SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

ILLUSTRATED.

VOLUME I.

JULY TO DECEMBER.

EDITED BY JOHN MICHELS.

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LIST OF AUTHORS.

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TERMS.

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"SCIENCE" occupies a field in Periodical Literature hitherto unoccupied, and is the only First Class Weekly Journal in the United States, devoted to Science, recognized by Scientists as their medium of communication.

To all who desire to be kept *au courant* with the progress of Science this Journal will be found invaluable. It is offered in a size convenient for binding, and will form two annual volumes with good Indexes, replete with original articles by the most eminent Scientists, and as a standard work of reference on scientific subjects should find a place in every Library and Household.

The list of Associate Editors is not yet complete, but so far it comprises the names of Professional Scientists who stand at the head of the various branches of Sciences with which they have identified themselves.

The Editor will be glad to hear from those who will take an interest in the success of this Journal, or who desire to co-operate in any way; and suggestions will always be welcomed and receive due attention. The intention of publishing "SCIENCE" has been known only to a few, but the following well known and esteemed Scientists have expressed either their intention to contribute, or their approval of the object of the Journal, and good wishes for its success.

Professor SPENCER F. BAIRD, and Dr. ELLIOTT COUES, Smithsonian Institution, Washington, D. C.; Professor EDWARD S. HOLDEN, Naval Observatory, Washington; Professor O. C. MARSH, Yale College; Professor BURT G. WILDER, Cornell University; Professor C. A. YOUNG, Princeton College; Professor S. W. BURNHAM; Chicago; Professor O. STONE, Observatory, Cincinnati; Dr. J. S. BILLINGS, National Board of Health, Washington; Professor WILLIAM A. HAMMOND, M. D., New York; Dr. EDWARD C. SPITZKA, New York; Prof. CLEVELAND ABBE, Army Signal Office, Washington; Professor EDWARD S. MORSE, Peabody Academy of Science, Salem, Mass.; Professor ALEX. A. JULIEN, Columbia College, New York; Dr. J. J. WOODWARD, Surgeon General's Office, Washington; Professor H. P. BOWDITCH, Cambridge, Mass.; Professor H. L. SMITH, Hobart College, Geneva, N. Y.; &c., &c., &c.

All Communications should be addressed to THE EDITOR, Box 3838, P. O. New York.

SCIENCE:

A Weekly Journal of Scientific Progress.

NEW YORK, JULY 3, 1880.

THE UNITED STATES NAVAL OBSERVATORY, WASHINGTON.

BY PROFESSOR EDWARD S. HOLDEN.

This institution has been long and favorably known to the scientific public, not only of the United States, but of the whole world. It was founded in 1844, and commenced its operations in 1845, and as it is now about to enter a new epoch of its existence by a removal to a new and better site in the District of Columbia, a brief account of its progress will not be without interest.

Astronomy did not flourish in America during the eighteenth century. A few observations were made by Professors at Harvard and Yale Colleges, and in Pennsylvania by RITTENHOUSE and others (in 1769). A telescope was mounted in 1830 at Yale College for regular astronomical observations, and the first observatory was built at Williams College in 1836, by Prof. HOPKINS. Mr. WILLIAM C. BOND, of Dorchester, a maker of chronometers, had erected a small observatory at his residence, and this was afterwards removed and formed the nucleus of the observatory of Harvard College. The observatories of Hudson, Ohio, (founded 1837), of the Philadelphia High School (1840), of West Point Military Academy (1841), of Cincinnatti (1843), of Georgetown, D. C., (1844), and the Naval Observatory (1842), were the first established, and these observatories all erected within the decade, 1835-1845, were the signs of a growing sense of the importance of astronomical research among the people.

Probably due credit has not been generally given to the efforts of General O. M. MITCHEL the astronomer of the Cincinnati Observatory, who, by lectures, treatises and personal influence, kept the subject before the reading public. In Congress a few intelligent men, like Mr. JOHN QUINCY ADAMS, had always advocated the establishment of an observatory which should be truly national, but great opposition to such an institution was constantly displayed, and so late as 1832 a bill appropriating money for the survey of the coast, contained the clause "provided that nothing in this act should be construed to authorize the construction or maintainance of a permanent astronomical observatory."

The final establishment of the Naval observatory came about in this wise, and it was due largely to the admirable abilities of Lieutenant GILLISS, of the Navy.

The exploring expedition of Admiral WILKES (1838-1842), proposed making astronomical observations in all parts of the world, and to utilize these, corresponding observations were required at home. These were made by GILLISS in a small observatory on Capitol Hill for the four years and they were of high excellence. The present observatory building was erected as a "depôt of charts and instruments" for the Navy from designs by GILLISS. The regulations of the Service required that GILLISS should be sent to sea, and the direction of the observatory was confided to Lieutenant MAURY, who retained it till 1861. A corps of astronomers was formed and a detail made of the officers from the line of the Navy to care for the chronometers, charts and instruments, and to collect hydrographical information, and this plan of organization continued till 1866, when the Hydrographic office was separated from the Observatory. Suitable instruments were provided and the observations were published in quarto volumes, twenty-two of which have appeared up to 1880. The main instruments were:

1. A Transit Instrument (by ERTEL, of Munich).
2. A Mural Circle (by SIMMS, of England).
3. A Meridian Circle (by ERTEL).
4. A Prime Vertical Transit (by PISTOR & MARTIUS, of Berlin).
5. An Equatorial (by MERZ, of Munich), with an Object Glass of 9.62 inches.

These instruments were kept steadily at work and thousands of observations were made and have been reduced and published. The mere index to these observations fills 74 quarto pages. Certain special publications deserve particular mention. A catalogue of 10,658 stars, observed with the instruments 1, 2, 3, and 4, has been made by Professor YARNALL. It may be said to have been his life work, as he made a large share of the observations and reduced all of them. This catalogue is of great usefulness.

The Wind and Current Charts of MAURY, which have been adopted the whole world over, were constructed from observations collected and discussed here. With the equatorial, three asteroids were discovered by Professor FERGUSON, and Professor HALL and himself observed a great number of comets and minor planets. The theoretical researches of Professor WALKER on *Neptune*, of Professor HUBBARD on comets, and the work of Professors COFFIN and HUBBARD on points of practical astronomy, all belong to this first epoch.

The second stage of the Observatory's life may be said to have begun in 1861, with the superintendence of GILLISS, and to have extended to the present time under the direction of Rear Admirals DAVIS, SANDS and RODGERS. Two new first-class instruments were purchased.

6. The Transit Circle (1865), made by PISTOR & MARTINS.

7. The 26-inch Equatorial (1873) made by ALVAN CLARK & SONS. Both have been kept in constant use. With the first, the sun, moon, major and minor planets have been constantly observed and the materials for a very large and important catalogue of stars (soon to be published) have been collected. The telegraphic longitudes of many points in the United States and elsewhere, have been determined by Professors HARKNESS and EASTMAN. We may mention among these the longitudes of Havana (Cuba), St. Louis, Detroit, Carlin and Austin (Nevada), Ogden (Utah), Bethlehem (Pa.), Princeton (N. J.), Cincinnati, Nashville, Columbus, Harrisburg, and others. The large equatorial, besides making a great number of observations of double stars (HALL and NEWCOMB), and of Nebulæ (HOLDEN), has been employed on the observations of the faint satellites for which it is better fitted than any other instrument existing. The masses of *Uranus* and *Neptune* have been determined by Professor NEWCOMB and the capital discovery of two satellites to *Mars* made by Professor HALL.

The theoretical researches of Professor NEWCOMB on the Lunar Theory and on Fundamental Stars, and of Professors NEWCOMB and HALL on Satellites, belong to this period.

The Transits of *Venus* (1874), and of *Mercury* (1878), have been most thoroughly observed and discussed by the various astronomers.

The solar eclipses of 1869, 1870, 1878 and 1880 have been also elaborately observed by parties sent from the observatory, and the results are all published except those for 1878 and 1880, which will shortly appear. The work done here on solar eclipses alone is of the first importance, and will greatly forward our knowledge of solar physics. There is no space to mention the miscellaneous work done: the chronometers of the Navy, the furnishing of standard time to the United States, the observations of meteors, all receive their share of attention.

The third epoch of the history of the Observatory commences with the effort to change its site to one less exposed to the sickly influences of the malaria which rises from the marshes surrounding the Observatory on the river side, and to one where the fogs from the same source will not seriously interfere with the complete use of the instruments. This subject has, since 1870, received more or less attention, but the first serious effort to change the site for these reasons was made in a report of the Superintendent in 1877.

"UNITED STATES NAVAL OBSERVATORY,
Washington, September 15, 1877.

"SIR: I found upon taking charge of the Observatory, that the malarious influences surrounding it were notorious, and that from May to about the middle of October the officers whose services were necessarily in the Observatory at night, paid the penalty in impaired health and in diminished efficiency. The fogs which arise from the river, driven by the prevailing winds, float above the instruments and lessen their usefulness.

* * * * * *

For these reasons, I earnestly recommend that a suitable site, north of the city and inside the District of Columbia, be procured for a new Observatory.

The area allotted to this purpose need not necessarily be more than twenty-five or thirty acres in extent; but as much as this is needed, since, if surrounded by dwellings or factories, the smoke would obscure the clearness of vision, the traffic would shake the instruments, and some high structure, if placed upon the meridian near our instruments, might hide a useful part of the heavens.

The present Observatory is in a very dilapidated condition.

* * * * * *

I have the honor to be, very respectfully,
Your Obedient Servant.
JOHN RODGERS,
Rear-Admiral Superintendent.

HON. R. W. THOMPSON,
Secretary of the Navy, Washington.

The accompanying papers show that the death of two superintendents, Captain GILLISS and Admiral DAVIS, was either caused or accelerated by malarial fever, and that the death of Professors FERGUSON, SPRINGER and HUBBARD, could be traced directly to this cause. The prevalent fogs are shown to interfere with observations.

In short, this report brought prominently forward a fact which had always been patent, viz.: that it was almost a crime and certainly an extremely poor use of the resources of the observatory, to continue its astronomers and its instruments in the present situation. A petition was presented to Congress (1878, Jan. 10), from prominent men of science, asking for its removal, and Jan. 16, 1878, a bill was introduced by Mr. SARGENT in the Senate, providing for the appointment of a commission to select a suitable site. In the mean time a plan for the new building had been prepared at the observatory, submitted to all the prominent astronomers of the country for their suggestions, corrected and adopted. The report of the Commission, consist, ing of Admiral AMMEN, U. S. N., Colonel BARNARD- U. S. A., and LEONARD WHITNEY, Esq., was made 1878, Dec. 7. It recommended the purchase of "Clifton," a beautiful site of 45 acres in Georgetown, situated on Rock Creek.

Unfortunately, it was not learned until after the report was made, that it had been seriously contemplated to build a railway down the valley of Rock Creek. This report was not acted on, owing to the fact that the presence of a railway would seriously interfere with the stability of the instruments. Therefore a new commission was appointed Feb. 9, 1880, consisting of Senator W. P. WHYTE, Representative L. MORSE, and Admiral RODGERS, U. S. N., under a bill approved Feb. 4, 1880, which appropriated $75,000 to the purchase and selection of a suitable site. The officers of the Observatory were directed to examine the many sites offered for sale. These lay in three different parts of the city: first, north of the capital near the Soldiers' Home Park, and near the Baltimore & Ohio Railroad; second, north of the main part of the city; third, northwest of the city, in Georgetown. The preferences were for the sites in the first section. Each site that was at all eligible was tried in the following way: the fundamental observations depend upon the accurate measures of the zenith-distances of stars. As the zenith is not a visible point the nadir point (which can be made visible, and which is directly opposite the zenith point) is chosen. A box of quicksilver is placed immediately beneath the meridian instrument and the position of the reflected images of the spider lines of the instrument observed; when these coincide with the spider lines seen directly, the instrument is vertical or it is pointing to the nadir. Such observations as these have to be made at all hours of the night and day, and anything that seriously interferes with them will prevent the taking of satisfactory observations. The question then was, to try each of the proposed sites with this test and to unhesitatingly reject any site which did not fulfill the conditions. To do this a post was firmly planted in the ground. On the top of this a flat basin containing quicksilver was placed. A telescope was directed towards the quicksilver about dusk, so that the image of the pole star should be seen in the telescope. This image usually showed as a neat quiet round disk. The times of the passing of railway trains was known, and at these moments the image of the star was watched. For many of the places tried, the vibration of the mercury surface caused by the tremors of the ground was so great that no image of the star could be seen for many minutes during the passing of the trains. This was a fatal objection, since similar observations may have to be taken at any moment of the night or day.

For those places near a public road the experiment was varied by causing a loaded wagon to be driven rapidly up and down. The experiments were always made at least twice to avoid errors, and only those places rejected which were plainly unsuitable on this account. No matter what might be their other advantages, if they did not stand this test they were useless for astronomical purposes.

The places just north of the city were rejected on account of the smoke always rising from the mass of chimneys, an artificial and constant fog. In this way the choice has been narrowed down to two places. One directly south of the great park of the Soldiers' Home and one in Georgetown. The first is so situated that to make it suitable for observatory purposes a very large quantity of land would have to be bought; the second place can be bought with the appropriation. The matter is in this condition at present. No choice has been made by the commission as yet. There is, of course, a great desire on the part of landowners to force the commission to buy land in their neighborhood, but the choice must finally be made on the principles heretofore adopted. The new Observatory is to stand for a century at least and no small and petty personal considerations should be allowed to enter.

THE PRACTICAL VALUE OF SCIENCE.

"I have endeavored to state the higher and more abstract arguments by which the study of physical science may be shown to be indispensable to the complete training of the human mind, but I do not wish it to be supposed that because I may be devoted to more or less abstract and unpractical pursuits I am insensible to the weight which ought to be attached to that which has been said to be the English conception of Paradise—namely, 'getting on.' Now the value of a knowledge of physical science as a means of getting on, is indubitable. There are hardly any of our trades, except the merely huckstering ones, in which some knowledge of science may not be directly profitable to the pursuer of that occupation. An Industry attains higher stages of its development as its processes become more complicated and refined, and the sciences are dragged in, one by one, to take their share in the fray."—*Huxley*.

A BIT OF SUMMER WORK.

BY PROFESSOR BURT G. WILDER, M. D.

Notwithstanding the number of "Summer Schools of Science" to be in operation this season, many teachers are likely to pass the vacation at a distance from the facilities afforded by organized laboratories. How shall they employ their time?

Doubtless they all need rest, and in most cases at least a fortnight should elapse before any intellectual labor is undertaken. An equal period of repose may well occur just before the renewal of teaching in the Fall. But the teacher who hopes to make his instruction each year more thorough and successful than the last, will be pretty sure to spend the remaining month or two in the search of help from books, and, while regretting the vagueness of the information thus obtained, may seldom think of making it more real by personal observation.

Now it is true that in some branches of science this may require appliances not readily obtained. This is the case with Chemistry and Physics, and some parts of Natural History. But Botany and Entomology may be pursued under almost any circumstances, and I venture to suggest that at least one kind of *anatomical* work may be carried on with but a slight amount of apparatus.

Obviously, the summer is not the most favorable time for study of the viscera, while anatomical details respecting the muscles, vessels and nerves are not especially required for ordinary instruction. But the *brain* is not only the organ least satisfactorily treated in the text-books, but at the same time the one concerning which the most should be known, from the double standpoint of physiology and psychology.

But how can the teacher procure brains, and how shall he preserve them when obtained?

The question is a perfectly natural one in view of the prevailing impression that cerebral structure is to be learned from the human brain alone. So far from correct is this idea, that from a single animal brain, perfectly fresh or well preserved, more may be gained than the average medical student learns from the human brains usually examined in the dissecting-room.

This is due to the fact that, excepting the absence of the occipital lobes of the hemispheres, the brains of the cat, the dog, the rabbit and the sheep present nearly all of the structural features of the human brain, while their smaller size and greater accessibility better adapt them for manipulation and for the preservation of the numerous specimens which are needed to display all parts of the organ.

Of the animals above named the cat seems to be the most favorable subject. It is always and everywhere obtainable; the brain is larger than that of the rabbit, and more easily extracted than those of the sheep and most dogs.

Some features of the brain, as the coloration of different parts, and especially the relation of the gray and white substances, are better seen upon fresh specimens; but the beginner will do well to examine hardened brains first, so as to become familiar with the form and relative position of the parts, and with their names.

Among the instruments needed for the removal and dissection of the brain the most essential are a very sharp knife, and a pair of "wire-nippers" with the blades set at a slight angle with the handles. *

As an aid to the study of the brain any work upon Human Anatomy will be found useful. The best are those of "Quain" and "Gray." Descriptions, without figures, of the brains of the sheep, and of the dog and rabbit, are given in the little works of Morrell and Foster and Langley. With some modification these apply to the brain of the cat. †

Finally, it is hardly necessary to urge that outline drawings be made of the brain as a whole, and of its parts as exposed by dissection. If this is done, by the end of the summer the teacher will have become better able to appreciate the peculiarities of the human brain when one comes in his way, and will have laid a substantial foundation for the physiological and psychological instruction which he may be called upon to impart.

ANTIPATHARIA OF THE "BLACK" EXPEDITION.—In vol. iv. No. 4 of the *Bulletin* of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass. (February), L. F. Pourtales describes twelve species of this interesting group taken in the Caribbean Sea (1878–79). In determining the species an attempt has been made to use the differences in the shape of the polyps, as well as the disposition and form of the spines to draw characters for a much-needed revision of their classification. It would seem as if there were at least two different types of spines: the triangular compressed and the more cylindrical. These latter are generally more densely set, even assuming sometimes a brush-like appearance, as in *Antipathes humilis*, a new and wonderfully spinous species, figured but not described by Pourtales. These cylindrical spines are also unequal on the two sides of the pinnules, being longer on the side occupied by the polyps, with a very few around the polyps. The triangular spines are disposed regularly in a quincuncial order around the pinnules, and in a cleaned specimen nothing indicates the place formerly occupied by the polyps. In one series, however, *A. desbonni*, the spines are in regular verticils. There would appear to be a connection between the shape of the polyps and the shape and disposition of the spines. Those species with triangular spines have polyps with longer tentacles than those with cylindrical spines, and the tentacles have a greater tendency to become regular in shape.

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* These nippers are imported from Germany by H. Boker & Co., of New York, and are for sale by A. J. Wilkinson & Co., of Boston, and Treman, King & Co., of Ithaca, N. Y. They cost about 75 cents.

† Hektograph copies of instructions for the removal, preservation and dissection of the cat's brain may be had upon application to Mr. F. L. Kilborne, Anatomical Laboratory, Cornell University, Ithaca, N. Y.

ELECTRICITY AS POWER.

BY FRANCIS P. UPTON, ESQ.

In the early history of electrical science, many forms of engines were made, by which the power of electricity could be shown. Each was as wonderful as the other to the unthinking observer; for, without apparent combustion of fuel, work was done. We find, among the largest of these engines, one used in St. Petersburg, to drive a small boat, and one in this country to propel a train.

The United States Congress voted a sum of money to Prof. Page to carry on his experiments and he built a very efficient motor. After many experiments, though it was found that any amount of power could be obtained, yet the expense was so great as to make it of no practical value. In a small machine, the consumption of zinc might not be noticed, while in a large machine it would be found to burn exactly as the work was taken. Now that the doctrine of energy is clearly understood, the folly of the attempt can easily be seen. In a battery the fires are fed with an expensive metal. The energy developed by the zinc, thus used, was given to it artificially when it was reduced from the ore. In order to obtain a convenient fuel, both the coal and zinc ore must be mined, and the latter reduced, absorbing in the reduction a very small per cent. of the energy of the coal used in the process. Thus batteries for furnishing power consume a fuel at least fifty times more expensive than coal.

Besides the cost of fuel, the atmosphere, so to speak, in which the zinc burns, must be furnished to it artificially in the shape of acids or solutions. Though this has nothing to do with the theoretical cost, yet in practice, it is found to be the largest item of expense. It resembles furnishing a boiler with air made by a chemical process, so far as the economy of combustion is concerned. Yet the convenience and reliability of a battery to burn zinc has, where very small amounts of power are required, allowed of its use commercially, since steam is extremely difficult to manage in fractions of a horse power.

To-day the practice has been entirely reversed from what the first experimenters expected to realize. For electricity is now entirely made by means of steam engines to drive large motors. The last few years have brought the means of generating and using electrical currents to such a high state of perfection that power may be with economy transferred by them.

The loss in transferring is double; if a machine converts fifty per cent. of the power it receives from a staem engine, only fifty per cent. of that can be utilized, that is, twenty-five per cent of the original; thus wasting seventy-five parts out of each hundred of energy. A sixty per cent. machine can render effective thirty-six per cent.; an eighty per cent. machine can turn into useful work sixty-four per cent., and so on. This wasting of power in the transmission is more than counterbalanced in a great many cases by its delivery at the point where needed; for example, from a waterfall to a field for ploughing and threshing, as has been done in France; or from the shore to the water for the purpose of driving a torpedo boat, as has been done in this country.

Lately experiments have been made to show the application of electricity to railroads. Mr. Siemens, in Berlin, and Mr. Edison, at Menlo Park, are experimenting with electrical railroads. Mr. Edison uses the rails as conductors of electricity, the current going in one and returning in the other. The wheels are insulated, so that, by means of brushes on them, the electricity may be brought to the moter, which is on a carriage. The moter is simply one of Mr. Edison's generating machines, laid on its side, and connected by suitable mechanism to the axle of the driving wheels. On an experimental track of one-half mile length, a speed of twenty to thirty miles an hour has easily been reached, in spite of heavy grades and sharp curves.

For elevated and underground railroads, this method has many advantages; it does away with all the smoke and noise from the puffing of the locomotive, and substitutes for the many locomotives a few stationary engines scattered along the route. Mr. Edison feels very confident of success, since his troubles so far have all been in transferring the power from the armature to the driving wheels. He thinks that if the armature is only reliable, experiment will lead to proper mechanical devices for transferring the power from the quick-running armature to the slower driving wheels.

The road will be very useful in mountainous regions, since the engine is quite light, and can be carried by trestle work and light earth work, over any country. The engine and boilers are not in this case put on wheels and required to push themselves over grades and around curves, but are placed in the valley below. Perhaps in many cases they may be done away with and water used to drive the generators.

For beach roads, in grand exhibitions, as feeders to main lines, and in many ways it is easy to see that use may be made of a properly constructed road. The gentle fluid, which has so quietly, for many years been the swift messenger of man, is now showing that it is also able to be a strong and lusty servant, and carry any load that it may be asked to take.

ELECTRICAL INSECTS.—It is not generally known that there are insects which possess the peculiar electrical properties of the Raia Torpedo and Gymnotus Electricus. Kirby and Spence, in their entomology, describe the *Reduvius Serratus*, commonly known in the West Indies by the name of the *wheel bug*, as an insect which can communicate an electric shock to the person whose flesh it touches. The late Major-General Davis of the Royal Artillery, well-known as a most accurate observer of nature, and an indefatigable collector of her treasures, as well as a most admirable painter of them, once informed me, that, when abroad, having taken up this animal and placed it upon his hand, it gave him a considerable shock, with its legs, as if from an electric jar, which he felt as high as his shoulder, and dropping the creature, he observed six marks upon his hand where the six feet had stood. Two similar instances of effects upon the human system resembling electric shocks, produced by insects, have been communicated to the Entomological Society by Mr. Yarrell; one mentioned in a letter from Lady de Grey, of Groby, in which the shock was caused by a beetle, one of the common *Elateridae*, and extended from the hand to the elbow on suddenly touching the insect; the other caused by a large hairy lepidopterous caterpiller, picked up in South America by Capt. Blakeney, R. N., who felt on touching it a sensation extending up his arm, similar to an electric shock, of such force that he lost the use of his arm for a time, and his life was even considered in danger by his medical attendant.

SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

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To Correspondents.

All communications should be addressed to the Editor—Box 3838, P. O., New York—with name and address of writer, not necessarily for publication without consent.

Scientific papers and correspondence intended for publication, should be written *legibly* on one side only of the paper. Articles thus received will be returned when found unsuitable for the Journal.

Those engaged in Scientific Research are invited to make this Journal the medium of recording their work, and facilities will be extended to those desirous of publishing original communications possessing merit.

Proceedings of Scientific Societies will be recorded, but the abstracts furnished must be signed by the Secretaries.

Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply, may be written in the form of an article.

To Subscribers.

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To Advertisers.

Terms for advertising may be obtained at the office of Journal, 229 Broadway.

SALUTATORY.

In presenting to the public the first number of "Science," we would briefly define its aim and scope, so that its position in the periodical literature of the country may be clearly understood.

While Literature proper, and Art, both ornamental and useful, nay, almost every distinctive social and economic interest in the United States, have their several organs for the interchange of views or the diffusion of information, Science still remains without any weekly journal exclusively devoted to the chronicling of its progress, and the discussion of its problems.

This may be stated without disrespect to many excellent weekly journals restricted to special branches of science, or allied to trade interests.

The field being thus open, after consultation with many of the leading scientists in this country, it has been decided to publish "Science" in its present form. Its aim will be to afford scientific workers in the United States the opportunity of promptly recording the fruits of their researches, and facilities for communication between one another and the world, such as are now enjoyed by the scientific men of Europe.

A distinctive feature in the conduct of this Journal will be that each department of science will be supervised by some recognized authority in that field of research, and it is believed that the names of these Associate Editors will be a guarantee that accuracy be maintained so far as possible.

There will be a department of "*Notes and Queries*," which cannot fail to be of benefit to those engaged in original research. By this means many may attain the speedy solutions of difficulties which otherwise might cost them much unprofitable labor.

It is the desire of the Editor that "Science" may, in the United States, take the position which "*Nature*" so ably occupies in England, in presenting immediate information of scientific events; the Smithsonian Institution and other scientific bodies have promised their co-operation in this respect, and representative men in all branches of science have cordially volunteered their aid towards making "Science" as useful as its foreign contemporary.

We shall supply with each volume a comprehensive Index. The size of the journal is convenient for binding, and it should form a valuable work of reference in every library.

A short time must elapse before our arrangements, at home and abroad, can be completed, but we trust that this journal, even in its earliest stages, will be welcomed by all interested in scientific progress.

As one of its "Occasional Papers" the Boston Society of Natural History has published a volume of great value on the "Geology of Eastern Massachusetts," by W. O. Crosby. It is evidently the result of long and competent investigation, is well illustrated, and contains a large and well-printed geological map of the region treated of.

Interesting discoveries are reported from Italy. Near Este, in the Venito, at the foot of the Eugancian Mountains, Prof. Prosdocismi discovered a prehistoric burial ground with many bronze and clay vessels. Eighty-two tombs were found, of which forty-four seemed to have been opened already by the Romans, while the contents of the others seemed untouched. The urns belong to three different periods; some are stained black with linear ornaments; others are striped red and black. Some vases are of such exquisite workmanship that they could, even to-day, serve as patterns. A small case of bronze is adorned with human and animal figures.

[illegible] of Leibnitz [illegible] a rough [illegible] is to be erected at Leipzig [illegible] the eastern side of [illegible] Church [illegible] philosopher [illegible]

The announcement [illegible]

[illegible] wide range [illegible]

[illegible] charm even to the [illegible]

[illegible] Mr. [illegible]

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A bronze statue of Leibnitz, measuring 3½ metres in height, is about to be erected at Leipzig, on the southern side of St. Thomas Churchyard. This memorial to the great German philosopher will be executed by Professor Hänel, of Dresden.

THE announcement is made that Dr. Carpenter the well known Microscopist, and author of one of the best works on the subject, will pay a visit to the United States during 1880. We can accord him the promise of a warm reception in this country, where he will be welcomed by all classes of Scientists, for his researches have covered a wide range of scientific investigations, which were recorded in language so felicitous, that he gave a charm even to the most abstruse subjects.

THE Twelfth and Thirteenth Annual Reports of American Archæology and Ethnology contain, as usual, several papers of great ethnological interest. From the Report of the Curator, Mr. F. W. Putnam, it is evident that much excellent work continues to be done in the museum, which is rapidly becoming one of the most valuable repertories of ethnology in the world. The papers are all connected with American ethnology, the most important probably being that of Mr. Baudelier, on the Social Organization and Mode of Government of the Ancient Mexicans.

ICHTHYOPHAGY is about to receive a new impetus by the organization of the Ichthyophagous club, the object of which is "to reveal to gourmets the unsuspected excellence of many neglected varieties of fish-food, and to make manifest to the people at large the still untried capacity of sea, lake, and river, to yield the materials of human noruishment." The first dinner of the Ichthyophagists will take place at the Rockaway Beach Hotel on the 30th instant, which though partially experimental, will include enough familiar components to satisfy the least adventurous taste.

The President of the club is John Foord, Esq., managing Editor of the New York Times, and Mr. E. G. Blackford, Treasurer, who will receive the names of those who desire to attend the dinner, and enroll themselves as Ichthyophagists.

DIATOMACEÆ *v.* DESMIDIACEÆ.

Dr. Jabez Hogg, the well-known professional microscopist and author of "The Microscope—Its History, Construction and Application," recently wrote a letter, in which he incidentally spoke of "Bacillaria paradoxa" as a desmid. On being challenged to give his reasons for such a classification, Dr. Hogg wrote the following letter:

BACILLARIA.

[17575].—Mr. Fedarb (17334) wishes to know my reasons for classing Bacillaria amongst Desmidiaceæ, and I beg him to understand that it is not my classification, but that of botanists who long ago claimed them; and as biologists have thought fit to acquiesce in this arrangement, I fear there is now no help for the microscopist; he must quietly submit. Ehrenberg, as many of your correspondents well know, placed them in his great family of Infusoria, but Kützing, and other naturalists, a few years ago, regarded Bacillaria paradoxa as a species of Algæ. In the last edition of Pritchard's "Infusoria," edited by men of repute, Bacillaria are placed in the family of Surirelleæ. The reason assigned for this is, that diatoms and desmids differ very little in their general characteristics. Both without much impropriety are said to be cellular plants inhabiting salt and fresh water. They certainly differ, inasmuch as diatoms have a dense silicious skeleton, usually divisible into two parts, or valves, and are without coloring matter or chromule. Desmids, on the other hand, have a non-silicious envelope, which is separable into two segments, and are filled with green coloring matter—chromule. The vital phenomena presented are nearly identical. Diatoms are more lively and have a more animal-like motion, and their silicious skeletons are almost indestructible, and their envelope is very transparent and of a gelatinous nature. Desmids, I believe, are destitute of the sarcode element, and are quite destroyed on being submitted to boiling. The movements of Bacillaria paradoxa are so remarkable, and so little understood, that in commenting upon them I was anxious to elicit the opinions of those whose opportunities for studying their habit were much greater than my own.

With reference to Mr. Fedarb's request, that I should specialize the "contaminating agents" of impure water, he will find that I have made some attempt to deal with this difficult question in the present number of the *English Mechanic.*

JABEZ HOGG.

At our request, Professor H. L. Smith, of Geneva, N. Y., who has made a special study of the Diatomaceæ, has written a comment on Dr. Hogg's explanation, which appears to effectually dispose of this matter.

NOTE.

It is really astonishing to see what errors one may fall into when writing upon a subject about which one is ignorant. "*Ne sutor ultra crepidam,*" is a maxim which has not lost its force yet. The arguments, if one can call them so, adduced above for classing diatoms with dismids are easily disposed of. The author does not seem to be aware that the family name Bacillariæ (adopted in the early days of microscopical study, for what we now call Diatomaceæ) has long been dropped; the name was given from the then most striking genus, Bacillaria, of which one of the species is B. paradoxa. I am not aware that any respectable Botanist, or Biologist has ever claimed, as asserted in the above communication, that diatoms and desmids are to be classed together, except that both are algæ. If, for this reason, Bacillaria paradoxa can be called a desmid, we may call, *e., g.*, since both are phænogams, Hepatica triloba a Honeysuckle. What is meant precisely by saying that the editors of Pritchard place Bacillaria in the family of Surirelleæ, or how it has any bearing on the question of calling it a desmid, is difficult to understand. Really the writer of the above note has very little comprehension of what he is driving at. No one knew better than Mr. Ralfs, editor of Pritchard, article diatoms, the distinctions between diatoms and desmids, and nowhere does he fail to keep them distinctly separate. It is not merely the silicious frustule, "skeleton" as it is called above, for many of the diatoms are not silicious, but it is their different structure, different internal substance, different modes of growth, that marks them as distinct; moreover desmids are not found, as is stated, in salt water, though diatoms are, and very abundantly too. In fine, not a single respectable writer, either in botany or biology can be cited, from Kützing down, who will call Bacillaria paradoxa a desmid. The question is not one of both being algæ; this every one now-a-days concedes; but it is as to the propriety of calling an acknowledged diatom (one that once gave the family name to this group of organisms,) a dismid.

H. L. SMITH.

IMPROVED THERMO-ELECTRIC APPARATUS.

At a meeting of the Physical Society on April 24, several papers were read by Mr. R. H. Ridout, F.C.S., including one on an Improved Thermo-electric Apparatus, of which the following is an abstract :—

Whilst most instruments of research have undergone a process of developments the beautiful instrument of Melloni does not appear to have progressed since the day of its inception. Much annoyance arises from the pile and galvanometer being separate, and it is a very common occurrence for a pile to be used with any galvanometer that comes first. In reality they are parts of one instrument, and should therefore be fixed to the same base-board. Treating them as parts of one whole, many defects are to be found in theory and construction, and also in the mode of using. I have made a critical examination of each part, and embodied the improvements in an instrument which, with the assistance of Mr. Browning, combines great delicacy and simplicity.

The defects in the theory of the pile are, that the essential or internal resistance must always be much less than the external resistance, and from the low tension of the current, the disparity cannot be wiped out by using a great length in the galvanometer. In practice the faults are,—(*a*), the junctions are too deep, and cause short circuiting ; (*b*), the bars are too long and give unnecessary resistance ; (*c*), they are too numerous ; (*d*), the junctions are too slender ; (*e*), the mass of matter to be heated is too great. These are remedied (*e*, *a*, and *d*) by placing the bars in glass tubes, connecting with them plates of copper ; (*b*), bars made half usual length ; (*c*), a single pair only is used.

The defects of the galvanometer are:—(*a*), the space nearest the needles is not utilized by the wire ; (*b*), the needles are not of the best shape ; (*c*), the suspension is troublesome. The remedies are :—(*a*), the wire is made into flat ribbon, and wound in one bobbin. and the needles mounted so as to permit this ; (*b*), the needles are flat oblong plates, taken from the same piece of steel, and magnatized in one piece ; an agate cup and pivot remedy (*c*).

In manipulation the faults are : (*a*), the several parts are not mutually adapted ; (*b*), junctions by different metals are exposed ; (*c*), the pile and galvanometer are connected first, when, in reality, they should not be connected till the pile has been exposed, or else the current generated abstracts the heat from the hot side, and lowers its temperature.

In the complete instrument, as made by Mr. Browning, the pile consists of a pair of elements ½in. long, the copper connections being circular plates 1-100in. thick, and ⅝in. diameter. The pile is supported by its thick copper terminals above the galvanometer, which consists of a flat copper ribbon, making about 20 turns round a pair of astatic needles, 1in. long, and ⅛in. broad, supported on an aluminium frame, and resting on a fine pivot by an agate cube. A contact key is placed at one side, and makes the only connection in the middle of the instrument. The whole is inclosed in a glass shade, having a perforation at the height of the face of the pile. A glass cone protects the front face from the extraneous heat, and a glass cap the back one. A directing magnet placed above the pile enables the readings to be taken in any position.

The source of heat being placed in front of the pile, the shade is turned round till the hole is in the axis of the pile, and left exposed for say 30 seconds. Contact is then made, when the deflection of the needle indicates the strength of the current very nearly. A very distinct deflection may be obtained from a person standing 6ft. from pile, and a common candle affects it at 3ft. Further, it shows that the walls of a room are of different temperatures, and in any clear weather radiation into stellar space is very evident. The whole thing can be put ready for experiment in half a minute, while, with other forms, the necessary adjustment usually takes more time than the experiments. The same form of galvanometer is also supplied separately.

ERRORS OF REFRACTION IN THE EYES OF MICROSCOPISTS.

BY JOHN C. MORGAN, M. D.

It will, I think, be at once admitted that the requirements in construction and adjustment of glasses, and the results of work done, must vary greatly with individualities of the workers' eyes.

One of the most important, but least thought of, is *astigmatism*,* a condition known to oculists as a common cause of occipito-cervical headaches, sometimes so severe as to be considered due to grave hyperæmia of the brain, or to " brain-fag," etc.

This defect consists in a diversity of curvature; hence, of refraction of one meridian of the cornea, as, for instance, the vertical, with another meridian (horizontal). One of these meridians may be "far-sighted," the other "near-sighted," or the difference may be more moderate.

Some slight degree of this is quite common, as many of your readers will discover on viewing a black line at a convenient distance in these and other positions. In one it will look black and sharp ; in another, at a right angle, pale, ill defined, and as if the rays were cut off by a longitudinal slit in a diaphragm. Such a slit, turned in various positions, has a curious effect, illustrating the influence of loss of the rays. Astigmatism similarly affects vision ; only, in this, dispersion is the immediate cause of loss.

Another and very simple test of astigmatism is "the point of light"—e. g., a gas flame, reduced to its smallest dimensions (of the yellow), when, to a normal eye, across a large room, it appears as a round point ; but not so to astigmatic or to other abnormal eyes. Dispersion of rays results from imperfect focussing ; and the object seems larger in consequence (but less bright). If this dispersion be only in one meridian of the eye (astigmatism), the apparent enlargement will be in *exactly the same position*, and the image will be long, not round, and thus the individual may note the precise angle in which a cylindroid lens must be worn, for "correction," and the restoration of the round image. If this meridian be short-sighted, the cylindroid must be concave ; if far-sighted, convex.

The experiment may be varied by using a dark card, with a ⅛ inch round hole in it ; when placed before a window, strongly illuminated, the point of light appears, of course, and it is more accurate in shape than the flame.

One point more. Spasm of the focussing apparatus (called " spasm of accomodation ") may derange the sphericity of the eye, and so affect vision. *Strained* vision is liable to this. On the other hand, the same apparatus may be paralyzed, and *ordinary* vision deficient, whilst the focussing of the microscope entirely *corrects* it.

A linear marking, long or short, on a diatom, or a scale, or a cell, must suffer the same variation in divers positions after the passage of the rays through the best glasses. Some of the disputes as to these may be traced, doubtless, to this cause ; and probably may be set at rest by the use of *astigmatic spectacles* with the microscope.

These are merely lenses of prescribed cylindrical curvature, whose axis is placed in the position of the abnormal corneal meridian, whereby its curvature is corrected. The general effect is to render the whole cornea practically spherical in form.

Astigmatism has been an injury to painters, as Turner, whose later pictures (the power of accommodation, or self-correction, being lost with age) are discovered to be distorted in consequence ; the tendency being to exaggerate the size of the paler dimension in painting it.

On the contrary, in microscopic drawing, as with the camera lucida, the improperly pale line will be perpetuated, and the perspective misrepresented ; and distortion of dimensions generally may be perpetrated by the most careful observers, and endless disputes may thus arise.—*American Journal of Microscopy.*

* From the Greek, *a, privative*, and stigma, *a point*—want of focal point.

ASTRONOMY.

Mr. Stone, F. R. S., the Radcliffe observer, has recently drawn the attention of astronomers to a most interesting system of stars in the Southern Hemisphere, which seems to present a remarkable case of an apparent connection between stars widely distant from one another.

Astronomers are familiar with cases of double stars, which seem connected together in some manner analogous to the Earth and Moon. But these stars are very close to one another, being only separated by a few seconds of arc. In the present case the stars form an isosceles triangle, with sides nearly 20 degrees in length and with a base of over 30 degrees. This system of stars consists of two stars ζ^1 and ζ^2 *Reticuli*, forming the apex of the triangle, and scarcely as bright as the fifth magnitude; ζ *Toucani*, a fourth magnitude star at the southern base angle, and ε. *Eridani*, a star of between the fourth and fifth magnitude, at the northern base angle. All four stars are invisible from England. Besides the apparent motion in Right Ascension and the North Polar Distance, which is possessed by all stars, astronomers have long recognized the fact that many stars possess a real independent motion in space, which though much smaller than their apparent motion, is too large for its existence to remain in doubt. In general this proper motion, as it is called, amounts to only a small fraction of a second of arc per annum; but in some few cases it amounts to considerably over a second of arc, or even to over two or three seconds of arc.

In forming the Great Catalogue of Southern Stars, which has been the main work at the Royal Observatory at the Cape of Good Hope, whilst under his direction, Mr. Stone was led to examine all the cases of supposed great proper motion in the Southern Stars of the British Association Catalogue. In the greater number of cases they were found to arise from defective observations, but in some few cases they were confirmed.

The most noteworthy instances were the group to which Mr. Stone has directed attention. From a careful consideration of each case, Mr. Stone arrived at the following conclusions:

That the four stars of the group under consideration have proper motions much larger than the average proper motions of stars.

That the stars have a common proper motion of more than a second of arc.

That each star of the group is moving away from every other star of the group, by quantities which are small compared with the common proper motion of the group.

That, roughly speaking, the velocities of separation are larger, the greater the present angular separation of the stars.

From these conclusions it seems probable that all these stars are slowly moving away from one common point, so that many years back they were all very much closer to one another, and may have formed part of one common star system.

With the present rate of motion of separation it must have taken these stars over three million years to have moved to their present positions from a point where they would have been close together.

Mr. Stone remarks that it appears to him that such a system of stars like a^1 and a^2 *Centauri*, which consist of two binary stars moving round each other, and with a large common proper motion, having by reason of that large common proper motion been brought sufficiently near to another binary double star to disturb the orbital motion of each, and change the motion of each from closed to open orbits. The whole question opened by Mr. Stone is one of the highest interest, and deserves still further investigation, when the proper time arrives.

THE NEBULA IN THE PLEIADES.

Some twenty years ago, Temple, whilst at Venice, discovered, with a four inch telescope, a fine bright nebula close to the bright star *Merope* in the *Pleiades*. It was elliptical in form, and covered an area of nearly a fifth of a square degree. Temple showed it to Valz and other astronomers, and it was seen by Peters with the eight inch equatorial of the Altona Observatory.

Subsequently it was looked for by other observers, either without success, or else seen as a very faint, indistinct object. Even Temple, though it is true with another instrument and in another locality, describes it as being far less distinct than when first seen. Subsequently, when observing near Florence with larger instruments, Temple saw the nebula as large and as bright as ever. Prof. Schiaperelli of Milan also observed it with the fine refractor at Milan, and describes it as bright and distinct, and completely surrounding the star *Merope* whilst outlaying portions seemed to extend as far as Electra. Schiaperelli remarks, it is singular that so many persons should have examined the Pleiades without paying attention to this great nebula, which, nevertheless, is so evident an object on a clear sky. Maxwell Hall, in Jamaica, also found the nebula very bright with a four inch telescope, and shows it as nearly half a square degree in area. Several astronomers came to the conclusion that the nebula was variable. Others even doubted its real existence, and were inclined to ascribe its supposed observation to the effects of atmospheric action. Of late it has been drawn by several observers, so that its real existence cannot be questioned. During this year it has been looked for by Mr. Common with the great 37-inch reflector at Ealing. The nebula was seen as a distinct object of considerable extent, but beyond it, and right within the *Pleiades*, were discovered two others, both long elliptical nebulas of tolerable well defined form. There seems reason to believe, therefor, that the entire background of the *Pleiades* is nebulous.

Dr. J. Lawrence Smith, of Louisville, Ky., has made a personal investigation of the great meteorite which fell in Emmett County in 1879, having visited the spot for the occasion. An interesting report may be seen in the *American Journal of Science*. The external appearance was that of a mass, rough and knotted like mulberry calculi, with rounded protuberances projecting from the surface. The larger portions were of a gray color, with a green mineral irregularly disseminated through it. The total weight of the portions found amounted to 307 pounds. The stoney part of this meteorite consisted essentially of bronzite and olivine, the three essential constituents being silica, ferrous oxides and magnesia. An analysis showed, that in composition the meteorite contained nothing that was peculiar. Its position, however, among meteorites is unique, on account of the phenomena accompanying its fall, especially the great depth to which it penetrated beneath the surface, and also because of its physical characters and the manner of association of its mineral constituents.

NATURAL HISTORY.

The Eggs of Eels.—We direct attention to an alleged discovery of eggs in eels, and also to the fact that both sexes had been observed nearly two hundred years ago. The following extracts are made from the Proceedings of the Royal Society about the year 1690:

"Until about twelve months since, it was currently believed that eggs had never been seen in eels, and it was considered quite an interesting discovery when a New England fisherman then discovered them in situ, and also observed specimens of eels both of the male and female sex.

"Thus a vexed point which had been discussed for two hundred years, was settled satisfactorily, when, as I understand, Professor Packard confirmed the fisherman's discovery.

"Recently, while looking over some papers read before the Royal Society of England, dating very far back, I found that a Mr. Benjamin Allen about the year 1690, read a paper before the society, claiming to have examined two eels, and 'found one with egg,' and another with 'fixed young ones, fastened to very small placentæ each, which was fixed to the intestine.' 'The eggs were on the outside of the intestine.' He also said, 'the parts distinguishing the sex are discoverable; those of the male affix to the extremity of the kidney; the female had a slender gland transversely lying near the bowel.'

"A discussion followed, and a Mr. Dale raised a doubt on account of Mr. Allen's anatomical details being inconsistent with nature, and from the fact of Leuwenhoek finding a uterus in all eels he examined, and also 'masculine seed,' from whence he conjectured they were Hermaphrodite.

"He, however, so far confirmed Mr. Allen on one point, as to state that one Walter Chetwynd, Esq., had in the month of May, 'found them to be viviparous, by cutting open the red fundaments of the females, from whence the young eels would issue forth alive.'

"No other member is reported as having spoken on the subject, and so the matter rested."

THE MODE OF SUCKLING OF THE ELEPHANT CALF.—In some of the accounts recently published of the birth of an elephant in a menagerie in America it is stated that up to this time naturalists had always believed that the elephant calf obtained its mother's milk by means of its trunk, and not directly by the mouth.

Whether this be the case or not, Aristotle was certainly an exception, as the following passage from the twenty-seventh chapter of the sixth book of his "Historia Animalium" (Ed. Bekker, Oxford, 1837) clearly proves—"O *δὲ σκύμνος, ὅταν γένηται, θηλάζει τῷ στόματι, οὐ τῷ μυκτῆρι, καὶ βαδίζει και βλέπει εὐθὺς γεννηθείς.*—"And the calf, when it is born, sucks with its mouth and not with its trunk; and it both walks and sees as soon as it is born"— (Nature.) J. C. G.

AT a recent meeting of the Buffalo Microscopical Club Mr. Jas. W. Ward exhibited a piece of glass which had been over a picture on one of the walls of his residence. It was covered with a very peculiar and interesting species of fungus, and withstood the action of soap and water in attempting to remove it. He attributed the growth to the exhalations of the breath of persons who had been in the room, and since noticing this fungus on the glass had examined several of a similar nature in other rooms and found them alike. Mr. Ward's observations brought forth an interesting debate, relative to the observations of the different members of the Club on similar growths. Dr. Howe thought it similar to the fungus which attacks the body of the *Musca domestica* (house fly). These are contagious, and can be given by one fly to another. Dr. Barrett likened it to the fungi which permeates the walls of hospitals and other public buildings. President Kellicott, since the matter had been brought to his notice, had examined the windows of the Central School building, and the City and County Hall, and found fungi on them, although not to such an extent as on the glass Mr. Ward exhibited.

Since the publication of Mr. Ward's notes, a Mr. Thomas Garfield has written to the *Scientific American* attributing the so-called fungus to a stain, or rust, which is often observed by glass makers on glass, caused by an excess of soda or potash, which produces an efflorescence. Mr. Ward, however, re-affirms that the patches are fungi, and he is satisfied of their vegetable and superficial nature.

MICROSCOPY.

The Hayden Trial Evidence.—Dr. Treadwell writes to the "*American Monthly Microscopical Journal*" disclaiming the assertions regarding the possibility of identifying human blood, which had been attributed to him. It was charged that Dr. Tredwell claimed, after measuring only four corpuscles (having accidentally lost the others), that ranged from $\frac{1}{2357}$ " to $\frac{1}{3366}$" in diameter, to have asserted: "I am quite positive that these *were human blood corpuscles*, and that they did not belong to the pig, sheep, goat, horse or cat." Dr. Tredwell now says, "I gave no opinion whatever as to any blood being human blood, except in distinction from the blood of some animal or animals named, and I defy any person to show that I have ever expressed such an opinion in any of the numerous cases in which I have testified."

On the half shell.—Mr. K. M. Cunningham suggests a quick way of getting marine diatomaceæ: by taking a peck of fresh oysters and brushing the back of each into a basin of water, this process will give Pleurosigmæ and Coscinodisci in abundance.

Thin glass covers.—A microscopist has taken the trouble to measure the thin glass covers purchased at a first class house, and found that in two ounces but one third was correct in their thickness $\frac{1}{150}$ to $\frac{1}{200}$ of an inch, two thirds belonging to a cheaper grade. Only one sixty-eighth were $\frac{1}{200}$ of an inch in thickness, the majority being only fit for opaque objects.

Infection from Mosquitos.—The discovery that mosquitos carry filaria in their probosces, and infect the human subject with that much dreaded worm parasite, has attracted considerable attention among the English Microscopists. The matter has been brought before the Quekett Microscopical Club, by Dr. Cobbold, the President, who is one of the highest authorities on this subject. Particulars of various cases were given in which it was proved that those suffering from filaria had received the contagion from mosquitos, and mosquitos themselves infected with filaria were shown.

Filaria are very minute worm-like parasites, which on entering the human body, breed until they increase to countless numbers. By recent advices we learn they have the power of entering and leaving the blood at pleasure; they usually invade the circulation about seven o'clock in the evening, and increase until about midnight, after which time they retire to other parts of the system.

Trichinæ in Fish.—It is again asserted that trichinæ have been found in fish, this time at Ostend, in Belgium. This is against previous experience, but as it is stated the worms were found in the flesh, it appears more probable that the statement may be correct.

Curious fungous deposit.—Dr. P. C. Jensen gives a drawing in "*New Remedies*" of a peculiar organized deposit, existing in a number of specimens of Acid Phosphoric. Dil., of commercial grades. Under a power of 75 diameters its appearance is that of a fibrous network very analogous in appearance to the Tela Contexta, as found in the mosses, anastomosing and exhibiting very well defined oblong muriform cells placed end to end. In the interstices of its central ramification are seen small bodies resembling nuclei. These nuclei are nearly double the size of the diverging fibres constituting the mass of the deposit. The color of the deposit is of a grayish white, with diffusive and elastic properties.

ELECTRICITY.

An interesting experiment, which seems to have a bearing on the action of Edison's friction telephone, has been recently described by Herr Koch. When a plate of platinum or palladium is polarized by means of an electric current, the friction of these metals against a plate of moistened glass increases immediately. To measure the friction, Herr Koch uses the metal in the form of a hemispherical button, resting on the bottom of a glass cup, filled with pure or acidulated water. The button serves as pivot to a magnetic needle, which oscillates under the action of the earth ; the decrease of the oscillations measures the friction of the pivot. Polarization is produced by the current of the Daniell element, one pole of which communicates with the metallic button, while the other terminates with a platinum wire entering the water of the cup. The polarization by hydrogen produces no effect, but polarization with the pole oxygenated is found verv efficacious. The friction was increased, through this polarization, in the ratio of 2 to 3, and sometimes in that of 2 to 4. This increase of friction appears immediately the circuit is closed, and disappears immediately when the current is reversed ; but it disappears slowly, like the polarization itself, when the circuit is merely opened. It increases with the electromotive force of the polarization by oxygen. Palladium behaves like platinum. Gold (18 carat) gave no effect.

M. Desprez has lately attacked the problem of transmitting, by means of an electric current, the motion of a motor A to a receiver at some distance B, as a rigid axis between the two would do, so that the angular velocity of B should be always equal in amount and sign to that of A. (The particular case was that of getting within a railway carriage a rotation identical with that of the motor wheels of the locomotive.) On the shaft of the transmitter A are fixed two commutators, each of which reverses the current that traverses it twice each turn ; but the positions of the shaft corresponding to these inversions do not coincide; they follow each other at intervals of a quarter of a turn. The receiver consists of a permanent magnet or electro-magnet, between the branches of which are two straight electro-magnets, capable of rotating round an axis which coincides with that of the magnet. The currents sent through these electro-magnets from the shaft A produce the desired effect. This apparatus (it is noted) effects the transmission of work of a motor from one point to another *with conservation of the angular velocity* (which has not been realized in any electric motor hitherto used), the latter varying from 0 to 2,400 turns per minute. The alternating currents required may be generated by a magneto-electric machine. Again, any motion may be considered as the resultant of two movements of rotation ; hence this apparatus, with a simple mechanism added, would serve for transmission of a motion of drawing, or writing.

The steadiness of the incandescent light over that of the arc has long been understood, but hitherto the cost of the one has been so great that practically it was out of the question for general use. This will account for the little progress made by the Werdermann light. The cost is due principally to the consumption of carbon. Again, it is well known that the consumption of carbon, in an atmosphere containing no oxygen, or in a vacuum, is reduced to a minimum. Many inventors have tried to make lamps to retain a perfect vacuum, but have failed. It is easy, however, to make a water-tight joint, and by surrounding this with water Mr. Brougham has solved one of the problems of the incandescent lamp. The oxygen originally in the lamp globe is quickly exhausted, and then the atmosphere consists of gases which do not combine with carbon, and the result is very slow combustion or disintegration. The water-tight joints having been obtained in the manner above indicated, the globe is partially filled with water, so that when placed over the lamp globe, the water is well over the cap. This water globe is fastened by means of clamps and screws. The inventor states that while the carbon burns away at the rate of six inches per hour in the open air, it burns only one-eighth of an inch per hour when in the water-covered globe. This shows an enormous saving in the cost of carbon, and if it can be shown that the saving thus obtained is greater than the cost of the extra power absorbed by the incandescent lamp over that of the arc, a decided step will have been made towards furnishing a light that can without difficulty be applied to ordinary sized rooms. We have seen this lamp, and can testify as to its steady light.

So long as the liquid in the vessel is above the cap of the lamp, no atmospheric air can enter the lamp globe, and at the same time the heat from the lamp is carried off or dispersed and the light diffused. Provision may be made for the removal and replenishing of the liquid in or for causing it to circulate, but we are of the opinion that the ground-glass globe will prove more satisfactory as it is than any addition to the apparatus can make it. We made inquiries as to the liability of the copper wedge to melt, but its size and its connection with so large and such good conductors removes all tendency in this direction. — *Electrician.*

A simple method of perforating glass with the electric spark is described by M. Fages in a recent number of *La Nature.* The apparatus required consists (1) of a rectangular plate of ebonite, its size, for a coil giving 12 ctm. sparks, about 18 ctm. by 12 ; (2) of a brass wire passing under the plate and having its pointed end bent up and penetrating through the plate—not farther. This wire is connected with one of the poles of the coil. A few drops of olive oil are placed on the ebonite plate about the point, and the piece of glass to be perforated is superposed, care being taken not to imprison any bubbles of air. The olive oil perfectly accomplishes the object of insulating the wire. One has then only to bring down a wire from the outer pole of the coil, on the piece of glass, above the point of the lower wire, and pass the spark. By displacing the glass laterally for successive sparks, it is easy to make a close series of holes in a few seconds.

A new form of electric lamp has been invented by Mr. Charles Stewart, M. A. It consists of a number of square carbon rods placed radially upon a disc of wood, or metal, in such a manner that the inner ends of the carbon rods form a complete circle. There is a circular opening in the wooden disc through which the electric light is seen from underneath. The carbons which are all forced toward the centre by a uniform pressure, move forward as they are consumed, and together form the positive electrode of the lamp. The negative electrode consists of a covered hemispherical cup of copper which before the current enters the lamp, rests upon the ring formed by the carbons. On the current entering the lamp an electro-magnet raises the metal electrode, and the electric arc is then formed between the circle of carbon and the metal electrode. There is a flow of water through the latter to keep it cool. The inventor claims for his lamp the following advantages. (1). It is automatic in its action. (2). Burns for a considerable period. (3). Throws no shadows. (4). Simple and inexpensive in structure. (5). The intensity of the light may be increased if desired.

THE TELEGRAPH AND EARTHQUAKES.—A recent letter from Mr. W. A. Goodyear, now director of the governmental mining and geological survey of San Salvador, states that more than 600 shocks of earthquakes were felt there during the last ten days of 1879. They were heaviest about Lake Ilopango, where a shock occurred on the 23rd of December, which broke the telegraph wire asunder and "made the ground on which we stood a perfect network of cracks, opened new springs of water, increased the rivulets in the vicinity to ten times their usual volume, muddied the waters of the lake in many places, and rolled hundreds of thousands of tons of rocks down the steep hills in the form of landslides." As a sequal to these earthquakes, came the irruption of a volcano in the middle of Lake Ilopango on the night of January 20th to 21st. The volcanic island resulting now measures over five acres in extent, and shoots up a column of steam into the air over 1000 feet in height. This is the first instance we have heard of earthquakes interrupting land telegraph lines, though there are cases on record of their interrupting cables.

Notes and Queries.

[1.] I am studying the character and extent of a substance called "Tuckahoe, or Indian Bread," for its Ethnological interest. I find that my knowledge of Botany is not sufficient, and desire reliable information upon the following points :

What is the nature of its growth and production ?
What is its geographical distribution ?
Its former use and preparation ?
In what kind of soil is it found ?
What authors have mentioned it ?
By what botanical names is it known ?
Has it any medicinal properties ?

J. H. G.

GENERAL NOTES.

FORMATION OF VINEGAR BY BACTERIA.—E. Wurm has investigated this matter, and his results prove, without doubt, that an active formation of vinegar from alcohol is obtained by means of Myccderma aceti (Bacterium mycoderma—Cohn), thus supporting Pasteur's view.

ORGANISMS IN BEET SAP.—The bodies known as "frog-spawn," which make their appearance after a time in the sap of the beet root, prove, on microscopic examination to be a species of bacterium, called by L. Cienkowski, Ascoccus Bilrothii.

PTYALIN AND DIASTASE.—T. Defresne has found that ptyalin converts starch into sugar, in the presence of impure gastric juice, as rapidly as it does in the mouth. Its action is, however, suspended by pure gastric juice; but on passing into the duodenum the ptyalin again becomes active. Diastase, on the other hand, is completely deprived of its power of converting starch into sugar by hydrochloric acid or by pure gastric juice. (Compt. Rend., 89, 1070.)

ABNORMAL COMPOSITION OF MILK.—According to C. Marchaud (Bied. Centr., 1872, pp, 769–770), the usual composition of human milk is as follows: butter, 36.8 ; lactose, 71.1 ; proteïn, 17 ; salts, 2.04, and water, 873 parts per thousand. When the amount of butter rises to above 52 parts, the milk is injurious to the child. The quantity of proteïn, which is much less than in cow's milk, cannot be exceeded without ill effects.

NUTRITIVE VALUE OF GRASS AT VARIOUS STAGES OF GROWTH.—E. von Wolff and others (Bied. Centr., 1879, pp. 736–744) cut grass three times in the early summer, in the years 1874 and 1877 ; the first cutting took place about the middle of May, the second at the beginning and the third at the end of June. The second cutting appeared to give the best results in the case of animals experimented upon, namely sheep and horses ; and, as a rule, it was found that more nitrogenous matter was excreted by the latter than by the former.

ANALYSIS OF TWO ANCIENT SAMPLES OF BUTTER.—G. W. Wigner and A. Church have examined a sample of Irish bog butter, which cannot be traced with any certainty to a particular locality. There is no doubt, however, that it is a perfectly authentic specimen, probably 1000 years old. The following results were obtained : volatile fatty acids, calculated as butyric, 6 per cent ; soluble fatty acids, not volatile, 42 per cent ; insoluble fixed fatty acids, 99.48 per cent ; glycerol, minute traces. The insoluble fatty acids contained 9 per cent. oleic acid, and 91.0 per cent stearic and palmitic acids.

The other sample of butter, which is much older, was taken some time ago from an Egyptian tomb. It dates from about 400 or 600 years before Christ. It was contained in a small alabaster vase, and had apparently been poured in while in a melting state. In appearance, color, smell and taste, it corresponds closely with a sample of slightly rancid butter. Analysis shows that the sample has not undergone any notable decomposition.

CHLORIDE OF PLATINUM.—Dissolve the metal in hydrochloric acid, 5 parts ; and nitric acid 3 parts—a florence flask is convenient for this purpose. When all the metal is dissolved transfer the solution to a porcelain evaporating dish, and apply heat until nearly the whole of the acid is expelled. Dissolved in water or in ether chloride of platinum is useful for imparting to brass articles a steel like appearance.

THE EFFECT OF CARBONIC ACID IN THE AIR UPON CROPS.—According to M. Marie-Davy, (Compt. rend. 90, pp. 32-35), an examination of the determinations of the amount of carbonic anhydride in the air, which have been made daily during the last four years at Montsouris, seems to show that the best crops have been produced in those years when the amount of carbonic anhydride has been below the average. The carbonic anhydride varies inversely with clearness of the sky, and is influenced by the oscillations of the great equatorial atmospheric currents.

RESPIRATIVE POWER OF MARSH AND WATER PLANTS.—It is a well-known fact that these plants are able to thrive in media which contain little or no oxygen. They are all very poor in nitrogen, and E. Freyberg has shown by a number of experiments, that this latter property accounts for the former. His investigations prove that the respirative power of plants varies with the amount of nitrogen they consume, and this, taken in conjunction with the fact that water-plants contain large air chambers which do not often need refilling, accounts for their being able to exist in media which contain very little oxygen.

A RAILWAY BREAK, which is instantaneously applied and continuous in its action, and which the inventor proposes to render automatic, is described by M. Hospitalier in *La Nature*. It is worked by means of two of the secondary batteries of M. Planté, each of these being charged by three Daniell cells. The action of the apparatus is dependent upon the adhesion of an electro-magnet to the axle of the wheels, by means of which two chains attached to levers carrying friction blocks, are wound upon a drum.

ARTIFICIAL DIAMONDS.—In regard to the successful work of Mr. Hannay, of Glasgow, in producing perfect artificial diamonds, it may be well to bear in mind the similar investigation carried on by Despretz, the noted French chemist. Some authorities allege that the results obtained by Despretz were in advance of those reached by Mr. Hannay, yet the former, at the conclusion of five years of labor, made the frank acknowledgment that he had not found the diamond proper, although he had obtained crystals of pure carbon possessing all the characteristics of the coveted prize.

CYANIDE OF POTASSIUM.—There are many substances which are difficult to procure, whereas the materials of which they arecomposed are within the reach of everybody. To make Cyanide of Potassium, use the following formula :

Yellow prussiate of potash	8 parts.
Carbonate of potash	4 parts.

Reduce the prussiate of potash to a coarse powder, and dry upon an open plate over a slow fire ; next dry the carbonate of potash thoroughly, when both substances are to be intimately mixed. Put the mixture in a crucible or deep iron ladle, and place in a clear burnt coke fire. When fusion takes place, stir occasionally with an iron rod. When the mass is thoroughly fused allow it to continue in that state for at least a quarter of an hour. If on dipping the iron rod into the melted mass the compound appears *white* on cooling, the ladle may be withdrawn from the fire, allowed to rest for a few minutes, when the cyanide which is formed, must be poured in patches on an iron slab or flagstone, care being taken not to allow the dross, which is chiefly iron, to pass out with the clear fused cyanide. The "dross" should be shaken out separately, and when cold washed with water to dissolve out the adherent cyanide, after which the washing water may be filtered and used as a solution of cyanide when required. Keep the cyanide in a wide mouth bottle well corked, and labelled.

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SATURDAY, JULY 17, 1880.

A REMARKABLE PECULIARITY OF AN ANTHROPOID BRAIN.

BY E. C. SPITZKA, M. D.

Among the features of the cerebral surface, on whose presence or absence the differential characters of the human brain as compared with that of the anthropoid apes, have been established by anatomists, the so-called transition convolutions occupy a prominent place. As is well known, the occipital and parietal lobes of the human brain are connected with each other by means of short gyri, which bridge over those fissures, which if uninterrupted would separate these lobes like a chasm. First described by Gratiolet as *plis de passage*, and known among English writers as annectant or transition gyri; it is the one among them which borders on the great longitudinal fissure that has been most closely studied.

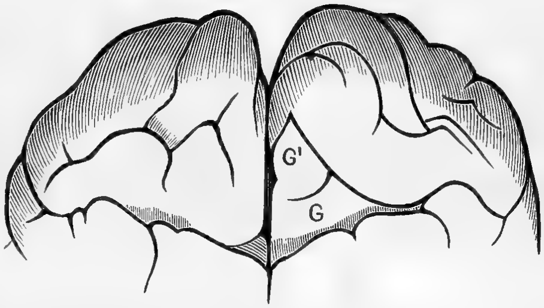
With exceptions to be noted, writers on the human and anthropoid brain agree in stating that the brain of the Chimpanzee differs from the human brain, in failing to exhibit this, the so-called first transition convolution, inasmuch as it is always concealed by the junction of the internal with the external perpendicular occipital fissures.

The same authorities also observe that while there is this sharp demarcation between the brain of the Chimpanzee and that of man, that another anthropoid resembles the human being in this very respect, namely the Orang, in which animal this fissure is present and superficial.

The decease of a large number of anthropoid apes which have been on exhibition at various times during the past few years at the New York Aquarium, and whose bodies were kindly placed at my disposal by the managers of that institution, has enabled me to extend the observations made on the brains of the Chimpanzee and Orang by previous writers.

The first Chimpanzee's brain obtained by myself, differed in no noteworthy respect from those described by Marshall, Gratiolet, Pansch and others, and was utilized for microscopical study.* The second, that of a very large animal, one which had reached the age of puberty, and weighing 389.86 grammes, presented the interesting anomaly I am about to describe, and whose demonstrable existence adds another proof to the many which have been accumulating, that there is no absolute and impassable line of demarcation between the human and simian brain. In the first place, as shown in the subjoined outline diagram, the occipital lobes of this brain are unsymmetrical.

On the right side the internal perpendicular occipital fissure does not coalesce with the external, while on the left side it does. The result is that on the right side, we have an excellently developed first transition gyrus [G, G′] evident and superficial, as in the human being, while on the left side it is concealed as in the ordinary Chimpanzee type. That is, the right side of the brain



Outline diagram of occipital end of cerebrum, dorsal view, x⅔.

shows a higher grade of development than the left. In so far as the left side is usually the better developed one this asymmetry is anomalous. Yet it shows that the old line of demarcation is not a correct one. Though the transition gyrus is concealed on the left side, yet a portion of it is visible, showing that on the whole this brain exhibits a tendency to a more human-like relation.

If we now proceed to compare the transition gyri of an Orang's brain with those of this Chimpanzee's right hemisphere, and of Man, we are struck by the observation that its disposition and proportions are more human-like in the Chimpanzee than in the Orang. And this applies to the Orang in my possession as well as to those figured by Tiedemann, Gratiolet and Bischoff. Taking the occipital lobe of the Orang as a whole, its physiognomy, if I may so term it, is lower and less human like, than that of the Chimpanzee.

The fact that the arrangement of the gyri and fissures bordering on the occipital fissures is thus shown to be inconstant, and that as Vogt has humorously shown, some of the South American monkeys resemble the human being more strongly in this respect than the average Chimpanzee and Gorilla (Pansch), should make us careful in basing fundamental characterization on such slight morphological factors.

I should state that Marshall and Bischoff while failing to ever find the gyrus under consideration appearing at the surface, yet have identified it as concealed in the depths of the perpendicular fissure.

In the occipital lobe of an imbecile recently executed for murder at St. Louis, and whose brain was referred to me for examination, I have found the external occipital fissure perfect as in the embryo, though intersected by collateral fisssures, and exhibiting a bevel, repeating to some extent its disposition in the anthropoid apes.

*The peduncular tracts of the anthropoid apes. *Journal of Nervous and Mental Diseases*, July, 1879.

THE Albert Medal of the Society of Arts for 1879, was presented to Sir W. Thomson for his electrical researches, especially those relating to the transmission of telegraphic messages over ocean cables, last week, at a meeting of the council, held at Marlborough House. At the same time the Albert Medal for the current year was presented to James Prescott Joule, F. R. S., for the researches by which he established the true relation between heat, electricity, and mechanical work. The *conversazione* of the Society was also held last week, when Mr. Donald Currie, M.P., was presented with the Fothergill Gold Medal, the award of which we recently announced.

DRY "MOUNTS" FOR THE MICROSCOPE.

BY PROFESSOR H. L. SMITH, HOBART COLLEGE, N. Y.

What shall we use to preserve dry mounts effectually? Many may think that nothing is easier; a cell of Brunswick black; a wax ring, or one of balsam; but the question is not thus easily to be disposed of. The writer has, within the last five years, mounted, or has had mounted under his supervision, some 15,000 slides of various microscopical objects, chiefly, however, foraminifera and diatoms; half of these were dry mounts.

Two things are important—the cell should be quickly and easily made, and the object when mounted in it should remain unchanged. There are very few cells as now made which will fulfil both these conditions, especially the latter. The deterioration of delicate dry mounts, and especially of test objects, sometimes within a few months after their preparation, but more or less certain in nearly every case, is well enough known.

All of the dry mounts of the Eulenstein series of diatoms, *e. g.*, which I have seen, are spoiled; and my cabinet is full of such preparations. Even Moller's do not escape, though they are, upon the whole, the most durable. I have abundance of amateur works that no doubt looked very beautiful just as they issued from the hands of the enthusiastic preparers, which are now, alas, mere wrecks; and worse than this, many choice and rare specimens, which I cannot replace, hopelessly ruined.

I believe that I was the first one to suggest the use of sheet wax for the bottoms of cells for foraminifera and other opaque objects, and of wax rings for diatoms and other transparent objects. (See Journal Quekett Club.)

The number of spoiled specimens, especially of diatoms and delicate transparent objects which I can now show, proves that this method of mounting is decidedly bad. I have lived to see the day when I shall be quite glad if the responsibility of suggesting such a nuisance as the wax ring can be transferred to some one else. For large opaque objects like most of the foraminifera, seeds, pollens, &c., the object itself is not so much injured, but the covering glass will, sooner or later, become covered (inside the cell), on the under surface, with a dew like deposit, which, when illuminated, will glisten almost like so many minute points of quicksilver, and though out of focus when the object is viewed, will show very disagreeably, like a thin gauze between; and with transparent objects these minute globules will not only dot the entire field, as so many dark or light points, but the object itself will appear as though it had been wetted.

Not long ago a well-known optician showed to me a spoiled slide "podura." The scales were very good and large—in fact, it was a slide which I had given to him, and it had been selected by myself in Beck's establishment in London as unexceptionably fine. This slide began slowly to show symptoms of "sweating." One scale after another appeared as though moisture had, in some mysterious way, penetrated to the objects; it was not water, however, for when the cover, after much trouble, had been removed, and warmed sufficiently to evaporate anything like water, the scales still exhibited the same appearance, and, in fact, the heat required to get rid of this apparent moisture was so great, that the scales were charred. When wax rings are used, this apparent wetting or "sweating" occurs quickly, and more disagreeable than this, innumerable elongated specks, possibly crystalline, appear all over the under surface of the cover-glass. The same trouble occurs when any of the ordinary asphalt preparations are used, and the only cement which I have thus far found to be tolerably successful is shell-lac thoroughly incorporated with the finest carbon (diamond black) such as is used in the preparation of the best printing inks; the solvent being alcohol, these rings dry rapidly, and the cover is attached by heating. Even these rings cannot be trusted, unless thoroughly dry, and spontaneous drying is better than baking. I have had preparations spoiled after mounting on asphalt rings, which had been made for over a year, and which had been subjected for several hours to the heat of a steam bath. With large, somewhat course objects, the defect is not so marked, but with delicate ones, and especially test objects, it is simply a nuisance. With care I think the shell-lac rings may answer pretty well. I have not tried the aneline colored rings. The moisture (whatever it is), and the crystalline specks, appear to be derived from the vaporizable parts of the wax, or cement, given off under conditions where one would suppose such a thing impossible; it is however a fact; I have the proof of it, and I dare say hundreds of others have, too plainly evident.

There is another mode of making cells which promises well for permanence. My attention was first called to this method by Dr. Tulk, of London, who suggested for this purpose the thin gutta-percha tissue, used by surgeons in the place of oiled silk, I have had special punches made, which cut neat rings from this tissue, and I have used these rings with the greatest satisfaction. I have no preparations of my own more than about two years old; these so far, show no signs of change. Dr. Tulk informs me that he has them ten years old, and still good as when new. I have noticed that in some recent papers in the mineralogical journals the writers, who with little experience, have so lauded wax rings, speak of "thin rubber" for rings, evidently they have seen somewhere the gutta-percha mount, and supposed it rubber—the latter will not answer, melted rubber will not become hard. One beauty of the gutta percha ring is the very moderate heat required; it is thus available for many objects which might be injured by the greater heat necesary for the asphalt or shellac rings. As these rings in the arrangement which I have spoken of, can be rapidly made, and as they can be kept for any length of time (shut away from the dust), they are at any moment ready as well as convenient for use. The preparation is first arranged, dried or burnt on the cover, the slide cleaned, a ring laid on the centre and on this the cover is placed; the whole is now held together by the forceps, and *slightly* warmed, just sufficient to soften the gutta percha; the forceps may now be laid aside, or used simply to press the cover home, warming the slide gently, also the cover; the perfect contact of the softened "tissue" with the cover and slide is easily recognized, and with a little care this can be effected very quickly, and nothing further is necessary. A finishing ring of colored cement makes a very neat mount, but it is not necessary.

ON MULTIPLE SPECTRA.

"Nunc age, quo motu genitalia materiai
Corpora res varias gignant, genitasque resolvant
Et qua vi facere id cogantur."
Lucretius ii., 61–2.

"Prima moventur enim per se primordia rerum:
Inde ea, quæ parvo sunt corpora conciliatu,
Et quasi proxima sunt ad vireis principiorum,
Ictibus illorum cæcis impulsa cientur
Ipsaque, quæ porro paulo maiora, lacessunt."
Lucretius, ii., 132–6.

"It is conceivable that the various kinds of matters, now recognized in different elementary substances, may possess one and he same ultimate or atomic molecule existing in different conditions of movement.

"The essential unity of matter is an hypothesis in harmony with the equal action of gravity upon all bodies."—*Graham's Researches*, p. 299.

In a recent paper* I showed that a study of the minute anatomy of spectra, both terrestrial and celestial, forces upon us the conclusion that both in the electric arc and in the hottest region of the sun the so-called chemical elements behave after the manner of compound bodies.

I then dealt more especially with the question of the basic lines in the various spectra, and it is clear that if at any one temperature, there be some lines only truly basic in the spectrum of any element, we at once divide the lines visible at that temperature into two groups, those which are basic and those which are not. This would give a compound origin to the lines, and this is the real point.

It is now years ago since the view was first held that the elementary bodies had double spectra, that is, that each, of at all events several, under changed conditions of temperature or electric tension, gave us now a fluted spectrum and now one composed of lines.

I glimpsed the idea some time afterward that the line spectrum was in its turn in all probability a complex whole, in other words that it was the summation of the spectra of various molecular groupings.

Recent work has to my mind not only shown that this is true, but that in the case of many bodies the complexity, and therefore the number, of the molecular groupings which give rise to that compound whole called a line spectrum, is considerable.

It is therefore important from my point of view to reconsider the evidence on which the assertion that the fluted bands and the line spectrum (taken as a whole) of a substance really belong to that substance, because if we find that this must be accepted and that it can easily be explained on the view that the two kinds of spectra are produced by different molecular groupings, the fact of other molecular groupings, giving rise to a complex line spectrum can be more readily accepted, contrary though it be to modern "chemical philosophy," as taught at all events in the text-books.

Plücker and Hittorf were, I believe, the first to point out that the same chemical substance, when in a state of gas or vapor, gave out different spectra under different conditions. On this point they wrote fifteen years ago:

"The first fact which we discovered in operating with our tubes . . was the following one:

"*There is a certain number of elementary substances which, when differently heated, furnish two kinds of spectra of quite a different character, not having any line or any band in common.*

"The fact is important, as well with regard to theoretical conceptions as to practical applications—the more so as the passage from one kind of spectrum to the other is by no means a continuous one, but takes place abruply. By regulating the temperature you may repeat the two spectra in any succession *ad libitum*." (Plücker and Hittorf on the Spectra of Ignited Gases and Vapors: *Phil. Trans.* Royal Society, 1865, part i. p. 6.)

Angström, whose name must ever be mentioned with the highest respect by any worker in spectrum analysis, was distinctly opposed to this view, and in the text which accompanies his *Spectre Normal* we find the following statement:

"Dans un Mémoire sur les spectres 'doubles' des corps élémentaires que nous publierons prochaînement, M. Thalén et moi, dans les Actes de la Société des Sciences d'Upsal, nous traiterons d'une manière suffisamment complète les questions importantes qu'on peut se proposer sur cet intéressant sujet. Pour le présent, je me borne à dire que res résultats auxquels nous sommes arrivés, ne confirment aucunement l'opinion émise par Plücker, qu'un corps élémentaire pourrait donner, suivant sa température plus ou moins élevée, des spectres tout-à-fait différents. C'est le contraire qui est exact. En effet en augmentant successivement la température, on trouve que les raies varient en intensité d'une manière trèscompliquée, et que, par suite, de nouvelles raies peuvent même se présenter, si la température s'élève suffisamment. Mais, independamment de toutes ces mutations, le spectre d'un certain corps conservera toujours son caractère individuel." *

Angström did not object merely on theoretical grounds. He saw, or thought he saw, room to ascribe all these fluted spectra to impurities.

He was strengthened in this view by observing how, in the case of the spectra of known compounds, there were always flutings in one part of the spectrum or another; a rapid induction naturally, therefore, ascribed all flutings to compounds. The continuity of the gaseous and liquid states of matter, let alone the continuity of Nature's processes generally, never entered into the question. For Angström, as for the modern chemist, there was no such thing as evolution, no possibility of a close physical relationship between elements, so called, driven to incandescence from the solid state, and binary compounds of those elements.

In a memoir, however, which appeared after Angström's death, and which, though under a different title, was in all probability the one referred to, this opinion was to a large extent recalled, and in favor of Plücker's view, in the following words:—

". . . Nous ne nions certainement pas qu'un corps simple ne puisse dans certains cas donner différents spectres. Citons, par exemple, le spectre d'absorption d'iode que ne ressemble en aucune façon au système des raies brillantes du même corps, obtenues au moyen de l'électricité; et remarquons de plus qu'en général tout corps simple, presentant la propriété d'allotropie, doit donner à l'état d'incandescence des spectres différents, pourvu que la dite propriété de la substance subsiste non seulement à l'état gazeux du corps, mais encore à la température même de l'incandescence. . . .

"Le soufre solid possède, somme on sait, plusieurs états allotropiques, et, d'après certaines observations, ce corps, même à son état gazeux, prendrait des formes différentes. Par conséquent, en supposant que cela soit vrai, le soufre gazeux doit donner plusieurs spectres d'absorptio, tandis que la possibilité d'un seul on de plusieurs spectres brillants dépendra de la circonstance suivante, savoir si les états allotropiques plus complexes de cette substance supporteront la température de l'incandescence, avant de se décomposer.

*"On the Necessity for a New Departure in Spectrum Analysis" (NATURE, vol. xxi. p. 8.)

* Angström sur "Le Spectre normal du Soleil," page 39.

"Il est bien évident que les cas dont nous venons de parler, ne forment pas une exception à la loi générale énoncée ci-dessus, savoir que chaque corps simple ne peut donner qu'un seul spectre. En effet, si l'on suppose que l'état allotropique est dû à la constitution moléculaire du corps, soit que les molécules se combinent les unes avec les autres, soit qu'elles s'arrangent entre elles d'une certaine manière, cet état allotropique possèdera au point de vue spectroscopique, toutes les propriétés significatives d'un corps composé, et par conséquent il doit être décomposé de la même façon que celui-ci par les effets de la décharge disruptive de l'électricité."[1]

I say that in this paper Angström recalled his own in favor of Plücker's view, because (as it has been remarked by Dr. Schuster[2]) the word "element" is used in a special sense—because in reality allotropic states are classed as compounds, that particular allotropic state which is to be regarded as truly elemental not being stated, nor any reason given why one should be thus singled out.

In the letter to which I have just referred Dr. Schuster gives an instance in which in order to show that elementary bodies did not really possess two spectra, a double spectrum was assigned to an acknowledged compound; the fluted spectra of hydrogen and carbon which differ from each other as widely as fluted spectra can, being both ascribed to acetylene.

Salet in his admirable work on the Spectra of the Metalloids,[3] was driven to the conclusion that many of these bodies must be held to possess two spectra. His conclusions are thus expressed:—

"Nous avons comparé le spectre d'absorption du brome et de l'iode à leur spectre électrique, et cette comparaison nous semble mettre hors de doute la possibilité des spectres doubles. . . .

"Nous avons obtenu, par voie électrique, un spectre primaire de l'iode correspondant à son spectre d'absorption. Le soufre, le sélénium et le tellure nous ont offert des spectres de combustion très-analogues aux spectres primaire obtenus par voie électrique, mais différant essentiellement des spectres des lignes. . . .

"Nous avons produit le spectre primaire de l'azote avec différents corps qui n'ont absolument de commun que l'azote; nous pensons donc avoir démontré qu'il appartient bien réellement à ce métalloïde." (*Annales de Chemie et de Physique*, 4 série, tome xxviii. pp. 70, 71).

In 1868 Wullner[4] gave his attention to this subject, and strongly supported Plücker's view of the existence of double spectra, indicating at the same time that the difference of temperature must be regarded as the sole cause of the phenomenon, adding, however, "a decomposition with further elements is not to be thought of." In the case of hydrogen he showed that the banded spectrum ascribed to acetylene really depended upon a change in the emissive power brought about by an alteration of temperature. Touching oxygen, he showed that three distinct spectra may be obtained, while in nitrogen two are observed.

I may say that in my early laboratory experiments I was at first led to think that, in the case of metallic vapors, Angström's first expressed opinion was correct, and I said so. But after more experience and knowledge had been acquired, I was compelled by the stern logic of facts to abandon it, and I showed, first, that more "orders" of spectra—to use Plücker's term—were necessary, and then that the line spectrum itself was in all probability compound; that is, that it was in some cases built up by the vibration of dissimilar molecules, some of which might even give us a fluted spectrum, if we could study them alone.

Although, however, in the views I have expressed on former occasions I have had the advantage of the support of the opinion of Plücker and Angström, and later of Dr. Schuster,[2] not to mention others, I am aware that though there is a general consensus among spectroscopic workers that double spectra cannot be ascribed to impurities, it is not absolute.

I propose therefore in this place to refer to a special case in which this question has been recently brought prominently forward.

[1] Ångström and Thalén's "Recherches sur les Spectres des Métalloïdes," p. 5.
[2] NATURE, vol. xv. p. 447.
[3] *Ann. de Chimie et de Physique*, 1873, vol. xxviii. p. 1.
[4] *Phil. Mag.*, sec. 4, vol. xxxvii. p. 405.

I have already stated that Angström, who was the first to map the line-spectrum of carbon, ascribed the flutings ordinarily seen in the carbon compounds to acetylene.

Now Attfield, in 1862, as a result of a most carefully conducted and admirably-planned set of experiments, came to the conclusion that the flutings were really due to carbon: in short, that carbon, like hydrogen, iodine, sulphur, nitrogen, and other bodies, had a fluted spectrum as well as one consisting wholly of lines.

The work of Attfield will be gathered from the following extract from his paper (*Phil. Trans.*, vol. clii. part 1, p. 221 *et seq.*):—

"On recently reading Swan's paper by the light that Professors Bunsen and Kirchoff have thrown on the subject, I came to the conclusion that these bands must be due to the incandescent carbon vapor; that, if so, they must be absent from flames in which carbon is absent, and present in flames in which carbon is present; that they must be observable equally in the flames of the oxide, sulphide, and nitride as in that of the hydride of carbon; and, finally, that they must be present whether the incandescence be produced by the chemical force, as in burning jets of the gases in the open air, or by the electric force, as when hermetically-sealed tubes of the gases are exposed to the discharge of a powerful induction-coil. . . .

"To establish the absolute identity of the hydro- and nitro-carbon spectra, excluding of course the lines due to nitrogen, they were simultaneously brought into the field of the spectroscope: one occupying the upper, and the other the lower half of the field.

"This was readily effected after fixing the small prism, usually supplied with spectroscopes, over half of the narrow slit at the further end of the object-tube of the instrument. The light from the oxyhydrocarbon flame was now directed up the axis of the tube by reflection from the little prism, while that from the oxynitrocarbon flame passed directly through the uncovered half of the slit. A glance through the eye-tube was sufficient to show that the characteristic lines of the hydrocarbon spectrum were perfectly continued in the nitrocarbon spectrum. A similar arrangement of apparatus, in which the hydrocarbon light was replaced by that of pure nitrogen, showed that the remaining lines of the nitrocarbon spectrum were identical with those of the nitrogen spectrum. In this last experiment the source of the pure nitrogen light was the electric discharge through the rarefied gas.

"The above experiment certainly seemed to go far towards proving the spectrum in question to be that of the element carbon. Nevertheless, the ignition of the gases having been effected in air, it was conceivable that hydrogen, nitrogen, or oxygen had influenced the phenomena. To eliminate this possible source of error, the experiments were repeated out of contact with air. A thin glass tube 1 inch in diameter and 3 inches long, with platinum wires fused into its sides, and its ends prolonged by glass quills, having a capillary bore, was filled with pure dry cyanogen and the greater portion of this gas then removed by a good air-pump. Another tube was similarly prepared with olefiant gas. The platinum wires in these tubes were then so connected with each other that the electric discharge from a powerful induction-coil could pass through both at the same time. On now observing the spectra of these two lights in the simultaneous manner previously described, the characteristic lines of the hydrocarbon spectrum were found to be rigidly continued in that of the nitrocarbon. Moreover, by the same method of simultaneous observation, the spectrum of each of these electric flames, as they

[1] Dr. Schuster's recently published investigations are as follows:—

Mr. Lockyer's investigations have shown that most bodies give us a continuous spectrum, as a gas, before they condense, and many at a considerable temperature above the boiling point. *Mr. Lockyer has rightly drawn the conclusion from these facts, that the atomic aggregation of the molecules is the cause of the different orders of spectra.*

That the discontinuous spectra of different orders (line and band spectra) are due to different molecular combinations, I consider to be pretty well established, and analogy has led me (and Mr. Lockyer before me) to explain the continuous spectra by the same cause; for the change of the continuous spectrum to the line or band-spectrum takes place in exactly the same way as the change of spectra of different orders into each other. Analogy is not a strong guide, yet some weight may be given to it in a case like the one under discussion, where experiment hitherto has failed to give a decided answer. (Dr. A. Schuster on the Spectra of Metalloids, *Phil. Trans.* Royal Society, 1879. Part i. page 38 and 89, note).

may be termed, was compared with the corresponding chemical flames, that is, with the oxhydrocarbon and oxy-nitrocarbon jets of gas burning in air. The characteristic lines were present in every case. Lastly, by similar inter-observation a few other lines in the electric spectrum of the hydrocarbon were proved to be due to the presence of hydrogen, and several others in the electric spectrum of the nitrocarbon to be caused by the presence of nitrogen. . . .

"The spectrum under investigation having then been obtained in one case when only carbon and hydrogen were present, and in another when all elements but carbon and nitrogen were absent, furnishes to my mind, sufficient evidence that the spectrum is that of carbon."

"But an interesting confirmation of the conclusion just stated is found in the fact that the same spectrum is obtained when no other elements but carbon and oxygen are present, and also when carbon and sulphur are the only elements under examination. And first with regard to carbon and oxygen. Carbonic oxide burned in air gives a flame possessing a continuous spectrum. A mixture of carbonic oxide and oxygen burned from a platinum-tipped safety-jet also gives a more or less continuous spectrum, but the light of the spectrum has a tendency to group itself in ill-defined ridges. Carbonic oxide, however, ignited by the electric discharge in a semi-vacuous tube, gives a bright sharp spectrum. This spectrum was proved, by the simultaneous method of observation, to be that of carbon plus the spectrum of oxygen. With regard to carbon and sulphur almost the same remarks may be made. Bisulphide of carbon vapor burns in air with a bluish flame. Its spectrum is continuous. Mixed with oxygen and burned at the safety-jet, its flame still gives a continuous spectrum, though more distinctly furrowed than in the case of carbonic oxide; but when ignited by the electric current its spectrum is well defined, and is that of carbon plus the sulphur. That is to say, it is the spectrum of carbon plus the spectrum that is obtained from vapor of sulphur when ignited by the electric discharge in an otherwise vacuous tube."

"Having thus demonstrated that dissimilar compounds containing carbon emit, when sufficiently ignited, similar rays of light, I come to the conclusion that those rays are characteristic of ignited carbon vapor, and that the phenomena they give rise to on being refracted by a prism is the spectrum of carbon."

This question was next taken up by Morren. He wrote[1] (in 1865) fifteen years ago:

"A la réception de cet intéressant et substantiel Mémoire, j'avoue que je ne regardai pas d'abord comme fondée l'assertion de M. Attfield. . . .

"Je me suis donc mis au travail avec la pensée préconçue de combattre l'assertion émise par le savant anglais; mais pas du tout, il résulte au contraire des expériences auxquelles je me suis livré que M. Attfield a raison, et que c'est bien la vapeur du carbone qui donne le spectre indiqué plus haut. . . .

"Si on fait brûler le cyanogène au moyen du chalumeau à deux courants, en faisant arriver au centre de la flamme du cyanogène un courant d'oxygène très-pur (cette condition est indispensable), on voit se produire un des plus beaux effets de combustion possible, et cette expérience est certainement une des plus magnifiques qu' on puisse réaliser sur la combustion des gaz. Il se produit, au milieu de la flamme *rosé-violâtre* du cyanogène, une boule d'un blanc vert éboulissant qui rappelle la lumière électrique produite par le courant de la pile entre deux charbons de cornue. Si le spectroscope est dirigé sur cette brillante lumière, on aperçoit, avec une splendeur merveilleuse, le même spectre de la partie bleue des flammes hydrocarburées. Ainsi donc c'est du charbon seul, mais à l'état de vapeur, qui forme cette boule brillante qui plus loin, par son union avec l'oxygène, va passer à l'état d'acid carbonique. Du reste ce spectre n'est pas seul; avec lui on voit, mais très-effacé, le spectre spécial du cyanogène, et celui-ci tend de plus en plus à disparaître à mesure que l'oxygène arrive avec plus d'abondance et brûle de mieux en mieux le cyanogène. Quant au spectre de l'azote, on ne l'aperçoit pas dans cette vive lumière. Le magnifique éclat de ce beau spectre, le plus beau qu'il m'ait été donné de voir, permet de bien comprendre l'aspect creusé et ombré avec une teinte croissante qu'on remarque dans les parties qui n'ont pas de raies brillantes, et même entre ces raies."

[1] *Annales de Chimie et de Physique*, 4 série, tome iv. p. 309, 312.

Four years later Dr. Watts devoted himself to this subject, and in 1869 his work was thus summarized by himself:[1]

"This spectrum [that consisting of the flutings in question] may be obtained from the flame of any hydrocarbon, though in many cases, owing to the faintness of the spectrum, only some of the groups can be recognized. In the flame of an ordinary Bunsen burner δ and ε are easily seen, γ and f are much fainter, and the red group can not be detected.

"This spectrum is proved to be that of carbon, inasmuch as it can be obtained alike from compounds of carbon with *hydrogen*, with *nitrogen*, with *oxygen*, with *sulphur*, and with *chlorine*. I have obtained it, namely, from each of the following compounds: olefiant gas, cyanogen, carbonic oxide, naphthalin, carbonic disulphide, carbonic tetrachloride, amylic alcohol, and marsh-gas."

That these conclusions, successively arrived at by Attfield, Morren, and Watts, are sound, I shall show in my next notice.—("*Nature*.") J. NORMAN LOCKYER.

(*To be continued.*)

[1] *Phil. Mag.*, October, 1869.

VALUE OF BISULPHIDE OF CARBON IN MICROSCOPICAL DEFINITION.

AT the last meeting of the R. M. S. (the last of the session), on the 9th instant, a paper was read by Mr. J. W. Stephenson, treasurer of the society, discussing the relative visibility of objects mounted in media of different refractive indices. Some time ago, Mr. Stevenson called attention to the fact that if diatoms were mounted in bisulphide of carbon their fine structure was rendered far more visible than when mounted in Canada balsam. Since the explanations given by Professor E. Abbe on the introduction of his new expression for apertures (*i.e.*, "numerical aperture"), by which the relative resolving power of different objectives is seen by the reading of the numerical apertures, Mr. Stephenson has come to the conclusion (in which he stated Professor Abbe agreed with him) that the *visibility* of objects is dependent on the *difference* of the refractive indices of the *object* observed and the *medium* in which it is placed. This he illustrates as follows:

Taking the refractive index of air as 1.0, and diatomaceous silex as 1.43, the visibility may be expressed by the *difference* 43.

Mr. Stephenson gave the following table:—

Refractive indices (taken approximately).		Visibility of silex (Refr. index = 1.43).
Water	= 1.33	10
Canada balsam	= 1.54	11
Bisulphide of carbon	= 1.68	25
Sol. of sulphur in bisulph.	= 1.75	32
,, phosphorus ,,	= 2.10	67

These data relating to visibility must, doubtless, be regarded in direct connection with the numerical aperture of the objectives of the illumination, as pointed out by Mr. Stephenson. He gave practical demonstrations of the views explained in his paper by exhibiting several slides mounted in the different media. I mention one slide of *Pleurosigma Elongatum*, mounted in sol of phosphorus in bisulphide of carbon, as presenting to the eye the strongest image that has come under my notice. According to Mr. Stephenson's theory, the visibility under these conditions would be about six times as great as that of the same object mounted in balsam. Is it possible to induce our professional object-mounters to take up the subject? Surely there are many amateurs of fine definition who would like to see the conditions of visibility pushed to the highest point, and who would amply repay the modicum of exertion needed to produce them.

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CO-OPERATION IN SCIENCE.

At this season of rest and recreation, naturalists are now wandering on many a hill and dale or upon the breezy shore, intent on collecting specimens which shall employ their leisure hours in the Autumn and coming Winter. From the close precincts of a city, who would not be free and join the little band, who with nets, bottles, corks and pins, are now seeking things bright and rare, which Nature has so lavishly scattered, and yet with such a cunning hand, that even the search and gathering affords delight to those who diligently seek.

The number of collectors is steadily increasing, but when we contemplate the immense ground to be covered, the necessity for increasing the force becomes but too apparent. Not only have the depths of mighty oceans to be explored, and the bottom of great seas investigated; but also tiny denizens of little rock pools must be described and classified. The surface of the earth might seem too immense to be minutely examined by the naturalist, but still he has to descend many hundred feet into the bowels of the earth to there find records of past life, so as to complete the great catalogue of Nature's works.

Earth, air, and water have thus to be surveyed, and still another world of life and form which is invisible to the natural vision of man. These microscopic forms are not to be neglected, for they decimate populations and destroy the industry of nations, and are so numerous, that the accumulation of their countless numbers rear up mighty ranges of mountains.

With such a work before collectors who are hopefully struggling to add to our garner of a knowledge of Nature, who cannot desire that their number may be increased. Yet how easily it might be done. From the fact that some of our best collectors are not scientific men, it would seem that a convenient division of labor is here suggested, namely: the amateur who collects, and the scientist who describes and classifies. Some men possess special qualifications for collecting, they discover localities which others would never dream of, and they have a happy knack of always finding what they are in search of. Such expertness may be largely due to constant practice, but the professional scientist finds himself tied to his study and laboratory, and has no time for these constant perambulations; he must therefore largely rely on the amateur collector for his material, as the latter must depend on him, for his technical and scientific knowledge.

If therefore, there are two classes who are dependent upon each other for the extension of a great and valuable work, let that co-operation be more distinctly recognized and accepted, for such is far from being the case at the present time, particularly in the United States, where of all places it might be expected to thrive.

One of the best means of organizing an efficient corps of amateur workers, is the establishment of local scientific societies, and the circulation of moderate priced but strictly first-class scientific journals.

In Europe, and especially England, scientific societies, chiefly supported by amateurs, are now doing excellent work; but in the United States (with a few exceptions) they drag on a miserable existence of poverty and inutility. What is the cause of this? Simply a want of co-operation between the amateur and professional scientist, and a due regard for each other's position.

To be effective, the officers and council of a scientific society should be taken from the best professional scientists in the neighborhood—men able to command and organize the work of the society, and give a character and tone to its proceedings. The amateurs must constitute the rank and file, accept that position, and keep to it, until by some special qualifications they may be promoted to a higher grade.

In England such is the case. Taking the case of the Quekett Microscopical Society, such men as Professor Huxley, Dr. Lionel Beale and Dr. Cobbold, the eminent helminthologist, have presided over the proceedings, and the result has been that over five hundred members have enlisted under such leadership. Compare this with the American Microscopical Society of New York established in the same year. Who ever heard of a paper read before this body, or a single piece of scientific work performed by one of its members? No fossil could be more inactive than this society; it exists on paper only, and for the benefit of a few officials.

A younger Microscopical Society, established in New York city about three years since, has been organized on an equally faulty basis, and now numbers but thirty members. The co-operation of the right men has never been asked, and probably would not be accepted, and in consequence, a future of inactivity and embarassment may be anticipated.

To make American Scientific Societies as effective as those in England, they must be organized on a sufficiently popular basis, to interest the sympathy and support of the public; and presided over by men of known scientific ability, whose presence will encourage the student, and give a character to the proceedings.

In regard to the aid given by scientific journals in promoting useful co-operation between the scientist and the student, we may state that one of the objects of "SCIENCE" is to promote such a consummation, and that aim will be constantly kept in view. As a step in the right direction we have here indicated some of the means, by which the icy barrier which now separates those who should be cordially united in a great work, may be gently thawed by the inspiring influences of united action and generous co-operation.

A SCHOOL of agriculture has been formed at Canterbury, New Zealand, situated at Lincoln, twelve miles from the city of Canterbury. This institution is under the direction of Mr. W. E. Ivey, comprises lecture theatre, library, museum, chemical laboratory. A farm of 500 acres is attached to the institution, a portion of which will be devoted to experimental purposes for testing the various methods of cultivation.

M. DAUBREE, director of the French School of Mines, has published an essay on Descartes, in which he summarises the services rendered by that philosopher to science. He reminds his readers that Descartes advocated the theory of an igneous origin for the earth, and he enters into a lengthened discussion of the objections which may be raised against the theory of actual causes.

THE Earl of Spencer, in a recent speech in the House of Lords, *admitted* the application of science to agriculture. He said: "Great attention had of late years been very properly called to the great aid which science gave to the various classes of manufactures and producers; and that principle applied with quite as great force to agriculture as to any other art. If science could enable our agriculturists to produce more from the land than they had hitherto done, it would add another to the many useful things it had been the means of accomplishing." These words might, with some propriety, have been spoken twenty years ago, and if they represent the present relation of science to British agriculture, much of the unprofitable results of farming in that country may be thus explained.

LORD Spencer said, that it had been at last decided to open a class for agriculture next August, at the Department of Science and Art. We commend Lord Spencer to a perusal of the reports of the department of Agriculture at Washington, especially that for 1878, in which the value of science to the agriculturist is very evident.

THE value of scientific journals has been attested to, by the humble class of astronomers who exhibit their telescopes at corners of streets in Paris, showing the moon, planets and other celestial objects which may be seen with telescopes of moderate quality. They state that since the publication of the *Astronomie Populaire* the number of their customers has nearly doubled.

IT appears from a statement by M. Flammarion that the scientific journalists of Paris meet monthly, when papers are read, and other business transacted.

M. J. M. GAUGAIN, the eminent French electrician, recently died at the age of seventy years.

At a recent trial in England, a gas company was sued for damages, the plaintiff having been rendered insensible by an escape of the company's gas, due to a breakage in their mains. The plaintiff alleged that he suffered for a considerable length of time after the accident, and was unfit for business. The jury accepted the view of Dr. Tidy and Dr. Hastings, who gave scientific evidence on the subject, they being of the opinion that the effect of inhaling coal gas was very transitory; and that if sufficient was not inhaled to cause death, it would shortly pass from the system, and its ill effects cease.

CORRESPONDENCE.

To the Editor of Science:

DEAR SIR:—In the Physical Laboratory we noticed last Winter a beautiful experiment with vapors. An alcohol lamp, burning, was put under the receiver of the air pump. A few strokes put out the flame. The air returning, a single stroke of the piston caused the receiver to fill with a dense and transient cloud, soon disappearing with a change of pressure in the receiver. This experiment has interesting relations to rain fall, and other meteorological phenomena.

G. M. MANSFIELD.

Laboratory of Asbury University.
Indiana, July 7, 1880.

A DARWINIAN STUDY.

By Alfred R. Wallace.

For the benefit of those unacquainted with entomology we may state, that many butterflies have two, or even three broods in a year. One brood appears in spring, their larvæ having fed during the preceding autumn, and passed the winter in the pupa state, while the others appear later in the year, having passed rapidly through all their transformations and thus never having been exposed to the cold of winter. In most cases the insects produced under these opposite conditions present little or no perceptible difference; but in others there is a constant variation, and sometimes this is so great that the two forms have been described as distinct species. The most remarkable case among European butterflies is that of *Araschnia prorsa*, the winter or spring form of which was formerly considered to be a distinct species and named *Araschnia levana*. The two insects differ considerably in both sexes, in markings, in color, and even in the form of the wings, so that till they were bred and found to be alternate broods of the same species (about the year 1830) no one doubted their being altogether distinct.

In order to learn something of the origin and nature of this curious phenomenon Dr. Weisman has for many years carried on a variety of experiments, breeding the species in large numbers and subjecting the pupæ to artificial heat or cold for the purpose of hastening or retarding the transformation. The result of these experiments is, that by subjecting the summer brood to severe artificial cold in the pupa state, it may be made to produce perfect insects the great majority of which are of the winter form, but, on the other hand, no change of conditions that has yet been tried has any effect in changing the winter to the summer form. Taking this result in connection with the fact that in high latitudes where there is only one brood a year it is always the winter form, Dr. Weismann was led to the hypothesis that this winter form was the original type of the species, and that the summer form has been produced gradually, since the glacial epoch, by the summer becoming longer and thus admitting of the production of a second or summer brood. This explains why the production of the winter form (*A. levana*) from summer larvæ is easy, it being a reversion to the ancestral type; while the production of the summer form (*A. prorsa*) from autumnal larvæ is impossible, because that form is the result of gradual development; and processes of development which have taken thousands of years to bring about cannot be artificially reproduced in a single season.

This hypothesis was supported by experiments with another two-brooded species, *Pieris napi*, with similar results, the winter form being produced with certainty by the application of cold to summer pupæ; and Mr. Edwards, in America, has made similar experiments with the various forms of *Papilii ajax*, finding that the summer broods can be changed into the winter form by the application of cold, while the winter broods can never be made to assume the summer form by hastening the process of transformation. In the Arctic regions and in the high Alps there is only one form of *Pieris napi*, which very closely resembles the winter form of the rest of Europe, and this could never be the least changed by rapidly developing the pupæ under the influence of heat.

Another curious case is that of one of the Lycænidæ (*Plebeius agestis*) which exhibits three forms, which may be designated as A, B, and C. The first two, A and B, are alternate broods (winter and summer) in Germany, while in Italy the corresponding forms are B and C, so that B is the summer form in Germany and the winter form in Italy. Here we see climatic varieties in process of formation in a very curious way.

That temperature during the pupa stage is a very powerful agent in modifying the characters of butterflies, is well shown by the case of *Polyommatus phlæas*. The two broods of this insect are alike in Germany, while in Italy the summer brood has the wings dusky instead of copper-colored. The period of development is exactly the same in both countries, so that the change must, it is argued, be attributed to the higher temperature of the Italian summer. It has been noticed that in Italy a large number of species of butterflies are thus seasonally dimorphic which are not so in Central and Northern Europe.

Dr. Weismann lays great stress on the varied effects of temperature in modifying allied species or the two sexes of the same species, from which he argues that the essential cause of all these changes is to be found in peculiarities of physical constitution, which cause different species, varieties, or sexes to respond differently to the same change of temperature; and he thinks that many sexual differences can be traced to this cause alone without calling in the aid of sexual selection. The general result arrived at by the laborious investigation of these phenomena is, that—"a species is only caused to change through the influence of changing external conditions of life, this change being in a fixed direction which entirely depends on the physical nature of the varying organism, and is different in different species, or even in the two sexes of the same species;" and he adds:—"According to my view, transmutation by purely internal causes is not to be entertained. If we could absolutely suspend the changes of the external conditions of life, existing species would remain stationary. The action of external inciting causes, in the widest sense of the word, is alone able to produce modifications; and even the never-failing 'individual variations,' together with the inherited dissimilarity of constitution, appear to me to depend upon unlike external influences, the inherited constitution itself being dissimilar, because the individuals have been at all times exposed to somewhat varying external influences." The present writer has arrived at almost exactly similar conclusions to these, from a study of the geographical distribution and specific variation of animal forms, as stated in an article on "The Origin of Species and Genera," which appeared in the *Nineteenth Century* of January last, and it is gratifying to find them supported by the results of a very different line of inquiry, and by the authority of so eminent and original an observer as Dr. Weismann.

A FOURTH STATE OF MATTER[1]

In introducing the discussion on Mr. Spottiswoode and Mr. Moulton's paper on the "Sensitive State of Vacuum Discharges," at the meeting of the Royal Society on April 15, Dr. De La Rue, who occupied the chair, good-naturedly challenged me to substantiate my statement that there is such a thing as a fourth or ultra-gaseous state of matter.

I had no time then to enter fully into the subject; nor was I prepared, on the spur of the moment, to marshal all the facts and reasons which have led me to this conclusion. But as I find that many other scientific men besides Dr. De La Rue are in doubt as to whether matter has been shown to exist in a state beyond that of gas, I will now endeavor to substantiate my position.

I will commence by explaining what seems to me to be the constitution of matter in its three states of solid, liquid, and gas.

I. First as to Solids:—These are composed of discontinuous molecules, separated from each other by a space which is relatively large—possibly enormous—in comparison with the diameter of the central nucleus we call *molecule*. These molecules, themselves built up of *atoms*, are governed by certain forces. Two of these forces I will here refer to—attraction and motion. Attraction when exerted at sensible distances is known as *gravitation*, but when the distances are molecular it is called *adhesion* and *cohesion*. Attraction appears to be independent of absolute temperature; it increases as the distance between the molecules diminishes; and were there no other counteracting force the result would be a mass of molecules in actual contact, with no molecular movement whatever—a state of things beyond our conception—a state, too, which would probably result in the creation of something that, according to our present views would not be *matter*.

This force of cohesion is counterbalanced by the movements of the individual molecules themselves, movements

[1] "On a Fourth State of Matter," in a letter to the Secretary of the Royal Society. By W. Crookes, F.R.S.

varying directly with the temperature, increasing and diminishing in amplitude as the temperature rises and falls. The molecules in solids do not travel from one part to another, but possess adhesion and retain fixity of position about their centre of oscillation. Matter, as we know it, has so high an absolute temperature that the movements of the molecules are large in comparison with their diameter, for the mass must be able to bear a reduction of temperature of nearly 300° C. before the amplitude of the molecular excursions would vanish.

The state of solidity, therefore—the state which we are in the habit of considering *par excellence* as that of *matter*—is merely the effect on our senses of the motion of the discrete molecules among themselves.

Solids exist of all consistencies, from the hardest metal, the most elastic crystal, down to thinnest jelly. A perfect solid would have no viscosity, *i.e.*, when rendered discontinuous or divided by the forcible passage of a harder solid, it would not close up behind and again become continuous.

In solid bodies the cohesion varies according to some unknown factor which we call chemical constitution ; hence each kind of solid matter requires raising to a different temperature before the oscillating molecules lose their fixed position with reference to one another. At this point, varying in different bodies through a very wide range of temperature, the solid becomes liquid.

II. In liquids the force of cohesion is very much reduced, and the adhesion or the fixity of position of the centres of oscillating molecules is destroyed. When artificially heated, the inter-molecular movements increase in proportion as the temperature rises, until at last cohesion is broken down, and the molecules fly off into space with enormous velocities.

Liquids possess the property of viscosity—that is to say, they offer a certain opposition to the passage of solid bodies; at the same time they cannot permanently resist such opposition, however slight, if continuously applied. Liquids vary in consistency from the hard, brittle, apparently solid pitch to the lightest and most ethereal liquid capable of existing at any particular temperature.

The state of liquidity, therefore, is due to inter-molecular motions of a larger and more tumultuous character than those which characterize the solid state.

III. In gases the molecules fly about in every conceivable direction, with constant collision and enormous and constantly varying velocities, and their mean free path is sufficiently great to release them from the force of adhesion. Being free to move, the molecules exert pressure in all directions, and were it not for gravitation they would fly off into space. The gaseous state remains so long as the collisions continue to be almost infinite in number, and of inconceivable irregularity. The state of gaseity, therefore, is pre-eminently a state dependent on collisions. A given space contains millions of millions of molecules in rapid movement in all directions, each molecule having millions of encounters in a second. In such a case the length of the mean free path of the molecules is exceeding small compared with the dimensions of the containing vessel, and the properties which constitute the ordinary gaseous state of matter, which depend upon constant collisions, are observed.

What, then, are these molecules? Take a single lone molecule in space. Is it solid, liquid, or gas? Solid it cannot be, because the idea of solidity involves certain properties which are absent in the isolated molecule. In fact, an isolated molecule is an inconceivable entity, whether we try, like Newton, to visualise it as a little hard spherical body, or, with Boscovich and Faraday, to regard it as a centre of force, or accept Sir William Thomson's vortex atom. But if the individual molecule is not solid, *à fortiori* it cannot be regarded as a liquid or gas, for these states are even more due to inter-molecular collisions than is the solid state. The individual molecules, therefore, must be classed by themselves in a disti ct state or category.

The same reason applies to two or to any number of contiguous molecules, provided their motion is arrested or controlled, so that no collisions occur between them ; and even supposing this aggregation of isolated non-colliding molecules to be bodily transferred from one part of space to another, that kind of movement would not thereby cause this molecular collocation to assume the properties of gas ; a molecular wind may still be supposed to consist of isolated molecules, in the same way as the discharge from a mitrailleuse consists of isolated bullets.

Matter in the fourth state is the ultimate result of gaseous expansion. By great rarefaction the free path of the molecules is made so long that the hits in a given time may be disregarded in comparison to the misses, in which case the average molecule is allowed to obey its own motion or laws without interference ; and if the mean free path is compatible with the dimensions of the containing vessel, the properties which constitute gaseity are reduced to a minimum, and the matter then becomes exalted to an ultra-gaseous state.

But the same condition of things will be produced if by any means we can take a portion of gas, and by some extraneous force infuse order into the apparently disorderly jostling of the molecules in every direction, by coercing them into a methodical rectilinear movement. This I have shown to be the case in the phenomena which cause the movements of the radiometer, and I have rendered such motion visible in my later researches on the negative discharge in vacuum tubes. In the one case the heated lampblack and in the other the electrically excited negative pole supplies the *force majeure* which entirely or partially changes into a rectilinear motion the irregular vibration in all directions ; and accordidg to the extent to which this onward movement has replaced the irregular motions which constitute the essence of the gaseous condition, to that extent do I consider that the molecules have assumed the condition of radiant matter.

Between the third and the fourth states there is no sharp line of demarcation, any more than there is between the solid and liquid states, or the liquid and gaseous states ; they each merge insensibly one into the other. In the fourth state properties of matter which exist even in the third state are shown *directly*, whereas in the state of gas they are only shown *indirectly*, by viscosity and so forth.

The ordinary laws of gases are a simplification of the effects arising from the properties of matter in the fourth state ; such a simplification is only permissible when the mean length of patch is small compared with the dimensions of the vessel. For simplicity's sake we make abstraction of the individual molecules, and feign to our imagination *continuous* matter, of which the fundamental properties—such as pressure varying as the density, and so forth—are ascertained by experiment. A gas is nothiug more than an assembly of molecules contemplated from a simplified point of view. When we deal with phenomena in which we are obliged to contemplate the molecules individually, we must not speak of the assemblage as *gas*.

These considerations lead to another and curious speculation. The molecule—intangible, invisible, and hard to be conceived—is the only true *matter*, and that which we call matter is nothing more than the effect upon our sense of the movements of molecules, or, as John Stuart Mill expresses it, "a permanent possibility of sensation." The space covered by the motion of molecules has no more right to be called matter than the air traversed by a rifle bullet can be called lead. From this point of view, then, matter is but a mode of motion ; at the absolute zero of temperature the inter-molecular movement would stop, and although *something* retaining the properties of inertia and weight would remain, *matter*, as we know it, would cease to exist.

NOTE BY THE DUKE OF ARGYLE.

In the very interesting communication from Mr. Crookes on "A Fourth State of Matter," which is contained in *Nature*, vol. xxii. p. 153, there is a paragraph at the end which advances, as it seems to me, some most disputable propositions.

Like many other questions of modern science, the question he raises is to a very large extent a question of definition. But questions of definition are questions of the very highest importance in philosophy, and they need to be watched accordingly.

Speculating on the ultimate conceptions of Matter which are affected by the discovery of it in "a fourth condition," Mr. Crookes says: "From this point of view, then, Matter is but a "mode of motion.'"

It has never appeared to me that this well-known phrase is a very happy one, even as applied to Heat. It is possible, of course, to consider Heat from this point of view. But then it is equally possible to consider all other phenomena whatever from the same point of view. Not only Heat, but Light, Sound, Electricity, Galvanism, and Sensation itself in all its forms, may be regarded as "modes of motion."

But at least in the application of this phrase to Heat there is an intelligible meaning, and not a mere confusion of thought. But as applied to Matter—as a definition of our ultimate conception of matter—it appears to me to confound distinctions which are primary and essential. "Motion" is an idea which presupposes Matter and Space. Motion has no meaning whatever except the movement of Matter in Space. To define Matter, therefore, as a "mode of motion," is to define it as Matter in a state of motion. But this definition necessarily implies that Matter can also be conceived as without motion, and accordingly Mr. Crookes is obliged to confess that "at the absolute zero of temperature inter-molecular movement would stop," and that after that, Matter would remain with all the "properties of inertia and of weight."

Again Mr. Crookes says: "The space covered by the motion of molecules has no more right to be called Matter than the air traversed by a rifle bullet can be called lead." No doubt this is true; but it implies what is not true, that the common idea of Matter is nothing but "the space covered by the motion of molecules." The popular idea attached to words of primary significance may not be always adequate or complete. But in my opinion they are generally much more near the truth, and more accurately represent the truth than most of the phrases which scientists are now inventing in the region of transcendental physics.

These phrases have their value and their interest as representing special and partial aspects of phenomena. But I hold that the unconscious metaphysics of human speech are often the deepest and truest interpretations of the ultimate facts of nature

ON A NEW JELLY-FISH OF THE ORDER TRACHOMEDUSÆ, LIVING IN FRESH WATER.

On Thursday last, June 10, Mr. Sowerby, the Secretary of the Botanical Society of London, observed in the tank in the water-lily house in Regent's Park a peculiar organism, of which he was kind enough to place a large number at my disposal on the following Monday.

The organism proves to be an adult medusa belonging to the order Trachomedusæ and the family Petasidæ of Haeckel's system (" System der Medusen," Erster Theil). It comes nearest among described genera to Fritz Müller's imperfectly known *Aglauropsis* from the coast of Brazil.

The most obviously interesting matter about the form under notice is that it occurs in great abundance in perfectly fresh water at a temperature of 90° Fahr.

Hitherto no medusa of any order has been detected in fresh water—except perhaps some stray estuarine forms (Crambessa ?).

It is exceedingly difficult to trace the introduction of this animal into the tank in the Regent's Park, since no plants have been recently (within twelve months) added to the lily-house, and the water is run off every year. Probably a few specimens were last year or the year before present in the tank, and have only this year multiplied in sufficient abundance to attract attention. Clearly this medusa is a tropical species, since it flourishes in water of the high temperature of 90° Fahr.

Mr. Sowerby has observed the medusa feeding on Daphnia, which abounds in the water with it.

The present form will have to be placed in a new genus, for which I propose the name Craspedacusta, in allusion to the relation of its otocysts to its velum.

It is one of the sub-class Hydromedusæ or Medusæ craspedotæ, and presents the common characters of the order Trachomedusæ (as distinguished from the Narcomedusæ) in having its genital sacs or gonads placed in the course of the radial canals. It agrees with all Tracholinæ (Trachomedusæ and Narcomedusæ) in having endodermal otocysts, and it further exhibits the solid tentacles with cartilaginoid axis, the centripetal traveling of the tentacles, the tentacle rivets (Mantel-spangen), the thickened marginal ring to the disk (Nessel-ring) observed in many Tracholinæ.

Amongst Trachomedusæ, Craspedacusta finds its place in the Petasidæ, which are characterized as "Trachomedusæ with *four* radial canals, in the course of which the four gonads lie, with a long tubular stomach and no stomach-stalk."

Amongst Petasidæ it is remarkable for the great number of its tentacles, which are *all* solid; and for its very numerous otocysts. Further, it is remarkable among all Hydromedusæ (velate medusæ, that is, exclusive of Charybdæa) for the fact that centrifugal radiating canals pass from the otocysts *into the velum, where they end cæcally.*

The genus may be characterized as follows:

MOUTH quadrifid, with four per-radial lobes.

STOMACH long, quadrangular, and tubular, projecting a good deal below the disk.

DISK, saucer-shaped, that is, flattened.

RADIATING CANALS 4, opening into the marginal canal.

GONADS 4, in the form of 4 oval sacs, depending into the cavity of the subumbrella from the four radiating canals.

MARGINAL or RING CANAL voluminous.

CENTRIPETAL CANALS (such as those of Olindias, Geryonia, etc.) absent.

TENTACLES solid; in three sets, which are placed in three superimposed horizons:—

1. A set nearest the aboral pole, of 4 large per-radial tentacles. These are the *primary* tentacles.
2. A second tier of (in large specimens) 28 medium-sized tentacles placed between these in four groups of seven. These are the *secondary* tentacles.
3. A third tier of (in large specimens) 192 small tentacles placed in groups of six between adjacent secondary tentacles. These are the *tertiary* tentacles.

TENTACLE-RIVETS (Mantel-spangen) connecting the roots of the tentacles with the marginal ring (Nessel-ring) are connected with all the tentacles of each of the three horizons.

OTOLITHS placed along the line of insertion of the velum —about eighty in number (fewer in small specimens). From sixteen to twenty are placed between successive per-radial tentacles arranged in groups of two or three between the successive secondary tentacles.

VELAR CENTRIFUGAL CANALS (which are really the elongated otocysts) are peculiar to this genus, passing from the otoliths (one inclosing each otolith) into the velum, and there ending blindly. They appear to correspond in character to the *centripetal* canals found in other Trachomedusæ in the disk.

OCELLI are absent.

[The presence of velar otocystic canals constitute the chief peculiarity of the genus Craspedacusta, and may necessitate the formation of a distinct family or sub-order for its reception. The minute structure of the otoliths and canal-like otocysts I am now engaged in investigating.]

The above characters are derived from the examination of *adult* male specimens, which were freely discharging ripe, actively motile spermatozoa.

The species may be known as CRASPEDACUSTA SOWERBII, nov. gen. et sp.—I name the species in honor of Mr. Sowerby, who discovered it, and to whose quick observation and courteous kindness zoologists are indebted for the knowledge of this interesting animal.

The sole character which I can give as specific over and above the generic characters summarized above is that of size. The diameter of the disk does not exceed one-third of an inch.

Locality.—The water-lily tank in the gardens of the Botanical Society, Regent's Park, London.

Very abundant during June, 1880. Probably introduced from the West Indies. E. RAY LANKESTER.
—*Nature.*

EVOLUTION OF LOCOMOTIVES IN AMERICA.

The question of priority in the use of the locomotive on railroads in this country is one of perennial interest. The literature on the vexed subject cemprises volumes. For the seven cities of Greece, which claim the honor of the birthplace of Homer, we have had almost as many States claiming the honorable distinction of first introducing the locomotive engine for service on the railroad. The idea of applying steam as the motive power on railroads had occurred to many of our engineers, stimulated as they were doubtless by the successful practice of England; and the introduction of the locomotive by Pennsylvania and South Carolina was almost synchronous; yet the former is fairly entitled to the distinction of priority,

Fortunately there is now living in San Francisco one of the veteran railroad men of the country, who is absolutely familiar with the interesting incidents of the early history of the railroad and the locomotive engine in this country.

The testimony adduced from these intelligent and trustworthy sources is absolutely conclusive; and it would seem that it ought to end the controversy about the claim to priority in the first practical use of the locomotive engine in this country.

1. The first locomotive engine placed and tried on any railroad in America was called the "Stourbridge Lion," and was imported from England for the Delaware and Hudson canal and railroad company. This engine arrived in New York May 17, 1829, and was set up in the yard of the West Point foundry machine shops and publicly exhibited for days to thousands of the first citizens of the country. It was brought from England by Horatio Allen, who made the first experimental trial of it at Honesdale, on the banks of Lackawaxen creek, Pa., August 8, 1829, when he "opened the throttle valve of the locomotive engine that turned the first driving wheel on an American railroad." This highly interesting statement was made by Mr. Allen in a speech delivered at Dunkirk on the occasion of the celebration of the New York and Erie railroad.

2. The first locomotive built in America for a purely experimental purpose was the "Tom Thumb," which was constructed by the now venerable Peter Cooper. This little machine was built for the purpose of testing the feasibility of a locomotive sustaining itself while running over curves, which was a mooted point among the engineers and scientists of that day. The engine weighed less than a ton, the cylinder was only three and a half inches in diameter, the boiler was about as "large as an ordinary kitchen boiler," and was vertical, with gun barrels for tubes. The first trial was made on the Baltimore and Ohio railroad, from the depot at Baltimore to Ellicott's mills, August 28, 1830.

3. The first locomotive built in America for actual service on a railroad was called the "Best Friend," and was constructed for the Charleston and Augusta railroad company. This pioneeer locomotive was built at the West Point foundry machine shops in New York City, and the work of fitting it up fell to the lot of Mr. Matthew. Immediately after the engine was completed it was placed on the company's road, and the first experiment with a train was made November 2, 1830, N. W. Darrell acting as engineer.

Some few days previous to the above date, or about the 20th of October, in accordance with a notice given in the Charleston papers, a public trial was made without any cars attached. It was on this occasion that the first American built locomotive turned its wheels for the first time on a railroad track. At the trial on November 2d the wooden wheels of the machine, which were constructed after the English practice, sprung and got off the track; but they were replaced by cast iron wheels, and on December 14th and 15th the engine was again tried and ran at the rate of 16 to 21 miles an hour with five cars carrying about 50 passengers, and without the cars it attained a speed of 30 to 35 miles an hour. In the Charleston *Courier*, March 12, 1831, there is an account of a later trial of speed of the "Best Friend," on which occasion, the writer remarks: "Safety was assured by the introduction of a barrier car, on which cotton was piled up as a rampart between the locomotive and the passenger cars." The second locomotive for service built in this country was called the "West Point," and was for the same road. It was also constructed at the West Point machine shops.

4. The first locomotive built in America for a northern road was called the "De Witt Clinton," and was the third American locomotive. It was for actual service on the Mohawk and Hudson railroad. This engine, like the others, was built at the West Point machine shops, and was also fitted up by Mr. Matthew; and when it was completed he took it to Albany, June 25, 1831, and made the first excursion with a train of cars over the road August 9, 1831. According to Mr. Matthew's statement, the "De Witt Clinton" weighed 3½ tons, and hauled a train of 3 and 5 cars at the speed of 30 miles an hour. It is especially noteworthy that both the cab and the tender of the "De Witt Clinton" were covered to protect the engineer from the weather—a "happy thought" of honest David Matthew, for which all American engineers at least ought to hold him in kind remembrance. About the middle of August the English locomotive, "Robert Fulton," built by the younger Stephenson, arrived and was placed on the Mohawk and Hudson road for service in the middle of the following September.

These locomotives had been used and fairly tested both on the southern and northern railroads, and the necessity for a radical change in their construction had become evident. Very soon John B. Jervis devised the plan of putting the truck under the forward part of the engine to enable it to turn sharp corners easily and safely. The machine so constructed was called the "bogie" engine. The first of these engines ever built was for the Mohawk and Hudson road, and was called the "Experiment." It was put on the road and ran by Matthew, who says it was as "fleet as a greyhound. The "Experiment" had been built to burn anthracite coal solely; after a while it was rebuilt and adapted to the use of any kind of coal, and its name was changed to the "Brother Jonathan." Shortly after these changes had been made the English locomotive "Robert Fulton," belonging to the same company, was also rebuilt and furnished with the truck, and named the "John Bull." The "Brother Jonathan" was a remarkable machine for those pioneer days. Mr. Matthew says of it: "With this engine I have crossed the Mohawk and Hudson railroad from plane to plane, 14 miles, in 13 minutes, stopping once for water. I have tried her speed upon a level, straight line, and have run a mile in 45 seconds by the watch. She was the fastest and steadiest engine I have ever run or seen, and I worked her with the greatest ease." This is certainly wonderful speed, and may be, as Matthew earnestly maintains it is, the fastest time at least on the American railroad record.

In comparison to the splendid and efficient engine of today, our first locomotives, built after the English model mainly, were clumsy and crude machines. Since then our improvements have been manifold and extraordinary, and the American locomotive is now pronounced the most "perfect railroad tool in the world." Its exquisite symmetry and flexibility, and its extraordinary powers must fill the mind of a veteran like Matthews—who has watched its growth from its infancy in this country—with feelings of generous admiration and pride. The English and American railroads and locomotives are strikingly contrasted by a writer in *Harper's Magazine* for March, 1879. English roads are short, solid, straight and level, and laid with the best rails in the world; and their massive and powerful, and rigid-framed engines are thoroughly adopted to those perfect roads. On the contrary, the American road is generally of great length, and being necessarily cheap it "goes as you please." Over these eccentric roads the American locomotive adjusts itself to every change of level both across and along the line; it takes curves that would be impossible for the rigid English engine; and, finally, it runs over a crazy track, up hill and down, in perfect safety. It has been well said that all that the English engine can do on a perfect road the American engine will do; and much more than this, it will do work on any road, however rough, hilly, curved and cheap. The name of the first American locomotive seems to have been inspired, for it has in the largest sense proved our "Best Friend."—*Cal. Scientific Press.*

LITERARY INTELLIGENCE.

We have received *The Microscopist's Annual* for 1879, published quite recently and dated 1880. It contains useful tables, rules, formulæ and memoranda, a list of microscopical societies, with officers, etc.; Directory of prominent microscope makers, dealers and importers in America and Europe. We trust that microscopists will patronize this thoroughly practical little work, and as it is issued by the Industrial Publishing Company, of Dey street, New York, at the nominal price of twenty-five cents, its expense can hardly be a bar to its purchase. We understand future numbers will be considerably enlarged.

THE announcement is made of a new bi-monthly magazine called *The Educational Review*, which will be devoted to the science and philosophy of education, in all its departments of thought and discussion. It will be conducted by Mr. Thomas W. Bicknell, whose great experience in educational literature cannot fail to make it a success, and worthy of the great subject it takes in hand.

GENERAL NOTES.

THE French journal, *La Lumiere Electrique*, is to receive the addition of a supplement (having separate pagination) in which will be given a *résumé* of recent discoveries and inventions. For the present, these supplements will be confined to the subject of electric lighting.

IN a new form of telephone-receiver brought before the French Academy by M. Ader, two plates are used, arranged in such a manner that the air can pass through a central hole in one to the other. The result is, much louder tones, the second plate acting as a sort of soundboard.

THE Corporation of Yale College have established a horological laboratory in connection with the Winchester Observatory, with the view of encouraging the manufacture of more refined apparatus for the measurement of time.

IT is reported that Mr. Swan, of Newcastle-on-Tyne, has succeeded in rendering his little electric lamp a success. He uses a carbon thread in a vacuum tube, which supplies a soft and steady light, well adapted for household purposes.

M. DU MONCEL has just published a third edition of his work, entitled "Le Telephone, le Microphone, et le Phonographe," the two previous editions (containing 5,500 copies each) having been exhausted in fifteen months. The numerous recent developments of the telephone and microphone are described, and 48 new engravings are added. The phonograph seems to have made but little progress since its appearance; M. Du Moncel, however, specifies a few improvements of it.

THE House of Lords' Committee have passed the preamble of the Bill for the construction of a subway available for all kinds of traffic, vehicular and passenger, under the Mersey, so as to connect the towns of Liverpool and Birkenhead. The total length will be 1 mile 6 furlongs 6½ chains, and the estimated capital required £500,000, the taking up of which is guaranteed by the Corporations of Liverpool and Birkenhead, the Mersey Dock Board, and the Great-Western Railway, each of whom are prepared to give security for one quarter the cost of construction. The engineers are Mr. John Fowler, of London, and Messrs. Law and Thomas, of Wrexham.

IN a recent note to the Paris Academy, Professor Marangoni gives the results he has arrived at in a study of the swimming-bladder of fishes. He states, first, that it is the organ which regulates the migration of fishes, those fishes that are without it not migrating from bottoms of little depth, where they find tepid water; while fishes which have a bladder are such as live in deep, cold water, and migrate to deposit their ova in warmer water near the surface. Next, fishes do not rise like the Cartesian diver (in the well-known experiment), and they have to counteract the influence of their swimming-bladders with their fins. If some small dead and living fishes be put in a vessel three-quarters full of water and the air be compressed or rarefied, one finds in the former case the dead fish descend, while the living ones rise, head in advance, to the surface. Rarefying has the opposite effect. Fishes have reason to fear the passive influences due to hydrostatic pressure; when fished from a great depth, their bladders are often found to be ruptured. Thirdly, the swimming-bladder produces in fishes twofold instability—one of level, the other of position. A fish, having once adapted its bladder to live at a certain depth, may, through the slightest variation of pressure, be either forced downwards or upwards, and thus they are in unstable equilibrium as to level. As to position, the bladder being in the ventral region, the centre of gravity is above the centre of pressure, so that fishes are always threatened with inversion; and, indeed, they take the inverted position when dead or dying. This double instability forces fishes to a continual gymnastic movement, and doubtless helps to render them strong and agile. The most agile of terrestrial animals are also those which have least stability.

A new process of extracting sugar from molasses has been proposed by M. Gayon. It is based on the destruction of the glucose of molasses by fermentation; the sugar remains unaltered, and is obtained by ulterior crystallization. The ferment employed is a pretty common mould, *Mucor circinelloides*. M. Van Tieghem found it in horsedung, and was the first to describe it. The ferment cells must not be confounded with those of beer-yeast, or *saccharomyces*. They differ in form, and, unlike beer-yeast, this *mucor* is powerless to produce glucosic and alcoholic fermentations of cane-sugar, whereas it acts like all alcoholic yeast on glucose and similar compounds. If, then, the cells of *mucor* be sown in a nutritive solution of cane-sugar and glucose, the latter alone ferments, the sugar remaining unaltered, whereas with beer-yeast *all* ferments. This conclusion was confirmed indirectly by experiments made with a view to ascertain the constitution of the inactive glucose of molasses by saccharimetric observation. M. Gayon has succeeded in fermenting 200 or 300 c.c. of molasses solution, and he remarks that by combining the process with osmosis one might, no doubt, extract, in the dry crystalline state, all the sugar which the glucose and the salts retain in molasses. (The Editor of the *Journal de Pharmacie* observes that it is only exceptionally that glucose exists in molasses in sensible proportion, and it is the salts that prevent crystallisation of the sugar; nevertheless, M. Gayon's researches are of much interest scientifically.)

THE blood of most slaughter-houses is usually dealt with in a primitive manner in open air, and without previous disinfection. This is obviously opposed to hygienic and economic laws. M. Vautelet has lately brought forward a process of treating all organic detritus from slaughter-houses for agricultural purposes. He uses sulphate of alumina, sulphuric acid, and nitric acid in fixed proportions. By addition of sulphuric acid to sulphate of alumina a bisulphate is formed, which, less soluble than the sulphate, quickly causes a complete coagulation of the blood. The *rôle* of the nitric acid is coagulation of the albumin of the blood and formation of nitrate. The matters are thus disinfected, and their fertilising power fully preserved.

THE geological changes which the English Channel has undergone are discussed in a recent communication to the French Academy by M. Hebert (June 7).

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The list of Associate Editors is not yet complete, but so far it comprises the names of Professional Scientists who stand at the head of the various branches of Sciences with which they have identified themselves.

The Editor will be glad to hear from those who will take an interest in the success of this Journal, or who desire to co-operate in any way; and suggestions will always be welcomed and receive due attention. The intention of publishing "SCIENCE" has been known only to a few, but the following well known and esteemed Scientists have expressed either their intention to contribute, or their approval of the object of the Journal, and good wishes for its success.

Professor SPENCER F. BAIRD, and Dr. ELLIOTT COUES, Smithsonian Institution, Washington, D. C.; Professor EDWARD S. HOLDEN, Naval Observatory, Washington; Professor O. C. MARSH, Yale College; Professor BURT G. WILDER, Cornell University; Professor C. A. YOUNG, Princeton College; Professor S. W. BURNHAM; Chicago; Professor O. STONE, Observatory, Cincinnati; Dr. J. S. BILLINGS, National Board of Health, Washington; Professor WILLIAM A. HAMMOND, M. D., New York; Dr. EDWARD C. SPITZKA, New York; Prof. CLEVELAND ABBE, Army Signal Office, Washington; Professor EDWARD S. MORSE, Peabody Academy of Science, Salem, Mass.; Professor ALEX. A. JULIEN, Columbia College, New York; Dr. J. J. WOODWARD, Surgeon General's Office, Washington; Professor H. P. BOWDITCH, Cambridge, Mass.; Professor H. L. SMITH, Hobart College, Geneva, N. Y.; &c., &c., &c.

All Communications should be addressed to THE EDITOR, Box 3838, P. O. New York.

SATURDAY, JULY 24, 1880.

THE SOCIETY OF AMERICAN TAXIDERMISTS.

BY WM. T. HORNADAY.

It is high time that the art of taxidermy should receive a new impetus from some source, if it is ever to rise above the level of an undignified, and rather unattractive trade. It is by no means universally looked upon as a fine art, it is certainly not patronized as such, and, until it is developed to a far higher state of perfection than it is at present, we are by no means sure it deserves to be. At present most taxidermists are fiercely jealous of each other and outsiders, and guard their little knowledge as a miser hoards his gold; and yet not a single taxidermist in America earns a competence, and no other position when once lost in one place is so hard to obtain in another. Taxidermists are agreed upon one point, and only one, viz.: that their art is one requiring as much anatomical knowledge and executive skill as either painting or sculpture, while each man, with but few exceptions is firmly convinced past all argument that *his* work is equalled by few and surpassed by none.

At present, taxidemy is not a popular art; as a profession, it is remunerative to the select few only, and even to those in a very moderate degree. But such results are but merited, and for this taxidermists have themselves to thank. If painters and sculptors had always been as narrow-minded, jealous, and absurdly exclusive of their knowledge as we have ever been (with but few exceptions) their art would stand no higher to-day than ours. I have known of taxidermists, who, when visited by other members of the profession, would invariably stop working the moment the visitor appeared and remain idle during his entire stay even though their specimen *spoiled.* Such men must think they are the only taxidermists in the world.

A great artist of any other description is ever ready and anxious to *learn*, even from the meanest sources sometimes; but your taxidermist soon knows too much to be taught anything by anybody, and to offer him any advice, or make an unfavorable criticism is to insult him. As a rule he refuses to teach his art to anyone, save at most fabulous prices. As a result of all this, taxidermy is not a popular art and not a tithe of its capabilities have yet been developed. Taxidermists have never combined to build up their art; from the very foundation, there have been no exhibitions, no well directed competition, no intelligent verdict as to the merits of this man or that, no interchange of ideas, no general and hearty dissemination of knowledge bearing upon this subject. The knowledge of the art is confined to a few, and so is the patronage.

The Society of American Taxidermists, the first of the kind ever organized, has been formed for the avowed purpose of developing the art of taxidermy, and elevating it to the position it should occupy beside the kindred arts of painting and sculpture. It has been formed not for the benefit of a few individuals, but with the higher, broader purpose of developing the possibilities of the art, and raising it to the level of a dignified and justly remunerative profession. Its members are practical, determined men who enter upon the work before them with all professional jealousy laid aside, and with the determination to work as one man. They propose to diffuse as widely as possible a correct knowledge of the methods employed in taxidermy, and by their work to create in the public a proper appreciation of their art as such. They believe that by combination, sharp but well regulated competition, and a few years of patient, earnest work and self improvement they will, in a measure, accomplish their object.

The Society has been started by the professional taxidermists in Prof. Ward's famous establishment at Rochester, N. Y., and already includes many well-known specialists in every branch of the art. It has received most cordial letters of encouragement and endorsement from such eminent scientists and patrons of taxidermy as Prof. Henry A. Ward, of Rochester, Prof. J. A. Allen, of Cambridge, Dr. Elliott Coues, of the Smithsonian Institution, and Dr. G. E. Manigault, of Charleston, S. C. Each of the above-mentioned gentlemen is an Honorary Member of the society.

The organization is steadily attracting candidates for membership from various parts of the United States, liberal minded professionals, and ambitious and enterprising amateurs who are only too glad of so fair an opportunity to follow up an attractive art.

The Society is to be national in all respects, and it is to be hoped it will yet wield an influence which will be felt in foreign countries. Indeed its members look forward to the day when there may be held under its auspices in this country a grand international exhibition of works in taxidermy.

The Society proposes to hold its first annual meeting and exhibition in the city of Rochester, on or about Dec. 20th of this year, at which a corps of carefully selected judges shall critically examine the objects in the exhibition and award the honors. Of course the judges will not be ordinary members of the society, and absolute fairness will be guaranteed. The objects entered for the exhibition will be divided into the following classes, which embrace work in every branch of the art:

A. TAXIDERMY PROPER.

First—Stuffed mammals, birds, reptiles and fishes, in groups.

Second—Single specimens.

Third—Heads. (Special attention is requested to the artistic arrangement of heads, especially those of small animals).

Fourth—Skins of all kinds.

Fifth—Crustaceans, in groups.

B. MISCELLANEOUS OBJECTS.

First—Animals grotesquely mounted.

Second—Ornamental articles, in which only portions of an animal are used, as fans, feather work, fire screens, rugs, footstools, etc.

President, Frederic S. Webster; Secretary, Wm. T. Hornaday. Office of the Secretary, 19 Rowley St., Rochester, N. Y.

C. ADJUNCTS TO TAXIDERMY.

Tools, eyes, materials, perches, leaves, rock-work, etc.

Already a large number of important objects are entered for exhibition, consisting chiefly of artistic groups, both large and small, and it is certain that there will be a fine display in class B, or household ornaments and decorations, most of which will be entirely new and original in design. A silver medal will be awarded to the finest single exhibit, a bronze medal to the best general exhibit, and a diploma of honor to the best exhibit in each of four natural classes, viz.: Mammals, birds, reptiles and fishes. A number of interesting papers and notes upon the various methods of taxidermy will be read at the general meeting and afterwards published in a volume as the proceedings of the society. From now until December each member will be busily engaged in putting forth all his skill and knowledge in the effort to win some of the honors offered for the highest excellence.

It is to be earnestly hoped that their vigorous and already successful movement will meet the hearty approval and co-operation of all American taxidermists, both amateur and professional, and that they will, by joining the society, and taking active part in the meetings and exhibitions, help to build up a powerful and influential organization, which is devoted to their best interests. The most unskillful amateurs are cordially welcomed as members, if they are but earnest in taking hold of the work in hand. It now remains to be seen how much liberality of mind, enthusiasm of purpose and ambitious enterprise will be awakened by this movement among American taxidermists.

DESCRIPTION OF SOME MONSTROSITIES OBSERVED IN NORTH AMERICAN COLEOPTERA.[1]

BY HORACE F. JAYNE.

The accumulation of material in some of the larger collections of Coleoptera of our fauna has suggested that a description of the more marked monstrosities might be interesting, and aid at some future time in throwing light on points of development not yet understood. I have, therefore, in this paper, described and figured those monstrosities which M. Mocquerys of Rouen, in his excellent work on Abnormal Coleoptera, calls "Monstrosities by Excess." Deformities by deficiency or incomplete development have not been considered as they do not seem of sufficient importance, and point only to accidents happening to the insects while in the larvæ or pupæ stage.

I desire to return my sincere thanks to Dr. Horn for the free use of his collection and library, for many suggestions and for kindly revising these pages; also to Dr. LeConte for the loan of specimens from his cabinet, and to Dr. Hagen for the use of specimens belonging to the Museum of Comparative Zoology at Cambridge.

[1] A paper read before the Am. Ent. Soc., June, 1880.

CALOSOMA TRISTE, Lec.

Fig. 1 represents a monstrosity on the right antenna of a specimen of *Colosoma triste*, Lec. It consists in the sixth joint bearing two branches of five joints. Fig. 1a, shows the antenna greatly enlarged. The first three joints are normal; the third a little dilated at apex. The fourth is normal in length but is one-half broader at apex. When viewed from above it is distinctly pyriform. The fifth joint is also of normal length but twice the width of that of the left side and slightly broader at apex. The sixth joint is pentagonal in form, in its widest place as wide as long. The apex is obliquely truncate on its inner and outer angles, presenting two unequal faces for the insertion of the two branches. The inner or posterior facet is much smaller and from it arises that branch with the joints exactly resembling the normal antenna. The anterior or outer facet is larger and gives insertion to an anterior or outer branch of five joints; the first being short and thick the others similar to the corresponding normal joints but smaller.

The specimen is in Dr. Horn's Cabinet. Collected in California.

CYCHRUS ANGUSTICOLLIS, Fisch.

Fig. 2 represents the deformed left anterior leg of a specimen of *Cychrus angusticollis*. The femur is greatly dilated a little beyond the middle and gives off from its superior border a tubercle moderately long and blunt at tip. This may possibly indicate an attempt at the development of a second leg. The femur is then narrowed and at apex is a little larger than the apex of the normal joint. The existence of a cotyloid cavity shows the former presence, and accidental loss of the tibia.

In the Museum of Comparative Zoology at Cambridge.

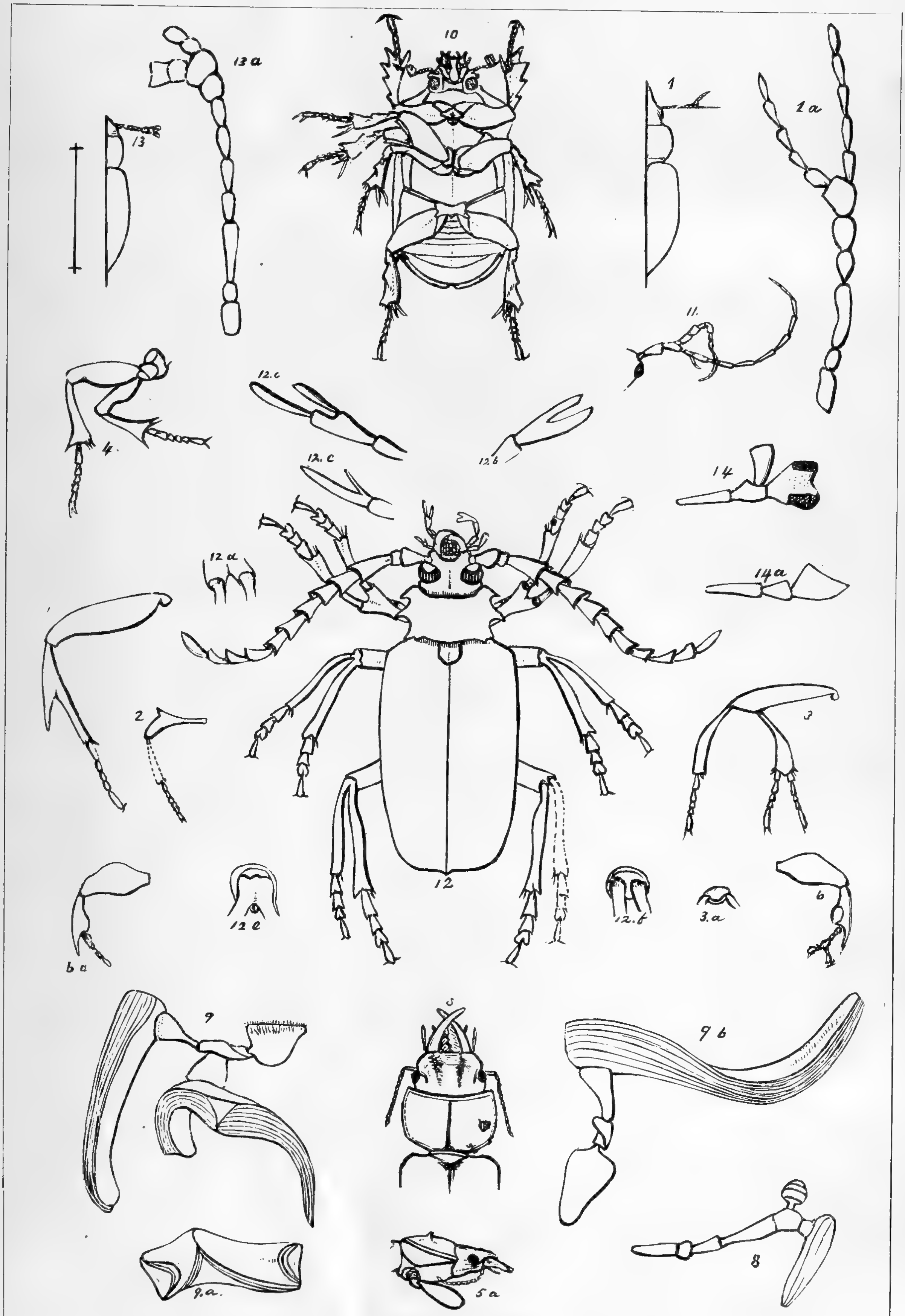
METRIUS CONTRACTUS, Esch.

A monstrosity in the middle left leg of a specimen of *Metrius contractus* is shown in fig. 3. The femur bears two tibiæ; the inner one bearing two full sets of tarsal joints. The femur is normal. The outer tibia, which may be regarded as the normal one, arises from the extremity of the femur and is somewhat shorter, stouter, and more curved than the tibia of the middle right leg. The inner tibia arises from the posterior side of the femur a short distance within the tip and is articulated with it by a separate cotyloid cavity, the two cavities however are confluent as seen in fig. 3a. It is distinctly arcuate, dilated toward the apex which is obliquely truncate at each angle. From each facet thus formed arises a tarsal joint of normal length, almost contiguous at their bases, and somewhat stouter than the succeeding joints which are normal in form but shorter than those of a normal tarsus. There are four terminal spurs to this tibia, two placed external to the outer tarsus, two within the inner.

In Dr. Horn's Collection.

PASIMACHUS PUNCTULATUS, Hald.

A specimen of *Pasimachus punctulatus* has seven legs; the extra one arising from a trochanter placed between the normal trochanter and femur of the left


13 a
13
10
1
1 a
11.
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12.c
12.b
12.c
14
12 a
14 a
2
3
12
12 e
12.b
3.a
6
b a
9
5
9 b
8
9.a.
5 a

middle leg. Fig. 4 represents the anomaly as seen from below. The coxa and trochanter are like those of the right leg. On the inferior surface, between the trochanter and femur and embraced in front and behind by the latter, is inserted a second trochanter; triangular in form, about half as wide and one-third as long as the normal one. It gives origin to the extra femur, which is two thirds as long and about three-fourths as stout as the main thigh. The tibia of this extra femur is perfect except that it is one-fourth shorter than the other; its spurs and tarsal joints and the claws of the latter being all normal. This abnormal leg is less chitinous than the others.

In Dr. Horn's Collection.

SCARITES SUBSTRIATUS, Hald.

I have tried to represent in fig. 5a monstrosity on the right side of the dorsal surface of the prothorax in a specimen of the *Scarites substriatus*. It consists of a tubercle about a thirty-second of an inch long, projecting outward and slightly forward. It arises a thirty-second of an inch transversely from the middle of the right margin of the thorax. It is deeply cleft on the summit, almost transversely. Fig. 4a, represents it when viewed from the side.

Collected in Texas. In Dr. Horn's Cabinet.

DYSCHIRIUS GLOBULOSUS, Say.

The anomalous right anterior leg of a *Dyschirius globulosus* is shown in fig. 6. Fig. 5a, represents the normal right leg. The deformity consists in the third joint of the tarsus bearing two branches of two joints each. The inferior terminal spur of the tibia is wanting. The first two joints of the right tarsus are normal; the third a little longer, more clavate, and obliquely truncate on each side at tip for the articulation of the double set of joints which follow. The two anomalous branches arise on each side of the sharp apex thus formed, one directed to the left, the other to the right. The first joint on each branch is a shorter and stouter than a normal fourth joint; while the terminal or claw joint does not differ greatly in length. The claw joint of the inner or left branch bears a pair of normal claws, the outer claw joint is somewhat broader and bears two sets of claws curved from each other.

In Dr. LeConte's Cabinet.

CHLÆNIUS DIFFINIS, Chaud.

Fig. 7 represents a deformity in the left middle leg of a specimen of *Chlænius diffinis*. The tibia at a point a little below the middle bifurcates, the inner bifurcation continuing to normal length bears the tarsal joints. The outer is about two-thirds as long as the inner. It appears from its size and form that this branch bore a set of tarsal joints similar to those seen on the inner; and this opinion is strengthened by the fact that the end is somewhat ragged and seems to have been broken off.

In Dr. Horn's Cabinet.

LICHNANTHE VULPINA, Hentz.

A specimen of this insect has an anomalous right antenna as shown in fig. 8. The first three joints are normal. The fourth, fifth, and sixth are fused into one joint twice as long as the third; the seventh appears to be connate with the first joint of the club. From the posterior outer border of the long fourth joint near the tip there arises a spherical club of three joints about the length of the third antennal joint. The first joint comprising the pedicle and base of the club, the second the centre, and the last the apex.

In Dr. Horn's Cabinet.

POLYPHYLLA DECEMLINEATA, Say.

Fig. 9 represents the right antenna of a specimen of *Polyphylla decemlineata* in which, in addition to the normal structure, the second joint bears a branch anteriorly, consisting of a single free joint which supports two clubs, placed transversely to the normal, of seven lamellæ each, united at their bases. The plane of the normal club is perpendicular to the plane of the abnormal, but in the figure the two are represented as in the same plane; the normal branch as seen from the outer side, the abnormal as seen from above. Fig. 9b, represents the left antenna. The basal joint of the right antenna is somewhat smaller and more inflated than that of the left. The second joint is twice as long as the corresponding one on the left antenna; the outer half of the anterior border being flattened for the insertion of the first joint of the abnormal branch, and its posterior border somewhat sinuate near the tip. The double club on the abnormal branch consists of two sets, of seven lamellæ each of unequal size, united at their bases at an angle of forty-five degrees, the outer scarcely longer than half the inner and more curved, while the inner is but little shorter than the club of the normal branch but more curved than it. The joint supporting these branches is obconical and much shorter than the second joint from which it arises. Fig. 9a, represents the double club as seen from below. The third joint of the normal or posterior branch is in form like that of the left antenna, but a fifth shorter. It bears a club of seven lamellæ, which is directed downward, and is about half as long as that of the left side, much narrower and feebly curved.

The insect is in Dr. LeConte's Cabinet.

STRATEGUS ANTÆUS, Fabr.

A specimen of this insect has the left middle leg triplicated. I have tried to represent this monstrosity in fig. 10. It may be regarded as made up of a normal leg with its trochanter entire. To the under surface of this normal femur are added two others, making together a pyramidal mass; free at their apices for about one-third their length. Those of the normal femur and the one nearest to it are closely placed, while the other diverges at an angle of about forty-five degrees. Each femur is provided with a tibia and tarsus. The tibia of the normal femur is not as greatly developed as the corresponding one on the right leg and those on the two abnormal femora are still less strongly marked.

In the Museum of Comparative Zoology at Cambridge.

TELEPHORUS ROTUNDICOLLIS, Say.

A specimen of this insect is deformed in the right antenna as shown in fig. 11. The third joint bears from its anterior surface an extra branch of six joints. The first joint of this antenna is much stouter than the corresponding joint on the left side. The second about half as long as the first and as stout. The outer half of the anterior border is flattened to receive the first joint of the abnormal branch. From its end arises the regular branch of nine joints, all of which are normal except the first which gives off near the middle of its posterior border a slender spine-like process, half as long as the joint itself, curving outward and backward. The abnormal branch which is composed of six joints is directed forward and outward. The first three joints are flattened and very wide proportionately, the last three cylindrical. The first joint is about as long as the one which bears it and at its base about half as wide as long but considerably wider at tip. The next joint is a little narrower than the tip of the first. Its length about equals its width. The third is one-third narrower than the second and almost twice as long as wide. In the figure it is represented as folded upon itself. The fourth joint is somewhat longer than the third and half as wide, almost twice as long as the fifth which bears the sixth a long slender joint which curves inward and is as long as the fourth and fifth together.

PRIONUS CALIFORNICUS, Motsch.

Fig. 12 represents in a specimen of *Prionus californicus* one of the most remarkable monstrosities that has probably ever occurred among Coleoptera—remarkable not only for extent but also for symmetry. The left maxillary palpus bears two terminal joints. In the right maxillary and the left labial palpi the terminal joint is bifid. Each femur bears two tibiæ furnished with tarsi and claws. The second joint of the left maxillary palpus appears to be composed of two joints closely connate, the anterior one much shorter than the other, each bearing a terminal joint of somewhat unequal lengths, as shown by fig. 12a. The terminal joint of the right maxillary palpus is deeply cleft at apex representing two joints connate at their basal halves; fig. 12b. The terminal joint of the left labial palpus gives off anteriorly from its base a second joint half as long and as stout as the other and connate with it; fig. 12c. The antennæ are normal. The anterior femora are normal in length and in articulation with their coxæ. They gradually widen from base to apex where they are more than twice as wide as a normal femur. Rhomboidal in section; the superior surface about one-third narrower than the inferior. The apices are dilated and deeply notched vertically, making two processes about as long as wide, each containing a normal cotyloid cavity with which the tibiæ are articulated in a normal manner. Fig. 12d, shows the femur and articulations as seen at the end. Of the tibiæ the anterior is somewhat shorter and about two-thirds as stout as the posterior, which is probably the normal one. The spurs, tarsi, and claws of both are similar. The middle femora are normal in length and form but about one-half stouter. The apices each contain one large cotyloid cavity. Into this, which is twice as wide as a normal one, is inserted a single broad condyle formed by coalescence of the condyles of the two tibiæ is shown in fig. 12e. The anterior of these is somewhat shorter and about two-thirds as stout as the posterior. Its tarsi are more slender and a little shorter. The articulation of the tibiæ with the left posterior femur is identical with that of the middle femora; fig. 12e. The anterior of the two tibiæ and its tarsus are about five-sixths the length of the posterior and one-half more slender. In the right posterior femur the articulation with the two tibiæ differs from all the others. The femur itself is about equal in thickness to the left but is a trifle more dilated at apex. This is not notched as in the anterior femora, but truncate. Each tibia is inserted into a distinct cotyloid cavity separated by a considerable interval; fig. 12f. The anterior of the two has been unfortunately broken off about one-fourth of an inch from the femur. The structure of the under side presents no departure from the normal standard.

Collected by Mr. Morrison in Washington Territory.

ELEODES PILOSA, Horn.

In fig. 13 is shown a specimen of *Eleodes pilosa*, the right antenna of which is deformed; the ninth joint bearing on its end two branches of two joints each. Fig. 13a, represents the antenna enlarged. The first seven joints are normal. The eighth and ninth equal each other in length, being slightly shorter than the seventh. The eighth is as wide as long. The ninth at base is as wide as long; at the middle almost twice as wide. From the anterior part of the end arises a branch of two joints which are flattened, almost connate and a little more than half as wide as the ninth joint and as long as wide. The last joint is sinuate at tip. From the posterior part of the end of the ninth joint arises a branch also of two joints which equal in length those of the other branch but are more cylindrical and more nearly resembling normal terminal joints.

From Nevada. In Dr. Horn's Cabinet.

HELOPS SULCIPENNIS, Lec.

Fig. 14 represents an anomaly in the right maxillary palpus of a specimen of *Helops sulcipennis*. Fig. 14a, shows the normal palpus. The anomaly consists in the second joint bearing two terminal joints, one from the outer end of the anterior border and one from the tip. The first joint of this palpus is normal. The second nearly so excepting a dilation and flattening of the anterior border into which the terminal joint is inserted. The latter joint, which in the figure is represented as seen from below, is foreshortened as its plane is nearly perpendicular to the plane of the palpus proper. When viewed from the side it is precisely similar to the terminal joint in fig. 12a. The other joint which arises from the tip of the second is really made up of two joints soldered together at the bases of their broad surfaces. It is consequently twice the thickness of the other terminal joint and at its free edge deeply grooved indicating the union of two joints, and presenting that silky appearance common to the free edge of the normal joint. It is shown in the figure as seen partially from below, partially from the side. The parts shaded are intended to represent the edges of the two joints in one, the dotted part the deep groove.

In Dr. Horn's Cabinet.

SCIENCE:

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JOHN MICHELS, Editor.

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To Correspondents.

All communications should be addressed to the Editor—Box 3838, P. O., New York—with name and address of writer, not necessarily for publication without consent.

Scientific papers and correspondence intended for publication, should be written *legibly* on one side only of the paper. Articles thus received will be returned when found unsuitable for the Journal.

Those engaged in Scientific Research are invited to make this Journal the medium of recording their work, and facilities will be extended to those desirous of publishing original communications possessing merit.

Proceedings of Scientific Societies will be recorded, but the abstracts furnished must be signed by the Secretaries.

Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply, may be written in the form of an article.

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To Advertisers.

Terms for advertising may be obtained at the office of Journal, 229 Broadway.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

We direct the attention of our readers to the approaching meeting of the American Association for the Advancement of Science, which will be held this year at Boston, commencing at 10 o'clock on Wednesday morning, the 25th of August.

As it is generally believed that the Boston meeting will be the largest and most important hitherto held, we are completing arrangements with the Executive, by which abstracts of all papers read may be printed in "SCIENCE" contemporaneously with the meeting of the Association, together with a full report of the proceedings; we have reason to believe that such a course will be welcome to the members of the Association, and useful in many respects. In a later number we propose to give fuller particulars in regard to this matter, and in the meantime would be glad to hear from those who will read papers at this meeting, particularly where illustrations are necessary, as by a little co-operation greater justice to the publication of such papers may be attained.

We may state for the convenience of non-members desirous of being admitted as members, that by paying the fees in advance (eight dollars), before the meeting, member's tickets will be sent, which will secure to new members and nominees the same privileges possessed by old members.

The attention of entomologists is directed to the annual meeting of the Entomological Club of the Association, which will be held at the rooms of the Boston Society of Natural History, on Tuesday, August 24th, at which all interested in entomology are invited to be present.

We notice by the prospectus that the suggestion made at Saratoga to form a sub-section devoted to Physiology and Anatomy will be carried out at Boston, and it is also probable that new sub-sections in Geology and Physical Geography will be formed.

We trust that all interested in science who can attend this meeting of the Association will not fail to be present, and that many new members will enroll themselves and take part in the proceedings.

Independent of the interest attached to the meeting of the Association several excursions have been planned, which will add greatly to the pleasure of those present, and cannot fail to make the trip to Boston one which will be long after recalled by many agreeable remembrances.

A NEW sulphate of alumina (sesquibasic sulphate of alumina) has been prepared by M. Marguerite. One method is by decomposition of alum of ammonia through heat. When the alum is heated to a red heat carefully, there remains after the operation anhydrous sulphate of alumina; if the calcination have been pushed further, there is partial decomposition. The matter held by the water gives a liquor which, concentrated, deposits crystals of the sesquibasate. Ordinary sulphate of alumina, dried and colored gently, gives the same reaction, and the new sulphate can also be got by the wet process. (See *Comptes Rendus*).

A CURIOUS geological effect has recently occurred in Sicily. On the morning of the 20th May the half of an old château at the seaside, between Catania and Acireale, fell in consequence of alteration of an enormous volcanic rock which had supported it. This rock was about 50m. high and 80m. in circumference; its form nearly cylindrical. It was placed on an older layer of lava, which forms a promontory. The rock is in such a state of disseggregation, that pieces can easily be detached with the hand. The effect seems due to superoxidation of the iron, which it contains in abundance, and to the action of carbonic acid on the calcareous matter in it. The château in question is a very old one, but its walls were entire, and one might visit it in all its parts. Half an hour before the fall, a family of tourists had taken up residence in it, with a view to visiting the Cyclops islands, which rise a short way off.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

TWENTY-NINTH MEETING.

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[illegible] Society of Natural History [illegible] August [illegible] invited to be present [illegible]

[illegible]

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

TWENTY-NINTH MEETING.

IN conformity with the vote at the last meeting, the Twenty-ninth meeting of the Association will be held at Boston, Mass., commencing at ten o'clock, A. M., on Wednesday the 25th of August, 1880. A large and efficient Local Committee has been formed and through its several subcommittees is engaged in perfecting the local arrangements for the meeting, which will be announced by special circular as early as possible. It is only necessary to state here that the members of the committee are desirous of doing everything in their power to promote the objects of the Association. As it is generally believed that the Boston meeting will be the largest and probably the most important hitherto held by the Association, every effort will be made by the officers of the Association residing in Boston and vicinity, co-operating with the Local Committee, to prepare a satisfactory general programme for the week (subject to the approval of the Standing Committee) in advance of the meeting, the substance of which will be given in the circular of the Local Committee. To this end several votes were passed at the last meeting of the Standing Committee in Saratoga, and the attention of members is particularly directed to the new clause in relation to titles of papers; also to the probable necessity of forming additional subsections.

The Headquarters of the Association will be at the Massachusetts Institute of Technology, where members will register as soon as possible after arrival. The Hotel Vendôme, corner of Commonwealth Ave. and Dartmouth St., has been selected for Hotel Headquarters.

The offices of the Local Committee and of the Permanent Secretary will be at the Institute of Technology. The General Sessions will be held in Huntington Hall in the same building. The several Sections, Subsections and Committees will have their places of meeting designated on the programme for Wednesday.

The circular which will soon be issued by the Local Committee will contain full information in relation to Hotels and Boarding houses in Boston and vicinity with which special arrangements have been made.

The *Permanent Subsection of Chemistry* will be continued at Boston under the chairmanship of Professor John M. Ordway, of Boston.

The *Permanent Subsection of Microscopy* will be presided over by Prof. S. A. Lattimore, of Rochester, and the active co-operation of microscopists is requested. Arrangements have been made for the proper care of instruments, etc.

The *Permanent Subsection of Anthropology* will be under the chairmanship of Prof. J. W. Powell, of Washington. For special circular and information in relation to this subsection, address Judge J. G. Henderson, *Secretary*, Winchester, Ill,

The attention of entomologists is directed to the annual meeting of the Entomological Club of the Association, which will be held at the rooms of the Boston Society of Natural History at two o'clock, on Tuesday August 24, at which all interested are invited to be present. Mr. S. H. Scudder, of Cambridge, President; Mr. B. P. Mann, of Cambridge, Secretary of the Club. (A special circular concerning this meeting will be sent to all requesting it.)

At the Saratoga meeting the desirability of forming a new subsection in the Association, which should be devoted to *Physiology* and *Anatomy*, was discussed with the special object of inducing members interested in Human Physiology to bring their papers before the Association and also of obtaining the active co-operation of Physicians and Surgeons in the work of the Association. The interest taken in the proposition at Saratoga was such that a permanent organization of the new subsection is expected at the Boston meeting. It will also, probably, be found necessary to form a subsection of Geology and Physical Geography at the Boston meeting.

Several excursions will be arranged for by the Local Committee. Among others, one is planned for Salem and another down the Harbor. On Thursday, the Association will probably hold a morning session in Cambridge, after which visits will be made to the various departments and Museums of the University, followed by a reception at the Botanic Garden and the Observatory in the evening.

All communications relating to the local arrangements for the Boston meeting must be made to the *Local Secretaries* at Boston, while all matters relating to membership and to the presentation of papers will be attended to by the *Permanent Secretary.*

Attention is specially requested to the following articles of the Constitution of the Association.

ART. 3. Any person may become a Member of the Association upon recommendation in writing by two members or fellows, nomination by the Standing Committee, and election by a majority of the members and fellows present in general session.

(Blank forms for recommendation to membership will be furnished on application to the Permanent and Local Secretaries, and, until the day of the meeting, they will receive the recommendations for the General Secretary; after the meeting has begun, recommendations must be given to the General Secretary.)

ART. 4. Fellows shall be nominated by the Standing Committee from such of the members as are professionally engaged in science, or have by their labors aided in advancing science. The election of fellows shall be by ballot and a majority vote of the members and fellows present in general session.

ART. 5. Any person paying to the Association the sum of one thousand dollars shall be classed as a Patron, and shall be entitled to all the privileges of a member and to all its publications.

ART. 7. The name of any member or fellow two years in arrears for annual dues shall be erased from the list of the Association, provided that two notices of indebtedness at an interval of at least three months, shall have been given; and no such person shall be restored until he has paid his arrearages or has been reëlected.

ART. 33. The admission fee for members shall be five dollars in addition to the annual assessment. On the election of any member as a fellow an additional fee of two dollars shall be paid.

ART. 34. The annual assesment for members and fellows shall be three dollars.

ART. 35. Any member or fellow who shall pay the sum of fifty dollars to the Association, at any one time, shall become a Life Member, and as such shall be exempt from all further assessments, and shall be entitled to the Proceedings of the Association. All money thus received shall be invested as a permanent fund, the income of which shall be used only to assist in original research unless otherwise directed by unanimous vote of the Standing Committee.

ART. 36. All admission fees and assessments must be paid to the Permanent Secretary, who shall give proper receipts for the same.

ART. 27. All members and fellows must forward to the Permanent Secretary, as early as possible, and when practicable before the convening of the Association, full titles of all the papers which they propose to present during the meeting, with a statement of the time that each will occupy in delivery, and also such abstracts of their contents as will give a general idea of their nature; and no title shall be referred by the Standing Committee to the Sectional Committee until an abstract of the paper or the paper itself has been received.

(Blank forms for giving the titles and abstracts of papers will be furnished by the Permanent Secretary on application. The Standing Committee particularly request, in order to facilitate the arrangement of the programme, that the titles and abstracts should be forwarded so as to reach the Permanent Secretary before August ninth. At the Saratoga meeting the *Permanent Secretary* was instructed *not to enter*, on the list of papers to be presented, any titles of papers until an abstract of the paper, or the paper itself, was received.)

Notice of errors in the printed list of Members of the Association, of change of address, and information respecting the decease of Members, should be sent to the Permanent Secretary in order that due notice may be taken of the same in the next volume of "Proceedings." It is particularly requested that the Permanent Secretary be notified at once of any errors in the names and addresses that will be given in the list in the Saratoga volume, as a revised edition of the list will be printed for circulation at the Boston meeting.

The Saratoga volume (vol. 28) will soon be distributed by mail to every member who has paid the assessment for the Saratoga meeting.

The volumes of the Proceedings of the Association (28 in number) can be obtained from the Permanent Secretary, at the price of $1.50 a volume; or any member wishing for ten or more volumes, in order to complete a set, may obtain them at $1.00 a volume. The volumes may be had bound in cloth for the extra price of fifty cents each, or in one-half Turkey morocco for the extra price of $1.00 each. Uniform cloth covers for the volumes will be furnished by mail at thirty cents each, or by express or at the meetings for twenty-five cents each. Copies of Volumes 2 and 26 will be received in exchange for other volumes or will be purchased at $1.00 each.

The Memoir on Fossil Butterflies, by Mr. S. H. Scudder, published by the donation of Mrs. Elizabeth Thompson, 4to, 1875, will be furnished at $2.00 a copy. The Transactions of the Association of Geologists and Naturalists, 1 vol. 8vo, 1843, bound in cloth, can be obtained at $3.00 a copy.

It will save much time and confusion at the meeting if members will send their assessments *in advance* to the Permanent Secretary, in return for which a Member's ticket, bearing a receipt for the Boston meeting will be forwarded. Members not intending to be present at Boston, are particularly requested to send their assessment in advance, and to those who specially request the same a copy of the Boston Daily Programme will be mailed.

The address of the PERMANENT SECRETARY, F. W. Putnam, Esq., will be *Salem*, Mass., *until August 1st*; after that time, and until the meeting has adjourned, his office will be at the MASSACHUSETTS INSTITUTE OF TECHNOLOGY, *Boston, Mass.*

ON "LIMNOCODIUM VICTORIA," A HYDROID MEDUSA OF FRESH WATER.

A short time since I received from Mr. Sowerby, Secretary of the Royal Botanical Society, a letter informing me of the occurrence of certain Medusoid organizations in the warm-water tank devoted to the cultivation of the *Victoria regia* in the Gardens of the Society. The letter contained a request that I should examine the animals with a view to their determination; Mr. Sowerby accompanied it with rough sketches, and offered to place specimens at my disposal.

The discovery of true freshwater Medusæ was so startling a fact that I lost no time in calling on Mr. Sowerby, with whom I visited the tank, and carried away such specimens as were needed for examination.

The water in the tank had then a temperature of 86° F., and was literally swarming with little Medusæ, the largest of which measured nearly half an inch in transverse diameter. They were very energetic in their movements, swimming with the characteristic systole and diastole of their umbrella, and apparently in the very conditions which contribute most completely to their well being.

As it now became evident that the Medusa belonged to a generic form hitherto undescribed, I prepared for the Linnean Society a paper containing the results of my examination, and assigning to the new Medusa the name of *Limnocodium victoria* (λίμνη, a pond, and κώδων, a bell). This was received and recorded by the secretaries on June 14, and read at the next meeting, on the 17th.[1]

The umbrella varies much in form with its state of contraction, passing from a somewhat conical shape with depressed summit through figures more or less hemispherical to that of a shallow cup or even of a nearly flat disk. Its outer surface is covered by an epithelium composed of flattened hexagonal cells with distinct and brilliant nucleus. The manubrium is large; it commences with a quadrate base, and when extended projects beyond the margin of the umbrella. The mouth is destitute of tentacles, but is divided into four lips, which are everted and plicated. The endoderm of the manubrium is thrown into four strongly-marked longitudinal plicated ridges.

The radial canals are four in number, they originate each in an angle of the quadrate base of the manubrium, and open distally into a wide circular canal. Each radial canal is accompanied by longitudinal muscular fibres, which spread out on each side at the junction of the radial with the circular canal.

The velum is of moderate width, and the extreme margin of the umbrella is thickened and festooned, and loaded with brownish-yellow pigment cells.

The attachment of the tentacles is peculiar. Instead of being free continuations of the umbrella margin, they are given off from the outer surface of the umbrella at points a little above the margin. From each of these points, however, a ridge may be traced centrifugally as far as the thickened umbrella margin; this is caused by the proximate portion of the tentacle being here adnate to the outer surface of the umbrella. It holds exactly the position of the "mantelspangen" or *peronia*, so well developed in the whole of the Narcomedusæ of Haeckel, and occurring also in some genera of his Trachomedusæ. Its structure, however, differs from that of the true *peronia*, which are merely lines of thread cells marking the path travelled over by the tentacle as the insertion of this moved in the course of metamorphosis from the margin of the umbrella to a point at

[1] Some facts in addition to those contained in my original paper are included in the present communication.

some distance above it, while in Limnocodium the ridges are direct continuations of the tentacles whose structure they retain. They become narrower as they approach the margin.

The number of the tentacles is very large in adult specimens. The four tentacles which correspond to the directions of the four radial canals, or the perradial tentacles, are the longest and thickest. The quadrant which intervenes between every two of these carries, at nearly the same height above the margin, about thirteen shorter and thinner tentacles, while between every two of these three to five much smaller tentacles are given off from points nearer to the margin, and at two or three levels, but without any absolute regularity; indeed, in the older examples all regularity, except in the primary or perradial tentacles, seems lost, and the law of their sequence ceases to be apparent.

I could find no indication of a cavity in the tentacles; but they do not present the peculiar cylindrical chord-like endodermal axis formed by a series of large, clear, thick-walled cells which is so characteristic of the solid tentacles in the Trachomedusæ and Narcomedusæ. From the solid tentacles of these orders they differ also in their great extensibility, the four perradial tentacles admitting of extension in the form of long, greatly-attenuated filaments to many times the height of the vertical axis of the umbrella, even when this height is at its maximum; and being again capable of assuming by contraction the form of short thick clubs. Indeed, instead of presenting the comparatively rigid and imperfectly contractile character which prevails among the Trachomedusæ and the Narcomedusæ, they possess as great a power of extension and contraction as may be found in the tentacles of many Leptomedusæ (Thaumantidæ, &c.) These four perradiate tentacles contract independently of the others, and seem to form a different system. All the tentacles are armed along their length with minute thread cells, which are set in close, somewhat spirally arranged warts.

The lithocysts or marginal vesicles are, in adult specimens, about 128 in number. They are situated near the umbrellar margin of the velum, between the bases of the tentacles, and are grouped somewhat irregularly, so that their number has no close relation with that of the tentacles. They consist of a highly refringent spherical body, on which may be usually seen one or more small nucleus-like corpuscles, the whole surrounded by a delicate transparent and structureless capsule. This capsule is very remarkable, for instead of presenting the usual spherical form, it is of enlongated piriform shape. In its larger end is lodged the spherical refringent body, and it thence becomes attenuated, forming a long tubular tail-like extension which is continued into the velum, in which it runs transversely towards its free margin, and there, after usually becoming more or less convoluted, terminates in a blind extremity.

The marginal nerve-ring can be traced running round the whole margin of the umbrella, and in close relation with the otolitic cells. Ocelli are not present.

The generative sacs are borne on the radiating canals, into which they open at a short distance beyond the exit of these from the base of the manubrium. They are of an oval form, and from their point of attachment to the radial canal hang down free into the cavity of the umbrella. Some of the specimens examined contained nearly mature ova, which, under compression, were forced from the sac through the radial canal into the cavity of the stomach.

While some of the characters described above point to an affinity with both the Trachomedusæ and Narcomedusæ, this affinity ceases to show itself in the very important morphological element afforded by the marginal bodies. In both Trachomedusæ and Narcomedusæ the marginal bodies belong to the tentacular system; they are metamorphosed tentacles, and their otolite cells are endodermal, while in the Leptomedusæ, the only other order of craspedotal Medusæ in which marginal vesicles occur, these bodies are genetically derived from the velum. Now in Limnocodium the marginal vesicles seem to be as truly velar as in the Leptomedusæ. They occur on the lower or abumbral side of the velum, close to its insertion into the umbrella, and the tubular extension of their capsule runs along this side to the free margin of the velum, while the delicate epithelium of the abumbral side passes over them as in the Leptomedusæ. It is true that this point cannot be regarded as settled until an opportunity of tracing the development is afforded; but in very young specimens which I examined I found nothing opposed to the view that the marginal vesicles were derived, like those of the Leptomedusæ, from the velum.

Important points still remain to be cleared up regarding the development of Limnocodium and the determination of the question whether the Medusa be derived from the egg directly or only through the intervention of a hydranlid trophosome. I have arranged, with Mr. Sowerby, some methods of observation by which I hope to obtain data for determination of these points.

If this be the case Limnocodium will hold a position intermediate between the Leptomedusæ and the Trachomedusæ; but as the greatest systematic importance must be attached to the structure and origin of the marginal vesicles, its affinity with the Leptomedusæ must be regarded as the closer of the two. GEO. J. ALLMAN.

THE ELECTRIC LAMPS OF M. TCHIKOLEFF.

M. Tchikoleff, the head of the electric lighting department of the Russian artillery, has addressed to *La Lumière Electrique* a communication, of which the following is a translation, in which he claims that the application of derived currents which has been successfully adopted with the lamps of MM. Lontin and Siemens, was employed by him as far back as the year 1871.

"Having experimented for a lengthened period with the Foucault and Serrin regulator lamps, which were considered to be the best at the period when I took up the question, I was able to observe in them the following defects:

1. Several lamps, arranged in series or in multiple arc in a circuit, would not continue to work.
2. These lamps could be worked only by very powerful currents, whereas with a lamp regulated by hand the voltaic arc could be obtained with weaker currents, giving of course a less intense light.
3. They worked with regularity only when the current was constant, or varied within very restricted limits.

I traced the cause of these defects to the fact that the working of the regulating mechanism was based upon a kind of equilibrium between the attractive force of an electro-magnet and the counteracting force of a spring. Such a system does not regulate the distance between the charcoal points, but only the general force of the current in the circuit. Now under these circumstances it is possible that, when two or more lamps are placed in series in a circuit, one of them may have its carbons in contact, whilst the carbons of the other lamp or lamps are at a greater or less distance apart, without the equilibrium between the electro-magnets and the counteracting springs being disturbed.

Now it was to obviate this defect that I endeavored to devise an arrangement which, whilst allowing each lamp placed in a circuit to be independent of the general intensity of the current and its variations, would enable it to maintain constant the resistance of its own voltaic arc, and this arrangement appeared to me obtainable by applying to the regulator lamps the principle of the *differential action of derived currents*.

It was in 1869 that I made the first experiment on the foregoing arrangement with a regulator lamp of M. Foucault, the counteracting spring of which I replaced by a supplementary electro-magnet traversed by a very weak derivation of the current, parallel to the voltaic arc. This electro-magnet was wound with a wire of high resistance, and the current producing the voltaic arc passed through the other electro-magnet. The armatures of these electro-magnets were placed at the two extremities of a rocking-lever, carrying at its centre of oscillation an arm which controlled the mechanism for increasing or diminishing the distance between the carbons; and the rocking-lever was in equilibrium when the voltaic arc possessed its normal resistance.

It is easy to understand that, with this arrangement, as the resistance of the arc becomes greater, the strength of the electro-magnet through which the current passes decreases, whilst the other electro-magnet becomes more powerful; so that the arm no longer remains in its vertical position, and by its inclination influences the mechanism which brings the carbons closer together. The contrary effect is of course produced when the inverse action occurs. An arrangement which, like the one in question, maintains constant the resistance of the arc evidently does not exert any effect upon the general intensity of the current in the circuit; for the variations of this intensity, outside of the lamp, exert always the same effect upon the two electro-magnets which control the latter.* The experiment with the Foucault lamp gave such good results that I decided to undertake the construction of a new lamp as free as possible from the defects inherent to the Foucault regulator, and to those based upon the same principle.

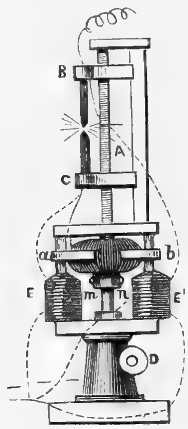

FIG. 1.

After some carefully made experiments with lamps having automatic regulation, and others regulated by hand, I ascertained that the latter would give a constant light with a much less number of battery cells than was requisite with the former. Thus with hand regulators I could obtain satisfactory results with 24 or even 20 Bunsen elements, whilst with the Foucault and Serrin regulators it was necessary to employ at least 40. The cause of this in the first place is that, in these regulating lamps the movements communicated to the carbons are always too sudden (*prompte*) for comparatively weak currents, and, in the second place, that these movements are constant instead of being proportionate to the intensity of the current passing by the voltaic arc. From this I naturally came to the conclusion that, in order to obtain a lamp suitable for practical working, it was necessary to apply the following three principles, which I consider as fundamental:

1. To maintain constant the resistance of the voltaic arc we should not employ a constant mechanical force such as that of a spring, but a weak derivation from the main current, parallel to the voltaic arc.

2. To obtain by means of a special derivation from the main current the movements augmenting or diminishing the distance between the carbons, in order that the rapidity of these movements may be proportionate to the intensity of the current producing the voltaic arc.

3. To make arrangements such that this rapidity of the movements communicated to the carbons should, at certain periods, be proportionate to the variations in their distance; that is to say to arrange the apparatus so that, in the case where the carbons have to be moved towards each other through an appreciable space, the movement communicated to them may be more rapid than when they have to be moved through a very short distance.

In 1871 I had constructed a lamp which fulfilled the two first of these principles, and which was brought before the Moscow Society of naturalists. In this system I employed as motor a small electro-magnetic machine of Froment, worked by a derivation from the principal current passing by the carbons; and above this electro-motor, the axis of which was vertical, were placed the two electro-magnets of the differential system above referred to. An armature common to both and suspended between their poles like a pendulum, reacted upon a double system of gearing, the axis of which, furnished with two angle-wheels of unequal diameter, would present to the electro-motor one or the other of these wheels according as one or the other of the two electro-magnets was the more energetic. As the wheels in question corresponded to two opposite points of the driving wheel, the movements produced were in opposite directions and could increase or diminish the distance between the carbons with a rapidity greater or less according to the intensity of the current, since the working of the motor was dependent upon this intensity. The drawing of this lamp has been in the polytechnic museum of Moscow since the commencement of the year 1873.

At the end of 1873, M. Jablochkoff, who at that period had a mechanical workshop at Moscow, being convinced of the superiority of the systems of constructing lamps on the derived current principle, made in his workshop a lamp on this principle. I shall not refer to the experiments with this lamp, which gave full satisfaction to several persons. For my own part, I was but partially satisfied, on account of its complication, and because it did not fulfil the third of the fundamental principles I had laid down.

In 1874, I arranged a new lamp, the design of which I brought before the physical section of the Moscow Society of Naturalists, and which is represented by Fig. 1.

E E′ are electro-magnets disposed like those on the other systems and having poles, *a b*, spread out in circular form as in the Gramme machine. K is a Gramme or Siemens ring, the rotary motion of which causes the carbons to move through the intermediary of a double-thread screw, A, and two nuts, B C, which carry the carbons. Lastly, D is a regulating screw, for the purpose of raising or lowering the luminous focus.

The current passes from the positive pole of the generator to the negative pole by three derivations, one of which includes the arc and traverses the ring by means of the contact-pieces *m n*; whilst a second, also including the arc, excites the electro-magnet E (or both electro-magnets in a given direction); and a third which, without passing by the arc, influences the high resistance magnet E′ (or both magnets in contrary directions), so that the action of this magnet upon the ring shall be in a reverse direction to that of E.

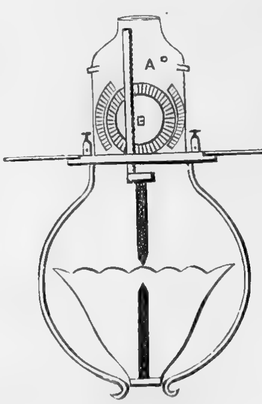

FIG. 2.

In consequence of this arrangement the action of the electro-magnets upon the ring K is almost *nil* when the arc possesses its normal resistance; but when the resistance of the arc augments the action of the electro-magnet, E becomes weakened, allowing E′ to preponderate, and the ring K will rotate so as to bring the carbons into closer proximity. The contrary effect will, of course, be produced if the resistance of the arc should diminish.

*This is somewhat obscure; what is meant, perhaps, is that the sum of the currents traversing the two magnets is, with the adjustments adopted, a constant value.—ED. *E*.

Experience has shown that with such a lamp it is possible to obtain, with regularity and safety, a good electric light with twenty-four Bunsen cells, and at first with even twenty cells. Some of these lamps have been in use in the Russian artillery since 1877. This lamp may also be constructed on the principle of the Wheatstone balance.

The form of my lamp intended for public lighting is represented by Fig. 2. The rod A, with the upper carbon-holder works by the effect of its own weight. When the current traverses the lamp the distance between the two carbons is maintained by the aid of helical coils, but these coils and the toothed wheel which controls the movements are worked, as in the former case, on the principal of derivati ons. When the current is interrupted, the carbons come into contact by the effect of the weight of the rod A.

I omit here certain details of construction which are of importance in order that the lamp may work properly.

To sum up, the advantages of my lamp may be enumerated as follows:

1. Its construction is extremely simple, it is free from clockwork mechanism, springs and electrical contacts.

2. It does not require preliminary regulation nor any manipulation before or during its working.

3. Several of these lamps may be arranged in series in a circuit, and they are always in due relation with the intensity and the tension of the current which is to act upon them.

4. The lamp can work with comparatively weak currents, and also produce a very powerful light when the power of the current is augmented.

I am convinced that the problem of the divisibility of the electric light by means of lamps having a voltaic arc can be solved only with the lamps based on the principle of the derivation of the current, which I discovered prior to Messrs. Lontin and Siemens.

Lamps with movable carbons offering a certain resistance between their polar extremities are moreover far preferable, from the point of view of divisibility, to lamps with fixed carbons (with carbons at a fixed distance?) which may offer great variations in the resistance of the arc, in consequence of impurities, the action of the wind, &c. These variations may in fact be greatly reduced in the former description of lamp, and it is not necessary with them to employ currents of such high tension or, if such currents be employed, additional lamps may be inserted in the circuit.

W. TCHIKOLEFF.

GENERAL NOTES.

CLIMATIC influences have of late been rather against phylloxera, which has shown, therefore, a decreased activity for a time. According to M. Boiteau, the treatment with sulphide of carbon and sulphocarbonate of potassium these past three years past seems to have had even a stimulating effect on the vines (besides ridding them of the insect). Some of the vines thus treated are flourishing better than before the parasite appeared.

M. CHARNAY, the leader of the expedition recently sent to Central America under the auspices of the governments of the United States and France, the expenses of which are to be largely borne by Mr. Pierre Lorillard, telegraphs that the Mexican government has signed a treaty giving him all the privileges and facilities he needs in making explorations and has appointed a representative to accompany him.

MARIE EKUNINA describes, in the *Journal für Praktische Chemie*, an investigation conducted in Professor Nencke's laboratory at Berne, on the causes of acid reaction of the animal tissues after death. This reaction is attributed to the decomposition of tissue juices, after death, by fungi. Volatile fatty acids first arise through commencing decomposition of albumin, but very soon the two lactic acids proceeding from glycogen are associated with these. The richer the tissue in carbohydrates, the longer does the acid reaction continue after death; this is especially the case with liver, muscles, and lungs. The shortest and weakest acid reaction is that in the pancreas. Sooner or later, in all tissues, the acid reaction passes over into an alkaline, while the decomposition of albumin increases, and there is much formation of ammonia.

CORRESPONDENCE.

We have been requested by a correspondent of Lieutenant Colonel Ross to publish the annexed letter, which at present may be accepted as an *ex-parte* statement, which complains of a wrong done to him by certain members of the Royal Society. But while placing our columns at the disposal of Col. Ross, we disclaim any personal responsibility in the matter, and will afford ample space for any reply which Professor H. E. Roscoe, or others concerned, may decide to forward to us for publication. Lt. Col. Ross is well known for his works on Blow-pipe Analysis, and has recently published a small manual on this subject, which we find favorably spoken of by the English Scientific press.—[ED.]

