, referred to in the "Apocryphal Acts of Peter and Paul"

IN the well-known description which Sir Charles Lyell gives of the changes of level of the shores of the Bay of Baiæ in the 30th chapter of his "Principles of Geology," there occur at pages 172-174 (vol ii. 10th edition) the following statements.

Speaking of the so-called "Temple of Serapis," and its adornment by the Emperor Alexander Severus between A.D. 222 and 235, he says: "From that era *there is an entire dearth of historical information for a period of* 12 *centuries*, except the significant fact that Alaric and his Goths sacked Pozzuoli in 410, and that Genseric did the like in 440 A.D." Again: "The period of deep submergence was certainly antecedent to the close of the 15th century"—a statement which he goes on to prove by a quotation from Loffredo referring to the year 1530, and a reference to documents cited by A. di Joris, one of which, dated 1503, speaks of land "where the sea is drying up."

Still more recently Professor Phillips, in his interesting volume on Vesuvius, speaking (p. 244) of the "Temple of Serapis," observes that, at the time of its adornment by the Emperor, "early in the 3rd century, it must have been in its original, or else in its second stage—perhaps we may adopt the latter view—there may have been a depression of 5ft., a new floor, restoration, and adornment. Nothing is absolutely known of any further change of level till the early part of the 16th century."

Thus both the authorities cited appear to agree that from the middle of the 3rd to the end of the 15th century there is a *total* absence of information. You may therefore possibly think that the following extract from the "Apocryphal Acts of Peter and Paul" possesses some interest, even though it may be difficult to agree on the approximate date of the writer. Your columns would scarcely be the place, nor am I competent, to discuss this last point, but as one of the MSS. collected by Professor Tischendorf for his edition of the Greek original is said to be of the end of the 9th century, it appears to me that we have here not only a rather quaint explanation of the immediate cause of the changes of level of the land at Pozzuoli and in its neighbourhood, but a distinct reference *at least* to six centuries before the Italian writers already quoted, not merely to the fact, but also to the extent, of the movement in question. Notwithstanding its suspicious legendary framework, the statement that "Pontiole sunk into the sea-shore about one fathom; and *there it is until this day*, for a remembrance, under the sea," has an air of *vraisemblance*, a ring of truthfulness, about it which I hope will justify my bringing the matter under the notice of those so much more competent than I am to assign to it its true value, and to whom it may possibly be new.

I quote from the translation by Mr. A. Walker, forming a portion of Vol. xvi. of the "Ante-Nicene Library," published by Messrs. Clark of Edinburgh, pp. 257–8.

F. FOX TUCKETT

Frenchay, near Bristol, Oct. 3

"And when Paul came out of Mesina he sailed to Didymus, and remained there one night, and having sailed thence, he came to Pontiole (Puteoli) on the second day. And Dioscorus the shipmaster, who brought him to Syracuse, sympathising with Paul because he had delivered his son from death, having left his own ship in Syracuse, accompanied him to Pontiole. And some of Peter's disciples having been found there, and having received Paul, exhorted him to stay with them. And he stayed a week in hiding, because of the command of Cæsar (that he should be put to death). And all the toparchs were watching to seize and kill him. But Dioscorus the shipmaster, being himself bald, wearing his shipmaster's dress, and speaking boldly, on the first day went into the city of Pontiole. Thinking, therefore, that he was Paul, they seized him and beheaded him, and sent his head to Cæsar. . . . .

"And Paul, being in Pontiole, and having heard that Dioscorus had been beheaded, being grieved with great grief, gazing into the height of the heaven, said: 'O Lord Almighty in Heaven, who has appeared to me in every place whither I have gone on account of Thine only begotten Word, our Lord Jesus Christ, punish this city, and bring out all who have believed in God and followed His Word.' He said to them, therefore, 'Follow me.' And going forth from Pontiole with those who had believed in the Word of God, they came to a place called Baias (Baiæ), and looking up with their eyes, they all see that city called Pontiole sink into the sea-shore about one fathom, *and there it is until this day, for a remembrance, under the sea.*

"And those who had been saved out of the city of Pontiole, that had been swallowed up, reported to Cæsar in Rome that Pontiole had been swallowed up with all its multitude."

Hereditary Deformities

IN the number of NATURE for Sept. 3, a letter from Mr. William Field appears with the title "Hereditary Deformities,' commenting on certain alleged facts quoted in an ethnological article in *Cassell's Popular Educator*, from Dr. Theodor Waitz's "Introduction to Anthropology," translated by Mr. Collingwood. Mr. Field justly remarks that facts of such a character, "if substantiated, would introduce *Accidental Distortion* as a co-worker with Natural Selection in the modification of species." But he puts the question—"Do these stories rest on a good foundation?" Personally, I do not know. All I can say is, that Dr. Waitz, whose scientific authority is unimpeachable, published them without expressing any doubts of their accuracy. They may be found with some alleged facts of analogous character, at pp. 83 to 85 of his first volume, as translated by Mr. Collingwood. Speaking of animals, he says:—

"Mutilations also are sometimes transmitted. Williamson* saw in Carolina dogs which have been deficient in tails for three or four generations, in consequence of one of their ancestors having accidentally lost it. A cow, three years old, which had lost by suppuration her left horn, produced three calves, which, instead of the left horn, presented only a small protuberance on the skin. Dogs and horses whose tails or ears are clipped, as the draught dogs in Kamtschatka, often transmit these deficiencies to their offspring." (p. 83.)

Referring next to man, he considers that there are "cases in which deformities have shown themselves hereditary" (p. 84.) He says:—

"Instances of hereditary blindness and deafness, and of alternating dumbness, so that every second or third child was deaf, are given by Lucas. Harris communicates a case of hereditary blindness in one eye, and of a double thumb on the right hand."

I omit other instances. He continues again:—

"Instances are not wanting of mutilations that have been transmitted from parents to children; such, however, occur less frequently. According to Blumenbach, the children of an officer, whose little finger had been cut across and become crooked, possessed an analogous defect. Gosse cites the case of an officer wounded in the battle of Eylau, who transmitted to his offspring a scar on the forehead. Other instances of hereditary deformities are found in Wagner" (p. 85).

As Waitz, it will be perceived, quotes Blumenbach, it may be mentioned that the last-named author has a paragraph headed, "Problem proposed. Can mutilations and other artifices give commencement of native varieties of animals?" After showing that some have answered the question in the affirmative and others in the negative, he adds, "I have not at present adopted as my own either the affirmative or negative of these opinions." See p. 203 of his Anthropological Treatises, translated by Mr. Bendyside.

New facts, capable of being severely tested, would be of great value.

THE WRITER OF THE ETHNOLOGICAL PAPERS IN "CASSELL'S POPULAR EDUCATOR"

* More specific reference to the authorities on which Waitz rested may be found in the foot-notes to his work. Some of the volumes he quotes are foreign, and not to be found in the British Museum Library.

The Stability of Turret Ships

SINCE the loss of the *Captain* an opinion has rapidly gained ground, not only amongst unscientific men, but even amongst those who from their education should have acquired some of the most simple laws of statics, that that noble ship toppled over on account of her being "top-heavy"—that the *Captain*, an armour-plated ship with a low freeboard, was more "top-heavy" than a broadside ship, with more than twice as much out of water! The fact is, that her weights were lower and not higher than those of other vessels, and therefore that her fault was not "top-heaviness."

In looking at the stability of a vessel we take two points—the "centre of gravity" and the "centre of buoyancy;" the former being a certain point at which, if the attraction of gravity impressed a single force equal in intensity to the sum of all its separate actions on the component parts of the body, the ultimate effect would be the same as it is under the system of separate actions which really exists. The latter is the centre of gravity of the volume of water displaced by the ship, and may be regarded as the pivot on which she would turn on heeling over. The vessel being in a vertical position, the centre of gravity is immediately over the centre of buoyancy, and she is in a state of unstable equilibrium, *i.e.* she is in the same fix as a walking-stick standing on end. So far in favour of "top-heaviness."

Suppose now that she heels over to one side, what will the effect on our centres be? Immediately she begins to heel over the centre of buoyancy travels outwards to the side towards which she heels, and the centre of gravity being fixed, there will come a point when it will exert a force to overcome the heeling over pressure. This travelling outwards of the centre of buoyancy depends wholly on the shape of the vessel, and will appear perfectly plain by drawing sketches of a ship lying at different angles. The more a vessel heels over, the further outwards does the centre of buoyancy travel, and the greater is the resistance offered to the heeling over pressure until she approaches a certain point, then the centre of buoyancy moves out at an increasingly slower rate, and finally reaches the position corresponding to that of her maximum statical stability.

Before this, if by any disturbing cause, such as the alteration of the wave slope, the ship were inclined beyond her position of maximum stability, the resistance to heeling would become less the farther she went, until she reached a position at which her moment of stability would be the same as before the disturbing force began to act. And in this position she would remain in unstable equilibrium if the disturbing forces were removed. But if she should pass this position before the disturbing forces, and the angular velocity caused by them, cease, the ordinary movement of the heeling over force would then be greater than the resistance offered by the stability, in any position through which she would pass, and she would be turned over.

Now the difference between a high and a low freeboard ship as regards stability under sail is this:—The position of maximum stability is reached soon after the immersion of the edge of the deck; and as a high freeboard ship does not immerse her deck until she has attained a large inclination, while a ship of low freeboard will immerse hers at a very much less angle, it follows that, in the latter case, the position of maximum stability and then of unstable equilibrium is reached at a comparatively small angle of heel, and a ship of this construction is much more likely to be capsized than one with a high freeboard. Of course, in a low-sided ship, the centre of gravity may be brought so low as always to be on the right side of the centre of buoyancy, but this is not practicable in an armour-plated turret ship.

From what we have here stated it will be seen that the error in design that made the *Captain* so much heavier than was expected, and draw six feet instead of eight, was not adding so much to her stability but was in reality lessening it, and, perhaps, was the cause of her loss; and that if we are to have armour-plated turret ships, they must either be built of low freeboard, to be propelled by steam alone, or of high freeboard sufficient to give stability for sails.

The feud between naval architects and the advocates of the turret system has been going on for ten years back—the former contending that a high side was necessary in a rigged sailing ship, and the latter, that if they put their guns in turrets, they could have low-sided ships, or, in fact, ships with no sides at all, after a certain amount of inclination.

That the loss of the *Captain* has resulted from a preventible cause is quite evident, and we have shown what that cause is; it, therefore, only remains that if we are to have sailing turret ships, we must have high freeboard for the sake of stability.

T. BELL LIGHTFOOT

Newcastle-upon-Tyne

The New Postal Act

AT page 474 Mr. Reeks complains as to the working of the new Postal Act. It seems intended to obliterate the old parcel post. He says, "Herbarium specimens are not excluded." Perhaps so; but they are not included. The provisions of the new Act are limited to books, written and printed matter, genuine trade samples and patterns, so far as regards the two ounces for a halfpenny. All parcels other than books, &c., as described above, go at the letter rate of one penny for each half-ounce.

The postal card is the thin end of the wedge that will hereafter open to us a regular letter rate of a quarter of an ounce for a halfpenny. For instance: we may now send ordinary business communications up to two ounces, thus embodying the matter of twenty postal cards, for the halfpenny, if folded in a paper wrapper. An ordinary business communication of half-ounce weight goes for the halfpenny, if folded as a letter but left unsealed.

I ask, If Government will now take an open letter for a halfpenny, why not take it closed at the same rate? Common sense will hereafter equalise this disparity. The parcel post, however, is at present discontinued. P. N. Row

October 14

Science and the Government

THE reason that the Government has refused to aid "the expedition to observe the approaching eclipse" is, that it is perfectly assured that "men of science and culture" are nothing but a set of lying impostors, and would swindle the public out of thousands of pounds to take an observation which might be done for 10*l.* and much less. The nation is fast beginning to perceive that astronomy is a monstrous cheat—and the Transit of Venus has no more to do with the distance of the sun than it has with the number of fingers on my hand.

JOHN HAMPDEN

[We congratulate Mr. Lowe on his ally, merely remarking that the same Government which has refused the Eclipse Expedition has granted 20,000*l.* for observations of the Transit of Venus. Perhaps Mr. Hampden can explain the cause of this inconsistency on the part of the Government.—ED.]

Insects upon a Swallow

G. H. H. mentions in NATURE of Sept. 22 his having found on a swallow in the month of August two slate-coloured insects. On the 15th of July last, when hauling a seine net in Studland Bay, near Poole, with some fellow members of the Linnean Society Club, I picked up at the foot of the low cliff a young sand-martin, which had seven of these parasites (*Ixodes plumbea*) affixed to the skin of the head, giving it the appearance of having a slate-coloured fleshy crest. The poor little bird exhibited the symptoms described by your correspondent. I found it panting and apparently exhausted on the ground, and it remained stationary without making any effort to escape. The fishermen of our crew informed me that they often pick up martins with these ticks adhering to them.

The Waldrons, Croydon, Sept. 26 HENRY LEE

Aurora Borealis

A FINE aurora was observed here on the evening of the 14th inst., between $8^h 30^m$ and $9^h 40^m$, which, in spite of bright moonlight, nearly equalled the splendour the display of September the 24th. During the early part of the evening the eastern quarter of the sky was covered with bands of light *cirri*, which had a general direction of E.N.E. to W.S.W., or nearly at right-angles to the magnetic meridian; it was among these clouds and in the N.E. that the beams of the aurora were first seen. At $9^h 0^m$ a magnificent rose-coloured ray was noticed in the N.N.W., extending from the horizon through *Vega* towards the zenith. The three stars, ζ, ϵ, and δ, of Ursa Major were for more than ten minutes enveloped in the crimson glow of the aurora.

Bedford, Oct. 18 THOS. G. ELGER

NOTES

WE are glad to be able to state that Dr. Wyville Thomson has entirely recovered from the attack of gastric fever which prevented his taking part in the *Porcupine* expedition this summer. He is at present going over the zoological collection brought home in that vessel, at the University of London, with Dr. Carpenter, and he reports some very remarkable additions to his new group of vitreous sponges, mainly from the coast of Spain and Portugal. These, with some others procured by Mr. Saville Kent, in Dr. Marshall Hall's yacht, will nearly double the number of known forms referred to the order. They are no pigmies. One of them forms a lovely lace-like vase upwards of three feet in diameter at the lip!

WE have to add to the list of candidates for the Regius Professorship of Natural History in Edinburgh the name of John Anderson, M.D. Edin., F.L.S., director of the Imperial Museum of Natural History, Calcutta. Dr. Anderson was attached as Naturalist to a recent expedition through the north of Burmah, and he is, we understand, now on his way home, bringing with him detailed accounts of the important additions which he is known to have made to science in that expedition.

MEMBERS of the University of Cambridge, and all who are interested in the study of physical science, will hear with pleasure of the munificent offer made to the University by its Chancellor, the Duke of Devonshire, contained in the following extract of a letter to the Vice-Chancellor:—"I find in the report, dated Feb. 29, 1869, (*sic*) of the Physical Science Syndicate, recommending the establishment of a Professor and Demonstrator of Experimental Physics, that the buildings and apparatus required for this department of science are estimated to cost 6,300*l.* I am desirous to assist the University in carrying this recommendation into effect, and shall accordingly be prepared to provide the funds required for the building and apparatus, so soon as the University shall have in other respects completed its arrangements for teaching experimental physics, and shall have approved the plan of the building."

AT the Commencement held on the 12th inst. of the Queen's University in Ireland, the Most Honourable the Marquis of Kildare, the newly-appointed Chancellor of the University, presided. After expressing the deep regret of the University at the death of their first Chancellor, the Earl of Clarendon, to whom the University and Colleges were indebted for much of their prosperity, the Chancellor mentioned that the Senate had decided to establish a special curriculum, in which science should have the predominance, and that the degrees of Bachelor and Doctor in Science would be given to those who passed the examinations in the subjects to be hereafter enumerated. He also referred to the examinations for women carried on by the University—the first of these was held in June last in Belfast and Galway—and mentioned that of thirty-three candidates who presented themselves, twenty-one acquitted themselves to the satisfaction of the examiners.

MR. M. R. PRYOR has been elected to a Fellowship of Natural Science in Trinity College, Cambridge. The examination was conducted by Prof. Liveing, Prof. Michael Foster (the new Prælector of the College), Mr. Trotter, and Mr. Hort, Fellows of the College, was open to all the University, and was on a par with the examinations in classics and mathematics, held at the same time for Fellowships. It is the first occasion that this has been done, and the first time that a Fellowship has been offered in Cambridge for competition in Natural Science. We sincerely trust the plan will be continued in Trinity, and that the example will be followed in other Colleges. It would probably contribute more than any other single thing to promote the study of Natural Science in the University, and give an impetus to it in the various schools throughout the kingdom. We sometimes think the Colleges are scarcely conscious of the power they are capable of exercising in this way, and of the responsibility which necessarily attaches to such power. The questions were of a very high order, and we understand the answers evinced so much power as well as knowledge, that the examiners would gladly have elected more than one candidate. We sincerely congratulate Mr. Pryor on having thus worthily won the highest competitive reward for Natural Science hitherto given in this country.

THE Senate of the Queen's University in Ireland has conferred on William King, M.D., Professor of Geology and Mineralogy in Queen's College, Galway, the honorary degree of Doctor of Science, in consideration of his eminence as a geologist.

THE following lectures in Natural Sciences will be delivered in Trinity and St. John's Colleges, Cambridge, during the Michaelmas term, 1870. On Electricity: Mr. Trotter, Trinity. On Chemistry: Mr. Main, St. John's. Instruction in Practical Chemistry will also be given. On Geology—(1) Palæontology; (2) Lyell's Principles of Geology; (3) Elementary Lectures: Mr. Bonney, St. John's. (Students of other Colleges can be admitted to these Lectures by arrangement with their college tutor.) On Elementary Botany: Mr. Trotter, Trinity. On Physiology: The Trinity Prælector of Physiology (Dr. M. Foster) at the New Museums.

MERTON COLLEGE, Oxford, announces an open Natural Science scholarship of the value of 80*l.* for five years, and one or more exhibitions of the value of 25*l.* for three years, to be awarded in December next. The examination will be in chemistry, physics, and physiology, and an opportunity will be given for showing a knowledge of practical work in chemistry and physiology. They will be awarded either for special excellence in one subject or for excellence in two out of three subjects, but no candidate will be examined in more than two subjects. There is no limit to age, but members of the University must not be over six terms standing.

THE Natural Science Scholarship at Lincoln College, Oxford, has been awarded to Mr. Schofield, of Owens College, Manchester. The National Science Demyship of 80*l.* a year at Magdalen College, Oxford, has not been filled up, there being no candidate up to the required standard.

THE Dublin University have just published the regulations for the examinations for women for 1871. They will be held in Dublin in March 1871, one for senior and one for junior candidates. The latter must not be above eighteen years of age. Examinations will also be held at any place where a ladies' superintending committee shall be constituted, and at least twenty candidates guaranteed to present themselves.

WE understand that Dr. Schimper, the palæontologist, escaped from Strasburg the day before the final closing of the gates at the commencement of the siege. Fears are entertained as to the safety of his fine collection.

IN addition to the literary prospects of the approaching season already announced, Mr. Murray's list of forthcoming books includes the following:—"On the Descent of Man, and on Selection in Relation to Sex," by Charles Darwin; with illustrations, 2 vols. crown 8vo. "The Student's Elements of Geology," by Sir Charles Lyell; with numerous woodcuts, post 8vo. "Scrambles among the Alps, 1860–69," by Edward Whymper, including the First Ascent of the Matterhorn, with Observations on Glacier Phenomena in the Alps and in Greenland; with 100 maps and illustrations, medium 8vo. "A Visit to High Tartary, Yarkand, and Kashgar (formerly Chinese Tartary), and Return Journey over the Karakorum Pass," by Robert Shaw; with map and illustrations, 8vo. Second and cheaper edition of "The Music of the Most Ancient Nations," by Carl Engel; with 100 illustrations, 8vo. A third edition of Kerr's "The Gentleman's House; or, How to Plan English Residences, from the Parsonage to the Palace;" with illustrations, 8vo. Among Mr. Bentley's announcements are—"Travels in the Air; a Popular Account of Balloon Voyages and Ventures, with recent attempts to accomplish the navigation of the air," by J. Glaisher, of the Royal Observatory, Greenwich; royal 8vo., with 133 illustrations. "The Marvels of the Heavens," from the French of Flammarion, translated by Mrs. Lockyer; crown 8vo., with numerous illustrations. A new and cheaper edition of "The Heavens," an illustrated handbook of Popular Astronomy, by Amédée Guillemin, edited by J. N. Lockyer, F.R.S.; demy 8vo., with 200 illustrations.

MR. M. C. COOKE'S Hand-book of British Fungi will be published in the course of the ensuing season by Messrs. Macmillan and Co. Although the first volume is already printed, its issue will be delayed until the whole work is ready. It will be a complete *vade mecum* for the British Fungologist, and will contain descriptions of all the species, and illustrations of all the genera. We are glad to see that the University of St. Lawrence has recognised Mr. Cooke's services to botanical science by conferring on him the honorary degree of M.A.

MR. VAN VOORST has in preparation "The Mollusca of the European seas," by Mr. Gwyn Jeffreys, F.R.S., in continuation of his work on "British Conchology."

AN English edition of M. Taine's "De l'Intelligence," revised with additions by the author, is in preparation, and will shortly be published by L. Reeve and Co.

WE have received a prospectus of a very energetic society located in the north of London, called the Hackney Scientific Association, which is now commencing its fourth session. It numbers among its officers many men of high scientific standing, and the following is its programme of papers for the season:—Recent Progress in Astronomy—W. T. Lynn; Comets—Henry T. Vivian; Observations on Solar Eclipses—William J. Dyer; Water—C. W. Stidstone; The Predicted and Observed Meteorology of 1870, and the Probable Weather for 1871—Frederic Pratt; A Catalogue of variable Stars, with Remarks upon their Physical Constitution—Albert P. Holden; The Calendar—Henry T. Vivian; The Methods used in endeavouring to determine Man's Antiquity—William H. Davis; Infusoria—William West; The Diseases prevalent in certain Geological Formations—H. W. Emons; Micro-Photography—C. W. Stidstone; The Habitable Condition of other Worlds, as affected by Temperature—Frederic Pratt; The Origin and Constitution of the Minor Planets, called Asteroids—Albert P. Holden; Inorganic Chemistry—George C. Fearn.

WE are glad to find in the number of our able contemporary, the *Academy*, for October, an extension of the scientific department. The paper will be published in future regularly on the 15th, instead of the second Saturday in each month.

WE learn from the *American Entomologist and Botanist* that the botanists of New York have formed themselves into a club, named, after one of the most distinguished botanists of America, the Torrey Club. The club publishes a monthly *Bulletin*, the object of which is "to form a medium of communication for all those interested in the flora of this vicinity, and thus to bring together and fan into a flame the sparks of botanical enthusiasm at present too much isolated." It is many years since a similar botanical club or society has existed in London.

MR. G. P. BIDDER has written to the *Times* describing a remarkable example of parhelia or mock suns observed by him on the 7th inst. at Canterbury:—"A portion of a circular arc was seen considerably above the halo and convex to it and the sun. The centre of the circle of which it formed part was, as nearly as I could estimate, close to the zenith. This arc had all the prismatic colours pure and distinct, and about 80° or 90° of it were visible. The following rough approximation represents the general arrangement of the whole: Altitude of the sun, about 12°; radius of the halo, about 25°; interval between the nearest points of halo and rainbow arc, about 25°; radius of rainbow arc, about 25°."

THE new buildings of the Taunton College School were formally opened during the past week. After a sermon from the bishop of the diocese and the inevitable luncheon, some very good speeches were made, among which we may especially mention those of the head-master, Rev. W. Tuckwell, Mr. J. G. Fitch, and Mr. E. B. Tylor. Mr. Tuckwell thus announced the programme of the system of instruction given at the school: "I may be allowed to say one word upon the nature of the education which we are striving to carry out, because to this is chiefly due the impulse in our favour which has so greatly altered our position. All who have been concerned with us are aware that in our curriculum we have departed widely from the ancient systems. We refuse to restrict our boys, as my own contemporaries were restricted, to the exclusive study of Greek and Latin; but while we give to these only a portion of our time, and find room for the higher mathematics, for physical science, for geography and history, for French and German, and for a careful study of English literature and language, we assert that by improved methods, by dexterous timing, and by greatly increased personal labour, we teach these new subjects thoroughly, and by no means neglect the old ones."

Engineering records the death, on the 16th inst., at New York, of Mr. Thomas Ewbank, the well-known writer on hydraulics. He was born at Barnard Castle, England, in 1792, and, after being apprenticed to a tin and coppersmith, came to London, where he spent all his spare time in scientific study. In 1819, being then a member of several learned societies, he emigrated to New York, where he was engaged for seventeen years in business as a manufacturer of lead, tin, and copper tubing, which occupation he relinquished in 1836 for purely scientific work. Besides contributions to scientific journals, and labours on various Government scientific committees, Mr. Ewbank was the author of "A Descriptive and Historical Account of Hydraulic and other Machines for Raising Water, both Ancient and Modern;" "The World a Workshop, or the Physical Relation of Man to the Earth;" "Life in Brazil, or the Land of the Cocoa and the Palm;" and numerous smaller publications.

THE United States steamer *Kansas*, now fitting out at the Washington Navy Yard for duty on the Tehuantepec and Nicaragua Expedition, was, according to *Engineering*, put in commission about a fortnight ago. The *Kansas* will be the principal vessel of the expedition. The survey in Nicaragua will embrace the route for a canal advocated thirty years ago by the Emperor Napoleon. That in Mexico by the Tehuantepec river possesses less interest, owing to the length and difficulties of the route.

THE first quarterly number of the *Journal of the Iron and Steel Institute* is announced to appear with the new year.

MR. ADOLPH HUBNER read an interesting and valuable paper on scientific observation in the interior of Port Natal, at a meeting of the Natural History Association of that colony on the 20th of August.

ON the 8th August Mr. S. Vincent Erskine read a paper before the Natal Natural History Society—Mr. John Robinson in the chair—on the Tsetse Fly. Mr. Erskine severely criticised Dr. Livingstone's statements, and denied that the fly was destructive to the life of the ox, horse, or dog. He affirmed that death was to be attributed more to change of grass or climate. The same evening Mr. Morant read a paper on the Entomology of the Free State and the Trans Vaal, particularly with regard to the butterflies.

THE expedition of Yale College students, under the leadership of Prof. O. E. Marsh, to which we referred last week, spent several months in the Rocky Mountain regions, investigating its flora and fauna, and collecting for the Yale Museum as fine collections as possible of the extinct animal remains found in such abundance in the tertiaries and cretaceous deposits of Nebraska, Dakota, and Wyoming. Leaving this region they will visit California, and after investigating the geology of the Pacific coast, will return through Colorado and Kansas, reaching New Haven, if possible, in November.

COCOA

COCOA is a valuable article of food that is becoming more and more in use in this country, and judging from the increased importations during the past three or four years, and the constant average of the coffee imports during the same period, it seems that cocoa is, in a measure, displacing coffee as a popular beverage.

The plant producing the cocoa of commerce is a tree seldom growing to a greater height than 17 or 18 feet. It is known to botanists as *Theobroma Cacao*. It bears an oblong fruit, ribbed longitudinally, measuring from six to ten inches in length and four to five inches across, and, when ripe, is of a yellow colour, changing to brown in drying.

It contains from fifty to one hundred seeds, and these seeds, after being washed, thoroughly dried in the sun, and roasted, form the cocoa-nibs of commerce.

Linnæus must have had a high appreciation of cocoa when he gave to the genus the name Theobroma, which is derived from *theos*, god, and *broma*, food, signifying it as food fit for a god. Cocoa contains a large amount of nutritive matter. In this respect it differs in a marked degree from tea and coffee; for while they are taken only in infusion and are used as refreshing beverages, cocoa is usually taken more in substance, and, as such, may be considered both as food and drink.

It was used in very early times in Mexico, whence it was introduced by the Spaniards into Europe about 1520. Humboldt tells us that it was extensively cultivated in the time of Montezuma, and the seeds were commonly used as money by the Aztecs. At the present time the cocoa-tree is largely grown in the West Indies, more especially in Trinidad, and over a great part of tropical America. Numerous varieties of the cocoa tree exist, some producing longer, or broader and some thinner or thicker skinned fruits, others producing larger, longer, or broader seeds, as the case may be. The seeds also vary in quality, according to the variety producing them or the place of their growth: thus Caraccas and Trinidad seeds are considered the finest, and some manufacturers use the names of the best districts as a recommendation to their wares.

The seeds are brought into this country in a dried state, and are roasted in revolving metal cylinders, the heat causes them to shrivel slightly so that the husks or skins are left loose and are removed by fanning. It is said that large quantities of these husks are imported from Italy under the name of "Miserable," and are used in Ireland by the poorer classes. The roasted seeds, after the husks are removed, are known as cocoa-nibs, but they are never seen in commerce in their whole form. The seed naturally divides by its two cotyledons, and in the process of winnowing each cotyledon gets broken into two or more pieces. To obtain the nibs and boil them in the old-fashioned way is certainly the surest way of getting genuine cocoa.

Some trouble, however, attends the preparation of the beverage in this form, the nibs requiring to be boiled an hour or two to extract their valuable properties. To obviate this, and to supply the public with a more convenient article, powdered cocoas, which require simply mixing with cold milk, boiling water being afterwards added, were introduced. These prepared cocoas opened a wide field for wholesale adulteration, the public, by using them, sacrificing purity for convenience in the preparation for the table.

These powdered cocoas are "prepared" by reducing the seeds to a fine paste by grinding them under heavy heated rollers—starch, flour, sugar, molasses, and, in the cheaper kinds, other ingredients less wholesome being added; after which, the whole mass is reduced to powder, packed in different forms, and sold under various trade terms, such as "Homœopathic Cocoa," "Soluble Cocoa," &c. Each manufacturer's individual preparation varies perhaps in flavour, according to the proportion or character of the ingredients added. The numerous forms of cake chocolate are prepared in the same way, vanilla being largely used in the flavouring, and the pasty mass being pressed into moulds instead of being reduced to powder. Rock cocoa and Flake cocoa are likewise prepared in a similar way, but are not so highly flavoured.

Few articles are more liable to adulteration than cocoa; and so many forms or qualities are known in trade, varying in price from 6*d.* up to 4*s.* per lb., that it is not surprising that in the cheapest forms the adulterants themselves should be of the commonest and worst description. If people would only trouble themselves to think that cocoa-nibs, which are simply the roasted seeds without any preparation, are retailed at 1*s*. 4*d*. per lb., how can they expect to obtain an equally genuine article in a finely-pulverised state, and packed in tinfoil and a showy outward cover, at the same price? which is what the so-called "Homœopathic" and similarly prepared cocoas are sold at. Expensive machinery in the first place, and the constant wear and tear of the same, the consumption of fuel in the steam apparatus, and the expense of packing, have all to be paid for by the consumer, not by charging him a directly higher money price, but by increasing the bulk or weight of the article by adding foreign substances of a much cheaper description, and, which is frequently done in the commoner kinds of cocoa, bad or damaged seeds themselves. There is one thing to be said in favour of our principal cocoa manufacturers, that they seldom advertise these powdered cocoas as genuine; they either leave out that important word altogether or call them "prepared" cocoas, and this word should be borne in mind by those who wish to avoid the prepared and to obtain the real article, and are consequently ready to pay a fair price for such. If it is impossible to procure genuine powdered cocoa at 1*s*. 4*d*. per lb., still more impossible is it at 6*d*., which is the price paid by the poorer classes for an article called "Soluble Cocoa," sold in ¼ lb. packets at 1½*d*. each, and largely consumed by them. The very fact of its low price ought to be sufficient to tell us pretty plainly that a very small quantity of cocoa, and that of an inferior description, is to be found in such a packet. It contains a large amount of common fat, the presence of which can be detected by smearing a little on a piece of glass, and can be still more clearly seen on a glass slide under a microscope. The addition of fat adds to the weight, while, to increase the bulk, a very large quantity of starch is added, which is the cause of the thickening of the beverage in the cup. If a little of this so-called cocoa be placed on the tongue and rubbed against the roof of the mouth, it will be found to grate against the palate, and, moreover, to have a decidedly chalky or earthy flavour. The spoon also grates against the sediment at the bottom of the cup, clearly showing the presence of mineral matter.

FIG. 1.—Section of Cocoa-Nib as seen under the microscope

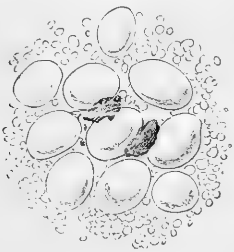


FIG. 2.—"Soluble Cocoa" as seen under the microscope

Until within the last few years, all these powdered cocoas were more or less "prepared," so that pure cocoa could not be obtained in this convenient form. An article, called "Cocoa Essence," recently introduced, has, however, dispelled this notion. We all know that the cocoa-seed naturally contains a large quantity of butter or fat (about 50 per cent.) which makes it too rich or heavy a beverage for many persons, and this more especially when we consider that other elements of nutrition, such as albumen, are also present. To deprive it entirely of its butter would be to take away one of its valuable principles; but it is possible to have too much of a good thing; therefore, by taking away about two-thirds of the butter the cocoa itself is not only improved in a dietetical point of view, but the addition of sugar, arrowroot, &c., is rendered unnecessary to take up or balance the fatty portion. Those who wish for pure cocoa in a convenient form should therefore obtain the "Cocoa Essence." It is sold in 3oz. packets at 6*d*. each. A small spoonful is sufficient for one cup, and, unlike the "Homœopathic," "Soluble," and other similar cocoas, it is not mixed with milk, but with a little boiling water, and stirred for a second or two until it is dissolved into a fine paste, the cup is then filled with boiling water, and milk and sugar added to please the taste. As no sugar is used in the manufacture of this article, it requires the addition of a larger quantity than any of the so-called prepared cocoas, and as no starch enters into its composition, the beverage is as clear as a cup of well-strained coffee. It is quite as portable as any of the packet cocoas, and as easily mixed. Its extra cost, in the first instance, is fully compensated by its purity, and by the fact that a smaller quantity is required for each cup. It is, moreover, a proof of the extensive adulteration of those kinds which are retailed at 1*s*. 4*d*. or 1*s*. 6*d*. per lb.

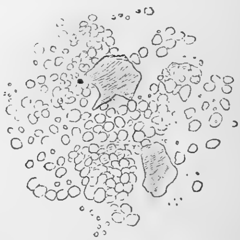


FIG. 3.—Microscopical appearance of "Cocoa Essence"

To such an extent has the public palate been led to prefer the flavour of many adulterated articles to that of the genuine, that we believe a great proportion of those who take cocoa really do prefer the thickened soup-like preparation made from the highly-flavoured and doctored sorts to an infusion of the pure seeds. If such people would think for one moment why and for what purpose they take this or that kind of food, and what are the properties and effects on the system of the articles they are supposed to consume, and what those of the articles they actually do consume, a much better state of things might be brought about, for, pending the appointment of a public analyst, the head of every household might make himself analyst to his own family, and so see that he does not get cheated either in pocket or health.

JOHN R. JACKSON

THE RECENT DEVELOPMENTS OF COSMICAL PHYSICS*

THERE are two conflicting theories with regard to education. The extreme partisans of one of these would have us believe that the great object of education is not so much to inform as to discipline the mind—the subject taught in a seminary may not in itself increase the student's real knowledge, yet if it tend to discipline his mind, it has proved its value in their eyes as a branch of education.

According to the upholders of this theory, our object in going to school is to leave it with a mind enlarged in its capacity for acquiring knowledge rather than stored with knowledge itself—having trained the soldier well, they would send him without scruple into the enemy's country, not only to fight his own way but to find his own weapons.

But there is another and opposite class of theorists who regard education not as an agent for training the mind so much as a means of storing it with knowledge. The extreme partisans of this theory would teach the student nothing but what is of apparent and immediate use; above all things they would teach him the sciences, both in their principles and also in the various details of their applications to the industrial arts of life.

The mind of the student who has undergone a training of this kind carried to its extreme, resembles the inmate of a house which is not so much well-furnished as filled full of furniture. In the accumulation of mere material, anything like plan or principle has been forgotten. It ought to be remembered that the value of a fact lies not in its existence somewhere in the mental storehouse, but in the readiness with which the mind can find it when required.

Now between these two extreme theories it is surely possible to steer a middle course—it is possible to avoid grounding on Scylla without being swallowed up in the vortex of Charybdis. In the material world, what would be said to a man who insisted upon developing bodily strength by a course of gymnastics without reference to food, or of another who insisted upon doing the same by a course of diet without reference to exercise? But is the separation more natural in the mental world? Is not that mind most perfectly disciplined which is at the same time most perfectly informed? The student who has been disciplined by only one branch of knowledge is, I venture to think, the possessor of a mind only partially disciplined, as it is only partially informed. He may not easily perceive his deficient discipline, but in after life he must often have cause to regret his deficient information. Nor is he whose mind is inordinately stored with scientific details one whit better off. Facts in education ought to be strictly subordinated to principles. A sound principle of science clearly understood will embrace a great multitude of facts, just as a simple rule of arithmetic will enable us to obtain a thousand products, each of which we should have to remember were it not for the rule. And in other branches of science, if the triumph of principle be not so apparent, it is only because we have a less accurate knowledge of its fundamental laws. It would be difficult indeed to say how many of the failures in the various walks of life are due to the neglect or ignorance of some principle which has been either omitted or dismissed from our calculations. From our leaders downwards we are a nation systematically ignoring principles, and in the minds of many a high esteem for fact is held to be quite consistent with a contempt for theory. A theory is not regarded as the very sap and life-blood of the tree of knowledge, but rather as the mildew that blights its leaves, or the worm that gnaws its root. Facts and theories are esteemed by this class of men to be sworn foes to each other, and the philosopher is supposed to live in a world of his own, rather hostile than otherwise to the world around him.

The existence of the two extreme educational theories to which I have alluded would thus seem to indicate the wisdom of a middle course. We ought to start from a platform as comprehensive as possible. Literature and science ought to go hand in hand in producing the well-trained and well-informed disciple. And while there ought to be a broad basis of instruction common to all, there should be raised upon this common basis several distinct departments, so that the student may have the opportunity of attaining proficiency in that of his choice.

Professor Stewart next touched upon the subject of energy.

If an egg be allowed to rest on its shorter axis, it will remain stationary, and any attempt to alter its position will be resisted by the egg. But the case will be different if we succeed, as perhaps we may, in causing it to stand on its longer axis, for in this position the slightest force will cause it to topple over. In the first case the egg is in stable, but in the second case it is in unstable, equilibrium.

If it stand upon its longer axis at the very edge of a table, we cannot tell whether the first slight breath of air will cause it to fall inwards upon the table or outwards over the table, to be dashed to pieces on the floor. It is a case where a very slight cause may determine a very considerable issue as far as energy is concerned. Whether the egg will retain its energy of position by falling on the table, or whether it will convert this into the energy of motion, and ultimately into heat, by falling upon the floor, is an issue that depends upon a cause too minute to come within the scope of our calculations.

Now we have two types of machines, and in one of these we take advantage of the principle of stability, while in the other we use the principle of instability. A clock is a very good instance of a machine of the first kind. When a good clock has been wound up, we are perfectly sure that at noon to-morrow both its hands will stand at twelve, and that its weight will have fallen through a distance which we can calculate with the utmost exactness, if we take the trouble, all its movements being capable of the most rigorous calculation. On the other hand, a mine that is about to be fired by means of an electric battery is a machine or combination in which advantage is taken of nature's unstable arrangements. The powder which is about to explode represents chemical instability, just as the egg on its longer axis represents mechanical instability. The slightest percussion, the smallest spark will rouse the imprisoned giant which it contains into volcanic energy. This spark has to be sent from a distance by the galvanic battery, and to do this we must complete the circuit. We cause the two wires to approach each other until they are only a very small fraction of an inch apart, but the contact is not yet complete—another touch, so slight as to be imperceptible, and the current passes, the powder is ignited, the mine explodes, and the fortress is hurled into the air. In such machines, great results, great transmission of energy, are due to the most trivial disposing causes. It depends at last upon the smallest conceivable movement of the wires conveying the current whether or not the fortress is to perish.

Nature also employs these two varieties of mechanism. In the solar system we have a clock on a large scale, only very much more accurate than any time-piece we can produce. The movements of every planet in the solar system are susceptible of the most rigorous calculation, and we have only to point our telescope properly in order to tell to the fraction of a second when a given planet will cross the field of view.

But in the living forms with which this world is so plentifully endowed, we have machines, which, viewed in their relation to matter, belong to the second order we have described. The object here is not regularity, but rather freedom of action. The motion of an animal is not like that of a planet—the latter yields to calculation, but the former defies it. Now it is probably somewhere in the mysterious brain chamber that the delicate directive touch is given which determines our movements, just as the slightest possible touch to the wire in the battery chamber explodes the distant mine. That mysterious thing we call life is not a bully who swaggers out into the open universe, upsetting the laws of energy in all directions, but rather a consummate strategist, who, sitting in his chamber before his wires, directs the movements of a great army.

While we are thus led to confine the directive action of life to the very boundary of the universe of energy, it must not, however, be supposed that we have solved the problem as to the nature of life. We have only driven the difficulty into a border land of thick darkness, into which the light of knowledge has not yet been able to penetrate. If there be truth in these statements, and if we see in a living being a machine in which great results are produced by an exceedingly small primeval impulse, are we not led to expect that the unstable forms of nature will here be largely made use of? We must not therefore be surprised that the materials of our bodily frames are eminently liable to decay, or that the very intensity of our life is to be measured by the rate of change taking place in the tissues of our bodies, so that possibly those parts which have during life the noblest and most delicate offices to perform are the very first to perish when life is extinct.

But this unstable matter, which is so wonderfully worked into

* Extracted from a Lecture delivered at Owens College, Manchester, introductory to the Session 1870-71, by Professor Balfour Stewart, LL.D., F.R.S.

our frames, is derived from the food we eat. This food does two things for us: it gives us energy in the first place, and in the second it furnishes our frames with a quantity of delicately organised tissue. But food is ultimately derived from the vegetable kingdom, and that kingdom derives it from the sun, so that we are led to regard our luminary as the ultimate material source not only of our energy, but also of our delicacy of construction.

To come now to our own luminary—very remarkable strides have lately been made in our knowledge of its physical constitution. It is difficult to say when and by whom the existence of sun spots was first remarked. Galileo, however, was the first to use them as the means of determining the elements of the sun's rotation. Besides these black spots on the sun's surface, equally mysterious forms have been seen to surround the sun on the various occasions of a total eclipse—these generally went by the name of red flames or red protuberances. Mr. Warren De La Rue was the first to prove that these phenomena were attached to the sun himself, and that the only office of the moon during an eclipse was to subdue the general light sufficiently to render them visible to the eye. While the red flames thus became objects of cosmical interest, Schwabe in Germany and Carrington in this country had both done much to increase our knowledge of sun spots. Schwabe, by a patient course of forty years' observations, had proved the existence of a well-marked periodical fluctuation in the amount and frequency of sun spots, the period of which was about eleven years. Carrington, again, had shown that the region of spots was a somewhat limited one, extending to about 20° or 30° on either side of the solar equator, so that a spot never appears at the sun's pole, and he had also detected a proper motion of spots. Schwabe and Carrington had, however, confined themselves to mapping down accurately what they saw; but De La Rue, by the introduction of celestial photography, was enabled to obtain autographs of the sun which might be studied at leisure with an absolute certainty of their being correet. A large number of such pictures has been already obtained, and they are in the process of examination by Mr. De La Rue, and those associated with him in this research.

Some of the preliminary results of this examination have already been published, and they seem to point to a connection between the behaviour and frequency of sun spots and the positions of the chief planets of the system.

Our acquaintance with the red flames has extended as rapidly as our knowledge of sun spots. It was discovered independently by Janssen and Lockyer, that these strange protuberances yield to the spectroscope under ordinary conditions of the sun, and without the necessity of waiting for a total eclipse. They exist, in fact, always round the sun, but their brightness is quenched in the diffused light which surrounds the sun's border. When, however, we apply a sufficiently powerful spectroscope, the diffused light—consisting of ordinary sun light, and therefore containing a great variety of rays—is drawn out into a long spectral ribbon, and has its brightness scattered or diffused over the various parts of this ribbon, while on the other hand the light from the red flames, consisting only of one or two kinds, appears in the spectroscope as one or two bright lines not having their intensity weakened by the scattering action of the spectroscope. They, therefore, stand out in the field of view, while the ordinary light disappears. Lockyer has found, by this means, that there is an envelope of incandescent hydrogen surrounding the glowing surface of the sun, into which there are frequent injections of heated matter from beneath, and in which there are violent huricanes often moving at the rate cf 100 miles a second. By the laboratory labours of Frankland and Lockyer, taken in connection with the solar observations of the latter, there is, I think, a probability of our ultimately ascertaining the pressure and the temperature as well as the chemical composition of the atmosphere of our luminary.

Descending now from the celestial bodies to our own earth, there is some reason to suppose that we are knit to our luminary, and possibly through him to the other members of our system by some other bond, besides that usually recognised. General Sir E. Sabine appears to have proved that disturbances of the earth's magnetism take place most frequently in those years in which there are most sun spots. This is confirmed by the experience of the present year, during which we have had a great number of sun spots, and frequent and large disturbances of the earth's magnetism.

I have already alluded to a possible connection between the behaviour of sun spots, and the positions of the planets; to which we may add, that Schwabe and other observers imagine that they have discovered traces of a periodical variation in the appearance of the planet Jupiter. All these observations would appear to indicate the existence of some unknown bond between the different members of the solar system.

But that department of cosmical physics which is of most immediate interest to ourselves, is undoubtedly the meteorology of our globe; and here it is of great importance to know whether the earth's climate and atmosphere are influenced in any way by the changes taking place in the atmosphere of the sun. Such a connection has not yet been traced, but it has hardly yet been sought for in a proper manner. Recent observations discussed by Baxendell, lead us to think there may be some connection between the daily changes in the earth's magnetism and the daily motions of the air. Coupling this with the fact that the frequency of terrestrial magnetic disturbances would appear to be connected with that of sun spots, we are led to contemplate at least the possibility of some connection between meteorology and sun spots.

If these remarks are of any value, they tend to indicate the probable union of the various branches of observational inquiry into one great cosmical research, and point to the wisdom of a very close union between the workers in the cognate fields of meteorology, terrestrial magnetism, and celestial physics.

At the present moment the prospects of meteorology are more hopeless than those of the other two branches. Our knowledge of the motions of the various components of the earth's atmosphere is extremely limited, and yet without this knowledge it is impossible to connect meteorology with the other branches of cosmical inquiry. If we endeavour to analyse the causes of this backward state of meteorological research, the first and most apparent is the magnitude of the problem.

We are too intimately associated with the earth and its atmosphere to be easily able to tell its motions. Strange to say, the meteorology of the sun is more easily studied than that of the earth, and we know already as much about the strength of solar storms as we do about that of terrestrial hurricanes.

But another cause of the backward state of Physical Meteorology arises from the fact that there are two branches of science, each of which may be furthered by meteorological observations. There is first the physiological branch of meteorology, the object of which is to trace the influence of climate upon animal and vegetable life; and there is next the physical branch, the object of which is to study the physics of the earth's surface, and more especially the motions of its atmosphere.

It is now high time that a separation should be made in the mind of the observer between these two branches of research. If he would rather pursue the physiological inquiry, let him say so definitely, but if he wish to pursue physical meteorology, let him clearly keep before his mind the object of all his labours. He should ask himself the question, what is the best system of observation, and what is the best method of reduction, to advance the great object of physical meteorology—a knowledge of the motions of the earth's atmosphere, and of the causes thereof? He should not fix upon a system of observations and a method of reduction that may possibly, but upon one that must necessarily, further this great object.

I have thus endeavoured in a few words to bring before you the recent advances in cosmical physics. Besides this, there are two other no less important branches of physical inquiry. We have the physics of organised beings, and we have also molecular physics. But there is this difference between these two branches and that of which I have now spoken:—To forward physiology or molecular physics we chiefly require experiment, but to forward cosmical physics we chiefly require observation. You are all aware that at the present moment a Royal Commission is inquiring as to the relation between Science and the State; and perhaps, therefore, you will permit me the opportunity of stating my views as to the manner in which this very necessary assistance may best be given. I think that those branches of science which demand for their extension experiments not requiring very great time may be furthered with much advantage in institutions such as Owens College. I believe it to be advantageous to bring the highest class of physical teaching into contact with research. If Government be disposed to grant pecuniary aid to such researches, an extension of the allowance made annually to the Government Grant Committee of the Royal Society would appear to be a very legitimate way of accomplishing this object.

The scientific professors of a college would thus have to state the aim of their research to a committee of the Royal Society entrusted with the disposal of Government means, and the requisite funds would be forthcoming. No one, I think, can doubt that the small sum of 1,000*l.* annually entrusted by Government to the Royal Society for miscellaneous experiments, is administered in a most praiseworthy manner; and if Government would be ready to grant, and the Royal Society willing to undertake, an extension of this trust, it would, I think, be a great point gained for this class of physical experiments. (I speak only of experiments, but the encouragement of experimenters is a point of equal importance.) But when we come to experiments and observations requiring great time, the case is very different. Certain experiments, whether from the great time they require or the great expense they demand, cannot be well performed in a college; while routine and long-continued observations such as those connected with the various branches of cosmical physics are of such a nature as to require a central establishment to superintend their organisation and reduction. There is thus, I think, the necessity for a central establishment of some kind devoted to that class of experiments and observations requiring great time, great space, and great expense for their completion.

Referring more particularly to Cosmical Physics, I feel convinced that meteorology should be pursued in connection with terrestrial magnetism and solar observations; and were a well-considered scheme for solving this great problem fairly introduced, I am sure that scientific institutions and individuals throughout the country would do all that they possibly could to promote this most important branch of physical research.

THE BRITISH ASSOCIATION

SECTIONAL PROCEEDINGS

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE

Barometric Predictions of Weather.—Mr. F. Galton, F.R.S. It has long been an established custom to consult the barometer to learn what the weather is likely to be. Now, I propose to investigate the value of this form of barometric authority by showing that it is possible to make strict use of the rules of prediction, notwithstanding the vagueness with which they are enunciated, and then, by comparing a series of carefully-made predictions with facts, to measure the degree to which they correspond. There is another form of barometric authority about which I do not propose to say anything here, namely, where the barometer is consulted in connection with the daily Weather Report. Owing to the new data thereby introduced, an inquiry into the value of those predictions would have to be conducted along an altogether different line to that which I am about to follow.

My comparisons between predictions and facts will be based upon the tracings of the continuously self-recording instruments at Falmouth, established by the Meteorological Committee appointed by the Royal Society, which have been published for the first quarter of the year 1869. It is, however, right to add, that some years ago I made an elaborate inquiry into the Dublin observations during a much longer time, which led, so far as it went, to the same conclusions as now.

I did not publish those inquiries, because I had a misgiving which was never wholly removed until I had the opportunity now afforded by the above-mentioned publication of studying the *continuous* records of instruments in large numbers. It is said that instrumental changes commonly occur in sweeps so large and steady that future changes in them may be to some extent predicted by a knowledge of what has occurred. An analysis of the Dublin observations, made at intervals of three hours, contradicted this assertion, but I felt they might be held insufficient to dispose of it. It might fairly be said that three hours was too long an interval between the observations, and that if the instruments had been read off more frequently, I should have been led to different conclusions. It was necessary to settle this doubt, because, as there is certainly some correspondence between the barometer and the weather, it followed that if it be possible to predict the movements of the former, we shall also, as a matter of course, be able in some degree to predict the latter. I therefore examined the tracings which represent the continuous records of the barometer and other instruments with great interest and care, and soon convinced myself that the irregularities of the barogram and thermogram were far too great to enable us to predict their course from moment to moment. We have only to place a paper upon them, so as to hide what follows any given instant, and to expose what precedes it, and to move the paper forward, step by step, guessing beforehand what we are to see, to be convinced of the vanity of our expectations. This basis of weather prediction is so manifestly unsound, that I shall not take any further notice of it.

We all know that the weather with which the barometer sympathises, is considered to consist of three independent variables—the velocity of the wind, its temperature, and its dampness. It is a question how far the direction of the wind need be reckoned as a fourth distinct influence. We also know that the velocity of the wind is the most important; it is said that when the two other variables are unchanged, and the velocity of the wind alone varies, the barometer may range through two inches, but that it can only range through a quarter as much when either the temperature or the damp are the sole variables. I therefore feel at liberty to begin by simply comparing the changes of the wind's velocity with those of the barometer, in order to obtain a provisional idea of the manner in which they go together.

Two things are very clear at first sight—the one is that the wind's velocity passes through numberless tumultuous variations of which the barometer takes no cognizance, and the other is that a connection decidedly exists between periods of storm and of fine weather, with barometric falls and rises. What, then, do we mean by *periods* of storm? How long is the period during which the velocity of the wind should be summed up and averaged, in order to be made to accord most closely with the barometer?

I made several trials, and protracted the results on the same time-scale as the corresponding barograms. The ordinates of the different points whose position I calculated represented the average velocity of the wind during a definite period at the moment indicated by the point; then I connected the points by a line drawn with a free hand. In this way I constructed a curve, every point of which represented the average velocity of the wind during the space of one hour, being half an hour before and half an hour after the instant corresponding to that point. In another curve, a three-hour period was adopted, and so on. Below all these I copied the barogram.

There could be no doubt, on inspecting those lines, that a one-hour period is far too short, that a three-hour is better, a six-hour better still, and that a twelve-hour is as good as can be obtained. Any period between twelve and sixteen hours seemed equally suitable for adoption, some parts of the curve improving in correspondence as the period was lengthened, and others falling off; but, after a sixteen-hour period, the curve of wind velocity became less varied than the barogram, and the maximum of correspondence was passed. Finding the twelve-hour system the most convenient to employ, I have adopted it here, leaving it to be understood that a different period might be taken within the limits named, without sensibly affecting the results.

The correspondence between the wind curves thus obtained and the barograms is respectably close, there being hardly a dip or rise in the one which has not a counterpart in the other; but they are far from being exactly alike. Neither do the changes, of course, in the two curves, bear an invariable relation in point of time to one another; but, as neither of them lags habitually behind, they must be considered *on the average* simultaneous.

I do not find the correspondence sensibly affected by making broad allowances for the neglected variables. Thus, on marking the epochs of cold and dry polar winds in one way and those of warm and moist equatorial winds in another way, little or no new light was thrown on the reason of the want of coincidence of the two curves. It seemed to me, from this trial, that the influence of temperature, damp, and wind's direction, is considerably less than was commonly believed.

The parallelism of the curves was as close in extreme positions as in mean ones, which confirms the common statement that we must look to differences of barometric height and not to the absolute height for signs of changing weather.

All this is easily compressed into a formula: h_1 h_2 are two successive barometric heights a few hours apart; v_1 (12) v_2 (12) are the corresponding twelve-hour averages of wind velocity; m is a simple factor to be determined by trial, then

$$h_1 - h_2 = m\left\{v_1(12) - v_2(12)\right\}$$

+ a function of temperature and another of damp, neither of which is of much importance.

m is strictly constant only for the same season, because the range of the barometer is wider in winter than in summer, for the same latitude because its range is smallest at the equator, and for the same locality because the wind's velocity may be checked by geographical conditions. Bearing this in mind, the value of m for the first quarter of the year, at Falmouth, as derived from about 100 selected equations of the form $\frac{h_1 - h_2}{v_1 - v_2}$ is -2, when the barometer is reckoned in hundredths of an inch and the velocity in miles per hour. In selecting the equations, I omitted all cases of abrupt change in any of the variables. Consequently our equation becomes

$$h_1 - h_2 = 2 \left\{ v_2 (12) - v_1 (12) \right\}$$

+ the functions of temperature and damp.

It may now be very reasonably asked how it is possible for the barometer to be affected by past and coming conditions of wind. Its sympathy with such considerable periods as six hours before and six hours after the moment of observation, cannot be accounted for on the hypothesis of each new phase of weather regularly making its first appearance high in the atmosphere, because, if it did so, each phase would necessarily disappear from above before it disappeared from the earth's surface, and, consequently, the barometric change would invariably precede the change of average wind velocity, which, we have already seen, it does not. What, then, is the explanation of the curious phenomenon, of the barogram corresponding with the average velocity of the wind, according to the system of twelve-hour periods?

The answer to this question will best be conveyed by a consideration of what we should expect the movements of the mercurial column to be if a suitably made barometer were plunged into troubled water. Its movements would not correspond to each ripple that passed vertically above its cistern, because it would be affected by all the disturbances in an area of surface water whose radius is a function of the depth of immersion. If it were plunged to the depth of many fathoms the mercury would wholly cease to oscillate, because the average level of the large area with which it sympathised would be constant, however much its surface might be broken up into undulations. If it were immersed to a suitable depth, the mercury would foretell the advent of each wave of exceptional size, before an exceptional height of water had arrived vertically above the barometer. It is easy and interesting to make an experiment to the same effect, by dipping a glass tube, open at both ends, straight into a pan of water, and disturbing the water with the hand. When the tube is dipped but a short way in, the water it encloses harmonises in its oscillations with the water that surrounds it, but this harmony is diminished, and the oscillations in the tube become more sluggish, as the tube is immersed more deeply, and at length they disappear altogether. In precisely the same way I believe the mercury in the barometer sympathises with atmospheric disturbances throughout a wide circle. Its height represents the average value of them at the moment of observation, and when a great atmospheric disturbance sets in, as is wont, from the westward, the barometer is affected some time before the arrival of the locus of greatest disturbance. The diameter of the circle which affects the barometer may admit of being determined in more than one way, but I am not now concerned with its linear measurement. What I am immediately in search of is, what the diagrams have already told me, that its diameter in relation to its usual rate of movement is such, that it is commonly twelve hours in passing over an observatory.

It appears to follow that the twelve-hour period for averages must apply not only to the wind but to all other elements of atmospheric disturbance, such as temperature and damp. Therefore the undetermined portion of our equation will be functions of t (12) and of d (12).

Without professing to decide the precise nature of those functions, we may be sure that it does not differ materially from a simple proportion, within the limits of meteorological records. The inferior importance of these functions makes a small error of still less consequence. I therefore assume the undermentioned portion of the equation to be

$$p \left\{ t_1 (12) - t_2 (12) \right\} + q \left\{ d_1 (12) - d_2 (12) \right\}$$

Calculating on the basis of the already quoted statement, that temperature and damp, unaided, may respectively affect the barometer to the amount of half an inch, p and q may both of them be considered equal to -1, when t is reckoned in degrees Fahr., and d is the vapour tension expressed in hundredths of an inch. For reasons already mentioned, I disregard the direction of the wind. Consequently the formula becomes

$$h_1 - h_2 = 2 \left\{ v_2 (12) - v_1 (12) \right\} + \left\{ t_2 (12) - t_1 (12) \right\} + \left\{ d_2 (12) - d_1 (12) \right\}$$

and I now proceed to utilise it, in making a series of predictions for comparison with facts.