LONDON, 11th June, 1880.

To the Secretary of the Royal Society.

SIR.—In forwarding a copy of my new work on the Blowpipe, for the Library of the Royal Society (which I did yesterday), I have the honor and pleasure to inform you for communication to them, that I have now, beyond reasonable doubt, discovered the coloring principle of the *Sapphire*, and can produce stones made chiefly of alumina, of almost any required tint of blue, green, or "amethyst," *without using any chromatic oxide whatever*, a discovery I believe to be quite unique, for, although a Belgian or French chemist has made real "rubies," he is obliged to color them with manganese or other metallic oxide. I do not propose, however, to communicate this secret to the Royal Society, as I at first intended, for the following reason: When in the Spring of 1873, the Secretary of your Society, with the discriminating perception of the useful and novel which is characteristic of men of genius, came to Woolwich to examine experiments which I was then (as a Captain in the Royal Artillery) making in blow-pipe analysis, and eventually read a paper on the subject before your Society, I little thought that influential opposition instituted by Fellows of your Society, would be the chief cause of retarding my humble efforts in the progress of this new science for nearly ten years. I have, however, the most reliable evidence to prove that Professor H. E. Roscoe, F. R. S., and another Fellow of your Society whom I need not mention here, circulated the most disparaging and depreciatory opinions regarding the novel statements on this subject contained in my work "Pyrology," (a detailed exposition of the views first propounded in the paper read before your Society,) the MS. of which was offered by me to Messrs. Macmillan & Co., in 1874, for publication and declined by them, presumably on the advice of Prof. Roscoe.

Of course I have no right, nor do I for a moment wish to complain of the adverse opinion of eminent men of science, though perhaps such opinions would be more suitably expressed in public so as to give me an opportunity of reply; but what I venture most respectfully now to complain of, is that one of my inventions in Blowpipe Analysis—the use of Aluminium plate—which had been disparaged as above mentioned, has now been *adopted* by that department of Owens College, Manchester, over which Prof. Roscoe so eminently and justly presides, and that a German work on the subject, translated in that department, has interpolated in it an account, spread over thirteen pages, of the very Aluminium plate reactions rejected by Prof. Roscoe in 1874, and, worst of all, the invention is attributed to somebody else in the index of the book, which has been adopted as a text book by the Owens College.

As I have sustained a serious loss by the publication of my work on the subject, chiefly through the opposition above referred to, I would most respectfully ask the council of your society whether they do not think it fair that I should reap any benefits *now*, derivable from my inventions or discoveries? I have the honor to be, Sir,

Your most Obedient Servant,
W. A. ROSS, Lt. Colonel,
Royal Artillery (retired list).

The Secretary Royal Society,
Burlington House, Piccadilly W.

THE "D-LINES" SPECTRA.—ARE THEY DUE TO WATER?

By Lieut-Colonel W. A. Ross, late R. A.

In the year 1834, Mr. Fox Talbot, F. R. S., attributed the all but omnipresent rays affording the above-named spectra to water, on account of its universality; but the celebrated experiment of Kirchoff has, since then, reversed that opinion, and bestowed the power of emitting orange rays upon sodium alone.

Is it not more reasonable, however, to suppose that sodium has more attraction for water than any other substance, than to imagine sodium contained in every possible substance, from the eternally-burning Sun himself, down to every particle of our atmosphere?

A man need not be a chemist, or pyrologist, to observe that, if he holds in platinum tongs a fragment of marble, or artificial carbonate of lime, or magnesia, before the blowpipe, a strong orange flame affording "D-lines" spectra is emitted by the fragment, *only so long as it is imperfectly calcined*, or, in other words, has lost its carbonic acid gas. When calcination is complete, it begins to glow brightly, and emits no colored "flame" at all. Let us call this orange flame (*a*) in this case. According to modern chemical theories (*a*) can only be one of two things; carbonic acid gas or sodium. It cannot be carbonic acid, which so far from being combustible, is used to "put out" flame. It can be proved not to be sodium by making it impinge upon a bead of pure transparent boric acid, in which it causes, after a time, *opalescence*. This opalescence is *removed* by the similar impingement upon the bead of an indubitable sodium orange flame. Reasonable chemists, therefore, will not be inclined to contradict Euclid that the same cause producing precisely opposite effects is a *reductio ad absurdum*. Moreover, for the supposed sodium to have been with the marble, and not with the marble after calcination—"*hic et ubique*," like the ghost of Hamlet's papa—is another absurdity, which no modern chemist would require us to believe, although some are very exacting if not always exact. May I hope that most chemists will be now inclined to admit, first, that (*a*) can be due neither to sodium nor to carbonic acid; and secondly, that it must, therefore, be due to something else: and this is the very impression that occurred to me after thinking profoundly over the matter for several years.

Indeed, I suggested to Mr. Hennessy, who was engaged in making atmospheric observations at Mussooree, in India, with a spectroscope belonging to the Royal Society—and who was elected an F.R.S. for his pains—that the "D-lines" were due to water, not sodium, so long ago as 1871, and he was very much struck by the suggestion.

The methods I adopted to prove the truth (or improbability) of the suspicion which thus arose in my mind were as follows:

1.—The whole of (*a*) in a weighed fragment of pure marble was obtained by fusing it carefully before the blowpipe in a transparent bead of boric acid, when the carbonic acid gas, which we may call (*b*), escaped in bubbles with great effervescence, (*a*) remaining behind as opalescence, and the lime (*c*) combining with a portion of the boric acid to form a ball contained in the bead—let us call the bead (*d*). I found the weight of the lime borate ball (*c a*) bore the same relative proportion to that of the marble (*a b c*) whether the latter had been previously calcined or not, so that the opalescence in (*d*) could not possibly have been part of (*c*) while (*b*) escaped in bubbles; therefore the opalescence *must* have been due to (*a*) or—

$$(a\ b\ c) + 2\ (d) - (b) = \left\{ \begin{matrix} \text{(ball)} \\ (c\ d) \end{matrix} \right\} + \left\{ \begin{matrix} \text{(hydrated boric acid)} \\ (a\ d) \end{matrix} \right\}$$

I could not isolate (*a*) any further than this, because (*a d*) or what I believed to be *hydrated boric acid* is, naturally, as soluble in water as ordinary boric acid, so I tried—

2.—Observing that platinum in considerable bulk also produces (*a*) in a ratio increasing with the decrease of temperature down to a dull red heat, I fused an ounce of crystallised boric acid in an open platinum dish at red heat, and obtained a new kind of boric acid, which turns brown with a strong *resinous smell*, on being ignited, like that of burning sealing-wax, affords a slightly-oranged "flame"—for the green pyrochrome of the boric acid overpowers that of (*a*),—and forms an opalescent bead, which reacts, as I believe, with much greater acidity before the blowpipe than ordinary boric acid does.

This I call *platinised* or *hydrated boric acid*, and shall be happy to show it to any chemist who may consider the matter to be sufficiently interesting.—*English Mechanic.*

AN IMPORTANT DISCOVERY.

A discovery in chemistry has just been published, which bids fair to influence agriculture in a manner that may be well described as revolutionary. It must soon compel the attention of farmers and manufacturers of artificial manures everywhere. The essential part which ammonia plays in vegetation need not here be dwelt upon, and no one will question the desirability of securing it cheaply and in quantity. The importance of the recent feat of Messrs. Rickman & Thompson, of England, disclosing a plan by which ammonium sulphate can be made and sold with profit at two cents a pound, will not, therefore, be liable to be overestimated. The following account of the process is taken from the *Chemical News:*

Within the last twenty years the manufacture of ammonia synthetically has been several times attempted, and though in every attempt it is probable that ammonia has been made, it has never been produced on a commercial scale. In all these attempts the process has been to combine the nitrogen and hydrogen directly at a low temperature, and receive the ammonia in solution in water, or by substitution, first forming a cyanide at a higher heat, and then indirectly producing ammonia by the decomposition of the cyanide, the result in both cases being ammonia in solution with water. Rickman & Thompson's procedure is altogether different, they produce ammonium chloride direct, either in dry powder or in solution, and this by the simplest and most inexpensive means. Instead of employing retorts, as in all other places, they merely use a closed brick furnace, the ash-pan of which regulates the supply of air, and they cause the vapor of water to be produced by the waste heat of the furnace itself. With the exception of about a bushel of coke for starting the furnace, the deoxidizing material and only fuel used is coal-dust. The great difficulty in making ammonia from the nascent hydrogen of water and the nitrogen of the air is the restricted limits of temperature between generation and decomposition, it being necessary that carbon, however used, should be at a full red heat to decompose the vapor of water, and at a bright-red heat to decompose ammonia. Now, ammonium chloride under the same conditions is simply volatilized and not decomposed. As chloride of sodium or of calcium is decomposed at a full red heat in presence of nascent ammonia, therefore one of these chlorides is mixed with the coal that ammonium chloride may be formed; so that if by chance the heat should be raised to a bright red, no loss will be sustained—the ammonium chloride is simply volatilized. By these means a greater range of working temperature is obtained. At the present time, with the consumption of from 20 to 28 lbs. of coal-dust and salt mixed, from two or three lbs. of ammonium chloride is formed.

A New Audiphone.—Further experiments on the timbre of musical instruments as rendered by the audiphone have led me to the selection of the following as a distinct improvement on the birchwood veneer, both for musical purposes and also for ordinary conversation. It has the same advantage as my previous form in not requiring to be held by the hand, it costs nothing, and requires no making. Take a sheet of stiff brown paper about 11 x 15 inches, the paper being such as is ordinarily used for making up heavy parcels. Put the ends together, the middle forming a loop, and hold the ends between the teeth. The paper must be pretty stiff, as the loop must stand out round and full, and of course the paper must be without folds or creases. Thomas Fletcher.

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SCIENCE

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SATURDAY, JULY 31, 1880.

THE CEREBRAL FISSURES OF THE DOMESTIC CAT, *Felis domestica*.

BY BURT G. WILDER, M. D.

The discovery of the electrical excitability of certain cerebral convolutions renders more than ever desirable some common nomenclature of the folds themselves and of the fissures by which they are defined. For various reasons, some of which were stated by me in 1873 (11,219), the *fissures* should first be identified, and their names agreed upon.

Two notable contributions to fissural homologies and terminology have been recently published by Julius Krueg. An abstract of the paper on Herbivora was given by Horsley in *Nature* for January 23, 1879. The second paper, upon the fissures of the Proboscidea, Hyracoidea, and Carnivora, was published in January of the present year, but did not reach the libraries in this country until May. The larger part of the paper is devoted to the Carnivora, and will prove more useful, practically, than the remainder. After a general historical sketch Krueg discusses the manner of formation of the fissures, taking the cat as less subject to variation than the dog. He then enumerates the fissures, with brief characterizations, under three heads: "Grenzfurchen, Hauptfurchen, Nebenfurchen." The detailed account of the fissures is divided into "Canidæ, Felidæ, Hyænidæ und Protelidæ, Viverridæ, Mustelidæ, Procyonidæ, Ursidæ, Phocidæ und Otaridæ." A separate historical sketch is given with each section, and four of the five folding plates of excellent outline figures are devoted to the carnivoral fissures.

In fulfillment of a purpose announced in 1873 (11,229), I have nearly ready for publication a somewhat extended paper upon the Gross Anatomy of the Brain of the Domestic Cat. The conclusions which I had reached respecting the nature, relations and nomenclature of the fissures accord in most respects with those of Krueg. In the hope that his paper may incite others to take up this branch of comparative anatomy, I desire, upon the present occasion, to point out the improvements which Krueg has made upon his predecessors, and at the same time to suggest some amendments to his views.

Krueg does not state whether the brain figured by him is intended as a type, or is merely selected from among the 12 adult brains which he examined. The following figures represent what seems to me to be a comprehensive type of the fissural pattern of the domestic cat, based upon at least 200 specimens, mostly prepared by myself.[1]

EXPLANATION OF THE FIGURES.

The figures are enlarged about two diameters. Fig. 1, the lateral aspect of the hemisphere and lobus

[1] In the Museum of Comparative Zoology at Cambridge, Mass., is a series of 42 cats' brains, more than half of which are young or fœtal, forming part of a collection to illustrate the Neurology and Embryology of Domesticated Animals made by me for the late Professor Agassiz. In the anatomical laboratory of Cornell University each student prepares, draws and dissects two or more brains.

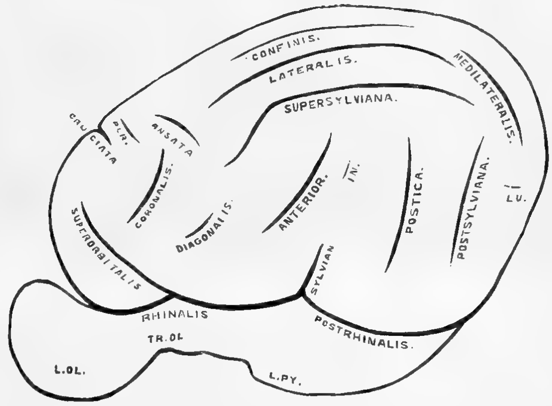

Fig. 1.

olfactorius; Fig. 2, the mesial aspect of the same, but as if viewed more from the cephalic region so as to expose the whole of the strongly curved fissura hippocampalis; hence the figure is somewhat foreshortened.

The constant fissures are shown as dark lines, the inconstant fissures as lighter lines. The f. olfactoria could not be shown upon these figures; it is a shallow groove upon the cephalic end of the hemisphere, and the lobus olfactorius rests in it.

The following abbreviations designate fissures: Ge.—Genualis; Ro.—Rostralis; Pmr.—Postmarginalis; Pr.—Postrhinalis; Pcr.—Postcruciata; Lu.—Lunata; In.—Intermedia. The name is placed above the fissure only in the case of the f. callosalis.

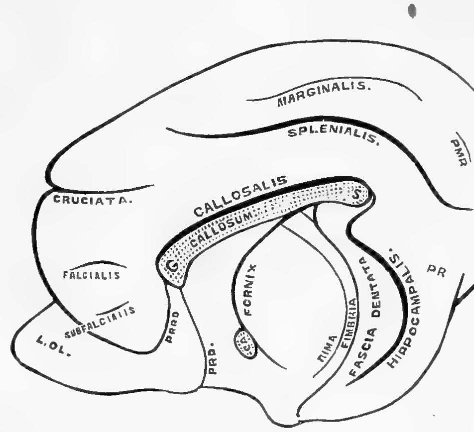

Fig. 2.

All of the names designate fissures, excepting the following names or abbreviations which refer to parts of the brain structure: Fornix; callosum—the corpus callosum; c. a.—the commissura anterior; rima—"the great transverse fissure;" fimbria—the corpus fimbriatum or taenia hippocampi; fascia dentata; S.—the splenium or caudal end of the callosum; G.—the genu or cephalic end; l. ol.—the lobus olfactorius; tr. ol.—the tractus olfactorius; l. py.—the lobus pyriformis.

The figures are diagrammatic, especially with reference to the structures represented upon the mesial surface; for instance the fornix is shown as a simple line.

The lines represent what may be called the *fissural integers*, and only those junctions are shown which, so far as I know, are constant in the cat. These are of the rhinal with the postrhinal, and of the sylvian with the point of their union; of the superorbital with the rhinal; of the callosal with the hippocampal, and with the preradical when it exists.

The following junctions I have never observed: Of the splenial with the postrhinal; of the splenial with the cruciate, which Guillot has seen once, and Krueg twice. Neither have I seen the union of the anterior and posterior fissures to form the "first or lowest arched fissure" of the Canidae. On the contrary, as stated by Krueg (2,613), and by myself (11,229), this union sometimes fails with domestic dogs; hence, in this as in many other respects, the cat presents less tendency to vary.

The following junctions are common: Of the diagonal with the anterior; of the postsylvian with the supersylvian; of the medilateral with the lunate, and with the lateral or the confinis; of the marginal with the post marginal; and of the ansate with the lateral or coronal or both.

A junction is usually marked by a less depth of the compound fissure at that point, constituting a concealed "transition convolution" or "pli de passage," which may be seen by separating the sides or by slicing off the cortex.

The fissura ansata is represented by me as a slightly curved line nearly at right angles with the lateral and coronal, and rather nearer the former. It is true, as stated by Krueg, that the apparent form of the fissure is usually triradiate; but the variations are so great that no single figure would fairly represent them all, and in two brains I have found the condition of things shown in the figure. This fissure demands fuller investigation, especially with reference to its representation in the human brain.

The sylvian fissure in the cat does not present the complexity observed by Krueg in some dogs and in Ungulata, and the "Insula" is not distinguishable.

My paper will contain a *synonymy* of the fissures of the cat's brain, with full references to the page and figure upon which a fissure is named or represented. In some cases there are 25 entries under a single head, and I trust the lists may aid others in the identification of the fissures as described by different authors.

ABRIDGED SYNONYMY OF THE CEREBRAL FISSURES OF THE DOMESTIC CAT.

ABBREV.	*Flower*, 1869. *Leuret*, 1839. *Huxley*, 1861, 1872.	*Owen*, 1868.	*Wilder*, 1873.	*Krueg*, 1880.	*Adopted in the present paper.*	
An.				Ansata	Ansata	An.
A.		Ant. branch of ectosylvian	Ant. upright of ectosylvian	Anterior	Anterior	A.
C.		Callosal			Callosalis	C.
Cf.		Part of medilateral	Part of medilateral	Confinis	Confinis	Cf.
Co.		Coronal	Coronal	Coronalis	Coronalis	Co.
Cr.	Crucial	Frontal	Frontal	Cruciata	Cruciata	Cr.
Di.				Diagonalis	Diagonalis	Di.
Fl.		Part of falcial		Genualis	Falcialis	Fl.
H.	Hippocampal. Dentate, H., 1861	Hippocampal		Hippocampi	Hippocampalis.	H.
L.		Lateral	Lateral	Lateralis	Lateralis	L.
Lu.					Lunata	Lu.
Ml.		Part of medilateral	Part of medilateral	Medilateralis	Medilateralis	Ml.
Mr.		Marginal		Suprasplenialis	Marginalis	Mr.
Ol.			Ectorhinal	Olfactoria	Olfactoria	Ol.
Pcr.				Postcruciata	Postcruciata	Pcr.
P.		Post. branch of ectosylvian	Post. upright of ectosylvian.	Postica	Postica	P.
Pmr.				Postsplenialis	Postmarginalis.	Pmr.
Prd.					Postradicalis	Prd.
Pr.		Part of ectorhinal	Part of rhinal	Rhinalis post	Postrhinalis	Pr.
Ps.		Postsylvian	Part of supersylvian	Suprasylvii post	Postsylviana	Ps.
Prrd.					Preradicalis	Prrd.
R.		Part of ectorhinal	Part of rhinal	Rhinalis	Rhinalis	R.
Sfl.		Part of falcial		Rostralis	Subfalcialis	Sfl.
Sp.	Callosomarginalis, (Fl. & H)	Supercallosal		Splenialis	Splenialis	Sp.
So.	Supraorbital, (Fl)		Presylvian	Præsylvii	Superorbitalis	So.
Ss.		Supersylvian	Postsylvian & supersylvian.	Suprasylvii	Supersylviana	Ss.
S.	Sylvian	Sylvian	Sylvian	Sylvii	Sylviana	S.
In.					Intermedia	In.

The foregoing is an abridgement of this synonomy limited to writers who have made special additions to the technical nomenclature, and excluding those who have employed phrases or vernacular names, or who have adopted the names of other writers in purely physiological papers. Notwithstanding the importance of the contributions of Flower, Huxley and Leuret, the technical names employed by them are so few that they may be given in a single column. It is due to Krueg to state that several of the names now given had been already used in his paper on the Ungulata, in which he included a diagram of a dog's brain.

The principles of anatomical nomenclature are hardly identical with those of taxonomy, but it seems right that priority should prevail excepting when the name implies an incorrect or doubtful homology, or is practically very objectionable. Hence, Owen's "Postsylvian" should not be displaced by Krueg's "Suprasylvi posterior," or his "marginal," by "suprasplenialis." Likewise, Flower's "supraorbital" has priority of my "presylvian," which Krueg has adopted. On the other hand, Krueg's "anterior" and "postica" are so much more usable than previous names as to be worthy of acceptance, especially as they may be regarded as abbreviations of the phrases by which Owen and myself designated the fissures in question. "Splenialis" also is to be preferred to "supercallosal" or "calloso-marginalis," so long as the human homologue of the fissure is uncertain. If *marginalis* be retained, *postmarginalis* will be better than "postsplenialis."

I am particularly gratified to find that Krueg admits as fissural integers the *ansata* and the *diagonalis;* the former I had intended to call *transversa*, and the latter *intermedia*, but Krueg's names must be retained. We agree also in regarding Owen's "medilateral" as composed of two fissures, which Krueg terms "medilateral" and "confinis." I had intended to leave Owen's name attached to the fissure which is really mesiad of the lateral, and to call the curved division *lunata*. I still think this would have been preferable; but as it is, the name *lunata* may be given to what would otherwise have been *sublunata*. I have applied the name *intermedia* to a fissure which Krueg mentions, but does not name.

Doubtless my readers, especially those who are especially interested in the physiological aspect of the subject, desire to learn the correspondence between the cat's fissures and those of monkeys and man. I hope that Krueg may shortly give us the benefit of his opinion. Meantime, I am obliged to admit my doubts with regard to all excepting the callosal, hippocampal, and olfactory; for the Sylvian is not yet fully understood. I believe that for a long time to come the most useful work will be done upon nearly related forms, and that each fissure should be monographed with respect to its constant and variable characters, its connections, its relations to internal structures or to more primary fissures, and especially its manner of formation.

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Flower, W. H. (28). On the anatomy of the Proteles cristatus. Zool. Soc. Proc., 1869, 474-496.

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CORRESPONDENCE.

To the Editor of "Science:"

Perhaps the following may interest the readers of "SCIENCE." It has always been my experience that a Black Snake, *Bascanion constrictor*, when confined with any other snake smaller than itself will invariably eat it. The following food has been eaten during the month of July, by a black snake five and a-half feet long, on exhibition at Central Park Menagerie: 3 leverits, 3 sparrows, 1 cat-bird, 1 small chicken, 1 black snake four feet long, 1 milk snake, 1 small rattlesnake; total weight, eight pounds.

W. A. CONKLIN,
Museum Building, Central Park.

DEATH OF A NATURALIST.

WE have to record the death of Mr. Green Smith, of Peterboro, New York, son of the late Gerard Smith, whose name will ever be remembered by those who value the cause of human liberty.

For many years past Mr. Green Smith left no oppor tunity neglected by which he could add to his fine collections of the birds of the United States. On one occasion he gave $1000 for 240 specimens of humming birds, and probably spent from ten to fifteen thousand dollars in forming his unique collection.

As Mr. Green Smith purchased specimens, they were prepared and mounted by the well-known taxidermist, Mr. J. G. Bell, of New York City, who appears to have been consulted by Mr. Smith on all occasions.

During his life Mr. Green offered his collection to the Museum of Natural History in Central Park, on the condition that the collection should be kept intact, and should bear the name of the generous donor. The offer, however, was declined by the trustees, on the ground that such a condition was inconvenient, and established a precedent which it was not well to encourage.

We have reason to believe that such refusal has been long since repented of, and some hope is expressed that this fine ornithological collection may still find a home in the Central Park Museum.

A GERMAN naturalist, in the course of inquiries as to the phosphorescence of the sea, has found that the phenomenon occurs whenever sea-fishes are brought into a three per cent. salt solution. The luminosity begins apparently in the eyes, spreads over the whole fish, and increases day by day. The fish after some time seems luminous throughout. The phosphorescent substance is a kind of mucus which appears dirty-white by day, and shines in the dark.

THE electric light is at last to be put to a crucial test in the city of London. Tenders are to be asked for the illumination of the principal thoroughfares of the area bounded by Cheapside and the Thames, from Blackfriars to London Bridge; the three bridges from London, Southwark and Blackfriars, along with Queen Victoria street and Ludgate Circus to Cheapside, through King William street to London Bridge, with a cross line from Cheapside to Southwark Bridge. No doubt there will be sharp competition.

A PLEA FOR THE METRIC SYSTEM IN MICROSCOPY.

By R. H. Ward, M. D., Pres. Am. Soc. of Micr.

One of the most important questions, theoretical and practical combined, which is now fairly before the microscopical world and still in an unsettled state, is that of gaining definiteness and uniformity in *micrometry*. In this field emergencies have arisen during the past year which have compelled me to take considerable responsibility, as well as to perform a large amount of work, trusting that the generous approval of my colleagues would accept and ratify what seemed at the time, and what seems now, most consistent with the interests of science and the dignity of this body. It will be remembered that a year ago, just at the close of our Indianapolis meeting, resolutions were offered favoring the metric system for micrometry, and the one hundredth of a millimeter as the unit to be employed, inviting foreign co-operation, and accepting an offer of standard micrometers from Prof. William A. Rogers, of the Astronomical Observatory of Harvard University. None of these points, save the last, were new or unconsidered. They had been studied at leisure for years by many members who were present. The metric system had been adopted by all the world except Russia, England and the United States; and its universal adoption was, as a rule, earnestly desired and favored by the educated and scientific classes. It had been adopted, or recommended, after mature deliberation, by the National Academy of Sciences, the American Metrological Society, the American Association for the Advancement of Science, by the American Society of Civil Engineers, the United States Coast Survey, the United States Marine Hospital Service, the American Medical Association, the Congress of Ophthalmologists, and by the largest State and local Medical Societies and by leading Medical Schools and Journals, by numerous Boards of Education, College Faculties and local Scientific Societies, and by experts in various branches of science and art. On the other hand the resolutions contained some minor faults, mostly in matters of taste or tact, which could have been easily remedied by reference to a committee. But there was no time for reference or for adequate discussion, and rather than discourage their object by failure or postponement, they were adopted and referred to the local Societies for consideration. They were passed unanimously, at a small session, it is true, but by the same vote which established this society and authorized its meeting here to-day. As too often happens, their incidental faults attracted more attention than their really scientific object. The unit proposed was evidently too long for integers and too short for fractions, and unlikely to receive a single approval either at home or abroad; the proposal of international action, though its object was universally approved, was in a form not likely to accomplish that object; and the liberal offer of Prof. Rogers was wholly misunderstood and perverted, until it took the form of the preposterous statement that it was proposed to make Prof. Rogers' micrometers standard as distinguished from those of other (!) makers, not the least amusing of all the blunders and absurdities of this precious statement being that of bringing the association, in any manner, by trade rivalry or mercenary considerations in relation with the work of one of our most generous scientists who has freely shared with the public every result of his labors, while pursuing them at an extravagant cost, and without a thought of pecuniary return. It soon became evident that an organized treatment of the subject was required to secure a proper and unprejudiced discussion of the objects of the resolutions. Feeling much responsibility as the presiding officer of this Society, and of one of the oldest of the local Societies, but having no authority to appoint an evidently necessary committee that should represent not only this Society but also sections of the country not yet named upon our rolls, I brought the subject before our local Association, and we invited all the Societies that could be reached to join with us in the selection of a National Committee for the consideration of this subject. The response from the large and active Societies, and from distinguished individuals, was a cordial and almost unanimous approval. Many of the Societies nominated to the committee members distinguished as specialists in this branch of microscopy; both Societies and eminent scientists contributed valuable opinions upon all the points at issue; and a large committee was organized which will, at a proper time, tender a report of progress to this Society. And while speaking of this committee, I will take the liberty of saying that it would be a pleasure to me, and I doubt not to all of us on this side of the lakes, if our friends from Toronto or Montreal, or any other points in the Dominion which may be represented here, would nominate members, and thus make it an American instead of a national body. To prevent confusion or misapplication of the practical suggestions which follow, and which naturally belong to this time and place, it is necessary to anticipate the report of the committee so far as to say that it will recommend to this Society to rescind its approval of the one-hundredth of a millimeter as the unit of micrometry, and to so modify the forms of the other resolutions as to leave the important questions of accurate measurement and convenient and scientific nomenclature in a favorable form for the attainment of valuable results.

Whether this Society, as such, shall continue to be known as actively interested in this reform, it is for you to say; though I sincerely hope that the members will unanimously agree with me in judging that it ought to do all that its influence, without dictation, can do in this direction. But I for one do not deem the decisions of Societies or other corporate bodies decisive and final. I am not much elated by their approval, or discouraged by their opposition. I have an average amount of respect for the motives but not for the efficiency of legislation. In State, in Church, in Science, it is possible and easy to carry out laws about in proportion as they are unnecessary. People who do not need government are easily governed. Persons who appreciate authenticated micrometers will use them if they can, with or without the approval of societies; and those who do not desire them will be about as little controlled by official decisions. While the encouragement and support of Societies and officials are welcome and valuable as far as it extends, I have more faith in the power of individual influence, and to that I look for an example which is able to settle this question beyond appeal.

In our micrometry we have the anomaly of a system of work capable of a precision almost, if not quite, unknown elsewhere to human art, for what other wholly artificial procedure possesses a demonstrated limit of accuracy inside of the 1-300,000th of an inch, and yet, until now, we have made no reasonable effort to free ourselves from avoidable errors known to be many times larger than that amount. While coal at $4.00 a ton and muslin at six cents a yard are, or at least pretend to be, measured with apparatus that has been carefully verified by standards of known quality, we have been measuring spaces almost infinitessimally small by standards of only commercial quality and possessed of manifest and uncorrected errors. This fact is too suggestive of the days when micrometers consisted of grains of sand and clippings of wire; with the odds against us that we know how to do better. Arrange your microscope so that it will magnify 3,000 or 4,000 times, making the one-thousandth of an inch on the stage seem three or four inches long through the lenses, then arrange an ocular micrometer so that the magnified one-thousandth of an inch shall be covered by, for instance, one hundred divisions of the ocular scale, and finally ascertain exactly how many of the one-thousandths of an inch on that or any other plate will be similarly measured by precisely the same one hundred divisions above it. Judging from my experience and from that of others who have tried the experiment, you will probably find a perfectly measurable discrepancy between the different spaces of the same name; so that even your own measurements, with the same apparatus, will not be comparable with each other unless, as is often done, you select some one average space as a basis of comparison, and are careful to use only that. Now we are trying to ascertain which of these various spaces is the correct one; or if not one is right, then to obtain one that shall be; or if that can not be done, at least to determine a known error from which we can compute definite results. This is not a question of makers, or dealers, or trade interests in any form, but of unmixed and independent science. We are attempting to procure a standard because we need it, and we hope for the cordial assistance of microscopists of really scientific spirit in the difficult work of attaining it, and in the almost equally important task of bringing it into general and respected use. I call this a standard for convenience, and not in a strict or ultimate sense. Strictly it is only an authenticated copy of a standard, or a portion of a standard, namely, of the world's standard meter or standard yard; and hence, the importance, not fully shared by the original metre itself, of corresponding perfectly with its theoretical length.

The adoption of the metric system has a formal sound, and its difficulties have been, to say the least, well represented. But, to the extent of its use in micrometry, it really presents no difficulties and many advantages. The value of the millimeter and its decimals must be made familiar to the mind for other purposes, even for the understanding of exclusively English literature, and to use it for our measurements and statements will merely assist to keep it fresh in mind. The English system, or rather tradition, presents no pair of units so convenient for the microscopist as the millimeter for large objects and the 1-1000th millimeter for small ones. For the purposes of most people, for use in micrometry alone, it is sufficient to remember that the millimeter is about one twenty-fifth of an inch, and surely this is no great intellectual task. Nor would it waste a large portion of a lifetime to learn the whole series from the meter down, remembering that, in round numbers, the meter is a yard, with three or four inches to spare, the decimeter one-tenth of that 40 inches, or 4 inches, the centimeter one-hundredth of that 40 inches, or 4-10ths of an inch, and the millimeter one-thousandth of that 40 inches, or 4-100ths, or 1-25th of an inch. The real difficulty lies, I believe, not in memorizing the value of the few new units required, but in the awkward and useless habit of stopping to translate every item from the new unit to an old one. Any one can add a few new words to his vocabulary, a few new units to his tables, without harm. The telephone and the phonograph have brought no disaster along with their new double Greek names. An educated person can learn in an hour all the new terms, values and proportions of the whole metric system, with its interesting and suggestive relations; and the time would be well spent though he never used the system again. But I know by experience that he can also use it again, easily. When you once learn by a little practice to think in the new units the same as in the old, the apprehended difficulties vanish unaccountably and can scarcely be brought to mind. If asked to estimate the width of this room in yards, only a child unfamiliar as yet with the practical use of measures would say to himself, "It seems to be about 90 feet, which would be 30 yards." You would rather look at the wall to see how many times longer than a yard it is. So if you will take a metric rule, learn well how the millimeter looks, and its dek, the centimeter, and learn to use it in measuring and estimating the size of suitable objects, such as insects or flowers, you will find it as easy to think in millimeters as in lines, inches, feet, or yards, to say nothing of the comfort of knowing that you are in no danger of being lost between several kinds of the same name.

Aside from the selfish though sufficient motive of our convenience, I hope we shall practically adopt the metric system, because we can thus contribute a trifle of influence toward its general introduction. It seems plain enough now that our country made a serious mistake in not adopting it at first; and I am satisfied that it is still best for us to use it, notwithstanding the greatly increased difficulties in our way. It is not questioned that this is the best system ever tried or proposed, and the only one that can possibly come into general use; it is not denied that it would simplify education, and substitute order and intelligible relations for the confusion of our present metrology; it possesses as many points of convenient relationship to our old system as could reasonably be expected in any new one; it is admitted to be excellently adapted to all scientific work; it has been satisfactory to mechanics and manufacturers who have actually used it; it has been gradually and completely introduced into large shops using costly tools and machinery, without serious expense, and to the satisfaction of the managers. Almost without exception its friends are those who have used it, and objections to it come from those who have not. You hear less of the evils it has caused than of those it would cause. Furthermore, it offers us a carefully elaborated scheme of international co-operation, which we have but to adopt in order to place ourselves in harmony with the rest; the metric system is all international. It also unifies almost entirely the records made by persons adopting different units, since a statement of size will be practically the same to the eye and to the ear, and will require no formal mathematical reduction, whether in centimeters, millimeters, or in decimals of a millimeter. Fortunately we have all tried the experiment for ourselves, in one department, and know what some of the objections are worth. Our system of currency is precisely like the metric series of weights and measures; and is marked essentially by the same evils and benefits. Who now believes that having adopted a currency incompatible with the English system has caused us a hundredth part of the trouble it has saved, notwithstanding that it lacked the advantage of putting us in harmony with the rest of the world? Who now feels cut off from the past because of the change, or regrets the loss of the pounds and shillings so long as he has dollars enough and of the right kind (it is not easy to satisfy everybody about that)? Who has found the poor oppressed and the laboring classes annoyed by the system we adopted? Who has yet incurred a burdensome expense in hiring accountants skilled in a foreign and to us obsolete nomenclature to compute from the records of the past how many pounds, shillings and pence our grandmothers paid for their bonnets, or our grandfathers for their ships or their farms? The truth is the new system is so much better for our present purposes that we are glad to use it as soon as we fairly know how; and I believe that the same would be true of the whole metric system. We do not undervalue the records of the past, with their elaborate computations, and tables, and surveys; but few of the people of to-day come in contact with these directly, and those few could afford the extra trouble for the sake of the far greater interests involved. It is not scholars to whom learning in an unfamiliar form is a terror; they will spend lifetimes in working over such lore, merely for the pleasure of the work. And so much of it as is required for use in the daily life of the illiterate world is consistantly modified, and modernized, and adapted, by specialists of various kinds who inherit the progress of the past but adopt the fashions of the present.

To adapt a homely phrase which has remarkably vindicated itself, in another field in recent history, the proper way to introduce the metric system is to introduce it; not to decide why others should use it, but to use it ourselves. Nor need we wait to be certain of the feasibility of securing its universal use. It may be profitably used in science though not accepted in trade. The chemists have adopted it fully and with satisfaction in their work, in their teachings and in their books; the physicians are adopting it in different parts of the country, and the microscopists may well enjoy its facilities whether others do so or not.—(*Inaugural Address*, *Buffalo*, 1879.)

EXPERIENCE, says the *Electrician*, has shown that the life of a submarine telegraph cable is from ten to twelve years If a cable breaks in deep water after it is ten years of age it cannot be lifted for repairs, as it will break of its own weight; and cable companies are compelled to put aside a large reserve fund in order that they may be prepared to replace their cables every ten years. The action of the sea-water eats the iron wire completely away, and it crumbles to dust, while the core of the cable may be perfect. The breakages of cables are very costly, and it is a very difficult matter to repair them, in comparison with a land line. A ship has to be chartered at an expense of $500 a day for two or three weeks in fixing the locality, and in avoiding rough weather, as cables can only be repaired in the calmest seasons. One break alone in the Direct Company's cable cost them £20,000 to repair, and the last chance left to the company was to make an agreement with the Anglo-American, so that they should be protected and have the use of that company's line when their own was stopped.

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Those engaged in Scientific Research are invited to make this Journal the medium of recording their work, and facilities will be extended to those desirous of publishing original communications possessing merit.

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Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply, may be written in the form of an article.

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PROFESSOR LEIDY'S "FRESH WATER RHIZOPODS OF NORTH AMERICA."

Dr. Leidy is acknowledged to be the highest authority on the subject treated in his great work, "Fresh Water Rhizopods of North America;" a criticism of the book becomes, therefore, a work of supererogation, and we reserve to ourselves the more pleasing task of pointing out its many beauties and particularly its importance as one of the most valuable contributions to the literature of microscopic forms of life.

Published by the Department of the Interior of the United States Government, and forming volume twelve of the "Report of the United States Geological Survey of the Territories" in charge of Professor F. V. Hayden, it is produced in a sumptuous form which no private publisher would have dared to imitate.

Dr. Leidy's Report covers about three hundred folio pages, illustrated by forty-eight full sized plates, printed in colors in the highest style of lithography.

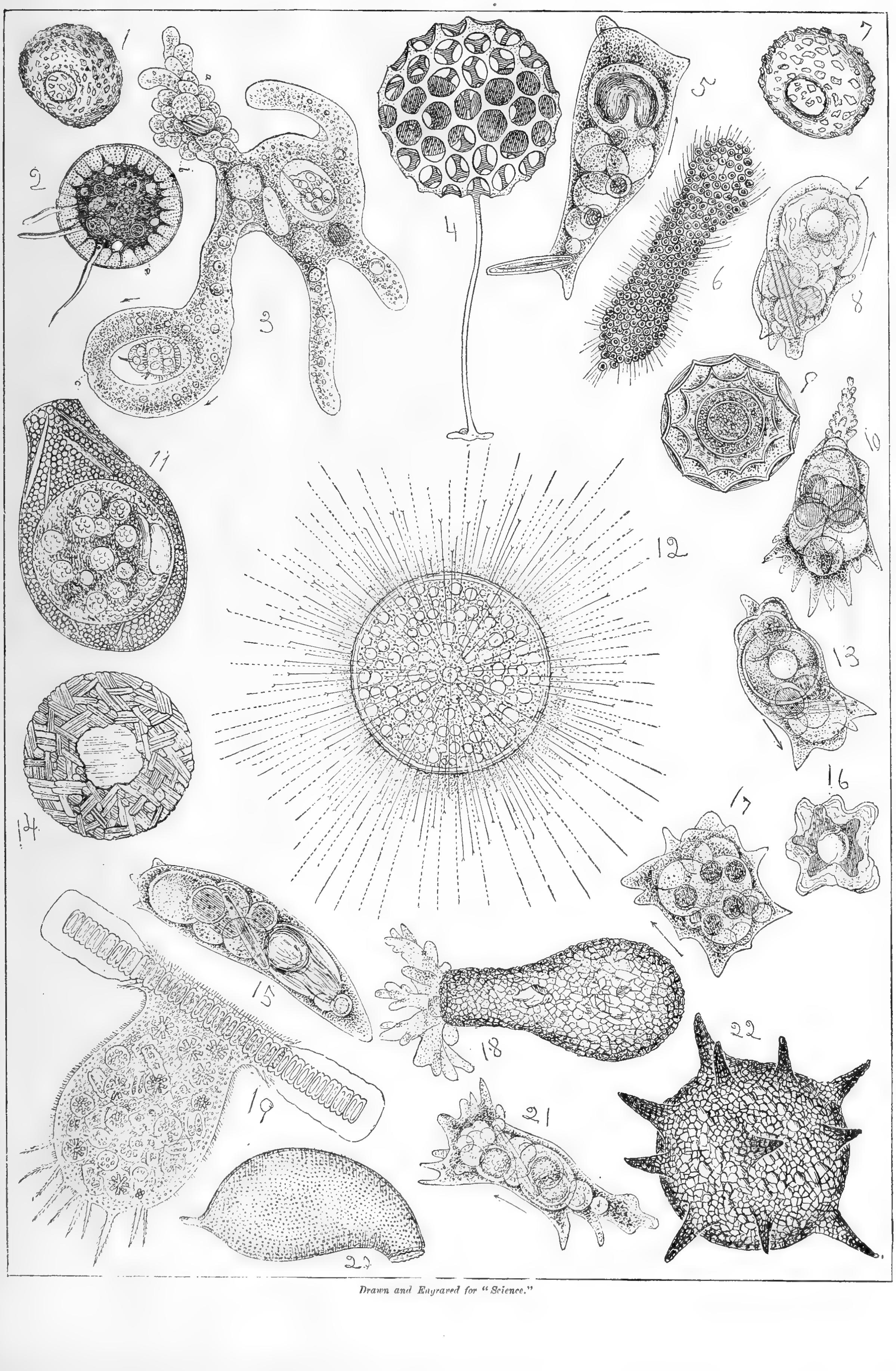
It may be a superfluous question to most of our readers, but as Dr. Leidy himself inquires in the first page of his work, "What are Rhizods?" In reply he says, "Rhizopoda are the simplest and lowest forms of animal life, constituting the first class of the Protozoa. They derive their name from the Greek word *rhiza*, a root, and *pous*, a foot. They are mostly microscopic beings, although sometimes sufficiently large to appear as conspicuous objects." We may add that the essential characters are the gelatinous structureless bodies, and the locomotive organs consisting of variable retractile root like processes (pseudo-poda or false feet).

Their minuteness is compensated for by their multitude and wide-world distribution; essentially aquatic they occur wherever there is moisture; the search for them may be commenced in the crevices of the stones at your door step, and may be continued in every marsh, pool, ditch, pond, lake, sea and ocean, and from the greatest depths of the latter to the snow lines of mountains.

The particular Rhizopods which form the subject of the book now under consideration, are those found in fresh water only, and Dr. Leidy expressly states that his attention, during the four years engaged, was directed more to the discovery and determination of the various forms occurring in the United States, than to the elaboration of details of structure, habits, modes of development, and other matters pertaining to their history.

Although it is professedly an illustrated catalogue of the fresh water Rhizopoda of North America, we find most interesting and valuable contributions to their life history which makes us regret that time and opportunity did not permit Dr. Leidy to extend his observations in this direction, for we know how exhaustive such a treatise would have been from his hands.

Instead of writing a discursive essay upon Dr. Leidy's book, already done by many brillant writers, which, with a work so purely technical, seems the least profitable method of treating the subject, we propose to take our readers through the book, acting the part of a friendly guide, trusting by the aid of twenty-two illustrations we have reproduced for this purpose, not only to do justice to the work in question, but to convey to those who have not



Drawn and Engraved for "Science."

studied the subject, a fair idea of the nature and form of Rhizopods.

In figure (3) we have a representation of the Amœba proteus, the most simple and wonderful of Rhizopodic forms—it is, to all appearance, a structureless gelatinous mass. As the *International Review* states (we fear somewhat rashly) it is without organs; they are certainly not permanently visible, and so far the most intelligent research has not discovered them; but the Amœba has the power of extemporizing such organs as are necessary for its existence, and has the means of reproducing its species. In the drawing, the pseudopoda, or false feet, are seen extended, and the animal is sketched while in the act of capturing an infusorial, by the act of joining the points of two of these pseudopoda.

In the Dinamœba mirabilis, figure (19), we find an advance made in structure, and a more specific form; the interior may be noticed to be full of desmids on which the animal feeds, and its posterior is widely expanded, so as to embrace a cord of Didymoprium.

In figure (18), Difflugia pyriformis, our Rhizopod is now found with a case or shell formed of irregular particles of quartz sand.

The empty shell of another of the same species, D. lobostoma, is shown at figure (14). The shell is composed of rectangular and oval plates, with dotted intervals.

Another of the Difflugia, D. Corona, at figure (22), still with a case formed of the same material, but of a somewhat different form, and having eleven spines; on the reverse side, the mouth would have been seen armed with teeth.

In figures (1) and (7), D. Constricta, we have further examples. In the original drawing they are colored a light redish-brown, whereas the previous examples were colored a blackish tint.

We now approach a beautiful design in form, the Nebela collaris, figure (11), the sarcode being contracted in the shape of a ball.

The design shown in figures (2) and (9), Arcella vulgaris, is still more beautiful and decidedly approaching the delicate patterns seen on the silicious valves of diatoms. The first in the original drawing is colorless, the second a light-brown, the sarcode being encysted.

In figure (20) is seen the empty shell of Cyphoderia ampulla, having a form somewhat similar to the Marine Rhizopods; the spur-like process may be noticed.

Clathrulina elegans, figure (4), is a very beautiful and remarkable form of Rhizopod, having a yellow colored lattice shell, enclosing sarcode in two balls, and supported by a stem of the same color. In other specimens Dr. Leidy shows the sarcode balls emitting numerous rays; this appears to be omitted in the present drawing.

At figure (6), Diplophrys Archeri, is a form of a different class taken from the swamp water on the mountains of Pennsylvania. Dr. Leidy describes them as composed of multitudes of minute globular individuals aggregated in masses, which in their movement causes the whole to undergo a change of shape. The corpuscle seen in each transparent body should be colored a bright cherry-red, to imitate the original drawing.

Acanthocystis chætophora, represented at figure (12), is very similar in form to the Actinophrys sol, or Sun animalcule, of the text books. The body is a finely granular protoplasm, invested with numerous delicate, silicious rays, implanted by minute basal disks; there are also numerous soft rays, like those of the Actinophrys, but distinguished from the silicious rays by the former ending in a simple pointed or furcate extremity.

We have now carried our readers briefly through Dr. Leidy's book, and have shown the beauty and variety of the forms studied. The interest attached to such investigations is obvious, and they are within the reach of every student. Considerable work has still to be done in this direction, and however great may be that done by Dr. Leidy, we feel sure that he would be the first to admit that so far the ground has been but broken.

To our mind it seems most important to concentrate future work on the genus Amœba, for therein lies the nucleus of the most valuable discoveries, which may even be of the highest importance to the human race. What is required in this direction is the constant and continuous observation of a single individual of the species, so as to arrive at its life history. We did hope that Dr. Leidy, in his present work, would have added to our knowledge on this point; but he frankly admits that neither himself nor others profess much knowledge relating to the reproduction of the Amœba.

It is, however, a question with us, if Dr. Leidy has not missed such an opportunity during his recent investigations.

At page 49 of this work on Rhizopods we notice Dr. Leidy describes what he calls an act of cannibalism, when he saw an individual (Amœba proteus) swallow and digest one of another species (Amœba verrucosa). We would ask, was this really an ingestive or a sexual act? Dr. Leidy takes the former view. With great respect for his experience, we suggest that it may have been the latter, and we do so on the following ground.

In 1873 the Rev. W. H. Dallinger, F. R. M. S.,

FIG. 1.

undertook, with Dr. J. Drysdale, to make a continuous microscopical investigation into the life history of a certain species of Monas. For this purpose they constructed apparatus which prevented the evaporation of the water, and the conditions were made perfect, to keep the specimen in a living state for a considerable time under the microscope. They then commenced to watch the monads in turns continuously, keeping the object in view without a break in observations for many days. During this close study of these monads, Messrs. Dallinger and Drysdale also observed an act so similar to that noticed by Dr. Leidy, that we will produce both statements for comparison.

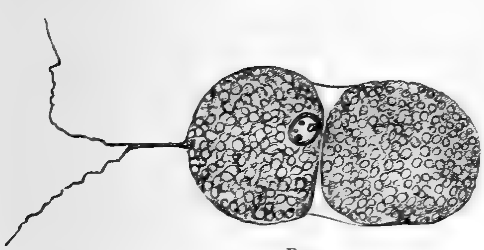


FIG. 2.

Messrs. Dallinger and Drysdale found the modes of re-production with the monads to be equally as varied as that known to exist with the Amœba, but there was one method which particularly attracted their attention.

Two monads at times met and touched their anterior ends, swimming freely together (figure 1), their bodies *then melted into each other*, (figure 2); it then became a single oblong mass, the line of juncture slowly disappearing, when after from six to 24 hours, it became rounded, (figure 3); at length the edges gave way, and myriads of minute points poured out, which were watched until they developed into perfect monads.

In figures Nos. (5, 8, 10, 13, 15, 16, 17, 21)* of our illustrations may be seen in the drawings, from Dr. Leidy's work, of what he observed of the mingling of two Amœbæ; he first speaks of their mutual approach, followed by an "embrace," when the jointed ends of the pseudopods *fused together*, and one sank deep into the body of the other, and eventually assumed the appearance of a sphere; further internal breaking up was then noticed, but later observation was not made. This is to be regretted as it seems quite probable that as "*the melting into each other*" was proved in the case of the monads to be a sexual act, that the *fusion*, when noticed by Dr. Leidy in the Amœba, may have been of the same character.

The necessity for the continuous mode of study

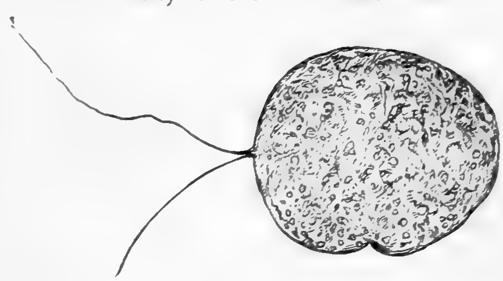


FIG. 3.

of such forms becomes more apparent every day, especially in the case of the Amœba, for in the very next paragraph to that just quoted, Dr. Leidy states "in my studies of Amœba proteus I have not been so fortunate as to trace its history from an early period, nor to discover its modes of reproduction."

There are other points to which we would refer, but having already exceeded our limit of space, we can only conclude by extending our thanks to Dr. Leidy for this his latest and most valuable contribution to SCIENCE, and to those who had the foresight and judgment to direct its publication.

* Figures (5, 8, 10, 13, 15, 16, 17, 21) illustrate the series of changes observed in the swallowing and digestion of an *Amœba verrucosa* by an *A. proteus*. From Bristol Marsh, Pennsylvania, August 27, 1876, 500 diameters. Fig. (16). *Amœba verrucosa*, comparatively quiescent with central contractile vesicle. Figs. (5, 8, 10, 13, 15, 17, 21). Successive changes in shape and relative position of *A. proteus* during the act of swallowing and digesting the former. Fig. (21). *A. proteus* approaching the *A. verrucosa* with anterior short diverging pseudopods. Fig. (8). The *A. proteus* embracing closely the *A. verrucosa* by a pair of digitate pseudopods, the points of contact of which being marked by the left hand arrow. Fig. (13). The *A. verrucosa* swallowed and forming a large sphere within the *A. proteus*. Fig. (10). The *A. verrucosa*, within the latter, has assumed an oval form, and is contained within a vacuole. The central contractile vesicle, which until now had remained persistent, had become less distinct. Fig. (15). The *A. verrucosa* has assumed a pyriform shape within a large elliptical vacuole and its contractile vesicle disappeared. Fig. (5). The *A. proteus* in the act of discharging a diatom while the *A. verrucosa* has become doubled on itself. Fig. (17). The remains of the *A. verrucosa* seen as five granular balls within the *A. proteus*. Later these balls disappeared and their material appeared to be diffused among the granular contents of the *A. proteus*.

THE FOURTH STATE OF MATTER.

A REFUTATION.

Translated for "SCIENCE" *by Gustave Glaser, Phil. D.*

It may interest the readers of "SCIENCE" to know the opinion held in Germany respecting those phenomena which led Mr. W. Crookes to believe he had discovered a fourth state of matter. For this purpose we have translated and abridged an article by Dr. J. Puluj, the well-known scientist of Vienna, published in the "*Chemiker Zeitung*:"

According to Mr. Puluj, the beautiful experiments of W. Hittorf published in 1869, under the title "Electrical Conductibility of Gases," have received too little attention from our scientists, it may be, on account of the modest title. The scientific labors of Goldstein, and some interesting researches of Reitinger and Urbanitzky have met with the same fate. W. Crookes, the renowned English chemist, to whom the writings of the above-named gentlemen were evidently unknown, made similar experiments, the results of which did not differ essentially from those of Mr. Hittorf. His conclusions were, however, entirely new; he declared that his experiments proved a fourth state of matter.

The conception was daring, still more daring the hopes which he and his friends based upon the discovery of "radiant matter." The cause of these high expectations is the following: When an electrical inductive current is led through a molten glass tube in which the air is attenuated to $\frac{1}{1000}$ of its density, there appears on the negative pole a blue (glimmering) light, which is separated by a dark space from the cluster of light at the positive pole. If a greater attenuation takes place, the cluster of light disappears and the glimmering light floats over the whole tube, while at the same time, next the electrode a second dark space appears which becomes greater with the greater attenuation. If the attenuation still further increases, the dark space fills the whole tube and the glass walls shine in a brilliant, green, phosphorescent light. Mr. Crookes now believes that this phenomenon of phosphorescence comes from the remaining gas, which at this high state of attenuation has passed into an ultra-gaseous state, a "fourth state of matter."

But these phenomena are very different at a higher pressure. Direct measurements have shown that the phosphorescence does not appear at the millionth attenuation, and that the thirty thousandth attenuation is sufficient to produce it. Besides, this attenuated gas retains its characteristic properties, which could not be the case if by this attenuation it became dissolved into the original molecules which form, as Mr. Crookes says, the basis of all.

That the physical properties of this remaining matter are not changed, but remain in strict accordance with the kinetic theory of gases, also proves that we have no new state, but simply a gaseous state of matter. For example, the above-mentioned phenomena, in experimenting with the lighter gases, are visible at a lower altenuation than in experimenting with the heavier gases. The supposition of the renowned chemist, Dumas, that our elements are only chemical combinations of higher order, and complicated aggregates of primitive molecules, has, undoubtedly, much probability about it, but even the strongest electrical currents, and the highest temperatures, have not been able to produce this final dissolution of the elements, therefore it is not likely that a high attenuation can.

Dr. Puluj's experiments go to show that Mr. Crookes' so-called radiant matter "consists of negative electric particles," which are torn off from the negative electrode and hurled away with immense rapidity. These electrode particles form a very beautiful metallic mirror on the glass walls. [Aluminum particles are the only ones which form no metallic deposits. This may be accounted for by their chemical constitution.] The conduction of the current, therefore, is effected by the convection of the electrode particles, in which static electricity is accumulated. We have here a case of molecular electric convection, analogous to that observed by Mr. Rowland in his experiments. This gentleman has demonstrated that when a movable horizontally placed metal ring, charged with static, positive or negative, electricity is made to rotate around a vertical axis, it will divert a magnetic needle suspended above it, in the same manner, as if an electric positive or negative current were to move in the same direction with, or in an opposite direction from, the rotation. These experiments of Rowland lead to the inference that an infinitely small electrical globe, in our case an electrode particle, will have a similar influence upon a magnet. As long as the globe and magnet are at rest, it is to be expected that no alternate effect will appear, but that this will be produced as soon as the little globe is put into violent motion. Because the electrode particles are negative electric, they represent a positive electric current, which moves in an opposite direction from the former. The electrode particles in motion are, therefore, real elements of an electric current, and are subjected to the law of Laplace. Their deviation takes place according to the following simple law: If we imagine that a plane is placed through the direction of the motion of the electrode-particle and through the north pole of the magnet, and suppose that a man is lying upon this electrode particle in the direction of the motion, and looking towards the north pole, then the electrode-particle will be diverted towards the left hand of this man, vertical to the imagined plane. This simple law gives a sufficient explanation for all the phenomena which a magnet produces in the radiant electrode-matter, and which were observed by Mr. Crookes as well as by Mr. Reitinger and Urbanitzky. It proves that the glimmering light at the negative pole is not a "magnetic" light, but the consequence of a molecular electrical convection, and it justifies the supposition that an electrified current or vapor which is led through a tube will deviate the magnetic needle in the same manner as an electrical current going through a telegraph wire.

The law of the indestructibility of force has already solved many problems which puzzled the scientist of earlier centuries. According to the same law, we must assume that when infinitely small projectiles of radiant electrode matter are hurled against the glass walls of the tube their motion is changed into molecular motion, and the glass walls are heated by the collisions, sometimes even to the melting point; but at a lower temperature the rays which are not very much concentrated only produce a phosphorescent light of the glass.

The extremely fine matter called ether, which fills all space and pierces all bodies, surrounds the molecules, as the atmosphere surrounds our globe. Each body and each molecule has in its normal state a certain quantity of this ether. When this quantity is greater than the normal quantity, the molecules, according to the "unitarian view" of elasticity, are positive electric; when it is smaller, they are negative electric.

Supposing now that a collision takes place between the negative electrode particles and the molecules of the glass walls of the tube, then the equilibrium will be restored at each point of collision and the molecules of the glass will lose their surplus of ether. At the same time a motion of the waves of ether will be observed, and this motion is felt by our optical nerves as phosphorescent light. Therefore the phosphorescence observed by W. Crookes is the result of the restoration of the ether-equilibrium and not of the heating of the glass, whose temperature during the appearance of this phenomenon is comparatively low.

At a lower degree of attenuation, the stream of electrode matter pushes back the attenuated gas, and this explains the dark space which appears in the tube. This dark space is analogous to the dark space in a gas flame, which is to be seen near the mouth of the gas tube, and is produced because the outstreaming gas pushes back the particles of air which, coming from an opposite direction, try to enter the tube.

* * * * * *

Another observation of Mr. Puluj also contradicts the conclusions of W. Crookes. Puluj has observed that, at a higher attenuation, the electrode is moving towards the aluminium side, *i. e.*, in opposite direction from that observed by W. Crookes. According to Mr. Crookes the cause of motion is a double one, the higher temperature of the electrode at the metallic side and the emission of electrode particles.

Both effects are opposite. At a lower attenuation the effect of the heat is greater, and the electrode moves in the direction of the wings of the radiometer, with the colder side ahead, at a higher attenuation, the effect of the emission of electrode particles is predominant. Radiant electrode matter and the electrode itself move in the *same* direction.

This remarkable discovery proves not only the incorrectness of Mr. Crookes' explanation, but is also in direct opposition to the principle of the preservation of the centre of gravity, which is made by Mr. Crookes the basis of his arguments.

The Vienna scientist draws from his observations the conclusion that the forces by which the electrode particles are torn off are not interior but *exterior* forces. When the electric current passes through the electrode, there is, according to his opinion, really a stream of extremely fine matter (ether) flowing, which not only tears off particles of the electrode, but also sets the whole electrode into motion.

This view seems to be a new proof of the unitarian hypothesis, which maintains that an electric current is nothing else but a current of ether.

Even if the number of scientists who follow the dualistic hypothesis of electricity is by far greater than that of the Unitarians, the view of the latter deserves at least our attention, especially when such men as Franklin, Secchi and Edlund approved it.

THE MAGNET IN MEDICINE

Translated for " SCIENCE " *by Thos. B. Columbia.*

Some recent researches made under the direction of Prof. Charot in his laboratory at the Salpêtrière have drawn attention anew to a therapeutic agent known for a long time, but to-day almost abandoned. We find, in fact, even in the works of the oldest authors, traces of attempts made by physicians to apply the magnet in the treatment of disease.[1] But the want of precise rules in its application and the appearance of mystery and of fancy which is attached to this kind of research explain the discredit into which this means of treatment has fallen.

We are indebted to Prof. Maggiorani for having undertaken, in about 1869, the restoration of magnetic therapeutics, by seeking to establish it upon rational and truly scientific principles.

It was in the train of the experiments undertaken by the Commission appointed by the Biological Society, of Paris, with the object of verifying the facts collected by M. Burq under the generic title of Metallotherapy,[2] that the first attempts toward the application of the magnet were made at the Salpêtrière. After the results obtained by the application of metals, it was natural to seek to clear up the singular phenomena by varying as much as possible the conditions of the experiment. In this way it was shown that the plates of the different metals were not the only agents capable of acting upon a certain class of diseases (neuroses, and particularly hysteria, organic affections of the cerebral nervous system). Similar results were attained with many physical agents : feeble currents, statical electricity, vibrations of sonorous bodies, differences of temperature, magnetized bars, electro-magnets, solenoids, etc. Very soon the magnetic bars were noticeable for the constancy of their action and facility of their use.

Magnets are, therefore, not endowed, from this point of view, with specific properties ; they form part of a group of physical agents which, to different degrees, possess the same power of impressing the nervous system and of giving rise to biological phenomena ; and although magnets are here particularly spoken of, it must not be forgotten that they are not the only ones concerned.

The status of the question has been clearly exposed by Dr. Vigoroux in the *Medical Annual* (1879). To this article I must refer those who wish to become acquainted with the *ensemble* of phenomena, which are included under the name *metalloscopic*. These studies, begun at the Salpêtrière, have given rise to active discussions. The facts announced have been confirmed, wholly or in part, in Germany by Müller of Grätz, Westphal, Vierordt, Schiff, Adamkiewicz of Berlin ; Benedick of Vienna, Rumpf of Dusseldorf ; in Italy, by Seppilli, Maragliani, and especially Maggiorani ; in England, by Gamgee, Sigerson, H. Tuke ; in France, outside of the work of the Commission, I will mention only the thesis of M. Aigre and the observations of MM. Dumontpallier, Vigouroux, Landouzy and Debove, who have verified the therapeutical action of the magnet. But the results obtained were sharply attacked on the other side of the Channel by Hughes, Carpenter and Noble, who attempted to explain them by "expectant attention." In a thesis read before the Faculty of Medicine of Paris in 1878, Mr. Oscar Jennings made himself the champion of the ideas expressed by these English writers.

As to what relates to the magnet itself we are going to show, summarily, the arguments upon which are based its physiological action and its therapeutical use.

The action of the magnet, among effects produced by other physical agents of which we have spoken (plates of different metals, electricity, vibrations of the diapason), presents itself in a more surprising way, and, indeed, in a way *à priori* prone to excite incredulity. The application is not direct. The magnet is not placed in contact with the skin of the subject upon whom the experiment is tried, as it is necessary to do with other metalic plates, its action being exerted at a distance. It is sufficient to influence the organism, and produce the same effects as other metals, to place the magnetized bar at a distance of one to two centimetres from the portion of the body upon which we wish to make an impression. All the experiments at the Salpêtrière have been made with these conditions. The effects produced in these cases were not attributed to the action of the metal, and belong properly to magnetism itself.

The magnet, let us say, acts in some way on the organism when in these special morbid conditions. Before speaking of the facts which prove peremptorily that this action exists, can we not, if not explain it, at least conceive of the possibility of such an effect. The action of

[1] Among the authors who have given attention to the action of the magnet in medicine, we may cite: Pliny the Younger, Paracelsus, Albert the Great, the older Hell (1770), Mesmer (1779), Andry and Thouret (1780), Becker (1829).

[2] See *La Nature*, Feb. 17, 1877.

physical forces upon biological phenomena has long since been admitted; who does not recognize the importance of heat, of light, and of electricity upon the vital manifestations? In medicine electricity, under its different forms, is daily employed in the treatment of a multitude of diseases. Why refuse to one physical force that which we accord to all the others? Why, if all physical agents are only varied forms of the same force, should they not all exert an action upon the organism in a measure different for each of them? And then why should not magnetism, which possesses in such a high degree this very singular property of influence at a distance, be able in physiological order to produce analogous effects?

If from conjectures we pass to the consideration of facts, we are forcibly convinced that this physiological action of magnetism at a distance does truly exist.

In physics, experiment leads to a, in some degree, tangible result; it is undeniable; it forces itself upon us. Bring the poles of a magnetized bar near to some iron filings and you have the conditions for the experiment; the iron is attracted and there is the result. Discussions may arise upon the theory, upon the interpretation of the fact, but the fact itself is always present. Furthermore, as it is easy to appreciate exactly all of the circumstances of the experiment, we are certain that with the same conditions given, we shall always obtain the same result. In a word, the experiment can be easily repeated. In physiology experiment is surrounded by the greatest difficulties, but the result is neither less significant nor less certain. As regards experiments made with the magnet, it can be shown that they fulfill all the conditions of certainty of physical experiments.

In the first place it is necessary that the application be well done; that is to say, that the magnet ought to be in good condition and properly placed. It is not necessary that the magnet should be very large, nor endowed with very energetic properties; it is sufficient if the magnetic force exist in an appreciable degree. The experiment has often been attempted with false magnets, that is to say, with bars or horeshoes of variable substance, zinc, copper, wood, etc., not possessing any magnetic action, but having all the appearances of true magnets. In these cases the experiment has always given negative results. Likewise, in making use of the electro-magnet, the action upon the organism takes place only when the established current gives to the soft iron its magnetic properties. It is necessary also that the magnet should be properly applied; the poles alone acting, the neutral portion should remain absolutely without effect. This is easily accomplished when using the magnet in the form of a horseshoe and by presenting it successively by its open and its closed side. A patient has his eyes bandaged; the magnet is applied to the region of the back, in such a manner that he shall have no knowledge of its position, and physiological phenomena always follow such application of the poles, and never follow the application at the neutral line.

But is it easy to verify the result obtained? How does the magnet work? How is its action demonstrated? Does it not depend, one will say, upon phenomena of sensibility which are purely subjective and appreciable with great difficulty to the experimenter, who is obliged to trust to the word of his patient? I will reply at once that it is sufficient to have assisted at a single one of the experiments at the Salpêtrière to be convinced that these phenomena, subjective it is true, can be easily rendered objective. A large needle made to pass unawares through the flesh of the subject whose eyes are kept carefully bandaged, shows in an absolutely objective way the profound anæsthesia which the parts have attained. But the phenomena of sensibility are not the only ones produced; the magnet has an influence upon temperature, as the thermometer distinctly shows.[1] It acts also on the *molilité* of the parts to which it is applied, provoking contractions of an intensity and of a duration which removes all suspicion of simulation. The physician must be a mere novice who could mistake a prolonged and voluntary contraction for a true contracture. Now, the magnet produces in certain cases true contractures. (*La Nature.*)

DR. P. RICHET.

(*To be continued.*)

NOTES AND QUERIES.

To the Editor of Science:

[1.] In the 3d. July number of SCIENCE, under "Notes and Queries," J. H. G. desires "reliable information concerning the Tuckahœ."

I will tell him what I know concerning it, in regular sequence with his questions:

1st. What is its geographical distribution?

I know, from North Carolina to Texas, along the gulf States—presumably elsewhere also.

2nd. What is the nature of its growth and production?

An underground fungus or root, growing under the surface like the truffle. Belongs to the gasteromycetous fungi, according to Lindley and Fries.

3d. Its former use and preparation?

Used as bread, by roasting in the ashes, both by Indians and negroes.

4th. In what soil found?

Sandy or loamy surface with sub-soil of clay.

5th. What authors have mentioned it?

Dr. McBride and F. P. Porcher of South Carolina; Clayton, Le Conte, M. J. Berkeley, Fries & Lindley.

6th. By what botanical name is it known?

Lycoperdon solidum.

7th. Has it any medicinal virtues?

I know of none.

It is considered very nutritious hence its name *tuckahœ*, which is Indian for *bread*, and is highly prized by the negroes, who eat it to this day.

There is a district of country in the eastern counties of North Carolina called Tuckahœ from the abundance of these subterraneous bodies.

Although the Tuckahœ has been placed among the fungi, yet there is considerable doubt as to its true position in the vegetable kingdom.

Analysis shows it to consist almost entirely of pectic acid, which would seem to remove it from the fungi, and yet the entire absence of vascular or cellular structure of bark, etc., would seem to remove it equally as far from the phænogams.

I hope these few *enseignments* may help J. H. G., and that he will favor us with the results of his further researches.

MRS. M. J. YOUNG,

HOUSTON, TEXAS, July 19th.

THE Rev. W. Cowell Brown, Wesleyan minister of Sheffield, has patented an invention which appears to be a simple and practical means of lessening the number of deaths by drowning. A chemical preparation is inserted in a portion of the coat, waistcoat or dress. It does not add to the weight, or in any way alter the appearance of the garment. The preparation is inserted between the lining and the cloth; in the case of a coat, it is placed on each side of the coat and up the back. The moment a man falls into the water the coat becomes inflated, and he cannot keep his head under the waves. The invention has already been thoroughly tested, and it is stated that it will sustain a person in the water as long as he can possibly endure the exposure, say forty-five or fifty hours.

[1] M. Broca presented to the Academy of Medicine, at the Session of February 7, a note from Dr. Henrot (of Reims) upon the action of the magnet in hemihypothermia. He states that the application of three magnets to the cold limb raised its temperature from 1.8° to 2.3° in twenty minutes, at the same time lowering the temperature of the normal member two-tenths of a degree.

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The list of Associate Editors is not yet complete, but so far it comprises the names of Professional Scientists who stand at the head of the various branches of Sciences with which they have identified themselves.

The Editor will be glad to hear from those who will take an interest in the success of this Journal, or who desire to co-operate in any way; and suggestions will always be welcomed and receive due attention. The intention of publishing "SCIENCE" has been known only to a few, but the following well known and esteemed Scientists have expressed either their intention to contribute, or their approval of the object of the Journal, and good wishes for its success.

Professor SPENCER F. BAIRD, and Dr. ELLIOTT COUES, Smithsonian Institution, Washington, D. C.; Professor EDWARD S. HOLDEN, Naval Observatory, Washington; Professor O. C. MARSH, Yale College; Professor BURT G. WILDER, Cornell University; Professor C. A. YOUNG, Princeton College; Professor S. W. BURNHAM; Chicago; Professor O. STONE, Observatory, Cincinnati; Dr. J. S. BILLINGS, National Board of Health, Washington; Professor WILLIAM A. HAMMOND, M. D., New York; Dr. EDWARD C. SPITZKA, New York; Prof. CLEVELAND ABBE, Army Signal Office, Washington; Professor EDWARD S. MORSE, Peabody Academy of Science, Salem, Mass.; Professor ALEX. A. JULIEN, Columbia College, New York; Dr. J. J. WOODWARD, Surgeon General's Office, Washington; Professor H. P. BOWDITCH, Cambridge, Mass.; Professor H. L. SMITH, Hobart College, Geneva, N. Y.; &c., &c., &c.

All Communications should be addressed to THE EDITOR, Box 3838, P. O. New York.

SATURDAY, AUGUST 7, 1880.

BIRD FURNITURE.

BY DR. J. B. HOLDER, ASST. SUPT. AM. MUSEUM.

It is fair to say that, hitherto, no serious effort has been directed toward the proper furnishing of museum cabinets. This is especially the case with reference to Ornithology. Excellent specimens, often rare, all most valuable, and many special pets, highly prized, are placed with such surroundings and accessories as to very much detract from the pleasing effect they are capable of producing. This is so patent that any intelligent observer must have noticed the incongruity, though the cause may not be clear to him.

This subject will bear a very much fuller consideration than we can now give it. We will, however, present the results of a deliberate and careful study of certain *desiderata* in this connection. The trustees of the American Museum of Natural History in Central Park, New York, in view of the great value and scientific importance of the Prince Maximillian collection of birds, owned by the museum, determined to have them mounted in a manner commensurate with their worth.

A large collection of skins of North American birds presented by Mr. Elliott, was also placed in the hands of the taxidermist. To be in keeping with the excellent work sure to come from Mr. Bell, the matter of perches and stands became of the first importance. In large museums specimens are frequently moved to allow of more varied views, or to give place to others near them: it is desirable to exhibit one bird facing, another sidewise, the next with its back in front; this involves a change or removal of label.

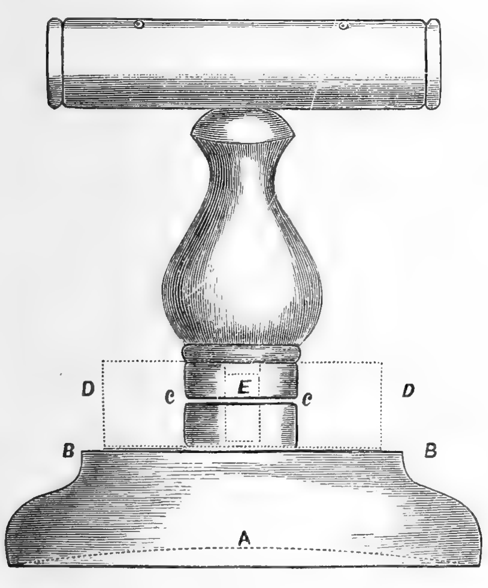

Fig. 1.

To simplify this, as the result of our experiments, we refer to the figure of the bird perch. The wood is of the plainest straight-grained mahogany, handsomely polished. Though somewhat more expensive, it is regarded economical to use such furniture, as in a large museum it is manifestly desirable to avoid any future overhauling, or what householders call "spring cleaning." The polished stands and perches only require the occasional use of a feather duster. Birds once mounted in this manner require nothing further, and remain intact for all time. The base of the perch is hollowed, as indicated by dotted lines at "A." This is to allow labels or written notes to be concealed safely beneath for reference.

The top of the base "B.B." is, practically, a tablet, upon which the label rests and rotates. The upright or column presents an appearance of completeness after the label is mounted; the bead just under which the label works forming the base, all below being out of sight.

Fig. 2.

"E" represents a bit of tin which embraces the label in front on its upper edge, and lies behind and in contact with it. A slender copper wire is passed around the tin, and is then compressed into the groove which surrounds the upright at "C.C." This wire is twisted behind the upright, and left projecting sufficiently to allow it to be held by the thumbnail while rotating the label upon the tablet, "B.B." It will be readily seen that this movement is easily made, and constitutes the chief point of interest in the perch. We think also that the proportions of the perch, the manifest harmony of parts, the fitting relations of label and tablet, each designed for the other in due proportions, may be regarded as improvements more or less in advance of the old methods.

Another group of birds than those that perch require flat stands; as ducks, walking birds, etc.

Our second illustration exhibits a device, very simple, yet suitable. It is desirable to place the label upon the stand, so that it may be removed or its locality changed readily. The block, as seen in the figure, has a bead on each edge; these beads are grooved to admit the label, which is of stiff parchment paper. The upper groove is deepest, so that the upper edge of the label may be pressed into it sufficiently to allow the lower edge to drop into the lower groove. It will be seen that we then have a label free to move, the entire periphery giving the utmost freedom for exhibiting every aspect of the specimen. Of course these blocks may be made with upright sides. Though the labels are more expensive for the bevelled ones, the latter have a more pleasing appearance.

COMPETITION BETWEEN THE ANILINE AND MADDER DYES.

BY A. S. MACRAE.

As these dyes are globularly used to the extent of some one hundred million dollars per annum; as they are as well known to the manufacturers of New England as to the horse-hide colorers of Japan, it may be interesting to inquire what effects, in *esse* and *posse* the one is having upon the other in commercial value. And as the market price invariably depends upon supply and demand, the source of the former must be examined into that the estimate of the latter may lead to judicious deductions.

Previous to the modern use of the above, indigo, cochineal, and the vegetable or wood dyes were altogether in vogue, and the inestimable appreciation of the indigo was primarily the cause which led to the discovery of aniline. The coloring matter of indigo has long been technically known as anil, and the manner in which it gave the name to aniline, has perhaps never been published before this present article. The botanist had ever been puzzled to know whence came the coloring matter of the indigo plant. Where it was indigenous the dyeing matter was inherent; but although the plant flourished almost anywhere in tropical climates, it invariably lost its color yielding power on this transportation! How was this? The botanist had to appeal to the chemist for explanation. Investigation demonstrated that the anil or coloring matter was solely due to the subsoil over which the indigo plant fructified, and that apart from this metaliferous or possibly bituminous earth, the coloring idiosyncrasy was lost. It will thus be seen that the article cannot be produced at will, but only where it and the soils are indigenous. However much this certainty baffled the botanist, it only set the chemist a-thinking. His analysis and synthesis showed beyond cavil, that anil, pure and simple, was neither more nor less than a hydro-carbonic compound, and that amongst some of these artificially produced compounds, anil, otherwise than the anil of indigo, might yet be discovered. The cheapest object for this research naturally suggested itself, and common coal-tar—the refuse of gas works—presented itself as the most economic basis of naphtha, and the matrix of an abundant hydro-carbon. It would be irrelevant here to trace the success which crowned the chemists' efforts to produce anil, or as it was now called, aniline, from this once—but now no longer so—rejected filth. But one portion of the discovery must be referred to, not only in demonstrating the discoverers' wonderful patience, but as proof of the capricious supply of this marvellous product. Coal tar, then, yields naphtha; naphtha, benzole; benzole, nitro-benzole; nitro-benzole, aniline. When the naphtha was first distilled from coal tar, no benzole was discovered in it, or, if it was discovered, in such small quantities as to defy remunerative production. But the trace was there, and as most auriferous deposits are discovered by traces, these said traces were pursued until the golden goal was scientifically and successfully attained. When the naptha was distilled by different temperatures, it was found that benzole was produced at one temperature that was smothered at another, and that by grading the distillations actual benzole could be eliminated in paying quantities! From this moment common coal-tar became the matrix of those valuable aniline dyes, which under the names of roseine, aniline reds and crimsons, Nicholson's blues, Humbolts, mauves, magentas, Bismark browns, oranges, iodine greens, purples, magdalas, violets, greens, phosphines, etc., have astonished the world for the last twenty years. Nearly all the dazzling colors worn now-a-days, that dim the sun and flaunt the eyes, are derived from the very cheapest of bases named, yet have arrived at such a value in the manipulation, that prices run from $2 to $30 a pound and in some cases even $6 an ounce.

At the period of these discoveries, madder had largely superceded indigo, cochineal and other dyes, and at its producable price was certainly the most economic dyeing product extant. Madder is neither more nor less than the ordinary madder root ground, a root capable of cultivation to an unlimited extent. Turkey in Asia, Italy, France, Spain, Holland, and Naples produced it in enormous quantities and British India soon followed suit. The importations into Great Britain at one time amounted to 50,000 tons, and at least a similar quantity was consumed in the countries of production. Unknown as madder may be by that nomenclature, every housewife knows it under the appellation of the "Turkey Red," the name manufacturers gave to their prints dyed by this article. Some idea of its consumption even in America may be given, when it is stated that the writer of this article saw some 500 tons of this madder in the manufactory of A. & W. Sprague & Co., of Providence, R. I., when he visited those works a few years ago.

If then aniline is used by the *pound* where madder is used by the *ton*, it may well be asked by merchants, manufacturers and dyers, what will be the effect of the competition between them? the one the limited production of human manipulation, the other the unlimited production of cultivated nature. We will examine the question.

"Every dog has its day," and in the day of aniline there was but one opinion, that it would sweep every other dye out of the vat-house. Not only was its application so simple, requiring solvents instead of mordants, but at the price, and especially at the price then current for all dyes, it was the cheapest, with given results. A cosmopolitan demand at once set in, therefore, for anilines, a demand which not only enhanced figures to famine prices, but which was far beyond the possibility of supply. That supply depended on coal tar; coal tar depended upon gas works; gas works, after all, are of limited number all over the world—*ergo*, the aniline supply could be but limited. As madder fell into a state of almost desuetude, prices naturally depreciated, until from an average of twelve cents a pound, it is not now worth two cents. Thus, as aniline became scarce, madder became cheap, and manufacturers were enabled to pit their "Turkey Reds" in the shape of Pompadour prints and their like, at prices the very best informed anilinists, or anybody else, never dreamt of. And this brings us to the issue.

We cannot now see, whatever we foresaw in bygone days, that madder and its derivatives, have anything at all to fear from aniline and its beautiful eliminates. As circumstances alter cases, so the position of the two chief dyes are equalized by the extent of the supply and the restrictions of demand. Aniline can not be produced *ad libitum*, madder can. Almost unlimited high prices will always be given for the former; but the latter, experience shows us for the first time, can be grown for almost unlimited low prices. The rich and the poor consumers can thus be well served; but madders go with the poor and therefore the popular prices of both may, nay they will, fluctuate as markets may dictate; but the fear that aniline will end in the supercession of madder is, we think, entirely groundless. The madder "day" is imminent, if not actual now-a-days, and wherever we go its "hues" are more prominent than those of its great competitors.

THE influence of magnetisation on the tenacity of iron has been lately studied by Signor Piazzoli. Iron wires were hung between two hooks and ruptured by pouring water into a vessel suspended from them. They were about 350 mm. long, and were inclosed in a spiral with four windings one over another, which were either all traversed by a current in one direction, or two by a current in one direction, and two by an equal opposite current, so that in both cases the wires were equally strongly heated by the spiral; but in one case they were magnetised, in the other not. The weights required to break wires annealed in charcoal—weight of one metre, $G = 0{\cdot}299$—were, during magnetisation, $P = 1260\text{-}1306$; without magnetisation, $P' = 1213\text{-}1270$. In the case of wires annealed in carbonic oxide—where $G = 0{\cdot}46$ g·—$P = 1732{\cdot}4\text{-}1742{\cdot}7$; $P' = 1703{\cdot}62\text{-}1719{\cdot}87$. In the case of wires annealed in hydrogen $P = 1289{\cdot}5\text{-}1310{\cdot}1$; $P' = 1263\text{-}1299{\cdot}7$. In each separate series accordingly the difference, $P - P'$ was frequently less than the difference between the highest and lowest weights required for rupture of apparently identical wires; still, the mean values in each of the fourteen series were from about 1 to 3 per cent greater for the magnetised than for the unmagnetised wires, showing that the tenacity of iron increases on magnetisation.

DEGENERATION.

By ALFRED R. WALLACE.

Degeneration causes an organism to become more simple in structure, in adaptation to less varied and less complex conditions of life. "Any new set of conditions occurring to an animal which render its food and safety very easily attained, seem to lead as a rule to degeneration; just as an active healthy man degenerates when he becomes suddenly possessed of a fortune; or as Rome degenerated when possessed of the riches of the ancient world. The habit of the parasitism clearly acts upon animal organisation in this way. Let the parasitic life once be secured, and away go legs, jaws, eyes and ears; the active and highly-gifted crab, insect, or annellid may become a mere sac, absorbing nourishment and laying eggs."

We see incipient cases of degeneration in the loss of limbs of the serpentiform lizards and the pisciform mammals; the loss of eyes in the inhabitants of caverns and in some earth-burrowers; the loss of wings in the Apteryx and of toes in the horse; and, still more curious, the loss of the power of feeding themselves in some slave-holding ants. More pronounced cases are those of the barnacles—degenerated crustacea, and the mites—degenerate spiders; while we reach the climax of the process in Ascidians—degenerate vertebrates, and such mere living sacs as the parasitic Sacculina and Lernæocera, which are degenerated crustaceans. Not only such lesser groups as the above, but whole orders may be the result of degeneration. Such are the headless bivalve mollusca known as Lamellibranchs, which are believed to have degenerated from the head-bearing active cuttle-fish type; while the Polyzoa or Moss-polyps stand in the same relation to the higher Mollusca as do the Ascidians to the higher Vertebrates.

While discarding the hypothesis that all savages are the descendants of more civilized races, Prof. Lankester yet admits the application of his principle to explain the condition of some of the most barbarous races—"such as the Fuegians, the Bushmen, and even the Australians. They exhibit evidence of being descended from ancestors more cultivated than themselves." He even applies it to the higher races in intellectual matters, and asks: "Does the reason of the average man of civilized Europe stand out clearly as an evidence of progress when compared with that of the men of bygone ages? Are all the inventions and figments of human superstition and folly, the self-inflicted torturing of mind, the reiterated substitution of wrong for right, and of falsehood for truth, which disfigure our modern civilization—are these evidence of progress? In such respects we have at least reason to fear that we may be degenerate. It is possible for us—just as the Ascidian throws away its tail and its eyes and sinks into a quiescent state of inferiority—to reject the good gift of reason with which every child is born, and to degenerate into a contented life of material enjoyment accompanied by ignorance and superstition."

This is very suggestive; but we may, I think, draw a yet higher and deeper teaching from the phenomena of degeneration. We seem to learn from it the absolute necessity of labor and effort, of struggle and difficulty, of discomfort and pain, as the condition of all progress, whether physical or mental, and that the lower the organism the more need there is of these ever-present stimuli, not only to effect progress, but to avoid retrogression. And if so, does not this afford us the nearest attainable solution of the great problem of the origin of evil? What we call evil is the *essential* condition of progress in the lower stages of the development of conscious organisms, and will only cease when the mind has become so thoroughly healthy, so well balanced, and so highly organized, that the happiness derived from mental activity, moral harmony, and the social affections, will itself be a sufficient stimulus to higher progress and to the attainment of a more perfect life.

For numerous instructive details connected with degenerated animals we refer our readers to the work itself—truly a small book on a great subject, and one which discusses matters of the deepest interest, alike to the naturalist and the philosopher.—*Nature*.

PHYSIOLOGY OF THE FRESH WATER MEDUSA.

The structure of this remarkable animal has already been investigated and described by Professors Allman and Lankester, with the result of showing that, although constituting a new genus, it is in all respects a true Medusa. After the publication of their papers I began to work out the physiology of the new form, and the following are the results which so far I have obtained.

The natural movements of the Medusa precisely resemble those of its marine congeners. More particularly, these movements resemble those of the marine species which do not swim continuously, but indulge in frequent pauses. In water at the temperature of that in the Victoria Lily-house (85° F.) the pauses are frequent, and the rate of the rhythm irregular—suddenly quickening and suddenly slowing even during the same bout, which has the effect of giving an almost intelligent appearance to the movements. This is especially the case with young specimens. In colder water (65° to 75°) the movements are more regular and sustained; so that, guided by the analogy furnished by my experiments on the marine forms, I infer that the temperature of the natural habitat of this Medusa cannot be so high as that of the water in the Victoria Lily-house. In water at that temperature the rate of the rhythm is enormously high, sometimes rising to three pulsations per second. But by progressively cooling the water, this rate may be progressively lowered, just as in the case of the marine species; and in water at 65° the maximum rate that I have observed is eighty pulsations per minute. As the temperature at which the greatest activity is displayed by the freshwater species is a temperature so high as to be fatal to all the marine species which I have observed, the effects of cooling are of course only parallel in the two cases when the effects of a series of high temperatures in the one case are compared with those of a series of lower temperatures in the other. Similarly, while a temperature of 70° is fatal to all the species of marine Medusæ which I have examined, it is only a temperature of 100 degrees that is fatal to the freshwater species. Lastly, while the marine species will endure any degree of cold without loss of life, such is not the case with the freshwater species. Marine Medusæ, after having been frozen solid, will, when gradually thawed out, again resume their swimming movements; but this freshwater Medusa is completely destroyed by freezing. Upon being thawed out, the animal is seen to have shrunk into a tiny ball, and it never again recovers either its life or its shape.

The animal seeks the sunlight. If one end of the tank is shaded, all the Medusæ congregate at the end which remains unshaded. Moreover, during the daytime they swim about at the surface of the water; but when the sun goes down they subside, and can no longer be seen. In all these habits they resemble many of the sea-water species. They are themselves non-luminous.

I have tried on about a dozen specimens the effect of excising the margin of the nectocalyx. In the case of all the specimens thus operated upon, the result was the same, and corresponded precisely with that which I have obtained in the case of marine species. That is to say, the operation produces immediate, total, and permanent paralysis of the nectocalyx, while the severed margin continues to pulsate for two or three days. The excitability of a nectocalyx thus mutilated persists for a day or two, and then gradually dies out—thus also resembling the case of the marine naked-eyed Medusæ. More particularly, this excitability resembles that of those marine species which sometimes respond to a single stimulation with two or three successive contractions.

A point of specially physiological interest may be here noticed. In its unmutilated state the freshwater Medusa exhibits the power of localizing with its manubrium a seat of stimulation situated in the bell. That is to say, when a part of the bell is nipped with the forceps, or otherwise irritated, the free end of the manubrium is moved over and applied to the part irritated. So far, the movement of localization is precisely similar to that which I have previously described as occurring in *Tiaropsis indicans* (*Phil. Trans.*, vol. clxvii.) But further than this, I find a curious difference. For while in *T. indicans* these movements of localization continue unimpaired after the margin of the bell has been removed, and will be ineffectually attempted even after the bell is almost entirely cut away from its connections with the manubrium; in the freshwater Medusa these movements of localization cease after the extreme margin of the bell has been removed. For some reason or another the integrity of the margin here seems to be necessary for exciting the manubrium to perform its movements of localization. It is clear that this reason must either be that the margin contains the nerve-centres which preside over these localizing movements of the manubrium, or much more probably, that it contains some peripheral nervous structures which are alone capable of transmitting to the manubrium a stimulus adequate to evoke the movements of localization. In its unmutilated state this Medusa is at intervals perpetually applying the extremity of its manubrium to one part or another of the margin of the bell, the part of the margin touched always bending in to meet the approaching extremity of the manubrium. In some cases it can be seen that the object of this co-ordinated movement is to allow the extremity of the manubrium—*i.e.*, the mouth of the animal—to pick off a small particle of food that has become entangled in the marginal tentacles. It is therefore not improbable that in *all* cases this is the object of such movements, although in most cases the particle which is caught by the tentacles is too small to be seen with the naked eye. As it is thus no doubt a matter of great importance in the economy of this Medusa that its marginal tentacles should be very sensitive to contact with minute particles, so that a very slight stimulus applied to them should start the co-ordinated movements of localization, it is not surprising that the tentacular rim should present nerve-endings so far sensitive that only by their excitation can the reflex mechanism be thrown into action. But if such is the explanation in this case, it is curious that in *Tiaropsis indicans* every part of the bell should be equally capable of yielding a stimulus to a precisely similar reflex action.

In pursuance of this point I tried the experiment of cutting off *portions* of the margin, and stimulating the bell *above the portions of the margin which I had removed.* I found that in this case the manubrium did not remain passive as it did when the *whole* margin of the bell was removed; but that it made ineffectual efforts to find the offending body, and in doing so always touched some part of the margin which was still unmutilated. I can only explain this fact by supposing that the stimulus supplied to the mutilated part is spread over the bell, and falsely referred by the manubrium to some part of the sensitive—*i.e.*, unmutilated—margin.

But to complete this account of the localising movements it is necessary to state one additional fact which, for the sake of clearness, I have hitherto omitted. If any one of the four radial tubes is irritated, the manubrium will correctly localise the seat of irritation, whether or not the margin of the bell has been previously removed. This greater case, so to speak, of localising stimuli in the course of the radial tubes rather than anywhere else in the umbrella except the margin, corresponds with what I found to be the case in *T. indicans*, and probably has a direct reference to the distribution of the principal nerve-tracts.

On the whole, therefore, contrasting this case of localisation with the closely parallel case presented by *T. indicans*, I should say that the two chiefly differ in the freshwater Medusa, even when unmutilated, not being able to localise so promptly or so certainly: and in the localisation being only performed with reference to the margin and radial tubes, instead of with reference to the whole excitable surface of the animal.

All marine Medusæ are very intolerant of fresh water, and therefore as the fresh water species most presumably have had marine ancestors,[1] it seemed an interesting question to determine how far this species would prove tolerant of sea water. For the sake of comparison I shall first briefly describe the effects of fresh water upon the marine species.[2] If a naked-eyed Medusa which is swimming actively in sea water is suddenly transferred to fresh water, it will instantaneously collapse, become motionless, and sink to the

[1] Looking to the enormous number of marine species of Medusæ, it is much more probable that the freshwater species were derived from them, than that they were derived from freshwater ancestry.

[2] For full account, see *Phil. Trans.*, vol. clxvii., pp. 744-745.

bottom of the containing vessel. There it will remain motionless until it dies; but if it be again transferred to sea water it will recover, provided that its exposure to the fresh water has not been of too long duration. I have never known a naked-eyed Medusa survive an exposure of fifteen minutes: but they may survive an exposure of ten, and generally survive an exposure of five. But although they thus continue to live for an indefinite time, their vigor is conspicuously and permanently impaired. While in the fresh water irritability persists for a short time after spontaneity has ceased, and the manubrium and tentacles are strongly retracted.

Turning now to the case of the freshwater species, when first it is dropped into sea water at 85° there is no change in its movements for about fifteen seconds, although the tentacles may be retracted. But then, or a few seconds later, there generally occurs a series of two or three tonic spasms separated from one another by an interval of a few seconds. During the next half minute the ordinary contractions become progressively weaker, until they fade away into mere twitching convulsions, which affect different parts of the bell irregularly. After about a minute from the time of the first immersion all movement ceases, the bell remaining passive in partial systole. There is now no vestige of irritability. If transferred to fresh water after five minutes exposure, there immediately supervenes a strong and persistent tonic spasm, resembling rigor mortis, and the animal remains motionless for about twenty minutes. Slight twitching contractions then begin to display themselves, which, however, do not affect the whole bell, but occur partially. The tonic spasm continues progressively to increase in severity, and gives the outline of the margin a very irregular form; the twitching contractions become weaker and less frequent, till at last they altogether die away. Irritability, however, still continues for a time—a nip with the forceps being followed by a bout of rhythmical contractions. Death occurs in several hours in strong and irregular systole.

If the exposure to sea water has only lasted two minutes, a similar series of phenomena are presented, except that the spontaneous twitching movements supervene in much less time than twenty minutes. But an exposure of even one minute may determine a fatal result a few hours after the Medusa has been restored to fresh water.

Contact with sea water causes an opalescence and essential disintegration of the tissues, which precisely resemble the effects of fresh water upon the marine Medusæ. When immersed in sea water this Medusa floats upon the surface, owing to its smaller specific gravity.

In diluted sea water (50 per cent) the preliminary tonic spasms do not occur, but all the other phases are the same, though extended through a longer period. In sea water still more diluted (1 in 4 or 6) there is a gradual loss of spontaneity, till all movement ceases, shortly after which irritability also disappears; manubrium and tentacles expanded. After an hour's continued exposure intense rigor mortis slowly and progressively develops itself, so that at last the bell has shrivelled almost to nothing. An exposure of a few minutes to this strength places the animal past recovery when restored to fresh water. In still weaker mixtures (1 in 8, or 1 in ten) spontaneity persists for a long time; but the animal gradually becomes less and less energetic, till at last it will only move in a bout of feeble pulsations when irritated. In still weaker solutions (1 in 12, or 1 in 15) spontaneity continues for hours, and in solutions of from 1 in 15 to 1 in 18 the Medusa will swim about for days.

It will be seen from this account that the freshwater Medusa is even more intolerant of sea water than are the marine species of freshwater. Moreover the freshwater Medusa is beyond all comparison more intolerant of sea water than are the marine species of brine. For I have previously found that the marine species will survive many hours' immersion in a saturated solution of salt. While in such a solution they are motionless, with manubrium and tentacles relaxed, so resembling the freshwater Medusa shortly after being immersed in a mixture of 1 part sea water to 5 of fresh; but there is the great difference that while this small amount of salt is very quickly fatal to the fresh-water species, the large addition of salt exerts no permanently deleterious influence on the marine species.

We have thus altogether a curious set of cross relations. It would appear that a much less profound physiological change would be required to transmute a sea-water jelly-fish into a jelly-fish adapted to inhabit brine, than would be required to enable it to inhabit fresh water. Yet the latter is the direction in which the modification has taken place, and taken place so completely that sea water is now more poisonous to the modified species than is fresh water to the unmodified. There can be no doubt that the modification was gradual—probably brought about by the ancestors of the freshwater Medusa penetrating higher and higher through the brackish waters of estuaries into the fresh water of rivers—and it would I think be hard to point to a more remarkable case of profound physiological modification in adaptation to changed conditions of life. If an animal so exceedingly intolerant of fresh water as is a marine jelly-fish may yet have all its tissues changed so as to adapt them to thrive in fresh water, and even die after an exposure of one minute to their ancestral element, assuredly we can see no reason why any animal in earth or sea or anywhere else may not in time become fitted to change its element.

GEORGE J. ROMANES.

A NEW GENUS OF RHINOCERONTIDÆ.

While the genus *Aphelops* must be regarded as the direct ancestor of the recent rhinoceroses with canine and incisor teeth, now confined to Asia and the Islands, the ancestral genus of the African forms and their extinct congeners, which are without the teeth named, is less known. It can now be shown that the missing genus inhabited North America, and that, like *Aphelops*, it is hornless. It may be named and characterized as follows: *Peraceras*, Cope; superior dentition; I. o; C. o; P-m. 4; M. 3; nasal bones weak, hornless.

This genus is established on a new basis recently discovered by Mr. R. H. Hazard, in the Loup Fork formation of Nebraska, which may be called *Peraceras superciliosus*. It is founded on a nearly perfect skull, which lacks the lower jaw. Its size is about that of the Indian rhinoceros. It is narrowed anteriorly, but is very wide between the orbits. Posterior to these it contracts rapidly, and rises to a rather elevated occiput. Saggital crest narrow; a prominent angle above each orbit. The premaxillary bone is narrow and weak. The nasal notch extends to above the middle of the third superior premolar. The occiput is rectangular in outline, with truncate summit. Its surface above is concave, divided by a strong median crest; lower down a vertical groove intersects its lateral border. The crests of the molar teeth are rather simple, and the posterior notch is soon isolated on attrition. Wear also isolates an external median fossa of the second premolar. Length of skull from end of premaxillary bone to condyles, M. 700; length of alveolar border of premaxillary, .025; length of molar series, .315; length of three true molars, .160; width of crown of second true molar at base, .075; superciliary width, .255.

This species is nearest to the *Peraceras malacorhinus*, a species which I formerly referred to *Aphelops*, but which I have little doubt belongs to the present genus. It differs from *P. superciliosus* as follows: In the latter species the front is wider, and is plane or concave, not convex; the superior edge of the maxillary is not wide and incurved, and has not the oblique ridges; the infraorbital foramen consequently has a more lateral opening. The nareal notch does not extend so far posteriorly by the one and a half molar teeth. The occiput is wider, is divided by a median crest not found in *P. malacorhinus*, and has the vertical lateral grooves much shorter. The acute supraorbital angle is not seen in the *P. malacorhinus*.

The rhinoceroses of the Loup Fork formation whose generic position can now be ascertained, are the following: *Paraceras malacorhinus; P. superciliosus; Aphelops meridianus; A. Negalodus; A. fossiger.—Am. Naturalist.*

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ANALYSIS OF WATER.

The trouble attending the making of analyses of water is considerable, and may account for the reluctance of chemists to make such investigations unless under special orders.

As the value of such analysis of the water supply of cities is great, especially at a time when the subject is receiving so much attention, we gladly welcome some valuable work accomplished in this direction at Newark, New Jersey, by Dr. Herman H. C. Herold, of that city, and placed at the disposal of "Science."

As Dr. Herold has made his calculations both for grains in the imperial gallon, and also according to the metric system, we reproduce both in tabular form. To this record is added the analysis of water from a New York well, also made by Dr. Herold, and described by him at page 13 of this Journal, forming part of No. 2, issued July 10th last.

The inference Dr. Herold draws after making these analyses, is that relatively the water from the Passaic river stands at the head of the list, as being the most favorable as a water supply for Newark, in comparison with that obtained from driven wells.

Still the Aqueduct Water (Passaic) is not in a satisfactory condition, its imperfections being due to impurities derived from the city of Newark itself, and not from Paterson and other towns above it, as the run of twelve miles would oxidize such organic matter.

The results of an analysis of the aqueduct water of Newark City, made by Dr. Herold in the month of June, as compared with the results of the analysis made in March, shows a decided deterioration in the condition of the water during the time between the two periods. To a very great extent this may be explained as being a result of natural causes. During the interval we suffered from a prolonged drought, the lowlands being thoroughly drained and converted into pools, the flood-tide, flowing farther up the river than is usual, carried with it much of the impurities of the city which are emptied into the river. The distance being short, these impurities did not have adequate opportunity for oxygenation and destruction. As will be seen by a comparison of the following tables, the amount of solid residue, 6.688 grains per imperial gallon, is about double that obtained by the former analysis, which showed only 3.147 grains per imperial gallon. The amount of organic matter has increased $2\frac{1}{2}$ times, or 0.957 grains against 0.378 grains of the former analysis. A still more alarming increase is found in the chlorine, 0.636 grains to 0.211 grains, found in the former analysis—an increase of 300 per cent.

Dr. Herold also states that his views regarding the advantages of securing a water supply for cities from running streams is strengthened by further examination of the question and everyday experience. Whatever organic matter may find its way into a running river is necessarily largely diluted. In the constant change of position and great increase of surface it is exposed to the oxygen of the air and also to that in the water; the plants along the bottom and sides of the stream are sure to absorb a certain proportion and by these means, if the water is only given far enough to flow, the matter contained in it cannot but be neutralized and to a very great extent destroyed. The great advantage to cities in being supplied from such a source is now generally conceded by all authorities who have made the subject of hygiene a study.

Constituents.	1880. Grains in an Imp. Gal.						
	Newark City Aqueduct.		Public Well, cor. Orange and Broad Streets, Newark.	Public Well, cor. Ferry and Congress Streets, Newark.	Balbach's Artesian Well, Newark.	Newark Driven Well.	New York Driven Well.
	March.	June.	March.	March.	March.	June.	June.
Total solid constituents	3.147	6.688	22.277	36.929	125 833	49.782	31.434
After incineration	2.377	......	17.673	21.312	92.849	......	......
Lime (CaO)	1.357	1.225	7.150	6.842	40.953	6.940	4.396
Sulphate of lime (Cao, SO_3, 2 HO)	None.	......	6.016	6.016	120.346	6.052	8.665
Carbonate of lime (CaO, CO_2)	2.220	......	9.270	8.717	3.155	7.940	......
Bicarbonate of lime (CaO, 2 Co_2)	......	......	......	......	......	......	3.304
Magnesia, (MgO)	Trace.	0.452	Trace.	Trace.	Trace.	2.468	3.162
Chloride of Magnesium (Mg Cl_2)	......	......	......	......	......	......	1.815
Carbonate of Magnesia	......	......	......	......	......	......	5.824
(5 MgO, 4 Co_2, 6HO)	......	......	......	......	......	5.981	......
Chlorim (Cl.)	0.211	0 636	3.392	5.939	0 845	8.486	6.374
Chloride of Sodium (Na Cl)	0.379	......	5.596	9 794	1 398	13.901	8.215
Soda (Na_2O)	......	0.318	......	......	......	12.187	4.315
Sulphuric Acid (SO_3)	None.	1.245	2.798	2.798	55.974	9.865	5.096
Sulphate of Sodium (Na_2O, SO_3)	......	......	......	......	......	11.188	......
Iron (Fe O)	Trace.	......	Trace.	Trace.	Trace.	......	......
Silica (Si O_2)	Not estimated	1.293	Not estimated.	Not estimated.	Not estimated.	4.701	1.736
Carbonic Acid (Co_2)	......	0.705	......	......	......	7.199	4.396
Organic and Volatile matter	0.378	0.957	1.395	12.408	None.	7.374	6.446

Constituents.	1880. Grammes in a Litre.						
	Newark City Aqueduct.		Public Well cor. Orange and Broad sts., Newark.	Public Well cor. Ferry and Congress sts., Newark.	Balbach's Artesian Well, Newark.	Newark Driven Well.	New York Driven Well.
	March.	June.	March.	March.	March.	June.	June.
Total solid constituents	0.0450	0.0956	0.3184	0.5278	1.7884	0.7115	0.4485
After incineration	0.0340	......	0.2526	0.3046	1.3270	......	......
Lime (CaO)	0.0194	0.0176	0.1022	0.9780	0.5853	0 0992	0.0692
Sulphate of lime (CaO, So_3, 2HO)	None.	0.0298	0.0860	0.0860	1.7200	0.0865	0.1236
Carbonate of lime (CaO, CO_2)	0.0346	0.0096	0.1325	0 1246	0.0451	0.1135	......
Bicarbonate of lime (CaO, $2CO_2$)	......	......	......	......	......	......	0.0471
Magnesia (MgO)	Trace.	0.0064	Trace.	Trace.	Trace.	0.0353	0.0451
Chloride of Magnesium ($MgCl_2$)	......	0.0050	......	......	......	......	0.0259
Carbonate of Magnesia	......	0.0105	......	......	......	0.0855	0.0830
5 MgO, $4CO_2$, 6 HO)	......	......	......	......	......	......	......
Chlorine (Cl)	0.0030	0.0090	0.0485	0.0849	0.0121	0.1213	0.0909
Chloride of Sodium (NaCl)	0 0050	0.0086	0.0800	0.1400	0.0200	0.1987	0.1172
Soda (Na_2O)	......	0.0035	......	......	......	0.1742	0.0616
Sulphuric Acid (SO_3)	None.	0.0175	0.0400	0.0400	0.8000	0.1410	0.0727
Sulphate of Sodium (Na_2O, SO_3)	......	......	......	......	......	0.1599	......
Iron (FeO)	Trace.	......	Trace.	Trace.	Trace.	......	......
Silica (SiO_2)	Not estimated.	0.0185	Not estimated.	Not estimated.	Not estimated.	0.0672	0.0248
Carbonic Acid (CO_2)	......	0.0101	......	......	......	0.1029	0.0627
Organic and Volatile matter	0.0054	0.0136	0.0104	0.1778	None.	0.1054	0.0920

We have been shown a number of photographs copyrighted by Mr. Geo. Cumming, of this city, entitled, 'Studies of the Color Glow, or Rectilinear Spectrum." The original colored line drawings which were exhibited in 1879, at the Academy of Design and American Institute, consist of geometrical forms drawn in straight lines, in many hues of color, forming central globes with bright scintillating effects. While the photographs give but the form on a much reduced scale without, of course, the chief beauty—the blended color-lines—they are curious as illustrating the depth of tone obtained by the camera from any given shade or tint—light green for instance coming out deep black and violet being almost lost in the process. The originator has more of an artistic than a practical feeling in their conception, and calls his various designs *spring*, *sunset*, *autumn*, *sunrise*, etc; his idea being to embody a theory of color with pleasing effect, rather than to stamp himself as either an artist or designer.

We desire to direct special attention to the meeting of the American Society of Microscopists at Detroit, on the 17th of this month, presided over by Professor H. L. Smith, of Hobart College, Geneva, N. Y.

The conception of such a national meeting of microscopists is most excellent, and under such able leadership the results of the meeting cannot fail to promote the extension of microscopical research, and its elevation to the high position it should occupy, as one of the greatest aids to our possession of scientific knowledge, the comprehension of the workings of Nature and "of things around us."

NEW SOURCES OF FOOD.

BY W. N. LOCKINGTON.

Advance in civilization is marked by an advance in the choice of food. In the words of Spencer, "There is an analogy between progress in bodily nutrition and progress in mental nutrition. The higher types of mind, like the higher types of body, have greater powers of selecting materials fit for assimilation."

As there is room for much further advance in mental nutrition, so is there for much advance in bodily nutrition. The choice of food has hitherto been determined empirically. Prejudice is the usual guide. A few experiments with foods, and finding some hitherto unused or little known article to be exceedingly nutritious, or to supply a want, they recommend its adoption, but either their recommendation is unheeded, or the new article wins its way into favor with exceeding slowness.

The multitudinous forms of animal and vegetable life could furnish us with many an article of food equal or superior to those in use. We have not yet been through the full range of nature in our search for food. Yet our wide-spreading commerce has made us familiar with many foods that were formerly unknown, so that, prejudiced though we are, our range of food is wide compared with that of our ancestors, or that of a savage, but almost all the plants we grow for food purposes, as well as almost all the animals we eat, are, if not those used by our own ancestors, those which have been used for ages by other peoples with whom we have come in contact. It is the same with animals and vegetables used in the arts. We have adopted them from others—few indeed have had their merits discovered and utilized.

The seed of certain grasses and certain leguminous plants have for thousands of years been the chief sources of nutriment procured by man from the vegetable world, and they fulfill his purpose well; but the two immense orders of *Leguminosæ* and *Gramineæ*, the latter entirely, the former chiefly composed of plants that are adapted for food, could furnish many additional species that would not only vary our dietary, but give us a supply or food under conditions that preclude the growth of species now in use. The number of fruits cultivated might be greatly increased. Almost every section of country furnishes some nut or berry which even in its wild state is pleasant to the taste.

What might not cultivation do for some of these. It has given us all the varieties of plum and cherry, apple and pear, from sour and unpromising originals, and the long category of vines from one European species. Many edible roots, stems and leaves have yet to be discovered or improved into value.

A few species of the order *Cruciferæ* are eaten, while the rest are neglected. Yet the whole order is good for food. A botanist could multiply examples throughout the range of vegetable life, but it will suffice here to give one more; the mushroom or fungus tribe, so little known, so much dreaded, yet containing so many edible species. Again and again it has been shown that the same amount of observation which enables a man to distinguish night-shade from the potato, or carrots from hemlock, would enable him to discriminate between the poisonous and edible mushrooms, yet only an enthusiastic band ever dares to venture beyond the conventional species. The species favored by the Anglo-Saxon is in ill-favor with the Italian, who has a wider range of edible fungi, as have also the Frenchman and the Russian.

As mushrooms can be grown in places where ordinary plants will not flourish, an increased taste for and knowledge of them would be of great benefit to our poor. If from the vegetable world we turn to the animal, we find prejudice and ignorance still more rampant. The Mosaic law is still obeyed in this matter by nations who break it in most others.

The ordinance which restricted the Israelites to the use, for food purposes, of such quadrupeds only as chew the cud and divide the hoof, was in the then state of knowledge a wise and safe one.

All such animals are herbivorous, and are better fitted for food than carnivorous mammals. They are of large size, furnish an abundance of healthy muscle, and have in many instances been domesticated for ages. But numerous other large animals are herbivorous also, and extensive series of small animals are graminivorous or frugivorous, devourers of seeds or fruits. Why should not these be eaten? The omnivorous pig, whose diet, at least in a state of domestication, is not particularly choice, and whose flesh is less nutritious and less wholesome than that of most other mammals, is largely eaten by man, yet the prejudice against horse flesh is almost universal among Aryans.

We occasionally eat a hare or rabbit, but the rest of the rodentia, mostly seed or root eaters, are neglected. The ground squirrels, a plague on the Pacific slope of the United States, would cease to be so were man to make a systematic onslaught upon them to gratify his taste. Their flavor is pronounced excellent by all who have tried them. The taste for this or that particular article of food is to a great extent acquired.

Many who ultimately become fond of oysters dislike them at first. The same remark holds true of many other foods in common use. The muscles of all birds and mammals are suitable for food when in a perfectly healthy condition. More care is necessary in the case of carnivorous mammals, since their flesh decays more rapidly; yet it is doubtful whether one person in ten could distinguish cat from rabbit were they cooked alike and the more tell-tale portions removed.

The strong or fishy flavor of marine mammals and birds would doubtless be objebted to by those whose gustatory nerves had learned to relish high game and Limburger cheese, yet as safe sources of nutriment they would at least be superior to the former.

Civilized nations of Aryan descent devour many mammals and birds, some batrachia and many fishes; but the intervening class of reptiles is almost wholly ignored. Why? Simply because of the pious horror of the snake. Lizards, as they have long tails, are viewed only a little less unfavorably, while tortoises—thanks to their widely different form—are accepted with some reservation; yet the flesh of snakes and lizards is as firm, as nutritious and as healthy as that of fishes, if not more so; and those who have eaten them when among peoples who do not share our prejudices, have had their own shaken. The Frenchman, who is a good cook, eats frogs; the Englishman cannot conquer his prejudice.

Leaving the vertebrata; the choice made by civilized nations among the invertebrata is highly eccentric.

A Spaniard or Frenchman relishes a cuttle fish, which an American or Englishman shudders at; and the harmless snail and slug, *per se* as good food as oysters, are esteemed by some nations and detested by others.

There is little doubt that the great majority of mollusks of sufficient size are healthy food, and that man has yet to discover among them many a *bonne bouche*.

Descending lower still, sea-urchins, sea-anemones and sea-cucumbers are eaten by some highly civilized nations, and who can tell how acceptable they might prove to an Anglo-Saxon could he but conquer the horror he feels at their appearance.

P. H. Gosse, so well known for his interesting works on natural history, tells us how he cooked the common sea-anemone of the English coasts (*Actinia mesembryanthemum*), and how fond his little one became of it, asking for "more tinnies."

Probably the classes of animals which are of least value as food to man are those included in the sub-kingdom Arthropoda, namely, insects, arachnids, myriapods and crustacea, the multitudinous types groaped together as *Vermes*, or worms, and the uni-celled organisms, or Protozoa. Some of the larger crustacea, known as crabs, lobsters, crayfish and shrimps, are eaten as delicacies, and it is probable that many other species are equally edible, but the vast majority of the class is only of value to man inasmuch as it furnishes food for larger marine animals.

Insects are eaten by many wild tribes. Some of the Indians of the Pacific coast find in the abundance of grasshoppers that plague the white man, an abundant store of food. Similar Orthoptera are largely consumed by the

natives of South Africa, and those of some of the Sunda Isles esteem dragon-flies a great delicacy, catching them, according to Wallace, on birdlimed twigs. Though there can be comparatively little nutriment in the soft bodies of insects, yet there is no reason for the horror with which they are regarded, as articles of food, by the Aryan races. A fancy for the flavor of the Rocky Mountain locust would go far toward decreasing the devastation of that dreaded pest.

Some of the marine worms are accounted delicacies by certain tribes, but the greater part of the varied forms belonging to the sub-kingdom must be regarded, so far as they are useful to man at all, as only indirectly so through their consumption by animals he feeds upon. The same may be said of the Protozoa, which, swarming in countless numbers in sea and river, lake and marsh, furnish food for the creatures above them.

Putting aside all question of protection or preservation of plants and animals now used as food, the examples adduced are sufficient to show that the range of foods might, with advantage, be greatly extended.

Much remains to be learned respecting the diseases and bodily states of cold-blooded animals and of the invertebrata before we can use them for food with the same confidence with which we eat beef, mutton or poultry. The diseases of the higher animals are, to a great extent, similar to our own, and we have learned how to discriminate; but we do not recognize the diseases of fishes, crabs and shellfish. The stories of poisonous fish probably arise from this source. Every year we hear of cases of poisoning, well authenticated, from eating mussels, lobsters or other crustacea, or mollusks, which are usually healthy food. All these creatures are subject to diseases which we have not yet studied, and it may be that at certain seasons, such as immediately after reproduction, some of them are unfit for food. This is one well-grounded cause of prejudice, but one which will be removed as our knowledge of the lower forms of life extends.

The animal and vegetable world furnish us with other things besides food. Materials of other descriptions furnish, by their manufacture, a means of procuring food to some, while the articles manufactured are of use to all. Commerce, which has made us familiar with foods previously unknown, has helped us still more in this direction, yet when we consider the great variety of vegetable and animal life, we cannot but believe that much more remains to be discovered, or, at least, utilized.

Other nations, many of them but semi-civilized, others barbarous, have, in these things, been our teachers. As maize and potatoes were known to the Indians before we learned to use them, so was *Phormiumtenax* to the Maoris, and cotton to the Hindoos and Chinese.

When it is remembered what vast industries depend upon the supply of fibrous plants, and, that a fibre with different qualities, as it could be applied to new uses, would start a new branch of trade; when we see how extensive are the manufactures carried on from gum-resins like caoutchouc, or gutta-percha, we must acknowledge that the discovery of a fiber or a resin with new uses would furnish a livelihood to many additional workers. Take paper for example. Until lately this article was made from linen rags, but as the supply of that material fell short of the demand, cotton waste, straw, the Yucca plant, and other vegetable materials came into use, and it is evident that it can be made from almost any fibrous substance reduced to a pulp. Few are the plants that cannot be utilized by man. If valueless for food or for building purposes, a fiber, a gum, an essential oil, a medicinal product, may be found in most.

The constantly-increasing stock of geographical and botanical knowledge brings new materials into the notice of scientists, and the constantly increasing needs of mankind brings them slowly into public notice. The secretion lately found in Arizona, upon the branches of *Larrea Mexicana*, and of another plant, may yet enable us to dispense with the imported lac from Asia. Chemically the two seem identical; practically the despised Indian, here again our teacher, has long ago proved its use in the mending and making of vessels for cooking purposes.

Here is a case of a new material furnished by the animal kingdom, for it appears almost certain that the secretion, like that of wax or honey, is elaborated by the insect from the juices of the plants it feeds upon. Insects, so little used for food, so terribly destructive to our food plants and annoying to our domestic animals, may yet yield to us many useful materials; may yet prove in this respect among the most useful of organisms. Silk, honey, wax, gum-lac, cochineal, all are insect products, elaborated by insects from plants; and the last two are the produce of *coccidæ*, those destroyers of our orchards and orangeries. Does not this point a way to the utilization, in some cases, of our insect pests?

The higher animals may not furnish us with many additional materials. Horn, hair, fur, wool, hides, feathers, bone, ivory, have their known uses. Improvement here is to be looked for rather in new uses for known materials than in the discovery of new ones. But the lower animals may yet yield us many useful substances. The great treasure house of the sea holds more than we have yet learned the use of. Shells, corals, the honey axes of soft-corals, and many other portions of marine animals, may be utilized for something more than show; and other secretions may be found as useful as those of the sepia. But though the number of useful species—useful either directly or indirectly—is so large that it includes probably the greater portion of existing organisms, yet some are far more useful than others, and some are directly injurious to more useful organisms. Such species need not be cultivated, except where they do not come into direct competition with more useful ones; but their consumption or use by man would diminish their numbers and give room for the more useful forms, which are now often permitted by man, even in his own cultivated fields, to be crowded out by the less useful. —*Scientific Press*, Cal.

Two eggs of the extinct great auk were sold by auction in Edinburgh recently, both being purchased by Lord Lilford, one at £100, the other at 102 guineas, probably the largest sum ever paid for a single egg, with the exception of that of the moa, a single specimen of which was sold at the same place in 1865, for £200.

Progress in Utilization of Solar Heat.—Since May, last year, M. Mouchot has been carrying on experiments near Algiers with his solar receivers. The smaller mirrors (0.80 m. diameter) have been used successfully for various operations in glass, not requiring more than 400° to 500°. Among these are the fusion and calcination of alum, preparations of benzoic acid, purification of linseed of oil, concentration of syrups, sublimation of sulphur, distillation of sulphuric acid, and carbonization of wood in closed vessels. The large solar receiver (with mirror of 3.80 m.) has been improved by addition of a sufficient vapor chamber and of an interior arrangement which keeps the liquid to be vaporized constantly in contact with the whole heating surface. This apparatus on November 18, last year, raised 35 litres of cold water to the boiling point in 80 minutes, and an hour and a half later showed a pressure of eight atmospheres. On December 24 M. Mouchot with it distilled directly 25 litres of wine in 80 minutes, producing four litres of brandy. Steam distillation was also successfully done, but perhaps the most interesting results are those relating to mechanical utilization of solar heat. Since March the receiver has been working a horizontal engine (without expansion or condensation) at a rate of 120 revolutions a minute, under a constant pressure of 3.5 atmospheres. The disposable work has been utilized in driving a pump which yields six litres a minute at 3.50 m. or 1,200 litres an hour at 1 m., and in throwing a water-jet 12 m. This result, which M. Mouchot says could be easily improved, is obtained in a constant manner from 8 A. M. to 4 P. M., neither strong winds nor passing clouds sensibly affecting it.

THE AMERICAN SOCIETY OF MICROSCOPISTS.

The third annual meeting of the American Society of Microscopists, the largest representative body of miscroscopists in America, will begin at Detroit, Mich., the 17th day of this month (August), and will continue four days.

The circular of the Society announces that the headquarters will be at the Detroit Female Seminary, No. 82 Fort Street West. Ample arrangements are being made by the (local) Griffith Club of Miscroscopy for the comfort and convenience of its guests. Free accommodations are to be furnished the members and delegates of the American Society at private residences, and the noted hospitality of the citizens of Detroit will undoubtedly be freely dispensed to the visiting Society.

The forthcoming meeting of the Society promises to be the most successful yet held. Several valuable papers will be presented, and new and original mechanism in the construction of stands will be shown and described. Also in the preparation of microscopic objects several valuable and much needed improvements will be presented.

The circular issued by the Society extends an invitation to microscopists who are not yet members to be present, also to join the Society, and participate in its business, both scientific and executive.

The last meeting of the American Society was held at Buffalo, N. Y., one year ago, and the results, both in the attendance and character of the papers read at that meeting, were highly encouraging to the lovers of miscroscopic work throughout the country. The influence exerted by these meetings has been productive of a great amount of good. Microscopic societies have been, and are, forming throughout the country. In Pennsylvania, New York, New Jersey, Michigan and other States good working societies have lately been formed, and a corresponding interest in scientific enquiry has been aroused. This cannot but be valuable to the communities in which these societies exist. This work must not be allowed to cease, and therefore we trust the National Society may have a long lease of life.

Not only in stirring up an interest in scientific work is the American Society valuable, but in original research it will yet make its name known, as even now among its members may be found many of the leading scientific workers with the microscope in this country. The officers of the Society, and of the Detroit meeting are as follows :

President,—Prof. Hamilton L. Smith, LL.D., of Geneva, N.Y.

Vice-Presidents, { Dr. W. Webster Butterfield, of Indianapolis, Ind., and Mr. C. C. Merriman, of Rochester, N. Y.

Secretary,—Prof. Albert H. Tuttle, of Columbus, Chio.

Treasurer,—George E. Fell, C. E., of Buffalo, N. Y.

Executive Committee, { Dr. W. B. Rezner, of Cleveland, Ohio, Dr. Carl Seiler, of Philadelphia, Pa., and Dr. W. C. Barrett, of Buffalo, N. Y.

THE TAY BRIDGE DISASTER.

The report of the Court of Inquiry appointed to investigate the circumstances of the fall of the Tay Bridge last December, which was fatal to so many hundred lives, has been made public, and the result is thus summarized and commented upon by *Nature :*

There appears to be some difference of opinion amongst the members of the court respecting the scope of the inquiry and the duties placed upon them by the Board of Trade, in consequence of which two separate reports appear together, one by Col. Yolland, Chief Government Inspector of Railways, and Mr. Barlow, President of the Institute of Civil Engineers, and the other by Mr. Rothery, the Wreck Commissioner. The former report describes in detail the design and method of erection adopted in the bridge, giving also a description of the various alterations in the plan which were rendered necessary as the work progressed.

The bridge was 3,465 yards in total length, divided into 86 spans, and it was the central portion, of 3,149 feet in length, which fell on the evening of December 28. As originally designed, this central position was to consist of lattice girders of 200 feet span, carried by brickwork piers somewhat over 80 feet in height from high-water level, but as the river bottom turned out to be different from what was expected from the borings, and the difficulty of obtaining a secure foundation greater, eleven spans of 245 feet and two of 227 feet were substituted, and braced iron piers were adopted in the place of brickwork, as imposing a less weight on the foundations. It is these piers which at the inquiry chiefly received attention, as there can be little doubt that they were the immediate cause of the catastrophe. The process of floating out and sinking the caissons for these piers has already been described in these columns, and so successfully was this—certainly the most difficult and hazardous part of the undertaking—accomplished, that no suggestion of insufficient strength has been made, and in the Report it is stated that there is nothing to indicate any movement or settlement in the foundations of the piers which fell.

The caissons were lined with brickwork and filled with concrete, on which was built a hexagonal pier of masonry carried up to 5 feet above high-water mark. Upon this pier was built up six cast-iron columns secured by holding-down bolts to the masonry at the angles of the hexagon. The columns were made up of lengths united by flanges and bolts, and connected with each other by horizontal struts and diagonal ties. The up-stream and down-stream columns were each 18 inches in diameter, the remaining four, 15 inches; all were inclined 12 inches inwards at the top. The piers thus formed were from 81 to 83 feet in height from the top of the masonry to the under-side of the girders. The diagonal bracing consisted of flat bars attached to the columns by means of "lugs" cast on them, being secured at one extremity by a screw-bolt passing through the lugs and bar, and at the other by a strap provided with a gib and cotter for tightening up. The horizontal struts consisted of two channel-bars bolted back to back to a single lug on each column.

It will thus be seen that all vertical load must be borne entirely by the columns, and with the exception of the small transverse resistance of the latter the whole of any lateral pressure must be transmitted by the bracing.

Whether as designed the bridge would have been strong enough for its work if the materials and workmanship had been good throughout is very doubtful, but, as carried out, the evidence shows distinctly that it was not sufficiently substantial for the heavy traffic and severe gales to which it was exposed. When everything was tight and in good order the bridge, at the time of its inspection by General Hutchinson in February, 1878, showed great rigidity under the tests imposed by him, but by October of the same year so much slackness had made its appearance in the bracing that, besides the ordinary keying-up by driving the cotters, more than 100 packing-pieces about three-eighths of an inch thick had to be introduced in different parts.

Respecting the immediate cause of the accident the Court states—"In our opinion the weight of evidence points out the cross bracing and its fastening by lugs as the first part to yield." This we believe the calculations of Dr. Pole and Mr. Stewart, taken in connection with the experiments of Mr. Kirkaldy, are quite sufficient to establish. With a wind pressure of 30 lbs. to the square foot on the windward girder and train, and half this amount on the leeward girder, the stress on the tie-bar most severely strained, would be 16·8 tons, or 10·18 tons per square inch; again, with a wind pressure of 40 lbs. to the square foot the stress on the tie-bar would be 22·4 tons. Now, as Mr. Kirkaldy's experiments, made by order of the court on some of the tie-bars removed from the bridge, showed that they broke with a load of from 19 to 23 tons, and the corresponding lugs with a load of 23 to 25 tons, it is pretty certain that the ultimate strength of this part of the structure would be reached by a wind pressure of 40 lbs. to the square foot.

And, in addition to this, more variation is to be expected in the strength of the lugs, as some at least were admitted to be of bad manufacture, and when the pier was most severely strained it would be some of the worst lugs in the lower tiers that would be the first to yield; thus the samples taken for testing would not be likely to embrace specimens of the lowest strength, as these would probably have already given way.

Again, it does not appear necessary to assume a wind pressure of 40 lbs. per square foot to ensure the destruction of the pier; the stresses above mentioned are due merely to the statical pressure, and it can hardly be denied in the face of the evidence respecting the details of the structure that there would be a great deal of motion due to backlash over and above the elastic yielding of the material. Thus a much lower pressure would produce the effects calculated for one of 40 lbs. per square foot.

The principal conclusions arrived at by the court are that there is no indication of settlement in the foundations, that the wrought iron employed was of fair strength, though not of high quality as regards toughness, that the cast iron was fairly good, that the main girders were of sufficient strength, and that the iron piers, though strong enough to sustain the vertical load, were insufficient to resist the lateral action of heavy gales from the weakness of the cross bracing and its fastenings; that the railway company did not enforce the recommendation of General Hutchinson by limiting the speed of trains over the bridge to twenty-five miles per hour, much higher speed being frequently run; that while of opinion that the fall of the bridge was occasioned by the yielding of the cross bracing and fastenings, it might possibly have been due to the fracture of one of the outward leeward columns.

Colonel Yollard and Mr. Barlow conclude by stating "that there is no requirement issued by the Board of Trade respecting wind pressure, and there does not appear to be any understood rule in the engineering profession regarding wind pressure in railway structures; and we therefore recommend that the Board of Trade should take such steps as may be necessary for the establishment of rules for that purpose."

Mr. Rothery, in his independent report, while stating that there is an entire agreement between himself and his colleagues in the conclusions arrived at from the evidence, goes further than they, and unhesitatingly apportions the blame among the different parties concerned. On the recommendation that the Board of Trade should establish rules providing for wind pressure, he differs from his colleagues, emphatically stating that it is for the engineering profession to make them, and evidently regards the superficial character of an official inspection as no great evil.

Where French engineers have long adopted 270 kilogrammes per square metre, and many English engineers, on the authority of Rankine, the equivalent 55 lbs. per square foot, while nearly the same figure is used in America, it seems strange that so much difference of opinion should be found to exist; but one thing at least is certain, that the instruments at present in use for measuring wind pressure are exceedingly crude and liable to error, and that until these are improved and much increased in number there is little chance of being on the spot when these excessive pressures occur, or of truthfully recording them when met with.

Respecting the transfer of these responsibilities to a Government Department, we believe that such apronstring policy would be fatal to the profession of the civil engineer; we would rather see the Board of Trade Inspection, which at least is formal and superficial, relaxed than any attempt made to increase its efficiency. The medical profession does not require a fatherly department to watch over its operations or give an opinion on an amputation; why then should the engineering profession? It cannot be too clearly understood that an engineering work cannot be successfully carried out by mere rule of thumb or even by the copious use of "Molesworth" or "Rankine"; each operation is to some extent a physical experiment, subject to known laws, but under variable conditions. The physicist and the engineer have already to a great extent established the laws for themselves, but it remains for the scientific engineer to carefully watch their operation, and thus gain that practical experience which will enable him to deal with each special case as it arises.

The conclusions we draw from the evidence and report are that the design of the piers was most imperfect, cheapness appearing to be the ruling element in every detail, a cheapness too that must have been completely delusive, as any money saved in first cost would soon, in such a rickety structure, have been swallowed up in maintenance. At nearly all points an absence of consideration for small details is most apparent, indicating probably that these were intrusted to some subordinate, who failed to appreciate their importance.

It is very far from our object in this article to hold up any particular individuals to blame for this disaster, but we should like to point out on whom the responsibility should rest if such a thing should occur again.

It would be quite impracticable for the Board of Trade to exercise such supervision over the selection of the material and the execution and erection of a large work throughout its progress, as would render its certificate of any value; we believe, therefore, that the undivided responsibility should rest on the engineer. Any dishonesty on the part of the contractor or his workmen—and we are sorry to believe this still exists in some cases—could be easily rendered hazardous by legal penalties.

Doubtless with the keen competition of the present day things must be "cut finer" than they used to be: but while we would remove any arbitrary restrictions imposed by Government on the judgment of those who ought to be best able to appreciate the particular conditions of their own work, we should be very sorry to see the introduction of flimsy structures or reckless traffic arrangements without it being understood on whom the responsibility rested in case of failure.

A letter recently sent to Professor Plantamour, director of the Geneva Observatory, gives the details of a singular phenomenon observed at Bonneville on the 25th of April. It was noticed during a rain storm, that the drops of water falling upon dark clothes, linen, umbrellas, left a dirty yellow spot verging on brown. The matter was given over to M. de Candolle, for investigation, who found that the powder which colored the yellow rain, contained only organic elements of vegetable origin. Observed dry, or in pure water, these *débris* had mostly a yellowish color, but some were colorless. They were generally formed of cells of small diameter, upon the walls of which were granulations consisting of the finest particles of the pulverulent matter of the rain drops. The advanced state of disaggregation of all these vegetable *débris*, did not allow of the determination of their origin; but the minuteness of the cells seemed to indicate that they belonged to young tissues. Amongst the fragments, with form so varied and irregular, were found some spores of cryptogams, but no grains of pollen were met with.

M. Dines has calculated that the amount of dew deposited on the ground in the course of a year would be represented by a layer of water about 40 millimetres (1.6 in.) in height, equivalent to 40 litres per square metre.

The Royal Society of New South Wales now numbers 430 members, exclusive of honorary and corresponding members. Mr. G. Bentham, Dr. Darwin, Prof. Huxley, Prof. Owen and Sir. J. D. Hooker have been elected honorary members, and Mr. R. Etheridge, jun., a corresponding member. The Clarke memorial medal for 1878 has been awarded to Prof. Owen, for 1879 to Mr. G. Bentham, and for 1880 to Prof. Huxley, for their contributions to palæontology, botany and natural history of Australia.

BOOKS RECEIVED.

THE CONSTRUCTION OF GAS-WORKS AND THE MANUFACTURE AND DISTRIBUTION OF COAL GAS. By William Richards, C. E. 6th edition Crosby, Lockwood and Co., 7, Stationer's Hall Court, London. 1880.

This is a new and enlarged edition of the work originally written by Samuel Hughes, C. E., but now enlarged, re-written by William Richards, C. E., who has brought his facts down to the most recent knowledge of the subject.

At a time when a revolution in our methods of illumination has become an established fact, and the death knell of gas has already been sounded, it is very convenient to find a hand-book full of the most valuable details respecting that method of illumination, written by one who is evidently a master of the subject. We find a great number of good engravings and many valuable tables. In view of the recent serious explosion of a gas main in London, England, the chapter on "Gas explosions" will be read with inrerest.

ELECTRIC LIGHT: ITS PRODUCTION AND USE. By J. W. Urquhart, C. E. Edited by F. C. Webb, M. I. C. E., M. S. T. E. Crosby, Lockwood & Co., London. 1880.

The object of the author is to present for general reading an account of the various methods of obtaining the electric light, both from voltaic and galvanic batteries, and it also treats of the various forms of dynamo-electric machines.

This work will serve the very useful purpose of placing within reach of those who are not acquainted with the history and growth of electric lighting, a clearly written description, well illustrated with wood engravings. It is not a text book, and the author makes no pretentions to teach electricians the art of electric lighting, but it is, in fact, a popular guide to the subject.

The rapid development in the various forms of electric lighting led to a re-arrangement of the book, even while it was being written; it is not therefore surprising to find that on certain points the work is already obsolete. The chapter on the Edison electric lamp was written twelve months since, and is devoted chiefly to his experiments with incandescent platinum, which has been long since abandoned, while the author merely speaks of the carbon lamps to prophesy their failure. Reading this, at this date, when every difficulty in the way of their practical use has been removed, and arrangements are in progress to produce them in sufficient numbers to permit their general adoption, we cannot but regret that prejudice has been allowed to derange the better judgment of those who have assumed to lead public opinion on this subject.

We do not propose to examine too critically what is admitted to be a popular work, and while we are not in accord with much we find in the book, we have no doubt that the numerous wood-cuts of electric generators and other apparatus will be most useful to those who desire to acquaint themselves with the various methods of electric lighting.

MANUAL OF THE ALKALI TRADE, INCLUDING THE MANUFACTURE OF SULPHURIC ACID, SULPHATE OF SODA AND BLEACHING POWDER.—By John Lomas. Crosby, Lockwood & Co. London. 1880.

This is a handsome volume of three hundred and fifty pages, containing two hundred and thirty-two illustrations and working drawings, and provides a complete hand-book for those intending to manufacturer Alkalis, or for those already in the field who desire to improve their plant, or become practically acquainted with the latest developments of the trade; it also may be useful for manufacturers to place in the hands of their managers and foremen, as a useful guide in their daily rounds of duty.

The author appears to have had fifteen years' of practical experience as an alkali manufacturer, during which time he states he has erected new plants, remodeled old works, and trained managers. We advise all engaged in the alkali manufacture, to procure this most practical work, as from the examination we have made of it, we feel sure a perusal will be the means of saving infinite time, patience and labor.

NOTES AND QUERIES.

[1.] In reply to the Query of J. H. G. in regard to Tuckahoo or Indian Bread, I regret not being able to give all the particulars asked for, but the following extract from the Treasury of Botany may be useful:

"Tuckahoo is the Americo-Indian name for a curious tuberous production, which is dug out of the ground in several parts of the United States, and which has been referred by Fries to the genus *Pachyma*. Like *Sclerotium*, however, *Pachyma* has no fruit, and there is some reason to doubt whether it has any pretensions to be classed with *Fungi* at all. It is composed entirely of pectic acid, and it is very probable that it is a peculiar condition of some root, though of what plant has not at present been ascertained. One similar production at least has been found in China, where it is supposed to possess medicinal virtues; and there is reason to believe that another exists there, attaining a diameter of several inches like the American Tuckahoo. As may be supposed from its chemical constitution, it affords a nutritive article of food, for which purpose it is dug up by the natives like the *Mylitta* or Native Bread of Tasmania, with which, however, it does not correspond in character. It is also employed occassionally as a material for making jelly, for which it is well adapted, the pectic acid of currants and other fruits being the principle which disposes their juice when boiled to form a jelly-like mass. The principal objection which is brought forward against the suppossed phaenogamous origin of the production is the absence of all trace of vascular or cellular structure like that of phaenogams, or of bark, except such as may be suppossed to arise from mere contact with the soil; but the dissimilarity of its structure and that of Fungi is quite as great, and the conversion of a fungus into pectic acid would be more surprising." J. R.

GENERAL NOTES.

CURIOUS ELECTRIC PHENOMENON.—At about 4.30 P. M. this day a severe thunder storm with a deluge of rain came up from the north-west, and lasted about an hour. At 5.30 my wife was standing at the window watching the receding storm, which still raged in the south, just over Leicester, when she observed, immediately after a double flash of lightning, what seemed like a falling star, or a fire-ball from a rocket, drop out of the black cloud about 25° above the horizon, and descend perpendicularly until lost behind a belt of trees. The same phenomenon was repeated at least a dozen times in fifteen minutes, the lightning flashes following each other very rapidly, and the thunder consisting of short and sharp reports. After nearly every flash a fire-ball descended. These balls appeared to be about one-fifth or one-sixth the diameter of the full moon, blunt and rounded at the bottom, drawn out into a tail above, and leaving a train of light behind them. Their color was mostly whitish, but one was distinctly pink, and the course of one was sharply zig-zagged. They fell at a rate certainly not greater than that of an ordinary shooting star. I have never witnessed a phenomenon of this kind myself, but my wife is a good observer, and I can vouch for the trustworthiness of her report. F. T. MOTT.

Bristal Hill, near Leicester, June 22, (Nature).

MM. LE BON and NOEL have extracted from tobacco smoke the following products, which they recently presented to the French academy in three flasks: 1, Prussic acid; 2, an alkaloid of agreeable odor, but as poisonous as nicotine; 3, aromatic principles still undetermined, but contributing, with the alkaloid mentioned, to give tobacco smoke its perfume. The alkaloid in question is thought to be identical with a compound, collidine, the existence of which has been observed in the distillation of various organic substances, but whose physiological and toxical proprieties have been overlooked.

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The list of Associate Editors is not yet complete, but so far it comprises the names of Professional Scientists who stand at the head of the various branches of Sciences with which they have identified themselves.

The Editor will be glad to hear from those who will take an interest in the success of this Journal, or who desire to co-operate in any way; and suggestions will always be welcomed and receive due attention. The intention of publishing "SCIENCE" has been known only to a few, but the following well known and esteemed Scientists have expressed either their intention to contribute, or their approval of the object of the Journal, and good wishes for its success.

Professor SPENCER F. BAIRD, and Dr. ELLIOTT COUES, Smithsonian Institution, Washington, D. C.; Professor EDWARD S. HOLDEN, Naval Observatory, Washington; Professor O. C. MARSH, Yale College; Professor BURT G. WILDER, Cornell University; Professor C. A. YOUNG, Princeton College; Professor S. W. BURNHAM; Chicago; Professor O. STONE, Observatory, Cincinnati; Dr. J. S. BILLINGS, National Board of Health, Washington; Professor WILLIAM A. HAMMOND, M. D., New York; Dr. EDWARD C. SPITZKA, New York; Prof. CLEVELAND ABBE, Army Signal Office, Washington; Professor EDWARD S. MORSE, Peabody Academy of Science, Salem, Mass.; Professor ALEX. A. JULIEN, Columbia College, New York; Dr. J. J. WOODWARD, Surgeon General's Office, Washington; Professor H. P. BOWDITCH, Cambridge, Mass.; Professor H. L. SMITH, Hobart College, Geneva, N. Y.; &c., &c., &c.

All Communications should be addressed to THE EDITOR, Box 3838, P. O. New York.

SATURDAY, AUGUST 14, 1880.

CONTRIBUTIONS TO ENCEPHALIC ANATOMY.

BY E. C. SPITZKA, M.D.

Having, through a piece of good fortune, come into the possession of a living iguana, and thence obtained the brain and cord in a perfectly fresh condition, I was enabled to make a study for the first time of the remarkable brain of this saurian.

As regards the exterior of the encephalon, it presents nothing very different from that of any other higher reptile. On a lateral view, however, it exhibits a much acuter basilar incurvation, approximating to the bird's brain in this respect. As in birds, also, the optic nerves leave the skull directly on emerging from the chiasm. It is remarked also that the optic lobes are far larger than in any reptile or bird thus far examined by anatomists; in fact, excluding the case of the finny tribes, it may be said that the iguana possesses the largest optic lobes in the animal kingdom. They are as massive in their grey and white tissues, and nearly as voluminous as the cerebral hemispheres.

The olfactory lobes and bulbs offer nothing special for consideration.

On a transverse section through the cerebral hemispheres, I am able to identify the component parts of the cornu ammonis of the mammalia. It appears that the medial thin wall of the cerebral vesicle corresponds, with its layer of closely packed pyramidal nerve cells, to the *stratum corporum nervorum arctorum* of Kuppfer, and it is indeed separated from the cortical layer of the convexity, which I believe to correspond, as far as the thin part extends, to the *sigma* of the cornu. At the lower end of the thin-walled vesicle, where a transition of nerve fibres from the *stratum corporum nervorum arctorum* (?) takes place, to the thalamus, and which therefore corresponds to the fornix, there is an accumulation of molecular nerve substance, projecting outwards into the ventricular cavity. This may represent one of the thalamic tubercles; I regard it as much more probable, however, that it corresponds to the body of the so-called fascia dentata.

Now, in sections exhibiting the above features, I find also another which is highly important, in so far as it tends to overthrow another one of the *dicta* on whose strength the sauropsidean and mammalian brains are distinguished. Immediately underneath the median longitudinal fissure, but *over* the third ventricle, there passes a fasciculus of white fibres, uniting the two hemispheres, and particularly that portion of each which corresponds to the cornu ammonis. This is unquestionably the corpus callosum, whose first appearance in the embryo and the lower mammalia we know to be intimately associated with the development of the cornu ammonis.

But it is when we reach the mesencephalon and the region posterior to it, that we discover the most remarkable features of this brain.

As in some other saurians, the cerebellum instead of being curved backward, and constituting a cap over a part of the lateral ventricle, as in the alligator and chelonia, is bent forward, and bound to the posterior face of the optic lobes by the arachnoid filaments. On separating and drawing it backward, thus making it correspond artificially in position with the cerebellum of the alligator, we find that between the optic lobes and the cerebellum there are two pairs of tubercles.

One of these pairs, which I have found as a concealed mass in turtles, and as a very distinct elevation in the alligator, ophidia and pseudopus, I was familiar with, and I had no hesitation in describing it as the post-optic ganglia corresponding to the posterior pair of the corpora quadrigemina. The other was at first new to me, but after a careful comparative study I found that it was nothing but an unusually large, and therefore more prominent representative of a ganglionic mass which I have noticed in fair development in the turtle, and which is even represented in an atrophic condition with the mammalia. As the pair of tubercles in the iguana lies intermediate to the optic and post-optic lobes, I propose for it the name of inter-optic lobes.

On a dorsal view these different parts lie about as follows: In front are the massive optic lobes touching each other broadly on the middle line, so that their posterior margins form a continuous semi-lunar curve, convex behind. Behind each optic lobe, and bulging out somewhat, laterally, we have the smaller but distinct post-optic lobes, which fail to come in contact in the median line, so that a shallow groove would separate them, if it were not filled out by another structure now to be described.

If we imagine the median furrow separating the optic lobes prolonged between the post-optic lobes, and crowd two little pea-shaped eminences on each side of this imaginary median line, so that the latter are bounded in front by the optic lobes, on the outside by the post-optic lobes, and behind by the cerebellum, we will have the precise situation of the inter-optic lobes. These eminences are not so remarkable for their absolute size (their surface extent being only half that of the optic lobes) as for the distinctness of their demarcation. I have obtained sections through their posterior third, in which these bodies are shown to be absolutely free.

Other sections further forward show that these ganglia crop out of a specialized division of the central tubular grey of the aqueduct, and that the visible eminences do not represent the true extent of the ganglia.

The trochlearis nerves arise behind the inter-optic lobes, and passing forwards and downwards, lie in the furrow between the optic and post-optic lobes, as in other reptiles. It is well known that in the mammalia they pass down behind the post-optic lobes. I look on this as an incidental and insignificant variation.

The remainder of the isthmus shows nothing especially noteworthy. The remarkable size of the oculo-motor nuclei, and the gigantic dimensions of their almost star-like multipolar nerve-cells, merits mention, as well as the fact that in this animal the nuclei of the third and fourth pairs constitute a common cell mass, unlike the relation in the mammalia, and that the third and fourth pairs arise almost in the same plane, the third from the ventral, the fourth from the dorsal extensions of the common nucleus.

I would call attention to the fact that the average dimensions of the cell nuclei of the auditory nerve nucleus equal those of the motor nuclei of the medulla and cord, and exceed some of them, and that the same statement applies to the cells as a whole. I make this statement in view of the recent communication of Dr. Mason before the American Neurological Association, though I do not claim to make it on the same basis of careful and extensive micrometric observations that his communication was based on, but on a general impression derived from repeated examinations which I think are sufficient to determine palpable differences.

The present preliminary report is taken from a communication made by me to the *Journal of Nervous Diseases* for last June, but I trust before long to submit to your readers a more exhaustive and illustrated record of this interesting and suggestive piece of cerebral anatomy.

DRY "MOUNTS" FOR THE MICROSCOPE.

BY PROFESSOR H. L. SMITH, HOBART COLLEGE, N. Y.

II.

In a former paper, SCIENCE No. 3, I made a few remarks upon this subject, and described the methods which I had found tolerably successful, viz.: the rings made of shellac and lampblack, and those punched out of gutta-percha tissue. The former appear to answer quite well, and the changes, if any, are very slight, yet I have, in a very few cases, observed a deterioration after the lapse of a year or so, probably from imperfect manipulation. Although I have not myself observed any great change in the gutta-percha mounts, I am not certain that they will stand prolonged use with immersion objectives without injury. I have mounted many specimens of delicate test objects for the Messrs. Spencer, and they are decidedly of the opinion that the shellac ring is the better for durability, and I am informed by Mr. Gundlach that the gutta-percha ring will not stand cedar oil. Mr. Phin has suggested that in time the gutta percha tissue will disintegrate. I have not yet noticed this, and do not think it will happen under the cover of a "mount" especially if protected by a ring of cement subsequently applied. If, however, such disintegration does, in time, happen to the tissue, this will be a great objection to its use. I have found that the "tissue" becomes so charged with electricity by handling, and also by the punching, that it interferes seriously with the latter operation, and thus makes it necessary to place strips of the "tissue" on thin moistened strips of paper, and to punch out both at the same time. The preparation of the shellac rings by the turn table obliges one to keep on hand a large stock all the time to insure perfect drying, and to have them always ready. I am obliged to have some 1000 or 1500 on hand in advance, and this necessitates a considerable outlay in stock, which will not always be convenient for amateurs. For the above reasons I now propose a new process which appears to meet all the desired wants, and which combines the advantages of the shellac cement and the gutta-percha rings.

The very simplicity of this process causes me to wonder why it was not thought of before. I take a sheet of thin writing paper, white or colored, and dip it into thick shellac varnish (shellac dissolved in alcohol), and hang it up to dry. When thoroughly dry it should have a good glaze of the varnish on it (different thickness of paper can be used according to depth of cell required). Out of this shellac paper I cut my rings, and these can be made in any quantity, and kept for any time. The process of mounting is simple. The slide is cleaned, and the flat paper ring placed in the centre; on this the cover is placed, having the object dried on it, and the two are held together by the forceps and gently warmed; this serves to attach the ring to the slide, and cover, at several points, so that the forceps may now be laid aside. The next step is to take a glass slip, (another slide), and laying this on the cover, to grasp the two slides at each end by the finger and thumb of the two hands, and pressing them tightly together, to warm the slide gently; by looking at the ring obliquely, on the under side, one can tell at once, when all the air is pressed out, and the adhesion is complete between the cover and the ring, and also the ring and the slide, and they must be held together a moment or two to cool. If the lac is sufficiently thick on the paper the adhesion takes place quickly, and with moderate heat, and there will be no danger of breaking the cover, unless it has been warped in the process of warming, which will sometimes occur when very thin glass has been heated too much for the purpose of burning off the organic matter, or when the support is too small in diameter, or when it is not flat. I think I may be able to induce the leading opticians to manufacture this paper and also the rings for sale; for special purposes the paper might be printed beforehand, so that, when mounted, the ring would show on the under side the name of the preparer, or of the object. I cannot conceive of anything more satisfactory than these rings. Many large objects which would be crushed if one used only the shellac rings made on the slide, by the use of the turn table, by the giving way of these by softening, and under the necessary pressure for attaching the cover, are perfectly protected by the paper rings. I am satisfied that the balsam mounts will be much less frequently used, as soon as we can find some *sure* dry process. The diatoms, as a rule, show much better when mounted dry, and with whole frustules, exhibiting both the side and the front view, also the mode of attachment, etc. The dry mounts are certainly to be preferred when they are desired for anything except pretty objects, and even for this latter purpose there is often a very great difference in favor of the dry mount. Although I have not used these shellac paper rings for any very great length of time, yet I can see no reason why they should not be equal to the simple shellac ring for durability, and very much superior to it in other respects.

SCIENCE AT BREAKFAST.

By The Editor.

The sterling goodness of Dr. Johnson's heart, notwithstanding many apparently blunt demonstrations to the contrary, was never more clearly demonstrated, than when he remarked to Boswell, "I encourage this house, for the mistress of it is a good civil woman, and has not much business."

The house referred to was the "Turk's Head Coffee House." But coffee houses, nay coffee drinkers, have much changed in outward form since the days of the sturdy old philosopher. The beau and the belle no longer, in picturesque costumes, discourse scandal, sipping the Eastern beverage from exquisite specimens of china ware, and tea and coffee, no longer a luxury, are now enjoyed by the toiling millions, and esteemed a blessing by all classes.

Although tea and coffee is universally used by the civilized nations of the world, few understand the natural potent properties of these substances, or are even conscious of their powerful action upon the human system, and as it is a subject interesting to so many, I offer the following sketch, treating of the more important points.

Coffee, tea and chocolate all contain in common a nitrogenised basis, to which they owe most of their important chemical properties. Tea and coffee even contain the self-same basis, denominated indiscriminately *theine* or *caffeine*. In chocolate the cocoa principle called *theobromine* is richer in nitrogen than the *theine*.

The chemical constituents of these substances are as follows: While in tea the basis is combined with tannic acid, in coffee it forms a salt, with a peculiar tannic acid, containing a greater proportion of nitrogen, which together with tannio-caffeic acid is united with potash into a so-called double salt. Tanno-caffeic acid when roasted, develops the agreeable odor of coffee.

Not only the same basis, but also two similar organic acids, one contained in tea, the other in coffee, increase the conformity, between the leaves of the former and the beans of the latter.

Legumin, cellulose, gum, sugar, citric acid in addition to oleine, and what is called palm-fat, accompany the organic acids and the theine of the coffee beans.

But the tea leaves, apart from the basis and the acids, are composed of albumen, cellulose, gum and wax, the green pigment of the plant and the volatile oil of tea

This peculiar oil is the principal source of the aroma of tea, by which, in spite of the conformity between tea and coffee, it essentially differs from the latter.

The inorganic constituents of tea and coffee are moreover different. While in coffee, chlorine, phosphoric and sulphuric acids are combined with potash, lime, magnesia and oxide of iron; tea contains another inorganic acid besides, consisting of manganese and a large proportion of oxygen.

So much for the chemical constituents of coffee and tea. Let us now examine their peculiar properties and nutritive qualities.

Chocolate from its large proportion of albumen is the most nutritive beverage, but at the same time from its quantity of fat, the most difficult to digest. But its aromatic substances strengthen the digestion. A cup of chocolate is an excellent restorative and invigorating refreshment even for weak persons, provided their digestive organs are not too delicate. Cardinal Richelieu attributed to chocolate his health and hilarity during his later years.

Tea and coffee do not afford this advantage. Albumen in tea leaves, and legumin in coffee berries, are represented in very scanty proportions, for while in the former the albumin is coagulated by boiling water, in the latter the legumin is prevented from being dissolved by the lime with which it is combined.

The praise of tea and coffee as nutritive substances is, therefore, hardly warranted, because, as restoratives for the body, the alimentary principles and not the elements are to be taken into account. The former principle cannot be ascribed to "Theine," which is excreted again as urea with surprising rapidity, and to this swift transformation tea and coffee owe their diuretic action, which is considerably assisted by the warm water of the infusion.

Tea and coffee, though of themselves not difficult of digestion, tend to disturb the digestion of albuminous substances by precipitating them from their dissolved state. Milk, therefore, if mixed with tea or coffee, is more difficult of digestion than if taken alone, and coffee alone without cream promotes digestion after dinner by increasing the secretion of the dissolving juices.

The volatile oil of coffee and the empyreumatic and aromatic matters of chocolate *accelerate* the circulation, which, on the other hand, is *calmed* by tea.

Tea and coffee both excite the activity of the brain and nerves.

Tea, it is said, increases the power of digesting the impressions we have received, creates a thorough meditation, and, in spite of the movements of thoughts, permits the attention to be easily fixed upon a certain subject; a sense of cheerfulness and comfort ensues, the functions of the brain are set in motion, the thoughts are concentrated and not apt to degenerate into desultoriness.

On the other hand, if tea is taken in excess, it causes an increased irritability of the nerves, characterized by sleeplessness, with a general feeling of restlessness and trembling of the limbs; spasmodic attacks may arise, with difficulty of inspiration in the cardiac region. The volatile oil of tea produces heaviness in the head, first manifesting itself in dizziness and finally in stupefaction.

These symptoms have been called an evidence of a real tea intoxication. Green tea, which contains much more of the volatile oil than the black, produces these obnoxious effects in a far higher degree than the latter.

While tea principally revives the faculty of judgment, and adds to this activity a sensation of cheerfulness, coffee acts also on the reasoning faculties, but without communicating to the imagination a much higher degree of liveliness.

Susceptibility to sensuous impressions is intensified by coffee; the faculty of observation is therefore increased, while that of judgment is sharpened, and the perceptions adopt more quickly certain forms, activity of thoughts and ideas is manifested, a mobility and ardor of wishes and ideals, which are more favorable to the shaping and combination of already premeditated ideas than to a calm examination of newly originated thoughts.

Coffee, also, if taken in excess, produces sleeplessness and many baneful effects very similar to those arising from tea drinking. Coffee, however, produces greater excitement, and a sensation of restlessness and heat ensues. For throwing off this condition fresh air is the best antidote.

Much depends upon the proper roasting of coffee, in which process it loses weight but increases in bulk, two pints of unroasted berries giving three pints when roasted.

Several empyreumatic substances created by roasting produce the reddish or brown color, and the tanno-caffeic acid, altered by roasting, produces the aroma; the sugar loses a part of its amount of hydrogen and oxygen, and is thus decomposed into burnt sugar or caramel.

Liebig states that the berries should be roasted until they are of a dark brown color. In those which are too dark there is no caffeine; and if they are roasted black, the essential parts of the berries are entirely destroyed, and the beverage prepared from them does not deserve the name of coffee. This fact should be noted by drinkers of *caffe-noir*.

The berries of coffee when once roasted, lose every hour, somewhat of their aroma in consequence of the influence of the oxygen of the air, the porosity of the roasted berries allowing it to penetrate easily. Liebig recommended a process by which much of this pernicious change can be avoided. "Strew," says he, "over the berries, when the roasting has been completed, and while the vessel in which it has been done is still hot, some powdered white or brown sugar; half an ounce to one pound of coffee is sufficient."

The sugar melts immediately, and by well shaking, or turning the roaster quickly, it spreads over all the berries, and gives each one a fine glaze, impervious to the atmosphere.

They have then a shining appearance, as though covered with a varnish, and in consequence lose their odor entirely, which, however, returns in a high degree, as soon as they are ground.

After this operation, they are to be shaken out rapidly from the roaster, and spread on a cold plate of iron, so that they may cool as soon as possible.

If the hot berries are allowed to remain heaped together, they begin to sweat, and when the quantity is large, the heating process by the influence of the air increases to such a degree, that the coffee is permanently damaged."

In this city I have often observed that coffee is roasted to too high a color, and filled into sacks too quickly, before the process of cooling is complete.

The preparation of coffee as a beverage is accomplished by three processes: first, by *filtration;* second, by *infusion;* and third, by *boiling*.

Liebig states that filtration gives often, but not always, a good cup of coffee. When pouring the boiling water over the ground coffee is done slowly, the drops in passing come in contact with too much air, whose oxygen works a change in the aromatic particles, and often destroys them entirely.

The extraction moreover is incomplete; instead of 20 to 21 per cent., the water dissolves only 11 to 15 per cent., and 7 to 10 per cent. is lost.

Infusion is accomplished by making the water boil and then putting in the ground coffee, the vessel being immediately taken off the fire and allowed to stand quietly for about 10 minutes.

This method gives a very aromatic coffee, but one containing very little extract.

Boiling is the custom in the East, and yields excellent coffee. The powder is added to the water when cold, and then placed over the fire and merely allowed to boil a few seconds. The fine particles of coffee are drunk with the beverage. If boiled long, the aromatic parts are volatilized and the coffee is then rich in extract, but poor in aroma.

Further, Liebig gives what he calls the best method; this I produce, not because I think the plan will make a coffee acceptable to most palates, but because Liebig speaks highly in its praise, and states that it is without those heating properties, common to most preparations, causing it to be rejected by many in delicate health.

"My method," said Leibig, "is the union of the second and third. The usual quantities of coffee and water are to be retained; a tin measure containing half an ounce of green berries, when filled with roasted ones, is generally sufficient for two small cups of moderate strength, or one so-called breakfast cup; one pound of green berries, equal to 16 ounces, yielding after roasting 24 tin measures (of ½ ounce each) for 48 small cups of coffee.

With three-fourths of the coffee to be employed, (after being ground), the water is made to boil for 10 or 15 minutes.

The one-quarter of the coffee which has been kept back, is then flung in, and the vessel immediately withdrawn from the fire, covered over and allowed to stand from five to six minutes.

In order that the powder on the surface may fall to the bottom, it is stirred around, the deposit then takes place, and the coffee poured off ready for use. In order to separate the dregs more completely, the coffee may be passed through a clean cloth, but generally this is not necessary and often prejudicial to the pure flavor of the beverage.

The first boiling gives the strength, the second addition the flavor. The water does not dissolve more than the fourth part of the aromatic substances contained in the roasted coffee.

The beverage when ready ought to be of a brown black color, somewhat like chocolate thinned with water; this want of clearness in coffee thus prepared, does not come from the fine grounds, but from a peculiar fat resembling butter, about 12 per cent. of the amount the berries contain, and which, if over roasted, is partly destroyed.

In the other methods of making coffee, more than half of the valuable parts of the berries remain in the grounds, and is lost.

"Judging," said Liebig, "as favorably of my coffee as I do myself, its taste is not to be compared with that of the ordinary beverage, but the good effects which my coffee has on the organism should be taken into consideration.

Many persons who connect the idea of strength or concentration, with a dark color, fancy my coffee to be thin and weak, but these were at once more favorably inclined, when I gave it a dark color by means of burnt sugar."

Adulteration of coffee sold in a ground state, is largely carried on, especially of that sold to the poorer classes —out of 34 samples purchased by an English analytical chemist in London, 31 contained chickory, chickory itself being adulterated with all manner of compounds.

There is no falling back, says Dr. Hopall, upon tea and chocolate, as these seem rather worse off than the coffee. Tea is not only adulterated here, but in China, while as to chocolate, the processes employed in corrupting that manufacture, are described as "diabolical." It is often mixed with brick dust to the amount of 10 per cent., ochre 12 per cent., and peroxide of iron 22 per cent., and animal fats of the worst description, while the names "Flake," "Rock," "Granulated," "Soluble," "Dietetic," are merely employed as disguises to cover the fact that they are compounds of sugar, starch and other substances.

The microscope is the most effective instrument in the work of detecting adulterations, the microscopic appearance of coffee and chickory being very distinctive, while the presence of starch granules discovers the particular cereal employed in adulterations.

The adulteration of coffee by the addition of chicory is fraudulent but harmless, chickory containing little that is injurious to the system; coffee indeed is the more active substance of the two; its effects on some delicate constitutions being so strongly manifested, that without a violation of language, it may almost be designated a weak poison.

Some persons positively like the flavor of chickory, others detest it; its presence, however, can be at once detected by its peculiar odor, and if thrown into cold water it imparts a deep tint, which coffee does not.

In conclusion, I offer a useful receipt of Leibig's for preparing coffee in a ground form for special cases, such as marches and journeys, where it is inconvenient to be burdened with the necessary machines for roasting and grinding; by this process its aromatic properties can be preserved.

One pound of the roasted berries is reduced to powder, and immediately wetted with a syrup of sugar, obtained by pouring on three ounces of sugar, two ounces of water, and letting them stand a few minutes.

When the coffee powder is thoroughly wetted with the syrup, two ounces of finely powdered sugar are to be added, mixed well with it, and the whole is then to be spread out in the air to dry. The sugar locks up the volatile parts of the coffee, so that when it is dry they cannot escape.

Ground coffee prepared in this way, and which lay exposed to the air for one month, yielded, on being boiled, as good a beverage as one made from freshly roasted berries.

I have described the mental influence of tea and coffee; much could be written on their influence upon modern society and civilization.

Anne Boleyn makes mention in one of her letters of having partaken of half a pound of bacon and a quart of beer for breakfast; now, after making due allowance for custom and habit, it must be confessed that modern ladies must rise from their morning meal of a cup of coffee with some bread and butter and an egg, with many different sensations and sentiments to those experienced by the fair Queen after her more masculine repast.

BACTERIA IN THE AIR.

M. Miquel has succeeded in seizing and numbering the spores or eggs of bacteria, and while confirming M. Pasteur's observation, that they are always present in the air, shows that their number presents incessant variations. Very small in winter, it increases in spring, is very high in summer and autumn, then sinks rapidly when frost sets in. This law also applies to spores of champignons; but while the spores of moulds are abundant in wet periods, the number of aërial bacteria then becomes very small, and it only rises again when drought pervades the soil, a time when the spores of moulds become rare. Thus, to the *maxima* of moulds correspond the *minima* of bacteria, and reciprocally. In summer and autumn, at Montsouris, one finds frequently 1,000 germs of bacteria in a cubic mètre of air. In winter the number not uncommonly descends to four and five, and on some days the dust from 200 litres of air proves incapable of causing infection of the most alterable liquors. In the interior of houses, and in the absence of mechanical movements raising dust from the surface of objects, the air becomes fertilizing only in a volume of 30 to 50 litres. In M. Miquel's laboratory, the dust of five litres usually serves to effect the alteration of neutral bouillon. In the Paris sewers infection of the same liquor is produced by the particles in one litre of air. These results differ considerably, it is pointed out, from those published by Tyndall, who says a few cubic centimètres of air will, in most cases, bring infection into the most diverse infusions. M. Miquel compared the number of deaths from contagious and epidemic diseases in Paris with the number of bacteria in the air during the period from December, 1879, to June, 1880, and certainly, each recrudescence of the aërial bacteria was followed at about eight days' interval by an increase of the deaths in question. Unwilling to say positively that this is more than a mere coincidence, he presents further observations regarding it. M. Miquel further finds (contrary to some authors) that the water-vapor which rises from the ground, from rivers, and from masses in full putrefaction is always micrographically pure; that gases from buried matter in course of decomposition are always exempt from bacteria; and that even impure air sent through putrefied meat, far from being charged with microbes, is entirely purified, provided only the putrid filter be in a state of moisture comparable to that of the earth at 0.30 metres from the surface of the ground.

The International Congress of Anthropology and Prehistoric Archæology holds its next meeting at Lisbon, on September 20-29, this year. Several important questions concerning the prehistoric archæology of Portugal will be discussed. Excursions will be made to several places of archæological interest.

DEYER'S ASTRONOMICAL RECORD.

MR. J. L. E. DEYER, of the Observatory of Trinity College, Dublin, has prepared and published *A Record of the Progress of Astronomy during the year* 1879.

This interesting digest is similar in every way to the summaries given for 1877 and 1878 by Professor Holden, in the *Annual Record of Science and Industry*. It was intended originally to add a bibliographical list of books and memoirs on Astronomy published during the year, but for various reasons this was left out. Such a list ought to embrace a longer space of time than a single year, and besides, the "Bibliographie Générale" the publication of which has recently been announced from the Brussels Observatory, is to include the year 1880. Mr. Deyer's paper therefore calls attention to such publications only as appeared to possess more than a passing interest. These are mentioned under the following heads: Spherical astronomy, theory of instruments, celestial mechanics, the sun, the moon, the inter-mercurial planet question, planets and satellites, comets, meteors and meteorites, fixed stars, nebulæ and clusters, photometry, history of astronomy, bibliography; observatories, miscellaneous notes.

Although the number of working observatories in this country is small, the present summary would indicate that these few had been reasonably active, since nearly one-third of the memoir (fifteen out of forty-seven pages) is devoted to the results of astronomical work done in the United States. O. S.

THE LATE MR. GREENE SMITH.

In regard to our statement in SCIENCE for July 31st, respecting Mr. Greene Smith's offer of his collection of specimens of birds to the American Museum of Natural History, we are reminded by Professor Burt G. Wilder, M. D., that shortly after the opening of Cornell University, in 1868, Mr. Smith presented that institution with a collection of 362 birds, mostly from North America, all perfect specimens and finely mounted.

We have authority for stating, that in regard to the present disposition of the late Mr. Greene Smith's collection, for the present, at least, it will remain in the possession of his widow. Mrs. Greene Smith informs us that she will devote her attention to making the collection as complete as possible, by the addition of the specimens now absent; and at some future time when she considers she has accomplished this task, she will present the collection to some institution, where it will be most appreciated, and do the greatest good.

THE use of steel for marine boilers has of late increased rapidly, but if the latest news from the Clyde is trustworthy, steel boilers have failed under the test, and have been condemned. Some eminent marine engineers refuse to use it, but several new passenger steamers have been fitted with boilers of steel, and a grave responsibility has been incurred by their owners.—*Eng. Mech.*

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DEPARTMENT OF AGRICULTURE.

At a time when the English Government appears to be awakening to the necessity of systematically bringing the light of science to bear on the various important agricultural problems which are continually forced upon public notice, it is an agreeable task to examine the reports of the Department of Agriculture at Washington, and to note the practical usefulness of the work there taken in hand, and the thoroughness with which it is performed.

The recent reports refer to one of the most important successes of this Department, that of obtaining crystalizable sugar from maize plants, which may be grown in most sections of the United States. Congress at once appreciated the value of this discovery and directed the Commissioner of Agriculture to furnish a report giving all the information in his power in regard to the manufacture of sugar from sorghum, its cost, the character and expense of the machinery necessary, together with statistics of the consumption and production of sugar in the United States and all matters bearing on the subject.

In the reply, which was made *seriatim*, we learn that the Department has thirty-two varieties of sugar producing sorghums and millet plants, all more or less valuable, according to the varying soils, climate, cultivation, seasons and process of manufacture. From these they have selected four, which in their opinion are best adapted to the ends in view. The most useful of these is the Minnesota Early Amber, the juice of which is said to granulate more readily than other varieties. It ripens early, yields bountifully an excellent quality of syrup, and the farmers who have raised this variety of cane record their experiences as showing it to be better than any other variety. The Department of Agriculture commends it for use in the Northern part of the United States in latitudes above Chicago.

Below this latitude the White Liberian Cane may be planted as auxiliary to the Early Amber, while in the latitudes of St. Louis and the region south of it, Honduras Cane should be added to the other two varieties, thus extending the season for working the cane many weeks beyond the period that could be utilized, if but one variety were planted. The Chinese Sorgo Cane ripens about two weeks after the Early Amber.

As the methods employed in making sugar from these plants have been already described, we need only add that experiments by the chemist of the Department during the last two years have demonstrated that there is practically little if any difference in the juice of the several varieties; that they all produce sugar which can be easily granulated, if the cane be taken at the proper season of growth, and that the only important question yet to be determined is as to the variety that will yield the largest amount in a given soil and climate.

We understand that only "*a fair measure of success*" has attended the manufacture of sugar, in the manner now under description, by farmers on a small scale, and we cannot too strongly endorse the sensible advice which has been tendered, that farmers should merely convert the juice of the stalks into a syrup, and that large central mills be established where the syrup may be converted by proper vacuum pans and centrifugals.

These central mills would have the same relation to this industry that the grist mills of a neighborhood bear to wheat and corn.

The making of sugar entails a process requiring considerable practice and experience, and we are not surprised to find that farmers find many difficulties in the way of success, and it will certainly pay them better to sell the syrup, to be converted under the direction of experts. We understand that in the Western States a gallon of dense syrup weighing, say 13 pounds, can be produced for 16⅝ cents (possibly less). This, if properly managed, should yield 6 to 8 pounds of sugar, and, if handled by the centrifugal, may be separated at a fraction of one cent per pound.

If this method of co-operation is carried out, we see no reason why the 2,000,000,000 pounds of sugar annually used in the United States should not be grown and manufactured within its boundaries and by native industry.

HARVARD UNIVERSITY.

The following record of original work in progress at Harvard University, forms part of an interesting article by J. R. W. Hitchcock, A. B.:

In the last publication of the American Academy of Arts and Sciences, in which, by the way, seven of the eight papers are by Harvard investigators, appear the following "Propositions in Cosmical Physics," by Professor Benjamin Peirce:

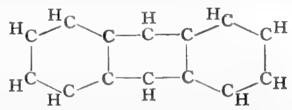
1. All stellar light emanates from superheated gas. Hence the sun and stars are gaseous bodies.
2. Gaseous bodies, in the process of radiating light and heat, condense and become hotter throughout their mass.
3. It is probable that their surface would become colder if there were not an external supply of heat from the collision of meteors.
4. Large celestial bodies are constantly deriving superficial heat from the collision of meteors, till at length the surface becomes superheated gas, which constitution must finally extend through the mass.
5. Small celestial bodies are constantly cooling till they become invisible solid meteors.
6. The heat of space consists of two parts: first, that of radiation principally from the stars, which is small, except in the immediate vicinity of the stars; the second portion is derived from the velocity with which the meteors strike the planet at which the observation is taken; and this velocity partly depends upon the mass of the star by which the orbit of the planet is defined, and partly upon the mass of the planet itself.
7. If the planets were originally formed by the collision of meteors, it is difficult to account for an initial heat sufficient to liquefy them, and, at the same time, to account for their subsequent cooling without a great change in the number and nature of the meteors; and any such hypothesis seems to invalidate the meteoric theory.
8. If the planets were not originally formed by the collision of meteors, their common direction of rotation becomes difficult of explanation.

Professor J. M. Peirce has recently published a set of "Mathematical Tables," in which the part relating to "Hyperbolic Functions" is entirely original. Other work in this department is represented by Professor Byerly's "Differential Calculus" and Mr. Wheeler's "Elementary Plane and Spherical Trigonometry."

The forbidding granite building called "Boylston Hall" conceals scenes of strange activity. Unwonted odors irritate the inexperienced nose of the visitor, and in the laboratories spectral shapes flit backward and forward behind clouds of vapor, occasionally lit up by lurid flames. These are the students; but in their private laboratories the professors pursue their own researches. Professor Cooke has been dealing with that unprincipled element, antimony, which has obdurately persisted in claiming two atomic weights, until he has successfully limited it to one. In connection with his laboratory-work, Professor Cooke is preparing a new edition of his "Chemical Philosophy." The results of his inorganic work have appeared from time to time in the publications of the Academy of Arts and Sciences.

Since the "Organic Laboratory" was established, in 1875, Professors Hill and Jackson have published twenty-five papers giving the results of their work, and have discovered one hundred new compounds. The discovery of new compounds, however, possesses as a rule no special importance, and is rather incidental to, than the result of, the main work. Two examples will indicate somewhat the character and object of organic investigations. The *composition* of uric acid has been long known to be $C_5H_4N_4O_3$, but its *constitution*—the exact arrangement of the atoms—has been uncertain. Chemists all over the world had endeavored to settle the question, but their failures resulted in eleven different formulæ for this one substance. Professor Hill, taking this uric acid $C_5H_4N_4O_3$, marked one part by replacing H by CH_3 (methyl); then treating the acid so as to split it up, he determined to which part the methyl was attached, and, by continuing his treatment, was enabled to reduce the possible formulæ from eleven to three, with strong probabilities in favor of one. This possesses a practical value, inasmuch as it will lead to a knowledge of the method of formation of uric acid in the animal body. Professor Hill's work on "Fur ferrol," found in the products of the distillation of wood, is interesting, as chlorophyll can probably be obtained from it.

An example of the curious subtleties of science is afforded by Professor Jackson's investigations of anthracene, which is obtained from coal-tar, and yields alizarine (madder-dye), used in dyeing pink and purple calicoes, Turkey reds, etc. Anthracene was known to consist of two hexagons of carbon with hydrogen atoms attached, united by two other carbon-atoms. Professor Jackson proved, by making anthracene artificially, that these two carbon-atoms are united to adjacent corners in each hexagon, thus:



These are but stray examples of the researches that are constantly being made by Professors Hill, Jackson, and their assistants. Brom-benzylbromides, parachlorbenzyls, and benzaldehyds, however fascinating they may be to chemists, would offer few charms to the general reader.

Since 1841 Dr. Asa Gray has devoted such leisure as he could command to his great work "The Flora of North America," a labor the magnitude of which only an experienced botanist can appreciate. Mr. Watson, Curator of the Herbarium, is assisting Professor Gray, and at present is classifying the flora of California. The new series of botanical text-books, edited by Dr. Gray, will shortly be completed. The titles will be as follows:

1. "Structure and Morphological Botany of Phænogamous Plants," by Dr. Gray.
2. "Physiological Botany" (Vegetable Histology and Physiology), by Dr. Goodale.
3. "Introduction to Cryptogamous Botany," by Professor Farlow.
4. "Natural Orders of Phænogamous Plants and their Special Morphological Classification, Distribution, Products," by Dr. Gray.

One of the most recent of Dr. Gray's botanical contributions to the Academy of Arts and Sciences was a paper on the "Characters of some New Species of Compositæ in the Mexican Collection, made by C. C. Parry and Edward Palmer," and a notice of "Some New North American Genera, Species, etc."

Professor Farlow's work in cryptogamic botany is doubly interesting on account of its direct practical application. At the Bussey Institution Professor Farlow has been investigating the diseases of plants, and latterly has been engaged upon algæ and fungi. Among his recent work is a paper on algæ for the United States Fish Commission, an examination of the causes of onion-smut and the diseases of trees for the Board of Agriculture, and an investigation of the algæ producing disagreeable tastes and smells in water, for the State Board of Health. His work resolves itself, speaking generally, into two kinds—one, the abstract descriptions and arrangements in families of algæ and fungi, and the other the detection of fungi in disease. As an example of the first, there is a European species of alga which constitutes the green scum on stagnant water. Several different varieties may be found in different places, but they have all been discovered to belong to the same family. To illustrate the second, there is a certain kind of fungus on cedar trees, but this has been ascertained to be only a first stage, and the fungus in its second stage is found upon several members of the apple family.

Professor Wolcott Gibbs has been carrying on researches on complex inorganic acids, and Professors Lovering and Trowbridge have been conducting purely physical investigations. Professor Trowbridge has introduced a method of instruction that necessitates a large amount of original research on the part of his students. This consists of lectures, given by the students instead of by the instructor, to the class. Although all the work at the Observatory really comes under the head of original investigation, the observations constantly taken in connection with the Observatory Time Service resolve themselves into mere routine work. An immediate and practical benefit is conferred by this Time Service, the signals of which reach Bangor, Lennoxville, in Canada, Albany, and New York, as well as different points in Massachusetts. The copper time-ball, held by a powerful electro-magnet at the top of the mast on the Equitable Life Assurance Building, Boston, is released at noon by the clock at Cambridge. During 1879 accidents caused a small error in its fall on two days only, and on three days it has been dropped at 12h. 5m. os.

The great equatorial of fifteen inches' aperture and the meridian circle whose telescope has an aperture of eight inches have been kept actively in use for the last three years. The former instrument has been devoted almost entirely to photometric work. The problem of astronomical photometry, roughly stated, is to determine the brightness of all the heavenly bodies, so that all may be compared with a single standard. Previous to the beginning of this work at the Harvard Observatory, photometric measurements had been made almost entirely upon the planets and brighter stars, and there was no definite knowledge of the amount of light emitted by the satellites and fainter stars. At the outset of the work several hundred measurements were taken of the brightness of the outer and inner satellites of Mars, which measures have been taken accurately nowhere else. The satellites of Jupiter and Saturn, including Hyperion, the faintest of Saturn's satellites, were similarly measured. In addition to measuring their brightness, a large number of determinations of the positions of the satellites were made. A comparison was also begun of the light of the sun and stars, with the idea of reducing all photometric measurements to a common standard— the light of the sun. This photometric work has been continued until the light of all the known satellites, except the two inner satellites of Uranus, has been measured.

One of the most important series of equatorial observations has been in connection with the eclipses of Jupiter's satellites. These phenomena have proved exceedingly valuable as a means not only of determining the orbits of the satellites themselves, but of measuring the distance of the sun or the velocity of light, and of obtaining terrestrial longitudes.

The observations of the mere appearance or disappearance of a satellite, however, can not be rendered sufficiently exact, and, to lessen the errors, photometric observations have been made of the satellites as they gradually enter or emerge from the shadow of Jupiter, using the planet itself or another satellite as a standard.

In order to furnish means for the comparison of the scales of stellar magnitude, employed by different astronomers in their estimate of the brightness of faint stars, a number of faint stars in the immediate neighborhood of the north pole were selected for photometric measurement, and a circular was distributed among astronomers requesting estimates of magnitudes of the same stars for comparison with such other, and with the results of the measurements made here. A series of measurements of all the planetary nebulæ has also been undertaken. This work with the great equatorial has necessitated the invention of a number of new photometric instruments, which have been devised by Professor Pickering and his assistants.

For nearly eight years Professor Rogers has been engaged upon one of the largest astronomical undertakings that has been successfully completed in this country. This is the observation with the meridian circle of the zone of eight thousand stars, between fifty and fifty-five degrees north, undertaken by this Observatory as its share in the determination of the position of the stars of the northern hemisphere. The observations were finished about a year ago, but some years will be required to complete the reduction and publication of this work.

The total number of observations for 1879 with the meridian circle, including about six hundred for the Coast Survey, was nearly three thousand. The scientists at the Observatory are now engaged in the task of determining the light of all the stars visible to the naked eye in the latitude of Cambridge. The meridian is used in observations like a transit instrument in connection with a new and elaborately designed photometer.

At the Museum of Comparative Zoology the staff of specialists is almost entirely occupied in the classification and arrangement of different collections and the publication of the results of their researches. The most important accessions during 1878 and 1879 are the extensive collections of the Blake dredging expedition, and the collections of birds, mammals, reptiles, and fishes, made by Mr. Garman at St. Kitts, Dominica, Grenada, Trinidad, St. Thomas, and Porto Rico, after he left the Blake. The Blake collections and specimens from the entomological, conchological, and ornithological departments are in the hands of well-known specialists for final investigation. Of the extensive work in progress it is impossible to give any details. The results are embodied in the extensive publications of the museum. Five volumes of bulletins have been published, averaging about a dozen papers each. The quarto publications will hereafter be issued as memoirs. The catalogues thus far published have been collected into Volumes I.–IV. of the memoirs. Five volumes of memoirs and the first part of the sixth have already appeared. The second part of the sixth and Vol. VII. are now in course of preparation or in press. Vol. VI. contains the great work upon which Professor Whitney is now engaged, "The Auriferous Gravels of the Sierra Nevada of California." The Sturgis Hooper Professorship of Geology, held by Professor Whitney, is noticeable as being founded solely for original research.

The dredging operations of the Coast Survey steamer Blake have not only aided zoological science by the information obtained in regard to echini, corals, crinoids, ophiurians, worms, hydroids, and others, but have added to geographical knowledge of the Caribbean Sea by showing the changes in form and distribution of lands along various groups of islands, and in the form of the land beneath the water. Professor Agassiz considers the deep-sea collections of the Blake the largest and most important ever made on this coast, and, when combined with the results of other expeditions sent out under the auspices of the Coast Survey, they make the collections at the museum but little inferior to those of the Challenger. During the coming summer Professor Agassiz will probably undertake another dredging trip in the Blake, following the course of the Gulf Stream to the north of the Bahamas, and dredging from the 100 to the 2,500 fathom line off the coast of the United States, so as to connect the isolated district with the deep-water fauna proper of the Atlantic.

Professor N. S. Shaler, Professor of Paleontology, in addition to his work at the museum, and as an instructor, has, since 1873, had charge of the Kentucky State Survey. Four volumes of reports and one of memoirs have been already completed, and one volume of memoirs and nine of reports are now in press. The recent writings of Professor Shaler are "The Origin and Nature of Intellectual Property," and several articles in the "Proceedings of the Boston Natural History Society," "The Atlantic Monthly," and "The International Review." The article by Professor Shaler in the latter magazine is entitled "Sleep and Dreams."

Scientific publications, based entirely or in part upon the entomological collection of the museum, are the new edition of the "Catalogue of the Diptera of the United States," by Osten-Sacken, published by the Smithsonian Institution, Part VIII. of the "Monographic Revision of the European Trichoptera," by R. McLachlan, published in London, and several papers by Dr. H. A. Hagen, the head of the department.

At the medical school the largest amount of original investigation is carried on in the physiological and chemical laboratories. In the former a number of new forms of apparatus are in use, which have been designed by Professor Bowditch and his assistants. Among these are an apparatus for keeping animals alive by artificial respiration; a dog-holder, canulæ for observations on the vocal cords of animals, without interfering with their natural respiration; unpolarizable electrodes used in studying certain problems in the physiology of the nervous system; a new form of apparatus for barometric measurements; and a novel plan for measuring the volume of air inspired and expelled in respiration. A new form of plethysmograph has been devised by Dr. Bowditch. This is an instrument for measing the changes in the size of organs, either hollow or solid, which are produced by variations in the conditions to which they are subjected. The essential part of Dr. Bowditch's invention is a contrivance by which fluid is allowed to flow freely to and from the organ to be measured without changing its absolute level in the receptacle into which it flows, while at the same time a record is made of the volume of the fluid thus displaced.

The more important work going on in the laboratory at the time of my visit consisted of experiments in regard to respiration, with special reference to the functions of the glottis and epiglottis, and trials of disinfectants with a view to ascertaining the temperature necessary to kill germs. A series of experiments was also in progress for testing the porosity of various stones used in building.

The results of the original work performed here have been recently published, together with an account of the physical apparatus in use at the school. Accounts of the most important investigations carried on during the last year are contained in the following papers: "Growth as a Function of Cells: Preliminary Notice of Certain Laws of Histological Differentiation," by C. G. Minot; "Effects of the Respiratory Movements on the Pulmonary Circulation," by H. P. Bowditch, M. D., and G. M. Garland, M. D.; "Pharyngeal Respiration," by G. M. Garland, M. D.; "Functions of the Epiglottis in Deglutition and Phonation," by G. L. Walton. This paper shows that the removal of the epiglottis does not seriously affect deglutition, and therefore it is not necessary for that process. The epiglottis, however, plays an important part in forming and modifying the voice, taking different positions during vocalization, changes of pitch, quality, and intensity.

In the chemical laboratory I found that Professor Wood had been examining the water-supply of Cambridge; and was then engaged in the investigation of the extent to which arsenic is being used in the manufacture or ornamentation of articles in general use, such as wall-paper, confectionery, playthings, etc. The results of this work will be published in the next report of the State Board of Health. Professor Wood is also writing the addition to "Ziemssen's Cyclopædia" on the subject of toxicology.

Dr. William B. Hills was engaged upon a special investigation in regard to the localization of arsenic in the animal economy.

The most important feature of original work at the school of late years has been Dr. Bigelow's introduction of the new operation of litholapaxy.

A number of interesting papers have been recently written by members of the faculty, some of which contain new discoveries of considerable scientific importance. I cite two: "Effects of Certain Drugs in increasing or diminishing Red Blood-Corpuscles," by Dr. Cutter; and "Alterations in Spinal Cord in Hydrophobia," by Dr. Fitz.

The School of Agriculture and Horticulture, called "The Bussey Institution," is located on the sunny slopes of Forest Hills, about five miles southwest from Boston. The labors of the professors connected with this institution have been even more in the line of original research than of instruction, though of late the lack of a sufficient endowment has interfered with the quality of work and the publication of the results.

A number of exceedingly interesting and valuable papers, however, have appeared in the "Bussey Bulletin," the titles of which give some indication of the character of the work. I give a few of the more important: "Hybridiza-of Lilies," by Professor Parkman; "Diseases caused by Fungi"—Professor Farlow; Examinations of Fodders," "Trials of Fertilizers," "Prominence of Carbonate of Lime in Soil-Water," "Importance as Plant-Food of the Nitrogen in Vegetable Mold"—Professor F. H. Storer; "The Potato-Rot," and "The Black Knot" (of plum and cherry-trees)—Professor Farlow.—*Popular Science Monthly.*

ON THE EFFECTS PRODUCED BY MIXING WHITE WITH COLORED LIGHT.

It was noticed several years ago that when white light was mixed by the method of rotating discs with light of an ultramarine (artificial) hue, the result was not what one would naturally have expected, viz.: instead of obtaining a lighter or paler tint of violet-blue the color inclined decidedly toward violet, passing, when much white was added, into a pale violet hue. Two attempts have been made to account for this curious fact: Brücke supposes that the light which we call white is really to a considerable extent red, and that the mixture of this reddish white light with the blue causes it to change to violet. Aubert, on the other hand, following a suggestion of Helmholtz, reaches the conclusion that violet is really only a lighter shade of ultramarine-blue. He starts with the assumption that we obtain our idea of blue mixed with white from the sky, which, according to him, is of a greenish-blue color. We then apply, as he thinks, this idea to the case of a blue which is not greenish, namely, to ultramarine-blue, and are surprised to find that the result is different.

It will be shown in the present paper that these explanations are hardly correct, since they fail to account for the changes, which, according to my experiments, are produced in other colors by an admixture of white. I prepared a set of brilliantly colored circular discs which represented all the principal colors of the spectrum and also purple; these discs were then successively combined in various proportions with a white disc and the effects of rapid rotation noted, a smaller duplicate colored disc uncombined with white being used for comparison. Under these circumstances it was found that the addition of white produced the changes indicated in the following table:

Vermilion became somewhat purplish.
Orange became more red.
Yellow became more orange.
Greenish yellow was unchanged.
Yellowish green became more green.
Green became more blue-green.
Cyan-blue became less greenish, more bluish.
Cobalt-blue became more of a violet blue.
Ultramarine (artificial) became more violet.
Purple became less red, more violet.

Exactly these same effects can be produced by mixing violet with the above mentioned colors. These experiments serve to explain the singular circumstance that when complementary colors are produced by the aid of polarized light, it is difficult or impossible to obtain a red which is entirely free from a purplish hue, a quantity of white light being always necessarily mingled with the colored light. In the case of the red, orange, yellow, ultramarine, and purple discs, I succeeded in measuring the amount of violet light which different proportions of the white disc virtually added to the mixture, and found that it is not directly proportional to the amount of white light added, but increased in a slower ratio, which at present has not been accurately determined.