Let h_1, h_2, be separated by an interval of six hours, which I will distinguish by the letter b; similarly let a represent the six hours that preceed h_1, and c the six hours that succeed h_2.

Now the average wind velocity during the twelve-hour period $a + b$ is half the sum of the average velocity during the six-hour periods a and b,

or $$v_1 (12) = \tfrac{1}{2} \left\{ v (a) + v (b) \right\}$$

also $$v_2 (12) = \tfrac{1}{2} \left\{ v (b) + v (c) \right\}$$

$$v_2 (12) - v_1 (12) = \tfrac{1}{2} \left\{ v (c) - v (a) \right\}$$

similarly for t and d.

Therefore our equation becomes

$$h_1 - h_2 = v (c) - v (a) + \tfrac{1}{2} \left\{ t (c) - t (a) \right\} + \tfrac{1}{2} \left\{ d (c) - d (a) \right\}$$

and

$$v (c) = h_1 - h_2 + v (a) + \tfrac{1}{2} \left\{ t (a) - t (c) \right\} + \tfrac{1}{2} \left\{ d (c) - d (a) \right\}$$

Using this simple formula, I selected all the periods during which the changes of the barometer had been abrupt or otherwise characteristically marked, and I calculated the values of v (c) during those periods, obtaining in this way a total of 106 predictions. Comparing these with the actual facts, I obtained a mean error of ten miles per hour. Next, in order to procure a standard whereby to ascertain the importance of this error, I obtained and took the mean of a series of differences between the same observed values of v (c) and v (b); in other words, I calculated what the mean error would be supposing it was invariably asserted that the average wind velocity for the next six hours would be the same as during the six hours just elapsed. The mean error in this case was only 7·7 miles per hour. This extraordinary result made me curious to learn whether the co-efficients of t and d might not be altered with advantage; so I first made them both $= 0$, in fact, ignoring the influences of temperature and damp altogether. The mean error again came out ten miles per hour, the gains and losses due to the correction having balanced one another. Secondly, I made the co-efficients each $= -2$, that is to say, I doubled the importance originally given to temperature and damp, and the mean error rose to 11·3 miles per hour.

The result of all this is, that, judging by the experience of 106 well-marked instances of change occurring at Falmouth during the first quarter of 1869, it is more unwise in the ratio of 10·0 to 7·7 to be guided by the barometer, than to say off-hand that the weather will continue as it has been. Also that there can be no gain and may be further loss, if the wet and dry thermometers be consulted as well.

It is, no doubt, possible that the errors I have assigned may be qualified in a trifling degree by other calculators. They may adopt periods of average and numerical co-efficients, somewhat differing from my own; also, their data as measured off from the instrumental tracings, may be more accurate than those that I made, but I feel satisfied there is no mistake in the broad truth of my results. After more tentative analysis than I care to describe, I believe it impossible to substantially improve these predictions, and the experience I gained from the Dublin observations makes me doubt whether a more extended examination would lead to different conclusions. The barometer, when consulted by itself, without a knowledge of the weather at adjacent stations, can claim but one merit, namely, to guide us in a form of storm which does not occur once a year in the British Isles, of a fall in the mercury outstripping in an extraordinary degree the increasing severity of the weather; and I believe it to be on account of this rare phenomenon here, and of the reports of sailors from hurricane latitudes, where it is much more frequent, that the fame of the instrument has been so widely spread. But for use in ordinary English gales, and still less in ordinary English weather, this inquiry shows the barometer to be one-third worse than no guide at all. It is better to base our expectations upon what has occurred, than also to take the barometer into our counsel. We easily see the reason of this to be, that the theory of prediction involves many postulates, every one of which must be strictly fulfilled in order that the result may be correct. But they are only true on the *average* and not in the individual case. The area with which the barometer sympathises is never exactly twelve hours in passing over us; the six-hour radius of that area, which has already gone by, is not an accurate sample of the demi-area of which it forms the central strip; neither is it at the moment of observation in the same condition as when it passed over us. Precisely the same may be added in respect to the six hours of weather which are the subject of prediction. It must also be especially borne in mind that whatever error may affect the twelve-hour average is necessarily doubled in the six-hourly prediction, because the difference between what was expected of the whole twelve hours, and what has been fulfilled in the first half of it, must be heaped on to the second half, which has therefore to bear an additional load of error, equal in amount to its rightful share. Thus, if 100 miles of wind had been expected, and eighty miles really came, in the twelve hours, the error of the expectation would be one in four; but if forty miles of wind had come in the first six hours, the prediction would assign sixty miles to the next six hours, whereas the fact would show forty miles, making an error in the prediction of two in four, or double the original error of expectation for the whole twelve hours.

Section B.—Chemical Science

On the Utilisation of Sewage, with Special Reference to the Phosphate Process.—Mr. David Forbes, F.R.S. &c. The processes at present employed for the treatment of sewage were classed under two heads: the purely mechanical and the mechanico-chemical. The former, which at best only effected a mere filtration of the sewage, have everywhere failed to effect any such purification of the sewage water as was necessary in order that it might be allowed to flow directly into the streams. Sewage irrigation was included in the latter class, not because any direct chemical treatment was employed, but for the reason that, whilst the soil acted mechanically as a filter to separate the solids, the chemical action exerted by the vegetation decomposed and assimilated the organic matter, ammonia, and other available substances in the liquids also. The more purely chemical processes, such as the treatment by lime alone, or in combination with chloride of iron, alum, sulphate of alumina, and, lastly, the so-called A B C process, were next alluded to, but regarded as failures, since the evidence brought forward not only proved that the affluent water had not been sufficiently purified, but also that the sewage manure obtained was, by the admixture of the materials employed in these processes, rendered of so low an agricultural value as to preclude its employment elsewhere than in the immediate neighbourhood of the sewage works.

Admitting that sewage irrigation was to be recommended wherever the local circumstances were favourable to its employment, it was maintained that under many circumstances it was neither applicable nor advantageous, and that in these cases it was preferable to employ a chemical treatment, which had the advantage of not requiring any large outlay for pumping or distributing machinery, or the purchase of large areas of ground for sewage farms. For this purpose an entirely new process was recommended, called the phosphate process, based on the property which hydrated phosphates have of combining with organic matter, whilst the ammonia also can be precipitated in the condition of the double phosphate of ammonia and magnesia.

The process was shown experimentally with Liverpool sewage, and consisted merely of adding a solution of certain phosphates, chiefly of alumina, in sulphuric or hydrochloric acid to the sewage, and afterwards a little milk of lime barely sufficient to neutralise the acid and give a faint alkaline reaction to the sewage; even if tinctorial matters of great intensity (ink was added in the experiments) were present, the liquor became immediately discoloured, the supernatant liquor resting quite clear above a precipitate of the phosphates along with all the insoluble matter and a large portion of the soluble organic matter and ammonia originally contained in the sewage. The authors of this process, Messrs. A. Price and D. Forbes, although they did not pretend to have extracted the entire amount of the ammonia and other matter valuable for agriculture from the sewage, or to effect an absolute purification of the affluent water, believed that, as the water so purified was free from any nauseous taste, so that it could be drunk without repugnance, was devoid of smell, and did not putrefy or emit any disagreeable odour even when left standing in an open vessel during the whole of the preceding hot summer, that it had been sufficently purified by the phosphate process as to permit of its being directly run off into rivers without detriment to the fish in them or the health of the inhabitants on their banks.

A most particular feature of this process when compared with the other processes of precipitation was, that the substances employed in effecting this purification were not detrimental to the agricultural value of the precipitated manure, but, on the contrary, added so much to its value as to enable it to bear the cost of transport to distant parts of the country, and thus showed some hope that the value of the manure might be sufficient to pay for the expense of treating the sewage.

Section C.—Geology

Remarks on newer Tertiary Fossils in Sicily and Calabria. Mr. J. Gwyn Jeffreys, F.R.S. In the last deep-sea exploring expedition in H.M.S. *Porcupine*, in the Bay of Biscay, and along the Atlantic coasts of Spain and Portugal, Mr. Jeffreys procured at considerable depths, and especially from 994 fathoms, many species of mollusca in a living or recent state, some of which had previously been regarded as fossil only, and extinct, and all as belonging to the newer tertiaries of Sicily and Calabria; and he believed that a record of the fact might lead to the further discovery of the geological phenomena which had caused the fossilisation of such species in that limited area. Several of these species inhabit northern, and even Arctic, seas; such are *Terebratula cranium*, &c. Other species now found in a living or recent state, are *Terebratula spheroidea*, &c. One of the last species, in the last list or category (*Fiparesepla papillesa*) was also dredged by Mr. Jeffreys last autumn, at Dröbak, in Norway; and he was of opinion that our knowledge of the Arctic marine fauna was very imperfect. The newer Tertiary fossils of Sicily and Calabria had been to a great extent investigated by Dr. Philippi, formerly of Cassel, Professor Seguera of Messina, the Abbé Brognone of Palermo, and Dr. Tiberi of Resina near Naples; and their collections had been examined by Mr. Jeffreys. Two suggestions or questions were submitted by the author of the present paper. 1. Have not all the deep-sea species of European mollusca originated in the north, and spread southwards in consequence of the great Arctic current? 2. Inasmuch as the pliocene division of the Tertiary formation is now ascertained to contain scarcely any extinct species, and the future explorations may reduce the percentage of such species to *nil*, may not that artificial division hereafter merge in the quaternary formation, and the tertiaries be restricted to eocene, miocene, and pliocene?

The President and Sir Roderick Murchison spoke of the great importance of this communication, and the latter hoped Mr. Jeffreys did not share the opinion of his colleague Dr. Carpenter, that their discoveries tended to upset modern geology.

Professor Duncan confirmed Mr. Jeffreys' statement with respect to the specific identity of corals from deep-water with those of the South-Italian tertiaries.

The Rev. H. W. Crosskey also addressed the Section as to the glacial fossils of Scotland being quaternary and not tertiary.

Mr. Jeffreys, in reply, begged to assure Sir Roderick, as one of the parents of English geology, that he need not be under any apprehension for his offspring, so far as the deep-sea explorations were concerned.

Modern and Ancient Beaches of Portland.—Mr. W. Pengelly. The Chesil Bank having been described, the author stated that he had found amongst the flints of which it was chiefly composed, specimens of Budleigh Salterton pebbles, some containing the fossils occurring in these pebbles. Some granite pebbles were probably from the valley of the Teign. From these specimens it was concluded that the transportation along the coasts of South Devon and South-west Dorset is up Channel, that is, in the direction of the prevalent winds. The Raised Beach of Portland Bill consists of 7 feet of yellow clay, the same of pebbles, sand and shells from the Raised Beach, and 50 feet of rock resting at sea-level on a shingle beach. The shells are species now living on the shore. The beach was held to indicate an elevation of the coast of not less than 50 feet; and the pebbles showed that at the time of their deposition the direction of transportation was the same as now. Portland was then an island.

On the Occurrence of Seams of Hard Sandstone in Middle Drift of East Anglia.—Mr. J. E. Taylor. This sandstone was composed of 66 per cent. of siliceous sand, cemented by 33 per cent. of carbonate of lime. It occurs immediately below the upper or chalky boulder-clay. Formerly it was employed at Norwich in building, the Castle being built of it. Even in beds later than the boulder-clay, specimens of indurated sandstones had been found by the author, proving, as he believed, that the older rocks owed their compactness more to the cementing material than to heat or pressure.

On the Palæontological Aspects of the Middle Glacial Formation of the East of England, and on their bearing upon the Age of the Middle Sands of Lancashire.—Messrs. Searles V. Wood, and F. Harmer. The authors gave a list of 65 species of shells obtained from the middle glacial sand in the neighbourhood of Yarmouth, of which a large proportion, about 20 per cent., are not now known to be living, and one of them, *Erycinella ovalis*, a coralline crag form, being almost generically extinct; among which they had found five or six shells apparently new. The formation altogether presents a decidedly southern aspect, with strong affinities to the crag, only two species (with the exception of the new forms) being unknown to the crag beds. On the other hand, the middle sand of Lancashire, as at present described, did not appear to have yielded any shell not now to be found in the seas of the immediate neighbourhood. They pointed out that the mere fact of the middle glacial sands of the east of England, and the middle sands of Lancashire being both of them underlaid and overlaid by boulder-clay, was altogether inconclusive, and urged that all the evidence we had at present before us would relegate the latter to a much more recent position in the glacial sequence. They suggested tentatively that the Lancashire sands might possibly prove to belong to the Hessle series, as there seemed to be a close resemblance between the list of shells from Kelsey Hill, believed by them to belong to the Hessle series, and those from Blackpool.

In the discussion which followed, Sir Charles Lyell was understood to say that he was inclined to accept the conclusion of the authors.

Mr. Hughes did not think there were as yet sufficient data to correlate the drifts of the East and West of England. But little was known of the relations of the drifts of the West to one another. We must first trace the included fragments to their origin in the mountains, and not form any theory to account for the origin and succession of the wide-spread drift of the lower country, which will not also account for the phenomena observed in the hills. As the result of his own observations, he described three drifts occurring in the district north of Liverpool, which might be roughly distinguished from one another.

1. A stiff blue clay with included fragments of the rocks of the immediate neighbourhood. This drift occurs at the highest levels up to 2,100 feet. The fragments are striated when the rock is of such a character as to preserve the scratches.

2. The ordinary stony clay drift which occurs along the valleys and runs up the hill-sides to about 1,800 feet. In the *Fell-top drift*, No. 1, the matrix is a very uniform stiff lead-coloured clay, no matter what it rests on; while the included fragments may have come from rocks about the same level and close at hand. In the *Hill-side drift*, No. 2, the matrix varies more according to the rock on which it rests; while the included fragments, which are more numerous than in the *Fell-top drift*, No. 1, are derived from higher up in the same drainage area; and, where different kinds of rock occur on opposite sides of the valleys, the drift on either side is chiefly derived from the rocks on the same side, as if it were the lateral moraine of a glacier coming down the valley.

3. In the lower valleys false-bedded sands and gravel, such as might be produced by the action of the sea at the end of the receding glaciers, overlap the clay drift No. 2, and are almost continuous with the great mass of gravel drift which is so largely developed on the lower ground of North Lancashire. But while these divisions are tolerably clear in a large way, in detail it is difficult to draw a line between them; and when we try to group all the drifts in and around the mountain districts of Wales and the North of England under one of these heads, or to fit all the observed phenomena into any scheme of regular increase and decrease of cold—any uniform submergence or elevation, we find many exceptions and complications. The false-bedded sands and gravels usually occur along the larger valleys at low levels, but sometimes we find similar sand gravel dovetailing into the boulder clay at various heights; in another place occurring along terraces 1,000 feet above sea-level. In the absence of organic remains, we cannot yet say which of these should be referred to marine action and which to fresh-water streams and ponds in and near the melting ice. Flints commonly occur in the lower gravels, but once a large unworn flint, about eight inches in diameter, was found in the re-sorted surface of the highest drift at about 1,900 feet above the sea. In the case of the Shap granite boulders, to the mode of distribution of which Professor Harkness has devoted much attention, the difficulties are more obvious, as the rock is so marked. Boulders cannot be formed except when the rock from which they are derived is above the ice and water. This limits the submergence and depth of the ice as to the maximum. But according to the view that the boulders were transported over Stainmore into East Yorkshire on floating ice, the south end of the Pennine range must have been submerged. This limits the submergence as to minimum. What was the line of transport of the Shap boulders *before* the submergence of Stainmore? Again, in the drifts on the top of Stainmore, we find not only boulders of Shap granite, but also fragments of the Permian brecciated conglomerate which can have come only from the bottom of the valley more than 1,000 feet below. Can we believe that these have been lifted by shore ice from time to time throughout that long submergence, or have we evidence of older drifts of very different origin being washed, sifted, and sorted by the encroaching sea. Again we find, even on the north side of steep mountain ranges, where we should have expected the glaciers to have lingered longest and to have ploughed out the old drift, that even boulder-clay has travelled up the hill from the lower ground, and must therefore be referred to a period when its transport was irrespective of the present valleys and mountain slopes.

In fact, there is much evidence to show that the land ice has often ploughed across and transported marine deposits, and the sea has often washed and re-sorted the *debris* brought down by land ice, and thus the drift has been used up over and over again. That might be the reason why we so often find fragmentary and rolled shells associated with perfect though delicate shells, which seem to be of the age of the deposit in which they are found. He quite agreed with Mr. Searles Wood as to the derivative character of some of the shells in the gravel drift recently described by Mr. Jamieson.

But even frequent oscillations of level would not alone be sufficient to account for the manner of occurrence of the marine drifts, especially when the palæontological evidence is considered. The agencies which produce the warm Western Ocean currents must have been in operation throughout the long period under notice, but the circumstances which determined the direction of those currents must have varied with the changes of level. He asked what would be the effect upon the Gulf Stream of a submergence or an elevation of a large part of the bed of the Atlantic to the amount of 2,000 feet or more? The shells of Moel Tryfaen, though of less Arctic character, might well be referred to the period of greatest general cold, provided the form of the sea-bed and distribution of land turned a warm ocean current on that part of the western coast.

He would, therefore, urge the expediency of adopting the method always taught by Professor Sedgwick to his pupils—first, to establish clearly the relation of the beds in each separate area, and to avoid obscuring an already complicated question by adopting prematurely in the West the local nomenclature of the East of England.

On Certain Glacial Phenomena in the Central District of England.—Rev. H. W. Crosskey. The author had determined the existence of glacial striæ in the central plateaux of England, and covering these markings on true boulder-clay, physically corresponding to the older "till" of Scotland. The clay with granite boulders in the midland counties was of marine origin. A succession of drift beds was established from an actual section showing a boulder-clay resting on the Bunter sandstone; second, sands and gravels with false bedding; third, a clay with pebbles; fourth, a bed of sand mixed with clay.

On some Thermal Springs in the Fens of Cambridgeshire.—Mr. F. W. Harmer. In several farm-yard wells near Chatteris, of the depth of about ten feet, the author had found water of the temperature of $74\frac{1}{2}°$ Fahr. on the 14th March, the air being but 37°; and in June of $79\frac{3}{4}°$, the air being then 70°. An analysis of the water by Mr. F. Sutton showed that the heat was not due to chemical causes. The fens being below the sea-level, and therefore permanently saturated with water at the depth of ten feet, and the phenomenon described being apparently continuous over an area of ten miles, and no doubt further, the cause producing the heat would not be an insignificant one. Mr. Judd, of the Ordnance Survey, affirms that the secondary rocks of this neighbourhood are extensively faulted, and may thus afford a communication with the central heat of the earth.

On the Matrix of the Gold in the Scottish Gold Fields.—Dr. Bryce. The author had found gold in the fragments of granite, and tracing it to the native rock, he obtained the crushing of a sufficient portion to prove that it was distinctly, though not remuneratively, auriferous.

Some Remarks on the Denudation of the Oolites of Bath.—Mr. W. S. Mitchell. The author held that there was no proof establishing the continuity of the Oolites. He thought they were accumulated locally, and, as it were, in patches, with currents sweeping in between. The sedimentary matter which followed filled in the spaces forming the Bradford clay. Denudation subsequently came into play, and the ready-yielding clay formed the valley.

On an Antholite discovered by Mr. C. P. Peach.—Mr. W. Carruthers. Various estimates of the position and of the structure of this fossil had been formed, but the specimens found by Mr. Peach established that the bud sprang out of the axil of a bract, and consisted of several scales supporting three or four flowers, having fruits which had been described as species of *Cardiocarpum.*

On the Sporangia of Ferns from the Coal Measures.—Mr. W. Carruthers. These organs were found in what were called coal-balls, from the beds of coal at Bradford and Halifax. They exhibited the structure, form, and attachment of the sporangia of some recent hymenophyllaceous genera.

Section D.—Biology

Department of Zoology and Botany

On the Larval State of Molgula, with description of several new species of simple Ascidians.—Mr. A. Hancock. The author described the tadpole larvæ of *Molgula complanata*, and referred to the Amœba-like form of larva described by Lacaze Duthiers, as found in *M. tubulosa*; but it may be doubted whether this species is a *Molgula*, and reasons for believing it to belong to a new genus *Eugyra* were given. Many new species were described belonging to the genera *Ascidia* (11), *Carella* gen. nov. (2), *Ciona* (1), *Molgula* (3), *Eugyra* (1). Many of these species were sent to Mr. Hancock by Dr. Bowerbank, Rev. A. Norman, and Mr. A. G. More.

On the Structure of the Shell in the Pearly Nautilus.—Mr. H. Woodward. After referring to the great interest attaching to the *Nautilidæ* on account of their vast geological and geographical range, the author proceeded to describe the structure of the shell with its septa and siphuncle, the latter structure being only found in the Cephalopoda and nearly confined to the Tetrabranchiate division of the class. The camerated structure, however, is found both among the Bivalves and Gasteropoda, and the author suggested that if any incipient character could be found leading up as it were to the siphuncle, we might fairly infer that that structure was only a more highly-differentiated form of shell-growth. Such incipient structure occurs in the *Ostreadæ* and *Spondylus*, in which the shell-muscle dips down from layer to layer, offering a rough similarity to the siphuncle in *Aturia* and some other *Nautili.* Mr. Woodward described the structure of the shell, and showed by actual dissection that no vascular system exists between the shell and the animal by means of the siphuncle. The siphuncle proves only to be a pearly tube, within which is another composed of an extension of the periostracum, and quite destitute of vascular or cellular structure. Shell structure proves, when once formed, to be dead matter, destitute of change, and can only be repaired when in contact with the mantle of the shell.

On a New Species of Coral.—Mr. W. S. Kent. In 1869, when examining the collection of Madrepores in the Paris Museum, the author found a worn specimen having a close general resemblance to *Alveopora fenestrata*, Dana. A superficial examination at once showed it to be quite distinct from that species, and the presence of numerous irregularly-disposed but perfect and well-developed tabulæ demonstrated its close relationship to the ancient genus *Favosites*; the cretaceous genus *Koninckia* forming the immediate connecting link. To this remarkable form he gave the name *Favositipora deshayesana.* Diagrams were exhibited showing the structure of this new coral, as also photographs of the original specimen.

The Secretary read a letter from Dr. J. E. Gray, of the British Museum, in which he described a new genus, *Callisphæra*, for the beautiful sponge from Setubal, described by Mr. Kent as *Pheronema grayi*, and which he believed differed in several particulars from the genus *Pheronema.* He also provisionally referred the *Holtenia pourtalesii* of Schmidt to the genus *Vazella*, and the same author's *Tetilla polyura* to the genus *Polyurella.*

Mr. J. Gwyn Jeffreys, who had just returned from the south of Europe, after having accomplished his part of this year's deep-sea exploring expedition in H.M.S. *Porcupine*, stated that in this cruise he had dredged across the Bay of Biscay, and along the coasts of Spain and Portugal to Gibraltar. The weather had not been favourable; but the depth reached was 1,095 fathoms. A large collection of Mollusca, Echinoderms, Corals, Sponges, and Hydrozoa, had been made. Half-a-dozen specimens of a beautiful new *Pentacrinus* (*P. wyville-thomsoni*) had been taken in 795 fathoms depth, between Vigo and Lisbon. Both Northern and Mediterranean species of shells were met with.

On the Vegetable Products of Central Africa.—Colonel Grant.

On the Parasitic Habits of Pyrola rotundifolia.—Mr. Gibson. In the discussion which followed this paper, the opinion was expressed that the parasitism of this species was not yet proved.

On the Desert Flora of North America.—Dr. C. Parry.

Note on Ribes spicatum.—Professor Lawson.

Mr. E. Birchall exhibited a beautiful collection of Hybrid Sphingidæ and other Lepidoptera.

Department of Ethnology and Anthropology.

Account of the Exploration of the Victoria Cave, Settle, Yorkshire.—Mr. Boyd Dawkins.

Account of certain remarkable Earth Works at Wainfleet, Lincolnshire.—Rev. C. Sewell.

On Ancient Sculptures and Works of Art from Irish Cairns.—Dr. Conwell. The author gave a brief account of his discoveries and researches during the summer of 1865, among the ruined remains of thirty-one cairns, extending along "the Loughcrew Hills," in the county of Meath, about two miles south-east of the town of Oldcastle. So far had these ancient remains escaped all public attention previously, that it was only during his investigation of them that an officer was sent from the Ordnance Department to insert them upon a map, which is now zincographed and sold by the Ordnance publishers. Revered and sacred in former ages as must have been these resting-places of departed splendour, standing out conspicuously on the peaks of a range of hills rising to the height of nearly a thousand feet above the level of the sea, in the proverbially flat country of Meath, it is very remarkable that the site has not yet been identified with any description, reference, or allusion, in the ancient annals of the country. The unroofed chambers, and the fragments of broken urns, afforded practical evidence that these ancient tombs had been plundered at some previous period, and this fact gives additional interest to the miscellaneous collection of articles of stone, bronze, iron, amber, glass, bone, &c., found in them.

In the remains of fourteen of the cairns, the large upright stones, to the number of 115, which formed the interior chambers of the cairns, were found inscribed with devices, almost entirely novel, sometimes *punched*, and at other times *engraved*, the diversity of which may be inferred from the fact, that out of all this number there are not two alike. No key has yet been found for reading or interpreting these devices. A series of drawings were exhibited of the symbols on thirty-one stones from *a single cairn* at Loughcrew, being exactly the number of inscribed stones in the two well-known cairns of Dowth and New Grange, taken together in the same county.

The following are some of the more remarkable objects of art which were found:—Several small stone balls of various colours, one of syenite beautifully polished, and nearly three inches in diameter, and another somewhat larger, of brownish red hæmatite or iron-stone; an oval object of jet-like appearance and polished; two pendants and a bead, all of different colours, evidently portions of a necklace of stone; a ring; probably part of an ear-ring, made of jade, and nearly worn across in one place; one polished flint nodule; one leaf-shaped flint arrow-head.

Of bronze, were found several small open rings, and a very perfect bronze pin, with ornamented head.

Of iron, as might be expected, in a much corroded state, some fragments of uncertain use, two small iron rings, a piece of iron resembling one leg of a compass; another an iron chisel or punch.

Of amber, some small beads of different shapes and sizes.

Of plain glass, some small beads differing in shape and colour; and one "*double bead*," imperforated, and of a soft green hue, and some glass fragments.

Of bone, were obtained two bone beads, some bone pins—one with ornamented shaft and a metallic rivet, apparently for attaching a bead; a collection of the remains of nearly 5,000 worked bone flakes of various sizes and patterns, in a few instances preserving their original polish, as if quite modern; and upwards of 100 of these ornamented in a very high order of art, with various circles, curves, ornamental puncturings, &c., of which no description could give an adequate idea. On one was found the representation of an antlered stag: but what may have been the uses to which these bone flakes were applied, Dr. Conwell expressed himself unable to come to any conclusion. As the accounts are very meagre of any articles of "historical value," having ever been extracted from cairns, the collection now brought under notice is probably the most curious and remarkable which has ever been found joined together under similar circumstances.

On some forms of Ancient Interment in County Antrim.—Dr. Sinclair Holden.

On a Discovery of Platycnemic Men in Denbighshire.—Mr. Boyd Dawkins and Prof. Busk. Mr. Dawkins explained that the remains to which he referred were found at a place in Denbighshire which rejoiced in the name of Perthi Chwaren. They were in a cave in the mountain limestone, and the explorers found from twenty to twenty-five human skeletons, and a large quantity of the remains of animals. The skeletons were interred differently from those of modern times, in that they were lying in confused heaps, which clearly showed that the people had been buried in a sitting posture. The cave (he said) was inadequate to contain such a large number of human corpses at one time, so it followed that it was used at different times, probably as a family mausoleum. There were also found bear's bones, fragments of mussel shell, a specimen of cockle, and a tusk, one of the largest he (Mr. Dawkins) had ever seen. There was likewise discovered some pottery, fragments of coal, and a splinter of iron which was not oxydised. The only evidence as to the antiquity of the cave was a fragment of flint. Flint was used by the Romans and Egyptians, and the discovery pointed to the fact that at one time flint was the only material in use, but it did not show that this deposit was of the date when no metals were known. Mr. Dawkins thought all the evidence went to show that the cave was of the Neolithic age.—Prof. Busk next gave his conclusions with regard to the skulls which were found in the cave. He said that the people whose remains had been discovered were of low stature, the skeletons being only from 5 feet to 5 feet 6 inches in height.

On Carved Stones recently Discovered at Nithsdale, Scotland.—Dr. Grierson.

On a Quartz Implement from St. George's Sound. — Mr. H. Woodward. This crystal of quartz (having its terminal planes preserved at both ends) was found by his colleague, Mr. Davis, among a number of other minerals in the British Museum, forming part of the Old Sloane Collection. On examination it was found to have written on it in ink, "St. George's Sound, N.W. coast of America, Captain Cook." The crystal had been used as a flint implement, one end being sharp and the other notched. It had an attachment for a wooden handle, which would admirably fit it for picking holes in the ice, through which the Esquimaux might fish.

On a Flint-flake Core from the River-gravel of the Irwell, Salford, Manchester.—Mr. John Plant. The upper valley of the Irwell, the author said, was overspread with silt and sandy layers. Terraces of above 200 feet in elevation were very distinct in places. The river now flowed over the beds of New Red sandstone, having contracted its bed for at least a mile to about sixty yards. The upper terrace was composed of sand and gravel of older age than the silts which fringe the banks. The pebbles of gravel were mainly derived from the pebble beds and eroded silt, others were flattened pebbles from the coal measures. Throughout these pebbles there were no flints, but bits of chert only from mountain limestone. The weapons of Lancashire were neolithic in character, so that the occurrence of a flint-flake was remarkable from its site in the barren desert of gravel and sand in the Irwell. Mr. Plant thought the specimen he exhibited belonged to the time when the east of England was in the occupation of the early palæolithic people of Europe.

Remarks on Stone Implements from Western Africa.—Sir John Lubbock, Bart.

Section E.—Geography

The Lines for Ship Canals across the Isthmus of Panama.—Gen. W. Heine. The author said that in his various explorations, extending over twenty years, he had often found the reports made by other explorers differed from existing facts. In cases where elevations of only 150 feet had been reported, 900 feet were found to exist; and few explorers seemed to have taken into consideration the geological formation, which often consisted entirely of porphyry and basalt, and was almost as hard as cast steel. After giving an account of the line proposed across the Isthmus of Tehuantepec, Honduras, Nicaragua, and nine lines proposed across the Isthmus of Darien, he came to the conclusion that only two lines were of a kind to deserve consideration, because the expense of constructing and working the canal would not be out of proportion to the benefit derived from it.

These two lines are—1. From Aspinwall along the line of the railway to Panama, with an extreme elevation of 269 feet, a length of thirty-five miles, through rocks of porphyry and basalt, and with but middling ports of entry. 2. From the Gulf of Darien through the rivers Atrato, Casarica, Paya, and Tingra, to the Gulf of San Miguel, with an extreme elevation of 186 feet, length 52 miles, through a soil composed of alluvial deposit with some thin ranges of greyish sandstone or schist, and with very good ports of entry.

The survey of the first line was very perfect, that of the second line less so, and a more exact level was desirable. Of the nineteen expeditions undertaken, twelve were of American origin, four were undertaken by Frenchmen, one by a native Columbian, and only two by Englishmen. Considering the vast interests England had at stake in shortening a marine passage to Australia, the west coast of America, the islands of the Pacific Ocean, including Japan, he was astonished at the lack of energy, especially as the very moderate expense of 1,000*l.* to 1,500*l.* would suffice for all necessary exploring purposes.

Section G.—Mechanical Science

Hydraulic Bucketting Engine for Graving Docks and Sewerage.—Mr. Percy Westmacott, C.E. This was a short paper describing the mechanical appliances devised by Mr. Westmacott, at the suggestion of Mr. George Fosbery Lyster, dock engineer, for the purpose of emptying the Herculaneum Graving Docks, Liverpool. They were devised with the view of working in conjunction with the system that the dock engineer had resolved to adopt for working the gates, capstans, &c., and thus save the erection of another steam engine and plant for the special purpose of emptying the docks. The system adopted was the hydraulic system. The essential feature of Mr. Westmacott's engine is a scoop-shaped bucket, constructed of wrought iron, and capable of lifting 14½ tons of water. It is undesirable to give the constructive details of the engine without the drawings by which the paper was illustrated; but the following facts may be mentioned:—The minimum lift at the high level discharge is 7 feet, and the maximum 23 feet, and about 5 feet more stroke is required for tipping up the bucket. About 3 feet per second is the usual average speed of the bucket in plunging or lifting. The filling is effected in 5 seconds, but the emptying occupies from about 12 to 15 seconds, owing to a contraction that had to be made at the front end of the bucket to suit the existing masonry in the well. This latter operation, with a free mouth to the bucket, should not require any more time than the filling, if even so much. The coefficient of effect obtained by this engine is as follows:—At 7 feet (minimum) lift, ·4; at 23 feet (minimum) lift, ·6—average, ·54; which will be found to compare not unfavourably with other appliances under the same conditions of working; but the loss occasioned by the choking of passages and gagging of valves or paddles is altogether avoided by this system, which, for this reason, is peculiarly well adapted for sewerage purposes.

The President, Mr. R. B. Grantham, C.E., and Mr. Oldham expressed approval of the engine. Mr. J. F. Bramwell, C.E., F.R.S., said that for short lifts the engine would be economical, but for very high lifts he thought it would be inapplicable. In reply Mr. Westmacott said there would be no difficulty in making a 40-feet lift, while beyond that height there might be two or three buckets each above the other.

On Appliances for the Production of Heavy Forgings.—Lieut.-Colonel Clay. In this short paper the author mentioned, under the following three heads, the improvements recently introduced into the manufacture of large forgings, as illustrated by his experience at the Birkenhead Forge:—1. Improved heating by the use of Siemens's regenerative gas furnaces. 2. Facilities for handling and removing large masses of wrought iron from the furnace to the hammer, and for moving them when under the hammer. 3. Improved hammers, with a clear unfettered fall, and with such width of standards as to give the workmen all the comfort and convenience possible in executing the necessary operations of shaping, forging, and cutting the material to the required form.

On Hammering and Stone-Dressing Machinery.—Dr. J. H. Lloyd. The author claimed to have devised machinery which was particularly applicable for cutting, sawing, chiselling, drilling, and dressing stone and other substances, for forging and hammering metals, and for working the tools in general by motive power, so as to supersede hand-labour. The invention has not yet been applied; indeed, the improved machinery as yet only exists in the state of a model. The paper was illustrated by numerous drawings.

REPORTS OF COMMITTEES

REPORT OF THE RAINFALL COMMITTEE

This report was read by Mr. G. J. Symons, the secretary of the committee. It commenced by referring to the steps taken last year to secure uniformity in the registration of rain by the observers throughout the country, and to the acceptance by the General Committee of the recommendation of the Rainfall Committee that additional observers should be obtained in parts of the country where at present such observers are far from one another. Dartmoor was last year quoted as an illustration; thither after last meeting Mr. Symons proceeded, and the result is that the number of stations in that district has been doubled. There are, however, still two parts of the moor where no one lives, and no one has yet been found willing to superintend a gauge. Reference is next made to other steps taken by the committee to secure returns from various other districts, and to the success of these efforts. The committee close this portion of their report by pointing out that to keep up an amateur staff adequate to the requirements of the subject, say from 1,500 to 2,000 observers, it is indispensable that a number of new ones be enlisted each year to supply vacancies caused by deaths and removals, and they therefore intimate their desire to receive through their secretary (Mr. G. J. Symons, 62, Camden Square, London) offers of assistance from parties willing to provide themselves with the inexpensive and simple gauge now generally in use. The report then proceeded to mention that the secretary has during the past year visited and examined the gauges in use at upwards of one hundred stations. By this personal intercourse greatly improved accuracy and uniformity of procedure is secured. The committee regret that through want of funds they have been unable to make any progress with the collection of old returns during the past year. The report then proceeds to describe certain experiments carried out at Calne, in Wiltshire, by Colonel Ward, with a view to determining the difference in the amount of rain collected at various heights above the ground, not so much with a view to determining the cause of this variation as its amount, and therefrom the possibility or otherwise of reducing observations made with gauges at different heights above the ground to what they would have been at some uniform datum. This portion of the report commences by a brief notice of the experiments made by Prof. Phillips at York in the years 1832-35, then pass on to illustrate the necessity for the determination of these corrections; thence to a description of the instruments employed, and their position; and then follow a heavy batch of tables of the calculations and the results which it is impossible to abbreviate. Part of the conclusions were exhibited in the form of diagrams representing the total rainfall on the surface of the ground, and its decrease at various altitudes above it, one diagram giving the mean annual decrease, and a series of twelve others the monthly curves; from these it was perfectly obvious that the difference between a gauge on the ground and one 20ft. high is in winter nearly three times as great as in summer, and hence it becomes evident that the mean annual correction is applicable to the total fall in one or more years only, and not to individual months, for each of which separate corrections are given. The report then proceeds to consider the most suitable height for the orifice of gauges to be above ground, and gives various reasons *pro* and *con*, finally concluding that 1ft., as hitherto adopted, be still recommended. The report next refers to the tables in an appendix giving the monthly fall of rain at about 300 stations during the years 1868-69, and to various calculations in different states of progress. The report concludes by pointing out the great work being done by the voluntary and entirely gratuitous services of nearly 2,000 observers, and suggests that it would be alike graceful and an economical act on the part of the Government were they to offer to relieve the observers from the cost of reducing and publishing the observations which are now by their accuracy and completeness accepted as a type by foreign countries and our own colonies, and which are found yearly more and more useful in relation to our manufacturing and commercial interests. The committee conclude with the following words:—"A few hundreds annually would probably suffice to hold together a body of practised observers which has no equal in the world, and which once broken up, could not be replaced, since, irrespective of the difficulty of training the new observers, the continuity of the observations would be destroyed."

SCIENTIFIC SERIALS

THE *Geological Magazine* for September (No. 75) opens with an important article by Mr. E. Ray Lankester, describing a new species of *Cephalaspis* (*C. dawsoni*) from the Devonian sandstones of Gaspé, in Canada. This fish is figured, as also a spim, *Machairacanthus sulcatus*, which was found associated with it. Mr. Lankester also describes the characters of *Scaphaspis knerii.*—Mr. Davidson continues his descriptions of Italian tertiary Brachiopoda, which he illustrates with two fine plates containing a great number of figures.—Mr. Alfred Marston contributes a paper on the transition beds between the Silurian and Devonian rocks; and Mr. Lankester describes and figures a supposed new species of *Terebratula* (*T. rex*), obtained from East Anglian drifts, but probably derived from beds of Portlandian age. The remaining articles in the number are a catalogue of mammalian fossils which have been discovered in Ireland, by Mr. R. H. Scott, and a reply by Archdeacon Pratt to some remarks by M. Delaunay on Mr. Hopkins's method of determining the thickness of the earth's crust.

THE *Journal of Botany* for October commences with some Observations on Willows, by the Rev. J. E. Leefe. Dr. Hance contributes some carpological notes on Chinese plants; and Mr. A. W. Bennett his paper on the relative period of maturity of the male and female organs in hermaphrodite plants, read at the Liverpool meeting of the British Association, of which an abstract has already appeared in our columns. Dr. Ferdinand von Müller has a note on some interesting plants gathered near Lake Barlee during Mr. Forrest's recent expedition; and among the borrowed abstracts is one of Mr. Bailey's useful paper on the natural ropes used for packing cotton bales in the Brazils, read before the Literary and Philosophical Society of Manchester.

THE two longest articles in the *American Naturalist* for September are a reprint of Mr. Darwin's memoir on the movements and habits of Climbing Plants, and a highly favourable review of Wallace's "Contributions to the Theory of Natural Selection." Prof. Cope contributes an article on the Fauna of the Southern Alleghanies, and Dr. C. C. Abbott one on Mud-living Fishes. One of the most interesting papers in the number is a very short one by Dr. William Stimpson on the Deep-water Fauna of Lake Michigan, containing a short account of a series of dredging operations which has been undertaken in this lake during the present year by the Chicago Academy of Sciences. At a distance of eighteen miles from Chicago, where the depth was fourteen fathoms, the sandy or gravelly bottom was found to be nearly barren of life. Between the distances of twelve and twenty-two miles from off Racine, the average depth was forty-five fathoms, and the bottom generally a reddish or brownish sandy mud. This bottom was found to be rather densely inhabited; the captures including a *Mysis* allied to Arctic forms, which led the author to refer its original entry into the lake to the cold period of the quaternary epoch, two species of *Gammarus*, a small white *Planaria*, and a new species of *Pisidium*. The investigation of the materials obtained by the dredging parties of the Academy is now in progress, and the results will be published in full with illustrations at an early period.

SOCIETIES AND ACADEMIES

BRISTOL

The Observing Astronomical Society.—Report of Observations made during the period from Aug. 7 to Sept. 6, 1870, inclusive.—*Solar Phenomena*:—Mr. T. G. E. Elger, of Bedford, writes: "Observers of solar phenomena have seldom an opportunity of witnessing such a fine outbreak of spots as that which took place during the last fortnight of August. After the disappearance of the large group observed in the S. hemisphere (about July 31), a comparative lull in solar activity ensued, lasting thirteen days; the spots which appeared during this interval presented no remarkable features, and were mostly confined to the S. hemisphere. On the 17th, in the N. hemisphere, a large scattered group was observed, which a few days before had consisted of a congeries of minute specks; on the 18th it was 2′ 55″ in diameter, and was followed by another group, 2′ 26″ in length; both these groups diminished very rapidly after the 19th. On the 20th the two largest groups on the disc were nearly central; one of them 36″, the other 54″, in diameter. Cloudy days intervened between the 21st and 24th. On the latter date the first indications of the approaching outburst were remarked. At $4^h\ 30^m$ there were three immense groups in the N. hemisphere, extending from the centre of the disc to the E. limb; the preceding group, which was made up of very light and ill-defined penumbræ, enclosing upwards of sixty separate black spots, measured 3′ 10″ × 1′ 49″. The second group was 1′ 20″ in length, the third was too near the line to be satisfactorily measured. From the 26th to the end of the month the north maculose zone was completely crowded with groups and isolated spots, while the corresponding S. zone contained only punctures and small clusters. The following are the lengths of the three largest groups observed on the 29th: 3′ 6″, 2′ 26″, and 1′ 57″. The spotted zone could be seen with the naked eye, protected by an ordinary dark glass at noon on the 28th; it had the appearance of a dusky belt parallel to the sun's equator. Fresh groups observed in the sun's N. hemisphere during August = 11; ditto observed in the sun's S. hempishere = 15. Maximum number of groups on disc = 13 (Aug. 29, $21^h\ 18^m$); minimum number = 4 (Aug. 20, $4^h\ 15^m$).—Mr. William F. Denning, of Bristol, observed the sun with his 3in. refractor, on Aug. 28, and reports that on this date four large groups of spots were visible in the northern hemisphere. In the N.E. quadrant two large groups were perceptible lying just above the equator. In the N.W. quadrant an irregular scattered group was seen near the limb, and another group near the centre of the disc was very conspicuous. The S.E. quadrant contained three small groups, while the S.W. quadrant was entirely free from visible spots.

Aurora Borealis.—Mr. H. Michell Whitley writes that on August 20 he observed a brilliant aurora. From $11^h\ 30^m$ to 12^h it was very well defined. Straight beams of light shot up from the N. horizon to an altitude of about 35°. "These streamers faded and reappeared in other places." Mr. Henry Ormesher, of Manchester, also witnessed this phenomenon. He says, "I first observed it at $11^h\ 40^m$, but from its appearance it must have been visible for some time previous. I determined the extent of its base to be as far as W. by N. to N.E. by N. From between these points streamers shot forth in rapid succession, to a very considerable altitude, a great many of them reaching to the zenith of my place of observation. Some of these streamers were very brilliant, particularly one which at $11^h\ 50^m$ shot forth from a point just beneath the Pointers in a direction towards the polar star, and onwards to the zenith. I should think this stream of light to have been of about five minutes' duration, during which time its colour changed from a dark straw to a yellowish tinge. At $12^h\ 10^m$ there was quite an auroral arch, whose centre was towards the magnetic pole, and extending from the before-mentioned points to an altitude of at least 40°. The brightness of this arch increased until about $12^h\ 14^m$, when it was exceedingly brilliant. During the whole of the time the sky was very clear, with the exception of a reddish glow, of which the aurora was the cause.

Meteors.—Very few meteors appear to have been observed on about August 10. Mr. Edmund Heison saw nine on the 10th, three on the 11th, and two on the 12th. The Rev. S. J. Johnson watched the sky from $10^h\ 45^m$ to $11^h\ 46^m$ on the same date, and only detected one. Mr. H. Michell Whitley, of Penarth, witnessed the appearance of two meteors on the evening of August 29. The first was visible at $10^h\ 25^m$, and was accompanied with a faint train. It passed downwards below Corona Borealis. The second was seen at $10^h\ 30^m$ to the W. of Aquarius. Both were equal to a first magnitude star. On the 30th the Rev. S. J. Johnson observed the train of a very brilliant meteor. From the appearance of this train it was evident that the meteor must have become visible a degree or so to the W. of *k* Draconis and have ended a degree or two to the E. of *a* Draconis.

NEW ZEALAND

Wellington Philosophical Society, July 10.—The value of the New Zealand Flax was fully discussed, and Dr. Hector exhibited the operation of the machine he is employing in testing the strength of the various fibres for the Commissioners who have been appointed to investigate the subject. The result, as far as yet obtained, tends to prove, that while the flax of the *Phormium tenax* dressed in the native manner greatly exceeds in strength either Russian hemp or Manilla; yet, when dressed by the machines in ordinary use, it is much inferior. The few samples of the fibre prepared by retting or carefully applied chemical processes, however, gave much better results.

July 17.—Mr. T. H. Potts described an egg of the Great Auk which is in his possession.

New Zealand Institute, July 23.—Anniversary meeting, his Excellency Sir G. F. Bowen, G.C.M.G., in the chair. The president, in adverting to the transactions of the Institute and affiliated societies during the past year, drew attention to the number of contributors on a great variety of subjects to the last issued volume, as proving that a large amount of intellectual activity and practical zeal exist among the associates, although debarred by the geographical circumstances of the colony from achieving frequent meetings. The address was chiefly directed to the necessity for practical scientific instruction; and he stated that the Board of the Institute, having been applied to, the Government had recommended that a course of lectures shall be established in connection with the Museum and Laboratory, on natural history, geology, chemistry, and mineralogy. In proposing the thanks of the meeting to the president, the Hon. Mr. Fox, Premier, stated that the scheme which his Excellency had propounded would be favourably entertained by the Government, who were very desirous of assisting the diffusion of sound scientific instruction, as it was an essential step towards developing the resources of the Colony.

BOOKS RECEIVED

ENGLISH.—The National History of Commerce: John Yeats (Cassells and Co.).—The Tr[illegible]e: a Method of Harmony and Modulation: G. Green (Novello).—The Forces of the Universe, part I.: G. Ferwick (Longmans).—The Adventures of a Young Naturalist: L. Biart (S. Low, Son, and Co.).

FOREIGN.—(Through Williams and Norgate)—Abhandlungen der mathematisch-physikalischen Classe der k. bayerischen Akademie der Wissenschaften 10er Band.—Flora der preussischen Rheinlände: Dr. Wirtzen.—Lehrbuch der Ingenieur- und Maschinen-Mechanic: Dr Weisbach.—Das Naphthalin und seine Derivate: M. Ballo.—Anleitung zur Ausmittelung der Gifte: Dr. R. Otto.—Leopold von Buch's gesammelte Schriften 2er Band.

CONTENTS

THURSDAY, OCTOBER 27, 1870

THE REPRESENTATION OF SCIENCE AT THE SCHOOL BOARD

AN impression seems to prevail that only those persons should be placed on the Metropolitan School Board who are already acquainted with the details of education. Against this principle we protest. We hope the new schools will be great improvements upon those which already exist. When we are told by the Bishop of Manchester that a third of these schools only are efficient, that a third are inefficient, and that a third are wholly useless, if not pernicious, it is high time that the whole system should be looked into by those who will come fresh to the inquiry, unencumbered with the ideas that have led to such disastrous results. We think, then, the public should look to that instructed body of men who are known as cultivators of science to represent them on the London School Board. Already we are glad to see signs that the class of persons we have named have found favour in the eyes of London electors. The selection of Professor Huxley and Dr. Elizabeth Garrett, as candidates for Marylebone, is highly creditable to that district of the metropolis; but their hands must be upheld by a very much larger number of candidates, if common sense and intelligence are to prevail at the councils of the School Board.

The points to which we think the earliest attention of the School Board ought to be directed, and in which men of science are likely to give the greatest assistance, are the following:—

1. *The sanitary condition of the schools.*—It is well known that in many cases our schools are foci of contagion, and the means of spreading contagious diseases. Children are frequently sent to school, not only from families where contagious diseases are present, but actually with disease upon them. No trouble should be spared to prevent this, and if necessary a short clause should be added by Act of Parliament to the Education Act, in order to punish those who in this way are the means of spreading around the destructive diseases. Nor is this all that is required. The school-rooms should be well ventilated, clean, and not overcrowded. Every Government school should be placed under the superintendence of the medical officer of health of the district in which it is placed, and he should report periodically to the School Board on the state of the school and on any departure from sanitary rules. Cleanliness should especially be encouraged and insisted on amongst the children attending the school, and if no means exist at home, baths and lavatories should be provided at the schools.

2. *The times of study.*—It is a fact well known to the physiologist, that the attention of the human mind can only be given with success to a particular subject for a limited time. The younger the brain is, the less the time during which knowledge can be taken in or retained. In opposition to these obvious facts, children are kept at their studies or in school for much longer periods than they can successfully learn. The consequence is that they remain in the close school-room whilst they ought to have been in the yard at play. This system is doubly wasteful, for both health and learning are sacrificed. The whole system of hours of study, and of play or of work, requires to be revised in our primary schools. The importance of play-grounds in the open air can hardly be overrated. It is only the practical physiologist who can appreciate the real value of muscular exercise, and the influence of fresh air from time to time during the day, to enable children to pursue their studies with success.

3. *The course of studies to be pursued.*—Here is where the Augean stable of a past education needs to be purified. The notion that when a child has learned to read, write, and cipher, he is educated, must be eradicated. These are at best but means, and are only the instruments by which education is conducted. It will be for the man of science to show his colleagues on the School Board that perhaps the better half of a liberal education may be obtained without books at all. This is the error that lies at the foundation of all our systems of education, whether conducted in our highest, middle-class, or national schools. The education of the senses by which the man is to get his living and to perform his duties in life is entirely neglected. Where attempts have been made to introduce the study of the natural sciences, it has been done solely by the aid of books, and not with that demonstration of the facts to the senses which is the only way in which such knowledge can be made useful. In a word, henceforward there must be a portion of every day taken up with teaching children by objects, specimens, or experiments, the nature of the great laws by which the universe is governed. We cannot argue here on the necessity for this knowledge. Look at that great German army, recently spoken of as the most wonderful military engine ever seen on the face of the earth. What makes it so? The intelligence of each individual of which it is composed. It is the same with wheels and pistons, spindles, hammers, chisels, and ploughs, as with guns and bayonets: the more intelligent the man is who wields or superintends them, the more successfully and prosperously will they do their work. Ten years ago Mr. Whitworth astonished the Manchester manufacturers with the account of the machines he had seen in America. "Why should we not have such machines here?" said the Manchester men. "Because," said Mr. Whitworth, "you have not intelligent hands to work them." And for these long ten years we have gone on talking about educating our working classes, and allowing priceless treasures to pass out of our hands. Every portion of Europe, as well as the United States of America, is stealing something of our rightful wealth and increasing our pauperism, because of our stolid indifference to the introduction of those branches of human knowledge which alone can properly develop the powers of industry and application, of which the English people are so wonderfully capable.

This great question of the introduction of Natural Science into all schools must be taken up by our School Boards throughout the kingdom. To delay it is to shelve it, and to commit an irretrievable error. It is now or never. If the present opportunity is neglected, all is lost. Let no heed be given to the cry that it is impossible to find teachers. If teachers cannot be found they must be made, and all old teachers must be told that unless they qualify in this respect they will be of no use. The cry of the example of our Universities must not be listened to.

We have nothing to do here with their failure to teach Natural Science, and thus to mislead where they ought to have led. What we now ask the people of England, and especially the people of London, is to put Men of Science on their School Boards.

E. LANKESTER

THE GLACIATION OF BRAZIL

Thayer Expedition: Scientific Results of a Journey in Brazil, by Louis Agassiz and his travelling Companions. Geology and Physical Geography of Brazil. By Ch. Fred. Hartt, Professor of Geology in Cornell University. With Illustrations and Maps. (Trübner and Co.)

THIS thick volume of 620 pages is the result of two visits to Brazil, the first with the Thayer Expedition, the second during a vacation holiday of "some months." The author has proposed to combine with his own personal observations all the information on this subject obtainable from other sources, and thus give a complete view of the present state of our knowledge of the geology and physical geography of this vast and interesting region. The design is an admirable one, but the execution of it is, in some respects, disappointing.

The first great fault of the book is, that it has been swelled by the introduction of much irrelevant matter. Mr. Hartt's own journeys were mainly along the coast, from Rio Janeiro to the Amazon, with occasional trips of a hundred miles or so into the interior, and he inflicts upon us pages of unimportant detail on the topography of small rivers, creeks, and harbours, which have no bearing on the geology or physical geography of the country. Detailed descriptions of the marine animals and fossils collected would also have been better in an appendix than in the body of the work where they are given. The arrangement of the book, too, is faulty, since it treats of the provinces of Brazil in succession, and makes no attempt to indicate the great physical divisions of the country, and there is not a single geological or physical map of Brazil, or of any part of it; the maps alluded to in the title-page being mere outline or sketch maps of small districts, or plans of harbours and mouths of rivers. Another strange defect is the absence of all measurement of heights. The author travelled without barometer or aneroid; he, consequently, everywhere roughly estimates his heights, and gives no sections, but a few "ideal" ones. Notwithstanding the bulk of this volume, it does not complete the geology of the voyage, for we are informed that Mr. St. John, another geologist attached to the expedition, who travelled more in the interior of the country, will give the results of his observations in a separate work.

But although we have thus plainly indicated the defects of the book, there is much valuable matter to be found in it. The author has been very diligent in examining all the chief authorities on Brazil, and has extracted from them most of their geological matter; and among the extracts from Spix and Martius, Prince NeuWied, Darwin, Gardner, Halfield, and others, are to be found many interesting passages desciiptive of the peculiarities of the scenery and geology of the country. The chapters on the coral reefs of the Abrolhos and on the gold mines of Brazil, the account of the exploration of the bone-caves by Lund, and the appendix on the Botocudos Indians, will furnish some interesting matter for the general reader, while the student of science will obtain (though with some difficulty) a notion of the general physical and geological characteristics of an almost unexplored region.

The most striking geological feature of tropical South America east of the Andes is the enormous extension of gneissic rocks, which appear to form the whole foundation and much of the surface of the country, from the cataracts of the Orinooko to Paraguay and the southern frontier of Brazil. All the great mountain tracts of Brazil and Guiana, as well as the low plain which separates the watersheds of the Orinooko and Amazon, are of this rock, which is considered to be of Laurentian age. Its characteristic features are the great dome-like masses and the conical peaks or pillars, generally of more or less smooth and rounded outlines, a peculiarity dependent on the decomposition of all exposed surfaces, which fall away in concentric flakes. Great hemispherical domes up to a thousand feet in diameter are one of the results of this decomposition wherever a more resisting mass has occurred. Still more extraordinary are the vertical pillars of rock, that rise up at intervals out of the forest to some hundreds, or, in the case of the Pedra lisa, in the province of Rio de Janeiro, to more than three thousand feet high. Similarly formed peaks or pillars in Fernando, Noronha, and St. Helena have been formed by injections of fluid felspathic lava. What an enormous amount of denudation do these isolated pillars indicate!

In South Brazil a few tracts of Silurian and Carboniferous rocks occur, but the next formation of any extent is the Cretaceous, which consists of sandstones, generally upheaved and fractured. Other sandstones, which cover an immense extent of country, and form the ranges of flat-topped hills from one to nearly three thousand feet high, called *taboleiros*, are in perfectly horizontal strata, and as these lie unconformably on the cretaceous rocks they are presumed to be tertiary, although no fossils have yet been found in them.

We now come to a very wide-spread, yet recent and superficial deposit, which is at once the most puzzling and the most interesting feature in Brazilian geology. This is a layer of clay or loam, varying in thickness from a few feet to one hundred, and wrapping in its folds hill and valley, over vast tracts of country, including the steep slopes and summits of some of the highest mountains. All Rio de Janeiro, and all the coast provinces visited by our author, were thus covered. It has been described in Minaes Geraes and San Paulo, and Prof. Agassiz has observed it in all the northern provinces as far as the Amazon valley. It covers alike the gneiss and the tertiary formations. This clay is of a red colour, and is evidently formed of the materials of the adjacent and under-lying rocks, but ground up and thoroughly mixed. There is never the least sign of stratification throughout its mass, although it very frequently rests on a thin layer of quartz pebbles. It contains, scattered through it, rounded and angular boulders of quartz, gneiss, and other rocks, and the surfaces upon which it rests are always more or less smooth and rounded. Our author always speaks of this formation as "drift," and he agrees with

Prof. Agassiz that its peculiarities are such as unmistakeably to indicate its glacial origin.

This is truly a startling conclusion, and one which has hitherto been received in this country with some incredulity. Prof. Agassiz was thought to be glacier-mad; but if we separate his theories from his facts, and if we carefully consider the additional facts and arguments adduced by Mr. Hartt in this volume, we shall be bound to conclude that, however startling, the theory of the glaciation of Brazil is supported by a mass of evidence which no unprejudiced man of science will ignore merely because it runs counter to all his preconceived opinions.