For the explanation of the above mentioned phenomena, Brücke's suggestion that white light contains a certain amount of un-neutralized red light is evidently inapplicable, since the effects are such as would be produced by adding a quantity not of red but of *violet* light, and for the present I am not disposed to assume that white light contains an excess of violet light. The explanation offered by Aubert does not undertake to account for the changes produced in colors other than ultramarine, and even in this case seems to me arbitrary; neither have I succeeded in framing any explanation in accordance with the theory of Young and Helmholtz which seems plausible.—PROF. O. N. ROOD, *American Journal of Science*

BERNARDINITE: ITS NATURE AND ORIGIN.

By J. M. STILLMAN.

In a previous number of this Journal[1] I published the results of a chemical investigation of a resinous substance from San Bernardino, sent to me by Hon. B. B. Redding, which was said to occur in the form of vein in detached masses, and the vein to be traceable for three miles. The finders (farmers or "ranchers" of that vicinity) sent at the same time pieces of rock as vein-stuff which contained this peculiar resinous substance in the crevices. Some months later

[1] III, vol. xviii, p. 57.

another specimen was sent to this University from Santa Aña in the same section of the country by a resident who stated in his letter that on throwing a match upon the ground he was surprised to see these rocks take fire and burn. He therefore sent a piece to be examined.

The specimens furnished to Mr. Redding were examined by me and the result published in the above mentioned article. The substance, which was extremely light, white and porous, almost chalky, was shown to be mainly a well-marked resin, leaving but a trace of an ash on combustion. No theory was advanced as to its origin, and attention was called simply to its structure :—" On fracture it presents a slightly fibrous structure. Under the microscope it exhibits a two-fold structure—a quantity of very fine, irregular fibers permeating a mass of a brittle, amorphous, structureless substance." Since that paper was written I have endeavored to obtain more definite information as to the origin and occurrence of this peculiar substance. The region of its occurrence is so remote and so inaccessible that it has been impossible for me to investigate the matter in person, and difficult to find competent persons whose business takes them into that region. However, from reports obtained through the agency of Mr. Redding, I feel tolerably confident that the true nature and origin of this substance has been cleared up.

It seems that there grows, and probably has grown for a long time, a species of conifer which exudes large masses of a resinous secretion from abrasions or wounds. These resinous masses are reported to attain considerable size, and to fall off from their own weight. However that may be, the detached resin either from fallen and decayed trees, or from living trees, becomes scattered over the surface of the country and mixed with surface soil and rocks. By a long process of evaporation, action of atmosphere, and the leeching and bleaching agency of the snow which covers the ground for a large portion of the year, these resinous masses lose all vestiges of volatile and soluble matter, and at the same time a fungus growth permeates and splinters the whole mass into minute fragments rendered coherent by the fibers of the fungus. Hence the two fold structure noted, the fungus growth as shown in the previous paper, amounting to less than 10 per cent of the mass.

The perfect change which has taken place in the resin by these agencies evidence that the resin must have been exposed for an indefinite period to atmospheric agencies, and have attained a position of equilibrium toward its surrounding conditions. It is therefore apparently entirely a surface formation, which however has in process of time become so mixed in with surface soil and rocks as in some instances to present the appearance of being *in situ*. (*American Journal of Science.*)

UNIVERSITY OF CALIFORNIA, May, 1880

EDUCATION OF YOUNG ASTRONOMERS.

France has of late shown a greatly increased activity in astronomical work, both in the improvement of existing, and the institution of new, observatories. The question of how to provide these with men thoroughly competent to carry on the work has come prominently forward.

Hitherto, the recruiting of the observatories has taken place in the most irregular manner, and without the help of any special schools, such as are provided for other scientific careers. The candidates who have presented themselves have often neither possessed the theoretical knowledge, nor the ardor and special aptitude necessary for a career so difficult.

At the Paris Observatory, where the staff is the most numerous, and the *matériel* of instruments most complete, a certain amount of practical instruction could be given, but this only at the expense of the ordinary service, and through the goodwill of the older officials, whose regulations did not comprise this surplus work.

But in provincial observatories education has been more difficult, if not impossible. From lack of funds, it is unfortunately often the astronomical professor of the local faculty who is also director of the observatory, and he has to divide his time between these two functions. Sometimes, too, this director, an excellent professor of mathematics and celestial mechanics, has not been sufficiently initiated in the practice of the very delicate observations of astronomy requiring much experience and skill. Lastly, the *matériel* of these observatories has remained hitherto in a state of regretable inferiority, which could hardly inspire the observers with zeal. It will be readily understood, then, how the number of astronomical observers has been very limited, to the prejudice of astronomical work and discovery in France. This is the more regretted since that country has not been wanting in great geometers, who have remarkably promoted the arduous science of celestial mechanics; the illustrious names of Laplace and Leverrier will here readily occur.

It was, then, an urgent matter to form as soon as possible a superior school of practical astronomy, and with this view a ministerial decree has recently been promulgated. With candidates carefully selected and instructed for some time in a systematic way under masters of the science, a number of able astronomers may be looked for, competent to make a good use of the excellent instruments and opportunities that are now being plentifully provided.

The duration of the studies (to be carried on in Paris) will be two years. The first year will be chiefly devoted to the theoretical and practical study of the meridian service, the fundamental base of the astronomy of observation, and to the use of portable instruments, comprising those with reflection, for it is necessary that every astronomer in an observatory should be capable of teaching the use of instruments employed in traveling, and methods of observation, to the explorers, now so numerous, who, on leaving, seek preparatory instruction, the determination of latitudes and longitudes, &c., in the course of their travels. The second year will be devoted to service of equatorials and physical astronomy. The first half of each year will be occupied in lectures, studies, and exercises. During the second half, the students will make the regular service of observations along with the officials of the observatory.

The lectures will be as follows: During the first year, theory of the meridian service, by M. Loewy; practice of meridian observations, by M. Périgaud; calculations of spherical astronomy, by M. Gaillot; use of portable instruments, by M. Mouchez. During the second year, physical astronomy, equatorials, and physics of the globe, by M. Wolf; applied celestial mechanics, by M. Tisserand.

Moreover, MM. Jamin and Desains, the eminent professors of the Sorbonne, will open their physical laboratories to the young astronomers, and direct them in their studies and the management of instruments and various experiments which may interest them, and facilitate their labors in physical astronomy. M. Mascart, director of the central meteorological office, will also put them *au courant* with recent progress accomplished by meteorological science and service.

The work and lectures will be arranged so as to allow the students to attend other courses at the College of France and of the Sorbonne, having some direct relation to astronomy, or capable of being useful to them for obtaining university diplomas.

THE science of human life has been the last to recognize that minute interaction of all the sciences which every other department of knowledge now readily admits. We allow at once that no man can be a good physiologist unless he posesses a previous acquaintance with anatomy and chemistry. The chemist, in turn, must know something of physics, while the physicist cannot move a step until he calls in the mathematician to his aid. Astronomy long appeared to be an isolated study, requiring nothing more than geometrical or arithmetical skill; but spectrum analysis has lately shown us its intimate interdependence upon chemistry and experimental physics. Thus, the whole circle of the sciences has become a continuous chain of cycles and epicycles, rather than a simple sequence of unconnected and independent principles.—PROF. GRANT ALLEN, *Popular Science Monthly*.

ON A PHOTOGRAPH OF JUPITER'S SPECTRUM, SHOWING EVIDENCE OF INTRINSIC LIGHT FROM THAT PLANET.

By Professor Henry Draper, M. D.*

There has been for some years a discussion as to whether the planet Jupiter shone to any perceptible extent by his own intrinsic light, or whether the illumination was altogether derived from the sun. Some facts seem to point to the conclusion that it is not improbable that Jupiter is still hot enough to give out light, though perhaps only in a periodic or eruptive manner.

It is obvious that spectroscopic investigations may be usefully employed in the examination of this question, and I have incidentally, in the progress of an allied inquiry,[1] made a photograph which has sufficient interest to be submitted to the inspection of the Astronomical Society.

If the light of Jupiter be in large part the result of his own incandescence, it is certain that the spectrum must differ from that of the sun, unless the improbable hypothesis be advanced that the same elements, in the same proportions and under the same physical conditions, are present in both bodies. Most of the photographs I have made of the spectrum of Jupiter answer this question decidedly, and from their close resemblance to the spectrum of the sun indicate that, under the average circumstances of observation, almost all the light coming to the earth from Jupiter must be merely reflected light originating in the sun. For this reason I have used the spectrum of Jupiter as a reference on many of my stellar spectrum photographs.

But on one occasion, viz.: on September 27, 1879, a spectrum of Jupiter with a comparison spectrum of the moon was obtained which shows a different state of things. Fortunately, owing to the assiduous assistance of my wife, I have a good record of the circumstances under which this photograph was taken, and this will make it possible to connect the aspect of Jupiter at the time, with the spectrum photograph, though I did not examine Jupiter with any care through the telescope that night, and indeed did not have my attention attracted to this photograph till some time afterwards.

I send herewith to the Astronomical Society for examination the original negative which is just as it was produced, except that it has been cemented with Canada balsam to another piece of glass for protection. Attached to the photograph is an explanatory diagram, intended to point out the peculiarities which are of interest. It will be noticed at once that the main difference is not due to a change in the number or arrangement of the Fraunhofer lines, but rather to a variation in the strength of the background. In the case of the moon the background is uniform across the width of the spectrum in any region, but in the case of Jupiter the background is fainter in the middle of the width of the spectrum in the region above the line *h*, and stronger in the middle in the region below *h*, especially towards F. The observer must not be confused by the dark portion where the two spectra overlap along the middle of the combined photograph.

In order to interpret this photograph it must be understood that the spectrum of Jupiter was produced from an image of the planet thrown through the slit of the spectroscope, by a telescope of 183 inches focal length, the slit being placed approximately in the direction of a line joining the poles of the planet. The spectroscope did not, therefore, integrate the light of the whole disk, but analyzed a band at right angles to the equator and extending across the disk. If either absorption or production of light were taking place on that portion of Jupiter's surface there might be a modification in the intensity of the general background of the photographed spectrum.

A casual inspection will satisfy any one that such modifications in the intensity of the background are readily perceptible in the original negative. They seem to me to point out two things that are occurring: first, an absorption of solar light in the equatorial regions of the planet; and second, a production of intrinsic light at the same place. We can reconcile these apparently opposing statements by the hypothesis that the temperature of the incandescent substances producing light at the equatorial regions of Jupiter did not suffice for the emission of the more refrangible rays, and that there were present materials which absorbed those rays from the sunlight falling on the planet.

If the spectrum photograph exhibited only the absorption phenomenon above *h*, the interest attached to it would not be great because a physicist will readily admit from theoretical considerations that such might be the case owing to the colored belts of the planet. But the strengthening of the spectrum between *h* and F in the portions answering to the vicinity of the equatorial regions of Jupiter bears so directly on the problem of the physical condition of the planet as to incandescence that its importance cannot be overrated.

The circumstances under which this photograph was taken were as follows: Longitude of observatory $4^h\ 65^m\ 29^s\cdot 7$ west of Greenwich. Night not very steady. Jupiter and the moon differed but little in altitude. Jupiter's spectrum was exposed to the photographic plate for fifty minutes, the moon was exposed for ten minutes. Jupiter was near the meridian. The photograph of Jupiter's spectrum was taken between $9^h\ 55^m$ and $10^h\ 45^m$, New York mean time, September 27, 1879.

I have suspected that perhaps there may have been an influence produced by the great colored patch on Jupiter which has made itself felt in this photograph. It may be that eruptions of heated gases and vapors of various composition, color, and intensity of incandescence are taking place on the great planet, and a spot which would not be especially conspicuous from its tint to the eye might readily modify the spectrum in the manner spoken of above.

SECULAR CHANGES IN THE EARTH'S FIGURE.

An interesting hypothesis has been promulgated before the French Academy by M. Faye. It has long been known from geodetic surveys and pendulum experiments that contingents and mountain ranges do not exert that attraction on the pendulum which might be expected of them, judging from the observed attraction of such isolated masses as Mount Schehallion, in Scotland, or the great pyramid. In fact, the deficiency of mountains in this respect is so striking that in order to account for it geologists and astronomers have imagined that there are vast cavities underlying continents and mountain chains. A somewhat different explanation of the feeble action of Himalayas on the pendulum has been offered by Sir George B. Airy, who supposes that the attraction of the mountains is counteracted by still fluid lakes of rock below them. But this suggestion does not meet the fact, elicited by M. Saigey, that the attraction on islands of the sea is greater than it ought to be. It appears to be clear, however, that there is a relative lack of matter under continents, and an excess of it under oceans. The hypothesis of M. Faye would seem to solve the problem in a very simple and reasonable manner. He holds that under the sea the earth's crust has cooled much more quickly than under dry land, and hence the solid sea-bed is denser and thicker than the sub-continental mass. Water is a good conductor of heat as compared with rock, and being liquid it is also able to convey heat from its underlying basin. Geodesy shows that the present figure of the earth is an ellipsoid of revolution; but if M. Faye's hypothesis be correct, it has not always been so. At first it was an ellipsoid, but the unequal cooling of the earth, due to the liquid mantle covering it, led to unequal stress and the elevation of continents where the crust was thinner. These continents, according to M. Faye, surrounded the north pole, and the level of the ocean over our hemisphere was raised, thus bringing the earth to a more spheroidal form. Finally, as the cooling continued, the austral continents attracted the oceans, and the figure became once more elipsoidal, as it is to-day. If this ingenious speculation were the true one, it would unquestionably help geologists to explain the origin of the glacial period.—*Engineering*.

* Read before the Royal Astronomical Society, May 14, 1880.

[1] See paper "On Photographing the Spectra of the Stars and Planets," read before the National Academy of Sciences, Oct. 28, 1879, and published in this Journal, Dec., 1879, and in *Nature*, Nov. 27, 1879.

TRANSLATIONS.

M. Stroh presented to the *Société des Ingénieurs Telegraphistes de Londres* a very interesting memoir upon the measurement of the adhesion which occurs when a voltaic current of a certain intensity is made to pass between two metals of the same nature. The author of this remarkable paper has also devised a special instrument for the purpose of measuring the force necessary to separate the adherent metals. It is perhaps not inopportune to remark here that this adhesion should be considered as a disturbing force in many experiments, and especially in telegraphy. The measurement of the amount of force required to overcome the adhesion gave the following results expressed in grammes:

Copper	0.15	Zinc	11.	Platinum	42.
Silver	0.15	Tin	14.	Iron	85.
Alluminium	2.50	Gold	17.	Steel	100.
Bronze	8.50	Lead	18.	Tempered Steel	225

When contact takes place between two different metals, the force of adhesion appears to correspond with that of the metal requiring the least force for its separation. Thus the adhesion of tin with iron would require 14 grammes for separation, the corresponding figure for tin, and not that of iron, which is 85. The importance of this matter is quite obvious, and seems to be well worthy of complete investigation. On microscopical examination of the two surfaces placed in contact, M. Stroh finds that the adhesion always arises from the fusion of the superficial layer by the electrical current.—*L'Electricité.*

WATER JETS AS A SOURCE OF ELECTRICITY.

Mr. J. Elster, a pupil of the celebrated Professor Quincke, is about to present a thesis, at Leipzig, upon this interesting question. The following, according to the *Journal Philosophique*, are the conclusions to which this young *savant* has arrived:

If any electro-motor force whatever is produced by a jet of water, there must have been contact of the particles of the water with a solid body. This condition proves that the electro-motor force is produced by the friction of the particles of water with the metal, so that only a small portion of the liquid contributes to the production of electricity.

Furthermore, when the rapidity of the water jet is increased or diminished, the effect obtained is proportional to the active force of the particles of the liquid, and depends also upon the nature of the body placed in contact with the water.

This theory seems to be very similar to that by which Faraday explained the production of electricity in the Armstrong machine. The conclusions reached by the author are opposed to those of many *savants*, notably the Swedish electrician, Ed. Lund.

1. The movement of a liquid, of itself, cannot produce electro-motor force.
2. The currents called electro-capillary are in the same case whether the fluid moistens or does not moisten the walls of the tube. In the first case the friction takes place between the adherent portion of the liquid and the molecules in motion. In the second case, the molecules of the liquid rub distinctly against the sides of the tubes.

Finally, his conclusion is that the electro-capillary currents, which he claims were discovered by Professor Quincke, are identical with the currents developed by friction, which have been demonstrated by Zollner on the rubber of electrical machines.—*L'Electricité.*

A NEW ELECTRIC PILE.

M. Reynier has devised a hydro-electric pile, comparable in energy, to the nitric acid couple, without its inconveniences. The zinc is immersed in a solution of caustic soda. The porous cup is made of parchment paper. The new couple is about twice as strong as the ordinary Bunsen couple, and is surpassed only by the rectangular Bunsen couple (after the model of Ruhmkorff). M. Becquerel, Sr., has already used a similar pile.—*La Science pour Tous.*

THE METRIC SYSTEM.

To the Editor of Science.

In connection with Prof. Ward's able "Plea for the Metric System in Microscopy," in SCIENCE for July 31st, it may be well to state that the "American Metric Bureau," of Boston, 32 Hawley street, has for some years been advocating the general use of the system, not only by publication of "The Metric Primer," and of various circulars and bulletins, but in the very practical way of supplying standard metric articles at much less than ordinary prices. As Prof. Ward well says, "the way to introduce the metric system is to use it ourselves," and judging by the experience of the students in my laboratory, who are advised not even to name the antiquated weights and measures, a pocket or desk rule is a more convincing argument than exhortation.

BURT G. WILDER.

GENERAL NOTES.

THE transformation of alcoholic liquids into vinegar has long been a matter of discussion. Pasteur holds that the formation of vinegar is a physiological phenomenon caused by vegetation of a particular bacterium, the *Mycoderma aceti*, while Liebig sees in it merely a chemical action of oxygen on alcohol. Recent observations by Herr Wurm, at the Breslau Institute of Plant-Physiology, are regarded as putting the former view beyond a doubt, and Herr Wurm has succeeded in effecting the industrial manufacture of vinegar in accordance with Pasteur's idea.

MR. BISSINGER, M. E., at Carlsruhe, Germany, gives the following results as obtained in his examinations of the several motors in regard to their relative cost per horse-power for each hour. It will be observed that the examination pertained principally to small motors. The relative cost per effective horse-power per hour is as follows:

100 h.-p.	steam engine	7.6
2 "	"	44.3
2 "	Lehmann's caloric engine	26.5
2 "	Hock's motor	40.0
2 "	Otto gas engine	26.4
2 "	Otto Lang gas engine	26.4
2 "	Schmidt's hydraulic motor, supplied with water from the city waterworks	95.00
2 "	obtained by horses and a gin	45.00
2 "	obtained by manual labor	200.00

A CALCULATION of the height of the atmosphere has been recently made by Herr Ritter (*Wied. Ann.*), starting from this principle: The quantity of heat which must be communicated to a mass of air cooled to absolute zero to bring it, under constant pressure equal to the atmospheric, to the state of the lowest layer of the atmosphere, is the calorific equivalent of the mechanical work which would have to be expended to lift this same mass of air from the earth's surface to the limits of the atmosphere. Supposing, first, our atmosphere to consist of a gas which would retain the properties of a perfect gas to absolute zero, he gets the height 25km. Then, making the same calculation for an atmosphere of pure water-vapor, he gets 350km. Considering, lastly, that while it is not possible to make an exact calculation for real gases, which certainly condense and solidify like water-vapor before reaching absolute zero, one must obtain a result little different from that found in the case of such vapor, he concludes that the height of our atmosphere must differ little from 350km.—a number which agrees well with that deduced by Schiaparelli from observation of falling stars.

ACCORDING to some recent experiments of M. Goulier, the coefficient of expansion by heat of a metal is independent of any pressure put upon the metal, and is the same under a tensile strain as under one of compression.

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The list of Associate Editors is not yet complete, but so far it comprises the names of Professional Scientists who stand at the head of the various branches of Sciences with which they have identified themselves.

The Editor will be glad to hear from those who will take an interest in the success of this Journal, or who desire to co-operate in any way; and suggestions will always be welcomed and receive due attention. The intention of publishing "SCIENCE" has been known only to a few, but the following well known and esteemed Scientists have expressed either their intention to contribute, or their approval of the object of the Journal, and good wishes for its success.

Professor SPENCER F. BAIRD, and Dr. ELLIOTT COUES, Smithsonian Institution, Washington, D. C.; Professor EDWARD S. HOLDEN, Naval Observatory, Washington; Professor O. C. MARSH, Yale College; Professor BURT G. WILDER, Cornell University; Professor C. A. YOUNG, Princeton College; Professor S. W. BURNHAM; Chicago; Professor O. STONE, Observatory, Cincinnati; Dr. J. S. BILLINGS, National Board of Health, Washington; Professor WILLIAM A. HAMMOND, M. D., New York; Dr. EDWARD C. SPITZKA, New York; Prof. CLEVELAND ABBE, Army Signal Office, Washington; Professor EDWARD S. MORSE, Peabody Academy of Science, Salem, Mass.; Professor ALEX. A. JULIEN, Columbia College, New York; Dr. J. J. WOODWARD, Surgeon General's Office, Washington; Professor H. P. BOWDITCH, Cambridge, Mass.; Professor H. L. SMITH, Hobart College, Geneva, N. Y.; &c., &c., &c.

All Communications should be addressed to THE EDITOR, Box 3838, P. O. New York.

SATURDAY, AUGUST 21, 1880.

ETHNOLOGY.*

FRAGMENTARY NOTES ON THE ESKIMO OF CUMBERLAND SOUND.

By Ludwig Kumlien.

The record of the voyage of the *Florence*, the vessel which conveyed the Howgate preliminary Polar expedition, has been printed by the Smithsonian Institution by request of Professor Spencer F. Baird, and forms the fifteenth of a series of papers intended to illustrate the collection of natural history and ethnology belonging to the United States, constituting the National Museum, placed in charge of the Smithsonian Institution by an act of Congress.

The report on "Ethnology" by Ludwig Kumlien is of great interest, and on this occasion we confine our attention to this part of the work, reserving other branches for future notice.

He states that the Cumberland Straits, Sound, Gulf or Inlet, extends from about lat. 65° N. to lat. 67° + N. It is the Cumberland Straits of Baffin, its original discoverer at the end of the sixteenth century; the Hogarth Sound of Captain Penny, who re-discovered it in 1839; and the Northumberland Inlet of Captain Wareham in 1841.

During the last quarter century it has often been visited by Scotch and American whalemen, ships frequently wintering on the southwestern shores.

It is at present unknown whether it be a sound or gulf; it is generally considered to be a gulf, but some Eskimo say that the Kingwah Fjord, one of the arms extending to the NE., opens into a large expanse of water, to them unknown. Icebergs are also sometimes found in this fjord which from their positions, seem to have come from the northward, and not from the south.

The eastern shore of this sound forms the western boundary of that portion of Cumberland Island which lies between its waters and Davis Straits, and known as the Penny Peninsula.

In about lat. 66° N. the Kingnite Fjord extends from the sound in a ENE. direction, and nearly joins Exeter Sound from Davis Straits; they are separated only by a portage of a few miles. The Cumberland Eskimo make frequent excursions to the eastern shore *via* these fjords, but seem to have extended their migrations but a short distance northward, finding Cumberland Sound more to their tastes.

The width of Cumberland Sound opposite Niantilic is about thirty miles, possibly its widest part. It is indented by numerous and large fjords, few, if any, of them having been explored; many islands are scattered along both shores, and in some instances form quite considerable groups.

The present Eskimo are few in numbers. We would estimate the entire population, men, women, and children, on both sides of the sound, from Cape Mercy on the east to Nugumeute on the west, not to exceed four hundred individuals. It is certain that within the last thirty years the mortality has been very great among them; even the whalemen remark an astonishing diminution in their numbers at the present day, as compared with twenty years ago.

Numerous traditions exist among them of the time when they warred with other tribes, and old men, now living, have pointed out to us islands that were once the scene of battles, where the besieged party was starved into submission by their enemies. According to the usual story, the hurling of stones was one of the most effective and common modes of warfare; this was especially the case when one party could get upon a ledge above the other. At the present day they are peaceful and quiet, have no recognized leader, and no desire to fight, even if their numbers would permit of it.

As the story goes, the present population were the victors in those fights, and took possession of the country they now inhabit. Some say they came from the northwest, and found another tribe, which they overcame and drove away. Their stories on this subject vary, and sometimes with this unusually interesting tradition, as well as many others, they get events of a very recent date hopelessly mixed up with the rest; and it is no unusual instance to find that some whaler, with a good imagination, has supplied and restored lost portions of the narrative, to their entire satisfaction; but these restorations are chiefly remarkable for their utter disregard of truth or possibility.

The following tradition is a translation from one of the most reliable natives we became acquainted with:

"A long time ago (*tichemaniadlo*) other Innuits (Eskimo) were found here; they were called 'Tunak'; they were very strong, very large, and had short legs and large arms; they had very wide chests. Their clothes were made of bear skins, and their knives from walrus tusks. They did not use bows and arrows, but only the harpoon-lance; they harpooned the reindeer in the water, from their kyacks, which were very large. The *Tunuks* made houses out of *stone*. They were able to lift large stones. We were afraid of them; we fought with them and killed them. They (the *Tunuks*) came in the first place from *Greenland*. The women made clothes from their own hair. They had no dogs at that time, but they made sledges and harnesses, and finally (*witchou* = by and by) put the harnesses on three rocks, one white, one red, and one black; they then called, and when they looked they found the stones had been transformed into dogs. After a time they got plenty of dogs; then they went about more. The present Eskimo could not understand their language. They lived to a great age (*E. tukewouk nami* = did not die!). Far to the west some Eskimo lately saw some *Tunuks*; they had bear-skin clothing. In the *Tunuks* land (where?) the *musk ox* (*oming muk*), bear, and seals are abundant. They build walls of stones on the land, and drive the reindeer into ponds, and catch them in kyacks. They have a large, long *callytong* (coat, or jumper jacket) that they fasten down around them on the ice while they are watching a seal's hole; underneath this garment, on the ice, they place a lamp; over this lamp they cook meat. Their eyes

* Bulletin (15) of the U. S. National Museum. Contributed to the Natural History of Arctic America made in connection with the Howgate Polar expedition 1877-78. Washington: Government Printing Office. 1879.

are sore all the time. We are afraid of them; do not like them; glad they have gone away."

This tradition differs somewhat in the particulars when told by different individuals, but the main points are essentially the same. Many will not tell it all; some, only parts of it. The ridiculous story about the dogs is firmly believed by the present Eskimo as the origin of these animals.

That the *Tunuk* have been seen of late years in the west is not improbable—that is, natives, different in dress and stature; but they were most likely the tribe known as the Pelly Bay Eskimo from the north shores of Hudson's Straits and from Fox Channel, they being larger and more robust than the Cumberland Eskimo of the present day. It is certain that since the whalers have begun coming among the Cumberland Eskimo, and introduced venereal diseases, they have deteriorated very much. They now almost depend upon ships coming, and as a consequence are becoming less expert hunters, and more careless in the construction of their habitations, which are merely rude temporary shelters made at a few minutes' notice. Great suffering often ensues from living in these miserable huts. The seal skin that should have gone to repair the tent is bartered to the whalemen for a little tobacco, or some valueless trinket, which is soon thrown aside. The men are employed to catch whales, when they should be hunting in order to supply the wants of their families; and the women, half clad, but sporting a gaudy calico gown, instead of their comfortable skin clothes, and dying of a quick consumption in consequence, when they should be repairing garments or preparing skins, are loafing around the ships, doing nothing for themselves or any one else.

The Cumberland Eskimo of to day, with his breech-loading rifle, steel knives, cotton jacket, and all the various trinkets he succeeds in procuring from the ships, is worse clad, lives poorer, and gets less to eat than did his forefathers, who had never seen or heard of a white man.

There is a practice among them that is probably of long standing, and is regularly carried out every season, of going into the interior or up some of the large fjords after reindeer. They generally go during the months of July and August, returning in September, to be on hand when the fall whaling begins. The purpose of this reindeer hunt is to procure skins for their winter clothing. Nearly all return to the sound to winter. They have regular settlements, which are hardly ever entirely deserted at any season. The principal ones are known as Nugumeute, Niantilic, Newboyant, Kemesuit, Annanactook, Oosooadluin, Ejujuajuin, Kikkerton. and Middliejuacktuack Islands, and Shaumeer, situate at different points on both sides of Cumberland Sound. During the winter they congregate at these points in little villages of snow-huts.

The present principal headquarters are at the Kikkerton Islands, or at Niantilie, according to which point the whalers winter. The old harbor of Kemasuit, once the winter harbor of whalers and a favorite resort of the Eskimo, is now deserted, except by a few superannuated couples, who manage to catch enough seal to live on.

As a rule, the present race is of short stature, the men from five feet three inches to five feet six. There are some exceptions, but they are in favor of a less rather than a greater height. The women are a little shorter. The lower extremities are rather short in proportion to the body, and bow-legs are almost the rule. This probably arises from the manner in which the children are carried in the mother's hood, as well as the early age at which they attempt to walk. The habit of sitting cross-legs may also have a tendency to produce this deformity. Their hands and feet are small and well formed. Their hands are almost covered with the scars of cuts and bruises. It seems that in healing the injured part rises, and is always afterwards disgustingly prominent. There is a great variation in the color of their skin, and a description that would answer for one might not apply at all to another. Even among those that are of pure breed there are some whose skins are no darker than a white man's would be if subjected to the rigors of wind and cold, and the never-removed accumulation of soot and grease. Others again seem to have been "born so." The children, when young, are quite fair. The eyes are small, oblique, and black or very dark brown. The hair is black, straight, coarse, and very abundant. It is rarely wavy or curly among the full-blooded Innuites.

There are, of course, exceptions to the above in case of half-breeds. Their faces are broad and flat, with rather large lips and prominent cheek-bones.

Infanticide is not practiced among the Cumberland Eskimo at the present day. I have learned from some of the most intelligent that this barbarous custom was in vogue in former times, however. Among the natives of Repulse Bay, and those living on the north shores of Hudson's Straits, it is practiced to a considerable extent, especially with the tribe known as the Pelly Bay natives. The practice is confined almost entirely to female children, the reason being, they tell us, that they are unable to hunt, and consequently of little account. It seems to have been referable to the same cause among the Cumberland Eskimo. Their intercourse with the whites seems to have modified some of the most barbarous of their primitive habits.

Twins are not common, and triplets very rare. The males outnumber the females. Infanticide may, to some extent, be the cause; but lung diseases, which are alarmingly prevalent, seem more fatal to the women than to the men.

Children are often mated by the parents while they are still mere infants. There is such an extreme laxity of morals that the young women almost invariably become wives only a short time before they are mothers.

It is impossible to say at what age the women cease to bear children, as they have no idea of their own age, and few are able to count above ten. Puberty takes place at an early age, possibly at fourteen with the female. They are not a prolific race, and it is seldom a woman has more than two or three children, and often only one, of her own; still many, or almost all, have children; but inquiry will generally divulge the fact that some of the children have been bought. Almost every young woman has or has had a child, but the identity of the father is in no wise necessary in order to insure the respectability of the mother or child. Such children are generally traded or given away to some elderly couple as soon as they are old enough to leave the mother. The foster-parents take quite as good care of such adopted children as if they were their own.

So far as we could learn, they do not generally practice any rites or ceremonies of marriage. The best hunter, or the owner of the largest number of dogs and hunting-gear, will seldom have any difficulty in procuring the woman of his choice for a wife, even though she has a husband at the time. It is a common practice to trade wives for short periods or even permanently. They appear to have marriage rites sometimes, but we could induce no one to tell us, except one squaw, who agreed to, but only on condition that we became one of the interested parties and she the other. This was more than we had bargained for, and, although generally willing to be a martyr for the cause of science, we allowed this opportunity to pass without improving it.

Monogamy is at the present time the most prevalent. Polygamy is practiced only in the case of a man being able to provide for two or more wives. Three, and even four wives rarely belong to one man. Neither two nor three wives in one hut make an altogether harmonious household; but all little difficulties are generally settled by the husband, in a manner better calculated to insure reverence to masculine strength than respect for superior intelligence.

The scarcity of women at present in proportion to the men makes polygamy a luxury only to be indulged in by the wealthy. Divorce, if it can be called by that name, is very frequent among them. All that is needed is that the husband tires of his wife, or knows of a better one that he is able to procure. Neither does it seem to trouble the woman much; she is quite sure to have another offer before long; and a change of this kind seems to benefit both parties. One rather remarkable and very laudable practice among these people is the adoption of young children whose parents are dead, or, as often happens, whose mother is the only recognized parent. Orphans, so to speak, are thus twice as common as among civilized nations. These children, whether bought or received as a gift, are always taken as good care of as if they were their own, especially if they are boys.

Among the Eskimo employed by the Florence was a family that had two children, who passed for brother and sister. One, the boy, was a nephew of "Eskimo Joe," of Polaris fame. He had been brought from the Hudson's Straits Eskimo, some two hundred miles to the south. He was a perfect little satan; and, though he gave us much annoyance, he was a never-failing source of amusement to us all. The girl, again, was a native of Exeter Sound, on the west coast of Davis Straits; still, both were considered as their own children, and well cared for.

Half-breeds are said to be of more irritable temperaments, and less able to bear exposure and fatigue, than the full-blooded Eskimo.

The food of the Cumberland Eskimo consists entirely of flesh, and in most sections of the sound, *Pagomys fœtidus*. In fact, this animal is their principal dependence for food, fuel, clothing, and light. The Eskimo will eat a few of the berries of *Vaccinium uliginosum* and *Empetrum nigrum*, the roots of *Pedicularis*, and occasionally a little *Fucus vesiculosus* in winter, but this constitutes a very small and unimportant part of their food.

As soon as the ice has fairly left the sound, the Eskimo hunter leaves the winter encampment, with his family and such portions of his household goods as will be needed, and takes a tour inland or up some of the large fjords after reindeer. The larger part of his possessions, including sledge, dogs, harnesses, winter clothing, etc., he secretes among the rocks in some unfrequented spot. His dogs are put on some little rocky islet, to shift for themselves. They eke out a scanty subsistence by making good use of their time at low tide, *Cottus scorpius* constituting the greater part of their food at this season.

There are at present so many whaleboats owned by these Eskimo, that they experience little difficulty in making quite extensive cruises, three or four families constituting a boat's crew. They will load a whaleboat to within an inch or two of the gunwale, and then set out for a few weeks of enjoyment and abundance. The squaws do the rowing and the "captain" stands majestically in the stern with the steering oar, while the rest of the men are either asleep or on the lookout for game. The cargo consists of their tent-poles, the skin-tents, pots, and lamps, with sundry skin-bags containing the women's sewing and skinning utensils. Their hunting-gear, of course, forms a quite conspicuous portion of the contents of the boat. Very few there are at present who have not become the possessors of a half-barrel, and this vessel occupies a conspicuous place in the boat, and is almost constantly receiving additions of animal matter in some shape; a few young eiders or gulls will soon be covered up with the intestines of a seal and its flesh. From this receptacle all obtain a piece of meat whenever they feel hungry. This vessel is never emptied of its contents, except by accident or when scarcity of material forbids its repletion; and, as the temperature at this season is well up in the "sixties" during the day, this garbage heap becomes so offensive as to be unbearable to any one but an Eskimo.

They proceed at a very leisurely rate, rowing for a few minutes and then stopping for a time, chatting, smoking, or eating. When they feel tired they haul up on the rocks and have a sleep, and then resume the journey in the same vagabond manner. If, while thus cruising, any live creature that they think there is any possibility they can capture comes in sight, all hands become animated, the oars are plied with redoubled energy, guns and spears are in readiness, and every one is eager for the sport. Hours are often consumed in chasing a half-grown duck or a young loon which when procured is but a bite; but the fun of the chase seems to be the principal object, and they enjoy it hugely. Thus they journey till they reach some suitable locality, when the boat is unloaded, the toopiks raised, the lamps put in their places, and all is ready for a grand hunt. The men divide and scatter over the mountains, leaving the camp in charge of the women and children; these busy themselves by hunting for and destroying every living creature that they can find.

On the return of the hunters, who perchance have brought some skins and a hunk of venison, there are joyous times in camp; the meat is disposed of first' and then the younger people engage in various games' while the older ones gather around some aged crone, who excitedly recounts the hunts of her girlhood days, plentifully intermixing stray portions of the old sagas and legends with which her memory is replete. Thus they live from day to day, the men hunting and the women stretching the skins, till the season comes around when they must return to the coast. Happy, contented, vagabond race! no thought of the morrow disturbs the tranquility of their minds.

When a deer is killed any distance from camp, the meat is cached, with the intention of returning after it in winter; but with what the wolves and foxes devour and what the Eskimo never can find again, very little is brought back.

Many have now firearms of some pattern or other; and though they will hunt for a ball that has missed its mark for half a day, they do not hesitate to fire at any useless creature that comes in their way. Those that have no guns use bows and arrows made from reindeer antlers. Sometimes the deer are driven into ponds, and even into the salt water, and captured in kyacks with harpoons.

(*Continued.*)

COAL.

By P. W. Sheafer, M. E., Pottsville, Pa.

I.

Coal is monarch of the modern industrial world, with its wonderfully diversified interests, and their ever expanding development. But supreme as is this more than kingly power at the present time, comparatively brief as has been the period of its supremacy, and unlimited, in the popular apprehension, as are its apparent resources, yet already can we calculate its approximate duration and predict the end of its all-powerful but beneficent reign. This is especially the case with our limited Anthracite; the more widely diffused bituminous having in reserve a much longer term of service—short indeed as a segment of the world's history, but so long, compared with an average human life, as to be of slight practical concern to the present generation.

The territory occupied by the anthracite coal fields of Pennsylvania is but a diminutive spot compared with the area of bituminous coal in Pennsylvania alone, to say nothing of its vast extent in other portions of the United States, and in Great Britain, France and Belgium. The area of the anthracite of the United States is but 470 square miles, not one-twentieth the size of Lake Erie, while the wide-spread bituminous coal fields cover twice the area of our four great lakes: the anthracite making but an insignificant showing on the map of the continent. But the comparison with the bituminous area is deceptive, unless the relative thickness of the two is taken into consideration. If the anthracite beds were spread out as thinly as those of the bituminous region they would cover eight times their present area, or 3,780 square miles. And, again, if the denuded spaces within the borders of the anthracite coal fields were covered with a deposit of coal as thick as we may justly suppose they once were, and as the remaining still are, the available area would be increased to about 2,000 square miles, or 1,280,000 acres; equal to a coal deposit of 92,840,960,000 tons.

Contemplating the number and extent of the coal beds, a total thickness of 107 feet, distributed in fifteen workable beds, interstratified with a full mile in thickness of rock and shale, we are lost in wonder at the luxuriant growth of tropical plants required to produce this vast amount of compressed fuel, and the mighty processes of nature by which it was placed in its present position. The ingenuity of scientists is taxed to account for this wonderful accumulation of fuel, once vegetable, now mineral; once waving in fresh green beauty on the surface of the earth, now buried under hundreds of feet of solid rock; once growing in a level deposit of mud so plastic that the lightest leaflet dropping on its surface, left its impress; now the mud hardened into slate, and the rank vegetation changed to hard and glittering coal, rising and falling in geologic hills and valleys, surpassing in number, depth, extent, sharpness of flexure and acuteness of angle, anything visible in the light of upper day.

Some slight idea of the growth of these ancient forests may be gained from the computation that to form only one of these large beds of coal required a deposit of vegetable matter perhaps one hundred feet in thickness. What shall we say then to the amount of vegetation stored away in the mammoth bed which extends through all three of the anthracite coal fields, covering an area of 300 square miles, with an average thickness of twenty feet, and containing, it is estimated, 6,000,000,000 tons of coal.

Not less wonderful and interesting than the coal deposits is the grand floor of conglomerate which underlies them; a vast sheet of rock, infinitely old, composed of fragments of other rocks infinitely older, bound together by an almost imperceptible cement which holds them so firmly that gunpowder will scarcely separate them. Whence came this great sea of pebbles, water rounded and water-borne to their present resting place? We find them now as the current has dropped them—masses of silex as large as ten-pound cannon balls, and almost as round, so shapely have they been worn by the action of some ancient current. These were deposited first, and then, in regular order, trending to the southwest, came sizes graduated down to those of a pea and grains of sand.

This more than marble floor bears few saurian foot prints; scarcely an impress of bird or beast or fish, or sign of animal life. Nothing but a bed of almost pure silica; a solid foundation on which to build up the mass of rock and the fossil fuel that we call anthracite, older than the hills and predestined for the use of coming man.

The pebble-laden flood ceased, and was followed by placid waters and gentle currents, bringing fine mud and silt to cover the rocky bed. Then the waters drained away, or the land rose, until fit for vegetable life, it was covered with the mighty flora of the carboniferous period. Again it sank, carrying with it its store of decayed and decaying vegetation, and another flood of pebbles rolled over it.

How many ages were consumed in the process so briefly described, who can tell? Nature's operations are on too vast a scale, and her working time too long to admit of hasty activity in the production of results. It may well be said that all the years since the creation of man would be too short a time to produce a bed of coal.

However long the process just described, it was of frequent repetition during the coal period; and thus we find pebble-beds, slate and coal in often recurring series, as in the following cross-section made at Trevorton, the western terminus of the middle Anthracite coal basin.

But through all the changes of time and scene, the upheavals and depressions, the submergence and emergence of the land, we find a remarkable uniforformity in the growth of plants, continuing almost without change throughout; sigillaria, lepidodendra, ferns, etc., following their kind, unvaried through successive series of strata, in each leaving their characteristic impress of stems and foliage on the enduring tables of the rocks. The coal flora is rich in variety and of great beauty, as Professor Lesquereaux's careful research abundantly testifies. Their exact forms show a quiet condition of the waters, at least during the deposit of the slate covering of the coal beds; and the intervening rocks show the same facts. When impressions of the flora are found in the solid coal itself, we have the same evidence; but this is of rare occurrence. The best impressions usually occur in the smooth top slate covering the coal beds.

When we examine the arrangement of the Pennsylvania Anthracite beds we wonder at their complexity. Without evidence of volcanic disruption, not even a protruded trap-dyke, or extensive up or down throw, we often find contortions and disturbances of the strata. The beds are rarely horizontal, but lie at every angle, and sometimes even pass the perpendicular and fold back upon themselves. In places they occupy our mountain summits, nearly 2,000 feet above the level of the sea, and again depressed more than 3,000 feet below it, making a variation of a mile in altitude. Yet the coal, which is the frailest material in all this rocky mass, is not destroyed, but generally in good workable condition—solid, almost crystalized, almost pure carbon, and frequently in beds too thick for economical working.

Faults in the Anthracite beds usually have a northwest and southeast direction, and show the beds compressed, and again correspondingly enlarged, but no sudden dislocations or breaking off of the strata. Soft coal, or dirt faults, are of common occurrence in the red ash or softer coals in the western end of the Anthracite fields.

The colored ash of burned coal is due, doubtless, to the presence of iron; but why this coloring matter is confined to the upper series of coals in the eastern portion of the range, and to the lower beds in the western district; and why there is a gradation in the middle district, from white ash in the lower to grey in the middle and red in the upper beds, are problems yet to be solved.

How shall we account for the great disturbance of the strata from their original horizontal position? Was it caused by volcanic force—of which there are no indications—or by contraction of the earth's crust? And if the latter, why is it confined to the Anthracite region, and not extended to the Bituminous also? And how shall we explain the isolation of the smaller coal fields, like those of Rhode Island, Richmond, Va., or Deep River, in North Carolina; or the disproportion in quantity between the limited area of Anthracite and the widespread fields of Bituminous? Why do we find an abundance of shells and remains of animal life in the latter, and rarely any in the former? A few saurian footprints recently found at the Ellangowan Colliery, in Schuylkill County, and a few shells found in the Glendower Pit, in the Wyoming Valley, are signal exceptions to an almost universal rule. After an exploration, covering the period from 1835 to 1850, Prof. H. D. Rogers and his corps of assistants failed to find any other specimens. Neither has Prof. Lesley in his new Geological Survey of Pennsylvania, or the writer in an experience of thirty years' residence and active service, underground and in surface explorations, been any more fortunate.

Nor in all this area do we find a single workable bed of iron or limestone, and scarcely a covering of fertile soil. The coal once exhausted, nothing is left but the worthless shell, desolate and deserted.

The Anthracite region, mainly confined to one-sixth the area of the four mountainous counties of Luzerne, Schuylkill, Carbon and Northumberland, in Pennsylvania, is crowded with an industrious population which increased fifty-one per cent in ten years; that is, from 229,700 in 1860 to 344,771 in 1870; whilst the four adjacent agricultural counties of similar area increased in the same time from 319,542 to 339,942, only six per cent. It is located on the parallel of 40° 30′, one hundred miles from any seashore, no part of it less than 500 feet above tide—near the headwaters of the large rivers that drain it—the Susquehanna, Schuylkill, Lehigh and Delaware. The noisy trains crossing the valleys and climbing the mountains all verge, day and night, to these hives of industry, where multitudinous steam engines are hoisting and pumping, and breakers crushing. Thousands of miles of railroad thread the surface and dive into the interior, to roll out the black diamond flood in millions of tons of fuel to warm and employ the nation.

In a second paper, I propose to offer some important statistics and information regarding the harvesting of coal.

As a supplement to articles in the last November and January numbers of the *American Journal of Science*, John M. Stockwell details his investigations into the general theory of the moon's motion as affected by the sun's attraction. While taking a rather despondent view of our present knowledge of the factors in lunar calculation, he admits that the general methods of computation are undoubtedly correct.

J. M. Stillman, in August *Journal of Science*, describes the appearance of a new resinous substance in a rocky matrix, from San Barnadino, Cal. It is found in detached masses, in vein form, over a distance of three miles. He seeks to explain its existence by ascribing it to exudations from existing conifers, but does not account for its paragenesis.

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Scientific papers and correspondence intended for publication, should be written *legibly* on one side only of the paper. Articles thus received will be returned when found unsuitable for the Journal.

Those engaged in Scientific Research are invited to make this Journal the medium of recording their work, and facilities will be extended to those desirous of publishing original communications possessing merit.

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Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply, may be written in the form of an article.

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AMERICAN ASTRONOMICAL WORK.

The progress of science in the United States cannot be better illustrated than by a brief review of the astronomical work now in progress, and the instruments at the command of those making observations in this country.

Taking as our authority the "Report on Observatories," published by the Smithsonian Institution, and the "Annual Record" prepared by Professor Edward S. Holden, of the U. S. Naval Observatory, Washington, we find that in seventeen States Astronomical Observatories are located, varying in degree of importance from the National Observatory at Washington, to the possessor of a two-inch achromatic telescope of its owner's own make. The work done with the latter instrument being most instructive as showing how much really good scientific work can be done with limited means when directed by intelligence well applied.

The State of New York can boast of twelve observatories, Michigan four, Pennsylvania three, Massachusetts, Connecticut, Ohio, Missouri, Iowa, each two, and Tennessee, California, Mississippi, Minnesota, Indiana, Kansas, Illinois, Maryland have each one observatory. It will thus be seen what an immense territory is covered by American astronomers, ranging from the shores of the Atlantic to the Pacific coast, and from the tropical regions of the Gulf of Mexico to Lake Superior on the North. Many of these observatories are supplied with requisite appliances of the most perfect description, while all, with one exception, have at least a good achromatic astronomical telescope.

For the benefit of those who desire to promote astronomical research, we may state that the single exception we refer to of an observatory without a telescope, is that of the Ohio State Observatory, the director of which is Professor R. W. McFarland, who states that he "was trying to get the authorities to do something," apparently with poor results.

Among the largest equatorials directed nightly to survey the heavenly bodies may be mentioned the great 26-inch instrument, by Messrs. Alvan Clark & Sons, at the Naval Observatory at Washington, under the charge of Professor Asaph Hall (who has already made such important discoveries with it), assisted by Professor Edward S. Holden; the Dearborn Observatory at Chicago possesses an 18½-inch equatorial (Alvan Clark); Harvard University employs a 15-inch equatorial by Mertz; the Allegheny Observatory, Pennsylvania, has a 13-inch instrument (Alvan Clark); the Morrison Observatory, Glasgow, Missouri, uses a 12½-inch instrument (Alvan Clark); Professor Lewis Swift at Rochester, New York, has charge of a 16-inch equatorial (Alvan Clark); the lady Professor of Vassar College, Poughkeepsie, has an excellent equatorial of 12⅓-inch (Alvan Clark), while lastly, Dr. Henry Draper at Hastings, N. Y., owns a 12-inch instrument, also by Alvan Clark.

This powerful battery of astronomical telescopes of the highest excellence might seem to be sufficient for one nation, but the national spirit of American enterprise appears to be strongly infused into this great branch of scientific research, for new astronomical telescopes of mammoth proportion and exquisite perfection are now in course of construction for United States observatories, which, in the hands of the able astronomers ready to receive them, will doubtless add to their already well-earned fame and the prestige of science in this country.

With these facts before us, we read without surprise the note by Professor O. Stone in our last issue, in which he says of a recently published "Record of the Progress of Astronomy during the year 1879," by Mr. Deyer, of Dublin, one-third of the memoir is devoted to the result of astronomical work done in the United States.

An article on this subject would be incomplete without a reference to the very perfect work of Messrs. Alvan Clark & Son, of Cambridge, Mass., who appear to have distanced both the English and the Continental opticians in the excellence of their objectives, and who have secured to the United States the honor of supplying the objective for the great equitorial about to be manufactured for the Russian Government, to be used in the Pulkowa Observatory by the distinguished astronomer, Otto Von Streuve. We also notice that of the forty observatories recognized by the Smithsonian Institute, seventeen have telescopes made by this firm. In regard to the work now in progress at the Messrs. Clark's establishment, it may be stated without exaggeration that the world awaits with eager expectancy the result of their labors.

We record with pleasure the very perfect harmony with which American astronomers co-operate and work, which has doubtless been a leading point in gaining the successes that have been attained. This is in strong contrast with the constant bickering among members of the Royal Astronomical Society and many English astronomers, some of whom have not thought it humiliating to charge the Astronomer Royal with ignorance, and a stubborn adhesion to error, and to allege that members of the council of the Royal Astronomical Society suppress the papers of their fellow members from personal and unworthy motives.

Of American astronomers, it might seem invidious to make a personal reference to particular men, but the names of Newcomb, Hall, Eastman, Holden, Stone, Burnham, Draper, Swift and Rutherford are familiar in all civilized countries, and respected wherever the science of astronomy is appreciated.

M. MASCART has been making some observations at the College of France, on atmospheric electricity, with a Thomson quadrant electrometer, the deflections of the needle being transmitted to a pencil. The two pairs of quadrants are kept at equal potentials of contrary sign by two poles of a battery which communicate with the ground; the needle is connected with a vessel letting flow a continuous stream of water into the outer air. Generally the potential of the air, always positive, is found much higher, and more uniform by night than by day. From 9 P. M. to 3 A. M,, it varies little, falls at daybreak, reaches a minimum about 3 P. M., and rises rapidly to a maximum about 9 P. M. It is commonly thought that there are two maxima, viz. morning and evening, and two minima, one in the daytime, the other at night. M. Mascart believes that insulation has been too much neglected.

A NEW ELECTRIC PILE DEVISED BY M. REYNIER.

Translated for "SCIENCE."

M. Emile Reynier, the electrician, and inventor of an electric lamp, which we have more than once had occasion to present to our readers, and which its author has never ceased to improve and perfect, with the view of making its use more satisfactory, more convenient, and more economical, has now arranged a pile, which is at the same time powerful and economical. This apparatus is composed of a glass vessel in the form of an oblong square, in which is immersed a sheet of copper bent upon itself, as shown in Fig. 1. Upon the bottom of this copper hook rests a cup of parchment, into which the zinc plate is placed, as shown in Fig. 2.

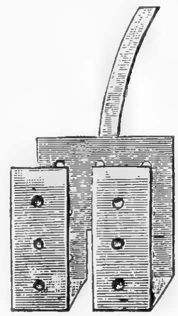

FIG. 1.

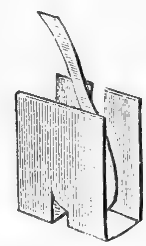

FIG. 2.

FIG. 1.—THE COPPER PLATE OF THE PILE OF REYNIER.
FIG. 2.—THE ZINC PLATE OF THE PILE OF REYNIER.

This vessel or porous diaphragm has this peculiarity, that it is made up of a conical sheet of parchment, and that corresponding with the rectangular or octagonal form, just as may be chosen, it is folded upon itself (Figs. 3 and 4) as indicated by the tracings of the diagrams (Figs. 5 and 6). The strongly marked lines in the figures represent the folds of the angles, the figures indicating the faces, whilst the lighter lines represent the intermediate folds which insure the stability of the system.

FIG. 3.

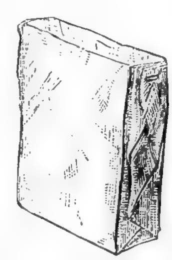

FIG. 4.

FIG. 3.—PARCHMENT DIAPHRAGM OF THE HEXAGONAL FORM.
FIG. 4.—PARCHMENT DIAPHRAGM OF THE RECTANGULAR FORM.

When the different parts are thus mounted, forming the group known as an element (Fig. 7), a solution of caustic soda is turned into the porous cup containing the zinc; into the outer vessel, a concentrated solution of the sulphate of copper. The two electrodes, zinc and copper, being placed in relation by the conductors, a constant chemical decomposition begins. This pile, which M. Reynier qualifies as hydro-electric, is superior, as a working electro-motor, to the Daniell pile, of sulphate of copper and sulphate of zinc; of equal size, and is about twice as powerful as the ordinary Bunsen pile of the laboratories, and is only surpassed by the special form of the Bunsen pile, devised by Ruhmkorff.

"The zinc is not amalgamated," says M. Reynier in his note to the Academy of Sciences, "nevertheless, it is not attacked when the circuit is open by the alkaline solution which bathes it; consequently, the quantity of zinc consumed must give precisely the measure of the amount of elec-

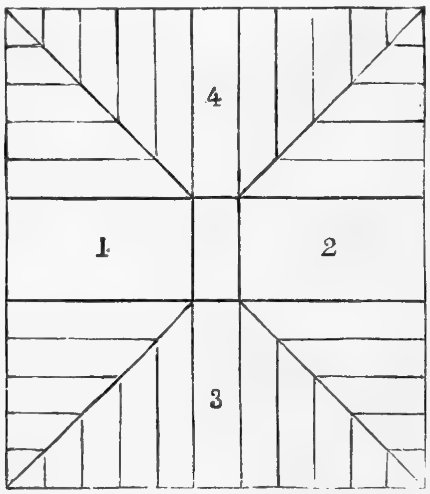

Fig. 5.

Fig. 5.—Parchment sheet traced for the rectangular form.

tricity disengaged. The new pile" he adds, "does not send off volatile products; hence the materials employed are not subject to waste. It is therefore possible to regulate the products of the chemical changes, and they may even be restored to their original state. It is necessary, to do this, to cause a quantity of electricity a little greater than that which has been disengaged by the pile, to traverse the exhausted liquids, dissolving the copper displaced, and removing the zinc dissolved. This renewal of the materials of the pile restores its electro-motor qualities. When elec-

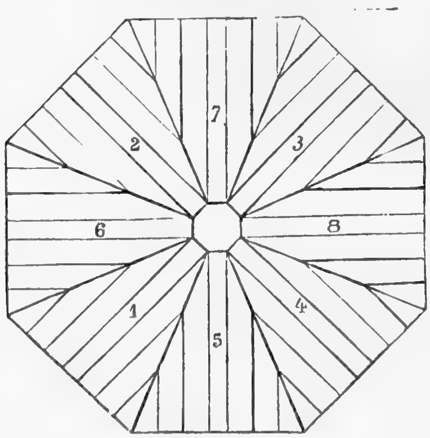

Fig. 6.

Fig. 6.—Parchment sheet traced for the hexagonal form.

tricity is thus produced by the aid of a powerful machine, it will be found to be stored up in the solutions and metals, in a state of energy, and can thus be readily set free or transported. The indirect transportation of electricity by this apparatus would be in most cases, of more practical use and more convenient than the direct transmission by cables.

"In fact, when fresh solutions only are used, the new couple has the advantage of a noticeable economy of material and manipulation over the ordinary nitric acid couple. Regarding the practical realization of the process of regeneration which must make my pile economically applicable to small electric motors and to private illumination, there are still certain obstacles of a practical nature which appear to me to be by no means insurmountable."

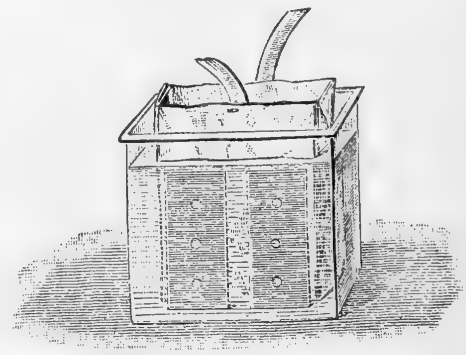

Fig. 7.

Fig. 7.—Reynier's Pile complete.

M. Reynier's pile has been submitted to repeated trials, notably by the *Société Française de Physique*, with fifty couples or elements; the inventor operated successively a voltameter, the electric motors of Gramme and of Deprez, a large Ruhmkorff coil, and an electric lamp with Serrin's regulator. A platinum wire 65 centimetres in length and half a millimetre in diameter, was maintained at a white heat for more than an hour, while the galvanometer failed to show the slightest decrease of power in the pile.—*La Science pour Tous.*

M. Poincaré presented to the Academy of Sciences, Paris, the results of an investigation of butcher's meat, in which he found cylindrical pointed elements with cuticles crossed by lines which seem outlines of cells, and which appear granulated. He thinks they may be phases or metamorphoses of tænioides, causing tænia in some eaters of raw meat.

Dr. J. Lawrence Smith has determined and named the new mineral Peckhamite found on the outer surfaces of the remarkable meteorite whose fragments were sown across the borders of Dickenson and Emmet Counties in northwestern Iowa. By an average of two of Dr. Smith's analyses it contains 49.55 per cent. silica, 16.44 per cent. ferrous oxide and 32.76 per cent. magnesia. By calculation of the oxygen ratio the formula $SiO_2RO + \frac{1}{2}(SiO_2R_2O)$ would represent its composition, suggesting two atoms of Enstatite or Bronzite plus one atom of Olivine. This is one of the most interesting meteorites known. Over 5,000 fragments of it weighing about 30 kilograms, have been collected from over a distance of eight miles long by one-half mile wide. Although the lumps have been lying on the wet prairie for nearly a year, they are not in the least rusted, and bear a great resemblance to nuggets of platinum. Dr. Smith surmises the rapid passage of the meteorite through our atmosphere caused its disintegration, pulverizing the stony part completely and leaving the nodules of neckiliferous iron untouched. This hypothesis is novel and plausible.

DR. PAUL BROCA.

Since the lamented death of Claude Bernard no name has been added to the necrology of France, which has caused more universal regret than that of Dr. Broca. Each, devoted to a special department of science, became illustrious from patient and untiring research which led to definite results. Claude Bernard's investigations into the glycogenic function of the liver stand a monument to his genius and indefatigable industry. Although Dr. Broca became famous as a surgeon and anatomist it is the work he did in the department of anthropology that has made his reputation world-wide.

He was born in 1824, at Sainte Foy la Grande (Gironde), became vice-president of the Academy of Medicine, officer of the Legion of Honor, and a member of several learned societies. During the greater portion of his life he was Professor of Surgical Pathology to the Faculté de Médecine, and surgeon to the hospitals. His numerous contributions to science relate chiefly to Anthropology, and undoubtedly France owes to Broca, more than any other, the advanced position she occupies as a promoter of this science.

DR. PAUL BROCA.

In 1861, he made the remarkable discovery that the seat of articulate language is situated near the third frontal convolution on the left side of the brain. From this time he devoted himself to the study of the cerebral convolutions and ganglia and thus opened up a field for scientific research hitherto almost unknown. His works on "cerebral localizations" and "comparative anatomy of the cerebral convolutions" were pioneers in this department of science, and are to-day standard authorities on this subject. To Broca is due the founding of the Anthropological Society of Paris, and later of the now celebrated École d' Anthropologie, with its magnificent museum, libraries and laboratories, and a complete course of lectures by a faculty of professors comprising such names as Mortillet, Bertillon, and Topinard. Broca himself had charge of the department of comparative anatomy of the primates.

His sudden death is supposed to have been due to a cerebral hæmorrhage, induced perhaps, by excess of labor and fatigue. Thus in the vigor of life and in the midst of his work, has died a scholar, philosopher and statesman, whose illustrious example will continue to enlighten the path of those who follow his imperishable footprints.

THE WINGED PHYLLOXERA.

J. S. Hyde, of Santa Rosa, California, a few days since, while examining some grape-vine roots infested with the *phylloxera* remarked to some friends present that there was little danger of a rapid spread of this pest, as the insect, in its winged form, had not yet appeared. The words had scarcely passed from his lips when one of the gentlemen cried out: "I see one with wings now!" On a more careful examination eight full-winged specimens were found; the next day four other specimens were discovered. Several of these insects were sent to Dr. Hilgard of the State University for examination.

The above announcement by Dr. Hyde is very interesting, and not the less so from the fact that the insects he sent to Prof. Hilgard, although truly a winged form, were sterile, and not capable of spreading the destruction which our vine-growers fear. In order to show the difference between the fertile and infertile winged-forms, we have reproduced engravings from Prof. Riley's drawings of the *phylloxera* which show clearly the distinction between the two. Fig. 1 is the fertile winged female, which thus far has not been observed in this State, unless some of the insects retained by Dr. Hyde are of this form. Fig. 2 shows the outline of each of the five specimens received by Prof. Hilgard. It will be observed that in the fertile winged female the abdomen is prolonged and that the body is about half as long as the whole insect, wings included, and is of a tapering, rounded form. In the sterile, the body is rarely one-third the length of the whole insect, and the abdomen is abruptly contracted, as shown in the engraving.

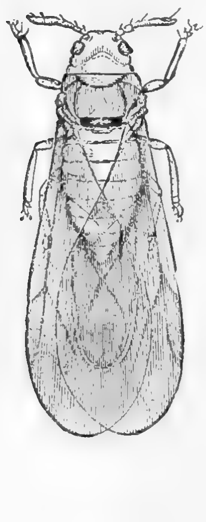

Fig. 1.

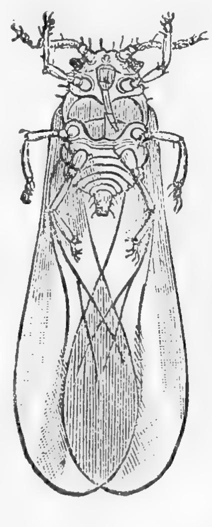

Fig. 2.

Concerning the finding of the sterile winged form by Dr. Hyde, Prof. Hilgard spoke at the meeting of the Horticultural Society as follows: "After all the matter still rests pretty much as we conjectured two years ago. I then stated that either the winged *phylloxera* was not here at all, or in very small numbers. There are five or six varieties of the insect non-winged, which live on the roots, and which produce a winged form towards autumn, which, like the wingless form, is simply a female which lays eggs without any connection on the part of the male. The winged female is simply an egg-layer, like the others. She lays a few eggs only. Some of these eggs produce males and others females. It seems to be necessary that there should be a kind of renovation of the race in that way. The winged insect is dangerous, of course, because it is capable of flying to some extent. In Europe the winged form has been known to traverse a distance of 30 or 40 miles at one jump, leaving districts between unaffected. In Sonoma the progress of the pest has been slow, and it has been thought that the winged insect was absent. The winged forms produce only two varieties—one fertile, the other infertile. I understand that in other countries the proportion of infertile insects is small. Of the eight specimens sent me by Dr. Hyde, I find that five are of the infertile kind. I infer, therefore, from the evidence thus far produced, that the spread of the pest will still be comparatively slow, depending upon the kinds that crawl instead of those that fly."

The announcement of Dr. Hyde, which was made to the *Rural Press*, will doubtless induce a still wider search for the winged insect. We shall be pleased to receive any specimens which may come to the eyes of our readers to determine their exact standing.

INSTANTANEOUS PHOTOGRAPHY IN A BALLOON.

PAUL DEMARETS.

Since the memorable day when the bold Pilâtre de Rozier and the Marquis d'Arlandes left the earth for the first time, up to the present, all aerial travelers have been struck with the clearness with which the celestial landscapes have opened up to their view. The idea of employing photography to fix these admirable contours is contemporaneous, so to speak, with the invention of Niepce and Daguerre.

To M. Nadar belongs the credit of making the first attempts, and to M. Dagon the merit of remarkable execution in bringing the ascensions under control. But in spite of these successes, thanks to the generous aerial hospitality of M. Henry Giffard, one would think that the fixation, at a distance, of celestial landscapes upon a sensitized plate in a balloon, was a mere chimera. In fact the rapidity of the motion of the aerostatic globe, and the rotation about its axis, would seem insurmountable obstacles.

Attention has been recently directed to the rapidity of impression which may be attained, and which I believe I have increased by the aid of certain re-agents; but one difficulty still to be surmounted was the want of some means by which the operculum could be closed with such rapidity that the operation would take only a fraction of a second.

The readers of *L'Electricité* know how M. Stein, the able experimentalist of Frankfort, sought for a solution of this problem. It has also described the ingenious apparatus which M. Janssen has made use of in his observatory at Mendon, by which he has secured great rapidity of action on burning a thread of silk which held the mechanism in position. But it was impossible to use this apparatus in an aerostat, although admirably adapted to observation from a fixed position. I should, therefore, have been unable to employ this method if the idea had not occurred to me of using electricity by the aid of the mechanism I am about to describe.

I take a ring, in the centre of which my objective is fixed, normally and solidly, and for the sake of illustration, we shall suppose it to be horizontal. A spring, attached to an arc, situated at the centre and parallel with the objective, presses a horizontal plate, parallel with the ring of the base. This plate bears a shoulder which rests upon a vertical piece of iron. This has a vertical motion within a solenoid. As soon as the current passes it is obliterated by virtue of the attraction, of which Mr. Page and Mr. Bourbouze have made such intelligent use. The plate, meeting no resistance, turns just as it reaches the shoulder. I can give to the spring any tension whatever, so that the time the movable plate takes to make a half or quarter revolution can be regulated at will. The impression is taken during the passage of the aperture in the movable plate across the aperture in the fixed plate. It is easily understood how it may be possible to shorten the time of passage, either by substituting a simple slit or by increasing the tension of the spring.

In order to estimate the time precisely during which an impression is taken, certain experiments are necessary which I have not yet been able to carry out, and which are of the most delicate kind. I may say that this time appears to me not to exceed eight or ten hundredths of a second. Should it be found that it is only one half of a hundredth, I shall not be at all surprised; for the clearness with which I have obtained my images proves the exposure to be so short, that, when the aerostat is moving six to ten metres in a second, it does not traverse any perceptible space while the aperture is uncovered.

A photograph taken at Rouen, in the vertical position, and with by no means favorable circumstances (it was after six o'clock in the evening), shows all the objects contained in a surface of three hundred metres square. I should think that I was then at a distance of 1100 metres from the earth.

Thanks to the excellent instrument of M. Trouvé the weight of my apparatus is only 700 grammes, and is so manageable that after having made a part of the connection I have obtained a marvellous view of the Seine, showing all of its numerous windings, even to Quilliboeuf, and perhaps still farther.—*L'Electricité.*

MR. BRAMWELL ON THE PERKINS SYSTEM.

In view of the report about to be made by order of the United States Government, on what is called the Perkins system, employed by the "*Anthracite*," the little screw steam yacht which recently crossed the Atlantic with so much success, the following report made by Mr. Bramwell at the request of the Perkins Engine Company before the departure of the yacht, may be of interest to engineers and those interested in steam navigation. The engines of the vessel, like her boilers, are of peculiar type, and are the invention of Mr. Loftus Perkins—of the direct acting inverted pattern with surface condensation. They consist of two cylinders, the after of which is bored in two diameters. The smaller diameter bore forms the high pressure cylinder, and receives steam from the boiler during the first half of the down stroke; the larger diameter is the medium or intermediate cylinder, and is supplied at the upstroke with the steam used in the smaller bore during the preceding downstroke. The exhaust from the large bore passes into a chamber, and thence to the low pressure or forward cylinder, giving a total expansion of thirty-two times. The distribution of steam in the after cylinder is effected by three lifting double-beat valves of somewhat peculiar construction, but the low pressure or forward cylinder is fitted with an ordinary slide-valve, having an expansion valve on its back. The condenser is fitted with galvanised wrought-iron tubes, rising vertically from a tube plate, and having closed tops. Within these tubes are smaller ones, through which the sea-water enters and passes down the annular spaces to the inlet of the circulating pump. The exhaust steam comes into contact with the exterior of the galvanised tubes, and, when condensed, is drawn off and returned to the hot well surrounding the upper part of condenser. The space between the high-pressure piston and the upper side of the intermediate piston is in connection with the chamber from which the low-pressure chamber is supplied with steam. The cylinders and covers are heated by steam, which circulates through wrought-iron pipes cast into the thickness of the metal, and they are also clothed to prevent loss of heat. The boiler is formed of rows of horizontal wrought tubes, 3 inches in external diameter, connected at frequent intervals by vertical thimbles, the whole series being contained in a wrought-iron double casing, having the space filled in with vegetable black. The boiler is supplied with fresh distilled water, a still being fitted in connection with the condenser to keep up the supply. The actual dimensions of the cylinders are high pressure 7¾ inches, diameter, intermediate 15 13–16 inches, and the low pressure 22 13–16 inches, the latter alone being double-acting. The stroke is 15 inches. These are the chief features of the engines. The trial carried out by Mr. Bramwell appears to have been confined to taking diagrams, and weighing the coal consumed, which was done with minute accuracy, the weight of the sacks being deducted from the gross total. But Mr. Bramwell says that before comparisons can be properly instituted between the economy of the engines of the *Anthracite* and those of different construction, the latter should be tried with the same rigor as characterised the trial of the former. We venture to think, however, that engineers will scarcely regard the trial as altogether what could be wished, for there are several questions of much interest, to which no answer can be found in the report. However, 128 diagrams were taken, and the net result shows that the consumption per horse-power was 1.7lb. per hour,—a very good result for engines so small, but not quite so low as might have been expected. The precautions taken by Mr. Bramwell to obtain a correct estimate were complete so far as they went, and his report is minute in its details. The throttle-valve, stop-valve, and other parts were sealed in the positions to which they were placed, and the coals having been weighed into sacks, the bunkers were closed and sealed. The trial lasted for 12 h. 3 min., but after the 15th cwt. of coal had been used the engines were allowed to run until they stopped through the burning down of the fire. For 10 hours, however, the mean revolutions were 130.7 per minute, the average indicated horse-power during nearly nine hours being 80.9. The loss of water during the whole 12 hours was 23½ gallons. The mean pressures of the various diagrams were ascertained by dividing the areas (obtained with the planimeter) by the length of the diagrams, a method which Mr. Bramwell thinks more accurate than measuring the height. The engines worked with remarkable smoothness and regularity, and with the exception of tightening up two glands about an hour after the start, there was not a spanner or hammer, or any tool used about the engines, nor was a single handle shifted during the 12 hours the vessel was under way. The link motion was in full gear during the whole run, with stop-valve full open, and throttle set so as to cause the engines to run about 130 revolutions per minute. About one gallon of lard oil was used, the cylinder and slide dispensing with lubricant in the Perkins system; grease being inadmissible where it is liable to come into contact with the steam in these engines. It is reported that the *Anthracite*, in her voyage across the Atlantic, used only 20 tons of coal, and 436 gallons of fresh water, and it would be of considerable interest, as Mr. Bramwell suggests, to have a thorough trial of a compound engine of about the same power, viz., from 70 to 90 horse-power. In connection with the trial upon which Mr. Bramwell reports there is a point which we should like to see elucidated. The boiler-pressure is supposed to have been somewhere about 360lb. on the square inch, but the maximum pressure on the first piston is only about 200lb., and the average in the first cylinder about 120lb., a rather serious discrepancy, though this ratio of loss is not unknown. —*Eng. Mech.*

PHYSICAL NOTES.

COPPER-PLATING ON ZINC.—The use of Cyanide baths for plating on zinc has the double disadvantage of being poisonous and expensive. Hess, it is stated, has overcome the objections by rendering the cyanide bath unnecessary. This he accomplishes by the use of an organic salt of copper, for instance, a tartrate. Dissolve 126 grammes sulphate of copper (blue vitriol) in 2 liters of water; also 227 grammes tartrate of potash and 286 grammes crystallized carbonate of soda in two liters of water. On mixing the two solutions a light bluish-green precipitate of tartrate of copper is formed. It is thrown on a linen filter, and afterwards dissolved in half a liter of caustic soda solution of 16° B., when it is ready for use. The coating obtained from this solution is very pliable, smooth, and coherent, with a fine surface, and acquires any desired thickness if left long enough in the bath. Other metals can also be employed for plating in the form of tartrates. Instead of tartrates, phosphates, oxalates, citrates, acetates and borates of metals can be used, so that it seems possible to entirely dispense with the use of cyanide baths.

MM. LETHUILLER and Pinel, of Rouen, have devised an electrical indicator, by means of which the water-level in steam boilers may be ascertained at any distance. The arrangement employed for this purpose consists of an indicating tablet, which may be placed in any part of the establishment, however remote from the boiler-house, in the office of the engineer or the superintendent, or within reach of the boiler inspector. This tablet is connected with the electric indicator, which is fixed at the top of a vertical tube above the boiler, by two electric conducting wires. At the lower part of the scale of the indicator are placed two pieces of copper, upon each of which is fastened a small plate. These platinum wires are superposed at a distance of 0.08 in. When the index, which is attached to a vertical rod connected with the float in the boiler, descends, it rest on the upper plate of platinum, depresses it, and puts it in contact with the lower plate. An electric current is thereby established from a battery connected with the apparatus, causing a bell on the indicator to ring, while at the same time the sign "low water" appears on the tablet. Similar pieces of copper and platinum are fixed at the upper part of the scale, and when the index reaches this limit, in consequence of the rising of the float, the bell rings as before, and the indication "high water" is shown on the tablet. In order to remove the warning word from the tablet, a button is pressed, which returns the indicating parts to their normal position.

IT is but a short time ago we were pleased to see an original article, written by a Japanese, on the combustion of carbon, at low temperatures, and again we are reminded of the "new departure" in an article on the determination of the acceleration due to the force of gravity, at Tokio, Japan (*Amer. Jour. of Sci.* for Aug.), in which the writer, Mr. T. C. Mendenhall, acknowledges the assistance of Messrs. Tenaka and Tenakadate, of the Department of Physics, of the Imperial University of Japan. The method employed was the usual one, which involves the use of a good chronograph and a break-circuit clock, together with an arrangement by means of which the experimental pendulum can be made to record its own beats upon the chronograph at any time. As the resistance offered to the pendulum, although small, is perceptible, it will interfere with its motion if the pendulum is obliged to operate the break circuit at each beat. Mr. Mendenhall obviates the difficulty by making the pendulum break the circuit but twice, once at the beginning of the period and once at the end. By this process the experiment need not be protracted, and yet a great degree of accuracy may be obtained. As the average duration of the experiment is only twenty minutes, differences of temperature may be neglected, and all the conditions may be maintained constant during the whole time of the swing.

PROF. JOSEPH LE CONTE, in an article read before the National Academy of Science, takes issue with Helmholz on some important points in the latter's conception of the Law of Listing. This law has important bearing on the phenomena of binocular vision. Its application, however, from the conclusive experiments of Prof. Le Conte, must be limited to other motions of the eye than those taking place in strong convergence. In thus differing from the high authority of the great German, Prof. Le Conte in a philosophical spirit worthy of more general imitation, deprecates the too common method of trying to verify the results of others, rather than to determine the law for one's self.

As a considerable difference exists between the results obtained by the formulæ of Le Verrier and Stockwell in calculating the longitude of the perihelion and the eccentricity of the earth's orbit, Mr. R. W. McFarland in August *Journal of Science*, gives a comparative table, in periods of 10,000, extending over 4,500,000 years. It is accompanied by a chart (with ordinates at intervals of 50,000) dividing the time into two periods, viz., for 3,250,000 years before, and 1,260,000 years after A. D., 1850. An inspection of the table shows that the motion of the perihelion is exceedingly irregular and occasionally retrograde.

JAS. CROLL, F. R. S., makes mention of an article written by himself in *Phil. Mag.* xxxiii., 1867, pp. 213-216, which may not have been before presented to the American public, in which he accounts for the remarkable fact, first observed we believe, by Mr. Glaisher, that the difference of reading between a black-bulb thermometer exposed to the direct rays of the sun, and one shaded, *diminishes* as we ascend into the atmosphere. Mr. Croll deduces from this, that radiation into stellar space is the medium for the preservation of snow in elevated places, and the protective action of aqueous vapor the cause of its melting in places where there is a greater snow-fall, a remark in perfect harmony with Prof. Tyndall's important discovery regarding the influence of aqueous vapor on radiant heat.

PROFESSOR HENRY DRAPER read a paper of great interest before the Royal Astronomical Society in May, which now appear for the first time in this country.—(*American Journal of Science.*) He gives facts which seem to point to the conclusion that it is not improbable that Jupiter is still hot enough to give out light, though perhaps only in a periodic or eruptive manner. He applied spectroscopy to the problem and submitted to the Astronomical Society the photograph upon which he based his ingenious speculations. We are glad to see that Prof. Draper has been assisted by his wife in these observations. Humboldt long ago suggested as an advantage to science that the finer senses of women be used in astronomical research.

A NEW and abundant locality for the mineral Danburite has been discovered by Mr. C. D. Nims, the mineral collector, in St. Lawrence County, N. Y., which is said by Messrs. Brush and Dana (*August American Journal of Science*), to be of considerable extent and importance. The mineral occurs massive, micro-crystalline and also in druses of magnificent appearance, where, in one instance, a crystal was found 4 inches long and 2½ inches macro-diagonal width. The crystals were originally embedded in a younger calcite which has been much eroded. The paragenesis (in a matrix of granitic rock) seems to be, from their description, quarz, danburite, pyroxene and tourmaline and last a pink calcite. It is also accompanied by pyrite. Messrs. Brush and Dana elaborate their description and enrich it with many angular measurements. The homœomorphism of topaz and danburite are conclusively demonstrated, and an opportunity has been seized to supplement and revise the observation made at the time Smith and Brush worked on the original mineral from Danbury.

O. A. M.

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SATURDAY, AUGUST 28, 1880.

BOOKS RECEIVED.

A TREATISE ON COMPARATIVE EMBRYOLOGY. By Francis M. Balfour, M. A., F. R. S., in two volumes. Vol. I. Macmillan & Co., London. 1880.

Macmillan & Co. have recently forwarded to us this very interesting volume of over 300 pages, abundantly illustrated, the first part of a work upon which we believe the author has been constantly occupied since the publication of his "Development of the Elasmobranch Fishes," in 1878. The second volume is still in press, and will deal with the Embryology of the Vertebrata; while this, the first volume, is devoted to the Invertebrata, omitting the Protozoan forms, and it includes in the beginning of the book an outlined history of the Ovum, as it appears in both the Vertebrate and Invertebrate types.

This is by far the most important book that Mr. Balfour has undertaken, and, in practical importance, takes the precedence of any work that has yet appeared in this branch of biological science.

In his "Elements of Embryology," written with Prof. Foster of Cambridge in 1874, and his "Elasmobranch Fishes," besides numerous contributions to the *Quarterly Journal of Microscopical Science*, and the societies, the author has won a position among European embryologists which makes this work doubly valuable. Merely for an expression of his opinion on many mooted questions, the book will be welcomed on both sides of the Atlantic; but it contains, moreover, as full a history of every form as scientific investigation up to the present time has furnished, with a manifest endeavor throughout to do justice to every investigator. The absence of such a work has long been felt by American students. Few of our libraries have been able to obtain, to anything like completeness, the works of biological specialists; in great measure because such works appear in the form of scattered memoirs, difficult to procure even at the time of publication. In the full field of French, German, Italian and Russian investigations, the danger of completely overlooking the researches of others is constantly discouraging. It may be in appreciation of this general want that the author has placed full notices of his sources of information at the end of each chapter where such reference is made, and, in addition to an index of subjects, has inserted at the close of the volume a classified bibliographical index. This renders each subject exceptionally clear, and places the student in a much fairer way of hunting up the literature of his specialty than has been possible hitherto. In these respects, the book is a model for works of this character.

The science of embryology, now ripe for an eclectic work of this description, has grown rapidly from its infancy in the middle of the present century to the importance of a separate and elaborate branch. With its voluminous literature, it is strange that with one exception, a small volume by Packard, no attempt has been made to collate opinions or handle the subject as a whole. In the phylogenetic light alone, Embryology ranks as a vital portion of Biology; in this connection may be quoted a few lines from the introduction: "It has long been recognized that the larvae and embryos of each group pass, in the course of their development, through a series of stages in which they more or less completely resemble the lower forms of the group." The author shows the bearing of the Darwinian theory upon this fact. While morphology may establish the relations of genera, we turn to Embryology for the basis of a wider classification. Its bearing upon Comparative Anatomy is a patent fact. So it is in the interest of the history of development, or in the relation of a given type to its progenitors, as well as in the morphology and physiology of individuals that embryology is of constantly increasing importance. This is, in part, pointed out by the author in the introduction of the work. More specifically he states the aims of the present work as two-fold: (1) To form a basis for Phylogeny (or the history of the race or group); and (2) to form a basis for Organogeny (or the origin and evolution of organs).

In course of a review of the phenomena of reproduction, as witnessed among the Protozoa and Metazoa, the transition from single to compound organisms is clearly stated: "It must be remembered that a single individual Metazoon, is equivalent to a number of Protozoa coalesced to form a single organism in a higher state of aggregation. It results from this that the segmentation of the ovum which follows the sexual act may be compared to the product of conjugation breaking up into spores, the difference between the two processes consisting in the fact that in the one case the spores separate to form an independent organism, while in the other they remain united, and give rise to a single compound organism."

The ovum is treated of in the first chapter as the natural point of departure in the cycle of development—first in its general, then in its special histories in different types. In the second chapter upon Impregnation and Maturation, an account is given of the remarkable researches of Fol and Hertwig, which surpass in the minute history of these changes the observations of any other naturalists. A chapter on Segmentation closes this introductory portion of the work. In view of the fact that this phenomenon hinges upon the disposition of, the presence, or the absence of food-yolk, the author proposes terms for three corresponding types of ova, as follows: (1) Alecithal for ova without food-yolk, or where it is evenly distributed; (2) Telolecithal, where the yolk is concentrated at one pole; (3) Centrolecithal, where the yolk is concentrated in the centre.

The reader is now ready for Part I, Systematic Embryology, in which the history of each group is treated from the formation of the germinal layers onwards, beginning with the simple parasitic forms, the Dicyemidae and Orthonectidae, passing through each invertebrate family whose development has been studied, and closing with the Echinodermata and Eteropneusta.

A detailed review, even of the author's conclusions, would be obviously out of place. Attention may, however, be called to one or two passages of interest, not only to the specialist, but to the general student of biology. The Coelenterata form an attractive group from the fact that they rarely, if ever, pass from the two layered condition, and the lowest forms, even when adult, "do not rise in complexity much beyond a typical gastrula." The larval form, the planula, is common to all except the Ctenophora. Referring to this, the author remarks: "Paradoxical as it may seem, it appears to me not impossible that the Coelenterata may have had an ancestor in which a digestive tract was physiologically replaced by a solid mass of amoeboid cells."

The chapter on the development of the Mollusca is very full and interesting.

In summary of the group Arthropoda, the genealogy of the Tracheata and Crustacea tends to throw doubts upon the uniting of the whole of the arthropoda into one phylum. In the first place, the Tracheata are descended from some terrestrial annelidan type allied to *Peripatus*. [This is the interesting proto-tracheate form collected by Mr. Moseley on the Challenger expedition, and found by him to possess trachea and nephridia, two organs which respectively demonstrate its affinities in opposite lines to the tracheate and annelidan groups.] The Crustacea on the other hand are clearly developed from a phyllopod-like ancestor, which can in no way be related to *Peripatus*. The conclusion that the Crustacea and Tracheata belong to two distinct phyla, is moreover confirmed by their development.

A chapter on the history of the germinal layers is promised in volume second. It is pleasant to find from the names of Agassiz, and Brooks, and others, that Embryology is gaining a sure foothold in this country.

The book throughout evinces the greatest ability and care. Clearness and truth will make it attractive to the student, and it may safely be predicted that a fresh impetus in embryological research among young students in this country and abroad will date from this publication. If this prove to be the case, the author may well feel repaid for his labor. H. F. O.

ON ANGULAR APERTURE OF OBJECTIVES FOR MICROSCOPES. By Geo. E. Blackham, M.D., F.R.M.S. New York Industrial Publication Company. New York, 1880.

We are glad to see that the vexed question of the angular aperture of the objectives has at length been treated in an exhaustive manner by Professor Blackham, who, by an untechnical method of treating the subject, has endeavored to interest a wide range of readers. The work has been produced in handsome form, and has eighteen sheets of diagrams. A critical review of this book will appear at a later date.

COAL.

BY P. W. SHEAFER, M. E., POTTSVILLE, PA.

II.

The fearful loss of good material involved in mining and preparing Anthracite, as shown in the accompanying tables, though greatly to be deplored, seems to be almost inevitable. The disposition of the coal in large solid beds, and in highly inclined positions, involves strong supports to keep the superincumbent mass from crushing and closing the avenues to the mines; and these supports must consist of massive pillars of the solid coal itself. Wooden props, however ponderous and strong, can only be used for the minor supports. Some of this pillar coal is ultimately removed, but much of it is inevitably lost, especially in the larger beds which frequently range from 20 to 40 feet in thickness, and are often inclined at an angle of from 40 to 70 degrees.

It is estimated that not more than 66 per cent. of the coal is ever taken out from the mines. That which is brought to the surface is run through a huge structure from 80 to 100 feet high, very appropriately called a "breaker," ingeniously contrived for the destruction of coal. There are over 300 of these immense buildings in the Anthracite region, costing on an average $50,000 each, or an aggregate of $15,000,000. To the top of these the coal is hoisted, and then descends through a succession of rolls and screens, emerging at the bottom, in a series of assorted sizes, from huge blocks of lump coal to unmerchantable dust, which forms a grievously large proportion of the whole. This process involves a loss of good coal, equal to 20 or 25 per cent. of the entire quantity mined. For the coal wasted in mining, say 40 per cent., and in preparing, 25 per cent., no one is paid; it is a total loss to landowner, miner and shipper.

Plans for utilizing the waste coal dirt, or culm of Anthracite collieries, have been frequently suggested, but none have come into general use. The Anthracite Fuel Company, at Port Ewen, on the Hudson, in 1877, used 90 per cent. coal dust and 10 per cent. fuel pitch, and made 300 tons of fuel per day, consuming over 50,000 tons of culm. The Delaware and Hudson Company also use at their mines 60,000 tons per annum. They now ship all their coal down to pea sizes, and consume the culm in generating steam. If all our coal companies would follow this excellent example it would enable them to sell half a million tons more coal, and burn the same amount of refuse, thus earning or saving half a million dollars per annum, to add to their revenues. The Philadelphia and Reading Railroad Company has recently introduced a method of burning coal dust in the furnaces of its engines, and the plan appears to meet with success.

The amount of water which drains into a mine from a mile or more of surface is enormous, for the average amount of rain and snow fall is 58,840 cubic inches per square yard annually, and the mines are liable to absorb not only the rain fall on the surface immediately over them, but all that which by contour of the surface, or by converging strata, tends towards them. On an average possibly five tons of water are hoisted for every ton of coal raised—another loss chargeable to mining.

The preponderance of waste coal seems excessive; but the writer's experience in surveys of certain tracts of land, and in preparing maps which show the area exhausted, compared with the amount marketed from ten or more collieries, in a period of 20 years, proves that the loss is not over-estimated, especially in the Mammoth Bed, whose average thickness is 25 feet. An eight-foot bed of coal yields much better in proportion. When they exceed six or eight feet in thickness, especially if steeply inclined, they are not only expensive to mine, but a large proportion of the coal must be left to support the rocky roof.

The Bituminous coals, particularly those of the United States, are not subject to these serious losses, and are quite cheaply mined and prepared. No breakers are required, as the only division is into coarse and fine coal, which are easily separated by screens; and the fine coal can be readily converted into coke, making a better condensed fuel than the coal in its natural shape. The Bituminous beds are nearly horizontal and rarely more than six feet thick, so that it is not necessary to leave extensive pillars; and as the coal is above water level, or in shallow basins, it is not necessary to put up extensive hoisting and pumping machinery. The simple, natural ventilation of American Bituminous mines also does away with the extensive and costly appliances for this purpose of Anthracite mines, in spite of which so many miners annually fall victims to the noxious gases.

The total amount of coal still to be mined, according to the accompanying tables, is 26,361,076,000 tons. The total waste, as experience has shown, is equal to two-thirds of the coal deposit, and reaches the appalling amount of 17,574,050,666 tons, leaving us only 8,787,075,533 tons to send to market. In all our calculations of Anthracite we have counted the area as if in a level plain, and made no allowance for the undulations which must necessarily increase the amount of coal. But as many of the flexures are abrupt and broken, making much faulty and refuse coal, it will cover any over-estimate of area or thickness we have made in our calculations.

Our tables show that 360,017,817 tons have been sent to market in the 58 years from 1820 to 1878, inclusive. Our consumption now amounts to 20,000,000 tons annually. The increase of production for the past ten years has been 187,112,857 tons. At this rate we shall reach our probable maximum out-put of 50,000,000 tons in year 1900, and will finally exhaust the supply in 186 years.

The present product of the Anthracite coal fields is (1878) as follows:

Southern	50	Collieries	6,282,226 tons.
Middle	161	"	3,237,449 "
Northern	132	"	8,085,587 "
Total	343	"	17,605,262 "

At this rate the eastern end of the northern field is being rapidly exhausted. The middle field, too, which contains the lower productive coals, is likely to cease extensive mining about the year 1900; while the western portion of the northern field, extending from Pittston to the western end, and the southern field from Tamaqua to Tremont, comprising about 100 square miles, which contain more coal beds and deeper basins, must furnish the supply for the coming years.

Partially successful experiments have been made to use petroleum as a substitute for coal to some extent. But is it not already evident, under the reckless prodigality of production, that this occult and mysterious supply of light and heat and color will be exhausted before the Anthracite, and can, at best, only temporarily retard the consumption of the latter?

As already intimated, the question of the exhaustion of our coal supply is scarcely more at the present time than a curious and interesting calculation. It has not yet become so grave and portentous as in Great Britain, where a commission, with the Duke of Argyle, Sir Roderick Murchison and Sir W. G. Armstrong at its head, was recently appointed by Parliament to ascertain the probable duration of the coal supplies of the kingdom. There it is serious indeed; for when Britain's coal fields are exhausted, her inherent vitality is gone, and her world-wide supremacy is on the wane. When her coal mines are abandoned as unproductive, her other industries will shrink to a minimum, and her people become familiar with the sight of idle mills, silent factories and deserted iron works, as cold and spectral as the ruined castles that remain from feudal times.

The modern growth and ultimate decadence of this great empire may be calculated from the statistics of her coal mines. In 1800 her coal product was about 10,000,000 tons; in 1854 it was 64,661,401 tons; and in 1877 it swelled to 136,179,968 tons. This period was a time of continued prosperity, when England ruled the world financially and commercially. In the 23 years from 1854 to 1876, inclusive, she produced the enormous quantity of 2,210,710,091 tons of coal; and, more wonderful still, exported only 222,196,109 tons—say ten per cent—consuming the rest within her own borders.

The average increase of her annual output has been 3½ per cent. Will it so continue? Or has she reached the summit of her industrial greatness and commercial supremacy, and will they now decline, and with it her naval and military power, the subservient agent, and, to a large extent, the creature and result of those great interests?

Our Anthracite product, compared with the coal product of Great Britain, is so small as to really seem insignificant. The English Commission counts as available all coal beds over one foot thick—we count nothing under two and a half feet thick, nor below 4,000 feet in depth—showing a net amount in the explored coal fields of 90,207,285,398 tons; estimated amount in concealed areas, 56,273,000,000 tons; total, 146,480,285,398 tons, distributed as follows:

	Explored.	Unexplored.	Total.
England	45,746,930,555	56,246,000,000	101,992,930,555
Wales	34,461,208,913	....	34,461,208,913
Scotland	9,843,465,930	No estimate.	9,843,465,930
Ireland	155,680,000	27,000,000	182,680,000
Total	90,207,285,398	56,273,000,000	146,480,285,398

The exhaustion of this magnificent mass of coal at this present rate of increase, viz.: three and a half per cent. per annum, is estimated by Professor Jevons as follows:

1876, actual output	133,300,000 tons.
1886, estimated annual output	186,600,000 "
1896, " " "	261,200,000 "
1906, " " "	365,700,000 "
1916, " " "	512,000,000 "
1926, " " "	716,800,000 "
1936, " " "	1,003,500,000 "

Thus in sixty years the output would be nearly eight times the present amount, and about one-fourth of the total amount to be found in Great Britain.

This vast estimate seems too enormous. It does not allow for great loss when cost of labor and much competion will prevent the working of small coal beds under two feet in thickness, or for the cost of mining when from 2000 to 3000 feet deep. Nor is it possible that Great Britain's industries and export trade combined will ever require so great a quantity. Modern discoveries and improvements, in applied science, tend to diminish the consumption. The 8,000,000 tons annually required for gas-works may be materially reduced by the use of the electric light. The domestic consumption, now equal to one-fourth the product, or 33,000,000 tons a year, may increase. But will not the iron manufactures be on the wane, and her coal exports—now ten per cent. of her coal product—fall off as those of other countries increase?

We have about 340 collieries and produce 20,000,000 tons per annum, or about 60,000 tons each. Great Britain has nearly 4000 collieries, and mines 132,000,000 tons, or 33,000 tons per colliery. The greater the yield per colliery the less the expense in mining. If we decrease the number of mines and increase their capacity not only to raise the coal, but to exhaust a constant current of foul air and dangerous gases, clouds of powder smoke and millions of gallons of water, we will reduce the cost of mining. Most of the Anthracite mining in the United States is now done at a less depth than 500 feet vertical; but as the coal nearer the surface becomes exhausted, the mines must go deeper and become more expensive.

What a folly it is to boast of our world's supply of Anthracite, and feverishly endeavor to force it into foreign markets, when we can so readily foresee its end? Would it not be wiser to limit its product, restrict its sale to remunerative prices, and consume it at our own firesides, and in our own manufactures?

The monopoly of the Anthracite coal fields by some seven corporations, which, according to the accompanying tables, now control about two-thirds of the whole, and the best coal area, must prove, under economic management, a profitable investment for their stockholders. Mining, selling and transporting their own coal, as they do, individual enterprise cannot hope to compete with them, and must vanish from the ground, and their only rivalry will be with each other, and with the Bituminous trade. Fortunately for the public, this rivalry will always be keen enough to keep the price of coal at a fair low rate of cost and profit.

The coal resources of Great Britain are all developed now, and in process of depletion; while in this country when our 470 square miles of Anthracite are exhausted, we have more than 400 times that area, or 200,000 square miles of Bituminous, from which to supply ourselves and the rest of mankind with fuel. The coal product of the world is about 300,000,000 tons annually. The North American continent could supply it all for 200 years. With an annual production of 50,000,000 tons, it would require twelve centuries to exhaust the supply. But with a uniform product of 100,000,000 tons per annum, the end of the Bituminous supply would be reached in 800 years. What the annual consumption will be when this continent supports a teeming population of 400,000,000 souls, as will be the case some day, must be left to conjecture. But with half that population, as energetic, restless, and inventive as our people in this stimulating climate have always been, under the hopes of success, such a country as this constantly holds out much to tempt ambition and reward enterprise.

If it be true, as Baron Liebig asserts, that civilization is the economy of power, we have it in our immense areas of Bituminous coal. There is no known agent that can answer as a substitute for the vast power and almost limitless usefulness of coal in its general adaptation to the wants of man; and that nation will maintain the foremost rank in enlightened modern civilization which controls, to the fullest extent, while it lasts, this wonderful combination of light and heat and force. We are wiser than our fathers; and from the modest but sublime altitude to which we are lifted by physicial science, and the far extended range of mental vision which it opens up to us, we can see farther into the plans of Providence than those who went before us, and can conjecture the early, if not the remote, future of the human race in our land and in other lands.

Happy that people whose legislators study the best mode of developing the natural resources of their country, and whose great men become great by improving the condition and promoting the welfare of the human race. The greatest of England's five Georges was not either of those who wore the crown, but plain George Stephenson, of Manchester, who rolled the world farther along the path of progress than all the others; and none of the royal Jameses did half so much for the civilization of his country as James Watt, whose boyish study of the steaming tea-kettle developed the giant power that does the world's work with an energy that is tireless and irresistible.

ETHNOLOGY.*

FRAGMENTARY NOTES ON THE ESKIMO OF CUMBERLAND SOUND.

By Ludwig Kumlien.

II.

They have an interesting custom or superstition, namely, the killing of the *evil spirit* of the deer; some time during the Winter or early in Spring, at any rate before they can go deer-hunting, they congregate together and dispose of this imaginary evil. The chief *ancoot*, *angekok*, or medicine-man, is the main performer. He goes through a number of gyrations and contortions, constantly hallooing and calling, till suddenly the imaginary deer is among them. Now begins a lively time. Every one is screaming, running, jumping, spearing, and stabbing at the imaginary deer, till one would think a whole mad-house was let loose. Often this deer proves very agile, and must be hard to kill, for I have known them to keep this performance up for days; in fact, till they were completely exhausted.

During one of these performances an old man speared the deer, another knocked out an eye, a third stabbed him, and so on till he was dead. Those who are able or fortunate enough to inflict some injury on this bad deer, especially he who inflicts the death-blow, is considered extremely lucky, as he will have no difficulty in procuring as many deer as he wants, for there is no longer an evil spirit to turn his bullets or arrows from their course.

They seldom kill a deer after the regular hunting season is over, till this performance has been gone through with, even though a very good opportunity presents itself.

Salmo salar, and one other species of *Salmo* that I could not procure enough of to identify, are caught to some extent in June and September in some of the larger fjords; they are mostly caught with a spear, but sometimes with a hook. (For description *vide* under hunting-gear, etc.)

When these fish are caught, they are put into a seal-skin bag, and it remains tied up till the whole becomes a mass of putrid and fermenting fish, about as repulsive to taste, sight and smell as can be imagined. *Cottus scorpius*, which contributes so largely towards the Greenlander's larder, is not utilized by the Cumberland Eskimo, except in cases of a scarcity of other food supplies; the fish is abundant in their waters, however, and fully as good eating as they are on the Greenland coast.

Birds and their eggs also contribute towards their sustenance in season; they are extremely fond of eggs, and devour them in astonishing quantities.

The "black skin" of the whale, called by them *muktuk*, is esteemed the greatest delicacy. When they first procure a supply of this food, they almost invariably eat themselves sick, especially the children. We found this black skin not unpleasant tasting when boiled and then pickled in strong vinegar and eaten cold; but the first attempts at masticating it will remind one of chewing India rubber. When eaten to excess, especially when raw, it acts as a powerful laxative. It is generally eaten with about half an inch of blubber adhering.

* Bulletin (15) of the United States National Museum. Contributed to the Natural History of Arctic America, made in connection with the Howgate Polar expedition, 1877-78.

The greater portion of their food is eaten raw, especially in Winter. When they cook at all, they only "simmer" it over their lamps in a pot of soapstone. These pots are from eight to twenty inches in length, usually about sixteen inches, and though of variable patterns, the length is generally three times the width or depth. Among such Eskimo as are able to procure old cast-away meat-cans from around the ships, tin has superseded the soapstone both for lamps and boiling-pots.

In Summer, especially when on hunting excursions, they very often "fry" meat by making a little fire-place of stones, and laying a flat piece of stone on the top. The opening to receive the fuel supply is to windward. For fuel at such times they use *Cassiope tetragona* and *Ledum palustre;* these shrubs make a quick and very hot fire. It would be comparatively an easy task for these people to gather enough *Cassiope tetragona* during the Summer to burn during the coldest weather, and not rely wholly upon blubber.

When the Eskimo have been simmering meat, especially seal, in their boiling-pots, they pour off the liquor and mix it with about an equal quantity of blood; this makes a thick and rather greasy soup that must be quite nourishing; the children are very fond of it. It seems possible that from this dish has originated the popular error that these people *drink oil*, a notion that is simply preposterous.

I found among some of these people a little spoon, or rather a miniature scoop, made of ivory, which they used to drink the soup with; it appears to be an old utensil, now going fast out of use, for they can now procure tin mugs. A reindeer's rib, pointed at one end, is used to fish up the meat with, and sometimes to convey it to the mouth. These instruments are found in the graves, but seem to be little used at the present day.

When a seal is brought to the encampment, especially if they have not been plenty for some days, all the villagers are invited to the hut of the lucky hunter, and the seal is soon dispatched. A couple of the younger men skin the animal and distribute the pieces to the assembled company as fast as needed. The testicles, being considered as the choicest titbit, are usually handed over to the hostess; the spinal cord is also rated as one of the choicest portions of the animal. During these feasts they gorge themselves to their utmost capacity, and are in good humor and hilarious. Though there may be ever so poor prospects to procure more food for the morrow, this does not deter them from gluttonously devouring the last morsel, and then go on allowance till they can get a fresh supply. I have seen them thus gorge themselves, and then lie down to sleep with a piece of seal meat by their side, which they attacked every time they awoke.

The intestines of birds, notably *Lagopus* and *Somateria*, are looked upon as choice parts, and birds brought to the encampment are generally "drawn" by the hunters. The fatty excrescence at the base of the upper mandible of the male *Som. spectabilis* is too great a temptation for them. It was with great difficulty that we could induce them to bring these birds to camp without having them thus mutilated.

[*Continued.*]

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 Spark from Menlo Park.

To the Editor of Science:

My note book is so full of observations made during a recent visit to Edison's laboratory, that I feel on looking it over as if I had struck an intellectual gold mine. The genius of Menlo Park is so exuberant, and his frankness—we may say *naiveté*—so unbounded, that we came into possession of many facts which we might almost commit a breach of confidence in exposing. I found him reserved, however, when the conversation was turned to the subject of the arc electric light, and avoiding criticism of the operations and machines of those inventors who have devoted themselves to its improvement and utilization. But he made quite merry over the opinions expressed to him by many of the sight seers who swarm to the laboratory. "Would you believe it possible," said Mr. Edison, "that in spite of the general and interesting descriptions I have seen in various publications of this and other countries, few of the visitors really know what they come to see when they ask to be shown the electric light? Many are disappointed, because we do not have a kind of inland light house with a 300 or 400 candle-power light in each pane of glass in the buildings. Others think it a 'poor show' when they examine an incandescent thread of 14 to 16 candle-power in bright sunlight."

There was one suggestion thrown off by him, while conversing about the arc electric light, which I think should not be suffered to remai nundeveloped; Mr. Edison is so devoted to 'his light' that he only has time to give an occasional thought in the other direction, and his power of concentration prevents the dispersion of his genius through a different medium. So I repeat, I do not think I am committing any breach of confidence in describing a sketch which grew up under my eye, drawn by his rapid and luminous pencil; for Edison possesses that peculiar quality of pictorial illustration which we have never seen, except in the sketches of that inventor-artist, the great Leonardo da Vinci.

"Our dynamo-machines," said he, "as we now build them, are especially constructed for the purpose of furnishing current for the incandescent lamp; but they are, of course, as easily adapted to the arc light as to other purposes. You see our lamp factory and electric railroad are run by them. A very simple addition to a machine would allow of its use in illumination where the production of reverse currents is necessary. Imagine the wire of a Gramme helix cut half way through the solenoid, the four ends joined two and two to a commutating wheel, and pairs of conductors leading to an arc light, say Jablochkoff's candles. Now, by intermittently joining the ends of the separated helices, by an appropriate arrangement on the ordinary commutator blocks, you will be able to use your main current for the small incandescent lamps, and the surplus for the arc lamp; thus supplying continuous and reverse currents from the same machine."

I hope this chance scintilla from the mind of the great inventor will be allowed to sink through the pages of my note book into your columns, without any violation of the proprieties. If it incite Mr. Edison, *en revanche*, to a development of the idea, we will bear the brunt of a, perhaps, just resentment.

F. T. Waters.

SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

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SATURDAY, AUGUST 28, 1880.

The annual session of the American Association for the Advancement of Science has been most brilliantly opened in Boston. The intellectual force now concentrated there will soon be flowing through all the channels of knowledge. Our columns next week will contain our reporter's account of the proceedings, and will be enriched by an address, in full, of the distinguished retiring President, Professor George F. Barker, whose learning and devotion to Science alone placed him in that elevated position. We have also obtained valuable and interesting papers by Professors Agassiz, Hall of Washington, and other distinguished participants, which will duly appear.

Mr. Paget Higgs, the well-known English electrical engineer, now in Boston, has given his opinion, through the New York *Herald* (August 27), on the durability of electric motors and their actual return in work. As the general introduction of Edison's electrodyramo-machine is being anxiously looked for wherever a constant supply of cheap power is necessary, it becomes of the first importance to consumers to know how long the new engines will last. Mr. Higgs' positive statement of their length of life will no doubt confirm many small manufacturers in New York in their intention to profit by this convenient source of power, which, rumor says, will soon be generally placed at their disposal. Mr. Higgs has run some of the older and less perfect electro-motors since 1867, and finds them to-day in perfect condition. As the fruit of his own experiment and observation of the work of the most experienced European electricians, Mr. Higgs emphatically denies that there is any extraordinary loss in using them to communicate power at a distance.

We drew attention to an educational scheme which has been recently inaugurated at the Paris Observatory for the purpose of training young astronomers. It may be interesting in this connection to know that Professor Stone, of the Cincinnati Observatory, has for a number of years been quietly but successfully pursuing a plan in almost every respect identical with that more recently inaugurated in Paris. A small number of selected graduates are admitted as students at the Observatory, pursue a systematic course of study in theoretical and practical Astronomy, and upon its successful completion receive a post-graduate degree from the authorities of the University.

The course of study carried on at the Paris Observatory is described in Science, August 14th. If there are other Observatories in the United States offering the same facilities as those initiated by Professor Stone, we shall be glad to hear from those who can give authentic information.

We are not surprised that universal regret is expressed at the loss by the New York Fishery Commission of their annual appropriation. It appears to be acknowledged that the Commission was doing good work, and we trust their present difficulties are but temporary, and will be removed when the matter can be considered by the Legislature.

We think the Commissioners would strengthen their hands in efforts to obtain a renewal of their appropriation, if they gave some attention to the coarser kinds of fish, the supply of which appears to be practically unlimited at our very doors, and yet for unaccountable reasons is retailed at exorbitant prices, even averaging that of meat.

Fish is a natural food product for the poor of cities situated on the coast, but the dealers combine to make it an expensive luxury, by limiting the supply. We are even told that they destroy it, rather than effect sales below the prices they have arbitrarily fixed.

There appears to be little encouragement for the Legislature to grant appropriations to increase the supply of fish and lower its price, if the dealers in combination have finally the power to limit the supply and to create an artificial value.

As one of the New York Fishery Commissioners is himself one of those who are most largely interested in the sale of fish, his knowledge on the subject must be considerable, and he would certainly promote the interest of the Commission by assisting to remove the evil of which we complain. While it may be a good work to load the table of the epicure with choice fish, it should be more satisfactory to restore to the poorer classes an article of food which nature has supplied with such a bountiful hand.

ELECTRO-MOTORS.

THEIR POWER AND RETURN.

J. HOSPITALIER.

The transmission of force from a distance, electric ploughing, the electric railroad, etc., have made electric motors and the conditions of maximum work and maximum return, quite the order of the day. In a previous article on the available force in batteries, we have determined, for the most usual forms, the quantity of energy that could be furnished by a certain number of elements in an external circuit of proper resistance, supposing no polarization and without variation of the internal resistance.

Is this maximum of available work entirely convertible into effective work? It is not, and we will show how this maximum should be reduced when a given electric energy is to be transformed into mechanical force.

Let us suppose, for instance, in numbers, which always strike the attention more than formulas, that we have a source of electricity of 100 volts, with an internal resistance of 1 ohm. It would be easy to realize the conditions by employing an electro-dynamic machine, separately excited, or 100 very large Bunsen cups, arranged for tension in 2 parallel series of 50 each. Putting into the circuit an external resistance equal to the internal, and supposing no polarization to exist and no change in the internal resistance, we obtain as elements for the electric circulation:

E.—Electro motive force	=	100 volts.
r.— Internal resistance	=	1 ohm.
R.—Exterior resistance	=	1 ohm.
$(r + R)$—Total resistance	=	2 ohms.
Q.—Quantity $\frac{E}{r+R} = \frac{100}{1+1}$	=	50 webers.

In these conditions we know that we have in the external circuit the maximum of available work, as deduced from the formula of Joule:

$$W = 10\ Q^2R \text{ meg-ergs} \qquad (a)$$

$$\text{or } W = \frac{Q^2R}{9.81} \text{ kilogram-meters} \qquad (b)$$

In the case before us we have:

$$W = 10 \times 50^2 \times 1 = 25{,}000 \text{ meg-ergs} \quad (1)$$

What can we do with this available electric work? If we make it traverse an inert wire it will heat it. All the electric energy will be transformed into heat, and in this wire will be developed a certain number of calorics C, per second:

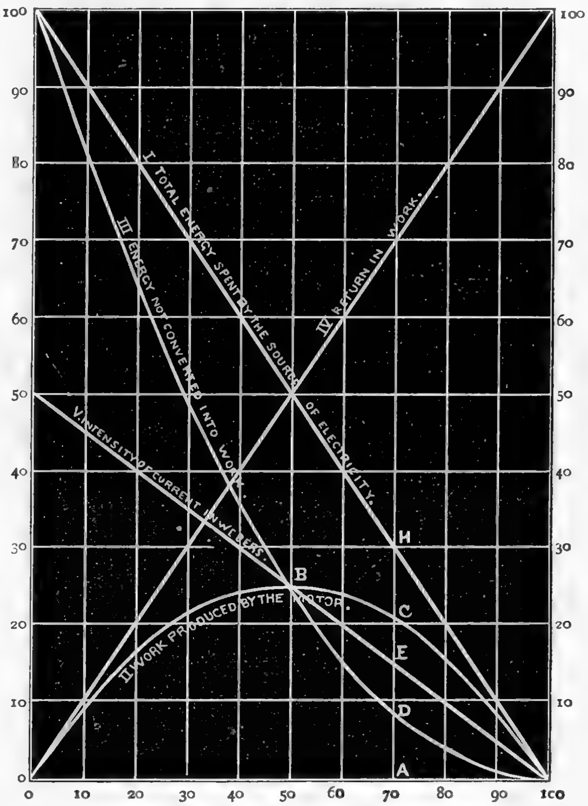
$$C = \frac{Q^2R}{9.81} \times \frac{1}{A}. \qquad (c)$$

A being the mechanical equivalent of heat 424.

Let us substitute for the inert resistance of a wire, an electro-motor of equal resistance with the wire, say 1 ohm in this particular case. Let us suppose this motor to be one of Gramme's magneto-electric machines, and that the resistance of the armature is equal to 1 ohm. If we put a break on the armature to prevent it turning under the influence of the passing current, we will not have any of the original conditions changed; the wire of the armature will be heated by the current, and a number of calorics C will be produced equal to that developed in the wire. Now let us make the armature turn under the action of the electric current. The rotary motion of this armature will develop a certain electro-motive force E′, inverse to that emanating from the source of electricity E, varying with the speed of the motor. It results in a diminution of the current, and can be expressed at each instant by the formula:

$$Q^1 = \frac{E - E'}{r + R}. \quad (d)$$

Hence the rotation of the motor diminishes the intensity of the current (and consequently the work of the motor) if a machine is employed as a source of electricity, or the consumption of zinc, if you employ a battery. The diagram shows how the different elements vary when the speed of the motor varies from zero (where the work developed is null) to a velocity such that the opposing electro-motive force E, which it develops, becomes equal to the electro-motive force of the source. It is seen that the energy expended by the source of electricity diminishes from the time the motor begins to turn (curve I.); similarly, the intensity of the current (curve V.) diminishes to zero when E and E′ become equal. Curve II. represents the work developed by the motor at different speeds. Let us suppose these speeds are proportional to the electro-motive forces—a hypothesis easily verified in a well constructed magneto-electric machine—then we see, by the diagram, an augmentation of the work produced, up to a point where the speed of the motor becomes 50. At this moment the work done is at a maximum, and represents but 50 per cent. of the work expended by the source of electricity. The energy converted into work (curve III.) is equal to what is unconverted (curve II.). If the speed augments beyond this point the work produced (curve II.) diminishes, but the return augments (curve IV.).



The work produced and the return are hence perfectly distinct things which are too often confounded. There is no impossibility in making the motor return 80 per cent. of the work expended by the source of electricity, on condition you do not make this source produce all the work which it can furnish. When, at the limit, the work produced becomes null, the return becomes equal to 1. The same conclusion is arrived at on comparing curves I. and II. It is thus seen that energy not converted into work, diminishes more rapidly than the total energy expended by the source of electricity. When the motor is at rest, the work is zero, all energy being transformed into heat. When $E' = \frac{E}{2}$, the diagram shows that the work is equal to the loss; curves II. and III. cut each other at B and the return is 50 per cent. Several consequences result from this. If you wish to obtain the *greatest* results from any given source of electricity, the electro-motor, turning at normal speed, must be so arranged as to develop a counter electro-motive force equal to the half of the original source. If the *best* results are wanted greater speed is required, by which a return in work is gained with a corresponding loss in the quantity of work produced.

Curves III. and IV. show why an electro-motor heats more when stopped than when turning at a certain speed; the intensity of the current is greater in the first case than in the second, the electro-energy not converted into work, diminishing with increase of speed, is converted into heat in the conducting wire. The two causes are correlative.

Let us cite a case having peculiar bearing on the transmission of power at a distance by electro motors, for instance, in electric traction on railways. Suppose our motor to turn at a normal speed developing a force of 70 volts. In this condition the work produced is represented (on the diagram) by A C, the work expended on the source of electric supply by A H, and the return is 0.70. If the existing work is augmented (by putting on a brake, for instance,) it will diminish the speed of the motor; but the curve II. shows that by this very diminuition of speed the work produced by the motor augments, and a new state of equilibrium is produced very close to the first. If, on the other hand, the resisting work diminishes, the speed will augment, and the work produced will diminish. Hence we see that the work of the motor augments with the resistance, and diminishes as well with it, a most favorable condition for regulating speed and maintaining it within certain bounds not far apart. This automatic governing is not to be found in any other motor. In the latter, special apparatus has to be called into play, as in the well-known case of steam.

This statement of the theoretical conditions affecting the functions of an electro-motor supplied from a given source, shows between what limits its different elements can be made to vary. The numbers which we have given for the maximum of work in batteries, as well as those given by M. Reynier in his work on the pile, have regard only to the total available energy in the external circuit, without consideration of the manner in which this energy is ultimately used. If, as in the above hypothetical case, it is desired to transform this energy into work of an electro motor, but half of the maximum work can be obtained. If, on the other hand, it is proposed to get the greatest *sum* of work in an indefinite time, the return can be augmented and collected up to as high as 80 and 90 per cent. of the energy represented by the expenditure of zinc in the battery, but then the pile does not produce its *maximum of work*.

The influences of the external resistances remain to be examined, such as are presented in transmitting force at a distance; also the resistance of the motor itself, and the practical returns obtained in certain special cases with motors of determinate type.

We will take occasion to recur to this subject after practical experience has had the last word. It is always well, however, to recall theoretical results, which never being altogether attained in practice, have an advantage in setting exact limits to our knowledge of what can be obtained from any given source of electrical supply; and. while destroying some illusions, proving some statements, which till now, have seemed too adventurous. (*La Lumière Electrique*, Aug. 7th.)

MULTIPLE SPECTRA.*

II.

I concluded my last article under the above heading with a reference to the case of carbon, and gave the results successively arrived at by Attfield, Morren, Watts, and others; these went to show that besides the line-spectrum of carbon mapped by Angström there exists a fluted spectrum of this substance.

Now comes my own personal connection with this matter.

In the year 1871,[1] I communicated to the Royal Society a paper in which the conclusion was drawn that the vapor of carbon was present in the solar atmosphere.

This conclusion was founded upon the reversal in the solar spectrum of a set of flutings in the ultra-violet.[2] The conclusion that these flutings were due to the vapor of carbon, and not to any compound of carbon, was founded upon experiments similar to those employed in the researches of Attfield and Watts, who showed that the other almost exactly similar sets of flutings in the visible part of the spectrum were seen when several different compounds of carbon were exposed to the action of heat and electricity. In my photographs the ultra violet flutings appeared under conditions in which carbon was the only constant, and it seemed therefore reasonable to assume that the flutings were due to carbon itself, and not to any compound of carbon, and this not alone from the previous work done in the special case of carbon, but from that which had shown that the fluted spectra of sulphur, nitrogen, and so forth, were really due to these "elementary" substances.

Professors Liveing and Dewar have recently on several occasions called this result in question. Professor Dewar, in a paper received by the Royal Society on January 8, 1880, writes as follows:

"The almost impossible problem of eliminating hydrogen from masses of carbon, such as can be employed in experiments of this kind, prove conclusively that the inference drawn by Mr. Lockyer, as to the elementary character of the so-called carbon spectrum from an examination of the arc in dry chlorine, cannot be regarded as satisfactory, *seeing that undoubtedly hydrogen was present in the carbon* used as the poles.

Subsequently, in a paper received by the Royal Society, on February 2, Messrs. Liveing and Dewar wrote as follows:

"Mr. Lockyer (*Proc. Roy. Soc.*, vol. xxvii. p. 308) has recently[3] obtained a photograph of the arc in chlorine, which shows the series of fluted bands in the ultra-violet, on the strength of which he throws over the conclusion of Angström and Thalèn, and draws inferences as to the existence of carbon vapor above the chromosphere in the coronal atmosphere of the sun, which, if true, would be contrary to all we know of the properties of carbon. We cannot help thinking that *these bands were due to the presence of a small quantity of nitrogen.*"

It will be seen that on January 8 Mr. Dewar alone attributed the flutings to a hydrocarbon, while on February 2 Mr. Dewar, associated with Mr. Liveing, attributed them to a nitrocarbon.

In fact in the latter paper Messrs. Liveing and Dewar published experiments on the spectra of various carbon compounds, and from their observations they have drawn the conclusion that the set of flutings which I have shown to be reversed in the solar spectrum is really due to *cyanogen*, and that certain other sets of flutings shown by Attfield and Watts to be due to carbon are really due to hydrocarbon.

As Messrs. Liveing and Dewar do not controvert the very definite conclusions arrived at by Attfield, Morren, Watts, and others, I can only presume that they took for granted that all the experimental work performed by these men of science was tainted by the presence of impurities, and that it was impossible to avoid them. I therefore thought it desirable to go over the ground again, modifying the experimental method so as to demonstrate the absence of impurities. Indeed I have started upon a research which will require some time to complete. Still, in the meantime, I have submitted to the notice of the Royal Society some results which I have obtained, which I think settle the whole question, and it is the more important to settle it as Messrs. Liveing and Dewar have already based upon their conclusions theoretical views which appear to me likely to mislead, and which I consider to have long been shown to be erroneous. To these results I shall now refer in this place.

The tube with which I have experimented is shown in Fig. 1: A and B are platinum wires for passing the spark inside the tube; E is a small tube into which carbon tetrachloride was introduced; it was drawn out to a long narrow orifice to prevent the rapid evaporation of the liquid during the exhaustion of the tube. The tube was bent upwards and a bulb blown at C in order that the spark might be examined with the tube end-on, as its found that after the spark has passed for some time a deposit is formed on the sides of the bulb immediately surrounding the platinums, thus obstructing the light. After a vacuum had been obtained the tube was allowed to remain on the Sprengel pump, to which it was attached by a mercury joint for the purpose of obtaining a vacuum for a long time, in order that the last traces of air and moisture might be expelled by the slow evaporation of the liquid.

* Continued from p. 29.

[1] *Proc. R. S.* No. 187, 1878.

[2] The approximate wave-length of the brightest member on the least refrangable edge is 3881.0.

[3] That is, in 1878.—J. N. L.

The carbon tetrachloride was prepared by Dr. Hodgkinson, who very kindly supplied me with sufficient for my experiments.

On passing the spark without the jar in this tube, the spectrum observed consists of those sets of flutings which, according to Messrs. Liveing and Dewar, are due to hydrocarbon, and the set of flutings which is reversed in the sun, and ascribed by Messrs. Liveing and Dewar to cyanogen, also appears in a photograph of the violet end of the spectrum, Fig. 2. On connecting a Leyden jar with the coil and then passing the spark the flutings almost entirely vanish and the line spectra of chlorine and carbon take the place of the flutings without either a line of hydrogen or a line of nitrogen being visible.

As a long experience has taught me that these tubes often leak slightly at the platinums after they are detached from the pump, so that the evidence of such a *pièce justificatif* is only good for a short time, I took the occasion afforded by

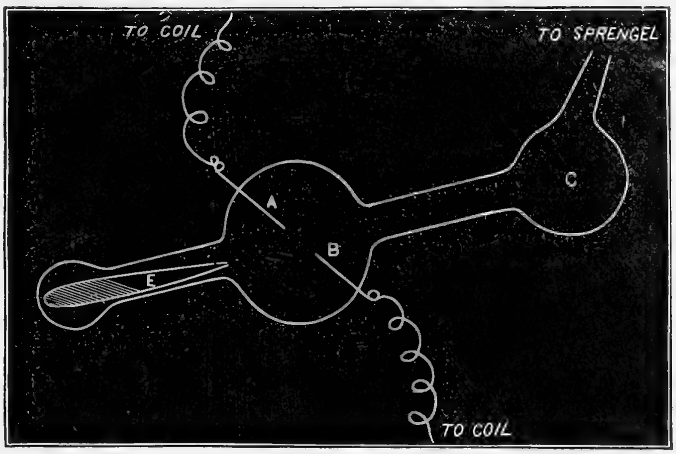


FIG. 1.

a visit of Dr. Schuster to my laboratory while the experiments wese being made to get my observations confirmed. He has been good enough to write me the following letter and to allow me to give it here:—

"*March* 21.

"MY DEAR LOCKYER.—The following is an account of the experiment which I saw performed in your laboratory on Monday, March 15:

"A tube containing carbon-tetrachloride was attached to the Sprengel pump. As exhaustion proceeded the air was gradually displaced by the vapor of the tetrachloride. The electrodes were a few millimetres apart. If the spark was

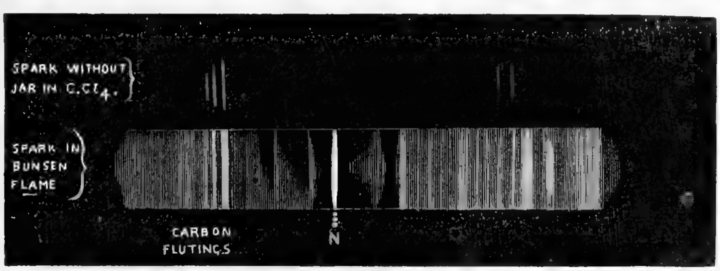


FIG. 2.

taken without a condenser in the vapour the well-known carbon bands first observed by Swan in the spectrum of a candle were seen with great brilliancy; I also saw the blue band which you said was identical in position with one of the blue bands seen in the flame of cyanogen or in the spectrum of the electric arc. When the condenser and air-break were introduced this spectrum gave way to a line spectrum in which I could recognize the lines of chlorine. *The lines of nitrogen were absent, not a trace of the principal double line in the green being seen.* The hydrogen line $H\alpha(C)$ was faintly visible when I first observed the spectrum, but it got gradually weaker and *finally disappeared altogether. When this line was no longer visible the condenser was taken out of circuit again, and the same carbon bands were seen as before.* These bands, therefore, show themselves with great brilliancy when a strong and powerful spark does not reveal the presence either of hydrogen or nitrogen. (Signed) ARTHUR SCHUSTER."

"March 21, 1880."

This result, which entirely endorses the work of Attfield and Watts, has been controlled by many other experiments. I have also repeated Morren's experiment and confirm it and I have also found that the undoubted spectrum of cyanogen is visible neither in the electric arc nor in the surrounding flame.

Hence then in the case of carbon, as in the prior cases of hydrogen, nitrogen and the like, those who hold that the flutings are due to impurities must, it would seem, abandon their position; for the flutings are undoubtedly produced by carbon vapor. Nor is this all; the suggestion that the various difficulties which have always been acknowledged to attend observations of this substance may in all probability be due to the fact that the sets of carbon flutings represent different molecular groupings of carbon, in addition to that or those which give us the line spectrum, and that the tension of the current used now brings one set of flutings into prominence and now another, seems also justified by the facts. This suggests the view that a body may have a fluted spectrum of compound origin as well as a line spectrum.

This conclusion is greatly strengthened by the preliminary discussion of a considerable number of photographs of the spectra of various carbon compounds.

A general comparison of the photographs first enables us to isolate the lines in the blue and ultra-violet portions of the spectrum (wave lengths 4300–3800) of the substance associated with the carbon in each case.

In this manner the lines seen in the photographs of the spectra of CCl_4, $C_{10}H_8$, CN, CHI_3, CS_2, CO_2, CO, &c., have been mapped, and both the common and special lines and flutings thus determined.

The phenomena seen with more or less constancy are a blue line, with a wave-length of 4266 ; a set of blue flutings, extending from 4215 to 4151 ; and another set of ultra-violet flutings, which extend from 3885 to 3843 (all approximate numbers).

In a photograph of the spectrum of the electric arc (with a weak battery) between carbon poles in an atmosphere of chlorine, the blue flutings alone are visible, whilst, when the *spark* is similarly photographed, the ultra-violet flutings and the blue line (4266) are also visible, whilst the blue flutings become fainter.

From this we may assume, in accordance with the working hypothesis of a series of different temperature furnaces, as set forth in the paper of December, 1878 (see Fig. 3), that the different flutings and the line correspond to different temperature spectra, the blue flutings to the lowest and the blue line to the highest temperature, whilst the ultra-violet flutings occupy an intermediate position.

According to this working hypothesis there should be the spectrum which contains the blue line alone and that which contains the blue fluting alone (Fig. 4). In comparing the spectra of carbon under different conditions, I find this to be true. *The blue line never appears in conjunction with the blue flutings, unless the ultra-violet flutings are also present.* In other words, the highest and the lowest hypothetical temperature spectra are never visible together without the spectrum of the intermediate hypothetical temperature.

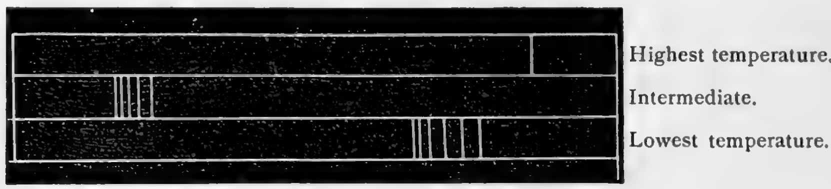


FIG. 3—Action of three different temperatures on a hypothetical substance, assuming three stage of complete dissociation.

But this is not all. By placing the spectra of the substances at different heat-levels, so to speak, I was enabled to construct a map, which not only indicates the mere presence or absence of the lines and their relative intensities, but shows a perfect gradation between the spectrum which contains the line alone and that which contains the blue flutings alone (Fig. 5). I would point out that there is nothing theoretical in this map. All the horizons depicted are copied from photographs of carbon under the conditions indicated, and theory has merely enabled me to arrange them *in order*.

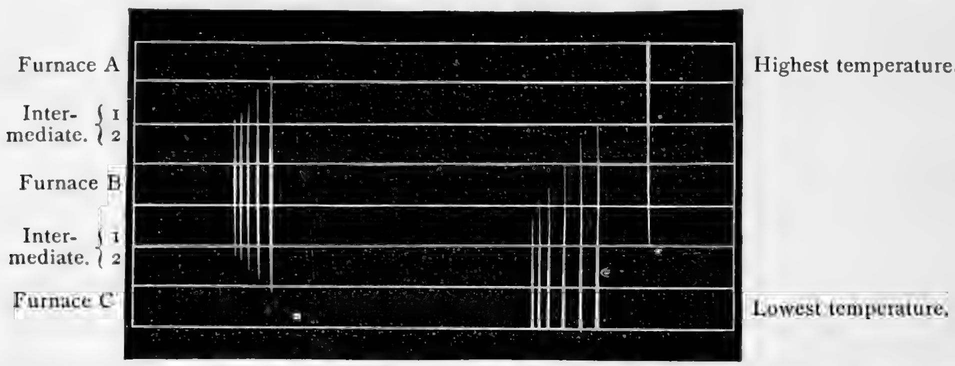


FIG. 4—Spectra of the hypothetical substance, in intermediate furnaces, assuming that the vapours are not completely dissociated.

This map I submit, therefore, bears out the hypothesis of differences of temperature indicated above, for it is seen a series of horizons forming a perfect gradation between that, while the blue line gradually thins out, the ultra-violet flutings appear first and grow in intensity. As these increase the blue flutings become visible, and further, as the

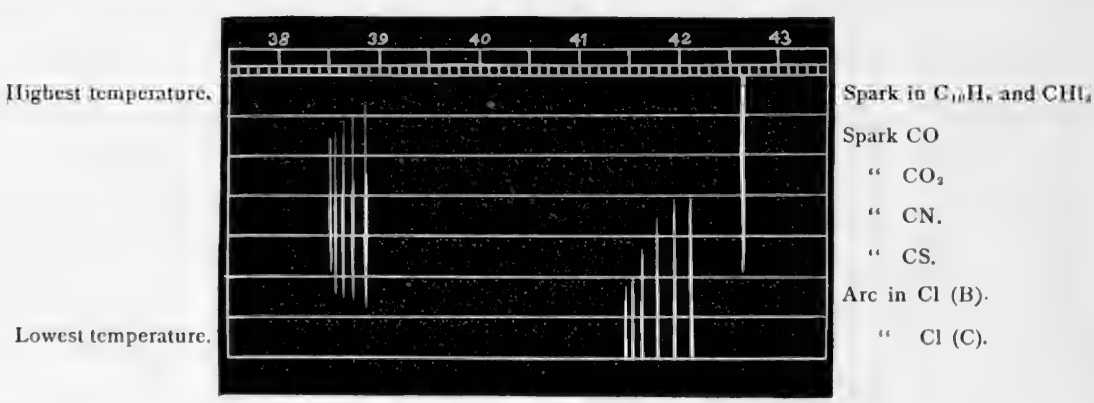


FIG. 5—The photographed spectra of some carbon compounds.

latter augments and the line disappears, the ultra-violet flutings gradually die out altogether.

It is philosophical to infer from these observations that not only are the line and flutings in question produced by carbon, but that the blue line (4266), since it is visible at the highest temperature, corresponds to the most simple molecular groupings we have reached in the experiments, and the flutings to others more complex.

The result to which attention is most to be directed in this place is that touching the two sets of flutings, and should future research justify the double conclusion (1) that these flutings are truly due to carbon, a result I accept, though it is denied by Angström and Thalèn; and (2) that the different flutings really represent the vibrations of different molecular groupings; a great step, and one in the direction of simplification, will have been gained.

Indeed it is much to be hoped that this ground will be at once worked over again by men of science who are both honest and competent: that the truth is sure to gain by such work is a truism.

I have so often taken occasion to refer with admiration to the work of Angström and Thalèn that I shall not be misunderstood when I say that their conclusions, to which such prominence is given, and on which such great stress is laid by Messrs. Liveing and Dewar, rest more upon theory and analogy than upon experiment.

Their work, undertaken at a time when the existence of so-called "double spectra" was not established upon the firm basis that it has now, and when there was no idea that the spectrum recorded for us the results of successive dissociations, gave, as I have previously taken occasion to state, the benefit of the doubt in favor of flutings being due to compounds, and it was thought less improbable that cyanogen or acetylene should have two spectra than that carbon or hydrogen should possess them.

Indeed, later researches have thrown doubt upon the view that the fluted spectra of aluminium and magnesium are entirely due to the oxides of those metals instead of to the metals themselves—and this is the very basis of the analogy which Angström and Thalèn employed.

The importance of the observations to which I have referred is all the greater because of the general conclusions touching other spectra which may be drawn from them. Thus from what I have shown it will be clear that if my view is correct, the conclusions drawn[1] by Messrs. Liveing and Dewar from the assumed hydrogen-carbon bands touching both the spectrum of magnesium and the spectra of comets, are entirely invalid. These conclusions are best given in their own words:—

"The similarity in the character of the magnesium-hydrogen spectrum, which we have described, to the green bands of the hydrocarbons is very striking. We have similar bright maxima of light, succeeded by long drawn-out series of fine lines, decreasing in intensity towards the more refrangible side. This peculiarity, common to both, impels the belief that it is a consequence of a similarity of constitution in the two cases, and that magnesium forms with hydrogen a compound analogous to acetylene. In this connection the very simple relation (2 : 1) between the atomic weights of magnesium and carbon is worthy of note, as well as the power which magnesium has, in common with carbon as it now appears, of combining directly with nitrogen. We may with some reason expect to find a magnesium-nitrogen spectrum. . . .

"The interest attaching to the question of the constitution of comets, especially since the discovery by Huggins that the spectra of various comets are all identical with the hydrocarbon spectrum, naturally leads to some speculation in connection with conclusions to which our experiments point. Provided we admit that materials of the comet contain ready-formed hydrocarbons, and that oxidation may take place, then the acetylene spectrum might be produced at comparatively low temperatures without any trace of the cyanogen spectrum or of metallic lines. If, on the other hand, we assume only the presence of uncombined carbon and hydrogen, we know that the acetylene spectrum can only be produced at a very high temperature, and if nitrogen were also present that we should have the cyanogen spectrum as well. Either, then, the first supposition is the true one, not disproving the presence of nitrogen, or else the atmosphere which the comet meets is hydrogen only, and contains no nitrogen."

[1] Paper read February 12, 1880.

The importance of the question here treated of comes out very well from these two extracts. We find the same spectral phenomenon at once called into court, and very properly called in, both to suggest the existence of chemical substances of which the chemist has never dreamt, and to explain the chemical nature of a large group of celestial bodies.[1]

There is little doubt that when a complete consensus of opinion is arrived at among the workers, other suggestions more far reaching still will be derived from the prosecution of these inquiries. For the present, however, the chief point to bear in point is that both in line-spectra and in fluted spectra we have indications which I think favor the view that in each case the origin is compound rather than simple.—*Nature.* J. NORMAN LOCKYER.

OBAN, *July* 20.

PHYSICAL NOTES.

FROM the above article we see that as far back as 1878, Mr. Lockyer communicated to the Royal Society a paper in which the conclusion was drawn that vapor of carbon was present in the solar atmosphere. This inference was founded upon experiments similar to those of Attfield and Watts, who showed that flutings are always present in different compounds of carbon exposed to the action of heat and electricity. This observation of Lockyer has been called in question by Liveling and Dewar, as they have found it an almost impossible problem to eliminate hydrogen from masses of carbon. This latter view has been long held by Edison, who, in a great number of experiments, some of which were participated in by Prof. Young, has found at the enormous heat developed by igniting a fine carbon thread $\frac{8}{1000}$ of an inch diameter, of high resistance, in air vacuum, until a light of 80 candles is reached, that only a carbon spectrum is given, until just a few seconds before the rupture of the loop, ***when a sharply defined hydrogen spectrum is observed.*** On the other hand, in an observation of the purified spectrum of carbon tetrachloride, Mr. Lockyer (*Nature*, August 5th) found only carbon appeared at high temperatures. It is an excellent index of the spirit of unbiased investigation in the author of (*Nature*, December, 1878) The Hypothesis that the so-called Elements are Compound Bodies, and still later, of the Universal Hydrogen Hypothesis, to learn from Mr. Lockyer that, both in line and fluted spectra, he thinks we have indications which favor the view that in each case the origin is compound rather than simple.

IN a communication from William Huggins, F.R.S., received June 16th, 1880, and published in the *American Journal of Science* for August, are embodied some observations on the nature of the spectrum of water, which may give rise to a question of priority. It appears that Dr. Huggins made a photograph of the flame of hydrogen burning in air, December 27, 1879, but did not publish the fact.

On June last, Messrs. Liveing and Dewar state, in a paper read before the Royal Society, that they have obtained a photograph of the ultra violet part of the spectrum of coal gas burning in oxygen, and in a note dated June 8th, they add that they have reason to believe that this remarkable spectrum is not due to any carbon compound, but to water. Professor Stokes (whose well-known monograph in *Phil. Trans.*, 1852, has furnished so much suggestive material for others to work upon in this very line), authorizes the statement that Dr. Huggins, in a let-

[1] With special reference to this last question, that of cometary spectra, one of acknowledged difficulty, I may perhaps be permitted to add here by way of note that the view I put forward some years ago touching the relation to this spectrum to that of the nebulæ has been lately strengthened by the observation that at a low temperature one of the brightest lines in the spectrum of iron is that coincident with the chief line in the nebula-spectrum.

ter bearing date 30th January, 1880, spoke of "a novel and interesting result," referring, probably, to the above-mentioned photograph. Since then, Dr. Huggins has taken a large number of photographs of the spectra of different flames, but only presents one (that of hydrogen) to the Royal Society. We regret this, both because of the loss to our general stock of science, in this unnecessary detention of the spectrum of carbon and its compounds, and because of the imminent probability of a repetition of these disagreeable questions of priority, as, on this side of the water (to the writer's knowledge), this particular subject is being eagerly studied under unique conditions.

The experiment of Dr. Huggins consists of first burning hydrogen *per se* in atmospheric oxygen, and then a mixture of oxygen with hydrogen in air. He finds the two spectra identical. For purposes of comparison, he very ingeniously photographs them on the same plate, in rapid succession, using the upper half of his spectroscope slit for the first, and the lower half for the second impression. As all the lines of both spectra fit each other exactly, without excess, it is evident that either represents the spectrum of water. The article referred to contains a partial spectrum, giving the characte ristic lines of wate.

PROF. J. TROWBRIDGE has recently studied the earth as a conductor of electricity and details some interesting experiments, and advances some bold speculations and prophecies in the *American Journal of Science* for August. In all the telephone circuits between Boston and Cambridge for a distance of about four miles, the ticking of the Observatory clock could be heard when transmitting time signals. This was attributed to the proximity of the telephone circuit wires to the time wires of the Observatory. Mathematical considerations, however, (Maxwell's Electricity and Magnetism, Vol. II., p. 209), will convince one that with telephones of the resistance usually employed, no inductive effect will be perceived between wires which run parallel to each other a foot apart for the distance of thirty or forty feet, even if ten-quart Bunsen cells be used. The transmission of these time signals is evidently not due to induction, but to tapping the earth, so to speak, at points which are not in the same potential. Running a wire five or six hundred feet long to ground at both ends, and putting a telephone in circuit, the ticking was distinctly heard when an exploration was made in an open field an eighth of a mile from the Observatory ; yet the same wire, under similar conditions, gave no sound when one mile away from the central line between the Observatory and the Boston office. With the boldness of a Gallileo, Professor Trowbridge deduces thence the theoretical possibility of telegraphing across the Atlantic without a cable. He says: "Powerful dynamo-electric machines could be placed at some point in Nova Scotia, having one end of their circuit grounded near them and the other end grounded in Florida, the conducting wire consisting of a wire of great conductivity and carefully insulated from the earth, except at the two grounds. By exploring the coast of France, two points on two surface lines not at the same potential could be found ; and by means of a telephone of low resistance, the Morse signals sent from Nova Scotia to Florida could be heard in France. Theoretically this is possible, but practically, with the light of our present knowledge, the expenditure of energy on the dynamo-electric engine would seem to be enormous."

A VERY curious observation has been made by M. J. Janssen of a remarkable inversion in a photographic image by exposure during different times. It passed from negative to positive with an intermediary neutral, invisible period. After a first exposure of $\frac{1}{1300}$ of a second a negative can be developed, a little longer exposure would dull the sharpness of the image ; then there soon arrives a point where the negative disappears entirely. By a still longer exposure a new phase occurs, a positive image starts out from the plate, with lights and shadows just the reverse of the first and as sharply defined. By allowing further action of the light a second neutral condition occurs. M. Janssen does not say by what state this is followed.—*Moniteur Sci.*

M. SCHEURER-KESTNER in a note to the Académic des Sciences, qualifies a previous statement that sulphuric acid attacks platinum, by new experiments. Absolutely pure sulphuric acid does not attack platinum, but if there be ever so small a content of nitrous acid, a very appreciable quantity of the vessel is dissolved, $\frac{1}{10000}$ being enough for the purpose. In one of his experiments, on 60 grams of sulphuric acid, two milligrams of platinum were dissolved. This fact should be verified by manufacturers of concencentrated sulphuric acid.

Mr. Albert Levy finds considerable variation in the ammoniacal contents of rain waters collected in the different quarters of Paris, but the annual means are identical. The per centages diminish from one month to the next, in passing from the cold to the hot season. The minimum at all stations was for the month of July, when there was present .93 of a milligram of nitrogen, against 1.35 in January. The potable waters of Paris are affected in exactly the same way. The reverse, however, is the case with the ammonia of the air which is most abundant in the hot season.—*Moniteur Scientifique*, Aug.

THE organisms described by Pasteur as the origin of epidemics and contagious disease, are so minute and few compared with the multiplying swarms of bacteria, etc., pervading all generating solutions, that it becomes necessary to provide a means of eliminating the masses of infusoria from solutions to be studied under the microscope. These microzoa haunt even the clearest drinking water at times, and it becomes highly important to easily determine their presence. M. Certes (Proceedings Acad. des Sciences), suggests the use of osmic acid as a sure means of killing them without destroying their tissues. He dips a glass rod into the solution to be examined and then into a 1½ per cent. solution of the acid ; washing this in a narrow test tube of distilled water, it is easy to collect what is necessary for examination. There are certain precautions to be taken as to cleanliness and time of immersion. By the use of a mixture of Paris violet in diluted glycerine, he finds it possible by uniform difference of tint, to easily distinguish cellulose, amylaceous matter and the vibrating cilia.

M. DE LESSEPS, as an argument against the quarantine system, read a letter to the French Academy of Science, from the engineer in charge of the preparatory work of the interoceanic canal, informing him that a number of persons had disembarked at the isthmus while sick of yellow fever, without having propagated the disease among the workmen. Following this communication of M. De Lesseps, M. Bouley said he could not allow the inference from such remarks to pass unchallenged. Admitting that what M. De Lesseps said was true, that quarantines are a constant inconvenience to commercial and maritime relations, yet this injury is in the highest degree compensated for by the guarantees given to the public health. Since the international sanitary police has been watching over Egypt, and preserving it from the invasion of cholera by strict quarantine, this disease had come to be less feared in Europe. It is by quarantine alone we shelter ourselves from those diseases which vessels so easily carry with them, particularly the yellow fever to which M. De Lesseps refers. The atmospheric conditions which he says render quarantines nugatory, cannot contribute to the propagation of epidemics, unless those who are attacked are allowed to land from the vessels which contain the germs. But these germs are not intangible exhalations, subtile vapors, effluvia which have a property of fatal expansion, against which we can do nothing. Quite the contrary is true. Thanks to the researches of experimental science, the principle of contagion is no longer unknown ; it has taken body and can be studied and followed in its manifestations. But even before this accession to our knowledge, practice, inspired by observation, had proved that strict surveillance of men and things coming from suspected countries would prevent the spreading of the germs. This is the province of the quarantine and by it alone can it be done. It is, then, necessary to maintain it in spite of the convenience to commercial and maritime relations.

OTTO A. MOSES.

SCIENCE:

A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.

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JOHN MICHELS, Editor.

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SATURDAY, SEPTEMBER 4, 1880.

THE ADVANCEMENT OF SCIENCE.

We cordially congratulate the managers of the American ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, on the very thorough success which has attended its twenty-ninth annual meeting, held last week at Boston.

We have in this issue devoted nearly the whole of our space to chronicling its proceedings, and we draw special attention to the masterly address of the retiring President, Professor George F. Barker, which we present in full.

The address of welcome delivered by the venerable Professor William B. Rogers, L. L. D., will also be read with interest; he traces the history of the Association from its cradle, when it was called the Association of American Naturalists and Geologists, to its high position at this moment, when, as he hopefully said, it may be even fairly on its way to overtake the BRITISH ASSOCIATION, which has a roll of membership of 3,500 persons, and an income of $12,500, and at the same time 1,000 life members.

The success of the present meeting, and the addition of nearly six hundred new members, would seem to warrant the most brilliant anticipations for the future of the Association; and if its members follow the excellent advice of Professor Rogers, and do whatever is in their power to "quicken scientific thought, to accumulate scientific facts and investigate scientific laws," and generally to advance science, the result must elevate this Association to a position second to no other in the civilized world.

We are also reminded by Professor Rogers that while the chief function of the Association is to advance the progress of science; the term advancement necessarily implies diffusion, it would, therefore, appear an appropriate moment to speak of the value of this Journal in this connection. In addition to our report in this issue the addresses of Professor Hall, of Washington, and Professor Agassiz will be published in full. Of the two hundred and eighty papers read before the Association, some will be published by us *verbatim*, commencing next week with that of Mr. Alexander Graham Bell on his new instrument, the Photophone, illustrated with twelve drawings, placed at our disposal by Mr. Bell; and of the other papers, we hope to give extracts of the most important.

If, then, the advancement of science necessarily implies its diffusion, we may, with justice, claim for this journal some credit in the great work, as Professor Rogers said, in sowing the seeds of science as widely as possible through the world, waking up in all quarters those latent spirits, whose inborn talent and tendencies will hereafter blossom and fructify in scientific results.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The twenty-ninth meeting of this Association met at Boston, Mass., on the 25th of last month, under the presidency of Professor Lewis H. Morgan, of Rochester, N. Y.

Professor George F. Barker having called the meeting to order, and introduced the President elect, the proceedings commenced by an address of welcome from Professor William B. Rogers, L. L. D., President of the Massachusetts Institute of Technology. After a few preliminary remarks, Professor Rogers continued as follows:

The American Association for the Advancement of Science has never yet held a meeting in this city of Franklin, and I may say, also, the city of Bowditch, not to mention the long line of other scientific worthies, prominent among whom is our great instructor, our adopted citizen, Louis Agassiz. It seems a fitting place for such an association to convene. Its spirit, its institutions, its history, its habits and sympathies, all favor such a reunion between its citizens and the advocates and votaries of science. It was my good fortune, if it is a good fortune of any man to be able to date back his life for a long period of years, to have been familiar with the cradle of this institution in the form in which it first presented itself as the Association of American Naturalists and Geologists. This, however, was not by any means the earliest congress of science assembled in the world. The origination of this thought of a parliamentary annual meeting of scientific men seems properly to belong to a great German philosopher and speculator (?), who as early as 1822 organized the German Association for the Advancement of Science. For eight or nine years this example was not followed, but in 1831 Brewster, aided by Brougham, established the great British Association for the Advancement of Science, which we are to regard as the parent institution from which we have sprung. This British association, meeting in the ancient city of York in 1831, had its annual assemblings for a series of years in all the great capitals and some of the secondary cities in Great Britain. Faithfully administering to the needs and stimulating the energies of scientific inquiry, and publishing its annual solid quarto, which is a library representing the progress of physical and natural science of that time comparable to any that can be presented on the shelves of any collection of books in the world. Now this British association is holding to-day its fiftieth annual meeting; and now, in the afternoon of its assembling, I can imagine clearly in my mind's eye some of those great dignitaries of science that are there assembled. I can think of Sir Joseph Hooker, of Sir William Thomson, of Huxley, of Tyndal, of Balfour Stewart, and of all the great worthies that illustrate physical, mathematical and natural science for the last generation; and as I look back on the records preceding the present year from the commencement of the association in Great Britain till this time, I find the chair of the presidency of that institution, as well as all the official characters connected with it, men who are or have been eminent for their promotion of scientific truths. I trust to-day before we shall have closed our assembling there will be transmitted by the cable a vote of greeting from the American Association of Science assembled here to the British association now assembled at Swansea.

Soon after this there came our American Association of Materialists and Geologists. I look around me and I think of the history of that active band of scientific workers, when all our State surveys were in their earlier states, when our geology, palcontology, our natural history in fact, in general was in a comparatively unexplored condition, and I feel saddened that I am the only member of the presidents of that early institution except one who has been, so far as intellectual laws are concerned, entirely removed from all association with scientific men. In the year 1847, during my presidency of this smaller institution, the plan was organized for a more extended and comprehensive form of social organization for the advancement of science; and in the year 1848, under the presidency of Mr. Redfield of New York, the first meeting of this enlarged association as it now exists was held in the city of Philadelphia. Since that time, consecutively year by year, this Association has assembled, save only during that dark period when, through most sad necessities, unfortunate circumstances and dreadful commissions, this association was compelled to hold its peace. But since 1865 the Association, with renewed vigor, has been prosecuting its work, and now we are assembled for the twenty-eighth time at an annual meeting to carry on this active labor of scientific instruction.

Now, what are the functions of such an Association? Its title tells. It is an association for the advancement of science, and it is expected and required of all those who become its members that they shall do whatever is in their power to quicken scientific thought, to accumulate scientific facts, to investigate scientific laws, or, in other words, to advance the progress of science throughout the world. But this term advancement necessarily implies diffusion, and while it is an association for the advancement of science it is no less an association for its diffusion, and this justifies in the highest degree the comparatively popular character of the meetings of the American Association. How can we best advance science but by sowing the seeds of science as widely as possible through the world, wakening up in all quarters where the association assembles those latent spirits, those unborn talents and tendencies which will hereafter blossom and fructify in scientific results. Thus it is, then, gentlemen, that we have our association assemblies here, and while I would not compare it as yet in point of numbers, in point of strength with the parent association in Great Britain, I see here to-day and hear from all quarters amongst those who are connected with the working operations of this meeting the enormous increase which is promised this association in its future growth. Let us think for a moment. For the last twenty years the British Association has had an average number on its rolls of members of all classes of 3,500; it has had an average attendance of nearly 2,500; it has had an average income from its members of $12,500, having at the same time 1,000 life members, and being able, practically and actively, to promote scientific research by the bestowal of grants for different departments of inquiry of a sum amounting of from $5,000 to $10,000 a year. Now, gentlemen of the Association and citizens of Boston, here is something for us to emulate. Here is a direction of progress in which we can be sustained by the strong and hearty approval, nay, the applause, of all scholarly and scientific men throughout the world. And, from what I have learned to-day, I do not doubt that the American Association of Science is fairly in the way to overtake the great association which is now assembled at Swansea, in regard to its numbers and its resources. And, as to the character of the works that are presented, of course in all such exercises the materials that are gathered together are of various qualities as well as shapes and dimensions. Let us now make it our special work to exclude from our annual reports all detailed publications which are not of a character actually to add to the stock of human knowledge, whether that knowledge be simply the gathering together of facts by careful processes of discernment, or the development of laws by careful mathematical investigation And, therefore, let it be our work, as I trust it will be, and has been already, in fact, suggested by our secretary, that these prolonged discussions, which, however valuable in the main they may be or not of the quality and character to belong to the transactions of a great body like this, shall be presented in small type and in abstract in the latter part of the volume.

I thank my friends for the patience with which they have listened to one who does not like to call himself an old man, but who still finds something of the sentiment of the war-worn soldier who likes always, if he have a kindly audience, to shoulder his crutch and fight his battles o'er again. [Applause.] If I have taken too much of your time I beg your pardon. As I have spoken in behalf of this committee of the city of Boston, let me conclude with my personal welcome in behalf of this institution, over which I have the honor to preside, and to say to you that the corporation and officers of the Institute of Technology are not only glad but they are proud to welcome the American Association for the Advancement of Science into this hall and to all the accommodations and comforts which it can offer.

The Mayor of Boston, the Hon. Frederick O. Prince, then addressed a few words of welcome, and was followed by His Excellency Governor Long, who delivered an additional address for the same purpose.

The response of President Morgan, on behalf of the Association, was as follows:

MR. CHAIRMAN:—The Association has listened with much pleasure to your address of welcome to the city of Boston. In no other city of our land are better appreciated the unity of the sciences and the brotherhood of scientific men. These are central ideas of this Association, and when we meet among a people whose hospitality is vitalized by intelligent sympathy, a powerful impulse is given to the work which it was designed to promote. I venture to predict, sir, that this meeting will become memorable in our history. It may seem singular that this session of the Association should be the first one held in the good city of Boston, during the long series of twenty-nine annual meetings. It has, however, met at Cambridge, which in the public eye is a part of Boston. We cannot and we ought not to separate Cambridge, with its noble university and its distinguished body of teachers, from Boston, in which the roots of Cambridge are planted. They are "one and inseparable" in association as in fame. Thus we are enabled to say that this Association is indebted to Boston for a peerless cluster of presidents: The illustrious and lamented Agassiz, to whom American science is so deeply indebted; the learned and gentle Wyman, whose loss we still mourn; these have ceased from among us, and their departure has rounded and completed their fame. Rogers, Peirce, Gould, Gray, Lovering yet remain with us, and, therefore, we cannot on this occasion speak of them as their distinction deserves. "*Seri in cœlum redeatis.*"

Mr. Mayor:—The American Association for the Advancement of Science is popular in its character, as it should be. Investigators in all departments of science are cordially welcomed to its membership. By this free intercourse of persons engaged in scientific pursuits, results of the highest importance are constantly attained. When the meetings of this Association become indifferent to the communities among which they are held, its usefulness will be near its end. There is a direct connection between the work upon which its members are engaged and the material prosperity of the country, in which all alike have an interest. Scientific investigations ascertain and establish principles which inventive genius then utilizes for the common benefit. We cannot have a great nation without a great development of the industrial arts, and this, in its turn, depends upon the results of scientific discovery as necessary antecedents. Material development, therefore, is intimately related to progress in science.

Your Excellency, Governor of the Commonwealth of Massachusetts:—Without intending to depart from the proprieties of the occasion, it may be proper to say, that

those of us who come from beyond the Hudson can but feel that in entering New England we reach the birthplace of American institutions. To some of us it is the land of our fathers, and we cannot approach the precincts of their departed presence without the sentiment of filial veneration. Here they laid, broad and deep, the foundations of American freedom, without which American science would have been an infant in leading strings to-day.

Mr. Chairman and Gentlemen :—With a grateful appreciation of the kindness of the people of Boston, the Association is now prepared to enter upon the regular work of the session.

GENERAL BUSINESS.

The association then proceeded to routine business. The permanent secretary gave notice that the following members of the assocation had died since the last meeting, viz.:

George W. Abbe, of New York, died September 25, 1879.
E. B. Andrews, Lancaster, Ohio.
Homer C. Blake, New York.
F. A. Cairns, New York.
Caleb Cook, Salem, Mass., died June 5, 1880.
Benjamin F. Mudge, Manhattan, Kansas, died November 21, 1879.
Thomas Nicholson, New Orleans, La.
Louis François de Pourtales, Cambridge, Mass., died July 18, 1880.

The financial report, presented by the secretary, showed for the first time since he has been in office a balance in favor of the sssociation. The total receipts during the year were $5430.35, principally from assessments and entrance fees. The disbursements were : Expenses of the Saratoga meeting, $189.82 ; publication of 1250 copies of proceedings of Saratoga meeting, $2142.64 ; salaries of permanent and assistanf secretaries, $1396. The balance in hand was $148.-24. The life membership fund amounted to $975.77.

The standing committee was then completed, and includes, besides the officers of the association, the following fellows : N. T. Lupton, F. W. Clarke, E. T. Cox, W. Harkness, O. T. Mason and S. A. Lattimore.

On motion from the floor, a standing committee was appointed by the president to prepare a message of greeting to the British Association, to be sent by cable. Professor W. B. Rogers, Asa Gray and N. T. Lapton were appointed, and sent the following despatch : "The American Association for the Advancement of Science, in session in Boston, sends cordial greetings to the British Association at Swansea, on the occasion of its fiftieth meeting."

The president was requested to appoint a committee of three to propose suitable resolutions of regret at the death of the late General Albert J. Moyer, of the United States Signal Service.

Over four hundred ladies and gentlemen were elected members of the association.

It was voted that, with the exception of Thursday, the morning session begin at ten o'clock and close at one o'clock ; and that the afternoon session begin at 2:30 o'clock and close at five o'clock. The general session then adjourned.

The Sections then organized. Section A was called to order in Huntington Hall. Professors A. W. Wright, A. M. Mayer and John Trowbridge were elected the committee to coöperate with the Vice President and Secretary of the Section, and the Chairmen and Secretaries of the Sub-Sections. F. H. Smith, A. E. Dolbear, J. M. Van Vleck and Thomas Hill were chosen on the nominating committee, which acts with the standing committee in the selection of officers for next year. The Section then adjourned. Alexander Agassiz presided at the meeting of Section B. G. L. Goodale, E. D. Cope and B. G. Wilder were chosen the sectional committee, and C. S. Minot, A. J. Cook, W. G. Farlow and Thomas Mahon, nominating committee. On motion of Dr. Minot, it was voted to form a Permanent Section of Biology. The Section then adjourned to Friday. In the afternoon Mr. Asaph Hall gave the Vice President's address of Section A at half-past two ; Professor J. M. Ordway read the Chairman's address to the Sub-Section of Chemistry at four ; at the same time Major J. W. Powell pronounced the Chairman's address before the Anthropological Section, while the official address in microscopy was admitted. The Entomological Club met at five o'clock, Mr. A. R. Grote in the chair. A communication from W. H. Edwards was presented ; Mr. McCook concluded his comment on the honey ant ; Mr. A. J. Cook offered some comment ; Mr. E. P. Austin exhibited plates ; an essay from S. A. Forbes was read, and Dr. G. F. Waters discussed it. In the evening the retiring President pronounced his great oration on life as a problem of chemistry and physics.

THURSDAY, AUGUST 27TH.

The second day of the meeting was spent by the American Association in Cambridge. At eleven o'clock an audience of nine hundred assembled in the Sanders Theatre to listen to the eulogy by Professor Alfred M. Mayer upon the late Joseph Henry, and to the annual address by Professor A. Agassiz before the natural history section. The audience included nearly all the members of the Association registered this year, with the addition of a large number from Cambridge. The Harvard professors are usually absent during the summer vacation, but on this occasion nearly the entire scientific faculty were present to receive and honor their friends and guests. At the short business meeting of the general session twenty new members were admitted to the Association, and the following resolution, offered by Dr. L. C. Le Conte, referred to a standing committee : " Resolved, that the constitution and by-laws be so amended as to establish a Section C of biology, with an organization similar to that of the two existing sections." After the addresses at the theatre dinner was served in Memorial hall, Mr. Martin Brimmer presiding, but made no remarks and gave no toasts. After dinner the ladies and gentlemen visited the scientific collections, especially the two museums, the mineralogical cabinet, the physical laboratory, the library and the historic points of Cambridge. At four o'clock the visitors gathered in about equal numbers at the botanic garden, the observatory and the house of Mrs. T. P. James. At the garden Professor Asa Gray spoke on the characteristics and distribution of the Rocky Mountain vegetation. Professor E. C. Pickering, the director, offered an opportunity for inspecting the observatory, while Mrs. James entertained those interested in keramics. In the evening there was a reception at Mr. and Mrs. A. Graham Bell's residence.

FRIDAY, AUGUST 28TH.

Little routine was required to be transacted, and the sections and sub-sections settled down to steady work. It was announced that so far nine hundred ladies and gentlemen had entered their names for membership, and that the attendance was a hundred-fold more than was usually present on former occasions.

Among the more important papers read were :

"Determination of the routine time of Jupiter, from observations of the red spot in 1879-80 ; together with the physical character and changes of the spot," by H. S. Pritchett.

"Determination of the comparative dimensions of the ultimate molecules, and deduction of the specific properties of substances," W. N. Norton.

"Friction of lubricating oils," C. J. Woodbury.

"Steady and vortex motions in vis-cous incompressible fluids," Thomas Craig.

"Spectroscopic notes," C. A. Young.

"Discussion of the phenomena observed in comparing the spectrum of the light from the limbs with that from the centre of the solar disk," C. S. Hastings.

"Maxima and minima tide predicting machine," W. Ferrel.

"Methods in use at the Observatory at Yale for the verification of thermometers and testing of time pieces," Leonard Waldo.

"Heat produced by magnetizing and demagnetizing iron and steel," John Trowbridge.

"Lecture experiments for the direct determination of the velocity of sound," W. A. Anthony.

"On the refractive index of metallic silver," Arthur W. Wright.

"On a form of vacuum tube for spectroscopic works," Arthur W. Wright.

"Progress made at the Observatory of Harvard College in the determination of the absolute coördinates of 109 fundumental stars ;" "A simple and expeditious method of investigating all the division errors of a meridian circle ;" "The systematic errors of the Greenwich right ascensions of southern stars observed between 1816 and 1831 ; " Preliminany determination of the equation between the British imperial standard yard and the metre of the archives ;" "The probable error of a single observation at sea, deduced from the observations of W. H. Bacon, Cunard steamer Scythia," all by W. A. Rogers.

CHEMISTRY.

Rotary power of glucose and grape sugar—H. W. Wiley.
Actinism—A. R. Leeds.
The occurrence of oxide of antimony in extensive lodes in Senora—Mexico.
Convenient scale and apparatus in gas analysis—E. W. Morley.
On the constitution of tartrates of antimony—F. W. Clarke.
Action of sunlight on glass—Thomas Gaffield.
Near ratio of oxygen to nitrogen in the atmosphere—E. W. Morley.

SUBSECTION MICROSCOPY.

"Microscopic studies in Central Florida," C. C. Merriam; "The errors of a few English, French and American stage micrometers," William A. Rogers; "Apparatus used in photographing microscopical objects." Samuel Wells; "A new freezing microtome," William Hailes; "Microscopical investigations of the Havana yellow fever," George M. Sternberg; "Permanent microscopic preparations of Amphibian blood corpuscles," S. H. Gage; "Permanent microscopic preparations of Plasmodium," S. H. Gage.

BIOLOGY.

"Comparative anatomy as a part of the medical curriculum," Harrison Allen; "Distinguishing species of Populus and Juglans by the young naked branches," W. J. Beal; "Observations on Japanese Brachiopoda," E. S. Morse; "An investigation of the peach yellows," B. D. Halsted; "Incomplete adaptation as illustrated by the history of sex in plants," L. F. Ward; "Evolution of parasitic plants," Thomas Meehan; "Anthrax of fruit trees, or the so-called fire-blight of the pear and twig-blight of the apple tree," T. J. Burrill; "Further notes on the pollination of Yucca, and on Pronuba and Prodoxus," C. V. Riley; "Fossil Dinocerata in the E. M. Museum at Princeton, N. J.," F. C. Hill; "Origin and Succession of Felidæ," E. D. Cope; "Preservation of fossil insects and plants at Malon Creek," J. W. Pike; "Menobranchus lateralis," P. R. Hoy.

GEOLOGY.

Before the geologists were presented ten essays: "The Cupriferous series in Minnesota," N. H. Winchell; "The excavation of the upper basin and clove of the Kaaterskill, Catskill Mountains, N. Y.," Alexis A. Julian; "Progress of geological investigation in New Brunswick, 1870–1880," L. W. Bailey; "The tertiary age of the iron ores of the lower silurian limestone valleys," H. C. Lewis; "Note on the Turquoise localities of Los Cerillos," B. Silliman; "Los Cerillos, New Mexico, an area of recent eruptive rocks with mineral veins," B. Silliman; "Iron mines of Ore Hill, Conn., and vicinity, and the making of pig iron," W. A. Stearns; "Law of land forming on our globe," Richard Owen; "Kames and eskors in Maine," George H. Stone; "Occurrence of tin ore at Winslow, Me.," C. H. Hitchcock.

ANTHROPOLOGY.

The anthropologists met to listen to the following essays: "Ethnology of Africa, illustrated by a large manuscript map," A. S. Bickmore; "Myths and folk lore of the Iroquois," Erminnie A. Smith; "Prehistoric altars of Whiteside county, Illinois," W. C. Holbrook; "Theory of primitive democracy in the Alps," D. W. Ross; "Ancient mounds in the vicinity of Naples, Illinois, Pt. II. Illustrated with skulls, pipes, copper axes, bone implements and other articles from the mounds," J. G. Henderson; "The mounds of Illinois," William McAdams; "Prehistoric and early types of Japanese pottery," E. S. Morse.

In the evening Mr. Alexander Graham Bell brought before the Association his recent discovery of the Photophone, and researches with Mr. Sumner Tainter in the production and reproduction of sound by means of light.

SATURDAY, AUGUST 28TH.

In general session a few new members were elected, and on motion of Professor Ormond Stone, the standing committee of the Association was instructed to refer the subject of standard time to a special committee. In section A a sub-section H of mathematics and astronomy was organized with Mr. Simon Newcomb, of Washington, as chairman, and Mr. Winslow Upton, of Washington, as Secretary.

The following papers were also read in the various sections and bi-sections.

PHYSICS.

"On the present condition of musical pitch in Boston and vicinity," Charles R. Cross and William T. Miller; "The Co-efficient of expansion of gas solutions," from the Messrs. E. L. Nichols and A. W. Wheeler, and "The new action of magnetism on a permanent electric current," by Mr. E. H. Hall; the latter being among the most important papers, theoretically considered, ever contributed by an American to the science of physics. Then came "A simple device for projecting vibrations of a liquid film without a lens," by H. S. Carhart; "Observations on some recent hailstorms in North Carolina," by J. R. Blake; and "Results of a magnetic survey of Missouri," by Francis E. Nipher.

MATHEMATICS AND ASTRONOMY.

"The solar parallax for meridian observation of Mars in 1877," by J. R. Eastman; "A note on zodiacal light," by H. C. Lewis, and a "Tidal theory of the forms of comets."

CHEMISTRY.

"On a solution of ferric gallate and ferric oxalate as a reagent for quantitive analysis of ammonia," N. B. Webster.
"Description of new substituted acrylic acids," C. F. Malberry.
"The valuation of indigo," L. M. Norton.
"The soil supply of nitrogen for plants," W. O. Atwater.
"Incrustations formed in pipes used in gas wells," H. L. Nason.
"A modification of Bertier's process for the valuation of coal," Charles E. Monroe.
"Observation on the temperature and chemical character of Mystic Lake, Mass," W. R. Nichols.

MICROSCOPY.

"On the limits of visibility with the microscope," A. E. Dolhear.
"Minute anatomy of the human Larnyx," Carl Seiler.
"Infusoria found in fresh ponds," S. P. Sharples.

NATURAL HISTORY.

"Endo-cranium and the maxillary suspensorium of the bee," G. Macloskie.
"Tongue in snakes and birds," C. S. Minot.
The age of the copper bearing rocks of Lake Superior, M. E. Wadsworth.
Structure and nomenclature of the brain, with special reference to that of the cat, Burt. G. Wilder (three papers).
Plan of the cerebro-spinal nervous system, S. V. Clevenger.

ANTHROPOLOGY.

Aboriginal pottery and stone implements, S. S. Holdeman.
Rude argillite implements, C. C. Abbott.
The Dacotah tribes, H. B. Carrujton.
Discoveries in the Mammoth, Wyandot and Luray caves, H. C. Hovey.

We propose to offer the readers of SCIENCE verbatim reports of the principal addresses, and lengthy abstracts of the leading papers, read before this important meeting of the Association, and will commence the series with that of the

ADDRESS OF PROFESSOR GEORGE F. BARKER, THE RETIRING PRESIDENT OF THE ASSOCIATION.

SOME MODERN ASPECTS OF THE LIFE QUESTION.

The number of roots in our equation of life increases the difficulty of solving it, but by no means permits the acceptance of the lazy assumption that it is altogether insoluble or reduces a sagacious guess to the level of the prophecy of a quack.—HAUGHTON.

LADIES AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The discovery of new truth is the grand object of scientific work. The exultation of feeling which comes from the possession of a fact, which, now, for the first time, he makes known to men, must ever be the reward of the scientific worker. As investigators and as students of science we are met here to-day at this our annnual session. Each of us during the past year has been endeavoring to push outward further into the unknown, the boundary of present knowledge. When, therefore, we thus meet together it is fitting that, from time to time, our attention should be called to the progress which has been made along some one of the various lines of research, and to the milestones which mark the epoch of advance along the way which science has traveled. Moreover, we may profitably sum up at such times the work done in particular directions, and encourage ourselves with prospective and retrospective glances. In these summings up, however, a difficulty arises. The range of modern scientific thought includes an immense area. The field of knowledge is already so vast, that, seen from the vertical distance necessary to make a wide survey, that small portion of it which is familiar to any one individual is scarcely visible. In consequence, to use a mechanical figure, the solid contents of a man's acquirements being given, the depth thereof is inversely as the area covered. He, therefore, who undertakes to speak even for one single department of science distributes his stock of knowledge over so broad a surface that in places it must be dangerously thin. It is, therefore, with a very keen sense of the temerity involved in the undertaking, that I ask your attention during the hour allotted me, to some points which appear to me to have been recently gained in the discussion of the question of life.

My friend and predecessor, Professor Marsh, opened his excellent address at Saratoga with the question "What is Life?" In a somewhat different sense I too ask the same question. But I fear it is only to echo his reply, "the answer is not yet." The result, however, cannot long be doubtful. "A thousand earnest seekers after truth seem to be slowly approaching a solution." And though the *ignis fatuus* of life still dances over the bogs of our misty knowledge, yet its true character cannot finally elude our investigation. The progress already made has hemmed it in on every side; and the province within which exclusively vital acts are now performed, narrows with each year of scientific research.

What now are we to understand by the word "Life" in this discussion? A noteworthy parallel is disclosed in the progress of human knowledge between the ideas of life and of force. Both conceptions have advanced, though not with equal rapidity, from a stage of complete separability from matter to one of complete inseparability. Life is now universally regarded as a phenomenon of matter, and hence of course, as having no separate existence. But there still exists a certain vagueness in the meaning of the term "life." Two distinct senses of this word are in use; the one metaphysical, the other physiological. The former, synonomous with mind and soul, at least in the higher animals, has been evolved from human consciousness; the latter has arisen from a more or less careful investigation of the phenomena of living beings. It need scarcely be said that it is in the sense last mentioned that the word "life" is used in science. The conception represents simply the sum of the phenomena exhibited by a living being.

Moreover, the progress which has been made in the solution of the life-question has been gained chiefly by investigation of special functions. But the functions of a vital organism are themselves vital. What then is the meaning of "vital" as applied to a function? Fortunately the answer is not difficult. "Life," says Küss, the distinguished Strasbourg physiologist, "is all that cannot be explained by chemistry or physics." Guided by such a definition the work of the physiological investigator is simple. He has only to test each separate operation which he finds going on in the organism and to declare whether it be chemical or physical. If it be either, then since each function is non-vital, the entire organism must be non-vital also. Hundreds of able investigators, provided with the most effective appliances of research, are now in full cry after the life principle. Naturally, a vast amount of collateral knowledge is accumulated in the process. The quantitative as well as the qualitative relations of things are fixed, and many important facts are collected.

With the object in view thus clearly defined, we are not surprised that great progress has been made. A vital process, like the catalytic ones of the older chemistry, was found by such research to be simply a process which, for want of sufficient investigation, is not yet understood. While therefore, undoubtedly, much work yet remains to be done in the realm still called vital, the prophetic vision is already bright which will witness the last traces of inexplicable phenomena vanish and the words expressing them relegated to the limbo of the obsolete.

As a first result of recent work, the living organism has been brought absolutely within the action of the law of the Conservation of Energy. Whether it be plant or animal, the whole of its energy must come from without itself being either absorbed directly or stored up in the food. An animal, like a machine, only transforms its energy. Lavoisier's guinea-pig, placed in the calorimeter, gave as accurate a heat-return for the energy it had absorbed in its food, as any thermic engine would have done. But the parallel goes further. The mechanical work of an engine is measured by the loss of its heat and not of its substance. So the mechanical or intellectual work of a living being is measured by the amount of food rather than the amount of tissue which is burned. The energy evolved daily by the human body would raise it to a height of about six miles.

But beside heat, work may be the out come of the organism; and this through the agency of the muscles. Their absolute obedience to mechanical law in their mode of action has been admirably established by Haughton. The work a muscle does, it does in contracting. It is to the mechanism of muscle-contraction that we are indebted for another illustration of our subject.

When work is done by a muscle in contracting, three changes are observed to take place in its tissue. First, there is a loss of its electric tension; second, there is an evolution of heat in it; and third, carbon dioxide appears there, and its reaction, before neutral, becomes acid.

Matteucci was the first to observe and to call attention to the remarkable similarity in structure and in the mechanism of operation, between striated muscular fibre and the electric organ of certain fishes. Recently, Marey has repeated and extended his observations. In structure, the electric organ is made up, like the muscle, of columnar masses each separable transversely into vesicular sections. In a torpedo weighing seventy-three pounds, there were 1182 of these columns, with 150 sections, on an average, in each, In the muscles which bend the fore-arm, there are 798,000 fibrillæ. As to the mechanism, alike in muscle and in electric organ, an electric current stimulates action on opening and on closing the circuit, but not when it is flowing; the same phenomena takes place in both with the direct and with the inverse current; both are reflex; stimulation of the electric nerve produces discharge, as that of the motor nerve causes muscular shock; an entire paralysis follows nerve-section; curare paralyzes both; and tetanus results in both from rapid currents or from strychnine.

Still more striking analogies are furnished by the investigation of the susurrus or muscular sound, first noticed in 1809 by Wollaston. This sound is produced by all muscles when in the state of contraction, the pitch of the note being not far from thirty vibrations per second. It is evidently only the intermittent discharge of the muscular fibre. A single excitation produces a muscular shock. As this production requires from eight to ten hundredths of a second, it is evident that if another stimulus be applied before the first has disappeared the two will coalesce; and when twenty per second reach the muscle it becomes permanently contracted or tetanized. By means of a very sensitive myograph, Marey has found that in voluntary contraction the motor nerves are the seats of successive acts, each of which produces an excitation of the muscle. In 1877, Marey examined similarly the discharge of the torpedo and found a most complete correspondence between it and muscular contraction. Since electric tension disappears from a muscle during contraction, is not the evidence conclusive that muscular contraction, like the discharge of the electric organ of the torpedo, is an electrical phenomenon?

Granting electric discharges to be the cause of muscle-contraction, what is its origin? That it is not carried to the muscle by the nerves follows from the fact that a muscle will still contract when deprived of all its nerve-fibres. It must therefore be generated within the muscle itself. To reach a solution of the problem we must obviously follow the analogies of its production elsewhere.

Perhaps no single question in physics has been more keenly discussed than this one of the origin of electric charge. The memorable conflict between Galvani and Volta, between animal electricity and the electricity of metallic contact, succeeded by the even more triumphant overthrow of the latter and the establishment ultimately by Faraday, of the electrochemical theory: these are facts fresh in all our memories. The justice of time however in this case, if it has been tardy, has been none the less sure. The experiments of Thomson have vindicated Volta and established the contact theory as a *vera causa.* And more curiously still, it now appears to be proved that both contact and chemical action underlie the production of that very animal electricity so stoutly battled for by Galvani and his associates.

Volta's experiments to prove that a difference of potential is developed by the contact of two heterogeneous metals were not crucial. But Thomson, repeating them with the aid of more delicate apparatus, has shown that whenever copper and zinc are brought in contact, the copper becomes negative to the zinc. In proof that the chemical action of atmospheric moisture was not the cause of the phenomenon, he showed that when a drop of water served to connect the copper and the zinc, no charge at all was produced. The fact may therefore be regarded as established, as the result of numerous and varied experiments, that a difference of electrical potential is always developed at the surfaces of contact of heterogeneous media. Not only is this true of solids in contact with solids, but also of solids with liquids, and of liquids in contact with each other. Of course the production of electricity by contact must result from a loss of energy elsewhere. In the opinion of Cumming, it is the loss of energy which is owing to the unsymmetrical swinging of the molecules on the two sides of the surfaces of contact, which reappears as difference of potential between the solids or as the energy of electrical separation.

But we may carry the sequence yet another step backward. The energy which is thus lost at the surfaces of separation must be heat, and this junction must be cooled thereby. Thus the production of thermo-electricity is seen only to be a special case of a general law, a view to which the well-known Peltier effect gives support. In this phenomenon, when two metals are joined together in the form of a ring and one junction is heated, a current is produced which cools the other junction. From a study of these conditions, Thomson has concluded that the absorption of heat in a thermo-electric circuit varies for different metals with the direction of the current. Thus in iron, the current from hot to cold absorbs heat, while in copper the current which absorbs heat is from cold to hot. In entire accordance with these results, are the conclusions recently reached by Hoorweg. Whenever two conductors come into contact, motion of heat results in the development of electricity, the current produced existing at the cost of heat at one part of the point of contact, and evolving heat at the other for a result. Hence all voltaic currents are thermo-currents.

To return to the muscle, it must now be apparent that the electrical charge which appears in its fibre may have its origin in so purely a physical cause as the contact of the heterogeneous substances of which the tissue is built up ; the maintenance of this charge being effected by chemical changes going on constantly in the substance of the muscle, by which the carbon dioxide is produced, which is shown to be a measure of the work done.

Conceding now, that muscular contraction is of the nature of an electric discharge, by what mechanism is the contraction effected ? A string of electrical masses, like a muscular fibril, would seem at first to oppose the view now advauced. Such a row of particles would indeed attract each other when electrified, and shorten the length of the whole. But the force of contraction would increase as the length diminished ; whereas the fact in the case of the muscle is precisely the reverse. Two theories have been advanced to account for the result. The first, proposed by Marey, likens the muscular fibre to a string of india-rubber which, when stretched, contracts upon the application of heat, thus transforming heat directly into work. The other, brought forward and strongly supported by Radcliffe, explains contraction by direct electric charge. Each fibre of the muscle, together with its sheath, constitutes a veritable condenser, the charge upon the exterior being positive, and upon the interior negative. When a charge is communicated to the fibre, lateral compression results from the attraction of the electricities of opposite name, and since the volume remains constant, elongation is the consequence — precisely as a band of caoutchouc, having strips of tin-foil upon its sides, may be shown to elongate when charged like a condenser. In this view of the matter the normal condition of the muscle is one of charge, of elongation. Contraction results from the simple elasticity of the muscle itself, the function of the nerve being only that of a discharger. Whether this theory represents the actual fact or not, in all its details, it is supported by the existence of *rigor mortis*, by the continued relaxation of muscle during the flow of the current, by the cessation of contraction on the free access of blood, and by many other phenomena otherwise difficult to explain.

From this brief review, does it not seem probable that the phenomenon of muscular contraction may be satisfactorily accounted for without the assumption of "vital irritability," so long invoked ? May it not be conceded that the theory that muscular force has a purely physical origin is at least as probable as the vital theory ?

Time would fail me to discuss the many other phenomena of the living body which have been found, on investigation, to be non-vital. Digestion, which Prout said it was impossible to believe was chemical, is now known to take place as well without the body as within it, and to result from non-vital ferments. Absorption is osmotic, and its selective power resides in the structure of the membrane and the diffusibility of the solution. Respiration is a purely chemical function. Oxyhæmoglobin is formed wherever hæmoglobin and oxygen come in contact, and the carbon dioxide of the serum exchanges with the oxygen of the air according to the law of gaseous diffusion. Circulation is the result of muscular effort both in the heart and the capillaries, and the flow which takes place is a simple hydraulic operation. Even coagulation, so tenaciously regarded as a vital process, has been shown to be purely chemical, whether we adopt the hypothesis of Schmidt that it results from the union of two proteids, fibrinogen and fibrinoplastic substance, or the later theory of Hammarsten that fibrin is produced from fibrinogen by the action of a special ferment.

One function yet remains which cannot be altogether omitted from our consideration. This function is that of the nervous system. In structure, this system is well known to us all. In composition, it is made up essentially of a single substance, discovered by Liebreich and called protagon, the specific characters of which have lately been confirmed by Gamgee. In function, the nerve-cell and the nerve-fibre are occupied solely in the reception and the transmission of energy, which is in all probability electrical. There is evidently a close analogy between the nerve and the muscle, the axis cylinder like the fibrilla being composed of cells, and having a positive electric charge upon the exterior surface, which has a tension of one-tenth of a volt. Haughton attributes *tinnitus aurium* to the discharge of nerve-cells.

The only objection raised to the electrical character of nerve-energy is based upon its slow propagation. Though thirty years ago Johannes Müller predicted that the velocity of nerve-transmission never could be measured, yet Helmholtz accomplished the feat very soon afterward. His results, like those of subsequent experimenters, show that the velocity of propagation of the nervous influence along a nerve, like that of electric transmission, is only about 26 to 29 metres in a second. But it should be borne in mind, as Lovering has pointed out, that electricity has no velocity, in any proper sense. That since the appearance of an electrical disturbance at the end of a conductor depends upon the production of a charge, the time of this appearance will be a joint function of the electrostatic capacity of the conductor and of its resistance. Since each of these values is directly proportional to length, it follows that the time of transmission will vary as the square of the length of the conductor. While therefore, in Wheatstone's experiment, he found that electricity required rather more than one-millionth of a second to pass through one-quarter of a mile of wire, it does not follow that it would traverse 288,000 miles in one second, as he assumed. Indeed, as Lovering has shown, its actual velocity would be only 268 miles in an entire second. Hence the marvellous discrepancies which have been observed in the results of experiments made to determine the velocity of electricity on long wires are explained.

In the nerve itself, therefore, the velocity of transmission may be supposed to be the less as its resistance is greater. Now, Weber has shown that animal tissues in general have a conductivity only one fifty-millionth of that of copper. And Radcliffe found that a single inch of the sciatic nerve of a frog measured 40,000 ohms ; a resistance eight times that of the entire Atlantic cable. In experimenting to confirm the above law of velocity, Gaugain measured the time of transmission of the electric current through a cotton thread 1.65 metres long and found it to be eleven seconds. Two similar threads placed consecutively, thus forming a conductor twice as long, required forty-four seconds for the passage of the current ; or four times as long. From these data the velocity in the short thread is at the rate of only 0.15 metre in one second ; and in the long one only about half this rate, of course. Hence the fact that the energy of nerve moves at the rate of only 28 metres per second is really no proof that it is not electricity.

The higher functions of the nerve-cell, those connected with mental processes, is a field too vast to be entered at this time. The double telegraph line of nerve, motor and sensor in their effect, but, as Vulpian has proved, precisely alike in function, are the avenues of ingress and egress. Every sensory impression is received by the *thalami optici;* every motor stimulus is sent out from the *corpora striata.* In the acts denominated reflex, the action goes from the spinal cord and is automatic and unconscious. Should the impression ascend higher to the sensory ganglia, the action is now conscious though none the less automatic. Finally, should deliberation be required before acting, the message is sent to the hemispheres by the sensory ganglia and will operates to produce the act. Based on principles which can be established by investigation, a true psychology is coming into being, developed by Bain Maudsley, Spencer and others. A physiological classification of mental operations is being formed which uses the terms of metaphysical psychology, but in a more clearly defined sense. Emotion, in this new science, is the sensibility of the vesicular neurin to ideas. Memory, the registration of stimuli by nutrition. Reflection is the reflex action of the cells in their relation to the cerebral ganglia. Attention is the arrest of the transformation of energy for a moment. Ratiocination is the balancing of one energy against another. Will is the reaction of impressions outward. And so through the list.

Among the physical aspects of the mind-question, the problem of the quantitative changes which take place in the organism is a very curious and interesting one. That the energy of the brain comes from the food will be disputed by no one in these days. Hence, the brain must act like a machine and transform energy. There is then a purely physiological representation of mental action, concerned with forces which are known and measurable. The researches of Lombard long ago showed the concomitant heat of mental action. Recent researches are equally interesting, which show that mental operations are not instantaneous but require a distinct time for their performance. By accurate chronographic measurement, Hirsch has shown that an irritation on the head is answered by a signal with the hand only after one-seventh of a second; that a sound on the ear is indicated by the hand in one-sixth of a second; and that when light irritates the eye, one-fifth of a second elapses before the hand moves. The mechanism of such a process is the following: Suppose the sound "A" is heard by the ear. After a latent period it is translated to some nerve cells and hence to the brain. From the brain it goes to other cells, ganglion cells, and to other nerves, and then to the different muscles of the chest and larnx, and then follows the audible response "A." Now since this whole process requires only one-sixth of a second, the question arises, how much of it is psychical. To answer it, the experiment is repeated but with this difference, that the particular sound to be used is unknown to the experimenter. Before the sound can be repeated by him therefore, a distinct act of discrimination is required, and the time taken is longer. Calling the time in the first experiment a, and in the second b, the difference $b—a$ is the time required for two distinct actions: one, that of distinguishing the sound, and the other, that of willing the corresponding movement. If now it be agreed that only the sound "A" shall be responded to when called, these may be separated since no other sound being responded to the latter action is eliminated. If the time now required be called c, the difference $c—a$ represents the time required for forming a judgment, and $c—b$ the time required for a volition. In making these measurements, Donders used an instrument devised by him, called a noëmotachograph and also a modification of it called a noëmotachometer. By these instruments different points of the body can be irritated, different sounds can be produced, and different color or letters can be shown, all by the electric spark. By subtracting the simple physiological time from the time given in any experiment, the time necessary for recognition may be obtained. By an addition to the apparatus, a second stimulus may be made to follow the first, either on the same or on a different sense; thus enabling the time necessary for a simple thought to be determined. As a result of his experiments, Donders found that the value $b—a$ in the case of a simple dilemma was seventy-five thousandths of a second, this being the time required for recognition and subsequent volition. In the same way $c—a$ has been shown to be forty-thousandths of a second, being the time required for simple recognition; there is left thirty-five thousandths of a second as the time required for volition. Moreover, by independent measurement with the noëmotachometer, exactly the same time, one twenty-fifth of a second, is found necessary to enable a judgment to be formed about the priority of two impulses acting on the same sense. If they act on different senses, more time is necessary. So also more time is required to recognize a letter by seeing its form than by hearing its sound. A man of middle age then, thinking not so very quickly, requires one twenty-fifth of a second for a simple thought.

Another important fact concerning nervous action is that its amount may be measured by the quantity of blood consumed in its performance. Dr. Mosso of Turin has devised an apparatus called the Plethysmograph—drawings of which were exhibited at the London Apparatus Exhibition of 1876—designed for measuring the volume of an organ. The fore-arm, for example, being the organ to be experimented on, is placed in a cylinder of water and tightly enclosed. A rubber tube connects the interior of the cylinder with the recording apparatus. With the electric circuit by which the stimulus was applied to produce contraction, were two keys, one of which was a dummy. It was noticed that, after using the active key several times, producing varying current strengths, the curve sank as before on pressing down the inactive key. Since no real effect was produced, the result was caused solely by the imagination, blood passed from the body to the brain in the act. To test further the effect of mental action, Dr. Pagliani, whose arm was in the apparatus, was requested to multiply 267 by 8, mentally, and to make a sign when he had finished. The recorded curve showed very distinctly how much more blood the brain took to perform the operation. Hence the plethysmograph is capable of measuring the relative amount of mental power required by different persons to work out the same mental problem. Indeed Mr. Gaskell suggests the use of this instrument in the examination room, to find out, in addition to the amount of knowledge a man possesses, how much effort it causes him to produce any particular result of brain-work. Dr. Mosso relates that while the apparatus was set up in his room in Turin, a classical man came in to see him. He looked very contemptuously upon it and asked of what use it could be, saying that it couldn't do anybody any good. Dr. Mosso replied, "Well now, I can tell you by that whether you can read Greek as easily as you can Latin." As the classicist would not believe it, his own arm was put into the apparatus and he was given a Latin book to read. A very slight sinking of the curve was the result. The Latin book was then taken away and a Greek book was given him. This produced immediately, a much deeper curve. He had asserted before that it was quite as easy for him to read Greek as Latin and that there was no difficulty in doing either. Dr. Mosso, however, was able to show him that he was laboring under a delusion. Again, this apparatus is so sensitive as to be useful for ascertaining how much a person is dreaming. When Dr. Pagliani went to sleep in the apparatus, the effect upon the resulting curve was very marked indeed. He said afterward that he had been in a sound sleep and remembered nothing of what passed in the room—that he had been absolutely unconscious; and yet, every little movement in the room, such as the slamming of a door, the barking of a dog, and even the knocking down of a bit of glass, were all marked on the curves. Sometimes he moved his lips and gave other evidences that he was dreaming; they were all recorded on the curve, the amount of blood required for dreaming diminishing that in the extremities. The emotions too left a record. When only a student came into the room, little or no effect appeared in the curve. But when Professor Ludwig himself came in, the arteries in the arm of the person in the apparatus contracted quite as strongly as upon a very decided electrical stimulation.

In an address of the retiring President of this Association, delivered but a few years ago, I find this sentence: "Thought cannot be a physical force, because thought admits of no measure." In the light of the rapid advances lately made in investigating mental action, we see that in two directions at least, in its rate of action and of its relative energy, we may already measure thought, as we measure any other form of energy, by the effects it produces.

Passing now to the consideration of the general question of the transformation of energy which is effected by living beings, attention may be called to one or two points in general physics, as bearing upon its solution. The great law of the dissipation of energy, as modified by Thomson from the statement of Clausius, is thus stated : "The entropy of the universe tends to zero." In other words, the energy of the universe available for transmulation is approaching extinction. This conclusion is based upon the fact that while every form of energy can be completely converted into heat, heat cannot be completely converted into other forms of energy, nor these into each other. Hence it arises that energy is being gradually dissipated as heat. Moreover, since transformation can only result when heat passes from a higher to a lower temperature, it follows that when that perfect equilibrium of temperaiure is reached toward which events are tending, there can be no other energy than heat ; and this absolutely *i*nconvertible, irrevocable. To apply this law to the present case, the muscle, for example, is a machine for transforming the energy of food into work. Since, consequently, this conversion is not complete, it follows that heat must appear as a necessary result of muscular action. The heat of animal life, consequently, is not heat especially provided ; it is simply the heat which inevitably results from an incomplete conversion of energy.

Again, the form of chemical action thus far assumed by physiologists fo account for the energy of the living animal has been combustion. But the science of thermo-chemistry, as developed in late years by Berthelot and Thomsen, has proved, that direct union of chemical substances may not only not evolve heat, but may actually absorb it. It appears, too, that thermal changes accompany all forms of chemical change, those of decomposition and exchange as well as those of synthesis. The animal absorbs highly complex substances as food, capable of innumerable stages of retrogressive metamorphosis before elimination. In each of these stages heat is evolved, being the energy successively stored up by the plant when it repeated these stages in the inverse order.

Another point of interest has reference to the modern views of capillarity. In 1838, J. W. Draper showed that capillarity is an electrical phenomena. Quite recently, Lippmann has developed and extended this view and fully confirmed it. Whenever the free surface of a liquid, curved by capillary action, is electrified it changes its form ; and conversely, when such a surface is made by mechanical means to change its form, an electromotive force is developed. Based upon this principle Lippmann constructed a capillary reversible engine and an extremely sensitive capillary electometer. The former, when a current of electricity was applied to it, developed mechanical work and ran as a motor. When turned by hand, it became an electromotor. In the animal organism there are it is true but a few free surfaces where this action can take place. But Gore has shown that the same phenomenon appears between two liquids in contact, their boundary being altered in character by electrification. Indeed, when we consider the production of electricity by osmose, and of heat and electricity both, by imbibition, both capillary phenomena, the wonder is not that so much energy is evolved by the organism, but that it is so little. If the physical and chemical changes which take place within the body took place without it, there would be an abundant evolution of energy. Can we doubt that these changes are the cause of the energy exhibited by the organism ?

Thus far, when we have spoken of a living being, we have had reference to the organism as a whole, and this of a rather complex kind. In this view of the case, however, we find that biological microscopists do not agree with us. "The cell alone," says Küss, "is the essentially vital element." Says Beale,—"There is in living matter nothing which can be called a mechanism, nothing in which structure can be discerned. A little transparent colorless material is the seat of these marvellous powers or properties which the form, structure and function of the tissues and organs of all living things are determined." And again, "However much organisms and their tissues in their fully formed state may vary as regards the character, properties and composition of the formed material, all were first in the condition of clear, transparent, structureless, formless living matter." So Ranvier : "Cellular elements possess all the essential vital properties of the complete organism." And Allman, in his address as President of the British Association last year, is still more explicit. "Every living being," he says, "has protoplasm as the essential matter of every living element of its structure." "No one who contemplates this spontaneously moving matter can deny that it is alive. Liquid as it is, it is a living liquid ; organless and structureless as it is, it manifests the essential phenomena of life." "Coextensive with the whole of the organic nature—every vital act being referable to some mode or property of protoplasm, it becomes to the biologist what the ether is to the physicist." From these quotations it would seem that even in the highest animal there is nothing living but protoplasm or germinal matter "transparent, colorless, and, as far as can be ascertained by examination with the highest powers of the microscope, perfectly structureless. It exhibits these same characters at every period of its existence." Neither the contractile tissue of the muscle, the axis-cylinder of the nerve, nor the secreting cell of the gland, is living, according to Beale. Hence it would be fair to draw the inference that no vital force should be required to explain the phenomena of the non-living matter of the body, such as the contraction of the muscle or the function of the nerve. If this be conceded it is a great point gained ; since the phenomenon of life becomes vastly simplified when we have to account for it only as exhibited in this one single form of living matter, protoplasm. In describing its properties, Allman includes this remarkable mobility, these spontaneous movements, and says, "They result from its proper irritability, its essential constitution as living matter. From the facts there is but one legitimate conclusion, that life is a property of protoplasm." Beale, however, will not allow that life is "a property" of protoplasm. "It cannot be a property of matter," he says, "because it is in all respects essentially different in its actions from all acknowledged properties of matter." But the properties of bodies are only the characters by which we differentiate them. Two bodies having the same properties would only be two portions of the same substance. Because life, therefore, is unlike other properties of matter, it by no means follows that it is not a property of matter. No dictum is more absolute in science than the one which predicates properties upon constitution. To say that this property exhibited by protoplasm, marvellous and even unique though it be, is not a natural result of the constitution of the matter itself, but is due to an unknown entity, a *tertium quid*, which inhabits and controls it, is opposed to all scientific analogy and experience. To the statement of the vitalist that there is no evidence that life is a property of matter, we may reply with emphasis that there is not the slightest proof that it is not.

Chemistry tells us that complexity of composition involves complexity of properties. The grand progress which Organic Chemistry has made in recent times has been owing to the distinct recognition of the influence of structure upon properties. Isomerism is one of its most significant developments. The number of possible insomers increases enormously with the complexity of the molecule. Granted that we now know several of the proteid group of substancs : how many thousand may there be yet to know ? Bodies of such extreme complexity of constitution may well have an indefinite number of isomers. Not only does chemistry not say that there cannot be such a thing but she encourages the expectation that there will be yet found the precise proteid of which the changes of protoplasm are properties. The rapid march of recent organic synthesis makes it quite certain that every distinct chemical substance of the living body will ultimately be produced in the laboratory ; and this from inorganic materials. Given only the exact constitution of a compound, and its synthesis follows. When, therefore, the chemist shall succeed in producing a mass constitutionally identical with protoplasmic albumin, there is every reason to expect that it will exhibit all the phenomena which characterize its life ; and this equally whether protoplasm be a single substance or a mixture of several closely allied substances.

But here a word should be said concerning a remarkable physical condition assumed by matter in organized beings. Graham, in 1862, drew the sharp line which separates colloid from crystalloid matter. "His researches have required," says Maudsley, "a change in our conception of solid matter. Instead of the notion of inert impenetrable matter, we must substitute the idea of matter which in its colloidal state is penetrable, exhibits energy, and is widely susceptible to external agents. This sort of energy is not a result of chemical action, for colloids are singularly inert in all ordinary chemical relations, but is a result of its unknown molecular constitution; and the undoubted existence of colloidal energy in organic substances, which are usually considered inert and called dead, may well warrant the belief of its larger and more essential operation in organic matter in the state of instability of composition in which it is when under the condition of life. Such energy would then suffice to account for the simple uniform movements of the homogeneous substance of which the lowest animal consists, and the absence of any differentiation of structure is a sufficient reason for the absence of any localization of any function and of the general uniform reaction to local impressions." Graham himself says: "the colloidal state may be looked upon as the probable primary source of the force appearing in the phenomena of vitality." The colloidal condition of the dynamical state of matter; the crystalloidal the static. The former, which is the rule in the organic kingdom of nature, is the exception in the inorganic. Aluminum and ferric hydrates, silicic acid and a few other inorganic substances, exist in the colloid condition. From analogy there would seem to be but little doubt that the colloid state of these bodies differ from their crystalloid state merely in the size of the molecule. In other words opal, which is colloid silica, is a polymer of quartz. If this theory be true there can be no doubt of the vastly greater complexity of a colloidal proteid molecule than of a crystalloid one. Now it is a very significant fact, in this connection, that not a single organic colloid has ever been synthesized. Gelatin, which is one of the best examples of a colloid, has a comparatively simple structure. And, although Gibbs showed, many years ago, that gelatin was probably an amido-derivative of the sugar group, yet no inverse process has yet given us this substance. That matter in the crystalloid and colloid forms may be chemically identical, differing only in the size of its molecule, may be quite possible. But it is also possible that the difference may be a physical one. To produce the colloid state from the crystalloid is by no means beyond the power of science. We qualify our previous statement then only so far as to say that when the chemist produces a body in the colloidal form, having the identical constitution of protoplasm, there is every reason to believe that it will have the properties of protoplasm.

The important question now arises whether, since the protoplasm of animals is identical with that of vegetables, and the latter is the food of the former, any protoplasm whatever is vitalized by the animal as such. That this identity exists would seem satisfactorily established. Though the protoplasm of vegetables is enclosed within a cellulose bag, it is only a closely imprisoned rhizopod. In the Nitella, it shows all its characteristic irritability, and from Vaucheria it escapes to exhibit all its amœboid movements. Spores swim about by cilia or flagella, and the cell division of the one kingdom is the same as that of the other. In plants, however, protoplasm seems to be associated with chlorophyll, whose function was for a long time supposed to be to decompose carbon dioxide under the influence of sunlight. But Draper in 1843, showed that this decomposition took place before the chlorophyll was formed. Recent researches have shown that the function of chlorophyll is wholly protective. The assimilative power of the protoplasm reaches its maximum in the orange and yellow rays. Now Bert has shown that the absorption band in the chlorophyll spectrum is in the exact position of this maximum. Hence, Gautier believes that this substance acts as a regulator of plant respiration, the greater or less amount of luminous energy thus absorbed and transformed, being utilized by the protoplasm and stored up. Growth and cell-division, however, are independent of orange light, and hence of chlorophyll. In the higher plants, these functions are performed by a separate and deep-lying set of cells. But in the lower, the same cell discharges both functions, assimilation going on in it during the day, and growth chiefly at night. Sachs had already proved that the maximum growth of plants takes place just before daylight and the minimum in the afternoon. This retarding action of sunlight upon growth is as curious as it is unexpected. It now appears that in orange light plants assimilate—absorb carbon dioxide and evolve oxygen—but do not grow—are not heliotropic; while in blue light they are heliotropic but do not give off oxygen. Chlorophyll, however, is not confined to vegetables; infusoria, hydras, and certain planarian worms are green from the presence of this substance, and Geddes has shown that such animals, placed in the sunlight, give off a gas which is more than half oxygen. These cells, moreover, contain starch granules.

A still more striking evidence of this intimate relationship has been developed by Darwin, in his researches upon insectivorous plants. Not only do these plants possess a mechanism for capturing insects, but they secrete a gastric juice which digests them. Nägeli has shown the presence of pepsin in yeast cells, and attention has lately been called by Wurtz and others to the juice of the *Carica papaya* which contains a pepsin-like substance capable of peptonizing fibrin completely. Moreover, there is the closest similarity between diastase and ptyalin; and the milk of the cow-tree, recently examined by Boussingault and found to resemble cream closely in composition, shows the presence of an emulsifying agent in the vegetable kingdom analogous to pancreatin in the animal.

Another most curious proof of the indentity of animal and vegetable protoplasm has been given by Claude Bernard, who has shown that both are alike sensitive to the influence of anæsthetics. A sensitive plant exposed to ether no longer closed its leaflets when touched. Assimilation and growth, as well as germination, are arrested by chloroform. The yeast plant when etherized no longer decomposes sugar to produce alcohol and carbon dioxide; while the inversive and non-vital ferment still acts to convert the cane-sugar into glucose; precisely as under these circumstances, the diastasic ferment converts the starch of the seed into sugar. By arresting anæsthetically the process by which carbon dioxide is absorbed and oxygen evolved, the true respiratory process, being less effected, now appears; and Schutzenberger has proved that the fresh cells of the yeast plan breathe like an aquatic animal.

It would seem then that the protoplasmic life of animals is identical with that of plants; a certain measure of destructive metamorphosis taking place in each, evolving energy and producing carbon-dioxide and water. When, however, this function is examined quantitatively, its maximum is seen to be reached in the animal. While the assimilative function characterizes the plant, the destructive function distinguishes the animal. Hence it is the function of the plant to store up energy, to produce the highly complex protoplasm. This, consumed by the animal as his food, continues his existence as a living being, the energy gradually set free by its successive steps of retrogressive metamorphosis, appearing as the work which he performs. If this view be correct, it would follow that every individual substance found in the animal—save only those which result from degredation—must be found in the plant upon which it feeds, and this is the fact. The myosin which Kühne has shown to be the distinctive proteid of muscle, Vines has found in the aleuron grains of the lupine and the castor oil plant, along with vitellini the special proteid of the vitellus. The researches of Weyl & Bischoff have proved that gluten is formed in the dough of wheat flour by the action of a ferment upon the globulin-substance or plant-myosin which it contains, precisely as Hammarsten has shown fibrin is produced in the action of a similar ferment upon fibrinogen. Not only this; Hoppe Seyler has extracted from maize the identical substance which has been shown by Liebriech to be the essential chemical constituent of nerve tissue, protagon.

The evidence then would seem conclusive that, since the protoplasm of the animal and the vegetable kingdoms is identical, the former in all cases being derived from the latter, the animal as such neither produces nor vitalizes any protoplasm. Two inferences seem naturally to follow from this conclusion: 1st, that all the properties of animal protoplasm, and of the animal organism of which it constitutes the essential part, must have a previous existence in the plant; 2d, that hence the solution of the life-question in the Myxomycetes will solve the life-problem for the highest vertebrate.

Another consideration which must not be left out of the account in any discussion of the life-question is the potent influence of environment. Ordinary examples of this influence pass before our eyes every day. Heat necessitates the germination of the seed, and light causes the plant to grow. Gravity obliges its root to grow downward and its stem to ascend. Certain sensations from without excite inevitably muscular contraction; and a ludicrous idea may provoke laughter in defiance of the will. Epidemic and epizootic diseases show the dependence of function upon external conditions, and the germ theory demonstrates the utter disproportionality of the cause to the effect. The remarkable similarity in the periodicity observed between sunspots and the weather has been extended to include the appearance of locusts and the advent of the plague. Even the body politic feels its influence, Jevons having established a coincident periodicity for commercial crises.

The modern theory of energy, however, puts this influence in a still stronger light. As defined hitherto, energy is either motion or position; is kinetic or potential. Energy of position derives its value obviously from the fact that in virtue of attraction it may become energy of motion. But attraction implies action at a distance; and action at a distance implies that matter may act where it is not. This of course is impossible; and hence action at a distance, and with it attraction and potential energy, are disappearing from the language of science. But what conception is it which is taking its place? By what action does the sun hold our earth in its orbit? The answer is to be found in the properties of the ether which fills all space. The existence of this ether, the phenomena of light and electricity abundantly prove. While so tenuous that Astronomy has been taxed to prove that it exerts an appreciable resistance upon the least of the celestial bodies, its elasticity is such that its transmits a compression with a well nigh infinite velocity. On the one hand, Thomson has determined its inferior limit, and finds that a cubic mile of it would weigh only one thousand-millionth of a pound; on the other, Herschel has calculated that, if an amount of it equal in weight to a cubic inch of air be enclosed in a cubic inch of space, its reaction outward would be upward of seventeen billions of pounds. Instead of being represented as is our air, by the pressure of a homogeneous atmosphere five miles in height, such a pressure would represent just such a homogeneous atmosphere five and a half billions of miles high, or about one-third the distance to the nearest fixed star! In Herschel's own words: "Do what we will, adopt what hypothesis we please, there is no escape in dealing with the phenomena of light, from these gigantic numbers, or from the conception of enormous physical force in perpetual exertion at every point throughout all the immensity of space."

Now, as Preston has suggested, if we regard this ether as a gas, defined by the kinetic theory that its molecules move in straight lines, but with an enormous length of free path, it is obvious that this ether may be clearly conceived of as the source of all the motions of ordinary matter. It is an enormous storehouse of energy, which is continually passing to and from ordinary matter, precisely as we know it to do in the case of radiant transmission. Before so simple a conception as this, both potential energy and action at a distance are easily given up. All energy is kinetic energy, the energy of motion. In a narrower sense, the energy of matter-motion is ordinary kinetic energy; the energy of ether-motion, which may become matter-motion, fills the conception of the older potential energy. Giving now to the ether its storehouse of tremendous power, and giving to it the ability to transfer this power to ordinary matter upon opportunity, and we have an environment compared with which the strongest steel is but the breath of the summer air. In presence of such an energy it is that we live and move. In the midst of such tremendous power do we act. Is it a wonder that out of such a reservoir the power by which we live should irresistibly rush into the organism and appear as the transmuted energy which we recognize in the phenomena of life? Truly, as Spinoza has put it, "Man thinks himself most free when he is most a slave."

Such now are some of the facts and fancies to be found in the science of to-day concerning the phenomena of life. Physiologically considered, life has no mysterious passages, no sacred precincts into which the unhallowed foot of science may not enter. Research has steadily diminished day by day the phenomena supposed vital. Physiology is daily assuming more and more the character of an applied science. Every action performed by the living body is sooner or later to be pronounced chemical or physical. And when the last vestige of the vital principle shall disappear, the word "Life," if it remain at all, will remain to us only to signify, as a collective term, the sum of the phenomena exhibited by an active organized or organic being.

I cannot close without speaking a single word in favor of a vigorous development in this country of physiological research. What has already been done among us has been well done. I have said with diffidence what I have said in this address, because I see around me those who have made these subjects the study of their lives, and who are far more competent to discuss them than I am. But the laborers in the field are all too few, and the reasons therefor are not far to seek. One of these undoubtedly is the high scientific attainment necessary to a successful prosecution of this kind of investigation. The physiological student must be a physicist, a chemist, an anatomist and a physiologist all at once. Again, the course of instruction of those who might fairly be expected to enter upon this work—the medical students of the country—is directed toward making them practitioners rather than investigators. In the third place the importance of physiological studies in connection with zoölogical research is only beginning in this country to receive the share of attention it deserves. I well remember the gratification I experienced in 1873 upon receiving a letter from Professor Louis Agassiz, asking me to give some lectures at Penikese upon physiological chemistry; a new departure for those times. In this view of the case it seems very appropriate that a new subsection of this Association should be just now in process of formation. We welcome warmly the body of men who form it, and we predict that from the new subsection of Anatomy and Physiology most valuable contributions will be received for our proceedings.

It is a beautiful conception of science which regards the energy which is manifested on the earth as having its origin in the sun. Pulsating awhile in the ether-molecules which fill the intervening space, this motion reaches our earth and communicates its tremor to the molecules of its matter. Instantly all starts into life. The winds move, the waters rise and fall, the lightnings flash and the thunders roll, all as subdivisions of this received power. The muscle of the fleeing animal transforms it in escaping from the hunter who seeks to use it for the purpose of his destruction. The wave that runs along that tiny nerve-thread to apprise us of danger transmutes it, and the return pulse that removes us from its presence is a portion of it. The groan of the weary, the shriek of the tortured, the voicéd agony of the babeless mother, all borrow their significance from the same source. The magnificence of the work of a Leonardo da Vinci or a Michael Angelo; the divine creations of a Beethovan or a Mozart; the immortal Principia of a Newton and the Méchanique Celeste of a Laplace—all had their existence at some point of time in oscillations of ether in the intersolar space. But all this energy is only a transitory possession. As the sunlight gilds the mountain top and then glances off again into space, so this energy touches upon and beautifies our earth and then speeds on its way. What other worlds it reaches and vivifies, we may never know. Beyond the veil of the seen, science may not penetrate. But religion, more hopeful, seeks there for the new heavens and the new earth, wherein shall be solved the problems of a higher life.

THE recent artificial production of the diamond is closely followed by an interesting synthesis, by M. de Schulten, resulting in the mineral analcine. On heating a solution of silicate of soda or caustic soda, in presence of an aluminous glass, to a temperature of 190° C. (374° F.) in a closed vessel, during forty-eight hours, small but very perfect transparent crystals, imbedded in gelatinous silica, were formed on the walls of the tube. They answer in every respect to the mineralogical characteristics of analcites.

THE REDUCTION OF CHLORIDE ORES.

For the benefit of those not familiar with the processes of reducing gold and silver ores, a brief explanation of what is meant by "free milling," an expression so often used by mining men, may not be out of place. In separating, by amalgamation, the precious metals from gangue or waste rock with which they are almost always associated, it is necessary to the success of the process to present the particles of gold or silver contained in the ore to the mercury with which they are to be alloyed, in such form that the latter can seize upon them readily. If these metals always occurred in nature in their pure metallic state, this would be a very easy matter. In free milling gold ores it is frequently only necessary to place the quicksilver beneath the stamps of the battery in which the ore is crushed, and upon an inclined copper plate over which the pulp is carried by water after it leaves the battery. The stamps, by reducing the rock to fine particles, release the minute scales and crystals of gold, which are readily taken up by the quicksilver, while the rock, for which the mercury has no affinity, is carried away as "tailings."

But silver rarely occurs in a native or pure metallic state. It is usually mixed with clorine, lead, iron, sulphur, manganese, copper, antimony and other base metals, and is found in the form of chloride of silver, argentiferous galena, in which the silver is in the form of a sulphide, and in many other compounds, for most of which quicksilver has no more affinity than it has for the common rock of the gangue. In most cases, therefore, if the silver ore was simply crushed and brought into contact with an amalgamating surface, little or none of the metal would be caught by the quicksilver and saved. Mercury has a strong affinity for metallic silver. stronger even than that of chloride, so that ff chloride of silver and quicksilver are brought together the mercury will seize the silver, forming an amalgam, and the chlorine which is released will escape as gas or unite with some other substance which presents itself and for which it has an affinity; but sulphur will not give up silver, with which it is chemically mixed, to mercury, unless the sulphur has first been driven off by fire. This process of converting chloride of silver into an amalgam is not an instantaneous one like the amalgamation of free gold, but requires several hours to be perfected, and it is hastened by the presence of other chemicals, such as sulphate of copper, sulphuric acid, and cyanide of potassium, the action of which it is unnecessary to explain here.

In order to reduce silver ores by amalgamation, it is necessary, as will be understood from the above explanation, to have the particles of metal either in a pure or chloritic state. When they are found in nature in either of these conditions they need no special treatment before being put into the mill, and the treatment of them is called "raw amalgamation." The process employed is to crush the ore to a fine pulp, and then transfer it to a large round iron tub, where it is agitated for several hours in hot water with quicksilver, some or all the chemicals I have named being added with common salt to promote the union of the mercury and the silver. If the silver in the ore is in the form of a sulphide, as it frequently is, and the amalgamation process of reduction is to be employed, the ores have to be roasted with common salt for several hours after they are crushed. Without explaining in full the chemical reactions, I may simply say that the heat volatilizes the sulphur mixed with the silver, and separates the salt into its constituents of chlorine and sodium, the first of which unites with the silver from which the sulphur has been driven off, and forms a chloride which is then ready for the amalgamating pan. The desulphurization and chlorination of an ore is an expensive process, and greatly increases the cost of reduction.

When such metals as lead, zinc, or copper are present in ores in large quantities, it is usually cheaper to reduce them by smelting, and by that process the lead and copper are generally saved and add to the value of the product. Almost any ore can be reduced by fire, if it is mixed in small proportions with other smelting ores. In large smelting establishments like those at Denver, Omaha, and Newark, N. J., where great varieties of ores are purchased, even free milling rock can be used to advantage; but the reduction of most free milling ores by fire, without mixture with others, would be ruinously expensive if not physically mpracticable.

ON CURRENTS PRODUCED BY FRICTION BETWEEN CONDUCTING SUBSTANCES AND ON A NEW FORM OF TELEPHONE RECEIVER.*

In a communication to the Royal Society of Edinburgh of date January 6, 1879, I showed that "electric currents were produced by the mere friction between conducting substances." The existence of these currents can be easily demonstrated either by a telephone or a Thomson's galvanometer. I have since found that these currents are, for all pairs of metals which I have yet tried, in the same direction as the thermo-electric current got by heating the junction of the same two metals. They are also approximately at least, stronger in proportion as the metals rubbed are far apart on the thermo-electric scale—the strongest current, as far as I have yet observed, being got by rubbing antimony and bismuth together. These observations clearly point to a thermo-electric origin for the currents; but it is possible that they may be due partly to the currents suggested by Sir William Thomson as the cause of friction, and partly, also, to contact force between films of air or oxide adhering to the surfaces of the metals.

Having ascertained that these friction-currents are of some strength and fairly constant, I proceeded to make several kinds of machines for producing currents on this principle. One of them consists of a cylinder of antimony, which can be rotated rapidly, while a plate of bismuth is pressed hard against it by a stiff spring. When this machine is included in the same circuit with a microphone and a Bell telephone, the current got from it is quite sufficient to serve for the transmission of musical sounds and also loud speaking. The transmitter, which I have found most serviceable in my experiments, is made by screwing two small cubes of gas-carbon to a violin, and placing between them a long stick of carbon pointed at both ends, the points being made to rest in conical holes in the carbon cubes. The looseness of the contact is regulated by a paper spring. This forms an excellent and handy transmitter for all kinds of musical sounds, and also serves very well for transmitting speech.

Seeing that friction between metals clearly produces a current, it seemed natural to inquire if the converse held good, that is, if a current from a battery sent across the junction of two metals affected the friction of the one upon the other. I have tested for this in a variety of ways, and the results obtained leave me in doubt whether to attribute them to variations in the friction, or to actual sticking porduced by fusion of the points of contact through which the current passes. The most noticable effect is produced when one of the rubbing bodies is a mere point, and the other a smooth surface of metal. This led me to make a modification of the loud speaking telephone of Mr. Edison, in order to get audible indications of changes of friction produced by the passing of a variable current. It consists of a cylinder of bismuth accurately turned and revolving on centres. The rubber-point is made of a sewing-needle with its point bent at right angles, and its other end attached to the centre of the mica disk of a phonograph mouthpiece. It is evident that this is only a loose contact, which can be perpetually changed. When this apparatus is included in the circuit with the violin-microphone and three or four Bunsen cells, the violin sounds, as was to be expected, are heard proceeding from the loose contact, even when the cylinder is not rotated. They are increased, however, in a remarkable degree by rotating the cylinder slowly, so much so that a tune played on the violin can, with proper care, be distinctly heard all over an ordinary room.

With regard to the explanation of this effect, it is evident that electrolysis can in no sense come into play, as is supposed to be the case in Edison's instrument. I am inclined to look for the explanation rather in the direction of the Trevelyan rocker, although the circumstances are considerably different in the two cases. In the rocker we have the heat passing from a mass of hot metal through two points of support to a cold block, whereas, in the other case, the heat is only intense at the points of contact, the rest of the metals being comparatively unaffected. The variations n the current produced by the transmitting microphone must

* Abstract of a paper read before the Royal Society of Edinburgh by James Blyth, M. A., F.R.S.E., on May 3, 1880.

cause corresponding variations in the heat at the point of contact of the needle with the cylinder, and this again produces a mechanical movement of the pressing point, as well as of the air surrounding it, sufficient to give forth sound-waves. If such be the case the effect should be different for different metals, those answering best which have the lowest thermal conductivity and also the lowest specific heat. That this is really so, is showing by substituting cylinders of other metals for the bismuth, all other things remaining the same. In this way I have compared lead, tin, iron, copper, carbon, and find that they all give forth the simple loose contact sound when the cylinder is stationary, but that it is only with bismuth that there is any very great intensification of the sound when the cylinder is rotated. Now, by consulting the appropriate tables I find that bismuth is a fraction lower than any other common metal in specific, while heat is much below them all, in thermal conductivity. This seems to bear out my explanation to a certain extent.

THE subject of a depraved taste in animals is an interesting one, which has not been studied as much perhaps as it might. In human beings it would seem to depend on ill-health of either body or mind, but in animals it would seem as if it might be present and the animal enjoy good health. One remarkable instance in an herbivorous animal we can vouch for. It occurred in a sheep that had been shipped on board one of the P. and O. steamers to help to supply the kitchen on board, but while fattening it developed an inordinate taste for tobacco, which it would eat in any quantity that was given to it. It did not much care for cigars, and altogether objected to burnt ends; but it would greedily devour the half-chewed quid of a sailor or a handful of roll tobacco. While chewing there was apparently no undue flow of saliva, and its taste was so peculiar that most of the passengers on board amused themselves by feeding it, to see for themselves if it were really so. As a consequence, though in fair condition, the cook was afraid to kill the sheep, believing that the mutton would have the flavor of tobacco. Another very remarkable case has just been communicated to us by Mr. Francis Goodlake: this time a flesh-eating animal in the shape of a kitten, about five months old, who shows a passionate fondness for salads. It eats no end of sliced cucumber dressed with vinegar, even when hot with cayenne pepper. After a little fencing it has eaten a piece of boiled beef with mustard. Its mother was at least once seen to eat a slice of cucumber which had salt, pepper and vinegar on it. The kitten is apparently in good health, and its extraordinary taste is not easily accounted for. Even supposing it once got a feed of salmon mayonaise, why should it now select to prefer the dressing to the fish?—*Nature*.

NATURAL ENEMIES OF THE TELEGRAPH.—There is, apparently, no apparatus so liable to be interfered with by what we may call natural causes as the electric telegraph. Fish gnaw and mollusks overweight the submarine conductors of the subterranean wires; while there is at least one instance of a frolicsome whale entangling himself in a deep sea cable, to its utter disorganization. It is stated that within the three years ending 1878, there have been sixty serious interruptions to telegraphic communication in Summatra, by elephants. In one instance, these sagacious animals, most likely fearing snares, destroyed a considerable portion of the line, hiding away the wires and insulators in a canebreak. Monkeys of all tribes and sizes, too, in that favored island, use the poles and wires as gymnasia, occasionally breaking them and carrying off the insulators; while the numerous tigers, bears and buffaloes on the track render the watching and repair of the line a duty of great danger. In Australia, where there are no wild animals to injure the wires, which are carried great distances overland, they are said to be frequently cut down by the scarcely less wild aborigines, who manufacture from them rings, armlets and other varieties of barbaric ornament. It has been suggested as a means of protection in this case that the posts should be constructed of iron, when the battery could be used to astonish any native climbing them with felonious intent.—*Scientific American*.

PHYSICAL NOTES.

IN an article of great length, extending through the last three numbers of the *Annalen der Physik und Chemie*, which exhibits extraordinary scope of research and ingenuity, the learned Professor Quincke exhausts the subject of electrical expansion. The following results are drawn from his investigation:

1. Solid and liquid bodies alter their volume when they are acted upon, the same as Leyden jars, by electrical forces.

2. This change of volume is not the effect of heat, but is mostly an expansion; though it may also be a contraction, as in the case of the fatty oils.

3. No change of volume was observable in gases under the action of electrical forces. If such occurred it was smaller than $\frac{1}{30000000}$ of the original volume.

4. There was an instantaneous change of volume in flint glass, but it took longer in German glass, which is a better conductor of electricity. By discharge of the coatings of spherical and tubular condensers, the glass resumes its original volume.

5. There is a simultaneous change of length and volume in tubular condensers.

6. The change of volume and length increases as the difference of potential in the coatings, and inversely as the thickness of the insulating substance of the condenser; and they are nearly proportional to the square of strength of potential and thickness.

7. Under otherwise equal conditions the expansion in volume and length differ according to the insulating substance of the condenser.

8. After the discharge of the coatings of the condensers, there is a residue, so to speak, of this change of volume, which is very small in the case of flint glass, but greater in German glass, and which seems to have some connection with the electrical polarization of the mass of the glass itself.

9. The change of mass and volume does not result from an electrical compression of the insulating substance.

10. In flint glass electrical expansion takes place equally in all directions, as in the expansions produced by increase of temperature, independent of the character and direction of the electrical forces.

11. Electrical change of length and volume takes place in glass nearly in the same way with increase of temperature, as the dialectric constants, or the electrical conductivity of the glass.

12. Action of electrical forces diminish the elasticity of flint glass, German glass, and caoutchouc, but increase that of mica and gutta percha.

13. The electrical piercing of glass and other substances is a result of the unequal electrical expansion of the insulator in different places.

14. By unequal electrical expansion solid and liquid substances are unequally dilated and become double refracting, as other similar substances do when heated.

15. Glass, when equally expanded, shows no electrical double refraction under electrical forces.

16. The relation of substances with positive and negative double refraction (to which Dr. Kerr first called attention), is explained by the way in which different substances change their exponents of refraction with their density and volume.

17. With a constant difference of potential in the coating of a condenser, after long charging, the electrical force varies in different layers of the insulative substance at the same time, or in the same place at different times.

M. BERTHELOT has recently made an apparatus for measuring the heat of combustion of gases by detonation, which consists essentially of a bomb suspended in a calorimeter.

MR. W. E. HIDDEN, the mineral collector, has discovered in Burke County, N. C., a new locality of Furgusonite. The mineral was chemically determined by Dr. J. Lawrence Smith.

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SATURDAY, SEPTEMBER 11, 1880.

THE PHOTOPHONE.

Mr. Alexander Graham Bell, whose contributions to electric science have been recognized at home and abroad to their fullest value, has written a paper on his latest invention, the Photophone, which we reproduce and abundantly illustrate.

It is a beautiful application of the telephone to the registration of the mechanical action of electricity set in motion by light; but it is not (as the world was lead to suppose by some ill informed journalist) a method of transmuting light pulsations into electrical ones, and then changing these back again into light. A beam of light is reflected upon a mirror diaphragm, which is set in motion by the voice or otherwise; the concentrated ray is then reflected so as to affect a piece of selenium in a telephonic circuit, which, by its varying conductivity, acts intermittently on the diaphragm of the telephone, and thus in the usual way reproduces the sound. The instrument is simplicity itself, but the results are of the highest popular and scientific interest.

That it is possible for even the ray of a star to produce a mechanical effect, was demonstrated when Edison used his Tasimeter for measuring the waves of radiant energy of Vega. We thought Bell had solved the problem, upon which Edison was at work when he became interested in the perfection of his electric light, but our hope has not been realized. The subject, however, is one of extreme interest, and it is not strange for the discoverers of the two telephonic systems to be simultaneously engaged in solving the natural corollary to their great propositions. But Edison has an advantage in the pursuit. His employment of the varying electrical conductivity of carbon allows him to introduce any amount of reserve power for mechanical purposes.

It is much to be regretted that Edison can not find leisure from the practical applications of his science to turn his attention to those problems which he is so eminently capable of solving. We vividly recall some experiments in this direction which he told us of during the Spring of 1878, while on a visit to his laboratory at Menlo Park. He allowed a beam of light to fall on the surface of a diaphragm connected with his carbon button, in the hope that by a surface and molecular action, it would be possible to transmit its motion to a receiving diaphragm, where a similar molecular tension would result in the reproduction of the original vibrations. A faint halo is said occasionally to have surrounded the diaphragm. We could not but believe this due to the excited imagination of Mr. Edison, for at the time he was enthusiastically engaged in testing the wondrous capacity of the tasimeter, which he was soon to use in eclipse observations on the Draper expedition.

He also tried to observe the effect of a beam acting on the diaphragm of a phonograph, whose cylinder revolved at enormous speed, hoping a line of phosphorescence might arise from the tinfoil where it came in contact with the needle. Mr. Edison said he employed the direct action of the light (in the last case), in preference to using electricity as a medium for it, because he feared there existed a difference between the vibratory periods of light and electricity, although their velocity was nearly the same. For a similar reason he sought to realize the instantaneous translation of light by using his motograph, in preference to the magnetic telephone which for this purpose is valueless, owing to the time required to charge and discharge the iron core. But the most interesting of these experiments is to come. He threw a beam of lamp light on a small mirror, fastened to a tuning fork, and reflected a ray upon a strip of hard rubber in the tasimeter, the button of the latter being in circuit with a telephone and battery. On setting the fork in motion, the Lissajous figure caused a movement of the rod, which resulted in the reproduction of the musical note.

But all these pretty experiments are but introductory to the more subtile question, how to translate light through other forms of motion back into light. We wish a hearty rivalry between the two discoverers; for Messrs. Bell and Edison will find the fields of science (like those of trade) yield best fruit when fertilized by competition.

We have received a copy of the Report made by Professor S. W. Burnham, to the "James Lick Trust," of Observations made on Mt. Hamilton, with reference to the location of Lick Observatory, but we are compelled by press of matter to postpone further reference to it until a future date.

We have authority for stating that the Rev. W. H. Dallinger, of England, has consented to become Governor and Professor of Natural Sciences, of Wesley College, Sheffield. We congratulate the trustees of this establishment on having secured the assistance of one who has done so much to elevate the standard of scientific research.

The published papers of Professor Dallinger are models of their kind, and largely quoted by the highest authorities who write on the progress of Biology.

We trust Professor Dallinger, in taking the management of Wesley College, may still be enabled to prosecute his exhaustive microscopical studies, by the methods originally devised by himself, which have already been so fruitful of results, and promise to revolutionize our knowledge of such forms of life.

We are requested to state by the trustees of the Lick Observatories that they will be glad to receive the publications of Observatories, and of Astronomical and Scientific societies, for the permanent library of the Lick Observatory. They inform us that the preliminary work on Mt. Hamilton has already been commenced, and will be prosecuted as rapidly as possible under the circumstances. The small equatorial of 12-inch aperture, has been ordered of Alvan Clark & Sons, and will be placed in position early in 1881; and the great equatorial, meridian circle, and other instruments, will be contracted for at an early day. It is not expected there will be any further delay in putting the Lick Observatory in complete working order, other than that incident to the importance and magnitude of the undertaking.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.*

GENERAL BUSINESS—MONDAY, AUGUST 30TH.

The fifth day of the meeting was devoted to general business, to essays in the departments, and to visiting Salem in the afternoon. In the general session some new members were elected, and it was agreed that when the Association adjourned, that it should be to Cincinnati, on August 17, 1881.

The following reports were made:

Mr. E. B. Elliott, on an uniform system of registering deaths, births and marriages; Prof. E. L. Youmans, on the treating of science in public schools; Mr. F. B. Hough, on the preservation of forests; Prof. Harkness also reported certain amendments to the condition of the Association, to be acted on next year. At present there are two full sections in the association, and it is proposed to establish eight, covering the following branches; A, Physics; B, Astronomy and Mathematics; C, Chemistry and its Application; D, Mechanical Science; E, Geology and Geography; F, Biology; G, Anthropology; H, Economic Science and Statistics. A permanent sub-section of Microscopy is also provided for. These changes will bring the association in close resemblance to that of the British association.

The reading of the papers in the various sections was continued, the subjects of which need not here be stated, as we shall offer a full tabulated list of all the papers read before this association, conveniently arranged for future reference.

* Continuation of Report from SCIENCE, Sept. 4.

TUESDAY, AUGUST 31ST.

The list of essays entered for reading was closed with the number 280. The following officers were elected for the Cincinnati meeting to be held in 1881:—President, Professor G. J. Brush, of Yale College; Vice-President of Section A, Professor A. M. Mayer, of Hoboken; General Secretary, C. V. Riley; Secretary, Section A, Professor John Trowbridge, of Harvard; Secretary, Section B, William Saunders; Treasurer, W. S. Vaux, of Philadelphia; Auditing Committee, Henry Wheatland, of Salem, and Thomas Meehan, of Philadelphia. Resolutions were adopted for a social re-union of the various sections on the second evening of future sessions. Resolutions were also passed recognizing the services to science of the late General Myer of the Signal Service, and the providing for the appointment of a committee to select a series of stars of stellar magnitude for standards, to be reported at the next meeting. Cable congratulations were sent to Michel Eugene Chevreul, senior member of the French Academy upon the completion of his ninety-fifth year. The reading of papers continued.

WEDNESDAY, SEPTEMBER 1ST.

The seventh and last day of the meeting was opened at the Institute of Technology, which had been found so convenient and well adapted for all purposes of the Association. Mr. George Engelman, of St. Louis, Mo., was chosen vice-president of the Natural History Section. The following gentlemen were elected a committee on stellar magnitudes: Professor E. C. Pickering, chairman, L. Boss, S. W. Burnham, Asaph Hall, William Harkness, E. S. Holden, Simon Newcomb, C. H. F. Peters, Ormond Stone and C. A. Young. The committee is to select a list of standard stars, to which the magnitudes of other stars may be referred. The following gentlemen were elected a committee on standard time: O. Stone, chairman, S. P. Langley, E. C. Pickering, J. R. Eastman, L. Boss, Leonard Waldie, J. K. Rees, G. W. Hough and H. S. Pritchett. The following resolution was passed:—

Dr. Charles T. Jackson, one of the founders and an early president of the Association of American Naturalists and Geologists, having, after many years of illness and seclusion, just passed away, it is fitting that this Association express its high appreciation of his long and valuable services, both as an original investigator in American geology and mineralogy, and as a teacher of chemistry, which will cause his name to be long held in honor and in grateful remembrance.

The following resolutions were passed on Tuesday:

Resolved, That the American Association for the Advancement of Science recognizes the value of contemporaneous observations at numerous and well-selected stations, and with standard instruments, as a first and indispensable condition of converting meteorology from a chaotic mass of useless facts into a science.

Resolved, That this Association acknowledges its obligations to the first secretary of the Smithsonian Institution for originating, supporting and cherishing such a system of meteorological observations throughout the vast domain of the United States until it had outgrown the resources of the institution, had justified its continuance by proved usefulness, and had awakened the fostering interest of the government.

Resolved, That, in the opinion of this Association, the welfare of commerce and agriculture, and the comfort of every member of the community have been promoted by the weather reports and weather charts which have been issued by the chief signal service at Washington, while they have, at the same time, furnished food for scientific thought.

Resolved, That the Association feel and would hereby express the great loss which this service has suffered in the recent death of its chief officer, General A. J. Myer, whose energetic administration of novel duties, seconded by his able corps of scientific assistants, has commanded universal respect at home and abroad.

Professor N. P. Lupton, of Vanderbilt University, was added to the committee on the best methods of scientific teaching in the public schools. The following were chosen a committee on the registration of deaths, births and marriages: E. B. Elliott, F. B. Hough, J. B. Kellebrew, Joseph S. Copes and E. T. Cox.

It was voted yesterday to accept the invitation from Montreal for the meeting of the Association in 1882.

CONCLUSION.

The sections had all adjourned in the afternoon. In the evening a general session was held in Huntington Hall, President L. H. Morgan in the chair. About 250 ladies and gentlemen were present. A committee was appointed to confer with the President of the United States on the appointment of a chief signal officer. The committee includes Professors Brush and Barker, Dr. Bell, President Gilman, Professor Harkness, Mr. L. H. Morgan, Professor Clarke and Mr. A. Hall. The Association voted its thanks to those who had helped toward making the re-union of 1880 so pleasantly successful. The respective resolutions were supported by remarks from Professor Harkness, Judge Henderson, Professor Nason, the Rev. Mr. Shackelford, Professor Lattimore, Dr. J. Lawrence Smith, the Messrs. Hovey and Procter, and from the chair. The American Association for the Advancement of Science was then pronounced adjourned, to meet again, for the thirtieth time, at Cincinnati, on the 17th of August, 1881.

WE continue the publication of the addresses, and offer this week that by Dr. Asaph Hall, of Washington, and the Eulogy, by Professor A. M. Mayer, on the late Professor Joseph Henry, both of which we present in full; also abstracts from the following papers prepared by the authors:—The Photophone, by A. G. Bell; Mounds of Illinois, by William McAdams; Determination of the Comparative Dimensions of Ultimate Molecules, by W. N. Norton; Plan of the Cerebro-Spinal Nervous System, S. V. Clevenger; Observations of the Planetary Nebulæ, by E. C. Pickering; Co-efficients of Gas Solutions (Cut) by E. L. Nichols and A W. Wheeler; The Wyandottes, by J. W. Powell; Ancient Agricultural Implements of Stone, by William McAdams; The Endo-Cranium and Maxillary Suspensorium of the Bee, by George Macloskie; Further Notes on the Pollination of Yucca, and on Pronuba and Prodoxus, by C. V. Riley; Simple Device for Projecting the Vibration of Liquid Films without a Lens, by H. S. Carhart; On Land Snails of the Palæozoic Period, by J. W. Dawson; The Structure of Mica Veins in North Carolina, by W. C. Kerr; Transformation of Planorbis, by A. Hyatt; The Languages of the Iroquois, by Mrs. E. A. Smith.

ADDRESS BY PROFESSOR ASAPH HALL.

Fellow-Members of the Association:—

Astronomy, in some of its forms, reaches back to the most distant historical epochs, and the changes that it has undergone during this long lapse of time give to this science a peculiar interest. In no other branch of human knowledge have we such a long and continuous history of the search after truth, of the painful struggle through which men have passed in freeing themselves from theories approved by the wise of their own times, and in overthrowing beliefs which had become incorporated into the life and culture of those times. Perhaps the grand array of the heavens, and the vast phenomena which they display, naturally led men to the invention of complicated theories; but these passed away at last before the test of observation, and the criticism of sceptical men; and the Copernican theory of our solar system, Kepler's laws of elliptical motion, and the Newtonian law of gravitation, gave to Astronomy a real scientific character.

The discovery of the laws that govern the motions of the heavenly bodies, and the construction of the theory of these motions, demanded from practical Astronomy better observations and a more accurate determination of the orbits of the planets and the moon, or of the constants that enter into the problems of celestial mechanics; and this demand led to an improvement in the instruments, and in the art of observing. The astronomers and instrument-makers of England and France led the way in these improvements. The great national observatories of those countries were established, and in England Flamsteed and Sharp, Bird and Bradley, were foremost in raising practical Astronomy to the condition of satisfying the demands of theory. But theoretical Astronomy was soon to receive a wonderful advancement. Perhaps no one contributed more powerfully to this progress than Lagrange. The writings of this man were models of simplicity and elegance, and yet so complete and general are his investigations that they contain the fundamental theorems of celestial mechanics. By the invention and perfection of the method of the variation of the arbitrary constants of a problem, and by the establishment of the differential equations of a planetary orbit depending on the partial differential coefficients of a single function, Lagrange reduced the question of perturbations to its simplest form, and gave the means of deducing easily the most interesting conclusions on the past and future condition of our solar system. To supplement this great theorist there was needed another kind of genius. Combining the highest mathematical skill with unequalled sagacity and common sense in its application, Laplace gathered up and presented in a complete and practical form the whole theory of celestial mechanics. Besides his numerous and brilliant discoveries in theoretical Astronomy, Laplace gave us some of the finest chapters ever written on the theory of attraction,* and a complete treatise on the calculus of probability.

By such labors as these the questions of Astronomy were brought into order and classified, and the attention of Astronomers was directed better than ever before to the determination of the quantities which must be found from observation. Moreover, the refinement of analysis and the completion of theory brought out new and more delicate questions, not less interesting, and requiring more complete investigation and more powerful instruments. The careful examination and study of the instruments and methods of observation became necessary, as well as complete and rigorous methods of reduction; and finally there was needed a critical and satisfactory method for the discussion of observations. For these last improvements in Astronomy we are indebted chiefly to the astronomers and mechanics of Germany.

Among those who contributed by means of their optical and mechanical skill to furnish Astronomy with the instruments necessary for its further advancement, no one holds a more honorable place than Joseph Frauenhofer. This man began his scientific work at the age of twenty-two, and died at thirty-nine, and yet in those seventeen years he gave to Astronomy great improvements in the manufacture of optical glass, driving clocks for equatorials, and telescopes and micrometers, that in the hands of Bessel and Struve gave to observations a degree of accuracy hardly thought of before. To such men as Frauenhofer and his co-workers, who have carried on and improved the construction of instruments of precision, practical Astronomy owes much; and yet, after all, the principal thing in a science is the man himself. No matter how excellent the instruments may be, the question whether they shall be used for the advancement of the science, and shall contribute the full value of their peculiarities to help towards increasing the accuracy of astronomical determinations.

* "Ein schönes Document der feinsten analytischen Kunst."—GAUSS

depends wholly on the astronomer. Again, astronomy is now so completely a science, and all its operations are so closely connected with theory, that no one is fit to have charge of an extended series of astronomical observations who has not a fair amount of theoretical knowledge. Without such knowledge his labor is apt to be thrown away, and is never so effective.

As a good example of what the modern astronomer should aim to be, we may take Bessel. To this man we owe a large part of our best methods for the examination and determination of the errors of our instruments, and the introduction of complete and rigorous methods for the reduction of observations. Bessel's reduction and discussion of Bradley's observations was a masterpiece of its kind, bringing out the value of Bradley's work, which had lain unnoticed for more than half a century, and forming a starting-point for sidereal astronomy. This work was continued and perfected in his tables for the reduction of astronomical observations, published twelve years afterwards; a work that has done more than anything else to introduce order and system into practical astronomy. In the discussion of instruments and the determination of their error , Bessel's conception of an instrument was that of a geometrical figure, and the positions of the lines and divisions of this instrument were considered with corresponding rigor. Although devoted almost entirely to astronomy, yet Bessel was an able mathematician, and of this he has left abundant proof. It seem to be necessary that a man should die and be forgotten personally before his work can be fairly estimated; but time adjusts these matters at last, and I know of no astronomer whose work promises to endure the judgment of the future better than that of F. W. Bessel.

It has been said that for producing the most puzzling compound of metaphysics and mathematics, something which has neither height nor depth, nor length nor breadth, and which no one can understand, the German mathematician is unequalled. And at the same time it must be said that, for clearness of conception, and beauty and precision of expression, Germany has produced in Gauss a mathematician who is unsurpassed, and who is worthy of a place by the side of Lagrange. Omitting all reference to the works of Gauss in theoretical astronomy and in geodesy, which are many and important, I refer here only to his method for the discussion of observations, and of deducing the most probable values of our constants. Almost the entire work of astronomy is a vast system of numerical approximation, in which the first steps are obvious and easy, but where the theory soon becomes complicated and the labor enormous. Thus the calculation of the approximate orbit of a planet or of a comet is the work of only a few hours; but the computation of the perturbations, and the correction of the elements from all the observations, may be the work of months and years. It is therefore of the highest importance that we should have a method for the discussion of observations that will give us the best result, and which will introduce order and system into this department of astronomy. Such a method is that of least squares. For the complete theory of this method, and for nearly all the arrangements and algorithms necessary for its practical application, we are indebted to Gauss. The invention and application of this method to the discussion of observations of all kinds seems to me one of the greatest improvements of modern times, and its proper use will lead to a steady progress in astronomy. We must remember, however, that this method does not undertake the improvement of the observations themselves, as some have seemed to think; but, when rightly used, it produces simply the best result we can hope for from a given series of observations. It does not, therefore, dispense with skill and judgment on the part of the astronomer, but one is tempted to say that, if he has not these prime qualities, then the next best thing for him to have is the method of least squares. The use of this method has become one of the chief characteristics of modern astronomy, and if we compare the results of its application with those of the older methods, we shall see its superiority. Thus, for example, no astronomer of to-day, who is accustomed to the modern methods of discussion, would be satisfied with the manner in which Bouvard represents in his tables the observations of Jupiter and Saturn, but would suspect at once some error in his theory of the motions of these planets.

The present condition of astronomy is the result of the continued labors of our predecessors for many generations; and to this result the lapse of time itself has largely contributed. For the full development of the secular changes of our solar system, for an accurate knowledge of the proper motions of the stars of our sidereal universe, and of the great changes of light and heat that are going on among them, the astronomer must wait until future ages. It is his present duty to prepare for that future by making the observations and investigations of his own day in the best manner possible; and to do this needs a careful consideration of the present condition of the science. Although the objects for observation have become so numerous, and the range of investigation so wide, that there is room for the most varied talent and skill, yet there is danger that there may be a waste of labor, either in duplicating work, or in doing it in an improper manner. Especially may this happen in observations of the principal planets of our system, and of the fixed stars. In the case of the planets the observations are abundant, and the orbits are already well determined, except that of Neptune, for which, on account of its slow motion, we must of necessity wait for time to develop its small peculiarities, if such there be. For all these planets the observations at one or two observatories are amply sufficient, and even then the observations ought to be confined to a short time near the opposition, or at quadrature, and so made that they may be easily combined into a single normal position, which will suffice for the theoretical astronomer. To scatter such observations over a period of several months is to throw away one's labor, and to leave to the computer the disagreeable duty of rejecting a part of the observations as useless. It seems to me, therefore, unwise for several observatories to continue heaping up observations of the four outer planets of our system, when ten observations a year of each planet will give all the data that are needed. Again, for all the principal planets, observation is now in advance of theory, except, perhaps, in the case of one or two of them. Thus, for Saturn, all the tables are decidedly in error, and, although an attempt has been made to accuse the observations of this planet, it is quite certain that the trouble lies in the theory; for in the case of Jupiter and Saturn we have the most complicated planetary theory of our system, and one that has not yet been completely developed. It seems to me, also, that observations of our moon might well be confined to one or two observatories. Here again observation is far in advance of theory, if indeed there be now in use a pure lunar theory. All the lunar ephemerides that we have are affected with empirical terms, and the lunar theory itself remains an unsolved mystery. In this case there is no attempt to impeach the observations. The trouble seems to be with the perturbations of long period, and this does not call for numerous observations during each lunation. By a proper consideration of these matters astronomers may, I think, save themselves much useless labor.

Observations of the fixed stars are of the utmost importance in astronomy, since the positions of the stars are the fundamental points on which depends our knowledge of the motions of the planets, the moon, and of the stars themselves; and it is on account of this fact that Bessel's tables, published in 1830, were of such great service, since they introduced correct and elegant methods of reduction, and clearly defined all the constants and epochs. We now have the positions of several hundred stars so well known that they may be safely used in the reduction of observations; and for these accurate positions we are largely indebted to the astronomers of the Pulkowa Observatory, who have made such absolute determinations a special work. There is still an opportunity for the improvement of these positions, and every well-executed determination will be of value; but it is doubtful if crude and irregular observations can add anything to our knowledge of the positions of these stars. Neither can the routine, mechanical style of observing, that is apt to prevail in large observatories, be of much use here. It would be better in most cases for such observatories to assume the positions of the fundamental stars, and to leave the farther improvement of their places to skillful astronomers who understand the theory of such work, and who carefully study and become masters of their instruments. In these refined observations the refraction of light by our atmosphere also plays an important part, and this question will need to be examined at every observatory that undertakes to do independent work. It is true that every new and good meridian instrument may, and perhaps ought, to contribute something towards removing constant errors, and giving us a more accurate knowledge of a star's position; but when this position is very well known, the only way for further improvement is through complete and careful observations, and their thorough reduction and discussion.

In the observations of double stars but little had been done before the present century, and the labors of W. Struve form the real starting-point in this branch of astronomy. These labors have been ably continued by his son, the present director of the Pulkowa Observatory, and the observations of these two astronomers, extending over a period of nearly sixty years, are of the greatest value for our knowledge of the motions of the double stars. This is a branch of the science into which irregular workers are apt to enter, and where some of them have done good service; but if any amateur astronomer will compare his own work with that of the Struves, and will study the methods followed by them in determining their personal and instrumental errors, and will emulate the steadiness with which they have followed out their purpose, he can do much to enhance the value of his labor. Here the observations are simple, and easily reduced, and the chief requisites are skill and patience on the part of the observer. He should not be discouraged because he obtains no immediate or great reward for his work, or public notice, or because some one who rants about the nebular hypothesis and kindred subjects, of which he knows nothing, is for a time the great astronomer of the day. The observer will learn finally that a good observation of the smallest double star, or of the faintest comet or asteroid, is worth more than all such vague talk. The observation has a positive value, however small, but the physical theories of the universe, of which modern popular science is so productive, are generally worse than useless.

The first step towards a rational and trustworthy knowledge of our sidereal universe must come from a determination of the distances of the stars. The solution of this problem was attempted soon after the Copernican theory of our solar system was established, when it was seen that we have a long base line for our measures, or the diameter of the earth's orbit, and it was supposed that the solution would be easy. These early trials were all failures, but they led to some very interesting and important discoveries, such as Bradley's discovery of the aberration of light; to the knowledge of the fact that the determination of the parallaxes, or the distances of the stars, although simple in theory, is practically a difficult question; and then to an improvement in the instrumental means of observation, to a careful study of the methods of observation and the instruments, and to a recognition of the necessity of a complete and rigorous reduction of the observations. An examination of these early attempts is an instructive study. It is only about forty years ago that the solution of this problem was at last attained, and then only by the application of the most powerful instruments and the best observing skill. An interesting result of the determinations of stellar parallax is obtained at once in the check it puts on speculations concerning the structure of the sidereal universe. The first astronomers who considered the parallaxes of the stars very naturally assumed that the bright stars are nearer to us than the faint ones, and therefore they observed the bright stars for parallax. Now, while this assumption may be true as a general statement, the actual determinations of parallax show that some of the faint stars which are not visible to the naked eye are much nearer to us than the brightest stars of our northern sky. Again it was assumed that a large proper motion is a certain index of a star's nearness to us; but observation shows that this also may be an erroneous assumption. This is a problem whose solution is only just begun, but already we know enough of its difficulties to see that we need the most powerful micrometrical apparatus that can be brought into use. The invention of some micrometer that, while as accurate as the present filar micrometer, would give the observer a much greater range of observation, and enable him to select suitable stars of comparison, is something much to be desired. At present the heliometer seems to be the best instrument for observations of this kind. Formerly it was thought that photography would furnish a good method for such delicate determinations; but so far the photographic methods have not given the necessary degree of accuracy in the measurements, and the astronomical use of photography is confined mostly to descriptive astronomy, where, especially in solar eclipses, it has rendered excellent service. Closely connected with the parallaxes of the stars and their proper motions is the interesting question of determining their motions to or from our sun, according to the theory of Doppler. Here likewise the numerical determinations are so discordant, that we cannot have much confidence in the results. In both these cases we need more powerful apparatus, and a complete and thorough investigation of the methods of observation. Perhaps some of the large instruments now constructing may be employed in these methods, and we may soon have better results.

A great advance has been made in catalogueing the fainter stars. This work was begun by the French astronomers nearly a century ago, and was continued by Bessel, Argelander, and others. An important step towards the completion of this work was taken by Argelander and his assistants in their great catalogue of the approximate positions of 324,198 stars, which was finished in 1861. This census of the stars will soon be extended, we hope, over the whole heavens; and it already forms the groundwork for the great zone observations of stars now going on in Europe and in this country, and which must be nearly finished. These observations will doubtless reveal many interesting cases of the proper motion of the stars, and will certainly form the basis for a knowledge of the motion of our solar system in space, and for sidereal astronomy generally, such as we have never had before. Our American observatories can render a good service by observing stars of southern declination, since our observatories are ten or twelve degrees farther south than those of Europe, and thus have an advantage of position which ought to be made use of; and which may serve to unite into a harmonious system the observations made in the northern and southern hemispheres. The work of mapping the very faint stars near the ecliptic has also been greatly extended, and it is to this extension that we owe the rapid increase in the number of the small planets between Mars and Jupiter. But besides aiding in the discovery of the asteroids, accurate charts of the small stars have a permanent value in giving us a knowledge of the heavens at their epoch, and also some idea of the distribution of the stars in space.

It is an interesting question whether, among the thousands of nebulæ that are scattered over the heavens, any of them show changes of form or of brightness. These objects seem to be at least as distant as the stars, and as they have sometimes an area of several degrees, they must be bodies of an enormous extent. That changes are going on in these bodies seems probable, but to be visible at such distances the changes must be very great. In this case there is need of much caution in the discussion of the drawings made at different epochs, and by different astronomers with telescopes of different power; since the nebulæ change their appearance with the telescope used, with different conditions of the air, and with a variation of their altitude above the horizon. Here the excellent photometers that have been recently invented, and which are being so well applied to the determination of the brightness of the stars, may give us assistance. Perhaps also new drawings of the nebulæ, and their criticism and discussion, and a full recognition of the difficulties of making such drawings, will soon lead to a decision of the question of their change of form. Since the study of the light of the stars with new and improved photometers has now become a specialty, we may look for more exact and continued observations of the variable stars. This is a matter of which we know but little, and it is one where a persevering observer may do good service. Although he may not find any immediate encouragement in the discovery of remarkable relations among these stars, or the probable cause of their variability, he will be collecting observations that must form the test of every theory. As examples of the result of intelligent and persevering observation, we have the case of the sun spots, which led directly to the discovery of their period and its singular variability; and that of the shooting stars, which has shown us a very curious relation between these meteors and the comets, and one which may open to us the most extensive views of the relations between our own solar system and other systems in space.

The present condition of astronomy, with its vast and rapidly increasing store of accurate observations, offers many interesting subjects to the theoretical astronomer. The observations of the stars are now so numerous, and have been so fully reduced and criticised, and the time during which the observations have been made is so extended, that we shall soon have excellent data for a new and very exact determination of the constant of precession. The orbits of the planets and the moon, and their masses, are now so well known that little uncertainty can arise from this source; and by taking into the calculation a great number of stars in different parts of the heavens, we may be able to determine the motion of the solar system in space, as well as the constant in precession. The constant of aberration also needs a new determination; and since this constant is so closely connected with the theory of light and its velocity, and the methods of its determination are still under discussion, it would be well if several astronomers could determine this constant independently. The value we now use was found by W. Struve from prime-vertical observations, and is apparently very accurate; but no astronomical constant should depend on the work of a single astronomer with a single instrument, when it can be determined so easily and by other methods. The old method of finding the value of this constant from the eclipses of Jupiter's satellites may yet give us a trustworthy value. The value of the other constant necessary for the reduction of observations, that of nutation, must be nearly that found by Peters in his well-known investigation of this question. This value may be verified by a new series of observations of Polaris, or of the declinations of stars situated so that this constant has its full influence on the reductions.

There are many subjects in astronomy that need investigation, but in most cases the labor required is very great, and the completion of the work would occupy a long time. This follows of course from the fact that, with the refinement of observations and their exact reduction, many small terms must be considered which formerly could be neglected. The lunar theory has been a vexed question for the last two centuries, and may remain so for a long time to come. This will no doubt be the case until some able astronomer, with the will and perseverance of Delaunay; shall undertake its complete revision. This question should now be looked on as a purely scientific one, and its definite solution should be undertaken. The theory should not be patched up by guesswork to fit the observations, but should be carried out with the utmost rigor. This is a problem to which a young and able mathematician may well devote his life, and we must expect its solution from some such clear-headed devotee of science. Several of the planetary theories need a new investigation, and some of them are already in the hands of able astronomers. That of Mercury is especially interesting in connection with the intra-Mercurial planets, and it is to be hoped that Leverrier's theory of this planet may soon have a careful revision.

Again, among the secondary systems, the satellites of Jupiter and Saturn offer many interesting questions to the astronomer. At present the satellites of Jupiter demand a more complete theory, and new tables of their motions. Corrected elements of these satellites may be required for reducing observations of their eclipses, and for deriving a new value of the constant of aberration. These satellites form a peculiar and interesting system, and their theory is so complicated that the labor of correcting their elements and forming new tables would be great, but still within the power of a persevering astronomer. The recent discovery of the connection of comets with streams of meteors has given additional interest to cometary astronomy, and there is plenty of hard work to be done in reducing observations, in computing perturbations, and in deducing the best orbits of the comets. The periodical comets have another interest, since they may give us information concerning the matter filling space. It seems to be probable from different reasons, such as the consideration of the light of the stars, that there must be matter spread throughout the celestial spaces; but the only heavenly body that has directly given us information on this subject is Encke's comet, which has a period of $3\frac{1}{3}$ years. For a long time the motion of this comet was very completely computed by Encke, whose calculations show very strong proof of a resisting medium. These calculations were continued by Von Asten, whose early death prevented him from finishing his work, and the theory of this comet is left in an unsatisfactory condition. It is very desirable that the motion of this comet should be completely investigated, and although the method of the special perturbations of the elements followed by Encke is probably the best that can be used, still in such a case it would be well to apply various methods. Here again, on account of the frequent returns of the comet, the labor of computation is very great, and probably would be enough fully to occupy the time of one astronomer. The interesting questions connected with the motion of this comet ought to induce some one to undertake this laborious work, and these questions are so important that two or three astronomers might well be employed on its theory.

The methods of astronomy have now become so well established, that the future advancement of the science is assured, especially since long intervals of time give an increased value to observations. Yet we may hope for improvement in instruments, for the introduction of new methods of observing, for better trained and more efficient astronomers; and perhaps also the rapid advancement of the physical sciences may furnish us with new and more powerful methods of investigation. There is an intimate relation between the instrument-maker and the astronomer, and they should understand each other better than is generally the case. It may seem a small matter that the divisions of a circle, or of a scale, should not be too finely or too coarsely cut; that the reading scale should not be placed in an inconvenient position, and that the illumination of the instrument should be carefully studied, and brought under the control of the astronomer; but these are really essential points, and, if not rightly arranged, are certain to weary the observer and to impair the quality of his work. Such mistakes will not be remedied until the makers better understand the uses of an astronomical instrument, and have correct ideas of the ends to be attained. Since our American opticians have placed themselves at the head of their craft, we may hope that our instrument-makers will do likewise, and that they will soon be able to furnish us with the best instruments of precision.

There is one point to which astronomers should give more attention, and from which we may reasonably hope that great advantages to astronomy may come; and that is to the selection of sites for new observatories. It is possible, perhaps probable, that our instruments may be greatly enlarged and improved, and that important discoveries and improvements in the manufacture of optical glass may be made; but it seems certain that we have within easy reach very decided advantages for astronomical work by the choice of better positions for our instruments. Very few American observatories have been established for the purpose of doing scientific work, or with much thought or care for their condition; but generally they are built in connection with some college or academy, and are the product of local and temporary enthusiasm, which builds an observatory, equips it with instruments, and then leaves it helpless. The atmosphere that surrounds us, and its sudden changes of temperature, are the great obstacles to the good performance of a telescope; and the larger the instrument, and the higher the magnifying power, the more serious are these hindrances. Now, with our present means of travel, we can easily place our instruments at an altitude of eight or ten thousand feet, and above a large part of the atmosphere. In this way we may be able to do with small instruments what at common altitudes can be done only with large ones; and when possible it is always better to use small instruments, since they are more easily handled, and are relatively stronger and better than large ones. Uniformity of temperature may be secured by seeking locations in the tropical islands, or on the coasts like that of California, where the ocean winds keep the temperature nearly uniform throughout the year. At great altitudes we may secure a clearness of vision that would be of the greatest value in the examination of faint objects, and by this means, and by persevering and continuous observation, interesting discoveries may be made. It is a matter of course that, except in the case of comets, the future discoveries in astronomy will belong to faint and delicate objects; but these are interesting, and should not be neglected. A uniform temperature, which secures good definition, and steady images of the stars, is necessary for accurate determinations of position, and for all measurements of precision. This condition is especially important in such work as that of stellar parallax, the determination of the constant of aberration, and wherever the yearly change of temperature may act injuriously. In the selection of better sites for observatories, I think we have an easy means of advancing astronomy.

As this science grows and expands, it will become more and more necessary to study the economy of its work, in order that astronomers may bestow their labors in the most advantageous methods, and may rid themselves of all cumbersome and time-consuming processes. The manner of publishing observations has already been much abbreviated, and improved, I think, by some of the European astronomers, and this change seems destined to become universal. As the positions of many objects are now well known, the need of printing all the details of the observation, such as the transit of the wires, the readings of the micrometers, etc., is very slight; and this printing may be safely abandoned. Even this change will lead to a great saving in the time and cost of printing. But this will necessitate a more complete discussion of the work and a more careful examination of the instruments; things to be desired, since they tend to lift the observer out of his routine, and make him a master of his business. There are objections to this change, and some of them are real, such as the importance of publishing a complete record; but this is overestimated, I think, since the original records ought always to be referred to in case of doubt; and other objections are factitious, such as the need of publishing a large a showy book in order to impose on the public.

We may hope also for improvements in theoretical astronomy, and for the better training and preparation of students of this science. I know that it is sometimes said that theoretical astronomy is finished, and that nothing more can be done. Such assertions come from professors who are old and weary, or from those young men who tire out early in life; but they are wrong. The improvements that Hansen has made in the theory of perturbations, and Poinsot's study of the theory of rotation, show what careful investigation may do, and assure us of further progress. It must be confessed that some of the astronomical work done in our country bears evidence that the astronomers did not understand the correct methods of reduction, and much of it shows evidence of hasty and ill-considered plans. This is perhaps a natural condition for beginners, but we trust that it has been outgrown. An actual need for the astronomical students of our country is a good book on theoretical astronomy, similar to Pontécoulant's work, in which the whole subject shall be presented in a complete form, such as we find in the *Mécanique Céleste*, together with an account of the improvements made by Gauss, Poisson, Hansen and others. There is no American book of this kind, and the English works are too partial, designed apparently to fit the student for college examinations, and not to give him a complete knowledge of the science. Such a book has hardly been attempted in our language, unless that of Woodhouse may be an exception, and it may be a long time in coming, since it requires a man qualified to do the work, and will involve an expense of labor in the preparation, and of cost in publishing, such as few are willing to incur. In the meantime it is far better for the student to go directly to the writings of Lagrange and Laplace, of Gauss and Poisson and other masters, rather than to spend time in reading second-rate authors who endeavor to explain them. And generally this will be found the easier way also, since the student avoids the confused notions and symbols, and the grotesque expressions and egotism of small men, and is lifted into the region of ideas and invention.

In presenting his exposition of the nebular hypothesis, which has since become so celebrated, Laplace says: "I present this hypothesis with the distrust which everything ought to inspire that is not a result of observation or of calculation." It is a singular fact that, among all the writings on the nebular hypothesis, I have never seen a reference to this presentation of it by its most distinguished advocate; and yet this is the true spirit of scientific astronomy. Laplace did not wish to exempt his own theories from criticism, and neither should anyone. In astronomy there is no final human authority, no synod or council, but simply an appeal to reason and observation. If a theory or a discovery be true, it will stand the test of observation and of calculation; if false, it must pass away to that Miltonian limbo where so many things have gone and are going. The question is sometimes asked, of what use is astronomy? and the reply generally made is that it has conferred great benefits on navigation and on commerce, since it is by means of his astronomical knowledge that the sailor determines the position of his ship on the ocean. There is a truth in this reply, but it is only partial. The great value of astronomy is that it is really a science, and that it has broken the path and led the way through which all branches of science must past if they ever become scientific. It is the spirit of honest, unrelenting criticism, and of impartial examination, that finally eliminates error and awards to every one his just due, that makes astronomy honorable and attractive; and it is by cultivating this spirit that astronomy confers its chief benefit, for it is this that shall break in pieces and destroy all false assumptions in science and in philosophy.

JOSEPH HENRY.

EULOGY BY PROFESSOR A. M. MAYER.

At the meeting of the Association in 1878, a committee, composed of Professors Baird, Newcomb and myself, was appointed to prepare a eulogy on our revered and lamented colleague and former president, Joseph Henry. This—I will not say labor, but duty of affection—has devolved on me alone. I would that the other members of this committee had laid before you their tributes to his memory, because for years they had been closely associated with him in his social and professional life in Washington. Yet, while Professor Henry had been the friend of their manhood, he was the friend of my boyhood; and during 25 years he ever regarded me—as was his wont to say—with a "paternal interest." To his disinterested kindness and wise counsels is due much, very much, of whatever usefulness there is in me. Hence I have said that it is a duty of affection for me to speak to you about one who was my beloved friend. I shall not, however, attempt a biography of Joseph Henry, nor will I speak of his administrative life as director of the Smithsonian Institution, for this is known and valued by the whole world. His best eulogy is an account of his discoveries; for a man of science, *as such*, lives in what he has *done*, and not in what he has *said*; nor will he be remembered in what he proposed to do. I will, therefore, with your permission, confine myself chiefly to Henry *as the* discoverer; and I do this the more willingly because I am familiar with his researches, and also because Professor Henry, from time to time, took pleasure in giving me accounts of these mental conceptions which preceded his work, led him to it and guided him in it. Rightly to appreciate a discoverer, we should not look at his work from our time, but go back and regard it from his time; we should not judge his work in the fulness of the light of present knowledge, but in the dim twilight which alone illuminated him to then unknown—but now well-known—facts and laws. I will, therefore, endeavor first to present you with a clear, but necessarily very concise, view of the state of our knowledge of electricity when Henry began his original researches in that branch of science, and then point out the value of his discoveries, by showing that they added to knowledge, and how they instigated and influenced the discoveries and inventions of other men. Henry began his electrical researches at the age of twenty-eight, in the year 1827, while he was professor of mathematics and natural philosophy in the Albany Academy. At these he continuously worked till 1832, when, at the age of thirty-three, he moved to Princeton College. After a year's break in his work, caused by the preparation of his course of lectures for the college, he is again at original research, and continues his contributions to electrical discoveries till 1842. Thus, during fourteen years, between the ages of twenty-eight and forty-three, he was a constant and fertile worker.

As with many other men of originality, Henry's first essays were in the direction of improving the means of illustrating well-established scientific facts and principles. His first paper of October, 1827, is interesting because it was his first. In it he improves on the usual apparatus which had been used by Ampère and others to show electro-dynamic actions, by employing several turns of insulated wire, instead of one, as had previously been the practice. Thus, for example, to show the directive action of the earth's magnetism on a freely-moving closed circuit, Henry covered copper wire with silk, and then made out of it a ring about twenty inches in diameter, formed of several turns of the wire. The extremities of this wire were soldered to zinc and copper plates. The coil was then suspended by silk filaments. On plunging the metal plates into a glass of dilute acid the ring rotated around its point of suspension till its plane took a permanent position at right angles to the magnetic meridian. By a similar arrangement of two concentric coils, one suspended within the other, he neatly showed the mutual actions of voltaic currents flowing in the same or opposite directions, which facts are the founda ions of Ampère's celebrated law. We now reach a period when Henry appears as a discoverer, and truly one of no mean order. As I remember his narration to me in the year 1859, it was as follows: He said that one evening he was sitting in his study in Albany with a friend, when, after a few moments of reverie, he arose and exclaimed, "Tomorrow I shall make a capital experiment!" For several months he had been brooding over Ampère's electro-dynamic theory of magnetism, and he was then deeply interested in the phenomena of the development of magnetism in soft iron, as shown in the experiments of Arago and Sturgeon. At the moment he had arisen from his chair it had occurred to him that the requirements of the theory of Ampère were not fulfilled in the electro-magnets of Arago and of Sturgeon, but that he could get those conditions which the theory required by covering the developing wire with a non-conductor, like silk, and then wrapping it closely around the soft iron bar in several layers; for the successive layers of wire coiling first in one direction and then in the other would tend to produce a resultant action of the current at right angles to the axis of the bar; and furthermore, the great number of convolutions thus obtained would act on a greater number of molecules of the bar, and therefore exalt its magnetism. "When this conception," said Henry, "came into my brain, I was so pleased with it that I could not help rising to my feet and giving it my hearty approbation." Henry did go to work next day, and to his great delight and encouragement discoveries of the highest interest and importance revealed themselves to him week after week. When he had finished his newly conceived magnet he found that it supported several times more weight than did Sturgeon's magnet of equal size and weight. This was his first original discovery.

I will now give, as far as possible, Henry's own words in narrating the subsequent investigations of these very interesting phenomena: "The maximum effect, however, with this arrangement and a single battery was not yet obtained. After a certain length of wire had been coiled upon the iron the power diminished with a further increase of the number of turns. This was due to the increased resistance which the larger wire offered to the conduction of electricity. Two methods of improvement, therefore, suggested themselves. The first consisted, not in increasing the length of coil, but in using a number of separate coils on the same piece of iron. By this arrangement the resistance to the conduction of the electricity was diminished and a greater quantity made to circulate around the iron from the same battery. The second method of producing a similar result consisted in increasing the number of elements of the battery, or, in other words, the projectile force of the electricity, which enabled it to pass through an increased number of turns of wire, and thus, by increasing the length of the wire, to develop the maximum power of the iron. To test these principles on a larger scale an experimental magnet was constructed. In this a number of compound helices were placed on the same bar, their ends left projecting, and so numbered that they could be all united into one long helix, or variously combined in sets of lesser length. From a series of experiments with this and other magnets it was proved that, in order to produce the greatest amount of magnetism from a battery of a single cup, a number of helices is required; but when a compound battery is used, then one long wire must be employed, making many turns around the iron, the length of wire and consequently the number of turns being commensurate with the projectile power of the battery. In describing the results of my experiments the terms *intensity* and *quantity* magnets were introduced to avoid circumlocution, and were intended to be used merely in a technical sense. By the intensity magnet I designated a piece of soft iron so surrounded with wire that its magnetic power could be called into operation by an intensity battery; and by a quantity magnet a piece of iron so surrounded by a number of separate coils that its magnetism could be fully developed by a quantity battery. "I was," said Henry, "the first to point out this connection of the two kinds of the battery with the two forms of the magnet, in my paper in Silliman's Journal, January, 1831, and clearly to state that when magnetism was to be developed by means of a compound battery one large coil was to be employed, and when the maximum effect was to be produced by a single battery a number of strands were to be used."

We will now return to Henry's study of the properties of his intensity magnet. This magnet was formed of a piece of iron one-fourth of an inch in diameter, bent in the U form and wound with eight feet of insulated wire. His batteries were two,—one formed of a single element with a zinc plate four inches by seven, surrounded by copper and immersed in dilute acid; the other, a Cruikshank's battery, or trough, with twenty-five double plates. The plates of this battery were joined in series, and altogether had exactly the same surface of zinc as that in the single-cell battery. The magnet was now connected directly to the single cell. The magnet held up seventy-two ounces. Then five hundred and thirty feet of number 18 copper wire led the current from the cell to the magnet; it now supported only two ounces. Five hundred and thirty feet more of the wire were introduced into the circuit, and then the magnet held but one ounce. In these facts Henry faced the same results as confronted Barlow five years before, and caused Barlow then to say: "In a very early stage of electro-magnetic experiments it had been suggested [by Laplace, Ampère and others] that an instantaneous telegraph might be established by means of conducting wires and compasses, but I found such a sensible diminution with only two hundred feet of wire, as at once to convince me of the impracticability of the scheme"; and such, at that day, seemed to be the common opinion of men of science. But this opinion is presently to be shown by Henry to be ill-founded, by reason of the ignorance of the relations which have of necessity to exist between the kind of battery and the kind of magnet in order to produce electro-magnetic action at a distance—relations which Henry was the first to discover. This accomplishment justly entitles him to be regarded as a man of genius and a discoverer of no mean order. This discovery will always remain the one important fact that was to be known, to be understood, and to be applied, before it was possible to have constructed any form of electro-magnetic telegraph. Let us see how Henry made this discovery.

After ending the experiments with the one-cell battery and reaching results which seemed to confirm the opinion of Barlow as to the "impracticability of the scheme" of an electro-magnetic telegraph, Henry attached his magnet to the second battery formed of twenty-five cells, arranged in series. The current from this battery was sent to the magnet through 1060 feet of the same wire as had been used in the experiments with the first battery of one cell. The magnet now lifted eight ounces. It had held up only one ounce, when with the same length of interposed wire the battery of one cell was used. He now attached his electro-magnet directly to the poles of the 25-cell battery, when, to his astonishment, it only held seven ounces. The same magnet it will be remembered, when attached to the one-cell battery, supported seventy-two ounces. Here were facts of the highest significance, and Henry was not slow to seize them in all their bearings. Referring to these experiments, he said in 1857: "These steps in the advance of electro-magnetism, though small, were such as to interest and astonish the scientific world. These developments were considered at the time of much importance in a scientific point of view, and they subsequently furnished the means by which magneto-electricity, the phenomena of dia-magnetism, and the magnetic effects in polarized light were discovered. They gave rise to the various forms of electro-magnetic machines which have exercised the ingenuity of inventors in every part of the world, and were of immediate applicability in the introduction of the magnet to telegraphic purposes. Neither the electro-magnet of Sturgeon nor any electro-magnet ever made previous to my investigations was applicable to transmitting power to a distance."

Not satisfied with the mere statement that his discovery was "directly applicable to Mr. Barlow's project of forming an electro-magnetic telegraph," he actually constructed one, some time during the year 1831, around one of the upper rooms of the Albany Academy. It was more than a mile in length, and made signals by sounding a bell. This was the first electro-magnetic telegraph which had worked through so great a length of wire. It was the first "sounding" electro-magnetic telegraph. The relative parts played by Henry and Morse are described in Henry's "Statement" published by the Smithsonian in 1857. "*The principles,*" says Henry, "*I had developed were applied by Dr. Gale to render Morse's machine effective at a distance.*" This statement seems to be as direct, as clear, as truthful, and as comprehensive as one can desire. I will take the liberty of remarking that had Henry taken out a patent in which he claimed as his invention an electro-magnet formed of two or more layers of insulated wire, Morse's patent would not have been so valuable. Remember, I speak not of the merit of the invention, but of the merit of the patent; for the invention, so far as Morse is concerned, would have remained the same, because one essential part of a Morse telegraph is Henry's intensity magnet, and certainly Morse never invented that.

If Ohm's law had been known to Henry, with all of its consequences, when applied to his discovery of the exaltation of the electro-magnetism of iron, in connection with his discovery of the proper relations necessary between batteries and magnet to get the greatest electro-magnetic effects, his discoveries would appear dwarfed, though yet of excellent workmanship. But did he at this time, 1827 to 1832, know of Ohm's law? I infer that Henry arrived at his discoveries independently of such knowledge, and for two-fold reasons. First, Ohm's law was published as late as 1827, in Berlin, and was received almost contemptuously. Henry was unable to read German, and Ohm's papers were first published in English in 1841. Secondly, from the manner in which Henry worked at his problems and viewed his results, I conclude that he had no knowledge of Ohm's laws; else why should he have been astonished at the effects when his intensity magnet was connected with his intensity battery? Henry, now in possession of powerful magnets, began to work on another problem. He tried to do the reverse of what he had already done. His magnet was made by the action of the electric current, and he now tried to obtain an electric current from the magnet; and he succeeded. Henry and Faraday independently discovered the means of producing an electric current and spark from a magnet. Tyndall speaks of this experimental results as the "Mont Blanc of Faraday's own achievements." A few words now will place Henry in his proper and just relations to these important discoveries. All the information he had received about Faraday's discovery was the account of Faraday's production of magneto-electricity by the sudden insertion of a magnet into a helix and its sudden withdrawal therefrom. Henry's experiment is entirely different, and certainly was entirely original with him; but it is essentially identical with another of Faraday's of which Henry had no knowledge. Thus it appears that, although Henry cannot be placed on record as the *first* discoverer of the magneto-electric current, he stands alone as its *second* independent discoverer.

Henry's next discovery was that of the induction of a current upon itself, or of the extra current, as it is sometimes called. Here he anticipated Faraday by nearly two years and a half in the observation of the fundamental facts. Notwithstanding an explicit disclaimer of Faraday, the credit of this discovery has been generally given to the latter. This is accounted for by the fact that, although Henry anticipated others in his observations, he had not leisure to follow them up to their full explanation until after Faraday had completely unraveled their nature. In 1838, after his return from a first visit to Europe, Henry discovered an entirely new class of phenomena in electrical induction. He first showed that an induced current may excite a second induced current in a neighboring closed conductor, that this last may induce a third current, and so on. These currents Henry styled currents of the first, second, third, etc., orders, and he showed that they alternate in their direction successively. He investigates the difference in these currents as they flow through different resistances. The same phenomena he tracks through the inductive sections of the discharge of the Leyden jar and of the frictional electrical machine, and shows how they differ from those produced by the voltaic battery. These researches are the most finished of Henry's investigations, and will ever be regarded as models of careful and thorough scientific work.

Henry had a versatile mind, and did not confine his attention to the study of electricity. His researches in molecular physics, though not extensive, are remarkable. Here his suggestions and methods have stimulated others to follow in the paths which he has pointed out. In 1839 Henry made a curious discovery as to the permeability of lead to mercury. He found mercury would even ascend a lead wire to the height of a yard in a few days. He even made what might be called syphons of lead, which would nearly empty a vessel of mercury by drawing the fluid over its sides. Subsequently, in 1845, with Mr. Cornelius, he proved that copper, when heated to the melting point of silver, would absorb the latter metal. In 1844 Henry was investigating the nature of the forces acting in liquid films. Studying the tenacity of the soap-bubble film, although his experiments could only furnish approximate results, they showed that the molecular attraction of water for water is really several hundred pounds to the square inch, and probably equal to the attraction of ice for ice. Another of Henry's investigations, having a practical bearing, should be more widely known than it is. Among his duties as chairman of the United States lighthouse board was the testing of the various physical properties of the oils submitted to the government for purchase. Fluidity was one of these properties for which it seemed most difficult to get reliable tests. Here he very ingeniously applied the theorem of Torricelli, which shows that equal quantities of all liquids of equal fluidity will flow out of an orifice in equal times. Henry found that with different oils the flow of equal quantities differed, the rapidity of flow of sperm oil exceeding that of lard oil in the ratio of 100 to 167. Alcohol proved to be less fluid than water. Henry took a deep interest in acoustics. His additions to this science were chiefly the results of experiments upon fog signals. He made extensive experiments with various sound-producing instruments, and eventually decided in favor of the steam syren fog-horn. He determined that these instruments send their sound farthest when tuned very near to the treble C, and he also showed the uselessness of applying reflectors to them. During eleven years Henry sought to advance the efficiency of our fog signals by experiments in all weathers. Many very puzzling facts were collected. Thus it was observed that a sound coming to a mariner against the wind would cease to be audible on the deck of his vessel while it continued to be heard at the masthead. It was also observed that upon approaching a fog-horn from a distance the intensity of sound would gradually increase, then die down rapidly, become inaudible through a space of three or four miles, and perhaps not reappear until the vessel was within a mile of the instrument. These facts demanded explanations, and for a long time remained enigmas to Henry, till one day he met with a paper by Professor Stokes, in which the effect of an upper current in deflecting a wave of sound is fully explained. This hypothesis of Stokes Henry was able to apply to the solution of the problems in question.

Henry's services to the light-house board were of great value to the country. The fact that his investigations showed that lard oil heated to about 250° Fahrenheit is superior in fluidity and illuminating power to sperm oil caused the substitution of the former for the latter. A dollar a gallon was saved, which amounts to about one hundred thousand dollars a year in favor of the government. In light and heat Henry made several investigations which we must pass over. One, however, is so important that it cannot be omitted. I refer to his application of the thermopile in determining the distribution of heat on the optical images of distant objects. In a bold, and wonderful experiment, he sought to study the distribution of heat on the surface of the sun. In 1845, with Stephen Alexander, he formed an image of the sun, by means of a telescope, upon a screen. In this screen was cut an aperture, closed by the surface of a thermopile. By a motion of the telescope, any part of the image could be brought upon the pile. A solar spot being present, he clearly proved that it emitted less heat than the surrounding parts of the luminous disc. This method of research was shown to Secchi. On his return to Europe the latter made no small repute by extending these observations, using Henry's methods, but often, I fear, not giving full credit to the originator. But let that pass, for the bread which Henry cast upon the waters has returned to our own shores, thanks to the genius of our colleague Langley.

It is impossible to crowd into one brief hour the thoughts which were his occupation during more than half a century. I have at least endeavored to exhibit the more important part of the labors of his life. What shall we think of them? Surely they are on as high a plane as those of any of his contemporaries, and show as much originality as theirs in their conception—as much skill in their execution. Yet it has been said that Henry was not a man of genius. As I have not been able to find that the philosophers who have the special charge of giving from time to time definitions of genius, have been able to come to any satisfactory conclusion among themselves, I will leave their company, and, with your liberty, take my definition from a book which, if we accredit Thackeray, is one of the very best, if not the best, novel ever written in English. After listening to this I will allow you to form your own opinions as to whether Henry did or did not possess genius. "By genius I would understand that power, or rather those powers, of the mind which are capable of penetrating into all things within our reach and knowledge, and of distinguishing their essential differences. These are no other than invention and judgment, and they are both called by the collective name of genius, as they are of those gifts of nature which we bring with us into the world. Concerning each of which many seem to have fallen into very great errors; for by invention, I believe, is generally understood a creative faculty, which would indeed prove most romance writers to have the highest pretensions to it; whereas by invention is meant no more, and the word so signifies, than discovery in finding out; or, to explain it at large, a quick and sagacious penetration into the true essence of all the objects of our contemplation. This, I think, can rarely exist without the concomitancy of judgment, for how we can be said to have discovered the true essence of two things, without discovering their difference, seems to me hard to conceive. Now this last is the undisputed province of judgment; and yet some few men of wit have agreed with all the dull fellows in the world in representing these two to have seldom or never been the property of one and the same person." My own judgment, if of any value, would rank the ability of Henry—I do not say his achievements—a little below that of Faraday. Indeed their lives and their manners of working were strangely alike. Faraday was the son of a blacksmith. He once wrote: "I love a smith's shop and anything relating to smithery. My father was a smith." Henry's father plied a schooner on the Hudson. Each started in life with moral and benevolent habits, well-developed and healthy bodies, quick and accurate perceptions, calm judgment and self reliance, tempered with morality and good manners—a good ground, surely, in which to plant the germs of the scientific life. Faraday was an apprentice to a bookbinder. Henry served in the same capacity under a blacksmith. Each, endowed with a lively imagination, was in his younger days fond of romance and the drama; and, by a singular similarity of accidents, each had his attention turned to science by a book which chance threw in his way. This work in the case of Faraday was "Mrs. Marcet's Conversations on Chemistry," and the book which influenced Henry's career was "Gregory's Lectures on Experimental Philosophy, Astronomy and Chemistry." Of Mrs. Marcet's book Faraday thus writes:—"My Dear Friend,—Your subject interested me deeply every way; for Mrs. Marcet was a good friend to me, as she must have been to many of the human race. I entered the shop of a bookseller and bookbinder at the age of thirteen, in the year 1804, remaining there eight years, and during the chief part of the time bound books. Now it was in those books, in the hours of the week, that I found the beginning of my philosophy. There were two that especially helped me,—the Encyclopœdia Britannica, from which I gained my first notions of electricity, and Mrs. Marcet's "Conversations on Chemistry," which gave me my foundation in that science. Do not suppose that I was a very deep thinker, or was marked a precocious person. I was a burly imaginative person, and could believe in the Arabian Nights as easily as in the Encyclopœdia. But facts were important to me and saved me. I could trust a fact and always cross-examined an assertion. So when I questioned Mrs. Marcet's book by such little experiments as I could find means to perform, and found it true to the facts as I could understand them, I felt that I had got hold of an anchor in chemical knowledge, and clung fast to it. Thence my deep veneration for Mrs. Marcet—first, as one who had conferred great personal good pleasure on me; and then as one able to convey the truth and principle of those boundless fields of knowledge which concern natural things to the young, untaught and inquiring mind. You may imagine my delight when I came to know Mrs. Marcet personally; how often have I cast my thoughts backward, delighting to connect the past and the present; how often, when sending her a paper as a thank-offering, I thought of my first instructress; and such thoughts will remain with me."

Henry wrote on the inside of the cover of Gregory's work the following words: "This book, although by no means a profound work, has, under Providence, exerted a remarkable influence on my life. It accidentally fell into my hands when I was about sixteen years old, and was the first book I ever read with attention. It opened to me a new world of thought and enjoyment; invested things before almost unnoticed with the highest interest; fixed my mind on the study of nature, and caused me to resolve at the time of reading it that I would immediately begin to devote my life to the acquisition of knowledge.—J. H." Each of these philosophers worked with simple instruments, mostly constructed by his own hands, and by methods so direct that he appeared to have an almost intuitive perception into the workings of nature; and each gave great care to the composition of his writings, sending his discoveries into the world clothed in simple and elegant English. Finally each loved science more than money, and his Creator more than either. There was sympathy between these men; and Henry loved to dwell on the hours that he and Bach spent in Faraday's society. I shall never forget Henry's account of his visit to King's College, London, where Faraday, Wheatstone, Daniell and he had met to try and evolve the electric spark from the thermopile. Each in turn attempted and failed. Then came Henry's turn. He succeeded, calling in the aid of his discovery of the effect of a long interpolar wire wrapped around a piece of soft iron. Faraday became as wild as a boy, and, jumping up, shouted, "Hurrah for the Yankee experiment." And Faraday and Wheatstone reciprocated the high estimation in which Henry held them. During a visit to England, not long before Wheatstone's death, he told me that Faraday and he had, after Henry's classical investigation of the induced currents of different orders, written a joint letter to the council of the Royal Society, urging that the Copley medal, "that laurel wreath of science," should be bestowed on Henry. On further consultation with members of the council it was decided to defer the honor till it would come with greater *éclat*, when Henry had continued farther his researches in electricity. Henry's removal to Washington interrupted these investigations. Wheatstone promised to give me this letter to convey to Henry as an evidence of the high appreciation which Faraday and he had for Henry's genius, but Wheatstone's untimely death prevented this. Both Faraday and he gave much thought to the philosophy of education, and in the main their ideas agreed. I may in this connection be excused from reading abstracts from a letter from Henry soon after he had received the news I had given my son his name. After a playful discusssion of the name Joseph, Jo and Josey, he says—what may be news to most of you: "I did not object to Henry as a first name; although I have been sorry that my grandfather, in coming from Scotland to this country, substituted it for Hendrie, a much less common, and, therefore, distinctive name." He then proceeds: "I hope that both his body and mind will be developed by proper training and instruction, that he may become an efficient, wise and good man. I say efficient and wise, because these two characteristics are not always united in the same person. Indeed, most of the inefficiency of the world is due to their separation. Wisdom may know what ought to be done, but it requires the aid of efficiency to accomplish the desired object. I hope that in the education of your son due attention may not only be given to the proper development of both these faculties, but also they will be cultivated in the order of nature; that is, doing before thinking; art before science. By inverting this order much injury is frequently done to a child, especially in the case of the only son of a widowed mother, in which a precocious boy becomes an insignificant man. On examination in such a case it will generally be found that the boy has never been drilled into expertness in the art of language, of arithmetic, or of spelling, of attention, perseverance and order; or, in other words, of the habits of an active and efficient life."

Henry was a man of extensive reading, and often surprised his friends by the extent and accuracy of his information, and by the original manner in which he brought his knowledge before them. Not only was he well versed in those subjects in which one might naturally suppose him proficient, but in departments of knowledge entirely distinct from that in which he gained his reputation as an original thinker. Although without a musical ear he had a nice feeling for the movement of a poem, and was fond of drawing from his retentive memory poetic quotations apt to the occasion. He was a diligent student of mental philosophy, and also took a lively interest in the progress of biological science, especially in following the recent generalization of Darwin; while the astonishing development of modern research in tracking the history of prehistoric man had for him a peculiar fascination. Yet with all his learning, reputation and influence, Henry was as modest as he was pure. One day, on opening Henry's copy of Young's Lectures on Natural Philosophy—a book which he has studied more than any other work of science—I read on the fly-leaf, written in his own hand, these words:—

> "In Nature's infinite book of secrecy
> A little I can read."—*Shakespeare.*

And did he not read a little "in Nature's infinite book of secrecy?" And did he not read that little well? May we all read our little in that book as modestly and as reverently as did Joseph Henry.

THE PHOTOPHONE.

By Alexander Graham Bell.

In bringing before you some discoveries made by Mr. Sumner Tainter and myself, which have resulted in the construction of apparatus for the production and reproduction of sound by means of light, it is necessary to explain the state of knowledge which formed the starting point of our experiments. I shall first describe the remarkable substance selenium, and the manipulations devised by various experiments: but the final result of our researches has evidenced the class of substances sensitive to light-vibrations, until we can propound the fact of such sensitiveness being a general property of all matter. We have found this property in gold, silver, platinum, iron, steel, brass, copper, zinc, lead, antimony, German silver, Jenkin's metal, Babbitt's metal, ivory, celluloid, gutta percha, hard rubber, soft vulcanized rubber, paper, parchment, wood, mica and silvered glass; and the only substances from which we have not obtained results are carbon and thin microscopic glass. We find that when a vibratory beam of light falls upon these substances they emit sounds,—the pitch of which depends upon the frequency of the vibratory change in the light. We find farther that, when we control the form or character of the light-vibration on selenium, and probably on the other substances, we control the quality of the sound and obtain all varieties of articulate speech. We can thus, without a conducting wire as in electric telephony, speak from station to station, wherever we can project a beam of light. We have not had opportunity of testing the limit to which this photophonic influence can be extended, but we have spoken to and from points 213 meters apart; and there seems no reason to doubt that the results will be obtained at whatever distance a beam of light can be flashed from one observatory to another. The necessary privacy of our experiments hitherto has alone prevented any attempts at determining the extreme distance at which this new method of vocal communication will be available. I shall now speak of selenium.

In the year 1817 Berzelius and Gottlieb Gahn made an examination of the method of preparing sulphuric acid in use at Gripsholm. During the course of this examination they observed in the acid a sediment of a partly reddish, partly clear brown color, which, under the action of the blow-pipe gave out a peculiar odor, like that attributed by Klaproth to tellurium. As tellurium was a substance of extreme rarity, Berzelius attempted its production from this deposit; but he was unable, after many experiments, to obtain further indications of its presence. He found plentiful signs of sulphur mixed with mercury, copper, zinc, iron, arsenic and lead, but no trace of tellurium. It was not in the nature of Berzelius to be disheartened by this result. In science every failure advances the boundary of knowledge as well as every success, and Berzelius felt that, if the characteristic odor that had been observed did not proceed from tellurium, it might possibly indicate the presence of some substance then unknown to the chemist. Urged on by this hope he returned with renewed ardor to his work. He collected a great quantity of the material, and submitted the whole mass to various chemical processes. He succeeded in separating successively the sulphur, the mercury, the copper, the tin and the other known substances whose presence had been indicated by his tests;—and after all these had been eliminated, there still remained

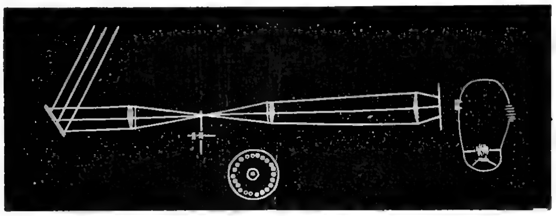


FIG. 1.

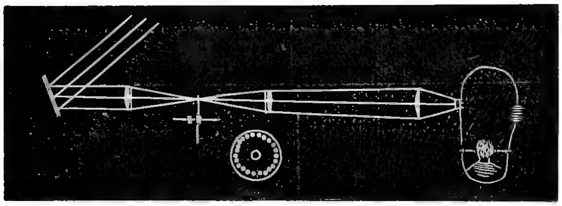


FIG. 2.

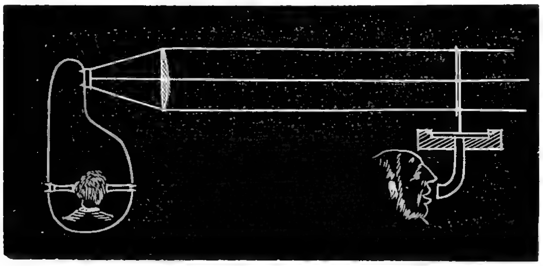


FIG. 3.

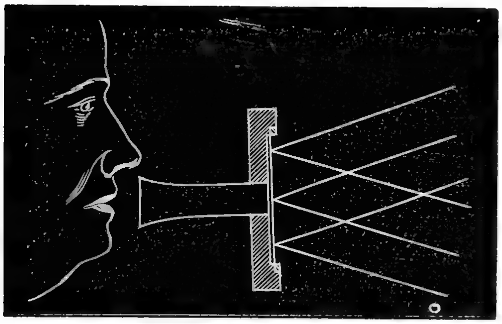


FIG. 4.

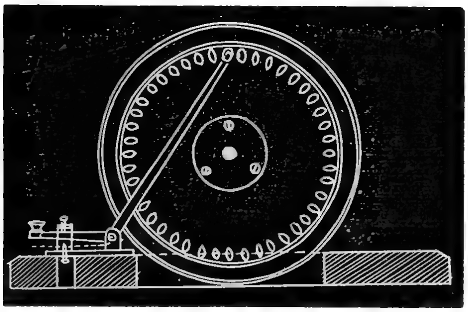


FIG. 5.

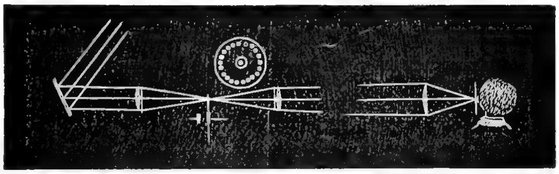


FIG. 6.

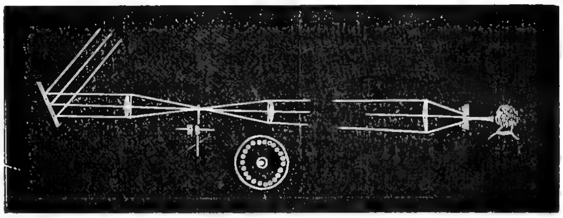


FIG. 7.

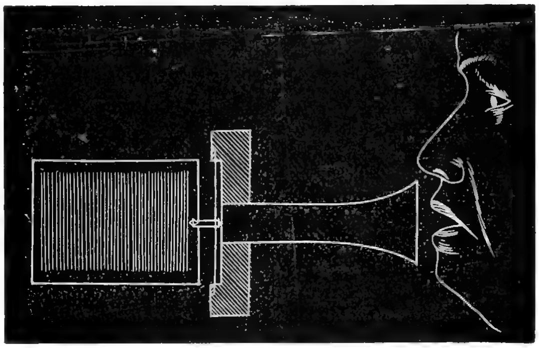


FIG. 8.

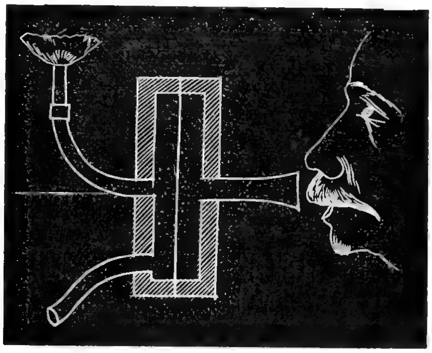


FIG. 9.

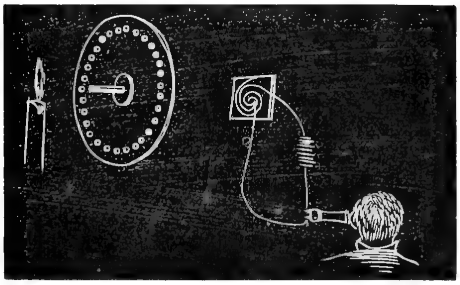


FIG. 10.

ILLUSTRATING PAPER ON PHOTOPHONE.

By ALEXANDER GRAHAM BELL.

1—The Interposition of Hard Rubber Plate. 2—The Light passed through openings in rapidly revolving diaphagm and reflected in selenious recesses. 3—Application of figure four. 4—Action of Voice on thin Plate of Silvered Mica. 5—Application of Morse system of Telegraphy to Photophone. 6—Listening directly to Receiving Plate. 7—Another form of Receiver. 8—One of the first forms, Voice passed through Slits. 9—Direct Action of Voice on Gas Flame. Action of Candle Light on selenius received.

a residue which proved upon examination to be what he had been in search of—a new elementary substance. The chemical properties of this new element were found to resemble those of tellurium in so remarkable a degree that Berzelius gave to the substance the name of "Selenium," from the Greek word *selene*, the moon—("tellurium," as is well known being derived from *tellus*, the earth,

Although tellurium and selenium are alike in many respects, they differ in their electrical properties; tellurium being a good conductor of electricity, and selenium, as Berzelius showed, a non-conductor. Knox discovered, in 1837, that selenium became a conductor when fused; and Hittorff, in 1852, showed that it conducted, at ordinary temperatures, when in one of its allotropic forms. When selenium is rapidly cooled from a fused condition, it is a non-conductor. In this, its vitreous form, it is of a dark-brown color, almost black by reflected light, having an exceedingly brilliant surface. In thin films it is transparent, and appears of a beautiful ruby red by transmitted light. When selenium is cooled from a fused condition with extreme slowness, it presents an entirely different appearance, being a dull lead color, and having throughout a granulated or crystalline structure, and looking like a metal. In this form it is perfectly opaque to light, even in very thin films. This variety of selenium has long been known as "granular" or "crystalline" selenium, or, as Regnault called it, "metallic" selenium. It was selenium of this kind that Hittorff found to be a conductor of electricity at ordinary temperatures. He also found that its resistance to the passage of an electrical current diminished continuously by heating up to the point of fusion, and that the resistance suddenly increased in passing from the solid to the liquid condition. It was early discovered that exposure to sunlight hastens the change of selenium from one allotropic form to another; and this observation is significant in the light of recent discoveries.

Although selenium has been known for the last sixty years it has not yet been utilized to any extent in the arts, and it is still considered simply as a chemical curiosity. It is usually supplied in the form of cylindrical bars. These bars are sometimes found to be in the metallic condition; but more usually they are in the vitreous or non-conducting form. It occurred to Willoughby Smith that, on account of the high resistance of crystalline selenium, it might be usefully employed at the shore-end of a submarine cable, in his system of testing and signalling during the process of submersion. Upon experiment, the selenium was found to have all the resistance required—some of the bars employed measuring as much as 1400 megohms—a resistance equivalent to that which would be offered by a telegraph wire long enough to reach from the earth to the sun! But the resistance was found to be extremely variable. Experiments were made to ascertain the cause of this variability. Mr. May, Mr. Willoughby Smith's assistant, discovered that the resistance was less when the selenium was exposed to light than when it was in the dark.

In order to be certain that temperature had nothing to do with the effect, selenium was placed in a vessel of water, so that the light had to pass through from one to two inches of water in order to reach the selenium. The approach of a lighted candle was found to be sufficient to cause a marked deflection of the needle of the galvanometer connected with the selenium, and the lighting of a piece of magnesium wire caused the selenium to measure less than half the resistance it did the moment before.

These results were naturally at first received by scientific men with some incredulity, but they were verified by Sale, Draper, Moss and others. When selenium is exposed to the action of the solar spectrum, the maximum effect is produced, according to Sale, just outside the red end of the spectrum, in a point nearly co-incident with the maximum of the heat rays; but, according to Adams, the maximum effect is produced in the greenish-yellow or most luminous part of the spectrum. Lord Rosse exposed selenium to the action of non-luminous radiations from hot bodies, but could produce no effect; whereas a thermopile under similar circumstances gave abundant indications of a current. He also cut off the heat-rays from luminous bodies by the interposition of liquid solutions, such as alum, between the selenium and the source of light, without affecting the power of the light to reduce the resistance of the selenium; whereas the interposition of these same substances almost completely neutralize the effect upon the thermopile. Adams found that selenium was sensitive to the cold light of the moon, and Werner Siemens discovered that, in certain extremely sensitive varieties of selenium, heat and light produced opposite effects. In Siemens's experiments, special arrangements were made for the purpose of reducing the resistance of the selenium employed. Two fine platinum wires were coiled together in the shape of a double flat spiral in the zig-zag shape, and were laid upon a plate of mica so that the discs did not touch one another. A drop of melted selenium was then placed upon the platinum-wire arrangement, and a second sheet of mica was pressed upon the selenium, so as to cause it to spread out and fill the spaces between the wires. Each cell was about the size of a silver dime. The selenium cells were then placed in a paraffine bath, and exposed for some hours to a temperature of 210° C., after which they were allowed to cool with extreme slowness. The results obtained with these cells were very extraordinary; in some cases the resistance of the cells, when exposed to light, was only one-fifteenth of their resistance in the dark.

Without dwelling farther upon the researches of others, I may say that the chief information concerning the effect of light upon the conductivity of selenium will be found under the names of Willoughby Smith, Lieutenant Sale, Draper and Moss, Professor W. G. Adams, Lord Rosse, Day, Sabini, Dr. Werner Siemens and Dr. C. W. Siemens. All observations by these various authors had been made by means of galvanometers; but it occurred to me that the telephone, from its extreme sensitiveness to electrical influences, might be substituted with advantage. Upon consideration of the subject, however, I saw that the experiments could not be conducted in the ordinary way for the following reason: The law of audibility of the telephone is precisely analogous to the law of electric induction. No effect is produced during the passage of a continuous and steady current. It is only at the moment of change from a stronger to a weaker state, or vice versa, that any audible effect is produced, and the amount of effect is exactly proportional to the amount of variation in the current. It was, therefore, evident that the telephone could only respond to the effect produced in selenium at the moment of change from light to darkness, or vice versa; and that it would be advisable to intermit the light with great rapidity, so as to produce a succession of changes in the conductivity of the selenium, corresponding in frequency to musical vibrations within the limits of the sense of hearing. For I had often noticed that currents of electricity, so feeble as to produce scarcely any audible effects from a telephone when the circuit was simply opened or closed, caused very perceptible musical sounds when the circuit was rapidly interrupted, and that the higher the pitch of sound the more audible was the effect. I was much struck by the idea of producing sound by the action of light in this way. Upon farther consideration it appeared to me that all the audible effects obtained from varieties of electricity could also be produced by variations of light acting upon selenium. I saw that the effect could be produced at the extreme distance at which selenium would respond to the action of a luminous body, but that this distance could be indefinitely increased by the use of a parallel beam of light, so that we could telephone from one place to another without the necessity of a conducting wire between the transmitter and receiver. It was evidently necessary, in order to reduce this idea to practice, to devise an apparatus to be operated by the voice of a speaker, by which variations could be produced in a parallel beam of light, corresponding to the variations in the air produced by the voice.

I proposed to pass light through a large number of small orifices, which might be of any convenient shape, but were preferably in the form of slits. Two similarly perforated plates were to be employed. One was to be fixed and the other attached to the centre of a diaphragm actuated by the voice, so that the vibration of the diaphragm would cause the movable plate to slide to and fro over the surface of the fixed plate, thus alternately enlarging and contracting the free orifices for the passage of light. In this way the voice of a speaker could control the amount of light passed through the perforated plates without completely obstructing its passage. This apparatus was to be placed in the path of a parallel beam of light, and the undulatory beam emerging from the apparatus could be received at some distant place upon a lens, or other apparatus, by means of which it could be condensed upon a sensitive piece of selenium placed in a local circuit with a telephone and galvanic battery. The variations in the light produced by the voice of the speaker should cause corresponding variations in the electrical resistance of the selenium employed: and the telephone in circuit with it should reproduce audibly the tones and articulations of the speaker's voice. I obtained some selenium for the purpose of producing the apparatus shown; but found that its resistance was almost infinitely greater than that of any telephone that had been constructed, and I was unable to obtain any audible effects by the action of light. I believed, however, that the obstacle could be overcome by devising mechanical arrangements for reducing the resistance of the selenium, and by constructing special telephones for the purpose. I felt so much confidence in this that, in a lecture delivered before the Royal Institute of Great Britain, upon the 17th of May, 1878, I announced the possibility of hearing a shadow by interrupting the action of light upon selenium. A few days afterwards my ideas upon this subject received a fresh impetus by the announcement made by Mr. Willoughby Smith before the Society of Telegraph Engineers that he had heard the action of a ray of light falling upon a bar of crystalline selenium, by listening to a telephone in circuit with it.

It is not unlikely that the publicity given to the speaking telephone during the last few years may have suggested to many minds in different parts of the world somewhat simliar ideas to my own.

Although the idea of producing and reproducing sound by the action of light, as described above, was an entirely original and independent conception of my own, I recognize the fact that the knowledge necessary for its conception has been disseminated throughout the civilized world, and that the idea may therefore have occurred to many other minds. *The fundamental idea, on which rests the possibility of producing speech by the action of light, is the conception of what may be termed an undulatory beam of light in contradistinction to a merely intermittent one.* By an undulatory beam of light, I mean a beam that shines continuously upon the selenium receiver, but the intensity of which upon that receiver is subject to rapid changes, corresponding to the changes in the vibratory movement of a particle of air during the transmission o a sound of definite quality through the atmosphere. The curve that would graphically represent the changes of light would be similar in shape to that representing the movement of the air. I do not know whether this conception had been clearly realized by "J. F. W.," of Kew, or by Mr. Sargent, of Philadelphia; but to Mr. David Brown, of London, is undoubtedly due the honor of having distinctly and independently formulated the conception, and of having devised apparatus—though of a crude nature—for carrying it into execution. It is greatly due to the genius and perseverance of my friend, Mr. Sumner Tainter, of Watertown, Mass., that the problem of producing and reproducing sound by the agency of light has at last been successfully solved.

The first point to which we devoted our attention was the reduction of the resistance of crystalline selenium within manageable limits. The resistance of selenium cells employed by former experimenters was measured in millions of ohms, and we do not know of any record of a selenium cell measuring less than 250,000 ohms in the dark. *We have succeeded in producing sensitive selenium cells measuring only* 300 *ohms in the dark, and* 155 *ohms in the light.* All former experimenters seemed to have used platinum for the conducting part of their selenium cells, excepting Werner Siemens, who found that iron and copper might be employed. We have also discovered that brass, although chemically acted upon by selenium, forms an excellent and convenient material; indeed, we are inclined to believe that the chemical action between the brass and selenium has contributed to the low resistance of our cells by forming an intimate bond of union between the selenium and brass. We have observed that melted selenium behaves to the other substances as water to a greasy surface, and we are inclined to think that when selenium is used in connection with metals not chemically acted upon by it, the points of contact between selenium and the metal offer a considerable amount of resistance to the passage of a galvanic current. By using brass we have been enabled to construct a large number of selenium cells of different forms. The mode of applying the selenium is as follows: The cell is heated, and, when hot enough, a stick of selenium is rubbed over the surface. In order to acquire conductivity and sensitiveness, the selenium must next undergo a process of annealing.

We simply heat the selenium over a gas stove and observe its appearance. When the selenium attains a certain temperature, the beautiful reflecting surface becomes dimmed. A cloudiness gradually extends over it, somewhat like the film of moisture produced by breathing upon a mirror. This appearance gradually increases, and the whole surface is soon seen to be in the metallic, granular or crystalline condition. The cell may then be taken off the stove, and cooled in any suitable way. When the heating process is carried too far, the crystalline selenium is seen to melt. Our best results have been obtained by heating the selenium until it crystalizes, and continuing the heating until signs of melting appear, when the gas is immediately put out. The portions that had melted instantly re-crystallize, and the selenium is found upon cooling to be a conductor, and to be sensitive to light. The whole operation occupies only a few minutes. This method has not only the advantage of being expeditious, but it proves that many of the accepted theories on this subject are fallacious. Our new method shows that fusion is unnecessary, that conductivity and sensitiveness can be produced without long heating and slow cooling; and that crystalization takes place during the heating process. We have found that on removing the source of heat immediately on the appearance of the cloudiness, distinct and separate crystals can be observed under the microscope, which appear like leaden snow-flakes on a ground of ruby red. Upon removing the heat, when crystalization is further advanced, we perceive under the microscope masses of these crystals arranged like basaltic columns standing detached from one another, and at a still higher point of heating the distinct columns are no longer traceable, but the whole mass resembles metallic pudding-stone, with here and there a separate snow-flake, like a fossil, on the surface. Selenium crystals formed during slow cooling after fusion present an entirely different appearance, showing distinct facets.

We have devised about fifty forms of apparatus for varying a beam of light in the manner required, but only a few typical varieties need be shown. The source of light may be controlled, or a steady beam may be modified at any point in its path. The beam may be controlled in many ways. For instance, it may be polarized, and then affected by electrical or magnetic influences in the manner discovered by Faraday and Dr. Ker. The beam of polarized light, instead of being passed through a liquid, may be reflected from the polished pole of an electro-magnet. Another method of affecting a beam of light is to pass it through a lens of variable focus. I observe that a lens of this kind has been invented in France by Dr. Cusco, and is fully described in a recent paper in "La Nature;" but Mr. Tainter and I have used such a lens in our experiments for months past. The best and simplest form of apparatus for producing the effect remains to be described. This consists of a plain mirror of flexible material—such as silvered mica or microscopic glass. Against the back of this mirror the speaker's voice is directed. The light reflected from this mirror is thus thrown into vibration corresponding to those of the diaphragm itself.

In arranging the apparatus for the purpose of reproducing sound at a distance, any powerful source of light may be used, but we have experimented chiefly with sunlight. For this purpose a large beam is concentrated by means of a lens upon the diaphragm mirror, and, after reflection, is again rendered parallel by means of another lens. The beam is received at a distant station upon a parabolic reflector, in the focus of which is placed a sensitive selenium cell, connected in a local circuit with a battery and telephone. A large number of trials of this apparatus have been made with the transmitting and receiving instruments so far apart that sounds could not be heard directly through the air. In illustration, I shall describe one of the most recent of these experiments. Mr. Tainter operated the transmitting instrument, which was placed on the top of the Franklin schoolhouse in Washington, and the sensitive receiver was arranged in one of the windows of my laboratory, 1325 L street, at a distance of 213 metres. Upon placing the telephone to my ear I heard distinctly from the illuminated receiver the words: "Mr. Bell, if you hear what I say, come to the window and wave your hat." In laboratory experiments the transmitting and receiving instruments are necessarily within earshot of one another, and we have therefore been accustomed to pooling the electric circuit connected with the selenium receiver, so as to place the telephones in another room. By such experiments we have found that articulate speech can be reproduced by the oxy-hydrogen light, and even by the light of a kerosene lamp. The loudest effects obtained from light are produced by rapidly interrupting the beam by the perforated disk. The great advantage of this form of apparatus for experimental work is the noiselessness of its rotation, admitting the close approach of the receiver without interfering with the audibility of the effect heard from the latter; for it will be understood that musical tones are emitted from the receiver when no sound is made at the transmitter. A silent motion thus produces a sound. In this way musical tones have been heard even from the light of a candle. When distant effects are sought another apparatus is used. By placing an opaque screen near the rotating disk the beam can be entirely cut off by a slight motion of the hand, and musical signals, like the dots and dashes of the Morse telegraph code, can thus be produced at the distant receiving station.

We have made experiments, with the object of ascertaining the nature of the rays that affect selenium. For this purpose we have placed in the path of an intermittent beam various absorbing substances. Professor Cross has been kind enough to give me his assistance in conducting these experiments. When a solution of alum, or bisulphide of carbon, is employed the loudness of the sound produced by the intermittent beam is very slightly diminished; but a solution of iodine in bisulphide of carbon cuts off most, but not all, of the audible effect. Even an apparently opaque sheet of hard rubber does not entirely do this. When the sheet of hard rubber was held near the disk interrupter the rotation of the disk interrupted what was then an invisible beam, which passed over a space of about twelve feet before it reached the lens which finally concentrated it upon the selenium cell. A faint but perfectly perceptible musical tone was heard from the telephone connected with the selenium. This could be interrupted at will by placing the hand in the path of the invisible beam. It would be premature, without further experiments, to speculate too much concerning the nature of these invisible rays; but it is difficult to believe that they can be bent rays, as the effect is produced through two sheets of hard rubber containing between them a saturated solution of alum. Although effects are produced as above shown by forms of radiant energy which are invisible, we have named the apparatus for the production and reproduction of sound in this way "*The Photophone,*" because an ordinary beam of light contains the rays which are operative.

It is a well-known fact that the molecular disturbance produced in a mass of iron by the magnetizing influence of an intermittent electrical current can be observed as sound by placing the ear in close contact with the iron. It occurred to us that the molecular disturbance produced in crystalline selenium by the action of an intermittent beam of light should be audible in a similar manner without the aid of a telephone or battery. Many experiments were made to verify this theory without definite results. The anomalous behavior of the hard rubber screen suggested the thought of listening to it also. This experiment was tried with extraordinary success. I held the sheet in close contact with my ear, while a beam of intermittent light was focussed upon it by a lens. A distinct musical note was immediately heard. We found the effect intensified by arranging the sheet of hard rubber as a diaphragm, and listening through a hearing-tube. We then tried crystalline selenium in the form of a thin disk, and obtained a similar but less intense effect. The other substances which I enumerated at the beginning of my address were now successively tried in the form of thin disks, and sounds were obtained from all but carbon and thin glass. We found hard rubber to produce a louder sound than any other substance we tried, excepting antimony, and paper and mica to produce the weakest sound. *On the whole, we feel warranted in announcing as our conclusion that souuds can be produced by the action of a variable light from substances of all kinds, when in the form of thin diaphragms.* We have heard from interrupted sunlight very perceptible musical tunes through tubes of ordinary vulcanized rubber, of brass and of wood. These were all the materials at hand in tubular form, and we have had no opportunity since of extending the observations to other substances.

I am extremely glad that I have the opportunity of making the first publication of these researches before a scientific society, for it is from scientific men that my work of the last six years has received its earliest and kindest recognition. I gratefully remember the encouragement which I received from the late Professor Henry at a time when the speaking telephone existed only in theory. Indeed, it is greatly due to the stimulus of his appreciation that the telephone became an accomplished fact. I cannot state too highly also the advantage I received in preliminary experiments on sound vibrations in this building from Professor Cross, and near here from my valued friend Dr. Clarence J. Blake. When the public were incredulous of the possibility of electrical speech, the American Academy of Arts and Sciences, the Philosophical Society of Washington and the Essex Institute of Salem recognized the reality of the results and honored me by their congratulations. The public interest, I think, was first awakened by the judgment of the very eminent scientific men before whom the telephone was exhibited in Philadelphia, and by the address of Sir William Thomson before the British Association for the Advancement of Science.

At a later period, when even practical telegraphers considered the telephone as a mere scientific toy, Professor John Pierce, Professor Eli W. Blake, Dr. Channing, Mr. Clarke and Mr. Jones, of Providence, R. I., devoted themselves to a series of experiments for the purpose of assisting me in making the telephone of practical utility; and they communicated to me, from time to time, the result of their experiments with a kindness and generosity I can never forget. It is not only pleasant to remember these things and to speak of them, but it is a duty to repeat them, as they give a practical reputation to the often repeated stories of the blindness of scientific men to unaccredited novelties, and of their jealousy of unknown inventors who dare to enter the charmed circle of science. I trust that the scientific favor which was so readily accorded to the telephone may be extended by you to this new claimant—the photophone.

PLAN OF THE CEREBRO-SPINAL NERVOUS SYSTEM.

By S. V. Clevenger, M. D.

(Abstract from the paper (B 41) read before the American Association for Advancement of Science, Boston, August 28th, 1880).

The great French and German cerebral anatomists Luys and Meynert had endeavored to declare the architecture of the human brain from a multitude of microscopic sections, but so intricate were the relationship of fibres, nerve-cells, arteries, veins, connective tissue, etc., that it was at once seen to be necessary to study lower animal life anatomically and physiologically before the plan could be determined. Luys did nothing in this direction, while Meynert went as far as the brains of small mammals. Spitzka has carried the scrutiny still farther. The scheme of Meynert started with the upper part of the cerebrum as the seat of consciousness and, working downward, his "projection systems" ended in the periphery.

The nerve fibres composing the cerebrum and cerebellum were mainly considered. The presence of a multitude of nerve-cells and ganglia dispersed throughout this region was unaccounted for, and as these were of undoubted importance and all well known to anatomists, it was seen by pathologists that these schemes were insufficient.

No scheme can be correct which ignores any part of the nervous organization, or excludes any form of life as anomalous. The conclusion I have reached, is that the sympathetic system of vertebrata corresponds to the general nervous distribution of invertebrata above protozoa, presiding over the nutritive functions. The vaso-motor has been differentiated from the sympathetic distribution, whose office is to produce the vermicular motions of the intestines. Differentiation proceeds dorsally because that portion of the animal which is in most constant contact with the changing molecular motion of the environment would be precisely the portion to give origin to the higher series of nerve divisions. The endoderm, after the gastrula, stage remains under control of the sympathetic system. The so-called cerebral ganglia of Vermes, are homologous with the spinal segments which afterwards become coalesced in the vertebrata. This is the second system to be developed and *Amphioxus* has not acquired the third or cerebral system proper. In *Trigla Adriatica*, the third system series may be seen developed dorsally upon the second or spinal cord. This third system is the intervertebral ganglia. *Fusion of several of the higher intervertebral ganglia produces the cerebellum*, and accounts for the co-ordinating function of that organ. The several cerebral lobes, the tubercula quadrigemina, mammillary eminence, Gasserian ganglion, olfactory lobe, olivary body, etc., are hypertrophied or atrophied (as the case may be) intervertebral ganglia. Projection systems and commissures, as the callosal, make their appearance in exact accordance with laws operative in the lower series.

The three systems develop gradually, and it may be said, commissurally one upon the other, and this scheme appears to account satisfactorily for physiological and pathological phenomena.

In adddition to its publication in the proceedings of the Association, the paper will be produced in full, in the *American Journal of Nervous aud Mental Disease*, for October, 1880.

ANCIENT AGRICULTURAL IMPLEMENTS OF STONE.

By Hon. William McAdams, of Otterville, Ills.

In the rich, alluvial soil about the mouths of the Missouri and Illinois rivers are found many of these ancient stone implements used by the Mound-builders in their rude agriculture. Mr. McAdams exhibited a fine collection of these implements.

They are all chipped from flint, or a hard silicious limestone, and some of them beautifully made. Some are nearly a foot in length and six inches wide at the broader end.

Some are made to be fastened to handles, like our modern spades. Others resemble our modern hoes, having a deep, lateral notch, to facilitate the fastening to a handle. Some of these stone hoes are made with such ingenuity as to have been effective implements.

Mr. McAdams also exhibited stone implements which evidently were made to fasten to some kind of stock to be pulled through the ground like a plow. As these ancient people had no domestic animals for this purpose, it is probable that manual force was used to perform the work. The broad cutting edge of these stone implements was highly polished from long use by the attrition of the soil.

Mr. McAdams had found these implements of agriculture in the ancient graves associated with pottery, some of which contained carbonized corn. Cobs in a carbonized state were found, and the speaker is of the opinion that these ancient people lived principally on corn and vegetables, which they cultivated to a considerable extent.

The paper elicited much interest in the association.

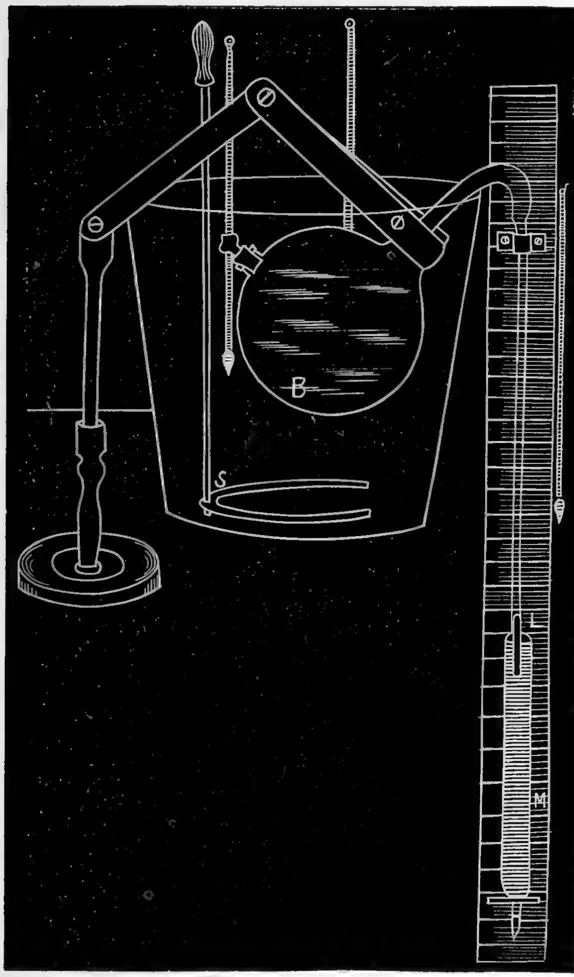
A very interesting report addressed to the committee of public health in France by M. Wurz, describes a process for retaining the green coloring of vegetables which is generally destroyed by boiling. It consists in the use of an excess of chlorphyl obtained from spinach (spinacia oleracea) which holds in its cells a large amount of coloring matter. A watery solution of this rendered alkaline by soda, is added to the boiling vegetable which is slightly acidulated with hydrochloric acid. The chemical result is common salt and a deposit of coloring matter on the organic tissue. There cannot now be any possible temptation for the unwarrantable dyeing of preserved vegetables by salts of copper or the employment of adulterants for obtaining that vivid coloring so attractive to the epicure.

ON THE CO-EFFICIENT OF EXPANSION OF GAS SOLUTIONS.

E. L. NICHOLS AND A. W. WHEELER.

The dilatometers used in this research differ essentially from those generally employed in the measurement of the expansion of liquids. Their form is shown in the accompanying figure. The bulb (B) contained about 286 grms. of the liquid—[at 15°C = 286.3425 grms.]

The dilatometer having been filled was placed in the bath in the position denoted in the figure. The lower end (*l*) of the vertical part of the neck was then immersed in mercury by sliding up the adjustable mercury tube (*m*) to the proper point. Upon cooling the liquid, the mercury rose in the neck of the dilatometer and from its height at various temperatures the volume of the solution was calculated.



This method was found to be of very great sensitiveness but its accuracy depends upon the careful observance of the following precautions. The neck must be accurately calibrated, the pressure must be kept nearly constant by readjusting, from time to time, the mercury-tube (*m*). Especial study must be made of the time which will elapse after each change of temperature ; before the contents of the dilatometer will have assumed the same temperature as the bath. The authors found this interval of time to be 30 minutes for 1° and 2° intervals of temperature and 35 minutes for a 5° change. The bath itself must be constantly stirred. Finally the thermometers studied and not only their freezing points and boiling points ascertained, but a careful comparison must be made with some standard thermometer which in turn has been properly calibrated and compared directly with the air thermometer.

The following solutions of ammonia gas were used :

NUMBER.	Percentage of NH_3.	Specific Gravity at 14° C.
1	29.00	.9009
2	16.19	.9373
3	7.96	.9673
4	5.61	.9766
5	2.12	.9913

Of these the volumes at various temperatures from + 20 °C to the freezing points of the solutions or in the case of the stronger solutions to —20°C were determined and the point of maximum density and freezing points noted. In the following table the volumes of each solution are compared with a unit volume of the same solution at + 4° C.

TABLE.

Volumes (observed values) of Aqueous Solution of NH_3 Gas.

(WATER)* PER CENT. NH_3=0.00.		PER CENT. NH_3=2.12.		PER CENT. NH_3=5.61.	
Temps.	Volumes.	Temps.	Volumes.	Temps.	Volumes.
20°.0	1.001744	14°.75	1.001114	19°.80	1.003180
18°.0	1.001348	9°.72	1.000413	14.80	1.001953
16°.0	1.000999	4°.76	1.000033	8.80	1.000726
14°.0	1.000701	4°.00	1.000000	4°.80	1.000106
12°.0	1.000451	2°.80	0.999958	4°.00	1.000000
10°.0	1.000345	1°.80	0.999941	1°.16	0.999594
8°.0	1.000114	— 0°.20	0.999941	— 1°.20	0.999398
6°.0	1.000030	— 5°.20	1.000296	— 4°.2	0.999298
4°.0	1.000000	....	....	— 5°.2	0.999272
2°.0	1.000073	....	....	—10°.2	0.999289
0°.0	1.000092	....	....	....	....

* The water volumes are taken from Rosetti's table.—Wülluer Phyrik Bd. III.

PER CENT. NH_3—7.96.		PER CENT. NH_3=16.19.		PER CENT. NH_3=29.00.	
Temps.	Volumes.	Temps.	Volumes.	Temps.	Volumes.
15°.00	1.002662	21°.80	1.007129	15°.00	1.007214
9°.80	1.001191	16°.80	1.004930	13°.01	1.005835
4°.80	1.000141	11°.80	1.002874	11°.06	1.004547
4°.00	1.000000	6°.80	1.001013	9°.15	1.003303
— 0°.20	0.999417	4°.00	1.000000	7°.28	1.002064
— 5°.20	0.998932	1°.80	0.999110	5°.33	1.000862
—10°.20	0.998740	— 3°.20	0.997514	4°.00	1.000100
—11°.20	0.998733	— 8°.20	0.996095	3°.27	0.999618
—12°.20	0.998760	—13°.20	0.994890	1°.16	0.998350
—13°.20	0.998794	—17°.20	0.994139	— 0°.80	0.997109
....	....	....	....	— 2°.51	0.996020
....	....	....	....	— 4°.38	0.994838
....	....	....	....	— 6°.40	0.993585

From these observed volumes the co-efficients of expansion were calculated and curves showing the volumes and co-efficients were plotted. The co-efficient curves are valuable in the determination of the points of maximum density, since they cut the base line at that point at a considerable angle, and serve to fix the temperature within 0.1° C.

The solutions of 2.12, 5.61, and 7.96 per cent. strength froze within the temperature interval reached by the common salt and ice freezing mixture used : the stronger solutions however remained in the liquid state. The following table gives the points of maximum density and the freezing points of the solutions :

Percentage.	Max. Density.	Freezing.	Saturation Pt.
0.00	4°.00	0°.00	100°.00
2.12	0°.80	— 5°.40(?)	93°.2
5.61	— 7°.20	—10°.6	83°.1
7.96	—10°.50	—14°.1	76°.4
16.19	......	......	59°.c
29.00	......	......	39°.8

It will be seen from these tables that the effect of ammonia gas in solution upon the water absorbing it is to increase greatly the co-efficient of expansion and to lower very rapidly both the points of maximum density and of freezing.

In these respects the gas acts just as a salt in solution would do. Gas solution and salt solution would seem to be closely related phenomena, each resulting in the formation of a mixed liquid, viz: of a liquid composed of two sets of independently moving molecules.

The effect of ammonia gas upon the volume of the water absorbing it is expressed by the following law:

When it is absorbed by water, the increase in volume for a constant temperature is directly proportional to the amount of gas absorbed.

This may be shown to be for NH_3 gas in water by plotting a set of curves with the volumes given in the above tables as ordinates and percentages of gas as abcissae. These curves, whatever temperatures be chosen, resolve themselves into straight lines. Since for the case of CO_2 gas in water the same law had been already found true by direct measurement of the change of volume due to the absorption of the gas at constant temperatures, we are warranted in suspecting the law to be a general one.

THE ENDOCRANIUM AND MAXILLARY SUSPENSORIUM OF THE BEE.

PROF. GEORGE MACLOSKIE, OF PRINCETON, N. J.

The endocranium of insects is produced by infoldings of the cranial wall, and although several groups (as Diptera, Hemiptera, Coleoptera, Lepidoptera,) have been represented as devoid of such structures, Prof. Macloskie finds an endocranium present in all these orders. The posterior or epicranial part of the skull has no internal processes. The clypeus, or "face," has a thick posterior ridge (just in advance of the antennæ). From this ridge descend, in bees and allied insects, two meso-cephalic pillars, reaching to the floor of the cranium, in front of the great foramen. These two pillars support the roof of the skull. They occur, with variations, in squash-bug, gadfly, mosquito, butterfly, and dragonfly. In the cockroach they take the form of a perforated plate, being united anteriorly by a cross-bar (which binds the mandibles together), and being webbed excepting at the centre. (Huxley's description of this in his Anatomy of the Invertebrates is inaccurate.)

The maxillæ and labium of the bee are supported by a long framework with elbows and hinges. This suspensorium is incorrectly represented in published figures. It is, in part correctly figured by Wolff, who misinterprets it (as if it were on the type of the mammalian skull). Its basal or posterior rods are attached close to the great foramen and to the base of the meso-cephalic pillars, and they are united by a thick web to the base of the skull. The mid-segment, consisting of a pair of bars, supports the maxillæ, and upon it is an anterior pair of bars supporting the labium. In its working, this frame-work embodies the principle of a recent patent for producing steady motion.

The methods by which the maxillæ and labium are protruded and withdrawn were described, also the relations and mode of working of the pharynx-parts in the mouth. The discovery of a double set of salivary glands was reported; a cephalic set supplying the inner tongue on the floor of the mouth, and the thoracic glands, sending their long duct forward to the labium. The inner structure of the bee's head was shown to be of the same pattern as in other insects, though varied in details. The paper was illustrated by diagrams and microscopic preparations.

NEW PLANETARY NEBULÆ.

BY PROFESSOR PICKERING.

He described the observations of the planetary nebulæ, are now in progress at the Harvard College Observatory. Besides measuring the light of these bodies, the spectrum of each has been examined by inserting a prism between the objective and eyepiece of the large telescope. A star is converted into a colored line of light, but the nebulæ, being nearly monochromatic, appears as a bright point. The difference is so marked that the idea suggested itself that by this means planetary nebulæ might be discovered, whose disks are so small that they can not otherwise be distinguished from stars. A search was accordingly undertaken on the evening of July 13th, by sweeping or moving the telescope so that a great number of stars could be examined in a short time. In a few minutes such a nebula was found, which with an ordinary eye-piece might readily be mistaken for a twelfth magnitude star. A similar object was also detected on the next evening. After this, sweeps on several evenings failed to reveal any new nebulæ, although it is estimated that the spectra of over a hundred thousand stars were examined.

On night before last, while continuing this work, an object with a remarkable spectrum entered the field. The light appeared to consist mainly of a band in the green, a line in the red and probably a fainter band in the yellow, the whole being superposed on a faint continuous spectrum. The new stars which blazed out in Corona in 1863 and in Cygnus in 1876, presented for a short time a similar spectrum, but with this exception the star noted above appears to be unique. It is too soon to form a theory regarding the nature of this body, as clouds interrupted the observations and barely allowed time for its identification. It proved to be the star known as Oeltzen 17681, and must therefore have had nearly its present brightness forty years ago.

The field for discovery by the method here given is far from being exhausted since, less than one hundreth part of the heavens has as yet been examined.

ON LAND SNAILS OF THE PALÆOZOIC PERIOD.

By DR. DAWSON, F. R. S., Principal of McGill University, Montreal.

The land snails occurring in the carboniferous and Devonian systems, of which six species are known, were noticed in detail. Two of these, *Pupa Bigsbii* from the coal formation of Nova Scotia, and *Strophites grandæva* from the Erian (Devonian) of St. Johns, New Brunswick, were described for the first time. Four of the known species belong to the different subdivisions of the old genus *Pupa*, and two are helicord or snail-like in form. They constitute a very isolated group of fossils, as none are known in older formations, and there are none newer till we reach the early Tertiary. Though all of somewhat distinct types, they all belong to one great family or sub-order of the *Pulmonifera*, and are all closely allied to types still living. All the species hitherto found are American, four being found in Nova Scotia and New Brunswick, and two in Illinois. The latter were discovered and described by the late Mr. Bradley. *Pupa vetusta*, the earliest known, was found in the material filling a hollow *Sigillaria*, by Sir Charles Lyell and Dr. Dawson in 1851. In the paper, which will probably appear in full in the AMERICAN JOURNAL OF SCIENCE, figures and descriptions of all the species are given, and their affinities and mode of occurrence are discussed.

FURTHER NOTES ON THE POLLINATION OF YUCCA AND ON PRONUBA AND PRODOXUS.

BY C. V. RILEY.

The author refers to the original paper on the Fructification of Yucca read at the Dubuque (1872) meeting of the Association and notices various criticisms since made upon its conclusions. The paper shows that none of these criticisms were warranted, and verifies the original observations and conclusions by subsequent experience. It points out the causes of error in that other writers have confounded related moths having similar *general* appearance but great structural differences and different habits. The characters of the Bogus Yucca Moth (*Prodoxus decipiens*), are given, and five new species, *Pronuba maculata*, *Prodoxus marginatus*, *P. cinerius*, *P. ænescens* and *P. intermedius*), are described, and the paper concludes with remarks which point to these diffent Yucca Moths as admirable illustrations of the derivative origin of species.

THE WYANDOTTES.

By Major J. W. Powell.

The Indians now known as the Wyandottes, were first found on the lower St. Lawrence. Subsequently they inhabited a narrow district of country on the shores of Lake Huron, and were known as the Hurons; later they lived in Michigan about Detroit; then in Ohio in what is known as Wyandotte county; from Ohio they were moved to Kansas and placed on a reservation; and from Kansas to the Indian Territory. In their wanderings from point to point, as they were driven from advancing civilization, a few of their number were left behind, so that the Wyandottes are scattered from the lower St. Lawrence to the Indian Territory along the route of their migration. These Indians call themselves Wundat; the etymology of the word is not known. In their social organization four units are recognized—the family, the gens, the phratry and the tribe. The family, as the term is here used, is nearly synonymous with household. It is composed of the persons who occupy one lodge, or, in their permanent wigwams, one section of a communal dwelling. The head of the family is a woman. The gens is an organized body of consanguineal kindred in the female line. "The woman carries the gens," is the formulated statement by which a Wyandotte expresses the idea that descent is in the female line. Each gens has the name of some animal—the form of such animal being its tutelar god. Up to the time when the tribe left Ohio, eleven gentes were recognized as follows: Deer, Bear, Highland Turtle (striped), Highland Turtle (black), Mud Turtle, Smooth large Turtle, Hawk, Beaver, Wolf, Sea Snake, Porcupine. In speaking of an individual he is said to be a Wolf, a Bear, or Deer, as the case may be, meaning thereby that he belongs to that gens; but in speaking of the body of people comprising a gens they are said to be relatives of the Wolf, the Bear, or the Deer, as the case may be.

There are four phratries in the tribe—the three gentes, Bear, Deer and Striped Turtle constituting the first; the Highland Turtle, Black Turtle and Smooth Large Turtle the second; the Hawk, Beaver and Wolf the third; and the Sea-snake and Porcupine the fourth. The eleven gentes as four phratries constitute the tribe.

The civil government inheres in a system of councils and chiefs. In each gens there is a council composed of four women. These four women councilors select a chief of the gens from its male members; that is, from their brothers and sons. This gentile chief is the head of the gentile council. The council of the tribe is composed of the aggregated gentile councils. The tribal council, therefore, is composed one-fifth of men and four-fifths of women.

The government of the Wyandottes, with the social organization upon which it is based, affords a typical example of tribal government throughout North America. Within that area there are several hundred distinct governments. In so great a number there is great variety, and in this variety we find different degrees of organization, the degree of organization being determined by the differentiation of the functions of government and the correlative specialization of organic elements.

A SIMPLE DEVICE FOR PROJECTING THE VIBRATIONS OF LIQUID FILMS WITHOUT A LENS.

By H. S. Carhart, A. M., Professor of Physics and Chemistry, Northwestern University, Evanston, Ill.

This instrument is designed to project upon the screen the vibrations of a film of soapy water produced by the voice or by an organ pipe. It might be called the self-projecting phoneidoscope. It differs from Sedley Taylor's phoneidoscope in three particulars: first, the vibrations are communicated to the film through the agency of a mouthpiece and a ferrotype diaphragm; second, the vibrations are projected on a screen; third, the film is employed to project itself without a lens.

It consists of a wooden tube, having a telephone mouthpiece at one end and expanding into a large funnel at the other, the funnel being of metal. In the side of the tube a stop-cock is inserted. A film is obtained in the open end of the funnel and a little air is then blown through the stop-cock. This distends the film slightly, causing it to act as a convex mirror. It is then placed in a beam of *sunlight* and reflects it at the proper angle. Upon singing a note at the mouthpiece a sharply defined system of waves is projected. Photographs of these have been taken. Caps fitting into the funnel and provided with a square or triangular opening, are also employed to give films of different shape.

THE LANGUAGES OF THE IROQUOIS.

By Mrs. E. A. Smith.

The language of each nation represents its thought. If these thoughts have remained unrecorded, it is from the language itself that they must be obtained by tracing out the origin, history and meaning of its words. Each word has its history, which it can be made to reveal by tracing out the origin, history and their most hidden secrets, and the thoughts, customs and beliefs of the originator be read as truthfully as if recorded by the historian's pen. For "words unaided cannot lie;" twenty words in Tuscarora represent supernatural beings. Does this leave a doubt as to the tendency of their minds? The Tuscarora word for burial ground signifies "placed in the ground in a sitting posture," proving that some time in the past such was their method of burial. The very structure of the Indian languages, where the words are so self-explaining, affords unlimited scope to the etymologist in his search into word history. There are two distinct periods in the modern history of the Iroquois. The inundation of new ideas on the advent of the white man introduced almost a new vocabulary, differing according to the ideas of the observers. For instance, the horse when first seen by the Senecas was drawing logs, hence was called a log drawer. Another tribe saw it carrying packs, and termed it pack-carrier. The Tuscaroras adopted the English word and term it *ha-hath*. It is quite remarkable that so few words have been borrowed from the English. And these have become so Indianized by prefixes and appendages or changes in their vowel sounds as to be scarely recognizable. Among them are: U-ts—oats; Sa-i tar—cider; Ha-hass—horse; Vi-ni-gair—vinegar; Qui-tair—Peter; Ta-wait—David; Tju-rus—Julius; Nay-yak-it-ando—jacket. Lastly was-tun for Boston, adding to this the plural suffix ha-kah, a term which in English might be interpreted *ites*. We have then Was-tun-ha-kah, or Bostonites, which in the Iroquois is the general term for Americans or the whole American nation. This almost supernatural intuition of the Indian mind crystallizes, I do not doubt, the opinion also and belief of at least 250,000 pale faces residing in the metropolis of Massachusetts. Of the length of some of incorporative words, which sometimes contain verb, subject, object, adjective or preposition, I would remark that the examples generally given in encyclopedias and works on language are almost entirely English Indian. That is, a missionary, perhaps, translating a portion of the Bible, finds some abstract word entirely beyond the comprehension of the Indian mind; he therefore takes Webster's definition of the word and translates that into the Indian in the form of one word until it has the appearance of the heading to a German railway time-table, the words consisting sometimes of forty letters and eleven or twelve syllables. The longest word thus Anglo-Indianized with which I have met is the Mohawk word for stove polish, the word itself being as indicative of the ingenuity of the inventor as the polish itself. It consisted of a glowing description of all the excellencies of said stove polish, which it required fifty-eight letters to express. The abstract nouns, represented as being absent from many of the Indian languages, are found in the Tuscarora, such as life, death, love, hate. An interesting feature of the language also might be traced in the preference given to the feminine gender instead of, as in the more ungallant English, to the masculine; for instance, the word theirs translates "two hers." The work I present is necessarily but a chrestomalthy compared to what can be done in the study of each of the Iroquois languages. Enough beauties, however, have been discovered through this mere insight to convince one that their possibilities were great. The reflection is, therefore, sad that in all probability fifty years hence these chrestomalthies, imperfect as they are, may be the only record of their former existence. Even now English is fast becoming the communicating medium of the people, as it is of the pulpit and the school. We can, therefore, safely predict that within the next century the Iroquois languages, as spoken by its six different tribes, will have become a thing of the past.

STRUCTURE OF MICA VEINS IN NORTH CAROLINA.

By W. C. Kerr.

At Danville, Va., Professor Kerr, of Raleigh, found veins or dykes which seemed to have been filled neither by fused matter nor by the ordinary mode of infiltration, but by a fine granular fragmented mass, derived from the containing bedded rocks, by the crowding, jamming and mechanical comminution of the rocks themselves. The mica veins in North Carolina are simply dykes of very coarse granite. When the crystallization becomes so coarse that the diameter of the mica sheets passes three or four inches, the dyke is called a mica vein. These veins are found in the upper Laurentian or Montalban, and may be considered characteristic of that horizon in North Carolina. The most productive veins are found in the high plateau between the Blue Ridge and the Smoky mountains, mostly in two or three counties. The amount of marketable mica produced per month is not more than two or three tons, although a much larger quantity could be obtained if the market demanded it. The most valuable of the present mica mines were opened and wrought by the mound-builders many ages ago on a much larger scale than now. There are evidences in the great river valleys in North Carolina of extensive glaciation in remote times, although the last glacial period is wholly unrepresented on the present surface. The protrusion of the eastern coast of North Carolina, about a hundred miles beyond the general Atlantic coast, is due to the interaction of the Arctic shore current and the Gulf stream, which collect the detritus thrown into the sea from Maryland to South Carolina, and drop them about Hatteras. This action has carried the coast of North Carolina to within fifty miles of the margin of the deep Atlantic channel, and, therefore, near its limit. The sounds behind the chain of sand islands or dunes, known as "The Banks," are rapidly silted up and converted into marsh and dry land by the sands blown over the dunes, and by the sediment brought down by the numerous rivers from the interior. The movement of the sand of these dunes was found to be about one foot per annum landward.

TRANSFORMATION OF PLANORBIS.

A PRACTICAL ILLUSTRATION OF THE EVOLUTION OF SPECIES.

By A. Hyatt.

The word evolution means the birth or derivation of one or more things or beings from others, through the action of natural laws. A child is evolved from its parents, a mineral from its constituents, a state of civilization from the conditions and surroundings of a preceding age. While evolution furnishes us with a valuable working hypothesis, science cannot forget that it is still on trial. The impatience of many when it is doubted or denied savors more of the dogmatism of belief than of the judicial earnestness of investigation. Every individual differs in certain superficial characters from the parent forms, but is still identical with hem in all its fundamental characteristics. This constantly recurring relationship among all creatures is the best established of all the laws of biology. It is the so-called law to heredity, that like tends to reproduce like. There seem to be only two causes which produce the variations which we observe; one is the law of heredity, the other is the surrounding influences or the sum of the physical influences upon the organism. The first tends to preserve uniformity, the second modifies the action of the first. The law of natural selection asserts that some individuals are stronger or better fitted to compete with others, in the struggle of life, than are others of the same species: hence they will live and perpetuate their kind, while the others die out. An erroneous impression exists, that Darwinian doctrines are more or less supported by all naturalists who accept evolution, but it is far from the truth. The Darwinian hypothesis is so very easy of application, and saves so much trouble in the way of investigation, that it is very generally employed, without the preliminary caution of a rigid analysis of the facts, and it is safe to say that it is often misapplied. A great amount of nonsense has been written about its being a fundamental law, in all forgetfulness that we are yet to find a law for the origin of the variations upon which it acts; it cannot be the primary cause of the variations, for the laws of heredity are still more fundamental. The speaker then described the situation and character of Steinheim, where numerous shells of the Planorbidæ are found in the strata, which have been very regularly deposited. Hilgendorf claims to have discovered great evidences of the gradual evolution of the various forms from the simplest and oldest specimens, but Mr. Hyatt has failed to find what Hilgendorf describes. By means of a lantern a number of illustrations of the shells were projected upon a screen, and quite fully described. Four lines of descendants were shown to branch out from four of the simplest forms, with all the gaps between the species filled with intermediate varieties. Each one of the lines or series has its own set of characteristic differences, and its own peculiar history. It is a fair inference from the facts before us, that the species from the progressive series, which become larger and finer in every way, owe their increase in size to the favorable physical condition of the Steinheim basis. Darwinists would say that in the basin a battle had taken place, which only the favored ones survived. Mr. Hyatt endeavored to present, in a popular manner, the life-history of a single species, the *planorbis levis*, and its evolution into twenty or thirty distinguishable forms, most of which may properly be called by different names and considered as distinct species. He also endeavored to bring the conception that the variations which led to these different species were due to the action of the laws of heredity, modified by physical forces, especially by the force of gravitation, into a tangible form. There are many characteristics which are due solely to the action of the physical influences which surround them; they vary with every change of locality, but remain quite constant and uniform within each.

MOUNDS OF ILLINOIS.

By W. McAdams, Otterville, Ills.

Mr. McAdams stated that during a period of some 25 years, when leisure permitted, he had been exploring in the mounds of the State. Within a radius of 50 miles from the mouth of the Illinois river there were many thousands of mounds erected by the past inhabitants of the country.

A map was shown illustrating the ancient works of the region, which include almost every variety of mound in the Union. Mr. McAdams has explored hundreds of these mounds, and collected a great quantity of valuable material illustrating the habits and customs of the people of that age. He gave illustrations of House, Burial, Temple and other Mounds.

Many of the small mounds in this section, the speaker thought, were the remains of dwelling places, originally made by placing poles on end, or in a vertical position, fastened at the top, and the whole covered with sod and earth. This structure, after being repaired from year to year, would finally decay, fall to prices and form a mound. In many of these mounds he had found ashes, remains of animals eaten, and other articles that would be found in such a primitive home. Of burial mounds there were several different kinds.

But comparatively few of the mounds contained valuable relics. In many of the mounds nothing at all of interest was found. It is an error to think that the ancient people always buried with the dead his personal effects.

He had, however, taken from mounds pipes, some of which are very peculiar, many kinds of sea shells, stone, copper and other ornaments, but seldom any weapons. Some of the copper ornaments shown were very curious and ingeniously made; among them were copper turtles, closely resembling the living animals, and large pipes of stone that represented the human figure in various positions. The speaker gave illustrations of mounds in which it would seem that sometimes on the death of their rulers a number of slaves or subjects were buried with him.

Mr. McAdams concludes from his explorations that the burial mounds show at least two distinct classes of people differing from our present indians.

The mound builders of the low lands of Illinois, like those of Ohio, were characterized by their peculiar pipes with the crescent base, the stem being a part of the base.

The potter makers, such as made the peculiar pottery of the region, were a different people, and imitated nature in their pottery, just as the mound builder did with his pipes. He had specimens on exhibition, and many illustrations showing this peculiar pottery representing men, animals, birds, fishes, shells and other things. The pottery makers' pipes were very unlike the mound builders', and were made for the insertion of a stem, the orifice generally being funnel shaped.

The speaker gave a spirited illustration of the great Temple mound, of Cohokia, opposite the mouth of the Missouri river, and describes it as a place of worship. This mound is 90 feet high. In the vicinity of this great mound were numerous flat square mounds called platforms. These platform mounds are usually ten or twelve feet high, and so large as often to contain on the summit farm-houses, with the out-buildings. In digging cellars, wells, etc., in these mounds, many relics were found; of these Mr. McAdams has a large collection. The speaker closed by describing a hitherto unknown earthwork, circular in form, one mile in circumference at the mouth of the Illinois river. Although the mounds occur in such great numbers and magnitude this seems to be the only earthwork in the region. Mr. McAdams expects to still prosecute his researches in this interesting locality.

DETERMINATION OF THE COMPARATIVE DIMENSIONS OF ULTIMATE MOLECULES; AND DEDUCTION OF THE SPECIFIC PROPERTIES OF SUBSTANCES.

By Prof. W. N. Norton.

In this paper a detailed exposition is given of the mechanical constitution of an ultimate molecule, the conditions of dynamical equilibrium are definitely stated, and several formulas investigated, representing its diverse mechanical features. From these definite mathematical expressions are deduced the general mechanical, physical, and chemical properties of substances. These are then employed in a detailed discussion of the properties of special substances. In this discussion the fundamental assumption is made that the atoms of different substances may differ in density, as well as in weight or mass. From this point of view it becomes possible to derive the comparative dimensions, and all the special features of the ultimate molecules of substances, from their molecular volumes and tenacities or co-officients of elasticity, as experimentally determined. The results of the numerical computations for a large variety of substances, from hydrogen to bismuth, are given in tables, and also represented graphically, and comparisons made with experimental results.

Chemical transformations are attributed to an effective force of electric tension developed by the contact of dissemilar molecules. An electro-motive force thus comes into play, determining an electric movement from one set of molecules to the other, and bringing them into approximate correspondence. The comparative values of the forces of electric tension, as well as of the electro-motive force, given in the tables, serve to make known the chemical relations of the substances considered. The chemical effects of heat are incidentally considered.

The entire discussion comprised in this and former papers may be epitomized as follows:

1—It has been shown that the mechanical laws and relations of bodies may be deduced from one general molecular formula; and that from their atomic weights, and certain comparative densities assigned to their atoms, may be derived definite expressions representative of the various properties of special substances.

2—We see that the deverse phenomena of Inanimate Nature are but different consequences of variations or inequalities of ethereal tension, produced by ethereal waves; and that, contemplated from the highest point of view, they may be conceived to result from the operation of one primary form of force on one primordial form of matter.

The publication of the papers read before the recent meeting of this Association will be continued in our next issue, September 18th.

LETTERS TO THE EDITOR.

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

When a publishing house prints a date at the foot of a title page it is not always a guarantee to the public that the matter of the book has a connection with the date. In a play, a novel, or even a history, the date of a new edition only suggests that some class of readers desires another form of the work. Bnt when the subject of the publication is of such a character as to require additions in the progress of events, it is necessary to enlarge, remodel, or amend the contents, to suit the advance of knowledge and the public need. This is generally announced on the production of a new book. Its advertisement, if not made in the preface, is invariably embodied in a date appended to the title page. In fact, so general has this custom become, that I do not think any one, who takes up a new book of this kind for the first time, would neglect to cast his eye upon the date of publication.

The other day, looking over the well filled shelves of Messrs. Appleton & Co., I picked up a book of this progressive class, to whose pages I have turned with pleasure duiing many years, for amusement and instruction. Its concise statement of the advances in physical science had always struck me as most complete. I purchased the book (Arnot's Elements of Physics) for old aquaintance sake, and, on reaching my library, looked through its familiar pages for the latest discoveries; but imagine my disgust, to find that the edition of 1880 made no mention of Telephone, Motograph or Phonograph, three applications of science which will make the last decade one of the most brilliant of the century.

This may not be a commercial, but it is surely a *scientific* fraud.

D. O. Farrow.

What constitutes an artificial mineral water is an important question to the consumer, for obvious reasons, and to the importer it is a serious matter, as commercial rivalry and custom duties have forced its consideration upon them and the authorities. Trouble has been caused in other countries, also, for want of a proper definition, and it has given rise to a German imperial decree in which a solution of the difficulty is attempted. This decree, reads as follows: "Under artificial mineral waters are included not only imitations of certain mineral waters as they occur in nature, but also is understood such other artificially prepared solutions of mineral substances as represent mineral waters, without corresponding in their chemical composition to natural waters."

BOOKS RECEIVED.

A PHYSICAL TREATISE ON ELECTRICITY AND MAGNETISM. By J. E. H. Gordon, B. A., Camb., Assistant Secretary of the British Association. In two volumes. [London : Sampson Low, Marston, Searle and Rivington. 1880.

One of our correspondents calls attention to what he considers a breach of privilege on the part of a publishing house, which affixes the date 1880 to a scientific text-book which does not mention the telephone or motograph. If this omission be a sin it is simply a sin of omission; but quite different is it in the case of Mr. J. E. H. Gordon, an English compiler of a work on electricity and magnetism, in two volumes, which has come to us, with a flourish of trumpets, across the Atlantic. This book, which gives many pages (we will not say, however, too many, as the subject is an interesting one) to De La Rue's beautiful experiments, and eighteen pages to Mr. Crook's etherial and radiant speculations with Mr. Gimingham's pretty tubes, only condescends to notice the Bell telephone in one brief page, and entirely ignores the existence of Edison's carbon telephone; although he recognizes the principle of the latter in Hughes' microphone, to which he gives great credit in another page. Aside from the unpardonable negligence evinced in this want of literary balance, which shows Mr. Gordon's incapacity as a book compiler, we here have a recurrence of an indignity unworthy of an Englishman. Mr. Gordon (who is an Assistant Secretary of the British Association) knows, or ought to know, that Mr. Preece exhibited an Edison musical telephone to the Association at their Plymouth meeting in 1877; and also that Edison's agent in London showed Mr. Hughes the carbon button and its properties in a telephone, three weeks before Mr. Hughes picked up the eliminated defects of the button as the principle of the microphone.

It does seem as if this ignoring the great services of the American Edison is but a part of a scientific conspiracy to falsify history. Where is the tasimeter, the most delicate electrical instrument for the measurement of radiant energy known to science? Where the motograph, that inexplicable wonder, which a telegraph company (more appreciative than Mr. Gordon) thought worth a hundred thousand dollars, the price they offered and paid for it? Where are their descriptions to be found in Mr. J. E. H. Gordon's Physical Treatise on Electricity and Magnetism? He does not deign to pen one line on the subject. This is either ignorance or folly; let Mr. Gordon accept which horn of the dilemma he thinks better.

However, considering the hasty manner in which the text of this book is thrown together, it can scarcely pass into currency, except as a beautifully illustrated catalogue of inventions and discoveries in which Mr. Gordon took no part. In this compilation (without reference, let it be distinctly understood, to the distinguished authors whose works are woven in without decent order or proportion), Mr. Edison shines by his absence.

The book cannot yet be purchased here, as the American buyers of the copyright are keeping it for the fall trade. We regret their connection with it, for what popularity can be expected, in this country, for a work on electricity that ignores the existence of Henry, Morse and Edison?

MANUAL OF HYDRAULIC MINING FOR THE USE OF THE PRACTICAL MINER. By T. F. Van Wagenen, E. M., New York, D. Van Nostrand, 1880.

Of all the problems presented to the mining engineer, there is none more important, nor simpler, than that contained in the subject of hydraulic mining of gold. It is only necessary to be sure of the premises and the results may be considered certain. There are, really, but two questions involved, water to move the soil, and place to put it in. If these conditions are fulfilled, it is not difficult to predict success to those who have but fair promise of paying ground. Once we know where is the dumping ground, how high the fall, and what the grade at command for sluicing boxes, all that has to be done is to bring water to the highest point above the workings; which, of course, presupposes it has been lead from the source to the place of fall on the least grade consistent with a sure and economical supply.

Much of the brain and sinew of the working classes in the far West has taken to this class of mining, as offering the most enduring profit and employment; but hydraulic mining requires something else besides mere will and muscle. For its successful application a certain knowledge of figures, rather than of mineralogy, is requisite. These hardy men do not always possess such knowledge, and for their instruction, Mr. Van Wagenen has written a little manual which will be read, studied and understood by many a practical miner.

The book can serve as a model for writers who have something valuable to say, and who wish to speak to men who have no desire to waste time in hunting for the truth.

PHYSICAL NOTES.

THE new electro-dynamic law of Clausius is receiving the deepest attention from the first electricians and mathematicians of Europe. The fundamental character of all his work and the acknowledged preëminence of his views, immediately demand an early investigation at the hands of his compeers. Already have Lorberg, Delsaulx, Frölich and others submitted this law to rigid analysis.

This new law of Clausius was advanced by the distinguished author only after finding himself unable to reconcile the two laws of Weber and Rieman with that simplicity which overwhelmingly addresses itself to our reason; and because they seem too complicated to be used as explanatory of these molecular currents so felicitously employed by Ampère in his theory of magnetism. These laws of Rieman and Weber require us to believe in the existence of two equal and opposite currents as originating all electrical action. In another essential point, Clausius (Annalen der Physic und Chemic X., 4, p. 609), finds himself compelled to differ from Rieman and Weber. They assume a *relative* motion between the electrified particles. Rieman using the word in its ordinary acceptation, according to which the difference of the components of velocity of the two electrical particles is made to represent the components of velocity of the relative motion; and Weber referring the relative motion to the mutual advance and retreat of the particles. Clausius rejects this method, which would confine one in the consideration of the subject to relative motion, and treats of both individual motions of the particles in their action on each other.

In the same number of the Annalen is an article by Herr Budde, on the laws of Clausius, in which, as an adherent of the theory, he exposes the fallacy of Frölich's interpretation.

THE contrary effects of sunlight in relation to certain chemical compounds, is noted by T. P. Blunt (*Analyst*. 1880, 79–81), who finds that an oxalic acid solution exposed to the light is rapidly decomposed, which is not the case in the dark. If this observation of Mr. Blunt is substantiated the use of that valuable re-agent in stoichiometry, where it serves as a basis for standardizing, will have to be restricted. From the ease with which oxalic acid is dried and weighed, and its non-corrosive nature, it has been considered almost invaluable in the working laboratory.

Mr. Blunt, on the other hand, finds that ferrous iodide requires the light in order to prevent decomposition. Does not this anomalous action of light point to a mechanical association and dissociation of molecules, analagous to that separation of tangible bodies effected by sound, as seen in Chladni figures?

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SATURDAY, SEPTEMBER 18, 1880.

Professor Alexander Agassiz's address, delivered in Saunders' Theatre, Cambridge, which we print in full on another page, must be considered one of the most important events of the great Boston meeting.

In his position as Vice-President of the Biological Section nothing could be more appropriate than the expression of his views upon the direction which modern biological research is taking. Animated by his own experience and convictions, his address was a deliberate and able attack upon the prevailing tendency towards too rapid generalization—a tendency which has been increasing during the last fifteen years, and is clearly the outgrowth of the intense desire of modern biologists to break down each and every barrier which obstructs our view in the history of development. Natural and laudable as is the desire to leave no stone unturned in our knowledge of the relationships of the different branches of the animal kingdom, it can only result in the obstruction of future investigators if it is not kept strictly within the limits of the truth. Phylogenetic inquiries add greatly to the zest of study, but should not be carried so far as to hamper or obscure the real end in view, which is, of course, truth and precision of statement, with the line sharply defined between what is actually seen and that which it is inferred ought to be seen.

Prof. Agassiz based his conclusions upon his comprehensive study of the sea-urchin. Stating as a premise the now well-known fact that in their embyological development the modern forms repeat the stages through which their ancestors passed in fossil history, he carefully traced the parallelism in a number of modern and fossil forms, giving an outline of his recent study. The results have been in all cases in positive confirmation of the above premise, and show the very close affiliation of the oldest and most recent forms, in general characters. But while the sea-urchins, with a comparatively small number of existing species, and with a comparatively complete fossil record, offer a tempting field for speculation, Prof. Agassiz denied his right to group the genera into anything like a complete genealogical tree. "If," he concluded, "when we take one of the most limited groups of the animal kingdom, we find ourselves engaged in a hopeless task, what must be the prospect should we attack the problem of other classes or groups of the animal kingdom, where the species run into thousands, while they number only tens in the case we have attempted to carry out? Shall we say 'ignorabimus' or 'impavidi progrediamus,' and valiantly chase a phantom we can never hope to seize?"

It was hardly to be expected that such an attack as this would pass unnoticed, and in fact, one of the features of the meetings of the Biological Section was a debate growing out of it, which took place on the following day. Prof. Cope had been reading an able paper upon the succession of the extinct Felidæ, pointing to the modifications of the teeth as a basis for forming a complete genetic series. At the close of his paper he called Prof. Agassiz's attention to the fact that here, in the cat family, was an instance leading in quite an opposite direction to that which Prof. Agassiz had assumed in his address the day before. An interesting discussion followed. Prof. Agassiz said he did not object to the grouping of genera into lines of descent where the structural characters were sufficiently homologous, but he did object to regarding such affinity as justifying the introduction of hypothetical links into other parts of the chain, and he did not see that the modifications of a single character, the teeth, warranted the phylogenetic conclusions which Prof. Cope had just reached. Prof. Burt Wilder added that, in his own study upon the pectoral muscles of the dog and other animals, he had found the fallacy of hasty generalization for genetic inferences, drawn from the muscles alone, would widely differ from the facts of actual relationship. Prof. Cope replied that in such questions all must admit that different values should be assigned to different parts of the animal frame, and among the hard parts, of course, he ranked, first, the limbs, then, he said, came the teeth. In justification of his arrangement of the extinct cats into two lines of descent from a common ancestor, he said that the complication of the brains confirmed the history told by the teeth. Prof. Agassiz emphatically repeated his statement of the day before and the discussion closed. We hope this address will be widely circulated and read; if received in the spirit in which Prof. Agassiz intended it, its effect will be admirable. The reaction from the theory of special creation is running strongly in every quarter, and in a day when we find ingenious speculations advanced even in small memoirs, every one must admit the necessity of a more conservative spirit. There is no danger of going into the old and opposite extreme, nor does Prof. Agassiz's address encourage the return movement. It is a re-statement of the old piece of advice—do not attempt to run before you are sure you can walk.

ADDRESS BY ALEXANDER AGASSIZ,

PALEONTOLOGICAL AND EMBRYOLOGICAL DEVELOPMENT.

Since the publication of the "Poissons Fossiles" by Agassiz and of the "Embryologie des Salmonidées" by Vogt, the similarity, traced by the former between certain stages in the growth of young fishes and the fossil representatives of extinct members of the group, has also been observed in nearly every class of the animal kingdom, and the fact has become a most convenient axiom in the study of paleontological and embryological development. This parallelism, which has been on the one side a strong argument in favor of design in the plan of creation, is now, with slight emendations, doing duty on the other as a newly discovered article of faith in the new biology.

But while in a general way we accept the truth of the proposition that there is a remarkable parallelism between the embryonic development of a group and its paleontological history, yet no one has attempted to demonstrate this, or rather to show how far the parallelism extends. We have up to the present time been satisfied with tracing the general coincidence, or with striking individual cases.

The resemblance between the pupa stage of some Insects and of adult Crustacea, the earlier existence of the latter, and the subsequent appearance of the former in paleontological history, furnished one of the first and most natural illustrations of this parallelism; while theoretically the necessary development of the higher tracheate insects from their early branchiate aquatic ancestors seemed to form an additional link in the chain, and point to the Worms, the representatives of the larval condition of Insects, as a still earlier embryonic stage of the Articulates.

Indeed, there is not a single group of the animal kingdom in which embryology has not played a most important part in demonstrating affinities little suspected before. The development of our frogs, our salamanders, has given us the key to much that was unexplained in the history of Reptiles and Batrachians. The little that has been done in the embryology of Birds has revolutionized our ideas of a class which at the beginning of the century seemed to be the most naturally circumscribed of all. Embryology and paleontology combined have led to the recognition of a natural classification uniting Birds and Reptiles on the one side and Batrachians and Fishes on the other. It is to embryology that we owe the explanation of the affinities of the old Fishes in which Agassiz first recognized the similarity to the embryo of Fishes now living, and by its aid we may hope to understand the relationship of the oldest representatives of the class. It has given us the only explanation of the early appearance of the Cartilaginous Fishes, and of the probable formation of the earliest vertebrate limb from the lateral embryonic fold, still to be traced in the young of the Osseous Fishes of to-day.

Embryology has helped us to understand the changes aquatic animals must gradually undergo in order to become capable of living upon dry land. It has given us pictures of swimming-bladders existing as rudimentary lungs in Fishes with a branchial system; in Batrachians it has shown us the persistence of a branchial system side by side with a veritable lung. We find among the earliest terrestrial Vertebrates, types having manifest affinities with the Fishes on one side and Batrachians on the other, and we call these types Reptiles; but we should nevertheless do so with a reservation, looking to embryology for the true meaning of these half-fledged Reptiles, which lived at the period of transition between an aquatic and terrestrial life, and must therefore always retain an unusual importance in the study of the development of animal life.

When we come to the embryology of the marine Invertebrates, the history of the development of the barnacles is too familiar to be dwelt upon, and I need only allude to the well-known transformations of the Echinoderms, of the Acalephs, Polyps, in fact of every single class of Invertebrates, and perhaps in none more than in the Brachiopods, to show how far-reaching has been the influence of embryology in guiding us to a correct reading of the relations between the fossils of successive formations. There is scarcely an embryological monograph now published dealing with any of the later stages of growth which does not speak of their resemblance to some type of the group long ago extinct. It has therefore been most natural to combine with the attempts constantly made to establish the genetic sequence between the genera of successive formations, an effort to establish also a correspondence between their paleontological sequence and that of the embryonic stages of development of the same, thus extending the mere similarity first observed between certain stages to a far broader generalization.

It would carry me too far to sketch out, except in a most general way, even for a single class, the agreement known to exist in certain groups between their embryonic development and their paleontological history. It is hinted at in the succession of animal life of any period we may take up, and perhaps cannot be better expressed than by comparing the fauna of any period as a whole with that of following epochs—a zoölogical system of the Jura, for instance, compared with one made up for the Cretaceous; next, one for the Tertiary, compared with the fauna of the present day. In no case could we find any class of the animal kingdom bearing the same definitions or characterized in the same manner. But apply to this comparison the data obtained from the embryological development of our present fauna, and what a flood of light is thrown upon the meaning of the succession of these apparently disconnected animal kingdoms, belonging to different geological periods, especially in connection with the study of the few ancient types which have survived to the present day from the earliest times in the history of our earth!

Although there is hardly a class of the animal kingdom in which some most interesting parallelism could not be drawn, and while the material for an examination of this parallelism is partially available for the Fishes, Mollusks, Crustacea, Corals, and Crinoids, yet for the illustration and critical examination of this parallelism I have been led to choose to-day a very limited group, that of Sea-urchins, both on account of the nature of the material and of my own familiarity with their development and with the living and extinct species of Echini. The number of living species is not very great—less than three hundred—and the number of fossil species thus far known is not, according to Zittel, more than about two thousand. It is therefore possible for a specialist to know of his own knowledge the greater part of the species of the group. It has been my good fortune to examine all but a few of the species now known to exist, and the collections to which I have had access contain representatives of the majority of the fossil species. Sea-urchins are found in the oldest fossiliferous rocks; they have continued to exist without interruption in all the strata up to the present time. While it is true that our knowledge of the Sea-urchins occurring before the Jurassic period is not very satisfactory, it is yet complete enough for the purposes of the present essay, as it will enable me, starting from the Jurassic period, to call your attention to the paleontological history of the group, and to compare the succession of its members with the embryological development of the types now living in our seas. Ample material for making this comparison is fortunately at hand; it is material of a peculiar kind, not easily obtained, and which thus far has not greatly attracted the attention of zoölogists.

Interesting and important as are the earliest stages of embryonic development in the different classes of the animal kingdom, as bearing upon the history of the first appearance of any organ and its subsequent modifications, they throw but little light on the subject before us. What we need for our comparisons are the various stages of growth through which the young Sea-urchins of different families pass from the time they have practically become Sea-urchins until they have attained the stage which we now dignify with the name of species. Few embryologists have carried their investigations into the more extended field of the changes the embryo undergoes when it begins to be recognized as belonging to a special class, and when the knowledge of the specialist is absolutely needed to trace the bearing of the changes undergone, and to understand their full meaning. Fortunately the growth of the young Echini has been traced in a sufficient number of families to enable me to draw the parallelism between these various stages of growth and the paleontological stages in a very different manner from what is possible in other groups of the animal kingdom, where we are overwhelmed with the number of species, as in the Insects or Mollusks, or where the paleontological or the embryological terms of comparison are wanting or very imperfect.

Beginning with the paleontological history of the regular Sea-urchins of the time of the Trias, when they constituted an unimportant group as compared with the Crenoids, we find the Echini of that time limited to representatives of two families. One of these, the genus Cidaris, has continued to exist, with slight modifications, up to the present time, and not less than one-tenth of all the known species of fossil Echini belong to this important genus, which in our tropical seas is still a prominent one. It is interesting here to note that in the Cidaridæ the modifications of the test are not striking, and the fossil genera appearing in the successive formations are distinguished by characters which often leave us in doubt as to the genus to which many species should be referred. In the genus Rhabdocidaris, which appears in the lower Jura, and which is mainly characterized by the extraordinary development of the radioles, we find the extreme of the variations of the spines in this family. From that time to the present day, the most striking differences have existed in the shape of the spines, not only of closely allied genera, but even in specimens of the same species; differences which in some of the species of to day are as great as in older geological periods. The oldest Cidaridæ are remarkable for their narrow poriferous zones. It is only in the Jura that they widen somewhat; subsequently the pores become conjugated, and only later, during the Cretaceous period, do we find the first traces of any ornamentation of the test (Temnocidaris) so marked at the present day in the genus Goniocidaris. As far, then, as the Cidaridæ are concerned, the modifications which take place from their earliest appearance are restricted to slight changes in the poriferous zone and in the ornamentation of the test, accompanied with great variability in the shape of the primary radioles. We must except from this statement the genera Diplocidaris and Tetracidaris, to which I shall refer again. The representatives of the other Triassic family become extinct in the lower tertiaries. The oldest genus, Hemicidaris, undoubtedly represents the earliest deviations from the true Cidaris type; modifications which affect not only the poriferous zone, but the test, the actinal and the abactinal systems, while from the extent of these minor changes we can trace out the gradual development of some of the characteristics in families of the regular Echini now living. The genus Hemicidaris may be considered as a Cidaris in which the poriferous zone is narrow and undulating, in which the granules of the ambulacral system have become minute tubercles in the upper portion of the zone and small primary tubercles in its actinal region, in which many of the interambulacral granules become small secondaries, in which the plates of the actinal system have become reduced in number, and the apical system has become a narrow ring, and finally in which the primary radioles no longer assume the fantastic shapes so common among the Cidaridæ.

We can trace in this genus the origin of the modifications of the poriferous zone, leading us, on the one side, through genera with merely undulating lines of pores to more or less distinct confluent arcs of pores, formed round the primary ambulacral tubercles, and, on the other, to the formation of open arcs of three or more pairs of pores. The first type culminates at the present day with the Arbaciadæ, the other with the Diadematidæ, Triplechinidæ, and Echinometradæ. This specialization very early takes place, for already in the lower Jura Stomechinus has assumed the principal characteristics of the Triplechinidæ of to-day.

Although in Hemicidaris the number of the coronal plates has increased as compared with the Cidaridæ, and while we find that in many genera, even of those of the present day, the number of the coronal plates is still comparatively small, yet, as a general rule, the more recent formations contain genera in which the increase in number of the interambulacral plates is accompanied by a corresponding decrease in the number of plates of the interambulacral area so characteristic thus far of the Cidaridæ and Hemicidaridæ, a change also affecting the size of the primary ambulacral tubercles. This increase in the number of the coronal plates is likewise accompanied by the development of irregular secondary and miliary tubercles, and the disappearance in this group of the granular tuberculation, so important a character in the Cidaridæ. With the increase in the number of the interambulacral coronal plates, the Pseudodiadematidæ still retain prominent primary tubercles, recalling the earlier Hemicidaridæ and Cidaridæ, and, as in the Cidaridæ proper, the test is frequently ornamented by deep pits or by ridges formed by the junction of adjoining tubercles. The genital ring becomes narrower, and the tendency to the specialization of one of its plates, the madreporite, more and more marked.

With the appearance of Stomechinus, the Echinidæ proper already assume in the Jura the open arcs of pores, the large number of coronal interambulacral plates, the specialization of the secondary tubercles, and the large number of primary tubercles in each plate. With the appearance of Sphærechinus in the early Tertiary come in all the elements for the greater multiplication of the pairs of pores in the arcs of the poriferous zones, while the gigantic primary spines of some of the genera (Heterocentrotus), and the small number of primary tubercles are structural features which had completely disappeared in the group preceding the Echinometradæ, to which they appear most closely allied.

Going back again to the Hemicidaridæ, it requires but slight changes to pass from them to Acrosalenia and to the Saleniæ proper; the latter have continued to the present day, and have, like the Cidaridæ, retained almost unchanged the characters of the genera which preceded them, combined, however, with a few Cidaridian and Echinid features which date back to the Triassic period. We can thus trace the modifications which have taken place in the poriferous zone, the apical and actinal systems, the coronal plates, the ambulacral and interambulacral tubercles, as well as in the radioles, and in the most direct manner possible indicate the origin of the peculiar combination of structural features which we find at any geological horizon. On taking in succession the modifications undergone by the different parts of the test, we can trace each one singly, without the endless complication of combinations which any attempt to trace the whole of any special generic combination would imply.

Leaving out of the question for the moment the Palæchinidæ, we find no difficulty in tracing the history of the characters of the genera of the regular Echini which have existed from the time of the Trias and are now living, provided we take up each character independently. Nothing can be more direct than the gradual modification of the simple, barely undulating poriferous zone, made up of numerous ambulacral plates covered by granules, such as we find it among the Cidaridæ of Trias, first into the slightly undulating poriferous zone of the Hemicidaridæ, next into the indistinct arcs of pores of the Pseudodiadematidæ, then into the arcs with a limited number of pores of the Triplechinidæ, and finally to the polyporous arcs of the Echinometradæ. What can be more direct than the gradual modification to be traced in the development of the primary ambulacral tubercles, such as are characteristic of the Echinidæ of the present day, from their first appearance at the oral extremity of the ambulacral system of the Hemicidaridæ, and the increase in the number of primary interambulacral tubercles, accompanied by the growth of secondaries and miliaries, which we can trace in Hemicidaridis, Acrosalenia, and Stomechinus,—the increase in number of primary and secondary tubercles being accompanied by a reduction in the size of the radioles and a greater uniformity in their size and shape?

But while these modifications take place, the original structural feature may be retained in an allied group. Thus the Cidaridæ retain unchanged from the earliest time to the present day the few primary tubercles, the secondary granules, the simple poriferous zone, the imbricating actinal system, and the few coronal plates, with the large apical system and many-shaped radioles; while in the Salenidæ the primary internambulacral tubercles, the secondary granules, the radioles, the genital ring, are recognized features of the Cidaridæ, associated, however, with an Echinid actinal and anal system, Hemicidarid primary ambulacral tubercles, and an Echinid poriferous zone. In the same way in the Diadematidæ, the large primary internambulacral tubercles are Cidaridian features, while the structure of the ambulacral tubercles is Hemicidaridian. The existence of two kinds of spines is another Cidaridian feature, while the apical and actinal systems have become modified in the same direction as that of the Echinidæ. The more recent the genus, the greater is the difficulty of tracing in a direct manner the origin of any one structural feature, owing to the difficulty of disassociating structural elements characteristic of genera which may be derived from totally different sources. This is particularly the case with genera having a great geological age. Many of them, especially among the Spatangoids, show affinities with genera following them in time, to be explained at present only on the supposition that, when a structural feature has once made its appearance, it may reappear subsequently, apparently as a new creation, while in reality it is only its peculiar combination with structural features with which it had not before been associated (a new genus), which conceals in that instance the fact of its previous existence. A careful analysis, not only of the genera of the order, but sometimes of other orders which have preceded this combination in time, may often reveal the elements from which have been produced apparently unintelligible modifications.

There is, however, not one of the simple structural features in the few types of the Triassic and Liassic Echini from which we can so easily trace the origin of the structural features of all the subsequent Echinid genera, which is not also itself continued to the present day in some generic type of the present epoch, fully as well characterized as it was at the beginning. In fact, the very existence to-day of these early structural features seem to be as positive a proof of the unbroken systematic affinity between the Echini of our seas and those of the Trias, as the uninterrupted existence of the genus Pygaster or Cidaris from the Trias down to the present epoch, or of the connection of many of the genera of the Chalk with those of our epoch (Salenia, Cyphosoma, Psammechinus, etc.).

Passing to the Clypeastridæ, we find there as among the Desmosticha that the earliest type, Pygaster, has existed from the Trias to the present time ; and that, while we can readily reconstruct, on embryological grounds, the modifications the earliest Desmosticha-like Echini should undergo in order to assume the structural features of Pygaster, yet the early periods in which the precursors of the Echinoconidæ and Clypeastridæ are found have thus far not produced the genera in which these modifications actually take place. But, starting from Pygaster, we naturally pass to Holectypus, to Discoidea, to Conoclypus, on the one side, while on the other, from Holectypus to Echinocyamus, Sismondia, Fibularia, and Mortonia, we have the natural sequence of the characters of the existing Echinanthidæ, Laganidæ, and Scutellidæ, the greater number of which are characteristic of the present epoch. If we were to take in turn the changes undergone in the arrangement of the plates of the test, as we pass from Pygaster to Holectypus, to Echinocyamus, and Echinanthidæ, we should have in the genera which follow each other in the paleontological record an unbroken series showing exactly what these modifications have been. In the same way, the modifications of the abactinal and anal systems, and those of the poriferous zone, can equally well be followed to Echinocyamus, and thence to the Clypeastridæ ; while a similar sequence in the modifications of these structural features can be followed from Mortonia to the Scutellidæ of the present period.

Passing finally to the Petalosticha, we find no difficulty in tracing theoretically the modifications which our early Echinoconidæ of the Lias should primarily undergo previous to the appearance of Galeropygus. The similarity of the early Cassiduloid and Echinoneoid types points to the same systematic affinity, and perhaps even to a direct and not very distant relationship with the Palæchinidæ. For if we analize the Echinothuriæ of the present day, we find in genera like Phormosoma many structural features, such as the shape of the test the character of the spines, the structure of the apical system, that of the poriferous zone, indicative of possible modifications in the direction of Pygaster or of Galeropygus, which have as yet not been taken into account.

Adopting for the Petalosticha the same method of tracing the modifications of single structural features in their paleontological succession, we trace the comparatively little modified paleontological history of the Echinoneidæ of the present day from the Pyrina of the lower Jura. This, in its turn, has been preceded by Hyboclypus and Galeropygus, while the Echinolampadæ of the present day date back, with but trifling modifications, to the Echinobrissus of the Lias, itself preceded by Clypeus ; and they have been subject only to slight generic changes since that time, Echinobrissus being still extant, while such closely allied genera as Catopygus and Cassidulus of the earlier Cretaceous are still represented at the present day ; the modifications taking place in the actinal system, in the ambulacal zones of the Echinoconidæ and of the Echinolampadæ showing the closest possible systematic affinity in these families. Starting again from Hyboclypus, with its elongate apical system, we naturally pass to Collyrites and the strange Dysasteridæ forms which, in their turn, are closely allied to the Holasteridæ. From Holaster on the one side, and from Toxaster on the other, we find an unbroken sequence of structural characters uniting the successive genera of Holasteridæ, such as Cardiaster, Offaster, Stenonia, Ananchytes, and Asterostoma, with Paleopneustes, Homolampas and the Pourtalesiæ of the present day, while from the genera of the Toxasteridæ we naturally pass to the cretaceous Hemiaster ; in the genus and the subsequent Micraster we find all the elements necessary for the modifications which appear in the Spantanginæ from the time of the Chalk to the present day. These modifications result in genera in which we trace the development of the fascioles, of the actinal, anal, and abactinal plastrons, of the beak, the formation of the petaloid ambulacra, first flush with the test, and little by little changed into marsupial pouches, the growth of the anterior groove and the manifold modifications of the ambulacral system in Spatangus Agassizia, and Echinocardium, often recalling in some of its features structural characters of families which have preceded this in time.