Mr. Hartt's facts and deductions have the more weight, because he is evidently not very enthusiastic on the subject, and because he fairly states the sources of error in observation, and fully discusses such other modes of explaining the facts as have been proposed. He acknowledges that in some cases the decomposed gneiss cannot be distinguished from the "drift;" but he shows that in the former the materials remain *in situ*, especially the quartz veins, while in the latter all are mixed and ground up together; and wherever the two are seen in contact for any distance, the sudden cutting off of the quartz veins at the drift, and other well-marked characters, render it impossible to confound them. He also adduces several other phenomena which are strongly indicative of a glacial origin. Both in the Orang Mountains and in Bahia there are valleys with no outlet, and in Alagoas there are many deep lakes in rock-basins. In the province of Bahia there are extensive bare, elevated, rocky plains thickly strewn with angular blocks of stone, some of which are erratics, and *exactly resembling the drift-covered plains of the north.* On similar elevated plains, far removed from any higher land, Mr. J. A. Allen (another member of the Thayer Expedition) found numerous deep and smooth pot-holes worn in solid gneiss. They were of various sizes, the largest seen being elliptical, eighteen feet long by ten wide, and twenty-seven feet deep. Similar pot-holes are known to be formed by glacial waterfalls, and they are found over the glaciated regions of New Brunswick and Nova Scotia. Heaps of débris, exactly resembling glacial moraines, have also been found both in the south and north of Brazil. Mr. Hartt is satisfied of their resemblance to true moraines in the valley of Tijuca near Rio, and Prof. Agassiz has described others still more perfect in Ceara, only four or five degrees south of the equator. After describing these in detail, he concludes: "I may say that in the whole valley of Hasli there are no accumulations of morainic materials more characteristic than those I have found here, not even about the Kirchet; neither are there any remains of the kind more striking about the valleys of Mount Desert in Maine, where the glacial phenomena are so remarkable; nor in the valleys of Loch Fine, Loch Awe, and Loch Long, in Scotland, where the traces of ancient glaciers are so distinct." It can hardly be maintained that the discoverer of glacial phenomena in our own country, and who has since lived in such a pre-eminently glaciated district as the Northern United States, is not a competent observer; and if the whole series of phenomena here alluded to have been produced without the aid of ice, we must lose all confidence in the method of reasoning from similar effects to similar causes which is the very foundation of modern geology. The only objection of any weight that has been made to this interpretation of the phenomena, is the fact of the absence of glacial striæ; but Mr. Hartt states that no striæ or polished surfaces have yet been found even in the extreme south of the continent where the glacial phenomena are undisputed. It is at best a negative argument, and as such cannot neutralise those of a positive nature. We must also remember that we have no indication of the age of the Brazilian or southern glacial epoch. It may have occurred much earlier than that of the northern hemisphere, and the greater lapse of time, combined with the powerful decomposing and denuding agency of tropical rains, may have obliterated most of such marks.

The physical difficulty of the conception of tropical glaciers may be resolved into the question of whether a subsidence to the extent of ten or twelve thousand feet may not have subsequently occurred; since a greatly increased elevation at a time when a severe glacial epoch reigned in the south temperature zone, would probably lead to the glaciation of the southern tropics down to what is now the sea-level.

A much more serious objection seems to be the biological one. If the whole surface of what is now Brazil was recently covered with a sheet of ice, whence has arisen the wonderfully rich and varied, and, in many respects, peculiar flora and fauna that now inhabit it? Judging from the map of the Atlantic given in Maury's "Physical Geography of the Sea," a rise of twelve thousand feet would only add a belt of about four hundred miles in width to tropical America; but a great part of this might have enjoyed a truly tropical climate, just as the valleys of Switzerland have a warm summer though in the immediate vicinity of glaciers. It seems probable, also, that the glaciation was a southern one, and did not extend far north of the equator, if it even reached so far, so that the whole of Venezuela and Guiana, with the additional belt of land due to elevation, might have been even more luxuriant and more densely populated than at present. There would thus have been an ample surface to support the ancestors of the existing fauna and flora of Brazil during the glacial epoch, just as there was sufficient land in Europe to support the ancestors of the existing European fauna and flora even when so much of the present surface was covered by a thick mantle of ice.

It must be stated that Mr. Hartt does not accept Prof. Agassiz's extraordinary hypothesis (which rests on a very slender basis of fact) of a great Amazonian glacier. He believes that the wide-spread beds of clays and sandstones, which Prof. Agassiz classes as glacial, are marine, and states that they agree perfectly with the tertiary beds in other parts of Brazil. The patches of drift, with erratics in the Amazon valley, may well have been produced by detached masses from the glaciers of the Andean and Brazilian highlands, which melted and deposited their load of drift in the warm waters of the ancient Amazon.

We have devoted so much of our space to this question of the Glaciation of Brazil, in the hope of attracting the attention of geologists to a country which offers such an interesting subject of inquiry, and which it is so easy and agreeable to explore. The facts, as stated by two careful observers, both thoroughly experienced in glacial phenomena, are undoubtedly such as to warrant the main conclusion drawn by them; and it is to be hoped that geologists will not ignore the facts because the conclusions seem improbable, as they so long ignored facts proving the antiquity of man for no other reason.

A. R. WALLACE

MODERN ANGLING

The Modern Practical Angler. A complete Guide to Fly-Fishing, Bottom-Fishing, and Trolling. By H. Cholmondeley-Pennell, Inspector of Fisheries. Illustrated by Fifty Engravings of Fish and Tackle. 16mo, pp. 286. (London: Fred. Warne and Co., 1870)

TO those readers of NATURE who are not acquainted with Mr. Pennell, the following quotation may serve as an introduction: "Fishing has been in a special sense my mistress—the fairest and most loving wife—in many a wild and lonely spot where, but for her gentle companionship and solace, I should have felt myself in every sense of the word alone;" whilst those of us who have for some time had an acquaintance with his writings, know that in making this confession he is perfectly sincere, and that he is one of the most devoted disciples of Izaak Walton; so that we cannot help wishing he were an "Inspector of Fisheries" (as he describes himself on the title-page), instead of being appointed by the Government to investigate the causes of failure and possibilities of improvement of our *oyster* fisheries. His book has only a partial resemblance to Walton's "Complete Angler." Those passages of pleasing simple eloquence, those fine sentiments, those virtuous precepts, in short, all those characteristics which have rendered Walton's book immortal, must not be looked for in Mr. Pennell's "Modern Angler." To imitate Walton successfully, would, indeed, require a genius of no common order; and Mr. Pennell has contented himself with giving a mere manual of the piscatorial "art" and "science" (we must not be too severe with enthusiasts about terms); and judging of it as such, we can sincerely say that it is the best and most useful handbook we have yet seen.

The book is divided into four parts, treating minutely of tackle, fly-fishing, trolling or pike-fishing, and bottom-fishing. The author takes credit for several inventions or improvements. Thus, for instance, he describes or figures the "Pennell-hook," in which "the medium between theoretical and practical requirements" is believed to be hit. We are glad to see him advocating a reduction in the number of artificial flies used at present; he proposes to substitute six typical flies, three for salmon and grilse, and three for trout, grayling, &c. We feel sure that these flies, together with those which are especially used at certain localities, will be quite sufficient for all purposes. Mr. Pennell has thought a great deal at the river-side; he is never satisfied with simply describing what, according to his experience, has proved to be the most successful method or the deadliest instrument; he always gives the reasons. Thus, in one of the chapters, we find expounded the "true theory of trout-flies," in a seconn the "theory of salmon-flies," and in a third, of white trout-flies; however, we are afraid that in expounding theories he will not be more successful in convincing his readers than the majority of theorists. For instance, to the question, For what does the salmon take the artificial fly? he gives the answer, "For its beauty and tempting appearance; probably it has an appetising effect." Let Mr. Pennell once watch a prawn (one of the principal articles of food of salmon in the sea) swimming in jerks through the water, and he will at once perceive that by means of our rod we impart to the fly the peculiar motion of the prawn, whilst the iridescence of the real creature is reproduced by the colours of the fly, which must vary according to the physical changes of the sky and water. No two things can be more unlike than a prawn and a dry artificial fly; no two things are more alike than a swimming prawn and that same fly in the water worked by a skilful hand.

But we must conclude our notice of this book, welcome and useful to every class of anglers. It is illustrated by numerous well-executed woodcuts, which are more instructive than the best descriptions. Lithographic plates of some of the more common freshwater fishes are evidently reproductions from the *Fisherman's Magazine.*

A. GÜNTHER

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

The Government and the Eclipse Expedition

WE are now within two months of the date fixed by Nature, whose name you so worthily wear, for a total Eclipse of the Sun, and it is not probable that she will postpone her appointment for a period sufficient to enable the joint committee of the Royal and Astronomical Societies to renew and succeed in their endeavour to teach the Government what such a phenomenon may, if duly observed, contribute to human knowledge. It may be taken for granted that no encouragement will be afforded by our thrifty rulers to an expedition of sixty-eight astronomers, projected for the quixotic purpose of collecting intelligence not calculated to increase the revenue.

This well-timed and praiseworthy frugality reminds me of some imputations which were, not long ago, cast at the Government by two eminent men of science, one of whom, the Astronomer Royal, lives to repent his injustice; the other, the late Dr. W. A. Miller, alas! can speak no more.

On the 31st March last, I read at a Society of Arts' conference, by request of the Council, a paper on the subject of the Relations of the State to Science. A discusssion followed, in the course of which the Astronomer Royal remarked that "having had a somewhat long connection with the Government, he was quite competent to say that there had never been any unwillingness, as far as instances occurred to him, to promote liberally the purpose of speculative science when brought before the Government, with a good cause shown, and upon the responsibility of some person in whom they placed confidence."

Here Mr. Airy lays down, with his usual clearness, the conditions necessary to induce our Government to promote liberally speculative science, namely, "a good cause shown, and on the responsibility of some person in whom they placed confidence." Our failure, then, to obtain aid on the occasion in question must have been due to one of three things—either a good cause was not shown, or the Government had no confidence in the persons showing it, or the Government differs considerably from Mr. Airy's portrait of it. Who were the persons whose worthiness of confidence is thus doubtful? The Astronomer Royal himself, the President of the Royal Society, and the President of the Astronomical Society; and these untrustworthy beguilers of our too liberal guardians of the public purse were the accredited representatives of a select joint committee of the two first scientific societies in the kingdom. As to the goodness of the cause, that is to be inferred from the character, requirements, and position of the three personages above indicated, who had the presumption to advocate it. To these two causes it is evident that the failure is due, and not to any want of liberality in statesmen, for whose readiness to promote speculative science Mr. Airy himself, one of the unsuccessful petitioners, has vouched.

The late Dr. Miller spoke as follows on the occasion above referred to:—"Gentlemen," he observed, "representing great branches of science, like the Astronomer Royal, were met by the Government with liberality; and scientific bodies, such as the Royal Society, when they made an application, after careful consideration of the objects they had in view, were also listened to with respect, and, in the majority of instances, their applications were granted. It appeared, therefore, that the great difficulty on the part of the Government was to be sure that what they expended was wisely laid out."

Dr. Miller, as Treasurer of the Royal Society, spoke with authority; he shared with the Astronomer Royal the error of imputing to Government a liberality towards science utterly inconsistent with that true economy for which they have, in so many other matters, earned high renown.

The whole transaction, considered as the latest notion in civilisation, is so ludicrous that it is difficult in speaking of it to do so seriously. It may be argued that Mr. Airy and Dr. Miller were perfectly correct in their picture of former Governments, but that our present Ministry act on different principles, and refuse to promote what their predecessors would have thought worthy objects. This hypothesis is so extremely disparaging to the Ministers of to-day, that I for one cannot adopt it. When the speeches I have cited were made I offered no reply, but I was far from assenting to the estimates of Government liberality to science in England given by Mr. Airy and Dr. Miller. I do not now for the first time say that, not only have English Governments, past and present, been backward in promoting science, but that no English Government has ever shown that it understood the value of science—how it should be promoted, or what were their duties respecting it. This inability to realise the requirements of Science is not confined to Governments—it largely pervades the most intelligent portion of the community—the whole governing class. For one man of high position and culture who comprehends the mode of advancing science, and the certain effects of advancing it, there are, not hundreds, but thousands, who have clear views regarding art, literature, and general education. Every man of wealth covers his walls with pictures and books, but how many equip a laboratory or an observatory? The knowledge of science which most educated men possess has been acquired by reading only, and their notions of what is needed to advance science, of the extent of field remaining still unoccupied, of the means requisite for cultivating it, and of the value of the produce, are, we find from every-day experience, exceedingly hazy.

The true explanation of the difference between the distinguished speakers and those who think with me I believe to be this—that the demands made upon the State by scientific men and bodies have hitherto been so moderate, so desultory, and so infrequent, that few of them could, with decency, be denied by men not utter barbarians. I am of opinion that the moderation of scientific men has been exceedingly detrimental to the nation, and utterly opposed to true economy. For it has led to the waste of the two most valuable gifts of Providence—the human intellect and the forces of nature. This moderation has encouraged the comfortable creed of English statesmen that private enterprise will do all that is needful, and that the utilisation of those two gifts forms no part of their national duties But suddenly those who have suffered Government to hug itself in its inaction, and live in a fool's paradise of indifference to duties which they do not understand, are startled by their own teachings recoiling on themselves in a rebuff unequalled for its narrowmindedness since the time of Galileo.

But a new school of scientific men is now astir, and this rebuff will quicken its energies and so do good. A Royal Commission on Science now sits, and at such a tribunal this wrong to Astronomy must be heard and judged. Let us hope that when next the sun is eclipsed, the darkness on the minds of men may not equal the darkness on the earth's face.

ALEX. STRANGE, Lieut.-Col.

The Geological Bearings of Recent Deep-Sea Explorations

YOUR report of the Proceedings of the Geological Section of the British Association (No. 51, p. 503) makes Sir Roderick Murchison say that "he hoped Mr. Jeffreys did not share the opinion of his colleague Dr. Carpenter, that their discoveries tended to upset modern geology." I have the authority of Sir Roderick to state, that he did not accuse me of any such absurdity; and that I should find what he did say on that occasion, in dissent from some of the views put forth by Prof. Wyville Thomson and myself, fully expressed in his introductory address, of which he has given me a copy. As, however, he there attributes to a passage in a lecture which I delivered eighteen months since at the Royal Institution, a sense which I never meant it to convey, I shall be obliged by your allowing me to give a precise explanation of my meaning.

The passage cited by Sir Roderick is as follows:—"The facts I have now brought before you, still more the speculations which I have ventured to connect with them, may seem to unsettle much that has been generally accredited in Geological Science; and thus to diminish rather than to augment our stock of positive knowledge; but this is the necessary result of the introduction of a new idea into any department of scientific inquiry."

Now I gave not the remotest hint of impugning those great doctrines of Stratigraphical and Palæontological succession, to which Sir Roderick refers as accepted by Uniformitarians, Catastrophists, and Evolutionists alike; my chief heresy being the indorsement of the doctrine of which my colleague, Prof. Wyville Thomson (himself a sound and accomplished Geologist), was the originator, "that we may be said to be still living in the Cretaceous Epoch." Our meaning was this:—There can now be no question that a formation, corresponding with the Chalk of the Cretaceous Epoch, alike in its material, and in the general character of its Fauna, is at present going on over a large part of the North Atlantic Sea-bed. This similarity is marked, not by the occurrence of a few types of life (like the Lingulæ and Terebratulidæ of the older formations, referred to by Sir Roderick Murchison), but by the persistence of those which constitute the formation itself, viz., the Globigerinæ, the Coccoliths and the Coccospheres; as also of numerous types of Echinodermata that were formerly considered as essentially Cretaceous, and of a great variety of those Sponges (including Xanthidia), and Foraminifera, whose abundance in the White Chalk is one of its most important features. The explorations carried on by the United States Coast Survey in the Gulf of Mexico have furnished results entirely coinciding with our own in many of these particulars.

Now it is, of course, quite open to any geologist to maintain that this Formation is a mere repetition of the Cretaceous, at a later date, under generally similar conditions. Such was, I presume, the idea of those who, several years before our researches began, had pointed out the conformity of the material of the Atlantic deposit with that of the old Chalk; and such had been my own belief, until Prof. Wyville Thomson suggested to me the probability of a continuity between the past and the present deposits, on the following ground:—The oscillations of the earth's crust, in the Northern part of the Northern Hemisphere,* during the whole Tertiary period, have not been shown anywhere to exceed 1,500, or at the most 1,800 feet, or 300 fathoms; and as the general depth of the North Atlantic Sea-bed ranges from twice to ten times that amount, there is no reason to suppose that the formation and accumulation of Globigerina-mud have been interrupted in any part of its duration. Now the termination of the Cretaceous Epoch is commonly regarded as having been marked by the elevation of the Cretaceous deposits of the European area into dry land; but there is no evidence that this change of level stopped the formation of Chalk in the deep sea elsewhere. On the contrary, according to the received doctrine of Geology, it is highly probable that coincidently with the elevation of the European area, there was a gradual subsidence of what is now the North Atlantic Sea-bed; so that the Globigerinæ and Coccoliths of the former area, with such accompanying types of animal life as could accommodate themselves to the change of conditions, would progressively spread themselves over the latter.

Now there is nothing more heterodox in this view than in M. Barrande's doctrine of "colonies," which is now, I believe, universally accepted as the explanation of a large and very important series of geological facts—the persistence, in certain outlying localities, of a Fauna characteristic of a formation stratigraphically inferior to that in which it presents itself. The only difference here is in the relative extent of the existing Cretaceous deposit in the North Atlantic, which may hold to that of Europe somewhat the relation that the English-speaking race which has colonised America does to that of the mother country, instead of

* This statement has been recently met by our friend Mr. J. Gwyn Jeffreys, who adverts to the well-known fact of the elevation of Tertiary strata in the South of Euaope to 11,000 feet. But there is no evidence, so far as we know, of any such elevation in the latitude of Great Britain, or north of it.

that which the Norse-speaking Icelanders hold to the modern Scandinavians.

If the facts be as I have now stated them, the *onus probandi* seems to me to lie upon those who affirm that a complete stop was put to the formation of Chalk before the commencement of the Tertiary period. If, on the other hand, the continuity of the existing Chalk-deposit with that which formed the Chalk of Dover Cliffs be admitted, the question, whether we can be rightly said to be "still living in the Cretaceous Epoch" seems to me one of terms rather than of essentials.

That we could not expect to find the Cretaceous Fauna *as a whole* in our modern Chalk, is evident from the considerations admirably set forth in a parallel case by the President of the Linnean Society, in his last Annual Address.

The difference is undoubtedly most marked in the *Mollusca;* only one shell, the *Terebratula caput-serpentis,* having at present been shown to be common to the Cretaceous and the Modern period. But the positive evidence of continuity afforded by the persistency of all the types which *make* the chalk, as well as of numerous forms of Echinoderms, Sponges, and Foraminifera which are among its characteristic inhabitants, appears to us to outweigh the negative evidence afforded by the larger amount of change that has taken place in the Molluscan Fauna, for which it would not be difficult to assign probable reasons.

Further, it is to be borne in mind that the successive beds of the Cretaceous formation differ from each other in a very marked manner; so that we could not expect to find, in any one deposit, more than a small part of that *ensemble* which is commonly spoken of as "the Cretaceous Fauna." What we mean by the expression to which Sir Roderick Murchison has taken exception, is simply that the facts and deductions we have brought together justify the assumption of the continuous prevalence of the same general Physical and Biological conditions, in the deep sea that separates the northern parts of the European and American continents, from the time when the Chalk of those continents was raised into dry land to the present date. This is perfectly compatible with those changes in the conditions of the shallower portions, which have given origin to the long succession of Tertiary deposits.

Passing from this topic, I now proceed to other points on which the researches of Prof. Wyville Thomson and myself appear to us to invalidate Geological doctrines that have gained general currency.

Up to the commencement of the recent exploration of the Deep Sea bottom by means of the Dredge, the doctrine propounded by Prof. Edward Forbes as to the limitation of Animal Life to a depth of 300 or 400 fathoms, and the consequently *azoic* character of all deposits formed at depths exceeding that amount, was generally accepted by Geologists; partly on account of the deservedly high authority of its originator, and partly because it appeared to afford a simple explanation of phenomena which had long perplexed Geologists and Palæontologists, viz., the occurrence at various epochs of vast accumulations of sedimentary strata apparently altogether devoid of organic remains. The indications obtained by the Sounding apparatus, of the existence, not merely of humble Foraminifera, but of Annelids, Echinoderms and Crustacea, at depths far exceeding Edward Forbes's limits, were not generally accepted, either by Zoologists or Palæontologists, as indicating the presence of a varied and abundant Fauna on the ocean bottom; for although Dr. Wallich, with a sagacity to which I have uniformly endeavoured to do full justice, had argued that they should be, it was specially noticed that these researches gave no evidence of the existence, at great depths, either of Mollusks or of Zoophytes—the two groups whose fossil remains are usually of the highest Palæontological significance.

Now the Dredgings which were carried down in the *Lightning* Expedition of 1869 to 650 fathoms, and in the *Porcupine* Expedition of 1869 to 2,435 fathoms, have established beyond all reasonable question that a varied and abundant Fauna may exist on the sea-bottom without any limit as to *depth* and *pressure;* and they have further rendered it probable that, putting aside those Animals which are necessarily restricted by the nature of their food to the depth to which living Vegetation extends, a large proportion may accommodate themselves by gradual modification to any amount of *change* in depth and pressure; so that the assumption that the occurrence of particular types is significant of the depth at which a formation was deposited, can no longer be upheld, except in the case of animals essentially littoral. For example, no doctrine has been more generally accepted than that of the limitation of the Pedunculate Crinoids to comparatively shallow water; the large West Indian representatives of that group being found growing on coral reefs, and a like *habitat* having obviously been peopled by them in the Carboniferous epoch. Yet, in the *Porcupine* dredgings of the present year, a large Pentacrinus, closely allied to the West Indian species, has been obtained near the coast of Portugal from a depth of about 800 fathoms; and the little Rhizocrinus, with another small Apiocrinoid, which I hope soon to describe under the name of Wyvillocrinus, were found last year, the former at 862 fathoms, the latter at 2,435.

Further, the *Lightning* and the *Porcupine* dredgings have fully established the position that the distribution of marine life is much more closely related to the *temperature* of the ocean-bottom, than to its depth. This is most clearly evidenced by the results of the careful exploration of the Channel of from 500 to 650 fathoms depth, which separates the plateau that supports the northern extremity of Scotland from the Faroe Banks. For we have shown that, whilst the *surface*-temperature of this Channel is everywhere nearly the same, and indicates a derivation of its upper stratum from a warmer source, a considerable part of the deeper portion of this Channel is covered by *a frigid stream,* bringing a temperature as low as 29·5° from the Arctic Ocean; this stream having in some places a depth of 2,000 feet. Thus the bottom of this Channel is divided into a *warm area,* on which the bottom-temperature at depths of from 500 to 600 fathoms is about 45°, and a *cold area* on which the bottom-temperature at like depths is 30°, or even lower. We have traced these two areas at corresponding depths within about twenty miles of each other; and where the bottom was unequal,—the slope of the plateau at the edge of the cold area, or of a bank in its midst, raising its bottom out of the *cold* stream into the *warm* which overlies it—a difference of 18·5° was found within *eight miles.* No contrast could well be more striking than that which presented itself between the Faunæ of these two areas. The Globigerina-mud was rigorously limited to the *warm;* and of the animals living on its surface a large proportion were characteristic of the warmer-temperate seas. The bottom of the *cold* area consisted of sand and stones; and of the animals which were abundantly distributed over it, a large proportion were essentially Boreal. In the shallower portions of the cold area, where an intermediate bottom temperature prevailed, an intermixture of the two Faunæ, corresponding with the border position of this area between the Temperate and the Boreal provinces, was readily traceable.

Here, then, we have the remarkable fact that two deposits may be taking place within a few miles of each other, at the same depth and on the same Geological horizon (the area of one penetrating, so to speak, the area of the other), of which not only the Mineral character but the Fauna are alike different;—that difference being due on the one hand to the direction of the current which has furnished their materials, and on the other to the temperature of the water brought by that current. If the *cold* area were to be raised above the surface, so that the deposit at present in progress upon its bottom should become the subject of examination, by some Geologist of the future, he would find this to consist of a Sandstone formed by the disintegration of older rocks, the Fauna of which would in great degree bear a Boreal character: whilst if a portion of the *warm* area were elevated at the same time, the Geologist would be perplexed by the stratigraphical continuity with the preceding of a Cretaceous formation, the production of which entirely depends upon the extensive development of the humblest forms of animal life under the influence of a higher temperature, and which includes not only an extraordinary abundance of Sponges, but a great variety of other animal remains, several of them belonging to the warmer-temperate regions. He would naturally suppose these widely different climatic conditions to have prevailed at different periods, and would probably have had recourse to the hypothesis of a "fault" to account for the phenomenon. And yet these Formations have been shown to be going on together, at corresponding depths, over wide contiguous areas of the sea-bottom; in virtue solely of the fact that one area is traversed by an Equatorial, and the other by a Polar current. Further, in the midst of the land formed by the elevation of the Cold area, our Geologist would find hills some 1,800 feet high, covered with a Sandstone continuous with that of the land from which they rise, but rich in remains of animals belonging to a more temperate province; and might easily fall into the mistake of supposing that two such different Faunæ occurring at different levels must indicate two distinct climates separated in time; instead of indicating, as they have been shown to do, two contemporaneous but dissimilar climates, separated only by a few miles horizontally and by 300 fathoms vertically.

But further: the Temperature-soundings taken in the *Porcupine* Expeditions of 1869 and 1870 have conclusively shown that a temperature as low as 36·5° prevails over the deeper parts of the North Atlantic sea-bed; this reduction being due to the pervasion of Arctic and Antarctic waters, which come to replace the superficial flow of Equatorial water (as in the Gulf Stream and other currents) towards the Polar areas. In conformity with this depression of temperature, many species of Mollusca, Crustacea, and Echinodermata, formerly supposed to be purely Arctic, have been found to range southwards in deep water as far as the Straits of Gibraltar; and we have shown it to be highly probable that an extension of the same mode of exploration would bring them up from the abysses of even intertropical seas, over which a similar climate prevails, and that an actual continuity may thus be found to exist between the Arctic and the Antarctic Faunæ. This idea was well put forth some years since by our excellent friend Prof. Lovén, of Stockholm, in his discussion of the results of the deep-sea dredgings executed by the Swedish Spitzbergen Expedition of 1861, under Torell. "Considering," he says, "the power of endurance in these lower marine animals, and recollecting the facts that properly Arctic species which live also on the coast of Europe, are generally found there at greater depths than in their proper home, and that certain Antarctic species very closely agree with Arctic species, the idea occurs that, while in our own seas and those of warm climates, the surface, the coast line, and the lesser depths are peopled with a rich and varied Fauna, there exists in the great Atlantic depression, perhaps in all the abysses of our globe, and continued from Pole to Pole, a Fauna of the same general character, thriving under severe conditions, and approaching the surface where none but such exist in the coldest seas."

But whilst the question of Deep-sea Temperature is one of the greatest Biological interest, its determination is of even greater importance to the Geologist, as affecting his interpretation of the phenomena on which his belief in a former general prevalence of a Glacial climate is founded. For if a Glacial temperature should be found now to prevail, and types of Animal life conformable thereto should prove to be diffused, over the deeper portion of the *existing* Sea-bed in all parts of the globe, it is obvious that the same may have been the case at *any* Geological epoch; for there must have been deep seas in all periods, and the Physical forces which maintain the oceanic circulation at the present time must have been always in operation, though modified in their local action by the distribution of land and water existing at any particular date. And as the elevation of the present deep-sea bed of even the Intertropical oceanic area would (if we have correctly interpreted the results of our own and others' observations) offer to the study of the Geologist of the future a deposit characterised by the presence of Polar types, so must the Geologist of the present hesitate in regarding the occurrence of Boreal types in any marine deposit as adequate evidence *per se* of the general extension of Glacial action into temperate or tropical regions. At any rate, it may be considered as having been now placed beyond reasonable doubt, that a Glacial Submarine climate may prevail over any area, without having any relation whatever to the Terrestrial climate of that area.

These views are offered by us with the more confidence, since they are in harmony with the deductions already drawn by Geologists of eminence from facts observed by them. Thus I find on my return from the Mediterranean a letter from Principal Dawson, of Montreal, from which I am sure he would permit me to make public the following extract:—

". . . In reading your recent interesting publications on the Life of the Deep Sea, it occurred to me to mention to you that the fact which you have proved on the European coast, as to the existence and action of cold Arctic currents on the bottom of the ocean, was affirmed by me years ago for the American coast, on geological and geographical evidence, and was applied to the explanation of the Post-pliocene climate. On the American coast we have the cold currents in shallower water than you have now; though in the Post-pliocene you had them in shallow water also. It is true that the Glacial theories of Agassiz and others have prevented the proper amount of attention to these facts; but I have insisted on them again and again, and fully believe that the varying distribution of the cold and warm currents, depending on the elevation and depression of the sea bottom, will account for most of the differences of climate indicated by fossils and boulders from the Laurentian to the Modern period. I have some new and unpublished facts on this subject, which I intend to bring out in connection with the work I am now doing with the help of your brother, in the Post-pliocene geology of Canada."