Apparently in striking contrast with the Echini of the secondary period and those which have succeeded them stand the Paleozic Echini ; but when we have examined the embryology of Echini, we shall be better prepared to understand their structure and the affinities of the Palæchinidæ with the Echini of the present day and their immediate predecessors.

Taking up now the embryological development of the several families which will form the basis of our comparisons, beginning with the Cidaridæ, we find that in the earliest stages they very soon assume the characters of the adult, the changes being limited to the development of the abactinal system, the increase in number of the coronal plates, and the modifications of the proportionally gigantic primary radioles.

In the Diadematidæ the changes undergone by the young are limited to the gradual transformation of the embryonic spines into those which characterize the family, to the changes of the vertical row of pores in the ambulacral area into arcs of three or four pairs of pores, and to the specialization of the actinal and abactinal systems.

In the Arbaciadæ the young stages are remarkable for the prominent sculpture of the test, for the flattened spines, for their simple poriferous zone, for their actinal system, and for their genital ring. The anal plates appear before the genital ring.

In the Echinometradæ the young thus far observed are characterized by the small number of their primary tubercles, the large size of the spines, the simple vertical row of pores, the closing of the anal ring by a single plate, and the turban-shaped outline of the test. Little by little, the test loses with increasing age this Cidaris-like character ; it reminds us, from the increase in the number of its plates, more of Hemicidaris ; then, with their still greater increase, of the Pseudodiadematidæ ; and, finally, of the Echinometradæ proper. The spines, following *pari passu* the changes of the test, lose little by little their fantastic embryonic, or rather Cidaris-like appearance, and become more solid and shorter, till they finally assume the delicately fluted structure characteristic of the Echinometradæ. The vertical poriferous zone is first changed into a series of connected vertical arcs, which become disjointed, and form, with increasing age, the independent arcs of pores, composed of three or more pair of pores, of the Echinometradæ.

In the Echinidæ proper we find in the young stages the same unbroken vertical line of pores, which gradually becomes changed to the characteristic generic types. We find, as in the Echinometradæ, an anal system closed with a single plate, and an abactinal system separating in somewhat more advanced stages from the coronal plates of the test. This is as yet made up of a comparatively small number of plates, carrying but few large primary tubercles, with fantastically shaped spines entirely out of proportion to the test, but which, little by little, with the increase of the number of coronal plates, the addition of primary tubercles, and their proportional decrease in size, assume more and more the structure of the genus to which the young belongs. The original anal plate is gradually lost sight of from the increase in number of the plates covering the anal system, and it is only among the Temnopleuridæ that this anal plate remains more or less prominent in the adult. In the Salinidæ, of which we know as yet nothing of the development, this embryonic plate remains permanently a prominent structural feature of the apical system.[1]

Among the Clypeastroids the changes of form they undergo during growth are most instructive. We have in the young Fibularinæ an ovoid test, a small number of coronal plates surmounted by few and large primary tubercles, supporting proportionally equally large primary radioles, simple rectilinear poriferous zones, no petaloid ambulacra,—in fact, scarcely one of the features we are accustomed to associate with the Clypeastroids is as yet prominently developed. But rapidly, with increasing size, the number of primary tubercles increases, the spines lose their disproportionate size, the pores of the abactinal region become crowded and elongate, and a rudimentary petal is formed. The test becomes more flattened, the coronal plates increase in number, and it would be impossible to recognize in the young Echinocyamus, for instance, the adult of the Cidaris-like or Echinometra-like stages of the Sea-urchin, had we not traced them step by step. Most interesting, also, is it to follow the migrations of the anal system, which, to a certain extent, may be said to retain the embryonic features of the earlier stages of all Echinoderm embryos, in being placed in more or less close proximity to the actinostome. What has taken place in the growth of the young Echinocyamus is practically repeated for all the families of Clypeastroids: a young Echinarachnius, or Mellita, or Encope, or a Clypeaster proper, resembles at first more an Echinometra than a Clypeastroid; they all have simple poriferous zones and spines and tubercles out of all proportion to the size of the test.[2]

When we come to the development of the Spatangoids, we find their younger stages also differing greatly from the adult. Among the Nucleolidæ, for instance, the young stages have as yet no petals, but only simple rectilinear poriferous zones. They are elliptical with a high test, with a single large primary tubercle for each plate, and a simple elliptical actinostome, without any trace of the typical bourrelets and phyllodes so characteristic of this family. Very early, however, this condition of things is changed, the test soon becomes more flattened, the petals begin to form as they do in the Clypeastroids, and we can soon trace the rudiments of the peculiar bourrelets characteristic of the family, accompanied by a rapid increase in the number of tubercles and in that of the coronal plates.

Among the Spatangidæ some are remarkable in their adult condition for their labiate actinostome, for the great development of the petals, for the presence of fascioles surrounding certain definite areas, for the small size of the tubercles, the general uniformity in the spines of the test, and the specialization of their anterior and posterior regions. On examining the young stages of this group of Spatangoids, not one of these structural features is as yet developed. The actinostome is simple, the poriferous zone has the same simple structure from the actinostome to the apex, the primary tubercles are large, few in number, surrounded by spines which would more readily pass as the spines of Cidaridæ than of Spatangoids. The fascioles are either very indistinctly indicated, or else the special lines have not as yet made their appearance; the ambulacral suckers of the anterior zone are as large and prominent as those of the young stages of any of the regular Echini. It is only little by little, with advancing age, that we begin to see signs of the specialization of the anterior and posterior parts of the test, that we find the characteristic anal or lateral fascioles making their appearance, only with increasing size that the spines lose their Cidaris-like appearance, that the petals begin to be formed, and that the simple actinostome develops a prominent posterior lip. In the genus Hemiaster, the young stages are especially interesting, as long before the appearance of the petals, while the poriferous zone is still simple, the total separation of the bivium and of the trivium of the ambulacral system, so characteristic of the earliest Spatangoids (the Dysasteridæ), is very apparent.[3]

From this rapid sketch of the changes of growth in the principal families of the recent Echini we can now indicate the transformations of a more general character through which the groups as a whole pass.

In the first place, while still in the Pluteus all the young Echini are remarkable for the small number of coronal plates, and for the absence of any separation between the actinal and abactinal systems and the test proper. They all further agree in the large size of the primary spines of the test, whether it be the young of a Cidaris, an Arbacia, an Echinus, a Clypeaster, or a Spatangoid. They all in their youngest stages have simple vertical ambulacral zones; beyond this, we find as changes characteristic of some of the Desmosticha, the specialization of the actinal system from the coronal plates, the formation of an anal system, the rapid increase in the number of the coronal plates, with a corresponding increase in the number of the spines and a proportional reduction of their size, the formation of an abactinal ring, and the change of the simple vertical poriferous zone into one composed of independent arcs.

In the Spatangoids and Clypeastroids we find common to both groups the shifting of the anal system to its definite place, the modifications of the abactinal part of the simple ambulacral system in order to become petaloid, and the gradual change of the elliptical ovoid test of the young to the characteristic generic test, accompanied by the rapid increase in the number of the primary tubercles and spines. Finally limited to the Spatangoids are the changes they undergo in the transformation of the simple actinostome to a labiate one, the specialization of the anterior and posterior parts of the test, and the definite formation of the fascioles.

Comparing this embryonic development with the paleontological one, we find a remarkable similarity in both, and in a general way there seems to be a parallelism in the appearance of the fossil genera and the successive stages of the development of the Echini as we have traced it.

We find that the earlier regular Echini all have more or less a Cidaris-like look,—that is, they are Echini with few coronal plates; large primary tubercules, with radioles of a corresponding size; that it is only somewhat later that the Diademopsidæ make their appearance, which, in their turn, correspond within certain limits to the modifications we have traced in the growth of the young Diadematidæ and Arbaciadæ. The separation of the actinal system from the coronal plates has been effected. The poriferous zone has either become undulating, or forms somewhat indefinite open arcs; we find in all the genera of this group a larger number of coronal plates, more numerous primaries, the granules of the Cidaridæ replaced by secondaries and miliaries, and traces of a Hemicidaris-like stage in the size of the actinal ambulacral tubercules.

Comparing in the same way the paleontological development of the Echinidæ proper, we find that, on the whole, they agree well with the changes of growth we can still follow to-day in their representatives, and that, as we approach nearer the present epoch, the fossil genera more and more assume the structual features which we find de-

[1] The young of the following genera have served as a basis for the preceding analysis of the embryonic stages of the Desmosticha: Cidaris, Dorocidaris, Goniocidaris, Arbacia, Porocidaris, Strongylocentrotus, Echinometra, Echinus, Toxopneustes, Hipponoë, Temnopleurus, Temnechinus, and Trigonocidaris.

[2] Among the Clypeastroids I have examined the young of Echinocyamus, Fibularia, Mellita, Laganum, Echinarachnius, Encope, Clypeaster, and Echinanthus.

[3] For this sketch of the embryology of the Petalosticha I have examined the young of Echinolampus, Echinoneus, Echinocardium, Brissopsis, Agassizia, Spatangus, Brissus, and Hemiaster.

veloped last among the Echinidæ of the present day. Very much in the same manner as a young Echinus develops, they lose, little by little, first their Cidaridian affinities, which become more and more indefinite, next their Didematidian affinities, if I may so call the young stages to which they are most closely allied, and, finally, with the increase in the number of the coronal plates, the great numerical development of the primary tubercles and spines, and that of the secondaries and miliaries which we can trace in the fossil Echini of the Tertiaries, we pass insensibly into the generic types characteristic of the present day.

Although we know nothing of the embryology of the Salenidæ, yet, like the Cidaridæ, they have in a great measure remained a persistent type, the modifications of the group being all in the same direction as those noticed in the other Desmosticha ; a greater number of coronal plates ; the development of secondaries and miliaries combined with a specialization of the actinal system not found in the Cidaridæ.

An examination of the succession of the Echinoconidæ shows but little modification from the earliest types ; the changes, however, are similiar to those undergone by the Clypeastroids and Petalosticha, though they do not extend to modifications of the poriferous zone, but are mainly changes in the actinostome and in the tuberculation. In fact, the group of Echinoconidæ seems to hold somewhat the same relation to the Clypeastroids which the Salenidæ hold to the Cidaridæ, and the earliest genus of the group (Pygaster) has remained, like Cidaris, a persistent type to the present day.

The earliest Clypeastroids are all forms which resemble the Fibularina and the genera following Echinocyamus and Fibularia ; they are mainly characterized by the same changes which an Echinarachnius or a Mellita, for instance, undergoes as it passes from its Echinocyamus stage to the Laganum or Encope stage. The comparison is somewhat more complicated when we come to the Spatangoids. The comparison of the succession of genera in the different families, as traced in the Desmosticha and Clypeastroids, is made difficult from the persistency of the types preceding the Echinoneidæ and the Ananchytidæ, which have remained without important modifications from the time of the lower Cretaceous ; previous to that time the modifications of the Cassidulidæ are found to agree with the changes which have been observed in the growth of Echinolampas. The early genera, like Pygurus, have many of the characteristics of the test of the young Echinolampas. The development of prominent bourrelets and of the floscelle and petals goes on side by side with that of genera in which the modification of the actinostome, of the test, and of the petals is far less rapid, one group retaining the Echinoneus features, the other culminating in the Echinolampas of the present day, and having likewise a persistent type, Echinobrissus, which has remained with its main structural features unchanged from the Jura to the present day. That is, we find genera of the Cassidulidæ which recall the early Echinoneus stage of Echinolampas, next the Caratomus stage, after which the floscelle, bourrelets, and petals of the group become more prominent features of the succeeding genera. Accompanying the persistent type Echinobrissus, genera appear in which either the bourrelets or petals have undergone modifications more extensive than those of the same parts in the genera of the Echinoneus or Caratomus type.

The earliest Spatangoids belong to the Dysasteridæ, apparently an aberrant group, but which, from the history of the young Hemiaster, we now know to be a strictly embryonic type, which, while it thus has affinities with the true Spatangoids, still retains features of the Cassidulidæ in the mode of development of the actinostome and of the petals, as well as of the anal system. The genera following this group, Holaster and Toxaster, can be well compared, the one to the young stages of Spatangus proper before the appearance of the petals, when the ambulacra are flush with the test, and when its test is more or less ovoid, the other to a somewhat more advanced stage, when the petals have made their appearance as semi-petals. In both cases the actinostome has the simple structure characteristic of all the young Spatangoids. The changes we notice in the genera which follow them lead in the one case through very slight modifications of the abactinal system, of the anterior and posterior extremities of the test, to the Ananchytid-like Spatangoids of the present day, the Pourtalesiæ, the genus Holaster itself persisting till well into the middle of the Tertiary period ; while on the other side we readily recognize in the Spatanginæ which follow Toxaster (a persistent type which has continued as Palæostoma to the present day) the genera which correspond to the young stages of such Spatangoids as Spatangus and Brissopsis of the present day, genera which, on the one hand, lead from Hemiaster (itself still represented in the present epoch), through stages such as Cyclaster, Peripneustes, Brissus, and Schizaster, and, on the other, through Micraster and the like, to the Spatangoids, in which the development of the anal plastron and fasciole performs an important part, while in the former group the development of the peripetalus fasciole and of the lateral fasciole can be followed. None of the genera of Petalosticha belonging to the other groups devolop any fasciole in the sense of cirumscribing a limited area of the test.

The comparison of the genera of Echini which have appeared since the Lias with the young stages of growth of the principal families of Echini, shows a most striking coincidence amounting almost to identity between the successive fossil genera and the various stages of growth. This identity can, however, not be traced exactly in the way in which it has usually been understood, while there undoubtedly exists in the genera which have appeared one after the other a gradual increase in certain families in the number of forms, and a constant approach in each succeeding formation, in the structure of the genera, to those of the present day. It is only in the accordance between some special points of structure of these genera and the young stages of the Echini of the present day that we can trace an agreement which, as we go further back in time, becomes more and more limited. We are either compelled to seek for the origin of many structural features in types of which we have no record, or else we must attempt to find them existing potentially in groups where we had as yet not succeeded in tracing them. The parallelism we have traced does not extend to the structure as a whole. What we find is the appearance among the fossil genera of certain structural features giving to the particular stages we are comparing their characteristic aspect. Thus, in the succession of the fossil genera, when a structural feature has once made its appearance, it may either remain as a persistent structure, or it may become gradually modified in the succeeding genera of the same family, or it may appear in another family, associated with other more marked structural features which completely overshadow it. Take, for instance, among the Demosticha, the modifications of the poriferous zone of the actinal and abactinal systems of the coronal plates, of the ambulachral and interambulachral systems, the changes in the relative proportion of the primary-tubercles, and the development of the secondaries. These are all structural features which are modified independently one of the other ; we may find simultaneous development of these features in parallel lines, but a very different degree of development of any special feature in separate families.

This is as plainly shown in the embryological as in the paleontological development. In the Cidaridæ there is the minimum of specialization in these structural features. In the Diademopsidæ there is a greater range in the diversity of the structure of the poriferous zone and of the coronal plates, as well as of the actinal system. There is a still greater range among the Echinidæ, while among the Salenidæ the modifications, as compared to those of the Echinidæ and Diademopsidæ, are somewhat limited again, being restricted as far as relates to the poriferous zone and coronal plates, but specialized as far as the actinal system is concerned, and specially important with reference to the structure of the apical system. The special lines in which these modifications take place produce, of course, all possible combinations, yet they give us the key to the sudden appearance, as it were, of structural features of which the relationship must be sought in very distantly related groups. It is to this specialty in the paleontological development that we must trace, for instance, the Cidarid affinities of the Saleniæ, their papillæ, the existence of few large primary interambulacral tubercles, the structure of their apical system, and their large genital plates ; while it is to their affinities with the Hemicidaridæ that we must refer the presence of the few larger primary ambulacral tubercles at the base of the ambulacral area, and by their Diademopsid and Echinidian affinities that we explain the indented imbricated actinal system with the presence of a few genuine miliaries. But all the structural features which characterize the earliest types of the Desmosticha can in reality be traced, only in a somewhat rudimentary form, even in the Cidaridæ. The slight undulation of the closely packed, nearly vertical poriferous zone is the forerunner of the poriferous zone first separated into vertical arcs and then into independent arcs. The limitation in the number of the rows of granules in the ambulacral zone, and their increase in size, is the first trace of the appearance of the somewhat larger primary ambulacral tubercles of the Hemicidaridæ and Saleniæ. The existence of the smooth cylindrical spines of the abactinal region of the test naturally leads to similar spines covering the whole test in the other families in the Desmosticha. The difference existing in the plates covering the actinal system from those of the coronal plates leads to the great distinction between the structure of the actinal system and of the coronal plates in some of the Echinidæ.

Passing to the Clypeastridæ and Petalosticha, we trace a parallelism of the same kind, and readily in the successive genera of fossil Clypeastroids, but often in widely separated genera, the precise modifications which the poriferous zone has undergone as it first becomes known to us in Echinocyamus and Fibularia, and as we find it in the most complicated petaloid stage of the Clypeastroids of the present day. We readily trace the changes the test undergoes from its comparatively ovoid and swollen shape to assume first that of the less gibbous forms, next that of the Laganidæ, and finally of the flat Scutellidæ; while we trace in the Echinanthidæ the persistent structural features of some of the earliest Clypeastroids, together with an excessive modification of the poriferous zone. Likewise for the Echinoconidæ we trace mainly the slight modifications of the poriferous zone and of the coronal plates, and finally, when we come to the Spatangidæ we find no difficulty in tracing from the most Desmostichoid of the Spatangoid genera, the modifications of a test in which the ambulacral and interambulacral areas are made up of plates of nearly uniform size, in which the anterior and posterior extremities are barely specialized, to the most typical of the Ananchytidæ, in which the anterior and posterior extremities have developed the most opposite and extraordinary structural features. In a similar way we can trace among the fossil genera of different families the gradual development of the actinal plastron from its very earliest appearance as a modification of the posterior interambulacral area of the actinal side, or the growth of the posterior beak into an anal snout, the successive changes of the anal groove, the formation of the actinal labium, or the development of the bourrelets and phyllodes from a simple circular actinostome, the gradual deepening of the slight anterior groove of some early Spatangoid to form the deeply sunken actinal groove. Equally well we can trace the modifications of the ambulacral system as it passes from the simple poriferous zones of the earlier Spatangoids to genera in which the petaliferous portion makes its appearance, and finally becomes the specialized structure of our recent Spatangoid genera, such as Schizaster, Moira, and the like. Finally we can trace, to a certain extent, the development of the fascioles on one side from genera like Hemiaster, in which the peripetalous fasciole is prominent, to genera like Brissopsis, Brissus, and the like, of the present day; on the other, perhaps, or in both combined, the formation of a lateral and anal fasciole from genera like Micraster in Spatangus and Agassizia. Thus we must, on the same theory of the independent modifications of special structural features, trace the many and complicated affinities which so constantly strike us in making comparative studies, and which render it impossible for us to express the manifold affinities we notice, without taking up separately each special structure. Any attempt to take up a combination of characters, or a system of combinations, is sure to lead us to indefinite problems far beyond our power to grasp.

In the oldest fossil Clypeastroids and Petalosticha, as well as in the Demosticha, we also find the potential expression of the greater number of the modifications subsequently carried out in genera of later date. The semipetaloid structure of some of the earlier genera of Spatangoids, the slight modifications of some of the plates of the actinal side near the actinostome, are the precursors, the one of the highly complicated petaloid ambulacra of the recent Spantangoids, the other of the actinal plastron, leading as it does also to the important differences subsequently developed in the anterior and posterior extremities of the test, as well as to the modifications which lead to the existence of a highly labiate actinostome. The appearance of a few miliaries near the actinostome constitutes the first rudimentary bourrelets.

Going back now to the Palæchinidæ, the earliest representatives of the Echini in paleozoic times, without any attempt to trace the descent of any special type from them, we may perhaps find some clew to the probable modifications of their principal structural features preparatory to their gradual disappearance. In the structure of the coronal plates, the specialization of the actinal and abactinal systems, the conditions of the ambulacral system, we must compare them to stages in the embryonic development of our recent Echini with which we find no analogues in the fossil Echini of the Lias and the subsequent formations. In order to make our parallelism, we must go back to a stage in the embryonic history of the young Echini in which the distinction to be made between the ambulacral and interambulacral systems is very indefinite, in which the apical system is, it is true, specialized, but in which the actinal system remains practically a part of the coronal system. But here the comparison ceases, and, although we can trace in the paleontological development of such types as Archæocidaris or Bothriocidaris modifications which would lead us without great difficulty, on the one side to the Cidaridæ, and on the other to the Echinothuriæ and Diadematidæ of the present day, we cannot fail to see most definite indications in some of the structural features of the Palæchinidæ of characteristics which we have been accustomed to associate with higher groups. The minute tuberculation, for instance, of the Clypeastroids and Spantangoids, already existing in the Melonitidæ, the genital ring, and anal system, are quite as much Echinid as Cidarid. The polyporous genera of the group represent to a certain extent the polypori of the regular Echini, and the lapping of the actinal plates of the Cidaridæ and of the coronal plates in some of the Diadematidæ, as well as the existence of such genera as Tetracidaris, of four interambulacral plates in Astropyga, and of a large number of ambulacral plates in some of the recent Echinometradæ, all these are Palæchinid characters which we can explain on the theory of the independent development of the structural features of which they are modifications. We should, hewever, remember, that the existence of a large number of coronal plates, especially interambulacral plates, in the Palæchinidæ, is a mere vegetative character, which they hold in common with all the Crinoids—a character which is reduced to a minimum among the Holothurians, and still persists in full force among the Pentacrini of the present day, as well as the Astrophytidæ and Echinidæ.

It would lead me too far to institute the same comparison between the embryonic stages of the different orders of Echinoderms and their earliest fossil representatives. We may, however, in a very general way, state that we know the earliest embryonic stages of the orders of Echinoderms of to-day, which, with the exception of the Blastoidea and Cystideans, are identical with the fossil orders, and that as far as we know they all begin at a stage where it would be impossible to distinguish a Sea-urchin from a Star-fish, or an Ophiuran, or a Crinoid, or an Holothurian—a stage in which the test, calyx, abactinal and ambulacral systems are reduced to a minimum. From this identical origin there is developed at the present day, in a comparatively short period of time, either a Star-fish, a Sea-urchin, or a Crinoid; and if we have been able successfully to compare, in the development of typical structures, the embryonic stages of the young Echini with their development in the fossil genera, we may fairly assume that the same process is applicable when instituting the comparison within the different limits of the orders, but with the same restrictions. That is, if we wish to form some idea of the probable course of transformations which the earliest Echinoderms have undergone to lead us to those of the present day, we are justified in seeking for our earliest representatives of the orders such Echinoderms as resemble the early stages of our embryos, and in following, for them as for the Echini, the modifications of typical structures. These we shall have every reason to expect to find repeated in the fossils of later periods, and, going back a step further, we may perhaps get an indefinite glimpse of that first Echinodermal stage which should combine the structural features common to all the earliest stages of our Echinoderm embryos.

And yet, among the fossil Echinoderms of the oldest periods, we have not as yet discovered this earliest type from which we would derive either the Star-fishes, Ophiurans, Sea-urchins, or Holothurians. With the exception of the latter, which we can leave out of the question at present, we find all the orders of Echinoderms appearing at the same time. But while this is the case, one of the groups attained in these earliest days a prominence which it gradually loses with the corresponding development of the Star-fishes, Ophiurans, and Sea-urchins, it has steadily declined in importance; it is a type of Crinoids, the Cystideans which culminated during Paleozoic times, and completely disappeared long before the present day. If we compare the early types of Cystideans to the typical embryonic Echinodermal type of the present day, we find they have a general resemblance, and that the Cystideans and Blastoids represent among the fossil Echinoderms the nearest approach we have yet discovered to this imaginary prototype of Echinoderms.

This may not seem a very satisfactory result to have attained. It certainly has been shown to be an impossibility to trace in the paleontological succession of the Echini anything like a sequence of genera. No direct filiation can be shown to exist, and yet the very existence of persistent types, not only among Echinoderms, but in every group of marine animals, genera which have continued to exist without interruption from the earliest epochs at which they occur to the present day, would prove conclusively that at any rate some groups among the marine animals of the present day are the direct descendants of those of the earliest geological periods. When we come to types which have not continued as long, but yet which have extended through two or three great periods, we must likewise accord to their latest representatives a direct descent from the older. The very fact that the ocean basins date back to the earliest geological periods, and have afforded to the marine animals the conditions most favorable to an unbroken continuity under slightly varying circumstances, probably accounts for the great range in time during which many genera of Echini have existed. If we examine the interlacing in the succession of the genera characteristic of later geological epochs, we find it an impossibility to deny their continuity from the time of the Lias to the present day. The Cidaris of the Lias and the Rhabdocidaris of the Jura are the ancestors of the Cidaris of to-day. The Saleniæ of the lower Chalk are those of the Saleniæ of to-day. Acrosalenia extends from the Lias to the lower Cretaceous, with a number of recent genera, which begin at the Eocene. The Pygaster of to-day dates back to the Lias; Echinocyamus and Fibularia commence with the Chalk. Pyrina extends from the lower Jura through the Eocene. The Echinobrissus of to-day dates back to the Jura. Holaster lived from the lower Chalk to the Miocene, and the Hemiaster of to-day cannot be distinguished from the Hemiaster of the lower Cretaceous.

Such descent we can trace, and trace as confidently as we trace a part of the population of North America of to-day as the descendants of some portion of the population of the beginning of this century. But we can go no further with confidence, and bold indeed would he be who would attempt even in a single State to trace the genealogy of the inhabitants from those of ten years before. We had better acknowledge our inability to go beyond a certain point; anything beyond the general parallelism I have attempted to trace, which in no way invalidates the other proposition, we must recognize as hopeless.

But in spite of the limits which have been assigned to this general parallelism, it still remains an all-essential factor in elucidating the history of paleontological development, and its importance has but recently been fully appreciated. For, while the fossil remains may give us a strong presumptive evidence of the gradual passage of one type to another, we can only imagine this modification to take place by a process similar to that which brings about the modifications due to different stages of growth,—the former taking place in what may practically be considered as infinite time when compared to the short life history which has given us as it were a *résumé* of the paleontological development. We may well pause to reflect that in the two modes of development we find the same periods of rapid modifications occurring at certain stages of growth or of historic development, repeating in a different direction the same phases. Does it then pass the limits of analogy to assume that the changes we see taking place under our own eyes in a comparatively short space of time,—changes which extend from stages representing perhaps the original type of the group to their most complicated structures,—may, perhaps, in the larger field of paleontological development, not have required the infinite time we are in the habit of asking for them?

Paleontologists have not been slow in following out the suggestive track, and those who have been anatomists and embryologists besides have not only entered into most interesting speculations regarding the origin of certain groups, but they have carried on the process still further, and have given us genealogical trees where we may, in the twigs and branches and main limbs and trunk, trace the complete filiation of a group as we know it to-day, and as it must theoretically have existed at various times to its very beginning. While we cannot but admire the boldness and ingenuity of these speculations upon genetic connection so recklessly launched during the last fifteen years, we find that with but few exceptions there is little to recommend in reconstructions which shoot so wide of the facts as far as they are known, and seem so readily to ignore them. The moment we leave out of sight the actual succession of the fossils and the ascertainable facts of post-embryonic development, to reconstruct our genealogy, we are building in the air. Ordinarily, the twigs of any genealogical tree have only a semblance of truth; they lead us to branchlets having but a slight trace of probability, to branches where the imagination plays an important part, to main limbs where it is finally allowed full play, in order to solve with the trunk, to the satisfaction of the writer at least, the riddle of the origin of the group. It seems hardly credible that a school which boasts for its very creed a belief in nothing which is not warranted by common sense should descend to such trifling.

The time for genealogical trees is passed; its futility can, perhaps, best be shown by a simple calculation, which will point out at a glance what these scientific arboriculturists are attempting. Let us take, for instance, the ten most characteristic features of Echini. The number of possible combinations which can be produced from them is so great that it would take no less than twenty years, at the rate of one new combination a minute for ten hours a day, to pass them in review. Remembering now that each one of these points of structure is itself undergoing constant modifications, we may get some idea of the nature of the problem we are attempting to solve, when seeking to trace the genealogy as understood by the makers of genealogical trees. On the other hand, in spite of the millions of possible combinations which these ten characters may assume when affecting not simply a single combination, but all the combinations which might arise from their extending over several hundred species, we yet find that the combinations which actually exist—those which leave their traces as fossils—fall immensely short of the possible number. We have, as I have stated, not more than twenty-three hundred species actually representing for the Echini the results of these endless combinations. Is it astonishing, therefore, that we should fail to discover the sequence of the genera, even if the genera, as is so often the case, represent, as it were, fixed embryonic stages of some Sea-urchin of the present day? In fact, does not the very history of the fossils themselves show that we cannot expect this? Each fossil species, during its development, must have passed through stages analagous to those gone through by the Echini of the present day. Each one of these stages at every moment represents one of the possible combinations, and those which are actually preserved correspond only to the particular period and the special combination which any Sea-urchin has reached. These stages are the true missing links, which we can no more expect to find preserved than we can expect to find a record of the actual embryonic development of the species of the present day without direct observation at the time. The actual number of species in any one group must always fall far short of the possible number, and for this reason it is out of the question for us to attempt the solution of the problem of derivation, or to hope for any solution beyond one within the most indefinite limits of correctness. If, when we take one of the most limited of the groups of the animal kingdom, we find ourselves engaged in a hopeless task, what must be the prospect should we attack the problem of other classes or groups of the animal kingdom, where the species run into the thousands, while they number only tens in the case we have attempted to follow out? Shall we say "ignorabimus," or "impavidi progrediamus" and valiantly chase a phantom we can never hope to seize?

CHEMISTRY AS AN ART, AND CHEMISTRY AS A SCIENCE.

By Professor J. M. Ordway.

Professor J. M. Ordway, of Boston, spoke of "Chemistry as an Art, and Chemistry as a Science," comparing both, and pointing out some recent lines of advancement. The past year, he said, has been one of laborious activity in chemistry, but it has not been marked by any epoch-making discoveries. Meyer's recent apparent resolution of the chlorine molecule has not, indeed, been verified by the carefully devised experiments of Crafts, but the latter does seemingly confirm the change of iodine by intense heat. The years 1879 and 1880 will rank hereafter as years in which Meyer found means to throw new light on the nature of the haloids. Twenty-four years ago Perkins sought for artificial quinine, and found instead a better than royal purple. Then, by various hands and in rapid succession, red and yellow and black and brown and blue dyes were brought out from what proved to be something more than aniline. Now the novelty is past, and the announcement of a new dye hardly creates a ripple of excitement. The twelve-year-old synthesis of alizarine has given us colors purer, brighter, faster and cheaper than those of the obsolescent madder. Of late, wool has been provided for, and the extinction of cochineal plantations is threatened by reds of surpassing brilliancy, durability and ease of application. Baeyer has recently effected the synthesis of indigo, and tropical indigo fields may in time share the fate of the madder farms of France and Turkey. But indigo itself will not continue to satisfy our demand. We have become accustomed to hues of a delicacy and richness that no one dared to dream of twenty-five years ago. The æsthetic taste of this generation has been too much pampered; and dyers will soon call for something uniting the brilliancy of the aniline blues with the fixedness of indigo, and its adaptedness to wool and cotton. And Germany which has done the most in studying out these extraordinary colored compounds, now furnished the most of the industrial fruits of seemingly unpractical researches. Investigation costs, investigation pays; in more senses than one our science "opens wide her everduring gates on golden hinges turning."

The passing years are bringing to light new elementary bodies, and new metals are becoming like new asteroids, of too little mass to influence the orbits of other planets, and too much out of sight to interest many. Within five years fourteen new metals have called for recognition; and in 1879 alone chemists have claimed the discovery of six. Of new alloys, manganesian copper is worthy of regard, since it may in a measure play the part for copper that *spiegeleisen* does for steel.

In 1620 Bacon published the second part of his "Novum Organum," wherein he pointed out the way to appeal to nature by experiment, instead of deriving all science from the teaching of the ancients. But his methods had little immediate influence on the science of the time. He relied on induction; and induction alone simply strings together dry bones. That perception of general principles which makes science comes not altogether from the mere collation of facts. We need something more than eyes to see.

The great chemist of two hundred and fifty years ago was Van Helmont. To him we owe the word gas, which he derived not from geist, but from chaos, as representing the original form of matter. When our forefathers were laying the foundations of this nation alchemy was in its dotage, and chemistry took its rise in a dim knowledge of the gases. The evolution of chemistry as a science was threefold. First, the study of the gases, then the study of heat, then the study of combining weights. Consider how much of what we now know depends on the gases that Cavendish, Black, Scheele and Priestley revealed. The study of combustion, respiration, vegetable growth, organic decay, geological transformation and hygiene involves the study of carbon dioxide. Carbon monoxide reduces the metals, and plays a part in the Bessemer process for making steel. The fuel of the future is to be coal resolved into a chaos of carbonic oxide and hydrogen. At the end of the last century Murdoch found a use for coal gas, and in its train came a host of secondary products having a marvellous effect on science and industry. A test came into chemistry when Beecher attempted to explain combustion. Vulcan of old made as good iron as the blacksmith requires to-day. As for quantity, Vulcan with all his Cyclops and the fires of Ætna could not produce as much in six days as the Cambria iron works turn out in six minutes. Glauber, with all his good sense, taught that the rays of the sun and stars shoot themselves into the earth, and finally became silver and gold. Perhaps he was a prophet, speaking in symbols which he understood not. Now we know that metallurgy does depend on the sun's rays. The sunshine of the carboniferous period has been materialized into coal beds, and now attains perfection in a metal of more real value than gold. In the chemical study of heat, Berthelot's recent work shows culminating progress, and is worthy of him who years ago almost created organic synthesis. After a review of some of the most abstruse speculations in theoretical and physical chemistry, Professor Ordway went on to discuss the importance of biological chemistry. This branch is yet in its infancy, and has few to tenderly care for it. Most chemists prefer to take easier subjects, but the interest in it is increasing. The field is large and there is room for many laborers. Proximate organic analysis still remains undeveloped, and the world does not comprehend the light that we already have. In fermentation, putrefaction, vitrification and zymotic diseases, life may intervene; but how much do we yet know as to what is cause and what is merely concomitant? It is pertinent to ask whether chemistry tends, as many think all physical science tends, to materialism? I believe no true science tends that way; it is the lack of liberal cultivation that leads to such dimness of vision. Materialism is no more prevalent now than among the Athenians, who had no physical science. We hear much of the culture of that people, as if æsthetics were the only science and floriculture the only culture. There is much in the training of the chemist to foster a wholesome skepticism and just intolerance; intolerance of human pride and skepticism of airy theories. In chemical practice the constant appeal to sensible tests and the precision of the balance checks reliance on hasty assumptions. The chemist soon learns that exact truthfulness in others and rigid honesty in himself lie at the very foundation of science and real knowledge; and he looks on laxity in experiment or statement as the unpardonable sin. No other subject is so well calculated to impress one with the idea that theories are but the changeable dress of science. We all wonder what will become of the atomic theory itself when its centennial comes round twenty-seven years hence.

SPECTROSCOPIC NOTES ON OBSERVATIONS—CHIEFLY SOLAR—1879-80.

By Prof. C. A. Young, of Princeton, N. J.

(*a*) The magnesium lines of the *b* group and the sodium lines have been seen several times (first on June 5, 1880) *doubly*-reversed in the chromosphere spectrum—*i. e.* a bright line appeared as usual in the centre of the broad dark shade, and then this bright line widened and a thin dark line appeared in its centre. The phenomenon seems to be the exact correlative of the double-reversal of the bright sodium lines observable in the flame of a Bunsen burner under certain circumstances.

(*b*) I have recently been able to repeat the observations on the H lines first made at Sherman in 1872. In the spectrum of the chromosphere I find both H_1, and H_2, (or K, as some call H_2) to be *always* reversed; and what is more, H_1 is *double*, the principal line, which is in the centre of the dark shade, being accompanied by another of about half the strength, one division of Angstrom's scale lower—*i. e.* less refrangible. Since last March I have always been able to observe the two H's whenever I could see h, and H_1 *invariably* double.

In the neighborhood of sun spots however, though both H and K are usually reversed on the solar disc, H_1 is *not* double; its attendant line therefore belongs strictly to the spectrum of the chromosphere, and seems to be identical with No. 271 of my catalogue of chromosphere lines, though its wave length is about 3969 instead of 3970.

The observations were made with grating of 17,280 lines to inch; collimator and telescope 12-inch focus.

(*c*) A high dispersion spectoscope has been constructed by combining the above-mentioned grating, having nearly four square inches of ruled surface, with collimator and telescope of 3 inches aperture and 42 inches focus, the magnifying power employed varying from 50 to 200. The apparatus is strapped to the tube of the equatorial, and thus kept directed to the sun, an image of which is formed on the slit by an anachromatic lens of 3 inches aperture.

The performance of the grating is admirable when perfectly flat—a force of ¼ oz. applied at one corner is however sufficient to distort the plate (of speculum metal) ⅜ inch thick by about 3½ inches square, to an extent which seriously impairs the definition; it is sensitive to such distortions to a degree entirely unexpected. This instrument doubles an enormous number of the Fraunhofer lines. Out of 47 lines between C and G marked by Thalen as common to the spectra of two or more bodies, 38 are double or triple, 3 are doubtful (from difficulty of identification), and 6 only are single so far as the instrument can show,

(*d*) Distortion of solar prominences by a diffraction spectroscope. Generally, in such an instrument, the forms seen through the opened slit are either disproportionately extended or compressed along the line of dispersion. If the angle between the normal to the grating and the view-telescope is *less* than that between the normal and the collimator, there will be compression or flattening, and *vice versa.* The mathematical investigation is very simple—

Let n be the order of the spectrum observed.

Let λ be the order of the wave length of the ray.

Let S be the distance between adjacent lines of grating.

Let τ be the angle between normal to grating and telescope.

Let κ be the angle between normal to grating and collimator, and finally $a=\tau+\kappa=$angle between telescope and collimator, which is supposed constant. Then from the fundamental conditions of spectrum formation $n\lambda=S$ (Sin. τ—Sin. κ) or Sin. $\tau=\frac{n\lambda}{S}+$Sin. κ, whence $d\tau=\frac{\text{Cos. }\kappa}{\text{Cos. }\tau}\,d\kappa$, or (Cos. $a+$Sin. a Tan. τ) $d\kappa$, whence, in general, $d\tau$ will not equal $d\kappa$.

Special cases—

1. If $\kappa=\tau$, there is no distortion—but also no dispersion; it is the case of simple reflection.

2. If $\kappa=0$, grating being kept normal to the collimator, then $\xi=$Sec. $a\ d\kappa$.

3. If $\tau=0$, grating being kept normal to the telescope and moving with it, then $d\tau=$Cos. $a\ d\kappa$.

4. If $a=90°$ $d\tau=$Tan. $\tau\ d\kappa$.

5. If $a=0$, $d\tau=d\kappa$ and there is no distortion. This is possible only by using the same tube both for collimator and view-telescope, the grating being slightly inclined. The principal difficulty with this form of instrument lies in the reflections from the surface of the object glass, which, it is hoped, may be avoided by a special construction of the lens. An instrument on this plan is in process of construction by the Clarks, for the Physical Laboratory at Princeton, and nearly completed.

ON THE THERMO-ELECTRIC ELECTRO-MOTIVE POWER OF FE. AND PT. IN VACUO.

By Prof. C. A. Young, of Princeton, N. J.

Eisner, a few months ago, published a paper asserting that the thermo-electric power of Antimony and Bismuth is destroyed by removing them from all contact with oxygen, and inserting them in an atmosphere of pure nitrogen. From this he argues that the thermo-electric force in general is due to the contact of the gases which bathe the metals. The following experiment was tried to test the theory.

By the kindness of Mr. Edison and Mr. Upton a vacuum tube was prepared in Mr. Edison's laboratory, containing an iron wire, about 2 inches long, firmly joined to two platinum terminals which passed through the walls of the tube; the tube was exhausted until a 2-inch induction coil spark would not pass ⅛ of an inch in the gauge-tube, indicating a residual atmosphere of about one-millionth. The wire was heated too in candescence during the exhaustion, in order to drive off any possible occluded gases. The platinum wires outside the tube were joined to iron wires, the joinings being covered by glass tubes slipped over them, and a sensitive reflecting galvanometer was included in the circuit. By laying the tube and connected joinings in the sunshine, and alternately shading one or several of the joinings, it was found that the electro motive power of the joinings within the tube was precisely the same as that of those without, and the development of current just as rapid. There was no trace of any modification due to the exhaustion.

ON THE ABSOLUTE INVISIBILITY OF ATOMS AND MOLECULES.

By Prof. A. E. Dolbear.

Maxwell gives the diameter of an atom of hydrogen to be such that two millions of them in a row would measure a millimeter, but under ordinary physical conditions most atoms are combined with other atoms to form molecules, and such combinations are of all degrees of complexity; thus a molecule of water contains three atoms, a molecule of alum about one hundred, while a molecule of albumen, according to Mulder, contains nine hundred atoms, and there is no reason to suppose albumen to be the most complex of all molecular compouuds. When atoms are thus combined it is fair to assume that they are arranged in the three dimensions of space, and that the diameter of the molecule will be approximately as the cube root of the number of atoms it contains, so that a molecule of alum will be equal to

$$(\sqrt[3]{100} = 4.64)\ \frac{4.64}{2000000} = \frac{1}{431000}\text{ mm.}$$

and a molecule containing a thousand atoms will have a diameter of $\frac{10}{2000000}=\frac{1}{200000}$ mm. Now a good microscope, will enable a skilled observer to identify an object so small as the $\frac{1}{4000}$ mm. Beale in his works on the microscope pictures some fungi as minute as that, and Nobert's test bands and the markings upon the *Amphiplura pelucida*, which are of about the same degree of fineness, are easily resolved by good lenses. If thus the efficiency of the microscope could be increased fifty times ($\frac{200000}{4000}=50$) it would be sufficient to enable one to see a molecule of albumen, or if its power could be increased one hundred and seven times it would enable one to see a molecule of alum.

Now Helmholtz has pointed out the probability that interference will limit the visibility of small objects; but suppose that there should be no difficulty from that source, there are two other conditions which will absolutely prevent us from ever seeing the molecule.

1st. Their motions. A free gaseous molecule of hydrogen at the temperature of O°C., and a pressure of 760 mm. mercury, has a free path about $\frac{1}{10000}$ mm. in length, its velocity in this free path being 1860 m. per second or more than a mile, while its direction of movement is changed millions of times per second. Inasmuch as only a glimpse of an object moving no faster than one millimeter per second can be had, for the movements are magnified as well as the object itself, it will be at once seen that a free gaseous molecule can never be seen, not even glimpsed. But suppose such a molecule could be caught and held in the field so it should have no free path. It still has a vibratory motion which constitutes its temperature. The vibratory movement is measured by the number of undulations it sets up in the ether per second, and will average five thousand millions of millions, a motion which would make the space occupied by the molecule visibly transparent, that is it could not be seen. This is true for liquids and solids. Mr. D. N. Hodges finds the path of a molecule of water at its surface to be .0000024 mm., and though it is still much less in a solid it must still be much too great for observation.

2d. They are transparent. The rays of the sun stream through the atmosphere, and the latter is not perceptibly heated by them as it would be if absorption took place in it. The air is heated by conduction, contact with the earth, which has absorbed and transformed the energy of the rays. When selective absorption takes place the number of rays absorbed is small when compared with the whole number presented, so that practically the separate molecules would be too transparent to be seen, though their magnitude and motions were not absolute hindrances.

ON THE AURORA AND ZODIACAL LIGHT OF MAY 2, 1877.

By Henry C. Lewis.

A simultaneous appearance of an aurora and the zodiacal light appeared on this evening, and a comparison between them is here given. The various changes of the aurora are given in detail. A remarkable feature was the formation of a bright streamer which maintained its position relative to the earth for nearly an hour. Meanwhile, the Zodiacal Cone, which was bright early in the evening had moved past the streamer and passed below the horizon. The streamer had remained, like the great pointer, fixed to the earth, and marking its motion, while the heavens revolved past it. This fact was conclusive evidence of the terrestrial character of the aurora and of the cosmical character of the zodiacal light. Another fact leading to the same conclusion was the character of their spectra. That of the zodiacal light was continuous, and that of the aurora was a line-spectrum—the former is such as would be given by sunlight reflected from matter in space; the latter would be given by an electric discharge through a gas.

OBSERVATIONS ON BRACHIOPODS.

By Prof. Edw. S. Morse.

Mr. Morse gave the anatomical details of some Brachiopods he had studied in Japan, and described the existence of a curious parasitic worm in a large species of Lingula. He also gave further facts regarding the so-called hearts of certain brachiopods, and expressed his belief that they were glands of some kind connected with the reproductive organs.

THE KAMES OR ESKARS OF MAINE.

By Geo. H. Stone, Kent's Hill, Me.

This paper is accompanied by a map showing the courses of the larger Kame-systems of Maine. Omitting short, isolated ridges of gravel, the map shows thirty distinct systems of Kame gravels, varying from five to one hundred and fifty miles in length. The total length of Kames and Kame-plains thus far mapped is about 2000 miles. The map is the result of amateur explorations made at intervals during the past four years.

The paper discusses the following points regarding the Kames:

1. *Kame drift compared with glacial drift.*

The facts show that Kame material has in general been transported farther than the morainal material which was originally derived from the same locality.

2. *The Kame streams.*

The Kames were deposited by currents flowing lengthwise of their courses, and in all but four undecided cases the currents flowed southwards. The Kame streams resembled sub-ærial rivers in their meanderings, their branches, and in all other respects. All the long systems in the State are much higher at their northern than at their southern ends. The water of these rivers is shown to have flowed faster on long down slopes than on up slopes. There is strong reason to believe that most of the water of the melting glacier escaped by superficial channels, unless near the terminal moraine. Except near the coast there are in Maine almost no signs of sub-glacial streams.

3. *The external forms of Kames.*

1. The single ridge. 2. Reticulated plains, composed of a series of reticulated ridges with enclosed funnels or lakelets. 3. The solid or continuous plains, which are broad, flat-topped ridges, showing few or no signs of separate ridges, and often of great height.

4. *The internal structure of Kames.*

Kames are of two kinds—1. The stratified Kame, which is the more common type. 2. The pell-mell Kame.

The same Kame may be stratified in one part of its course and pell-mell in another.

5. *Action of the sea upon the Kames.*

During the Champlain period the sea stood at a height, in the central parts of Maine, about 300 or 350 feet above the present sea level. The Kames are plainly overlain by the marine clays, and the sea greatly modified their form. The difference between the Kame that has been under the sea and that which has not is often very great, and conclusively proves that the Kames proper cannot have been a marine deposit.

6. *Topographical relations of the Kames.*

No general law of relationship between the Kames and the relief forms of the land can be derived from local observations, for there are many purely local relationships. The only invariable rule thus far established is that the Kames never cross hills more than about 200 feet higher than the country lying to the northward. Maine is traversed by numerous ranges of hills trending eastward or northeastward, and the Kame systems never cross the high ranges except by low passes. Low hills they cross freely. The inference of the writer is that the Kames were deposited when the glacier was so far melted that the higher hills rose above the ice surface, and hence the only escape for the waters southward was by the low passes.

7. *Distribution of the Kames.*

A line joining the northern extremities of the Kames is nearly a straight line; it trends nearly northeast and is roughly parallel with the coast. North and west of this line there are occasional short ridges of Kame origin, but no long systems have yet been discovered.

(The publication of papers read before the recent meetings of the American Association for the Advancement of Science will be continued in our next number.—Ed.)

MOUNTAIN SITES FOR ASTRONOMICAL OBSERVATIONS.*

By Professor O. Stone.

The question of employing mountain sites for Astronomical Observations is one of considerable importance, and two papers, recently published, add materially to the meagre literature on this subject. The first of these is a report of a series of observations on Mt. Ætna made by Professor Langley during a visit to Europe in the winter of 1878.

The lower portion of Mt. Ætna is described as densely inhabited and covered with rich vegetation. At an elevation of about 2000 feet, however, this suddenly ceases, and above this, with the exception of a few uninhabited plantations of chestnut trees which extend to an altitude of 4000 or 5000 feet, there exist only wastes of lava. At an elevation of some 4200 feet a station was established in a hut built of lava, known as "Casa del Bosco."

Professor Langley remained upon the mountain from Christmas until January 14. His instruments consisted of a 3¼ inch telescope loaned by the superintendent of the U. S. Naval Observatory and a spectroscope provided with a Rutherford speculum metal grating of 17,296 lines to the inch, and with collimating and observing telescopes of 1.1 inch aperture and 14 inches focal length.

On clear nights, at ordinary elevations, Professor Langley does not recognize steadily more than six stars in the Pleiades, and on an ordinary clear night at Allegheny he cannot steadily see the companion of Polaris with less than two inches aperture. On Mt. Ætna, however, he could see steadily, notwithstanding the moonlight, nine stars of the Pleiades, with the naked eye, the companion of Polaris, with an aperture of 1.6 inch and 11m. companions of β Leporis and α Tauri, and ι and σ Orionis with 3¼ inch. From this he concludes that stars of about two thirds the brightness of those visible in England under like telescopic power can be seen on Ætna at the elevation of Casa del Bosco. As far as transparency was concerned, a noticable advantage over stations at a lower altitude was also shown by observations of the sun.

The results in regard to steadiness of definition were not so satisfactory. Although there was probably less tremor in the stars as seen from Mt. Ætna than would have been the case at a lower station, the difference was not great.

The other paper referred to is the report of Mr. Burnham to the Trustees of the "James Lick Trust" of Observations made on Mt. Hamilton with reference to the location of the Lick Observatory.

Mt. Hamilton seems to have been first suggested as a site for the observatory by Professor Holden in 1874, and afterwards approved by Professor Newcomb. The elevation of the summit is 4250 feet, or only a little greater than that of Professor Langley's station at Casa del Bosco. On this summit Mr. Burnham erected a temporary observatory in which was mounted the magnificent six inch Clark refractor, with which nearly all his double star discoveries had been made. It was equatorially mounted with circles and driving clock. The eye-pieces gave powers up to 400. In addition, a set of meteorological instruments were employed.

Mr. Burnham remained upon Mt. Hamilton just 60 days, from August 17 to October 16, with the exception of three days, September 21–23, spent in San Francisco. As the seeing was first class for the 14 nights immediately preceeding the 3 days he was absent, it is fair to presume the same conditions continued. During the whole time only 11 nights were cloudy or foggy, and of the remaining nights there were 42 when the seeing was *first class* and 7 when it] was medium, and ***no poor*** nights when the sky was clear. Besides obtaining remarkable results in the examination of delicate test objects, a search was made for new doubles, and at the close of the report Mr. Burnham gives a catelogue with observations of 42 such objects, 10 of which have a distance of less than 1″. A great many objects were examined by daylight but the air, during the greater part of the day at least, appeared to be no steadier than would be ordinarily found elsewhere.

In conclusion, Mr. Burnham says: "So far as one may judge from the time during which these observations were made, there can be no doubt that Mt. Hamilton offers advantages superior to those found at any point where a permenent observatory has been established. * * * * * * The ease with which close pairs can be seen, almost down to the horizon, will be apparent from the southern declination of many of the new double stars. * * * * Close pairs can be observed at least down to 42° south declination. * * * * *

"What has been said about the advantages of Mt. Hamilton for astronomical purposes, is of course, based upon what was seen during the time spent on the mountain. This was my first visit to the Pacific coast, and hence I have no personal knowledge concerning other seasons of the year. From inquiries in various quarters I am satisfied there was nothing about this season unusual, and there seems to be every reason for supposing, as the same cloudless sky and dry air prevails from about March until the commencement of the rainy season, near the close of the year, that the whole of this interval would be equally favorable for the use of the telescope.'

One of the most remarkable and interesting conditions observed was the dryness of the atmosphere. The average difference between the wet and dry bulb thermometers was 18°.4 during the first five weeks of Mr. Burnham's residence on Mt. Hamilton and every night was first class when this difference reached 15° and upwards.

Notwithstanding, however, the advantages of a mountain site for an observatory, there are many drawbacks. Even the loneliness of the situation is a disadvantage to the greatest activity. Taking everything into consideration, therefore, probably as favorable a location as any for the next great American observatory is to be found on the plateaus of Colorado.

*Observations on Mount Etna, by S. P. Langley. From the *American Journal of Science*, Vol. XIX, July, 1880.

Report to the Trustees of the "James Lick Trust," of Observations made on Mt. Hamilton with reference to the location of Lick Observatory; by S. W. Burnham. Chicago, 1880.

We have received the following publications from the U. S. Department of the Interior (Bureau of Education).

The Indian School at Carlisle Barracks which acquaints educators and school officials with the interesting experiments of training Indian children in the knowledge and usages of civilized life, in progress during the past eight months at Carlisle Barracks.

Vacation Colonies for Sickly School Children This subject has for some time received the attention of this department, and as early as 1872 papers by that distinguished and benevolent physician, J. M. Toner, M. D., of Washington, were published, advocating free camping grounds or parks, where poor children and their parents could lodge during the excessive heat of summer.

Progress of Western Education in China and Siam. This is an interesting account of the progress of Western ideas and educational methods in China and Siam, forwarded to the department by the United States Minister at Peking, and the United States Consul at Bangkok.

Legal Rights of Children. This is an elaborate report covering nearly a hundred pages, and treats of the rights of children in the various States of the Union, including education, and also a comparative view of the systems of education in the different States established to give force and effect to those rights, and thus assure the welfare of the individual and the State.

OFFICE OF "SCIENCE,"
229 BROADWAY, NEW YORK.

NOTICE.

WE are preparing a list of Professional Scientists, and of all those engaged in scientific investigations in the United States We ask the co-operation of our subscribers and the heads of Universities and Colleges, by forwarding to us lists of Scientific men connected with such institutions.

We invite all engaged in original research, whether professionals or amateurs, to forward their names and addresses for registration, giving their names and style in full, and the particular line of research followed.

We desire that the list of Physicists shall be very complete as we understand that such a list does not exist, even at Washington.

We guarantee to forward all names and addresses received by us to the Smithsonian Institute, Washington, as we understand from the Secretary that such a course would be acceptable.

Compliance with this request will result in many conveniences to those concerned, as when the lists are analytically arranged, those desirous of communicating with their fellow workers will have facilities for so doing, and papers and notices can be distributed without delay. Scientific men, at present dispersed over a wide territory, will thus be united, so as to make co-operation possible when occasion requires.

Address,

THE EDITOR OF "SCIENCE,"
BOX 3838, P. O., NEW YORK.

SCIENCE:

A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.

Vol. 1, No. 10. . . . . September 4, 1880.

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SCIENCE

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

ILLUSTRATED.

Entered in the Office of the Librarian of Congress, at Washington, D. C.

Vol. I—No. 13. September 25, 1880. Price 10 Cents.

SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

PUBLISHED AT
229 BROADWAY, NEW YORK.
P. O. Box 3838.

SATURDAY, SEPTEMBER 25, 1880.

AN article in the *North American Review*, over the signature of Edison, confirms our editorial remarks, made on the 10th of July last, respecting the true condition of his system of electric illumination.

The course of Edison has been consistent, and from first to last he has emphatically stated that the results arrived at last January practically demonstrated the success of his system for the ends in view, and that nothing remained to be done but to improve his lamp and generator, to bring both to as near a state of perfection as a long series of exhaustive experiments would permit.

Of course, Edison has also had to master the enormous mass of details incident to the practical working of his electric lamp on a large scale for general use, and that he has accomplished both tasks within a year must be a matter of astonishment to all who have any conception of the work done; but Edison seems born to overcome difficulties that appall other men, and the fertility of his mental resources appears unbounded.

In the discussion of scientific questions affecting vested interests, impartial treatment and justice to the innovator are lost sight of. Better things, however, might have been expected from some of those who have misled the public in regard to this matter. Under the belief that Edison's electric lamp was a failure, thousands of dollars have been lost by those who have invested their money in electric light companies which have tried to force systems of lighting, fundamentally wrong in principle, and ridiculously unfit for general illuminating purposes.

There is one fact which places the sincerity of Edison above reproach; he has left the merit of his system of electrical illumination to assert its own supremacy with the public, and has neither paraded his light in great cities, nor gone on a lecturing tour, as other eminent inventors have done; and lastly, he has spent thousands upon thousands of dollars in perfecting his system.

On his system of electric illumination Edison has staked his time, money, and reputation. He now states that he has succeeded. Let those who are wise accept the situation.

—We see by a notice in a recent number of the *Veterinary Gazette* that a French palæontologist has discovered the osseous remains of an extinct species of horse at one of the "palæolithic stations" in his country. The species resembled our recent horse more closely than any other fossil species, but the remarkable feature was noted that the so-called "splint bones" (the lateral metacarpals) are separate and distinct from the great metacarpal or "canon bone," while in the modern horse these are co-ossified for the greater part of the length of the former. It thus constitutes a connecting link between the Hipparion and Equis genera. The number of fossil remnants discovered indicated that over a hundred thousand animals had perished in that locality, and the explanation given for this accumulation is that a large herd of animals, seized with that panic that horse-herds are liable to, rushed over a precipice and were thus killed *en masse*. A fuller account is promised in *Kosmos*, the journal from which the notice is taken, and we will refer to it in due time more fully.

There appears to be an uneasy feeling in certain English scientific circles; the complaint is openly made that the recognition of science (when compared with that received from society by the liberal arts) is inadequate, and calls for an immediate remedy. Contributions, to be levied from the State, and distinctions to be conferred by Government or the Crown, are suggested, and one writer proposes that new life peerages should be conferred on eminent scientific men, the titles being endowed with the salary of a junior lord, which, we believe is about five or ten thousand dollars a year; the selection in some cases to be made from the holders of certain offices, such as the Master of the Mint, the Astronomer Royal, or the Presidents of the Royal Society and British Association.

THE AUGUST PERSEIDS, 1880.

By EDWIN F. SAWYER.

The annual display of August meteors occurring during the first half of the month, with a strong maximum on the 9th and 10th, has been watched for this year with the usual attention of meteor observers, and a successful series of observations have been obtained.

Although little important information has been added to our present knowledge of this well-known meteor stream, yet its fluctuating intensity from year to year is an important element to record.

The results of the observations so far as heard from indicate that the display as observed this year exceeded but slightly in intensity the shower as recorded last year, when, instead of a maximum display as anticipated being observed, the shower proved to be a very meagre one, in fact, representing a minimum phase of its return. Thus the existence of an eight-year period for this shower, as suspected and pointed out by Dr. Phipson,* appears to lack confirmation.

* See his work entitled "Meteors, Aerolites and Falling Stars," page 159.

The shower was observed by the writer this year on the evenings of the 8th and 9th (the sky being unfortunately overcast on the 10th and 11th), and the display found to be a feeble one.

A three-hours' watch on the 8th, from 9 to 12h., revealed 37 meteors. Of these, 16 were Perseids, giving as an hourly rate of Perseids (allowing for time spent in registering such tracks as were well observed) 6, and for all meteors 14.

The following table shows the number of meteors recorded each hour on the 8th during the watch, and also the calculated horary number for one observer looking towards the East:

Duration of Watch.		Length of Watch.	No. of Meteors seen.	Perseids.	Calculated Horary No.		State of Sky.
From	To				All Meteors.	Perseids.	
h. m.	h. m.	h.					
9 0	10 0	1	9	6	10	7	Very
10 0	11 0	1	11	3	13	4	Clear.
11 0	12 0	1	17	7	20	8	
9	12h.	3h.	37	16	14	6	

The magnitude of those recorded were as follows:

	=♃	> 1Mag.*	=1Mag.	=2Mag.	=3Mag.	=4 and fainter.		Total.
Perseids....	1	1	3	6	2	3	=	16
Others.....	0	0	2	4	6	9	=	21
Total.....	1	1	5	10	8	12	=	37

The radiant point of the Perseids was deduced as at R. A. 38°+56°. Two showers in Cepheus furnished the majority of the uncorformable meteors recorded, their deduced positions being at R. A. 5°+75° and R. A, 332°+60°. The evening of the 9th was generally clear (a few clouds at times but slightly interfering with the observations), and a watch of four hours, from 9 to 13h., was sustained, 91 meteors being recorded. Of these, 54 or 59.4 per cent. were Perseids, 12 or 13 per cent. Cassiopeids, and 25 or 27.3 per cent. belonged to feebler showers in Andromeda, etc.

The number recorded each half hour, and the calculated horary number, were as follows:

Duration of Watch.		Length of Watch.	No. of Meteors seen.	Perseids.	Cassiopeids.	Calculated Horary No.		State of Sky.
From	To					All Meteors.	Perseids.	
h. m.	h. m.	m.						
9 0	9 30	30	9	5	2	22	12	Clear.
9 30	10 0	"	10	8	0	24	19	"
10 0	10 30	"	10	6	3	25	14	FewCl'ds.
10 30	11 0	"	11	7	2	26	16	Clear.
11 0	11 30	"	14	10	2	35	23	FewCl'ds.
11 30	12 0	"	15	7	2	37	16	"
12 0	12 30	"	11	5	1	26	12	Clear.
12 30	13 0	"	11	6	0	26	14	"
9	13h.	4h.	91	54	12	28	16	

Meteors thus appeared thickest between 11 and 12h., when the hourly rate for all meteors was about 36, and of Perseids 20. The shower in Cassiopeia appears of considerable intensity, and probably the confounding of these meteors (Cassiopeids) with the true Perseids (the two radiants lying approximately near one another) may account for the large hourly rate of meteors being recorded as belonging to the Perseids by ordinary and occasional observers not discriminating enough, or who are not aware that two distinct showers exist in this region of the sky. The magnitude of those recorded on the 9th were as follows:

	=♃ or ♀	> 1Mag.*	=1Mag.	=2Mag.	=3Mag.	=4 and fainter.		Total.
Perseids....	4	5	8	13	9	15	=	54
Others.....	1	0	4	2	7	23	=	37
Total....	5	5	12	15	16	38	=	91

The radiant point of the main Perseid stream was very accurately deduced from several very short tracks near the focus, and from one perfectly stationary meteor of the 1st mag., visible two seconds and very exactly noted, as at R. A. 44¾°+56¼°. A secondary Perseid radiant was reduced from a few short tracks, and one very nearly stationary meteor, as at R. A. 55°+57°. Among the bright meteors recorded was one at 12h. 55m., which equalled ♀ (Venus) in brightness, and was of a blue color, with path from R. A. 260°+67½° to 212°+66°. This meteor came from the direction of Cygnus. A letter received from Mr. W. F. Denning, F. R. A. S., of Bristol, England, informs the writer that the shower was well observed in England. Mr. Denning at Bristol recorded from August 6 to 13, inclusive, 419 * during a period of 16½h. watching, and of these 240 were Perseids. He found the hourly rate of all meteors on the 9th to be 44, and of Perseids 28. On the 10th (when it was foggy) 34 and 28, respectively. The radiant point appeared to shift in R. A. (increasing) every night, for while on August 6 it was at R. A. 38°+56° and August 7–8 at R. A. 41°+55°, it was at R. A. 48°+57° on August 11–12, and at R. A. 49½°+57½° on August 13th. The meteors were also successfully observed at the Royal Observatory, Greenwich, where the greatest hourly number on the 10th was determined to be about 25, and also by Major Tupman, Mr. Corder, and other prominent observers.

CAMBRIDGEPORT, Sept. 12, 1880.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, 1880.

(Continuation of papers read.)

NOTES ON JAPANESE PULMONIFERA.

By PROF. EDW. S. MORSE.

In this communicetion Mr. Morse called attention to the occurrence of a number of species of land snails in Yeso, identical with forms occurring in New England.

He also showed the occurrence of two species of slugs in Japan, which are also common in New England.

While he had met with most of the fresh water genera of Pulmonifera in Japan, he had never yet found an example of *Physa*.

PROBLEMS IN WATSON'S CO-ORDINATES.

By THOMAS HILL, D.D., LL.D.

IN this paper Dr. Hill investigates the equation $p=A(a-b\,\mathrm{Sin.}\ m\nu)^n$; giving his principal attention to the case in which $b=m=1$, and $n=-1$, which represents a curve like a figure 8 with its top concave, somewhat like the sign for Taurus. When $a=0$, this becomes a parabola; and when $a>2$, an oval, not an ellipse; a new illustration, in Dr. Hill's opinion, of the fact that the ability of members of two groups of forms to assume an intermediate form affords but a very slight presumption, if any, for a community of origin in the group.

FRICTION OF LUBRICATING OILS.

By C. J. H. Woodbury.

The resistance existing between bodies of fixed matter moving with different velocities or directions presents itself in the form of a passive force, which results in the diminution or destruction of opponent motion. Modern science has demonstrated that this destruction is only apparent, being merely the conversion of the force of the moving body into the oscillation of the resisting obstacle or into that molecular vibration which is recognized as heat. Direct friction refers to the case where the two bodies are in actual contact and mediate friction where a film of lubricant is interposed between the surfaces, and it is this which applies to nearly every motion in mechanics where bodies slide upon each other. The coefficient of friction is the relation which the pressure upon moving surfaces bears to resistance. Mr. Woodbury limited his discussion to a description of the apparatus for measuring the friction of lubricating oils, the method of its use and the results obtained with a number of oils in the market which are used for lubricating spindles. Previous investigation of nine different oil-testing machines used showed that none of them could yield consistent duplicate results in furnishing the co-efficient of friction. The paper mentioned the circumstances which must be known or preserved constant,—temperature, velocity, pressure, area of frictional surfaces, thickness of the film of oil between the surfaces, and the mechanical effect of the friction. The radiation of heat generated by friction must be reduced to a minimum, and no oil should be allowed to escape till subjected to attrition. Therefore a dynamometer is required which is instantaneous and automatic in its action. Mr. Woodbury described in detail the construction of his instrument and the mode of its operation, which was too elaborate to be reproduced in an abstract. The operation of the machine under equal conditions with the same oil gives results which are as closely consistent with each other as could be expected from such physical measurements. Much of the slight irregularity was due to the variable speed of the engine. The results were remarkably uniform, but they do not agree with the laws of friction, as given in works on mechanics, but the co-efficient of friction varies in an inverse ratio with the pressure. Friction varies as the area, because the adhesiveness of the lubricant is proportional to the area, and the resistance due to this cause is a larger fraction of the total mechanical effect with light than with heavy pressures. The lubricant used is one of the most important factors in the cost of power. In the present condition of engineering science it is impossible to state what exact proportion of the power used by a mill is lost in sliding friction, but in a print-cloth mill only about 25 per cent. of the power is utilized in the actual processes of carding, spinning and weaving the fibre, not including the machinery engaged in the operation, leaving 75 per cent. of the power as absorbed by the rigidity of the belts, the resistance of the air and friction. Mr. Woodbury concludes that the successful operation of a spinning frame is far more closely dependent upon the individual management in respect to the conditions of band-tension, lubrication and temperature of the spinning room than all other causes combined. Not that some forms of spindle are not superior to others, but without wise supervision the most desirable forms of spindle must fail to show the merits due to the skill of their promoter. The lubricating qualities of an oil are inversely proportional to its viscosity; the endurance of a lubricant is, in some degree, proportional to its adhesion to the surfaces forming the journal. An ideal lubricant, in these respects, would be a fluid whose molecules had a minimum cohesion for each other, and a maximum adhesion for metallic surfaces. Viscous oils adhere more strongly to metal surfaces, hence it is obligatory to use such thick lubricants on heavy bearings. With light pressures more fluid oils are admissable, and in all cases the oils should be as limpid as possible. Oils with great endurance are likely to give great fractional resistance, and in the endeavor to save gallons of oil, many a manager has wasted tons of coal. The true solution of the problem of lubricating machinery is to ascertain the consumption of oil and the expenditure of power, both being measured by the same unit, namely, dollars. Mr. Woodbury detailed his experiments in measuring the fluidity of oils, and gave the data for determining the safety and efficiency of a lubricant.

THE LAW OF LAND-FORMING ON OUR GLOBE.

By Prof. Richard Owen, M.D., LL.D.*

The truth of a general law can best be proved by such a large collection of co-incident facts as to carry conviction to the scientist. But in a synopsis all that can be done is to state the law and suggest a few prominent demonstrations, leaving it for the reader to trace with compasses or string, on a good globe or large map of each separate continent, those phenomena presented, and such other analogous details as may suggest themselves.

General Law: *The land shows itself above the ocean level, in definite multiple proportions, by measurement; the unit is the angular difference between the axis of revolution and the axis of progression.*

For convenience, as that angle has been lessening for some centuries, we might call it $24°=\frac{360}{15}°$.

The greatest width and length of continents $=3\times24°=72°=\frac{360}{5}°$.

Consequently, the radius for continents$=36°=\frac{360}{10}°$.

The measure for oceanic distances is the complement of $24°=66°$.

The ratio of land to water is as 100 : 275.

The ratio of 24° to 66° :: 100 : 275.

All measurements are to be estimated at the equator.

The above general law may, for the purpose of demonstration, be subdivided.

I.—*First subdivision or section of the law.*—Many longitudinal elevations and depressions on the earth's surface, especially near the greatest median, north and south, extension of each continent, coincide with some meridian. Although this is partly due to early cooling and shrinkage, probably all continents have been extended north and south by successive depositions, as great river-deltas are usually found near the southern terminus of that median line. On these median lines we seldom find volcanoes.

Demonstration.—As the details regarding North America are most familiar, illustrations will be taken chiefly from that continent, although the law applies as well to all the others. In North America the greatest elongation is about in long. 96° W. of Gr. Near that line, as we shall see later, are found the foci of land forming for our continent, and not far distant the great rivers which drain the Mississippi valley. From Boothia Felix to the Gulf of Mexico we have no volcanoes, and the only earthquake action (near New Madrid, etc.) is due to a great circle of force crossing diagonally as shown subsequently.

II.—*Second Subdivision of the Law.*—Although the median lines of continents run north and south, the outlines or trends of continents form, with the meridians, angles of about 23½° (as I pointed out in "Key to the Geology of the

* Former State Geologist of Indiana, and for fifteen years Professor of Natural Science in the Indiana State University.

Globe," published in 1857) and thus constitute great circles from the Arctic to the Antartic circles, along which (perhaps from the earth's crust being thinner than in the middle of continents) important seismic phenomena, such as volcanic and earthquake action are frequent and abundant.

Demonstration.—Elevating the north pole of the globe 23½°, and bringing the straits of Macassar and of Bali* to the eastern horizon, we find the wooden horizon mark the general trend of Asia from the volcanoes of Java and Celebes, passing through the volcanic regions of Japan, Kuriles, and Kamchatka, and skirting the Japan warm stream. On the opposite side of the globe this great circle passes from Alaska to the basaltic region of Lake Superior, then through South Carolina and the Bahamas to the earthquake region of Caraccas, etc., explaining the convulsion in South Carolina of 1811–12, just before the destruction of La Guayras and Caraccas. Revolving the globe from west to east 72°, or 1-5th of 360°, we bring the coast of Africa to the horizon; 72° more will give the trend of South America, passing between Madeira proper and Porto Santo, where Lyell observed a *continental* difference, especially among the mollusks, as well as the seismic force adequate to elevate the British coast (Lyell says in glacial epoch) at least 600 feet, and Scandinavia, in historic times, at some points five feet per century, total as much as 700 feet (see Lyell's "Principles," vol. I., p. 133.) On the other side ef the globe it may have furnished the dynamics of some volcanoes in Japan and Solomon's Archipelago, as well as the earthquakes of New Zealand near Cook's Straits. The trend of North America, just 72° west of the above, passes from a volcanic region between Mexico and Central America, along between the Appalachians, which it raised, and our Atlantic seaboard, nearly parallel to the Gulf Stream, and up to the Geysers and volcanoes of Iceland, coming round by the Field of Fire (Baker) on the Caspian, and through the ancient volcanic trap of Hindostan, consequently is older than the South American trend. The last or fifth trend either separated Australia from New Zealand, or more probably brought the latter up recently, as in it we find quaternary formations, such as the gigantic Dinornis.

III.—*Third subdivision of the law.*—An addition to the dynamics of land-forming is found in there being for each northern continent two foci of consolidation, which may have resulted from shrinkage causing depression of adjoining seas or seismic elevation of the plastic crust. The northern focus, when two exist, is near the continental median line and arctic circle; the other occupies the geographical centre of the continent. Concentric circles around these foci not only mark important additions to the land and orography of each continent, but especially pass as they enlarge from the areas of older geological formations to those of newer.

Demonstration.—A radius of 24° from the geographical central focus often marks the outline of the continent proper, while that of 36° embraces usually some of the adjoining islands, leaving out perhaps some peninsulas. Between these two circles we find almost exclusively cenozoic formations (tertiary), and outside of 36° in the three southern continents quaternary. The details of North American geology must suffice in an abstract, designating for the other continents simply the position of the foci. The northern focus for North America is in Boothia Felix. With a radius of 24° from that point we reach the southern point of the V shaped area near Lake Superior, as laid down by Prof. Dana at p. 149 of his "Manual," where the archæan meets the paleozoic. A more extended radius passes through the coal of northern Iowa, of Michigan, New Brunswick, and Newfoundland. A radius of about 29°–30° gives us the mesozoic of Kansas and the Triassic (a red sandstone with bird tracks) of Connecticut and Massachusetts.

Removing our center to the west shore of Lake Superior, a radius of 11° to 12° gives us Silurian (Lower and Upper) from Niagara to near Springfield, O., Lexington and Frankfort, Ky., Nashville, Tenn., dominating at least the eastern half of the circle, while the west was still under water. A radius of 12° to 13° marks the Applachian and other coal fields from north of Harrisburg, southwest through Tuscaloosa, Ala., to Arkansas and Texas. A radius of 15° is Mesozoic, curving from the Cretaceous of Utah and Colorado through that of Arkansas and Tennessee to that of New Jersey. A radius of 24° outlines the continent from Cape Breton and Cape Sable to the Golden Gates; while with from 18° to 24° we pass through the *marine* Tertiary of Nevada, California, Northern Mexico, Texas, Louisiana, Mississippi, Florida, Georgia, South and North Carolina, Maryland and New Jersey to Martha's Vineyard and Barnstable, Mass. The circle of 36° embraces Yucatan and Honduras; reaching to near Lake Nicaragua it encloses several islands near our Pacific Coast and takes in part of Alaska, as well as a portion of Greenland.

The northern focus for *Europe* is in Scandinavia, Lat. 68° N., Long. 22° E.; the geographical and later centre is in Lat. 49½° N., Long. 20° E.

The northern focus for *Asia* is in about Lat. 71° N., Long. 99° E.; the centre is in same Long., and in Lat. 51° N.

For *South America* the centre is on the Tropic of Capricorn, in about Long. 65° W.

For *Africa* the centre is at St. Thomas' Island, where the Magnetic Equator of dip crosses the terrestrial Equator.

For *Australia* the centre is on the Tropic of Capricorn, Long. 148° E.

These are approximate, and may require slight modifications.

IV.—*Fourth Subdivision of the Law.*—Besides these three modifying influences, toward the close of the Mesozoic and beginning of the Cenozoic, the Western Alps became a dynamic focus, reaching, according to Elie de Beaumont, their present height during the Miocene Period or at its close, while the eastern Alps reached their present height during the Pliocene. Mount Rosa is nearly, if not quite, the geographical centre of the entire dry land on the globe; and the Alps connect with the Himalayas and Andes of similar geological age by a great circle or belt of immense seismic activity.

Demonstration.—A radius of 9° from Mt. Rosa defines accurately the Miocene Tertiary on the east coast of England, also in the middle of Denmark; through Prussia it is Eocene, but Miocene again in Austria, Calabria, Sicily, Algiers and Central Spain. With a radius of 36° from Mt. Rosa, we describe a curve from the Miocene of the eastern flanks of the Urals to that of Spitzbergen and Greenland; contrasting this radius somewhat, we follow the Carboniferous and Peruican rocks of the Urals to Spitzbergen. The great circle pointed out as passing from the Alps to the Himalayas and Andes marks chiefly Tertiary regions.

Summary.—The dynamics of land forming would seem, from the foregoing demonstrations, to comprise first a longitudinal force, scarcely if at all seismic, adding to continents chiefly by aqueous depositions, as each northern continent, near the termination of the median line, has a large river delta. Secondly, there is an Arctic-Antartic force, mostly along continental coast lines, and connected with active seismic phenomena of elevations and depressions; apparently from these being thinner portions of the earth's crust than at continental medial elongations. Thirdly, in each continent there are radii and circles connected with one or two important foci, which have not only aided in defining the geographical limits of each continent, but also in bringing geological deposits in successive curves of increase to or near the surface; possibly because the wave impulse directly under the plastic focus sends its molten contents to equidistant circles beneath the plastic crust. Lastly, the geology of each continent has also been somewhat modified, especially in cenozoic times, by the Alpine central focus (or terminal axis from the centre) of the dry land hemisphere.

As corollaries, attention may be called to two additional great circles of activity which are secondaries to that phase

* At these straits, though only about fifteen miles across, Wallace found as great a difference between the flora and fauna as if they had been a thousand miles apart, nearly all the animals south-east of that line being marsupial, while northwest the chief type was and is carnivorous.

of the ecliptic whose longest and shortest day, for our northern hemisphere, would coincide with the north and south plane, passing through the Alpine focus and also through the node of intersection for the terrestrial and magnetic Equator. This gives us one great circle from Behring's Straits to its antipodal Antarctic, due south from Mt. Rosa; the other from Scandinavia, at the Arctic Circle, to the antipodal point on the Antarctic, which will be found due south from Behring's Straits. As these ran through the northern hemisphere, the course of one from the volcanoes of Sumatra is nearly parallel to the formerly described Asiatic continental trend as well as the Japan Gulf Stream, and nearly parallel again through North and South America to said Asiatic trend prolonged, whereby a region is inclosed of Nevada geysers, New Madrid earthquake region, Arkansas and Virginia hot springs, Cuban, Venezuelan, Grenadan, Peruvian, and Chilean volcanic and earthquake regions. The course of the other, while running nearly parallel to the North American east coast trend, is from the thirty-nine volcanoes (see Dana's Manual, p. 703) of Central America to the geyser and volcanoes of Iceland, thus inclosing between it and the North American trend our Gulf Stream, probably even aiding to heat it; while on the opposite side of the globe the inclosed line embraces the Hindoo Cush and Western Himalaya elevations; the disturbed regions of Hindostan and islands in the Bay of Bengal (some brought up within the Historical Period) as well as the numerous volcanoes of Sumatra.

The evident connection of these laws with Terrestrial Magnetism, Mining and Mineralogy, Archæology and Ethnology, is left for future discussion.

AN INVESTIGATION OF THE VIBRATIONS OF PLATES VIBRATED AT THE CENTRE.

By THOMAS R. BAKER.

MOST of the plates used were window panes of various shapes and sizes. They were vibrated by rubbing an attached glass rod. The tubes, which were about $\frac{3}{16}$ of an inch in diameter and 20 inches long, were attached at right angles to the face of the plate with sealing wax. The support for the plate was a rubber cap, the common lead-pencil eraser, fitted on the end of a post projecting from a disk of lead. A short rubber-capped lead pencil fixed upright in a wooden block answers the purpose just as well.

The plate was balanced on the support, the tube standing upright and held loosely between the thumb and forefinger of the left hand. Then catching the tube between the moistened thumb and forefinger of the right hand and rubbing downward the vibrations of the plate were produced.

Different tones were obtained from the same plate by varying the pressure and the position of the thumb and finger. Each plate yielded from *one* to *six* tones, the number increasing with the size and thinness of the plate. A plate 10 in. by 14 gave *six* tones, one 4×4 gave *two*, and one 3×3 gave but *one*.

The interval between the lowest and second tones of a 10×12 plate was *two octaves and one tone;* between the second and third, a *diminished sixth;* and between the third and fourth, an *augmented fourth.* The greatest interval found between the lowest and highest tones of a plate was more than *four* octaves, and the greatest interval observed, considering the tones of all the plates tried, was more than *five* octaves.

Plates were reduced in size by cutting strips an inch broad from them, and a test was made of the tones of each plate thus produced. A plate 12 inches square was cut down to 11 in. by 12, then to 10x10, and so on until it was reduced to one 2 inches square. By this operation there was furnished a series of eleven plates closely alike in thickness and structure.

The intervals between consecutive tones of each plate of this series down to the plate 7x7 were almost uniform, namely; *two octaves and a fourth* between the lowest and 2nd tones, a *seventh* between the 2nd and 3d, and a *fourth* between the 3d and 4th. From the plate 8x8 to that 3x3 the intervals between the lowest aud 2nd tones were almost uniform, being about *one octave and a fourth.* The other intervals were variable. The difference in pitch of corresponding tones of consecutive plates was with few exceptions, uniform down to the plate 7x7, namely; *three semitones.*

The following is a summary of the facts derived from these experiments: 1. The difference in pitch of the lowest and 2nd tones of all plates tried between the sizes 10 in. by 14, and 7 in. by 7, was *two octaves* to *two octaves and a fourth,* and the difference in pitch of corresponding tones of square plates between the sizes 8 in. by 8, and 3 in. by 3 was *one octave and a fourth.* 2. The intervals between the tones of plates giving *not more than five* tones diminished as the pitch increased, but this was not true of plates giving *more than five* tones. 3. The pitch of tones given by a series of plates which varied in size as the square of a series of numbers whose common difference is one made a sudden leap from one uniform scale to another.

The forms of these variations were learned in the usual way by vibrating the plates with sand sprinkled over them. The figures were copied by placing the plate over paper which had been wet with a solution of potassium bichromate and dried in the dark. The plate and paper were exposed to diffused light or to the vertical rays of the sun. The paper not hid by the sand soon darkened and when this change had taken place the plate was removed and a lead pencil run along the bands of lighter colored paper representing the sand lines. This paper was then placed on white paper and the figures copied by pressure. About 150 sand figures were copied and traced.

The vibrating of plates at the centre as here described, seems to be the best method for class illustration, the main object being to show the formation of sand figures. To vibrate a plate at the centre in this way, expensive apparatus is not needed, a pane of window glass, a glass tube and a rubber eraser—the essential articles—being procured at the cost of a few cents. To vibrate a plate in the ordinary way, a clamp and bow costing several dollars are necessary. Moreover a plate vibrated at the centre will, I think, yield to the ordinary experimenter more tones than one vibrated at the edge.

A simple method of showing the vibration in parts of a rod and a string was suggested by the vibrating plate.

The end of a piece of glass tubing was drawn into a long fine thread, and the tube attached with sealing wax to a long narrow plate near one end. Then when the plate was vibrated so as to yield a low tone, the glass thread vibrates in parts forming a series of spindle-like segments.

A piece of sewing thread was stretched from one end of the narrow plate to the other over the free end of the vibrating rod and fastened to the plate with bees-wax. Then at a low tone of the plate the thread vibrated in segments.

TYPES OF POTTERY.

BY PROF. EDW. S. MORSE.

The earlier types belonging to the shell heaps of Japan were described and illustrated by specimens from each of the deposits examined by Prof. Morse and his special students.

The pottery of Yeso was nearly all cord-marked, while the shell heap pottery of the middle of Japan had a much less proportion cord-marked.

In the southern portions of Japan, at Higo, cord-marked pottery was extremely rare.

He remarked on the extreme diversity in the shape and ornamentation of the pottery in different places in Japan.

The pottery of Yeso resembling the pottery of the Northern United States; the pottery from the central portions of Japan finding their resemblance to the pottery found in Porto Rico and Jamaica. He also spoke of the hard blue pottery supposed to be Korean, and associated with it a red pottery, which might have been made by the same people. This was lathed-turned. Other forms were mentioned and illustrated by examples.

MOUNT HAMILTON, CAL.

We present our readers with a view of Mount Hamilton, the site selected for the Lick Observatory. Previous to any decision being finally arrived at, Mr. S. W. Burnham, of Chicago, was directed to make a report upon the fitness of the selection for the purpose. He states that "in accordance with an arrangement made with the Trustees of the James Lick bequest to make a series of astronomical observations for the purpose of determining the atmospheric condition of that location, with reference to its adaptation for the proposed Lick Observatory (originally suggested by Prof. Edward S. Holden, in 1874, and subsequently approved by Prof. Simon Newcomb, in 1879), I left Chicago on August 10, 1879, arrived in San Francisco on the evening of August 15, and left for Mt. Hamilton the next morning in company with Capt. Richard S. Floyd, President of the Trustees. The summit was reached during the afternoon of the same day. The telescope, which was already on the ground, was hurriedly unpacked, temporarily set up in the observatory, and used that night."

SITUATION OF MT. HAMILTON.

The city of San Jose, the nearest point of railroad connection from Mt. Hamilton, is 50 miles south of San Francisco. Mt. Hamilton, by the highway, is 26 miles from San Jose, nearly east, and is reached by a good road constructed two or three years since by the county of Santa Clara. In order to keep the grade within the limit of six feet in one hundred, the last portion of the road is carried up the ridges of the mountain by a circuitous route. The distance between the Observatory and San Jose, in an air-line, is only 13 miles.

The approximate geographical of the Observatory Peak is:

Longitude	121°	36′	40″	W.
Latitude	37°	21′	3″	N.

The elevation of this point is 4,250 feet above the level of the sea. The north peak, which is about three-fourths of a mile distant, is 140 feet higher. The ridge between is a good trail connecting the two peaks. The sides of the mountain, in most directions, are very steep, and form an acute angle at the summit. The view from the peaks is unobstructed in every direction, there being no higher ground within a radius of 100 miles. In this connection the e report of Messrs. Herrmann Bros., the engineers who surveyed the road, will be of interest:

"The scope of the horizon from Mt. Hamilton takes in more ground, according to Prof. Whitney's judgment, than almost any similar peak in the United States, there being no obstruction to the view from any quarter. It is remarkably free from fogs and clouds, as we had ample occasion to observe during our last winter's stay on the mountain when locating the road. The bearings of the most notable objects are as follows, the distances being taken, when out of our county, from our most reliable maps:

Mt. Loma Prieta	S. 35° 5′ W., 19½	miles.
Mt. Thayer	S. 51° 18′ W., 19¾	"
Mt. Poucher	S. 38° 35′ W., 6	"
Block Mountain	S. 87° W., 27⅓	"
Mt. Tamalpais	N. 51° 20′ W., 66	"
Mission Peak	N. 47° 55′ W., 16	"
Mt. Story	N. 25° 45′ W., 10¼	"
Mt. Diablo	N. 21° 45′ W., 39½	"
Mt. Sautana	S. 37° E., 35	"
Murphy's Peak	S. 6° 5′ W., 15	"

None of these points reach the altitude of Mt. Hamilton. Of those within a radius of 20 miles the Loma Prieta reaches 3,800 feet, Thayer 3,550, and Block Mt. 2,800. All the rest are between 1,500 and 2,500 feet. Of the further peaks Mt. Diablo is 3,856.

The formation of Mt. Hamilton, as of all the near surrounding ridges, is of trap rock. The high points, not worn down by the atmosphere and the action of the rain, are, therefore, very hard as soon as the upper crust is removed. In building the road we struck this hard rock at six or seven points on and near the cone, with a good prospect of finding it continuous and getting harder in the same proportion in going deeper. It has broken through the older formations at several points, near the base of the mountain, where it shows the same character, only intensified. At the top it appears as a greenstone porphyr, with small larkspur veins, exceedingly hard, without any defined strata, but in large boulders worn smooth, and generally flat on one side, and cemented together by other material less hard and easier to work. At a great many places the metamorphic slate, uplifted by the later upheavals, shows in considerable bodies, one of them being on the south side of the Observatory Peak, and nearly opposite one of the hardest points of porphyr."

At sunset the Pacific Ocean is seen over the summit of the Coast Range at various points, and occasionally a snow-covered mountain was seen in a northerly direction, supposed to be Lasson Butte, the distance of which is about 175 miles. The great range of the Sierra Nevada, about 130 miles distant, came out sharp and distinct at sunrise. There were many very distant peaks in the east and south-east which could not conveniently be identified. As an illustration of the transparency of the atmosphere, I may mention a fact communicated to me by Prof. Davidson, of the U. S. Coast Survey. He was at work in the Sierra Nevada, at an altitude of over 10,000 feet, and was able to see with the naked eye the five-inch mirror of a heliotrope 175 miles distant.

For a critical *resumé* of the work done by Mr. W. S. Burnham upon Mount Hamilton, and the results he arrived at, we refer our readers to the article on "Mountain Sites for Astronomical Observatories," in our last week's issue.

The opinion of Mr. Burnham is summed up in the last words of his report when he observes that "Mount Hamilton would be much more desirable, and more could be accomplished there with a large telescope than at any other place where an observatory has yet been established. So far as there have been opportunities for judging, it is obviously an appropriate place for erecting and maintaining the telescope to be constructed under the Lick deed of trust, and required to be "superior to, and more powerful than, any telescope ever yet made." With such an instrument in such a field wonderful discoveries may be made. The only limit to the size of the object glass would be found in the mechanical difficulties attending its construction. No refractor that can be made in the present state of the art would be unsuitable, so far as the observed conditions would enable one to judge. It is impossible to overestimate the great discoveries which might be made and the important work done with a first-class object glass of thirty inches or more aperture, as perfect in all respects as the instrument at the Naval Observatory at Washington."

VIEW OF MOUNT HAMILTON, CAL.—SITE OF THE LICK OBSERVATORY.

(Fac simile from official report.)

MEETING OF THE AMERICAN SOCIETY OF MICROSCOPISTS AT DETROIT.

The third annual meeting of this society began at Detroit Tuesday, August 17th. The meeting was held in the Detroit Female Seminary, a building well adapted to the purpose, as, besides the large hall where the regular sessions were held, it was we l provided with rooms of sufficient capacity for the accommodation of the leading manufacturers of microscopes and accessories, thus enabling them to make a fine exhibit of the latest and most approved forms of instruments and accessories. This, we may mention, is an interesting feature of the meetings of this society, which, if not carried too far, may be productive of a great amount of value to microscopic students in enabling them to examine a large variety of instruments and apparatus, which otherwise could not be done without visiting the leading centres of manufacture at considerable expense.

The meeting was opened by the retiring president, Dr. R. H. Ward, of Troy, N.Y., introducing to the society the president-elect, Prof. H. L. Smith, of Geneva, N. Y. After the applause which greeted the new president had subsided, prayer was offered by the Rev. W. W. Hammond, of Detroit.

Prof. E. C. Wetmore, president of the Griffith Club of Microscopy, on behalf of that club, extended to the visiting society an address of welcome, and then introduced the Hon. J. J. Bagley, who stated that it was with pleasure he welcomed the visitors to the beautiful city of Detroit.

Regular business was then taken up. Secretary Prof. A. H. Tuttle announced that the Executive Committee had recommended to membership the following gentlemen, who were elected: Hiram A. Cutting, Linenburgh, Vt.; J. W. Crumbaugh, M.D., Lancaster, Pa.; Jno. Phinn, Esq., Editor *American Journal of Microscopy*, New York City; L. R. Sexton, Rochester, N. Y; S. O. Gleason, M.D., Elmira, N. Y.; T. S. Updegraff, M. D., Elmira, N. Y.; Lee H. Smith, M.D., Buffalo, N. Y.; F. O. Jacobs, Newark, Ohio; W. G. Lapham, Northville, Mich.; Nathan W. Lord, Columbus, Ohio; Sydney H. Short, Denver, Colorado; Gen. Wm. Humphrey and J. F. Main, M.D., Jackson, Mich.; O. W. Owen, M.D., Prof. E. C. Wetmore, Chas. R. Ferris and Fred Seymour, Detroit, Mich.

AFTERNOON SESSION.

The meeting was called to order by President Smith, and the proceedings commenced by Mr. George E. Fell, of Buffalo, N. Y., who gave a description of a series of plates he had prepared to illustrate on a large scale the structure of the human tooth. The speaker said these plates constituted a series of enlarged sectional drawings (cut transversely) exhibiting the structure, microscopical and general, of a human molar tooth. The average size of the drawings is six inches square.

Beginning with a top view of the crown surface of the tooth the student is successively introduced to the structure and conformation of the enamel, cementum, dentine and pulp cavity, up to the fangs of the tooth, as located in the alveolus of the superior maxillary bone. Mr. Fell stated that the drawings were prepared from a series of sections of a tooth prepared by himself for the microcsopical study of its structure. His object in preparing them was to add another to the numerous aids offered to the medical and dental student in becoming acquainted with the structure of the human tooth. The plates were made up of a series of drawings overlapping each other, and finely colored, so that the various sections could be unfolded and each successive layer, of the interior structure of the tooth, consecutively exhibited. For the purpose of locating the positions of the different sections an enlarged side-elevation of a tooth (a modified copy of that prepared by Dr. F. G. Lemercier, of Paris), was used. upon which the position of the sections were defined.

Professor D. S. Kellicott, of Buffalo, N. Y., read a valuable paper upon the "*Lernescera Tortua*" a parasite harbored by the cat-fish or bull-head, and found in the river water near Buffalo. The reader stated that he had not found a locality where the parasite was at all abundant. He had only one specimen prepared for observation. It was found on a fish of ordinary size, and was deeply buried in a tumor, caused by its own presence, just back of the pectoral fin of the fish. After extraction it remained alive for several hours. The parasite could be distinguished by the naked eye, but to make out its minute structure, the best lens was required. The reader stated that with a Bausch and Lomb ½ inch objective of 98° angular aperture he had obtained the best results, making out structure which he was unable to see satisfactorily with lenses of a lower angular aperature. A very full description was given of this newly described Lemeoceran, and the whole address was attentively listened to by those present. A paper of this description by Prof. Kellicott, is specially valuable from the fact that he is one of the best authorities upon the subject of fish parasites in the country, having discovered many new species on fish inhabiting the inland lakes and rivers.

The next paper was on "The Relation of Medium-Power Objectives to Micro-Biology," by Mr. W. G. Lapham, of Northville, Michigan. The paper was of interest to students, giving the effects of the use of different objectives, and was full of hints and statements deduced from observation and the speaker's experience. The author thought that there ought to be a great National University, with a library and professors of Microscopy, and indeed of every branch of that particular science. The deduction from the essay was that with a "four-tenth objective any one could see all that they wanted to in micro-biological research."

It is quite unnecessary to state that this view was controverted by many present. While it may be admitted that with a *properly constructed* wide angled $\frac{4}{10}$ objective, very much that is ordinarily observed in micro-biological work may be seen, when eye pieces of different powers are used. To confine the work of the microscope of to-day to such powers as might be obtained with these combinations would obliterate, to a great extent, the widest field at present open to the microscopic student in original research President Smith commented upon the paper and raised objections to some of the views propounded. Secretary Tuttle also differed with the author on some points of his essay.

Mr. C. M. Vorce, of Cleveland, Ohio, read a paper on "Penetration of Objectives; Is it a Defect or an Advantage?" This paper was a very sensible *resumé* of a subject which has occupied the minds of microscopists for a long period of time, and upon which differences of opinion still exist. Mr. Vorce took the ground that there is yet work for the penetrating lens as well as the lens of wide angle with less penetration. Of the two series he would prefer the wide angled, defining, comparatively non-penetrating lenses, if the microscopist was unable to possess both series.

Following the election of new members the Society adjourned until 10 o'clock Wednesday morning.

The second day's session opened with a good attendance of members and visitors.

After the reading of minutes, etc., the executive committee reported the following as approved applicants for membership: The Rev. Wm. D'Orville Doty, Rochester, N. Y.; Rosa M. Redding, Newcastle, Ind.; Chas. Shepard, M. D., Grand Rapids, Mich.; W. B. Sprague, M. D., Detroit; Allen Y. Moore, Coldwater, Mich.; W. G. White, Buffalo, N. Y.; William A. Clapp, New Albany, Ind.; John Sloane, New Albany, Ind.; Richard J. Mohr, Fairfield, Iowa; Albert McCalla, Fairfield, Iowa. The gentlemen named were accordingly elected members of the society.

Prof. C. M. Vorce read the first paper of the day, which was entitled "The Microscopic Examination of Writings for the detection of forgery," etc.

The speaker treated the subject at length, saying that he had a great deal of interest in the matter, and directed his attention both to the verification and signatures and general writing. He had considered, first, the general characteristics of writing; second, special characteristics, modifications of, or departures from, general characteristics. There were five elements which determined the character of a person's handwriting: The paper, the pen, the ink, the personal qualifications of the writer, and the conditions under which the writing was done. Any one of these being changed from the ordinary conditions, the microscopic conditions of the writing were almost sure to be changed also. So far as the paper is concerned, its glazed surface is the only characteristic which affects writing. The harder and smoother the surface the better defined is the writing upon it, and the better chance there is of determining any erasure, change, or interpolation. On paper of good quality, with a good pen and readily flowing ink, the lines of writing present a tolerably even contour, depending upon the rapidity, pressure, the amount of ink in the pen, etc. The speaker illustrated at length on the blackboard the various widenings or "webs" which are always found at points where two lines cross, explaining how a variation of speed, a change in the kind of ink and other causes affected this web. Upon rough paper the lines always have a ragged edge; the webbing is, if anything, less than upon hard, smooth paper. As to the pen, he stated that when a steel one was used the paper always showed a distinct groove or cutting on its surface, especially at the edges of the heavy lines. When a pen is old and corroded, the paper looks as though cut with a knife. The various qualities of ink were discussed, together with the effect on the appearance of the writing which copying in a letter press has. Some inks will not write well on paper that has been lithographed, running unevenly, as though the paper were greasy. By the fourth condition, the qualifications of the writer, the speaker meant his skill, method, physical ability, etc. A person much accustomed to writing usually writes at a good speed and without hesitation. The writing, in quality, is apt to look alike at all points on the page. Where writing is done slowly it is not so regular and the curves are not so smooth and geometrical. Where a habitually light writer attempts to make a heavy stroke, the shading is irregular. The same is true where a person accustomed to writing with a heavy stroke attempts to write light. These differences are such that they can be usually discovered with the aid of the microscope, and when a writer concentrates all his faculties on the appearance and character of the writing it never has the easy, flowing appearance which it otherwise would have. The tremor in the writing of aged persons, he stated, it was nearly impossible to imitate. The fifth condition, the circumstances under which the writing was done, had as much to do with its appearance as any other cause. One who habitually uses a flexible gold pen writes very differently with a steel pen. The reverse is equally true. Persons who are accustomed to write sitting usually cannot write as well standing. The practical application of these and other facts in the examination of writing requires patient investigation, much of it apart from the simple use of the microscope. In the great majority of cases the microscopic investigation is utterly useless without a corresponding outside investigation. The signatures to letters are apt to vary more than those written elsewhere. Letters produced as specimens of a person's handwriting are very apt to prove deceptive. Sometimes it is impossible from expert testimony to determine the character of the suspected writing. As an instance, the speaker related that he had in his possession a genuine promissory note in which a man had misspelled his own name in the signature. Had he died and there been a contest as to the signature it could hardly have been decided as anything else than a forgery. Unfortunately, however, the man lived to pay the note, thus spoiling a very good chance for a nice case of expert evidence.

Ex-President Ward discussed this paper at some length, his remarks particularly relating to the individual peculiarities of writers being noticed more or less in their handwriting. He considered it a very important factor in the detection of forgeries, etc.

The next paper was on "Mounting Materials," by Dr. Carl Seiler, of Philadelphia. He said the microscopists of both Europe and America were divided into two classes on this important question. Many believed that balsam should be the only material used in most cases and others as decidedly glycerine. He was of the opinion that all tissues which can be hardened and cut into sections are best mounted in balsam, and such specimens as membranes, hairs, cilia, etc., are best mounted in glycerine. If one wished to show delicate, fine lines he should use glycerine. The advantages of balsam are that it does not destroy colors, makes a specimen clear and does not deteriorate. The disadvantages are that the specimen is apt to shrink, and the process of drying is very slow. The advantages of glycerine are that delicate membranes may be preserved, while its disadvantages are that it always interferes with the coloring. The specimen also tends to deteriorate. Specimens mounted in glycerine are very apt to suffer from leakage. There are substances which in some cases combine the advantages of both, without the disadvantages of either. Among these the speaker mentioned Farrant's medium and Damar's cement.

This topic was discussed by Treasurer Fell; Dr. Younghusband, of Detroit; Dr. Seiler, of Philadelphia; Mr. J. H. Fisher, of Rochester, N. Y.; Mr. C. M. Vorce; W. H. Walmsley, of Philadelphia; Secretary Tuttle; President Smith and others.

The Society adjourned until the afternoon.

Afternoon Session.

At this session President Smith announced the Committee on the Griffith Award as follows, viz.: W. H. Walmsley, Prof. D. S. Kellicott and Mr. J. H. Fisher.

A work on "Angular Aperture of Microscope Objectives," by Dr. Geo. E. Blackham, F. R. M. S., was presented to the Society by the President on behalf of the author, to whom the thanks of the Society were extended.

The discussion of the paper read by Dr. Seiler at the morning session was continued, after which Mr. J. H. Fisher, of Rochester, N. Y., read a very interesting paper entitled "Notes on the Structure, Development, and Position, of a (supposed) Undescribed Flagellate Infusiorium." He referred at first to the but little explored domain of the lowest forms of animal life, which so nearly approach the vegetable. The Infusorium which he described he found in a small pond of stagnant water near Mount Hope. The body of the little animal was shaped like a cylindrical flask, green in color, the mouth resembling the neck of a bottle, and provided with a flagellum presumably for both prehensive and sustentatory purposes. The animalcule was minutely described, with its habits. It had no red eye-speck. Spines were equally distributed over it. It could not be identified, he thought, with any known species. Mr. Fisher provisionally named it *Laguncula piscatoris*.

This paper was discussed by Mr. Lapham, of Northville, Mich., who said he had seen an organism almost identical with it, except that its outer shell was composed of a series of successive plates.

The next paper was by Mr. William H. Walmsley, of Philadelphia, on "The Use of Wax Cells in connection with White Zinc Cement for Fluid Mounts." The methods employed by Mr. Walmsley, which he stated had given him great satisfaction, both as to the durability of the cell and the neatness of the mounts, was essentially the coating of the ordinary wax cell with white zinc cement. He gave his most approved formulae for the preparation of the cement which he discovered quite a number of years ago, and explained his manner of using it. He exhibited slides with cells from four to six years old, which had resisted the action of the fluid contained within them, without any apparent change. The paper was discussed by Mr. Fell, Mr. Fisher, and several other gentlemen. Mr. Walmsley, in reply to a question, said the cement would sometimes turn yellow.

Discussion was here discontinued, and the Society adjourned.

The address of President H. L. Smith was delivered in Whitney's Opera House in the evening. Prof. Smith said he thought they had very great reason to congratulate themselves upon the results attained at the two previous annual meetings. He might also speak of the wonderful improvements which had been made in the microscpe; but these would be less desirable than a discussion of some special question. He announced his subject to be "Deep sea soundings, and the relation of microscopic Algæ to deep sea animal life, with a few remarks upon evolution." He began with a glowing description of the wonders and beauties of the ocean. He then related the various stages by which it became known that it was possible for life to exist at great depths in the sea, and recounted the voyages of the United States vessel *Tuscarora*, and the English vessels *Challenger* and *Lightning* in their efforts to add to human knowledge concerning deep sea life.

Prof. Smith has in his possession material obtained from the soundings made by the *Tuscarora*.

He described the methods used to obtain specimens of the animal and vegetable life to be found three or four miles below the surface of the ocean. He then made a logical and lengthy argument to show that the low forms of deep sea life may furnish another link in the line of proof which is causing scientific men to tend so largely to the evolution theory.

The paper was lengthy and will appear in full in the proceedings of the Society.

THURSDAY'S SESSION.

Following the reading of the minutes, the Executive Committee reported the name of P. L. Hatch, M. D., of Minneapolis as a member of the Society. He was duly elected.

The secretary also read a report of the Executive Committee in reference to amendments to the constitution. The amendments propose the election of honorary members; the election of secretary and treasurer for three years; making the vice-presidents the auditors of the treasurer's accounts and the treasurer the custodian of the society's property; making the terms of the officers begin at the conclusion of each annual meeting; and providing that if any member shall fail for two years to pay his dues he shall forfeit his membership. The report was accepted and the amendments will come up for action next year.

The Executive Committe also adopted a resolution which was approved by the Society, limiting the sale of the publications on hand, viz: The Proceedings of the Indianapolis and Buffalo meetings, to the members of the society to fill out sets. This action was deemed necessary in view of only a limited number of copies of these proceedings being on hand.

The nominating committee reported the following officers for the ensuing year:

President—J. D. Hyatt, president of the New York Microscopical Society.

Vice Presidents—Geo. E. Blackham, M. D., Dunkirk, N. Y., and W. B. Reoner, M. D., Cleveland, O.

Secretary—Prof. Albert H. Tuttle, Columbus, O.

Treasurer—Geo. E. Fell, Buffalo, N. Y.

Executive Committee—W. H. Brearly, Prof. J. H. Fisher, Prof. Albert H. Chester.

The report was adopted, and they were duly elected.

"Demonstration of Capillary Circulation in Man," was the title of a paper by Dr. D. C. Hawxhurst, of Battle Creek, Mich.

The process of examining the capillary circulation in the lip of a man was described. The lower lip was rolled over a support, and the microscope arranged to view the circulation.

Proper means were taken to steady the head. Clamps were applied to the lips so as to cause an engorgement of the capillary vessels. The method was that of a German scientist.

A power of about 100 diameters was used. The speaker related many interesting experiments, and also explained the effects produced by treating the lip with chloroform, ammonia, acids, glycerine, etc.

The paper was discussed by Dr. Seiler and Mr. Fell, these gentlemen deeming the power too low to be of much service. Dr. Seiler stated that other portions of the body were better adapted for viewing the circulation than the lip, and did not believe the method pursued would be fraught with results of scientific value.

The next paper was by Dr. Carl Seiler, of Philadelphia. "Describing an Improvement in a Microscope Stage." He said last year at a meeting of the society he set forth the necessity for certain improvements in the microscope of the future, one of which was an increased movement of the stage, giving at least four inches play in each direction. Mr. Walmsley, agent for R. & J. Beck, of London, had a binocular made by that English firm, embodying the improvements suggested. Dr. Seiler exhibited the instrument, which he said was particularly valuable in examining large specimens, such as sections of tumors, the vocal organs, or anything requiring a large stage movement to bring the whole of the specimen successively into play.

W. H. Bullock, of Chicago, described a microscope which he had specially arranged for examining rock sections. It was arranged with improved facilities for minute measurements, and had admirable arrangements for illumination of opaque objects, etc.

He also described a new section cutter devised by Prof. Burrill, of Illinois. It had some valuable features about it, notably the manner of holding the knife so that it could be inclined to any angle, with reference to the cutting surface. The well-hole was so arranged that it could be raised and lowered by the micrometer screw, carrying the material to be cut with it. This, it was claimed, offered some advantage over the ordinary "well hole." The arm which carried and supported the knife worked on a brass plate, a corresponding portion of the arm working in a groove cut in the plate, insuring with even an unsteady hand a true and perfect section. It was claimed that sections, the $\frac{1}{1000}$ of an inch in thickness, could be cut with this apparatus.

Mr. W. H. Griffith, of Fairport, N. Y., read the last paper of the Session, describing the new Griffith Club Portable Microscope. He took from his pocket a small narrow case which he opened. Inside was discovered the disjointed parts of a microscope. On placing them together, which was done in a very short time, a very complete instrument was the result. With this little instrument, which we cannot now describe very minutely, the lowest to the highest powers may be used. It is provided with the Society screw, coarse and fine adjustment, the latter on a principle believed to have never before been applied to the microscope, and which is capable of being used on larger stands. The body is composed of tubes which may be drawn out to the standard length of ten inches. In the field this little instrument may be used to advantage, being provided with a wood screw by which it may be secured to the side or branch of a tree or even to a fence-rail. It may then be used with the highest powers. The mirror is hung so that it may be used for transmitted or reflected light. If the owner is in need of a turn-table, by simply arranging a few screws and laying the instrument down on its side he may go to work "ringing" slides to his heart's content. The instrument was made for Mr. Griffith by Messrs. Bausch and Lomb, of Rochester, N. Y.

Prof. T. J. Burrill, Professor of Botany and Horticulture at the Illinois Industrial University, followed with a paper on "The So-called Fire-Blight of the Pear and Twig-Blight of the Apple Tree." His remarks, bearing as they do upon a subject of general interest, are given at some length.

He said the widespread and disastrous disease of the Pear tree, called Fire-blight, and that no less prevalent and alarming one known as Twig-Blight of the Apple tree, are due to the same immediate agency. They are identical in origin, and similar in their pathological characteristics, as *a priori* reasoning might have indicated. The Quince and probably other plants, among which may be named the Butternut, the Lombardy Poplar, and the American Aspen, also suffer from the same disease. From descriptions it was very probable that the "yellows" in the Peach will be found due to a similar cause. The immediate and exciting cause is a living organism producing butyric fermentation in the carbonaceous compounds, starch, etc., in the cells of the affected plants, especially in those of the bark outside of the liber. This organism, if really specifically distinct, is closely allied to the butyric *vibrion* of Pasteur and *Bacillus amylobacter* of Van Tieghem. The disease has been known in this country over 100 years. Various theories have been advanced, and one by one disproved, except the one of fungus growth. In 1878 the writer announced to the Illinois Horticultural Society the discovery of bacteria apparently connected with the disease. His investigations were carried on in an orchard where there were 94 Apple trees, 20 Pear trees and 1 Quince. "After finding myriads of bacteria in the fluids of the diseased tissues," he said, "I inoculated several Pear and Apple trees with what to me, at the time, were unsatisfactory but not uninstructive results. Beginning on the first day of July, 1880, I experimented in various ways at different times upon 66 trees of the Pear, Apple and Quince. Of the numerous applications of the virus upon the unbroken bark or leaves none were successful. Of the inoculations there were successful 63 per cent. of the Pear, 30 per cent. of the Apple, and 100 per cent. of the Quince. Upon the Pear and Quince trees used for the experiments, the disease appeared only in a single case except as the direct result

of the inoculation. This latter was sometimes performed with a knife, sometimes with a needle, always with careful precautions and close subsequent examination. Such experimental limbs as permitted it were cut and preserved like herbarium specimens, and are exhibited with the paper."

The organism found answers fairly to the description of Pasteur's butyric *vibrion*. They are usually oblong, rounded at the ends, mostly connected, two together. Their motions are not rapid, consisting of turning in every direction, and sliding irregularly forward. They are found within closed cells, in the open spaces, and in immense numbers in the viscid exudations from the diseased bark and leaves. The most conspicuous alteration observed in the tissues is the disappearance of the starch grains from the cells. The cell walls are left intact, and the protoplasmic portions remain until after the starch is mostly absorbed and appears to suffer little change until death ensues. The disease is, *par excellence*, one of the bark. The leaves die in consequence of this, or are themselves invaded, either primarily or secondarily, by the destroyer. The progress of the disease is always slow, but the leaves of an affected limb often turn black quite suddenly, perhaps according to meteorologic conditions. In diseased bark, before change has taken place visible from without, and while the leaves are still green and fresh, an active fermentation occurs. This continues until desiccation or the exhaustion of the fermentable substances puts an end to the process. The products of this fermentation are Carbon dioxide and Butyric acid, or a closely similar substance. From the fact that virus from the Pear affects the Apple tree, and vice versa, the speaker argued that the disease was similar in each. The experiments tended to show that the virus is harmless upon the epidermis of healthy plants, nor does it penetrate through the breathing pores. The speaker exhibited drawings of the cells of a healthy plant and a diseased one, showing that the starch in the latter was gradually absorbed. He obtained the virus from diseased trees, where it is exuded, and placed it in distilled water. Upon the dead leaves and branches the virus dried and looked like varnish. When redissolved it retains its vitality. The simple puncture of a bark of a tree with a needle which had been dipped in the virus would be sufficient to cause its death. Prof. Burrill exhibited a small vial containing about a teaspoonful of the virus in solution, which he said was sufficient to destroy a whole orchard.

The Griffith Award.

The committee appointed to examine the specimens of adulterations of commercial articles, and to award the prize, a fine objective, offered by Mr. E. H. Griffith, for the best mounted specimens, reported that C. M. Vorce was the only contestant and that his exhibits of coffee and butter were fine ones. He was therefore entitled to the prize.

President Smith presented it to him in a brief speech, and he accepted, regretting that there had been no other contestants.

A resolution offered by Prof. Burrill, that the president and vice-presidents elect of the society be appointed a committee to report upon some plan for uniformity in size and naming of eye-pieces and tubes, was adopted.

The report of the treasurer Mr. George E. Fell, showed $266.06 on hand, and $450.75 due the society, of which the treasurer regarded $114.69 as being very certain of being paid, making total assets $380.81. The report was adopted.

Prof. Griffith renewed his offer of a ½ inch objective or its equivalent for the best mounted slides showing adulterations in commercial articles, accompanied with the best Thesis upon the specimens submitted. His offer was accepted with thanks.

The Society then adjourned to meet at such time and place as the Executive Committee may determine upon.

The Soiree, which was given in the evening at Merrill Hall, by the members of the American Society and the local microscopists, was in every way successful, and gave great satisfaction.

PRESERVATION OF FOSSIL INSECTS AND PLANTS ON MAZON CREEK.

By J. W. Pike, Vineland, N. J.

Mazon Creek is a branch of the Illinois River, which it joins at Morris, Grundy Co., Ill. It has carved its channel down into the blue shale, which lies above the Morris coal seam, and exposed the ironstone nodules which contain the fossil plants and insects.

Scientific interpretation rests upon comparison. We compare this coalbed with other deposits of carbon, and with those now forming, and ascribe it to an ancient swamp or wet land surface. The shale above is compared with other clay-beds and with the mud of bays and lakes, and we conclude that it is the product of a subsidence and of deeper water. The fringing swamp had advanced upon higher ground, and from it floated the fern leaves and insects that were buried in the accumulating clay of the deeper basins. Leaves that sink upon the mud of a lake will rest flat upon the upper layer, and are buried under the layers that follow. So, too, the leaves in the Mazon shale are conformable to its lines of stratification. Over the shale are beds of sandrock. Compare them with beds of sand and clay now being formed over the peat and clay of the sinking Atlantic coast. It becomes clear that the beds of coal, shale, and sandstone on the Mazon are the product and record of a subsidence in the carboniferous period.

Metamorphism.—The shale immediately around the fossils was transformed into clay-ironstone nodules by the deposition of ferrous carbonate. The concreting force has emanated from the fossils, because the nodules take their general shape. The iron deposit has not merely filled the spaces between the particles of clay, but has crowded them apart and thickened the strata, making them concavo-convex above and below the fossils. Specimens exhibited show the continuity of the strata from the soft outlying shale through the nodules, their thickening and resulting convexity, the conformability of the leaves, etc.

These biological records, like primitive human inscrpitions, were written in nature's picture-language, only they are incomparably more perfect. Like the cuneiform of the Assyrian tablets it was done upon soft clay, but the clay was hardened automatically by the writing itself, and not by baking. Like the castings of the founder who surrounds his models with moist sand, these are casts; but they are casts of the delicate structure of ferns and insects, moulded in fine clay by the gentle touch of moving water. These inscriptions were not carved on the exposed and crumbling surface of monuments, but were sealed up in the concretions, and lay buried in the clay, beyond the reach of wear and decay, during the incalculable periods of the Permian, Triassic, Jurassic, Cretaceous and Teritiary. After the ages of ice and prairie lakes, the waters of the Mazon dug their channel through lake deposits, ice drift, carboniferous sandstone, and into the blue shale. The fossil bearing nodules were washed out of the softer shale, mingled with granitic gravel and strewn in the river bed. Exposure to the air changed the blue ferrous compound to ferric or red oxide. These nodules spontaneously divided into halves, disclosing these exquisite pictures of the ferns, insects and creeping things of the carboniferous lowlands. Per-oxidation continues till the iron separates from the clay. Thus the half of a nodule, with a fern pictured on its surface, may become a geode—a hard red brown shell of iron enclosing the clay in an ochery form in its interior; or it may, in the process, crack and crumble into flakes and fragments. The collector, therefore, must now anticipate the denuding forces, and dig the concretions out of the shale of the river's banks and bottom, and crack them for himself.

CAVES IN JAPAN.

By Prof. Edw. S. Morse.

Mr. Morse described a number of artificially-constructed caves which he had examined in various parts of Japan, giving sketches of them upon the black board.

These caves varied considerably in their design, but agreed in their general proportions, and were evidently intended as receptacles for the dead. They were excavated in soft rock on the sides of hills—the apertures small and in some cases showing grooves for the adjustment of slabs of rock or other material to close them. The absence of remains in these caves could be explained from the fact that in earlier times outlaws and refugees often used them as places of shelter and residence, and laws had finally been passed by the governors of some of the districts causing the caves to be filled up, or their entrances obstructed, to prevent their being used in this manner.

THE IRON ORES OF THE BRANDON PERIOD*

By Henry Carroll Lewis.

The theory that a great portion of the iron ores of our lower Silurian limestone valleys are of a tertiary age was first proposed by Prof. E. Hitchkock, but has been rejected by many geologists. The present paper describes in full recent discoveries, made by the writer, of lignite associated with limonite iron ores in the limestone valley of Montgomery County, Penn., and shows their relation to the deposit at Brandon, Vt., and their bearing upon a theory of the age of iron ores in similar positions in the Atlantic States. The lignite of Brandon, lying within beds of plastic clay, kaolin and iron ore, was shown by Lesquereaux to be of tertiary age. Lesley afterwards described strata of lignite in a similar position at Chambersburg, Penn., but regarded them as local deposits of late date. More recently Prime has found lignite in a plastic clay at Ironton, Penn., and supposed it to have been transported by a glacier. The present paper shows that in each of these cases the lignite lies far below the surface drift, and that, as at Brandon, the latter lies unconformably upon the plastic clays containing the lignite.

The occurrence of lignite in connection with limonite iron ore, plastic clay, kaolin and firesand in a number of places in Montgomery County, Penn., is described, and it is shown that these localities lie in a line corresponding to the line of strike of all the iron ores of the valley. Overlying the plastic clay which contains the lignite is what appears to be a decomposed lower Silurian hydromica slate, and for this reason the iron ores had been supposed to be of primal age. It is shown that this decomposed material and the underlying iron ores have been originally derived from lower Silurian slates, and have been re-stratified in an age intermediate between Triassic and Upper Tertiary.

The iron ores of this region may be divided into four classes: (1) Gneissic Ore; (2) Primal Ore; (3) Tertiary (Brandon) Ore; (4) Drift Ore. The last two classes of ore are often found at the same locality; the latter lying unconformably upon the former. The paper discusses at length the age of the drifts containing the latter. Notwithstanding the fact that a region of triassic red shale lies north and east of the valley, not a single fragment of such rock occurs in this drift. The pebbles are composed almost wholly of Potsdam sandstone,—a material now in great part eroded away in this vicinity. The evidence is strong that this drift was not caused by any flood from the north. That it is older than the Glacial Epoch is also shown both by the great amount of erosion it has suffered, and by the fact that in the adjoining triassic region no trace of drift occurs. It seems to have been formed at a time when hills of Potsdam sandstone, since eroded, stood as a barrrier between the limestone valley and the triassic rocks to the north. It is of interest to find that the pebbles of the sub-cretaceous clays of New Jersey are also formed of Potsdam. The four gravels of different ages of the Delaware valley are described, and it is shown that the drift ore of the Montgomery County valley belongs to the oldest of these, and is of Tertiary age.

It follows that the strata containing iron ore and lignite, which underlie unconformably to this drift, are yet older. Some facts point to a Wealden age, but the identity of the deposits with that at Brandon, in which Tertiary plants are found, indicates a middle Tertiary, perhaps *Oligocene* age. Since an exact geological age cannot at present be assigned to these deposits, it is thought best to group them together under the name of the *Brandon Period.*

* Read before the A. A. A. S., Boston, 1880.

Attention was directed to another deposit of lignite and iron ore near Augusta, Ga., recently found by N. A. Bibikov. Its geological situation and the section given is remarkably similar to those of Brandon, Chambersburg, Ironton and the Montgomery County Valley, and with them indicates the existence of a great inland fresh-water formation of Eastern America, during the Brandon Period, once fifty miles broad and nearly a thousand miles long.

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

We have received the first publication of this Society, which was organized on the 7th of April last. The objects of this Society are to promote the Arts and Sciences connected with Engineering and Mechanical construction, by means of meetings for social intercourse and the reading and discussion of professional papers, and to circulate, by means of publications, the information thus obtained.

Mechanical, civil, military, mining, metallurgical and naval engineers and architects may be candidates for membership to this Society, the initiation fee of members and associates being $15 and their dues $10—payable in advance.

The first President is Professor Robert R. Thurston, of the Stevens Institute, Hoboken. The Society starts with two life members—Thomas A. Edison, of Menlo Park, and George H. Norman, of Boston, and 189 ordinary members of different grades. We wish this Society success, and shall chronicle the work it performs. Those who desire to become members should address Lycurgus B. Moore, 96 Fulton street, New York city.

PHYSICAL NOTES.

The beautiful proof that a constant current of electricity flowing through a thin gold plate can be deflected by a magnet, was exhibited by E. H. Hall on the 28th of last October, at Johns Hopkins University, and already we see how fruitful it is in suggestion to other scientists. Bolzmann, in a paper read before the Academy of Sciences in Vienna, calls attention to the fact that is possible to calculate the absolute velocity with which the electricity flows through the gold plate, and gives a formula.

A. von Ettinghausen also verifies Hall's observations and deductions, in a thorough article containing plates of original apparatus. (Carl's Reportorium, Vol. xvi., No. 9, p. 574.)

Dr. Hall himself, in the September number of *American Journal of Science*, gives another paper on the subject, with detail of additional experiments, in which, besides gold, he uses silver, platinum, iron, nickel and tin, as thin conductors. For further information on this most instructive and interesting subject references should be made to the above-mentioned articles.

It may be convenient to scientists who have had dealings with the late firm of Hall & Benjamin, of 191 Greenwich street, New York, one of the largest dealers in chemical and physical apparatus in this country, to know that J. & H. Berge, of 95 John Street, New York, have purchased everything appertaining to that business.

The old friends of Mr. Hall will be glad to learn that he remains in the business, and may be communicated with as before.

The catalogue of these united firms has been placed before us, and shows the magnitude of the business they conduct, and the great facilities they offer scientific men in the production of every kind of philosophical apparatus. This catalogue is a handsome volume of over 200 pages, illustrated throughout, and we advise chemists and physicists to apply for a copy.

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NOTICE.

We are preparing a list of Professional Scientists, and of all those engaged in scientific investigations in the United States. We ask the co-operation of our subscribers and the heads of Universities and Colleges, by forwarding to us lists of Scientific men connected with such institutions.

We invite all engaged in original research, whether professionals or amateurs, to forward their names and addresses for registration, giving their names and style in full, and the particular line of research followed.

We desire that the list of Physicists shall be very complete as we understand that such a list does not exist, even at Washington.

We guarantee to forward all names and addresses received by us to the Smithsonian Institute, Washington, as we understand from the Secretary that such a course would be acceptable.

Compliance with this request will result in many conveniences to those concerned, as when the lists are analytically arranged, those desirous of communicating with their fellow workers will have facilities for so doing, and papers and notices can be distributed without delay. Scientific men, at present dispersed over a wide territory, will thus be united, so as to make co-operation possible when occasion requires.

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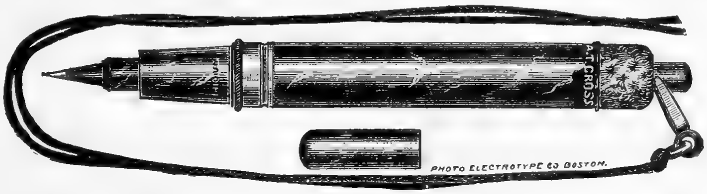

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Scientific papers and correspondence intended for publication should be written *legibly* on one side only of the paper. Articles thus received will be returned when found unsuitable for the Journal.

Those engaged in Scientific Research are invited to make this Journal the medium of recording their work, and facilities will be extended to those desirous of publishing original communications possessing merit.

Proceedings of Scientific Societies will be recorded, but the abstracts furnished must be signed by the Secretaries.

Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply may be written in the form of an article.

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SATURDAY, OCTOBER 2, 1880.

WE are pleased to remark some prospect of renewed astronomical activity at the Dearborn Observatory, Chicago. This institution was, for a period of more than ten years, in possession of the largest refracting telescope in existence—the object glass of which has an aperture of eighteen and one-half inches. The great telescopes which have since been built, and are now in process of construction, have the apparent effect of dwarfing the Chicago telescope, which, at the time it was made, was a great advance on every thing that had preceded it. There seems to be the best of reason for doubting, however, whether any other instrument at present in existence is surely superior to the Chicago refractor for efficient astronomical work. MR. S. W. BURNHAM, distinguished for his researches in double stars, speaks with authority in this matter—"I know of no object, faint or otherwise, which has been seen at Washington or elsewhere, that cannot be seen perfectly here [at Chicago] and accurately measured." Professor NEWCOMB, in his "Uranian and Neptunian Systems, Investigated with the 26-inch Equatorial of the United States Naval Observatory, Washington," remarks that Ariel and Umbriel, the inner satellites of Uranus, "are visible only when the atmosphere is very fine, and are then difficult objects," and considers it very doubtful whether these objects have ever been seen with an aperture so small as twelve inches. Director HOUGH, of the Dearborn Observatory, states that near the time of the planet's opposition, these satellites can readily be seen and measured, under ordinary atmospheric conditions, with the Chicago telescope. If, as is quite possible, the Chicago refractor should prove to be quite as effective in actual observation as some of the larger telescopes of a later day, we shall have another of those instances frequently forced upon the astronomer, wherein his computation of the adequacy of a particular instrument does not tally with its observational effectiveness. Every astronomer, then, must regret that so competent an instrument must, through lack of endowment, be lying mainly idle, or, at the most, only employed by those who are able to turn it to scientific observation without pecuniary compensation. The valued work of Mr. BURNHAM with this instrument, in the discovery and observation of double stars, is well known. Professor HOUGH, in connection with Professor COLBERT, conducted a series of observations of Jupiter at the late opposition. Owing to the discordance in the determinations of the ellipticity of the planet's disk from observation, their attention was given to a new determination of this quantity, with these results:

By Professsr HOUGH	1—16.23
By Professor COLBERT	1—16.73

The English Nautical Almanac uses the value 1–13.71, while the value 1–16.40 is adopted in the American Ephemeris. With the same magnifying power, 638 diameters, the absolute polar and equatorial diameters of the planet were observed to be, for the mean distance of Jupiter from the Sun:

	POLAR.	EQUA'L.
By Professor HOUGH	36".319	38".704
By Professor COLBERT	36".030	38".316

Assuming a solar parallax of 8".81, the measures of Professor HOUGH give for the equatorial diameter 90,570 miles, and for the polar diameter 85,000 miles.

Measures of the angle of position of the north edge of the equatorial belt show that it had the same direction around the entire circumference, and that this direction (exactly parallel to the planet's equator) was maintained throughout the entire opposition. Very complete measures of the apparent latitudes and widths of the several components of the belt system of Jupiter were also made, the great red spot co-inciding very nearly with one of these belts. The reduced measures of apparent latitude show very clearly that the belts were arranged symmetrically on either side of the equator, three being in the northern and three in the southern hemisphere of Jupiter. The report on these observations is accompanied with wood-cuts showing the red spot, the belt system, and, to some extent, the structure of the great equatorial belt. From the observations of this spot, Professor COLBERT has computed the time of rotation of the planet on its axis: he finds it to be 9h. 55m. 34.2s., differing about eight seconds from the value hitherto considered the most probable.

Micrometric measures of the diameters of the four satellites of Jupiter were made on three nights, the resulting values being, at mean distance of the planet:

I.	II.	III.	IV.
1".114	0".980	1".778	1".457

The actual diameters of the satellites given by these measures are 2610, 2290, 4160, and 3410 miles, respectively.

But the superior quality of the object glass of the Chicago refractor is more effective with such objects as the satellites of Uranus; micrometric observations were secured as follows:

Of Ariel, on four nights.
Of Umbriel, on one night.
Of Titania, on eight nights.
Of Oberon, on seven nights.

And this, notwithstanding that the observations were begun late in the opposition, and were interrupted by an unusual amount of cloudy weather. We should like to see the superior light-gathering power of this object glass turned toward systematic figuring of the fainter nebulæ.

We may mention the meridian circle of the Dearborn Observatory—a fine instrument constructed by the celebrated REPSOLDS, of Hamburg, and which must have few equals in this country. It must be the occasion of serious regret that such a splendid piece of mechanism is put only to the task of the mere determination of time, when it is adequate to the determination of the exactest sort of fundamental star-positions. We may be permitted the hope that the creation of a new fund by the citizens of Chicago may ere long contribute to the very possible result of placing the Dearborn Observatory on a permanent footing as one of the first institutions of the kind in this country.

The Royal Danish Academy of Sciences has recently offered a prize of 320 crowns for the best discussion of the theory of the accidental errors of a clock. These errors may be divided into two classes, those arising from errors in the time observations and those depending upon the quality of the clock. These latter in turn may be divided into those depending upon the irregularities of the rate of the clock and those which are independent of the rate. The discussion must include a practical method of determining the value of each of these kinds of probable errors independent of the others.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, 1880.

(Continuation of papers read.)

ON PATENT LAWS AS A MEANS FOR THE ADVANCEMENT OF SCIENCE.

BY PROF. B. S. HEDRICK, of Washington, D. C.

THE proper aim of science was defined to be the making of discoveries. The discoverer of a new mineral, a new plant, a new law of nature, or a new world, has no proprietory right in his discovery. The honor and distinction he obtains is his reward. The discovery, then, cannot be the subject of a patent. The laws of nature, the properties of matter, the physical forces, the laws of their generation and government, are like the earth, the air, the water, the common property of all. Property in the former, as in the latter, is created by enactment. But in civilized communities the reason for the law is that something has been added to what was given by nature. The land has been fenced, ploughed, planted, or buildings placed upon it. That gives the foundation for proprietory right, and public policy requires that this be recognized, and civil, municipal and common law does this in the case of the land, the air, and the water. The patent laws do the same when discoveries, the properties of matter, the forces, the laws which govern them, are made to take the shape of useful inventions. The invention which the inventor created is secured to him as his property for a period at least. But note the laws themselves. It is the reflex action of the inventor that acts to advance science. Illustrations were given by referring to Watts' steam engine in advancing our knowledge of the laws of heat; the telegraph in giving an immense development to the source of magnetism and electricity; and now the telephone and other kindred inventions serve to push our knowledge into the farthest and outermost borders. The probation given by the patent laws enable the great host of investigators to carry on their researches, and instead of becoming a tax or burden to the commuity, they help themselves and bear a full share of the ordinary burdens of society. Reference was made to Wheatstone, Bessemer, Perkin, Graibe, Sir William Thompson, and others in Europe, and to Morse, Page, Henry, Gale, Bell, Edison, and many other members of our association, men who have greatly advanced science, and have received of the rewards which flow from the operation of patent laws.

THE MEAN RATIO OF OXYGEN TO NITROGEN IN THE ATMOSPHERE.

BY PROFESSOR E. W. MORLEY.

In the afternoon Prof. E. W. Morley presented the following remarkable conclusions from experiments: When the air at a given place is cold and the barometer high, there may sometimes be a vertical descent of cold air. Samples collected at such times are more likely to approach the composition of the upper atmosphere than those collected at other times. If there be any cause which tends to produce an excess of nitrogen in the upper atmosphere, the average per cent. of oxygen in many samples collected as mentioned, will be lower than that of other samples. Therefore, to determine whether there be any difference in the composition of the lower and upper atmosphere, Professor Morley collected samples of air during each time of unusual cold and high barometer from September, 1878 to April, 1879. In 1878 the average amount of oxygen in these was 0.16 per cent. below that of other samples. In 1879 the average was 0.12 per cent. lower. Careful revision fails to detect any source of error. Professor Morley was led, therefore, to presume that the upper atmosphere, is acted on by a cause tending to remove part of the oxygen, and to pursue the inquiry by means of a series of daily analyses in duplicate of air for six months, and a comparison of the results of analysis with the thrice daily maps of the United States Signal Service. He finds a deficiency of oxygen at times, and only at the times, when a vertical descent of air at or near the place of collection may be inferred with a fair degree of probability from these maps, and sometimes a deficiency when a vertical descent may be regarded as reasonably certain.

MAXIMA AND MINIMA TIDE-PREDICTING MACHINE.

BY WILLIAM FERRILL.

This machine is merely prospective as yet, and is designed to indicate, by means of indices on its face, the times and heights of high and low water for any tide station. These have been determined heretofore by means of laborious computations. The mathematical principles upon which the proposed machine is based, and also the internal structure of the machine, are both very complex, and no idea of them can be conveniently given here. The face of the machine is to be 20 inches by 16 inches, and the depth of the case 6 or 8 inches. The face contains an hour circle 10 inches in diameter, and a lunar and solar index turning around the same centre with slightly different velocities, the one pointing out the lunar time and the other the solar time elapsed from the time of an assumed epoch, as the first of January. There is also an index moving vertically, indicating the heights of high and low water. The machine is designed to stand upon a desk, and the power is the left hand applied to a crank on the side, leaving the right hand free to record the result as read from the face of the machine. The crank is turned until the lunar index comes in conjunction with the upper or positive end of a needle, also in motion, when the solar index indicates the time of high water and the vertically moving index the height of high water. The same for low water when the lower index comes in conjunction with the negative or lower end of the needle. This is continued from high to low and from low to high water and from day to day, the result being recorded as read off. The mechanical difficulties in the construction of the machine are very great, but not considered insuperable.

ON THE DEFICIENCIES OF METEOROLOGICAL WORK IN DATA OF VALUE TO AGRICULTURE, AND MEANS FOR SUPPLYING THEM.

By WILLIAM MCMURTRIE.

Meteorological records, as they are and have been and are being made, are deficient in many of those data which have the most important influence upon farm crops. Temperatures are recorded, but they are always observed in the shade. Rainfall is given, but often in such a way as to render its record of no value in the study of the development and condition of crops, because no indication is given as to the way in which it is distributed ; light being of little importance to meteorologists generally, while it is one of the most potent factors in the development of vegetable and animal life, has been almost completely ignored. Late investigations have proven conclusively the importance of the tension of atmospheric electricity upon vegetation, and it should be regularly observed and recorded. In fact, meteorologists have principally confined themselves to the record and study of such conditions as enable them to predict the approach and occurrence of storms, thus looking more to the commercial than to the cultural side. Gasparin was the first to call attention to the importance of the relations of Meteorology to agriculture, and he has had at least two active followers—Quetelet in Belgium, and Marie Davy in France. Through the instrumentality of the latter there has been established, near Paris, an observatory of Agricultural Meteorology, where observation and record of all the conditions above named is made. The results already obtained have shown great practical value, and worthy of the means and labor required in securing them. In this country we have nothing similar to it. Our Signal Bureau, as nearly perfect as may be for the purposes for which it was designed, is devoted to the record and study of those observations as will render possible the prediction of future conditions which may affect human affairs, than such as may influence the development of crops. Besides this, the number of stations at which observations are made in this country is too limited, being not over 800, while for agricultural work 3,000 would not be excessive. Additional work should, therefore, be carried on, and observations at a larger number of stations made and recorded, to be discussed in connection with the records of observations made upon the condition of the crops. The nature of the work is such that it should be undertaken by the Department of Agriculture, and the organization of the latter with the 2,300 reporters it already employed would be well adapted to it. Fortunately, General Le Duc, the Commissioner of agriculture, is in favor of the establishment of such work in the Department, but will require congressional support to enable him to do so. The plan of work suggested by the author is as follows: 1. The establishment of a system of observation and record among the reporters of the Department of Agriculture, and others whose co-operation could be secured throughout the United States and Territories, with instructions to observers to keep careful records of the conditions of atmospheric pressure, temperature in its various relations, relative humidity, evaporation of moisture, winds, light, tension of atmospheric electricity, occurrence of dews, fogs and frosts, and report them at stated intervals of time to the Department for consideration and permanent record. I. The collection of meteorological records from every part of the world, from which to construct detailed tables showing the relations of all the conditions named above, and may influence the growth and health of vegetation. 3. The construction of maps showing the geographical distribution of crops, to be used in connection with the meteorological or climatic data to be collected.

PRELIMINARY ACCOUNT OF A SPECULATIVE AND PRACTICAL SEARCH FOR A TRANS-NEPTUNIAN PLANET.

By D. P. TODD, M. A., Assistant in the Office of the American Ephemeris and Nautical Almanac.

So early as the year 1834, HANSEN was credited with expression of the opinion, in correspondence with the elder BOUVARD, that a single exterior planet would not account for the differences between the tabular and observed longitudes of the planet Uranus. Dr. GOULD, however, in his "Report on the History of the Discoveryof Neptune," says: "I have the authority of that eminent astromener himself (HANSEN) for stating, that the assertion must have been founded on some misapprehension, as he is confident of never having expressed or entertained that belief."

Professor PEIRCE's criticism of the investigations of LE VERRIER, to the effect that his predicted orbit of Neptune was so widely discordant from its observed orbit as to indicate that his computations did not pertain to the actual disturbing planet, elicited from him the reply that the perturbations of Uranus due to a possible planet exterior to Neptune might readily cause an uncertainty of $5''$ to $7''$ in the fundamental data of his research.

In 1866, the Smithsonian Institution published the general tables of Neptune, by Professor Newcomb. In the investigation of its orbit the author proposed: "3. To inquire whether those motions [of Neptune] indicate the action of an extra-Neptunian planet, or throw any light on the question of the existence of such a planet." He concludes (page 73) that it is "almost vain to hope for the detection of an extra-Neptunian planet from the motions of Neptune before the close of the present century."

In 1873, the Smithsonian Institution published the general tables of Uranus, by Professor Newcomb. His success in the treatment of the theory of Uranus was such that astronomers generally may be said to have been satisfied from the smallness of the longitude-residuals, that there existed no evidence of perturbative action upon Uranus other than that actually taken into account in the construction of the tables. It is well known, however, that since the publication of these tables the error of longitude has been increasing.

Sometime in the spring of 1874, the first preliminary outline of the very simple method which I have here employed in the treatment of planetary residuals with reference to exterior perturbation, suggested itself to me. For more than three years very little opportunity offered for consideration of the problem of a trans-Neptunian planet. In August, 1877, however, I began to devote the larger portion of my leisure time to the theoretic side of the question. It was soon evident that no certain hold upon any possible cause of exterior perturbation could be obtained from the residuals of Newcomb's tables. And I may remark here that I have consequently chosen the term *speculative* rather than *theoretic* as applying more fitly to the investigation which preceded the actual telescopic search.

It did not seem to me that the magnificent researches of Le Verrier and Adams on the perturbations of Uranus should be taken as models in the present investigations, for two reasons :

(1) The residuals of longitude which must form the basis of the investigation are not sufficiently well marked to justify the execution of so laborious a research, especially if it be found that a simple, rational treatment, unencumbered with the refinements of analysis, may be fairly interpreted as indicating the position of an exterior perturbing body with merely a rough approximation.

(2) Even in the case of Uranus, and the theoretic search for Neptune, where the residuals of longitude were very strongly marked, many of the elements pertaining to the disturbing planet, which Adams and Le Verrier sought to determine theoretically, turned out afterward, when their real values became known, to have been indicated with only meagre precision. Much less should we now expect these elements to be given with any certainty in the case of a planet exterior to Venus.

This provisional treatment of the residuals of Uranus was undertaken, then, as a preliminary to the proposed telescopic search to determine whether that search was worth undertaking, and if so, at what point approximately it was best to begin.

I.—We now consider, *seriatim*, the errors of the elements of the perturbed planet—errors which the very hypothesis of a disturbing body introduces, and which must have entered into the tables of the inferior planet, as constructed independently of unknown exterior perturbation. We consider what the effect of these errors may be, and how far it may be eliminated or subtracted from the residuals of the actual theory of the planet. These residuals are, of course, first corrected for any known error of theory or tables, or erroneous masses of known perturbing planets.

(1) *The error of mean distance of the perturbed planet.*—Any error of radius vector enters very largely into the residuals of heliocentric longitude, if the observations are made at any considerable intervals from the planet's opposition. If it is suspected that the error of radius vector will vitiate the residuals of longitude, we may avoid its effect by passing to residuals of geocentric longitude. Or we may confine our research to the mean residuals of observations near the opposition points, and symmetrically placed with reference thereto. The effect of erroneous radius vector is thereby eliminated.

(2.) *The error of periodic time of the perturbed planet.*—If the residuals are examined graphically, the eye will readily detect whether any correction to the periodic time is advisable. If, in general, the mean line of the residuals is nearly a right line, and makes a given angle with the line of zero-residual, it may fairly be concluded that the residuals need a correction depending directly on the time, the magnitude of the co-efficient of which is indicated by the divergence of the two residual-lines.

I had considered the problem only thus far when it occurred to me to apply the method, only partially developed, to the determination of an approximate position of Neptune from the residuals of Bouvard's Tables of Uranus, published in 1821. Taking also the residuals from observations up to 1824, and not permitting myself a knowledge of the longitude of Neptune at any epoch, a very little labor gave me an approximate position of the disturbing planet from which, it now appears, Neptune might easily have been found some twenty years in advance of its actual discovery.

When my work had advanced to this stage, a mere chance threw in my way a copy of Sir John Herschel's *Outlines of Astronomy*, (which I had never before examined): I at once observed that my treatment of the residuals of Uranus with reference to a planet exterior to Neptune was quite similar to his "dynamical" exposition of the perturbations of Uranus arising from Neptune itself. And I was farther gratified to find that he had given a very full and lucid statement of the effect upon the longitude-residuals caused by errors of the third and forth elements of the perturbed planet—the error of eccentricity, and the error of longitude of perihelion.

(3.) *The error of eccentricity of the perturbed planet.*—(See Sir John Herschel's *Outlines of Astronomy*, page 536.

(4.) *The error of longitude of perihelion of the perturbed planet.*—(Ibid., page 537.)

When the longitude-residuals have been corrected in this manner, we proceed on the assumption that any outstanding residuals are due to unexplained exterior perturbation.

II.—Of the seven elements of the disturbing planet, we must assume a value of one: the values of three others, together with the mass of the disturbing planet, we may consider as theoretically determinable from the longitude-residuals themselves.

(1.) *The mean distance of the disturbing planet.*—Regarding the next order of distance beyond Neptune as occupied by the planet for which we are searching, I assumed, as a first value of mean distance, $a = 46.0$: this value seemed to be indicated by a fair induction. The periodic time of the planet would then be 312 years, and conjunctions with Uranus would occur nearly at intervals of 115 years.

(2.) *The eccentricity of the disturbing planet.*—Even with the large residuals of Uranus employed in the investigations of Le Verrier and Adams, the derived value of the eccentricity of Neptune was entirely illusory. The several values of eccentricity of Neptune resulting from their investigations are as follows:

Adams (*first hypothesis*)	0.16103
Le Verrier	0.10761
Adams (*second hypothesis*)	0.120615

The eccentricity given by investigation of the orbit of Neptune from observations of the planet was:

Newcomb (*Tables of Neptune*)	0.0089903

We should, therefore, expect nothing of any attempt to arrive at the eccentricity of an orbit exterior to that of Neptune.

(3.) *The longitude of perihelion of the disturbing planet.*—Much the same remark obtains in reference to this element. The several values of longitude of perihelion of Neptune, resulting from the researches in perturbations of Uranus, are as follows:

Adams (*first hypothesis*)	315° 57′
Le Verrier	284° 45′
Adams (*second hypothesis*)	299° 11′

The longitude of perihelion given by observations of the planet is:

Newcomb (*Tables of Neptune*)	46° 6′ 39″ .7

Evidently it would not be wise to include this element in the investigation.

(4.) *The epoch of the disturbing planet.*—If we can obtain even a rough approximation to the value of this element, the end of the investigation is fully attained. An inspection of the outstanding residuals, graphically exhibited, will show, without further labor, the epochs of maximum disturbance. We may prepare an approximate perturbative curve, the epochs of maximum disturbance of which shall be in harmony with the assumption of mean distance of the exterior planet. By applying this to the plot of outstanding residuals, we may decide at what points the application of the perturbative curve best accounts for them. The amount of excursion in its several sinuses we need not, for this purpose, attend to with any great care: this will depend upon the mass and distance of the disturbing planet; and, that it will be unavailing to attempt any determination of the mass in the present case will be evident from the fact that the mass of Neptune, from the theoretical investigations of LeVerrier and Adams, was widely discrepant:

Adams (*first hypothesis*)	0.0001656	$\frac{1}{6039}$
Le Verrier	0.0001075	$\frac{1}{9300}$
Adams (*second hypothesis*)	0.00015003	$\frac{1}{6666}$

While the most reliable mass of Neptune from observation was:

Newcomb (*motion of the satellite*)	0.00005160	$\frac{1}{19380}$

We thus have the inverse problem of perturbation reduced to a very simple rational form. The residuals of longitude of Uranus were next treated in accordance with this method.

In his *Investigation of the Orbit of Uranus*, Newcomb presents three series of residuals; the mass of Neptune finally adopted in the tables, $\frac{1}{19700}$, corresponds very nearly to the mean of the first and third series. But the mass of Neptune which was employed in this investigation is that given by Newcomb's discussion of the motion of the satellite of Neptune, and is $\frac{1}{19380}$. Our first step, then, was to correct these mean residuals into accordance with this adopted mass.

Afterward, examining these corrected residuals according to the method just related, in reference to unexplained perturbing action, I concluded that Uranus was in conjunction with an exterior perturbing body between the years 1780 and 1795, and that another conjunction would take place at some time before the close of the present century. The most probable position of the exterior planet I therefore considered to be about 170° of longitude; the probable error of the position I considered roughly 10°. This result was reached on the morning of the 10th of October, 1877. During the few days immediately following I reviewed this examination, as much as possible independently of the previous result, and at the same time varying the assumed mean distance. With a value of $a = 52.0$ (which I finally considered inductively the most probable) I set down the longitude of the exterior planet equal to 162° ± 6°. This result was reached on the evening of the 14th of October. I now turned my attention toward a similar treatment of the residuals of Neptune, with a slight hope of getting a confirmatory result. Two suppositions agreed in fixing the longitude at about 180° to 200°, respectively. I therefore, on some day in the latter part of

October, 1877, wrote down as the exposition of all my inquiry the following results:

EXTERIOR PLANET.—Longitude (1877.84), 170° ± 10°.
Mean distance from the sun, 52.0.
Period of revolution about the sun, 375 years.
Mean and daily motion, 9''.46.
Angular diameter, 2''.1.
Stellar magnitude, 13+.
Longitude of ascending node, 103°.
Inclination of orbit to ecliptic, 1° 24'.

If a new disturbing planet exists in the longitude here indicated, nearly a century must elapse before its existence can be asserted at all positively from the residuals of Neptune alone.

I should never have been able to execute the telescopic search consequent upon the investigation just related, had it not been for the courteous offices of Rear Admiral Rogers, Superintendent of the Naval Observatory, and Professor Hall, in charge of the great refractor. It was with this instrument—the 26-inch equatorial—that the search was conducted. It seemed to me that I should begin the search at a point about 20° preceding that indicated as the most probable position of the planet, and continue it to a point following by the same distance. But, a careful search extending over a zone of this length, and of sufficient width to be certain to contain the supposable planet, would be a work of such magnitude that I could not expect its completion under several years. I therefore had recourse to an inductive determination of the inclination and longitude of node of the planet's orbit.

I computed anew the position of the invariable plane of the solar system. A differential comparison of its inclination with the inclination of the orbits of the major planets, gave, with little uncertainty so far as the mere induction was concerned, the inclination of the orbit of the trans-Neptunian planet equal to 1° 24'. Similarly I obtained for the longitude of node, though not so certainly, 103°. For the preliminary search I determined to fix the latitude-limits of the zone at a width of one degree to the north and one degree to the south of this adopted plane. To these elements I strictly adhered, with the intention, however, of alternately increasing and decreasing the inclination, and varying the longitude of node if I should arrive at no successful result from the search of this limited zone.

I may remark that the detailed plan of the instrumental search had been completely digested and written out as early as the 5th of September. To assist in a decision as to what method of search I should employ, I had recourse to an inductive consideration of the real diameters of the known planets of the solar system. I arrived at the result that a diameter of 50,000 miles might be taken as the minimum value for a planet next beyond Neptune. On this assumption, the mean distance of 52·0 gave for its apparent diameter 2''.1. I did not, therefore, hesitate in adopting the method of search depending upon the detection of the planet by contrast of its disk and light with the appearance of an average star of about the thirteenth magnitude. In the actual search, a power of 600 was often employed, but most of the search was conducted with a power of 400 diameters.

On thirty clear, moonless nights, between the 3d of November, 1877, and the 5th of March, 1878, this search was carried on after the manner I have indicated.

After the first few nights I was surprised at the readiness with which my eye detected any variation from the average appearance of a star of a given faint magnitude: as a consequence whereof my observing book contains a large stock of memoranda of suspected objects. My general plan with these was to observe with a sufficient degree of accuracy all suspected objects. On the succeeding night of observation these objects were re-observed; and, at an interval of several weeks thereafter this observation was again verified. At 3 A. M., the 6th of March, 1878, the search was discontinued—my observing book ends with the following note:

"The adopted plane of orbit of trans-neptunian planet is now searched (without break) from

v=146.8°
to v=186.1.°"

I have much confidence in this telescopic search—my aim was to sweep the zone so carefully that there should be no pressing need of duplicating it.

I ought not to conclude this paper without adverting to the apparently long delay of its publication. From the very beginning I had approached the entire problem of search for a trans-neptunian planet with resolute direction toward the end which I regarded of the highest scientific import—that of *finding the possible planet at the earliest moment;* if I were successful, observations of its position would then be secured at once, and an accurate determination of its elements would be a matter of earlier realization —it seeming improbable that any prior chance observation would ever be brought to light. After pursuing the theoretic side of the question for a short time, I saw clearly that many years must elapse before the perturbing action of this body on any interior planet would afford anything like pronounced evidence of its existence; recourse must be had to the practical telescopic search. So I tarried longer with the residuals of Uranus only in the hope of a possible shortening of the search by some indication that the planet was more probably in one portion of the heavens than in another. After the telescopic search, which I was conducting, had been temporarily brought to an end, by circumstances beyond my control, I was not without hope of effecting some arrangement whereby I might resume the search at an early day, and carry it to a satisfactory conclusion. After much thought upon the apathetic reception with which the magnificent researches of Adams and LeVerrier had met, I reached the conclusion that no competent observer would be led to continue the search through knowledge of the little work of speculation that I had done. And, as the work was undertaken with the end always in view of finding the planet, it did not appear that any advantage would result from its publication.

It will be remarked that this matter now assumes a very different aspect: the publication of a recent memoir *On Comets and Ultra-Neptunian Planets*, by Professor George Forbes, of Glasgow, assigns, by a method of investigation entirely independent of my own, a position to a possible trans-neptunian planet which may be regarded as in exact coincidence with that which I have deduced. The assumption of a mean distance 100, indicated in Professor Forbes' paper, will not appreciably destroy the representation of the residuals with which I have dealt. I have not yet been able to convince myself that the remarkable harmony of the results of the two investigations is simply a chance agreement; and, with the hope that the accumulated evidence of the existence of a far exterior planet may not fail to incite some observer in possession of sufficiently powerful telescopic means to a vigorous prosecution of the search, I have prepared this preliminary paper in order that attention may be called to the matter in sufficient advance of the opposition-time now approaching. I may add here, that, should a careful and protracted search of the region adjacent to the indicated longitude prove unavailing, no more certain test of the existence of a trans-neptunian planet admits of application within the next few years than that of telescopic search of a limited zone extending entirely around the heavens—a search which I have been hoping, for more than two years past, for an opportunity to undertake, but which I see no present prospect of realizing.

NAUTICAL ALMANAC OFFICE, Washington, August 4, 1880.

IN the province of Keen-chang, China, $15,000,000 worth of a peculiar vegetable wax is annually produced. It is formed on the twigs of an evergreen tree (Ligustrum lucidum), whose oval leaves furnish homes for myriads of insects. These, during the spring, produce a thin skin over the leaves, from which exudes a waxy substance that hardens in the month of August. The twigs are then cut and boiled in water, by which means the purified wax is easily separated.

AN IMPROVED ELECTRO-MOTOR.*

By THEODORE WIESENDANGER.

While recently many minds have been at work, with more or less of success, to produce improvements in dynamo-generators of electric energy, very few have given their special care and attention to the development of the electro-motor. Experience has taught us hitherto that the efficiencies of one and the same machine for action and reaction, or for use either as a generator, or by the inverted process as an electro-motor, stand in a certain and direct proportion to each other, or that our most efficient generators, such as the Siemens, Brush and Gramme, machines prove also the most effective motors, and on the other hand that inferior dynamo-machines invariably are inefficient motors. It would, however, be hazardous to conclude from these results that this rule should hold good for all future machines, and from the results of researches I have recently made, I come to the conclusion that the motors which are to supersede those now in use could not be employed as generators. Dynamo-machines, such as now constructed, only prove efficient when their field-magnets are able to retain at all times (*e. g.*, even when the machine stands at rest) a certain and very considerable amount of residual magnetism, and for that reason their cores are made of retentive material, hard cast iron, as is the case in the Brush and Gramme machines ; or if the cores consist of soft iron they are attached to large masses of hard cast iron. in such a manner that the latter are inclosed in the magnetic circuit, and form part of the cores.

Generators of the same kind, when made small in size, have cores much larger and heavier in proportion, and, moreover, the baseplate, or, as in the Weston machine, a heavy retentive cylinder, is made to form a portion of the field-magnets. But all efforts hitherto made to produce efficient small dynamo-machines with cores of soft iron only, have resulted in absolute failure, although men of the highest genius have made repeated and prolonged efforts to solve that most difficult of problems.

These curious facts conclusively prove that the theory explanatory of the action of dynamo-machines, as now universally adopted, viz., the theory of inductive action and reaction between the field-magnets and the armature, cannot any longer be considered complete or satisfactory ; for even wrought iron, especially when occurring in large masses, always contains an appreciable amount of residual magnetism, more especially after it has once been subjected to strong magnetization, and if the above theory were correct and complete, then the smallest possible amount of residual magnetic energy, augmented by repeated action and reaction, would be sufficient for the starting of such a machine to action. This, however, experience proves *not* to be the case, and the theory, although stoutly adhered to, must be either abandoned or amended.

The inventors of the most recent electro-motive engines have worked—perhaps unconsciously—upon the idea that the construction and action of electro-motors are based altogether upon the same laws as those of dynamo or magneto machines, and, in accordance with that assumption, the field-magnets of the Desprez motor are made to consist of large and heavy masses of magnetized steel.

Experimenters have also for a long time past clung to the idea that the efficiency of an electro-motor—or the amount of energy to be obtained from such a machine by means of a current of given strength circulating in the coils of its armature only—bears a definite and direct proportion to the magneto-inductive power of its field-magnets, and that an increase of power in the field-magnets alone must necessarily produce greater capabilities of the machine.

This, however, is a mischievous theory, because erroneous in its very principles, and development would only lead to the hypothesis of perpetual motion. On the contrary, starting from the consideration of the fact that a very small magnetic needle, if acted upon by one of the poles of another and very powerful magnet, has its polarity destroyed or reversed, and that if one of its poles, say the N pole, is presented to a similar (N) pole of the large magnet, the former will instantly lose its characteristic qualities and be attracted by its overpowering opponent, we can only come to the one rational conclusion that the power or the field-magnets of an electro-motor, as compared to that of the magnet or magnets constituting its armature, should not surpass the limit of some certain ratio, to be determined yet by experiments carefully conducted, and that, if it surpasses the limit, the capabilities of the machine must be impaired. Acting on this principle, I have constructed a motor in which the power of the field magnets is as nearly as possible equal to that of an armature, the core of the former being very light and made entirely of soft iron ; and the satisfactory results obtained from this machine are a sure sign that further investigation of the subject and experiments made with a view of determining the exact ratio of power between the magnets and armature will result in further improvement.

Another and very important consideration in the construction of dynamo-machines and electro-motors has not yet received that care and attention from scientific investigators which would lead to immediate progress. It is the method of motion of the revolving armature with regard to its approaching to, or receding from the poles of the field-magnets. In nearly all the machines now constructed the polar faces of the cores of the field-magnets and those of the armature are of such a shape, and the latter is caused to revolve in such a manner, that only in a small portion of the revolution its poles either approach the poles of the field-magnets or recede from them. But the most successful production of induced currents will be achieved, and the greatest amount of power will be derived from a motor, if attention is paid not merely to the *one* condition, that the armature should revolve in the most highly concentrated magnetic field possible, but also that nearly the entire motion of the revolving armature should be either one of approach or of withdrawal. Let us first of all consider the case of a machine with two poles only of field-magnets and two poles of the revolving armature.

It is usual to give the active faces of the former such a shape that a section of the same represents a portion of a true circle. See Fig. 7.

In the ordinary machines now in use the radius of the circle described by the outline of the revolving armature, and that of the larger circle described in portion by the section of the inner or active faces of the poles are nearly the same, and the two circles are concentric. (See Fig. 7.) The pole *g* of the armature only approaches the pole A of the field-magnets while moving from *c* to *d*, or where the intensity of the magnetic field of A is at its minimum. When continuing its motion from *d* to *r* and to *f*, the pole *g* can no longer be said to approach A, because the distance between the respective surfaces remains constant.

I therefore propose that the devices shown in Figs. 9 and 6 should be adopted. The radius of the circle, part of which is formed by the section *d*, *r*, *c*, is considerably larger than that of the circle described by the outline of the field of motion of the armature ; *d*, *r*, *c* is, moreover, considerably less than the half of a circle and the three circles *d*, *r*, *c*, *f*, *e*, *h*, and that described by the outline of the field of motion of the armature are not concentric. The pole *g* of the armature, when in motion, approaches the pole, A, not only in its course from *e* to C, but also when in the most intense magnetic field of A, viz., whilst moving from C to *z* and *d*. Fig. 11 represents a section of the field-magnet's cones E F and G H, and pole's pieces N and S cast in two halves and mounted on a base board, to which they are fixed by the two bolts R and T. The same principles may be applied to machines with field-magnets of more than two poles (see Fig. 3) ; or the armature itself may be made of such a shape as to work under the conditions above stated (Fig. 4). But even if the poles of the armature and those of the field-magnets are of the ordinary shape, a machine with more magnets will be more perfect in its action than one with two poles only. Fig. 10 illustrates a machine in which the armature during nearly the whole of its motion either approaches to or recedes from

*A paper read before the British Association.

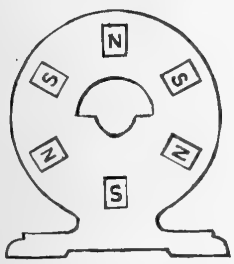

Fig. 1.

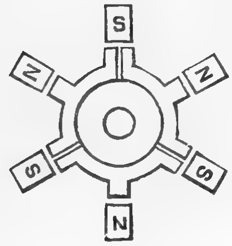

Fig. 2.

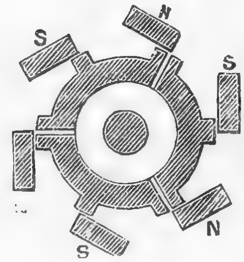

Fig. 3.


Fig. 4.

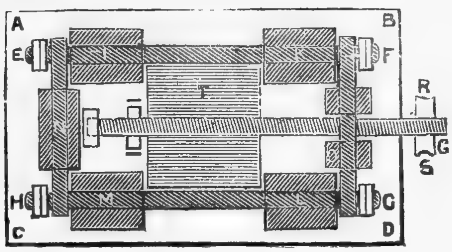

Fig. 5.


Fig. 6.

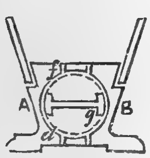

Fig. 7.

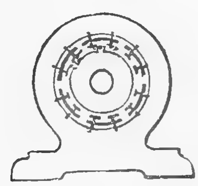
Fig. 8.

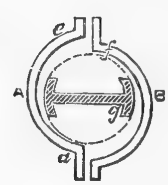

Fig. 9.

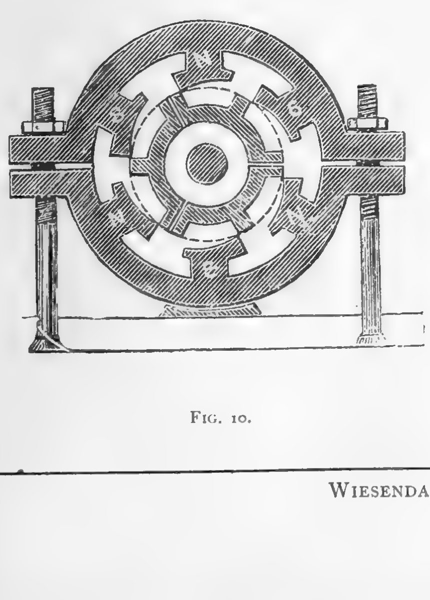
Fig. 10.

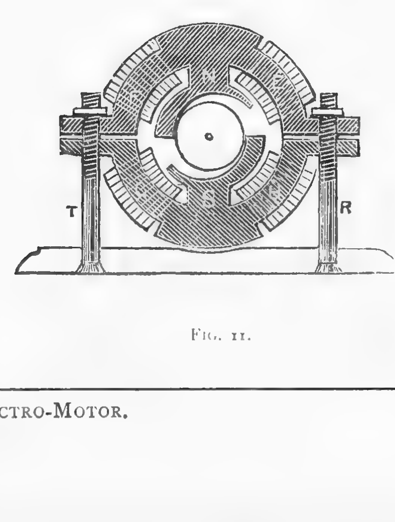

Fig. 11.

Wiesendanger's Improved Electro-Motor.

the pole of the field-magnets. In such machines the motion of the poles of the armature is also more in a line, coincident with the line of attraction as exercised between the two systems of poles ; while in machines with field-magnets of two poles only the motion of the poles of the armature is at times at angles of 45 degrees to one degree from the direct pull.

I may, perhaps, be allowed to call attention to another matter of importance, awaiting further research. We find that in the three types of dynamo-machines, as constructed by Siemens, Gramme and Wilde, the relative positions of the axes of the field-magnets and those of the armatures are altogether different. Yet the three systems work well. We are unable, however, to state with certainty which positions of the axes are the best, or why any one of these positions should be better than the others, and in the face of experience, the theory of tubes or lines of force is little more than a hypothesis, with all its diffusion, vagueness and uncertainty.

Having so far considered general principles chiefly, I now beg to describe this motor (Fig. 5). A B C D is a wooden baseboard, E F G H a frame, consisting of the two parallel round rods F E and G H, and the two flat bars F G and E H, made of the best wrought iron, and carefully softened. The four bars are screwed together at the corners, and supported by four brass brackets over the baseboard. These inner rods form the compound core of the field magnets, a combination, as it were, of two horseshoe magnets, whose similar poles (S S and N N) form the junctions. Thus we have practically two poles only, a S and a N pole. Six coils of insulated copper wire are wound over the different portions of this core, shown in the drawing ; the active pole-pieces are left exposed for a long distance, bearing no coils, The spindle P L, which carries a Siemens armature of the old form, or an armature with a compound tubular core ; the commutator and pulley traverses the flat crossbar F H. The core of the armature is made of sheets of charcoal-iron, and it bears a coil of stout insulated copper wire. The commutator is of the ordinary kind, consisting of two half-tubes of brass, insulated from each other and from the spindle, and each forming one of the terminals of the coil. Fig. 2 represents a sectional view of a compound machine, acting on the same principles ; Fig. 1 is a view of the two-end castings which hold the field-magnet. This machine contains a system of six field-magnets and six poles, and a compound armature with six poles. The current is to be reversed six times for each revolution, and to accomplish this I have devised the following commutator (see Fig. 8) : —In these machines, also, the poles of the field-magnets or those of the armature may be of such a shape as to be nearly always approaching to, or receding from, each other, while in active motion,

The development of most important machines is destined to reach a certain stage of perfection, when further improvements cannot be accomplished by the inventor unaided ; the second and important factor needed then is the co-operation of inventive and investigative talent with capital, This stage of perfection has been reached in the steam-engines, gas-engines, printing-machinery, etc., and it may be said to be rapidly approached by the progress made in dynamo-machines and electro-motors.

The development of the latter machines is followed by the scientific world with greater interest, and it evokes more eager expectations than that of other machinery, chiefly because it is not, and cannot be, identified with the solution of a problem limited within the confines of mechanical difficulties and commercial interests, but necessitates a further and deeper investigation into that great and subtle power, electricity, whose manifestations are so striking in their effects, so mysterious in their nature, so promising of great results in an immediate future, so fertile a field of research to the pioneer of science.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. 1880.

The British Association for the Advancement of Science met at Swansea on the 4th of September last, under the presidency of Dr. Ramsey, who took as the subject of his address, "The Recurrence of Certain Geological Phenomena in Geological Time." His object appeared to be to show that all known geological formations have been produced under physical circumstances closely resembling, if not identical, with those with which we are more or less familiar. Through the various geological epochs he traced this identity of operations in respect to the metamorphism of rocks, the products of volcanoes, the upheaving and denudations of mountain chains, the deposit of great *inland areas* of salt, a recurrence of fresh-water conditions in lakes and estuaries, and glacial influences. His conclusion was that from the Laurentian epochs down to the present day all physical events in the history of the earth have varied neither in kind nor in intensity from those of which we now have experience.

The conclusions drawn from this address are summed up in the closing words of Professor Ramsey's discourse, as follows:

"In opening this address, I began with the subject of the oldest metamorphic rocks that I have seen—the Laurentian strata. It is evident to every person who thinks on the subject that their deposition took place far from the beginning of recognized geological time. For there must have been older rocks by the degradation of which they were formed. And if, as some American geologists affirm, there are on that continent metamorphic rocks of more ancient dates than the Laurentian strata, there must have been rocks more ancient still to afford materials for the deposition of these pre-Laurentian strata. Starting with the Laurentian rocks, I have shown that the phenomena of metamorphism of strata have been continued from that date all through the later formations, or groups of formations, down to and including part of the Eocene strata in some parts of the world. In like manner I have shown that ordinary volcanic rocks have been ejected in Silurian, Devonian, Carboniferous, Jurassic, Cretaceo-oolitic, Cretaceous, Eocene, Miocene, and Pliocene times, and from all that I have seen or read of these ancient volcanoes, I have no reason to believe that volcanic forces played a more important part in any period of geological time than they do in this our modern epoch. So, also, mountain chains existed before the deposition of the Silurian rocks, others of later date before the Old Red Sandstone strata were formed, and the chain of the Ural before the deposition of the Permain beds. The last great upheaval of the Alleghany Mountains took place between the close of the formation of the Carboniferous strata of that region and the deposition of the New Red Sandstone. According to Darwin, after various oscillations of level, the Cordillera underwent its chief upheaval after the Cretaceous epoch, and all geologists know that the Alps, the Pyrenees, the Carpathians, the Himalayas, and other mountain chains (which I have named) underwent what seems to have been their chief great upheaval after the deposition of the Eocene strata, while some of them were again lifted up several thousands of feet after the close of the Miocene epoch. The deposition of salts from aqueous solutions in inland lakes and lagoons appears to have taken place through all time—through Silurian, Devonian, Carboniferous, Permian, Triassic, Jurassic, Cretaceous. Eocene, Miocene, and Pliocene epochs—and it is going on now. In like manner fresh-water and estuarine conditions are found now in one region, now in another, throughout all the formations or groups of formations possibly from Silurian times onward ; and glacial phenomena, so far from being confined to what was and is generally still termed *the* Glacial Epoch, are now boldly declared by independent witnesses of known high reputation, to begin with the Cambrian epoch, and to have occurred somewhere, at intervals, in various formations, from almost the earliest Palæozoic times down to our last post-Pliocene "Glacial Epoch."

If the nebular hypothesis of astronomers be true (and I know of no reason why it should be doubted), the earth was at one time in a purely gaseous state, and afterwards in a fluid condition, attended by intense heat. By and by consolidation, due to partial cooling, took place on the surfate, and as radiation of heat went on the outer shell thickened. Radiation still going on, the interior fluid matter decreased in bulk, and, by force of gravitation, the outer shell being drawn towards the interior, gave way, and, in parts, got crinkled up, and this, according to cosmogonists, was the origin of the earliest mountain chains. I make no objection to the hypothesis, which, to say the least, seems to be the best that can be offered, and looks highly probable. But, assuming that it is true, these hypothetical events took place so long before authentic geological history began, as written in the rocks, that the earliest of the physical events to which I have drawn your attention in this address was, to all human apprehension of time, so enormously removed from these early assumed cosmical phenomena, that they appear to me to have been of comparatively quite modern occurrence, and to indicate that from the Laurentian epoch down to the present day, all the physical events in the history of the earth have varied neither in kind nor in intensity from those of which we now have experience. Perhaps many of our British geologists hold similar opinions, but if it be so, it may not be altogether useless to have considered the various subjects separately on which I depend to prove the point I had in view."

MATHEMATICS AND PHYSICS.

The address was delivered by the president, Prof. W. Grylls Adams. In it he dealt with the subject of magnetic disturbances, and pointed out that in many instances the disturbances at the various stations of observations were not precisely alike, showing probably the change of the direction or intensity of the earth's magnetism arising from the solar action upon it. He believed there was a sufficient cause for all our terrestrial magnetic changes, for these masses of metal were ever boiling up from the lower and hotter levels of the sun's atmosphere to the cooler upper regions, where they must again form clouds to throw out their light and heat, and to absorb the light and heat coming from the hotter lower regions; then they became condensed and were drawn again back towards the body of the sun, so forming those remarkable dark spaces or sunspots by their down-rush towards the lower levels. In these vast changes, which we know from the science of energy must be taking place, but of the vastness of which we can have no conception, we have abundant cause for the magnetic changes which we observe at the same instant at distant points on the surface of the earth, and the same cause acting by induction on the magnetic matter within and on the earth may well produce changes in the direction of its total magnetic force, and alter the direction of its magnetic axis. These magnetic changes on the earth will influence the declination needles at different places, and will cause them to be deflected. The direction of the deflection must depend on the situation of the earth's magnetic axis, or the direction of its motion with regard to the stations where the observations are made. Thus, both directly and indirectly, we find in the sun not only the cause of diurnal magnetic variations, but also the cause of these remarkable magnetic changes and disturbances over the surface of the earth.

CHEMISTRY.

The address was delivered by the president, Dr. J. H. Gilbert, F.R.S., who referred mainly to the subject of agricultural chemistry, and in the course of his remarks said, referring to the assimilation of carbon, that the whole tendency of observations was to conform to the opinion put forward by De Saussure about the commencement of the century, and so forcibly insisted upon by Liebig, forty years later, that the greater part, if not the whole, of the carbon, was derived from the carbonic acid of the atmosphere. Judging from more recent researches, it would seem probable that the estimate of one part of carbon or carbonic acid in 10,000 of air was more probably too high than too low as an estimate of the average quantity in the atmosphere of our globe. Large as was the annual accumulation of carbon from the atmosphere over a given area, it was obvious that the quantity must vary exceedingly with the variation of climatical conditions. It was, in fact, several times as great in the case of the tropical vegetation—that of the sugar-cane, for instance. And not only was the greater part of the assimilation accomplished within a comparatively small portion of the year, but the action was limited to the hours of daylight, whilst during darkness there was rather loss than gain. In a general sense it might be said that the success of the cultivator might be measured by the amount of carbon he succeeded in accumulating in his crops. And as the amount of carbon accumulated depended on the supply of nitrogen in an available form within the reach of plants, it was obvious that the question of the sources of the nitrogen of vegetation was one of first importance. The result of experiments that had been conducted went to prove—first, that without nitrogenous manure, the gramineous crops annually yielded, for many years in succession, much more nitrogen over a given area than was accounted for by the amount of combined nitrogen annually coming down in the measured aqueous deposits from the atmosphere; second, the roots yielded more nitrogen than the cereal crops, and the leguminous crops much more still; and third, that in all cases—whether of cereal crops, root crops, leguminous crops, or a rotation of crops—the decline in the annual yield of nitrogen, when one was supplied, was very great. The next point referred to was the condition of the nitrogen in our various crops. They could not say that the whole of the nitrogen in the seeds with which they had to deal existed as albuminoids. But they might safely assume that the nearer they approached to perfect ripeness, the less of non-albuminoid nitrogenous matters would they contain; and in the case of the cereal grains, at any rate, it was possible that if really perfectly ripe, they would contain very nearly the whole of their nitrogen as albuminoids.

GEOLOGY.

The address was delivered by the President, H. C. Sorly, LL.D., F.R.S., who took for his subject the comparative structure of artificial slags and eruptive rocks. His conclusions may be thus summed up:

The objects I have described may be conveniently separated into three well-marked groups, viz.: artificial slags, volcanic rocks and granite rocks. My own specimens all show perfectly well-marked and characteristic structures though they are connected, in some cases, by intermediate varieties. Possibly, such connecting links might be more pronounced in other specimens that have not come under my notice. In any case, the facts seem abundantly sufficient to prove that there must be some active cause for such a common, if not general, difference in the structural character of these three different types. The supposition is so simple and attractive, that I feel very much tempted to suggest that this difference is due to the presence or absence of water as a gas or as a liquid. In the case of slags it is not present in any form. Considering how large an amount of steam is given off from erupted lavas, and that, as a rule, no fluid cavities occur in the constituent minerals, it appears to me very plausible to suppose that those structures which are specially characteristic of volcanic rocks are, in great measure, if not entirely, due to the presence of associated or dissolved vapor. The fluid cavities prove that water was sometimes, if not always, present as a liquid during the consolidation of granitic rocks, and we can scarcely hesitate to conclude that it must have had very considerable influence on the rock during consolidation. Still, though these three extreme types appear to be thus characterized by the absence of water, or by its presence in a state of vapor or liquid, I think we are scarcely in a position to say that this difference in the conditions is more than a plausible explanation of the differences in their structure. Confining our attention to the more important crystalline constituents which are common to the different types, we may say that that the chief structural characters of the crystals are as follows: (*a*) Skeleton crystals, (*b*) Fan-shaped groups, (*c*) Glass cavities, (*d*) Simple crystals, (*e*) Fluid cavities. These different structural characters are found combined in different ways in the different natural and artificial products, and for simplicity I will refer to them by means of the affixed letters. The type of the artificial products of fusion may generally be expressed by $a + b$ or $b + c$; that is to say, it is characterized by skeleton crystals and fan-shaped groups, or by fan-shaped groups and glass cavities. In like manner the volcanic group may be expressed occasionally by $b + c$, but generally by $c + d$, and the granitic by $d + e$. These relations will be more apparent if given in the form of a table as follows:

Slag type........	$a + b$	$b + c$		
Volcanic type.. ..		$b + c$	$c + d$	
Granitic type				$d + e$

Hence it will be seen that there is a gradual passage from one type to the other by the disappearance of one character and the appearance of another, certain characters in the meanwhile remaining common, so that there is no sudden break, but an overlapping of structural characteristics. It is, I think, satisfactory to find that, when erupted rocks are examined from such a new and independent point of view, the general conclusions to which I have been led are so completely in accord with those arrived at by other methods of study.

ANATOMY AND PHYSIOLOGY.

The address was delivered by Mr. F. M. Balfour, F.R.S., one of the vice-presidents of the section, who observed that in the spring of the present year Prof. Huxley delivered an address at the Royal Institution, to which he gave the felicitous title of "The Coming of Age of the Origin of Species." It was, as Prof. Huxley pointed out, twenty-one years since Mr. Darwin's great work was published, and the present occasion, Mr. Balfour remarked, was an appropriate one to review the effect which it had had on the progress of biological knowledge. There was, he might venture to say, no department of Biology the growth of which has not been profoundly influenced by the Darwinian theory. When Messrs. Darwin and Wallace first enunciated their views to the scientific world, the facts they brought forward seemed to many naturalists insufficient to substantiate their far-reaching conclusions. Since that time an overwhelming mass of evidence has, however, been rapidly accumulating in their favor. Facts which at first appeared to be opposed to their theories have one by one been shown to afford striking proofs of their truth. There are at the present time but few naturalists who do not accept in the main the Darwinian theory, and even some of those who reject many of Darwin's explanations still accept the fundamental position, that all animals are descended from the common stock. To attempt in the time at his disposal to trace the influence of the Darwinian theory on all the branches of anatomy and physiology would be wholly impossible, and he would confine himself to an attempt to do so for a small section only. There was perhaps no department of Biology which had been so revolutionized by the theory of animal evolution as that of development or Embryology. The reason of this is not far to seek. According to the Darwinian theory, the present order of the organic world has been caused by the action of two laws, known as the laws of heredity and of variation. The law of heredity is familiarly exemplified by the well-known fact that offspring resemble their parents. Not only, however, do the offspring belong to the same species as their parents, but they inherit the individual peculiarities of their parents. It is on this that the breeders of cattle depend, and it is a fact of every-day experience amongst ourselves. A further point with reference to heredity to which he must call their attention was the fact that the characteristics which display themselves at some special period in the life of the parent are acquired by the offspring at a corresponding period. Thus, in many birds the males have a special plumage in the adult state. The male offspring is not, however, born with the adult plumage, but only acquires it when it becomes adult. The law of variation is, in a certain sense, opposed to the law of heredity. It asserts that the resemblance which offspring bear to their parents is never exact. The contradiction between the two laws is only apparent. All variations and modifications in an organism are directly or indirectly due to its environments; that is to say, they are rather produced by some direct influence acting upon the organism itself, or by some more subtle and mysterious action on its parents; and the law of heredity really asserts that the offspring and parent would resemble each other if their environments were the same. Since, however, this is never the case, the offspring always differ to some extent from the parents. Now, according to the law of heredity, every acquired variation tends to be inherited, so that, by a summation of small changes, the animals may come to differ from their parent stock to an indefinite extent. Mr. Balfour then referred to what he spoke of as a concrete example of the application of these two laws, his object being to demonstrate how completely modern embryological naming is dependent on inheritance and variation, which constitute the keystones of the Darwinian theory. He maintained that "The Origin of Species" afforded explanations of important embryological facts, and added that no explanation, for instance, could be offered of the fact that a frog in the course of its growth has a stage in which it breathes like a fish, and then why it is like a newt with a long tail, which gradually becomes absorbed, and finally disappears. To the Darwinian the explanation of such facts is obvious. The stage when the tadpole breathes by gills is a repetition of the stage when the ancestors of the frog had not advanced in the scale of development beyond a fish, while the newt-like stage implies that the ancestors of the frog were at one time organized very much like the newts of to-day. The explanation of such facts has opened out to the embryologist quite a new series of problems. Having examined these in regard to phylogeny and organogeny, and entering into elaborate scientific details and arguments, Mr. Balfour concluded by remarking that although the present state of our knowledge on the genesis of the nervous system is a great advance on that of a few years ago, there is still much remaining to be done to make it complete. The subject, he urged, was well worth the attention of the morphologist, the physiologist, or even the psychologist, and we must not remain satisfied by filling up the gaps in our knowledge by such hypotheses as he had been compelled to frame. New methods of research will probably be required to grapple with the problems that are still unsolved; but when we look back and survey what has been done in the past, there can be no reason for mistrusting our advance in the future.

RELATION OF VERMONT ARCHÆOLOGY TO THAT OF THE ADJACENT STATES.*

By Dr. George H. Perkins.

Vermont is a very barren region archæologically as compared with many parts of the West, yet thorough investigation has shown that even there interesting results may be obtained. We not only have found a not inconsiderable number of stone relics, but we have also found, as we think, an interesting relation between these specimens and those from surrounding States. West of the Green Mountains we find our greatest variety of objects, and we find at least two classes, and perhaps more, which should be referred to different people. Here and there, but especially near Lake Champlain, we find objects of copper, and polished stone much more skillfully made than most of the specimens found in New England. In certain graves found near Swanton, and described fully at the Portland meeting of this Association, we find this class of objects. A peculiar form of slate knife (or lance?), polished and with notched haft, is found in Western Vermont, but occurs in greater abundance across the lake in New York and in Central New York. At Palatine Bridge Mr. S. L. Frey has discovered graves of the same kind as those found at Swanton. Taking these finer specimens of ancient workmanship as a basis of comparison, leaving out of account the ruder stone objects and the pottery, we can duplicate most of our Vermont specimens in Central New York, and also we find from Western New York and the mounds of Ohio many which are identical in all essential characters. This is true of shell and copper beads, of copper spear-heads, of stone tubes, axes, gorgets, banner stones and other objects. As we go westward we find these specimens increasing in number and of greater variety, and we also find a few forms absent. These specimens seem to me sufficiently characteristic and numerous to warrant the inference that in them we have a record of a people who emigrated from Ohio through New York, crossed Lake Champlain and reached as far east as the Green Mountains, where they stopped. They also appear not to have reached further north than Northern Vermont, nor further south than the southern end of Lake Champlain.

The other class of relics is composed of ruder objects associated with pottery. So far as I know no pottery has been found with the first class of relics. This pottery is quite unlike that from the mounds or most of that found

* Read before the A. A. A. S., Boston, 1880.

anywhere west of Central New York. The stone implements, and I believe there are none of copper, are ruder and less varied than those first mentioned and are found not only in Western Vermont, but also over the eastern portion of the State and the other New England States. The pottery, occurring chiefly in fragments, is incised and cord-marked and decorated with a great variety of patterns made up of straight lines, circles, &c. This and the stone objects, which seem to be associated with it, appear to be the work of a different and less highly cultivated people than those who made the finer specimens first mentioned, and their makers appear to have lived all over New England and Eastern New York. Thus we have evidence of the former occupation of Western Vermont by a widely spread people, of much skill in the manufacture of stone objects; a people having commenced with those living in the copper region of Lake Superior, and with those living in Florida or some portion of the South, for the shell beads are, some of them, if not all, made from Southern species of mollusks, and also of an ancient, but later occupation by a people of less wide distribution and less development in arts.

THE INDIAN CENSUS.*

Colonel Garrick Mallery, U. S. A., now attached to the Bureau of Ethnology at Washington, discussed last Monday a subject of national interest. On the nine previous occasions when the census of the United States was directed to be taken, the Indians, not taxed, forming a part neither of the voting population nor of any basis of representation, were simply disregarded. The present law provides for the enumeration and the ascertainment of their statistics. This change in legislation may have arisen from the abandonment of the doctrine of necessary extinction, the *feræ naturæ* theory combated by Colonel Mallery at the Nashville meeting of the Association in 1877, and from the probability of the early absorption of many of the Indians into the body of the taxable and voting population, which renders them of future political importance, a factor the effect of which should be estimated. It is also probable that the interest in ethnologic research, noticeable throughout the country, has influenced Congress. General Walker, the able superintendent of the census, has availed himself of an agency that never before existed. The Bureau of Ethnology, lately established by act of Congress and now under the direction of Major Powell, was entrusted with the whole of the duty in question. Without the preparation already made by the Bureau of Ethnology the work could not be done accurately, and by scientific methods. It might possibly have fallen into the hands of mere office seekers, perhaps of persons interested in the concealment if not perpetration of frauds. The enumeration of the Indians is difficult. Though restrained more or less successfully within specified limits, they are still apt to range over large regions, and to be away, for long periods from the place of their compulsory or voluntary habitation. This is especially the case in Summer, and the day of June fixed for the general census being inappropriate, the first day of October was selected instead. There are other causes interfering with accuracy. If fraud is attempted it is assisted by an enlarged paper-number of recipients of rations, and the Indians themselves are tempted to swell their lists, both for rations and annuities. Hostile or troublesome bands, under differing circumstances, seek to exaggerate or conceal their military strength. The aboriginal reluctance of each person to give his own name, and of all to speak of deceased relatives and friends is well known. These and many other obstacles require that the duty shall be in charge of persons familiar with the Indian customs, who both know what to look for and how to find it. The forms and schedules of the general census being wholly inapplicable, others have been prepared with great care. They are five in number. 1. *Population.* Each sheet is confined to one family in one dwelling, that unit being of much greater importance in savage and barbaric than in civilized life. The location of the dwelling is given by legal and natural subdivisions, also its description; if a house, whether of brick, stone, adobe, frame or log; if pueblo, whether stone or adobe; if lodge, whether of cloth, skins, slabs, poles, brush, bark, tule, stone or earth. The head of the family, often a woman, is first designated, and the relationship of each person to that head. For each individual the Indian name is given, with the English translation of that name; also the English, Spanish, French or other name habitually used. This serves not merely for identification, but brings out the names originally designated on the system of the *gens* organization, and also the title or sobriquet generally bestowed in after-life from some achievement or circumstance often of sociologic, if not historic, interest. Mixture of blood between several tribes, and between Indians and whites and negroes, is noted, and all matters relating to advance in civilization, such as wearing citizen's dress, amount and kind of personal and real property ownership, in which is recognized cultivation of land and sources of subsistence. 2. The schedule for *vital statistics* inquires into the causes of deaths during the past year, and the prevalence of the diseases to which Indians are subject; among other interesting points obtaining in the Indian tongue a statement from the head of the family, or medicine man, of the cause of death, thus showing the aboriginal theories of diseases. 3, *Industries*, embraces every appropriate particular under that head, classified for full and mixed bloods, and adopted whites and negroes, all by tribes instead of by families and individuals, as in the "population" schedule, and with details more useful for statistical purposes. 4, *Education*, is on the same principle. Schedule 5 guides and simplifies research into the wondrous system of ramified consanguinities and affinities, on which savage society is founded and depends. The work of the present census of the Indians will be of great practical value. It will correct some popular errors which have obstructed judicious legislation, confused statesmanship and misled philanthropy, and will render frauds difficult of perpetration. The schedules also show that advantage has been taken of this opportunity to lead research into points of deep scientific interest.

*Read before the A. A. A. S. Boston, 1880.

EXPERIMENTS ON THE STRENGTH OF YELLOW PINE.*

By PROF. R. H. THURSTON.

The elasticity of yellow pine timber as used in construction is very variable, the modulus varying from one to three millions, the average being about two millions in small sections, and a little above one and a half millions in large timber.

The highest values are as often given by green as by seasoned timber, and that, under sixteen square inches section and fifty-four inches length, at least, the magnitude of the modulus of elasticity is independent of the size of the piece.

The density of the wood does not determine the modulus; since the figure varies sometimes directly and sometimes inversely with the density, even where the wood is as nearly as possible in the same condition as to seasoning.

A high modulus usually accompanies high tenacity and great transverse strength, but it is not invariably the fact that maximum ultimate strength is accompanied by initial stiffness.