In conclusion, I venture to anticipate that the words with which I concluded my lecture at the Royal Institution, "On the Results obtained in the *Lightning* Expedition of 1868," will be found to have been fully justified by those of the "*Porcupine* Expeditions" of 1869 and 1870; and that whatever may be thought of the notion that "we are still living in the Cretaceous epoch," we have furnished adequate proof that the formation of Glacial beds was not limited to any special Geological period, but that they are now, and have been through all time, in course of deposition:—"The facts I have now brought before you, still more the speculations which I have ventured to connect with them, may seem to unsettle much that has been generally accredited in Geological Science, and thus to diminish rather than to augment our stock of positive knowledge; but this is the necessary result of the introduction of a new idea into any department of scientific inquiry. Like the flood which tests the security of every foundation that stands in the way of its onward rush, overthrowing the house built only on the sand, but leaving unharmed the edifice which rests secure on the solid rock, so does a new method of research, a new series of facts, or a new application of facts previously known, come to bear with impetuous force on a whole fabric of doctrine, and subject it to an undermining power which nothing can resist, save that which rests on the solid rock of Truth. And it is here that the Moral value of Scientific study, pursued in a spirit worthy of its elevated aims, pre-eminently shows itself. For, as was grandly said by Schiller in his admirable contrast between the Trader in Science and the true Philosopher, 'New discoveries in the field of his activity which depress the one, enrapture the other. Perhaps they fill a chasm which the growth of his ideas had rendered more wide and unseemly; or they place the last stone, the only one wanting, to the completion of the structure of his ideas. But even should they shiver it into ruins, should a new series of ideas, a new aspect of nature, a newly-discovered law in the physical world, overthrow the whole fabric of his knowledge, *he has always loved truth better than his system*, and gladly will he exchange her old and defective form for a new and fairer one.'"

WILLIAM B. CARPENTER

On a Method of Ascertaining the Rate of Ascent of Fluid in Plants

WHEN conducting a series of physiological experiments on the transpiration of fluid by leaves, it became a matter of importance to determine the rapidity of ascent of fluid. My colleague, Prof. Church, had suggested for another series of experiments the use of lithium citrate, a salt easily taken up by plants, and one which can be detected with the greatest readiness by means of the spectroscope. Preference was given to the citrate, because of its containing an organic acid, and on this account not likely to meet with any obstruction to its passage from the tissues. This method I have used with great success. In one experiment the fluid had risen nine inches in thirty minutes, in another five and a half inches in ten minutes. This method is greatly superior to the use of colouring matters, which seem to experience considerable resistance in their passage through the vessels. Full particulars of these and numerous other experiments in the same direction will shortly be published. W. R. M'NAB, M.D.

Royal Agricultural College, Cirencester, Oct. 20

The Aurora Borealis

HAVING read the two accounts of Aurora Borealis in this week's number of NATURE, I hope the following brief account of the very beautiful one that occurred here may not prove uninteresting. On Friday, the 14th Oct. at 8.15 p.m., I noticed a bright appearance towards the north-west, somewhat resembling the moon rising, and on going to the front of the house which faced the north, saw that the whole of the horizon from west to south-east was lit up with a bluish white light. Gradually long streaks of the same colour stretched themselves up almost to the zenith, and then a blood-coloured light formed the higher portions, while the lower kept the bluish white colour already noticed.

Then appeared in the west a long broad band reaching to the zenith, consisting of a number of narrower bands, alternately red and bluish white, and through this the stars could be seen in their natural colour. At 9.30 the blood-red colour of the higher parts had almost disappeared, and long narrow streaks of yellowish white light extended up from the horizon.

At the lower part of the whiter light, in a northerly direction, appeared shadows having a somewhat rectangular form. (The town lies just in front of where I was standing, so that these shadows may have been caused by the ascending smoke.)

At 10.30 the red colour remained only in the west, and a narrow arc of bluish white light extended from north to about north-west, at about 25° above the horizon. The air was very calm, there being but a slight movement from the north-west. The moon was shining brightly all the time. Not having access to magnetic instruments I am unable to state how they were affected. On the 12th the barometer had fallen suddenly to 28·5°, and a violent westerly wind prevailed all day.

Dublin, Oct 18 T. W. PHILIPS

THIS evening, October 24, occurred one of the grandest displays of auroral lights which has probably ever been witnessed in these latitudes.

As I was, at a few minutes after seven o'clock, passing through the Observatory with the intention of observing with the heliometer, my attention was attracted by the brightness of the northern portion of the sky. On going out into the North Garden, I perceived that this was due to a general illumination of the sky of about that intensity which is produced by the rising of the full moon on the sky immediately above it, the moon itself not being visible. The contrast between this white illuminated sky, and the deep ordinary blue on the south side of the zenith, was very striking, the two portions being with moderate accuracy separated by the prime vertical.

On the south side of the zenith was observed what appeared to be an illuminated cloud, extending nearly from the zenith in a south-easterly direction for about forty degrees. Finally, there was at this time a well-defined arch of light, corresponding pretty accurately in position with the equator, and visible from east to west nearly to the horizon; and, beneath this arch, the sky was unusually dark, the darkness not being due to cloud or mist, as the stars were seen with their usual distinctness.

There was a small tendency at intervals to a display of streamers, but they were not conspicuous. From these phenomena I was led to expect, in the course of the evening, a grand display of aurora, and I was quite prepared for the summons, which I received from Mr. Keating, the assistant on duty, at eight o'clock, to come down and witness it.

The spectacle at this time was most magnificent. The northern portion of the heavens was nearly covered with crimson light of great intensity, and the sight was so fine that, for a few moments, I was occupied only with the admiration which it excited.

On proceeding to observe it more particularly, I saw that it consisted mainly of two large sheets of crimson light, one chiefly on the east side of the sky and the other on the west.

The eastern sheet extended generally from Polaris to Capella towards the zenith, which it did not, however, at this time quite reach. The most brilliant sets of streamers had their centres passing through these stars, and, after a few minutes, the extreme eastern portion was tolerably well defined by Perseus and Cassiopeia.

The western sheet was equally well defined, as lying between α Lyræ and α Aquilæ, but its brilliancy and the rapid change produced by the streamers were inferior to those exhibited in the other portion at the time when I observed it.

It is also worthy of remark, that the two portions seemed to be connected by an illuminated fleecy or cirrus cloud a little south of the zenith. This apparent cloud was, I believe, also a portion of auroral light, as I examined it a few minutes afterwards when it exhibited more of the auroral character.

At this time the portion of the eastern sheet, which had passed through Polaris, became separated from its more eastern portion passing through Capella, and formed a distinct sheet, while the western sheet was apparently drifting still more towards the south-west.

In a few minutes the intensity of the light diminished rapidly, and, as it was fading, my attention was attracted to a very beautiful feature in the phenomena exhibited. Just below the red light of the most western position, was a most brilliant bluish white light vaguely defined but very intense. It was most probably pure white, the bluish appearance being the effect of contrast with the red. The arch which I had observed earlier in the evening was now much brighter, and extended in the direction of the equator to the eastern portion of the heavens, where there was soon a similar effulgence of white light, but not quite so intense.

The darkness of the sky (perfectly free from cloud) beneath the bright arch was now much more conspicuous than it had been in the earlier part of the evening.

In a few minutes the whole faded away, and, excepting some small remains of the phenomena in the north, nothing unusual was visible. The grandest part of the phenomena continued for about half an hour, that is, from eight till half-past eight o'clock. Later in the evening a tolerably bright bank of auroral light was visible above the Northern horizon, and another brilliant display occurred, as described by Mr. Lucas.

For a considerable time during the first display, Mr. Lucas was watching from the tower of the Observatory, where he commanded a full view of the northern half of the heavens, and saw, at about $8^h\ 15^m$ to $8^h\ 20^m$, an ill-defined dark segment along the north horizon, from which white streamers issued through the whole extent, very much resembling an ordinary aurora, but which might be easily passed over in the grandeur of the display.

Mr. Lucas watched from 10 o'clock, but saw nothing except the white light extending under the Great Bear, till $10^h\ 30^m$, at which time white streamers shot up to Polaris and Beta Cephei, changing to intense red. At $11^h\ 10^m$ two sets of streamers appeared, one near Alpha and Beta Ursæ Majoris, and the other a little to the east of Beta Draconis, the former going eastward to *h* Ursæ Majoris, and the latter westward past Gamma Draconis. These were succeeded by some at Alpha Lyræ, combining with the last mentioned, and the mass, of an intense red colour, travelled slowly to Alpha Aquilæ, where it remained for a considerable time, as did that of Ursa Major, while the part of the heavens appeared perfectly clear. At $11^h\ 30^m$ streamers again shot up at Beta Ursæ Minoris, and between Gamma Draconis and Alpha Lyræ for a short time, and a few faint indications were visible till a little after 12, when the sky appeared to have regained its usual appearance.

From the relation of others it appears that another brilliant display took place still later in the night.

R. MAIN,
Oxford, Oct. 26 Radcliffe Observer

HAVING occasion to leave my house at 6.40 (the time given is Greenwich mean time throughout, and the bearings and directions were estimated from the pole-star, not compass) this evening, I was immediately struck with an unusual amount of uniformly diffused white light in the west and north-west. In a very few minutes a band of the same colour, but brighter, appeared, extending continuously across the sky, from W.S.W. to E.N.E. It was sharply defined, somewhat variable in breadth, but commonly about 6°. It lasted for nearly a quarter of an hour, and gradually disappeared, from the east westward. The diffused light, previously mentioned, remained unaffected. There were a few clouds in the south, but none elsewhere.

From 7.15 I went out of doors at intervals of a quarter of an hour to observe, but saw nothing but the diffused light already spoken of, which seemed to have acquired a greenish tinge, until 8 o'clock; when there was a band, broader than that first noticed, and of fiery rose colour, which extended from due east or not more than a degree or two north of it, towards the west, becoming gradually fainter in the latter direction. In a very few minutes the band became continuous from east to west, from 10 to 12° broad, and at the meridian, where it culminated, about 54° above the horizon at its upper edge, as estimated from the immersion of the pole-star within it, and which was occasionally much obscured by it. The red colour was equally pronounced in the east and in the west, but less so at the meridian and for about 15° on each side of it. Below the band, from N.W. to N.E. the sky was free from the red colour, but still retained the greenish diffused light. Soon after the formation of the complete band, streamers, varying from white to various degrees of red, shot up from every part of the northern half of the horizon towards the zenith, beyond which some of them extended. Those from the north, chiefly of white light, crossed the band at right angles, whilst the others cut it more and more obliquely according as they were nearer to the east and west. I watched it continuously with a large party of friends until something after half-past eight, and then left it in full vigour. Before nine it had in great measure disappeared, but the diffused white light still lingered, gradually becoming more and more limited in extent, though remaining equally pronounced, until 10.30; when it occupied the small segment of the sky comprised within about 5° on each side of the north point of the horizon, and from 6° to 7° above it.

Lamorna, Torquay, Oct. 24 W. PENGELLY

THE AMERICAN GOVERNMENT ECLIPSE EXPEDITION

AT the last session of the Congress of the United States of America, an appropriation of 6,000*l.* was made for the observation of the Total Eclipse of the Sun, under the direction of Professor Benjamin Peirce, the Superintendent of the U.S. Coast Survey. This generous act of legislation was suggested by one of the ablest statesmen of America, the Hon. John A. Bingham, of Ohio, and passed both houses unanimously.

An officer was immediately sent to examine the various places, and obtain all the local information which might be required to select the most favourable positions for observation. The expedition has been divided into two parties, each of which consists of about twelve persons. One party is under the immediate direction of Prof. Peirce, and will observe in Sicily; and the other is under the direction of Prof. Winlock, the director of the Observatory of Harvard University, and will observe in Spain. Almost all the astronomers of the expedition were upon the central path of the great eclipse which occurred in America in August 1869, so that they have already been under fire, and are prepared for the sudden outburst of the total obscuration.

The observations for precision will be entrusted in each party to an experienced officer of the survey, who will be upon the ground at least a fortnight before the eclipse. He will have the instruments all properly mounted and protected, the time well observed, and the arrangements made so that the principal observers of the physical phenomena may find everything in readiness when they arrive. Their presence will not, therefore, be required till within a few days of the eclipse. The officers upon whom this duty has devolved are Mr. Schott and Mr. Dean, assistants of the Coast Survey.

The spectroscopic observations have been chiefly arranged by Professor Winlock, assisted by Professors Young and Morton. New and peculiar methods have been prepared for preserving a record of the lines of the spectrum for subsequent measurement and discussion.

The photographic preparations are varied and original. The party of Prof. Peirce will have photographic apparatus prepared by Mr. Rutherford of New York, with lenses especially ground for the purpose under his direction by Fitz of New York, and young Fitz will himself superintend this portion of the observations. The party of Prof. Winlock will have its photographic apparatus prepared, under the directions of the Professor, by Clarke, of Cambridge, and will use lenses ground by Clarke. Alvan Clarke, Jun., will also assist in these observations. Prof. Winlock's new method of photographing the sun through a long tube will be used in a portion of this class of observations. In both parties arrangements are made for long and short exposures in different instruments during the period of totality.

The polariscopic observations will be made by Prof. Pickering in the party of Prof. Winlock.

General observations of the corona will be made by as many of the party as possible, and it is hoped that Steinheil's hand comet-seekers will be especially available for this class of observation. Hand spectroscopes will also be used by several of the party, and it is hoped that in the preparations for this portion of the service material assistance will be derived from Mr. Lockyer's suggestions.

It is worthy of notice that two of the ablest officers of Engineers of the United States' Army have been detailed by the War Department to accompany the Expedition. They are Major Abbott, whose name is familiar to hydraulic engineers through his connection with General Humphrey's Monograph upon the Mississippi river, and Captain Ernst. B. P.

DR. W. ALLEN MILLER

WE have already referred to the lamented death of Dr. W. A. Miller, and now give a short sketch of his life. Dr. Miller was born at Ipswich in 1817, and received part of his education in Merchant Taylors' School. He obtained, however, his first insight into chemistry in a school belonging to the Society of Friends, at Ackworth, in Yorkshire. At the age of fifteen he was apprenticed to his uncle, who was surgeon to the General Hospital at Birmingham, and at the age of twenty he entered King's College, where (we quote from an obituary notice in the *Chemical News*) his knowledge of chemistry attracted the attention of Professor Daniell, who, during the illness of the laboratory assistant, engaged his services. In 1840 he visited Germany, and passed some time in Liebig's laboratory at Giessen. In the same year he was appointed to the post of Demonstrator in the Laboratory of King's College. In 1841 he became Assistant Lecturer to Professor Daniell, and also took his degree of M.D. in the University of London. He also assisted Professor Daniell in various scientific inquiries, and conducted the experiments on the electrolysis of saline compounds, his name being associated with that of Daniell in the paper that appeared in the *Philosophical Transactions* for 1844, In the following year he became a Fellow of the Royal Society, and on the death of Professor Daniell succeeded to the vacant chair. At this period he became greatly interested in the subject of spectrum analysis, in which he worked with great activity as an observer of the various phenomena which were then attracting the attention of the scientific world. He was a member of the Council of the Royal Society from 1848 to 1850 and from 1855 to 1857, being elected treasurer in November 1861, thereby becoming vice-president of the society. About this time his highly-trained mind and great knowledge were utilised to the highest degree in a joint research with Mr. Huggins on the spectra of stars and nebulæ, and in this class of researches his loss will be as severely felt as it will be at King's College, the Council Board of the Royal Society, and other places where his calm and sound judgment was conspicuous.

Professor Miller received the degree of LL.D., Edinburgh; of D.C.L., Oxford; and of LL.D., Cambridge. He also received the gold medal of the Astronomical Society, in conjunction with Mr. Huggins. At the time of his death he was a member of the Royal Commission which is now considering the whole question of scientific instruction and the advancement of science. His contributions to scientific knowledge, beyond those we have mentioned, were not large, his time being much taken up, as is the case with too many of our best scientific men, with teaching. His "Elements of Chemistry" is a valuable work which has long been favourably known, and has gone through several editions.

AUGUSTUS MATTHIESSEN

THE sad death of Dr. Augustus Matthiessen, which we briefly referred to in a previous number, has bereft English chemical and physical science of one of the most arduous and successful workers who ever entered her ranks. Born January 1831, in London, he from early youth upwards, manifested a great liking for chemistry, but it was not until he came of age that he entered upon its study in earnest at the University of Giessen, where he subsequently took his doctor's degree, and afterwards at Heidelberg, where, for nearly four years, he worked under the guidance of Bunsen and Kirchhoff. His first paper, "On the Preparation of the Metals of the Alkalies and Alkaline Earths by Electrolysis," appeared in the *Annalen der Chemie und Pharmacie* for March 1855, and was devoted to a description of the preparation and properties of the metals calcium and strontium, then isolated for the first time. Calcium he found to be a metal of the colour and glance of bell metal, exceedingly ductile and malleable; using water as the exciting fluid, he found it to be electropositive to magnesium, and electronegative to sodium and potassium, which at once explained why it could not be obtained from its chloride by the action of sodium or potassium at high temperatures. Next in order is a paper of his in Poggendorf's *Annalen* for 1857, communicated by Kirchhoff, in whose laboratory the results were worked out, entitled, "On the Electric Conductivity of Potassium, Sodium, Lithium, Magnesium, Calcium, and Strontium." Following this, appear in Poggendorf's *Annalen* for 1858 two communications from him "On the Electric Conductivity of Metals," and "On the Thermo-electric Series." On his return to London he worked some time at the Royal College of Chemistry under Hofmann, and published a paper "On the Action of Nitrous Acid on Aniline." Hunt had described phenol, free nitrogen and water as the products of this reaction, but he found that an intermediate reaction took place, by which ammonia was formed; extending his experiments to ethyl and diethylaniline, he obtained ethylamine and diethylamine. It was this reaction which first led him to the study of narcotine, which afterwards in his hands yielded such splendid results. After working diligently several years in a laboratory which he fitted up for himself in Torrington Square, he was appointed Professor of Chemistry to St. Mary's Hospital in 1862. It was about this period that his most important researches were carried out in conjunction with Dr. Vogt, Von Bose, Holzmann, &c., and published in a series of papers in the Philosophical Transactions of the Royal Society, to which he was admitted a Fellow in 1861. Some of the most important of these papers are those "On the Influence of Temperature on the Electric Conducting Power of Metals." It was this research which proved the important fact, that the conducting power of the pure metals decreased to the same extent between 0° and 100° C.; two remarkable exceptions, however, to this law, Iron and Thallium were the subject of a later paper; "On the Specific Gravity of Metals and Alloys;" "On the Chemical Nature of Alloys," in which he showed that nearly all the two-metal alloys may be considered as solidified solutions of the one metal in the other. Also a long series of determinations of the influence of temperature on the conducting power of alloys. He also made a most careful redetermination of the expansion of water and mercury, and found that Kopp's coefficients were slightly too low. He was a very active member of the committee appointed by the British Association "On the Standards of Electrical Resistance," and it was one of the alloys discovered by him which was finally adopted for the reproduction of the now well known B A unit of electrical resistance. His later chemical work is embodied in a series of papers in the *Philosophical Transactions*—"On the Chemical Constitution of Narcotine"—published partly in conjunction with Prof. Foster, and partly with Dr. Wright. In these he shows that one, two, and three atoms of methyl can be successively removed from narcotine, and also describes a large number of interesting derivatives of the same. In 1869 he was appointed Professor of Chemistry in St. Bartholomew's Hospital, and in the same year received the Royal Society's Gold Medal for his published researches on the metals and the opium alkaloids. One of the most important results of his last investigation is the discovery of the relation between morphia and codeia, the latter simply containing one of methyl more than the former; although, however, he succeeded in obtaining apomorphia from codeia, he was never able to reconvert apomorphia into morphia, and thus form morphia direct from codeia. At the time of his death he was occupied with the experiments on the chemical nature of pure cast-iron, of the Committee appointed to inquire into which he was a member, and also with experiments with a view to determine whether the specific heat of platinum was constant at high temperatures, and if so, to employ it in the construction of a standard pyrometer. He was also prosecuting his researches on the opium bases, and had already arrived at interesting results, which we believe will shortly be published. All the beforementioned researches display an enormous amount of manipulative skill, and there is little doubt that his success was mainly due to the wonderful acuteness of his powers of observation, and also to his great perseverance; but it is indeed surprising that, labouring under the physical disadvantages he did, he should have been able to attain such ends.

At a time when England can least afford it, she has lost one who had not only done a vast amount of valuable work, but who, there was every prospect, would do as much more in the future.

BRITISH EDIBLE FUNGI

MUSHROOMS and their congeners seem never have been in good repute since Agrippina employed one of the tribe to poison her husband, and Nero with villanous pleasantry called it the "food of the gods." With proverbial tenacity the bad name thus incurred has clung to the whole family of agarics, and what within certain limits might be called a wholesome dread has become a deep-rooted and irrational prejudice, excluding from popular use a really valuable class of vegetable esculents. We cannot altogether go along with those enthusiastic mycophagists who recognise a substitute for meat in every edible fungus, and dilate on the ozmazome and other nutritious properties of the tribe; but we readily acknowledge that their merits as secondary sources of food-supply have hitherto been unduly neglected. The great difficulty always felt in advocating the claims of the class to more extensive use has arisen from the want of some definite rules, some formula at once simple in expression and universal in application, by which to distinguish the noxious from the innocent members. Pliny, in his Natural History, goes so far as to say that the first place amongst those things which are eaten with peril must be assigned to agarics, and he expresses his surprise at the pleasure which men take "in so doubtful and dangerous a meat." But his observations show that fungi of all sorts, including even such growths as the *Fistulina hepatica*, were known to his countrymen and eaten by them without scruple. Indeed, in one particular the wisdom of the ancient Romans seems to have been superior to that of their descendants, for, while Horace lays down the rule—

> Pratensibus optima fungi
> Natura est; aliis male creditur—

the modern Ædiles of the Roman market condemn to instant destruction every specimen of the meadow mushroom (*A. campestris*) which comes within their reach. Although, however, it is not always easy to distinguish the wholesome from the unwholesome fungus, and the organs of sight and smell require some training before they can be wholly trusted in the matter, yet the dangers have been greatly exaggerated, and, as a matter of fact, hogweed is more often mistaken for parsnip and aconite for horse-radish than are *Boletus sátanas* and *Amanita verna* for their innocent brethren. No better opportunity for engaging in the study of this branch of natural history could be found than that which the present season affords; and if the treatises of Mr. Berkeley, Dr. Badham, or Mr. Worthington Smith be not at hand, the following notes on the chief edible fungi which are now to be met with may prove acceptable to some of our readers.*

With the ordinary meadow mushroon *(A. campestris)* and its near relative the horse mushroon (*A. arvensis*), every one is familiar, and both of them have occurred in profusion this autumn. Against the latter an unfounded prejudice prevails in some districts, but its larger size and coarser texture require only a little extra cooking to develop the flavour and correct indigestibility. In spite of all that has been said to the contrary, we maintain that these agarics are entitled to the first place, and for the second much rivalry exists between the orange-milk mushroom (*Lactarius deliciosus*) and the Parasol Agaric (*Agaricus procerus*). Both are readily distinguishable, and may be eaten with equal impunity. The former is chiefly found in plantations of Scotch fir and larch, is of an orange-brown colour, and firm flesh, and yields, when bruised, an exudation of orange-red milk, which turns green after a few minutes' exposure. The latter is common in pastures, and may be recognised by its tall habit, the stalk gradually enlarging at the base, the umbo of a brownish colour with spots or patches, and the gills white and unconnected with the stem. The plum

FIG. 1.—*Lactarius deliciosus* (Orange-milk Mushroom). Under fir-trees, in autumn; colour, brown-orange; milk at first orange, then green; diameter, 3 to 10 inches.

mushroom (*A. prunulus*) is for the autumn months what the St. George's mushroom (*A. gambosus*) is for the spring —a large fleshy fungus, delicate in flavour, though not so choice as the *Orcella*, for which it is often mistaken. It is to be found in shady places pretty generally throughout England, and is conspicuous from its whiteness. The gills are close together and of a pale rosy hue, and the smell of the plant has been compared to that of fresh meal.

We must mention two other fungi, common enough and easily recognised, but of their culinary virtues we do not entertain a very high opinion. These are the puff-ball, and the maned agaric (*Coprinus comatus*). The former needs no description, and perhaps others may be more fortunate than we have been in detecting the latent flavour of omelette which it is said to possess. The latter is called by Dr. Bull the "agaric of civilisation." We have met with it in farmyards, on lawns, on railway-cuttings, and, in fact, in nearly every waste place. It looks like an attenuated cocoon, snow-white at first, but gradually changing in colour and splitting upwards in a dozen places. The gills, white at first, become pink and then black; the last stage, which is very quickly reached, presaging the immediate dissolution of the plant, which gradually deliquesces into an inky-black fluid.

It would be easy to amplify this list, but we desire to avoid all risk of confusing the tyro's mind with too many details, and have purposely confined our remarks to those fungi which belong to the autumn season.

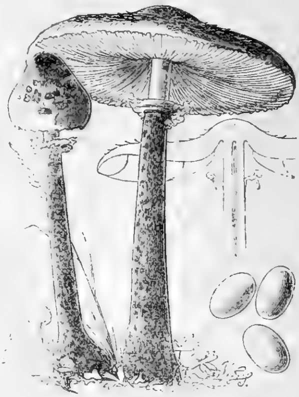


FIG. 2.—*Agaricus procerus* (Scaly Mushroom). Pastures, &c., in autumn colour, pale brownish buff; diameter, 5 to 12 inches.

One caution must be added. All agarics are more wholesome fresh than stale, and with some the neglect of this rule may lead to unpleasant consequences. It is rigidly enforced in the Roman market, where all specimens which are "muffi, guasti," or "verminosi" are seized and thrown into the Tiber, and it should be distinctly understood in every English kitchen into which even the common mushroom is allowed to enter. The fungus which to-day successfully simulates a sweetbread, may tomorrow simulate with equal success a handful of snuff.

FIG. 3.—(1) *Agaricus orcella* and (2) *Agaricus prunulus* (Plum Mushroom Woody places, in autumn; colour, snow white, with pale rose gills; diameter, 2 to 4 inches.

Our illustrations are taken from Mr. W. Robinson's admirable little manual, to which we have already alluded, and are from the facile pencil of Mr. Worthington Smith. Here will be found also instructions for cooking all the most common edible species.

C. J. ROBINSON

* At the conclusion of "Mushroom Culture, its Extension and Improvement" (London: Warne, 1870), Mr. W. Robinson gives some useful information, derived chiefly from the above authorities, and from the Proceedings of the Woolhope Field Club.

NOTES

DESIROUS of aiding the English Eclipse Expedition, Prof. Peirce has addressed the following letter to Mr. Lockyer. It is to be hoped that observers will take advantage of the opportunity so magnificently afforded them :—

"Fenton's Hotel

"MY DEAR SIR,—I have been directed by the Government of the United States to have the best possible observations made of the total eclipse of next December. If I could aid the cause of Astronomy by assisting the observers of England in their investigation of this phenomenon I should be greatly pleased. I take the liberty therefore to invite your attendance, and also that of other eminent physicists of England, with either of the parties of my expedition, one of which will go to Spain and the other to Sicily.—Yours very respectfully and faithfully,

"BENJAMIN PEIRCE

"J. Norman Lockyer, Esq., F.R.S."

Of course it would have been better had English observers, who have devoted their attention to solar physics, gone out under the English flag; but science is of no country, and they may well be proud to join such a distinguished corps as that with which they are asked to associate themselves.

WE are also informed that arrangements are being made for a deputation to urge on Mr. Gladstone, as a last resource, the importance of not abandoning the Eclipse Expedition, and to point out the especial inopportuneness of such a course at a time when neither France nor Germany can send out expeditions, and when, if we withdraw, the whole burden of observation will fall upon America. It will be remembered that the reason given by the Admiralty for refusing the loan of a Government ship was "that Parliament did not place money at the disposal of the Naval Department for such purposes." If this should be the only difficulty with the Admiralty, it will not be hard for the Deputation to find precedents, from the time of Captain Cook downwards, for sending out such expeditions. It is to be hoped that Mr. Gladstone may take a more liberal view of the subject than the Lords of the Admiralty have done, and that the leaders of science in this country will show themselves less supine in the matter than they have hitherto done.

WE are informed that the work done by Dr. Carpenter in the *Porcupine* expedition of the present year has satisfied him of the justice of the views advanced in his reply to Prof. Wyville Thomson's lecture, as to the over-estimation of the heating and moving action of the real Gulf-stream. It is Dr. Carpenter's intention to bring his views on this subject before the Royal Geographical Society.

THE Science and Art Department of the Committee of Council on Education have published a second edition of their Directory, with regulations for establishing and conducting Science Schools and Classes, superseding those previously printed.

THE amount of interest shown in Natural Science at Oxford cannot be better illustrated than by noticing the long list of lectures which are to be given this term. These, exclusive of the purely mathematical, are as follows :—1. University Lectures: Professor Phillips, F.R.S., "On the Composition and Structure of the External Parts of the Earth." Professor Lawson, "On the Minute Anatomy of Plants." Professor Rolleston, F.R.S., "On Anatomy and Physiology." Professor Pritchard, F.R.S., "On Astronomy." Professor Clifton, F.R.S., "On Elementary Statics," and a continuation of his lectures "On Heat." Mr. Wyndham, for the Professor of Chemistry, "On Elementary Principles of Chemistry." The Professor of Zoology will give assistance to all who are working at the Articulata, the collection of which he is arranging. All these are free public lectures, and are illustrated by experiments, and are all largely attended. 2. College Lectures, &c. : Mr. A. V. Harcourt, F.R.S., Lees Reader in Chemistry, "On the Chemistry of the Metals," at Christ Church. Mr. A. W. Reinold, Lees Reader in Physics, "On Hydromechanics and Heat," at Christ Church. Mr. J. B. Thompson, Lees Reader in Anatomy, "On Comparative Osteology," at Christ Church. Mr. Heathcote Wyndham, "On Chemical Philosophy," at Merton. Mr. Abbay, "On Elementary Physics," at Merton. Mr. Chapman, "On Anatomy and Physiology," at Magdalen. These lectures are open to all other Colleges on payment of a small fee.

MAGNIFICENT displays of the Aurora Borealis have been witnessed in London on two nights of the present week, Monday the 24th, and Tuesday the 25th inst. It will be interesting to hear from distant subscribers the extent of the area over which the phenomenon was visible. In addition to the letters printed this week, we have received others describing the display from C. Pocklington, Poole; E. C. Walker and T. H. Waller, York; E. Dukinfield Jones, Fermoy, Co. Cork, and others. *Apropos* of one of these displays, a correspondent of the *Pall Mall Gazette* thus states the view taken of it by the inhabitants of a little village through which he passed :—"They were all standing outside their houses gazing at the heavens. 'There is France for you,' said one of them to me as I approached him. I requested an explanation, and found that not only he but all his neighbours attributed the blood-red light in the sky to the burning of Paris. 'Gad, how it blazes!' I heard a man remark. 'They're a gettin' it hunder now,' said another; and so on through all the village. At a garden gate of nearly the last house I observed a respectable-looking man with a telescope, with which he was rolling the sky: 'It is rum,' he said to me, 'and very sublime; but the d——d asses, I can't make 'em believe it is only the Southern Cross.' I rather think he was the schoolmaster of the parish."

PROF. PETERS, of Clinton, New York, announces the discovery of a new planet (No. 112) on September 19th, of the eleventh magnitude, to which he gives the name Iphigenia. The following are given as the elements of planet No. 111 (Até) :—

Epoch, 1870, Sept. 0, 0 mean Berlin time

M	=	205°	17′	21″·0	
π	=	122	53	7·3	mean eq. 1870
Ω	=	306	26	28·4	mean eq. 1870
i	=	5	1	21·4	mean eq. 1870
ϕ	=	5	49	10·6	
μ	=			858·392	
log. a	=	0·4108808			

The planet is now nearly stationary, and is of the twelfth magnitude.

PROFESSOR LUTHER, of the Königsberg Observatory, records in the *Astronomische Nachrichten* the death, at the early age of 26, of Dr. F. Tischler, Observer in the same Observatory. Dr. Tischler was born at Breslau in 1844, and after pursuing his studies at Königsberg and Bonn, and publishing a treatise on the path of Tuttle's comet, was appointed Observer at the former place in 1867. At the outbreak of the Franco-German war, he obeyed a summons to serve in the Prussian infantry, and was seriously wounded in the battle before Metz, on August 14th, and succumbed to his injuries on September 30th.

THE first meeting of the Zoological Society for the session will take place on Tuesday, the 1st of November, when the following papers will be read :—1. The Secretary: "On Additions to the Society's Menagerie." 2. Prof. W. Peters, F.M.Z.S.: "Contributions to the knowledge of *Pectinator*, a genus of Rodent Mammalia from North-Eastern Africa." 3. Mr. C. Darwin, F.R.S.: "Note on the Habits of the *Chrysoptilus campestris*."

THE first meeting for the session of the Chemical Society will be held on Thursday evening, Nov. 3, when Mr. A. H. Elliott will read a paper "On the Analysis of Cast-iron."

THE first meeting of the Linnæan Society will be held on Thursday evening, November 3, when papers will be read by Dr. Mansel Weale "On the Fertilisation of Orchids and Asclepiads," and "On a Solitary Bee from South Africa."

THE forthcoming exhibition of the Photographic Society of London will be inaugurated by a *Conversazione* open to members and their friends, to be held at the Architectural Gallery, 9, Conduit Street, on Thursday evening, Nov. 8, at seven o'clock. The Exhibition will remain open during the remainder of November from 9 A.M. till dusk daily. Intending exhibitors are requested to send in their works not earlier than Nov. 1, nor later than Nov. 3.

WE shall be happy, in accordance with a suggestion from a correspondent, to insert (gratuitously) in our advertisement columns, a list of "Scientific books wanted" by any of our subscribers who are unable to obtain them through the ordinary channels.

THE following lectures will be delivered during the ensuing season at the London Institution, Finsbury Circus. Educational Courses—Mondays, at Four. Eight lectures on Chemical Action, by Professor Odling; Oct. 31; Nov. 7, 14, 21, 28; Dec. 5, 12, 19, 1870. Six lectures on the First Principles of Biology, by Professor Huxley, Jan. 23, 30; Feb. 6, 13, 20, 27, 1871. Eight lectures on Astronomy, by R. A. Proctor; March 6, 13, 20, 27; April 3, 17, 24; May 1, 1871. Conversazione lectures —Wednesdays, at half-past seven, On Dust and Disease, by Professor Tyndall, Dec. 21, 1870. On Alizarine and other Colouring Matters, by W. H. Perkin, Feb. 15, 1871. Stained Glass æsthetically considered with reference to Modern Art, by Henry Holiday, March 15, 1871. Evening courses—Thursdays, at half-past seven. Two lectures (with instrumental music) on the Acoustics of the Orchestra, by Dr. W. H. Stone, Nov. 10, 17, 1870. Two lectures on Precious Metals and Precious Stones, by Professor Morris, Nov. 24; Dec. 1, 1870. Two lectures on Count Romford and his Philosophical Work, by W. Mattieu Williams, Dec. 8, 15, 1870. Two lectures on Music, Characteristic and Descriptive, by John Ella, Jan. 12, 19, 1871. Four lectures on the Action, Nature, and Detection of Poisons, by F. S. Barff, Jan. 26; Feb. 2, 9, 23, 1871. Six lectures on Economic Botany, or Vegetable Substances used for Food, and in the Arts and Manufactures, by Professor Bentley, March 23, 30; April 6, 20, 27; May 4, 1871.

A COURSE of three lectures (on Nov. 4th, 7th, and 8th) will be delivered at the Gresham College, Basinghall Street, by Dr. E. Symes Thompson, on the Organs of Respiration in Health, on Hay Fever, and on the Respiratory Organs in Disease. The lectures, which are illustrated by diagrams and chemical experiments, are free to the public, and commence each evening at 7 o'clock.

MR. VAN VOORST announces the following works in preparation:—"The Natural History of the British Diatomaceæ," by Arthur Scott Donkin, M.D. "Heads of Lectures on Geology and Mineralogy," delivered at the Royal Military College, Sandhurst, by Prof. Rupert Jones. "The Ornithology of Shakespeare," by J. E. Harting. "The Natural History of the Azores," by F. DuCane Godman, F.L.S. A fourth edition of "Yarrell's British Birds," edited by Prof. Alfred Newton. A fourth edition of Prof. Rymer Jones's "Organisation of the Animal Kingdom." "Dr. Bevan on the Honey Bee," a revised and enlarged edition, by W. Augustus Munn. "Prof. Frankland's Lecture Notes for Chemical Students." Vol. 2 (Organic Chemistry).

THREE hundred bags of a remarkable-looking seed, new to British commerce, have recently been brought into this country as an oil seed; they were shipped from Lisbon to Liverpool, but are believed by the Liverpool merchants into whose hands they have been delivered to have come originally from the east coast of Africa. Mozambique is, in all probability, the port from whence they were first shipped, seeing that they are the seeds of *Telfairia pedata*, a tall climbing cucurbitaceous plant, native of the coast opposite Zanzibar. These seeds have somewhat the colour and appearance of almonds, but they are flat, nearly circular, and about 1½ inches across; they are covered with a very closely reticulated net-work of woody fibre, and the kernel is about the colour and hardness of that of a Brazil nut and contains a large quantity of oil, which is probably intended for use in this country as a culinary oil. The kernels, however, are of a rank, bitter taste, though they are stated in books to be as sweet as almonds. The fruit is very large, and is said frequently to contain as many as 250 seeds. Two species only are known of the genus, the one under consideration and *T. occidentalis*, native of the opposite or West Coast of Africa.

WE are glad to call attention to the fact that the *American Journal of Science and Arts*, which has from its commencement been the leading vehicle for the original papers of the scientific men of America, will be continued after the close of the present year as a monthly journal. This increased frequency of publication will, it is believed, meet a wish often expressed by authors for a more rapid interchange of views, and an earlier knowledge of the progress of research; and the editors hope that the friends and patrons of science will aid in promoting its wider circulation. We believe that there are many public and private libraries, and reading-rooms, throughout the country, which are not yet supplied with this journal, which is certainly one of the most important of existing scientific publications.

THE bastion in front of Fort Bicêtre, known as Bastion No. 87, is manned by the members of the Ecole Polytechnique. The professors of the college have consented to serve under their former pupils, wherever these have been selected as lieutenants. In this bastion may be seen MM. Bertrand, Bonnet, Langier, Frémy, Tissot, Laguerre—all members of the Institute, professors at either the College de France or at the Sorbonne—daily at their posts in the bastion, which has already acquired the reputation of being one of the best mounted among the fortifications of Paris.

THE Council of the Institution of Civil Engineers print a list of 38 subjects, respecting which they invite communications, as well as upon others; such as (1) Authentic Details of the Progress of any Work in Civil Engineering, as far as absolutely executed (Smeaton's Account of the Eddystone Lighthouse may be taken as an example); (2) Descriptions of Engines and Machines of various kinds; or (3) Practical Essays on Subjects connected with Engineering, as, for instance, Metallurgy. For approved Original Communications, the Council will be prepared to award the Premiums arising out of special funds devoted for the purpose.

THE shock of an earthquake was felt last Thursday at 11·30 A.M. both in the United States and in Canada. The motion, accompanied by a loud rumbling noise, was felt at Boston, New York, Montreal, Toronto, and St. Katherine, and throughout the provinces of Ontario and Quebec. The shock lasted twenty seconds, and appeared to travel eastward.

THE towns of Reading and Maidstone have been among the earliest to throw themselves into the new movement in favour of Technical Education. Classes in various departments of Natural Science are being formed at both places.

PROF. ZOELLNER ON THE SUN'S TEMPERATURE AND PHYSICAL CONSTITUTION *

AMONG the characteristic forms of the protuberances † the observation of which the spectroscope with widened slit has rendered possible at all times, is to be found a not inconsiderable number of such, whose appearance at once conveys the conviction to every impartial observer that we have here to deal with vast eruptions of incandescent hydrogen masses.

It is probably impossible, without quitting the range of known analogous occurrences and at the same time the conditions explanatory of cosmical phenomena, to assume any other cause of these eruptions than the *difference of pressure* of the issuing gas in the interior and on the surface of the sun. The possibility of such a difference of pressure presupposes, necessarily, the existence of a separating layer between the inner and outer masses of hydrogen, the latter of which, as is known, form an essential constituent of the sun's atmosphere.

The assumption of the existence of such a separating layer is, at first sight of the above-mentioned protuberance phenomena, so cogent that it even forces itself as undeniable upon observers who, like Respighi, do not hold it to be improbable that electrical forces could be the cause of such eruptions.

Keeping, however, to the more simple and therefore more natural assumption of a difference of pressure, we have to deal with a phenomenon which, on the application of the mechanical theory of heat and gases, is capable of yielding most important information as to the temperature and physical constitution of the sun.

For perfect gases the mechanical theory infers from its premises,—firstly, the law of Mariotte and Gay-Lussac; secondly, the constancy of the relation of the specific heats at constant volume and constant pressure.

This constant, therefore, when determined according to known methods for a definite gas, must, from the point of view of the mechanical theory of gases, be considered, similarly to the atomic weight of a body, as invariable, and must on no account be placed in the category of other empirical constants, such as the conducting power of bodies for heat, or the co-efficient of expansion of solid and liquid bodies, &c. These constants only hold good within those limits within which they are determined by observation, and lose their significance when employed far beyond those limits.

Under this supposition I consider the eruptive protuberances as a phenomenon of the efflux of a gas from one space into another, the pressure in both spaces during the discharge being assumed constant, and neither a communication nor an abstraction of heat as taking place.

Let A be the heat-equivalent of the unit of work,
- v the velocity of efflux of the gas in the plane of the opening,
- g the intensity of gravity on the sun,
- x the relation of the specific heats of the gas at constant pressure and constant volume,
- c the specific heat of the gas at constant volume referred to an equal weight of water,
- t_i the absolute temperature of the gas in the inner space, from which the efflux takes place,
- t_a the absolute temperature of the issuing gas in the plane of the discharge opening,
- p_i the pressure of the gas in the inner space,
- p_a the pressure in the plane of the discharge opening.

Then, according to the dynamical theory of heat, we have, under the assumptions which have been made, the following two relations‡ between these nine magnitudes:—

$$A\frac{v^2}{2g} = xc\left(t_i - t_a\right) \qquad (1)$$

$$\frac{t_i}{t_a} = \left(\frac{p_i}{p_a}\right)^{\frac{x-1}{x}} \qquad (2)$$

Further, let a_1 be the mean height of the barometer in metres of mercury,
- ρ the density of the gas under consideration at the temperature of melting ice, and under the pressure a_1 on the earth's surface,
- σ the density of the gas contained in the inner space under the pressure p_i, and at the absolute temperature t_i,
- a the coefficient of expansion of the gas for 1° C.

Conformably to the law of Mariotte and Gay-Lussac, we then have also the following relation:—

$$\sigma = \frac{\rho}{a_1 a} \frac{p_i}{t_i} \qquad (3)$$

The pressure p_a in the plane of the discharge opening may, under the suppositions made, be considered as identical with the pressure which the atmosphere of the sun exercises at the *niveau* of the above-mentioned separating layer, *i.e.*, at its base.

Let, in this case,
- p_a be the pressure at the base of the atmosphere,
- h a certain height above the base,
- p_h the pressure at this height,
- t the absolute temperature in the atmosphere, which, in consequence of insufficient knowledge of the law of temperature is assumed to be everywhere constant,
- g the gravity of the sun at the base of the atmosphere,
- r the radius of the separating layer,
- ρ_1 the specific gravity of mercury at the temperature of melting ice,
- g_1 the intensity of gravity on the earth's surface,
- a_1 the mean height of the barometer,
- ρ the density of the gas composing the atmosphere at the temperature of melting ice, and under the influence of the quantities g_1 and a_1;

we then obtain, by a known method of deduction, the following relation,

$$\text{log. nat. } \frac{p_a}{p_h} = \frac{\rho g r h}{\rho_1 g_1 a_1 a t (r + h)} \qquad (4)$$

In order to combine this with the three previous equations, a double assumption must be made:

First, that the essential constituent of the sun's atmosphere, which produces the pressure p_a, consists of the same gas as escapes from the interior of the sun during eruptive protuberances.

Secondly, that the absolute temperature t of the atmosphere, may be considered as essentially agreeing with the absolute temperature at the *niveau* of the opening during the discharge.

Having regard to the object of the present memoir, I consider the admissibility of the first assumption as sufficiently established by observations, since the discovery of the so-called chromosphere has given the proof that the whole surface of the sun is surrounded by an atmosphere of hydrogen of very considerable extent.

The correctness of the second assumption I infer *from the luminosity of the base of all eruptive protuberances not differing to any extent from that of the chromosphere.* When it is considered that the constant mean temperature t in Formula (4), which, in consequence of the want of knowledge of the law governing the decrease of temperature, is substituted for the temperatures falling with the height h, evidently must correspond to a layer near the base,* this temperature becomes at the same time approximated to that belonging to the outer surface of the separating layer.

By virtue of the first assumption, the value ρ in Formula (4)

* *T. Zöllner. Ueber die Temperatur und physische Beschaffenheit der Sonne.* Berichte Kön. Sächs. Gesellschaft der Wissenschaften. Sitzung am 2 Juni, 1870.

† The forms of the protuberances may be divided into two characteristic groups—into the vapour- or cloud-like and into the eruptive formations. The preponderance of the one or the other type appears partly to be dependent on local conditions on the surface of the sun, partly on the time, so that at particular periods the one, at others the other, type preponderates. The striking resemblance of the cloud-like formations to terrestrial clouds is readily explained, when it is considered that the forms of our clouds are due, not to the particles of water suspended in them, but essentially to the nature and manner in which the differently heated and agitated masses of air are spread out. *The particles of aqueous vapour are, in terrestrial clouds, simply the material by means of which the above-mentioned differences between the masses of air are rendered evident to us. The glow of the incandescent masses of hydrogen is the cause of the visibility of the clouds of the protuberances.*

‡ Zeuner, Grundzüge der mechanischen Wärmetheorie. 2 Aufl. 1866, p. 165.

* With regard to the increasing density of the layers of air towards the base, the temperature introduced into Formula (4) must, apart from the special law for the decrease of temperature, always agree with the temperature of a layer which lies deeper than $\frac{h}{2}$. This difference, which, as a simple calculation shows, is in general a very considerable one, seems to me to be entirely disregarded in the barometrical estimation of heights, in which, as is known, the mean temperature of the two stations is made use of, and to give a simple explanation of certain periodical phenomena which have lately been urged.

becomes identical with the analogous one in (3), and, in consequence, of the second

(2.)

$$t = t_a$$

The theoretical foundations and essential assumptions necessary for the treatment of the phenomena in question on the sun's surface having been explained, a reconstruction and simplification of the above equations, more suitable to the object in hand, may well follow.

If H denote the height to which a body with the initial velocity v on the sun's surface is hurled up in a vertical direction, then, taking the diminution of gravity into account, we have:

$$v^2 = 2gH \frac{r}{r+H}$$

or,

$$\frac{v^2}{2g} = \frac{rH}{r+H}$$

This value substituted for $\frac{v^2}{2g}$ in Equation (1) gives:

$$t_i = \frac{rHA}{xc\,(r+H)} + t_a;$$

or taking $\frac{rHA}{xc\,(r+A)} = a$, and, according to our assumption, $t_a = t$, we obtain the following for Equation (1):

$$t_i = a + t \qquad \text{(I.)}$$

Further, let

$$\frac{x-1}{x} = \frac{1}{q}$$

$$\frac{\rho}{a_1 a} = b$$

$$\frac{g}{g_1 \rho_1} = m$$

The Equations (2) (3) (4) become then converted into the following:

$$\frac{t_i}{t} = \left(\frac{p_i}{p_a}\right)^{\frac{1}{q}} \qquad \text{(II.)}$$

$$\sigma = b\,\frac{p_i}{t_i} \qquad \text{(III.)}$$

$$p = p_h e^{bm\frac{rh}{(r+h)t}} \qquad \text{(IV.)}$$

In addition there is obtained from these four equations by elimination the following:

$$\sigma = \frac{b\,p_h}{a+t}\left(\frac{a+t}{t}\right)^q e^{bm\frac{rh}{(r+h)t}} \qquad \text{(V.)}$$

This equation, of course, expresses the density, σ, of the compressed mass of gas only as a function of the three magnitudes p_h, h, and t; if, therefore, under the assumptions made, three out of the four values considered can be determined by observation, either positively or within certain degrees, the fourth can then be determined. And in fact, partly by spectroscopic, partly by other, observations, fixed extreme values can be obtained for the magnitudes σ, p_h, and h, so that thus a limit for t, that is to say, for the outer hydrogen atmosphere in the neighbourhood of the incandescent liquid separating layer, is also obtained. This value substituted in Equation (1) the value of H being known, gives then at once a value for the inner temperature t_i, and from (III.) and (IV.) fixed values for p_i and p_a can be obtained with equal ease.

(3.)

Now to proceed, however, to the discussion of numerical values, I will commence with Formula (I.)

The lowest value which can be attached to t is evidently 0. We then obtain for the inner temperature t_i the minimum value:

$$t_i = a = \frac{rHA}{xc\,(r+H)} \qquad (5)$$

Having regard to the extreme tenuity, and therefore slight resistance, of the atmosphere even at a very moderate distance from the sun's surface, the value of H may, for the sake of simplicity, be put as equal to the mean height of the eruptive protuberances. A more detailed discussion of the conditions under which this is allowable will be given later.

Protuberances three minutes high are not of very rare occurrence; to keep, however, as close as possible within the limit of an estimation of mean value, I will assume H to be only 1 minutes.

Adopting the metre and centigrade degree as units, I take the heat equivalent A as $\frac{1}{424}$. The product xc is, according to the latest researches of Regnault, for hydrogen 3·409. According to Dulong, the value of x for hydrogen is 1·411.

The numerical value of r requires a somewhat more detailed discussion. It is, according to the preceding, the radius of the separating layer from which the protuberances break forth. There then arises the question, whether this value agrees with that of the sun's radius; that is, whether this separating layer coincides or not with the portion of the sun's luminous disc, which we have made use of for our measurements.

The late researches of Frankland and Lockyer, St. Claire, Deville, and Wüllner have proved that the discontinuous spectrum of hydrogen and other gases can, by increase of pressure, be converted into a bright luminous and continuous spectrum, the bright lines of the discontinuous spectrum passing through a series of very characteristic changes, on the pressure being gradually raised, which principally, as for instance by the line Hβ, consist in a widening out and increasing indefiniteness of outline.

These changes permit within certain limits an estimation of the intensity of the pressure on the spot in question, and Frankland and Lockyer have already hazarded such conclusions. They arrive at the result "that at the lower surface of the chromosphere the pressure is very far below the pressure of the earth's atmosphere."

The researches of Wüllner, I believe, allow even the conclusion *that the pressure at the base of the chromosphere, or at the outer edge of the sun's luminous disc, must lie between 50mm. and 500mm. of a mercury barometer on the earth's surface.*

According to this, the presence of dark lines on a continuous ground in the sun's spectrum no longer compels the conclusion that this continuous spectrum is caused by the incandescence of a solid or liquid body. The continuous spectrum can equally well be considered as produced by the incandescence of a strongly compressed gas.

Wüllner has, in fact, proved this for the sodium line; for in his account of the above-mentioned researches he remarks:—

"At a pressure of 1230mm. the maximum at H_a recedes still further; the whole spectrum is truly dazzling; the sodium lines appear as beautiful dark bands;* consequently, the light of hydrogen gas is sufficiently intense to produce a Fraunhofer's line in a sodium atmosphere—a proof that the light of an incandescent solid is not necessary."

From this it follows that the radius of the visible portion of the sun's disc need not be considered as identical with that of the supposed separating layer, but that the latter probably must be looked upon as situated beneath the layer where, through increased pressure, the spectrum of the hydrogen atmosphere becomes continuous. This view is strongly supported by a consideration of the phenomena of the sun's spots.

However different the views as to the nature of the sun's spots may be, almost all observers agree that the nuclei of the spots must lie deeper than the surrounding portion.† Partly from direct (De La Rue, Stewart, Loewy), partly from indirect (Faye) observations the depth is assumed to be about 8″.‡

If, then, the nuclei of the sun's spots are considered as scoriaceous products of a local cooling down on an incandescent liquid surface and the penumbra as clouds of condensation, which at a certain height crown the coasts of these slag islands, the simplest assumption is, that the (according to this theory) necessarily liquid surface is identical with the surface of the separating layer in question from which the protuberances break forth. The radius r of this surface therefore, the observed semi-diameter of the sun being expressed in seconds, would be approximately—

$$r = R - 8''$$
$$r = 15'52''$$

* In consequence of the high temperature in the tubes, sodium volatilises out of the glass. At a pressure of 1000mm. the sodium lines are still luminous.

† Spoerer says, however, "We consider the spots to be cloudlike formations far above the luminous surface of the sun's body. The penumbra is simply a collection of smaller spots, through the spaces between which the luminous surface is visible above which the spot is situated." (Comp. Pogg. Ann. lxxviii. 270.)

‡ From calculation of Carrington's observations Faye finds this depth t be ·005 — ·009 of the sun's radius. (Comp. Rend. lxi. 270.)

Accepting Hansen's determination of the mean parallax of the sun, 8″·915 we obtain

$$r = 680{,}930{,}000 \text{ metres};$$

consequently,

$$8'' = 5{,}720{,}500 \text{ metres.}$$

We have accordingly, in order to get at a numerical estimation of the absolute minimum temperature in the space from which an eruption 1·5 minutes high breaks forth, to introduce the following values into Formula (5):—

$$r = 680{,}930{,}000$$
$$H = 64{,}370{,}000$$
$$A = \tfrac{1}{424}$$
$$xc = 3{\cdot}409$$

It is then found,

$$t_i = 49{,}690^\circ$$

If for H a double so high a value be taken, viz., the by no means rarely observed height of eruption of three minutes, a minimum value

$$t_i = 74{,}910^\circ$$

is obtained.

The question arises here, however, are we at all authorised to introduce the extreme observed heights of protuberances at once into our formulæ as values of H, in which H denotes the height to which a body hurled up from the surface of the sun would rise if there were no resistance. If in fact, and it is conclusively proved by observation, we are dealing with ascending masses of nascent hydrogen, the ascent can also take place according to the Archimedean principle, similarly to the heated masses of air, which are thereby lighter than the surrounding portions, issuing from a chimney. It is however at once manifest that both causes of motion with regard to the time in which the masses reach a certain height are essentially different. Without entering more specially into this circumstance, it is clear that the time which, in virtue of the Archimedean principle, a protuberance requires to reach a certain height H, must under all circumstances be greater than the time expended by a body thrown up with a certain initial velocity, and without resistance to the same height H.

Consequently, a possibly correct observation of the time which an ascending protuberance requires to attain a certain height may serve as a criterion, whether we have to regard this height as the result of the first cause or not, and only in the former case can this height be made use of as an integrating constituent in the above formulæ.

According to the assumptions made, the exit opening (Ausströmungsöffnung) of the protuberances is situated in the incandescent liquid separating layer at a depth $h = 8''$ below the visible border of the sun's disc. The height of a protuberance from the plane of the exit-opening was expressed above by H.

Let now:

τ = the time occupied by the protuberance in passing from the opening to the height H,

τ_1 the time occupied by the protuberance in passing from the height h, i.e., from the outer border of the photosphere, to the height H,

v, the velocity at the exit opening,

v_1, the velocity at the height h.

Then assuming the first cause, and disregarding the decrease in the intensity of gravity (g) we obtain the following equations:—

$$\tau = \sqrt{\frac{2H}{g}} \qquad \tau_1 = \sqrt{\frac{2(H-h)}{g}}$$

$$v = \sqrt{2gH} \qquad v_1 = \sqrt{2g(H-h)}$$

Then making

$$H = 64{,}370{,}000 \text{ m.}$$
$$h = 5{,}722{,}600 \text{ m.}$$
$$g = 274{\cdot}3 \text{ m.}$$

we have

$$\tau = 11 \text{ min. } 25 \text{ sec.} \qquad \tau_1 = 10 \text{ min. } 54 \text{ sec.}$$
$$v = 187{,}900 \text{ mm.} = 25{\cdot}32 \text{ geogr. miles.}$$
$$v_1 = 179{,}400 \text{ mm.} = 34{\cdot}17 \quad ,, \quad ,,$$

If, therefore, we observe a velocity of ascent of the quoted magnitude, we are entitled to make use of the height obtained by the protuberance in the above time in our equations. I have often observed such a rapidity of evolution, and annex a sketch of a protuberance, the observed velocity of ascent of which agreed well with the value above found.

With respect to the enormous initial velocities of motion, Lockyer has by his magnificent observations of the change in refrangibility of the light arrived directly at results of exactly the same order.

Lockyer,* during the short period of observations of this nature, found 40 and 120 English miles per second as maximum values for the velocities of vertical and horizontal gas currents in the chromosphere. The above values expressed in English miles gave,

$$v = 123{\cdot}1 \text{ English miles,} \quad v_1 = 117{\cdot}7 \text{ English miles,}$$

and agree, therefore, with Lockyer's values.

But movements of such magnitude pre-suppose, necessarily, according to the mechanical theory of heat, differences of temperature of 40,690° C. for hydrogen.

We shall, accordingly, be able to ascertain the actual temperature if we can succeed in determining the temperature, t, of the outer hydrogen atmosphere at a certain spot. Why this temperature is taken as agreeing approximately with the temperature in the vicinity of the exit opening has already been discussed.

(4.)

An extreme value for t is obtained by discussion of Eq. V. This equation is:—

$$\sigma = \frac{b\,p_h}{a+t}\left(\frac{a+t}{t}\right)^q e^{\,b m \frac{r h}{(r+h)t}}$$

The density σ of the included mass of gas is in this expressed as function of the three magnitudes p_h, h and t. I shall now show that σ must not exceed a certain value, by which the value of t is also indirectly fixed within a certain limit, since the magnitude $p_h + h$ are determined within certain limits by the observations already quoted.

Stress has already been laid upon the fact that the explanation of the eruptive protuberances necessarily requires the assumption of a separating layer which separates the space from which the eruptions break forth from that into which they discharge. Only by such a separating layer are the requisite differences of pressure made possible.

With regard to the physical constitution of the separating layer the further assumption must necessarily be made, that it consist of a substance other than in a gaseous condition. It can therefore only be liquid or solid. If, having regard to the high temperature, we exclude the solid condition, there then only remains the assumption, that the separating layer consists of *an incandescent liquid.*

With respect to the inner masses of hydrogen bounded by this layer, two assumptions seem on superficial considerations possible, viz.:

1. The whole interior of the sun is filled with incandescent hydrogen: the sun therefore resembles a vast hydrogen bubble surrounded by an incandescent liquid envelope.

2. The hydrogen masses which burst forth during the eruptions are local accumulations in vesicular spaces, which form in the surface layers of an incandescent liquid mass, and which break through their envelope in consequence of the increasing tension of the included gas.