The pseudo moduli, determined by taking considerable deflections, are usually not greatly different from those determined from small deflections and light loads. The values of these moduli often decrease with increase in deflection.

An inspection of the woods tested plainly indicates, in the opinion of the writer, that the density of the pines is so considerably modified by the amount of pitch contained in the sap channels that it cannot be regarded as indicative of the strength of the timber. Where quite free from sap the wood usually exhibits increase of strength and elastic resistance to deflection, with increase of density.

The strength of timber, otherwise similar, is greatly affected by its structure, and the resistance offered to stresses applied transversely is greatest when the sections

*Read before the A. A. A. S., Boston, 1880.

of the timber taken transversely exhibit most nearly vertical lines of grain.

The modulus of rupture by transverse stress varies, for yellow pine, from $R=\frac{2}{3}\frac{Wl}{b\,d^2}=10{,}000$ to 17,000, the highest values being usually obtained from well-seasoned wood. An average value may be taken as $R=13{,}000$ for good timber, which in the formula $W=C\frac{b\,d^2}{l}$ gives $C=866$ pounds or, practically, $W=9000\frac{b\,d^2}{l}$ for good yellow pine.

The modulus of rupture varies as irregularly and with as little regard to size or density of the material as does the co-efficient for elasticity.

In the use of such materials, the only safe course for the designing and constructing engineer is evidently to adopt a moderate value of the modulus in proportioning his work, and by careful inspection and test to secure the rejection of all material which is not of good quality.

As has been seen, careful inspection may sometimes lead to the selection of material twenty-five per cent. superior to the average of good timber, and fifty per cent. more valuable than the lower grades such as are often sold in our markets.

The Paper was illustrated by a series of tabulated statements, being the result of experiments made to arrive at the conclusions prescribed in this abstract.

BOOKS RECEIVED.

MORTUARY CUSTOMS AMONG THE NORTH AMERICAN INDIANS.*

The primitive manners and customs of the North American Indians are rapidly passing away under influences of civilization and other disturbing elements. In view of this fact, it becomes the duty of all interested in preserving a record of these customs, to labor assiduously, while there is still time, to collect such data as may be obtainable. This seems the more important now, as within the last ten years an almost universal interest has been awakened in ethnologic research, and the desire for more knowledge in this regard is constantly increasing. A wise and liberal government, recognizing the need, has ably seconded the efforts of those engaged in such studies by liberal grants from the public funds; nor is encouragement wanted from the hundreds of scientific societies throughout the civilized globe. The public press, as the mouth-piece of the people, is ever on the alert to scatter broad-cast such items of ethnologic information as its corps of well-trained reporters can secure. To induce further laudable inquiry, and to assist all those who may be willing to engage in the good work, is the object of this preliminary work on the Mortuary customs of the North American Indians, and it is hoped that many more laborers may, through it, be added to the extensive and honorable list of those who have already contributed.

It would appear that the subject chosen should awaken great interest since the peculiar methods followed by different nations, and the great importance attached to burial ceremonies, have formed an almost invariable part of all works relating to the different peoples of our globe; in fact no particular portion of ethnologic research has claimed more attention.

In view of these facts it might seem almost a work of supererogation to continue a further examination of the subject; for nearly every author, in writing of our Indian tribes, makes some mention of burial observances; but these notices are scattered far and wide on the sea of this special literature, and many of the accounts, unless supported by corroborative evidence, may be considered as entirely unreliable. To bring together and harmonize conflicting statements, and arrange collectively what is known of the subject, has been the writer's task. This volume forms the third of a series, the first of which, entitled "Introduction to the Study of Indian Languages," was written by Major J. W. Powel, the director of the Bureau of Ethnology, Washington; the second being by Col. Garrick Mallery, and entitled, "Introduction to the Study of Sign-Language among the North American Indians."

The following provisional arrangement of burials has been adopted in arranging the facts presented in this work.

1. BY INHUMATION in pits, graves, holes in the ground, mounds, cists, and caves.
2. BY CREMATION, generally on the surface of the earth, occasionally beneath.
3. BY EMBALMMENT, or a process of mummifying, the remains being afterwards placed in the earth, caves, mounds or charnel house.
4. BY AERIAL SEPULTURE, the bodies being deposited on scaffolds, or trees, in boxes or canoes.
5. BY AQUATIC BURIAL, beneath the water or in canoes, which were turned adrift.

Major J. W. Powel gives the assurance that to those who are willing to take part in this work by earnest and faithful research, Dr. Yarrow will give full credit for their work in his final publication, and we would suggest that those able and willing to assist should put themselves in communication with the Bureau of Ethnology, Smithsonian Institution, Washington, and request instructions as to the best methods of recording their work.

*Introduction to the study of Mortuary Customs among the North American Indians. By Dr. H. C. Yarrow, Act. Asst. Surg. U. S. A., Smithsonian Institution, Washington, Bureau of Ethnology. J. W. Powell, Director.

WE have received the second chapter of a serial article, published in the *Journal of Nervous and Mental Diseases*, and entitled the "Architecture and Mechanism of the Human Brain." Its author, Dr. Spitzka of this cityf one of our own contributors, intends in this article to, build up the brain before the reader's eye, as it were, beginning with the simplest foundations and gradually erecting thereon the higher superstructures which are the basis of the intellectual operations. Throughout the chapters thus far issued the writer has interlarded hundreds of interesting and suggestive observations drawn from the fields of Comparative Cerebral Anatomy and Embryology. The style is not the least creditable feature of the work, and especially its preliminary chapter, which is as easy reading as a novel, and the complex features of the structure of the most complete organ in the body becomes the property of the reader almost without effort on his part.

The recent number of the *American Journal of Microscopy* contains, among other articles, the following: *Pelomyxa, Palustris*, and other Rhizopoda, by W. G. Lapham—An improved glass for the collection and examination of Deposits (with drawings): Highest Magnifying Powers, by Allen Y. Moore: Several letters of interest, reports of societies, and useful notes.

We were pleased to see *Nature*, in a recent number, give a handsome recognition of the merits of this journal which we conscientiously indorse.

The American Monthly Microscopical Journal for August has also some very useful articles on the preparation and mounting of objects. It gives a New Form of Injecting Apparatus, by Mr. Justin Spaulding: A Useful Culture-Cell, by Dr. George M. Sternberg: Histology of the Fœtal Lung. There are also two articles of a series now publishing by this journal, which will prove valuable to microscopists—the Classification of the Protista, by Häckel (translated), and a description of the "Family Volvocina."

OFFICE OF "SCIENCE,"
229 BROADWAY, NEW YORK

NOTICE.

WE are preparing a list of Professional Scientists, and of all those engaged in scientific investigations in the United States. We ask the co-operation of our subscribers and the heads of Universities and Colleges, by forwarding to us lists of Scientific men connected with such institutions.

We invite all engaged in original research, whether professionals or amateurs, to forward their names and addresses for registration, giving their names and style in full, and the particular line of research followed.

We desire that the list of Physicists shall be very complete as we understand that such a list does not exist, even at Washington.

We guarantee to forward all names and addresses received by us to the Smithsonian Institute, Washington, as we understand from the Secretary that such a course would be acceptable.

Compliance with this request will result in many conveniences to those concerned, as when the lists are analytically arranged, those desirous of communicating with their fellow workers will have facilities for so doing, and papers and notices can be distributed without delay. Scientific men, at present dispersed over a wide territory, will thus be united, so as to make co-operation possible when occasion requires.

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Scientific papers and correspondence intended for publication should be written *legibly* on one side only of the paper. Articles thus received will be returned when found unsuitable for the Journal.

Those engaged in Scientific Research are invited to make this Journal the medium of recording their work, and facilities will be extended to those desirous of publishing original communications possessing merit.

Proceedings of Scientific Societies will be recorded, but the abstracts furnished must be signed by the Secretaries.

Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply may be written in the form of an article.

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JOHN MICHELS, Editor.

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SATURDAY, OCTOBER 9, 1880.

Two or three weeks ago we complained of the coldness of British writers in neglecting to recognize and acknowledge the full scientific value of Bell's and Edison's discoveries, and we condemned, more particularly, their omission from Gordon's illustrated catalogue of recent advances in electrical science, which had then just appeared in London. To day, after having read a four column pean of eulogy and encomium on Prof. Bell and his Photophone, in the last number of *Engineering* we make a candid confession, that, in order to arrive at the real estimate of an American discovery in the average English mind, it is indispensable that the 'mean' of English criticism should first be drawn.

The concluding paragraph of this article reads as follows: "Who can say to what great fields of science this one discovery of Prof. Bell may not lead, fields of research not limited in locality to this earth, but reaching to the planets, and to the farthest limits of visible stellar space. It is by a beam of light that the modern astronomer is able to analyse the chemical constitution of the farthest stars and nebulæ, and is enabled to detect and to deal with metallic vapors through distances of thousands of millions of miles as surely as in his own laboratory, who, after Prof. Bell's experiments will have the hardihood to affirm that sounds taking place in the far off regions of the universe may not one day be heard on the earth, and new fields of acoustical astronomy may not be opened to the intelligence of man. When such a time arrives, the thought of the poet will be clothed with the truth of the fact, that "Light is the voice of the stars."

In the same strain which excites *Engineering* to this transcendental flight of fancy, may we not also hope, in the future, to catch the whisperings of Venus as she waltzes among her heavenly companions, and if we dare to reach so far in our aspirations for the perfection of the Photophone, may we not yet be able to hear the reflections of light, mixed with heat, which Mars, an ardent admirer of old, throws to that splendid luminary as they near each other? It is also true that Mr. Edison has helped to begin that sort of business; for did he not, long since, catch the warmth of the coronal beams, when the sun withdrew behind fair Luna's screen, and didn't Mr. Lockyer (who was there) tell us all that happened? Let us not, however, go too far and admit that the era has arrived which Gulliver predicted, when he discribed the process of the philosopher of Laputa who extracted sunbeams out of cucumbers.

Engineering should know that the Photophone is but a simple machine for registering heat waves that have impinged upon a piece of hard rubber, and that these waves, originally set in motion by the voice, when made to act on any material expansible by heat, will reproduce, more or less effectually, the original motion which gave them birth. Such an instrument is the Tasimeter, which Professor Bell has stripped of its swaddling clothes and made to talk.

Considerable alarm has been created among those interested in horse flesh by certain reports circulated regarding a new so-called epizootic among horses. We have been at some pains to collect reliable *data* concerning this matter, and have found, as we anticipated, that its importance is greatly exaggerated by enterprising reporters of daily papers. We have become satisfied of the fact that the distemper now prevailing in New York, has nothing in common with the epizootic which was such a memorable feature of the year 1873, and so severe a one that hardly a carriage could be seen on our streets, while but few of the horse-car lines were able to keep their conveyances running with any regularity.

Veterinarians are accustomed to expect a more or less severe endemic of catarrhal troubles among horses about the first of October of every year. The horse is very liable to atmospheric influences, far more so than the human species, and the changes in the weather occurring about that time suffice to produce an apparent epidemic of catarrhal troubles among them. In some years few, in others many horses are affected; the present year the number has been so large as to temporarily interfere with business, but this is exceptional.

The disease lasts but a few days, the main trouble is a bronchitis associated with a slight catarrh of the nasal mucus membrane; for a period of from twelve to forty-eight hours there is also a febrile disturbance. The highest temperature recorded by a veterinarian, from an observation of fully one thousand cases has been

$106\frac{1}{2}^{\circ}$ Fahrenheit. The disorder is not fatal, it hardly requires any treatment even; only in debilitated or very old animals, or such as are overworked by inhuman owners, may fatal complications arise. Few deaths have taken place; a veterinarian in Yorkville who has visited stables containing an aggregate of a thousand affected horses, has had but a single death, that of an animal overdriven while convalescing, and in which pulmonary congestion resulted.

There is as yet no proof that the affection is contageous; it is rather endemic than epidemic. The rapidity with which it has successively appeared in Boston, New York and Chicago, speaks more in favor of an atmospheric cause than of transmission by contagion. A Boston microscopist asserts that bacteria or micrococci are active factors in its transmission, but he makes the statement, rather as an inference, than on the basis of observation. The same veterinarian, to whom we owe the communication of several facts here mentioned, tried to inoculate his own horse with the disease, by introducing the discharged matter from sick horses into its air passages, and failed in this and other experiments of the same kind. It is also observed that the endemic has appeared more frequently and affected more horses in large, well ventilated stables, in which the influence of outside changes in the temperature is quickly felt, than in close and confined quarters where the air is, if more impure, warmer, and the oscillations of the outside temperature less suddenly made manifest than in the former.

As far as this city is concerned, the *acme* of the endemic is past, and owners of horses frightened by sensational reports in the daily papers are recovering their wonted composure. If it has done nothing else the distemper has taught the one lesson, that when a horse is ill, the policy of getting as much work out of him as possible is, not to speak of its barbarity, exceedingly short-sighted, for no vigorous animals have perished in this endemic, except such as those in whose case this "penny wise, pound foolish" idea had been carried out.

PROFESSOR EDWARD C. PICKERING, of Cambridge, describes a novel celestial object observed by him on the 28th of August last, which presented a faint continuous spectrum with a bright band near each end. Clouds interfered, and barely permitted an identification with Oeltzen 17681, or a position in 1880 of R. A., 18h. 1m. 17s.; Dec., 21° 16′.

The object might be mistaken for a temporary star, like that in Corona in 1863, and the bands assumed to correspond to the Hydrogen lines C and F. Professor Pickering appeared to be unable to determine whether it was a nebula, a mass of incandescent gas resembling a nebula in character but not in constitution, or whether it was a star with a vast atmosphere of incandescent gas of a material not as yet known to us. The discovery of this object, in his opinion, greatly increases the difficulty of distinguishing between a star and a planetary nebula.

The observation was made on the 24th of August and described on the 2d of September, but in consequence of the fact that Professor Pickering sent his communication to a foreign journal, three thousand miles away, it was thus the second week in October when it came before the American public.

SCIENCE IN FRANCE AND GERMANY.

Dr. C. K. Akin has written a series of letters from Pesth to Professor G. C. Stokes, Secretary to the Royal Society, who was one of the Royal Commission on Scientific Instruction. These letters are dated 1870, but are now published for the first time by *The Journal of Science*, London.

In what may be called a supplemental communication Dr. Akin describes the condition of the most prominent scientific institutions in France and Germany. His remarks on the system of centralization, and abuse of the authority of those who profess an infallibility in respect to the human mind will be read with interest.

He states that these scientific magnates, the recognized "authority" in Germany, instead of rendering encouragement to students, positively check and impede all progress outside of their own circle, keep out new men with novel ideas as long as possible, so as to hold their own sway.

But we will leave Dr. Akin to make his own statement:

"The French Academy is in some respects similar to the Royal Society, and the points in which it differs from the latter are not, in my opinion, to its advantage. In the first place, the members of the Academy are salaried by the Government, but their emoluments are not sufficient to live upon, or to keep them, so to speak, in working order; nor do they perform any specific service to Science or the State for the money. The Academy, next, is divided into a certain number of sections, according to the several branches of science, and the number of members in each section is strictly limited. As that subdivision is invariable, while the relative importance of the sciences is fluctuating, the abuse has crept in of electing members into a wrong division. On the other hand, such a proceeding not being always practicable, highly distinguished men are excluded from the Academy for many years if their proper sections happen to be full; while if, from the dearth of cultivators or accidents of mortality, the number of vacancies happens to be great, the standard of admission is considerably lowered. The Academy publishes weekly its proceedings or "Comtes Rendus," which, from the celerity and regularity of their publication, are a valuable means of conveying rapid information; on the contrary, its transactions or "Memoires" are issued in a very irregular and dilatory manner. The practice of examining and reporting upon communications submitted has fallen into almost complete disuse; and the prizes, which are in a considerable number, are in a great part awarded upon the antiquated principle of putting forth questions. I have thus rapidly drawn the most distinctive features of the French

Academy, roughly yet faithfully; and I feel constrained to confess my inability to comprehend the enthusiasm which there appears to exist in certain quarters in England for this institution, and which shows itself in the desire to copy it. I have dwelt in a former letter upon the functions which any society should perform in order to be called useful, and I cannot bring myself to believe that those of the French Academy correspond in any way to the model.

I have spoken, in a former communication, in words of unavoidable eulogium, of the German Universities and the position which they occupy among similar institutions in Europe. Still I do not find in their organization anything that I should be prepared to recommend for imitation or adoption. I shall presently mention the mischievous effects which the Universities in Germany, like the Academy in France, exercise on scientific development, according to my belief, when I shall enter upon the discussion of the principles which underlie the organization of both: here I wish merely to give an opinion upon the institution of so-called *privat-docenten*, which is generally considered as most characteristic of the German University system, and which has many admirers out of Germany. A *privat-docent* is simply a lecturer who, as a rule, receives no pay from government or the University, but may take fees from the students: he is simply a private tutor, who, in consideration of having passed an examination or other ordeals before the proper authorities, is admitted to the use of the public lecture-rooms. In my opinion the fellowships in the English Universities—if only Fellows were elected upon a better principle—are much more advantageous; and if the now somewhat dormant institutions of lecturers and prælectors in the colleges were more largely developed, the English Universities would have nothing to envy from, and much to boast over, those of Germany in this respect.

The principal aim of the German Universities, as well as of the French Academy, is to uphold the principle of *authority* in science, which has a great many effects that are detrimental to its progress. Authority in science means infallibility, and it means also stagnation. But the essence of science is development, which is identical with change, and variation from ancient theories or received doctrines. The French Academy has generally not been favorable to novelties started out of its own precincts, as is shown by its treatment of such men as Fresnel, Fourier or Melloni. I know also of a case in which it was found impossible to get a correction or mention of mistakes, which one of its members had happened to make, inserted in the proceedings of the Academy, notwithstanding repeated attempts. The desire to have this done was supposed to imply *naïveté*. In a similar way the German Universities enforce a certain uniformity in the preparation of scientific students, and they measure all ability by a fixed yet arbitrary standard. Investigation must be *schulgerecht*, as it is called—for which the French have the word *classique*, but I doubt whether there be any real equivalent in English. A mind of independent character or original turn has thus a hard struggle for existence; for, in order to get recognized, it must be fashioned on the approved pattern. Men like Davy or Faraday are consequently unknown to the history of German or French science, as their irregular preparation would have debarred them from coming under notice, and still more so from making their way. On the other hand, great errors are propagated and kept up under the wing of authority; and if once a philosopher has obtained a certain sway, or formed a so-called "school," his teaching will be kept up long after its errors have been detected. Thus certain theories are still taught all over Germany in physics which are manifestly untenable, and to attack them is punished more severely than heresy is in religion nowadays. Theories propounded by new men are generally overlooked. On the other hand, I could tell an instance in the recent history of physical science where a discovery undoubtedly not novel and manifestly incomplete has been accepted on the Continent as an unexpected revelation proof against all doubt, because it was appropriated by names possessing authority. What constitutes authority in science it were difficult to define; yet its worship, although it be opposed to the very spirit of science, is in Germany and France, so to speak, without bounds. It were easy to prove by example that the test of infallibility is not applicable, if such a thing could be imagined with respect to a human mind. Not only are the instances numerous where the authorities of one age have been scouted by those of the succeeding, but even in the works of the greatest among them, whose reputations were acquired on the strength of real intellect and conspicuous services, schoolboys nowadays frequently may point out glaring mistakes committed or upheld by great masters only one generation behind.

I have mentioned in a former letter the well-known fact that a German philosopher who wished to bring out some novel theory in his country encountered so many difficulties that he absolutely went mad. Another who started similar ideas about the same time, having been repulsed in one quarter, took it for granted that the same had happened to him also in another, where it was not the case, so hopeless did he consider his endeavor to obtain a hearing. Actually these ideas took wing in England, but not before, communicated also to the French Academy, they had been allowed to rest unnoticed in its archives for years (like the memoirs of Abel), notwithstanding repeated instances to have them examined. I also have it out of the mouth of one, who is actually himself a chief authority on physical science in Germany, that an early work of his, now the principal foundation of his fame, had proved injurious to his university career, for being of too novel a character. It is a slight consolation to the individuals concerned, for the anxiety or pain they have suffered, to have had their names recently enrolled on the list of members of the French Academy, or to have received an honorary title from a German University; and the damage which is done to science by such proceedings, in all cases serious, is in many irreparable. Authority, whether exercised by academies or universities, would have its uses if it facilitated the endeavors of students during the early and more trying periods of their career, in which encouragement and aid are most welcome and needed; but if, instead, it check or impede novices, and establish merely a kind of confraternity, the chief end of which is to keep new men out as long as feasible, and to uphold its own sway, I make bold to say that the liberty of thought reigning in England, notwithstanding its abuses, is a far more valuable safeguard for science, the very life of which is progress. Now, if the Royal Society, transformed into or superseded by an academy, were to arrogate to itself that kind of domination which the Académie des Sciences exercises in France, or if the English universities endeavored to absorb all the intellectual life of the nation, or to fashion it in their own way, as is the case in Germany, the superiority of England, which has made it the head-quarters of scientific progress and the mother country of so many amateurs more distinguished in science than most French academicians or German professors, would probably be gone.

TOXICOLOGY.—An Italian commission, including among its members Prof. Selmi, is examining the methods for the detection of poisonous alkaloids in the viscera, with especial reference to the so called "ptomaines,"—alkaloids which under certain circumstances may be generated during the putrescence of animal matter.

NOTE ON THE ZODIACAL LIGHT.*

By Henry Carwill Lewis.

The results of a series of observations upon the zodiacal light made by the writer, extending over a period of nearly five years, is here recorded. The special precautions taken, both to train the eye to detect faint lights, and to prevent bias on the part of the observer, were given in detail. The zodiacal light may be divided into three portions—the *zodiacal cone;* the *zodiacal band;* and the *gegenschein*. This division is convenient in observation, saves confusion in description, and may be in part a natural one.

The zodiacal cone.—This, the zodiacal light proper, of most authors, is the well-known cone of light rising along the ecliptic, and best seen in the winter months in the West, immediately after the disappearance of twilight. The time of shortest twilight coincides with its greatest brilliancy. Several observations are given when the writer saw it cast a distinct *shadow* at that time. Its comparative brightness with the Via Lactea at different seasons were given, and its relation to the ecliptic discussed. It was stated that the cone in our latitude is not symmetrical; and that while its axis of greatest brightness lies exactly upon the ecliptic, its axis of symmetry is north of that line. An inner short cone of greater brightness was described. The warm color was shown to be due to atmospheric absorption. No pulsations were ever observed which could not be explained either by atmospheric changes or by changes in the eyesight of the observer. No periodic changes in the zodiacal light were observed; the same series of changes occurring each year with an equal amount of brilliancy. It was shown that while the zodiacal cone is frequently seen by moonlight, the moon appears to have no appreciable influence upon it. The account of the zodiacal cone closes with a description of its *spectrum*, which is always continuous and free from bright lines.

The zodiacal band.—This is an extremely faint zone of light, somewhat wider than the Via Lactea, which, like a strip of gauze, is stretched across the sky along the zodiac from horizon to horizon, and which can be seen at all times. It is a belt which forms a very faint prolongation of the zodiacal cone, and which, like it, is best seen when the ecliptic makes a large angle with the horizon. It is so faint that it can only be seen with difficulty. The best method of observing it is described. It is brightest along an inner line, and fades off more suddenly on its southern than on its nothern edge. It has a width of about 12°, and its central line is slightly north of the ecliptic. Observations prove the zodiacal band to be a constant and invariable phenomenon.

The gegenschein.—The gegenschein is a faint patch of light, some 7° in diameter, which nightly appears in that part of the zodiacal band, which is 180° from the sun. Night after night it shifts its place so as to keep opposite to the sun. It is decidedly brighter than the zodiacal band, and occasionally a central nucleus about 2° in diameter, of greater brightness, can be observed. While the brighter portion of the gegenschein is circular, its faint boundaries have sometimes the form of an oval, whose major axis is parallel to the ecliptic. A large number of maps of its position among the stars have been made, which show that while its central point is always 180° in longitude from the sun, it has a latitude of + 2°.

The moon zodiacal light.—An oblique cone of light in the proximity of the moon was described by Rev. G. Jones, but has not been detected by the writer. The light preceding moonrise rises at right angles to the horizon, and seems purely atmospheric. One observer has described comet-like tails on either side of the moon. The writer holds that such appearances are caused by diffraction through floating vapor, since they are never seen on clear nights.

The horizon light.—The phenomenon to which this name is applied, though having no connection with the zodiacal light, is so continually observed with the latter, and at certain seasons is so apt to be confounded with portions of it, that it is necessary to take it into account. The horizon light is a faint band of light with parallel sides, lying all around and parallel to the horizon, and separated from it by an interval of darkness. It is brightest, and terminates most abruptly on its lower edge. This sharp lower edge is 5° above the horizon, while the diffuse upper edge varies in altitude with the state of the atmosphere. The horizon light has a mean width of about 15°. It is purely atmospheric and appears to be caused by reflected starlight. It becomes very bright when the moon is above the horizon. Below the horizon light is a very dark space here called the *absorption band.* This quenches the light of the Via Lactea, the zodiacal cone, and all except the largest stars and planets, which last, while in it, are deeply colored. In the summer, when the ecliptic is low, the horizon light frequently blends with the zodiacal band.

THE ACTION OF SUNLIGHT ON GLASS.*

By Thomas Gaffield.

As great a variety of tints and colors appears after exposure to sunlight as is witnessed in the original specimens. A general classification of the changes of color produced by the sun in colorless glasses is as follows: 1. From white to yellowish. 2. From greenish to yellowish-green. 3. From brownish-yellow and greenish tints to purple. 4. From light-green or greenish-white to bluish. 5. From bluish and other tints to darker tints of the same colors. Every specimen of colorless glass exposed ten years shows some change of color or tint, except some white flint glass, such as is used for fine glassware and optical glass. The optical glasses with the exception of two specimens of crown, which became of a yellowish color, showed only a very slight change of tint, leading some to the opinion that oxide of lead, which enters largely into its composition, may act as a protecting shield against change by sunlight exposure. In experimenting for ten years with colored glasses of the main spectral colors (red, orange, yellow, &c.), no change was observed in any pot-metal specimens (colored throughout the body) save a slight darkening of the purple. A change to a purplish or yellowish color was observed in the colorless body of some of the flashed and stained specimens, when looking through the edges of these glasses, which are originally colored on the surface only. The sunlight coloration is not sufficient to be noticed in an observation through the surface of the glass. An experiment with pot-metals not of the primary colors, but of the intermediate ones which most nearly approach those which are produced in colorless glass by sunlight exposure, showed the following changes: First, from brownish tints to a flesh color; second, from flesh color to tints of violet or purple; third, from amber, olive and purple to darker tints of the same colors.

It is interesting to know that, so far as such colors in pot-metal were used in the old cathedral windows, the results of these experiments prove that they must have changed in color or tint, and that the glass which we see in these old churches to-day, and which has suffered sunlight exposure for centuries, must be of very different hue from that which it exhibited when it left the artist's studios or the glass factories of the mediæval ages. It is a curious fact, noticed by Pelouze and Percy, and confirmed by Mr. Gaffield's experiments, that, with some exceptions among the colored specimens, all of the glasses changed in tint or color by sunlight exposure can be restored to their original color by the heat of a glass-stainer's kiln, and can again be colored after a second exposure to sunlight; and that this coloration by sunlight and de-coloration by heat (of about the temperature of red heat) can be carried on indefinitely. Diffused light will also color glass, but only with a greatly diminished effect, proportioned to its comparison with the power of the direct rays of the sun.

ON A SOLUTION OF FERRIC GALLATE AND FERRIC OXALATE AS A REAGENT FOR THE QUANTITATIVE ANALYSIS OF AMMONIA.*

By Prof. N. B. Webster, of Norfolk, Va.

Preparation.—Ferric sulphate in solution is decomposed by gallic acid, and the resulting black ferric gallate is par-

* Read before the A. A. A. S., Boston.

tially decomposed by oxalic acid till the color is reduced to a bluish-black tinge.

Application.—A suitable quantity of the re-agent, prepared as above, is added to a solution of free ammonia or its carbonate, in the same way that Nessler's solution of mercuric per-iodide is used in Manklyn's well-known process.

Result.—The combination of the ammonia with part or all of the oxalic acid of the colorless ferric oxalate of the re-agent, and the blackening of the solution by the re-formation of ferric gallate.

Estimation of Ammonia.—By an imitation of a standard solution of ammonia with the re-agent, as in Wanklyn's mode of Nesslerizing. When the solution to be tested and the imitation solution correspond in color, it is inferred that they contain equal quantities of ammonia. In this process the standard ammonia test should be made from the *carbonate*, and its strength may be such that one litre shall contain one milligramme of ammonia, or one part in a million. Another and more direct way of estimating ammonia is by adding a standard test solution of oxalic acid to the blackened solution of the re-agent and liquid to be tested, till the original color is produced, and from the known quantity of oxalic acid used to calculate the quantity of ammonia in the resulting oxalate. Chemists will find this re-agent both convenient and sensitive.

THE UNITY OF NATURE.

BY THE DUKE OF ARGYLL.

In the preface to the first edition of the "Reign of Law," published in 1866, the following passage occurs:—"I had intended to conclude with a chapter on Law in Christian Theology. It was natural to reserve for that chapter all direct reference to some of the most fundamental facts of Human Nature. Yet, without such reference, the 'Reign of Law,' especially in the 'Realm of Mind,' cannot even be approached in some of its very highest and most important aspects. For the present, however, I have shrunk from entering upon questions so profound, and of such critical import, and so inseparably connected with religious controversy."

The great subject spoken of in this passage has ever since been present with me. Time, indeed, has only increased my sense of its importance. But the years have also added, perhaps in more than equal proportion, to my sense of its depth and of its difficulty. What has to be done, in the first place, is to establish some method of inquiry, and to find some secure avenue of approach. Before dealing with any part of the Theology which is peculiarly Christian, we must trace the connection between the Reign of Law and the ideas which are fundamental to all religions. It is to this preliminary work that the following chapters have been devoted. Modern Doubt has called in question not only the whole subject of inquiry, but the whole faculties by which it can be pursued. Until these have been tested and examined by some standard which is elementary and acknowledged, we cannot even begin the work.

It has appeared to me that not a few of the problems which lie deepest in that inquiry, and which perplex us most, are soluble in the light of the Unity of Nature. Or if these problems are not entirely soluble in this light, at least they are broken up by it, and are reduced to fewer and simpler elements. The following chapters are an attempt to follow this conception along a few of the innumerable paths which it opens up, and which radiate from it through all the phenomena of the Universe, as from an exhaustless centre of energy and of suggestion.

It is the great advantage of these paths that they are almost infinite in number and equally various in direction, To those who walk in them nothing can ever come amiss. Every subject of interest, every object of wonder, every thought of mystery, every obscure analogy, every strange intimation of likeness in the midst of difference—the whole external and the whole internal world—is the province and the property of him who seeks to see and to understand the Unity of Nature. It is a thought which may be pursued in every calling—in the busiest hours of an active life, and in the calmest moments of rest and of reflection. And if, in the wanderings of our own spirit and in the sins and sorrows of Human Life, there are terrible facts which resist all classification and all analysis, it will be a good result of our endeavors to comprehend the Unity of Nature, should it lead us better to see, and more definitely to understand, that which constitutes The Great Exception.

I commend these chapters to the consideration, and I submit them to the criticism, of those who care for such inquiries. Like the former Work, of which this is a sequel, some parts of it have appeared separately in another form. These have been reconsidered, and to some extent re-written; whilst a new meaning has been given to the reasoning they contain by the place assigned to them in a connected treatise.

The publication of it as a series of Articles, before its final appearance as a volume, will afford me, I hope, the advantage of hearing and of seeing what may be said and written of its errors or of its deficiencies. Perhaps, also, it may afford me an opportunity, before the whole of these Articles have appeared, of writing at least one more chapter on an important subject, for which leisure fails me now.

I.

GENERAL DEFINITIONS AND ILLUSTRATIONS OF THE UNITY OF NATURE—WHAT IT IS, AND WHAT IT IS NOT.

The system of Nature in which we live impresses itself on the mind as one system. It is under this impression that we speak of it as the Universe. It was under the same impression, but with a conception specially vivid of its order and its beauty, that the Greeks called it the Kosmos. By such words as these, we mean that Nature is one whole —a whole of which all the parts are inseparably united—joined together by the most curious and intimate relations, which it is the highest work of observation to trace, and of reason to understand.

I do not suppose that there is any need of proving this—of proving, I mean, that this is the general impression which Nature makes upon us. It may be well, however, to trace this impression to its source—to see how far it is founded on definite facts, and how far it is strengthened by such new discoveries as science has lately added to the knowledge of mankind.

One thing is certain: that whatever science may have done, or may be doing, to confirm man's idea of the unity of Nature, science, in the modern acceptation of the term, did not give rise to it. The idea had arisen long before science in this sense was born. That is to say, the idea existed before the acquisition of physical knowledge had been raised to the dignity of a pursuit, and before the method and the results of that pursuit had been reduced to system. Theology, no doubt, had more to do with it. The idea of the unity of Nature must be at least as old as the idea of one God; and even those who believe in the derivation of Man from the savage and the brute, cannot tell us how soon the Manotheistic doctrine arose. The Jewish literature and traditions, which are at least among the oldest in the world, exhibit this doctrine of the purest form, and represent it as the doctrine of primeval times. The earliest indications of religious thought among the Aryan races point in the same direction. The records of that mysterious civilization which had been established on the Nile at a date long anterior to the call of Abraham, are more and more clearly yielding results in harmony with the tradition of the Jews. The Polytheism of Egypt is being traced and tracked through the ready paths which led to the fashioning of many Gods out of the attributes of One.[1] Probably those who do not accept this conclusion as historically proved may hold rather that the idea of the unity of Nature preceded the idea of the unity of God, and that Monotheism is but the form in which that earlier idea became embodied. It matters not, so far as my present purpose is concerned, which of these two has been the real order of events. If the law prevailing in the infancy of our race has been at all like the law prevailing in the infancy of the individual, then Man's first beliefs were derived from authority, and not from either reasoning or observation. I do not myself believe that in the morning of the world The-

[1] Renouf, "Hibbert Lectures," 1879. p. 89.

ism arose as the result of philosophical speculation, or as the result of imagination personifying the unity of external Nature. But if this were possible, then it would follow that while a perception of the unity or the unity of Nature must be at least as old as the idea of one Creator, it may be a good deal older. Whether the two ideas were ever actually separated in history, it is certain that they can be, and are, separated at the present time. A sense and a perception of the unity of Nature—strong, imaginative, and almost mystic in its character—is now prevalent among men over whom the idea of the personal agency of a living God has, to say the least, a much weaker hold.

What, then, is this unity of Nature? Is it a fact or an imagination? Is it a reality or a dream? Is it a mere poetic fancy incapable of definition; or is it a conception firmly and legitimately founded on the phenomena of the world.

But there is another question which comes before these. What do we mean by unity? In what sense can we say that an infinite number and a variety of things are nevertheless one? This is an important question, because it is very possible to look for the unity of Nature in such a manner that, instead of extending our knowledge, or rendering it more clear and definite, we may rather narrow it, and render it more confused. It has been said that all knowledge consists in the mere perception of difference. This is not accurate: but it is true that the perception of difference is the necessary foundation of all knowledge. For if it be possible to give any short definition of that in which essentially all knowledge consists, perhaps the nearest approach to such a definition would be this: that knowledge is the perception of relations. To know a thing and to understand it, is to know it in its relation to other things. But the first step in this knowledge is to know it as distinguished from other things. The perception of difference comes before the perception of all other and higher relations. It is well, therefore, to remember that no increase of knowledge can be acquired by a willful forgetfulness of distinctions. We may choose to call two things one, because we choose to look at them in one respect only, and to disregard them in other respects quite as obvious, and perhaps much more important. And thus we may create a unity which is purely artificial, or which represents nothing but a comparatively insignificant incident in the system of Nature. For as things may be related to each other in an infinite variety of ways—in form, or in size, or in substance, or in position, or in modes of origin, or in laws of growth, or in work and function—so there are an infinite number and variety of aspects in which unity can be traced. And these aspects rise in an ascending series according to the completeness of our knowledge of things, and according to the development of those intellectual faculties by which alone the higher relations between them can be perceived. For the perception of every relation, even that of mere physical continuity, is purely the work of mind, and this work can only be performed in proportion to the materials which are supplied, and to the power of interpretation which is enjoyed. It is very easy to rest satisfied with the perception of the commoner and more obvious relations of things to each other, and even to be so engrossed with these as to be rendered altogether incapable of perceiving the finer and less palpable relations which constitute the higher unities of Nature. New relations, too, by no means obvious, but discovered by analysis, may, from the mere effect of novelty, engross attention far beyond their real importance. Nay, more—it may be said, with truth, that this is a danger which, for a time at least, increases with the progress of science, because it must obviously beset special subjects of inquiry and special methods of research. The division of labor necessarily becomes more and more minute with the complication of the work which is to be done, and branches out into a thousand channels of inquiry, each of which finds its natural termination in the ascertainment of a special series of relations. The chemist is engaged with the elementary combinations of matter, and finds a unity of composition among things which in all other aspects are totally diverse. The anatomist is concerned with structure, and separates widely between things which may nevertheless be identical in chemical composition. The physiologist is concerned with function; and, finding the same offices performed by a vast variety of structures, ranges them across all their differences under a single name. The comparative anatomist is concerned with the relative place or position of the parts in organic structures; and, although he finds the same part in different creatures performing widely different functions, he nevertheless pronounces them to be the same, and to be one in the homologies of an ideal archetype. But each of these inquirers may be satisfied with the particular unity which his own investigations lead him specially to observe, and may be blind altogether to the unity which is next above it. And so it may well be that the sense of unity in Nature, which Man has had from very early times, reflected in such words as the "Universe," and in his belief in one God, is a higher and fuller perception of the truth than is commonly attained by those who are engrossed by the laborious investigation of details. This is one of the many cases in which the intuitions of the mind have preceded inquiry, and gone in advance of science, leaving nothing for systematic investigation to do, except to confirm, by formal proofs, that which has been already long felt and known.

I have already indicated the sense in which the unity of Nature impresses itself on the intelligence of Man. It is in that intricate dependence of all things upon each other which makes them appear to be parts of one system. And even where the connection falls short of dependence, or of any visible relation, the same impression of unity is conveyed in the prevalence of close and curious analogies which are not the less striking when the cause or the reason of them is unknown.

I propose in this chapter to specify some of the signs of unity which the study of Nature has more definitely revealed, and consider how far they carry us.

There is one sign of unity which, of itself, carries us very far indeed. It is the sign given to us in the ties by which this world of ours is bound to the other worlds around it. There is no room for fancy here. The truths which have been reached in this matter have been reached by the paths of rigorous demonstration. This earth is part of the vast mechanism of the heavens. The force, or forces, by which that mechanism is governed are forces which prevail not only in our own solar system, but, as there is reason to believe, through all Space, and are determining, as astronomers tell us, the movement of our sun, with all its planets, round some distant centre, of which we know neither the nature nor the place. Moreover, these same forces are equally prevailing on the surface of this earth itself. The whole of its physical phenomena are subject to the conditions which they impose.

If there were no other indications of unity than this, it would be almost enough. For the unity which is implied in the mechanism of the heavens is indeed a unity which is all-embracing and complete. The structure of our own bodies, with all that depends upon it, is a structure governed by, and therefore adapted to, the same force of gravitation which has determined the form and the movement of myraids of worlds. Every part of the human organism is fitted to conditions which would all be destroyed in a moment if the forces of gravitation were to change or fail. It is, indeed, evident that a force such as this must govern the whole order of things in which it exists at all. Every other force must work, or be worked, in subordination to it.

Nor is gravitation the only agency which brings home to us the unity of the conditions which prevail among the worlds. There is another: Light—that sweet and heavenly messenger which comes to us from the depths of Space, telling us all we know of other worlds, and giving us all that we enjoy of life and beauty on our own. And there is one condition of unity revealed by Light which is not revealed by gravitation. For, in respect to gravitation, although we have an idea of the *measure*, we have no idea of the *method*, of its operation. We know with precision the numerical rules which it obeys, but we know nothing whatever of the way in which its work is done. But in respect to Light, we have an idea not only of the measure, but of the mode of its operation. In one sense, of course, Light is a mere sensation in ourselves. But when we speak of it as an external thing, we speak of the cause of that sensation. In this sense, Light is a wave or an undulatory vibration, and such vibrations can only be propagated in a medium which, however thin, must be material. Light, therefore, reveals to us the fact that we are united with the most distant worlds, and with all intervening space, by some ethereal atmosphere, which embraces and holds them all. Moreover, the enormous velocity with which the vibrations of this atmosphere are propagated proves that it is a substance of the closest continuity, and of the highest tension. The tremors which are imparted to it by luminous bodies rush from particle to particle at the rate of 186,000 miles in a second of time; and thus, although it is impalpable, intangible, and imponderable, we know that it is a medium infinitely more compact than the most solid substance which can be felt and weighed. It is very difficult to conceive this, because the waves or tremors which constitute Light are not recognizable by any sense but one: and the impressions of that sense give us no direct information on the nature of the medium by which those impressions are produced. We cannot see the luminiferous medium except when it is in motion, and not even then, unless that motion be in a certain direction toward ourselves. When this medium is at rest we are in utter darkness, aud so are we also when its movements are rushing past us, but do not touch us. The luminiferous medium is, therefore, in itself, invisible; and its nature can only be arrived at by pure reasoning—reasoning, of course, founded on observation, but observation of rare phenomena, or of phenomena which can only be seen under those conditions which Man has invented for analyzing the operations of his own most glorious sense. And never, perhaps, has Man's inventive genius been more signally displayed than in the long series of investigations which first led up to the conception, and have now furnished the proof, that Light is nothing but the undulatory movement of a substantial medium. It is very difficult to express in language the ideas upon the nature of that medium which have been built up from the facts of its behavior. It is difficult to do so, because all the words by which we express the properties of Matter refer to its more obvious phenomena —that is to say, to the direct impressions which Matter makes upon the senses. And so, when we have to deal with forms of Matter which do not make any impressions of the same kind—forms of matter which can neither be seen, nor felt, nor handled, which have neither weight, nor taste, nor smell, nor aspect—we can only describe them by the help of analogies as near as we can find. But as regards the qualities of the medium which causes the sensation of Light, the nearest analogies are remote, and what is worse, they compel us to associate ideas which elsewhere are so dissevered as to appear almost exclusive of each other. It is now more than half a century since Dr. Thomas Young astonished and amused the scientific world by declaring of the luminiferous medium that we must conceive of it as finding its way through all Matter as freely as the air moves through a grove of trees. This suggests the idea of an element of extreme tenuity. But that element cannot be said to be thin in which a wave is transmitted with the enormous velocity of Light. On the contrary, its molecules must be in closest contact with each other when a tremor is carried by them through a thickness of 186,000 miles in a single second. Accordingly, Sir J. Herschel has declared that the luminiferous ether must be conceived of not as an air, nor as a fluid, but rather as a solid—"in this sense at least, that its particles cannot be supposed as capable of interchanging places, or of bodily transfer to any measurable distance from their own special and assigned localities in the universe."[2] Well may Sir J. Herschel add that "this will go far to realize (in however unexpected a form) the ancient idea of a chrystalline orb." and thus the wonderful result of all investigation is that this earth is in actual rigid contact with the most distant worlds in space—in rigid contact, that is to say, through a medium which touches and envelops all, and which is incessantly communicating from one world to another the minutest vibrations it receives.

The laws, therefore, and the constitution of Light, even more than the law of gravitation, carry up to the highest degree of certainty our conception of the Universe as one;—one, that is to say, in virtue of the closest mechanical connection, and of the prevalence of one universal medium.

Moreover, it is now known that this medium is the vehicle not only of Light but also of Heat, whilst it has likewise a special power of setting up, or of setting free, the mysterious action of chemical affinity. The beautiful experiments have become familiar by which these three kinds of ethereal motion can be separated from each other in the solar spectrum, and each of them can be made to exhibit its peculiar effects. With these again the forces of galvanism and electricity have some very intimate connection, which goes far to indicate like methods of operation in some prevailing element. Considering how all the forms of Matter, both in the organic and in the inorganic worlds, depend on one or other, or on all of these—considering how Life itself depends upon them, and how it flickers or expires according as they are present in due proportion—it is impossible not to feel that in this great group of powers, so closely bound up together, we are standing very close indeed to some pervading, if not universal, agency in the mechanism of Nature.

This close connection of so many various phenomena with different kinds of movement in a single medium is by far the most striking and instructive discovery of modern science. It supplies to some extent a solid physical basis, and one veritable cause, for part, at least, of the general impression of unity which the aspects of Nature leave upon the mind. For all work done by the same implement generally carries the mark of that implement, as it were of a tool, upon it. Things made of the same material, whatever they may be, are sure to be like in those characteristics which result from identical or from similar properties and modes of action. And so far, therefore, it is easy to understand the constant and close analogies which prevail in that vast circle of phenomena which are connected with Heat, Light, Electricity, Chemical and Vital Action.

But although the employment of one and the same agency in the production of a variety of effects is, no doubt, one cause of the visible unity which prevails in Nature, it is not the only cause. The same close analogies exist where no such identity of agency can be traced. Thus the mode in which the atmosphere carries Sound is closely analogous to to the mode in which the ether carries light. But the ether and the atmosphere are two very different agents, and the similarity of the laws which the undulations of both obey is due to some other and some more general cause of unity than identity of material. This more general cause is to be found, no doubt, in one common law which determines the forms of motion in all Matter, and especially in highly elastic media.

But, indeed, the mere physical unity which consists in the action of one great vehicle of power, even if this were more universally prevalent than it is known to be, is but the lowest step in the long ascent which carries us up to a unity of a more perfect kind. The means by which some one single implement can be made to work a thousand different effects, not only without interference, and without confusion, but with such relations between it and other agents as to lead to complete harmonies of result, are means which point to some unity behind and above the implement itself—that is to say, they point to some unity in the method of its handling, in the management of the impulses which, receiving, it conveys, and in the arrangement of the materials on which it operates.

No illustration can be given of this higher kind of unity which is half so striking as the illustration which is afforded by the astonishing facts now familiar as to the composition of solar light. When we consider that every color in the spectrum represents the motion of a separate wave or ripple, and that in addition to the visible series there are other series, one at each end of the luminous rays, which are non-luminous, and therefore invisible—all of which consist of waves equally distinct; when we consider farther that all these are carried simultaneously with the same speed across millions of miles; that they are separable, and yet are never separated; that they are more accurately together, without jostling or confusion, in perfect combination, yet so that each shall be capable of producing its own separate effect—it altogether transcends our faculties of imagination to conceive how movements of such infinite complication can be united in one such perfect order.

And be it observed that the difficulty of conceiving this is not diminished, but increased, by the fact that these movements are propagated in a single medium; because it is most difficult to conceive how the particles of the medium can be so arranged as to be capable of conveying so many different kinds of motion with equal velocities and at the

[2] "Familiar Lectures on Scientific Subjects," p. 285.

same instant of time. It is clear that the unity of effect which is achieved out of this immense variety of movements is a unity which lies altogether behind the mere unity of material, and is traceable to some one order of arrangement under which the original impulses are conveyed. We know that in respect to the waves of Sound, the production of perfect harmonies among them can only be attained by a skillful adjustment of the instruments, whose vibrations are the cause and the measure of the aerial waves which, in their combination, constitute perfect music. And so, in like manner, we may be sure that the harmonies of Heat, Light, and Chemical Action, effected as they are amongst an infinite number and variety of motions, very easily capable of separation and disturbance, must be the result of some close adjustment between the constituent element of the conveying medium and the constituent elements of the luminous bodies, whose complex, but joint, vibrations constitute that embodied harmony which we know as Light. Moreover, as this adjustment must be close and intimate between the properties of the ether and the nature of the bodies whose vibrations it repeats, so also must the same adjustment be equally close between these vibrations and the properties of Matter on which they exert such a powerful influence. And when we consider the number and the nature of the things which this adjustment must include, we can, perhaps, form some idea what a bond and bridge it is between the most stupendous phenomena of the heavens and the minutest phenomena of earth. For this adjustment must be perfect between these several things—first, the flaming elements in the sun which communicate the different vibrations in definite proportion; next, the constitution of the medium, which is capable of conveying them without division, confusion, or obstruction; next, the constitution of our own atmosphere, so that neither shall it distort, nor confuse, nor quench the waves; and, lastly, the constitution of those forms of Matter upon earth which respond, each after its own laws, to the stimulus it is so made as to receive from the heating, lighting, and actinic waves.

In contemplating this vast system of adjustment, it is important to analyze and define, so far as we can, the impression of unity which it makes upon us, because the real scope and source of this impression may very easily be mistaken. It has been already pointed out that we can only see likeness by first seeing difference, and that the full perception of that in which things are unlike is essential to an accurate appreciation of that in which they are the same. The classifying instinct must be strong in the human mind, from the delight it finds in reducing diverse things to some one common definition. And this instinct is founded on the power of setting differences aside, and of fixing our attention on some selected conditions of resemblance. But we must remember that it depends on our width and depth of vision whether the unities which we thus select in Nature are the smallest and the most incidental, or whether they are the largest and the most significant. And, indeed, for some temporary purposes—as, for example, to make clear to our minds the exact nature of the facts which science may have ascertained—it may be necessary to classify together, as coming under one and the same category, things as different from each other as light from darkness. Nor is this any extreme or imaginary case. It is a case actually exemplified in a lecture by Professor Tyndall, which is entitled "The Identity of Light and Heat." Yet those who have attended the expositions of that eminent physical philosopher must be familiar with the beautiful experiments which show how distinct in another aspect are Light and Heat; how easily and how perfectly they can be separated from each other; how certain substances obstruct the one and let through the other; and how the fiercest heat can be raging in the profoundest darkness. Nevertheless, there is more than one mental aspect, there is more than one method of conception, in terms of which these two separable powers can be brought under one description. Light and Heat, however different in their effects—however distinct and separable from each other—can both be regarded as "forms of motion" among the particles of Matter. Moreover, it can be shown that both are conveyed or caused by waves, or undulatory vibrations in one and the same ethereal medium. And the same definition applies to the chemical rays, which again are separable and distinct from the rays both of Light and Heat.

But although this definition may be correct as far it goes, it is a definition nevertheless which slurs over and keeps out of sight distinctions of a fundamental character. In the first place, it takes no notice of the absolute distinction between Light or Heat considered as sensations of our organism, or as states of consciousness, and Light or Heat considered as the external agencies which produce these sensations in us. Sir W. Grove has expressed a doubt whether it is legitimate to apply the word "Light" at all to any rays which do not excite the sense of vision. This, however, is not the distinction to which I now refer. If it be an ascertained fact, or if it be the only view consistent with our present knowledge, that the ethereal pulsations which do, and those which do not, excite in us the sense of vision, are pulsations exactly of the same kind and in exactly the same medium, and that they differ in nothing but in periods of time or length of wave, so that our seeing of them or our not seeing of them depends on nothing but the focusing, as it were, of our eyes, then the inclusion of them under the same word Light involves no confusion of thought. We should confound no distinction of importance, for example, by applying the same name to grains of sand which are large enough to be visible, and to those which are so minute as to be wholly invisible even to the microscope. And if a distinction of this nature—a mere distinction of size, or of velocity, or of form of motion, were the only distinction between light and heat—it might be legitimate to consider them as identical, and to call them by the same name. But the truth is there are distinctions between them of quite another kind. Light, in the abstract conception of it, consists in undulatory vibrations in the pure ether, and in these alone. They may or may not be visible—that is to say, they may or may not be within the range of our organs of vision, just as a sound may or may not be too faint and low, or too fine and high, to be audible to our ears. But the word "heat" carries quite a different meaning, and the conception it conveys could not be covered under the same definition as that which covers light. Heat is inseparably associated in our minds with, and does essentially consist in, certain motions, not of pure ether, but of the molecules of solid or ponderable matter. These motions in solid or ponderable matter are not in any sense identical with the undulatory motions of pure ether which constitute light; consequently when physicists find themselves under the necessity of defining more closely what they meant by the identity of heat and light, they are obliged to separate between two different kinds of heat—that is to say, between two wholly different things, both covered under the common name of heat—one of which is really identical in kind with light, and the other of which is not. "Radiant" heat is the kind, and the only kind of heat, which comes under the common definition. "Radiant" heat consists in the undulatory vibrations of pure ether which are set up or caused by those other vibrations in solid substances or ponderable matter, which are heat more properly so called. Hot bodies communicate to the surrounding ethereal medium vibrations of the same kind with light, some of these being, and others not being, luminous to our eyes. Thus we see that the unity or close relationship which exists between heat and light is not a unity of sameness or identity, but a unity which depends upon and consists in correspondences between things in themselves different. It has been suggested that the facts of nature would be much more clearly represented in language if the old word "Caloric" were revived, in order to distinguish one of the two very different things which are now confounded under the common term "Heat"—tha is to say, heat considered as molecular vibration in solid or ponderable matter. and heat considered as the undulatory vibrations of pure ether which constitute the "heat" called "radiant." Adopting this suggestion, the relations between light and heat, as these relations are now known to science, may be thrown into the following propositions, which are framed for the purpose of exhibiting distinctions not commonly kept in view:

I. Certain undulatory vibrations in pure ether alone are light ether (1) visible, or (2) invisible.

II. These undulatory vibrations in pure ether alone not Caloric.

III. No motions of any kind in pure ether alone are Caloric.

IV. Caloric consists in certain vibratory motions in the molecules of ponderable matter or substances grosser than the ether, and these motions are not undulatory.

V. The motions in ponderable matter which constitute Caloric set up or propagate in pure ether the undulatory vibrations which constitute light.

VI. Conversely the undulatory vibrations in pure ether which constitutes Light set up or propagate in grosser matter the motions which are Caloric.

VII. But the motions in pure ether which are Light cannot set up or propagate in all ponderable matter equally the motions which are Caloric. Transparent substances allow the ethereal undulations to pass through them with very little Caloric motion being set up thereby; and if there were any substance perfectly transparent, no Caloric motion would be produced at all.

VIII. Caloric motions in ponderable matter can be and are set up or propagated by other agencies than the undulations of ether, as by friction, percussion, &c.

IX. Caloric, therefore, differs from Light in being (1) motion in a different medium or in a different kind of matter; (2) in being a different kind of motion; (3) in being producible without, so far as known, the agency of Light at all. I say "so far as known," because as the luminiferous ether is ubiquitous, or as, at least, its absence cannot anywhere be assumed, it is possible that in the calorific effects of percussion, friction, &c., undulations of the ether may be always an essential condition of the production of Caloric.

It follows from these propositions that there are essential distinctions between Light and Heat, and that the effect of luminiferous undulations or "Radiant" Heat in producing Caloric in ponderable matter depends entirely upon, and varies greatly in accordance with, the constitution or structure of the substances through which it passes, or upon which it plays.

The same fundamental distinction applies to those ethereal undulations which produce the effects called Chemical. No such effects can be produced upon substances except according to their special structure and properties. Their effect, for example, upon living matter is absolutely different from the effect they produce upon matter which does not possess vitality. The forces which give rise to chemical affinity are wholly unknown. And so are those which give rise to the peculiar phenomena of living matter. The rays which are called Chemical may have no other part in the result than that of setting free the molecules to be acted upon by the distinct and separate forces which are the real sources of chemical affinity.

What, then, have we gained when we have grouped together, under one common definition, such a variety of movements and such a variety of corresponding effects? This is not the kind of unity which we see and feel in the vast system of adjustments between the sun, the medium conveying its vibrations, and the effect of these on all the phenomena of earth. The kind of unity which is impressed upon us is neither that of a mere unity of material, nor of identity in the forms of motion. On the contrary, this kind of unity among things so diverse in all other aspects is a bare intellectual apprehension, only reached as the result of difficult research, and standing in no natural connection with our ordinary apprehension of physical truth. For our conception of the energies with which we have to deal in Nature must be molded on our knowledge of what they do, far more than on any abstract definition of what they are; or rather, perhaps, it would be more correct to say that our conception of what things are can only be complete in proportion as we take into our view the effects which they produce upon other things around them, and especially upon ourselves, through the organs by which we are in contact with the external world. If in these effects any two agencies are not the same—if they are not even alike—if, perhaps, they are the very antithesis of each other—then the classification which identifies them, however correct it may be, as far as it goes, must omit some characteristics which are much more essential than those which it includes. The most hideous discords which can assail the ear, and the divinest strains of heavenly music, can be regarded as identical in being both a series of sonorous waves. But the thought, the preparation, the concerted design—in short, the unity of mind and of sentiment, on which the production of musical harmony depends, and which it again conveys with matchless power of expression to other minds—all this higher unity is concealed and lost if we do not rise above the mere mechanical definition under which discords and harmonies can nevertheless be in this way correctly classed together. And yet so pleased are we with discoveries of this kind, which reduce, under a common method of conception, things which we have been accustomed to regard as widely different, that we are apt to be filled with conceit about such definitions, as if we had reached in them some great ultimate truth on the nature of things, and as if the old aspects in which we had been accustomed to regard them were by comparison almost deceptive; whereas, in reality, the higher truth may well have been that which we have always known, and the lower truth that which we have recently discovered. The knowledge that Light and Heat are separable, that they do not always accompany each other, is a truer and juster conception of the relation in which they stand to us, and to all that we see around us, than the knowledge that they are both the same in respect of their being both "modes of motion." To know the work which a machine does is a fuller and higher knowledge than to know the nature of the materials of which its parts are composed, or even to perceive and follow the kind of movement by which its effects are produced. And if there be two machines which, in respect to structure and movement and material, are the same, or closely similar, but which, nevertheless, produce totally different kinds of work, we may be sure that this difference is the most real and the most important truth respecting them. The new aspects in which we see their likeness are less full and less adequate than the old familiar aspects in which we regard them as dissimilar.

But the mind is apt to be enamored of a new conception of this kind, and to mistake its place and its relative importance in the sphere of knowledge. It is in this way, and in this way only, that we can account for the tendency among some scientific men to exaggerate beyond all bounds the significance of the abstract definitions which they reach by neglecting differences of work, of function, and of result, and by fixing their attention mainly on some newly discovered likeness in respect to form, or motion, or chemical composition. It is thus that because a particular substance called "Protoplasm" is found to be present in all living organisms, an endeavor follows to get rid of Life as a separate conception, and to reduce it to the physical property of this material. The fallacy involved in this endeavor needs no other exposure than the fact that, as the appearance and the composition of this material is the same whether it be dead or living, the Protoplasm of which such transcendental properties are affirmed has always to be described as "living" protoplasm. But no light can be thrown upon the facts by telling us that life is a property of that which lives. The expression for this substance which has been invented by Professor Huxley is a better one—the "Physical Basis of Life." It is better because it does not suggest the idea that Life is a mere physical property of the substance. But it is, after all, a metaphor which does not give an adequate idea of the conceptions which the phenomena suggest. The word "basis" has a distinct reference to a mechanical support, or to the principal substance in a chemical combination. At the best, too, there is but a distant and metaphorical analogy between these conceptions and the conceptions which are suggested by the connection between Protoplasm and Life. We cannot suppose Life to be a substance supported by another. Neither can we suppose it to be like a chemical element in combination with another. It seems rather like a force or energy which first works up the inorganic materials into the form of protoplasm, and then continues to exert itself through that combination when achieved. We call this kind of energy by a special name, for the best of all reasons, that it has special effects, different from all others. It often happens that the philosophy expressed in some common form of speech is deep and true, whilst the objections which are made to it in the name of science are shallow and fallacious. This is the case with all those phrases and expressions which imply that Life and its phenomena are so distinguishable from other things that they must be spoken of by themselves. The objection made by a well-known writer,[3] that we might as well speak of "a watch force" as of "a vital force," is an objection which has no validity, and is chargeable with the great vice of confounding one of the clearest distinctions which exist in Nature. The rule which should govern language is very plain. Every phenomenon or group of phenomena which is clearly separate from all others, should have a name as separate and distinctive as itself. The absurdity of speaking of a "watch force" lies in this—that the force by which a watch goes is not separable from the force by which many other mechanical movements are effected. It is a force which is otherwise well-known and can be fully expressed in other and more definite terms. That force is simply the elasticity of a coiled spring. But the phenomena of Life are not due to any force which can be fully and definitely expressed in other terms. It is not purely chemical, nor purely mechanical, nor purely electrical, nor reducible to any other more simple and elementary conception. The popular use, therefore, which keeps up separate words and phrases by which to describe and designate the phenomena of Life, is a use which is correct and thoroughly expressive of the truth. There is nothing more fallacious in philosophy than the endeavor by mere tricks of language, to suppress and keep out of sight the distinctions which Nature proclaims with a loud voice.

It is thus, also, that because certain creatures widely separate in the scale of being may be traced back to some embryonic stage, in which they are undistinguishable, it has become fashionable to sink the vast differences which must lie hid under this uniformity of aspect and of material composition under some vague form of words in which the mind makes, as it were, a covenant with itself not to think of such differences as are latent and invisible, however important we know them to be by the differences of result to which they lead. Thus it is common now to speak of things widely separated in rank and functions being the same, only "differentiated," or "variously conditioned." In these, and in all similar cases, the differences which are unseen, or which, if seen are set aside, are often of infinitely greater importance than the similarities which are selected as the characteristics chiefly worthy of regard. If, for example, in the albumen of an egg there be no discernible differences either of structure or of chemical composition, but if, nevertheless, by a mere application of a little heat, part of it is "differentiated" into blood, another part of it into flesh, another part of it into bone, another part of it into feathers, and the whole into one perfect organic structure, it is clear that any purely chemical definition of this albumen, or any purely mechanical definition of it, would not merely fail of being complete, but would absolutely pass by and pass over the one essential characteristic of vitality which makes it what it is, and determines what it is to be in the system of Nature.

Let us always remember that the more perfect may be the apparent identity between two things which afterwards become widely different, the greater must be the power and value of those invisible distinctions—of those unseen factors—which determine the subsequent divergence. These distinctions are invisible, not merely because our methods of analysis are too coarse to detect them, but because apparently they are of a nature which no physical dissection and no chemical analysis could possibly reveal. Some scientific men are fond of speaking and thinking of these invisible factors as distinctions due to differences in "molecular arrangement," as if the more secret agencies of Nature gave us the idea of depending on nothing else than mechanical arrangement—on differences in the shape or in the position of the molecules of matter. But this is by no means true. No doubt there are such differences—as far beyond the reach of the miscroscope as the differences which the microscope does reveal are beyond the reach of our unaided vision. But we know enough of the different agencies which must lie hid in things apparently the same to be sure that the divergences of work which these agencies produce do not depend upon, or consist in, mere differences of mechanical arrangement. We know enough of those agencies to be sure that they are agencies which do, indeed, determine both arrangement and composition, but do not themselves consist in either.

This is the conclusion to which we are brought by facts which are well known. There are structures in Nature which can be seen in the process of construction. There are conditions of matter in which its particles can be seen rushing under the impulse of invisible forces to take their appointed place in the form which to them is a law. Such are the facts visible in the processes of crystallization. In them we can see the particles of matter passing from one "molecular condition" to another; and it is impossible that this passage can be ascribed either to the old arrangement which is broken up, or to the new arrangement which is substituted in its stead. Both structures have been built up out of elementary materials by some constructive agency which is the master and not the servant—the cause and not the consequence of the movements which are effected, and of the arrangement which is their result. And if this be true of crystalline forms in the mineral kingdom, much more is it true of organic forms in the animal kingdom. Crystals are, as it were, the beginnings of Nature's architecture, her lowest and simplest forms of building. But the most complex crystalline forms which exist—and many of them are singularly complex and beautiful—are simplicity itself compared with the very lowest organism which is endowed with Life. In them, therefore, still more than in the formation of crystals, the work of "differentiation"—that is to say, the work of forming out of one material different structures for the discharge of different functions—is the work of agencies which are invisible and unknown; and it is in these agencies, not in the molecular arrangements which they cause, that the essential character and individuality of every organism consists. Accordingly in the development of seeds and of eggs, which are the germs of plants and animals respectively, the particles of matter can be traced moving, in obedience to forces which are unforseen, from "molecular conditions" which appear to be those of almost complete homogeneity to other molecular conditions which are of inconceivable complexity. In that mystery of all mysteries, of which physicists talk so glibly, the living "nucleated cell," the great work of creation may be seen in actual operation, not caused by "molecular condition," but determining it, and, from elements which to all our senses, and to all our means of investigation, appear absolutely the same, building up the molecules of Protoplasm, now into a sea-weed, now into a cedar of Lebanon, now into an insect, now into a fish, now into a reptile, now into a bird, now into a man. And in proportion as the molecules of matter do not seem to be the masters but the servants here, so do the forces which dispose of them stand out separate and supreme. In every germ this development can only be "after its kind." The molecules must obey; but no mere wayward or capricious order can be given to them. The formative energies seem to be as much under command as the materials upon which they work. For, invisible, intangible, and imponderable as these forces are—unknown and even inconceivable as they must be in their ultimate nature—enough can be traced of their working to assure that they are all closely related to each other, and belong to a system which is one. Out of the chemical elements of Nature, in numerous but definite combinations, it is the special function of vegetable life to lay the foundations of organic mechanism; whilst it is the special function of animal life to take in the materials thus supplied, and to build them up into the highest and most complicated stuctures. This involves a vast cycle of operations, as to the unity of which we cannot be mistaken—for it is a cycle of operations obviously depending on adjustments among all the forces both of solar and terrestrial physics—and every part of this vast series of adjustments must be in continuous and unbroken correlation with the rest.

Thus every step in the progress of science which tends to reduce all organisms to one and the same set of elementary substances, or to one and the same initial structure, only adds to the certainty with which we conclude that it is upon something else than composition, and upon something else than structure, that those vast differences ultimately depend which separate so widely between living things in rank, in function and in power. And although we cannot tell what that something is—although science does not as yet even

[3] Mr. G. H. Lewes.

tend to explain what the directive agencies are or how they work—one thing, at least, is plain: that if a very few elementary substances can enter into an untold variety of combinations, and by virtue of this variety can be made to play a vast variety of parts, this result can only be attained by a system of mutual adjustments as immense as the variety it produces, as minute as the differences on which it depends, and as centralized in direction as the order and harmony of its results. And so we come to understand that the unity which we see in Nature is that kind of unity which the mind recognizes as the result of operations similar to its own—not a unity which consists in sameness of material, or in identity of composition, or in uniformity of structure, but a unity which consists in similar principles of action—that is to say, in like methods of subordinating a few elementary forces to the discharge of special functions, and to the production, by adjustment, of one harmonious whole.

And of this unity, we who see it, and think of it, and speak of it—we are part. In body and in mind we belong to it, and are included in it. It is more easy to admit this as a general proposition than really to see it as truth and to accept all the consequences it involves. The habitual attitude of our thoughts is certainly not in accordance with it. We look on "Nature" as something outside of us—something on which we can look down, or to which we can look up, according to our mood; but in any case, something in which we are exceptions, and which we can and ought to regard from an external point of view. It may be well, therefore, to consider a little more carefully "Man's place in Nature"—his share and position in that unity which he sees and feels around him.

AN IMPROVED MICROTOME.

By WM. HAILES, M.D., Professor of Histology and Path. Anat., Albany Medical College.

This instrument is designed especially for use in the working laboratories of our medical schools and colleges, where large numbers of sections are required for microscopical examination.

It may be employed as a simple instrument or as a freezing microtome, arranged for ice and salt—ether spray, rhigoline, etc., etc.

The employment of ice and salt (coarse) is preferred, because it costs but little and freezes the mass solidly and quickly, and, if desired, 500 or 1000 sections can be obtained in a few moments' time of freezing is about seven minutes, except in very warm weather when it requires a few moments longer.

The instrument does not work so satisfactorily in warm weather, owing to the rapid melting of the surface of the preparation. It is absolutely necessary that the mass should be frozen solid, or the sections cannot be cut smoothly.

An extra freezer may be employed, and while one specimen is being cut the other may be frozen, and by exchanging cylinders (they being interchangeable), no delay is necessary to its continuous operation.

The art of cutting is readily acquired, and when the preparation is frozen it is the work of a few moments to obtain several hundred sections. Two hundred sections, or more, if desired, can be made each minute, and of a uniform thickness of about $\frac{1}{200}$ of an inch (thinner or thicker, from about $\frac{1}{2400}$ inch to about $\frac{1}{200}$ inch, according as pointer is set). See explanation of cut No. 1. The delivery, ease and rapidity with which they can be cut, must be seen in order to be appreciated. It is not necessary to remove the sections from the knife every time, but twenty or thirty may be permitted to collect upon the blade; they lie curled or folded up upon the knife, and when placed in water straighten themselves out perfectly in the course of a few hours. The knife I employ is an ordinary long knife from an amputating case. Perfectly fresh tissues may be cut without any previous preparation, using ordinary mucilage (acaciæ) to freeze in, but most specimens require special preparation. If preserved in Müller's fluid, alcohol, etc.,

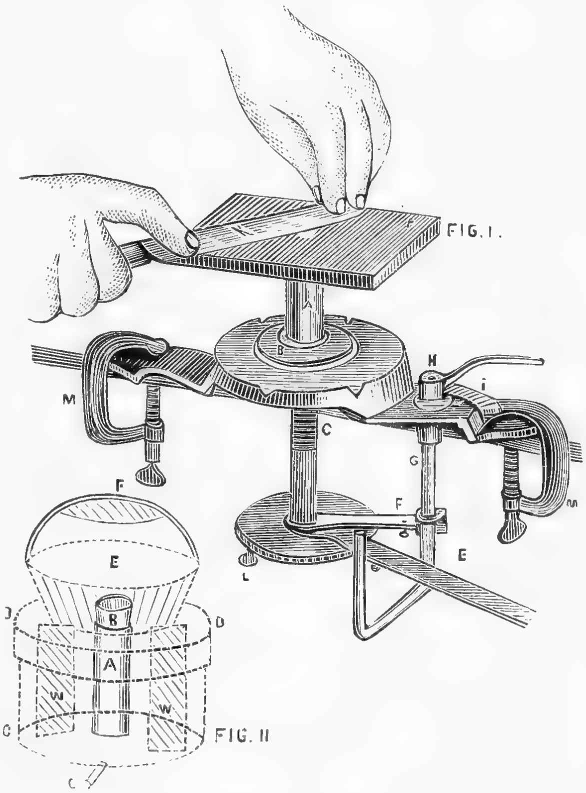


Fig. 1.—Poly-microtome (without freezing apparatus). A, small well, fitting on pyramidal bed-plate; B, pyramidal bed-plate containing different sizes; C, micrometer screw; D, ratchet-wheel attached to screw; E, lever actuating the micrometer screw by means of a pawl engaging in teeth of ratchet-wheel; F, arm carrying a dog, which prevents back motion of screw; G, regulator for limiting the throw of lever, and consequently governing the micrometer screw; H, lever nut for fixing regulator; I, index, with pointer and graduated scale, from $\frac{1}{2400}$ inch to $\frac{1}{200}$ inch; K, knife for cutting sections; L, knob to turn micrometer screw direct when pawls are detached; M, table clamp; T, table cf microtome, with glass top to facilitate cutting.

Fig. 2.—A, B, tube containing specimen which is surrounded by freezing mixture in tin receiver C, D; E, F, revolving hopper with wings; W, W, for stirring the ice; G, outlet for melted ice.

they require to be washed several hours in running water; then, according to the suggestion of my friend, Dr. David J. Hamilton, F. R. C. S., etc.,* University of Edinburgh, Scotland, the specimen is placed in a strong syrup (sugar, two ounces; water, one ounce), for twenty-four hours, and is removed to ordinary mucilage acaciæ for forty-eight hours, and is then cut in the freezing microtome.

The sections may be kept indefinitely in a preservative fluid: ℞ glycerinæ, ℥ iv; aquæ destil ℥ iv; acidi carbolici gtt, iij; boil and filter. (Dr. Hamilton). The addition of alcohol, ℥ ij, is advisable.

* See "A New Method of Preparing Large Sections of Nervous Centres for Microscopical Investigation."—*Journal of Anat. and Phys., Vol. XII.*

CHEMICAL NOTES.

THE OPTICAL PROPERTIES OF MIXTURES OF ISOMORPHOUS SALTS.—H. DUFET has verified the law which he communicated to the Academy, April 8, 1878, *i. e.*, that a crystal formed of a mixture of two isomorphous salts has indices of refraction, which vary continuously with its composition, so that the variation in the value of the index is proportional to the number of equivalents of one of the salts introduced into the mixture.

INFLUENCE OF TEMPERATURE ON THE DISTRIBUTION OF SALTS IN THEIR SOLUTIONS.—In all salts the concentration of the heated portion decreases and that of the cold part increases. The difference thus established increases with the original concentration. In the series of the alkaline chlorides the differerence is so much the greater for the same absolute concentration as the molecular weight is higher. The phenomenon seems to have no relation with the curve of solubility. C. SORÉT.

RISE OF THE ZERO-POINT IN MERCURIAL THERMOMETERS.—The zero-point rises further and more quickly in thermometers of "crystal" glass than in those free from lead. The rise of the zero-point is much more rapid at the outset, and tends probably towards a limit for a very prolonged heating at a fixed temperature. The effect of an elevated temperature renders the thermometer more stable under the influence of heat at any lower temperature.

J. M. CRAFTS.

DEVELOPMENT BY PRESSURE OF POLAR ELECTRICITY IN HEMIHEDRAL CRYSTALS WITH INCLINED SURFACES.—Whatever may be the determining cause, whenever a hemihedral non-conducting crystal with inclined surfaces occurs, there is a formation of electric poles in a certain direction; whenever the crystal expands the disengagement of electricity takes place in an opposite direction.

MM. JACQUES and PIERRE CURIE.

ACTION OF PERMANGANATE UPON POTASSIUM CYANIDE.—This reaction produces much nitrite and a little urea in an alkaline medium, whilst, if the liquid is acidified with sulphuric acid, urea is formed in abundance. The simultaneous formation of two incompatible compounds, urea and nitrous acid, under the influence of permanganate, shows that the nitrogen of the cyanogen is exposed at once to an oxidising action and to hydrogenisation.

E. BAUDRIMONT.

COMPARATIVE SOLUBILITY OF LEAD PHOSPHATE AND ARSENIATE IN DILUTE ACETIC ACID.—One part of lead arseniate dissolves in 2703.05 parts of dilute acetic acid at 38.94 per cent. One part of lead phosphate requires only 782.90 parts of the same acid. ARMAND BERTRAND.

CAUSE OF THE ACID REACTION OF ANIMAL TISSUES AFTER DEATH.—The acid reaction is due to a decomposition of the fluids in the tissues effected immediately after death by the action of Schizomycetes. At first volatile fatty acids appear to be derived from the incipient decomposition of the albumen, speedily followed by the two lactic acids produced from glycogen. The richer a tissue in carbohydrates, the longer this acid reaction prevails after death, as in the liver, the muscles and the lungs. It is briefest and faintest in the pancreas. In the later hours of putrefaction, the lactic acids disappear and are succeeded by succinic acid. Sooner or later an alkaline reaction sets in throughout the tissues, much ammonia being evolved from the decomposition of the albumen.

MARIE EKUNINA.

CONSTITUTION OF THE SALTS OF ROSANILINE AND OF ANALOGOUS COLOURING-MATTERS.—If rosaniline is a triamidic aromatic carbinol its salts will be of two classes: the first series is to be regarded as the ether of a tertiary aromatic alcohol; the second series comprises the salts of this ether, which is itself a triacid amine.

A. ROSENSTIEHL.

RESIN OF PALISANDER WOOD.—A. TERREIL and A. WOLFF ascribe to this resin the composition $C_{24}H_{21}O_{6}$. It is very soluble in alcohol in all proportions, less soluble in ether, chloroform, and carbon disulphide, and insoluble in water. Nitric acid transforms it into an acid, which crystallizes in very fine orange-yellow needles, united in tufts.

SOLUBILITY OF RECENTLY PRECIPITATED CARBONATE OF LIME IN AMMONIACAL SALTS IN PRESENCE OF AN EQUIVALENT PROPORTION OF SODIUM CHLORIDE.—At the temperature of 10° to dissolve 1 grm. calcium carbonate there are required:

Ammonium hydrochlorate..................	13 980 grms.
" sulphate	8.380 "
" nitrate............................	14.438 "

ARMAND BERTRAND.

PURIFICATION AND REFINING OF FATTY MATTERS.—To determine whether an oil is pure, M. OCTAVE ALLAIRE takes a piece of carbonate of soda (crystal), the size of a nut, dissolves it in its own bulk of water, and shakes it up with the oil under examination in a bottle. If the oil becomes turbid, and gives, on settling, a solid bulky deposit, it has been badly purified. Oils which act upon the metal of lamps and form deposits of verdigris are also to be rejected as impure. Commercial samples often contain 10 to 15 per cent. of free oleic acid.

REMARKS ON THE PLATINUM SULPHOCYANIDE OF V. MARCAGNO.—This body is not a platinum sulphocyanide, but a potassium platino-sulphocyanide, having in its anhydrous state been long ago analysed by M. Buckton. Nor does it yield a proof of the octo-atomicity of platinum, which in this case, as in many others, is tetratomic.

G. WYROUBOFF.

A COMPOUND OF TITANIUM TETRACHLORIDE AND ACETYLE CHLORIDE.—On mixing these two bodies the compound in question is immediately precipitated in small yellow brilliant spangles resembling lead iodide. On exposure to moisture these crystals undergo a change, liberating hydrochloric acid. They may be preserved in dry air, or preferably in dry hydrochloric acid. They melt at 25° to 30° and crystallize on cooling. In contact with alkaline solutions they are decomposed, forming an alkaline acetate and chloride, titanic acid which is precipitated, and water. The analysis of the crystals yielded results agreeing with the formula $C_2H_3OCl+TiCl_4$. ARMAND BERTRAND.

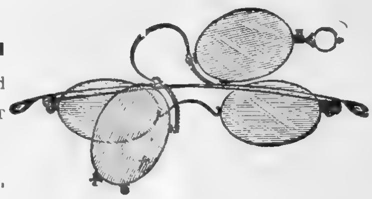
CERTAIN PROPERTIES OF MIXTURES OF METHYL CYANIDE WITH COMMON ALCOHOL AND METHYLIC ALCOHOL.—In order to separate methyl cyanide from alcohol it is necessary to submit the mixture to fractional distillation in order to classify the products; then to dissolve the largest possible quantity of calcium chloride in the mixture, boiling at the lowest temperature in order to absorb the alcohol; then to distil again in the water-bath, and to submit again the product thus obtained to fractional distillation. A very rich cyanide is thus obtained, from which the last traces of alcohol are eliminated by distillation over a small quantity of phosphoric anhydride, and by rectification to remove the small quantity of ethyl oxide and acetate which arise from the reaction of the phosphoric acid.

C. VINCENT and B. DELACHANAL.

DETERMINATION OF UREA BY SODIUM HYPOBROMITE.—C. MEHU criticises the memoirs of M. Fauconnier and M. Jay (*Bulletin de la Soc. Chimique*, xxxiii., pp. 102 and 105). In opposition to the former of these chemists he finds that the presence of cane-sugar distinctly augments the quantity of nitrogen evolved from urea by means of sodium hypobromite. In opposition to M. Jay he considers that it is easy to meet with starch syrups, which give off merely an insignificant quantity of ammonia if heated with caustic soda.

A SEALED paper from the firm of Scheurer-Rott, opened at the last session of l'Académie des Sciences, refers to an improvement in alizarin steam reds, by printing upon pieces previously prepared with emulsive oil, mixed in certain cases with hypochlorite of soda. For producing cadmium sulphide directly upon the fibre, M. Schmid prints a mixture of arsenious acid, sulphur, sodium acetate, and cadmium nitrate. A fine yellow is obtained by steaming for 1 to 2 hours and an orange is produced by increasing the sodium acetate.

A paper by M. Ziegler was opened, recommending an addition of arsenic or boracic acid to the colour beck in dyeing madder reds and roses.



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A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.

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SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

Published at

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P. O. Box 3838.

SATURDAY, OCTOBER 16, 1880.

We have for several years entertained a favorable opinion regarding the advisability of establishing a well equipped observatory for the almost exclusive purpose of astronomical discovery. One has only to recount the labors of American astronomers during a brief term of years to remark the great advancement of their science, which has resulted from the direction of energy toward this end. Professor Bond's discovery of a new satellite and a dusky ring to the planet Saturn; Mr. Burnham's well-known discoveries of new double stars; the discovery of the companion of the bright star Sirius by Mr. Alvan G. Clark; the discovery of fifty or sixty small planets between Mars and Jupiter by Dr. Peters and Professor Watson; the independent discovery of three or four comets by Professor Swift; the discoveries of intra-Mercurial planets, at the time of the eclipse of 1878, by Professors Watson and Swift; the extraordinary discovery of the two satellites of Mars by Professor Hall; the brilliant spectroscopic discoveries by Dr. Henry Draper of the existence of oxygen in the sun, and of the inherent heat of the planet Jupiter—are recalled at once. We might add greatly to the list without difficulty; but that is not necessary for the support of the belief that astronomers have not discovered all there is to discover in the solar system even, although their labors have been very arduous, and their means of research most powerful. We should be inclined to predict a scientific record of great importance and usefulness for any observatory of high instrumental capacity, which should set out upon a line of systematic observation, with reference to astronomical discovery simply. It is gratifying, therefore, to learn that the new observatory, now in process of erection at Rochester, N. Y., would seem to be dedicated to this sort of work. Professor Lewis Swift, of that place, has, we believe, been installed the life director of that institution, constructing and endowed by the munificence of Mr. H. H. Warner, an enterprising merchant of Rochester, and entitled, from its founder, the Warner Observatory. About $50,000 will be expended in the construction of the observatory proper, and the connected structure. The Messrs. Clark, of Cambridgeport, are now making a large refracting telescope (aperture of the object-glass, sixteen inches) for this new observatory. We regret that, in the proposed construction of this edifice, the architect should, in some measure, have resorted to the former system of building observatories—that of mounting the great telescope upon a pier of masonry built high up from the surface of the ground. A series of properly conducted experiments will usually indicate, however, whether this method is free from objection in any particular case. We note a connected contrivance—hitherto unknown in astronomy—a passenger-elevator to the floor of the dome. We shall express the hope that the abundance of new devices with which this new observatory is to be supplied may not be marked, as is frequently the case, by a less amount of good astronomical work than is performed in observatories of like capacity, where nothing is for convenience and everything for pure utility.

A lecture on "Microphysiology" was recently delivered before the Polytechnic Association of New York, by a person having an unenviable reputation for making extravagant assertions on scientific questions. It has been widely reported by the public press, and we notice that a claim is made that the origin of Bacteria and minute forms of life in the atmosphere has been discovered by the lecturer.

It was also asserted at the same time that microscopical organisms can be developed in the laboratory under conditions which exclude atmospheric contact, a fact in direct contradiction to the exhaustive experiments of Tyndall and others.

The problems thus professed to be solved have defied the intelligent research of such men as Huxley, Dallinger, Beale, Sanderman and Bastian, aided by the most powerful and perfect objectives obtainable. The present assertions to the contrary will, therefore, be received with humor by those acquainted with the subject, if the mischief caused by such reckless statements be not considered.

The announcement made at the same time of the discovery, by the lecturer, of a new form of objective, the extended application of which nearly doubles the present limit of the magnifying power of microscopical objectives, requires but a passing notice.

This individual appears to have fallen into the error of supposing that the excellence of a microscope is to be determined by the greatness of its magnifying power. On the contrary that instrument must be considered the most efficient which renders the details of an object perceptible with the lowest power. Destinctness of definition, by which is meant the power of rendering all the minute lineaments clearly seen, is a quality of greater importance than mere magnifying power. Indeed, without this quality mere magnifying power ceases to have any value.

At present there is an honorable competition between Spencer and Tolles, of America, Powell and Lealend, of England, and Zeiss, of Germany, as to who shall produce the most perfect microscopal objectives; and it would be a difficult matter to decide which of these firms possesses the greatest merit in workmanship. Zeiss, with his oil immersion system, may have obtained the credit of a temporary advantage, but similar forms of objectives are now being manufactured in this and other countries with success.

These makers are bringing to bear on their work all the most recent discoveries in optical science, and if any advance is made in the magnifying power of objectives, we shall expect to find it produced by such skilled opticians.

PALÆONTOLOGICAL RESEARCHES.

BY PROF. HENRY S. WILLIAMS, Ph. D., Cornell University.

I.

Genesee Slate. Fauna and Flora of Station xxxiv. d., H. S. W.

On the eastern shore of Cayuga Lake, N. Y., near the head, is a fine exposure of the boundary strata of the Hamilton and Chemung periods. Careful examination has been made of the upper part of the Genesee slate as it occurs in Burdich's Ravine, the face of the high fall. (Station xxxiv. H. S. W.) Here the lowest Portage sandstone lies about 60 feet above the surface of the lake, and the characteristic Genesee slate follows immediately under it. The following species were obtained in the slate between four and five feet below the sandstone stratum, forming the base of the Portage group:

Discina lodensis, Van.—abundant.
Discina truncata, Hall—frequent.
Lingula spatulata, Van.
Lingula concentrica—(of Vanuxem's Rep't, but not Conrad's species). See beyond.
Tentaculites fissurella H.—abundant. (See beyond).
Leiorhynchus quadricostatus, Van.
Chonetes lepida, Hall.
Aviculopecten fragilis, Hall.
Orthoceras—(subulatum ?).
Ambocœlia umbonata, Con.
Avicula speciosa, Hall.
Impression of part of *Goniatites?*.
Plants, three well marked forms.

This fauna has several interesting forms in it.

The recurrence of Marcellus forms noticed by Hall, in Geol. 4th Dist. N. Y., p. 222, 1843, is seen to be more marked than was observed by him.

The *Tentaculites fissurella*, Hall, may prove to be *Styliola* (*2 p.*) but if so, the same form is repeated in the Genesee slate from the Marcellus shale.

It is difficult to be satisfied with the recognition of this form in *Styliola*, since annulated forms occur together with the smooth ones, and except in the annulations are not to be separated from the true *Styliola* forms. The shells are very frail and crushing may account for the longitudinal folds in part, as it does in some of the Orthoceratidæ.

This fact is noticed by Hall in the Marcellus forms (in Illustrations Der Fossils, Pl. xxvi.) and the "prevailing form," fig. 14, is the prevailing form in the Genesee, and among the specimens just collected the annulated forms do not differ in size from the smooth ones, and the latter are often larger.

Discina lodensis, Van. occurs in abundance, and with some variation, but the form called *D. truncata, H.* is distinct and does not show gradation into the former. Still this is also distinct from the Lingula which Vanuxem figured, but did not describe in Geol. of 3d Dist., N. Y., p. 168, fig. 4. Vanuxem refers the species to Conrad's *Lingula concentrica*, which is evidently a mistake since Conrad's species, *L. concentrica*, is from the Helderberg mountain, in limestone, and is $\frac{3}{4}$ inch long (see Geol. Rep't, N. Y., 1839, p. 64). The species found in association with *L. spatulata* is nearly 5 millemetres long and 3.3^{mm} broad, and the cardinal margin is broadly, evenly rounded, and not attenuated as in *spatulata.*

L. spatulata, Van. is nearer the size figured by both Hall and Vanuxem (from 4 to 4.5^{mm}) instead of approaching 7.5^{mm} ($\frac{3}{10}$ inch) as stated by Hall in the description (Pal. of N. Y., vol. 4, p. 13). These are of the ordinary size of *Lingula spatulata, Van.* as they have been observed by the author. The *Lingula concentrica* (of Van. not Con.), is distinguished from the *Discina truncata* by the absence of the indentation or truncation, and the extension of the margin beyond the umbo, as well as other characters not as easily observed.

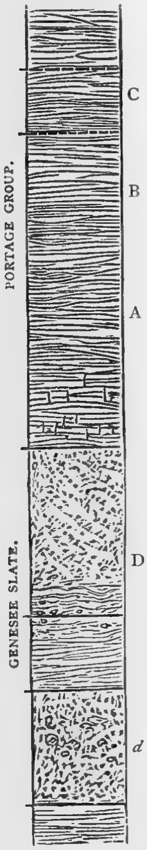
The Chonetes found is distinctly the *Chonetes lepida* of Hall, and not *setigera.* Still this may prove a variety of *setigera* upon further study; the two occor together in the Moscow shales and Marcellus, and in other strata of the Hamilton.

Only a single specimen of *Aviculopecten fragilis* was found, but this distinct and characteristic.

Ambocœlia umbonata, Con. was found in several beautifully preserved specimens.

And one of the dorsal valves is marked on the outer surface by concentric rows of minute short interrupted radiating lines, and when magnified resembles very closely the figure of *Spirifer prematura* on plate 33 of Hall's Pal., of N. Y., Pal. 4, fig. 32. Further study of these forms will probably develope interesting facts.

SECTION AT STATION XXXIV. H. S. W. SCALE 1^{cm} 1 ft.



Avicula speciosa, Hall. This species is represented by several specimens small and large, some of quite large size, but showing the characteristics of the Portage representatives.

This fact is especially interesting as the form has not been recorded from outside Portage rocks, and though this stratum is but a few feet below the base of the Portage, it is distinctly below and in the midst of characteristic Genesee slate.

It will be observed that this brings the species into the Hamilton Period. There are also some well marked plant-remains, one linear grass-like form, another sturdy branching form the relations of which have not been made out.

The dip of the base of the Portage in one direction was determined. Three stations were examined a thousand feet apart, and in nearly a straight line running North and South, and the elevation of the base of the stratum A of the Portage determined relative to the level of the lake.

St. XXXII.	base of A above lake level........	7 ft.
" XXXIII.	" " "	35½ "
" XXXIV.	" " "	57.9 "

These being 1000 feet apart, the dip is nearly 50 feet in 2000 feet.

The first 1000 feet showing 28½ feet and the second 1000 feet showing 22½ feet nearly. Thus the dip is not uniform, a fact further shown by a study of the rocks further South where the dip is much less, as was determined by careful survey of strata near the top of the Portage.

The accompanying diagram shows the general nature of the section at the three stations XXXII., XXXIII., and XXXIV. The scale is one centimeter to the foot. C, and A, and lower part of B contain concretionary nodules of iron pyrites; A, and C, sandstones, are separated by the shale B, which is more or less arenacious and differs decidedly from the Genesee slate below, which is the characteristic mud shale, black, and very fine in texture with arenaceous streaks in it toward the top.

The fossiliferous stratum whose fauna is described, is *d*, lithologically scarcely defined from the shales above and below.

THE TELEPHONE AMONG THE INDIANS.

The United States Fish Commission has lately connected, by telephone, its Salmon Hatching Stations at Baird, on the McCloud river, California, with the establishment for breeding the California trout five miles further up the river and the apparatus is now in thoroughly good working order. The Indians look on in blank amazement and call the instrument the *Klesch-teen*, or speaking spirit.

A REMARKABLE METEOR.

By Edwin F. Sawyer.

While engaged in recording meteors on the evening of Oct. 9th, I observed a very remarkable one at 10 h. 25 m. C. M. T., low down in the east, which calls for special mention. My attention was first attracted to what appeared a stationary meteor $>$ 1 mag. near γ (Gamma) Orionis, and of a deep orange color. While noting its accurate position, the meteor very slowly (motion hardly perceptible) began to descend towards the horizon, where it disappeared behind some houses. It remained perfectly stationary for at least a second after it was first observed, and it occupied 6 seconds in traversing an observed path of 10°. The meteor's brightness decreased slowly as it approached the point of disappearance being at this point of the 3d mag. No streak was observed. The exact point of appearance was at R.A. $76^{\circ}+5^{\circ}$ and it vanished at R.A. $76\frac{1}{2}^{\circ}-5^{\circ}$ near β Orioms (Rigel). Duplicate observations of this meteor would be of value.

Cambridgeport, Mass., Oct. 10, 1880.

THE "YELLOWS" OF THE PEACH TREE.

By Prof. T. J. Burrill, Illinois Industrial University.

A peculiar disease of the peach tree known as the "yellows," has long been the scourge of the principal peach growing districts of our country. Its appearance somewhat recently, in Michigan, caused much alarm, and since its occurrence throughout great orchards in some of the best fruit districts of the State, special attention has been called to it.

In "Science" for September 25th, 1880, page 162, there appeared an abstract of a paper read by me before the American Society of Microscopists at Detroit, upon the blight of pear and apple trees. In this paper I expressed the opinion that the "yellows" of the peach tree would be found due to an organism similar to that found to be the cause of the pear tree blight. This opinion was based upon my knowledge of the latter disease, upon the thoroughly confirmed contagious character of the "yellows," and upon the failure of competent investigators to find, after extended research, any thing like the ordinary parasitic fungi. It was long ago conceded by entomologists that the disease did not arise from the depredations of insects.

I am now able to confidently assert that this devastating disease of the peach is caused by Bacteria!

These minute, moving, living things are found in great numbers within the cells of the diseased tree. They are apparently specifically different from those of the pear tree, being comparatively much more slender. What I take to be the typical form—all vary considerably—is very nearly 1 μ by 3.5 μ (.0000343 in. by .0001202 in.), made up of several not very evident articulations. They rest in some stages nearly or quite motionless, and in this condition show a curious peculiarity of lying in ranks, side by side. In other periods of development they move in an unsteady, undulating manner with considerable rapidity; they turn, twist and tumble on their sides, on end, now drifting with the current, now swarming in an inextricable maze in the field of a first-class one-tenth objective.

As the Bacteria increase the starch grains, stored by the tree for its own nourishment disappear, and I doubt not further investigation will prove that, as in the blight of the pear and apple, butyric fermentation takes place. The diseased tree probably suffers in other ways from the presence of these minute parasites, but we may say with truth that it really starves to death. Its food, gathered from the earth and air, assimilated by the leaves and stored for immediate or future use, is ruthlessly seized upon and destroyed. No doubt this takes place at all times of the year, when the temperature of the surrounding air is considerably above the freezing point; but the Bacteria are probably most active in the summer time.

Judging from my experiments upon the pear tree, the destroyers only gain entrance to the tissues of the tree through wounds in the epidermis or bark; but it is possible that at the time of flowering they penetrate by way of the stigma, which is not protected by an impervious coating.

The cellulose tissue of the tree is not destroyed, and it is still a puzzle how the Bacteria, minute as they are, pass from cell to cell. As in the pear, it is probably a very slow process, and is not connected with the circulation of fluids in the tissues.

The discovery of Bacteria as the cause of disease in plants may prove a notable contribution to the "germ theory" of disease in animals.

THE ANTIQUITY OF MAN IN EASTERN AMERICA, GEOLOGICALLY CONSIDERED.*

By HENRY CARVILL LEWIS, A. M.

In the course of an investigation of the surface geology of southeast Pennsylvania, the writer has determined some facts, regarding one of the gravels, which, bearing directly upon the antiquity of man in America, become of interest. In former papers the writer has shown that the gravels of the Delaware Valley belong to several distinct ages; and if therefore at any place the remains of man are shown to occur, it will be important to know to which of these gravels they should be referred.

The surface formations of southeast Pennsylvania may be divided into five clays and four gravels. These are, beginning with the oldest: (1) *Jurasso-cretaceous* plastic clay, seen at Turkey Hill, Bucks Co.; (2) Tertiary clays of the "*Brandon Period,*" associated with the iron ore, kaolin and lignite of the Montgomery County Valley; (3) "*Bryn Mawr gravel,*" often found at elevations of 400 ft., characterized by the presence of an iron conglomerate and of pebbles of Potsdam, but never of Triassic rocks, and conjectured to be late Tertiary; (4) "*Branchtown clay*" of similar age; (5) "*Glassboro' gravel,*" of latest Pleiocene age, found also on the watershed in New Jersey, between the Atlantic and the Delaware, and known by its pebbles of Niagara limestone and of other fossiliferous rocks; (6) "*Philadelphia red gravel,*" of Champlain age, which contains numerous boulders of all materials, fragments of Triassic rocks, etc., which shows flow-and-plunge structures and wave action on a large scale, which rests on a decomposed gneiss, and which is confined to the river valley; (7) *Philadelphia brick clay*, which, with its boulders, rests upon the last, aud like it, appears to have been deposited by the waters of the melting northern glacier; (8) "*Trenton gravel,*" a sandy river gravel forming the bed of the Delaware; (9) the modern *alluvial mud* now forming in the tidewater swamps.

Of these formations, one of the least conspicuous at Philadelphia is that now called the Trenton gravel. It is a true river gravel, rising here but a few feet above the water, and forming a quicksand when below water level. It is of gray color, and contains pebbles composed entirely of the rocks which form the upper valley of the river. Unlike older gravels, it has very few quartz pebbles, and its pebbles are generally flat. In the middle of the river at Philadelphia it is 100 ft. deep. On tracing this gravel up the Delaware it is found to rise higher above the river and to extend farther back from it as we proceed up stream. Thus, at Bristol it extends two miles back from the river, and is bounded by a well-marked hill, upon which rest the older gravels. At Trenton, the limit of tidewater, the narrow upland portion of the valley begins; and from there up this gravel is shallow, and confined to the river bed. The oceanic gravels trend across New Jersey, and are no more seen. Two surface formations alone remain—the river gravel of past glacial age, and the brick clay, with its boulders, of champlain age. The first lies within the last, and both can be traced up to the great terminal moraine near Belvidere. It is to be especially noted that the Trenton gravel is newer than a drift of champlain age. It is in this Trenton gravel, and in this gravel only, that traces of man are found.

The Trenton gravel at the locality which gives it its name, is remarkably well exposed. Trenton is at the point where a long narrow valley with continuous downward slope opens out into a wide alluvial plain, and where the rocky floor of the river suddenly descends below ocean level. It is here that the bulk of a gravel, swept down the upper valley, would, on meeting tidewater, stop in its course, and with its boulders be heaped up in a mass, immediately afterward to be cut through by the river. It was thus that a cliff of gravel 50 ft. high was here formed, the river having cut through the gravel instead of flowing upon it, as at Philadelphia. This explanation dispenses with the necessity of assuming, as some geologists have done, the submergence of the land by the ocean at the time of the deposition of the gravel. That Southern N. J. was at that time dry land is shown by the fact that this gravel at Trenton extends inland a few miles only, and having filled up a bar in the ancient flooded river, is bounded by hills of the older gravel which forms Southern N. J.

There are many facts indicating that the Trenton gravel is a true river gravel and not a glacial moraine, which are detailed in the present paper. The absence of glacial marks on the rocks, the stratified character of the gravel, the topography of its banks, the comparative amount of its erosian and the character of its materials, all point to the conclusion that it was deposited by a great flood of the river; and this, when taken in connection with the fact that it lies within a channel cut through gravel deposited by the waters of the melting glacier indicates a past glacial and comparatively recent age of the Trenton gravel.

The important bearing of this fact upon the antiquity of man on the Delaware, which, as will appear, depends directly upon the age of this gravel, is here apparent. Calculations based upon the erosive power of running water show that the time necessary for the river to cut through this gravel down to the rock need not have been long. On the

* Read before the A. A. A. S., Boston, 1880.

other hand, no such flood as deposited this gravel has ever occurred within the historical epoch. No such large boulders are ever now carried down the river. No modern rainstorms could cause such a flood. It is difficult to assign any other cause than that of a melting glacier. Yet such a glacier could hardly be the great Northern glacier, for these gravels are much newer than those of the Champlain epoch. There is here evidence of a second and more recent glacier in the Delaware valley.

The hypothesis of a *second glacial epoch* seems to explain all the facts observed. A similar period in Europe—the reindeer period—is supported by many facts. Should such a period not be traced in America, the date of the melting glacier must be made much more recent than that generally assigned.

The relics of man which occur in the Trenton gravel, and which were first found by Dr. C. C. Abbott, are of great interest. In shape, in size, in workmanship, and in material the implements here found are quite different from those used by the Red Indian. These "palæoliths" are imbedded at various depths in undisturbed Trenton gravel. There are two points which offer strong evidence that they are as old as the gravel. The first is the fact that modern Indian implements ("neoliths"), although abundant on the surface, never occur more than a few inches below it, and are never associated with the palæoliths, which are found at depths of from five to forty feet below the surface. This fact alone argues a different age for the two classes of implements. The second fact is that, when found below the surface, the palæoliths always occur in the Trenton gravel and never in older gravels. The writer has gone over, with Dr. Abbott, much of the ground where the implements occur, and it was very interesting to find that it was only within the limits of the Trenton gravel, previously traced out by the writer, that Dr. Abbot had found implements below the surface. Here, then, is the strongest probability, even if the implements were found on the surface only, that they belonged to and were of co-eval deposition with the river gravel.

The implements found in the river gravels of Europe are of similar type, though as a rule perhaps less rude. It is of interest to find that very similar implements have been used by the Eskimos, and it is probable that that race, now living in a climate and under conditions perhaps similar to those once existing in the Delaware, may have some kinship with the pre-Indian people of this river. The occurrence of bones of arctic animals in the Trenton gravel indicates a period of cold.

All the evidence now gathered points to the fact that at the time of the Trenton gravel flood, man, in a rude state, lived upon the ancient banks of the Delaware. If future archæological work can show a connection between this people and the Eskimos, it may be appropriate to call the period of the Trenton gravel and of this palæolithic people —a period perhaps following a second glacial age—the ***Eskimo period***, a name more suggestive, and derived from a higher order of beings than that which gave the name "Reindeer Period."

While others have held that the occurrence of implements in the Trenton gravel indicates the existence of man in inter-glacial or even pre-glacial times, the writer believes that the investigations here described indicate the origin of man, at a time which geologically considered, is recent. Neither in the Champlain deposits, in the morainic material of the north, or in any older gravels have undoubted traces of man been discovered.

The actual age of the Trenton gravel, and the consequent antiquity of man in the Delaware, cannot be determined by geological data alone. It is the aim of this paper to define man's antiquity in relation to geological rather than to historical events. If, in showing that the Eskimo period is the last of the geological ages, it does not necessarily follow that it is by any means recent; it must be remembered, on the other hand, that its high antiquity is not proven by the facts thus far observed.

The conclusions to which the facts seem to point are briefly summarized as follows:

1. That the Trenton gravel, the only gravel in which implements occur, is a true river deposit of post-glacial age, and the most recent of all the gravels of the Delaware valley.

2. That the palæoliths found in it really belong to and are a part of the gravel, and that they indicate the existence of man in a rude state at a time when the flooded river flowed on top of this gravel.

3. That the data obtained does not necessarily prove, geologically considered, a vast antiquity of man in Eastern America.

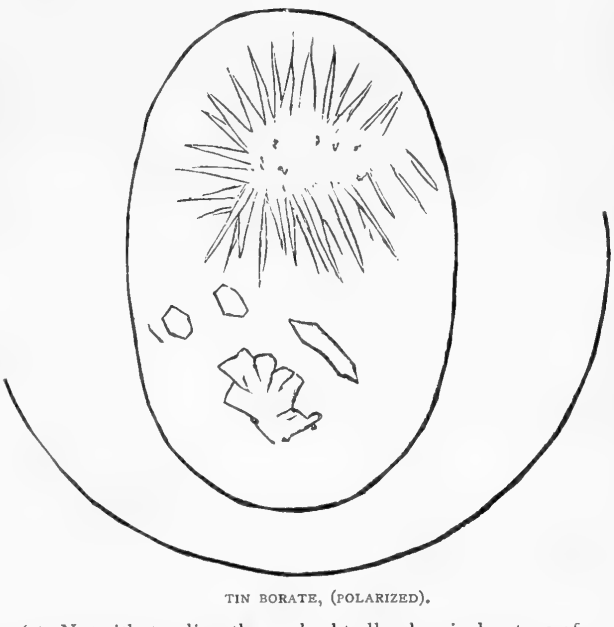
PYROLOGY, AND MICROSCOPICAL CHEMISTRY.

By W. A. Ross, Lt. Colonel, late R. A.

(1). In the year 1869, at Simla, India, having applied a trace of oxide of cobalt to a bead of boric acid before the blowpipe, I observed that, instead of dissolving, as I had been led to expect, small round black spots were formed, which, appearing perfectly round through the clear bead from every point of view, seemed to be spherides or balls. It was afterwards found that 14 oxides form such balls in boric acid, B. B., among which the most useful pyrological was that of *calcium*.

(2). I found, by the average of five assays, that the weight of the calcium borate ball, extracted by boiling water in which it is utterly insoluble, while the containing bead is rapidly dissolved—was a *constant multiple* of the weight of the calcined lime taken to make it, and that this multiple was 4.5. Thus, if $w =$ the weight of the ball, the formula $\frac{w}{4.5}$ represented the quantity of pure lime in it. If *calcium hydrate* was taken, instead of calcined lime, a clear ball was still formed within the bead, which latter became opaque through opalescence, and as the balance showed that this ball also contained the above mentioned proportion of calcined lime, the opalescence was attributed to chemical water.

(3). Circumstances of a painful nature, which I need not here relate, prevented my going further into this matter for eight years, but I vainly solicited the Microscopical Society to take it up, and having been enabled this year (about two months ago) to purchase a binocular microscope, with polariscopic apparatus attached, I fitted a small spectroscope I had by me into one of its eye-pieces with cotton wool, etc., and renewed my examination of these boric acid balls.



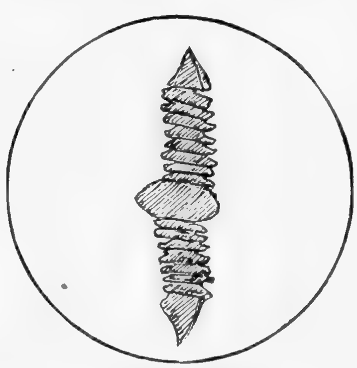
TIN BORATE, (POLARIZED).

(4). Notwithstanding the undoubtedly chemical nature of the combination I have called "a calcium borate ball," the phenomenon of ball formation itself is obviously as much related to the subject of molecular physics as to chemistry, and seems explainable briefly as follows: All liquids having cohesion have, under circumstances of equilibrium, a tendency to take the globular form, because it is the form in which the particles are at the least mean distance from the centre of gravity. As most oxides do not dissolve in boric acid, if the latter in its viscous state has very nearly the same specific gravity as the fused oxide, but is not miscible with it. This forms a ball with a tendency to occupy the centre of the bead, as oil does in water or water in oil, and the microscope now showed me, with reference to silica, that what I had supposed, looking through an ordinary lens, to be siliceous crystals adhering to calcium borate balls, formed by the mineral *Wollastonite* in boric acid were, in reality, thousands of *inner* transparent balls floating inside each calcium borate ball.

TUNGSTONE BORATE.

Similarly, therefore, it may be assumed, that a second borate, if it is not miscible with the first borate, but if it has a stronger cohesion, will take the place of an inner bead, and so it may be presumed with a succession of oxides.

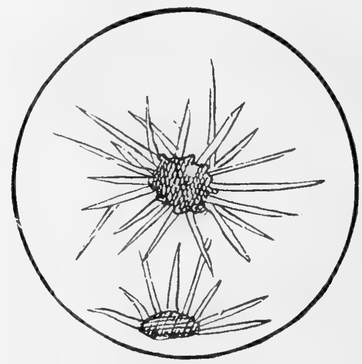
(5). This assumption however demands the concession that each inner ball is a *single* borate, notwithstanding that it must obviously derive its boric acid from the containing borate ball, which, being ascertained, as in the case of calcium, to possess only its definite proportion of that acid, must in that case take the exact proportion of boric acid from the outer bead, which it has to give up to the inner ball.



TUNGSTONE BORATE.

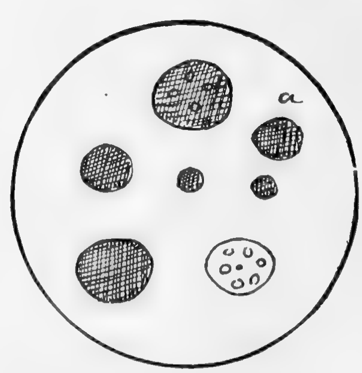
(6). To determine therefore, by actual experiment, if the inner ball in the case of *Wollastonite* was a silico-borate of calcium or a simple borate, I made a *large* calcium-borate ball with pure eggshell lime, in a bead of boric acid; extracted it by boiling the bead in water; made a bead on new platinum wire with the extracted ball; and, applying pure silica to it before the blowpipe, found that it would *not* now form balls within the calcium borate, although it would do so readily enough when the whole was surrounded by a bead of boric acid. On the contrary silica, zirconia, yttria, glucina, alumina, etc—all the "earths" in fact, which will not form balls *per se* in boric acid, dissolve rapidly and transparently in calcium borate when held as a bead by itself on platinum wire, but form balls within it when the whole is surrounded by a bead of boric acid, so that I submit the conclusion, that as regards silicate of lime, the inner balls may be composed of a new substance, SILICON BORATE, or alternatively silicate of boron.

(7.) I found that, as in the case of calcium hydrate (2) silica, however chemically pure, invariably gave off a certain amount of matter which caused opalescence in the boric-acid bead, *before* forming the inner balls above mentioned, from which phenomenon I argue that, if silicon borate is presumed to be formed, it is reasonable to infer that what we call silica is in reality *silicon hydrate*, and that a regular chemical interchange of components takes place.



TITANIUM BORATE.

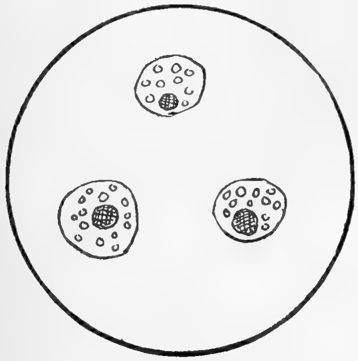
(8.) Alongside the inner "silicon borate" balls in the large calcium borate balls afforded by the mineral *Wollastonite* (from a Freibourg Cabinet) in a bead of boric acid, are numerous spherical enclosures, exhibiting under a ¼-inch objective, a brownish amethystine color, similar to that imparted by manganese to borax held in an oxidising flame, and, on referring to the account of this mineral in Dana's "System of Mineralogy, 1877," I find that from .2 to .9 of manganic dioxide are supposed to have been detected in certain specimens by Stromeyer, Weidling, and Whitney. But manganese itself forms balls *per se* in a bead of boric acid, and in *no* case, within my observation, do ball-forming oxides produce these *inner* balls in calcium-borate; indeed, from the ordinary law of physics, such a circumstance is an impossibility, and I have mounted boric acid beads of the single colored balls derived from manganese dioxide, and manganese silicate with lime, beside a bead containing the triply-enclosed colored balls derived from *Wollastonite*, which I would submit therefore, may be due to a NEW EARTH of the silica type.



MANGANESE CALCIUM BORATE.

a. Manganese Borate—one Calcium Borate Ball accidentally present

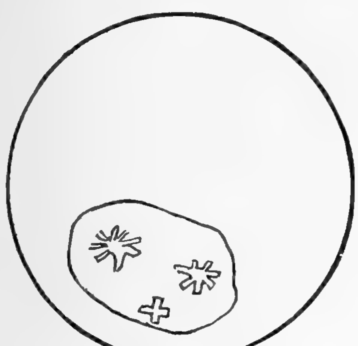
(9.) I would only add here that the acid oxides, as WO_3, TiO_2, etc., which also fail to form balls *per se*, in boric acid, remaining there before the blowpipe in *fragments*, colored or not, as the case may be, form, instead of inner balls in calcium borate, inner crystals, some of which are very beautiful and characteristic, especially by polarised light.



SILICON BORATE.

(10). I now feel that it was necessary to confirm these qualitative experiments by quantitative ones, and to establish the existence of this new substance, whatever it may be, which, eliminated from calcium hydrate, causes *opalescence* in a bead of boric acid before the blowpipe without at all lessening the due proportion of calcium in the borate ball formed—upon the reliable authority of the balance.

(11). As I invariably found that *pure chemically prepared silica* from Dr. Schuchartt, of Görlitz in Prussia, affords *per se* an orange flame before the blowpipe, and opalescence to a bead of boric acid containing a calcium borate ball (7); I chose this single substance, as left by the best analytical chemists in Europe to see if I could not resolve it—by *weighing* them—into two substances, and I submit that the results (the truth of which any chemist may easily ascertain for himself in his own laboratory by simply repeating these experiments) are sufficiently constant, under varying conditions, to warrant the immediate consideration of the unbigoted chemist.



URANIUM BORATE.

I. DR. SCHUCHARTT'S SILICA FROM GÖRLITZ.

	Mqrs.
(1). $Si\,O_2$ dissolved in a calcium borate ball, B. B.	2.5
(2). Weight of siliceous ball	18.5
(3). Weight of siliceous ball after extraction from 1st bead (2)	30.0
(4). Weight of siliceous ball after extraction from 3d bead	26.7
(5). Weight of siliceous ball after extraction from 5th bead, when no more opalescence was given off.	23.0

II. POWDER OF PURE ROCK CRYSTAL.

(1). $Si\,O_2$ dissolved in a calcium borate ball, B. B.	2.5
(2). Weight of siliceous ball	22.3
(3). Weight of siliceous ball after extraction from 1st bead	30.7
(4). Weight of siliceous ball after extraction from 4th bead	27.8
(5). Weight of siliceous ball after extraction from 5th bead	24.8

III. THE SAME AS ABOVE.

(1). $Si\,O_2$ dissolved in a calcium borate ball	2.5
(2). Weight of siliceous ball	29.0
(3), Weight of siliceous ball after extraction from 1st bead	38.5
(4). Weight of siliceous ball after extraction from 5th bead	34.1

IV. MEXICAN OPAL (*nearly transparent*).

(1). $Si\,O_2$ dissolved in calcium borate ball	2.5
(2). Weight of siliceous ball	25.8
(3). Weight of siliceous ball after extraction from 1st bead	37.5
(4). Weight of siliceous ball after extraction from 3d bead	33.5
(5). Weight of siliceous ball after extraction from 6th bead	28.8

V. MEXICAN OPAL (*again*).

(1). $Si\,O_2$ dissolved in calcium borate ball	2.5
(2). Weight of siliceous ball	18.0
(3), Weight of siliceous ball after extraction from 1st bead	30.0
(4). Weight of siliceous ball after extraction from 4th bead	28.5
(5). Weight of siliceous ball after extraction from 5th bead	26.9

INCREASE OE WEIGHT.

Experiment I.

Operation (5) — (2) = 23 — 18.5 = 4.5. (Mqrs. Mqrs. Mqrs.)

Weight of Opalescent Matter = (3) — (5) = 30 — 23 = 7. (Mqrs. Mqrs. Mqrs.)

Experiment II.

Operation (5) — (2) = 24.8 — 22.3 = 2.5.

Weight of Opalescent Matter = (3) — (5) = 30.7 — 24.8 = 5.9

Experiment III.

Operation (4) — (2) = 34.1 — 29.0 = 5.1.

Weight of Opalescent Matter = (3) — (4) = 38.5 — 34.1 = 4.4.

Experiment IV.

Operation (5) — (2) = 28.8 — 25.8 = 3.0.

Weight of Opalescent Matter = (3) — (5) = 37.5 — 23.8 = 13.7.

Experiment V.

Operation (5) — (2) = 26.9 — 18.0 = 8.9.

Weight of Opalescent Matter = (3) — (5) = 30.0 — 26-9 = 3.1.

NOTE.—The illustrations accompanying this article are made from rough sketches of crystals drawn, without camera luada from the microscopes. The shaded parts indicate color. The crystals were made by dissolving the oxide BB in a calcium borate ball held *as a bead* on platinum wire; crushing this bead to powder, and applying some of this powder BB to a boric acid bead.*

Mr. Barkas, of Newcastle-on-Tyne, Eng., has taken a very practical method of encouraging observational Astronomy, by offering to meet any of his townsmen who may be sufficiently interested and show them Jupiter, Saturn and other objects through an excellent telescope. From small beginnings great things often arise, and we should not be surprised if this offer, to be at a certain spot on a given night, led to the establishment of an observing society in Newcastle.

* In forwarding the above communication to "SCIENCE," Col. Ross states that it was originally prepared to be read at the recent meeting of the British Association.—(ED.)

TRANSFORMATION OF PLANORBIS.

A PRACTICAL ILLUSTRATON OF THE EVOLUTION OF SPECIES.

By Prof. Alpheus Hyatt.

II.

But we see that both the favored and unfavored found their appropriate spheres, and that even the deformities were perpetuated, and became distinctive of species.

Another characteristic which does not come under the dominion of any law of natural selection is the inevitable tendency to form an asymmetrical spiral in all the later occurring members of each series, whether progressive or retrogressive.

The lecturer then explained, by the aid of diagrams and a model, that the forms of shells are due to the successive imbricated layers built up by the border of the mantle in all mollusks. Secondly, that any force tending to compress one part of the secreting border more than another would occasion a narrowing of the imbricated layers of that part, and cause a twist or spiral to be formed. Thirdly, that the aspect of all the spirals examined shows that the shells are acted upon by such a force—gravitation, and in no other way can we account for their shape, and the obvious direction in which the compression of the border takes place.

The mathematical regularity of the spirals is explained if we admit the constant action of a universally distributed physical force upon the building up of the shell.

Diseased and outgrown, or old shells, were shown in order to enforce the fact that when an animal becomes weakened the shell shows its effect by the irregularities of the spiral. The excessively irregular forms of the oyster show that when the action of gravitation is in part eliminated the asymmetry is proportionately greater or less, and also that distortions occur in the internal soft body, as in the gills, and in the distribution and structure of the blood-vessels and mantle, which are quite different on the lower side of the adult oyster and upon the upper.

The oyster and all lamellibranchs grow *not in the direction of effort, but in that of least resistance.* The clam and the mussel were adduced to show this as well as the oyster. The forms of these shells are bilateral, but their anterior ends are compressed more than their posterior portions, therefore the valves grow faster towards the posterior than towards the anterior ends. Sooner or later when any soft-bodied animal lies habitually on its side, the originally bilateral or spherical form of the free animal must become distorted, as is the case with all attached animals, like the attached forms of protozoa, sponges, coelenterata, echinodermata, and so on. It is not difficult to show that their spiral, spherical, or bilateral symmetry is proportional, in all cases known to the speaker, to the amount of freedom in the growth of the parts; the freer the part the more symmetrical, the more attached or supported the more asymmetrical. Examples of shells like those of *Magilus antiquus* were cited in support of this view.

The attraction of gravitation is eliminated during the growth of this shell, by the coral which surrounds it; and the result, as also in the cases of many of the Vermetidæ, which receive a similar perfect support, is the formation of a wholly irregular tube, though the young are, while still free, provided with the ordinary turreted shell.

To show that the bilaterality of soft parts was produced by the attraction of gravitation on a soft growing body, the lecturer described several illustrations, especially the case of the Eolidæ, which have a coiled shell in the young, but lose this and become, during growth, perfectly symmetrical and soft-bodied. He also showed, that in no other way can we account for the extraordinary mixture of asymmetry in the shells and symmetry in the softer, free moving parts of the same animal among the Gasteropoda and other animals. The effects of heredity were also discussed, and it was shown that when a symmety, as distortion, was introduced, it occurred usually on the outer whorl, or during the latter stages of the growth, and that as time went on, this same characteristic appeared at earlier and earlier stages in the growth of successive descendants. The final effect of this law is the entire replacement of older ancestral characteristics by those which are newly introduced.

Thus the turreted asymmetrical spiral is found, as in the Steinheim shells, to gradually replace the more nearly symmetrical form of the immediate ancestors and the absolutely symmetrical form of the disc or shell, ovishell, as it is called, in all species. It was claimed that this law of heredity was absolute and independent, as one of the results of growth; and, that neither the variations, such as the formation of the asymmetrical spiral, nor its perpetuation and increase in successive generations of forms could be attributed to any law of natural selection.

The lecturer then, however, proceeded to show that the differences between the different series of shells could only be accounted for on the supposition of advantage and disadvantage, and took the ground that the Darwinian hypothesis applied perfectly to the explanation of the survival of only four distinct varieties out of the many which emigrated into the Steinheim basin, and tried to prove this by numerous instances quoted from Verrill and other authorities, showing that uniform physical causes must have a certain uniformity of result, which was not the case with the differences of the different series.

When, however, the action of natural selection had maintained the new differences for a certain length of time, until they had begun to be inherited, he claimed that it ceased to have any farther effect upon the organization.

Wherever the species might be found or whatever the surroundings there would be one thing absolutely certain; the forms during their growth would repeat the selected differences during their early stages of growth. In other words, the characteristics originally established by reason of their advantage or disadvantage in the battle of life, as soon as they become fixed in the organization, are no longer under the control of natural selection, which must vary with the immediate surroundings, but under that of heredity by acceleration.

The conclusions, besides those given in your report, were as follows:

"At the base of this conception of an animal lies growth."

Arising by growth through processes, which have been extensively studied, are, the bud, the egg, and all the phenomena connecting animals and plants according to the laws of heredity.

The action of growth and heredity, under the constant control of physical forces * gives the forms and many of the characteristics which distinguish a form from its immediate parents or ancestors, or from the forms occurring in other localities; in other words, the variations. The mutual action and re-action of animals and plants upon one another according to the laws of natural and sexual selection, etc., give it fixity in the organization to certain of these variations.

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* Of course, in this view, the physical force is the immediate cause of every condition of symmetry of form, as well as of every variation not derived from inheritance. The animal, in other words, is looked upon as a plastic, growing organism, acted upon from outside by physical forces, which modify it perpetually, and upon which it re-acts by means of its powers of growth and heredity. The former tend to cause perpetual variation, the latter to preserve the type by renewing—"rejuvenating" it perpetually in each successive generation.

We cannot account for the suitability of organisms, and their adaptations to every situation in time, as the distribution on the existing surface of the earth, or for the results of experimental zoology, without acknowledging the paramount influence of physical forces.

Nor can we, on the other hand, account for the comparative invariability of the embryo for indefinite periods of past time, or for the preservation of the type in spite of the perpetual changes introduced by physical changes on the earth's surface, unless due weight be given to the reaction of the growth forces and heredity by acceleration, which tend to preserve original types comparatively unchanged.

An organism is not entirely at the mercy of the elements, but possesses a power which, within a certain sphere, acts not only for the preservation of its life, but also for the preservation of its own characteristics, and, through heredity, causes the perpetual recurrence of similar characteristics and similar changes, what are usually called parallelisms, in suceessive generations of genetically connected individuals, forms a species wherever they occur in time, and under whatever circumstances of local distribution upon the surface of the earth.

Manufacture of Factitious Butter in thr United States.—A compilation from American and English sources shows that factitious butter contains only 1.823 per cent. of butyrine, caprine, caproine, and capryline, as against 7.432 per cent. in the natural product.—*Moniteur Scientifique.*

THE CUPRIFEROUS SERIES IN MINNESOTA.*

The paper of Prof. N. H. Winchell, State Geologist, of Minnesota, was a brief statement of the relations of that formation to the ranges of crystalline rock that form the northwestern border of the Lake Superior Basin. He concluded, by two lines of investigation, that the Cupriferous series is of the age of the New York Potsdam, and that it falls within the horizon of some part of that group which the Canadian geologists have designated the Quebec.

One line of argument related to an examination in the field of the stratigraphy, in which the sedimentary beds are seen to pass by metamorphic changes through various forms to fully crystalline rocks styled granite and gneiss. These crystalline rocks, which are spread over large areas, are intimately associated with the igneous rocks of the Cupriferous Series, from which, however, they are constantly distinguished by certain mineralogical differences The writer also parallelized the igneous beds of the northwestern coast ranges with the Labradorite, or Norite, rocks of Canada, and suggested the possibility that the "Laureutian" Eozoon, said to occur in this terrane, may be of the age of the Lower Silurian.

ON COLOR BLINDNESS.*

By Dr. B. Joy Jeffries.

Dr. Jeffries first described the natural condition of the color-sense, and illustrated some of its peculiarities relating to color-blindness. The complimentary after-image of a color can be readily seen by gazing at the red setting sun, when, if we turn our eyes to the east, we shall see a *green* rising one. Looking steadily at a yellow spot on white, and turning away, we see a blue one, etc. After looking intently at the red or green light on a railroad or vessel, one cannot help momentarily seeing the reverse color. The centre of the retina has the greatest power of form-perception; we must fix our eye steadily to see anything very distinctly. The same with color. All colors fade in intensity outward from the centre of the retina. In a central zone we can distinguish all three of the colors now considered primary, viz., red, green, and violet. In a zone outside of this our red perception fails, and in the outer portions still of the retina green fails, and we see blue or violet only. Now, we have *red*, *green*, and *violet* blindness, resembling, so to speak, the conditions of these zones. This must not, however, be too strictly construed. Color-blindness may be best described thus: Those who are red or green (one involving the other) or violet-blind see all objects having these colors as gray or grayish in the proportion in which they are color-blind and the depth of the pigment. A color mixed with their faulty one will be, so to speak, *muddy*: Many thousands of examinations have been made all over the world, with the same result. He has tested 17,695 males, finding 739 color-blind in greater or lesser degree, viz., about 4 per cent. In females it is very rare, which, however, their familiarity with the colors does not account for. He tested 13,893 females, finding only ten color-blind. Age, race, color, education, condition of civilization—all seem to have no effect, as tests have been now made from the north pole to the equator, and throughout Europe and America. It is congenital, and largely hereditary, It may be artificially produced by putting a person in a cataleptic or hypnotic state; also those color-blind who can be put in this state can be temporarily relieved of their defect. It may be cured by tobacco and alcohol poisoning, by injuries affecting the head, and by disease. It is a symptom of some brain disease of constitutional origin.

It can be palliated by gas light, or by looking through pale lemon-colored glass, or by looking through a solution of the aniline dye called fuchsine. All this does not cure, but simply changes the relations of light and shade for the color-blind, by which alone they distinguish their faulty colors. The reader briefly described how difficult it was formerly to detect color-blindnes, and referred to his manual in explanation of the manner in which persons affected escape. Thanks to recent observers and workers in the field we now have methods which are simple, and readily and quickly carried out by competent experts.

These facts have led the United States Government to undertake its control in the army, navy, and marine hospital service. Unusual examination of seamen is not yet compulsory. Its great value to the sailor was particularly explained. Standard tests and standard powers of sight and color-perceptions are not yet determined by the United States. An International Commission to determine these has been proposed in a bill now before Congress. The future value of such a commission was explained, and the audience urged to assist in having the system carried out.

As to the railroads of the country, Dr. Jeffries quoted from his book, now a United States manual ; "The difficulties are very great. Here the interests and the safety of the community have to contend with ignorance, prejudice, pecuniary considerations and incredulity born of supposed immunity from danger." This has proved most true, and even at this date, three years and a half since he, in this same room, called public attention to the danger from color-blindness, but one State, Connecticut, has passed laws controlling color-blindness and visual defects among railroad employés. And here in this State most violent attempts have been made to prevent the action of the law in protecting the community, even politics being introduced.

The practical tests, approved of over the world and recommended at the International Medical Congress at Amsterdam, September, 1879, and directed by the Connecticut Board of Health in charge of the control, were then thoroughly shown and explained. Practical illustration was also given the audience by a color-blind gentleman who kindly consented to exhibit his infirmity in the cause of science. These tests were Holmgren's with the colored worsteds (used by Dr. Jeffries in our public schools and the association), Donders and Daae's modification of this method, Stilling's pseudo-chromatic cards, Woinow's disk, Pflüger's letters, and finally Donders' method with reflected light and transmitted light, and Holmgren's with colored shadows,—these last two being for the purpose of determining the quantitative color-sense so necessary in deciding the fate of a railroad employé or pilot. The simple, practical use of these methods in the hands of competent experts was shown and proved in testing thereby the color-blind present. Dr. Jeffries explained in detail how theoretical was the attempt to decide the color-sense by lanterns and flags used on land or sea, and how readily mistakes would thus be made. The worsted test can, by competent experts, be quickly made in the paymaster's car, for instance, whereby no man is taken from his duty. The color-blind can thus be sorted out, and subsequently re-examined with these additional tests as a means of control, and to exhibit their precise defect to those in authority. During and after the reading of the paper color-blind gentlemen present were exhibited, to their astonishment and to that of the bystanders. All present were asked if convinced by what was shown of the danger, to exert their influence in the cause of control of color-blindness. Dr. Jeffries stated in conclusion that he had hoped to have been able to touch upon the most interesting point of the development of the color-sense and the education of our color-perception. He was, however, only able to briefly refer to the work going on of the study of color development, and particularly the contributions to it by his friend Dr. Hugo Magnus, of Breslau. Dr. Jeffries' work in testing all the Boston school-children showed, as it has elsewhere, the great ignorance on the part of males of color-names, aside from color-blindness. He exhibited Dr. Magnus' color-chart for teaching children colors and their names, which received a diploma of honor from the International Medical Society at Amsterdam. The use of this he explained, and said he was engaged with Mr. Prang in its introduction into this country among our schools, both private and public. The great purpose of this special work being to teach the child to associate with the word the color and the color sense, as well as the mere color names.

Occurrence of Ozone on Evaporation of Different Liquids.—A few drops of ether or alcohol are let fall upon a paper equally moistened with cadmium iodide-starch solution, and the volatile liquids set on fire. After their cooperation the paper is found to be turned blue from the formation of ozone.—*Polyt. Notizblatt.*

* Read before the A. A. A. S., Boston, 1880.

AN INVESTIGATION OF THE VIBRATIONS OF PLATES VIBRATED AT THE CENTRE.

By Professor Thomas R. Baker.

Since the publication of the paper under the above heading we have received from Professor Baker two drawings illustrating the same, which we now produce.

The first, Fig. 1, shows Professor Baker's method of producing the sand pictures, useful for class illustration:

Fig. 1.

Most of the plates used were window panes of various shapes and sizes, they were vibrated by rubbing an attached glass rod. The tubes, which were about $\frac{3}{16}$ of an inch in diameter and 20 inches long, were attached at right angles to the face of the plate with sealing wax. The support for the plate was a rubber cap, the common lead pencil eraser, fitted on the end of a post projecting from a disk of lead. A short rubber-capped lead pencil fixed upright in a wooden block answers the purpose just as well.

The plate was balanced on the support, the tube standing upright, and held loosely between the thumb and forefinger of the left hand. Then catching the tube between the moistened thumb and forefinger of the right hand and rubbing downward the vibrations of the plate were produced.

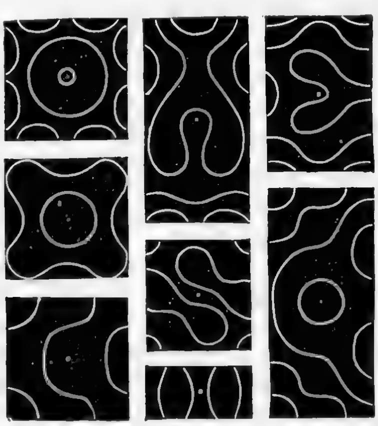

Fig. 2.

Fig. 2 represents copies of various sand pictures thus produced. He states:

"The figures were copied by placing the plate over paper which had been wet with a solution of potassium bichromate and dried in the dark. The plate and paper were exposed to diffused light, or to the vertical rays of the sun. The paper not hid by the sand soon darkened, and when this change had taken place the plate wasre moved and a lead pencil run along the bands of lighter colored paper representing the sand lines. This paper was then placed on white paper, and the figures copied by pressure. About 150 sand-figures were copied and traced."

For a summary of the facts derived from these experiments we refer our readers to Science, Vol. I., No. 13, September 25th, 1880, page 157.

FIELD WORK BY AMATEURS.*

By Helen Harelin Walworth.

It is announced, I believe, that one of the aims of the American Association for the Advancement of Science is to make Natural Science popnlar, to encourage its pursuit among all classes of people. It is because I have such an understanding of its aims that I presume to speak a word in behalf of the class who love science, yet can give to it but a limited portion of their time and thoughts.

Such a class of persons are important factors in the development of every department of knowledge and art. The professor, the artist, the specialist may have higher aims; they certainly do more thorough work, yet they would scarcely be understood, appreciated and encouraged if there did not exist the intermediate class who admire, applaud and exhibit the work they cannot themselves perform.

I therefore deprecate the scorn with which the professional too often contemplates the dabbler in his specialty, as he will perhaps designate the amateur. "A little knowledge is a dangerous thing" only when it is pretentious. A mere elementary knowledge of any natural science is a proposition from which reason starts; it is a foundation on which thought builds, and a height from which imagination takes its flight. It is an education in all other knowledge, because it demands attention, observation and accuracy with well-defined expression.

How can the popular interest in science be stimulated and increased? A majority of educated people shrink with aversion from the memory of tasks performed at school. The bare mention of a natural science recalls pages of unpronounceable words and incomprehensible classifications. Yet, if a practical geologist or botanist will take any three of these individuals into the field with him and beguile them into breaking rocks or gathering flowers scientifically, two out of every three will be delighted with the occupation, and will strive to recall the classical names which inspired them with disgust while they were merely theoretical. It is then only while science is an abstraction that it repels; render it practical and it invariably attracts.

In every city and village of our country we find numerous clubs and societies devoted to special objects of literature and art, and a few to science. These last are rare, they would be numerous and active if slight encouragement were given to them by those who have the ability to guide and direct. Such clubs and associations should begin with a short and well directed course of reading, accompanied, if possible, by a few interesting lectures as a preparation for field work which should not be delayed through timidity or a feeling of ignorance. A few visits to the field by a geological club will serve to arouse enthusiasm, and inspire a desire for research, which months of reading would not accomplish. It cannot be urged that many live in localities where there is nothing to study, for I believe it may be safely stated that uninvestigated scientific facts lie over and under every square mile of the United States. Yet I have heard the members of a geological club, who studied exclusively in the class-room, make such a plea. When visiting their city I said to one of them, "What rocks have you in this vicinity?" The person addressed looked at me with unqualified surprise and answered, "We have none." I exclaimed, "you have a river and hills, and many railroad cuttings, the foundation of things must be visible somewhere." But this individual insisted that there was absolutely nothing to examine within walking or driving distance of that city. There is, of course, a difference in varying localities. In Davenport, Iowa, where there is now a well-established Academy of Science, located in its own fine building, and displaying a great museum, a few years ago there were but half a dozen persons who met in a hired

* Read before the A. A. A. S., Boston, 1880.

room to talk informally about science. They soon, however, began collecting and investigating in the suburbs of their city; it is, as you know, the region of ancient mounds. Their discoveries have been remarkable and valuable.

In Saratoga Springs, where I reside, we have, in a limited way, an interesting geological region, and we have an active Field Club. It labors under disadvantages, having had no regular instruction, and no course of lectures, but it has been assisted by two gentlemen who have had some experience in geological research. As this club is now established upon an apparently permanent basis, some account of its efforts may not be amiss. There are between thirty-five and forty members, the larger number studying geology; a few botany, and others, who are studying art, accompany these to sketch from nature. This community of interest among those who are pursuing different studies has the advantages of economy in the hiring of vehicles and in the purchase of instruments like the microscope, which can be used in common.

The Saratoga Field Club makes excursions into the country every Saturday, when the weather is favorable for field work; they also have in-door meetings once a week, to compare and examine specimens; papers are then read on subjects relating to special objects of study and discussion, and conversation concerning them is encouraged. Meetings are also held during winter preparatory to the summer work.

We have in Saratoga the rocks of the Laurentian, the lower Silurian of the drift, the Champlain and Hudson river periods. But in the Laurentian granite alone there is an endless variety for those who are interested in minerals. Then, too, one experiences a certain awe in handling the oldest rocks that formed a boundary of the world's first continent. The gloom of that almost lifeless age seems still to creep along the dark, stout foliage that strives to cover the baldness of these venerable rocks. Worn and ground by the action of ages they display few picturesque forms, but strength and endurance seem moulded into shape among their rounded hills, while nestling among their unattractive gray shadows are found the garnet, the chrysoberyl, the tourmaline and other beautiful gems. The Potsdam sandstone lying above the granite, shows great variety and beauty of color, and Ruskin says very justly, "that nature tempts us, like foolish children as we are, to read her books by the pretty colors in them." The ripple marks and glacial scratches of this rock are also countless and interesting. The calciferous sand-rock coming next in succession, and upon which the western half of our village rests, is in many places brilliant with crystals and finely-marked with Fucoids; it bears also whole acres of the marvelous concentric Stromotopara, which is peculiar to this vicinity. The Trenton limestone, next above this is, as usual, rich in fossils, and an afternoon amid its quarries will render the members of the Field Club oblivious of heat or cold or fatigue in their search for Erinoids, Brachiopods and Trilobites. Such interest is scarcely diminished in their laborious wanderings in other directions among the Hudson river slates and shales for the rarely found Graptolites. The morains and pebble-laden hills of the drift period are sought out and discussed. The sands of the Champlain, and the terraces of the Hudson river periods are subjects for thought and surmise as we ride over the country toward some definite object of investigation. The great geological fault which has given birth to our justly renowned mineral springs, coming forth as they do from tue hidden fossil oceans of the buried centuries, stimulates us to ponder and to inquire. Yet for years most of the members of this club have walked blindly through these treasures, seeing, but not observing; knowing, but not seeing. A new world has been opened to them, and this world of nature and of science would be a revelation to hundreds of others if they were induced to engage in out-of-door studies.

The public mind has been awakened to an interest in science by means of the popular lectures delivered by men of acknowledged fame, and also through numerous popular publications. These have been a preparation for field work which can now be pursued with enthusiasm and profit. A search for geological facts in the fields affords an admirable means of self-discipline. In the beginning each one sees all that he seeks and believes that all he sees is of immense value, or he goes to the other extreme and pronounces everything worthless. He will be vexed with himself, crushed and mortified by turns, but each blunder will be an important lesson, and soon he will begin to discriminate, to learn and to search, until he finds himself, like the hunter in pursuit of game, eager, excited, and ever ready for a new chase.

When the guidance of a professor of Geology cannot be procured, much may still be done with the use of proper text-books, and the State Geological Surveys, especially if there are a few men or women in the association who have some experience in field work. In every community a few gentlemen will be found who possess such knowledge. It is a deplorable fact that few women possessing such knowledge can be found in any community, except, of course, in Boston. I say deplorable, because scientific training is, of all others, that which women need to correct the defects which, as a class, they display—defects which have become inherent through continuous superficial training. In this case like must cure like, for it will require several generations of women, gradually trained to scientific methods of thought and investigation, to eradicate the slipshod mental habits of the women of to-day. A few are struggling toward better and clearer ways, but the difficulties to be overcome prove the low standard of their starting point. Is it right that woman should be ignorant of the scientific facts embodied in the useful and beautiful things she handles? If these facts are of value to the world they are of value to women individually.

Invite women, then, to enter upon this field of labor, and science will gain thereby. Enlist the enthusiasm, the self-sacrifice and vitality of women in the cause o science, and a new principle will stir the remotest member s of the body of scientific knowledge.

The effect of this labor upon the lives of women is beyond calculation. Where they are now weak, both physically and mentally, they will become vigorous and strong; where they are complaining and sentimental, they will grow cheerful and wise. Their restless longings will move into healthful channels, and they will learn to think, to observe, and to perform with accuracy and deliberation. They will discover that the ability to learn and to do is not a mere knack to be caught, but that it is the result of continuous and pains-taking labor.

Believing, as I do, that a practical knowledge of natural science will do more for the advancement and emancipation of woman than any laws that can be made, or any rights which can be granted to her, I appeal to the learned gentlemen of this association to invite and encourage women to labor in the various departments of scientific investigation.

The progress of woman depends on the exercise and discipline of her mental powers and the proper expenditure and economy of her physical powers. Both means are to be obtained mainly through a knowledge of the natural sciences, and they will take and retain their hold upon her more readily by means of out-of-door work.

There are also many men engaged in the professions and in business who would make time for open air excursions if they thought a study of natural objects feasible. In such studies of local geology the amateur may, by chance, make valuable discoveries, and he may in time become enlisted as an enthusiast and specialist. Goethe says that in science "treatment is nothing, all effect is in discovery; every new phenomenon that is observed is a discovery, and every discovery a property." If, then, it is allowable and desirable for amateurs to study science practically, it is important for them to receive suggestions and instruction from professors and specialists. In botany several American publications have been issued, which serve as admirable guides for such persons. In geology I know of but one popular book on field work. That is an English publication; we need one especially adapted to American geology. A series of articles published in one of the popular magazines, and bearing a name of authority, would give a wholesome impetus to this work, and would reach many persons who desire information concerning it. These vague desires and feeble reachings after such knowledge should be noticed and cherished, for in these there may exist some of the future discoveries and triumphs of science.

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 FOURTH FORM OF MATTER.

To the Editor of SCIENCE:

Science to-day recognizes but three forms of matter, viz.: the solid, the liquid, and the gaseous; though the existence of another form still more tenuous than the gaseous has been suspected by some distinguished men, and among them FARADAY, prince of scientists.

FARADAY, in 1819, delivered a series of lectures before the Royal Institution, on the general properties of matter, in one of which, entitled, "On Radiant Matter," he thus expressed himself: "If we conceive a change as far beyond vaporization as that is above fluidity, and then take into account also the proportioned increased extent of alteration as the changes rise, we shall, perhaps, if we can form any conception at all, fall not far short of *radiant matter*." Later he pointed out that matter may be classed under four states, viz.: solid, liquid, gaseous and radiant, and demonstrated the *probability* of the existence of the last. So far went FARADAY, and no one who has ever attempted it has gone any farther.

This "fourth" form can be accepted only upon a demonstration that is beyond question or doubt. It must be as positively distinct from the gaseous as that is from the liquid, or as the liquid is from the solid, and its existence must be more than inferential or imaginary. Its effects on a sufficiently large scale must be seen or felt. Then will men recognize it as the "border-land" of matter, beyond which only spirit can be.

That such a form of matter exists can to-day be confidently asserted, for modern science is demonstrating it in many ways. Around and about us, and known to all men, is an element that apparently satisfies every requirement; an element more efficient than steam, vapor, or gas; having numerous and varying forms, and with as many names; an element that is generically called electricity. It is the chief study of scientific men at present. It is matter; subtle, swift, powerful, manifold in operations, invisible, and with the strange power of multifold transformations. It passes as a *substantial* thunderbolt at one moment, and at the next is diffused into almost infinite tenuity. In the electrical fireball it moves at times slowly; in the telegraph with inconceivable velocity; in the cyclone with utmost power and regularity; and in such incalculable phenomena as that of the Minnesota flouring mills disaster, with marvelous explosiveness. As radiant matter it is everywhere present, as far as we can discover. Some idea of the ever-present and ever-ready state of this element is conveyed in the fact that not only every body and substance, but almost every method of dealing with substance manifests its presence. Earth in every part, and air, are pervaded by this mighty universal power, strong as gravity itself.

PROF. WILLIAM E. CROOKES, of the *Royal Society, London*, has sought for this kind of matter "in the shadowy realm between the known and unknown," which for him, he says, "has always had peculiar temptations." The shadowy realm of imagination cannot be fruitful in furnishing the substantial and reliable data required in scientific investigation. He has, however, inadvertently contributed to the resources of science by exhibiting some striking experiments that show the peculiar action of electricity *in vacuo*. In claiming to have discovered his "fourth form," in the imagined play of imaginary molecules or atoms, while at the same time having in use an electric battery, competent to all the phenomena, he appears to rather speculate than reason. As well claim that the spirits are at work in his tubes. Electricity in this case appears too subtle for its master.

It is, therefore, to the radiant, all-pervasive electrical or magnetic matter, that we must look for a candidate for the high honor. FARADAY'S classification may henceforth stand as legitimate, viz.: solid, liquid, gaseous, radiant. The latter may be considered as dominating and interpenetrating all the rest. It is the form in which life and motion reside, or through which they are communicated. Electricity now appears to be the underlying form, or substratum, out of which come light, heat, magnetism, gravity, etc., and recent experiments of EDISON, BELL, COULON, and others show the fact that most wonderful transmutations belong to this matter of the "radiant form."

It may be said that scientists are not altogether agreed as to the materiality of electricity, yet it *is* material to the consciousness of every thoughtful man. But, if matter, to which of the forms does it belong, solid, liquid, or gas? or do we find it in all? Plainly it transcends in qualities and powers each and all the recognized forms and is beyond their definition. No one would declare it to be solid, nor would it be called gaseous, and though it may pass under the definition, "A *power* in nature styled the electric *fluid*," yet it is not scientific to call a fluid one of *the powers of nature*. It were more satisfactory to relegate it to the realm of spirit-force at once; but what scientist would do that? We shall yet find in a fourth or radiant form, the true interpretation of all the most mysterious phenomena of matter.

HENRY RAYMOND ROGERS, M.D.,

Dunkirk, N. Y., Oct. 11, 1880.

COMPARATIVE ZOOLOGY.

That the extinct fish-like reptiles known as Ichthyosauri, in some cases, at least, produced living young, is the conclusion of a Report upon the subject to the recent meeting of the British Association by Prof. Seeley, a brief abstract of which is given in *Nature* for September 16. In several specimens, notably those at Tubingen, the perfectly preserved young are enclosed within the ribs of the parent. In estimating the zoological significance of viviparity it should be borne in mind that while it is constant among the mammals, some sharks are oviparous and others viviparous, and that among serpents not only does this diversity of function exist, but even some species seem to be variable in this respect. B. G. W.

NOTES AND QUERIES.

[2.] WHO first used the phrase "Foramen of Monro"? In a paper entitled "The Foramina of Monro: Some Questions of Anatomical History," in the Boston *Medical and Surgical Journal* for August 12, Prof. B. G. Wilder demonstrates, upon various lines of argument, that the foramina were named in honor of Alexander Monro, *secundus*, and not *primus*, as stated by some writers. It is now desirable to ascertain by whom the name was introduced into the anatomical vocabulary. Monro's "Observations upon the Nervous System" was published in 1783, hence the phrase must have originated at some later date. MEDICUS.

A REMARKABLE solar protuberance was observed by M. Thollon, on August 30, at the Paris Observatory. About 11 A.M. he saw it rise from the eastern limb, as a vertical, thin, and very brilliant, luminous jet. The displacement of the line in C in the spectroscope corresponded to a velocity of 35 kilometres per second, and the protuberance rose to a height equal to half the solar radius, or about 343,000 km. After rising, it enlarged to prodigious dimensions, its brightness sensibly diminishing especially near the base. By about 1 P.M. it had become hardly visible. A curious fact is, that while the lower and middle part of this protuberance gave a deviation of the line C towards the violet, the top presented a nearly equal deviation towards the red.

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JOHN MICHELS, Editor.

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SATURDAY, OCTOBER 23, 1880.

The recent discussions on the proposed international copyright have served to display the prejudices and animosities of those engaged in the controversy, rather than a true exposition of the actual principles involved.

It is not difficult to discover that under the plea of maintaining the rights of authors, leading publishing houses on both sides of the Atlantic are manœuvring for a "literary treaty" which shall result in their own benefit only.

How little the author has at stake as a result of all this agitation may be gathered from a statement by one who writes on the subject, perhaps not in the interest, but undoubtedly under the patronage, of one of the largest New York publishing houses.

He says: "If the author's interest in a book is represented by 10 per cent. the publisher's interest is represented by 90 per cent." It is not, therefore, surprising to find these champions of author's rights feebly contending for the protection of the works of literary men, but strongly united to secure a monopoly of the business interests involved.

The American publishers who previously were unwilling to concede to foreign authors even their "ten per cent. interest," give at length a tardy consent to a copyright treaty, provided their "90 per cent. interest" is made secure, and their possession of the monopoly rendered impregnable by law.

The New York *Herald*, on the 4th of October last, stated this case as it now stands with admirable judgment, and we are glad to find the powerful influence of this journal taking ground which is in perfect accord with the view we maintain on this important matter. The *Herald* says:

"The corner-stone of the proposed treaty is that protection in this country be given to British authors on condition that they republish here within three months after publication in Great Britain, and on the further condition that the work be issued here by an American publisher. On like terms British copyright is to be extended to American authors.

"That this scheme would work to the profit of the largest publishing houses in this country, if not of American publishers generally, there is little reason to doubt. It would be a protective measure in their interest. It would create a monopoly in their favor. It would compel foreign authors to come to them or pay the penalty of piracy. It would have no material advantages for the great reading public in either country, and so far from being favorable to either British or American authors it would work against the best interests of both. It would drive both, in order to get foreign protection, to deal with publishers three thousand miles away, and to bear the expense, loss of time, labor, and inconvenience of republication. Still more burdensome and unjust would be the condition requiring the author to republish in the foreign country within a short time after publication at home or lose all his foreign rights and claims to protection."

No impartial reader can peruse the above extract without admitting the justice of the writer's conclusions; he rips off the thin disguise which covers this ridiculous treaty, and reveals the true purpose of those engineering the movement.

The intemperate language employed by the organ of some publishing houses on this question should be noted. The Editor of *Popular Science Monthly* assails Wilkie Collins (who advocates the only right principle of international protection to literary property) with uncalled for severity; he is called "a common-place scold," and his temperate and forcible article on the subject is termed "a blast, which did not amount to much," and as "a perverse and unhelpful utterance." Was it in good taste for the same writer to tell Mathew Arnold that "he was devoid of sense?" But the conclusion of this article demands more than a passing notice, as it conveys a threat, expressed in language which is very significant considering the house from whence the publication emanates, and may be taken in the light of an ultimatum from the publishing interests to their literary patrons.

It may be remembered that Wilkie Collins simply asks that an author may possess "by law (on conditions with which it is reasonable to comply) the same right of control over his property in his book, in a foreign country, which the law gives him in his own country." This is what the New York *Herald* advocates, and we would concede to authors of all countries.

The *Popular Science Monthly* states that "if Mr. Collins [and of course all other authors] has any idea of getting it, he "had better possess his soul in great patience," for he will assuredly have to wait a long time before he gets what he wants."

As the writer of this threat has no authority to dictate such an order, or the power to enforce it, it may be safe to assume that it was written under the instruction of those who have the power to give it effect. To such a menace there can be but one reply; much as we desire to see justice done to authors and their rights assured by international treaty, we would, without hesitation, decline concessions thus tendered, and hampered with conditions which would destroy the essence of the gift. We would rather counsel literary men "to possess their souls in great patience," and calmly await the day when their adversaries' souls may be possessed with moderation and justice, or public opinion be sufficiently strong to secure for them their just rights.

THE reported arrival at the port of New York of two male *woolly* elephants of dwarfish dimensions, has resulted in a request from a subscriber for authentic information regarding these strange animals.

It was alleged that the elephants in question were discovered upon the Himalaya mountains, and that the hairy covering found upon their bodies, and the great diminution of the natural size, were due to the cold of that region.

The inference to be drawn from such a description was, that the present specimens were in a measure a return to the extinct *Elephas primigenius*, the remains of one of which was found at the mouth of the river Lena in Siberia, with the flesh still in a good state of preservation, showing the skin covered with hair.

We find that the new arrivals were not found on the Himalaya mountains, but were purchased at the Parah River, Malay Peninsula, after the ship had left Calcutta. Their size is normal, for their age is not four and six years; but, probably, the smaller is about twelve months old and still feeds on milk, while the larger specimen is about two years old.

All young elephants are covered with hair, which afterwards falls off as they increase in age. These animals have this infantine crop of hair somewhat abundant, but not to an extent to create any especial wonder.

As Mr. Conklin, of the Central Park Menagerie, states, our knowledge of baby elephants is very limited in this country, and perhaps after all, the apparent excess of hair on the flanks of these animals may be normal. The young elephant, born at Philadelphia about nine months since, had a similar crop of hair, but not to the same extent.

Dr. Spitzka, of New York, who has seen these young elephants, confirms the opinion we have given, and states that they are not a new species or even a variety; and he believes that the hair will eventually disappear, and even now finds, on the larger specimens, bald spaces.

We do not desire to spoil the speculation on these animals by stating the price at which they were sold on their arrival here, but the multiplication table has not been without its use to create an artificial value.

ACCURACY IN THERMOMETERS.

By recommendation from the Winchester Obsevatory, a bureau has been established at Yale College with the practical view of accurate verification of these instruments. Any person may send thermometers to this institution for the purpose of having them compared with the standard thermometer, and any variation from the accurate standard will be recorded. For the purpose of defraying the expense of these comparisons, the following scale of charges has been adopted for this verification: For standard meteorological thermometers, one dollar; for ordinary meteorological thermometers, fifty cents; for ordinary maximum thermometers, seventy-five cents; for ordinary minimum thermometers, seventy-five cents; for clinical thermometers, fifty cents. In case more than eight instruments of one kind are submitted at the same time, twenty per cent. will be deducted from these charges. Clinical thermometers, in numbers of two dozen or more, will be verified for four dollars a dozen. For thermometers of exceptional pattern, the charge will vary according to the character of each instrument. Communications relative to this subject may be addressed to Leonard Waldo, New Haven, Conn.

THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON.

The accumulation of material at Washington illustrative of the several branches of Anthropology, has drawn together a large number of specialists in Comparative Anatomy, Archæology, Ethnology, Linguistics, and Sociology. For mutual improvement a number of these gentlemen have organized the above-named society, with Major J. W. Powell as President, Dr. Elmer R. Reynolds as Secretary, and Professor Otis T. Mason as Corresponding Secretary. The facilities which the Army Medical Museum and the Smithsonian Institution, with its Bureau of Ethnology, furnish for the preservation of valuable papers obviate the necessity for a voluminous journal of the Society. We have made arrangements, however, to present abstracts of communications and discussions on the week succeeding the meetings, which take place on the first and the third Tuesday of each month. The following is a *résumé* of the proceedings of Tuesday evening, October 20th:

The Anthropological Society met in the Smithsonian Institution, Major J. W. Powell in the Chair. After the reading of the minutes the following papers were communicated; "Notes on the Identity and History of the Shawano or Shawnee Indians," by C. C. Royce; "Civilization," by Mr. B. W. Hough. Mr. Royce stated that his paper was an introductory effort to a thorough study of the Shawnese, who were the Bedouins and Ishmaelites of our territory at the time of its first settlements. The early home of these people is shrouded in mystery. After carefully going over the Jesuit relations and other early histories, the author concluded with the bold proposition that the Massawomacks, the Eries or cats, the Satanos of Colden and the Shawanons of later writers were one and the same people. These several tribes were then followed with the minutest care, so far as the early writers throw any light upon the subject. The latter part of the paper was a detailed account of the wars, treaties and fortunes of this people from 1755 to the present day. The discussion upon Mr. Royce's communication was participated in by Colonel Garrick Mallery and Major J. W. Powell.

Hr. Hough's paper related to the influence of the inheritance of knowledge and character as making progress in civilization possible. Each individual and each race is the outcome of all those material and psychical causes which have co-operated to bring them into existence. These facts were used by the author to show that the civilization of our race cannot be forced upon another race. By a multitude of examples Mr. Hough illustrated the methods by which the causes which give rise to races and phases of culture are brought together and co-operate to their end. The paper was discussed by Mr. Lester F. Ward, Major J. W. Powell, Professor O. T. Mason and Colonel Garrick Mallery.

THE PRIORITY OF THE LATE FRIEDRICH MOHR IN REGARD TO THE PRINCIPLE OF THE CONSERVATION OF ENERGY.

By Dr. Geo. W. Rachel.

In an interesting essay entitled, "*On the History of Forces*," published by Dr. C. K. Akin in the *Philosophical Magazine*,[1] occurs the following passage:

"There has been of late a good deal of controversy regarding the priority of invention or discovery of this last named principle [Correlation of Forces] and it may be interesting, in a historical point of view, to take cognizance of passages of much earlier date than hitherto relied upon as establishing such priority, and upon which I have in the majority of cases rather accidentally lighted."

The controversy alluded to by Dr. Akin in the passage quoted, may be said to have continually attracted the attention of scientists since the above reference was made to it sixteen years ago. It is well known that Tyndall's authoritative statement of Dr. Robert Julius Mayer's priority has been accepted almost universally since it first appeared.

And yet there always was a number of scientific men who held another opinion; but in spite of their protests, even to-day the authority of Tyndall prevails, and the popular notion all over the civilized world is that Mayer first discovered and proclaimed the great principle in question. This view, however, is erroneous, and in this paper the writer intends to prove, by the introduction of documentary evidence, that the late Prof. Fr. Mohr, *of Bonn, was the first who, in clear and unmistakable language, proclaimed the principle of the "correlation of forces" and the "mechanical theory of heat" on which it is based.*

The history of the essay, on which his claim of priority in this matter rests, is a peculiar one, and since the circumstances attending its loss and its re-discovery have had a direct bearing on the controversy in question, they are worth mentioning.

It is certainly a unique occurrence, that a scientist should, for a period of thirty years, have been absolutely unaware of the fact that an article which for the first time in the history of science states a principle of the utmost importance, had actually been published in one of the scientific periodicals to which he sent his MS; and this was due only to the failure, on the part of the publisher or the editor, to transmit a copy of the number containing the paper to the author. But this is what really happened in the case of Prof. Mohr's article "*On the Nature of Heat.*"

Mohr first sent it to Poggendorff, but the latter declined its publication for the peculiar reason that "*it did not contain any new experimental researches.*"[2] It was therefore returned to Mohr and by him, in turn, sent to Prof. Baumgartner, at Vienna, who, at that time (1837), in conjunction with Dr. Von Holger, published and edited his *Zeitschrift für Physik und Verwandte Wissenschaften.* Not being informed by these gentlemen what had been done with the paper, he supposed it to have been shelved by them on grounds similar to those which prompted Poggendorff's refusal. It was only by an accidental reference to this essay in one of his later works[3] that he chanced to hear of it again.

Dr. Akin wrote to him that it had been published by Baumgartner and von Holger, in the fifth volume of their *Zeitschrift*, etc., p. 419, a passage of it having been quoted by him [Dr. A.] in the essay refered to above.

The files of this magazine—which had a very limited circulation—having become scarce, since, shortly after its publication had ceased, the publishing firm was dissolved, it was very difficult to procure a complete set. Thus it was that Mohr had to wait many months until, in response to a request, he received a copy of the volume in question, sent for temporary use only by the librarian of the *Vienna Polytechnic School*, Prof. Hlasiwetz. A letter, accompanying it, contained the following passage:

" . . . . I am happy to be able to congratulate you on this important essay, *which puts your priority in regard to the question of the mechanical theory of heat beyond any doubt.* I am glad *furthermore that I should have been* instrumental in the re-discovery of this hidden treasure[4]. . . ."

Very soon after Dr. Adolph Barth, of *Leipzig*, the present editor of *Poggendorff's Annalen*, succeeded in hunting up a full set of files of the *Zeitschrift*, etc., and presented them to Prof. Mohr.

Since then the paper in question has been twice re-published in full. The first time by the author himself in one of his later works, with an explanatory statement, containing the history of its loss and re-discovery as given above,[5] and again by Dr. Hermann Klein in the seventh volume of his scientific monthly, the "Gæa" in the year 1871.

Although Mohr has never pressed his claim to priority, it is certainly due to his memory that all the facts in connection with it should become fully known. It is always to be regretted, when personal considerations—not to say animosities—come into play in such questions; but it seems as if this very matter had been destined to be the subject of an unceasing feud which has at times even taken the shape of a personal quarrel. Only a few years ago Prof. Dühring was 'dismissed' from *Berlin University* by a vote of the faculty, because he had accused Prof. Helm-

[1] Phil. Mag., 4th series., vol. XXVIII., No. 191, December, 1864; pp. 470-477.

[2] For the same reason it was that the editor of the great *Annalen* declined five years later the publication of Dr. Mayer's paper on the same subject, and it therefore appeared in Liebig's *Annalen der Chemie* (42, 240.)

[3] *Mechanische Theorie der chemischen Affinitat, Fr. Vieweg; Braunschweig*, 1868.

[4] The letter bears date of Oct. 17, 1868, so that the author received and first saw the article *fully thirty-one years after its publication.*

[5] *Allgemeine Theorie der Bewegung und Kraft, etc. Fr. Vieweg, Braunschweig*, 1869, pp. 80-106.

Note.—It is to Mohr, and the fate of this essay, that Dr. Akin refers in a passage contained in his latest letter to Prof. Stokes, which was published in No. 15 of "Science." On page 179 of this Journal he says: "Another [Mohr] who started similar ideas about the same time, having been repulsed in one quarter (Pogg. Ann.) took it for granted that the same had happened to him also in another (B. & v. H. *Ztschr. f. Phys.*, &c.) where it was not the case, so hopeless did he consider his endeavor to obtain a hearing."

It is a strange coincidence that the same humiliating experience was reserved for the last months of the great man's life. For, the last three essays which he wrote, each one of them, abounding in new and original ideas, were also declined by the editors of "Liebig's *Annalen*." This unwarrantable procedure so disgusted the family of the venerable philosopher that they decided after his demise not to publish them in Germany at all. They have honored the writer by intrusting to him the publication of these valuable essays, and the readers of "Science" will soon have opportunity to judge, for themselves, of the new and striking views advanced by this great thinker.

HOLTZ of plagiarism (from Dr. MAYER), in the very same matter, and used unbecoming and disrespectful language towards his colleague.

Prof. TAIT also, as our readers are well aware, has re-opened the controversy and has attacked DR. MAYER and his champion, PROF. TYNDALL, in an unnecessarily virulent and aggressive manner.

It is very likely that both these gentlemen would have acted quite differently in this matter had they been informed of the fact that MOHR's priority dates back to 1837. PROF. TAIT only refers to it *in the preface to the second edition of his "Lectures on some recent advances" &c.*,* stating that he had, until very shortly before its publication, not seen or heard of MOHR's writings on the subject, whose indisputable claim to priority he at the same time admits.

In the beginning of this paper we have given the introduction to DR. AKIN's interesting, though as it seems, not very widely known essay, and we shall now proceed to give, in the same order as that adopted by him, the various passages —almost exclusively from MOHR's paper—which DR. AKIN cites in support of his view "that they must invalidate all claims to priority of an earlier date."

DR. AKIN continues (l. c. p. 473):

"The following is an extract from PLACIDUS HEINRICH's: *Die Phosphorescenz &c.*. published in 1872: 'Meanwhile we know, at least with a certain degree of reliability, that nothing is lost in Nature . . . . everything may be explained by constant interchange; the one gains by the loss of the other; the one takes its origin from the disappearance of the other. Therefore there is no such thing as loss in the Universe, only change and interchange.'[1]

The next quotation is from a paper by DR. MOHR, of Koblenz, "*On Heat*," published in 1837:

'Aside from the 54 chemical elements at present [1837] known, there is but one agency in Nature and this we call *Force;* it may under suitable circumstances appear as motion, chemical affinity, cohesion, electricity, light, heat and magnetism, and by means of each of these different forms all the others may be produced. . . . If, by the *force of your arm* you remove an induction-coil from a magnet, an *electric current* will originate in the surrounding spiral wire (helix), which, on being interrupted, appears as a spark, or if the conducting wire is reduced in size, as a glowing wire (*heat and light*); it will also *magnetize* a steel needle if it is conducted through a helix surrounding it; it will decompose the water it passes through, annihilating its *chemical affinity* and its *cohesion* at the same time, and since the thin Platinum-wire, Ampère's helix and the apparatus for the decomposition of water, may be interpolated at the same time in one and the same chain, it is evident that *the force of the human arm may appear under different conditions as heat, light, chemical affinity, magnetism and cohesion.*'[2]

"The passage is followed by two more pages, showing in greater detail the connexion and transmutability of the several known forces and a transcript or translation of which I [Dr. Akin] hope to give at some future occasion. The author concludes his observations with the following judicious remarks:

'Without any doubt all physical phenomena produced by the so-called Imponderabilia may be classified under one of these heads . . . But there remains an immense amount of labor to be done, before, starting from this passing suggestion, we shall arrive at a complete insight into the nature of these things.[3]

"With regard to heat, besides showing that its nature or form is *motion*, which is the principal object of the paper, the writer states (p. 422):

'What . . . annuls (destroys) a force, must itself be a force.'[4]

"And again (p. 422):

'What . . . produces (causes) a force must itself be a force.'[5]

Whence he concludes, considering the effects of heat (p. 421):

'Heat appears as a force in innumerable cases.[6]

"Considering the remarks of Placidus Heinrich as a casual generalization which is found in the writings of other authors of the last century and the beginning of the present (RUMFORD, DAVY, FARADAY and others), and considering furthermore the fact that Mohr's object was to show that heat is not imponderable matter, but consists of the oscillatory motion of ponderable matter, and to prove that this is the case with all the other Imponderabilia so-called, and that because all of the latter, are introconvertible and convertible into forces, we are justified, nay even forced by irresistible logic to declare all these agencies—forces as well as motions—to be different manifestations of one and the same thing, *it is established beyond any doubt that* FRIEDRICH MOHR *was the first who in clear and convincing language stated the great principles in question.*

"This does, of course, not detract from the merit of MAYER or HELMHOLTZ, or any other author who has arrived independently at similar conclusions, and no one is more ready than MOHR himself to give credit to whom it is due. In the explanatory statement given with the reprint of the essay, he quotes TYNDALL's reference to DR. MAYER's paper mentioned above and indorses the praise contained in it with all his heart. He says:

"I fully accept this statement by TYNDALL. The laying down of *the mechanical equivalent of heat* is one of the principal points in this matter, but it does not exhaust it. MAYER had stated it and calculated it from known facts to be 365 Kilogrammometers. That this figure is not the same we now adopt (424 KM.), does not detract from MAYER's merits in the least; the foremost thing was *to lay down the principle that mechanical motion has its equivalent in heat, that one originates from the other, that both are therefore equivalent to wit: motions.*[7]

"But he continues:

'While thus with all my heart recognizing the great merits of MAYER (and JOULE), I would be unjust towards myself if I should pass in silence over my former writings, which by peculiar circumstances have not become generally known.[8]

**McMillan & Co., London*, 1877.

1. . . Unterdessen wissen wir wenigstens soviel mit Zuverlässigkeit, dass in der Natur nichts veloren geht . . alles erklärt sich durch einen steten Umtausch; das eine gewinnt durch Verlust des Andern: das Eine entsteht durch das Verschwinden des Andern. . . Also im Universum nie Verlust, nur Wechsel und Umtausch. . . (Vol. II., s. 283.)

2. Ausser den bekannten 54 chemischen Elementen gibt es in der Natur der Dinge nur noch ein Agens und dieses heisst *Kraft;* es kann unter passenden Verhältnissen als Bewegung, chemische Affinität, Cohäsion, Electricität, Licht, Wärme und Magnetismus hervortreten, und aus jeder dieser Erscheinungsarten können alle übrigen hervorgebracht werden . . Vermöge der Kraft des Armes reisst man die Inductionstrolle von einem Magneten los, es entsteht in dem darum geschlungenen Schraubendrahte ein electrischer Strom, welcher bei Unterbrechung als Funke, oner bei verengerter Leitung als glühender Draht (*Wärme und Licht*) erscheint; derselbe erregt *magnetische Palarität*, wenn er als Schraubendraht um eine Stahlenadel geleitet wird; er zersetzt das Wasser wodurch er geleitet wird, und hebt zugleich seine *Affinität und Cohäsion* auf; und da nun der dünne Platindraht, die Ampèresche Schraube und der Wasserzersetzungsapparat gleichzeitig in derselben Kette eingeschlossen sein können, so leuchtet ein, *wie die Kraft, der Armes unter verschiedenen Verhaltnissen als Warme, Licht, Chemische Affinität, Magnetismus und Cohasion zum Vorschein gekommen ist.* (Baumgartner's Zeitschr. f. Physik, &c. Vol. V., s. 442-43.)

* In his reprint, Mohr remarks that at most places where the word "*Kraft*" was used in this first essay, he would now have "*Bewegung.*" —R.

3. Ohne Zweifel lassen sich alle physikalischen Erscheinungen der sogenannten Imponderabilien unter einer dieser Rubriken bringen. . . Es bleibt aber von dieser flüchtigen Andeutung bis zur vollkommen Einsicht in die Natur der Sache noch unendlich viel zu thun übrig. (s. 445.)

4. Was . . eine Kraft aufhebt, muss selbst eine Kraft* sein.

5. 'Was . . eine Kraft hervorbringt, muss selbst eine Kraft sein.

6. Die Wärme erscheint in unzähligen Fällen als eine Kraft.

7. Ich acceptire diese Aeusserung von TYNDALL vollständig. Die Aufstellung *des mechanischen Aequivalentes der Wärme* ist ein Haupttheil der ganzen Lehre, aber es erschöpft sie nicht. MAYER hatte (LIEBIG's *Annalen*, 42, 240) das Aequivalent ausgesprochen und aus bekannten Thatsachen zu Kilogrammometern berechnet. Dass diese Zahl nicht dieselbe ist die wir jetzt annehmen (424 KM.) benimmt dem Verdienste MAYER's nicht das Geringste; *die Hauptsache war die Aufstellung des Satzes dass die mechanische Bewegung ein Aequivalent in der Wärme habe, dass eines aus dem andern entstehe dass beide also gleichartig sind nämlich Bewegung.* (s. 80.)

8. Indem ich die grossen Verdienste MAYER's (und JOULE's) in diesem Zweige der Wissenschaft mit vollem Herzen anerkenne, würde ich gegen, mich selbst eine Ungerechtigkeit begehen, wenn ich nicht meine fruheren Arbeiten, die durch einen besondern Umstand nicht zur allgemeinen Kenntniss gekommen sind, stillschweigand ubergehen wollte. (s. 82.)

"With a modesty that contrasts strongly with the severe language used by PROF. TAIT, he continues :

'To claim priority by insisting on former labors and successes seems to me inadmissible ; but nobody will be able to disclaim an essay, printed with its date in a scientific periodical, since even unpublished papers, if they have their date reliably fixed, are deemed admissible for the purpose.[9]

"And refraining with considerate carefulness from making a direct or even implied charge of plagiarism, such as DUHRING made against HELMHOLTZ and TAIT makes against MAYER, he concludes :

'If a scientist has deposited with an Academy or left with the publisher of some periodical an article with its date in a sealed envelope, he can at any time afterwards prove his claim of priority by opening it ; the second discoverer is, however, then justified in declaring that he had not and could not have had any knowledge of the contents of that letter ; *this he cannot say of a periodical regularly published and accessible to everybody.*[10] '

PROF. TAIT commits an error in supposing that the original paper by MOHR was published in Liebig's *Annalen der Chemie.* This is probably due to the fact that in the *Annalen der Pharmacie*, of which MOHR was an associate editor, a short synopsis of his paper appeared under the general heading of a "*Revue für das Jahr* 1837." (24, 141), bearing the same title, "*Ueber die Natur der Wärme.*" The charge of plagiarism which TAIT distinctly and deliberately makes on the supposed fact that MAYER's essay appeared in the same *Annalen* where MOHR's original paper was printed, can no longer be sustained ; and one may reasonably hope henceforth to hear of it no more. It should never have been uttered.

In conclusion, it may be stated that, in thus presenting for the first time in the columns of "SCIENCE" the complete documentary evidence of Prof. MOHR's priority in regard to the discovery of the great principle of the correlation of forces and the conservation of energy, we have been actuated by no other motive than that which underlies all science, viz.: to seek and proclaim the truth and nothing but the truth, and at the same time to do justice to whom justice is due. And no man deserves more to be accorded an honor which he is entitled to than FRIEDRICH MOHR, whose rare genius and masterly mind never betrayed him into committing such errors of judgment as may be laid at the door of almost every other writer on the subject.

WYANDOTTE GOVERNMENT,

A SHORT STUDY OF TRIBAL SOCIETY, DELIVERED AT THE BOSTON MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, AUGUST 1, 1880.

BY MAJOR J. W. POWELL, Director, Bureau of Ethnology, Washington.

In the social organization of the Wyandottes four groups are recognized, the family, the gens, the phratry, and the tribe.

THE FAMILY.

The family, as the term is here used, is nearly synonymous with the household. It is composed of the persons who occupy one lodge, or in their permanent wigwams, one section of a communal dwelling. These permanent dwellings are constructed in an oblong form of poles interwoven with bark. The fire was placed in line along the centre, and was usually built for two families, one occupying the place on each side of the fire.

The head of the family is a woman.

GENS.

The gens is an organized body of consanguineal kindred in the female line. "The woman carries the gens," is the formulated statement by which a Wyandotte expresses the idea that descent is in the female line. Each gens has the name of some animal, the ancient of such animal being its tutelar god. Up to the time when the tribe left Ohio, eleven gentes were recognized, as follows :

Deer, Bear, Highland Turtle (striped), Highland Turtle (black), Mud Turtle, Smooth Large Turtle, Hawk, Beaver, Wolf, Sea Snake, and Porcupine.

In speaking of an individual he is said to be a Wolf, a Bear, or Deer, as the case may be, meaning thereby that he belongs to that gens ; but in speaking of the body of people comprising a gens they are said to be relatives of the Wolf, the Bear, or the Deer, as the case may be.

There is a body of names belonging to each gens, so that each person's name indicates the gens to which he belongs. These names are derived from the characteristics, habits, attitudes, or mythologic stories connected with the tutelar god.

The following schedule presents the name of a man and woman in each gens as illustrating this statement :

	Indian.	English.
Man of Deer gens	De-wa-ti-re	Lean Deer
Woman " "	A-ya-jin-ta	Spotted Fawn
Man of Bear "	A-tu-e-tes	Long Claws
Woman of Bear "	Tsa'-man'-da-ka-e'	Grunting for her Young
Man of Striped Turtle gens	Ta-ha'-so^n-ta-ra-ta-se	Going Around the Lake
Woman of Striped Turtle gens	Tso-we-yuñ-kyn	Gone from the Water
Man of Mud Turtle gens	Sha-yan-tsu-wat'	Hard Skull
Woman of Mud Turtle gens	Ya^n-däsh-shu-räs	Finding Sand Beach
Man of Smooth Large Turtle gens	Hu^n'-du-cu-tá	Throwing Sand
Woman of Smooth Large Turtle gens	Tsu-ca-e^n'	Slow Walker
Man of Wolf gens	Ha-ro'-u^n-yû	One Who goes About in the Dark' a Prowler
Woman " "	Ya^n-di-no	Always Hungry
Man of Snake gens	Hu-ta-hu'-sa	Sitting in Curled Position
Woman " "	Di-je-rons	One Who Ripples the Water
Man of Porcupine gens	Ha^n-du'-tu^n	The One Who Puts Up Quills
Woman of Porcupine gens	Ke'-ya-runs-kwa	Good-Sighted

THE PHRATRY.

There are four phratries in the tribe, the three gentes Bear, Deer, and Striped Turtle constituting the first ; the Highland Turtle, Black Turtle, and Smooth Large Turtle the second ; the Hawk, Beaver, and Wolf the third ; and the Sea Snake and Porcupine the fourth.

This unit in their organization has a mythologic basis, and is chiefly used for religious purposes, in the preparation of medicines, and in festivals and games.

The eleven gentes as four phratries constitute the tribe.

Such is the social organization of the Wyandottes.

Each gens is a body of consanguineal kindred in the female line, and each gens is allied to other gentes by consanguineal kinship through the male line, and by affinity through marriage.

To be a member of the tribe it is necessary to be a member of a gens ; to be a member of a gens it is necessary to belong to some family ; and to belong to a family a person must have been born in the family so that his kinship is recognized ; or he must be adopted into a family and become

9. Eine Prioritat durch Behauptung fruherer Arbeiten and Erfolge beanspruchen zu wollen, halte ich für unzueässig. allein einen in einer wissenschaftlichen Zeitschrift gedruckten und mit dem Datum versehenen Aufsatz geltend zu machen wird Niemand zurückweisen können da sogar ungedruckte Aufsätze, wenn sie ein sicheres Datum haben zu diesem Zwecke zugelassen werden. (s. 84.)

10. Hat ein Naturforscher bei einer Akademie oder bei dem Herausgeber einer Zeitschrift durch einen verschlossenen Brief Datum genommen, so kann er nachher durch Offnung des Briefes seine Prioritätsanspruche beweisen ; der zweite Entdecker kann aber dann mit Recht sagen, dass er von dem Inhalte des Briefes keine Kentniss hatte und nicht haben Konnte, *das kann er aber bei einer regelmäs sig erscheinenden und Jedem zugänglichen Zeitschrift nicht sagen.* (s. 84.)

a son, brother, or some definite relative; and this artificial relationship gives him the same standing as actual relationship in the family, in the gens, in the phratry and in the tribe.

Thus a tribe is a body of kindred.

Of the four groups thus described, the gens, the phratry and the tribe constitute the series of organic units; the family or household as here described, is not a unit of the gens or phratry, as two gentes are represented in each—the father must belong to one gens and the mother and her children to another.

GOVERNMENT.

Society is maintained by the establishment of government, for rights must be recognized and duties performed.

In this tribe there is found a complete differentation of the military from the civil government.

CIVIL GOVERNMENT.

The civil government inheres in a system of councils and chiefs.

In each gens there is a council, composed of four women, called Yu-waí-yu-wá-na. These four women councillors select a chief of the gens from its male members—that is, from their brothers and sons. This gentile chief is the head of the gentile council.

The council of the tribe is composed of the aggregated gentile councils. The tribal council, therefore, is composed one-fifth of men and four-fifths of women.

The sachem of the tribe or tribal chief is chosen by the chiefs of the gentes.

There is sometimes a grand council of the gens, composed of the councillors of the gens proper and all the heads of households and leading men—brothers and sons.

There is also sometimes a grand council of the tribe, composed of the council of the tribe proper and the heads of households of the tribe and all the leading men of the tribe.

These grand councils are convened for special purposes.

METHODS OF CHOOSING AND INSTALLING COUNCILLORS AND CHIEFS.

The four women councillors of the gens are chosen by the heads of households—themselves being women. There is no formal election, but frequent discussion is had over the matter from time to time, in which a sentiment grows up within the gens and throughout the tribe that, in the event of the death of any councillor, a certain person will take her place.

In this manner there is usually one, two or more potential councillors in each gens who are expected to attend all the meetings of the council, though they take no part in the deliberations and have no vote.

When a woman is installed as councillor a feast is prepared by the gens to which she belongs, and to this feast all the members of the tribe are invited. The woman is painted and dressed in her best attire, and the sachem of the tribe places upon her head the gentile chaplet of feathers, and announces in a formal manner to the assembled guests that the woman has been chosen a councillor. The ceremony is followed by feasting and dancing, often continued late into the night.

The gentile chief is chosen by the council women after consultation with the other women and men of the gens. Often the gentile chief is a potential chief through a period of probation. During this time he attends the meetings of the council, but takes no part in their deliberations, and has no vote.

At his installation, the council women invest him with an elaborately ornamented tunic, place upon his head a chaplet of feathers, and paint the gentile totem on his face. The sachem of the tribe then announces to the people that the man has been made chief of the gens, and admitted to the council. This is also followed by a festival.

The sachem of the tribe is selected by the men belonging to the council of the tribe. Formerly the sachemship inhered in the Bear gens but at present he is chosen from the Deer gens from the fact, as the Wyandottes say, that death has carried away all the wise men of the Bear gens.

The chief of the Wolf gens is the herald and sheriff of the tribe. He superintends the erection of the council house, and has the care of it. He calls the council together in a formal manner when directed by the sachem. He announces to the tribe all the decisions of the council, and executes the directions of the council and of the sachem.

Gentile councils are held frequently from day to day and from week to week, and are called by the chief whenever deemed necessary. When matters before the council are considered of great importance, a grand council of the gens may be called.

The tribal council is held regularly on the night of the full moon of each lunation and at such other times as the sachem may determine; but extra councils are usually called by the sachem at the request of a number of councillors.

Meetings of the gentile councils are very informal; but the meetings of the tribal councils are conducted with due ceremony. When all the persons are assembled, the chief of the Wolf gens calls them to order, fills and lights a pipe, sends one puff of smoke to the heavens and another to the earth. The pipe is then handed to the sachem who fills his mouth with smoke, and, turning from left to right with the sun, slowly puffs it out over the heads of the councillors who are sitting in a circle. He then hands the pipe to the man on his left, and it is smoked in turn by each person until it has been passed around the circle. The sachem then explains the object for which the council is called. Each person in the way and manner he chooses, tells what he thinks should be done in the case. If a majority of the council is agreed as to action, the sachem does not speak, but may simply announce the decision. But in some cases there may be protracted debate which is carried on with great deliberation. In case of a tie, the sachem is expected to speak.

It is considered dishonorable for any man to reverse his decision after having spoken.

Such are the organic elements of the Wyandotte government.

FUNCTIONS OF CIVIL GOVERNMENT.

It is the function of government to preserve rights and enforce the performance of duties. Rights and duties are co-relative. Rights imply duties, and duties imply rights. The right inhering in the party of the first part imposes a duty on the party of the second part. The right and its co-relative duty are inseperable parts of a relation that must be maintained by government; and the relations which governments are established to maintain may be treated under the general head of rights.

In Wyandotte government, these rights may be classed as follows:

First:—Rights of marriage.
Second:—Rights to names.
Third:—Rights to personal adornments.
Fourth:—Rights of order in encampments and migrations.
Fifth:—Rights of property.
Sixth:—Rights of person.
Seventh:—Rights of community.
Eighth:—Rights of religion.

To maintain rights, rules of conduct are established, not by formal enactment but by regulated usage. Such custom-made laws may be called regulations.

MARRIAGE REGULATION.

Marriage between members of the same gens is forbidden, but consanguinial marriages between persons of different gentes are permitted. For example, a man may not marry his mother's sister's daughter, as she belongs to the same gens with himself; but he can marry his father's sister's daughter, because she belongs to a different gens.

Husbands retain all their rights and privileges in their own gentes, though they live with the gentes of their wives. Children, irrespective of sex, belong to the gens of the mother. Men and women must marry within the tribe. A woman taken to wife from without the tribe, must first be adopted into some family of a gens other than that to which the man belongs. That a woman may take for a husband a man without the tribe, he must also be adopted into the family of some gens other than that of the woman. What has been called by some ethnologists endogamy and exogamy, are correlative parts of one regulation, and the Wyandottes, like all other tribes of which we have any knowledge in North America, are both endogamous and exogamous.

Polygamy is permitted, but the wives must belong to different gentes. The first wife remains the head of the household. Polyandria is prohibited. A man seeking a wife consults her mother, sometimes direct, and sometimes through his own mother. The mother of the girl advises with the women councillors to obtain their consent, and the young people usually submit quietly to their decision. Sometimes the women councillors consult with the men.

When a girl is betrothed, the man makes such presents to the mother as he can. It is customary to consummate the marriage before the end of the moon in which the betrothal is made. Bridegroom and bride make promises of faithfulness to the parents and women councillors of both parties. It is customary to give a marriage feast in which the gentes of both parties take part. For a short time at least, bride and groom live with the bride's mother, or rather in the original household of the bride.

The time when they will set up housekeeping for themselves is usually arranged before marriage.

In the event of the death of the mother the children belong to her sister or to her nearest female kin, the matter being settled by the council women of the gens. As the children belong to the mother, on the death of the father the mother and children are cared for by her nearest male relative until subsequent marriage.

NAME REGULATIONS.

It has been previously explained that there is a body of names, the exclusive property of each gens. Once a year, at the green-corn festival, the council women of the gens select the names for the children born during the previous year, and the chief of the gens proclaims these names at the festival. No person may change his name, but every person, man or woman, by honorable or dishonorable conduct, or by remarkable circumstance, may win a second name commemorative of deed or circumstance, which is a kind of title.

REGULATIONS OF PERSONAL ADORNMENT.

Each clan has a distinctive method of painting the face, a distinctive chaplet to be worn by the gentile chief and council women when they are inaugurated, and subsequently at festival occasions, and distinctive ornaments for all its members, to be used at festivals and religious ceremonies.

REGULATIONS OF ORDER IN ENCAMPMENT AND MIGRATIONS.

The camp of the tribe is in an open circle or horseshoe, and the gentes camp in the following order, beginning on the left and going around to the right:

Deer, Bear, Highland Turtle (striped), Highland Turtle (black), Mud Turtle, Smooth Large Turtle, Hawk, Beaver, Wolf, Sea Snake, Porcupine.

The order in which the households camp in the gentile group is regulated by the gentile councillors and adjusted from time to time in such a manner that the oldest family is placed on the left, and the youngest on the right. In migrations and expeditions the order of travel follows the analogy of encampment.

PROPERTY RIGHTS.

Within the area claimed by the tribe each gens occupies a smaller tract for the purpose of cultivation. The right of the gens to cultivate a particular tract is a matter settled in the council of the tribe, and the gens may abandon one tract for another only with the consent of the tribe. The women councillors partition the gentile land among the householders, and the household tracts are distinctly marked by them. The ground is re-partitioned once in two years. The heads of households are responsible for the cultivation of the tract, and should this duty be neglected the council of the gens calls the responsible parties to account.

Cultivation is communal; that is, all of the able-bodied women of the gens take part in the cultivation of each household tract in the following manner:

The head of the household sends her brother or son into the forest or to the stream to bring in game or fish for a feast; then the able-bodied women of the gens are invited to assist in the cultivation of the land, and when this work is done a feast is given.

The wigwam or lodge and all articles of the household belong to the woman—the head of the household—and at her death are inherited by her eldest daughter, or nearest of female kin. The matter is settled by the council women. If the husband die his property is inherited by his brother or his sister's son, except such portion as may be buried with him. His property consists of his clothing, hunting and fishing implements and such articles as are used personally by himself.

Usually a small canoe is the individual property of the man. Large canoes are made by the male members of the gentes, and are the property of the gentes.

RIGHTS OF PERSON.

Each individual has a right to freedom of person and security from personal and bodily injury, unless adjudged guilty of crime by proper authority.

COMMUNITY RIGHTS.

Each gens has the right to the services of all its women in the cultivation of the soil. Each gens has the right to the service of all its male members in avenging wrongs, and the tribe has the right to the service of all its male members in time of war.

RIGHTS OF RELIGION.

Each phratry has the right to certain religious ceremonies and the preparation of certain medicines.

Each gens has the exclusive right to worship its tutelar god, and each individual has the exclusive right to the possession and use of a particular amulet.

CRIMES.

The violations of rights are crimes. Some of the crimes recognized by the Wyandottes are as follows:

1. Adultery.
2. Theft.
3. Maiming.
4. Murder.
5. Treason.
6. Witchcraft.

A maiden guilty of fornication may be punished by her mother or female guardian, but if the crime is flagrant and repeated, so as to become a matter of general gossip, and the mother fails to correct it, the matter may be taken up by the council women of the gens.

A woman guilty of adultry, for the first offence is punished by having her hair cropped; for repeated offences her left ear is cut off.

THEFT.

The punishment for theft is two-fold restitution. When the prosecutor and prosecuted belong to the same gens, the trial is before the council of the gens, and from it there is no appeal. If the parties involved are of different gentes, the prosecutor, through the head of his household, lays the matter before the council of his own gens; by it the matter is laid before the gentile council of the accused in a formal manner. Thereupon it becomes the duty of the council of the accused to investigate the facts for themselves, and to settle the matter with the council of the plaintiff. Failure thus to do is followed by retaliation in the seizing of any property of the gens which may be found.

MAIMING.

Maiming is compounded, and the method of procedure in prosecution is essentially the same as for theft.

MURDER.

In the case of murder, if both parties are members of the same gens, the matter is tried by the gentile council on complaint of the head of the household, but there may be an appeal to the council of the tribe. Where the parties belong to different gentes, complaint is formally made by the injured party, through the chief of his gens, in the following manner:

A wooden tablet is prepared upon which is inscribed the totem or heraldic emblem of the injured man's gens, and a picture writing setting forth the offence follows.