Under the first assumption a stable equilibrium could only exist when the sp. gr. of the liquid boundary layer is lower than that of the layer of gas directly beneath. The density of a ball of gas, the particles of which obey the laws of Newton and Mariotte, increases, however, from the exterior to the interior, consequently the sp. gr. of the separating layer must necessarily be lower than the mean sp. gr. of the sun; if, on the other hand, the mean sp. gr. of the sun be taken as the extreme sp. gr. of the liquid separating layer, this value would at the same time involve the assumption that all the deeper layers, therefore the layer of gas immediately below, possess the same sp. gr.

The interior of the sun would then no more consist of a gas, but of an incompressible liquid. All these properties are evidently a necessary consequence of the assumption, that the sp. gr. σ of the compressed mass of gas which breaks forth during eruptions attains its maximum value, viz., that of the sun's mean sp. gr.

Then in this case the first assumption is changed into the second, viz., that the sun consists of an incompressible liquid, in which local accumulations of incandescent hydrogen masses form near the surface, which on the necessary differences of pressure burst forth from the hollows containing them as eruptive protuberances.

However small the hollows may be assumed to be in special

* Proceedings R.S. No. 115 (1869). Comp. Rend. lxix. 123.

cases, the sp. gr. of the enclosed gas masses must not be taken as higher than that of the enclosing liquid, since otherwise the compressed gas masses would, in virtue of the Archimedean principle, sink down into the interior of the sun.

The sp. gr. of the sun is, according to the latest determinations,

$$1{\cdot}46.$$

Substituting this value for σ and for a (in Formula V.), the above found value 40,690, also for h the value 8″ in metres, we obtain for the extreme values $p_h = 0{\cdot}050$ m. above given, the following values of t:

$$\text{for } p_h = 0.500 \text{ m.} \quad t = 29{,}500^\circ$$
$$\text{for } p_h = 0{\cdot}050 \text{ m.} \quad t = 26{,}000^\circ,$$

therefore in mean, $t = 27{,}700^\circ$.

On differentiating Equation (5) by t, the differential quotient $\frac{d\sigma}{dt}$ is negative. From this follows that the values found above for t are also minimum values.

From the mean value of t for the temperature of the sun's atmosphere the value of p_h is found $= 0{\cdot}180$ m. These values will be those made use of in the following calculations.

It may be noticed in connection with the high numbers obtained for the temperature values, that they are about eight times higher than the temperatures of combustion of a mixture of detonating gas as found by Bunsen, and that iron must permanently exist in a gaseous condition in the sun's atmosphere.

With the above value for $t = 27{,}700^\circ$ we obtain from Formula (I.) for the inner temperature

$$t_i = 68{,}400$$

substituting these two values of t and t_i in Formula (II.) we have

$$\frac{p_i}{p_a} = 22{\cdot}1$$

i.e., the pressure in the interior of the space from which the protuberances break forth is 22·1 times greater than the pressure on the surface of the liquid separating layer. Further substituting the value for t in Formula (IV.) and assuming as before the value of h to be 8″, we have

$$\frac{p_a}{p_h} = 766{,}000$$

as the relation of the pressure on the fluid surface of the sun to the pressure at the height h, where the hydrogen spectrum, in consequence of the pressure, begins to become continuous.

Substituting for p_h the above value of 0·180 m. mercury, we have

$$p_a = 184{,}000 \text{ atmospheres,}$$

and consequently for $p_i = 4{,}070{,}000$,,

If the depth be calculated at which in the interior of the liquid mass of the sun which has a sp. gr. of 1·46, and simply as the result of the hydrostatic pressure, this maximum pressure of p_i would be attained, it is found that this would occur at a depth of 139 geographical miles below the surface, *i.e.*, at a depth of about 1·46 arc seconds, or $\frac{1}{655}$ of the sun's semidiameter.

Even if the liquid condition be put quite out of question, and, under assumption of a much larger atmospheric envelope of hydrogen, the depth in it be calculated, at which the atmospheric pressure becomes equal to the inner pressure p_i, it is found that even assuming a temperature of 68,400°, that depth is only 27″ below the visible edge of the sun's disc, or about $\frac{1}{38}$ of the sun's apparent semidiameter.

This circumstance shows how rapidly the pressure must increase towards the interior of the sun's body, and thus justifies the assumption that in the interior of the sun, even at such high temperatures, the permanent gases, for example, hydrogen, can only exist in an incandescent liquid condition.

(5)

A surprising result is obtained if, under the assumption of a nitrogen or oxygen atmosphere of equal weight and temperature to the hydrogen atmosphere above considered, the pressure be calculated which is reached in those atmospheres at heights at which the hydrogen spectrum commences to become continuous. If at a depth of 8″ below the visible edge of the sun's disc, *i.e.*, a the riwean of the supposed separating layer, the pressure of the three atmospheres of hydrogen, oxygen, and nitrogen be assumed as equal, and that $p_a = 184{,}000$ atmospheres, a value which from the above, corresponds to the assumed value of p_h. The following values are obtained for the pressures at the temperature above found $t = 27{,}700^\circ$ on the surface of the sun's *visible* disc in the three atmospheres:—

$$\text{Hydrogen } p_h = 180 \quad \text{millimetres.}$$
$$\text{Nitrogen } p_h = 323\tfrac{1}{1078} \quad ,,$$
$$\text{Oxygen } p_h = 124\tfrac{1}{1088} \quad ,,$$

It follows from these that, the assumptions made, the quantities of the two latter gases are, in proportion to the quantity of hydrogen in that layer in which the spectrum of the latter commences to be continuous, infinitely small. This would, as is evident, also be the case if the weights of the two atmospheres were assumed to be many million times greater, although having regard to the specific gravity, a 14-times smaller weight of nitrogen and a 16-times smaller weight of oxygen would suffice, in order that under the assumed conditions the density of these two gases should coincide at the base with that of hydrogen.

According to our former considerations, the sun's mean specific gravity would also in this case have to be assumed as the maximum value of the density at the base of these atmospheres, and it is easy to calculate, with the help of Formula 3, and the known specific gravities of oxygen and nitrogen, how high the weights of these two atmospheres would have to be assumed in order to attain this maximum value.

As result is obtained, that the weight of the oxygen atmosphere could only amount to ·56, that of the nitrogen atmosphere to ·64 of the weight of the existing hydrogen atmosphere.

If therefore the simultaneous existence of these three gases on the sun's surface be assumed, and the influence of atmospheric motion be disregarded, the rays emitted by the continuous spectrum of the hydrogen layers would, on their path to our eyes, pass through so small a number of incandescent nitrogen and oxygen particles, that the absorption caused thereby is a vanishing one, and therefore, as is in fact the case, the presence of oxygen and nitrogen in the sun's spectrum could not be demonstrable by dark lines.

Although the motion of the gases is active in lessening the differences just considered, the existence of the chromosphere proves clearly the slight influence of this action in consequence of the great intensity of gravity, and the considerable height of the layer considered (compare Formula 4).

In order, however to explain through the circumstance indicated the absence of lines in the sun's spectrum of two bodies of such universal distribution on the earth as nitrogen and oxygen, the very slight emissive power of the permanent gases in proportion to that of volatilised bodies must also be taken into consideration. If the emissive power of different gases at the same temperature for rays of the same refrangibility be referred to equal very minute weights of these gases,* the before-quoted experiment of Wüllner's, in which the small amount of sodium volatilised in the Geissler's tube emitted more light than the hydrogen gas under a pressure of 1,000mm., gives a beautiful proof of the extraordinary difference of emissive—and consequently according to the theorem of Kirchhoff of absorptive—power of different gases at the same temperature. Only by consid ration of this circumstance is the contradiction removed, which could be deduced against the above explanation of the absence of nitrogen and oxygen lines from the fact that in the sun's spectrum the lines of bodies are present whose vapour densities, as a consequence of their simple relation to the atomic weights, must be much higher than the density of oxygen and nitrogen.

From these considererations, partly directly and partly indirectly through a longer series of conclusions, a detailed exposition of which I reserve for another occasion, the following result:—

1. From the absence of lines in the spectrum of a star shining in its own light, the absence of the corresponding element must not be inferred.

2. The layer in which the reversal of the spectrum occurs is different for each element—the higher the vapour density, and the lower the emissive power of the element, the nearer it is situated to the centre of the star.

3. For different stars, under otherwise similar conditions, this layer lies the nearer to the centre the greater the intensity of gravity.

4. The distances of the reversing layers of the separate elements, both from the centre of the star and from each other, increase with an increase of temperature.

5. The spectra of different stars are under otherwise similar conditions the more rich in lines, the lower their temperature and the greater their mass.

6. The great difference of intensity of the dark lines in the sun's spectrum and other fixed stars does not depend only on the differences of absorptive power, but also on the depths at which the reversal of the respective spectra takes place.

In conclusion, I would offer a few remarks on the application of the observations carried out on rarefied gases to the heavenly bodies. Lecoq de Boisbandrau* has recently pointed out, with

* Compt. Rend. lxx. p. 1091.

reference to Wüllner's investigation on the variability of spectra at different pressures and temperatures, that the results obtained must only be applied with the greatest care to the conditions of pressure of the sun's atmosphere, as the changes in the spectra are due far more to temperature than to pressure. But even under the assumption that this conjecture should become verified by special experiments, this circumstance would influence the results brought forward in this communication but in a slight degree. For the nature of the function (Formula 5) which served us in determining the temperature of the atmosphere is such that the pressure p_h under which the hydrogen spectrum becomes continuous may be varied within very wide limits without thereby causing any considerable alterations of the requisite temperature. Thus it was shown above that, by introducing the extremes of the pressure assumed which were in the proportion of 1 : 10, the temperature values resulting were only in the proportion of 1 : 1·5.

Nevertheless, the separation of the influences which pressure and temperature exercise on the nature of the spectrum of luminous gases must be regarded as a problem the solution of which is of the highest importance for astrophysics.

THE BRITISH ASSOCIATION

SECTIONAL PROCEEDINGS

Section A.—Mathematical and Physical Science

Rainfall: its Variation with Elevations of the Gauge.—Mr. Charles Chambers, F.R.S. The fact is well known to meteorologists that the quantities of rain received in gauges placed at different heights above the ground diminish as the elevation of the gauge increases. Several attempts have been made to explain this phenomenon, but none of them are so satisfactory as to discourage the search for other causes that may contribute substantially or mainly to its production. Hence the submission for the consideration of the British Association of this further attempt. One of the principal causes of rain is undoubtedly the transfer, effected by winds, of air charged with moisture in a warm damp district to a colder region, where the vapour it contains is partially condensed. The temperature of the lower as well as of the higher horizontal strata of the atmosphere being reduced by this transfer, it may fairly be inferred that condensation of vapour may also occur in the lower as well as in the higher horizontal strata. The rain caught by a gauge at any given elevation will therefore be the sum of the condensations in all the strata above it, and thus the lower a gauge be placed the greater will be the quantity of rain received by it. Again, it is known by observation, that there is at all times a greater or less difference of electrical tension between the atmosphere and the surface of the ground. If then—in accordance with the views of Prof. Andrews as to the continuity of the liquid and gaseous states of matter, from which it follows that the change of other physical properties must also be continuous—we regard the particles of vapour suspended in the electric bodies in relation to the dielectric principal constituents of the atmosphere, they will be polarised by induction from the ground. This polarisation will give rise to an attraction between every particle and the neighbouring particles above and below it; and being stronger in the particles near the ground than in those more remote, the tendency of the particles to coalesce—which will increase, by their mutual induction, as two neighbours approach each other—will be greatest near the ground. Thus it may be, each particle gathering to itself its neighbours in succession till their united density exceeds that of the atmosphere generally, some rain drops are formed, and that in greatest abundance near the ground. If this be the true cause of any substantial part of the phenomena in question, then, as the variation in intensity of electrical polarisation of the particles will vary with the height most rapidly near the ground, so the variation in the rainfall near the ground should be more rapid than at a greater elevation, and such is indeed the fact. Also, if the idea be correct, it will probably serve to explain other phenomena which it was not specially conceived to meet; and so it does. For, first, it requires that the rainfall over even ground, where the electrical tension is relatively weak, should be less than over similarly situated forest land, where at the tops of the trees, ends of branches, and edges of leaves, the tension is high, and this is in accordance with observation. And, secondly, the tension being relatively high at the tops of the elevations of a mountainous district, the rainfall should be greater there than in the neighbouring plains; this, again, is borne out by observation. Further, at the commencement of a passing thunderstorm, a sudden heavy shower of rain will often fall for a few moments and then suddenly cease. May not this arise from the approach, by the agency of opposite wind currents, of detached masses of differently charged clouds, the process just described of formation of rain drops going on rapidly in each mass as the two come near each other, and stopping when, by a flash of lightning between them, the two masses are brought into the same electrical condition? An experimental test of this idea would be to repeat Dalton's measures of the pressure of vapour in the vacuum space of a mercurial barometer tube—filling that space with air and a little water, and compare the values found when the mercury was charged with electricity and when not so charged. If in the former case a less pressure was found, we might conclude that the particles of vapour are really susceptible of electric induction, and the amount of difference existing would enable us to estimate whether the attractions of the particles upon each other were strong enough to cause the formation of rain-drops hypothetically attributed to them above.

* A perfect transparency of the gas mass to all rays emitted by itself is here assumed, a supposition which is the nearer the truth the smaller the weights compared.

Section C.—Geology

On the Mountain Limestone of Flintshire and part of Denbighshire.—Mr. G. H. Morton. Minute details of the physical structure of the region, and lists of the fossils, showed that these beds have been erroneously referred to the Millstone Grit, and that they were really Mountain Limestone, the shales and sandstones being intercalated among the typical rocks. The white limestone was an ancient coral reef, with the organisms exquisitely preserved. Mr. Hughes protested against co-relating with the Yorkshire beds, while Mr. Bailey supported the opinions of the author.

On the formation of Swallow-holes, or Pits with vertical Sides, in Mountain Limestone.—Mr. L. C. Miall. The author distinguished between cavities formed by direct excavation and those produced by subsidence of part of the roof of a cavern. The curious pits near the Buttertubs Pass at the head of Swaledale, were regarded as typical of the first kind, and their appearance and mode of formation were described, especially the vertical fluted sides and the isolated fluted pillars, which were ascribed to the action of dropping water, aided by pebbles. A basin is first formed upon a ledge of rock, and as the excavation proceeds it produces a semi-cylindrical scar, with sharp ridges upon the face of the limestone wall, as if cut by a gauge. The presence of a thick surface-covering of alluvium or drift was necessary to absorb and retain the rainfall, and to distribute it slowly and regularly. The limestone of a bare plateau furnishes fissures in great variety, but they are not true swallow-holes. Regular and well-marked joints were also necessary to the production of fissures, as they permitted the ready escape of the waters of erosion. The texture of mountain limestone, and its power of receiving and retaining sharp impressions, gave the peculiar features to the swallow-holes excavated in it. Some swallow-holes were due to the subsidence of an undermined crust. These frequently lie in a line, sometimes in a ring round a hill-side. A particular description of some near Ripon was given, and the testimony of eye-witnesses as to their sudden appearance was quoted. Swallow-holes are often disguised by surface accumulations. Many conical hollows in drift are probably due to concealed cavities of subsidence.

On the Stratigraphical Distribution of the British Fossil Gasteropoda.—Mr. J. L. Lobley. This was the third of a series of reports by the author on British fossil mollusca. By the help of diagrams were shown the distribution of the species, and the range, increment, decrement, and maximum development of the genera, families, and orders of the *Gasteropoda*. The cainozoic deposits contain the greatest number of genera and sub-genera, though they are numerous also in both the mesozoic and palæozoic rocks. A large number of genera and sub-genera are characteristic of single formations, and these are especially numerous in the carboniferous limestone, the lower lias, the middle Eocene, and the older and newer Pliocene. Details of the range and of the distribution of species of each of the families of *Gasteropoda* were given, and the large groups or orders were similarly passed in review. The remarkable contrast between the distribution of the species of *Holostomata* and *Siphonostomata* was pointed out, the former being very largely developed in each of the three great divisions of the stratified rocks, while the other and more highly organised order is absent from the palæozoic, has only a few species in the Mesozoic, but is largely represented in the cainozoic formations. The distribution of the species without reference to generic family or ordinal divisions showed that the maximum number occurs in middle Eocene strata, from which beds 420 species have been described. The total number recorded in the paper exceeds 3000, but a great number of these are recurrent. The author insisted, in concluding, that as the recognised formations were of different values, as they had been unequally explored, as many formations were wanting in Britain, as the organic remains in the different formations had not been subjected to a uniformly rigid scrutiny, and as the British area, compared with the whole world, was so small, only the most general conclusions could be drawn from this investigation as to the progress of life on the globe.

On the Glacial and Post-Glacial Beds in the Neighbourhood of Llandudno.—Mr. H. F. Hall. The necessity for a more exact definition of boulder clay, and for discarding a name which is made to include a series of beds formed under very different conditions, was insisted upon. The section at Llandudno exhibited a base bed formed by the action of an ice-cap covering the whole land down to the sea level, which ground together the different materials of the bed. The overlying beds lie unconformably on this base bed, and show land and water conditions connected with a much more genial climate. From the section, the author concluded that colour is no criterion for deciding as to the base bed, as it varies in each district with the underlying rock; that the materials of the base bed are obtained from the rocks of the neighbourhood; that this bed was the result of the pressure and passage of land-ice disintegrating the surface of the land which it capped; that to this bed, which is invariably denuded and has the superposed beds lying unconformably upon it, the name "Boulder-clay" should be restricted; and that the red clay, over the sands and gravels which overlie the base bed, is variable in colour and constituents, showing a change which produced extensive denudation in more northern regions, the materials being spread over the sea bottom mixed with pebbles and boulders, which fell from melting or stranded icebergs and ice-flows. The author said there was a hope of being able to co-relate the beds of the eastern districts with those he had described.

Remarks on the Fossils from the Railway Cutting at Huyton.—Mr. W. Carruthers. The great value of this collection, made by the Rev. H. Higgins, depended as much upon the comparatively limited number of species met with as on the fine state of preservation in which they occurred. It was possible to arrive at considerable—in some cases absolute—certainty as to the different parts of the same species. Of the four species of *Calamites*, the materials existed in the specimens from Huyton for reconstructing the entire plant of at least one. The roots, long considered to be a distinct plant under the name *Pinnularia*, were present in great abundance. It had a delicate fistular stem of the type described by Professor Williamson at a previous meeting of the section, but of great size. The scars of the fallen branches were shown in several specimens as well as the foliage, which was preserved in the early bud condition, as well as in its fully developed state. Several fruits showing the structure of the cone, described by the author under the name *Volkmannia binneyi*, but with differences that were at least of specific value. A cone having the structure of that described by Professor Williamson probably belonged to *Calamites longifolius*, with the foliage of which it was associated in these beds. Specimens of *Sphenophyllum* were exhibited and referred to *Calamites*. The light thrown on the structure of *Lepidodendron* by the specimens was then dwelt on, and especially two undescribed cones—one long and slender, with a single sporangium on each scale, the other short and having two sporangia on each scale. The stem and foliage of *Flabellaria*—a palm-like lycopodiaceous genus—occurred among the fossils, as well as several species of beautifully preserved ferns. Two specimens of insect remains had also been found—the one by Messrs. Clementshaw and Smith, young gentlemen whose interest in natural science was due to the revival of those studies in our great schools, and whose personal efforts had greatly contributed to its advancement at Rugby. Professor Williamson contended that the interpretation he had given on a former occasion of the structure of the stems of *Calamites* was more in accordance with the hundreds of specimens he had examined than those just expressed; but, in reply, Mr. Carruthers maintained, on structural grounds, the correctness of the views he had expounded.

SECTION D.—BIOLOGY

Department of Zoology and Botany

The Secretary read a paper by Dr. J. E. Gray, F.R.S., *On the Whalebone Whales of the Southern Hemisphere.* The author remarked that formerly the number of Cetacea was believed to be very limited, and that each species was supposed to have a very extensive geographical distribution. At one time, even, the hunchback of the Cape of Good Hope was supposed to be the same species as the whale of the North Sea. The author gave a list of the true whales, or Balænidæ, the hump-backed whales, and the Physalidæ. Five species of the first group were described, three of the second, and one of the last group. Reference was also made to three apparently different forms of Finne whales, known only from having been seen swimming.

Dr. Cunningham read a paper *On the Terrestrial and Marine Fauna of the Strait of Magellan and Western Patagonia.*

Professor Van Beneden read some notes "*Sur les Parasites*"—One frequently finds described under the title of "Parasites" animals which do not demand more than a place to live on, and do not live at the expense of their neighbour, such, for example, as the Adamsia by the side of the Pagurus. Some of these do not completely enjoy their liberty, as the Coronula on the whale. This type I would designate under the name of Oikasitæ, whereas those which are perfectly free I would designate as Coinositæ. The true Parasites may also be divided into groups: those that have no communication with the exterior are the Xenositæ, these, like the Cysticercus, are possibly only transitory forms: others, having arrived at the end of their journey, live in the open passages of organs, occupying themselves with reproduction, and these I would designate Nostocitæ; and lastly, those which appear to stray by the way, without a hope of arriving at the end of their journey, and indeed only by chance returning to the good road, such as the vesicular and agamic worms which frequent the flesh of carnivores, I would call Planositæ.

Professor Van Beneden exhibited a specimen of a species of Echinorhynchus, apparently new, lent to him for exhibition by Dr. John Barker of Dublin.

On Brackish-water Foraminifera.—Mr. H. B. Brady. The author described a form of Foraminifera from a fresh-water pond, some five or six miles from the sea, and while describing in addition a large number of new species from brackish water, he also alluded to the fact that he had met with some Foraminifera whose tests had withstood the action of acids. Without wishing positively to assert the absolute presence of chlorophyll granules, as occurring in some species, he might yet mention that he had found traces of it in the test of some of the forms he had examined.

On a stock-form of the parasitic Flat-worms.—Mr. E. Ray Lankester. This worm was found parasitic in *Tubifex rivulorum* from the Thames. It had the form of a fluke with very mobile head, no alimentary tube, a very elaborate vascular system, and simple generative organs. A small mobile tail was attached to one end of the worm at the opening of the vascular system. This tail was only paralleled by that of the *Cercariæ* or larvæ of Flukes. A worm known as Caryophyllœus, which lives in carp and tench, was stated to be exactly like the new worm in respect of its mobile head, wrongly held to be the tail by Emile Blanchard. The tailed form *Uroscolex*, Mr. Lankester considered to be the larva of *Caryophyllœus*, and hence we have in this simple worm a representative of the common ancestors of all the Trematods and Cestoids. Mr. Lankester said he was informed by Professor Van Beneden, that last year Caryophyllœus had been shown to have a six-hooked embryo.

On Worms from Thames mud.—Mr. E. Ray Lankester. The author showed that the tons of red worms which are the only non-microscopic tenants of the foul parts of the Thames at London, consist of three distinct species, and a natural hybrid between two also occurs, as he demonstrated from minute study of their characters. The species are *Tubifex rivulorum*, *T. umbellatus* and *Limnodrilus*, sp. *incert.*; the last very abundant. Mr. Lankester then mentioned the gregarinæ of these worms, and the discovery of their pseudo-naviculæ having long stiff processes, so that they run into the worms like pins, and in this way penetrate into previously uninfested worms. The formation of the spermatophors of this group of Annelids (the Oligochæta) discovered by Mr. Lankester—Professor Claparède having mistaken them for Opalinoid parasites—was also detailed.

Department of Anatomy and Physiology

On the Embryonal Development of the Hæmatozoön (*Bilharzia*).—Dr. Cobbold. After commencing with a general description of this remarkable parasite, Dr. Cobbold proceeded to notice the manner in which the larvæ escaped from the eggs; and also their subsequent activity and remarkable alterations of form and structure. He had obtained ample evidence of the existence of a complicated water-vascular system, similar to that described by Dr. Guido Wagener, as occurring in the larva of *Diplodiscus*. The prevalence of the Bilharzia disorder in Egypt and at the Cape was well known; and it had recently been suggested by Dr. Aitkin, that these parasites had some connection with the so-called Delhi boils. He refrained from entering into professional details in this matter: but stated that he had performed a large number of feeding experiments on small fishes, crustaceæ, and molluscs, with the view of putting the question of injection beyond the region of mere conjecture. Dr. Cobbold added that he had obtained for a month past about 10,000 eggs of Bilharzia daily, from a case under his care.

Dr. Cobbold exhibited the heart of a dog filled with Hæmatozoa causing the animal's death. He had received the specimen from Mr. Robert Swinhoe, H.B.M. Consul at Amoy, China, accompanied by a note from the donor, stating that the dog "died on the 18th of April, 1869, at Shanghai, after three days of great suffering." Hitherto, following the authority of M. Bohe-Moorea Diesing and other systematists, he had been accustomed to regard this form of entozoon as the species called *Spiroptera sanguinolenta*; but, in the author's opinion, this view would have to be changed. He hoped, before long, to be able, by further investigation, to set this point at rest. The presence of entozoa in the heart and blood-vessels of animals and man is much more common than is supposed. Thus, MM. Grube and Delafond, who examined 480 dogs, found Filariæ present in nearly 5 per cent. Most of these parasites, however, were of microscopic size; being probably the brood of the species marked *Filaria sanguinis* in Dr. Cobbold's list. They estimated that these verminiferous dogs severally harboured from 11,000 to 224,000 of these larval hæmatozoa.

Note on Methæmoglobin.—Mr. E. Ray Lankester. It was shown that carbonic acid, when passed through a solution of oxyhæmoglobin, gave rise to two new bands in addition to those of the oxyhæmoglobin itself. This was the nitrite-hæmoglobin of Dr. Gamgee, and the brown cruorine of Mr. Sorby, also identical with methæmoglobin as described by Preyer. Addition of a minute quantity of acetic acid to this solution caused the disappearance of the oxyhæmoglobin bands and intensification of the two new bands, which are those of what really was originally called methæmoglobin by Hoppe Seyler. It can be formed by the passage of CO_2 alone if a weak solution of hæmoglobin is used, as was done by Heynsius, who mistook this product for hæmatin. Its band in red is not identical with that of hæmatin as supposed by Hoppe Seyler and Heynsius, and all previous observers, including Hoppe Seyler, Preyer, Gamgee, and Sorby have missed the second band in blue (the fourth of the mixture of oxyhæmoglobin and methæmoglobin) now figured and described. It was shown that no separation of an albumen accompanies the change of hæmoglobin into methæmoglobin, whilst hæmatin results solely from a splitting up of the hæmoglobin into it and an albumen.

The action of certain Vapours and Gases on the red Blood Corpuscles.—Mr. E. Ray Lankester. These experiments were made with Stricker's gas chamber, which enables the observer to study gradual changes, caused by gaseous reagents, as to the change of form caused by atmospheric air in the red corpuscle of the frog, which had been acted on first by CO_2 as observed by Stricker, was shown to be equally produced by hydrogen, or by carbonic oxide, or by diminution of pressure, hence it was simply to be ascribed to the diffusion of the carbonic acid. The action of cyanogen gas, carbonic oxide, alcohol vapour, chloroform vapour, and especially of ammonia vapour, was described. A distinction was insisted on between mere definition of the nucleus—as caused by some agents—and granulation of the nucleus. The normal living frog's red corpuscles was inferred to be *usually* free from any appearance of definition of the nucleus, and to be devoid of an envelope or œcoid, though owing its form to a remarkable condition of tension, which was readily destroyed by physical agents.

On the Relations of Fins of Fishes to one another.—Professor Humphry.

Department of Ethnology and Anthropology

The Pre-Turkish Frontagers of Persia.—Mr. H. Howorth. In continuation of the previous paper the author showed with the assistance of Vivien St. Martin, Thomas, Prinsep, &c., that after the first century, the Indo-Scyths were called Kouschauk by the Armenians, Koneichang by the Chinese, that their great king Kanichka who was a convert to Buddhism, and introduced that religion into China and Thibet, was, with his people, previously a fire-worshipper, and that the form of Mithraism, which was introduced at Rome by Pompey and derived by him, in the first instance, from the Parthians, was the original religion of the Massagetæ and the Indo-Scyths.

On the decay of the power of the Indo-Scyths, the remains of the nation were conquered by the Avares or White Huns, and are called by Procopius, Priscu, and Cosmas, White Huns, and Ephtalitæ, and by the Persians Hainthelah. The etymology of these names shows they were the Yuetchi or Massagetæ, led and governed by a caste of Huns.

In latter days these White Huns are to be identified with the Khazars, the ancestors of the Circassians. Thus the Circassians are proved to be lineal descendants of the Massagetæ. That the Circassians are allied to the Thibetans was long ago showed by Mr. Hodgson in the Journal of the Asiatic Society. This is the first time their genealogy as a race has been clearly traced out, and it opens up a new light on Asiatic Ethnography.

On the Manx of the Isle of Man.—Dr. King.

SECTION G.—MECHANICAL SCIENCE

On Ashton and Storey's Steam-Power Meter.—Mr. Ashton. The apparatus described in this paper, as its name implies, shows at all times the measure of the power developed by the steam engine to which it is applied, and registers the aggregate of that power during any required period of time. The mechanisim is very like that of a well-balanced chronometer. The whole of the indicating mechanism is very light, and mounted so as to move with great freedom; and the power required to work it is exceedingly small. Its indications would be especially valuable in the case of steamships. The apparatus has been in practical use about a year.

In the discussion which followed this paper the invention was very highly praised.

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ERRATA.—Page 464, second column, line 30, for "monodont" read "homodont"; line 35, for "but its analogue in front has" read "but, unlike its nalegue in front, has."