The gentile chief appears before the chief of the council of the offender, and formally states the offence, explaining the picture-writing, which is then delivered.

A council of the offender's gens is thereupon called and a trial is held. It is the duty of this council to examine the evidence for themselves and to come to a conclusion without further presentation of the matter on the part of the person aggrieved. Having decided the matter among themselves, they appear before the chief of the council of the aggrieved party to offer compensation.

If the gens of the offender fail to settle the matter with the gens of the aggrieved party, it is the duty of his nearest relative to avenge the wrong. Either party may appeal to the council of the tribe. The appeal must be made in due form, by the presentation of a tablet of accusation.

Inquiry into the effect of a failure to observe prescribed formalities developed an interesting fact. In procedure against crime, failure in formality is not considered a violation of the rights of the accused, but proof of his innocence. It is considered supernatural evidence that the charges are false. In trials for all offences forms of procedure are, therefore, likely to be earnestly questioned.

TREASON.

Treason consists in revealing the secrets of the medicine preparations or giving other information or assistance to enemies of the tribe, and is punished by death. The trial is before the council of the tribe.

WITCHCRAFT.

Witchcraft is punished by death, stabbing, tomahawking, or burning. Charges of witchcraft are investigated by the grand council of the tribe. When the accused is adjudged guilty, he may appeal to supernatural judgment. The test is by fire. A circular fire is built on the ground through which the accused must run from east to west, and from north to south. If no injury is received, he is adjudged innocent; if he falls into the fire, he is adjudged guilty. Should a person accused or having the general reputation of practising witchcraft become deaf, blind, or have sore eyes, ear ache, headache, or other diseases considered loathsome, he is supposed to have failed in practising his arts upon others and to have fallen a victim to them himself. Such cases are most likely to be punished.

OUTLAWRY.

The institution of outlawry exists among the Wyandottes in a peculiar form. An outlaw is one who by his crimes has placed himself without the protection of his clan. A man can be declared an outlaw by his own clan, who thus publish to the tribe that they will not defend him in case he is injured by another. But, usually, outlawry is declared only after trial before the tribal council.

The method of procedure is analogous to that in case of murder. When the person has been adjudged guilty, and sentence of outlawry declared, it is the duty of the chief of the Wolf clan to make known the decision of the council. This he does by appearing before each clan in the order of its encampment, and declaring in terms the crimes of the outlaw and the sentence of outlawry, which may be either of two grades.

In the lowest grade it is declared that, if the man shall thereafter continue in the commission of similar crimes, it will be lawful for any person to kill him; and if killed, rightfully or wrongfully, his clan will not avenge his death.

Outlawry of the highest degree makes it the duty of any member of the tribe who may meet with the offender to kill him.

MILITARY GOVERNMENT.

The management of military affairs inheres in the military council and chief. The military council is composed of all the able-bodied men of the tribe; the military chief is chosen by the council from the Porcupine gens. Each gentile chief is responsible for the military training of the youth under his authority. There is usually one or more potential military chiefs who are the close companions and assistants of the chief in time of war, and in case of the death of the chief take his place in the order of seniority.

Prisoners of war are adopted into the tribe or killed. To be adopted into the tribe it is necessary that the prisoner should be adopted into some family. The warrior taking the prisoner has the first right to adopt him, and his male or female relatives have the right in the order of their kinship. If no one claims the prisoner for this purpose he is caused to run the gauntlet, as a test of his courage.

If at his trial he behaves manfully, claimants are not wanting, but if he behaves disgracefully he is put to death.

FELLOWHOOD.

There is an interesting institution found among the Wyandottes, as among some other of our North American tribes, namely, that of fellowhood. Two young men agree to be perpetual friends to each other, or more than brothers. Each reveals to the other the secrets of his life, and counsels with him on matters of importance, aud defends him from wrong and violence, and at his death is chief mourner.

The government of the Wyandottes, with the social organization upon which it is based, affords a typical example of tribal Government throughout North America. Within that area there are several hundred distinct governments. In so great a number there is great variety, and in this variety we find different degrees of organization, the degree of organization being determined by the differentiation of the functions of government and the correlative specialization of organic elements.

Much has yet to be done in the study of these governments before safe generalizations may be made. But enough is known to warrant the following statement.

Tribal government in North America is based on kinship in that the fundamental units of social organization are bodies of consanguineal kindred either in the male or female line: these units being what has been well denominated "gentes."

These "gentes" are organized into tribes by ties of relationship and affinity, and this organization is of such a character that the man's position in the tribe is fixed by his kinship. There is no place in a tribe for any person whose kinship is not fixed, and only those persons can be adopted into the tribe who are adopted into some family with artificial kinship specified. The fabric of Indian society is a complex tissue of kinship. The warp is made of streams of kinship blood, and the woof of marriage ties.

With most tribes military and civil affairs are differentiated. The functions of civil government are in general differentiated only to this extent, that executive functions are performed by chiefs and sachems, but these chiefs and sachems are also members of the council. The council is legislature and court. Perhaps it were better to say that the council is the court whose decisions are law, and that the legislative body properly has not been developed.

In general crimes are well defined. Procedure is formal, and forms are held as of such importance that error therein is *prima facie* evidence that the subject matter formulated was false.

When one gens charges crime against a member of another, it can of its own motion proceed only to retaliation. To prevent retaliation, the gens of the offender must take the necessary steps to disprove the crime, or to compound or punish it. The charge once made is held as just and true until it has been disproved, and in trial the cause of the defendant is first stated. The anger of the prosecuting gens must he placated.

In the tribal governments there are many institutions, customs, and traditions which give evidence of a former condition in which society was based, not upon kinship, but upon marriage.

From a survey of the facts it seems highly probable that kinship society, as it exists among the tribes of North America, has developed from connubial society, which is discovered elsewhere on the globe. In fact, there are few tribes tnat seem scarcely to have passed that indefinite boundary between the two social states. Philologic research leads to the same conclusion.

Nowhere in North America have a people been discovered who have passed beyond tribal society to national society based on property, *i.e.*, that form of society which is characteristic of civilization. Some peoples may not have reached kinship society; none have passed it.

Nations with civilized institutions, art with palaces, monotheism as the worship of the Great Spirit, all vanish from the priscan condition of North America in the light of anthropologic research.

Tribes, with the social institutions of kinship, art with its highest architectural development exhibited in the structure of communal dwellings, and polytheism in the worship of mythic animals and nature-gods, remain.

THE GENESIS OF CERTAIN IRON ORES.*

By Dr. T. Sterry Hunt, LL. D., F.R.S.

Dr. Hunt began by considering the presence of iron, generally in a ferrous condition, in mineral silicates, in the crystalline rocks, and its liberation therefrom by the sub-ærial decay of these as hydrous ferric oxide. This, as is well known, is, by the agency of organic matter, again reduced to ferrous oxide, which is dissolved in natural waters by carbonic acid or some organic acid, from which solutions it may be deposited either as hydrous peroxide (limonite, etc.,) as carbonate (siderite), as silicate, or as sulphide (pyrite, etc.), in all of which forms iron is found in sedimentary deposits. As regards the formation of siderite, he described experiments which show that solutions holding five grammes of ferrous carbonate dissolved as di-carbonate in a litre of water, are spontaneously decomposed in close vessels at the ordinary temperature, and deposit two-thirds of their iron as a white crystalline (hydrated) mono-carbonate, with liberation of carbonic-dioxide. This serves to render more intelligible the reduction and segregation of iron as siderite in earthy sediments, as long since pointed out by W. B. Rogers, for the ores of the coal-measures.

The intervention of soluble sulphates, and their reduction through organic agency to sulphides, determines the formation of sulphide of iron in sediments. The generation of a bi-sulphide (pyrite or marcasite) was then discussed, and it was shown that the ferrous mono-sulphide, which naturally is first generated, may fix a further portion of sulphur and thus form a more stable compound. One example of this is seen when recently precipitated hydrous ferrous sulphide is brought in contact with a solution of a ferric salt, which takes up a portion of the iron, leaving sulphur free to unite with the undecomposed sulphide, and form therewith a very stable higher sulphide of iron. Experiments now in progress lead the writer to believe that sulphur liberated from soluble sulphides may, in a similar manner, unite with ferrous sulphide, and thus help us to explain the generation of pyrites in nature, in the presence of water, at ordinary temperatures.

The changes of siderite and pyrite under atmospheric influences were next considered. The latter by oxidation yields, as is well known, ferrous sulphate. Its frequent conversion by sub-ærial decay into limonite was conceived to be due to the intervention of water, holding carbonates, which, conjointly with oxygen, changes it into hydrous peroxide (limonite), which often retains the form of the pyrites. The transformation of carbonate of iron into hydrous peroxide is a familiar fact.

Limonite ores may thus be produced in three ways. They are sometimes formed by the peroxidation and precipitation of dissolved ferrous salts, as in the so-called bog-ores; but more frequently from the alteration *in situ* of deposits of pyrite or of siderite. Such are the limonites which mark the outcrops of beds or veins of pyrites in the decayed crystalline rocks of the Blue Ridge. The similar ores found in the decayed Taconic schists of the great Appalachian valley can be shown to be due in some cases to the alteration of included pyritous masses, and in others to the alteration of similar masses of siderite, both of which are found in the unaltered Taconic rocks, as, indeed, at various other horizons in the geological series.

If we take the specific gravity of pyrites at 5.0, we shall find that its complete conversion into a limonite of sp. gr. 4.0 would be attended with a contraction of only 2.7 hundredths, while if the limonite have a sp. gr. of 3.6, there would be an augmentation of 10.7 p. c. With siderite of sp. gr. 3.6, on the contrary, its conversion into limonite of the same density would result in a contraction of 19.5 p. c., and into lemonite of sp. gr. 4.0 to a contraction of 27.5 p. c. The evidences of this contraction may be seen in the structure of the limonite derived from siderite. The process operates from the surface of the masses, often resulting in the production of geodes. Their structure will generally serve to distinguish the sideritic from the pyritic limonites.

These differences were illustrated in the history of various iron ores in the Appalachian valley, and it was further pointed out that the pyritic limonites, other circumstances being equal, should be freer from phosphorus than those derived from siderite, since the native carbonates almost always contain phosphates, from which pyritous deposits are comparatively free. The source of limonites thus becomes a question of importance to the metallurgist. In conclusion it wus pointed out that deposits of manganese ores are, in some cases at least, generated by the alteration *in situ* of manganous carbonates, by a process analogous to that by which limonite is produced from siderite.—

* Read before the A. A. A. S., Boston, 1880.

MICROSCOPY.

NEW CELL FOR OPAQUE OBJECTS.

I desire to call the attention of the microscopists and preparers of objects generally to the new rubber cell for opaque slides, recently devised by me. A considerable experience in mounting opaque slides during the past few years has convinced me that much of the labor incident to it could be avoided, if a cell of suitable material and shape could be produced at a nominal cost. This, I think, has now been attained, and I take pleasure in submitting one for which I claim convenience, cheapness, and general utility. With it the amateur can produce a slide fully as perfect, and with as great a degree of neatness as can the professional. The cell is of hard rubber, highly polished, and of attractive shape; the base is solid, thus giving a black back-ground of rubber; around the top is a ledge fitted to receive a one-half inch cover glass; this, being secured by a little shellac or any similar cement, completes the mounting. The cell may be attached to a glass slip by any cement, before or after preparation. For exchanges it offers superior advantages, inasmuch as the cell, with objects enclosed, may be sent through the mails independent of the glass slips, the recipient attaching them. In this way a saving is made in postage, and no risk of loss by slips being broken in transit.

They will solve the problem which often perplexes the student or collector who is crowded for cabinet room. Many objects for future reference may be mounted in this simple cell, numbered and put away without a slide, a cabinet drawer holding two hundred of them, while but forty slides could be accomodated in the same space.

The above sectional view conveys a good idea of its shape, the dotted line indicating the position of the thin glass cover.

I have made arrangements to have them supplied by the following firms at thirty cents per dozen, five cents extra on single dozens to cover cost of postage and box, and they may be obtained from the parties mentioned below or from the subscriber. In remitting small sums three cent postage samps may be used.

Geo. S. Woolman, No. 116 Fulton St., New York; Jas. W. Queen & Co., Chestnut St., Philadelphia; Bausch & Dransfield, Arcade, Rochester, N. Y.; W. H. Bullock, No. 126 So. Clark St., Chicago, Ills.

In conclusion I would add that I have had these rubber cells prepared without regard to any pecuniary gain to myself, hoping they may prove an aid to those engaged in microscopical research.

H. F. Atwood,
No. 50 Hamilton Place, Rochester, N. Y.

[We have seen a sample of Mr. Atwood's rubber cell, and consider it a very perfect arrangement for opaque objects.—Ed.]

THE TWO KINDS OF VIVISECTION—SENTISECTION AND CALLISECTION.

Professor BURT G. WILDER, M. D., of Cornell University, writing to the *Medical Record*, says: Is it not time for the distinct verbal recognition of the difference between painful and painless experimentation upon animals?

All well-informed persons are aware that the vast majority of vivisections, in this country at least, are performed under the influence of anæsthetics; but the enthusiastic zoölaters, who desire to abolish the objective method of teaching physiology, practically ignore this fact, and dwell chiefly upon the comparatively infrequent operations which are attended with pain.

Having read the arguments upon both sides, and had some correspondence with leaders of the anti-vivisection movement, I have been led to think that the discussion may be simplified, and a right conclusion sooner reached, if we adopt new terms corresponding to the two kinds of experimentation.

To use words with no warrant of ideas may be foolish, but it is not necessarily a mark of wisdom to refrain from the employment of terms which have a real significance.

Let us consider an analogous case. Aside from color and size, the *cat* and the *leopard* are almost identical, and are commonly regarded as two species of one genus. Suppose a community to be unacquainted with the cat, but to have suffered from the depredations of the leopard, which they call *felis*. Now, suppose some domestic cats to be introduced and to multiply, as is their wont. In the first place, for a time at least, it is probable that the same name, *felis*, would be applied to the smaller animal, with perhaps a qualifying word. In the second place, should there be certain persons, both devoid of interest in the cats and filled with pity for the mice devoured by them, is it not likely that they would endeavor to include the cats under any ban which might be pronounced against the leopards? Would they not be apt to succeed, especially with the more ignorant and impressionable members of the community, so long as they could assert without contradiction that the "mouse-eater" was only a *felis* upon a smaller scale? Would not even the reputation of the leopards suffer by reason of the multitude of the cats thus associated with them? In short, would full justice be done to either animal until their differences of disposition should be admitted to outweigh their likeness of form and structure, and be recognized by the use of distinctive names?

In like manner there are those who ignorantly or wilfully persuade themselves and others that all experiments upon animals are painful because some of them are now, and most of them were in former times; also, that painful experiments are common because vivisection in some form is generally practiced. It is all *vivisection*, and as such it is "cruel, revolting, or brutalizing."

Having waited long in the hope that some candid discussion of the whole subject might contain the needed terms, I venture to suggest that painful vivisection be known as *sentisection*, and painless vivisection as *callisection*. The etymology of the former word is obvious; the distinctive element of the latter is the Latin *callus*, which in a derived sense, may denote a nervous condition unrecognized, strictly speaking, by the ancients.

some idea of the relative numbers of callisectionists and sentisectionists may be gained from the fact that I have been teaching physiology in a university for twelve years, and for half that time in a medical school; yet I have never performed a sentisection, unless under that head should be included the drowning of cats and the application of water at the temperature of 60° C. (140° F.), with the view to ascertain whether such treatment would be likely to succeed with human beings.

I think that even elementary physiological instruction is incomplete without callisection, but that sentisection should be the unwelcome prerogative of the very few whose natural and acquired powers of body and mind qualify them above others to determine what experiments should be done, to perform them properly, and to wisely interpret the results. Such men, deserving alike of the highest honor and the deepest pity, should exercise their solemn office not only unrestrained by law, but upheld by the general sentiment of the profession and the public.

FEELING AND FUNCTION AS FACTORS IN HUMAN DEVELOPMENT.*

By LESTER F. WARD, A. M.

Sociology is now recognized as a legitimate branch of Anthropology.

The great French philosopher, Auguste Comte, although the first to introduce the word *Sociology*, did not venture to use this term extensively himself, but preferred the expression *Social Physics*, which must therefore be accepted as the true definition of sociology as intended by the father of the science.

It is important to remember this fact and to preserve throughout this necessary connection between social science and physical science. This, however, has not always been done. The phenomena of human development, may be contemplated from two quite distinct points of view, only one of which has thus far received sufficient attention. These two points of view are those respectively of feeling and of function, and it is the first of them that has been neglected. According to the usual method of approaching such questions, man is regarded as a being requiring for his preservation a certain amount of nourishment and for his perpetuation the begetting of offspring. The two essential factors from this point of view are the functions of nutrition and reproduction. Around the first of these cluster the industrial activities, and upon the second is founded the family. Out of these grow all the later and more complex characteristics of civilization. According to the other method of contemplating human development, man is regarded as a being endowed with feelings. These feelings are in the nature of desires. The existence of such desires involves the effort to gratify them, which effort in turn gives rise to human activities. The condition of society at any time is the result of these activities, just as from the point of view of function, nutrition and reproduction are the two primary essential factors; so, from the point of view of feeling, the gustatory and sexual appetites are the primary and essential factors. The advantage of the latter method over the former is that it affords, as the other does not, a scientific basis for the investigation of the laws of anthropology. The action of an organism in seeking the satisfaction of a desire finds an exact parallel in the action of a chemical molecule in seeking combination with others, or that of a column of air in rushing in to fill a vacuum. The desires of individuals constitute true forces, identical in all respects with the physical forces which other sciences deal with, and all branches of anthropology, including that of sociology, at once take their places as true sciences. This antithesis may perhaps be rendered more striking by considering function as the object which nature seeks, and feeling as that which man seeks. The object or end of nature is the preservation and perpetuation of existing life; that of man, and of all beings endowed with feeling, is the satisfaction of existing desires. The former is objective and constitutes a biological process; the latter is subjective, and is a moral or sociological process.

Properly understood these precesses possess no natural or necessary relation to each other. It is easy to imagine a person wholly destitute of taste. Indeed such cases are on record. The pleasure derived from the contact of nutritious substances with the tongue and palate is obviously distinct from the benefit which it confers upon the system after digestion. Such a person as we have supposed would none the less need food because he had no desire to partake of it.

It is still more easy to conceive of a total absence of the sexual instinct, and this is a much more common pathological condition found in practice. Here the feeling is still more obviously distinct from the function.

Why then do these desires and their functional results so universally accompany each other? The answer is that this apparently "pre-established harmony" of things having no necessary relation or resemblance has been the result of natural adaptation.

The agreeableness of the acts of nutrition and reproduction exists because without it nutrition and reproduction could never be secured. The existence of these pleasures,

* Read before the A. A. A. S., Boston, 1880.

as of all other pleasures, and all pain also, is explained on the theory of selection.

It is desire alone which leads to action. Among the lower animals it is the momentary impulse which always determines action. Hence these, if destitute of these passions in the gratification of which they preserve their existence and continue their kind, would speedily perish.

In man both these desires are strong and constitute the motive, either direct, or indirect, to the greater part of his acts.

There are of course other desires, many of which may be regarded as derived from these, but some of which are apparently also original and natural, but whatever they may be they are in the nature of forces, and all the desires taken together may be appropriately called the *Social Forces*. These social forces readily fall into two groups and each of these is capable of subdivision into subordinate groups, as the following table will show :

Essential Forces.	Preservative Forces.	Positive,	gustatory (pleasurable)
		Negative,	protective (painful.)
	Reproductive Forces.	Direct	(The sexual instinct.
		Indirect	Parental and consanguineal affections.
Non-essential Forces.	Esthetic.		
	Emotional.		
	Intellectual.		

Space forbids the elaboration of this table, and indeed it scarcely requires it. I will only say a word on the last group named in it, the intellectual forces. Upon this point much confusion, and as I think, error prevails. It is at the present time at least, a very small and uninfluential group. Properly it embraces nothing beyond the mere yearnings of the intellect. Its only basis is the pleasure of intellectual action.

I strenuously object to throwing the whole effect of mind in social development into the class of social forces. The social forces are indeed psychic, but they are not intellectual. The intellect is in no true scientific sense a force. It is not a motor influence.

It is characteristic of every true natural force that the body impelled or attracted by it moves in a straight line from the impelling or towards the attracting object. If it move in a curve or any but a straight line this is always due to a plurality of forces acting in different directions. This is true of all the social forces. Desire, wholly unaccompanied by reason, always impels in a direct line towards its object. This is illustrated most clearly by the acts of the lower animals.

The fly buzzing against the transparent pane until exhausted without sufficient intelligence to try another locality is an example daily witnessed. Moths seeking a flame regardless of its destructive power, and rising with scorched wings, plunging anew into the fatal charm, show the action of a force scarcely higher than the purely mechanical. It is so with every form of desire. But for the intellectual agency, to however slight a degree, all animal action, human action included, would be of this direct character. The influence of mind sustains the same relation to the true forces of desire that the rudder of a ship, moved by the helmsmen, sustains to the sails acted upon by the wind. As it is not the former that propels the ship so it is not mind that propels society. The great results which are collectively termed civilization are the direct outcome of these impulsive social forces, guided, of course, by intellect or reason. All the efforts that have been put forth have been made solely for the satisfaction of present desires. The end really reached has not been the end sought. Function has been totally ignored and feeling alone consulted. The ends of Nature have been attained, not directly as objects of pursuit, but only indirectly through the means of Nature which are the ends of the feeling creature.

It has been remarked that owing to adaptive influences these naturally independent lines leading respectively to the ends of Nature and the ends of the sentient organism converge to the same point. The effects produced by obeying the desires in most cases are the effects necessary to preserve, perpetuate, and develop the organism. But here is the fundamental distinction to be noted. These functional effects are secondary. It is not to secure them that the acts are performed. The beings performing them take no thought of them. The only effect in the mind of the agent is the satisfaction of a present desire. It may be safely said that this is almost universally the case even in human action.

But it may be asked what difference it makes, inasmuch as the indirect or functional end is always secured by the previous harmony brought about by adaptation.

With non-progressive beings like the lower animals, it may be admitted that it makes but little difference. Here the chief interest centres on biological questions, questions of anatomy, histology, morphology, etc., and therefore the objective or biological standpoint is usually, though not always, sufficient. But with man, a progressive being, whose actions transform the entire face of the planet and lift him by rapid steps from one plane of activity and life to another, it becomes of the utmost importance that the true nature of his motives be scientifically understood ; that the effects produced be attributed to their true immediate causes and not to indirect or merely incidental ones. Nutrition is not an end of human conduct in seeking food ; it is the satisfaction of hunger. A family is rarely a direct desideratum in human life. Every physician knows how often it is an object of dread. It is only an incident. The great blessings of accumulated wealth have never been the immediate object of industry and financiering skill. These are the direct results of that great derivative passion called avarice which has been so unjustly condemned. Industry, commerce, art, and often invention flow from the "love of money," which has been most superficially called "the root of all evil," when it is really the root of nearly all good in civilization. Labor is performed and heroic deeds achieved not to make the world richer and happier or set examples of nobility for future ages, but to secure the immediate wants of the individuals performing them, to gain money and applause, to win the fair and to support them. Avarice, ambition, love, each has accomplished its direct results in the true civilization of the race.

PROFESSOR EDWARD D. COPE

The bibliography of Professor Edward D. Cope has been ably written by Professor William Hosea Ballou, one of our subscribers :—Professor Ballou states that, "the life of Prof Cope is the index of all that is romantic in science. A sketch of his literature would be void of much of the interest attached without notation of some of the points in his most extraordinary career. At the early age of sixteen he began writing on scientific matters, though he must have attained twenty-four years when his writings first began to attract attention. He is one of the few living writers who has been able to successfully turn at will from any department of living biological forms to those whose remains are found only in fossil state. From studies of this nature he boldly enters the realms of metaphysics, bringing out an astounding number of genuine contributions to knowledge. In the bodies of learned men of which he is a leading figure, he astonishes all who hear him by the facility with which he addresses or converses on topics under discussion. He seems both in his writings and speeches a man prolific in voluminous knowledge of kindred subjects. His investigations have already resulted in his naming upwards of 1,000 species new to science, besides innumerable genera. He has written on every existing family of vertebrates, and revolutionized the classification of the amphibious animals by utilizing the skull as a source of differential characters. The classification of fishes has also been much modified by him.

The best part of his work is undoubtedly comprised in his paleontological (extinct animal) studies which have distinguished him throughout the scientific world. In 1879 the Royal Geological Society of Great Britain awarded him a medal for doing the most work in this line of any individual for the year."

This interesting memoir can be found in the *Chicago Field*, for August 21 and 28, and with the list of Professor Cope's literary papers and contributions, occupies eleven columns of that journal.

TWO NEW METHODS OF FIGHTING INJURIOUS INSECTS.*

By Prof. A. J. Cook.

The Codling Moth has been, and is, the most serious pest to the American pomologist. All previous remedies have only destroyed the Imago insect, after the larva of the same had destroyed the apples. London Purple, applied as a liquid mixture, one pound of the poison to 100 gallons of water, sprinkled on the trees once in May and again in June, has saved the fruit of the following season. Upon picking the fruit in August, the most delicate chemical test could find none of the poison on the apples.

Bisulphide of carbon, so excellent in fighting museum pests, and so much esteemed as a specific against the grape phyloxera in Europe has been tried by the author of the paper to destroy the Cabbage Maggots, *Anthomyia brassicæ*, and other insects which infest subterranean stems, etc., with excellent success.

A hole is made in the ground, the liquid poured in and the hole quickly filled with earth, which is pressed down with the foot. The hole is made with a small rod, close to the plant, and about a table spoon-full of the liquid poured into each hole.

SOME OF THE INFUSORIA FOUND IN FRESH POND, CAMBRIDGE.*

By S. P. Sharples.

This paper was a general review of observations on the water of Fresh Pond, as delivered in the City of Cambridge. These observations extend, at intervals varying from a few days to a month, over three years. Particular attention was called to the fact that there seems to be a marked periodicity in the forms of life in the water, some appearing at certain seasons and then disappearing again. This periodicity is not always annual, but may embrace a period of time covering several years. Attention was called to the necessity of continued observation of a water in order to understand its character. The periodical bad taste of certain waters was referred to, and the suggestion made, that in order to discover the cause of this, extended observations were necessary, as frequently the cause had entirely disappeared when the investigation commenced. A new species of Annurea observed in the pond, was described as follows: Annurea Longirostris, lorica, with four anterior and one posterior spine; three of the anterior spines short about half the length of the lorica; the fourth twice the length of the lorica; the posterior of the same length as the long anterior, otherwise resembling Annurea Stipata, though more slender.

BOOKS RECEIVED.

The Metric System and Interchange of Weights and Measures. By B. Beach, Jr., and E. N. Gibbons, Principals of the Fifth Avenue School of New York City. G. P. Putnam's Sons. New York. 1880.

The metric system has been adopted by all civilized nations except Russia, England and the United States, and its universal adoption is earnestly desired by the educated and scientific classes of this country. It has been adopted or recommended by the National Academy of Sciences, the American Metrological Society, the American Association for the Advancement of Science, by the American Society of Civil Engineers, the United States Coast Survey, the United States Marine Hospital Service, the American Medical Association, the Congress of Ophthalmogists, by leading medical societies and journals, by numerous boards of education, college faculties and local scientific societies. It is also our wish that contributors to this journal should, on all occasions, use the metric system, and we have pleasure in directing attention to the present cheap and handy little manual, written for those desirous of making use of the metric system. As a class-book in schools it will prove very valuable, as rules and examples are given for working out problems for all weights and measures, answers to which are given, on an extra sheet, with each book. We advise all who are undecided as to the policy of using the metric system to read Dr. R. H. Ward's able plea for its introduction in No. 5 of "Science," published the 31st of July last. He concludes with the practical observation that the proper way to introduce it is to use it yourself. We believe the publication of Messrs. Beach and Gibbons's manual will be a great aid in securing its universal use.

Fourteen Weeks in Physics. By J. Dorman Steele, Ph. D., F. G. S., author of "Fourteen Weeks in Natural Science." A. S. Barnes & Company. New York, Chicago and New Orleans.

This is an excellent elementary work on Physics adapted to the class-room, written in a happy style to interest the student and well supplied with illustrations. The author employs simple language, which is readily intelligible, and the experiments are within the reach of every pupil. In order to familiarize the pupil with the metric system it is constantly employed in the problems. As an introduction to the study of physics we consider this work one of the best for the use of young students.

The Young Chemist.—A book of Laboratory work for beginners. By John H. Appleton, A. M., Professor of Chemistry in Brown University. Second Edition. Price 90 cents by mail. Cowperthwaite & Co., Philadelphia.

The purpose of this book is to aid in the instruction of pupils in chemistry by the experimental or object method, and the author has attempted to remove at least one objection to this method by economizing the time of the instructor, and we are glad to admit that the cost of supplies has been greatly reduced of late. Professor John H. Appleton claims with truth the following advantages for his work :—*First*, the apparatus described and the supplies called for, are of the simplest character. *Second*, the experiments are described in clear and simple language, and in direct form ; the pupil can hardly fail to perform them successfully, even without the special aid of the teacher. *Third*, dangerous experiments have been excluded. *Fourth*, the chemical elements are discussed in a scientific order, which, while it aids the memory, does so upon correct principles. *Fifth*, formulas and reactions are introduced freely, so that the student learns the new nomenclature and new notation without suspecting it. This work is not an experiment, the first edition having been used with success by Professors of great experience.

A Short Course in Qualitative Chemical Analysis. By Professor John H. Appleton, A. M., Brown University. Fourth Edition. Price, 90 cents by mail. Cowperthwaite & Co. Philadelphia.

The author has used this work in his own class for many years, and to those who are unable to purchase the more bulky and costly manuals this little work will be found of great use. It has the advantage of brevity and compactness. It prescribes the most direct and simple course of analysis, and presents a large number of formulas and reactions. This work and the "Young Chemist," by the same author, will be found very suitable for those who require a reliable introduction to the study of chemistry.

* Read before the A. A. A. S., Boston, 1880.

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SATURDAY, OCTOBER 30, 1880.

SMYTH'S *Celestial Cycle* in its day was probably the most valuable companion which had at that time been prepared for the use of amateur astronomers. The second volume is known as the *Bedford Catalogue*, and contains an excellent list of the most interesting double stars, nebulæ and clusters, with descriptions, and much other valuable information. When published, this *Catalogue* was received with such favor that the Royal Astronomical Society bestowed upon its author a gold medal. In presenting the medal, the president of the Society, Sir G. B. Airy, called attention to the fact that the original observations upon which the *Catalogue* was based had not been placed at the command of the Society, and hoped that such would be done at no distant period. A careful examination of the *Cycle* now shows that it is full of inaccuracies. Mr. Burnham called attention to these some years ago, but the subject attracted no general attention until a paper by Mr. Herbert Sadler, a member of the Council of the Royal Astronomical Society, appeared in the *Monthly Notices* for January, 1879, in which Mr. Sadler used language which might easily be construed into a charge of dishonesty on the part of Captain Smyth. The words actually used were as follows:

"I have thought it better, therefore, as the charge I have brought against the *Bedford Catalogue* is of a very serious character, to place an asterisk against the symbol of the observer whose erroneous measure Smyth appears to have followed, so that anyone may be able to detect the source of Smyth's error at a glance in cases where he has presumably copied the measures of others."

This criticism raised a perfect storm in the Society. As Mr. Burnham had originally called attention to the inaccuracies of the *Cycle*, he immediately set to work re-observing the stars of the *Bedford Catalogue*, and has published his results in the June number of the *Monthly Notices*. This paper contains about 350 measures of 148 stars, which he has compared with the measures of Captain Smyth. Mr. Burnham divides these stars into two classes: Those which had and those which had not been carefully measured by any other observer up to the time of the publication of the "Cycle of Celestial Objects;" and concludes that the measures of the former class are in the main correct, while those of the latter class are either roughly approximate or grossly inaccurate; in fact, are not micrometrical measures at all in the usual sense of the term. In explanation of the remarkable character of the "Cycle" measures Mr. Burnham says:

"We know that the observations in the *Bedford Catalogue*, which, so far as the double stars are concerned, could have been easily made in one year, are scattered over a series of years. It may, I think, be fairly assumed that they were made in leisure moments, without that care which a more zealous and experienced observer would bestow; with no definite idea of their publication and use; and as an amusement rather than as a serious astronomical work. If we assume that at the beginning the observer made it a practice, in measuring double stars, of setting the micrometer wires in accordance with the previous measure of other observers, for the purpose of identification, or for some other reason, and with the intention of making such changes in the wires as the appearance of the object seemed to warrant, we have at once a complete explanation of the very close agreement with other measures." This explanation seems reasonable and implies no dishonesty on the part of Captain Smyth.

Immediately following Mr. Burnham's paper is one by Mr. Knobel, who calls attention to the fact that the majority of these so-called measures have a weight 1 assigned, and that Captain Smyth repeatedly asserts that such are mere guesses. Mr. Knobel accounts for many of the discrepancies in position angles by errors in computation.

Both of these interesting papers give a pretty clear insight into the *Bedford Catalogue;* and, although it is undoubtedly true that the principle upon which it secured the medal of the Royal Astronomical Society was a wrong one, as the Astronomer Royal showed at the time of presentation, still in its preparation Captain Smyth performed a useful service, and all lovers of astronomy will be glad to know that Mr. Chambers is preparing a new edition which will embody the progress of astronomy up to 1880.

THE United States Fish Commission has completed its summer's work at the Newport Station, and its parties have returned to Washington. The Fish Hawk, the steamer of the Commission, is now at Wilmington receiving the remainder of its fish-hatching apparatus for use during the winter.

The work has been successful beyond any expectations. Among the acquisitions of three days' work on the edge of the Gulf Stream were fifteen new species of fishes, one hundred and seventy-five species of mollusks, of which one hundred and fifteen were new to southern New England, sixty-five new to America, and thirty or more undescribed. Corresponding acquisitions have been made in other branches of marine zoology.

THE next United States Congress will act on a bill, reported in the last Congress, in support of an International Commission to agree on standard tests for color blindness and standard requirements for visual power in navies and merchant marines. Dr. R. Joy Jeffries, A. M., of 15 Chestnut street, Boston, Mass., will be glad to have public or private statistics or information in relation to this subject.

The attention of those interested in Hygiene reform is directed to the Hygiene Convention and Exhibition of inventions, mechanical contrivances and processes relating to sanitary and household economies to be held at Wellesley, Mass., commencing November 3rd, and ending November the 9th.

A most attractive programme has been arranged, which reflects the highest credit on those who have organized the arrangements. Tickets and programmes can be obtained at the St. Nicholas Hotel, New York; Hotel Wellesley, Wellesley, Mass., or of the Executive Committee, 158 Tremont Street, Boston, Mass.

THE COMETS.

There are now four comets visible with a good telescope, but none of them can be seen with the naked eye. They are all growing fainter, and after a few weeks they will become invisible, even in the most powerful telescopes.

The first is the one discovered by Mr. Schærbele at Ann Arbor, Michigan. This is in the morning sky, and its position for November 4 will be:

A. R. = 5 h. 18.9 m. Decl. South = 7° 33′.

The second is the one discovered by Mr. Hartwig, at Strasburg, Germany; and also, independently, on the next night by Professor Harrington, of Ann Arbor, Michigan. The position of this comet on November 2 will be:

A. R. = 18 h. 21.7 m. Decl. North = 9° 59′.

It is thought by Professor Winnecke that this comet is a return of the one of 1506.

The third is the comet discovered by Mr. Lewis Swift, at Rochester, New York, on October 10. This is a faint object, and its position on November 2 will be nearly as follows:

A. R. = 22 h. 0.0 m. Decl. North 34° 15′.

No orbit of this comet has been computed.

The fourth comet is the one with a period of seven and a third years, and known as Faye's, having been discovered by M. Faye, of Paris, in 1843. The orbit of this comet has been investigated in an admirable manner by Professor Axel Moeller, of Lund, Sweden, and its motion is nearly as well known as that of a planet. The ephemeris furnished by Professor Moeller for the present return is almost exactly correct. The position of this comet for November 2 will be:

A. R. = 22 h. 53.5 m Decl. South = 0° 25′.

Since this comet is always at a great distance from the sun, it is a faint object, even on the most favorable occasions. It will soon be invisible except in the larger telescopes.

Washington, Oct. 28, 1880 A. HALL.

ETHNOLOGY.*

FRAGMENTARY NOTES ON THE ESKIMO OF CUMBERLAND SOUND.

By LUDWIG KUMLIEN.

III.

Since whalers began to cruise in the Cumberland waters, they have found that it is decidedly to their advantage to hire boats' crews of natives to assist in the capture of whales. They make good whalemen. When such crews are secured, they wisely count in all of their family in the bargain, so that to secure the services of a crew of seven men one must feed thirty or more. While working for whalers, the Eskimo depend almost wholly on the ship for their food supply; as a consequence, they are fast becoming poor hunters and prefer to lounge around a vessel and pick up such scraps as offer themselves rather than to strike out for themselves and live independently and in comparative plenty.

As to meals, or regular meal-times, they eat when hungry, if they have anything. They always eat in the morning before going out to hunt; but the principal meal is in the evening, on their return. When supplied with rations by the ships, they often have their regular meals aboard; but this does in no wise hinder them from taking their usual evening allowance of raw meat when they return to their huts.

That the Eskimo possess considerable powers of abstinence cannot be disputed; but it is not so remarkable after all, for they certainly have had ample experience in this direction. That they are able to bear temporary or sustained exertion better than the whites is doubtful. They are acclimated and have clothing suited to the climate, and readily adapt themselves to the rude shelter of a snow-bank, if necessary; but give a healthy white man as good clothes, and he will stand as much fatigue, and perhaps more.

While hunting with the Eskimo, we often had our noses and faces frozen, when the cold did not seem to affect the Eskimo in the least; but when it came to a tramp through the snow all day long, few of them would stand it any better than we could.

Some have judged their powers of endurance from the manner in which they will follow their game; but it seems to us that it is rather their wonderful patience, for we have known them to follow animal tracks for a whole day, when we confess we could not discover the faintest trace of a track, except at long distances apart. They will discover many traces of animals on the snow that a white man would pass by and not notice. When traveling either on the ice or water, they make the journey by short, easy stages, stopping as soon as they feel the least tired, and recruiting; if

* Bulletin (15) of the United States National Museum. Contributed to the Natural History of Arctic America, made in connection with the Howgate Polar Expedition, 1877-78.

they were required to walk a given distance, as on a regular march, they would give out.

The Cumberland Eskimo are known to make better and more beautiful clothing than the tribes of Northern Hudson's Bay and Straits. During the summer, and, in fact at all seasons, except when the weather is very severe, the outer garment of the men is made from the skins of adult—or, more properly speaking, yearlings, as they are the best—*Pagomys fœtidus*. In very cold weather, they betake themselves to deer-skin clothing; but as these clothes are less strong than the sealskin, they make the change as soon as the weather permits. The women wear the deer-skin clothes much later in the season than the men; their dress is also made of the same kind of seal, unless they are fortunate enough to procure *Collocephalus vitulinus*, which skins are so highly prized that they use them even though there is only sufficient for a part of the fronts of their jackets.

Both the men and women wear a garment the exact duplicate in shape under the outer one; this garment is made either from the young seal in the white coat or of reindeer.

The coat of the men does not open in front, but is drawn on over the head like a shirt, and has a hood that fits the head snugly, while the woman's hood is large and loose, and the jacket is quite loose-fitting, so as to receive the child, which is always carried in the hood. The woman's jacket further differs from the men's in being shorter in front, and ending in a rounded point, while behind it reaches quite to the ground in the form of a lance-shaped train. This appendage is caught up in the same manner as the fashionable train of the present day among civilized nations, when the condition of the ground is unfavorable for its trailing. After all, is not this fashion borrowed from the Eskimo? There is often an approach towards this prolongation in the men's jackets, especially when made of deer skin, but never so long as on the woman's. Neither do little girls have a long train to the jacket; but as soon as they arrive at the age when they are no longer looked upon as children, they learn to imitate their mothers. There are never any pockets in the jackets of either sex, the hood serving for this purpose.

The pants of the men are made from the same material as the coat, with the exception that the young seal in the white coat is often used for the outer as well as the inner garment. The pants reach only to the upper part of the pelvis, and are kept up by means of a string around the body. They reach a little below the knee, where they are met by the boots. When made of deer skin, they are usually ornamented by fringes of cut skin around the lower edges.

The women's pants differ from the men's in being composed of two separate pieces, the lower reaching from a little below the knee to the middle of the thigh, and are kept in place by a string which runs to the upper edge of the other portion. The lower portion of these pantaloons is removed while they are at work in their igloos, and the bare thigh used, as a board would be, to lay the seal skin on while cleaning the blubber from it. The women have the habit of thrusting their hands between the upper and lower pantaloons the same as we do in a pocket; in fact, they use this space as a sort of pocket.

Little girls wear their breeches like the men till they get to be ten or twelve years of age. Very small children are dressed in a fawn-skin jacket without attached hood; but their heads are, nevertheless, well bundled up in a double fawn-skin hood that fits the scalp closely. This hood is never removed, except perchance by accident, till the child outgrows it. The lower extremities are usually not clad at all.

The children are carried *on* the mother's back inside her jacket. The cut of the jacket is such that the child goes down as far as the mother's waist, when the closeness of the jacket prevents it going any farther. The hood allows the child freedom for its arms and head, but the legs are cramped underneath its body, and this is probably one cause of bow-leggedness and possibly the shortness of the lower extremities. I have seen the Eskimo mother, with a child fast asleep in her hood, building a toopik. This work often necessitated her stooping over so much as to seemingly endanger the dumping of the infant over her head on the ground; still, it did not seem to inconvenience the child in the least as it slept soundly through the whole proceeding.

The *kămik*, or, as generally pronouncea, *kumming*, or boots, are principally made from the skins of adult *Pagomys fœtidus*, with the hair off, the soles being made from the skin of *Phoca barbata*. For Winter wear a very beautiful and serviceable boot is made from the skin of reindeer legs sewed together lengthwise; they are used only in dry snow, being quite useless when the snow is wet. Another style of boot is to have the leg of netsick skin, but with the hair on. These boots reach nearly to the knee, and are kept in place by means of a string around the top, and also secured by a seal-skin cord passing over the instep and around the heel. They are generally sewed with sinews from reindeer; but for boots the sinews from the dorsal vertebræ of *Beluga catodon* are preferred when they can be procured.

The stocking worn next to the foot is of heavy reindeer skin, the hair side next the foot; they reach above the knee. Over the stocking is worn a sort of slipper made from the eider-duck. The bird is skinned by making an incision on the back near one wing; through this opening the body is removed. The skin is cleaned of the fat by the Eskimo's teeth, and the skin farther prepared by chewing it. The tail-feathers are removed, and this end becomes the toe of the slipper, the feather side being worn inside. Its upper edges are bound with some kind of skin to give it additional strength, and if the entire slipper is covered with cloth will last a long time. They are very warm and comfortable. *Larus glaucus* is often used for this purpose. For children they use *Uria grylle* and *Rissa tridactylus* skins. Over all this is worn another slipper made from the netsick skin, with the hair on, and the hair side worn outward and the hair pointing from the toe backwards. This very much facilitates the drawing on of the boot.

For summer wear the young of the netsick in the woolly coat is substituted for reindeer for the stockings. Dog skin is also sometimes used for stockings, but not so commonly among the Cumberland Eskimo as among those of Hudson's Straits, who use dog skins for pants as well as stockings.

All the clothing is sewed with sinews, reindeer or white whale. The reindeer sinews are dried in bulk as they come from the animal, and are split off as needed. The fibres are separated as fine as necessary, and then drawn quickly between the teeth to secure a more uniform size. The women all sew towards themselves, using the thimble on the first finger; they seldom use but one kind of seam: the edges of the skin are carefully matched together, and joined by sewing over and over the overcast seam. Their thimbles (called *tikik*, also signifies first finger) are made from the skin of *Phoca barbata;* in shape they are merely an oblong piece sufficiently large to cover the point of the finger; a rim is cut around the outside edge for about one-half its length; this forms a sort of loop under which the finger is passed, and in this manner it is kept in place. We found this style of thimble much more convenient than the metal one of the usual form.

Very few of the Cumberland Eskimo at the present day use anything but steel needles, or bone ones made after the same pattern. We have seen an instrument said to have been used as a needle that is considerably different from anything we ever saw before. An Eskimo brought it to us and wanted a hatchet in exchange. We thought it certain he would return and offer to trade at our terms; but he did not, and we never saw him again. This tool was almost exactly like an awl in shape, but had an *eye* near the point. They must have had to thread this instrument for every stitch. The needle part was apparently of deer horn and the handle of walrus ivory.

The favorite and principal tool of the women is a knife shaped like an ordinary mincing-knife. Nearly all the Cumberland Eskimo have now procured iron enough from some source or other so that they can have an iron knife of this pattern. Before they could procure enough iron they made the knife of ivory, and merely sank flakes or pieces of iron into the edge, in the same manner as the natives of North Greenland do at the present time. This same practice of sinking iron flakes into the edge was also used on their large skinning knives, which were made from a walrus tusk, and much after the pattern of an ordinary steel butcher-knife. Some of these ivory knives have no iron in them; but at the present time they are used principally, if not entirely, for cutting snow and removing ice from their kyacks.

The women seldom use any other kind of knife than such as just described. With them they remove the blubber from the skins, split skins, cut up meat, and, when sewing, this instrument is used instead of scissors. They begin a garment by sewing together two pieces of skin and shaping them as they go along by means of the knife, cutting for an inch or two and then sewing. They always *push* the knife *from* them when working it.

Tattooing does not seem to be as prevalent now as formerly, for it is mostly on the aged women that one finds it at present. The markings resemble India ink in appearance, and are done with gunpowder at present. Still, some use the old method, by taking the juice of *Fucus vesiculosus*, L. (or a closely allied species) and some small algæ that apparently contain a good deal of iodine, and mixing with lampblack

Instances came under our observation of people of apparently great age—say seventy years and over, to judge from appearances; they had gray hair (a rare thing among the Eskimo), and were nearly blind; the women had the teeth worn close to the gums by chewing skins.

It is impossible to arrive at any definite conclusion regarding their age, as they keep no record of time, and can not refer to any past event by any means of notation. We could not learn of the rudest attempt at picture-writing or hieroglyphics; and, as they possess no records whatever, their traditions are handed down from generation to generation without being fixed by any means which allow even an approximate estimate of their growth and prosperity.

Most of them are unable to count beyond their ten fingers, and many are unable to go over six; some, again, are said to have names for numbers to twenty, but they are few. The numerals are differently pronounced, and we found difficulty in getting one sufficiently conversant with them to give us the numerals to ten.

One =*Atáusa*, or *atausat.*
Two = *Mácho.*
Three = *Pingasuit*, or *pingasat.*
Four = *Séseminé*, or *sesemat.*
Five = *Tódlimené*, or *tódlimát.*
Six=*Aúkbinigan.*
Seven=*Pingashuing* (?).
Eight=*Aukbinigan-machoni* (6 and 2).
Nine=*Schischimani* (? ?).
Ten=*Kowolin.*

Above ten they are said to count their toes and take ten and one, ten and two, &c.; but we were unable to find one who knew their names. They will tell you they have caught seals or birds up to six, but if more they generally put it *amashuadly* (a good many), which may be any number from seven upwards.

In the treatment of the sick they are very superstitious, and in fact they resort almost entirely to their *ancoot*, *angekoks*, or medicine men.

The following is a Greenlander's legend that proposes to give a reason why people die: "The cause of people's dying is laid to a woman, said to have discoursed thus: 'Let the people die gradually, otherwise they will not have room in the world.'"

Others relate it in this manner: "Two of the first people quarreled. One said: 'Let it be day and let it be night, and let the people die.' The other said: 'Let it only be night and not day, and let the people live. After a long wrangle it came to pass as the first had said."

It is interesting that this same curious legend exists among the Eskimo of Cumberland Sound; they say though that "those who quarreled finally arranged matters and had both *entire* day and *entire* night at the different seasons, so that both parties might be suited."

The lungs of *Lepus glacialis* are considered as a sure cure for boils and all manner of sores; they draw, they say, and their manner of applying them is the same as we would a poultice. They must be applied as soon after the animal's death as possible, and while they are yet warm.

In cases of scurvy they never use *Cochliaria*, but the stomach of a freshly killed reindeer, with the vegetable contents, instead. If the scurvy patient be very bad, the limbs are bound with pieces of the deer's stomach, whale or seal's blubber, or any kind of fresh meat. If a whale can be caught at such a time, the patient is sometimes bodily shoved into the carcass, or the lower extremities only are sunken into the flesh.

The most prevalent disease among them seems to be lung disease; it is alarmingly common, and consumption probably kills more than all other diseases combined.

The whalemen have introduced venereal diseases among them, which have spread at a terrible rate, and devastate the natives almost like a pest.

I could not learn that they have any knowledge of the medical properties of any plant or shrub. Some of the coarser kinds of *algæ* are procured at low tide from the cracks in the ice, and eaten raw, but only because they are fit to eat, they say; the roots of *Pedicularis* are also sometimes eaten.

When the women are about to be confined they are placed in a small snow-hut, if it be winter, and in a little skin tent, if summer, by themselves. Their only attendant is a little girl, who is appointed by the head *ancoot* of the encampment. A little raw meat—deer, if they have it—is put into the hut with her, and she is left to give birth to the child as best she can. The reason she is removed from her tent is, that should mother or child die in the tent nothing pertaining to the equipment of the establishment could ever be used again, not even the tent-covering or the husband's hunting-gear. In some instances they are obliged to modify this custom somewhat. We have known them to cut the tent-cover about two feet from ground all around and use the upper portion. A man's wife accidentally shot herself in her igloo, but the gun was too great a sacrifice; he used it, but the rest of his household effects were left to waste away where they lay. We knew of another instance where the tent-poles were brought into use again in the course of a year after a death had occurred beneath them.

As soon as the mother with her new-born babe is able to get up and go out, usually but a few hours, they are taken in charge by an aged female *ancooi*, who seems to have some particular mission to perform in such cases. She conducts them to some level spot on the ice, if near the sea, and begins a sort of march in circles on the ice, the mother following with her child on her back; this manœuvre is kept up some time, the old woman going through a number of performances the nature of which we could not learn, and continually muttering something equally unintelligible to us.

The next act is to wade through snow-drifts, the aged *ancoot* leading the way. We have been informed that it is customary for the mother to wade thus barelegged, but (whether from modesty or the temperature of —50° F. we cannot say) on some occasions this part of the performance is dispensed with.

When a sick person gets so far gone that they deem recovery improbable, he is removed from the hut, and either dragged out upon the rocks to die, or a little snow shelter may be constructed for him, and some scraps of raw meat thrown in to him. Usually such proceedings are apt to end fatally to the patient, even though his ailment might not have been so dangerous had proper care been taken. We know of one instance where a man was thus put out to die seven different times; but he recovered and crawled back to his igloo, and looks now as if he was good for a number of years yet. Stories are common of how aged and infirm people are put out of the way by the younger ones, to rid themselves of a useless burden; but of this we know nothing from personal observations, or from reliable sources.

Occasional instances of suicide happen, generally when the person is afflicted with some incurable disease. Hanging seems to be the favorite mode of killing themselves.

The *ancoot's* manner of operating is various, and almost every one has some method peculiar to himself. We could get but a glimpse of some of them, as they are averse to having a white man witness their performances, and we had the greatest difficulty in getting any one to explain to us their meaning. The following legend is supposed to give the directions for becoming an *ancoot;* it is interesting that this legend does not differ essentially from the Greenlander's. (*Vide* Grœnlands nye Perlustration, Eller Naturel-Historie, Hans Egede, 1741.)

We would here add that those who become *ancoots* are only such as are naturally possessed of a more penetrating mind than their fellows, generally the biggest rascals in the encampment, who seldom pay any attention to what is right or just, but ply their vocation so as to win for themselves renown among their fellows, and possess themselves of any coveted article as remuneration for their services.

Any one wishing to become an *ancoot* must go away a long distance from where there is any other person. Then he must find a large stone, and seat himself by it, and call on *Torngarsuk.** This spirit will then make himself present to him. The would-be *ancoot* will at first be very much frightened at the arrival and appearance of this spirit, so much so that he is seized with severe pains, and falls down and dies, and remains dead for three days. Then he comes to life again, and returns home a very wise man.

An *ancoot's* duty is, first, to mutter over the sick, that they may become well again; secondly, he will talk with *Torngarsuk*, and get information from him as to how he must manage so that they will have success in their undertakings; thirdly, of him he learns if any one is about to die, and what the cause is, or if some unusual death or misfortune is about to occur to the people.

Their devotion and belief in the *ancoots* are unlimited; they can never be induced to trespass on the commands or disbelieve the prophecies of these important personages. When one has been a very suc-

* *Torngarsuk* of the natives of South Greenland, and *Tornarsuk* of North Greenland, is the highest oracle, the master spirit of these people. There are many spirits of less power, called *Tornæt;* these can be seen only by the *angekoks*, after their meeting with *Torngarsuk*. It appears that this word signifies the greatest spirit of Good, as well as of Evil. They now call the Devil *Torngarsuk*, and in their ancient beli f their God, so to speak, the same.

cessful *ancoot* for a long time he may become a great *ancoot;* this necessitates a period of fasting, and then, as the story goes, an animal they call *amarook* (the same word is used for wolf, and for an animal which is probably mythical, unless it can be a *Gulo*) comes into his hut and bites the man, who immediately falls to pieces; his bones are then conveyed to the sea, where he lives for some time as a walrus; he finally returns among his people, a man in appearance, but a God in power.

If the prophecy of an *ancoot* does not come to pass as he had said it would, any phenomenon of nature, as a halo, corona, aurora, etc., is sufficient to have broken the spell, and the *ancoot* loses nothing of his reputation by the failure, for it is then believed that the measure, whatever it might have been, was not pleasing to *Torngarsuk*.

The people come to these soothsayers after all manner of information. We knew of one case where a young woman asked an *ancoot* if her yet unborn child would be a boy or girl. He retired outside the hut for a few moments, and when he returned he said it would "be a boy"; but he adds, "If it is not a boy, it will be a girl"! For this valuable information he charged three seal skins and a knife. As a general thing, the *ancoots* are paid according to their reputation; still, it is very seldom they refuse to give them what they ask for in return for their valuable services.

They seem to have an idea of a future state, but what we denominate as the region down below they consider as the best place. In Egede's "Grœnlands nye Perlustration, year 1741," is given a legend which is almost exactly the same as one that is found among the Cumberland Eskimo at the present day. But Egede says, in the Danish translation, "Himmel," heaven, as though this was the equivalent for the Greenlander's word; the Eskimo of Cumberland say "topani," which means simply "up." They do not distinguish any difference in the soul's condition after death, or rather of the two places where they expect to live hereafter; one differs from the other only in this wise, that if death is caused by certain means they go to the one, and if they die a natural death they go to the other.

The following is their idea of the future: "In the spirit-land *all* will have it as good or better than they had it on earth." Yet they designate two places where the soul goes after death, viz: "Some go up; others far down into the earth." But the lower place is considered preferable. This is described as a beautiful land, with everlasting sunshine, where the seal and reindeer abound in fabulous quantities, and food is consequently abundant. To this latter place go only such as are killed by other Eskimo, women who die in child-birth, such as drown in *salt* water, and *whalers;* they think, this being the better place, it is a sort of recompense for the suffering they underwent on earth; all the rest go up.

In this connection, we will mention that the Cumberland Eskimo think the *aurora borealis* is the spirits of dead Eskimo dancing and having a good time generally. It has even considerable influence over them, and they are well pleased to see a bright *aurora*. The Greenlanders, on the other hand, say it is the spirits of dead Eskimo *fighting*.

MULTIPLE SPECTRA[1]

III.

I have endeavored to show in the previous articles that there are many facts which justify the conclusion that the same elementary substance in a state of purity can under different conditions give us spectra different in kind. To those spectra to which special reference is now made the names of *lined* and *fluted* have been given to mark their chief point of difference, which is that in lined spectra we deal with lines distributed irregularly over the spectrum; while in fluted spectra we deal with rythmical systems.

This was the first point, and I showed that the idea was suggested that the lined and fluted spectra, though produced by the same substance, were produced by that substance in a different molecular condition.

I have pointed out that both in lined and fluted spectra taken separately there was evidence of still further complication, that is, that a complete lined spectrum of a substance and a complete fluted spectrum of a substance, was the result of the vibration not of one kind of molecule only, but probably of several.

So that in this view we have to imagine a series, in some cases a long series, of molecular simplifications brought about by the action of heat, and ascribe the spectral changes to these simplifications.

To understand my contention, and one objection which has been taken to it, in the clearest way, let us suppose that there is a substance which gives us, under different conditions, three spectra, which we will term *a*, *b*, and *c*. My view is that these spectra are produced by three distinct molecular groupings brought about by successive dissociations. On the other hand, it is objected that they are produced by *one and the same molecule* struck, as a bell might be struck, *in different ways* by the heat waves or the electric current passing among the molecules.

In my memoir entitled "Discussion of the Working Hypothesis that the so-called Elements are Compound Bodies," I remarked as follows:—

"I was careful at the very commencement of this paper to point out the fact that the conclusions I have advanced are based upon the analogies furnished by those bodies which, by common consent and beyond cavil and discussion, are compound bodies. Indeed, had I not been careful to urge this point, the remark might have been made that the various changes in the spectra to which I shall draw attention are not the results of successive dissociations, but are effects due to putting the same mass into different kinds of vibration or of producing the vibration in different ways. Thus the many high notes, both true and false, which can be produced out of a bell with or without its fundamental one, might have been put forward as analogous with those spectral lines which are produced at different degrees of temperature with or without the line, due to each substance when vibrating visibly with the lowest temperature. To this argument, however, if it were brought forward, the reply would be that it proves too much. If it demonstrates that the *h* hydrogen line in the sun is produced by the same molecular groupings of hydrogen as that which gives us two green lines only when the weakest possible spark is taken in hydrogen inclosed in a large glass globe, it also proves that calcium is identical with its salts. For we can get the spectrum of any of the salts alone without its common base, calcium, as we can get the green lines of hydrogen without the red one.

"I submit, therefore, that the argument founded on the over-notes of a sounding body, such as a bell, cannot be urged by any one who believes in the existence of any compound bodies at all, because there is no spectroscopic break between acknowledged compounds and the supposed elementary bodies. The spectroscopic differences between calcium itself at different temperatures is, as I shall show, as great as when we pass from known compounds of calcium to calcium itself. There is a perfect continuity of phenomena from one end of the scale of temperature to the other."

Not only is what may be termed the bell hypothesis op-

[1] Continued from p. 107.

posed to the law of continuity, as I endeavored to show in the last paragraphs quoted, but it appears never to have struck the objectors that it is also opposed to the theory of exchanges as it is generally enunciated, on which the whole of our supposed knowledge of extra-terrestrial matter depends. If vapors, when relatively cool, do not absorb the same wave-lengths which they give out when relatively hot, what becomes of some of the most noted exploits of our nineteenth-century science?

Take the case of sodium. Three distinct spectra have been mapped for it. There is first the yellow line seen in a Bunsen flame, then the green line seen alone in a vacuum tube when the vapor is illuminated by an electric glow, and again there is the fluted absorption spectrum, without any lines, seen when sodium is gently heated in hydrogen in a glass tube. If we have here the same molecule agitated in different ways, I ask which is the true spectrum of sodium? And what right have we to say that sodium exists in the sun because the yellow line is represented? Why do we not rather say that sodium does *not* exist in the sun because the fluted spectrum is *not* represented.

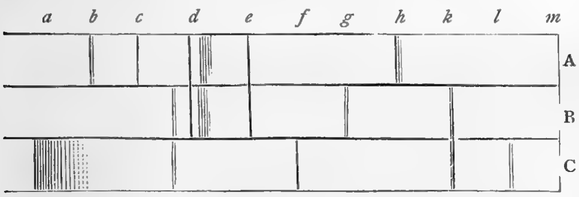

FIG. 1.—A. Highest temperature. C. Lowest temperature.

It is not necessary to enlarge upon this point because the difficulty in which the theory of exchanges is thus landed is obvious, while, if we acknowledge different molecular groupings in the vapors of the same chemical substance, and apply the theory of exchanges *to each grouping*, then the teachings of that theory become more numerous and important than before.

It is of course of the highest importance to see whether there is any *experimentum crucis*—any mode of inquiry—by which the theory can be settled one way or the other.

I submit that the results of experiments based on the following considerations ought to be accepted as throwing light on the question.

1. At different temperatures the brilliancy of the spectral lines of the same substances as ordinarily observed changes enormously. See if these changes can be produced *at the same temperature* by employing those experimental conditions which will be most likely to bring about different molecular conditions if such exist.

2. At a low temperature some substances give us few lines while at a high one they give us many. Vapors, therefore, already glowing with full lines at a low temperature, say in a flame, should give us all their lines when the vapor is suddenly subjected to a high one, say by the passage of a high tension spark. On the bell hypothesis the spectrum should change with the mode of striking. On the dissociation hypothesis this should only happen for the lines of those molecular groupings which are *from other considerations* held to be more simple. If the flame has brought the substance to its lowest state, the passage of the most powerful spark should not cause the flame spectrum to vary.

Now what are the "other considerations" above referred to? This necessitates a slight digression.

In the *Phil. Trans.* for 1873[1] I gave an historical account, showing how, when a light source such as a spark or an electric arc is made to throw its image on the slit of a spectroscope, the lines had been seen of different lengths, and I also showed by means of photographs how very definite these phenomena were. It was afterwards demonstrated that for equal temperatures chemical combination or mechanical mixture gradually reduced the spectrum by subtracting the shortest lines, and leaving only the long ones.

On the hypothesis that the elements were truly elementary, the explanation generally given and accepted was that the short lines were produced by a more complex vibration imparted to the "atom" in the region of greatest electrical excitement, and that these vibrations were obliterated, or prevented from arising, by cooling or admixture with dissimilar "atoms."

¹ Phil. Trans., 1873, p. 254.

Subsequent work, however, has shown[1] that of these short lines *some* are common to two or more spectra. These lines I have called basic. Among the short lines, then, we have some which are basic, and some which are not.

The different behavior of these basic lines seemed, therefore, to suggest *that not all of the short lines of spectra were, in reality, true products of high temperature.*

That some would be thus produced and would therefore be common to two or more spectra we could understand by appealing to Newton's rule: "Causas rerum naturalium non plures admitti debere quam quæ et varæ sint et earum phænomenis explicandis sufficiant," and imagining a higher dissociation. It became, however, necessary to see if the others would also be accounted for.

Now if not all but only *some* of the short lines are products of high temperature, we are bound to think that the *others* are remnants of the spectra of those molecular groupings first to disappear on the application of heat.

At any particular heat-level, then, some of the short lines may be due to the vibrations of molecular groupings produced with difficulty by the temperature employed, while others may represent the fading out of the vibrations of other molecular groupings, produced on the first application of the heat.

In the line of reasoning which I advanced a year ago,[2] both these results are anticipated, and are easily explained. Slightly varying Fig. 2 of that paper, we may imagine furnace A to represent the temperature of the jar spark, B that of the Bunsen burner, and C a temperature lower than that of the Bunsen burner (Fig. 1.)

Then in the light of the paper the lines *b* and *c* would be truly produced by the action of the highest temperature, *c* would be short and might be basic, while of the lines *h* and *m*, *m* would be short and could not be basic, because it is a remnant of the spectrum of a lower temperature.

So much then by way of explanation; it is clear that to make this reasoning valid we must show that the spark, or better still the arc, provides us with as ummation of the spectra of various molecular groupings into which *the solid metal which we use as poles* is successively broken up by the action of heat.

We are not limited to solid metals; we may use their salts. In this case it is shown in the paper before referred to[3] that in very many cases the spectrum is one much less rich in lines.

The experimental work has followed two distinct lines. I shall refer somewhat in detail to the results obtained along each. The first relates to the extraordinary and beautiful phenomena and changes observed in the spectra of vapors of the elementary bodies when volatilized at different temperatures in vacuum tubes. Many of the lines thus seen alone and of surpassing brilliancy, are those seen as short and faint in ordinary methods of observation, and the circumstances under which they are seen suggest, if we again apply Newton's rule, that many of them are produced by complex molecules.

In this case the appeal lies to the phenomena produced when organic bodies are distilled at varying temperatures; the simplest bodies in homologous series are those volatilized at the lowest temperatures; so that on subjecting a mixture of two or more liquids to distillation, at the beginning a large proportion of the more volatile body comes over, and so on.

The novelty of the method consists in the use of the luminous electric current as an explorer and not as an agent for the supply of the vapors under examination; that is to say, the vapors are first produced by an external source of heat, and are then rendered luminous by the passage of the current. The length and bore of the tube therefore control the phenomena to a certain extent.

¹ *Proc. R. S.*, vol. xxviii. p. 159. ² *Proc. R. S.*, vol. xxviii. p, 162.
³ *Phil. Trans.*, 1873, p. 258.

A form of apparatus which I have found to answer very well is shown in the accompanying woodcut (Fig. 2).

A is the tube or retort containing the metal experimented on in its lower extremity, and having a platinum wire sealed into it at a distance of about two inches from the lower end, the other end being drawn out and connected by a mercury joint to an ordinary Geissler tube, which is connected by another mercury joint to the Sprengel pump C.

Another form of tube which I have used is prepared by inserting two platinum poles into a piece of combustion tubing sealed at one end, and after inserting the metal to be experimented on, drawing out the glass between the platinums to a capillary tube.

I have also tried inserting the platinum pole at the end of the retort, so that the spark passes from the surface of the metal, but this arrangement did not answer at all.

Some other modifications have been tried, but the first form I have described is that which I have found to answer best, so far as the trials have yet gone.

D is the spectroscope.

E is the lens used for focussing the image of the Geissler tube on the slit.

off can be found by examining the spectrum of this capillary tube.

I now give an account of the phenomena observed when we were working with sodium, in order to show the kind of phenomena and the changes observed.

After a vacuum has been obtained the retort is heated gradually. The pump almost immediately stops clicking, and in a short time becomes nearly full of hydrogen. The spectrum of the capillary then shows the hydrogen lines intensely bright. After some time the gas comes off far less freely, and an approach to a vacuum is again obtained. Another phenomenon now begins to show itself: on passing the current a yellow glow is seen, which gradually fills the whole space between the pole in the retort and the metal; its spectrum consists of the lines of hydrogen and the yellow line of sodium, the red and green line being both absent until the experiment has gone on for some time.

As the distillation goes on, the yellow glow increases in brilliancy, and extends to a greater distance above the pole, and the red and green lines presently make their appearance as very faint lines.

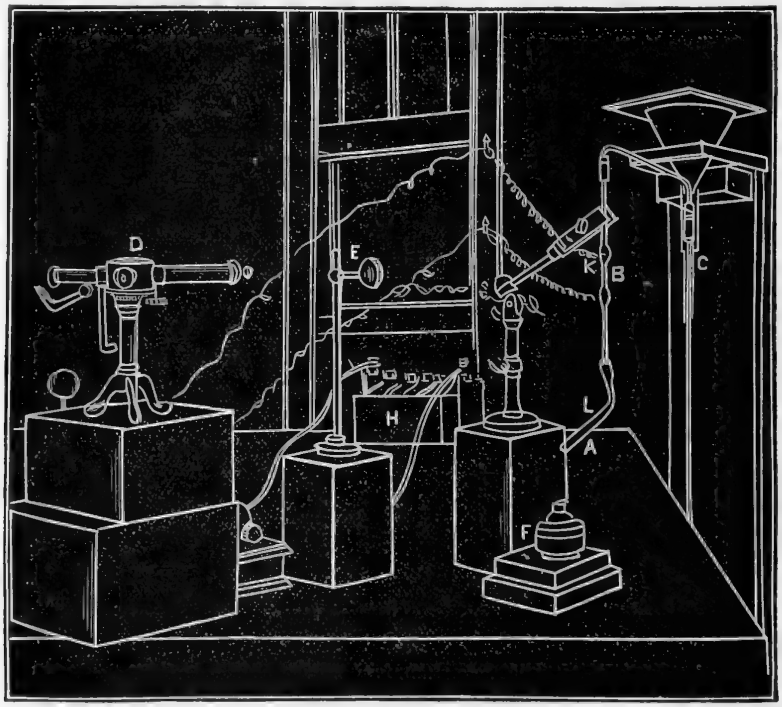

FIG. 2.—Distillation Apparatus.

F is the spirit lamp for heating the retort.

H is the battery.

K and L are the wires connected with the coil.

In the second cut (Fig. 3) the method of observing the spectrum of the vapours close to the surface of the metal is indicated; the same letters apply, D′ being, however, in this case a direct-vision spectroscope, which was sometimes employed for convenience.

For determining the exact positions of the lines in the spectrum of the vapor in any part of the retort, a larger spectroscope, with its illuminated scale, was used in the place of the direct-vision spectroscope.

The secondary wires of the coil were connected, one with the pole in the upper bulb at B. and the other with the platinum at A.

B is an ordinary Geissler tube with two bulbs separated by a capillary tube. The great advantage of this arrangement is that this capillary portion can be used for ascertaining what gases or vapors are carried over by the pump without any interference with the retort, both wires being connected with the Geissler tube. If, for example, we are working with sodium which contains an impurity of hydrocarbon, the moment at which it begins or ceases to come

The upper boundary of the yellow is quite sharp, the lines and fluted spectrum of hydrogen appearing above it.

After the yellow glow-giving vapor (which does not attack the glass) has been visible for some time, the pump is stopped and the metal *heated more strongly*. On passing the current a little while afterwards, a very brilliant leaf-green vapor is seen underlying the yellow one, and connected with it by a sap-green vapor. The spectra then visible in the tube at the same time are—

Leaf-green ...	Green and red lines of sodium and C of hydrogen; D absent.
Sap-green ...	Green, red, and yellow sodium lines of equal brilliancy and C of hydrogen.
Yellow	D alone and C.
Bluish-green	C and F and hydrogen structure.

To observe the green sodium line alone it is necessary to point the direct-vision spectroscope just above the surface of the metal where the green is strongest. It is also necessary to guard against internal reflections from the glass, as this may sometimes cause the D line to be seen by reflection from the surface.

This method of inquiry has been tried also with potassium, calcium, and some other metals, and with metallic salts.

With potassium and calcium we get the same inversion of phenomena, the yellow-green lines of potassium being seen without the red; while in the case of calcium the blue line alone was seen.

The fact that in these experiments we get, as before mentioned, vapors which at one and the same time exhibit different colors and different spectra at different levels in the tube, at once suggests the phenomena of fractional distillation.

It is also suggested, as a result of the application of this new method, that in the case of a considerable number of chemical substances not only the line spectrum is compound in its origin, as I suggested many years ago, but that a large number of the lines is due to molecular groupings of considerable complexity, which can be kept out of re-action by careful low temperature distillation.

So much then for one method. Now for the other.

In this I have attempted to gain new evidence in the required direction by adopting a method of work with a spark and a Bunsen flame, which Col. Donnelly suggested I should use with a spark and an electric arc. This consists in volatilizing those substances which give us flame spectra in a Bunsen flame and passing a strong spark through the flame, first during the process of volatilization, and then after the temperature of the flame has produced all the simplification it is capable of producing.

The results have been very striking; the puzzles which a comparison of flame spectra and the Fraunhofer lines has presented us find, I think, a solution; while the genesis of spectra is made much more clear.[1]

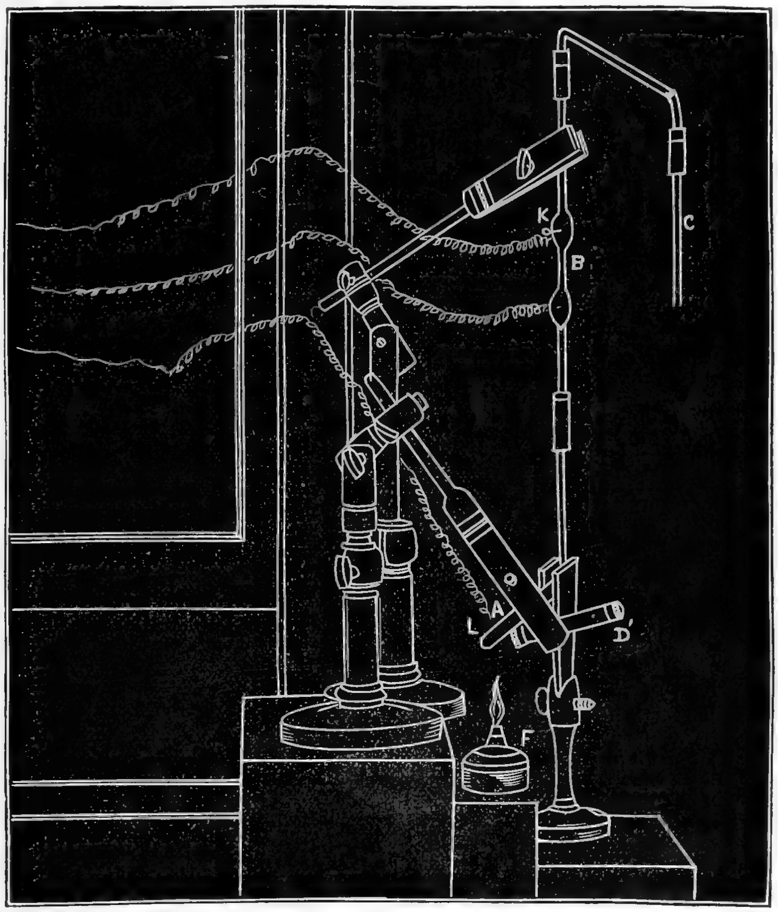

FIG. 3.—Position of Spectroscope for discovering Vapors close to the Metal.

To take an instance, the flame spectrum of sodium gives us, as its brighest, a yellow line, which is also of marked importance in the solar spectrum. The flame spectra of lithium and potassium give us, as their brightest, lines in the red which have not any representatives among the Fraunhofer lines, although other lines seen with higher temperatures are present.

Whence arises this marked difference of behavior? From the similarity of the flame spectrum to that of the sun in one case, and from the dissimilarity in the other, we may imagine that in the former case—that of sodium—we are dealing with a body easily brokeu up, while lithium and potassium are more resistant; in other words, in the case of sodium, and dealing only with lines recognized generally as sodium lines, the flame has done the work of dissociation as completely as the sun itself. Now it is easy to test this point by the method now under consideration, for if this be so then (1) the chief lines and flutings of sodium should be seen in the flame itself, and (2) the spark should pass through the vapor after complete volatilization has been effected without any visible effect.

Observation and experiment have largely confirmed these predictions. Using two prisms of 60° and a high-power eyepiece to enfeeble the continuous spectrum *of the densest vapor produced at a high temperature*, the green lines, the flutings recorded by Roscoe and Schuster, and another coarser system of flutings, so far as I know not yet described, are beautirully seen. I say largely, and not completely, because the double red line and the lines in the blue have not yet been seen in the flame, either with one, two, or four prisms of 60°, though the lines are seen *during volatilization* if a spark be passed through the flame. Subsequent inquiry may perhaps show that this is due to the sharp boundary of the heated region, and to the fact that lines in question represent the vibrations of molecular

[1] I allude more especially to the production of triplets, their change into quartets, and in all probability into flutings, and to the vanishing of flutings into lines, by increasing the rate of dissociation.

groupings more complex than those which give us the yellow and green lines. The visibility of the green lines, *which are short*, in the flame, taken in connection with the fact that they have been seen alone in a vacuum tube, is enough for my present purpose.

With regard to the second point, the passage from the heat-level of the flame to that of the spark after volatilization is complete, produces no visible effect, indicating that in all probability the effects heretofore ascribed to *quantity* have been due to the presence of the molecular groupings of greater complexity. *The more there is to dissociate, the more time is required to run through the series, and the better the first stages are seen.—Nature.* J. NORMAN LOCKYER.

THE RICHMOND DIATOMACEOUS EARTH.

The stratum of fossiliferous earth underlying the cities of Richmond and Petersburg, in Virginia, when first discovered by Prof. Wm. B. Rogers, in 1842, was supposed to be peculiar to those localities; the further investigations of geologists have shown it to be a material constituent in various parts of the great Tertiary formation which bounds the continents of North and South America, and, perhaps, those of the Eastern Hemisphere also. To Mr. Charles Stodder we are indebted for the interesting and suggestive fact, that a stratum of infusorial earth, apparently the same as the Richmond deposit, has been struck at a depth of five hundred feet at Fortress Monroe, in boring an Artesian well.

The deposit at Richmond has long been famous with microgeologists for the great variety of beautiful forms it contains; the illustrious Ehrenberg having assigned to it one hundred and twelve species—nearly double the number to be found at any other place on the Atlantic coast; and the subsequent researches of microscopists have shown it to be perhaps the richest deposit of the kind in the world, every new preparation of the earth revealing some forms not before noticed, many of the most interesting remaining unnamed or described to the present day. The stratum varies in thickness from twenty to forty feet, and Major Bolton, engineer of the Church Hill tunnel, at Richmond (which runs through the deposit for three-fourths of a mile), informed me that at certain points of that excavation it reached a maximum thickness of eighty feet. In addition to an inexhaustible supply of the Diatomaceous earth, that work brought to light thousands of fossil remains of the gigantic marine monsters that, long ages ago, swam in the deep ocean over the spot where the city now stands.

An observation of the sections made by the various water courses which cut through the plateau on which Richmond is built, shows the deposit to be nearly level—its upper surface about fifteen or twenty feet below the top of the ground, and perhaps one hundred feet above tide-water. The Petersburg deposit was regarded by Prof. Tuomey as belonging to a different geologic era from that at Richmond, as evinced by the fact of his finding the casts of Pectens and other Meiocene fossils below that deposit, while at Richmond they are found above. The great difference in the character of the two deposits would also indicate this, the Petersburg Diatoms being generally much more transparent than the Richmond forms, and differing also materially in species. Upon exposure for some time to the weather, this earth assumes an almost snowy whiteness, and crumbles to a fine powder, but as first dug from the depths of the earth it resembles bituminous coal in color and solidity—so tough and hard is it, that in removing it from the tunnel it was blasted with gunpowder just as any other rock. Its composition, as nearly as can be estimated in a general way, is—10 per cent. unbroken forms of the Diatomacæ, 25 per cent. fine white sand, and the balance fine clay, formed, perhaps, mostly of the decomposed and broken Diatoms, the whole mass interspersed with many sponge spicules and a few Polycistena, and so strongly impregnated with alum that many of the wells and springs in Richmond are injured by it. To the microscopist this deposit is a source of unfailing interest, whilst the most inexperienced in such matters, upon being shown the wonderful forms found in it, are struck with surprise and delight. Had the pre-historic man possessed a microscope it might have been supposed that the forms seen in this deposit may have suggested the forms of many of his appliances, as in it may be found models of almost all the implements used by savages, whether for war, the chase, or in domestic life; witness, for instance, his stone hatchets, arrow and spear heads, knotted clubs, boomerangs, &c.; a catalogue of such matters used by civilized people would embrace plates, dishes, cups, saucers, knives, forks, scissors, balls, tops, spectacles, watches, anchors, dumb-bells, cannon, coin, musical notes and many other articles; the investigator being constantly startled by the strange resemblance which hundreds of these ancient natural forms bear to things in every day use. Certain varieties, however, predominate, and their distribution varies with level and locality, the upper portion of the stratum being comparatively poor in forms, while they increase in number and variety as we descend to the lower levels. The genus Coscinodiscus seems to characterize this earth, and of it there are dozens of varieties varying from the (microscopically) enormous C. gigas to the minute and elegant C. stelliges which resembles closely a finely polished opal, requiring a lens of wide aperture and considerable power to show its areolations. Orthosira marina is everywhere abundant, whilst many beautiful forms of Navicula are found in every gathering. Amongst these we may specially note two kinds of Pleurosigma, one of which, P. angulatum, is a favorite test Diatom, and the other, which it is is proposed to call P. Virginica, (as it is the most common form of Pleurosigma in the Virginia earths), is remarkable for the beauty of its contour, which exactly copies a willow leaf, and the want of uniformity in its striæ, which are much coarser in the middle than at the ends of the valves. It can be easily resolved with a good ¼ in. Objective, without the aid of oblique light. The genus Triceratium, is also well represented by many beautiful varieties, the handsomest of which is, perhaps, T. Maylandica, which can be resolved with almost the same ease as P. Virginica, Isthmia enervis, Biddulphia Tuomeyii, Terpsinæ musica, Anlacodiscus crux, Navicula lyra, Gonphonema, Heliopelta, Asterolampra Concinna, Asteromphalus, Brookeii, and Synedra, are more rarely met with.

From the great variety in the markings on these valves, a slide of the earth, properly prepared, becomes one of the best and most interesting tests for the performance of objectives, from the lowest to the highest powers in general use. On some of them, for instance, the areolations may be seen with a simple triplet, whilst on others a first-class objective of wide angular aperture, aided by all the modern refinements of illumination, is needed to show them.

Mr. C. L. Peticolas, of 635 Eighth street, Richmond, Va., has sent us for inspection a few of his recently mounted slides of the Richmond earth, prepared by a new process for separating the Diatoms from the extraneous matter. We have pleasure in stating that these slides show the leading characteristics of this deposit very clearly and beautifully. To those who are studying these forms of fossil diatomaceæ, the slides of Mr. Peticolas will be very acceptable, and they should be added to the cabinet of all who possess a good microscope.

A USEFUL list of the Longicorn beetles, or wood borers, constituting the vast number of insects injurious to our forest, shade, and fruit trees, may be found in the October number of *The American Entomologist*, briefly arranged in the order of their most recent classification.

THERE are two beetles in the United States, both commonly called "*Fire-flies*," which are now known to be luminous in their larval as well as in their perfect state; the one *Photurus Pensylvanica*, De Geer, the other species *Photurus pyralis*, Linn. Both the males and females of these species have wings, and therein they differ from the true Glow Worm of England (*Lampyris noctiluca*), the female of which is wingless and emits a much more brilliant light than does her winged mate.—C. V. Riley.

AMERICAN MUSEUM OF NATURAL HISTORY, NEW YORK.

The customary autumn reception at the American Museum of Natural History, Seventy-seventh street, near Central Park, took place on the 27th instant, from two to five o'clock in the afternoon. The attendance was not so large as upon former occasions. Among the trustees present were noted Mr. Robert L. Stuart, President ; Messrs. Robert Colgate, Benjamin H. Field, Adrian Iselin, Morris K. Jesup, James M. Constable, Joseph W. Drexel, Frederic W. Stevens, Hugh Auchincloss, Oliver Harriman, ex-Governor E. D. Morgan, John H. Sherwood, R. H. Keene, Professor Eggleston, Rev. Dr. Trimble, Arkansas ; Professor Daniel S. Martin, Rutger's College, with many others.

The additions and improvements since the last reception, in May of the present year, may be briefly summarized as follows: In the lower hall the Binney and Bland collection of land and fresh water shells formed a new feature. It contained the typical specimens that are to be met with in the works of those authors, and was presented by twelve members. The whole was enclosed in a desk case, placed between cases R and K. Besides this the skeletons of three Australians were there to challenge the attention of scientists. These interesting specimens were the gift of Mr. Morris K. Jesup, and may be inspected in case A. In the main hall, the Maximilian collection of birds, attracted the attention of visitors ; they have been re-mounted on the new stands described in an article in "SCIENCE," October 7 last, under the title of "Bird Furniture, by Dr. Holder, the Assistant Superintendent. We direct the attention of those making collections of Natural History specimens, to these stands ; they are inexpensive, and possess many advantages. The North American collection was increased by six hundred specimens.

The gallery stairway showed a detailed ethnological map of Africa, drawn on a large scale by Professor Bickmore. The additions to the Ethnological Collection from the South Seas consist of a war canoe (case No. 1), New Zealand weapons and carvings (case No. 3) and stone axes from New Guinea (case No. 4). The set of ornaments and carvings from British Columbia, presented by Mr. H. R. Bishop (case M), proved to be interesting, as many items were included which appeared unfamiliar to most people present. The Geological Hall received seven geological maps of Eastern North America, some Encrinites and other fossils from various formations. Cases A, B, C, D and E were re-arranged and labelled. In the desk cases specimens were placed which served to illustrate Dana's " Manual of Geology." The rearrangement and labelling of the portions alluded to are a considerable improvement.

Altogether the several collections and their belongings presented the appearance of being well kept and arranged accrding to the best scientific principles. The trustees say that the elevated railroad has brought a greatly increased number of visitors to the Museum, and they hope to make it still more attractive as a place of instructive amusement. It has been suggested that such advanced classes of the higher schools and colleges as are making a regular study of natural history could find in this collection an excellent opportunity for advancing themselves in their chosen branch of education. If professors or teachers would accompany their pupils periodically through the Museum, giving progressive lectures on the different subjects presented for consideration, it is believed that the results would, under all aspects, be most beneficial.

ASTRONOMICAL NOTES.

Dr. W. L. Elkin has made a re-discussion of the various series of observations of α Centauri for the determination of the value of its parallax. These include the observations of Henderson, Maclear, Moesta and E. J. Stone. Besides a recomputation of the absolute parallax, Dr. Elkin has selected the observations which were made on nights when both α and β Centauri were observed, and from these determined their relative parallax. The discussion includes a new determination of the orbit of α considered as a double star, as well as a discussion of the relative proper motions of α and β. From a careful examination of each series he concludes that Maclaer's is the only one worthy of confidence. He finds that, although the probable error of Moesta's series is small, the annual variation may be accounted for as the effect of changes in temperature. Maclear's observations give for the relative parallax of α and β: $0''.50 + 0''.08$.

Dr. Henry Draper has succeeded in photographing the bright part of the nebula in Orion in the vicinity of the trapezium. The photographs show the mottled appearance of this region distinctly. They were taken by the aid of a triple objective of eleven inches aperture made by Alvan Clark and Sons, and corrected especially for the photographic rays. The exposure was for fifty minutes. A detailed description of the negatives has not yet been published, but will be soon. O. S.

SWIFTS' COMET.

By Ed. E. Barnard.

The large comet discovered by Prof. Lewis Swift on the night of Octer 10th in R. A. 21 h. 30 m. north declination 17° 30', is now in excellent position for observation. On October 21st it followed the fourth magnitude—star κ *Pegasi* by somewhat less than 1°. At 8 h. Washington *m. t.*, I determined its position by the aid of a ring micrometer, R. A. 21 h. 42¼ m. Dec. + 25° 1'. The following night, October 22d, its position was at 10 h. 20 m. R. A. 21 h. 44 m. 3 sec- Dec. + 26° 2'. It is moving moderately fast in a north. easterly direction. It was observed again on the nights of October 23d and 24th. The comet is perfectly transparent. At each observation it passed over a number of small stars, 8 or 9 mag., these were seen through its very centre ; they were slightly dimmed by the material of the comet.

It appears large and diffused with a slight condensation at the middle or the preceding side, with probably faint evidences of a diffused tail.

It can be seen with a very small telescope, being plainly visible on the 24th in my 1⅛ in. finder.

Nashville, Tenn., *October* 25.

BOTANY.

The first annual Report upon Useful and Noxious Plants, presented by Professor T. J. Burrill to the Illinois State Board of Agriculture, contains a paper suggesting the more general cultivation of the Catalpas (*Catalpa bignonioides.*) Professor Burrill states : "I write 'these trees' advisedly, believing that the two kinds now known as the common and the hardy, or the eastern or southern and the western, are really different species. The wonder is that botanists had not long ago detected this difference and that in our manuals of botany the two had not been given under specific names.

At Urbana, Ill., in 1880, the one came into flower the first week in June ; the other was nearly three weeks later, being in full flower about June 24th. They differ in other respects quite as much as well recognized species of oak, ash and cotton-wood ; much more than described species of willow. But *Catalpa bignonioides*, Walt., is the only name to be found in the ordinary books, devoted to the flora, in whole or part, of North America. In 1853 Dr. Warder, of Ohio, noticing the showy flowers of some trees at Dayton, Ohio, and supposing these to be a *variety* of the well known species with this peculiarity, named the variety *speciosa*. It now appears that this large flowered kind is the common indigenous form found in the States of Indiana, Illinois, Kentucky, Tennessee, Wisconsin, Arkansas, etc., and botanists will doubtless henceforth write *Catalpa speciosa*, Warder, as a distinct species. Contrasted with *Catalpa bignonioides* the flowers are earlier and larger; the seed pods are larger; the bark is darker, and does not scale off, giving quite a different aspect to the trunk of a mature specimen ; the growth is more erect, causing a better bole and finer head, and the tree is not so liable to be killed by the severities of winter. Added to all this the trees are so characteristically different that anyone can readily distinguish them. In *C. bignonioides* they are narrow and the fringe of the wing is close and pointed ; in *C. speciosa* the larger seed has a wider wing, terminated at each end with a broad fringe of softer hairs. Unfortunately most of the cultivated Catalpa trees in Illinois have been of the tender species, and, although the wonderful durability of the wood has long been known, its liability to 'winter-kill' and its irregular, crooked growth has prevented its being planted for timber."

Much space is also given in this report to the subject of Fungi on living plants, which are more disastrous to crops than the ravages of insects. These forms include rusts, smuts, mildews, rots, blights, etc., the rust alone on wheat taking from the former more than all the tax collectors, and creating such losses as to frighten cul tivators from their business. Professor Burrill regrets that the study of Fungi receives so little attention in this country, and says the number of American botanists who have published original accounts of the development of any fungous species may be counted on the fingers of one hand. As much practical importance and scientific interest is attached to such a study, we trust many botanists may in the future give more of their time to original investigations in this direction.

MICROSCOPY.

The American Monthly Microscopical Journal for October, describes a warm stage for the Microscope, by Professor E. H. Bartley. It has the advantage of being so simple that it can be constructed at home with a few inexpensive materials. We once saw this apparatus shown by the inventor at the New York Microscopical Society, and considered it a success.

Simple forms of mechanical figures are described by Mr. J. Sullivant.

Professor C. C. Merriman's interesting paper on the microscopical collections made by him in Florida, occupies nine pages of this number.

We regret that Mr. Frederick Habirshaw's "*Catalogue of the Diatomaceæ*" will be delayed in the publication for the want of sufficient promise of patronage. This may arise from many not understanding the nature of the work or its construction. If the editor of the *American Monthly Microscopical Journal*, who has the matter in hand, would print a sample page in his journal, it might remove such a difficulty. We believe we have heard the author himself state that the book in question would be useless to those not having a full library of works on the subject, so as to avail themselves of the ample references he makes to the published literature of the subject.

The demand for such a work must be very limited ; on the other hand a condensation of all this literature would be very welcome. A comprehensive work on the diatomaceæ, well illustrated, is much wanted and if issued in monthly parts, at a moderate price, would command a fair sale both at home and abroad.

Mr. Habirshaw appears to have the material for such a work in his possession, and he has given ample proof of his literary ability to undertake the task.

We think ten dollars would be better invested in a work such as we suggest, than five dollars in an index to a scattered literature, which the purchaser can never hope to possess.

PHYSICAL NOTES.

Observation Made on a Group of Rays in the Solar Spectrum.—L. Thollon figures and describes a group of four rays, situate in the Orange. Two of these rays belong to Iron, their wave-lengths being respectively 5976·1 and 5974·6. The other two are Telluric, and their wave-lengths are 5976·35 and 5974·36.

Cause of the Variations of the Fixed Points of Thermometers.—J. M. Crafts cites some experiments which reduce to nothing, or almost so, the part played by pressure in the permanent elevation of the zero-point. The glass blown at the lamp and exposed for a long time to the action of heat diminishes in volume by means of some internal change, and it is not demonstrated that pressure plays any part whatever in the phenomenon. The particles of glass which have been removed asunder whilst it was being blown do not return immediately to their normal position at a lower temperature ; we observe disturbances for some time, and finally the glass may remain for a long time in a state of tension at the ordinary temperature. The action of heat at a given temperature (*e.g.*, 355°), giving a greater mobility to the particles, favors their return to the normal position, and gives scope to a contraction. But the glass, when cooling from this latter temperature retains a part of the displacement peculiar to 355°. On heating again to a lower temperature (*e.g.*, 300°) a new decrease of volume is produced, so that a very slow cooling, which produces successively all these effects upon the particles of glass, must ensure the greatest stability.

Rapid Alcoholic Fermentation.—In order to effect rapid fermentation for the destruction of the sugar contained in wines, J. Bouissngault suggests that the sample be mixed with water and yeast, and placed in connection with an exhausting syringe, reduces the pressure, and thus which eliminates the alcohol as fast as formed. The fermentation, not being checked by its presence, goes on till all the sugar is decomposed.

Inconveniences, from a Physiological Point of View, of the Substitution of Amylic Alcohol for Ether in Stas's Process for the Detection of Morphine.—As amylic alcohol, even in very small doses, produces in animals systoms closely resembling narcotism, and as it is not readily removed from the cadaveric extract, physiological experimentation in confirmation of the chemical reactions of morphine is rendered untrustworthy.—*Compes Rendus.*

Influence of the Galvanic Current on Bacteria F. Cohn and B. Mendelsohn carried out their experiments to verify the assertion of Schiel, that the galvanic current prevented the development of Bacteria. The results were that a feeble current from one pair of elements had no perceptible effect ; a current from two elements rendered the solution inactive at the positive pole ; a current from five, continued for twenty-four hours, completely sterilized the whole solution, and deprived it of its power to infect another solution. The solution at the positive pole was first affected ; with the stronger current the liquid became acid at the positive and alkaline at the negative pole. The induction current had no perceptible effect on the Bacteria.—*Jour. Chem. Soc.*

Effect of Age on the Quality of Iron.—Previous tests have shown that iron, subject to even fifty years of use and exposure, is not perceptibly changed in quality, either in strength or elasticity. Professor Thurston recently tested the remains of the Fairmount Suspension Bridge, which had been in use forty years, and found the iron in no manner deteriorated.

Phosphorescent Lighting.—Dr. Phipson takes Sulphide of Barium, or some other substance which is rendered phosphorescent by the solar rays, and encloses it in a Geissler tube, through which he passes a constant electric current of a feeble but regular intensity. He claims to obtain in this manner a uniform and agreeable light, at a lower cost than that of gas.

BOOKS RECEIVED.

Sugar Analysis—A Description of the Methods used in Estimating the Constituents. By M. Benjamin, Ph. B. New York. 1880.

To those who desire a concise and practical guide to this subject, Dr. Benjamin's essay, published in pamphlet form with twenty illustrations, will perhaps be more serviceable than a more elaborate work. The essential facts for a general comprehension of the subject have been judiciously arranged.

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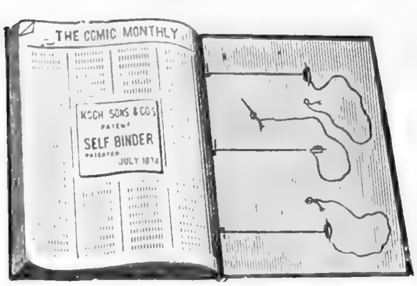

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Proceedings of Scientific Societies will be recorded, but the abstracts furnished must be signed by the Secretaries.

Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply may be written in the form of an article.

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SATURDAY, NOVEMBER 6, 1880.

During the year 1877 the Microscopical Section of the Indianapolis Lyceum of Natural History addressed a letter to the various Microscopical Societies throughout the United States, requesting expressions of their views touching the desirableness of a National Convention of the microscopists of the country, for the purpose of taking the necessary steps to form a National Association for the promotion of the progress of microscopical science, and for the benefit and advantage of all concerned therein.

Favorable replies having been received, The National Microscopical Congress, pursuant to a call, met at the Court house of Indianapolis, Indiana, on the 14th of August, 1878.

At this convention nearly fifty gentlemen, representing the Microscopical Societies of the United States, decided by a resolution, unanimously passed, that "it is desirable to have a National organization for the promotion of microscopical science."

Thus The American Society of Microscopists was called into life, under the presidency of Dr. R. H. Ward, of Troy, N. Y. It met a second time at Buffalo, N. Y., on the 14th of August, 1879, and thirdly at Detroit, Michigan, on the 21st of August last.

It may be possible that the expectations of some persons in regard to the useful results of this Society have not been fulfilled; if such a feeling exists, we are not aware of its having been expressed; for our part we congratulate The American Society of Microscopists upon the results so far obtained, and feel sanguine for the good work it may accomplish in the future.

The address of the first president, Dr. R. H. Ward, was a model of its kind. The long extract we gave from it in "Science" for July 31, last, under the title of a "Plea for the Metric System in Microscopy," showed the master hand of an accomplished writer and earnest worker.

Dr. Ward's address was of a most practical nature, and well adapted to inspire enthusiasm; he dwelt upon the many instances in which the value of the microscope had been demonstrated, and recalled the many fields for microscopical work still fully open and recognized, but yet unoccupied, and concluded by showing that microscopical study might be made not only a source of pleasure to ourselves, but an effective aid to science and humanity.

The third annual meeting of this Society held under the Presidency of Professor H. L. Smith, of Geneva, N. Y., was fully reported in this journal on the 25th of September last.

As a professional microscopist Professor H. L. Smith, the second President, has so distinguished himself, that his reputation as an authority on the Diatomaceæ and other allied forms, is not confined to this continent, but acknowledged wherever such studies are intelligently pursued.

On referring to our report we find that Professor Smith congratulated the Society on its progress, and the meeting adjourned hopefully, after having elected as President for the new year Mr. J. D. Hyatt, of our city, a gentleman eminently qualified to occupy the chair of his predecessor.

Such being the history and present standing of The American Society of Microscopists, we notice with some surprise that the editor of the *American Monthly Microscopical Journal*, in his issue for September last, makes a proposition, to give this Society a *coup de grace*, by proposing that the Society shall disorganize, and its members join the Association for the Advancement of Science.

The reasons for such action are stated to be as follows: 1. The Society has not received the support of microscopists. 2. The officers of the Society have been inexperienced men, who have not directed it properly. 3. A supposed necessity created by the writer, that this Society must meet at the same place and time as the A. A. A. S., it being then infered that as the latter Society has a *subsection* of microscopy, there arises the difficulty of having two meetings on the same subject simultaneously; therefore one should be abandoned, the preference being given to the American Society of Microscopists for such act of self-sacrifice. 4. Can the American Society of Microscopists show any reason why it should exist? If it cannot, the inference is obvious.

This question having been raised in a journal devoted to microscopy, and by a gentleman who formed one of the original Committee of Organization, it appears only courteous to the writer, to direct the attention of microscopists to the subject. We suggest that they should give some expression of opinion, if they desire the integrity of this Society. For ourselves, we shall strongly support the maintenance of the American Society of Microscopists, on account of our decided faith in its usefulness, and necessity for its existence, and for the reason also, that no real cause has been shown for its disbandment.

Taking the charges of the editor of the *American Monthly Microscopical Journal* in the order presented, we would say: 1st. That we have the authority of the late President, Professor H. L. Smith, that the Society *has* received sufficient support to make it a success. 2d. That the Society has been unexceptionally fortunate in the selection of officers, that they have proved themselves to be experienced men, and *have* "directed properly." 3d. That the Society does not deem it necessary to meet in conjunction with the A. A. A. S., and has voted down all resolutions for so doing. The assertion to the contrary is therefore perfectly gratuitous, and the fact that those who propose it, also made it a reason for breaking up the Society, has the appearance of a desire to lead the Society to such an end. 4th. The demand made upon the Society by one of its members, to show cause why it should exist, appears slightly presumptuous and ill-timed. As a suggestion *before* the establishment of the society it might have had some weight, but after the third annual meeting, and the congratulations of its President on its success, the proposition is unseasonable. We would remind the editor of the *American Monthly Microscopical Journal*, when he challenges the American Society of Microscopists to show the *raisen d'etre* for its existence, that fifty delegates, representing the microscopists of the United States, in his presence passed a resolution in the following words: "*We think it desirable to have a National Organization for the promotion of Microscopical Science.*" We consider this a conclusive answer to the present querest, and to all others who in future raise such a question.

The article we have referred to states, that "if the American Society of Microscopists does not decide to meet next year in convention with the A. A. A. S., at Cincinnati, *that the next meeting will be its last.*" As the writer also states, that if it does so meet, the necessity will arise for it to be "disorganized," and as one of these alternatives is inevitable, the fate of the society would appear to be sealed.

As we believe these difficulties to be purely imaginary, we are ready to grant the American Society of Microscopists a long term of existence, and a future of utility and progress. If any of our readers are of a contrary opinion, our columns are open for an expression of their views.

LAW ACCORDING TO WHICH THE METALS, AND THEIR ORES, CAME TO, OR NEAR TO, THE SURFACE OF THE EARTH.

By Prof. Richard Owen, M.D., LL.D.

In the abstract of a paper read before the A. A. A. S., which appeared in the issue of "Science" for September 25, 1880, allusion was made, in the closing paragraph, to the connection between the law of land-forming and that of metallic development.

We might reasonably expect that the metals requiring temperatures from 2000 degrees to over 2500 degrees F. to melt them (such as iron and gold) would be the first to solidify, as our earth cooled; and therefore more likely to exist among older rocks than such metals as zinc, lead[1] and tin, which melt at a comparatively low temperature; and consequently could not become solid until the earth's crust had cooled to 773 degrees, 612 degrees, and 442 degrees, the melting points respectively of these metals. Such we find to be the fact. Furthermore, Faraday demonstrated that all substances, when suspended freely between the jaws of a powerful horseshoe magnet, would place themselves either *paramagnetically*, the same as iron and some other metals, or *diamagnetically*, the same as bismuth and numerous other bodies; and the magnetism developed, for the time being, in that horseshoe magnet, may be, and often is, produced by powerful currents of electricity.

It has been proved that there are constantly currents of electricity passing in the earth's crust, chiefly in an opposite direction from the earth's revolution, perhaps therefore operating mainly in causing a freely suspended needle to place itself at right angles to the plane of those so-called currents.

It seems therefore, further, not unreasonable to expect that metals, when about to solidify, if free to permeate cavities in all directions, should assume, relatively to these currents of electricity, respectively either a paramagnetic or a diamagnetic position. Such seems in reality to have been the case: Iron, manganese, platinum, nickel, cobalt (and probably other paramagnetic bodies, but time has not permitted this latter investigation) will be found chiefly occupying north and south belts, corresponding pretty generally with meridians, while gold, silver, copper, tin, lead, zinc, antimony, bismuth and other diamagnetic bodies will be found in east and west belts, sometimes on regular parallels, of which the terrestrial north pole is the centre, sometimes in east and west curves, having one or other of the Continental foci (pointed out in the law of land-forming) as their centre. The apparent law, then, briefly formulated, may be thus expressed:

The paramagnetic metals, in consolidating, arranged themselves along north and south belts, usually near the median line of each Continent, and are found in older rocks as well as newer. Diamagnetic metals are most commonly to be found in belts, not necessarily continuous, but running more or less east and west, end except perhaps in the case of gold, silver and cop-

[1] Although lead is found sometimes in silurian and carboniferous rocks, yet Dr. Dana shows (at page 148 of his Manual of Mineralogy) that such is not its true age. Speaking of Galena, he says: "In Derbyshire, England, the deposits contain fossils of permian rocks, showing that, although occurring in subcarboniferous limestone, they were much later in origin."

per, not so much in older rocks as in later formations. Demonstration:

I.—PARAMAGNETIC METALS AND THEIR ORES.

1. *Iron.*—A considerable portion of this metal as magnetite, both in the United States and in Scandinavia, is magnetic, possessing polarity. Specimens from Magnet Core, Ark., and from near Pilot Knob, Missouri, in the United States, and from Dannemora, in Sweden, present this character very strongly.

(*a*) Nearly all the important iron of the United States (as can be ascertained from Dana's Mineralogy, or readily seen by inspecting Map No. VIII in Cornell's Phys. Geogr.) occupies a north and south belt, between the meridians of 77° and 91° long. W. of Gr.) extending north and south from Lake Superior to Alabama, average lat., say 40°[2]. On the opposite side of the globe, this belt, prolonged in a great circle, will include the main iron belt of Asia.

(*b*) The iron of Europe is in a belt about (20) twenty degrees or nearly 800[3] statute miles in width, namely from long. 5° W. to long. 15° E., extending north and south from Scandinavia to Tunis: thus being about 90° distant from the American-Asiatic belt.

(*c*) The only other iron laid down in Cornell is in a belt from near the Urals to a deposit in Persia, half way between the European and the Central Asiatic iron belt, or about 45° distant from each of these.

(*d*) Dana mentions some iron in the region of San Francisco, which would be about 45° west of the Pennsylvania portion of the main United States belt.

2. *Manganese.*—The localities given by Dana for this metal would fall almost, if not entirely, within the iron belts of North America and Europe: showing a similar tendency to assume a paramagnetic direction.

3. *Platinum.*—The chief localities of platinum, as given in Dr. Dana's Manual of Mineralogy; namely, the Ural Mountains and North Granada, South America, as well as Canada and North Carolina, where traces have been found, fall within the United States iron belt. Borneo and places in Minas Geræs, Brazil, where some platinum has been taken out, are very slightly east of the above-named belt.

4. *Nickel and Cobalt.*—Ores of these metals are found, according to Dana, in Cornwall, Sweden, Norway, France, Saxony and the United States (Missouri, North Carolina, Pennsylvania): all again within one or other of the paramagnetic iron belts. Some nickel found in New Caledonia would occupy a position very nearly half way between the iron belt of Central Asia and the iron of California.

Without going into further details at present regarding the paramagnetic metals and their ores, let us examine some of the

II.—DIAMAGNETIC METALS AND THEIR ORES.

1. *Gold.* This metal cooling among the first on the globe is found in old formations; but appears to be also injected into, or deposited in, the fractures and fissures of rocks having a more recent age. The earlier east and west ranges or belts, in which it occurs more or less, often correspond, as already stated, with parallels of latitude; thus we find gold on a belt of the parallels 55° to 60° N. lat., comprising the gold of Alaska, Scotland, Sweden and the Urals; again in a belt ranging from lat. 45° to 50° N., embracing the gold of British Columbia, Washington Territory and Oregon, of Lake Superior, Canada and Nova Scotia, in North America. We find on the same parallels or belt, in the eastern continent, the gold of the Alps, Tyrol, Hungary and the Altai Mountains. Another zone or belt, in about lat. 35° to 38° N., runs from California and Arizona, through Georgia and North Carolina, and is prolonged through Spain, Thibet, China and Japan. A more southerly belt marks the gold of Central America and New Granada (United States of Colombia, S. A.), also of Western and Eastern Africa (about 5° N. of the Equator), as well as of Ceylon, Java and Borneo. The most southerly belt, in about lat. 22° to 32° S., embraces the South American gold localities, the gold washings of South Africa, the rich mines of Australia, and almost includes the gold of New Zealand.

In some cases, without making the belt so broad, gold localities can be traced on one and the same curve, using either the northern focus of each continent for a centre, or occasionally the more southern continental focus. For instance, using the Boothia Felix focus as a centre, an arc unites the gold of Oregon with that of Canada, while from the Lake Superior focus a curve sweeps from the gold of the Sierra Nevada and the Sacramento Valley in California to that of Georgia and North Carolina.

2. *Silver.* The most noted localities for this useful metal can be readily traced in North America, Europe and Asia, on belts running east and west, often at vertical intervals of about 4° to 5° apart, or say every 300 statute miles. Thus we have a belt from the silver of Norway to that in the Urals; then another, from Montana, Idaho, Wyoming and Lake Superior regions, continued through England, France and Saxony to the Altai Mountains; a third silver girdle runs from California and Arizona, through Utah and Colorado, thence to Spain. A fourth shows that of Mexico on the same parallel with the silver of China. Then come the rich mines of South America, in three successive belts (that of Venezuela, of Brazil and of Bolivia), with nothing to correspond in the eastern continent.

3. *Copper* can equally readily be traced along belts on diamagnetic parallels; such as one in Scandinavia connecting with the Urals. A second, on the parallel of the rich Lake Superior region takes in the copper of Cornwall, of France, Thuringia, Hungary, Siberia, China and Japan; a third can be found embracing the mines of Arizona, New Mexico, Tennessee and North Carolina, also of the Island of Cyprus, of Turkestan and Persia; a fourth gives us the copper of Cuba, Africa, Arabia and Hindostan, as shown in map No. VIII, of Cornell's Phy. Geogr., by Steinwehr. Of the two copper belts in S. America, the more Southerly is on the same parallel with the copper of Australia.

4. *Tin* (although sparingly distributed, except in two or three localities) follows the same rule: First belt, Massachusetts, New Hampshire, Cornwall (England), Saxony, Austria, and Russia. Second belt,

[2] A degree of longitude in latitude 40° is about 53 statute miles; consequently the width of the belt is nearly 750 miles.

[3] The average latitude of European iron being about 54° to 55° N., we may call a degree equal to 40 statute miles, thus giving the above result.

California and Spain. Third belt, Dureago (Mexico), Peru and China. Fourth belt, Malacca and Banca. Fifth. Bolivia, (S. America) Queensland and Northern New South Wales, in Australia, in about the parallel of 22° to 23° S. Lat.

5. *Lead.* The diamagnetic arrangement of the localities in which this metal is most abundantly found, may be rendered equally apparent, whether we follow the Galena and other ores in belts, on parallels around the globe, or connect these metallic deposits by curves from the Boothia Felix focus, for North America, and the Scandinavian focus for Europe. Thus, first belt in North America, from Arctic focus, Idaho, Wisconsin, Iowa, Northern Illinois, Vermont, New Hampshire and Maine; second belt, Nevada, Utah, Colorado, Missouri, Southern Illinois, New York, Connecticut; third belt, California, New Mexico, Arkansas, Tennessee, and North Carolina; fourth, Fort Yuma, and Arizona; fifth, the argentiferous Galena of Mexico.

In Europe there are from the Scandinavian focus four belts; first, that of Scotland and Saxony; second, of England and Bohemia; third, the lead mines of France; fourth, those of Spain, often argentiferous.

6. *Zinc.* From the Scandinavian focus, we trace one curve, which marks the zinc belt of England, Belgium and Germany; another that of France and Austria. In Asia, from the North Siberian focus, a belt connects the zinc of the Alati mountain with that of China. In the United States, if we take the Lake Superior focus as a center, we can bring within one belt the various zinc ores of Tennessee, Virginia, Pennsylvania, and the abundant deposits of New Jersey, as well as the zinc localities of New York, Vermont, New Hampshire and Maine.

7. *Antimony.* From the Lake Superior focus, a semicircle unites, in one belt, the ores of Antimony found in Maryland, New Hampshire and Maine, while just outside is a curve or zone uniting the mines worked in Sonora (Mexico) with those of New Brunswick.

In Europe, with Mount Rosa for a center, the Tertiary circles (radius nine degrees), described in the former communication, passes through the zinc of Cornwall (England), of Spain and of Hungary.

8. *Bismuth.* A belt in the United States, with Lake Superior for a center, unites the bismuth found in Montana, Arizona and Colorado, with that of Georgia and South and North Carolina.

In Europe, the bismuth of Norway and Sweden are in one curve from the Scandinavian focus; that of England, Saxony and Bohemia constitutes a second curve. . Bismuth is also found in Australia, nearly on the parallel of latitude on which it is obtained in Chili and Bolivia (South America).

These demonstrations, or coincident facts, may, perhaps, suffice to test the truth of the law, which appears to be similar in character to that governing the formation of land.

Metals and metallic ores would seem, then, most frequently, to have arranged themselves, particularly when diamagnetic, as a large majority of bodies are, in curves, equi-distant from some dynamic focus.

It is hoped the above generalization may aid the miner and mineralogist in their search after mineral wealth.

THE UNITY OF NATURE.

By the Duke of Argyll.

II.

Man is included in the Unity of Nature, in the first place, as regards the composition of his body. Out of the ordinary elements of the material world is that body made, and into those elements it is resolved again. With all its beauties of form and of expression, with all its marvels of structure and of function, there is nothing whatever in it except some few of the elementary substances which are common in the atmosphere and the soil. The four principal gases, with lime, potash, and a little iron, sodium, and phosphorus, these are the constituents of the human body —of these in different combinations—and, so far as we know, of nothing else. The same general composition, with here and there an ingredient less or more, prevails throughout the whole animal and vegetable world, and its elements are the commonest in the inorganic kingdom also. This may seem a rude, and it is certainly a rudimentary view of the relation which prevails between ourselves and the world around us. And yet it is the foundation, or at least one of the foundations, on which all other relations depend. It is because of the composition of our body, that the animals and plants around us are capable of ministering to our support—that the common air is to us the very breath of life, and that herbs and minerals in abundance have either poisoning properties or healing virtue. For both of these effects are alike the evidence of some relation to the organism they affect; and both are in different degrees so prevalent and pervading, that of very few things indeed can it be said that they are wholly inert upon us. Yet there is no substance of the thousands which in one manner or another affect the body, which does not so affect it by virtue of some relation which it bears to the elements of which that body is composed, or to the combinations into which those elements have been cast.

And here we ascend one step higher among the facts which include Man within the unities of Nature. For he is united with the world in which he moves, not only by the elements of which his body is composed, but also by the methods in which those elements are combined—the forces by which they are held together, and the principles of construction according to which they are built up into separate organs for the discharge of separate functions. Science has cast no light on the ultimate nature of Life. But whatever it be, it has evidently fundamental elements which are the same throughout the whole circle of the organic world; the same in their relations to the inorganic; the same in the powers by which are carried on the great functions of nutrition, of growth, of respiration, and reproduction. There are, indeed, infinitely varied modifications in the mechanism of the same organs to accommodate them to innumerably different modes by which different animals obtain their food, their oxygen, and their means of movement. Yet so evident is the unity which prevails throughout, that physiologists are compelled to recognise the fundamental facts of organic life as "the same, from the lowest animal inhabiting a stagnant pool up to the glorious mechanism of the human form."[1]

This language is not the expression of mere poetic fancy, nor is it founded on dim and vague analogies. It is founded on the most definite facts which can be ascertained of the ultimate phenomena of organic life, and it expresses the clearest conceptions that can be formed of its essential properties. The creature which naturalists call the Amœba, one of the lowest in the animal series, consists of nothing but an apparently simple and formless jelly. But simple and formless as it appears to be, this jelly exhibits all the wonder and mystery of that power which we know as Life. It is in virtue of that power that the dead or inorganic elements of which it is composed are held together in a special and delicate combination, which no other power can preserve in union, and which begins to dissolve the moment that power departs. And as in virtue of this power the constituent elements are held in a peculiar relation to each other, so in virtue of the same power does the combination possess peculiar relations with external things. It has the

[1] On the Nervous System, by Alex. Shaw. Appendix to Sir Charles Bell's "Anatomy of Expression."

faculty of appropriating foreign substances into its own, making them subservient to the renewal of its own material, to the maintenance of its own energy, and to the preservation of its own separate individuality. It has the faculty, moreover, of giving off parts of itself, endowed with the same properties, to lead a separate existence. This same substance, which when analyzed has always the same chemical composition, and when alive has always the same fundamental properties, is at the root of every organism, whether animal or vegetable. Out of its material all visible structure is built up, and the power which holds its elements together is the same power which performs the further work of molding them into tissues—first forming them, and then feeding them, and then keeping them in life. This is as true of the highest organism of Man as it is of the lowest, in which visible structure begins to be. The phenomena of disease have convinced physiologists that all the tissues of the body are freely penetrated by the protoplasmic corpuscles of the blood, and that the primordial properties displayed in the substance of an Amœba, which has no distinguishable parts and no separate organs, afford the only key to the fundamental properties of every animal body. One eminent observer assigns so high a place to this protoplasmic matter as the primary physical agent in the building of the House of Life, and in its renovation and repair, that he considers all its other materials, and all its completed structures as comparatively "dead."

But the unity of Man's body with the rest of Nature lies deeper still than this. The same elements and the same primary compounds are but the foundations from which the higher unities arise. These higher unities appear to depend upon and to be explained by this—that there are certain things which must be done for the support of animal life, and these things are fundamentally the same from the lowest to the highest creatures. It is for the doing of these things that "organs" are required, and it is in response to this requirement that they are provided. Food—that is to say, foreign material—must be taken in, and it must be assimilated. The circulating fluids of the body must absorb oxygen; and when this cannot be done more simply, a special apparatus must be provided for the separation of this essential element of life from the air or from the water. Sensation must be localized and adapted to the perception of movements in surrounding media. The tremors of the atmosphere and of the luminiferous ether must first be caught upon responsive—that is to say, upon adapted—surfaces, and then they must be translated into the language of sensation—that is to say, into sight and hearing. The heat evolved in the chemical processes of digestion and of oxygenation of the blood must be made convertible into other forms of motion. The forces thus concentrated must be stored, rendered accessible to the Will, and distributed to members which are at its command. These and and many other uniform necessities of the animal frame constitute a unity of function in organs of the widest dissimilarity of form, so that however different they may be in shape, or in structure, or in position, they are all obviously reducible to one common interpretation. They do the same things—they serve the same purposes—they secure the same ends—or, to use the language of physiology, they discharge the same functions in the animal economy.

But more than this; even the differences of form steadily diminish as we ascend in the scale of being. Not only are the same functions discharged, but they are discharged by organs of the same general shape, formed on one pattern, and occupying an identical position in one plan of structure. It is on this fact that this science of comparative anatomy is founded, and the well-established doctrine of "homologies." The homology of two organs in two separate animals is nothing but the unity of place which they occupy in a structure which is recognized as one and the same in a vast variety of creatures—a structure which is one in its general conception, and one in the relative arrangement of its parts. In this clear and very definite sense, the body of Man, as a whole, is one in structure with the bodies of all vertebrate animals; and as we rise from the lowest of these to him who is the highest, we see that same structure elaborated into closer and closer likeness, until every part corresponds—bone to bone, tissue to tissue, organ to organ. It is round this fact that so many disputants are now fighting. But all the controversy arises, not as to the existence of the fact, but as to its physical cause. The fact is beyond question. In a former work[2] I have dwelt at some length on the bearing of this fact on our conceptions of "Creation by Law," and on the various theories which assume that such close relationship in organic structure can be due to no other cause than blood relationship through ordinary generation. At present I am only concerned with the fact of unity, whatever may be the physical cause from which that unity has arisen. The significance of it, as establishing Man's place in the unity of Nature, is altogether independent of any conclusion which may be reached as to those processes of creation by which his body has been fashioned on a plan which is common to him and to so many animals beneath him. Whether Man has been separately created out of the inorganic elements of which his body is composed, or whether it was born of matter previously organized in lower forms, this community of structure must equally indicate a corresponding community of relations with external things, and some antecedent necessity deeply seated in the very nature of those things, why his bodily frame should be like to theirs.

And, indeed, when we consider the matter, it is sufficiently apparent that the relationship of Man's body to the bodies of the lower animals is only a subordinate part and consequence of that higher and more general relationship which prevails between all living things and those elementary forces of Nature which play in them, and around them, and upon them. If we could only know what that relationship is in its real nature and in its full extent, we should know one of the most inscrutable of all secrets. For that secret is no other than the ultimate nature of Life. The great matter is to keep the little knowledge of it which we possess safe from the confusing effect of deceptive definitions. The real unities of Nature will never be reached by confounding her distinctions. For certain purposes it may be a legitimate attempt to reduce the definition of Life to its lowest terms—that is to say, it may be legitimate to fix our attention exclusively on those characteristics which are common to Life in its lowest and in its highest forms, and to set aside all other characteristics in which they differ. It may be useful sometimes to look at Life under the terms of such a definition, in order, for example, the better to conceive some of its relations with other things. But in doing so we must take care not to drop out of the terms so defining Life anything really essential to the very idea of it. Artificial definitions of this kind are dangerous experiments in philosophy. It is very easy by mere artifices of language to obliterate the most absolute distinctions which exist in Nature. Between the living and the non-living there is a great gulf fixed, and the indissoluble connection which somehow, nevertheless, we know to exist between them is a connection which does not fill up that gulf, but is kept up by some bridge being, as it were, artificially built across it. This unity, like the other unities of Nature, is not a unity consisting of mere continuity of substance. It is not founded upon sameness, but, on the contrary, rather upon difference, and even upon antagonisms. Only the forces which are thus different and opposed are subordinate to a system of adaptation and adjustment. Nor must we fail to notice the kind of unity which is implied in the very words "adaptation" and "adjustment"—and, above all others, in the special adjustments connected with organic Life. There are many unions which do not involve the idea of adjustment, or which involve it only in the most rudimentary form. The mere chemical union of two or more elements—unless under special conditions—is not properly an adjustment. We should not naturally call the formation of rust an adjustment between the oxygen of the atmosphere and metallic iron. When the combinations effected by the play of chemical affinities are brought about by the selection of elements so placed within reach of each other's reactions as to result in a given product, then that product would be accurately described as the result of co-ordination and adjustment. But the kind of co-ordination and adjustment which appears in the facts of Life is of a still higher and more complicated kind than this. Whatever the relationship may be between living organisms and the elements, or elementary forces of external Nature, it certainly is not the relationship of mere chemical affinities. On

[2] "The Reign of Law."

the contrary, the unions which these affinities by themselves produce can only be reached through the dissolution and destruction of living bodies. The subjugation of chemical forces under some higher form of energy, which works them for the continued maintenance of a separate individuality—this is of the very essence of Life. The destruction of that separateness is of the very essence of death. It is not Life, but the cessation of Life, which, in this sense and after this manner, effects a chemical union of the elements of the body with the elements around it. There is indeed an adjustment—a close, an intricate adjustment—between these and the living body; but it is an adjustment of them under the controlling energy of a power which cannot be identified with any other, and which always presents phenomena peculiar to itself. Under that power we see that the laws and forces of chemical affinity, as exhibited apart from Life, are held, as it were, to service—compelled, indeed, to minister but not allowed to rule. Through an infinite variety of organisms, this mysterious subordination is maintained, ministering through an ascending series to higher and higher grades of sensation, perception, consciousness, and thought.

And here we come in sight of the highest adjustment of all. Sensation, perception, consciousness, and thought—these, if they be not the very essence of Life, are at least—in their order—its highest accompaniments and result. They are the ultimate facts, they are the final realities, to which all lesser adjustments are themselves adjusted. For, as the elementary substances and the elementary forces of Nature which are used in the building of the body are there held by the energies of Life under a special and peculiar relation to those same elements and to those same forces outside the body, so also are they held in peculiar relations to those characteristic powers in which we are compelled to recognize the rudimentary faculties of mind. Sensation is the first of these, and if it be the lowest, it is at least the indispensable basis of all the rest. As such, it cannot be studied too attentively in the first stages of its appearance, if we desire to understand the unity of which it is the index and result. We have seen that the mechanism of living bodies is one throughout the whole range of animal Life—one in its general plan, and one even in the arrangement of many of its details. We have seen, too, that this unity rests upon that other—in virtue of which all organisms depend for the maintenance of their life, upon adjustments to certain physical laws which are held, as it were, in vassalage, and compelled to service; doing in that service what they never do alone, and not doing in that service what they always do when freed from it.

And now we have to ask what that service is? We can only say that it is the service of Life in all its manifestations, from those which we see in the lowest creatures up to the highest of which, in addition, we are conscious in ourselves. I say "in addition"—because this is the fundamental lesson of physiology and of comparative anatomy—that the principle and the mechanism of sensation are the same in all creatures, at least in all which have the rudiments of a nervous system. This identity of principle and of structure in the machinery of sensation, taken together with the identity of the outward manifestations which accompany and indicate its presence in animals, makes it certain that in itself it is everywhere the same. This does not mean, of course—very far from it—that the range of pleasure or of pain consequent on sensation—still less the range of intelligent perception—is the same throughout the animal kingdom. The range of pleasure or of pain, and still more the range of intelligent perception, depends on the association of higher faculties with mere sensation, and upon other peculiarities or conditions of organization. We all know by our own experience, when comparing ourselves with ourselves in different states of health or of disease, and by observing the like facts in others, that the degree of pleasure or of suffering, of emotion or of intellectual activity, which is connected with sensation, may be almost infinitely various according to various conditions of the body. But this does not affect the general proposition that sensation is in itself one thing throughout the animal kingdom. It cannot be defined in language, because all language is founded on it, assumes it to be known, and uses the metaphors it supplies for the expression of our highest intellectual conceptions. But though it cannot be defined, this at least we can say concerning it, that sensation is the characteristic property of animal life; that it is an affection of the "anima," of that which distinguishes animate from inanimate things, and that as such it constitutes one of the most essential of the fundamental properties of mind. So true is this, that the very word "idea," which has played a memorable part in the history of speculation, and which in common speech has now come to be generally associated with the highest intellectual abstractions, has had in modern philosophy no other definite meaning than the impressions or mental images received through the senses. This is the meaning attached to it (although, perhaps, no writer has ever adhered to it with perfect consistency) in the writings of Descartes, of Locke, and of Bishop Berkeley; and it is well worthy of remark that the most extreme doctrine of Idealism, which denies the reality of matter, and, indeed, the reality of everything except mind, is a doctrine which may be as logically founded upon sensation in a Zoophyte as upon sensation in a Man. The famous proposition of Bishop Berkeley, which he considers as almost self-evidently true, "that the various sensations, or ideas imprinted on the sense, cannot exist otherwise than in the mind perceiving them," is a proposition clearly applicable to all forms of sensation whatever. For every sensation of an organism is equally in the nature of an "idea" in being an affection of the living principle, which alone is susceptible of such affections; and it is plainly impossible to conceive any sense-impression whatever as existing outside a living and perceiving creature.

We are now, indeed, so accustomed to attach the word "idea" to the highest exercises of mind, and to confine the word "mind" itself to some of its higher manifestations, that it may startle some men to be told that sensation is in itself a mental affection. We have, however, only to consider for a moment how inseparably connected sensation is with appetite and with perception, to be convinced that in the phenomena of sensation we have the first raw materials and the first small beginnings of Intelligence and of Will. It is this fundamental character of sensation which explains and justifies the assertion of philosophers—an assertion which at first sight appears to be a mere paradox—that the "ideas" we receive through the senses have no "likeness" to the objects they represent. For that assertion, after all, means nothing more than this—that the impressions made by external things upon living beings through the senses, are in themselves mental impressions, and as such cannot be conceived as like in their own nature to inanimate and external objects. It is the mental quality of all sensation, considered in itself, which is really affirmed in this denial of likeness between the affections of sense and the things which produce those affections in us. It is one of the many forms in which we are compelled to recognize the inconceivableness of any sort of resemblahce between Mind and Matter, between external things and our own perceptive powers.

And yet it is across this great gulf of difference—apparently so broad and so profound—that the highest unity of Nature is nevertheless established. Matter built up and woven into "organs" under the powers of Life is the strong foundation on which this unity is established. It is the unity which exists between the living organism and the elements around it which renders that organism the appropriate channel of mental communication with the external world, and a faithful interpreter of its signs. And this the organism is—not only by virtue of its substance and composition, but also and especially by virtue of its adjusted structures. All the organs of sense discharge their functions in virtue of a purely mechanical adjustment between the structure of the organ and the particular form of external force which it is intended to receive and to transmit. How fine those adjustments are can best be understood when we remember that the retina of the eye is a machine which measures and distinguishes between vibrations which are now known to differ from each other by only a few millionths of an inch. Yet this amount af difference is recorded and made instantly appreciable in the sensations of color by the adjusted mechanism of the eye. Another adjustment, precisely the same in principle, between the vibrations of Sound and the structure of the ear, enables those vibrations to be similarly distinguished in another special form of the manifold language of sensation. And so of all the other organs of sense—they all perform their work in virtue of that purely mechanical adjustment which places them in a given relation to certain selected manifestations of external force, and these they faithfully transmit, according to a code of signals, the nature of which is one of the primary mysteries of Life, but the truthfulness of which is at the same time one of the most certain of its facts.

For it is upon this truthfulness—that is to say, upon a close and efficient correspondence between the impressions of sense and certain realities of external Nature—that the success of every organism depends in the battle of life. And all Life involves a battle. It comes indeed to each animal without effort of its own, but it cannot be maintained without individual exertion. That exertion may be of the simplest kind, nothing more than the rhythmic action of a muscle contracting and expanding so as to receive into a sac such substances as currents of water may bring along with them ; or it may be the more complex action required to make or induce the very currents, which are to bring the food ; or it may be the much more complex exertions required in all active locomotion for the pursuit and capture of prey ; all these forms of exertion exist, and are all required in endless variety in the animal world. And throughout the whole of this vast series the very life of every creature depends on the unity which exists between its sense-impressions and those realities of the external world which are specially related to them. There is therefore no conception of the mind which rests on a broader basis of experience than that which affirms this unity—a unity which constitutes and guarantees the various senses with their corresponding appetites, each in its own sphere of adapted relations to be exact and faithful interpreters of external truth.

A still more wonderful and striking proof is obtained of the unity of Nature, and a still more instructive light is cast upon its source and character, when we observe how far-reaching these interpretations of sense are even in the very lowest creatures ; how they are true not only in the immediate impressions they convey, but true also as the index of truths which lie behind and beyond—of truths, that is to say, which are not expressly included—not directly represented—in either sensation or perception. This, indeed, is one main function and use, and one universal characteristic of all sense-impressions, that over and above the pleasure they give to sentient creatures, they lead and guide to acts required by natural laws which are not themselves objects of sensation at all, and which therefore the creatures conforming to them cannot possibly either see or comprehend. It is thus that the appetite of hunger and the sense of taste, which in some form or other, however low, is perhaps the most universal sensation of animal organisms, is true not only as a guide to the substances which do actually gratify the sense concerned, but true also in its unseen and unfelt relations with those demands or laws of force which render the assimilation of new material an indispensable necessity in the maintenance of animal life. Throughout the whole kingdom of Nature this law prevails. Sense-perceptions are in all animals indissolubly united with instantaneous impulses to action. This action is always directed to external things. It finds in these things the satisfaction of whatever desire is immediately concerned, and beyond this it ministers to ends of which the animal knows nothing, but which are of the highest importance both in its own economy and in the general economy of Nature.

The wonderful instincts of the lower animals—the precision and perfection of their work—are a glorious example of this far reaching adjustment between the perceptions of sense and the laws which prevail in the external world. Narrow as the sphere of those perceptions may be, yet within that sphere they are almost absolutely true. And although the sphere is indeed narrow as regards the very low and limited intelligence with which it is associated in the animals themselves, it is a sphere which beyond the scope of their intelligence can be seen to place them in unconscious relation with endless vistas of co-ordinated action. The sentient actions of the lower animals involve not merely the rudimentary power of perceiving the differences which distinguish things, but the much higher power of profiting by those relations between things which are the foundation of all voluntary agency, and which place in the possession of living creatures the power of attaining ends through the employment of appropriate means. The direct and intuitive perception of things which stand in the relation of means to ends, though it may be entirely dissociated from any conscious recognition of this relation in itself—that is to say, the direct and intuitive perception of the necessity of doing one thing in order to attain to another thing—is in itself one of the very highest among the preadjusted harmonies of Nature. For it must be remembered that those relations between things which render them capable of being used as means to ends are relations which never can be direct objects of sensation, and therefore the power of acting upon them is an intuition of something which is out of sight. It is a kind of dim seeing of that which is invisible. And even if it be separated entirely in the lower animals—as it almost certainly is—from anything comparable with our own prescient and reasoning powers, it does not the less involve in them a true and close relation between their instincts and the order of Nature with its laws.

The spinning machinery which is provided in the body of a spider is not more accurately adjusted to the viscid secretion which is provided for it, than the instinct of the spider is adjusted both to the construction of its web and also to the selection of likely places for the capture of its prey. Those birds and insects whose young are hatched by the heat of fermentation have an intuitive impulse to select the proper materials, and to gather them for the purpose. All creatures, guided sometimes apparently by senses of which we know nothing, are under like impulses to provide effectually for the nourishing of their young. It is, moreover, most curious and instructive to observe that the extent of prevision which is involved in this process, and in the securing of the result, seems very often to be greater as we descend in the scale of Nature, and in proportion as the parents are dissociated from the actual feeding or personal care of their young. The Mammalia have nothing to provide except food for themselves, and have at first, and for a long time, no duty to perform beyond the discharge of a purely physical function. Milk is secreted in them by a purely unconscious process, and the young need no instruction in the art of sucking. Birds have much more to do—in the building of nests, in the choice of sites for these, and after incubation in the choice of food adapted to the period of growth. Insects much lower in the scale of organization, have to provide very often for a distant future, and for stages of development not only in the young but in the *nidus* which surrounds them.

There is one group of insects, well-known to every observer—the common Gall-flies—which have the power of calling on the vegetable world to do for them the work of nest-building ; and in response to the means by which these insects are provided, the Oak or the Rose does actually lend its power of growth to provide a special *nidus* by which the plant protects the young insect as carefully as it protects its own seed. Bees, if we are to believe the evidence of observers, have an intuitive guidance in the selection of food, which has the power of producing organic changes in the bodies of the young, and by the administration of which, under what may be called artificial conditions, the sex of certain selected individuals can be determined, so that they may become the mothers and queens of future hives.

These are but a few examples of facts of which the whole animal world is full, presenting, as it does, one vast series of adjustments between bodily organs and corresponding instincts. But this adjustment would be useless unless it were part of another adjustment between the instincts and perceptions of animals and those facts and forces of surrounding Nature which are related to them, and to the whole cycle of things of which they form a part. In those instinctive actions of the lower animals which involve the most distant and the most complicated anticipations, it is clear that the prevision which is involved is a prevision which is not in the animals themselves. They appear to be guided by some simple appetite, by an odor or a taste, and they have obviously no more consciousness of the ends to be subserved, or of the mechanism by which they are secured, than the suckling has of the processes of nutrition.

The path along which they walk is a path which they did not engineer. It is a path made for them, and they simply follow it. But the propensities and tastes and feelings which make them follow it, and the rightness of its direction towards the ends to be obtained, do constitute a unity of adjustment which binds together the whole world of Life, and the whole inorganic world on which living things depend.

I have called this adjustment mechanical, and so, in the strictest sense, it is. We must take care, however, not to let our conceptions of the realities of Nature be rendered indistinct by those elements of metaphor which abound in language. These elements, indeed, when kept in their proper places, are not only the indispensable auxiliaries of thought, but they represent those perceptions of the mind which are the highest and the most absolutely true. They are the recognition—often the unconscious recognition—of the central unities of Nature. Nevertheless, they are the prolific source of error when not closely watched. Because all the functions and phenomena of Life appear to be strictly connected with an apparatus, and may therefore be regarded as brought about by adjustments which are mechanical, therefore it has been concluded that those phenomena, even the most purely mental, are mechanical in the same sense in which the work is called mechanical which human machines perform. Are not all animals "automata?" Are they not "mere machines?" This question has been revived from age to age since philosophy began, and has been discussed in our own time with all the aid which the most recent physiological experiment can afford. It is a question of extreme interest in its bearing on our present subject. The sense in which, and the degree to which, all mental phenomena are founded on, and are the result of mechanical adjustments, is a question of the highest interest and importance. The phenomena of instinct, as exhibited in the lower animals, are undoubtedly the field of observation in which the solution of this question may best be found, and I cannot better explain the aspect in which it presents itself to me, than by discussing it in connection with certain exhibitions of animal instinct which I had occasion to observe during the spring and summer of 1874. They were not uncommon cases. On the contrary, they were of a kind of which the whole world is full. But not the less directly did they suggest all the problems under discussion, and not the less forcibly did they strike me with the admiration and the wonder which no familiarity can exhaust.

IMPROVEMENT OF THE MISSISSIPPI RIVER.*

By Prof. W. H. Ballou.

The Mississippi River is the most gigantic parasite known to men. The least possible estimate, computed from data in hand, shows that the annual average for the last thirty years, of money expended on it for improvements, and lost through its depredations, exceeds $7,000,000. Fully one-third of this sum is used by the government, States and private individuals to keep the stream and its tributaries in an "improved condition." The table will show the average of the expenditures obtained for the last thirty years:

Expenditures of the States of Mississippi, Louisiana and Arkansas on levees since 1849	$100,090,000
Expended by the government and private individuals—estimate	50,000,000
Damage by floods, ice gorges, etc., to levees, property, life etc.	80,000,000
Total	$230,090,000
Average per annum, $7,669,666.	
To this may be added 26,772,379 acres of land granted to the above States by the government in 1849, the value being about $10 per acre	267,773,790
Total	$497,813,790
Average per year, $16,000,000.	

Only those who are acquainted with the stream and its peculiarities have an idea how unmanageable it is. The unstable condition of the soil of the country through which it flows renders it an object of distrust to the inhabitants of its border. Such is the treacherous condition of its relations that for sixty-two years the ingenuity of man has contrived no check on its action. The causes of this condition of things are found partly in the river-bed. The sedimentary deposit varies from 60 to 100 feet in depth. It is generally composed of silt, with a mixture of clay and sand, which, having been deposited by the river, is at its disposal to lie still or be shifted about. It is evident that no ordinary construction can long stand unless it has a foundation penetrating this bed to a rock stratum. The great bridge at St. Louis, for instance, has its piers resting on the limestone bed-rock, under a sedimentary deposit of seventy feet. The railway bridge at the mouth of the Minnesota river has its piers lodged on a slender stratum of hard earth sixty feet beneath the river's bottom. It is further admitted that in boring through this stratum a soft layer was struck, which would not uphold the rod's weight. At Cairo, Ill., in 1877, the United States corps of engineers, under Lieutenant D. W. Lockwood, made borings to a depth of 87 feet without encountering any stratum harder than sand. At this point the machinery broke down and operations were suspended. At a depth of 33 feet the auger penetrated a cottonwood log, hardly ready to decay, showing conclusively the facility with which the river makes its own bed. At the same place it is stated on good authority that piles, one on another, have been driven to a depth of 125 feet without encountering a rocky stratum.

The story of its great width is even more remarkable. Near Cairo, Ill., the river moved a mile out of its course in one year, and is continually changing at that point. Still more remarkable are the operations of the Missouri river. At one time Council Bluffs enjoyed its presence in the immediate proximity, and the benefits of its commerce, in consequence of which the city became the terminus for Western railways in preference to Omaha, three times its size. These railroads erected depots and stationed offices of general Western superintendents there. The Union Pacific constructed an immense bridge, and in common with other railways built a union depot at the Bluffs. No sooner was the work completed, than the Missouri performed the rare feat of moving its course to Omaha, three miles away. There is no end to instances of this kind on a smaller scale. It may be safely asserted that from its narrowest point the Mississippi varies to twenty miles in width. It is no wonder, then, that the present embankment system is inadequate. Appropriations are only asked at present for embankments as far north as Cairo. It is evident, however, that the sedimentary bed extended nearly to the source of the Mississippi, and that not only must the 110 miles from New Orleans to Cairo be embanked, but also the greater shore line above the latte city on both this river and the Missouri. An explanation of the frequent destruction of levees, dikes and embankments is found in the method of their construction. When the current leaves the middle and runs along one side of the stream, the bank is rapidly torn down. At this point the corps of engineers proceed to build a dike to resist the destructive force. A rip-rap is first constructed which consists of a raft covered with long poles, placed cross-wise in alternate layers. This is loaded with heavy stones and sunk near the shore. Outside of it long poles are driven to a depth of twenty or thirty feet, and sometimes to twice these depths. Brush and stones are heaped upon their foundation until a perpendicular embankment is completed on a level with the top of the bank. One would think that this ponderous dike would stand for ages. But so vacillating is the silt bed underneath that the water keeps working the outer edge with powerful results. The embankment settles, sometimes toppling over, and again dropping suddenly from sight. Often the water works in behind these constructions and leaves them out in the stream. Thus it happens that the river is at work at innumerable points, tearing away its banks and defying the structures in use to hold it in check.

In its work of destruction the current has some formidable aids. In the winter ice floats down continually. So immense are these cakes at times that three, and even two coming down stream abreast will get caught on the sides of the river, in some narrow channel, and form a bridge. This bridge effectually holds back all oncoming ice. The great and small cakes coming down in large quantities join under, over and behind the bridge, piling up to a great height, forming a gigantic gorge. This mass finally breaks away; no power yet inaugurated by the hand of man is able to withstand it. Embankments, boats, live stock, people,

* Read before the A. A. A. S., Boston, 1880.

forests, houses, other property are borne down stream. One gorge alone has been known to sweep away $3,000,000 worth of property, besides making a tremendous destruction of life. A gorge will often require three days to pass a given point.

Another enemy to investigation and to embankments are the snags which infest the river. These, in their worst form, are large trees with roots and limbs. So rapidly are they loosened and borne down that the government is required to keep several snag boats constantly at work. Perhaps the greatest of all enemies to embankments is the period of high water. At this time most of the country adjoining the river, known as the "bottom lands," is flooded to a greater or less depth. This is a most dangerous period, the result of which is awaited with anxiety by land-owners involved. The various floods occurring since 1850, principally in that year, and those of 1864 and 1874, have carried away 200 miles of embankment between New Orleans and Cairo alone, which will cost the government alone $6,000,000 to repair.

There are many schemes offered for the construction of permanent embankments. Some are practicable to an extent, and others are but empty air. It is evident, however, that the government can never secure sufficient funds to inaugurate a system of embankments which shall have a foundation resting on the bed-rock below the river's bottom. Captain Charles M. Scott proposes a method which is, in brief, to weight and sink a reach of trees with their roots in such a manner as shall change or keep the current within bounds. A careful consideration of this method shows that after every high-water season these trees would be "reaching" in all directions along the river. Captain James B. Eads once proposed a system of ditches which shall narrow the river in wide places and compel the current to cut a deeper channel. As I understand this method, it is hardly practicable. There are other methods proposed. That of Captain Cowden is, perhaps, worthy of trial, though I am compelled to believe that it must be accompanied by a permanent system of levees. A very simple method, which has a semblance of practicability, is being experimented on near Omaha and at Nebraska City by Major C. R. Suter. An examination of this exhibits a simplicity which may circumvent the action of the water. No rip-rap is sunk and no piles are driven down. The sloping bank is covered with a mattress of brush. Stones are piled on this to a thickness of seven or eight feet, which holds the bank in its place and retains its sloping form. The water seems to have little inclination to work under this as in the case of a perpendicular embankment. I believe it is the invention, for the most part, of Professor L. E. Cooley, late professor of Engineering in the Northwestern University at Evanston, Ill., and now in charge of the works at Nebraska City, Major Suter also employs a simple and inexpensive method of changing the current of the river where it is wearing away the bank. A line is fastened to a buoy near the centre. Branches of trees are tied along one-half of this, leaving the other half bare. Anchors are attached at both ends of the rope and the half without bushes is run up the river as a guy, while the buoy holds up the centre of the rope at the surface. A line of brush then runs from the surface diagonally to the bottom. A series of these is placed out in the stream near where the damage is being done. The sediment coming down stream catches on the brush, sinks and forms a bar, and either breaks the force of the current or throws it out into the stream away from the endangered bank. This latter method has long been in use by the Corps of Royal Engineers with success.

Hegar's formula for an effective non-poisonous preservative and antiseptic is as follows:

℞ Salicylic acid	20	parts.
Boracic acid	25	"
Potassium carbonate	5	"
Dissolve in hot water	500	"
Glycerine	200	"
Then add oil of cinnamon, oil of cloves, each 15 parts, dissolved in alcohol	500	"

It is an exterminator of moths and vermin and has a pleasant odor.

ON THE NUTRITIVE VALUE OF FISH.*

By Prof. W. O. Atwater.

This paper gives the results of an investigation made under the auspices of the Smithsonian Institution and the United States fish commission. They included analyses of a large number of specimens of more common food fishes, whose details, though quite extended, were mainly of theoretical value. Some of the applications, however, were of much practical interest. In 100 pounds of the flesh of fresh cod we have 83 pounds of water and only 17 pounds of solids, while the flesh of the salmon contains only 66½ per cent. of water and 33½ per cent. of solids; that is to say, about one-sixth of the flesh of cod and one-third of that of salmon consists of solid, that is, nutritive substances, the rest being water. Lean beef, free from bone, contains about seventy-five per cent. water and twenty-five per cent. solids. The figures for some of the more common sorts of fish were:

	Solids. per cent.		Solids. per cent.
Flounder	17.2	Halibut, fat	30.7
Cod	16.9	Mackerel	22.2
Striped bass	20.4	Shad	30.7
Bluefish	21.8	Whitefish	30.4
Halibut, lean	20.6	Salmon	33.6

If we take into account not the flesh only but the whole fish as sold in the market, including bones, skin and other waste, the actual percentages of nutritive material, is, of course, smaller. Thus the following percentages of edible solids were found in samples analyzed:

Flounders	7.1	Shad	14.8
Cod	10.5	Shad	18.7
Mackerel	11.4	Lake trout	13.6
Halibut, lean	15.6	Salmon	25.6
Halibut, fatter	27.2		

The subject has of late attracted unusual attention. The chemico-physiological investigation of the past two decades has brought us where we can judge with a considerable degree of accuracy from the chemical composition of a food-material what is its value for nourishment as compared with other foods. The bulk of the best late investigation of this subject has been made in Germany where a large number of chemists and physiologists are busying themselves in the experimental study of the laws of animal nutrition. They have already got so far as to feel themselves warranted in computing the relative values of our common foods, and arrange them in tables, which are coming into popular use. The valuations are based upon the amounts of albuminoids, carbo-hydrates and fats, each being rated at a certain standard, just as a grocer makes out his bill for a lot of sugar, tea and coffee, by rating each at a certain price per pound, and adding the sums thus competed to make the whole bill. A table was given showing the composition of a list of animal foods. Thus it appeared that, while medium beef has about three-fourths water and one-fourth solids, milk is seven-eighths water and one-eighth solids. Assuming a pint of milk to weigh a pound, and speaking roughly, a quart of milk and a pound of beefsteak would both contain the same amount—about four ounces—of solids. But the quart of milk would not be worth as much for food as the pound of steak. The reason is that the nutrients of the steak are almost entirely albuminoid, while the milk contains a good deal of carbo-hydrates and fats, which have a lower nutritive value. According to the valuations given, taking medium beef at 100, we should have for like weights of flesh free from bone:

Medium beef	100.0	Bluefish	85.0
Fresh milk	23.8	Mackerel	86.0
Skimmed milk	18.5	Halibut	88.c
Butter	124.0	Lake trout	94.0
Cheese	155.0	Eels	95.0
Hens' eggs	72.0	Shad	99.0
Cod (fresh fish)	68.0	Whitefish	103.0
Flounders	65.0	Salmon	104.0
Halibut	88.0	Salt mackerel	111.0
Striped bass	79.0	Dried codfish	346.0

These figures differ widely from the market values. But we pay for our food according, not to their value for nourishing our bodies, but to their agreeableness. Taking the samples of fish at their retail prices in the Middletown, Conn., markets, the total edible solids in striped bass came to about $2.30 a pound, while the Connecticut river shad's

* Read before the A A. A. S. Boston, 1880.

nutritive material was bought at 44 cents per pound. The cost of the nutritive material in one sample of halibut was 57 cents, and in the other $1.45 per pound, though both were purchased in the same place at the same price—15 cents per pound, gross weight. In closing, Professor Atwater referred to the widespread but unfounded notion that fish is particularly valuable for brain food on account of its large content of phosphorus. Suffice it to say that there is no evidence as yet to prove that the flesh of fish is specially richer in phosphorus than other meats are, and that, even if it were so, there is no proof that it would be on that account more valuable for brain food. The question of the nourishment of the brain and the sources of intellectual energy are too abstruse for speedy solution in the present condition of our knowledge.

ANATOMY OF THE TONGUE IN SNAKES AND OTHER REPTILES, AND IN BIRDS.*

By Dr. C. S. Minot.

The tongue arises as a protuberance on the floor of the mouth, which in the course of development acquires a muscular system ; the latter appears first in the reptiles. The principal muscles are the longitudinal arising from the hyoid bones, morphologically a part of the branchial muscles. In the crocodiles these are the only muscles found. In the snakes, however, proper lingual muscles play an important part, there being a distinct vertical muscle between the *Ceratoglossi*, three distinct transverse muscles, one superior and two inferior, and finally a longitudinal muscle immediately under the upper surface of the free portion of the tongue. Each muscle is distinct and separate throughout its whole course; they can all be traced with facility. The disposition of the nerves and other parts of the tongue was also described. The examination of the tongue of an *Ameiva*, the common long-tail lizard, revealed a structure in all its features identical with that of the snake's tongue. This offers a confirmation of the view that lizards and snakes are closely related, for in no other class of reptiles has a snake-like tongue been observed. On the other hand, the tongue of the Chamæleon is peculiar. It has been previously studied by several authors, all of whom have committed important errors. The whole tongue is exceedingly complicated and difficult to understand. The arrangement of the muscles is the most remarkable yet observed among animals, and they cannot be homologized with the muscles of the tongue of any other animal, until our knowledge of the subject shall be greatly enlarged. Dr. Minot stated, while he had made new observations on the tongue of the chamæleon, that he had been led to recognize more clearly, than previous writers, the difficulty of explaining the mechanism of the organ. The tongue of birds presents a uniform type, distinct from that of any reptile. The tongue has its simplest and lowest form in the crocodiles, is much advanced in the snake and fissilingual lizards, remarkably transformed in the chamæleon, and presents a special type in birds. These points are brought out by numerous microscopical observations on the nerves, blood vessels and other parts.

SOME FACTS AND THEORIES BEARING A RELATION TO THE DISTRIBUTION OF ORGANIC FORMS ON THE GLOBE.†

By W. H. Davis.

The author commenced by pointing out the fact that the inorganic conditions which surround us are in a state of change, ceaseless, and ever varying ; and illustrated this portion of the subject by references to denudation and re-deposition of existing land surfaces. It was then shown that these inorganic changes could not take place without at the same time producing an effect on the organic world commensurate in some degree with the intensity of the inorganic change ; this led up to the question of the same area of the earth's surface at successive periods possessing a varying fauna and flora, and the light thrown by palæontological investigation upon the changes of land surface that had taken place, and this knowledge of past conditions in its turn throwing an instructive light upon the former range of the various orders and genera of organic beings. Thus it was, that as there was a perpetual ebb and flow and ceaseless interchange of inorganic structure, so the forms and types of life affected by these influences are also in a continual and corresponding state of unrest, from the necessity of the two conditions being in harmony with each other, the organic and the inorganic.

The first problem, therefore, was, seeing that a change of the organism was necessitated by a variation in the conditions of existence, whether these changed conditions as they arise were of themselves capable of inducing structural differences in organized forms subjected to their influences. Starting with the negative view, it was pointed out that there were but two courses open to the organism affected—migration or extinction ; but the former cause of itself involved a minor change of conditions, and as in the life history of the earth, a second, third, or greater number of migrations were necessitated, at last the probabilities were of the environment of the organism being so varied from its primary condition that extinction in this case must also ensue. Thus a form persisting through several or many periods of geological time would be impossible ; but as this was contrary to many observed facts, the converse view was discussed, and actual structural modifications due to changed conditions referred to, as in the case of animals and plants introduced into West Africa, South America, and other regions. Mimicry was also instanced as evidence of the influence of inorganic form on living organisms. In man the Europeo-American nation of the United States was quoted as an instance of a race being formed under our very eyes.

It may, of course, be urged that the differences here pointed out are only of a character such as might be anticipated to have arisen, and that, pendulum-like, they vibrate through a very small arc, and in no way give rise to fresh species, still less to fresh genera. The next point, therefore, that comes in for consideration is whether these structural differences are ever commutative. We have seen that the change which can be produced in a single species is not an alteration in respect of one character only, but an alteration of many characters affecting different parts and portions of the same organism. Now these modifications, small as they are (in comparison with the question of a complete change of species), certainly did not leap into being in an instant, but have exhibited themselves gradually. Here, then, is a starting-point for the cumulative evidence. The changes themselves, even so far as they have gone at present, are but expressed cumulative results, and having become once established, it is only in accordance with what we have already seen to be the case, that with a further change of surroundings, a corresponding modification must ensue, or extinction alone must follow. But in this argument we are not altogether left to the evidence as visible to the eyes of mankind during the historic period, but a mass of the facts of palæontological history, some embryological investigations, and many zoological observations are absolutely inexplicable save on these grounds. If we trace the connections of the reptilian and avian forms, the progressive stages in time of the Equidæ, or the changes in structure of the more lowly Ammonitidæ, the same answers must be given, that the extremes observed in the respective groups have been the result of a cumulative modification due to the types of life being in a condition of instability, and ever seeking to bring themselves into harmony with their inorganic surroundings.

In further illustration of this portion of the subject, sympathetic modification or correlative adaptation may be noted, as when the change of one structure in an animal induces changes in other structures remote and apparently unconnected with it, as in the pigeon, the beak and toe lengthening and shortening in unison.

Degeneration was strongly insisted upon as a factor in producing fresh types, equally with progressive modification.

Passing, then, to the various views entertained as to the causes of the present geographical distribution of life, the doctrine of specific centres was explained, the author maintaining that this idea was, in effect, but the old teleological argument that every organism was created for a definite

* Read before the A. A. A. S., Boston, 1880.

† Read before the Metropolitan Scientific Association, London, England, Oct. 12, 1880.

and fixed purpose; that it was specially adapted to its original design; and, finally, fixed where its adaptation had fullest scope. This view was strongly opposed by arguments based upon parasitism, showing that there had been a gradual variation in design as different circumstances arose, and fresh materials came to hand.

NEW SPECIES OF MOLLUSCA AND ECHINODERMS.

Professor A. E. Verrill describes in detail, in the last number of the *American Journal of Science*, many new species of Marine Fauna, discovered on the southern coast of New England, during the present season by the large party, under the auspices of the U. S. Fish Commission, of which Professor Spencer F. Baird is a Commissioner.

The following is a list of the new species, described by Professor A. E. Verrill and Mr. Sanderson Smith, with the exception of *Luidia elegans*, described by Perrier.

MOLLUSCA.

Heteroteuthis tenera, sp. nov.—*Calliteuthis*, gen. nov. *Calliteuthis reversa*, sp. nov.—*Alloposus*, gen. nov. *Alloposus mollis*, sp. nov.—*Cymbulia calceola*, sp. nov.—*Pleurotoma Agassizii*, sp. nov.—*Pleurotoma Carpenteri*, sp. nov. *Scalaria Pourtalesii*, sp. nov.—*Scalaria Dalliana*, sp. nov.—*Lamellaria pellucida*, sp. nov. *Lepetella*, gen. nov. *Lepetella tubicola*, sp. nov.—*Lovenella Whiteavesii*, sp. nov.—*Calliostoma Bairdii*, sp. nov. *Margarita regalis*, sp. nov. *Margarita lamellosa*, sp. nov. *Turbonilla Rathbuni*, sp. nov. *Turbonilla formosa*, sp. nov.—*Pleurobranchæa tarda*, sp. nov. *Philine amabilis*, sp. nov. *Diaphana* (*Utriculus*) *gemma*, sp. nov. *Doris complanata*, sp. nov. *Cadulus Pandionis*, sp. nov. *Loripes lens*, sp. nov. *Modiola polita*, sp. nov. *Pecten fenestratus* (?)

ECHINODERMS.

Asterias Tanneri, sp. nov. *Odontaster*, gen. nov. *Odontaster hispidus*, sp. nov. *Archaster Americanus*, sp. nov. *Archaster Agassizii*, sp. nov. *Luidia elegans*.

A POISONOUS PRODUCT OF FERMENTED INDIAN CORN.

If the grains of Maize, or Indian Corn, be subjected to fermentation, they become dark in color without changing form, and are found to contain, in considerable amount, a body which may be extracted by alcohol. After the removal of the alcohol by distillation, there is obtained a residue, from which, after long standing, an oil separates. This oil is brown in color, has a sharp, bitter taste, and a sp. gr. of 0.925. It forms soaps with alkalies, is soluble in alcohol and ether, and becomes resinous when exposed to the air. It acts as a poison on the animal system, and in certain other properties is very similar to strychnia.—Coeytaux, *Chemiker-Zeitung*.

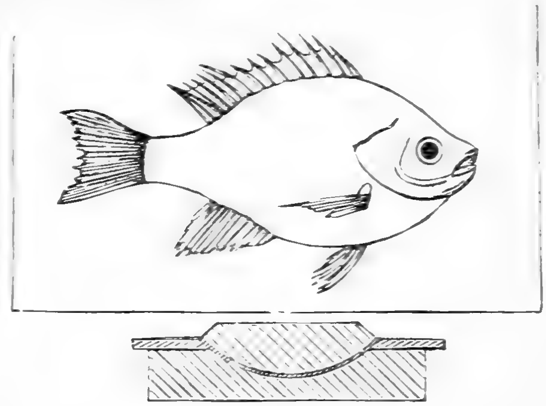
A PHYLLOXERA CONGRESS was held last month in Spain, at Saragossa, for the consideration of all topics connected with the grape Phylloxera. *The American Entomologist*, basing its opinion on the recovery of vines in Solano County, Cal., asserts its belief that the ravages of Phylloxera will have its day, and that from causes, not far to seek, the vine will again grow on the very lands which have been lately ravaged. For fear of the Phylloxera the Turkish Government have forbidden the introduction of any plants whatever into the territories of the Sultan.

IT is said that the Cochineal insect, which is a native of Mexico and Central America, thrives well in Florida.

CRYSTALS OF HÆMINE.—F. Högyes has examined crystals from the blood of men, oxen, swine, sheep, dogs, cats, rabbits, guinea-pigs, mice, pole cats, poultry, pigeons, geese, ducks, *Rana esculenta* and *temporaria*. All have one crystalline form only. They belong either to the monoclinar or triclinar system, probably the former.

TAXIDERMY.

Mr. Herman E. Davidson suggests an improvement in the art or method of mounting skins of fishes, which consists in forming a rigid mold of plastic material on the surface of the skin to be mounted before it is detached from the body of the fish, and thereafter removing the soft portion from the skin and stuffing or filling before it is removed from the mold, whereby the natural form and convexity of the fish are preserved.



This improvement may be understood by the annexed drawing. It will be seen that Mr. Davidson takes a moldboard having a portion removed corresponding with the outline of the body of the fish, exclusive of median fins, and inserts the body of the fish in the opening, the median fins resting against the face of the board, and forming a mold of plastic material upon the body of the fish projecting beyond the other face previous to removing the soft parts and stuffing the skin. The soft parts are then removed from the skin resting in the mold, and plastic material, adapted to solidify, is then poured in.

BOOKS RECEIVED.

THE JOURNAL OF NERVOUS AND MENTAL DISEASES, for October, 1880. Office No. 70 Monroe street, Chicago, and G P. Putnam's Sons, New York.

The opening article is by Dr. S. V. Clavenger, consisting of his paper read before the American Association for the Advancement of Science, entitled "Plan of the Cerebro-spinal Nervous System." An abstract of this paper was furnished to "SCIENCE," by Dr. Clevenger, and appeared in this jonrnal of the 11th of September last. Specialists should not fail to read the paper now presented in detail, as it forms an important addition to the literature of this subject. Dr. Edward C. Spitzka contributes two papers, the first a continuation of his "Contributions to Nervous and Mental Pathology," and "Contributions to Encephalic Anatomy." In the latter article Dr. Spitzka takes up the subject and methods of a study of the Ichthyopsidean brain. As we shall probably reproduce this article for the benefit of the readers of "SCIENCE," further reference to it at present is unnecessary. We cannot, however, refrain from expressing our satisfaction at finding that Dr. Spitzka continues to devote his attention to original research in this direction; our knowledge of human anatomy has been greatly extended by the investigation, of naturalists, into the lower forms of life, and if higher results are attained, it will be by such indefatigable and intelligent work as is manifested in this paper of Dr. Spitzka. The other articles in this number are, "Contributions to Psychiatry by James G. Kiernan, M. D. A case of Diffuse Myelitis, by Dr. J. C. Shaw, and Dr. John S. Woodside. A case of Acute Myelitis, by S. G. Webber, M. D., and a case of Meningo-encephalitis, by H. M. Bannister, M. D.

DO SHARKS SWALLOW THEIR YOUNG FOR PROTECTION?

To the Editor of Science:

SIR: In the New York *Tribune* for July 7, 1880, under the heading "A Female Shark Captured," was printed a statement made to a reporter by Mr. Eugene G. Blackford, well and favorably known to the community, not only as one of the largest dealers in fish at Fulton Market, but also as an enthusiastic co-operator with the United States Fish Commission. The essential parts of the statement are as follows: "A Porbeagle shark (*Lamna punctata*)* was caught off Great Neck, Long Island, and came into my possession not more than seven hours after death. From the immense size of her stomach I thought she must have swallowed a barrel or two of moss-bunkers, and to gratify my curiosity I opened her. I found ten little shark, evidently her offspring, and all just the same size—exactly two feet long. I should say they were about six months old, for a shark when hatched from the egg measures about four inches. It has been a disputed question among fishermen whether young sharks in time of danger do not seek safety in their mother's stomach. I think this case proves that they do, for the little ones were perfectly sound, with no mark of digestion upon them."

In answer to a request for more explicit information, Mr. Blackford, August 25, wrote me a letter from which, with his permission, I extract the following: "Just as the men were about to open the shark I was called away. When I returned they showed me the ten young, but I did not preserve them. The men said they were in the stomach, but the viscera had been thrown away, so I could not verify their statement. As I understand it, sharks are oviparous and not viviparous, so they could not have been in the womb. Will you kindly let me have your opinion on this? Should another specimen ever come into my hands I shall take great care that it is scientifically examined if you think it is of importance."

In reply, I informed Mr. Blackford that many species of sharks bring forth living young, and expressed the opinion that the little ones found by his men were really in the enlarged oviducts rather than in the true stomach, and suggested that he should publish some qualification of the original account, or authorize me to do so for him.

As time passed without my hearing from him, I concluded that he was preparing a note upon the subject, and hoped, moreover, that no journal under scientific supervision would give even a qualified sanction to the original statement without making enquiries like my own. Nevertheless, the paragraph in the *Tribune* was copied, substantially, into the *Scientific American* for July 31, and formed the basis of the following "Zoological Note" in the *American Naturalist* for October: "Mr. E. G. Blackford states that ten sharks, two feet in length and apparently about six months old, were taken from the stomach of a mackerel shark (*Lamna punctata*), as if they had got there to avoid danger. Still, it is probable that sharks may eat their young."

Upon hearing of this dissemination of the error, Mr. Blackford wrote me on the 19th and 28th of October: "I was under the impression that I wrote you before to make what corrections, you saw proper, to the shark story. I have given corrected statements to the *World* newspaper, and to *Forest and Stream*, and should be only too happy to have you make further corrections, as there is nothing I regret so much as to have anything purporting to emannate from me that looks like a yarn."

Availing myself of Mr. Blackford's permission I repeat, that, while it may not be right to deny the possibility of young sharks seeking refuge from danger—as do some snakes—by entering the mouth of the parent, the case in question furnishes not a particle of evidence in favor of that idea, and the little sharks were probably the unborn young, closely packed in the enlarged oviducts of the mother.

B. G. WILDER.

CORNELL UNIVERSITY, ITHACA, N. Y., *Nov.* 4, 1880.

* It is also known as the Mackerel shark, and has been named *Lamna spallanvanii*, and *Isuropis dekayi*.

A FINE BOLIDE.

By EDWIN F. SAWYER.

A bolide, the most brilliant I have ever witnessed, appeared on the evening of October 25th, at 8 h. 10 min. C. M. T. It commenced near ε Persei, at R. A. 55° + 40° Dec., and exploded above α Aurigae (Capella), at R. A. 76° + 52° Dec. When first observed it was as bright as a 1st mag. star, and of a deep orange color. As it slowly proceeded it grew rapidly brighter, (its color in the meanwhile changing to light yellow and then white) first exceeding ♃ (Jupiter) in brilliancy, and then ♀ (Venus), and at the moment of the explosion was many times brighter than the latter planet. Several distinct flashes were noted during its flight. At the moment of explosion (accompanied by a very vivid flash) the nucleus appeared somewhat elongated and of a pale green color, while the points of light and sparks radiating from it were of a beautiful red. During the first half of its course there appeared to be little or no streak accompanying the bolide; but during the latter part of its path a broad and intensely white streak was observed, which, however, disappeared almost immediately after the extinction of the nucleus.

The bolide was three and a half seconds in traversing a path of 17°, its velocity gradually increasing up to the point of explosion. No detonation was heard, although particularly listened for.

The radiant point of this bolide was somewhere along its apparent path prolonged backwards. This path when carried back passes a few degrees north of ε Arietis, and remarkably close to a radiant point of an important meteor shower, lately determined and particularly described by Mr. W. F. Denning, F. R. A. S., in the *Science Observer*, Vol. I, No. 7. This shower appears to endure from October 20th to November 13th, with a maximum on October 31-Nov. 4th. The meteors from this shower are generally slow moving and very bright. Two bodies, equal in brightness to Venus, were observed in England on the evening of November 4th, 1877, by Messrs. Wood and Backhouse, and traced to this radiant.

The position of this special meteor stream is at about R. A. 43° + 22° near ε Arietis. Several quite bright and short meteors (including one nearly stationary) belonging to this shower were observed by the writer from Nov. 7 to 19th, 1879, and giving a well determined position as at R. A, 47° + 24°. A rough sketch of the bolide as it appeared at the moment of explosion is here given.

Duplicate observations of this fine bolide are desired and would be of great value.

Cambridgeport, Oct. 31st, 1880.

SCIENCE:
A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.
Vol. I, No. 19. - - - - - - - - November 6, 1880.

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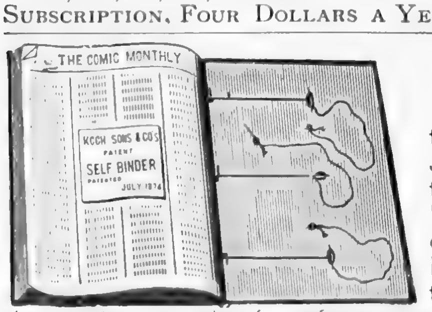

NOTICE.

Subscribers and Readers of "SCIENCE" are respectfully informed that we have made a contract for Portable Binding Covers for this Journal, with Messrs. Koch Sons & Co., who hold a patent for the best cover in the market. The usual retail price is a dollar, but if a sufficient number respond to this notice to enable us to order not less than 100, they can be supplied at 60 cents each, including the name of Journal stamped in handsome gold letters on the cover. As the convenience of these covers is very great, keeping the numbers together for reference, and preserving them for permanent binding, we desire all readers of "SCIENCE" to possess them; they will be delivered at the contract price without any profit on our part.

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SCIENCE

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

ILLUSTRATED.

Entered in the Office of the Librarian of Congress, at Washington, D. C.

Vol. I—No. 20. November 13, 1880. Price 10 Cents.

SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

Published at
229 BROADWAY, NEW YORK.
P. O. Box 3838.

SATURDAY, NOVEMBER 13, 1880.

At the request of Col. W. A. Ross, of England, we publish his open letter to Professor Sorby, who was President of the Chemical Section of the British Association recently, when that body declined to permit a paper, prepared by Col. Ross, to be read.

Col. Ross forwarded this paper to us, and we published it on the 16th ultimo, so that those who desire to judge of the propriety of its rejection, may form their own opinion.

A writer in the last number of the *Chemical News*, of London, a journal well able to appreciate good chemical work, who gives Col. Ross credit "for his interesting and valuable chemical researches," offers in detail an instance of their utility, and acknowledges that Col. Ross's two works, *Manual of Blowpipe Analysis*, and *Pyrology*, are the standard English authorities on this branch of analytical chemistry. It appears to be a strange state of things when such a man must contend against a system of repression and bitter antagonism from those following the same line of investigations, and would seem incomprehensible if similar cases were not continually coming to the surface; thetreatment of Prof. Mohr, and other instances mentioned in Dr. Akin's letter, which recently appeared in "Science," however, gives a key which solves much of the mystery.

We are not prepared to offer an opinion in regard to the dispute which gave rise to the letter of Col. Ross to Professor Sorby, but the mere fact of a man suggesting "boric or phosphoric acid as a fluid menstruum, instead of borax or microcosmic salt," hardly appears to justify this ostracism from the society of scientists, unless such an innovation is an indictible offense. We have heard of the consequences of speaking disrespectfully of the Equator, but we should have thought that the conduct of a man who insists on using "an aluminium plate" instead of "sticks of messey and obscuring charcoal," would arouse the compassion, rather than the resentment of his fellow chemists, if he be in error.

Seriously, we regret any obstruction to Col. Ross's work; when we consider that the studies which he so ably describes may be conducted with apparatus costing only a few shillings, and that results of the highest order in analytical chemistry may be arrived at, who cannot desire to see encouragement extended to such a practical scientific pursuit? We advise Col. Ross to quietly continue his work, and cease to notice any apparent opposition; if he is ignored by *authority*, let him on his part ignore *authority*, and trust to the sterling merit of his work for its ultimate vindication; his time is surely too valuable to devote to a useless correspondence.

We published, in our issue of the 23rd ult., a paper by Dr. George W. Rachel, claiming for the late Professor Friedrich Mohr, the honor of first publishing the now accepted principle of the *Conservation of Energy*. Like the original article of Professor Mohr on "*The Nature of Heat*," which was at first declined by publishers, this just tribute to his memory, penned by Dr. Rachel, was denied admission to the pages of the scientific monthlies. To-day we publish a later contribution from the same source, in which a biographical sketch of the late Friedrich Mohr is presented to the readers of "Science" by Dr. Rachel, who has compiled it from original papers placed in his hands for the purpose, by the trustees and family of Mohr. The author has accomplished his task with fidelity and moderation, and the authentic record he presents of a life of utility and self-sacrifice will doubtless be read with interest by our readers. In the *Popular Science Monthly* for July last, a short sketch of the life of Professor Mohr was produced, written by Dr. Fredrick Hoffman, of New York. The essay was brief. We are not aware of the extent of the materials which were at the command of Dr. Hoffman, who, while giving the highest praise to Mohr for his high chemical attainments, made the briefest reference to his claim of making the great discovery of the *Conservation of Energy*, which must forever link his name with physical science.

A meeting of the National Academy of Sciences will be held at Columbia College, New York, commencing on Tuesday, the 16th of November. We trust that the President, Professor William B. Rogers, who is at present sick, may recover sufficiently to preside at the meeting. As yet only seven papers have registered.

ON SOME NEEDED CHANGES AND ADDITIONS TO PHYSICAL NOMENCLATURE.

BY PROFESSOR A. E. DOLBEAR.

I.—Physics is now defined to be the science of energy. Previous to 1840 what was known concerning energy was embodied in Newton's Laws of Motion, and was confined to what we may call molar mechanics, to distinguish it from atomic and molecular mechanics, which has since that time been developed. Friction was looked upon as resulting in an absolute loss of energy, and no attempt appears to have been made to find it in other forms. Both Rumford and Davy proved, to their own satisfaction, that friction resulted in the *creation* of heat—an idea entirely different from the conception of heat then in vogue, that it consisted in imponderable corpuscles. No attempt was made to find the quantitative relation between molar energy expended and the heat produced, so that many years elapsed before any advance was made beyond the qualitative work of Rumford and Davy. Even for a time after Faraday's researches had established a quantitative relation between chemical reactions and electricity, the facts were looked upon as rather curious information, out of relation with physics proper, and so the latter was kept strictly what is involved in

$$\text{Energy } E = \frac{m\,v^2}{2},$$

the *form* of the energy being modified by so-called "Mechanical Powers," the lever, the pulley, the inclined plane, etc. Since 1840, however, through the labors of Mayer, Joule, Thomson and others, the quantitative relations between the various known forms of energy have been determined with great precision, and has led to a complete and inclusive generalization of the laws of energy—namely, that the energy in the universe is a constant quantity, the form that it may assume at a given time and place depending solely upon the other forms of energy which are present at the same place at that time. By the *form of energy* is meant *the character of the motion that embodies the energy*, for when there is no motion there is no energy, so that each different form of motion is a different form of energy. Rectilinear motion is a different form from rotatory motion, inasmuch as in rectilinear motion there is a change of position in space of the centre of mass, while rotatory motion does not involve such change, yet both embody energy though each in a special form and each should have a specific name.

Generically, all motion of translation in space is called mechanical motion or molar motion, and its energy, once called its *vis viva*, is proportional to $\frac{m\,v^2}{2}$, and is true for masses of all dimensions. Nevertheless, what a given amount of energy will do depends solely upon its form. Rectilinear motion cannot continuously produce rotatory motion; but vibratory motion can. For convenience in descriptive work as well as for clearness of conception—the latter of great importance—it is necessary to have specific names for the various forms of energy. As each form embodies a particular form of motion, one will only need to specify the various possible forms of motion in order to cover all possible cases. We have then the following table for such mechanical or molar motions:

$E = \frac{m\,v^2}{2}$ {
- Rectilinear, like a locomotive upon a straight track
- Rotatory, like that of a balance wheel.
- Vibratory, like that of a tuning fork.
- Curvilinear, like that of a projected cannon ball.
- Spiral (unusual), like some forms of projected rockets.
- Vortical, like smoke rings.

As the energy of each of these forms is expressed by the same formula there is no way of identifying either of them, except by some roundabout expression as "The energy of vibration," "The energy of curvilinear motion." It is true that for one of these forms we have a particular name, *sound*, for vibratory motion, provided its frequency is within the limits of hearing, but as the same name is applied to the sensation itself we are without a distinctive name.

II. If instead of large masses we consider atoms and molecules, it will be clear at once that the same *forms* of motions are possible as with the large masses, and the same general descriptive terms are applicable. Thus for an atom there is a rectilinear motion which we call its free path, but for its vibratory motion we use a distinct and and specific name, *heat*. Also for the rotation of the atom in its own plane, we have the specific name, *electricity;* for possible curvilinear spiral or vortical motions there are no names.

The energy embodied in atomic and molecular motions exclusive of rectilinear, that is, that do not involve a change of position of the centre of mass or of inertia of such atom is generally called *internal energy*, and if we let ε represent its value then the complete expression for the energy of the atom will be

$$E' = \varepsilon \frac{m\,v^2}{2}.$$

Now such changes and conditions as are involved in what we call *latent heat*, *specific heat* and *specific inductive capacity* are all involved in that factor ε, but the terms specific heat and latent heat are certainly misapplied, for whatever the forms of energy may be, they are certainly not heat, consequently not vibratory. Specific names then are needed for these.

III. The observed transferrence of energy from one atom to another without contact has necessitated the theory of the ether, and we know the rate of transmission of some forms of energy in this medium to be 186,000 miles per second. It follows then that energy resides in this medium in some form, and it is a matter of experiment to determine the particular form. Thus what is called light and sometimes heat is known to have an undulatory form, and the mechanism of the conditions may be easily perceived. Thus, let the dark ring V (Fig. 1), represent an atom of any matter, say hydrogen (the simplest form of vortex ring). Suppose it to vibrate its fundamental, then will the point *c* move over the line *a b*, and the circle will assume an elliptical form alternating with another ellipse with major axis at right angles to the former, in the line *a*, *b*′. The line *a b* represents the displacement of the point *c*, in other words, it is the amplitude of vibration of the ring. It is such vibratory motions of atoms that constitutes what we call heat, and we know furthermore that such vibratory motion sets up in the ether surrounding the atom-undulations which constitute what are called rays. Such undulations *c d* travel outwards in every direction, and the length *c d* is called a *wave length* $\lambda = \frac{V}{n}$, *v* being distance traversed during *n* vibrations, *v* being quite independent of the amplitude *a b*. As such displacement *a b*, whether it be small or large, sets up corresponding motions in the ether, it follows that any displacement of matter in ether, whether it be a part of an atom or the whole atom, that is, whether it be so called internal energy or external energy, will originate in the adjacent ether a corresponding movement, which will travel outwards with a velocity which will depend solely upon the translatory property of the ether. This property is sometimes called *elasticity*, but as elasticity is a property of matter and ether is not matter, and as the actual velocity of transmission is so many times greater than in any elastic matter we know, I prefer to say I don't know anything of the specific properties of ether, and do not say that it is even elastic. The undulatory motion in ether is utterly unlike the vibratory motion of the matter that originates it and it ought not to be called by the same name. Furthermore, as atoms differ in mass, so will their rates of vibration differ when

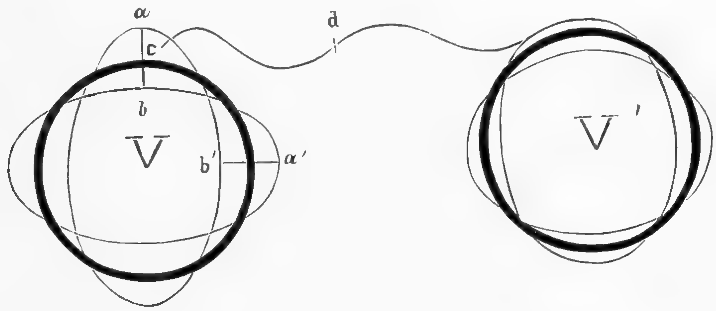


Fig. 1.

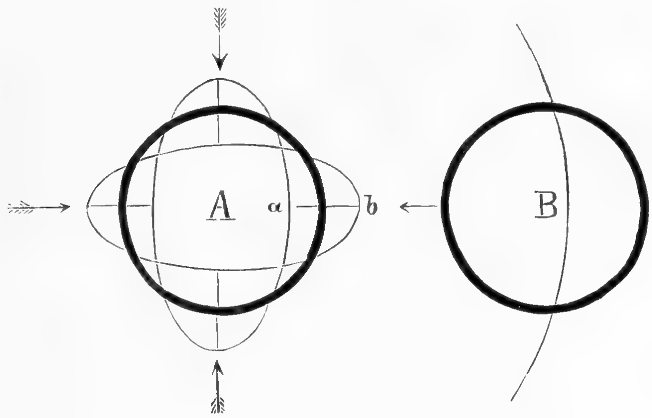


Fig. 2.

they possess the same absolute amount of energy. Velocity in this case will be equal to amplitude *a b*, the space point *c* passes over during one vibration. If *m* and *m'* be two atoms of different masses having equal energy of vibration, then,

$$E = \frac{m v^2}{2} = \frac{m^1 v'^2}{2} \text{ and } \frac{m}{m'} = \frac{v'^2}{v},$$

that is, the square of their velocities is inversely as their masses, so that wave length in the ether will vary as the mass of the atom. As such rays in ether vary only in amplitude and wave length, not in form nor in the medium, it is time to stop speaking of some of them as heat rays, some of them as light rays, and still others as actinic rays. These names characterize *effects*, not the rays themselves; what any one will do depends solely upon what kind of matter it falls upon. What we call light itself is purely a physiological phenomenon and does not exist independent of eyes, and it is hence improper to speak of the velocity of light, however convenient the expression may be. It is what produces light or heat or photographic effects that has velocity and this has the more appropriate name of *radiant energy*.

For a similar reason it is manifestly improper to speak of the temperature of space. Absolute space can have no temperature, for temperature is a function of matter. The temperature that a mass of matter would have in space must depend first upon its own constitution, and second, upon the number and wave lengths of the rays of radiant energy that fall upon it, and these would not necessarily be alike in any two points in space. Let V and V′ be two atoms at any distance apart, then if any ray from V falls upon V′, the latter will be made to vibrate provided its possible rate of vibration coincides with V, in which case it is a simple example of sympathetic vibration, the amplitude only being less than that of V. If its possible rate is not the same then it will not be vibrated by the ray; in other words it will not be heated by it and consequently it will have no temperature.

IV. Again, consider other physical conditions in and about a vibrating body. Bring any light body that is free to move in proximity to a vibrating tuning fork and such body will be apparently attracted by the prong and will stick to it while the vibrations continue. The average density of the air near the fork is less when it vibrates than when it is still, and consequently any object near it will be more pressed by the air on the opposite side than on the side adjacent to the prong. Precisely similar conditions are present with a vibrating atom. Let A (Fig. 2), be such an atom as before vibrating in its slowest period, *a b* will be the amplitude of vibration, then will there be a less density in the ether at each of the four extremes of the major and minor axes of the ellipses, and consequently a pressure at the four points in the direction of the arrows. The space within which the density is appreciably less may be called the *field* of the atom and if another atom B be wholly or in part within that field it will be subject to pressure towards A. If atom B vibrates synchronously with A there will be no more than a brief temporary disturbance when the two will adhere together by pressure from without and will then constitute what is called a molecule. If, however, the vibratory period of B is not commensurate with A's then after impact the two must separate, either to renew contact and recession or to bound away quite out of each other's field. The same may happen to two similar atoms when the amptitude of vibration becomes very great, they may bound quite out of each other's field, only renewing contact but not cohesion. This is called *dissociation*. This tendency to unite exhibited by atoms and explained as due to purely mechanical conditions was formerly called chemical affinity, but is now called *chemism*. The selective agency observed being due to relative rates of vibration, the possibility of uniting and the strength of the compact depending upon the harmonic relations involved. The motion set up in the ether at the parts of maximum displacement which results in chemism is different from the undulatory and may be distinguished from it as *pulsatory*.

If an atom spins upon an axis at right angles to its plane then any point c, (Fig. 1) in the circumference will be displaced the diameter of the atom every half rotation, and this displacement must set up in the ether a disturbance as great as though the amplitude of vibration had been as great as the diameter of the atom, but the motion of the point c being continuous and uniform instead of vibratory, the motion in the ether must be helical, the diameter of the helix at the atom being just equal to the diameter of the atom, but expanding outwards as a cone and is sometimes treated as a line or tube of force in treatises on electricity and magnetism. This motion in the tube will be right handed or left handed, depending upon which side of the atom the motion is traced from.

Now all the phenomena of magnetism tend to show that wherever it is present there matter is rotating in a plane at right angles to the direction of the magnetic axis, the magnetism being a form of energy in ether, being related to rotating atoms as undulations in ether are to vibrating atoms.

Lastly, there are many good reasons for the belief that matter itself consists of vortex rings of ether in the ether, and that they also embody a certain form of energy, which simply on account of its form is persistent, that is, unlike other forms of energy it is not exchangeable with them. In this view inertia is a law of energy and not a property of matter.

The following table gives a synoptical view of the various forms of energy and the names they have. Where there are no names an interrogation point is placed to indicate the lack. To the writer it appears as if each specific form of energy should have a specific name, but he is aware of the difficulty of finding suitable names and getting them adopted. If this want is felt by others then a committee of suitable persons might be appointed by the American Association for the Advancement of Science, who might consider and recommend appropriate names as did the British Association for Electrical Science some years ago.

TABLE OF FORMS OF ENERGY.

1. *Mechanical or Molar Motion.*

	Form.	*Name.*
$E = \frac{mv^2}{2}$	Rectilinear	?
	Rotary	?
	Vibratory	Sound.
	Curvilinear	?
	Spiral	?
	Vortical	?

2. *Atomic and Molecular.*

$E' = \varepsilon \frac{mv^2}{2}$	Rectilinear	Free path.
	Rotatory	Electricity.
	Vibratory	Heat.
	Curvilinear	?
	Spiral	?
	Vortical	?

3. *Atomic and Molecular.*

$E' - E = \varepsilon$	?	Specific heat.
	?	Latent heat.
	?	Specific Induc. Capac.

4. *In Ether.*

$E = ?$	Rectilinear		?
	Vibratory	Pulsatory	Chemism.
		Undulatory	Radient Energy.
			Pseudo: Light. Heat. Actinism.
	Rotatory, Curvilinear,	Circularly and Ellipticaliy	Polarized Light.
	Spiral		Magnetism.
	Vortical		Matter.

THE SPANISH MACKEREL AND ITS ARTIFICIAL PROPAGATION.

By CHAS. W. SMILEY.

This fish, *Cybium Maculatum*, is in general appearance very like the common mackerel. It is larger, however, averaging seventeen to twenty inches in length. When first described it did not exist in our waters, but was abundant in the Gulf of Mexico and the Caribbean Sea. Its first appearance was about 1850. It then began to be taken as a food fish. It began to be caught in the Chesapeake about 1870. About 1872 or 1873 it appeared in Narragansett Bay, when three or four hundred were taken at one haul of the seine, but the fish did not subsequently reappear.

The Chesapeake Bay has been annually visited by large schools for several years, where it is known as the "Bay mackerel." None were known to have been marketed there prior to 1870, but in 1879 1,300,000 of this fish were sold, and the season of 1880 is expected to yield 2,000,000. They were taken in pound nets and gill-nets. At Cherrystone, Md., there are fourteen pounds, which average a catch of 500 to a day. As many as 4,000 per day have been taken in a single pound on the eastern shore of the Chesapeake, while 2,500 is not a rare catch with one pound. The Bay fish are, however, smaller and leaner than those caught further north.

As this fish refuses the hook its capture is limited to pounds and nets. The first pound in the Bay was built in 1875. Now there are 164. The first gill-net used there was in 1877, while now there are 175 men fishing by this means. A net 100 fathoms long will average forty fish per twenty-four hours, the fish weighing from one and a half to two pounds each.

In the New York market the price per pound ranged from eighteen to thirty cents during 1879; for May, 1880, from fifteen to forty cents; but owing to the large shipments in June the price fell to ten to fifteen cents. On special occasions the fish have been sold readily at one dollar per pound. The catch of 1873 at Newport, R. I., was sold at prices varying from seventy-five cents to one dollar per pound.

This fish usually appears in the Chesapeake in May, when the temperature has reached 65° or 70°, and the number increases until the middle of June. They remain abundant until September, and diminish as the temperature of the water falls, until, in early October, nearly all have disappeared. They come in small schools, but later get scattered, and often quite isolated. Before leaving, the schools seem to be somewhat reformed.

The United States Fish Commission, under the management of Professor S. F. Baird, the Secretary of the Smithsonian Institution, has long desired to experiment upon the artificial propagation of this fish, but has been deterred by the lack of knowledge of its spawning time and places. These were both discovered June 1st by Messrs. Earll and McDonald, Assistants of the Commissioner. At that date the lower Chesapeake, especially Mobjack Bay, was found to contain large numbers of spawning mackerel. This opened the way for experiments, and Professor Baird was ready to seize upon the opportunity. He directed Mr. Earll to make every effort to hatch some fish.

June 21 Mr. Earll started for Crisfield, Md., on the eastern shore of the Chesapeake, and during the following ten days there conducted his experiments.

He found the number of eggs produced by a single fish to be from 50,000 to 500,000, according to the size of the fish, the latter number having been taken from a fish weighing one and three-fourths pounds. Instead of all the eggs ripening at once, as is true in the case of the shad, only a part are thrown at a time, and at intervals of a few days, probably extending through two or three months. This is analogous to the cod, which deposits its eggs at intervals during five or six months. Different individuals of mackerel were found to vary in their time of spawning; some ripening a considerable time before others, and the males seeming to ripen somewhat in advance of the females. From 40,000 to 130,000 eggs were obtained at one time from a single fish. The shad, however, yields only 20,000 to 30,000 as its fruits of an entire season. The cod, on the other hand, are so prolific that a twenty-one pound fish has yielded 2,700,000 eggs, and a seventy-pound fish has yielded 9,000,000 eggs.

When the fish had remained in the nets several days Mr. Earll found that the most of the spawning females had deposited all their ripe eggs. The greatest quantities were secured from individuals that had remained in the pound but a few hours. It is believed that when confined the female presses against the netting in its efforts to escape and produces an abnormal discharge of eggs; but it would result in the impregnation of a much larger number of eggs than would chance to be fertilized in a natural way. The males and females being caught side by side in considerable numbers, both eggs and milt would be present in the water in such quantities that they could not fail to come in contact before vitality is lost. A half hour after contact with the milt, the eggs swell and become too hard to be broken by pressure of the thumb and finger. Their specific gravity is now so nearly equal to that of salt water that when the water is at rest they float upon its surface, remain suspended in the water, or occasionally sink slowly to the bottom. The least current will cause them to be distributed through the liquid. Mr. Earll discovered a small oil globule in each egg which serves the purpose of buoying it. The impregnated egg is also so transparent that the fishermen, who are not usually very observing, would never suspect their presence. The eggs are smaller than eggs of almost any other species, and have an average diameter of only one-twenty-eighth of an inch. It has been estimated, it will be seen, that 21,952 would make a cubic inch, and a quart of 57¾ cubic inches would hold 1,267,728 eggs.

The period of hatching is greatly influenced by the temperature of the water. The average temperature during the experiments at Crisfield was 84° Fahrenheit. Ten hours after contact with the milt the outline of the fish could be discerned by the naked eye. The fish is formed with the curve of the back at the lowest point of the egg. In fifteen and one-half hours the fish began hatching. In eighteen hours one-half of the eggs had hatched, and in twenty hours all were out. Experiments in water at 78° Fahrenheit showed that twenty-four hours were necessary for hatching. A more remarkable effect of temperature is observable in the case of the cod. In water at 45° cod have been hatched in thirteen days, but in water at 31° fifty days were occupied in hatching.

The newly-hatched mackerel are about one-eighth of an inch in length, and so small as to escape through wire cloth with thirty-two threads to the inch, and are almost colorless. The food sac, situated well forward, is quite large in proportion to the body, the anterior margin extending to the lower jaw. While floating on its back for several hours, during its helpless condition, it passes safely over the heads of its enemies, and is protected from being wrecked in sand or weeds. After a few hours, becoming more vigorous, it gets to a depth of an inch or more below the surface of the water. After a day or two the food sac is less prominent, and the fish experiences less difficulty in swimming at various depths. The young mackerel hatched by Mr. Earll were so hardy that forty were confined in a goblet without change of water for two days before the first fish died; others placed in water which was allowed to cool gradually and immediately transferred to water ten degrees warmer, were not injured in the least. In fresh water they slowly sank and died in a few hours. Mr. Earll also found that a fair percentage of eggs could be hatched in still water with but one or two changes during their development. Eggs taken at 6 P.M., and allowed to remain in a basin of water till morning, when another change was made, hatched with very small percentage of loss. Samples of all the different stages of development were preserved in alcohol and glycerine for the National Museum. Over half a million were hatched by the various methods and at various times.

The apparatus used in these experiments consisted simply of floating boxes with bottoms made of wire cloth. The cloth was plated with nickel to prevent injurious action of the salt water, and contained thirty-two wires to the inch. After it was found that a lot of fish had escaped through it, only the shells remaining to prove that hatching had actually taken place, the wire and each aperture were covered with coarse cotton cloth. The boxes were provided with covers for protection against storms, or wind, or rain, but were provided with openings on the sides to admit fresh water from above.

The commissioner has been intensely gratified at these results due to the ingenuity of Mr. Earll. They open the way to the systematic propagation of the species in waters where they do not now exist, and to the countless multiplication of them in the Chesapeake. The season being in mid-summer will not conflict with the shad season of the Spring, the salmon season of the Fall, or the cod season of the Winter. The eggs are much more abundant and hatch more easily and rapidly than those of any fish now propagated. During the four days consumed in hatching a lot of shad, five lots of mackerel could be hatched, and during the twenty-four days necessary to hatch one lot of cod-fish, thirty-two lots of mackerel would be produced. A suitable station for hatching was chosen at Cherrystone, Md. The fishermen are kindly disposed and will render every assistance. It is hoped that young fish may be thus successfully planted as far North as Narragansett Bay.

SMITHSONIAN INSTITUTION, Washington, D.C., *November* 6, 1880.

THE ISLAND OF MONTREAL.*

By WILLIAM BOYD.

A considerable portion of the waters of the Ottawa, at the foot of the Lake of Two Mountains, divides on the Island of Montreal. The branch that is directed to the northern part of the island soon sub-divides on Isle Perrot. There rapids are in each of the sub-branches. The sub-branches encounter the St. Lawrence on its northern side at two points,—shortly after it leaves the Cascades Rapids and below Isle Perrot, from that island's inner shore. The waters of the St. Law-rence bound also, indirectly, the southern side of the Island of Montreal, flowing in the same river-bed with the Ottawa, but beyond or outside its stream. The water of the St. Lawrence is greenish, that of the Ottawa reddish-brown. The two rivers run side by side unmixed to the Ottawa's lowermost mouth, at the foot of the Island of Montreal; and thence onward in the same manner, with increased volume on the part of the Ottawa, to Lake St. Peter, where they finally mingle. If the Ottawa should cease to exist and the St. Lawrence remain, what is now the Island of Montreal would probably—from the high level of the then Lake of Two Mountains, and from a great fall which would, on the extinction of the Ottawa, take place in the St. Lawrence below the Cascades Rapids—be an island no longer; but if the St. Lawrence should disappear and the Ottawa remain, the Island of Montreal would continue to be an island still. Therefore the writer is of the opinion that the Island of Montreal is an island not in the St. Lawrence as has heretofore been held, but in the Ottawa.

FRIEDRICH MOHR'S LIFE AND WORKS.

By DR. GEO. W. RACHEL.

On September 28, 1879, *Prof.* FRIEDRICH MOHR, one of the greatest philosophers Germany has ever produced, died after a short illness at *Bonn* on the Rhine. He was born at Koblenz on November 4, 1806, and, therefore, at the time of his death, was nearly 73 years old. In spite of this advanced age, he remained active and bright almost to the very moment of death, dictating to his daughter Anna until within a few hours of it in his usual clear and coherent manner.

His father was a pharmacist and proprietor of one of the principal drug-stores of the town; he is described as having been unusually proficient in the arts of his trade, and an ardent lover of his special profession as well as of science in general. A wealthy man, comparatively speaking, he bestowed great care on little FRIEDRICH, the only surviving child of six. The opportunity offered to the sickly, quiet boy who had to be kept from school during the greater part of his boyhood, was eagerly taken advantage of by him. Test-tubes and retorts almost took the place of play-toys with him, and his involuntary leisure enabled him to lay the foundation for his future greatness, viz.: an ability for labratory work almost unsurpassed. Thus it was that his methods as well as many of the instruments and apparatus he devised, are found to-day in every laboratory and are used all over the globe wherever chemistry has an abode.

* Read before the A. A. A. S., Boston, 1880.

In spite of the loss of time, occasioned by his continued ill-health, his sharp, grasping intellect enabled him to pass with honors the examination for admission to university study (*Abiturienten-Examen*) at the comparatively early age of seventeen (1823). After five years of further practical work as an apprentice and clerk in his father's store and pharmaceutical labratory, he went to *Heidelberg* (1828) studying chemistry with GMELIN. After another lustrum spent in various studies, there as well as at *Berlin*, and finally at *Bonn*, he returned to his home at *Koblenz* (1833) with the degree of *Ph. D.** After having passed his *State's Examination*, he married, and had, at the time of his death, two daughters and two sons. The faithful companion of his life, an excellent wife and mother, also survives him.

In 1840 his father died and he then took charge of the inheritance faithfully, and for seventeen years conducted the *Mohren-Apotheke*, as the establishment was popularly called. In 1857, however, he disposed of it in order to devote, in retirement, his entire energies to scientific research. He did so for a period of about six years, and then had the misfortune to become involved in pecuniary difficulties arising from the failure of a manufacturing establishment belonging to his son-in-law, in which he had been special partner. The honest fulfillment of all his engagements cost him nearly all his fortune, and was the direct cause of his removing to *Bonn*, where he settled in 1864 as lecturer (*Privatdozent*). The chair of Pharmacy becoming vacant a year and a half after his settlement at *Bonn University*, he was appointed to the place. He owed this appointment to the direct influence of *Emperor* then *King* WILLIAM, and the *Empress* then *Queen* AUGUSTA. The latter always took a lively interest in his welfare, which dated from their long residence at *Koblenz Castle* before the Prince's accession to the throne. The Princess had always been fond of the great man's company, his conversational powers and his manner of reading being of an unusually high degree of perfection.** In a letter to the writer, his accomplished daughter, *Miss* ANNA MOHR, who acted as his amanuensis for many years, states:

" In our family circle where he felt himself surrounded by loving care, I have never seen him otherwise than happy and contented. Full of feeling and sparkling with humor, he always was appreciative of everything that is noble and beautiful in art and nature. Music and poetry were always especial favorites with him and while BEETHOVEN, MOZART, HAYDN and WEBER were his ideals in the former, GOETHE, SCHILLER, SHAKSPEARE, HOMER, etc., were to him as old acquaintances. His wonderful memory enabled him to recite for hours SCHILLER'S Ballads or his *William Tell;* GOETHE'S *Faust;* HOMER'S *Iliad*, and many, many other works of those and other poets. And not only was his recitation masterly and perfect, his reading power of serious, as well as of comical pieces was unsurpassed. . . . . He would at the same time master any dialect, new to him, in a few hours, and his many friends and acquaintances owe him many hours of bliss and happiness and many a pleasant evening."

After having thus fonnd a congenial sphere of action, his genius—no longer dragged down by pecuniary cares—attained full sway. In quick succession he published that series of, not very numerous, yet very important, works which will make his name immortal. His lectures also, those at the University as well as many others which he delivered in clubs and societies at *Bonn*, *Cologne*, *Koblenz*, *Krefeld* and other neighboring cities and his many contributions to scientific as well as other magazines and periodicals won him the hearts equally of his students and his lay-hearers. Of this the immense throng of people, belonging to all classes of society, that attended his funeral, was a sure indication.

But, in spite of all this popular recognition, he was not allowed to take that commanding position to which he was entitled by the superiority of his genius. We need only remind the reader of DR. AKIN'S letter published in No. 15 of " SCIENCE," to suggest the causes for the otherwise almost incomprehensible fact that MOHR remained an " *Extra-Ordinarius*" up to the time of his death. The reason is obvious. Even the Hohenzollern did not undertake serious intervention in his interest in regard to this matter ; for, although his loyalty and patriotism were proverbial, his radical views in regard to things theological which he always fearlessly confessed, and his unflinching attacks on erroneous views in science, regardless of what position those who proclaimed them might chance to hold, were sufficient causes for the failure of the powers that be to promote his attempts. He remained undisturbed because he recognized *authority* in matters political, but he was not promoted, because he did not feel bound by any *authority* in theological and scientific matters, unconditionally.

The *Emperor* desisted from interfering after experiencing a resistance on the part of " *Cultusminister* FALK " against MOHR'S promotion, which he could not overcome.

When MOHR settled at *Bonn University* as a *Privat-Dozent*, he was 57 years of age, *i. e.*, older by several decades than the average of his colleagues, being the senior of most members of the regular faculty themselves. But more than this, he had already at that time shown his inclination and his ability to reform, nay, to revolutionize some of the many branches of science which he mastered (theoretically and practically). This was more than mediocrity and even famous men are willing to endure. And to just such influences MOHR himself—who knew all the various intrigues against and reports about him, which he never raised his finger publicly to lay bare or refute—attributed the bad treatment which he received.

His eldest son, MR. CARL MOHR, an able chemist, and an accomplished contributor to scientific magazines, writes feelingly, about this matter, as follows :

"....yet it would be interesting to expose without fear or favor the dark doings of that '*official science*' of such men as ——, ——, ——,* etc. These men do not want to recognize anybody as their equal who does not sail under their colors ; followers and panegyrics are all they care to be surrounded with. But they hate and fear men of an independent turn of mind who dare have convictions of their own and dare express them, regardless of consequences to either themselves or others. Father has, for instance, by his sharp and telling hits of criticism in his Commentary to the '*Pharm. Bor.*' made enemies of the whole official clique of ——, ——, ——, and others at *Berlin*. Those men, instead being thankful to a man, far superior to them, who has pointed out errors, and shown how to correct them, have persecuted him to their hearts' content. When, therefore, the commission for the preparation of the '*Pharmacopœa Germanica*' was to be appointed, he was excluded from the list of commissioners intentionally and ostentatiously. The man who really was the *Nestor of Pharmaceutics in Germany*, author of such unrivalled standard works as his '*Pharmcopœa universalis*,' the *Commentary*, etc., above referred to, a ' Manual of Pharmaceutical Practice,'** a ' Text-book on the Art of Dispensing,'*** and others, was ignored insuch a disgraceful manner. It was a shameful performance, one that has no equal in the whole history of Science."

The narrative of these occurrences is one of the best illustrations of DR. AKIN'S views, as expressed in his letter to PROF. STOKES, alluded to before.

But, although the illustrious man was thus slightingly treated by men, generally far inferior, none of them superior to him, principally on account of his superiority and of the fact that most of his views and arguments were un-

* The honorary title of *M. D.* was conferred upon him in later years ; he also received the title of *Medizinalrath*, and was for a period of over thirty years the pharmaceutical adviser and member of the *Rhenish Medical Council*. He furthermore was elected corresponding or honorary member by several academies, numerous pharmaceutical and scientific associations in general, among the former being the *American Pharmaceutical Association* and the *Philadelphia College of Pharmacy*.

** He at this time delivered a course of lectures to *Princess* AUGUSTA at his house, comprising experimental chemistry and applied mechanics (models of steam-engines, etc., being prepared for this special purpose), the *Prince* and his eldest son (now the *Crown Prince of Germany*) attending when they stopped at the Castle.

* The list of names—I am sorry to say—embraces some of the most renowned professors at *Bonn* and at *Berlin*. MOHR'S intention was, as I am informed, to give a detailed account of the various intrigues against him in a work he was about to publish, when death overtook him ; to accuse his persecutors and enemies, and lay at their door the guilt, of having deprived him of due recognition and promotion to the place and honors of an '*Ordinarius*,' and to justify before the public his conduct of not having until then stooped to answer and refute the indignities thus heaped upon him. G. W. R.

** *Lehrbuch der Pharmaceutischen Technik* ; published in *six* editions (first, 1846) trveral times translated into French and English.

*** Rezeptinkunst.

answerable, * he was highly esteemed by the independent members of the profession at large in Germany. The last meeting of the '*German Apothecaries' Association*' which he attended, reading the inaugural address, was made the occasion of quiet and impressive ovations which the modest man received with deep feeling. The kind and respectful regard with which the venerable scientist was treated by almost all the members present, was often referred to by him in his family-circle with pride and satisfaction during the twelve months which were still allotted to him. He felt at that day, if ever, that he had not lived in vain, and that the seed which he had sown would not be lost, but that coming generations would yet profit from and be benefited by it.

Besides the works mentioned, he wrote the following: *The Mechanical Theory of Chemical Affinity, etc.*;[1] *General Theory of Motion and Force as a basis of Physics and Chemistry*:[2] *Manual of volumetric Analysis*;[3] edited five times, and last, but not least, a *History of the Earth; Geology founded on a new basis*,[4] which was edited twice.

Of the various improved analytical methods devised by him may be mentioned the proposal to use oxalic instead of sulphuric acid (GAY-LUSSAC), to determine the relative proportion of alkalies and acids contained in a salt; his combination: Sodic Carbonate against Iodine-solution; or, better still, Sodic Hyposulphite against Iodine-solution, and his beautiful determination of Chlorine, by the use of Argentic-Nitrate-Solution, with Potassium Chromate as indicator; of the many instruments invented by him, the self-acting stirring apparatus, with clock-work arrangement, a pill-machine, an apparatus for preparing infusions by the use of steam, another for extraction by means of ethers, and his improved burette, with compressing faucet. His *Manual of Volumetric Analysis* in which these devices and many others of like importance are described, is considered one of the first standard works in the domain of analytical chemistry, and has been translated into various languages.

The attempt, first made by him in his *Mechanical Theory of Chemical Affinity*, to promulgate the theory that chemical affinity is a mode of motion, inherent in matter, and is measurable only in so far as we can measure the heat that is liberated and bound up during the union or separation of two elements, is one of his greatest efforts. LIEBIG † himself always valued this work very highly, and it is certainly one of the most brilliant manifestations of MOHR'S genius, as will be seen from the following extracts:

"The union of two bodies by combustion always liberates a certain portion of this motion which appears in the shape of heat. Another portion remains in the product of combustion. We are only able to measure the former, not the latter, and even of the former we are unable to say how large an amount is due to one body and how much to the other. If one gramme of hydrogen unites with eight of oxygen, 34,462 units of heat are liberated. These indicate the amount of motion which both gases contained when yet ununited, as compared with the water resulting from the union. In the latter itself there is yet a certain amount of motion, as its liquidity and its proneness to vaporization successfully prove. From the oxygen contained in the water we are able to liberate yet another amount of heat by uniting it with potassium or with zinc, because potassa and zinc oxide are more apyrous than either potassium or zinc. Now, it is impossible to know what portion of the 34,462 units of heat comes from the hydrogen, and which from the oxygen, and furthermore, what amount there is yet in the water. Therefore, we cannot reduce to an absolute measure this property of chemical affinity, only the portion that is liberated in the shape of h at.

"This example also shows how enormously more efficient the motion that bodies contain as chemical affin'tv, is than that which they contain as heat. The water produced (9 grammes) contains 9 u. of h., while the mixture of both gases before union, contained only 2¼ u. of h. (the specific heat of hydro-oxygen-gas being 0 25). But, since there was a development of 34,462 u. of h. during their union, by combustion, it follows that the motion existing in the mixture as chemical affinity, is 15,316 times that contained in it as heat."

Again:

"If iron develops heat while oxidizing, the dense condition of the oxygen in the resulting ferric oxide is certainly an effect due to chemical affinity, but the potential energy of the oxygen is no longer found in the oxide; it has been separated. The heat liberated during the combustion of the iron in oxygen-gas is the surplus of motion which iron and oxygen contain more than Ferric Oxide."

And again:

"If carbon and oxygen unite to form carbonic acid, there is no change of volume, and so it is with a mixture of chlorine and hydrogen. Their specific gravity after chemical union is the same as before such union took place, because their volume remains unchanged; yet a great amount of heat has been liberated.

"Thus it is not true, as has been formerly assumed, that we may compare—for the purpose of measurement—chemical affinity to mechanical force by calculating the amount of force necessary to compress a mixture of gases so as to give it the density possessed by the product resulting after chemical union has taken place. (Hydro-oxygen-gas against water."

The quintessence of this unique volume is contained in these two theses:

1. Loss of chemical affinity or liberation of heat always means: *Higher specific gravity, higher melting point, higher boiling point, insolubility, chemical indifference, rigidity and development* of but *little heat* on combustion.

2. Increase of chemical affinity and absorption of heat always mean: *Lower specific gravity, lower melting point, increasing solubility, proneness to chemical combination, softness, development of much heat* on combustion.

Mr. CARL MOHR, in a biographical sketch of his father's life and works, says of them:*

"These two axioms comprise almost the whole range of chemical processes, and they are a *mechanical theory of chemical affinity*, in the very same sense that we have a *mechanical theory of heat.*

"As an example to illustrate the first thesis, the reader needs to be reminded only of the chemical relation that exists between acids and bases: sulphuric acid against alkalies, such as caustic potassa or quick-lime. The process of saturation is accompanied by the liberation of considerable heat; the products have a very high melting point and are chemically indifferent.

"To illustrate the second thesis, a good example is the formation of carbon sulphide. As is well known, this process requires a considerable expenditure of heat, and the product thus obtained is volatile at a temperature far below the degree of temperature required for its formation. This heat, taken up by the carbon sulphide, is contained in it as chemical affinity, and is evident by its low specific gravity, its low boiling point, its proneness to decomposition, and the increased development of heat during the combustion of its elements."

The great principle underlying all this reasoning is that of "*the conservation of energy and the correlation of forces*," of which, as we have shown, he was the first exponent.†

[1] Mechanische Theorie der chem. Affinität; Braunschweig, Fr. Vieweg & Sohn; 1868.

[2] Allgemeine Theorie der Bewegung und Kraft, als Grundlage der Physik und Chemie; Braunschweig, Fr. Vieweg & Sohn; 1869.

[3] Lehrbuch der chemisch-analytischen Titrirmethode; Braunschweig, Fr. Vieweg & Sohn; 1855 (1877).

[4] Geschichte der Erde; eine Geologie auf neuer Grundlage; Bonn, M. Cohen & Sohn; 1866 (1875).

* Many of the suggestions contained in his earlier works were made use of in the '*Pharm. Germ.*' by the very men who were his life-long enemies, because these and other views were freely and sometimes sharply expressed by MOHR. Many of his original ideas on other subjects than *Pharmaceutics*, especially those on *Geology*, were also confirmed by later evidence; but this equally did not serve to reconcile his opponents in this line of research which comprised nearly all '*official geology*' in Germany.

† At the eve of its publication, the great chemist wrote to MOHR as follows:

"I am impatient to see your new book, for you seem to have treated in it of nearly everything that we need to know something definite about, in order to make chemistry a real science; nobody as yet has really had a clear conception of affinity; we simply stuck to facts, that was all. It has been just so with the melting-point, the gaseous condition, the boiling-point, etc."

* *Archiv der Pharmacie*; Vol. 216, 1880.

† See the paper published in No. 17 of "SCIENCE."

The work, as may be gleaned from the few quotations given above, abounds in a variety of new and original ideas, many of them elaborated, others rather fragmentary, but all of them bearing the stamp of the author's genius and containing an inexhaustible source of information and elucidation on the somewhat abstract subject of chemical theory. It may be added that MOHR does not approve of the modern valuation of atoms, but uses exclusively the old formulæ.

The *Allgemeine Theorie, etc.*, is written in the same vein, in fact it is introduced by the author as a supplement to the former, the 'Mech. Theory of Chem. Aff.' Its purport is to lay out the different regions of physics as far as they relate to motion and force; it also gives an outline of the method by which the correlation of forces and especially its grandest practical achievement, the 'Mechanical Theory of Heat,' should be made the basis of natural philosophy.

MOHR insists particularly on the complete eradication of the wrong use of the two words: motion and force: "Motion (*actual energy* of the English writers) implies a change of place, and consequently force (*potential energy*) comprises those states in which a change of place does not obtain." (l. c. p. 12).

"Steam force is therefore a correct expression and designates the tension (potential energy) of the steam. As soon as the piston moves, the tension disappears (potential is converted into actual energy). In the flying wheel and the balancier we have motion (actual energy) and *not* force (potential energy) when 'running empty,' but motion *and* force, when there is some work done at the same time" (Ibid.)

The translation of these few passages will suffice to show the principal aim of the author: To obtain perfect clearness of expression by distinguishing between the two forms, to either of which every '*causa efficiens*' in nature belongs, viz., force (potential energy) and motion (actual energy). Prompted by the same desire, he makes these divisions in classifying those '*causæ efficientes*,' to wit:

"A. MOTIONS.

1. *Motion of bodies* or *progressive motion.*
2. *Light* and *Heat* in the state of radiation or *radiating motion.*
3. *Common, conducted heat* or *conducted motion.*
4. *Galvanism* or *flowing motion.*
5. *Chemical Affinity* or *clinging inherent motion.*

B. FORCES.

1. *Gravity.*
2. *Magnetism.*
3. *Electricity.* (*Static.*)
4. *Cohesion.*"

This division is based on the property of introconversion which is peculiar to the former, not to the latter. The five 'motions' are introconvertible and also convertible into either of the latter (the forces), while these are only convertible into the former.

The reason for his peculiar view that *heat does exist in two different forms of motion*, he states in these words:

"After due consideration of the matter in question, I feel compelled to separate *radiating* from *conducted heat* Radiating heat is not really heat: it does not expand bodies; it does not act on the thermometer nor on the thermo-electric column. That is what conducted heat does. The fact that radiating heat is converted into conducted heat, as soon as it strikes a body which does not reflect it, is no reason why we should declare the heat-rays to be heat already, for the luminous rays and the galvanic current must undergo the same change (before being capable of acting on the thermometer or on the thermo-electric column.) The mode of motion obtaining in the heat-*ray* differs so radically from that in *conducted* heat, that the separation adopted above seems to me fully justified. The circumstance that the heat-ray does not penetrate the different fluids of our eyeball, or if it do penetrate them, is not transmitted through the optic nerve, constitutes only a mechanical difference as compared with the visible ray of light, and we justly conclude that this depends entirely on their different wavelength."

The manner in which the various modes of motion and of force are measured by means of the application to them of the mechanical theory of heat, is particularly dwelled upon. From this it appears that of the *five* modes of motion, *two* only—mechanical motion (of bodies) and heat are reducible to absolute measurement. These two may be compared by means of the unit of heat, expressed in kilogrammometers.

Another line of thought which MOHR has first dared to follow and to pronounce upon, is the one dealing with the real value of the use of mathematical expressions in natural philosophy.

He holds that the value of mathematics is only secondary, and is, as a rule, greatly overrated. He says:

. . . "For, such propositions are proved by logical reasoning only, not by applied mathematics. A formula is nothing but the mathematical expression of the true inwardness of certain phenomena, as it has been previously found by intellectual observations and reasonings. What is obtained as a result through the formula, is in it from the beginning; it is not at all a discovery made by the mathematic an. If the first equation is wrong, the conclusions arrived at will be wrong. The ancient mathematicians who knew nothing of algebra, logarithms and the differential calculus, had to reason logically exclusively; even the lack of the Arabic numerals was a great drawback to them. That we are able, by the help of all these advantages, to deduce quicker and more accurately a number of relations from a given equation, only diminishes the intellectual merit of our work but not the practical value of the result. Mathematics have only one object, to wit: to evolve from certain given conditions the unknown quantity."

It cannot be denied that there has been, of late, an obvious tendency to overestimate the value of mathematics in the philosophical investigation of physical problems. And it is not the least of MOHR's many merits that he has conclusively shown that their importance is secondary only, and that their intrinsic value is far below that of logical deductions. "A mathematician can never discover a new law in physics; that is done by observations only and by the logical reasonings, based on such observations."

By thus defining the proper limits and the legitimate domain which mathematics should occupy in natural philosophy, MOHR has certainly rendered an invaluable service to numerous students of nature. Great numbers of such have been frightened by the many hundreds of pages filled with mathematical formulæ, with which we now see treatises on physics so profusely interspersed.

A perusal of this somewhat fragmentary work does not fail to leave the impression in the mind of the reader what a great misfortune it was that the author should have been called away before he had given us a more complete and comprehensive treatise on the subject; numerous requests addressed to him with this view were invariably met, as we are informed, by a modest decline, on the ground that much was to be done before this could be ventured upon.

His immense capacity for progressive thought is best illustrated by quoting from one of the "*Appendices*," happy after-thoughts, to this work. There he says:

"In determining the conception of the word *force* in contradistinction to *motion*: gravity, cohesion and magnetism seem to be without any inherent motion, as we have no indication that there is such motion. On the contrary, the tension of compressed air, the action of an explosive mixture, etc,, appear in the form of inherent molecular motion, which could appear dead only in comparing it to mechanical motion (of solid bodies). In a compressed gas there is perceptible heat and chemical affinity (motion). The perceptible heat acts on the thermometer only, but not as a force on the walls (of the enclosing vessel), while that amount of heat which is available for expansion, and which, while the (movable) wall of the enclosure (piston in cylinder of steam engine) is receding, is actually used for expansion, implies tension directed outwardly. Since, therefore, we recognize in this the effects of actual energy (*motion*) as being of the same nature as those of potential energy (*force*—gravity, for instance), acting by pressure or tension (supported or suspended weight), the question arises *whether magnetism, gravity and cohesion are not different forms of an inherent molecular motion of which we have, as yet, not the slightest conception.*"

This idea has occupied his mind during the last years of his life particularly, and he has published one of a series of articles* on the subject of cohesion and the atomic theory, the latter of which he attacks with all the acuteness we are wont to find in his writings.

In regard to MOHR'S *Gesehichte der Erde*, which he rightly calls: *Geology on a new basis*, it is impossible to say little, if it should be spoken of at all. Suffice it, however, to state that it is not what is usually called *Neptunism* what MOHR advocates; it is free from any kind of "ism," as the man who wrote it himself was. But in the generally accepted plutonistic geology, fire plays such an omnipotent *rôle*, that any deviating view, any appreciation of the true state of things geological, is believed to deserve that name from the outset. The so-called Neptunism of MOHR has, however, nothing in common with the impossibilities of the Old Neptunistic School of WERNER and his followers.

There is this radical difference between the views, as yet generally accepted, and those advanced by MOHR:

His aim is to show *in what manner* the several minerals and rocks, composing the outermost, accessible strata of our earth, have been formed; the *question of time* he considers to be not only irrelevant, but also very difficult of ascertaining. His firm conviction of the falsity of the nebular hypothesis and the previous fluid state of the earth and the present molten state of its interior has been strongly supported of late, not only by other *savants*, but by a number of facts and reasonings. The foremost among the former is embodied in the results of careful measurements of temperature in the deep boring (4052') at *Speremberg*, near *Berlin*, conducted by the most learned and experienced mining officials of the Prussian Government. They show that the old supposition of 1° C. increase of temperature to each 100' is fallacious, and that this increase diminishes constantly in going down. Here is the table:

Depth.	Temperature.	Increase per 100 ft.	
700 Rh. ft.	15 654° Réaumur	1.097° R.	2.468° F.
900	17 849	1.047	2.356
1100	19 943	0.997	2.243
1300	21.937	0.946	2.128
1500	23.830	0.896	2 018
1700	25 623	0.846	1.904
1900	27 315	0.795	1.789
2100	28 906	0.608	1.368
3390	36 756		

While for the lowest 100 feet it was: 0.445° R. (1.001° F.)

"This would make the final result, as stated by MOHR," says Dr. KLEIN,* "very well founded, nay, indisputable, viz.: 'that the increase of temperature *will cease altogether;*' not at a depth of 5000' or 6000', as MOHR will have it, but, at all events, at a depth considerably below 100,000'."

Thus, in addition to the many astronomical, physical and chemical reasons, which are irreconcilable with the nebular hypothesis and with the theory of the molten interior of the earth, derived from this hypothesis, MOHR has pointed out an irrefutable fact which supports this position.** For, continuing the slight increase found in a proportionate ratio, at the point where there will be no more increase, the temperature actually obtaining will fall much below the melting point of lead; this had been predicted by MOHR on other grounds, before the measurements made at *Speremberg* had been published.

Chemistry, especially, is ingeniously applied to geology in MOHR'S work, and from the chemical constitution and physical properties of the various minerals and the rock-formations which they compose, conclusions are arrived at which throw a new light, in many cases, on the probable origin and subsequent metamorphosis of the various component parts of the surface of our globe. To mention only one important result, we may refer to the different properties of the Silicate rocks having volcanic origin, as compared with those having, until lately, been supposed to have a 'plutonic' origin. The several properties which the one kind possesses are not found to be properties of the other, and MOHR, therefore, takes strong ground to deny their fiery origin.

Among these properties, demonstrating their crystalline origin, may be mentioned the following:

1. That Feldspar, Augite, Hornblende, Mica, Rock-crystal, Quartz, etc., as well as the rocks which are composed of these minerals, all show minute cavities containing water which are not found in rocks that have undoubtedly been molten, such as Obsidian and other lavas.

2. That the specific gravity of the silicic acid contained in them is greater (2, 5—2, 6) than that in volcanic rocks (2, 5—2, 3).

3. That by melting these crystalline rocks (Basalt, Granite, etc.,) their specific gravity is reduced in the same proportion.

4. That petrified wood, fossils, and other pseudomorphoses of organic origin, and even organic matter unchanged (Asphalte in Granite), is found to be enclosed in them.

5. That the crystalline rocks decay much easier and quicker than the volcanic molten rocks.

6. That many chemical actions, combinations, etc., would be impossible if a molten condition was presupposed, etc.

These and other properties of less importance undoubtedly form a strong array of proofs against the '*plutonic*' or fiery origin of the Silicate Rocks.

The formation of lime-deposits takes place according to MOHR in the ocean by the following bio-chemical processes:

The sulphate of lime contained in the sea-water is assimilated by the marine plants (*Algæ* and *Fucus* especially), and by them decomposed in the course of their vital processes. While the sulphur enters into albuminous compounds, the lime unites with carbonic acid, and both go to form part of the plant itself. The plants serve as nourishment to the myriads of minute animals (*Rhizopoda* and *Foraminifera*), which populate the oceans, and while the carbonate of lime serves to build up their shell, the sulphur is eliminated by their bio-chemical process as sulphurous and hydrosulphuric acid. These shells which lie from 10 to 15 feet deep at the bottom of the oceans, are the chalk and lime-beds of the future.

The presence of organic matter in meteoritic masses and the absence of carbon in meteoritic iron are pointed out by MOHR as further proofs against the plutonistic and in favor of the crystalline origin of the heavenly bodies. The chapter on these mysterious visitors from the celestial spaces is the longest and certainly one of the most interesting.

His views, especially in regard to the constant metamorphosis of rock-strata, are in fair way of becoming generally accepted—although his name is not as yet mentioned in connection with them.*

Another theory which refers the formation of coal-fields to the deposition of immense masses of sea-weeds and tangs at the bottom of the ocean, has been greatly supported by the discovery that Iodine and Bromine are regularly found in the smoke-black from chimneys where coal is burned.

The book abounds in new and original researches as well as in bold deductions; and even those who do not agree with the author will find in it an almost inexhaustible source of information, and will experience that great delight which the writings of a great thinker always give to an impartial reader.

In conclusion, it may be safely said that MOHR belonged to those whose writings and the results of whose labors will not lose interest as time passes, but will rather be more and more generally appreciated. He has said or written but very little that he was forced to revoke; on the contrary, many of his views have stood against the attack of time and of his adversaries, and many of the latter have been forced to admit that he was right.

Personally, as has been mentioned already, FRIEDRICH MOHR was the most amiable of men, and the Editor of the '*Gaea*,' DR. H. KLEIN, who was a near friend of his during many years, rightly says of him (in a private letter to the writer):

"In every respect MOHR was a man who would be an ornament to any period of Human History."

* LIEBIG'S "Annalen;" Vol. 195, 1879; pp. 133-213.

NOTE. The rest of this series which the editor of the *Annalen* declined to publish, are those mentioned in the foot-note on p. 203 of No. 17 of this journal.

* *Die Fortschritte der Geologie*, 1874-'75; *p.* 57.

** In a future article this subject will be exhaustively treated, and the position, taken by VOLGER, MOHR and RADENHAUSEN in regard to the nebular hypothesis, as well as that of *Sir* WILLIAM THOMSON POULET, SKROUP, STERRY HUNT and others in regard to the Earth's molten Interior, will be thoroughly treated of and criticized. A letter, also, from the pen of one of America's first astronomers will be quoted, in which this eminent scientist states his views on the subject in a manner which adds the additional weight of his superior authority to the evidence adduced against this Hypothesis. G. W. R.

* Strata of Shales, Mica-Schist, Calcareous Schist and Gneiss not infrequently are so uniformly spread out, in the same locality, that there can be no doubt of their common origin.

CORRESPONDENCE.

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

PYROLOGY.

To H. C. Sorby, Esq., F.R.S., LL. D., etc.

My Dear Sir,—As you are the only scientific Englishman of note who seems to have studied blowpipe crystallizations with the view of applying your observations geologically, I will not apologize for addressing you on this interesting subject, and I do so publicly for the following reason, which I think ought to be made public:

The last ten years of my life have been wasted in vainly trying to show that blowpipe chemistry, if studied on the rational basis of ordinary chemical analysis, *i. e.*, employing acids or alkalies and not salts, in the first place, as weapons of attacking substances, will inevitably lead to new discoveries and most interesting results in what scientific men of all branches are agreed to term their common pursuit—the revelation of Nature's secrets,—but my humble efforts have been met, in England, not merely with what Mr. Crookes has called "the conspiracy of silence," but with the most determined, if not underhand, opposition. Indeed, a foreigner considering the circumstances related to him, in which neither my purse, nor time, nor mental efforts, have been spared—hitherto, only to my own disadvantage—said "it appeared more as if I had been trying to commit a felony in England than to advance science there." I readily admit that the three exceptions to this category of eminent scientific enemies constitute a trinity of talent not easily found elsewhere, but then there are only three, if three of the leading men of science in Britain, and I fear therefore, that it was rather unadvised of me to forward my paper on this subject to you for presentation to the British Association, lately assembled at Swansea, and which was returned to you by the Secretary of the Chemical Section (to whom you seem to have forwarded it) as being "Unsuitable."

It will therefore afford you some consolation to learn that this paper, so ignominiously rejected at Swansea, was read before the German Association at Dantzic, on the 23d of September, by one of the chemists on the Swansea Committee, Professor Gilbert Wheeler, of the University of Chicago, United States of America, who had it translated into German for the purpose, and he informed me that one of the learned gentlemen there expressed "his astonishment" that the paper (a very brief one) had not been read in England;—adding, "in *our* country, when anything is objected to in a paper, *that* constitutes the greater reason for reading and discussing it."

So much for personal matters, and now, putting individual injustice or recognition aside, does it not appear to you that the rejection of any contribution, however feeble, towards the advancement of science "by a section of an association originally organized for that purpose by two Scotsmen—Sir David Brewster and Sir Roderick Murchison—shows a lack of what Sir John Herschel terms "that central thread of common sense on which the pearls of analytical research are invariably strung?"

The question seems to me not to be "Has England as many learned professors as Germany or France?" but have the masses of the people—the people, for instance, whom we may see so devoutly thronging the public houses and gin palaces in London on Sunday evening, when anything in the shape of scientific instruction would be considered "a desecration of the Sabbath"—have these poor religious people as much opportunity and possibility, within their little means, afforded them of acquiring practical scientific knowledge (which after all, underlies all art and labor) as the same classes have in Germany, France, or America?

The following little anecdote, among many other similar ones, shows that they have not. The other day, passing a book stall in the West of London, I asked a youth of 19 or 20, in charge, "if he had any books on chemistry?" "Chemistry," said he—"what's that?" I rejoined to this rather startling question—"I suppose you are a pupil of the London School Board?"—to which he replied "yes." I then said "what did they teach you, if they didn't teach chemistry?"—whereupon, to my grave satisfaction, he said "Oh, we learnt all about placental mammals, and vertebrata and all that"—an answer which shows that Mr. Huxley's remonstrances with the London School Board have not been altogether in vain.

With this little illustration of the state of things scientific at our very doors I will conclude this letter, and propose, with your leave, to consider in my next, the subject of your admirable address to the geological section at Swansea, of which you are President.

W. A. Ross, Lieut.-Col., R. A.

CHEMICAL NOTES.

The Sun has a Sensible Inductive Action on the Earth, even when its Magnetic Power is simply equal to that of our Globe. Induction of the Moon by the Earth and Diurnal Lunar Variation of the Terrestrial Compass.—M. Quet has shown that the sun induces the earth in various manners; by its rotation, by the speed of the earth in its orbit, by the rotation of the earth, and by the variations which it experiences in its electric constitution. The electromotive forces due to the three first-mentioned causes are:—The first 14 times greater than the second, and the second 72 times greater than the third.

The Variations of the Coefficient of Expansion of Glass.—J. M. Crafts has summed up, in his former papers, the most important theories on the variation of the fixed points of thermometers, but the variation of the coefficient of expansion of glass, which presents a much more serious inconvenience, has hitherto escaped notice. If this coefficient varies, the interval between two fixed points varies, and the graduation becomes inexact. In thermometers heated for a long time to 355°, the coefficient of expansion decreases, so that whilst the zero-point is raised by t degrees, the point 100° is raised to $100° + t + l$.

Tungstoboric Acid.—According to D. Klein, this acid differs in its constitution from various other borotungstic acids which have been prepared, and is the analogue of the unknown decatungstic acid. It is formed by the union of 9 mols. tungstic acid, 1 mol. dimetaboric hydrate, with elimination of 6 mols. water. Its composition is—

$$9WO_3,B_2O_3,4H_2O.$$

Products of the Distillation of Colophonium.—Ad. Renard has isolated a carbide, which he names heptene, of the sp. gr. 0.8031 at +20°. It is without action upon polarized light, and boils at 103° to 106°. He examined its behavior with reagents.

Dilatation and the Compressibility of Gases under Strong Pressures.—E. H. Amagat concludes from his researches that the coefficient of expansion of gases for temperatures above the critical temperature increases with pressure up to a maximum, on passing which it decreases indefinitely. The maximum diminishes for the more elevated temperatures, and finally disappears. For pressures lower than the critical pressure the deviation, which is at first positive at a temperature sufficiently low, becomes null, and then negative as the temperature increases; but, proceeding from a certain negative value, it diminishes indefinitely without changing its sign. For the pressures comprised between the critical pressure and a superior limit, special for each gas, the period during which the deviation is positive is preceded by a period where it is negative, so that the deviation changes its sign twice.

New Results of the Utilization of Solar Heat Obtained at Paris.—M. A. Pifre's improved apparatus enables him to utilize 80 per cent. of the solar heat, thus obtaining, at Paris, 12.12 cal. per minute and per square metre of surface exposed to the sun.

Remarkable Instance of Lightning Ascending Vertically.—A. Trécul perceived, during the storm of the evening of August 19th, lightning ascending perpendicularly behind the trees of the Place Jussieu, apparently from the conductors of the wine magazine.

BOOKS RECEIVED.

AMERICAN SCIENCE SERIES—BOTANY—FOR HIGH SCHOOLS AND COLLEGES. By Charles E. BESSEY, M.Sc. Ph.D., Professor of Botany in the Iowa Agricultural College. Henry Holt & Company, New York, Large 12mo. 1880.

Circumstances, ever varied in their nature, daily remind us of the progress of science, but the production of a really valuable manual devoted to some special line of research not only gives direct evidence of progress already achieved, but hopefully suggests future advancement. For these reasons, we welcome a new manual of botany, written by Professor Charles E. Bessey, of the Iowa Agricultural College, which presents many advantages over previous publications having the same object in view, and must prove one of the most valuable aids to a true knowledge of the vegetable kingdom which the advanced student can possess.

Although modestly styled by the author *"An Introduction to the Study of Plants,"* the work appears to leave little unexplained which is requisite for a comprehension of the anatomy and physiology of the vegetable. It is not claimed that the material is new, but the original arrangement of the matter to secure a more logical presentation of the subject, is apparent throughout the work.

Professor Bessey directs attention to two innovations which he has made, consisting of the "recognition of seven quite well marked kinds of tissue," and that of "raising the Protophyta, Zygosporeæ, Oosporeæ and Carposporeæ to the dignity of Primary Divisions of the Vegetable Kingdom, co-ordinate with the Bryophyta, Pteridophyta and Phanerogamia," in the hope that they may serve to give a clearer and more accurate notion of the structure of plants.

To those unacquainted with the German language, and to whom, therefore, the works of the German botanists are as sealed books, the present manual will prove particularly valuable, as free use has been made of the works of Sachs, DeBary, Hofmeister, Strasburger, Nägeli, Schwendener and others, while many of the cuts in Sachs' "Lehrbuch" have been reproduced.

One of the greatest charms of Professor Bessey's manual consists of a great number of excellent illustrations, which have been selected with great judgment, presenting over five hundred and fifty forms of vegetable life.

Professor Bessey divides his manual into two Parts, the First of which is based on Sachs' "Lehrbuch," the general plan of which is closely followed. The first chapter appropriately opens with a description of the "active and vital" principles of all vegetable organisms, "*Protoplasm.*" Following the plant cell, is discussed the cell wall, the formation of new cells the product of the cell, tissues, inter-cellular spaces, and secretion reservoirs, and so on until the plant body is gradually built up. The last three chapters of this portion of the work relate to the chemical constituents of plants, the chemical processes in the plant, and the relations of plants to external agents.

The student having thus become familiarized with the anatomy and general structure of plants, the author, in Part Two, presents his plan of classification, which, as we have stated, is based on that made use of by Sachs for the lower orders of plants, while that for the higher plants conforms more nearly to the system of classification recognized in this country and in England. Professor Bessey divides the vegetable kingdom into six divisions, as follows:

I. Protophyta.	IV. Carposporeæ.
II. Zygosporeæ.	V. Bryophyta.
III. Oosporeæ.	VI. Pteridophyta.

VII. Phanerogamia.

This is a departure from the classification which has so long been followed in the English works on botany, the familiar groups of Algæ and Fungi are not recognized, the terms being retained only when general reference is made to the Chlorophyll-bearing and the Chlorophyll-free Thallophytes, Professor Bessey stating that, under his arrangement, the term Algæ implies a Thallophyte which contains Chlorophyll, and that by a Fungus is understood one which is Saprophytic or Parasitic in habit, and which is, in consequence, free from Chlorophyll.

In the classification of the Diatomaceæ, that proposed by Professor H. L. Smith, one of the best authorities on the subject, has been wisely followed, which divides the order into three tribes, each containing several families.

As the classification of the Diatomaceæ is as yet largely artificial, we presume the one adopted by Professor Smith is provisional.

We have probably indicated sufficiently in this outline the leading characteristics of this last, and, in our opinion, the best Manual of Botany. Its merits are apparent throughout the work, and it is evident that Professor Bessey has spared no pains to render his work perfect and worthy of the great subject treated.

We trust it will receive the attention it deserves, and we commend it to every student of botany.

In connection with the above Manual of Botany by Professor Bessey, we would direct attention to a series of twenty-four botanical microscopical slides offered by Messrs. James W. Queen & Co., of Philadelphia.

Although Professor Bessey's work is abundantly illustrated, there can be no question respecting the value of having at hand the natural specimens, so that, with the descriptions still fresh in the memory, we may go direct to nature, and there not only verify the author's statements, but make independent observations of physiological facts.

While we strongly advise all engaged in such studies to make their own sections and preparations, few possess the requisite knowledge and manipulative skill to produce perfect specimens. We, therefore, with pleasure, suggest to students, and especially to instructors, that they obtain the twenty-four vegetable preparations offered by Messrs. Queen & Co. They are the most perfect microscopical slides we have seen, and the specimens are all either single or double stained, thus demonstrating the presence of protoplasm and structure, essential to a comprehension of anatomical and physiological botany.

They will also serve as excellent models for the student to imitate, in learning to prepare his own slides.

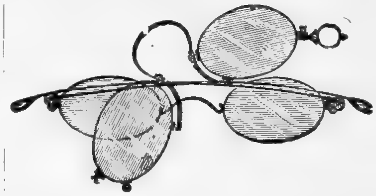
NOTES AND QUERIES.

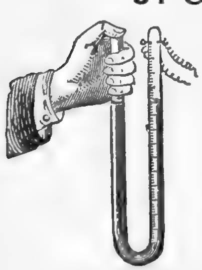
[3.] I have not yet succeeded in obtaining the pure white crystals of Iodide of Potassium by Liebig's method. Where is the difficulty, and do the following equations represent the reactions?

$$(1.)\ 2\ P + H_2O + 2\ I + 2\ Ba\ Co_3 + 2\ Ba\ H_2O =$$
$$Ba_3\ (PO_4)_2 + Ba\ I_2 + 2\ Co_2 + 2H_2O.$$
$$(2.)\ Ba\ I_2 + K_2\ So_4 = 2KI + Ba\ So_4.$$

X.

[4.] MOUNTING FRESH BLOOD.—In mounting slides of fresh blood, I occasionally find the corpucles subsequently vanish. Will some reader of SCIENCE state the cause, and give a remedy. J. R. B.





SCIENCE:

A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.

Vol. I, No. 20. - - - - - - - - November 13, 1880.

CONTENTS.

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SCIENCE

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

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Proceedings of Scientific Societies will be recorded, but the abstracts furnished must be signed by the Secretaries.

Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply may be written in the form of an article.

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JOHN MICHELS, Editor.

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SATURDAY, NOVEMBER 20, 1880.

THE SIGNAL SERVICE.

The question of the appointment of a Chief Signal Officer in the room of the late General MYER is immediately interesting. It is a question which has a direct bearing upon the scientific activity of the country, as well as upon the more important and more practical matter of making accurate weather forecasts, and displaying storm signals for the benefit of commerce.

The first and greatest use of the Weather Bureau is to make itself valuable to every individual in the United States, through accurate and prompt predictions, and thus to justify the annual expenditure of nearly $1,000,000. At present about 80 per cent. of the predictions are fulfilled, which is a fair showing—indeed, a very creditable one. Most unprejudiced persons, familiar with the routine of the Signal Service, will admit that General MYER had carried the efficiency of the service about as far as it could have been carried under an organization like his own; and the country may feel confident that, whoever is appointed to succeed him, the use fulness of the Signal Service as a Weather Bureau, that is to predict storms for the benefit of commerce, agriculture, etc., will not be greatly diminished. The present routine is so well established that we may be sure for some time at least, of the same proficiency.

But meteorologists know that this percentage can be increased. To do this, scientific investigation must be carried on in various ways, and by competent persons. The vast material now accumulated by the Bureau must be examined, discussed, and the laws—empirical and other—deduced. This can only be done under intelligent and sympathetic direction, by men trained in the methods of physical and mathematical science. This is the first great want. But again, the Signal Bureau has grown, under General MYER's vigorous administration, to be a vast machine, composed of many parts—officers and men—and controlling many instruments. For example, the many military telegraph lines of the West, several thousands of miles in length.

Again, the service must look not only to the continuance of peace at home and abroad, but to the contingency of a war in which trained signal men may be wanted. The military post of Fort Whipple, Virginia, is entirely devoted to the training of the enlisted men of the signal service for their varied duties as meteorological observers, signal men and military telegraph men, directly under the charge of officers of the army, who themselves become familiar with these varied and important duties.

These and other obvious reasons make it plain that, if the proper scientific efficiency of the Weather Bureau can be maintained it will be highly advantageous to keep the Signal Office where it now is, *i. e.* as an important Bureau of the War Department.

At present three different plans are advocated for the filling of the existing vacancy:

FIRST, The appointment of a colonel of the line who has had experience in the plains, and to whom the Brigadier General's commission would be a fitting reward; Generals HAZEN and MILES are mentioned in this connection.

SECOND, The appointment of some officer who has learned the art of administration during our war, by commanding large bodies of troops, and whose duties and studies since the war have been of a sort to fit them for this position: Generals ABBOT, PARKE, COMSTOCK, WARREN and POE, of the Engineers, are of this class.

THIRD, The appointment of a scientific civilian meteorologist, as Prof. Loomis, Prof. Cleveland Abbe or Dr. Daniel Draper.

Two faculties are required in the person to be appointed: First, he must be an able administrator; and secondly, he must be capable of understanding and directing scientific investigations.

If the appointment is made from the first class named above, it is likely that we shall have good administration, and that the present efficiency of the service will be maintained, but that no advances will be made. It is difficult for the necessary forward steps to be made under the direction of men in middle life, now first called upon to examine and approve of the methods of physical science. If the appointment is made from civil life, it is likely that tne men of great and acknowledged ability then named, while devoting their attention to the many troublesome details incident to the management of a large body of men,—would deprive science of the benefits to be derived from minds which have been engaged for a lifetime upon one branch of research.

The best interests, both of the people, who pay for the bureau, of the army, to which it is a school of instruction, and of science, which looks to it for a thorough reorganization of its old methods (which were often clumsy and antiquated) and for a decided step in the direction of investigation and research,—would probably be most surely advanced by the appointment of one of the accomplished Officers of Engineers named above. Each of these gentlemen is entirely competent to administer the complicated business of the office, as each of them commanded, during the war, a brigade, division, corps, or even army, and as each of them since the war has been engaged in work where strictly scientific ability is required. Each of them has shown in both capacities marked strength, and the appointment could not go wrong if made from their number.

It is not the purpose of this article to advance the personal claims of any one, but to point out the direction in which, after careful thought, it seems the signal service may be led to the maximum of usefulness and efficiency, both to the people and to science.

TO ASTRONOMERS.

The value of the work performed by the astronomers of the United States is now fully recognized, and has become an important factor in the progress of astronomy. They have at their command some of the finest instruments that have been produced, while their power to make good use of them is testified by the brilliant discoveries which they have recently made, forming most important records in the annals of the science.

We are glad to find that the publication of this journal meets an important want which is admitted to exist by astronomers, viz., a ready means of communication. We have received letters from Mr. Burnham, of Chicago, and from others, on this subject, and to-day Mr. Swift, of Rochester, makes the following statement, in a letter to us, enclosing a valuable astronomical paper:

"Of course you are aware that there is not, in this country, a single journal devoted exclusively to astronomy; and for ephemerides of comets we have to depend on the *Astr. Nachr.*, but as it is printed in German no amateur takes it. Now if you would give a prompt ephemeris of all comets so that amateurs can ascertain where they are, or if on the discovery of every new comet a special circular be sent immediately to each subscriber announcing it and giving position, direction and rate of motion, and if everybody knew they could and would be thus informed, hundreds would take it ["SCIENCE"]. It would be a great satisfaction for them to know that they are to be kept weekly posted on a subject not mentioned by a single weekly publication on this continent."

"It is a great consolation to know that there is no comet in the sky, for it relieves him of all suspense, and it is equally so to be told, at so cheap a rate, where it is and all about it. I could immediately notify you of all discovered by me, or telegraphed to me, from the Smithsonian Institution. I shall be pleased to call the attention of my friends, both here and elsewhere, to your JOURNAL, to increase its circulation that it may be liberally sustained."

In regard to the above letter, we beg to announce that it will be our aim in the future to comply with the suggestions so ably expressed, and indeed have partially anticipated them.

We have, by courtesy of a distinguished member of the Naval Observatory at Washington, arranged for a weekly report compiled from their library by a gentleman perfectly familiar with practical astronomy, and in connection with the Smithsonian Institution and all astronomers at Washington. This will embrace a *resumé* of both foreign and home literature, and especially will give immediate notice of astronomical information received at that establishment.

Professor Asaph Hall has recently furnished us with two communications, and we trust will in the future continue to favor us with notes. Professor Edward S. Holden will also occasionally give us the benefit of information coming within his knowledge. Professor Stone, of the Cincinnati Observatory, has already placed us under many obligations for constant communications, and up to date is one of our most esteemed correspondents. Professor Burnham, of Chicago, has also engaged to give us astronomical information in his special department, and is now only delayed, by the condition of the atmosphere, from making some important observations with the great Dearborn Equatorial, to be published in "SCIENCE." Professor Swift, of Rochester, as his letter states, will communicate to us immediate notice of results obtained with his new and magnificent instrument by Alvan Clark, and lastly, Mr. Sawyer, of Cambridgeport, undertakes to report on his interesting systematic observations of meteoric phenomena.

As "SCIENCE" is published weekly this information will be mailed to astronomers every Friday evening, and should important astronomical information reach us early in the week, we undertake to mail a special despatch, giving the information mentioned by Professor Swift. We think this programme will be a prompt compliance on our part, with the request made in Professor Swift's letter, and we trust will be acceptable to astronomers; we further ask the co-operation of all possessing, or in charge of, observatories to put themselves in communication with us and make suggestions, as it is our desire to make the most perfect arrangements, and to offer in "SCIENCE" a medium for universal intercourse for those engaged in astronomical studies.

In regard to other branches of science, equally important arrangements are being made and will be shortly announced.

CONTRIBUTIONS TO ENCEPHALIC ANATOMY. —THE OBJECTS AND METHODS OF A STUDY OF THE ICHTHYOPSIDEAN BRAIN.

By E. C. SPITZKA, M. D., NEW YORK.

II.

Inasmuch as Huxley's class of the Ichthyopsida contains the lowest of the living vertebrate forms, it would appear one of the most important undertakings for the cerebral anatomist to determine the structural relations of the brain, spinal chord and principal nerves in that class. In fact, *a priori*, the student might conclude that the anatomy of a a simple brain like that of a fish would represent a sort of rough and rudimentary sketch of the fundamental features of the higher mammalian brain, and that for this reason alone, its study would be essential to the human anatomist.

Nothing could be more erroneous!

Any one familiar with the visceral and osteological anatomy of the fish tribes will bear me out in the statement, that however convenient it may be to pigeon-hole the Amphibia, Elasmobranchi, Teliosts, Ganoids, Dipnoi and Marsipobranchi in one great class, on the strength of the formal common character, that they have no amnion at the embryonic period, and always have gills at some time of or throughout life,* that there are actually more fundamental diversities between the different primary groups of this class than between at least one group of this class and the Sauropsida.

* These are the only constant characters separating them from other groups, and it is even doubtful whether we are justified in denying the existence of the morphological representative of the amnion in all the anamnia.

As it would be difficult to find an archetype of the vertebral skeleton in any ichthyopsidean, so it is a task requiring far more discrimination and careful study than is generally devoted to this subject to determine the cerebro-spinal archetype in any member of this group, aside from the protean amphibians. For there are greater differences between the architecture of a shark's and a pike's, a herring's and a sturgeon's, an electric eel's and a lamprey's, than between an amphibian and a mammalian brain. While the differences between the brain of a frog and of a man can almost all be referred to quantitative variations in the relative proportions of similar and homologous parts, the differences between the brains of the other animals named are of a qualitative character. It actually becomes a question whether a homology between the parts of an amphibian and of a shark's brain can be established.

Notwithstanding the difficulties enshrouding this subject, both writers on human and on comparative cerebral anatomy skim over the subject with a remarkable *nonchalance*. The latest compilation on the human brain * neglects any mention of the fact that the cerebral lobes of fishes are commonly solid, informs the student that there are symmetrical halves in these animals constituting a cerebellum, and repeats the statements of as old an author as Cuvier without the slightest reference to the recent controversy on the homology of the fish's brain, in which Gegenbaur, Fritch, Stieda and Maclay have taken part.

The text book on Zoology used at most of our colleges, Packard's work, on passing through the ordeal of criticism at the hands of Wilder, is shorn of nearly every statement it makes regarding the fish's brain, since scarcely a reliable one is contained in the volume.

The question of the true homology of the fish's brain being still *sub judice*, the human cerebral anatomist can only lose time, and writers on the human brain only confuse their students by devoting attention to this problematical subject.

It is a legitimate field of study for the zootomist alone, and in its morphological respects the subject bids fair to prove rich in surprising and suggestive results, which, when once established on the basis of observation, may be utilized by the human anatomist and physiologist in generalization.

The questions to be determined will appear from the following; their answer is as yet a desideratum.

1st. A careful surface study of the brain of at least one representative of each great group should be made. Careful and enlarged representations of each such brain as projected in the five cardinal views, namely, the dorsal, ventral, lateral, anterior and posterior should be drawn, and the brains preserved for reference, in the manner to be detailed.

2d. A median section of each such brain should be made, and delineated, in order to expose the axis contours of the ventricular cavities.

* "The Brain as an Organ of the Mind," by H. Charlton Bastian, 1880.

3d. A longitudinal section nearly parallel with the former, running from the anterior prolongation of the olfactory bulb through the middle of *each* cerebral and optic lobe, and striking the lateral convoluted mass of the medulla oblongata, could be made from the same brain, as a supplement to the elucidation of the internal contours.

4th. One horizontal dissection exposing the ventricular floors, from above, and another exposing the ventricular roofs from below, will still further clear up these relations.

5th. A series of transverse sections, taken perpendicularly to the peduncular axis, will be essential to a comprehension of the relations of the ventricles and deeper parts for each altitude. The sections should be taken at distances of from one to three millimetres apart, according to the size of the brain, then preserved in separate bottles and labeled in numerical order.

All these preparations should be made from brains hardened in absolute alcohol, and the dissections should be made after the brain has been kept thus for one month, if the working season is in summer, and one or two weeks or even a few days, if the season is winter.

My plan, when engaged in this and similar work, has been to expose the cranial cavity by cutting away the surrounding parts with a strong knife until the brain level is reached. This requires very little practice. Then the lateral walls are broken away with a forceps, or cut away the same knife, and the student may then clear up the tracks of the cranial nerves for a short distance. The brain is not to be removed from the skull base, but left in contact with it, a smooth round head of a needle may be employed to bread up the arachnoid attachments there, and facilitate the penetration of alcohol to the basilar parts, but this is all that should be done. The brain must be immersed in alcohol, with the base of the skull in connection therewith, at least by means of the emerging nerve roots, else the topography may become disturbed.

The membranes (excepting the dura of the convexity) should not be touched, for it is desirable to trace their connections with plexiform structures penetrating the fissures and cavities of the encephalon, as these may be of service in explaining certain homologies.

Alcohol is selected as the preserving fluid for the reason that it does not render the specimens too brittle for coarse dissection, which the chromic salts do, nor distorts the contours as does glycerine.

The transverse sections can be made in a microtome, moving the piston the distance of the thickness of the required section, before each section is cut. Previous to each cutting, the imbedding matrix should be removed to a little below the level of the section. All other sections can be made without a microtome, it being well, however, to fix the brain in a wax or a paraffine layer, poured on a glass plate. Adherent particles of the material thus used can be subsequently removed with turpentine, when the specimen is prepared for permanent preservation. It is needless to add that all sections and dissections can be done a hundredfold better under the surface of a fluid like alcohol or water, than by simply wetting the knife with these fluids, as text-books direct.

All the work so far mentioned is only preparatory however. It is merely destined to furnish on the one hand a topographical guide to the more important work which is to follow, on the other to supplement the ascertained relations of ganglionic masses and fascicular tracts by a plastic conception of the encephalic segments which contain them. The work which is to follow is far more tedious, but also far more important; its methods are those employed in studying the microscopic anatomy of embryos.

For the purposes of microscopic anatomy the brains of smaller species are as preferable, as those of the larger species are desirable for the coarse anatomy. The brain of a sturgeon twelve inches long, will show all the microscopical details as well, and be easier of manipulation than that of one twelve feet long. The latter's had best be devoted to naked eye study.

If the weather is cold, the animal perfectly fresh, and the specimen can be kept in a temperature near the freezing point (it should never reach or drop below the latter,) the brain can be immediately transferred to a solution of chromic acid of a light sherry color. In my experience this tint, tested in a two ounce graduate, is a far more reliable gauge than any weighing by so many grains to so many ounces, that is ordinarily recommended. After staying a week in this solution, it is transferred to one of bichromate of potash, having the same color. Here it remains, care being taken to have always at least one hundred times as much fluid volume as specimen volume, until the desired degree of hardness is attained. The latter is hard to describe in words, but an adequate conception can be best conveyed by saying that the specimen should be unyielding to pressure, and yet not altogether inelastic. The membranes will now separate readily, and the specimen, first washed in water, is transferred to a neutral (long stood, and repeatedly filtered and mouldless) carmine solution, so concentrated as to appear black in a depth of six inches. Here the specimen is left for from one to three weeks, according to the size of the brain. Then it is again washed, put in water containing two per cent. of glacial acetic acid for twenty-four hours, washed again, transferred to proof spirit for a day, then finally to absolute alcohol, until such time as the observer is ready to make his sections.

When this time arrives (and it is best not to defer it over a month) the brain stained and hardened as it is, is transferred to clove oil, which penetrates and drives out the alcohol in a few days. The translucency of the specimen is a sign that this has been accomplished. It is then taken off, the superfluous clove oil drained from the surface, and imbedded in a microtome with paraffine. The superfluous matrix being removed with each section, the cutting is done with turpentine, and each section, stained and transparent, can be transferred to its appropriate slide and mounted, so that the order in which each section belongs is preserved. This is an important advantage.

If the weather is warm, the brain should be submitted to absolute alcohol for a day before entire removal from the skull, then put in a mixture of methyl-alcohol and bichromate of potash, of a muddy beer color (thirty grains of the salt to the ounce of alcohol) for a week, and subsequently, for a variable time according as the specimen will harden, to simple Müller's fluid. The staining, cutting and mounting can be done exactly as in the former case.

Specimens prepared by the first method of hardening will furnish better results for the medulla, those hardened with the second will yield more complete specimens of the higher ganglia. It is a well known fact that fluids that will harden the medulla oblongata well will sometimes fail to render the cerebrum and mesencephalon fit for cutting.

Of course the most important series of sections will be one taken transversely to the peduncular axis. This should be made first, therefore, and studied in conjunction with the delineations made from the coarse specimens. Now the student having familiarized himself with the precise topography and extent of every ganglion, cortical expanse and fibre mass, is ready to proceed to more complicated inquiries, that is to study the *relations* of fibre masses. How he may proceed where a fasciculus does not run in a straight plane, I have indicated in a previous contribution to this journal.*

It is needless to say that in addition to these methods, which may be called systemic ones, inasmuch as they are calculated to reveal homologies and relations, that all other methods of hardening and staining may be used to study the finer and finest histology. They are of less importance, however, both to the zootomist and neurologist, than is generally supposed.

Now a word as to the objects of such an inquiry, for unless the investigator has a definite point in view, and a provisional notion of the subject he intends to develop, his work will be barren of result, save he stumble on some revelation accidentally.

a. The close relation between the cerebral lobes and the olfactory lobes of fishes may, if studied in all the groups, particularly the lampreys, lead to the establishment of a homology with the so-called cerebral lobes of the higher invertebrates.

b. The fact, which we have every reason to suspect to be a fact, that the cerebral lobes of fishes are the true homologues of the cerebral hemispheres of the mammalia, sauropsida and amphibia, requires to be definitely established. Prof. Burt G. Wilder questions this homology, on the ground that the cerebral lobes of bony fishes are solid, and contain no ventricles. That so acute an observer, one to whom we owe so much in the line of correction of gross errors which have found their way into standard text books, could lean his objection on such a doubtful basis, shows how catholic must become the principles, if I may so term them, of cerebral anatomy. The embryological development of the fish's brain presents features which no other vertebrate brain exhibits in the course of development, namely, the entire central nervous axis is apparently solid. In truth it is hollow, but the cavity is a mere slit, the walls of which are in contact, and when the cerebral lobes become solid they do so by the fusion of these walls and the obliteration of the slit. The ventricle is therefore not an essential feature of the cerebral hemisphere, and as if to prove this fact beyond a doubt, we find that among animals as nearly related as sharks, some have true ventricles in these lobes communicating with the third ventricle, while others have them as solid as the bony fish.

c. The derivation of the olfactory bulb, a structure often and unwarrantably confounded with the olfactory lobe, can be best studied in fishes.

d. The same applies to the cerebral epiphysis and hypophysis, still known by the improper titles of pineal and pituitary glands.

e. The relations of the peculiar *lobi inferiores* to the optic nerve, and the asserted homology of the *corpora candicantea* require confirmation.

f. The question of the homology of the cerebellum and optic lobes, which is in a very unsettled state to-day, is yet unanswered. Wilder, in his paper on the brain of the *Chimæra*, has exposed the fallacious interpretations which most authors have made in this regard. His essay will prove valuable to those engaged in this inquiry. Possibly the discovery by myself of the entire distinctness of the post-optic and the hitherto unknown inter-optic lobes in reptiles, from the optic lobes proper, may assist in unraveling the true relations.

g. Since among fishes we find many examples of remarkable development of the periphery, I need but instance the rostrum of Spatularia, the great lateral expansions of the skate, the asymmetry of the Flounder, the rudimentary eyes of Amblyopsis, the marsupium of the Hippocampus, and the immense jaw of the Angler, an inquiry dealing with the relations of nerve centres to the projected peripheries may be expected to furnish many suggestive facts bearing on the projection doctrine.

All through these lines it will be seen that as in every other branch of morphology a study of embryonic development is an essential to a proper knowledge of the fish's brain. A brief consideration of the methods to be employed in this field of the study will not be out of place.

Spawn can be obtained living from our fish hatching depots, whose superintendents will be found very obliging towards those requiring material for scientific study. The different stages of development, extending to beyond the period when the young fry escapes, can be obtained by permitting the ova to develop under the eye of the observer in a hatching trough.

The ova of bony fishes are dropped into a solution of chromic acid, or Müller's fluid; better, a few specimens are taken out each day and dropped each into differently strong solutions of the former and into the latter. I know of no standard strength that will yield uniform results, and have while working in this field in Vienna lost thousands of ova by following the routine directions.

From the chromic acid and Müller's solutions the spawn is transferred to alcohol in from two to

* Part I. of this series, *Journal of Nervous and Mental Disease*, 1887, p. 668.

twelve days, the younger the germ the less time should it be exposed to chromic acid. After having been in alcohol a week it is transferred to a sherry wine colored solution of bichromate of potash for a period sufficient to harden it.

With a cataract needle the investigator will then cut a trench around the embryo, cutting through the vitelline membrane, which fixes the embryo to the vitellus, and then lift it away and remove it from the latter, which, brittle and crumby, cannot be cut. The staining in a solution of carmine, as described for adult brains in this paper, will require from one to four days, according to the size of the embryo. Of each stage three series of sections are necessary, one transverse, one horizontal, and a third, the most important, sagittal, that is parallel to the median plane.

All these minutiæ, however wearisome they will prove, are necessary, and he who has thus with his scalpel, reagents and razor, constructed an open volume of natural specimens, will find himself richly rewarded by the richness in detail, the manifold character of the morphologies, and the suggestive character of the relations exposed.

The material for such a study can be obtained in a fresh state from no one locality. The student residing in New York will have to take a vacation trip to the Mississippi; he living in Chicago a corresponding trip to the Atlantic coast.

In the West he will find the great lake catfish, the lake sturgeon, the Amia calva, the gar-pike, and the remarkable spatularia, the brains of all of which should be studied. Possibly he may obtain the fresh water lamprey (Hylomyzon), but one brain which he should not neglect is that of the blind fish of the Kentucky caves, whose examination is destined to clear up somewhat the true relations of the *lobi inferiores* and the optic lobes. On the Atlantic coast all the bony fish, obtainable in the fresh waters of the West, besides a rich variety of salt water forms, also the lamprey, the shark and ray are obtainable. A trip to the Bermudas or the Florida coast, occupying about two weeks, will increase the student's *repertoire* with a host of tropical and sub-tropical genera.

WEIGHT, SPECIFIC GRAVITY, RATES OF ABSORPTION, AND CAPABILITIES OF STANDING HEAT OF VARIOUS BUILDING STONES.

By Hiram A. Cutting, Ph. D., State Geologist Vermont.

Having during the past year instituted, and carried out, a series of experiments to ascertain, as nearly as possible, the capabilities of the various materials used in the construction of so called fire proof buildings, to stand heat, I submit, in tabulated form, the result of such experiments, hoping they may be of use to the architects, quarrymen and Insurance companies of our country, and also of some interest to those interested in science.

In connection with the capabilities of the various building stones to stand fire and water, I have taken their specific gravity, and weight per cubic foot, so that the identity of the various stones could at any time be compared, and if in the working of a quarry there was a change in gravity, or weight, that it could be easily detected, and thus all who choose could know whether the tests given would apply or not.

I have procured sample specimens of the most important building stones in the United States, and Canada, and, after dressing them into as regular form as possible, three by four inches, and two inches in thickness, I have taken their ratio of absorption, which ratio I have expressed in units of weight, according to the amount of water taken up. If 450 units of stone absorbed one unit of water, I have expressed it thus: 1 + 450, meaning that the stone weighed 450 units when immersed, and 451 when taken from the water.

To accelerate the process of absorption I have placed the specimens in water under the exausted receiver of an air pump. I find that in this way as much water is absorbed in a few minutes as in days of soaking. When specimens were removed from the water, I have, before weighing, dried their outsides with blotting paper. In relation to the specific gravity, I have not followed "Gilmore's" rule in full. He weighed the specimens in air, immersed them in water, and allowed them to remain until bubbling had ceased and then weighed them in water, after which he took them from the water, dried them outside with bibulous paper, and weighed them again in air. From this last weight he subtracted the weight in water, dividing the dry weight by the difference.

This gave a specific gravity subject to two sources of error. I have followed the more frequent custom of weighing the dry stone, using pieces of two or three pounds in weight, and then immersing them in water. After the usual saturation I have taken their weight in water, subtracting it from the dry weight in air, and then dividing the dry weight by the difference. This gives the specific gravity of the rock itself, as usually found, which is what we desire, and I believe as it would generally be in buildings constructed of the given material. The specimens were previously dried by long exposure to a temperature not exceeding 200° Fah. To verify this I have taken specimens from the quarries direct, and after weighing, have brushed them over with paraffine dissolved in naphtha, weighing them again so as to ascertain the exact amount of paraffine, which made no visible change in the stone, other than to keep out water. I have then weighed in the usual way, and thus obtained the exact specific gravity of the stone as in the quarry, and I find my method used, as stated, to give the best results, and so have adopted it.

After this I have placed them in a charcoal furnace, the heat of which was shown by a standard pyrometer. In many instances I have placed them side by side with dry specimens, but have been unable to note any marked difference in the action of heat, beyond this, that the dry specimens became sooner heated, I have, however, no doubt that the capacity of a stone to absorb water is against its durability, even in warm climates, and vastly more so in the changeable and wintry climate of New England. It is here often frozen before any considerable part of the moisture from Autumn rains can be evaporated,

When the specimens were heated to 600° Fah., I have immersed them in water, also immersing others, or the same, if uninjured, at 800° and 900°, that is if they are not spoiled at less temperatures. I find that all of these samples of building stones have stood heat without damage up to 500°. At 600° a few are injured; but the injury in many cases commences at or near that point. When cooled without immersion they appear to the eye to be injured less, but are ready to crumble, and I think they are many times nearly as much impaired, and always somewhat injured, when water produces any injury.

I would remark that my experiments with granites show that there is quite a range in their capabilities of standing heat, a range in fact much greater than I anticipated. With the sandstones the difference is also marked, as is their power of absorption. When exposed to the heat wet, they show a marked difference in the time required to heat them, the saturated ones seeming to resist the heat for a time; but when equally hot they crumble the same as those not previously saturated. Their relative worth can be seen by the table. The conglomerates stand heat badly; while the limestones and marble stand best of all (up to the point where they,by continued heat, are changed to quick lime) except soapstone, and a species of artificial stone made under the McMurtire & Chamberlain patent. The indications are, from this and other samples of artificial stone, that it may be possible to make an artificial stone cheaper and better for fire proof buildings than our native quarries furnish; and we hope this possibility may receive attention. But comments are useless, as the facts set forth in the tables speak for themselves.

I give you results in tabulated form below.

GRANITES.

No.	KIND.	LOCALITY.	Specific Gravity.	Weight of One Cubic Foot.	Ratio of Absorption.	First Appearance of Injury.	Crumbles or Cracks Slightly.	Cracks Badly or Becomes Friable.	Injured so as to be Worthless for a Building.	Melted or Ruined.
				Lbs.		Deg. Fah.	Deg. Fah.	Deg. Fah.	Deg. Fah.	Deg. Fah.
1	Light colored	Hallowell, Me	2.638	164.8	1 + 790	800	900	950	1000	1100
2	"	Fox Island, Me	2.642	165.1	1 + 680	700	800	850	900	1000
3	Denning's Quarry	Mt. Desert, Me	2.631	164.1	1 + 716	800	850	950	1000	1100
4	Light colored	Rockford, Me	2.600	162.5	1 + 482	600	800	850	900	950
5	Red	Red Beach, Calais, Me	2.636	164.7	1 + 560	800	850	900	950	1000
6	Light colored	Oak Hill, Me	2.526	157.8	1 + 310	800	850	900	950	1000
7	Red	Stark, N. H	2.631	164.1	1 + 534	600	700	800	850	950
8	Colored medium	Concord, N. H	2.636	164.7	1 + 778	800	900	950	1000	1200
9	Sanborn's Quarry	Plymouth, N. H	2.649	165.5	1 + 685	800	900	950	1000	1200
10	Carter's Quarry	Ryegate, Vt	2.647	165.4	1 + 790	800	900	950	1000	1200
11	"	Woodbury, Vt	2.634	165.8	1 + 784	800	900	950	1000	1200
12	Wetmore & Morse's Quarry	Barre, Vt	2.651	165.6	1 + 720	800	900	950	1000	1200
13	Syenite	Quincy, Mass	2.660	166.2	1 + 650	750	800	850	900	1000
14	Gray	Croton, Conn	2.800	175.0	1 + 818	700	750	800	900	900
15	Common	Woodstock, Md	2.648	165.5	1 + 394	700	750	800	900	900
16	"	Port Deposit, Md	2.700	168.7	1 + 816	800	900	950	1000	1100
17	Scranton County Quarry	Richmond, Va	2.727	170.5	1 + 398	750	800	850	900	1000
18	Old Dominion Quarry	" "	2.674	167.7	1 + 402	750	800	850	900	1000
19	Light colored	St. Cloud, Minn	2.690	168.2	1 + 280	700	700	800	850	900
20	"	Stanstead, P. Q	2.833	177.0	1 + 420	800	900	1000	1000	1200
21	Coarse	North Halifax, N. S	2.698	168.6	1 + 584	700	800	800	900	900
22	"	Gauauogue, P. O., Can	2.687	167.9	1 + 736	800	850	900	950	1000

SANDSTONE.

1	Freestone	Portland, Conn	2.380	148.7	1 + 27	850	900	950	1000	1100
2	"	North of England	2.163	135.5	1 + 27	850	900	950	950	1000
3	Seneca Stone	Montgomery Co., Md	2.500	156.2	1 + 26	850	900	900	950	950
4	Sandstone	Salem, Md	2.452	153.2	1 + 24	850	900	950	1000	1100
5	"	Seneca, Md	2.410	150.6	1 + 40	900	1000	1100	1200	1200
6	Montrose Stone	Ulster Co., N. Y	2.661	166.3	1 + 314	900	1000	1100	1200	1200
7	Freestone	Belleville, N. J	2.350	146.8	1 + 27	900	950	1000	1100	1100
8	"	Nova Scotia	2.424	151.5	1 + 240	900	950	1000	1100	1100
9	S. Carboniferous	Br. Phillipe, N. S	2.353	147.0	1 + 19	900	950	950	1000	1000
10	Freestone	Dorchester, N. B	2.363	147.7	1 + 26	800	850	900	1000	1000
11	Cincinnati Stone	Cincinnati, O	2.188	136.1	1 + 23	900	950	1000	1100	1100
12	Potsdam Sandstone	McBride's Corners, O	2.333	145.8	1 + 28	800	850	900	1000	1100
13	Berlin,Stone	Cleveland, O	2.210	138.1	1 + 22	850	900	1000	1100	1100
14	Potsdam	McBride's Corners, O	2.500	156.2	1 + 22	850	900	950	1000	1000
15	Euclid Stone	Near Cleveland, O	2.290	143.1	1 + 35	850	900	950	1000	1000
16	Berea Stone	Berea, O	2.254	140.8	1 + 20	850	900	950	1000	1000
17	Amherst Stone	Amherst, O	2.200	137.5	1 + 18	850	900	changes	color.	1000
18	Brown Stone	Humbletown, Penn	2.346	146.6	1 + 28	850	900	950	1000	1000
19	Potsdam Sandstone	Beauharnois, P. Q	2.512	157.0	1 + 38	850	900	950	1000	1000
20	Sandstone	Murray Bay, P. Q	2.577	161.0	1 + 36	900	950	1000	1100	1100
21	"	Cheat River, W. Va	2.632	164.5	1 + 80	800	850	900	1000	1100
22	Freestone	Acqua Creek, Va	2.183	136.4	1 + 16	900	950	1000	1100	1200
23	Brown Stone	Manasses, Va	2.348	146.7	1 + 17	850	900	1000	1100	1200

LIMESTONE.

1	Limestone	Baltimore, Md	2.917	181.8	1 + 340	900	1000	1100	1200	1200
2	"	Bedford, Ind	2.478	154.8	1 + 280	850	900	1000	1200	1200
3	Cincinnati Limestone	Hamilton County, O.	2.204	137.7	1 + 28	850	900	950	1200	1200
4	Potts Blue	Springfield, Penn	2.666	166.6	1 + 280	850	850	900	1000	1200
5	Dolomite Limestone	Owen Sound P. O	2.571	160.6	1 + 480	850	900	1100	1200	1200
6	Trenton Limestone	Montreal, P. Q	2.706	169.1	1 + 316	900	950	1000	1200	1200
7	Limestone	Isle La Motte, Vt	2.696	168.5	1 + 320	950	1000	1100	1200	1200

CONGLOMERATES.

1	Conglomerate	Roxbury, Mass	2.708	169.2	1 + 49	700	800	900	1000	1000
2	Potomac Stone	Point of Rocks, Md	2.724	170.2	1 + 60	600	700	800	900	900
3	Conglomerate	Cape a La Aisle, P. Q	2.645	165.3	1 + 80	600	700	800	900	900

No.	KIND.	LOCALITY.	Specific Gravity.	Weight of One Cubic Foot.	Ratio of Absorption.	First Appearance of Injury.	Crumbles or Cracks Slightly.	Cracks Badly or Becomes Friable.	Injured so as to be Worthless for a Building.	Melted or Ruined.
	MARBLES.									
						Deg. Fah.	Deg. Fah.	Deg. Fah.	Deg. Fah.	Deg. Fah.
1	Tuckahoe	Westchester Co., N. Y.	2.794	194.6	1 + 298	900	1000	1200	1200	1200
2	Ashley Falls	Ashley Falls, N. Y.	2.742	171.3	1 + 280	900	1000	1100	1200	1200
3	Snow Flake	Westchester Co., N. Y.	2.848	178.0	1 + 380	950	950	1000	1200	1200
4	Tennessee	Dougherty's Q'y, E. Tenn	2.711	169.4	1 + 320	950	950	1000	1200	1200
5	Duke Marble	Near Harper's Ferry, Va.	2.812	175.7	1 + 340	1000	1000	1100	1200	1200
6	Black Marble	Isle La Motte, Vt.	2.682	176.6	1 + 320	1000	1000	1100	1200	1200
7	Sutherland Falls	Rutland, Vt.	2.666	166.6	1 + 342	1000	1000	1100	1200	1200
	SLATES.									
1	Sabin's Quarry	Montpelier, Vt.	2.869	179.3	1 + 110	800	850	900	1000	1200
	SOAPSTONES.									
1	Soapstone	Weathersfield, Vt.	2.668	166.7	1 + 3 8	1200	----	----	----	----
	ARTIFICIAL STONE.									
1	Artificial Stone	McMurtire & Chamberlain's patent	2.235	139.7	1 + 280	750	800	1100	1200	----

MINERAL WAX, A RESUMÉ.

By M. BENJAMIN, PH. B.

Geographical Distribution. Mineral wax or ozocerite (from ὄζειν, to smell, and κηρός, wax) is found in a sandstone in Moldavia, in the vincinity of coal and rock salt. It also occurs in large quantities at Borislav, near Drohobycz, and at Dzwiniacz, near Stainstawow in Galicia, a province of Austria. The mines are situated at the northern foot of the Carpathian Mountains. It has also been found at several other places in the same province. Small quantities have been discovered in England, at Binney Quarry, Linlithgowshire; at the Urpeth Colliery, Newcastle-on-Tyne, and in Wales. In this country it has been found in Texas, in Utah and in California, about fifty miles northeast of Los Angelos, among the Sierra Madre Mountains In Utah the mineral occurs in shale beds, out of which the ozocerite appears as exudations. These shale beds are quite extensive—some forty to sixty miles long by twenty wide, and from seventy to forty feet in thickness. It is thought that by digging and boring the supply of the wax may be increased.

Geologically it is presumed that these beds were formed in a tertiary lake or peat bog. Prof. J. S. Newberry suspects that it will be found to be an evolved product, the distillation of beds of cretaceous lignite and the residue of a petroleum unusually rich in paraffine. The foreign deposits are considered to be about of the same age.

Mode of Occurrence. It is generally found (referring to Galacia) in thin layers and small pieces which must separated from the matrix in which they are found. The smallest pieces are only obtained by a process of washing. It is sometimes found in lumps or layers from one to three feet in thickness, a lump sometimes weighing several hundred weight.

Physical Properties. It is like a resinous wax in consistency and translucency, sometimes with a foliated structure. Its color is brown or brownish yellow by transmitted light and leek green by reflected light. The poorer qualities, which are colored black and are either too soft from abundance of petroleum or too hard (asphalt like in character), are mainly used for the production of paraffin. It possesses a pleasantly aromatic odor. The American variety is described as black in the mass, sections of which are translucent.

Its Chemical Nature. The specific gravity of ozocerite is 0.94 to 0.97. According to Dana it ranges from 0.85 to 0.90.

Its melting point is variously given as follows:

The Moldavian,	84°	Malaguti.
Urpeth mineral,	60°	Johnson.
Galacian,	60°	Höfstadter.
Utah,	61°5	Newberry.
Moldavian,	62°	Schrötter.
From Slanik,	62°	Glocker.
Galacian,	63°	Wagner.

The boiling point is likewise differently given by the authorities:

Urpeth mineral,	121°	Johnson.
Moldavian,	210°	Schrötter.
Moldavian,	300°	Malaguti.
Utah, between	300° and 380°	Newberry.

Concerning this last determination, Dr. S. B. Newberry says; 1.5 grammes of the substance were treated with about 300 c. c. of cold ether, and allowed to stand for twenty-four hours. The substance was decanted through a filter, evaporated, and the resulting mineral tested to obtain the melting point. This treatment gave me a fraction equal to 25.4 per cent. of the original substance, and having a melting point of 49.° C. The residue was again treated with 200 c. c. of cold ether for about the same time, and gave a further product equal to 9.1 per cent. of the original mass, fusing at 61°. On boiling the undissolved portion in about 500 c. c. of ether the whole mass went into solution, and upon evaporation was found to have a fusing point of 67°. It distills without decomposition, is not altered by strong acids, and very little by hot alcohol. The Moldavian variety dissolves but slightly in ether, whereas that found at Urpeth dissolves in this medium to the amount of four-fifths, and separates on evaporation in brown flecks, which melt at 38.°9 to a yellowish brown liquid. The solubility of the variety found in Utah has been sufficiently referred to in the remarks on its fusing point. The composition of ozocerite has been found to be:

	Moldavian.		Urpeth.	Utah.
	Malaguti.	Schrötter.	Johnson.	Newberry.
Carbon	85.75	86.20	86.80	86.15
Hydrogen	15.15	13.77	14.06	13.75
	100.90	99.97	100.86	99.90

It is supposed to be a compound of several members of the paraffine series, which are represented by the general formula $C_n H_{2n+2}$, and perhaps containing certain of the olefines $C_n H_{2n}$, a very full description of the chemical composition of a nodule of ozocerite found at Kinghornness, Scotland, was given in a paper read by W. Ivison Macadam, at the Sheffield meeting of the British Association,* last year.

Process of Manufacture. The crude mineral (ozocerite) is melted with water in order to remove any sand or other earthy impurities with which it is likely to be mixed. It is then run into cakes weighing about two pounds each. Another authority states that crude hydrocarbon is first melted and drawn off; the residue boiled with water, to the surface of which any remaining ozocerite rises; the whole allowed to stand for several hours for any suspended impurities to settle out. The melted wax which was drawn off is poured into moulds, which hold from 100 to 120 pounds. These cakes are then shipped to the various factories in England, Moldavia and Vienna, where it is purified and converted into illuminating oils and paraffine. A portion of it is directly treated on the island of Swatoi Astrow, in the Caspian Sea, near the Peninsula of Apscheron. There it is distilled in flat bottomed iron retorts provided with leaden worms, each of these retorts holding from 1,500 to 2,000 pounds.

Sixty-eight per cent. of distillate is obtained, sixty parts of which are paraffine and eighty parts oil. According to Grabowsky, the products of such a working may be tabulated as:

Benzine	2 to 8 per cent.
Naptha	15 to 20 " "
Paraffine	36 to 50 " "
Heavy lubricating oils	15 to 20 " "
Coke	10 to 20 " "

The oil thus obtained is yellow, opalescent, possesses an ethereal odor, and has a density varying between 0.75 and 0.81. Each distillate yields a quantity of a light oil boiling below 100°, which is used for purifying the paraffine, as will be shown further on. The crude paraffine thus obtained from the first distillation is yellow in color and tolerably pure. It is treated by the hydraulic press and the expressed oil redistilled in order to obtain any remaining paraffine. The pressed paraffine is melted and treated at from 170° to 180° with five per cent. of sulphuric acid, washed, neutralized with lime, and then rapidly distilled, then cast in plaques and again pressed. The cakes thus obtained are treated with twenty-five per cent. of the light oil and again melted and pressed; finally, they are treated with steam for the purpose of eliminating the last traces of essential oil. The material resulting from this treatment is a perfectly pure and colorless substance, free from all odor, transparent, and so hard as to exhibit in large blocks an almost metallic sound.

An improved method of bleaching ceresine, paraffine, petroleum, stearine and other fatty matters has been patented in Germany within a few months. The process consists in heating ozocerite to 170°–200° C. About twenty per cent. of the hydroxides of aluminium, iron, manganese and magnesium or the silicates of aluminium and magnesium are added to the molten mass. The treatment is repeated several times with the clear liquid, which separates upon standing. The residues are then treated with steam to remove ceresine and to restore the hydroxides.

*See *Chemical News*, vol. XI., p. 148.

TEXTILE FABRICS OF THE ANCIENT INHABITANTS OF THE MISSISSIPPI VALLEY.*

By Judge J. G. Henderson.

He showed that the modern Indians and these ancient people are bound together by a similarity in the instruments and processes of spinning and weaving. The materials used were the bark of various trees, the nettle, and the hair of the bear, buffalo, deer and dog. In working up the vegetable substances, the bark was first macerated. After being dried, it was spun in a multitude of ways. The rudest process was rolling on the thigh. The next step was a rude spindle which passed through various processes of evolution to the modern spinning-wheel. The speaker then proceeded to show the gradation of elaboration through which the loom has passed into the process of weaving. Judge Henderson's paper was illustrated by a series of drawings, collection of raw materials, and models of spindles and looms.

OCCURRENCE OF TIN AT WINSLOW, ME.*

By Professor C. H. Hitchcock.

After exhibiting specimens of the ore, etc., which is ordinary tin-stone, and is associated with margarite, fluveite, beryl and arsenical pyrites, Professor Hitchcock observed that there are twelve veins of this ore, in twenty feet of rock, their geological relations being identical with those of the tin veins of Cornwall. A bar of tin weighing fourteen ounces was also shown; it is the largest bar ever made in this country. Professor Hitchcock considers this locality the most promising tin-bearing locality yet discovered in the United States.

MICROSCOPY.

At a meeting of the State Microscopical Society of Illinois, held at Chicago, on the 8th ultimo, a new Microscope stand was exhibited by Mr. W. H. Bullock, specially designed for lithological work.

"The stage was made to rotate concentrically on the same plan adopted in his large instruments, and was graduated to read with a vernier to minutes. Both the minor and sub-stage were mounted on graduated circles, and arranged so as to swing over the stage, either separately or in unison. The sub-stage was made in two cylindrical fittings. The lower one carrying the polarizing prism, could be readily swung to one side, while the upper carried the achromatic condenser. The polarizing prism was mounted with a circle graduated to degrees, and was fitted with a stop for marking the position of the prism. The analyzer was mounted above the objective, somewhat after the manner of a Wenham prism, and could be slid in and out of position with the same facility, and also carried, if desired, a quartz film. It was, he said, a matter of great convenience for the lithologist to be able to pass from the use of ordinary to that of polarized light, without loss of time, and with the instrument on exhibition, this change could be effected in less time than a change of objectives with a double nose piece. The stand was also provided with a goniometer eye-piece, which was fitted with a calc film and and analyzing prism, both separable at pleasure."

The instrument, as above described, appears to be well adapted for the end in view, but we would remind Mr. Bullock that Swift, of London, has arranged the polarizing prism and the analyzer in equally convenient positions for instant use; the former he attached to his patent condenser, under the stage, while the analyzer was fitted exactly as Mr. Bullock described. Such instruments have been made for upwards of ten years, and have been used in this country.

Mr. Beck, of London, who was present, must have been quite familiar with the instrument we have described. We have always found the arrangement to work admirably, and are surprised that makers do not generally adopt the system in all Microscopes.

* Read before the A. A. A. S., Boston, 1880.

SWIFT'S COMET.

The comet discovered by Swift on the 10th of October last, has again attracted general attention from the announcement by Mr. Chandler in *Special Circular No. 7*, to SCIENCE OBSERVER, that it seems to be identical with Comet III., 1869 (Tempel), and from the announcement by the Astronomer Royal to the Smithsonian Institution, of a comet discovered by Lohse, at Lord Lindsay's observatory, Dun Echt, November 7, which proves to be an independent discovery of the same object. It has already been followed for nearly a month by astronomers in America, and its elements were computed by Mr. Chandler as accurately as possible from the data at hand.

The following observations made by Prof. Eastman with the Transit Circle of the Naval Observatory, Washington, together with the resulting elements and ephemeris computed by Mr. Upton, have been kindly furnished for publication.

COMET, SWIFT, 1880.

Observations made with the Transit Circle at the Naval Observatory, Washington, D. C.:

	R. A. H. M. S.	DECL.
October 25	21 50 8.74	+ 28° 29′ 4″.9
November 1	22 12 33.12	35° 32′ 48″.1
" 7	22 45 6.26	42° 26′ 8″.3

ELEMENTS.

T=1880. Nov. 8.00411, Wash. M. T.

π= 42° 2′ 13″ }
Ω=295 48 23 } Mean Eq. 1880.0.
ι= 7 22 16 }
log. q=0.04220.

COMPUTATION OF MIDDLE PLACE.

	OBS. COMP.
$d\lambda \cos\beta$ =	− 15″
$d\beta$ =	+ 4″

EPHEMERIS. WASHINGTON—MEAN MIDNIGHT.

DATE.	R. A. H. M. S.	DECL.	Intensity of Light.
1880—November 16	0 13 15	+ 52° 8′.7	1.11
" 20	1 7 41	54 31.2	1.08
" 24	2 5 44	54 59.6	0.99
" 28	2 59 22	53 38.8	0.86
December 2	3 43 26	51 2.5	0.72
" 6	4 17 21	47 50.5	0.58

In order to show the remarkable accordance with the elements of III., 1869, we give the elements of this latter comet as published by Dr. Bruhns, Astron. Nach. 1788:

COMET III., 1869.

T = 1869, Nov. 20. 85426. Berlin, M. T.
π = 41° 17′ 12″.5 }
Ω = 292° 40′ 28″.8 } Mean Eq. 1870.0
ι = 6° 55′ 0″.0 }
log. q. = 0.042416.

Assuming the two to be identical, and the comet to move in an eclipse having a period of 12 days less than 11 years, we shall have—

Semi-major axis = 4.93589
Eccentricity = 0.7767.

The intensity of light on November 7 is taken as unity. On this scale the intensity on October 10, when the comet was discovered, was 0.36. It reaches a maximum brightness about November 16, and it is probable that observations can be continued till near the end of the year, before the comet becomes too faint.

It presents an ill-defined disc, several minutes in diameter, but owing to the brightness of the moon, it can be seen for the next week, only with the larger instruments. If the identity of these two comets is finally established, and there seems to be no reasonable doubt of it now, a recomputation of the elements, embodying all the reliable observations made in 1869, will be very desirable, and will doubtless soon be undertaken. W. C. W.

WASHINGTON, *Nov.* 15, 1880.

THE NEW PERIODIC COMET.

This comet, discovered by me at midnight of October 10–11, is destined, from present indications, to become one of considerable celebrity, notwithstanding it will not be visible to the naked eye. The computation of the elements of its orbit reveals the fact that they are almost identical with those of Comet III., 1869, and hence it becomes what in astronomical language is called a periodic comet. This will have a period of not over 11, and probably only 5½ years, in which case it must have returned unobserved to perihelion about the middle of the year 1875. In either case it will be a periodic comet of short period.

I am indebted to the kindness of Prof. S. C. Chandler, Jr., of Boston, for the following set of elements, which, however, owing to the inexact determinations of the three positions used for their computation, must, of course, be considered only as approximations. They are, no doubt, near enough to the truth to establish the fact that Comet IV., 1880, is a return of Comet III., 1869, for it is almost an impossibility for two different comets to come into our system possessing physical characteristics so similar, and having elements so nearly alike. I copy both sets of elements for comparison :

	Comet III., 1869.	Comet IV., 1880.
Per. passage.	Nov. 20.854.	Nov. 7.714.
	° ′ ″	° ′
Lon. per.	41 17 12.5	41 41
Lon. node	292 40 28.8	295 25.4
ι	6 55 0	7 21.7
Log. q.	0.042416	0.04262
Motion.	Direct.	Direct.

If the above supposition regarding the identity of the two comets be true, it will add another to the list of periodic comets, bringing the number up to eleven. Their names are as follows :

Name.	Period.
Halley's	76.75 years.
Encke's	3.30 "
Winnecke's	5.54 "
Brorsen's	5.58 "
Biela's	6.61 "
D'Arrest's	6.64 "
Tempel's (1867)	6.00 "
Tempel's (1873)	5.16 "
Faye's	7.44 "
Tuttle's	13.66 "

From the above list I have rejected De Vico's comet, which should not have been placed there, as the supposed periodicity has never been verified by an observed return.

There can be but little doubt that to this list should be added comet I, 1880, commonly called the great South American comet, with elements and general appearance almost identical with the great comet of 1843, one of the most remarkable comets mentioned in history. It was seen in the daytime, close to the sun's limb, glowing like a coal of fire. Of all known comets, it has made the nearest approach to the sun. It was truly said of it : "It exhausted its head in the manufacture of its tail," for it was nearly all tail.

As an evidence of the advance which cometary astronomy has made in our times, it may be stated that up to 1822 one only, (Halley's) periodic comet, was known. The number of such is doubtless very great, in fact computation makes the number several hundred, but until verified by actual returns to perihelion, the question of periodicity cannot be affirmed with positiveness. Every few years a new one is added to the list, but during the centuries and milleniums which are to come, the number must swell to thousands.

Prof. Chandler is computing a new set of elements from more trustworthy data, but, as the comet is running well with those first published, the new set will probably differ but little from the first. The discovery of this comet was immediately cabled to Europe, and I have received official announcement that the cablegram was duly received, but it seems that it was not discovered there until November 7, when, not knowing but it might possibly he a new one, it was cabled here as such.

It has never, to my knowledge, been published in this country, that the Vienna Academy has rescinded its offer of prizes for the discovery of comets; therefore I expect no gold medal for the discovery of this, but your readers may be surprised, perhaps pleased, to learn that Mr. H. H. Warner, the well-known medicine man, who is building the new observatory for my use, gave me his check for $500 for its discovery. This, together with the three gold medals awarded me by the Imperial Academy of Sciences of Vienna, is a partial remuneration for the labor and the unknown suffering endured from cold and want of sleep during the many years I have followed comet seeking in the open air, with no protection from the piercing winds of our northern winters.

The following are a few positions of the comet from Chandler's ephemeris for Washington midnight.

	h. m. s.	° ′
November 20	1 9 18	Dec. + 54 3
24	2 6 19	54 25
28	2 58 39	53 3

LEWIS SWIFT.

ROCHESTER, *Nov.* 17, 1880.

COMET E 1880.

This comet, discovered by Mr. Swift on October 10th, proves to be an interesting object. An orbit has been computed by Mr. Winslow Upton, of the Naval Observatory, from the observation made here by Professor Eastman, and there can be no doubt that this is a return of the comet discovered by Mr. Tempel, November 27, 1869, since the elements of the two orbits are very nearly alike. The periodic time of this comet is therefore nearly eleven years, and its mean distances from the sun is a little less than that of Jupiter. A. HALL.

WASHINGTON, *Nov.* 11, 1880.

ASTRONOMICAL NOTES.

THE corrections employed in reducing the double star observations of M. Otto Struve, given in Vol. IX., of the Poulkova observations were only provisional. Since the publication of that volume definitive corrections have been computed by M. Dubiago, and the corrected results are now published as an appendix.

AT the meeting of the American Association this Summer, Professor Stone gave a description of the continuation of Argelander's *Durchmusterung* now in progress at the Cincinnati Observatory. The zone will extend from 23° to 31° south declination. A four inch equatorial is employed.

PART III of the *Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac* is devoted to Master Michelson's determination of the velocity of light. A minute description of the apparatus employed is given, together with the determination of the errors to which the observations were subject. In the latter part of the work several objections to the plan followed by Foucault are considered.

VOL. VI, of the *Annales de l'Observatoire de Moscow* contains an interesting series of observations of Jupiter made during the opposition of 1879. Nearly forty drawings are given, twenty-seven of which were made at times when the large red spot was visible.

AN attempt to photograph stellar spectra was made by Drs. Huggins and Miller, as long ago as 1863, but not with the best of success. Dr. Huggins has published in the last volume of the Philosophical Transactions, the results of a recent, and this time successful, attempt, and at the end of the paper has given a map of the spectra of several of the stars observed. These are α Lyrae, Sirius, η Ursae Majoris, α Virginis, α Aquilae, α Cygni, and Arcturus. With the exception of the latter these are all white stars and were observed on account of the remarkable circumstance of the absence of the K line in one of the earlier photographs of Sirius. "The photographs present a spectrum of twelve very strong lines. Beyond these lines a strong continuous spectrum can be traced as far as S, but without any further indication of lines. The least refrangible of these lines is co-incident with the line (ζ) of hydrogen near G. The next line in order of greater refrangibility agrees in position with h of the solar spectrum. The thirdl ine is H, K, if present at all, is thin and inconspicuous. The nine lines which follow do not appear to be co-incident with any of the stronger lines of the solar spectrum." The symmetry of arrangement of these lines is such as to suggest that they are the spectrum of a single substance, perhaps hydrogen.

The spectrum of Arcturus is very different from that of the other stars named, but quite similar to that of the sun. The spectrum is crowded with a vast number of fine lines, and in further contrast with the class of white stars the line K is very broad and winged and more intense than H. Beyond K the lines are broader and more intense and arranged more or less in groups with fine lines between. Although the crowding continues as far as the spectrum can be seen on the plate the position and arrangement of the lines beyond H is quite different from those in the solar spectrum.

Photographs of the spectra of Venus, Mars and Jupiter were also taken, but these showed no modification whatever of the solar light. In the case of the moon most of the photographs presented differences in the relative intensity of the ultra violet region, but nothing that could be taken as evidence of the existence of a lunar atmosphere. O. S.

Prof. C. A. Young, of Princeton, has been fortunate enough to obtain one of the finest large-crown glass discs ever cast. It is of French manufacture, 22 inches in diameter and without a flaw. Alvan Clark & Sons are finishing it for the new Princeton refractor.

Dr. B. A. Gould, Director of the Cordoba Observatory, Argentine Republic, was in Boston, November 3, on a visit to this country and returns to Cordoba on the steamer of the 27th November. His address is 110 Marlboro street, Boston.

Dr. Elkin, whose work on the Parallax of α Centauri has been previously noticed, is spending a few weeks in Washington. He expects to leave shortly for the Cape of Good Hope, where he will continue his investigations upon the Parallax, using for that purpose Lord Lindsay's four-inch Heliometer, which he is to take out with him.

W. C. W.

BOOKS RECEIVED.

British Thoughts and Thinkers—Introductory Studies—Critical, Biographical and Philosophical. By George S. Morris, A. M. Lecturer on Philosophy in the Johns Hopkins University, Baltimore—S. C. Griggs and Company, Chicago. 1880.

To trace the progress of the human mind and its highest aspirations, must always demand the close attention of an author of the highest intelligence and perfectly unbiased reasoning faculties; because it is easy to understand that, with such a mass of material to draw from, deductions of the most varied character may be drawn, which may accord with almost any form of belief or system of philosophy, by a mere judicious selecting and rejection of authorities.

The present author has evidently commenced his task with certain philosophical convictions strongly established in his own mind, and the purpose of his book is to place them in proper order before his readers, showing the high authorities that may be cited for their support and as evidence of their truth.

The aim of Professor George S. Morris is to assert the idealism which is innate in the universal mind of man, which is no accident, but a constituent and necessary element of human nature, and in fact, that which constitutes it. This idealism teaches mind to have faith in itself, to know itself. He refers to mind, or conscious intelligence, as an active function, not simply a passive possession; strictly passive, it were no longer intelligence, for then inactive, it would not have intelligence of itself. He states still further that intelligence is only of the intelligible, reason apprehends only what is rational—mind therefore can comprehend no world which is not permeated with its own attributes; the absolutely unintelligible, irrational, being inconceivable, and hence utterly incapable of being brought into relation to mind is for it no better than the non-existent.

Mind therefore seeks itself in the universe, chiefly in forms of law, order, purpose, beauty—it must reduce its conception of the universe, given first in the form of isolated, unexplained impressions, to the order and harmony of a rational and hence explicable apprehensible whole. And this search, this necessity of mind, again, precisely, is *idealism*.

Such in the view of Professor Morris, is the law, the universal tendency and the inherent necessity of mind.

Man having no exact conception of an idea apart from the mind which possesses it, cannot conceive rationality, except as the attribute and living function of a mind or spirit. The rationality therefore found in nature is an *absurdum* unless viewed as the direct or indirect effect and function of self-conscious spirit. The idealism (in theory) which holds fast to these axioms, acknowledges God, whose rational power and wisdom it detects in all things. So man in his humble way is brought into direct and sympathetic relation with the universal, all-pervading, all-explaining power.

Such being the strong belief of Professor Morris he naturally reads with horror, in the works of Mr. Herbert Spencer, of Man being merely sensitive flesh, and morality the irresponsible result of physico-organic evolution, and not the self sustaining work or requirement of the ideal true man.

As representatives of two opposite shades of opinion, it would scarcely be possible to select more appropriately, two men with more divulgent views than Professor Morris and Mr. Spencer. The former sees no limit to the possibilities of his system of reasoning, while the latter insists that whatever is not cognizable, through the investigations of phenomena by the peculiar method, and with the peculiar and generally recognized limitations of physical science, is arbitarily held to be unknowable.

It is clear that Professor Morris approaches the subject of Mr. Spencer's system of philosophy strongly biased against it, and when he stigmatizes Spencer's views as gratuitous, extra-scientific, absurd, contradictory and dogmatic, we would caution students, for whom this work is principally written, to read the works of Spencer before accepting Professor Morris's conclusions.

The work which we now review will doubtless command a large circulation. It was founded on a course of lectures delivered at the Johns Hopkins University, Baltimore, and is, therefore, well adapted for students, but as a work for the general reader it will prove highly attractive, presenting in a small compass a synopsis of the works and record of the lives of such men as Edmund Spenser, Richard Hooker, Shakespeare, Bacon, Hobbes, John Locke, George Berkeley, David Hume, Sir William Hamilton, John Stewart Mill, Herbert Spencer, and others.

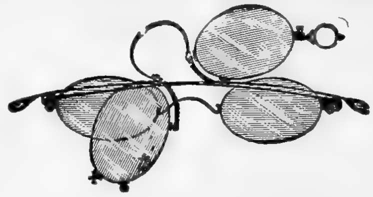
Credit is due to Professor Morris for his skillful method of handling subjects presenting so many difficulties, and the general arrangement of the work is harmonious, consistent and intelligible. The appearance of this work at the present time is most opportune, and as an introduction to the line of thought which speculative philosophy has taken, from Lord Bacon's time to the present day, a more useful book cannot be selected. The deliberate opinions, so forcibly and ably engrafted throughout the work, while merely intended to point the way to correct views, considered from the position taken by the author, may even carry conviction with them. We, however, strongly advise the student to accept the book in the spirit in which it is offered, and to regard it as an invitation to reflection and more systematic study rather than as a substitute for it.

PHYSICAL NOTES.

Polar Electricity in the Hemihedral Crystals with Inclined Surfaces.—MM. Jacques and Pierre Curie have shown that all the facts hitherto observed agree in showing that in all the non-conductive substances with inclined surfaces which have been examined there is the same connection between the position of the hemihedral facettes and the direction of the phenomenon of polar electricity. The physical signification of the above will be better understood by saying more colloquially, but more tersely, that the more pointed extremity of the hemihedral form corresponds to the positive pole by contraction, whilst the more obtuse extremity corresponds to the negative pole.—M. P. Thenard claims that the same phenomenon was observed by his son fifteen years ago.

Production of Crystals of Chromium Sesquichloride of a Persistent Green Color.—M. A. Mengeot allows hydrochloric acid to act upon potassium bichromate dissolved in water. If the solution is allowed to evaporate for about ten months the bottom of the vessel is found lined with deep violet crystals of chromium sesquichloride, but among these large violet crystals are some small green crystals of a salt of chromium. According to all authorities the green salts are only formed at 100°; they are not crystalline, and they gradually pass into the violet condition. But the production of these green crystals takes place at common temperatures, and they have remained green for more than two years.

Researches on Basic Salts and on Atacamite.—M. Berthelot considers that in this compound, $CuCl,3CuO,4HO$, the water serves as the chief connecting link. A metallic salt may be completely precipitated and the resulting liquid neutralized without an equivalence between the precipitating alkali and the acid of the metallic salt, a portion of the lattar being carried down in the precipitate. A great number of metallic salts behave in an analogous manner. M. Berthelot has also found that the transformation of the simple ethers into alcohols corresponds in a state of solution to a thermic phenomenon, which is almost *nil*.



SCIENCE:

A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.

Vol. I, No. 21. - - - - - - - - November 20, 1880.

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JOHN MICHELS, Editor.

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SATURDAY, NOVEMBER 27, 1880.

The editorial on the American Society of Microscopists has called forth a reply from Professor Hitchcock, who, as editor of the *American Monthly Microscopical Journal*, publicly raised the question of the necessity for the dissolution of the Society. Professor Hitchcock now states that when he spoke of the leaders being incompetent, as a cause of the want of success of the Society, he did not refer to the Presidents who have held office, but to some people, whom he does not name, who were active in the organization of the Society.

We accept this explanation, as it removes an unnecessary personal question from the discussion, although it is not clear why mention should be made of these delinquents now, if the Society has never been in their power.

Other minor reasons may now be dropped, as Professor Hitchcock states that his objection to the Society is fundamental, and does not alone rest on the side issues he described so fully.

In another portion of this issue we publish a statement bearing on this matter, which will appear next month in the *American Naturalist*, an advanced sheet of which has been kindly furnished to us. This is written by Dr. R. H. Ward, of Troy, the first President of the Society. Dr. Ward puts the case in his usual clear and forcible manner, and the simple merits of the case, from a scientific point of view, are stated with precision.

It now appears that nearly half of the delegates, who created the Society, were opposed to its organization as a separate body; a part of these have since formed a "cabal," and like the original inhabitants of the Cave of Abdullah, are restless and discontented, determined on the destruction of the Society, rather than to promote its success.

Dr. Ward authoritatively calls upon the members of the Society for unity of purpose and action; he gives excellent reasons for keeping the Society intact and maintaining its independence and freedom; but if the peaceful work of the Society can be continued only by the sacrifice of the opinions of the majority to those of the turbulent minority, then he is willing to let the sacrifice be made. In a word, Dr. Ward says, cease the squabbling and get to work.

We quite endorse Dr. Ward's advice, and are equally indifferent respecting the name of the organization; the reasons he gives for not amalgamating with the A. A. A. S., will carry conviction to those not influenced by personal or petty considerations. Why should the Society cancel its freedom of action, become a mere sub-section of another Society, and be hampered with a set of rules and regulations which are most undesirable, and from which there can be no escape?

We might add that the A. A. A. S. is becoming already overloaded with its sections and sub-sections, and if the work to be done at its meetings increases at the present ratio, the resources of the Society to perform it in a week will be very heavily taxed.

We find no fault with Professor Hitchcock for the article he prepared, as he evidently is but the mouthpiece of many members of the Society, and rather give him credit for his candid utterances. This undercurrent of restlessness is as old as the Society, and it is as well that he has given public expression to it; we, however, trust that he will admit the force of Dr. Ward's reasoning, and as Editor of a Microscopical Journal, endeavor to use his influence to restore full harmony to the Society, and remind those who prefer the sub-section of the A. A. A. S. to the American Society of Microscopists, that no impediment exists to the gratification of their wishes; two courses are open to them; they can make use of either of the societies, or even attend both.

THOSE interested in the progress of Physiology in this country will be glad to learn that, at their recent meeting, the Regents of the University of Michigan appointed Dr. Charles H. Stowell Assistant Professor of Physiology in the Department of Medicine and Surgery. Dr. Stowell is a graduate of the Institution, and since 1876, has been in charge of a flourishing laboratory of Practical Physiology and Histology which was then established at the suggestion of the veteran professor of anatomy and physiology, Dr. Corydon L. Ford. Dr. Stowell has also been delivering part of the physiological lectures, and has made some interesting observations and experiments. B. G. W.

THE NATIONAL ACADEMY OF SCIENCES.

The National Academy of Sciences met on Tuesday, the 16th inst., at Columbia College, New York city, and continued in session during the three following days. The President, Dr. William B. Rogers, was prevented by sickness from being present, and the chair was occupied by Professor O. C. Marsh, of Yale College, the Vice-President of the Society.

Among the members present were: John H. C. Coffin, U. S. N.; Professor George F. Barker, Philadelphia; James Hall, Albany; Samuel H. Scudder, Cambridge, Mass.; Professor Charles F. Chandler, Columbia College; Professor Walcott Gibbs, Cambridge, Mass.; J. Hammond Trumbull, Hartford; J. Sterry Hunt, Montreal; Professor B. Silliman, Yale College; Professor E. C. Pickering, Cambridge, Mass.; Professor C. A. Young, Princeton; Louis M. Rutherford, New York; E. H. F. Peters, Hamilton College; Edward S. Morse, Salem, Mass.; Professor Edward D. Cope, Philadelphia; Professor H. A. Newton, New Haven; Professor Alfred M. Meyer, Hoboken; Professor J. S. Newberry, Columbia School of Mines; Professor Henry Morton, Hoboken; Professor John W. Draper, Hastings, N. Y. Professor Ogden N. Rood, and Professor Eggleston, New York; Professor S. F. Baird, Washington; Professor William H. Brewer, of Yale College, and Professor A. Guyot, of Princeton, N. J.; Professor George J. Brush, of New York.

Professor Marsh, after calling the Academy to order, stated that the present session was for the reading of scientific papers only.

We postpone until next week the report of the papers read at this meeting of the Academy, to enable authors to prepare abstracts, or correct those already rendered.

THE ANTHROPOLOGICAL SOCIETY.

The Anthropological Society of Washington met November 16, in the Smithsonian Institution, Dr. J. Meredeth Toner in the chair. Two papers were read: "Aboriginal Remains in the Valley of the Shenandoah River," by Dr. Elmer R. Reynolds, and "Tuckahoe or Indian Bread-root," by Professor J. Howard Gore. Dr. Reynolds was one of a company sent out last Summer to examine the celebrated Luray cave. While upon this journey he was so fortunate as to discover in the vicinity of Luray a group of three very interesting mounds, one of which he examined in person and received the report of the exploration of others from some of the residents of the valley. The tumulus opened by Dr. Reynolds was identical in its strata with many opened in the Mississippi valley, and refutes the oft-repeated theory that no mounds are to be found on waters emptying into the Atlantic ocean. There were in this mound forty-three chipped implements, four tablets, pieces of pottery, four plates of mica, charred bones (indicative of cremation), quartz crystals, lumps of white quarzite and rude flakes. These objects were grouped about the head of the buried chieftain.

In regard to the second paper, Mr. Gore first mentioned the circumstances which suggested the subject for investigation, and the unsettled condition of the various theories now held concerning the nature and use of Tuckahoe. The early writers attributed to it great nutritive qualities, and nearly every author writing upon the subject since then has made the same assertion. In order to determine its exact value as an article of sustenance to the Indians, it was necessary to ascertain the geographical distribution, and the prevalence of Tuckahoe in those localities.

This was accomplished by sending circulars of inquiry through the Smithsonian Institution to nearly every Cryptogamic Botanist in the United States, to the newspapers along the Atlantic coast and in the Mississippi valley.

It is found that it is more or less abundant in the States from New Jerssy to Florada, in Kansas and Arkansas.

The question "Does its growth depend upon circumstances always existing?" was answered by giving an outline of the process of its development, and specimens were exhibited by way of proof. Likewise the means by which it could have been found by the natives, if its value as food was sufficient to pay for the trouble.

Its exact nutritive value was determined by an elaborate analysis made by Dr. Parsons, which gave only three-fourths of one per cent. of nitrogenous matter; this being insufficient to repair the waste in the animal tissues it was pronounced *valueless* as *food*.

The speaker then suggested that there must have been other roots or tubers called Tuckahoe, and quoted from a number of histories, showing that a root by this name was frequently described, which was entirely different from the one in question, finally succeeding in identifying five roots, which were once known as Tuckahoe, or similar to roots known as such. Also the derivation of the word Tuckahoe given the speaker by the distinguished Ethnologist, Dr. Trumbull, shows that it is from "pluckqui," meaning something round, or rounded, and not from a word meaning bread as heretofore supposed.

The conclusion then given was, that Tuckahoe was a term applied to all roots which were rendered esculent by cooking, until all of these, except *Pachyma cocos*, received a special name, this alone retaining the appellation Tuckahoe; and that when we read of Tuckahoe contributing so largely towards the support of the aborigines, we can only know that an edible root was referred to. The paper was illustrated by six large charts, giving twelve Botanical Synonyms, eight Affinities, five roots once known as Tuckahoe; an analysis of one of these, showing that it was nutritious, ten Indian Synonyms, and an analysis of Tuckahoe.

ASTRONOMY.

THE VELOCITY OF LIGHT.

Vol. I, Part III, of the "Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac," containing the experiments upon the velocity of light, made by Master A. A. Michelson, U. S. N., has just been published. Mr. Michelson read a paper upon this subject at the St. Louis meeting of the American Association in 1878, and has since published the results of his work in the *American Journal of Science, Third Series, vol.* 18, *page* 390, so that his method of investigation (an improved form of Foucault's method) may be considered not unfamiliar. In brief this method is as follows: A beam of light is allowed to pass through a slit and to fall upon the face of a mirror free to move about a vertical axis. From this free mirror the light passes through a lens of long focus, and falls upon a fixed plane (or slightly concave) mirror, from which it is returned through the lens to the movable mirror, and thence, if the mirror is at rest, to the slit. If, however, the movable mirror is made to revolve rapidly, the light will not return directly to the slit, but will be deviated by a certain amount which depends upon the time it takes the light to transverse twice the space between the mirrors, and also upon the distance through which the mirror has revolved during that time.

It is upon the accuracy of the measurement of this displacement that the value of the determination largely depends; and to render the displacement as great as possible, Mr. Michelson placed the revolving mirror within the principal focus of the lens, and increased the speeed of rotation. The lens, having a focal length of 150 feet, was at a distance of about 80 feet from the revolving mirror, and the fixed mirror at a distance of about 2000 feet. By this arrangement, with a speed of rotation of 257 turns per second, he obtained a deflection of 115 millimeters; whereas, Foucault using a speed of 400 turns per second, and causing the light to traverse a distance of 20 meters, had obtained a deflection of 8 millimeters. The revolving mirror was driven by a turbine-wheel operated by a blast of air. Its speed, which was measured by an electric tuning-fork, was readily adjusted by a stop-cock, and the deflection was measured by a micrometer.

Mr. Michelson gives a most careful discussion of the errors of his constants, including the determination of the value of the micrometer screw, the rate of vibration of the tuning fork, etc., and concludes with the consideration of several "objections" which have been suggested from time to time.

The final value for the velocity of light *in vacuo* is 299944 ± 51 (in air, 299864), or, in round numbers, 299940 kilometers per second = 186380 miles per second,* the remarkably small error, ± 51 kilometers, being composed of the total constant error in the most unfavorable case, and the probable errors of observation. This quantity, ± 51 kilometers, cannot be said to express the *probable error* of the determination, in the ordinary acceptance of the term; combining, as it does, *probable errors*, strictly speaking, and estimated constant errors.

These experiments were made by Master Michelson at the Naval Academy, Annapolis, at private expense, and to him the entire credit is due. A new determination of the velocity of light, embodying essentially the same arrangement, but with more elaborate and expensive apparatus, is now being made under official auspices by Prof. Newcomb, Superintendent of the Nautical Almanac.

It is probable that in this way the most accurate value of the solar parallax, so essential to astronomy, can be deduced.

ASTRONOMICAL MEMORANDA.—(Computed for the meridian of Washington, D. C., November 29, 1880):

	H.	M.	S.
Sidereal time os mean noon	16	35	50
Equation of time		11	15

mean noon *following* apparent noon.

The *Sun* is 21° 39′ south of the equator, at meridian transit, and will continue to move south until December 21.

The *Moon* reached its last quarter on November 24d. 8h. 47m. It does not come to the meridian until 10 A.M. of November 30.

Mercury was in inferior conjunction, November 23, and is not now visible to the naked eye. It precedes the sun by about 52 minutes, and is five degrees farther North.

Venus is plainly seen in the southwest; a short time after sunset; following the sun by 2h. 33m., and gradually increasing the distance. Its declination is 24° 43′ south.

Mars is at present too close to the sun for observation.

Jupiter, though gradually growing fainter, is still the most brilliant object in our eastern sky at evening. It passes the meridian at 8 P.M., at an altitude of 54° above the southern horizon. Its more exact position at that time is: Right Ascension, oh. 37m. 43s.; declination, +2° 28′.

Saturn, less brilliant than Jupiter, is, notwithstanding, equal or superior to the larger planet in point of interest. It is readily found about 13° E. by N. of Jupiter, presenting a good view of the southern side of its rings.

Uranus is in right ascension 11h. om. 52s. declination +7° 9′.

Neptune is in excellent position for observation, reaching the meridian at about 10 P. M. It was in opposition on November 4, and may now be found in Right Ascension 2h. 41m. 27s. declination +13° 46′.

A new 10 in. equatorial, with an object glass by Merz, has been presented to the Geneva Observatory by its director, Prof. Emil Plantamour. It is to be devoted to observations of the major planets and their satellites, of parallax of stars, and of double stars, with occasional observations of minor planets.

DR. Schmidt has made a new determination of the time of rotation of Jupiter upon its axis, from observations in 1879 and 1880, of the red spot upon its disc. His preliminary discussion gives for the time of rotation 9h. 55m. 34.42s.

IN a letter to *Nature*, dated October 2, Prof. Pickering, of Harvard College Observatory, announces that the period of Ceraski's new variable star is probably 2.5 days, instead of 5 days, as previously published by Schmidt. It is especially remarkable for the rapidity of change during part of its period. The total variation is from about the 6.7th to the 10th magnitude. The approximate place for 1881 is, R. A. oh. 51m. 48s. Dec. +81° 14′.1. W. C. W.

WASHINGTON, D. C., Nov. 23, 1880.

MICROSCOPICAL COLLECTIONS IN FLORIDA.*

BY DR. C. C. MERRIMAN.

It has been my fortune during the past two Winters to spend a few weeks in the regions of Central Florida. Lake Harris is the most southern and the most beautiful of the cluster of lakes which forms the source of that exceedingly picturesque river, the Ocklawaha. With high banks, and surrounded by a belt of hummock land as rich as any that Florida affords, this lake is becoming settled upon, and its lands are fast being taken up by enterprising southerners for orange-groves and pine-apple plantations. The sojourner will find the society of this lake-settlement intelligent and hospitable beyond anything that would be expected in so new and pioneer a country. The vegetation of this almost tropical region is so full of interest to the microscopist, and the causes conducing thereto so peculiar, that I have thought them deserving of especial mention and illustration.

The absence, or at least the rarity of frosts injurious to vegetation in these lake districts, gives the longest possible season for the growth and maturity of such organs as are best, or especially, adapted to the exigences of Florida plants. There is a period of rest, usually comprising about the three Winter months, after which vegetation takes up and continues its growth again as if there had been no period of interruption; so that practically there is a continous development of plant life, whether annual or perennial, from birth to death.

The soil of Florida, as of all the South-Atlantic seaboard, is sandy and naturally barren. No polar glaciers have ground up for these regions, as for the Northern States, a rich and abundant alluvium, sufficient in itself for the production of a rapid and vigorous vegetation. The South has apparently only the siftings of our Northern soil, carried down to the ocean by rivers, and then washed up by the sea-waves to form their interminable sandy plains. But to compensate for this natural infertility of soil, the atmosphere, especially of Southern Florida, abounds in all the elements of plant growth. The winds which come up from the Gulf on one side, or the Atlantic on the other, are charged with moisture, and bear also minute quantities of nitric acid and saline compounds; while the exhalations from the swamps and marshes furnish in abundance the salts of ammonia and carbonic acid. Now to utilize these precious products from the air, it is necessary for plants to

* NOTE.—Foucault's experiments gave the velocity as 185200 miles per second.

*Read before the Sub-section of Microscopy of the A. A. A. S.

have peculiar organs, such as absorbing glands, glandular hairs, stellate hairs, protecting scales, and a variety of other special appendages. All these have been developed by time and necessity, in remarkable profusion and perfection in the vegetation of Southern Florida. Although the meagre soil produces no nutritious grasses, and scarcely enough of an honest vegetation to keep an herbivorous animal from starving; yet there is an abundant flora such as it is—air plants, parasitic growths, insectivorous plants, and strange herbs seeking a livelihood in any other way than the good old honest one of growing from their roots. It is this fact which makes the microscopial interest of botanical researches in Central Florida. One can scarcely examine with a two-thirds objective the flowers, leaves or stems of any plant growing there without discovering some beautiful or striking modification of plant hairs, or scales, or glands, or other absorbing or secreting organs.

We will notice first the Onosmodium as found in Florida —*O. virginianum*. It grows from Virginia south, but is more glandular I think, in Florida than anywhere else. It will be almost the first plant one would stop to observe on entering the pine woods—a dark-green, narrow-leaved, biennial herb; its straight stem of the second year's growth, about a foot high, bearing a raceme-like cluster of flowers, coiled at the end, and straightening out as the flowers expand. The leaves of this plant are thickly studded on both sides with stiff transparent hairs, lying nearly flat on the surface, and all pointing toward the tip end of the leaf. At the base of each hair is a cluster of glandular cells, amounting sometimes to fifty or more, arranged in beautiful geometrical forms. When pressed and dried in the herbarium, the body of the leaf turns to a dark green, almost black, and on this back-ground, with a half-inch objective, the hairs stand out like sculptured glass, and the glands like mosaics of purest pearls. I think it is the most attractive opaque object that can be shown under the microscope.

That these glandular cells, covering, as they do, nearly half the surface of the leaves, especially the upper surface, and differing from all other vegetable cells, subserve an important purpose in the sustenance of the plant, there cannot be any doubt; but just what that purpose is, or what is the mode of operation, I think, has never been ascertained.

In the same locality will very likely be found the most beautiful of all the Croton plants, the *C. argyranthemum*. Unlike the other Crotons, which are bushes, this is an herb growing only about a foot high, with a milky sap which exudes when the stem is broken. The leaves are silvery, verging in some cases to a bronze color, and are thickly covered on the upper side with most remarkable and beautiful stellate scales. The flower-buds and stem, when pressed, make much more beautiful opaque objects than the leaves.

The object of these scales is, without doubt, to prevent the too rapid evaporation of the moisture stored up in the plant. They are the exquisitely woven blankets which preserve the precious juices so laboriously gathered. The same kind of covering is spread over the leaves and stems of all the air-plants of Florida, and doubtless for the same purpose. The well-known Florida moss, although not a moss, but a member of the pine-apple family (*Tillandsia usneoides*), is an exceedingly beautiful object under the microscope. Each hanging stem is overlaid with filmy white scales, every one of which is fastened in its place by what would seem to be the stamp of some miniature seal on golden-tinted wax. This plant, as ordinarily seen on the live-oaks near cities, is a dirty-looking and unattractive object, and goes by the name of "black moss." But in out-of-the-way places, removed from the dust and smoke of settled localities, it is pearly white, and exceedingly beautiful both to the naked eye and under any power of magnification. Florida moss should be preserved with only a very slight pressure, just enough to make the threads lie straight. After it has dried in this way, small cuttings may be mounted in the ordinary cells for opaque mounting.

On the high banks of the lake, and in the adjoining fields may be found the large-leaved and vigorous-growing Calicarpa (*C. Americana*), sometimes called the French mulberry, a bush growing some five or six feet in height. The under side of the leaves of this plant are nearly covered with little round, yellow, sessile glands, flattened on top and marked off into eight ten sections by ribs like those on a melon. They are in immense numbers—something like thirty thousand to the square inch—over half a million on a good-sized leaf. Under a light net-work of branching glandular hairs, viewed with a two-thirds objective, these polished ambor-colored disks glisten like a spangle of golden beads. The same kind of glands is found on the leaves of many other shrubs in Florida—the sweet myrtle (*Myrica cerifera*), the low-ground blueberry (*Vaccinium tenellum*) a certain bush or dwarf hickory (*Carya glabra*) and some others. These glands have been variously called resin dots, resin glands and odoriferous glands. So far as I can judge, however, they are not connected with any resinous or odoriferous secretions. From their almost perfect resemblance to the terminal bulb of the mushroon glands of the Pinguicula and Drosera, which are known to be absorbing glands, the probability is that these also serve to absorb moisture and ammonia from the atmosphere and from rains. Although I am free to acknowledge that the position of the glands, being for the most part on the under side of the leaves, militates somewhat against this view of their purpose.

Great care will have to be taken in pressing and drying vegetable specimens in the moist climate of Florida. The little threads of the mould fungus will be sure to creep over the surface of the leaves, spoiling them for microscopical material, if they are not quickly and effectually dried. For this purpose it is well to have a good supply of the bibulous botanical paper, and to change the specimens every day to fresh sheets for at least four or five days. The sheets, after being once used, should be spread out in the sun to dry. A weight of about thirty pounds may be used for the pressure.

The objects heretofore mentioned are all for opaque mounting. Almost every preparer of slides has his own favorite method for this kind of work. I myself prefer the use of the transparent shellac cells. Clarified shellac is dissolved in alcohol, and filtered through cotton-wool under a bell-glass, and with the application of heat. The solution is evaporated down until it is so thick that it will only just run—almost a jelly. In this condition it can be put on a slide with a camel's hair brush on the turn-table, and very quickly worked up into a ring with the point of a knife, used first on the inside to make the cell of the size wanted, and then on the outside to turn the cement up into a compact ring. Two or three applications of the cement, with intervals of a day or two after each, will make cells of sufficient depth for all ordinary specimens. These cells dry quite slowly; and if artificial heat is used it must be increased only very gradually, otherwise vapor of alcohol bubbles will make their appearance in them. A small ring of Brunswick black may be made in the inside of the cell, to which, when thoroughly dry, the object may be fastened with a very little liquid marine glue. In this case both sides of the leaf can be seen, which is often desirable. In all opaque mountings a minute aperture should in some way be left open into the inside of the cell, so that it shall not be hermetically sealed up. This little precaution will save an innumerable number of failures.

The collector in Florida will not fail to secure a supply of the leaf stems of the castor oil plant (*Ricinus communis*). In regions beyond the influence of frosts, this plant grows continuously from year to year, and becomes quite a tree. It is only in such a growth that the spiral tissue of the fibro-vascular bundles is fully perfected. The castor oil plants grown in our climate during one short season, will furnish very little spiral tissue, mostly spotted ducts and scalariform cells. There is no more beautiful object for multiple staining than thin longitudinal sections through the woody fiber, the vascular tissues, and the pith cells of well matured leaf-stems of the castor oil plant. I will briefly describe my process of making these stainings. After being decolorized in chlorinated soda, the sections may be left for half a day or more in a solution of carmine in water containing a few drops of aqua ammoniæ; then for half an hour in a rather weak solution of extract of logwood in alum water, and finally 10 to 15 minutes in a weak solution of anilin violet or blue in alcohol. From this they can be carried through absolute alcohol into turpentine, and mounted in balsam at any time thereafter. If successful in this staining you will have the pith cells in red, the spiral tissue in blue, the wood cells in purple and the stellate crystals in green or yellow.

But the chief objects of interest to the microscopists in the vegetation of Florida, are the insectivorous plants. Not only are they more abundant, and, as I think, more perfectly developed in the central lake regions of Florida, but some varieties are found there differing, it seems to me, from any found elsewhere. I desire particularly to mention one which I discovered, and which perhaps might be entitled to rank as a new species.

In a lagoon-like basin at the side of a small lake near Lake Harris, in water from two to three feet deep, I found numerous specimens of the insectivorous plant known as the Drosera or Sun-dew, growing thriftily and floating about among the scattered water-weeds, without any attachment whatever, indeed with very little root of any kind, the dead leaves that hung down in the water seeming both to buoy it up and to hold it upright. This plant differs from all the described species of Drosera, so far as I have been able to to ascertain, in having an upright, leaf-bearing stem from four to five inches long, in floating free on the water, and in having unusually long, vigorous and numerous leaves. As I never found this floating Drosera in any other location, and as there was an abundance of the ordinary *Drosera longifolia* growing on the adjoining shore, I could not resist the suspicion that at this very spot in some past time a plant of the *longifolia* had by accident become uprooted, and floated out on the water—that finding it could capture insects even better on the water than crowded among shore plants, it adapted itself permanently to its new location and modes of growth. It appeared to me quite within the bounds of probability that here was an instance of the evolution of a species *in loco*.

The Drosera or "sun-dew" is found on the margins of nearly all small ponds and permanently wet places throughout the south. It is a small red plant, growing close to the ground, and glistening in the sunlight. Its little whorl of expanded leaves forms a circlet as beautiful as any flower, and often so very small that I have frequently mounted whole plants with flower-stalk and buds on one slide. Each leaf of the Drosera has, spread out on its upper surface and edges, from two to three hundred arms, called tentacles because endowed with the power of motion, and of such varying lengths that when naturally incurved their ends just meet at the centre of the leaf. Each tentacle has at its extremity a pad, like an extended palm, with a ridge raised lengthwise upon it, and in this palm is a bundle of spiral vessels connected with the same tissues in the leaf. Now all the tentacles secrete and exude from the glands at their ends a little drop of a very adhesive fluid; and the glistening of these drops in the sunlight on their usually bright red back-ground, gives to the plant its beauty and its name of the "sun-dew." An insect attracted to and alighting on these leaves is inevitably held fast. The tentacles by which it is held very soon begin to bend towards the centre of the leaf, carrying the fly with them. Then in some mysterious way, intelligence is communicated to the other tentacles, and they too begin to turn towards the centre of the leaf, in the course of an hour or two completely covering the captural prey. If the insect is caught entirely on one side of the leaf, then only the tentacles of that side inflect. The glands, after envelopment, exude a gastric fluid which dissolves the nitrogenous matter in the body, after which, by another change of function, they absorb and carry down into the plant all this nutritious little feast. In the course of three or four days the tentacles again expand and prepare themselves for another capture.

There are several reasons which lead me to believe that these unique and most wonderful organs of the Drosera are a direct and special development from the common, simple mushroom glands, which are found on many plants, and which have for their primary function to absorb moisture and ammonia from the atmosphere and from rains. I found on the calyx and flower stem of the Drosera an abundance of these mushroom glands. Indeed the flower stem with its buds furnishes by reason of them, an exceedingly beautiful object for the microscope, both in a natural state and when prepared by double staining.

I have found it quite a general rule as regards plants, that whatever organs, such as stellate hairs or glands, the leaves may possess, the calyx and stem of the flower will show them in far greater luxuriance and beauty. The stellate hairs of the Deutzia, the Crotons, and the Shepherdias are far more numerous and striking on the flower buds than on the leaves. The mushroom glands which are found on the leaves of the Saxifrage and Pinguicula, are multiplied many fold in number and attractiveness on the calyx and flower stem of these plants. So I regard that this was once the case with the Drosera; and that the mushroom glands, which are now found on the flower, were then common to the leaves. A process of evolution has transformed them on the leaves into those wonderful motile arms adapted to the capture of insects, but has left them unchanged on the flower, where that function would be of no use to the plant. I occasionally find in my preparations a solitary mushroom gland among the tentacles of the leaf—a remnant of a race that has been supplanted. There is found in Portugal a plant very similar to the Drosera, the Drosophyllum, which has still only the mushroom glands on its leaves, and catches insects in great quantity by loading them down with the viscid secretion which these glands abundantly pour forth.

To exhibit the very delicate structure of the leaf and tentacles of the Drosera, it is necessary to color them but slightly. The danger will be in over-staining; therefore, after decolorizing and immersing for a few hours in the carmine solution, the specimens should be exposed to only a very weak fresh solution of logwood for fifteen or twenty minutes. If the anilin blue is resorted to at all, it must be in a very weak solution. A mounting of a leaf and a stem with flower buds in one cell in camphorated or carbolated water, makes a very pretty and complete slide for the Drosera.

The Utricularia is a floating, carnivorous plant which grows in the shallow water of quiet ponds. On the surface of the water from five to seven leaves are spread out like the spokes of a wheel, and from the centre of these leaves the plant sends upward its flower stalk and downward its root-like branches, floating freely in the water. Among the thickly branching fibres of these long submerged stems, are perched innumerable little bladders or utricles, not much larger than the head of a pin, each provided with a mouth, at the bottom of a sort of funnel of bristles, closed with a cunning little trap-lid which opens inward, engulfing and imprisoning whatever minute creatures or substances may happen to be resting on it. In these sacks during the growing season, we will find numerous microscopic water fleas, mites and beetles, with grains of pine pollen and other floating particles. The organic bodies will be found in all stages of digestion, showing that the plant derives nourishment from such captured prey; and apparently its only means of livelihood is trapping.

When taken from the water and dried under slight pressure, the submerged portions of the Utricularia will be found literally covered with diatoms; and many very interesting chrysalids of water-insects will be found attached to them. These will all be washed off if the plant is bleached in chlorinated soda. To preserve them it will be necessary to remove the color in alcohol, and besides to handle very carefully. The staining can only be single; and I have found a weak solution of eosin in water, to be the best material for coloring, showing at the same time the structure of the utricles and the captures contained in them. Specimens of new growths, showing the just forming utricles and the peculiar circinate mode of growth, should be included on the slide. The mounting should be in camphorated water.

The Pinguicula, another of the insectivorous plants, is found abundantly on the more open plains, and not far from wet places. It is a compact rosette of very light green leaves, growing close to the ground, from the centre of which rises a single flower-stalk, eight or ten inches high. The leaves have their edges turned up, forming a shallow trough, and on the upper surface are mushroom glands, which exude a viscid secretion. Insects are caught and held by this sticky substance until they die. The nutritious matter is then dissolved out by an acid secretion, and is ultimately absorbed into the substance of the plant by the glands on the leaf. The edge of a leaf when excited by a capture will bend over upon it for a short time ; merely for the purpose, I think, of more effectually securing it, and of bathing it in the secretions. The calyx and flower-stalk, as I have already mentioned, are thickly covered with the same mushroom glands that are found more sparingly on the leaves. I have never seen any evidence that the flower appendages take any part in the digestion of insects. They seem to be rather in the nature of an ornamentation than of anything useful. For exhibition, therefore, or for double-staining, the calyx and flower stem will be found by far the most attractive part of the plant. The best way to preserve them, as well as all such small material, until wanted for use, is to put them green into a common morphia vial with a few drops each, of alcohol and water, and then to cork and seal them up tight with melted beeswax. To prepare them for the slide these objects may be treated precisely as recommended for sections of castor-oil plant, but should be mounted in a weak solution of glycerine in camphorated water.

If cells are made of rings punched out of the thin sheets of colored wax, used by artificial flower makers, and then coated with either liquid marine glue, or a mixture in equal parts of gold size and gum damar, dissolved in benzole, this method of liquid mounting may be as easily and safely performed as mounting in balsam. In very many cases simple water, made antiseptic in any manner, will be found far preferable to any other media, both for retaining the full and distended forms of minute organs, and for bringing out the delicate markings of vegetable structure which the highly refractive balsam would entirely obliterate.

There is only one other insectivorous plant found in Florida—the pitcher plant—*Sarracenia variolaris*, a species growing only in the South-Atlantic States. It is found in low and wet places among the open pine-barrens, but is not as abundant as the others which have been mentioned. The leaf is a hollow, conical or trumpet-shaped tube, with a flange or wing running up one side, and a hood which arches over the orifice of the tube. During the growing season this tube is usually more than half filled with water, which we must suppose secreted by the plant itself, because the hood effectually sheds all rain-water from it. Crowded into the bottom of the tubes of mature leaves, we shall almost invariably find a mass of the hard and indigestible parts of insects. These creatures have been in some way attracted into that suspicious looking receptacle, and once in have been unable to get out again. A mere partially covered tube, however, with a little water in it, is by no means a fly-trap. Not one insect in a hundred would fall into that well and drown, if there were not some special device absolutely preventing it from crawling upward. Now a microscopical examination of the inside of the hood and tube of the pitcher plant reveals the most skilful contrivances for securing insect prey that could possibly be imagined. In the first place, there are in the upper part of the receptacle and about the mouth, great numbers of sessile glands which secrete abundantly a sweet fluid, very attracting to ants and flies. Further, there is on the inner surface of the hood and mouth, a formidable array of comparatively long pike-pointed spines, all pointing backward and downward. These grade off into a shorter, more blunt, but still exceedingly sharp-pointed spines, which overlap each other like tiles on the roof of a house. This kind of coating lines the tube for a third of the way down, the spines growing finer until at last they grade off into regular hairs which line all the lower part of the tube ; spines and hairs all pointing downward. An insect attempting to retrace its steps after its ambrosial feast, would find nothing which it could penetrate or grasp with the hooklets of its feet ; and the wetness of the spines, from the constantly overflowing glands, would probably prevent it from making use of any other device that insects may have for climbing glazed surfaces. As a matter of fact no creature comes out of that prison-house, unless it be with the single exception of one cunning spider, which in some way finds a safe and rich retreat under the hood of its great vegetable rival.

The bodies of the captured prey fall into the fluid in the tube and are macerated or decomposed, but without any signs of putrescence. Therefore the plant must at once absorb the animal matter, for otherwise this would cause the infusorial life, which is called putrefaction.

In order to show the internal structure of the pitcher-plant leaf, it will be necessary to separate the cuticle which bears the spines and glands from the rest of the leaf. To do this, pieces cut from the leaf, and preferably those showing the transition from one kind of spines into another, after being soaked in water, may be put into common nitric acid, and this brought up to the boiling point over an alcohol lamp. They should then be immediately washed in several waters, when it will probably be found that the cuticle, both the inner and the outer, has already separated from the parenchyma. The specimens will need no further bleaching, and may be stained either in eosin, dissolved in water, or in anilin blue in alcohol. As there is only one kind of tissue to be stained, it will be impossible to get more than one color in them. They should be mounted, or kept in water very slightly acidulated with carbolic acid.

I cannot but regard the pitcher plant as the most highly developed, and the most specialized in its organization of any of the insectivorous plants. It differs more widely from ordinary vegetation, and has more special and adapted contrivances about it, than any of the others. Now, as I believe that the truth of the modern evolutionary theory will be eventually brought to the test by well-studied monographs, made by microscopists, on some such highly differentiated organic structures as this pitcher plant, I do not deem it a digression to present here briefly some inferences which seem to me to arise from the developmental history of this particular plant. Of course, if the pitcher plant was developed from other and ordinary plants, it had at one time the simple plain leaves of common herbs. It must have early commenced in some way, to appropriate insect food on these leaves, because every essential change was for the betterment of the plant in this respect. The stem of the leaves soon began to put out flanges or wings on each side —the phyllodia of the botanists, which are not uncommon among plants. And these outspread wings must have assisted in the absorption of insect food that was washed down among them. Then the edges of the wings turned up, and curved around towards each other, until finally they met and grew together, forming a tube and a much more complete receptacle for decomposing animal bodies. A South American genus, the *Heliamphora*, is just in this condition at the present time. Then from some unknown cause and in a way exceedingly difficult to explain, our *Sarracenia* changed entirely its manner of capturing insects. The leaf bent over the orifice of the tube, forming the hood, and those remarkable spines and tiled plates were developed on the inside of the hood and tube, growing backwards, contrary to the order of Nature. When all this was accomplished and fully completed, but not before, our plant could commence its career as the most successful trappist of either the vegetable or the animal kingdom.

Now, according to the Darwinian theory, all these transformations were the result of innumerable slight and accidental variations, each one of which happened to be so beneficial to the particular plant concerned, that it got the start of all the others, and every time run them all out of existence. One cannot tell how many million times this extinction and reproduction must have occured, before our marvellously perfect little fly-trap was finally produced. Excuse me if I confess that not all the canonical books of Darwin are sufficient to make me put faith in the miracles of accidental evolution. I believe in the fact of the gradual development of the organic kingdoms ; for all science teaches it. But I believe it was governed and guided by forces more potent than accident or chance. The Being, or the first cause, if you will, that originated the simple elements of matter, and endowed them with the power and the tendency to aggregate into developing worlds, might equally as well have endowed certain of them with the power and the tendency to aggregate into ever advancing organisms. There is no chance, in the myriad forms of crystalline and chemical substances ; then why should there be in the scarcely more varied colloid forms of living matter ? In a world that unfolds from chaos in one steady line of progress, that shows only design at every advancing stage, I must logically place somewhere at its commencement the Almighty fiat of a Designer.

THE UNITY OF NATURE.

By the Duke of Argyll.

III.

ANIMAL INSTINCT IN ITS RELATIONS TO THE MIND OF MAN.

The Dipper or Water-ousel (*Cinclus aquaticus*) is well known to ornithologists as one of the most curious and interesting of British birds. Its special habitat is clear mountain streams. These it never leaves except to visit the lakes into which or from which they flow. Without the assistance of webbed feet, it has extraordinary powers of swimming and of diving—moving about upon and under the surface with more than the ease and dexterity of a fish—hunting along the bottom as if it had no power to float—floating on the top as if it had no power to sink—now diving where the stream is smooth, now where it is quick and broken, and suddenly reappearing perched on the summit of some projecting point. Its plumage is in perfect harmony with its haunts—dark, with a pure white breast, which looks exactly like one of the flashes of light so numerous in rapid streams, or one of the little balls of foam which loiter among the stones. Its very song is set to the music of rapid waters. From the top of a bank one can often get quite close to it when it is singing, and the harmony of its notes with the tinkling of the stream is really curious. It sings, too, when all other birds but the Robin is silent—when the stones on which it sits are circled and rimmed with ice. No bird, perhaps, is more specially adapted to a very special home and very peculiar habits of life. The same species, or other forms so closely similar as to seem mere varieties, are found in almost every country of the world where there are mountain streams. And yet it is a species having no very near affinity with any other bird, and it constitutes by itself a separate genus. It is therefore a species of great interest to the naturalist, and raises some of the most perplexing questions connected with the "origin of species."

In 1874 a pair of these birds built their nest at Inverary, in a hole in the wall of a small tunnel constructed to carry a rivulet under the walks of a pleasure ground. The season was one of great drought, and the rivulet, during the whole time of incubation and of the growth of the young in the nest, was entirely dry. One of the nestlings, when almost fully fledged, was taken out by the hand for examination, an operation which so alarmed the others that they darted out of the hole, and ran and fluttered down the tunnel towards its mouth. At that point a considerable pool of water had survived the drought, and lay in the paths of the fugitives. They did not at all appear to seek it; on the contrary, their flight seemed to be as aimless as that of any other fledgeling would have been in the same predicament. But one of them stumbled into the pool. The effect was most curious. When the young bird touched the water, there was a moment of pause, as if the creature were surprised. Then instantly there seemed to wake within it the sense of its hereditary powers. Down it dived with all the facility of its parents, and the action of its wings under the water was a beautiful exhibition of the double adaptation to progression in two very different elements, which is peculiar to the wings of most of the diving birds. The young dipper was immediately lost to sight among some weeds, and so long did it remain under water, that I feared it must be drowned. But in due time it reappeared all right, and being recaptured, was replaced in the nest.

Later in the season, on a secluded lake in one of the Hebrides, I observed a Dun-diver, or female of the Red-breasted Merganser (*Mergus Serrator*), with her brood of young ducklings. On giving chase in the boat, we soon found that the young, although not above a fortnight old, had such extraordinary powers of swimming and diving, that it was almost impossible to capture them. The distance they went under water, and the unexpected places in which they emerged, baffled all our efforts for a considerable time. At last one of the brood made for the shore, with the object of hiding among the grass and heather which fringed the margin of the lake. We pursued it as closely as we could, but when the little bird gained the shore, our boat was still about twenty yards off. Long drought had left a broad margin of small flat stones and mud between the water and the usual bank. I saw the little bird run up about a couple of yards from the water, and then suddenly disappear. Knowing what was likely to be enacted, I kept my eye fixed on the spot; and when the boat was run upon the beach, I proceeded to find and pick up the chick. But on reaching the place of disappearance, no sign of the young Merganser was to be seen. The closest scrutiny, with the certain knowledge that it was there, failed to enable me to detect it. Proceeding cautiously forwards, I soon became convinced that I had already overshot the mark; and, on turning round, it was only to see the bird rise like an apparition from the stones, and dashing past the stranded boat, regain the lake,—where, having now recovered its wind, it instantly dived and disappeared. The tactical skill of the whole of this manœuvre, and the success with which it was executed, were greeted with loud cheers from the whole party; and our admiration was not diminished when we remembered that some two weeks before that time the little performer had been coiled up inside the shell of an egg, and that about a month before it was apparently nothing but a mass of albumen and of fatty oils.

The third case of animal instinct which I shall here mention was of a different but of an equally common kind. In walking along the side of a river with overhanging banks, I came suddenly on a common Wild Duck (*Anas Boschas*), whose young were just out. Springing from under the bank, she fluttered out into the stream with loud cries and with all the struggles to escape of a helplessly wounded bird. To simulate the effects of suffering from disease, or from strong emotion, or from wounds upon the human frame, is a common necessity of the actor's art, and it is not often really well done. The tricks of the theatre are seldom natural, and it is not without reason that "theatrical" has become a proverbial expression for false and artificial representations of the realities of life. It was therefore with no small interest that on this, as on many other occasions, I watched the perfection of an art which Mrs. Siddons might have envied. The labored and half convulsive flapping of the wings, the wriggling of the body, the straining of the neck, and the whole expression of painful and abortive effort, were really admirable. When her struggles had carried her a considerable distance, and she saw that they produced no effect in tempting us to follow, she made resounding flaps upon the surface of the water, to secure that attention to herself which it was the great object of the manœuvre to attract. Then rising suddenly in the air, she made a great circle round us, and returning to the spot, renewed her endeavors as before. It was not, however, necessary; for the separate instinct of the young in successful hiding effectually baffled all my attempts to discover them.

Let us now look at the questions which these several exhibitions of animal instinct cannot fail to suggest; and first let us take the case of the young Dipper. There was no possibility of imitation here. The rivulet beneath the nest, even if it had been visible to the nestlings, had been dry ever since they had been hatched. The river into which it ordinarily flowed was out of sight. The young Dippers never could have seen the parent birds either swimming or diving. This, therefore, is one of the thousand cases which have driven the "experience" school of philosophy to take up new ground. The young Dipper here cannot possibly have had any experience, either through the process of incipient effort, or through the process of sight and imitation. Nature is full of similar cases. In face of them it is now no longer denied that in all such cases "innate ideas" do exist, and that "pre-established harmonies" do prevail in Nature. These old doctrines, so long ridiculed and denied, have come to be admitted, and the new philosophy is satisfied with attempts to explain how these "ideas" came to be innate, and how these harmonies came to be pre-established. The explanation is, that though the efficiency of experience as the cause or source of instinct must be given up as regards the individual, we may keep it as regards the race to which the individual belongs. The powers of swimming and diving and the impulse to use them for their appropriate purpose, were indeed innate in the little Dipper of 1874. But then they were not innate in its remote progenitors. They were acquired by those progenitors through gradual effort—the trying leading to success, and the success again leading to more trying—both together leading first to special faculty; then to confirmed habit, and then, by hereditary transmis sion, to instinct, "organized in the race." Well, but even if this be true, was not the disposition of the progenitors to make the first efforts in the direction of swimming and diving, and were not the organs which enabled them to do so, as purely innate as the perfected instinct and the perfected organs of the Dipper of to-day? Did there ever exist in any former period of the world what, so far as I know, does certainly not exist now—any animal with dispositions to enter on a new career, thought of and imagined for the first time by itself, unconnected with any organs already fitted for and appropriate to the purpose? Even the highest acquirements of the Dog, under highly artificial conditions of existence, and under the guidance of persistent "interferences with Nature," are nothing but the special education of original instincts. In the almost human caution of the old and well-trained pointer when approaching game, we see simply a development of the habit of all predatory animals to pause when close upon an unseen prey—a pause requisite to verify the intimations of smell by the sense of sight, and also for preparing the final spring. It is true that Man "selects," but he can only select out of what is already there. The training and direction which he gives to the promptings of instinct may properly be described as the result of experience in the animal under instruction; and it is undoubtedly true that, within certain limits (which, however, are after all very narrow), these results do tend to become hereditary. But there is nothing really analogous in Nature to the artificial processes of training to which Man subjects the animals which are capable of domestication. Or if there be anything analogous—if animals by themselves can school themselves by gradual effort into the development of new powers—if the habits and powers which are now purely innate and instinctive were once less innate and more deliberate—then it will follow that the earlier faculties of animals have been the higher, and that the later faculties are the lower, in the scale of intelligence. This is hardly consistent with the idea of evolution,—which is founded on the conception of an unfolding or development from the lower to the higher, from the simple to the complex, from the instinctive to the rational. My own belief is, that whatever of truth there is in the doctrine of evolution is to be found in this conception, which, so far as we can see, does seem to be embodied in the history of organic life. I can therefore see no light in this new explanation to account for the existence of instincts which are certainly antecedent to all individual experience —the explanation, namely, that they are due to the experience of progenitors "organized in the race." It involves assumptions contrary to the analogies of Nature, and at variance with the fundamental facts, which are the best, and indeed the only, basis of the theory of evolution. There is no probability—there is hardly any plausibility—in the supposition that experience has had, in past times, some connection with instinct which it has ceased to have in the present day. The uniformity of Nature has, indeed, often been asserted in a sense in which it is not true, and used in support of arguments which it will not sustain. All things have certainly not continued as they are since the beginning. There was a time when animal Life, and with it animal instincts, began to be. But we have no reason whatever to suppose that the nature of instinct then or since has ever been different from its nature now. On the contrary, as we have in existing Nature examples of it in infinite variety, from the very lowest to the very highest forms of organization, and as the same phenomena are everywhere repeated, we have the best reason to conclude that, in the past, animal instinct has ever been what we now see it to be—congenital, innate, and wholly independent of experience.

And, indeed, when we come to think about it, we shall find that the theory of experience assumes the pre-existence of the very powers for which it professes to account. The very lowest of the faculties by which experience is acquired is the faculty of imitation. But the desire to imitate must be as instinctive as the organs are hereditary by which imitation is effected. Then follow in their order all the higher faculties by which the lessons of experience are put together—so that what has been in the past is made the basis of anticipation as to what will be in the future. This is the essential process by which experience is acquired, and every step in that process assumes the pre-existence of mental tendencies and of mental powers which are purely instinctive and innate. To account for instinct by experience is nothing but an Irish bull. It denies the existence of things which are nevertheless assumed in the very terms of the denial: it elevates into a cause that which must in its nature be a consequence, and a consequence, too, of the very cause which is denied. Congenital instincts, and hereditary powers, and pre-established harmonies are the origin of all experience, and without them no one step in experience could ever be gained. The questions raised when a young Dipper, which had never before even seen water, dives and swims with perfect ease, are questions which the theory of organized experience does not even tend to solve; on the contrary, it is a theory which leaves those questions precisely where they were, except in so far as it may tend to obscure them by obvious confusions of thought.

Passing now from explanations which explain nothing, is there any light in the theory that animals are "automata?" Was my little Dipper a diving machine? It seems to me that there is at least a glimmer shining through this idea—a glimmer as of a real light struggling through a thick fog. The fog arises out of the mists of language—the confounding and confusion of meanings literal with meanings metaphorical—the mistaking of partial for complete analogies. "Machine" is the word by which we designate those combinations of mechanical force which are contrived and put together by Man to do certain things. One essential characteristic of them is that they belong to the world of the not-living; they are destitute of that which we know as Life, and of all the attributes by which it is distinguished. Machines have no sensibility. When we say of anything that it has been done by a machine, we mean that it has been done by something which is not alive. In this literal signification it is therefore pure nonsense to say that anything living is a machine. It is simply a misapplication of language, to the extent of calling one thing by the name of another thing, and that other so different as to be its opposite or contradictory. There can be no reasoning, no clearing up of truth, unless we keep definite words for definite ideas. Or if the idea to which a given word has been appropriated be a complex idea, and we desire to deal with one element only of the meaning, separated from the rest, then, indeed, we may continue to use the word for this selected portion of its meaning, provided always that we bear in mind what it is that we are doing. This may be, and often is a, necessary operation, for language is not rich enough to furnish separate words for all the complex elements which enter into ideas apparently very simple; and so of this word, machine, there is an element in its meaning which is always very important, which in common language is often predominant, and which we may legitimately choose to make exclusive of every other. This essential element in our idea of a machine is that its powers, whatever they may be, are derived, and not original. There may be great knowledge in the work done by a machine, but the knowledge is not in it. There may be great skill, but the skill is not in it; great foresight, but the foresight is not in it; in short, great exhibition of all the powers of mind, but the mind is not in the machine itself. Whatever it does is done in virtue of its construction, which construction is due to a mind which has designed it for the exhibition of certain powers and the performance of certain functions. These may be very simple, or they may be very complicated, but whether simple or complicated, the whole play of its operations is limited and measured by the intentions of its constructor. If that constructor be himself limited, either in opportunity or knowledge, or in power, there will be a corresponding limitation in the things which he invents and makes. Accordingly, in regard to Man, he cannot make a machine which has any of the gifts and the powers of Life. He can construct nothing which has sensibility or consciousness, or any other of even the lowest attributes of living creatures. And this absolute destitution of even apparent originality in a machine—this entire absence of any share of consciousness or of sensibility, or of will—is one part of our very conception of it. But that other part of our conception of a machine, which consists in its relation to a contriver and constructor, is equally essential, and may, if we choose, be separated from the rest, and may be taken as representative of the whole. If, then, there be any agency in Nature, or outside of it, which can contrive and build up structures endowed with the gifts of Life—structures which shall not only digest, but which shall also feel and see, which shall be sensible of enjoyment conducive to their welfare, and of alarm on account of things which are dangerous to the same—then such structures have the same relation to that agency which machines have to man, and in this aspect it may be a legitimate figure of speech to call them living machines. What these machines do is different in kind from the things which human machines do; but both are alike in this—that whatever they do is done in virtue of their construction, and of the powers which have been given to them by the mind which made them.

Applying now this idea of a machine to the phenomena exhibited by the young Dipper, its complete applicability cannot be denied. In the first place, the young Dipper had a physical structure adapted to diving. Its feathers were of a texture to throw off water, and the shower of pearly drops which ran off it, when it emerged from its first plunge, showed in a moment how different it was from other fledgelings in its imperviousness to wet. Water appeared to be its "native element," precisely in the same sense in which it is said to be the native element of a ship which has been built high in air, and of the not very watery materials of wood and iron. Water, which it had never seen before, seemed to be the native element of the little bird in this sense, that it was so constructed as to be and to feel at home in it at once. Its "lines" had been laid down for progression both in the air and water. It was launched with a motive-power complete within itself, and with promptings sufficient for the driving of its own machinery. For the physical adaptation was obviously united with mental powers and qualities which partook of the same pre-adjusted harmony. These were as congenital as the texture of its feathers or the structure of its wing. Its terror arose on seeing the proper objects of fear, although they had never been seen before, and no experience of injury had arisen. This terror prompted it to the proper methods of escape, and the knowledge how to use its faculties for this object was as intuitive as the apparatus for effecting it was hereditary. In this sense the Dipper was a living, breathing, seeing, fearing and diving machine—ready made for all these purposes from the nest—as some other birds are even from their first exclusion from the egg.

The case of the young Merganser is still more curious and instructive with reference to the same questions. The young of all the *Anatidæ* are born, like the gallinaceous birds, not naked or blind, as most others are, but completely equipped with a feathery down, and able to swim or dive as soon as they see the light. Moreover, the young of the Merganser have the benefit of seeing from the first the parent bird performing these operations, so that imitation may have some part in developing the perfection with which they are executed by the young. But the particular manœuvre resorted to by the young bird which baffled our pursuit was a manœuvre in which it could have had no instruction from example—the manuœvre, namely, which consists in hiding not under any cover, but by remaining perfectly motionless on the ground. This is a method of escape which cannot be resorted to successfully except by birds whose coloring is adapted to the purpose by a close assimilation with the coloring of surrounding objects. The old bird would not have been concealed on the same ground, and would never itself resort to the same method of escape. The young therefore, cannot have been instructed in it by the method of example. But the small size of the chick, together with its obscure and curiously mottled coloring, are specially adapted to this mode of concealment. The young of all birds which breed upon the ground are provided with a garment in such perfect harmony with surrounding effects of light as to render this manœuvre easy. It depends, however, wholly for its success upon absolute stillness. The slightest motion at once attracts the eye of any enemy which is searching for the young. And this absolute stillness must be preserved amidst all the emotions of fear and terror which the close approach of the object of alarm must, and obviously does, inspire. Whence comes this splendid, even if it be unconscious, faith in the sufficiency of a defense which it must require such nerve and strength of will to practice? No movement, not even the slightest, though the enemy should seem about to trample on it; such is the terrible requirement of Nature—and by the child of Nature implicitly obeyed! Here, again, beyond all question, we have an instinct as much born with the creature as the harmonius tinting of its plumage—the external furnishing being inseparably united with the internal furnishing of mind which enables the little creature in very truth to "walk by faith and not by sight." Is this automatonism? Is this machinery? Yes, undoubtedly in the sense explained before—that the instinct has been given to the bird in precisely the same sense in which its structure has been given to it—so that anterior to all experience, and without the aid of instruction or of example, it is inspired to act in this manner on the appropriate occasion arising.

Then, in the case of the Wild Duck, we rise to a yet higher form of instinct, and to more complicated adaptations of congenital powers to the contingencies of the external world. It is not really conceivable that Wild Ducks have commonly many opportunities of studying each other's action when rendered helpless by wounds. Nor is it conceivable that such study can have been deliberately made even when opportunities do occur. When one out of a flock is wounded all the others make haste to escape, and it is certain that this trick of imitated helplessness is practiced by individual birds which can never have had any such opportunities at all. Moreover, there is one very remarkable circumstance connected with this instinct, which marks how much of knowledge and of reasoning is implicitly contained within it. As against Man the manœuvre is not only useless, but it is injurious. When a man sees a bird resorting to this imitation, he may be deceived for a moment, as I have myself been; but his knowledge and experience and his reasoning faculty soon tell him from a combination of circumstances that it is merely the usual deception. To Man, therefore, it has the opposite effect of revealing the proximity of the young brood, which would not otherwise be known. I have repeatedly been led by it to the discovery of the chicks. Now, the most curious fact of all is that this distinction between Man and other predacious animals is recognized and reflected in the instinct of birds. The manœuvre of conterfeiting helplessness is very rarely resorted to except when a dog is present. Dogs are almost uniformly deceived by it. They never can resist the temptation presented by a bird which flutters apparently helpless just in front of their nose. It is, therefore, almost always successful in drawing them off, and so rescuing the young from danger. But it is the sense of smell, not the sense of sight, which makes dogs so specially dangerous. The instinct which has been given to birds seems to cover and include the knowledge that as the sense of smell does not exist to the like effect in Man, the mere concealment of the young from sight is ordinarily, as regards him, sufficient for their protection: and yet I have on one occasion seen the trick resorted to when Man only was the source of danger, and this by a species of bird which does not habitually practice it, and which can have had neither individual nor ancestral experience. This was the case of a Blackcap (*Sylvia Atricapilla*), which fell to the ground, as if wounded, from a bush, in order to distract attention from its nest.

If now we examine, in the light of our own reason, all the elements of knowledge or of intellectual perception upon which the instinct of the Wild Duck is founded, and all of which, as existing somewhere, it undoubtedly reflects, we shall soon see how various and extensive these elements of knowledge are. First, there is the knowledge that the cause of the alarm is a carnivorous animal. On this fundamental point no creature is ever deceived. The youngest chick knows a hawk, and the dreadful form fills it with instant terror. Next, there is the knowledge that dogs and other carnivorous quadrupeds have the sense of smell, as an additional element of danger to the creatures on which they prey. Next, there is the knowledge that the dog, not being itself a flying animal, has sense enough not to attempt the pursuit of prey which can avail itself of this sure and easy method of escape. Next, there is the conclusion from all this knowledge, that if the dog is to be induced to chase, it must be led to suppose that the power of flight has been somehow lost. And then there is the further conclusion, that this can only be done by such an accurate imitation of a disabled bird as shall deceive the enemy into a belief in the possibility of capture. And lastly, there are all the powers of memory and the qualities of imagination which enable good acting to be performed. All this reasoning and all this knowledge is certainly involved in the action of the bird-mother, just as certainly as reasoning and knowledge of a much profounder kind is involved in the structure or adjustment of the organic machinery by which and through which the action is itself performed.

There is unquestionably a sense, and a very important sense, in which all these wonderful operations of instinct are "automatic." The intimate knowledge of physical and of physiological laws—the knowledge even of the mental qualities and dispositions of other animals—and the processes of reasoning by which advantage is taken of these,—this knowledge and this reasoning cannot, without manifest absurdity, be attributed to the birds themselves. This is admitted at least as regards the birds of the present day. But surely the absurdity is quite as great if this knowledge and reasoning, or any part of it, be attributed to birds of a former generation. In the past history of the species there may have been change—there may have been development. But there is not the smallest reason to believe that the progenitors of any bird or of any beast, however different in form, have ever founded on deliberate effort the instincts of their descendants.

[*To be Continued.*]

PROFESSOR JAMES C. WATSON.

Professor James C. Watson, Director of the Observatory of the University of Wisconsin, died at Madison, Wis., on the morning of November 23, after an illness of but three or four days.

Professor Watson was born on January 28, 1838, and was therefore nearly 43 years of age. He graduated at the University of Michigan in 1857, remaining there as instructor and Professor of Mathematics and Astronomy till 1863, at which time he was made Director of the Ann Arbor Observatory. He held this position till 1878, when he accepted the Directorship of the Washburn Observatory at Madison. He made observations upon the total solar eclipse of 1869 in Iowa, and that of 1870 in Sicily; and in 1874 had charge of the very successful American Expedition, which observed the transit of Venus at Peking, China. In 1870 he received the Lalande gold medal from the French Academy of Sciences, for his various astronomical works and discoveries. His most elaborate writings are: *A Popular Treatise on Comets* (1860) and *Theoretical Astronomy, relating to the Motions of the Heavenly Bodies revolving around the Sun in accordance with the Law of Universal Gravitation, with Numerical Examples and Auxiliary Tables* (1868). In addition to these, he has published from time to time, in *Gould's Astron. Journ., Astron, Nach., Am. Journ. of Sci.*, etc., short papers relating, for the most part, to the discovery and observations of asteroids, and the computations of comet orbits. For several years he gave especial attention to the search for asteroids, and in this work was eminently successful, discovering, in all, twenty-one of these bodies, between the years 1863 and 1877. At the time of his death, Professor Watson was engaged in building and equipping one of the finest observatories in America. The meridian circle, which is to contain several new features suggested by himself, is now in the hands of the Clarks, and will not be finished, probably, for nearly a year. Other instruments of the highest order are either already mounted and in operation, or are in course of completion. Careful preparations had been made also for a systematic search for the planet Vulcan, a problem in which Professor Watson was deeply interested. W. C. W.

THE AMERICAN SOCIETY OF MICROSCOPISTS.

(From advanced sheets of *American Naturalist*, for December; Microscopical Department under the direction of Dr. R. H. Ward.)

Probably no thoughtful person who attended both meetings this summer, the American Society of Microscopists at Detroit, and the Subsection of Microscopy, A. A. A. S., at Boston, failed to notice the nearly equal division of strength between the two conventions. The personal attendance at the meetings was about equal, though mainly of different individuals; the number of papers read was precisely the same, and it is only fair to say that in interest and importance they were very evenly divided. It is obvious that if the strength of the two meetings could have been combined in one, the result would have been far more adequate and satisfactory. This reflection has derived force from the well known fact that in the Microscopical Congress at Indianapolis, nearly half the voices were in favor of joining with the A. A. A. S., instead of forming a separate society, the latter course being adopted in the critical vote by a majority of one. From first to last, it has been of great and conceded importance to combine all our strength in one enterprise; but the obstacles which originally rendered this impossible, still remain, and it is evident that indiscreet controversy might increase and perpetuate the difficulties it was designed to remove. It would be absurd to ask persons, accustomed to attend the meetings of the great society, and highly valuing its opportunities for intercourse with leading minds in various departments of science, to abandon that for any narrow organization, however attractive might be its field. On the other hand the new society could not profitably be united with the old, as has been proposed, without a more cordial and general support of such a procedure than could at present be hoped for. The subordination to greater interests, which would be encountered in uniting with the great society would be more than counterbalanced, in many minds, by the social and scientific advatages gained; and the fact that many of the papers read would be excluded from the Proceedings by a necessity which admits only contributions new to science, would be of little consequence, since popular papers gain an earlier and a wider distribution through the popular journals; but a more serious difficulty arises from the localities in which the meetings of the A.A.A.S., are sometimes held. The large and powerful society can afford to appoint meetings, not unfrequently, for the sake of cultivating local interest in science, in localities which would be unavailable for the microscopical meetings. A joint meeting at Boston would have given a large increase of vitality; the same will not be equally true of all other localities.

If for these or any other reasons, it should be impracticable to combine the two societies at present, the greatest advantages would doubtless be secured by such a policy as would show, on both sides of the question, a reasonable and considerate regard for the interests of the other. The very large minority at Indianapolis acquiesced in the formation of a new society with the understanding that the times and places of meeting were to be so chosen as to best accommodate those who might wish to attend both. This policy, if fully carried out, would not prevent meeting at the same place when expedient, and would not require it when some other correlated place would be advisable. It would give many of the advantages of union, with entire freedom from its difficulties. It is the least that could in reason be asked, or that could in common courtesy be granted, as a means of securing a cordial and harmonious support for the new society.

THE first number of a periodical, devoted to the subject of instruments, will be issued January 1, 1881. It will be published in Berlin under the name of the "Zeitschrift für Instrumentenkunde," and will be prepared by a board of twenty-one editors, including the most noted instrument makers of Europe and representatives of different branches of science in which instruments of precision are employed. Such a periodical is greatly needed, and the names of the editors are a guarantee of its success. O. 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.*]

To the Editor:

It seems to me that the editorial article concerning the American Society of Microscopists, which appeared in "SCIENCE" on page 225, so far as it relates to what I have presumed to write elsewhere, is likely to place me in a false position before your subscribers. I will not ask for the space that would be necessary to discuss the merits of the question; suffice it to say that, with many others, I was inclined to regard the proposition to form a National Organization of Microscopists with disfavor from the first, not owing to any feeling of opposition to such an organization, but purely from considerations of expediency. Your remarks, however, would lead one to suppose that I had made a direct criticism of the officers of the society, which is not the fact. The only words I have written that could possibly be construed to such a meaning, are these: "We have regarded the establishment of the society as a worthy experiment, but as one mainly conducted by a few leaders, who had not the necessary support from microscopists generally to insure its success, nor sufficient experience to directly it properly." I have nothing to retract; but if any explanation is necessary, I have only to say that my language was intended to apply to those who were most active in forming the society in the beginning, not to the officers who have since been elected. Nevertheless, to quote again from my article, I wrote: "Once more we desire to say, in order that no person may misunderstand us, either wilfully or otherwise, that we are not moved by any spirit of opposition to the American Society of Microscopists." The course which I strongly advocate, because it seems to me that it would be beneficial to both organizations, is that the American Society should disband and its members unite with the A. A. A. S. It is true that this plan has met with opposition from the Society, but if I read the signs aright, the same resolution to do this, which was indignantly voted down at Indianapolis, will be more carefully considered if offered next year.

The question that presents itself to my mind is not: Can the American Society of Microscopists be made to exist as an independent organization, by the efforts of a few enthusiastic members? but it is rather: Can research with the microscope be fostered better by such an organization, or by the sub-section of the A. A. A. S.? The microscope is an instrument that is used in many branches of scientific study, but microscopy is not the name of any science. A local microscopical society may, indeed, be a centre of attraction of scientific men generally,; but it is doubtful if a national microscopal society can ever prove sufficiently attractive to induce its members to travel half way across the continent to attend its meetings. Respectfully,

NEW YORK, *November* 11, 1880. R. HITCHCOCK.

To the Editor of Science:

Vol. 1, No. 10, of the Humbolt Library, is an essay on "The Theory of Sound in its Relation to Music" by Prof. Pietro Blaserna, of the Royal University, of Rome. It is interspersed with illustrations and demonstrations of a very interesting character, and written in a popular manner.

Every musician must feel the touch of a kindred soul as the subject reaches the historical phase, whilst the criticisms, on national influence upon music, are very impartial and indeed beautiful. The temperate scale is referred to, in too scourging a manner, which should rather be applied to the dogmatic assertions of would-be-musicians who have failed to acquaint themselves with scientific truths.

The essay is very useful as the stepping stone to a very much involved subject, and may perhaps be endorsed as a whole, with the exception of what follows and the consequences entailed thereby—namely the conclusion at which he arrives that "vibration is the CAUSE and sound the EFFECT," in reply to which, note as follows:

A. 1. The laws of inertia apply to aerial as well as to solid bodies, only in a less degree.

2. The vibration of air may be made apparent to the ear or the eye, within the limits of their perception.

3. These limits are not throughout coincident.

4. The perception by eye and ear simultaneously is only possible within the limits of coincident perceptions.

Therefore to expect that what is popularly called sound, viz., the perception of vibration by the ear, should be concomitant with the perception by the eye, is evidently absurd in all cases except within the limits in which those perceptions coincide—otherwise vibration would always be heard, when seen.

B. 1. Air in the undisturbed enjoyment of inertia will never vibrate.

2. It may however be made to do so, by applying dynamic power or energy, either muscular force, mechanical force, electricity, heat, etc., as in the drum, siren, thunder, sham-whistle, or what is popularly called the "kettle singing."

3. The vibrating air may then be apparent to the nerves of sight, hearing, or feeling.

Therefore it is the EFFECT of a disturbing cause, and may be studied either objectively or subjectively through any of those perceptions.

We listen to the sound of the bell—what we perceive by the ear is the vibration of air, the exciting cause of which is the energy, which set the bell in motion. The bell itself being the mechanical vibrator and resonator—the loudness of the sound results from the manner of applying the energy—what musicians sometimes call the "mode of attack," and do we wish to know the relation existing between the energy and the vibration? All that is necessary, *thanks* to *Balfour Stewart*, is to use his formula, viz., that "energy is proportional to the square of the velocity," velocity in this case being as the number of vibrations per second. So the vibrations of the harmonic series being related to the fundamental as the whole numbers, the energy necessary to produce this series increases in the ratio of the square root of the vibrations. The resistance necessary to overcome this increasing energy is peculiarly attested by the lip of the cornetist, in the production of the ascending harmonic series.

JOHN H. RHODES.

NEW BRUNSWICK, *November* 13, 1880.

INTENSITY OF CERTAIN PHENOMENA OF ATMOSPHERIC ELECTRICITY OBSERVED IN THE NORTH OF THE SAHARA.—L. Amat has observed that in tropical countries the electric phenomena of the atmospheric stratum in contact with the soil are more distinct than in colder climates.

METHOD OF DETERMINING THE FATTY ACIDS CONTAINED IN OILS.—M. Carpentin takes a small flat-bottomed flask or a medicine phial holding about 250 c.c. Into this phial are measured 50 c.c. of the sample of oil, and 100 c.c. of alcohol at 90 per cent., and 3 or 4 drops of tincture of tumeric are added. The phial is then corked and violently shaken. The phial is then placed under a Mohr's burette containing a solution of 40 grms. pure sodium hydrate per litre of water. As 40 grms. soda saturate 282 of oleic acid, 1 c.c. of the liquid, containing 0.04 grm. soda, corresponds to 0.282 grm. of oleic acid. If another fatty acid has to be determined this number is modified accordingly. The alkaline liquid is carefully dropped into the phial, which is shaken. When a red coloration appears it is corked, agitated for a considerable time till the yellow color reappears, the alcohol having extracted a fresh quantity of acid out of oil. These operations are continued until the red color becomes permanent. The number of c.c. and the fraction of a c.c. consumed are then multiplied by 0.282 grms., in order to find the quantity of oleic acid present in the sample examined.

BOOKS RECEIVED.

METHODS AND RESULTS.—Description of an improved Vertical Clamp for the Telescopes of the Theodolites and Meridian Instruments—United States Coast and Geodetic Survey—Appendix No. 13—Report for 1877—Washington Government Printing Office, 1880:

The advantages of this improvement, which has been devised by Mr. George Davidson, an assistant of the United States Coast Survey, may be briefly stated as follows:

I. The telescope is clamped with sufficient firmness to admit of its being moved in altitude in the vertical plane by the slow-motion screw.

II. The clamp may be made to hold the transit-axis so gently that a very delicate tap on the telescope will bring the latter to the desired elevation.

III. The top of the clamp is open, so that it permits the telescope to be lifted out for reversal and readily replaced in the Y's without carrying the clamp with it.

IV. The jaws of the open clamp remain during reversal in the same position as when unclamped before the reversal of the telescope.

V. There is no tendency to lift the vertical plate through eccentricity of the slow-motion screw, and consequently no resultant movement of the transit axis in azimuth.

We advise those who would like to know more of this improved clamp to address directly to Mr. Davidson, whose address is United States Coast and Geodetic Survey, San Francisco, Cal.

MANUAL OF THE VERTEBRATES OF THE NORTHERN UNITED STATES. By DAVID STARR JORDAN, Ph. D., M. D., Professor of Natural History in Indiana University, 3d Edition. Revised and Enlarged. Jansen, McClurg & Company, Chicago, 1880.

This book, which was originally written to afford collectors and students who were not specialists, a ready guide for identifying the families, genera and species of our vertebrate animals, is now again presented to the public in a third edition, which would appear to indicate that the work meets a demand made by Naturalists, and has been received with approval.

This is a purely technical work, the author confining himself strictly to details necessary to be understood for scientific classifications, while signs and abbreviations are freely used to reduce the matter to its lowest limits.

The author has been assisted by such eminent naturalists as Dr. Elliott Coues, Professor E. D. Cope, Dr. Theodore Gill, Professor H. E. Copeland, Mr. E. W. Nelson, Mr. B. H. Van Vleck, Mr. C. H. Gilbert and Dr. A. W. Brayton, and efforts have been made to include in this edition the results of recent investigations in this department of scientific research.

The ground covered by this work includes the district east of the Mississippi river, and north of Carolina and Tennessee, exclusive of marine species.

The work concludes with a good glossary of the principal technical terms used in the book, a glossary of specific names, and also an index to names of genera and higher groups with their derivations.

This manual of the vertebrates will prove valuable, not only to students, but to the large class of amateurs who desire to classify the forms included in this work.

THE ELECTRIC LARYNGOSCOPE, by A. WELLINGTON ADAMS, M.D. [Reprint from the Archives of Laryngology, Sept. 1880.]

We are once more reminded, by this little pamphlet, of the manifold applications of the electric light in the practical departments of medicine and surgery. Dr. Adams claims for the instrument he has devised, the following advantages: 1. The application of what is the nearest approach to sunlight—the electric light—in such a way as to bring it under perfect subjection and be readily manipulated. 2. The establishment of a permanent relationship between the source of light and the throat mirror. 3. The use of a light which emits neither gas nor heat, and is of such concentration and intensity as to illuminate the respiratory tract down to a point nearly an inch below the "bifurcation," so that every detail in the larynx and trachea down to that point is sharply defined in the throat mirror, and if the latter be large and slightly concaved, any particular detail requiring special structural examination may thus be greatly magnified.

THE VARIATIONS OF THE FIXED POINTS OF MERCURIAL THERMOMETERS, AND THE MEANS OF RECOGNIZING THEM IN THE DETERMINATION OF TEMPERATURES.—J. Pernet agrees with M. Crafts that the part played by pressure in the permanent elevation of the zero-point is very trifling, if it exist at all.

BORO-DECI-TUNGSTIC ACID AND ITS SODIUM SALTS.—According to D. Klein, if tungstic acid in excess is dissolved in a boiling solution of borax with twice its molecular weight of boric acid (crystalline), the ebullition kept up for some hours, the undissolved tungstic hydrate filtered off the resulting solution deposits crystals of boric acid and sodium polyborates. The mother-liquor, if concentrated and placed in a vacuum, deposits first borax and then the exceedingly soluble sodium salt of the new acid, containing 2 mols. of constitutional water.

APPEARANCE OF OZONE ON THE EVAPORATION OF VARIOUS LIQUIDS AS A LECTURE EXPERIMENT.—R. Böttger recommends to moisten a piece of paper uniformly with starch containing cadmium iodide, to let fall upon it a few drops of alcohol or ether, and to set the latter liquid on fire. After its evaporation the paper is found turned decidedly blue in consequence of the formation of ozone.—*Pol. Notizblatt*, 35, 95.

SINGULAR BEHAVIOR OF STANNOUS CHLORIDE WITH POTASSIUM CHLORATE.—R. Böttger states that if 2 parts of stannous chloride and 1 part potassium chlorate, both previously pulverized, are rubbed together in a porcelain mortar, the mixture becomes very hot, chlorous acid and watery vapor are evolved, and there remains a yellowish white mass, which, if dissolved in boiling water, deposits potassium perchlorate in micaceous crystals. The mother-liquor contains tin oxychloride.

HYPOCHLORINE AND THE CONDITIONS OF ITS ORIGIN IN PLANTS.—M. Pringsheim has demonstrated the existence of a body in the green cells of plants, which he named hypochlorine on account of its relation to chlorophyll. He has quite recently described, in a paper, its occurrence and its microchemical characters.

CHLORIDES OF CAMPHOR.—The products which arise on the action of phosphorus pentachloride upon camphor are affected by the quantity of the phosphorus chloride present and by the temperature. If every increase of temperature is prevented no hydrochloric acid appears, and there is formed a homogeneous camphor dichloride in theoretical quantities. Pfaundler's dichloride, and the body melting at 60° and described as monochloride, are probably merely mixtures. F. V. SPITZER—*Wien. Anzeiger*, 1880, 71.

DECOMPOSITION OF SIMPLE ORGANIC COMPOUNDS BY ZINC-DUST.—The higher alcohols from ethylic alcohol upwards, on distillation over zinc-powder which was heated to 330°, to 350°, were decomposed into the corresponding olefine and hydrogen. Under the same circumstances methylic alcohol is resolued into carbonic oxide and hydrogen. HANS JAHN.—*Wiener Anzeiger*, 1880, 73-74.

NEW SYNTHESIS OF DIMETHYL-ACRYLIC ACID.—This compound is formed along with ethylisoxy-valerianic acid when brom-iso-valerianic ether is brought in contact with sodium ethylate in absolute alcohol. E. DUVILLIER.—*Ann. Chim. Phys.*, 19, 429.

SCIENCE:

A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.

Vol. I, No. 22. - - - - - - - November 27, 1880.

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SCIENCE

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ILLUSTRATED.

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Those engaged in Scientific Research are invited to make this Journal the medium of recording their work, and facilities will be extended to those desirous of publishing original communications possessing merit.

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Both questions and answers in "Notes and Queries" should be made as brief as possible; an answer appearing to demand an elaborate reply may be written in the form of an article.

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SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

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NOTICE TO SUBSCRIBERS.

We consider it due to those subscribers who have favored us with their subscriptions, previous to the publication of our club rates, that they should have the privileges of the list. They can therefore send us subscriptions for any of the publications named at the reduced double rates, less $4, the subscription price of "SCIENCE."

Since the publication of the club rates last week, we have received rates from the proprietor of *The American Journal of Science and Arts*, the terms of which are $6 a year. The club rate with SCIENCE will be $8.50 per annum.

EARNEST advocates of a higher order of education now regard with satisfaction the prospect of the establishment of a State University in Texas upon a sound, financial basis.

As early as 1839 public land amounting to fifty leagues were dedicated to found a university for this State, and when commissioners were appointed to locate the city of Austin, forty acres were reserved, and forever devoted as a site for the University of Texas.

For twenty years the matter remained in abeyance, but in 1858 an attempt at organization was made, the Legislature passing an act for the immediate establishment of the University, and one hundred thousand dollars were appropriated from the State Treasury for the purpose. The approach of the civil war led to a second postponement, and a third attempt in 1866 was equally unsuccessful.

The present prospects for the future of the University of Texas are very encouraging to those who desire its early establishment and successful organization. Professor Oscar H. Cooper, in an article in the *International Review*, gives the following information on the subject, which will be read with interest:

He states that the constitution adopted in 1876 supersedes all previous legislation and is the organic law of the State. Its provisions concerning the University are wise and generous. It directs the Legislature to inaugurate the institution as soon as practicable, secures to the funds all previous appropriations, directs that only the interest on the funds shall be used, and adds to already growing resources one million acres of the public domain—a territory considerably larger than Rhode Island. It prescribes the object of the University to be "the promotion of literature and the arts and sciences," and incorporates as a branch of the University, for instruction in agriculture, the mechanic arts and sciences connected therewith, the State Agricultural and Mechanical College, already in 1871 under the federal appropriation for such institutions.

It requires that the location of the University shall be determined by a vote of the people of the whole State, and directs that a College or branch University be established and maintained for the instruction of the colored youths of the State.

Thus the policy of past legislation has been sacredly to guard and freely to augment the resources of the University until they became ample for founding an institution worthy of the name. About half of the land donation to the University has been sold for about $500,000, and the proceeds have been either invested in five, six, or seven per cent. State bonds, or held in ten per cent. land notes.

The sum of $100,000, appropriated to the University in 1858 was borrowed by the State, and in 1866 was replaced by five per cent. State bonds. The invested funds therefore amount to nearly $600,000, and by the sale of lands are steadily increasing. The accrued interest will, at the end of the present year, amount to more than $200,000, and the annual interest on the invested capital exceeds $40,000. The unsold lands are worth at present $1,500,000. The endowment funds, buildings, grounds, etc., of the department of Agriculture and the mechanic arts are valued at $400,000. The University of Texas is worth, therefore, exclusive of $200,000 accrued interest, $2,500,000, and this superb endowment is enhancing in value with the growth of the State in wealth and population. Few even of the most famous institutions of the world began their career on so generous a foundation, and neither Harvard nor Yale was so wealthy at the completion even of their first century.

The people of Texas are said to be now showing a keen interest in the question of education, and, no longer contented with these magnificent provisions for the future, demand the immediate execution of the scheme, the Governor no doubt expressing the popular wish, when he stated "I am opposed to waiting longer."

The probability that the University of Texas will be almost immediately organized has already called for expression of opinions from those who are anxious for it to fulfill the best hopes of its promoters, and Professor Cooper leads the van of those who, with hopes and fears, already see danger ahead, and would be in time with their council.

The advice of Professor Cooper is most excellent, but in part it appears to us somewhat superfluous; that "the first President of the University of Texas should be pre-eminently an organizer, conversant with the best systems both in America and Europe, and alive to the growing demands of the age, and that the instructors should be the best men, sought without regard to section or creed," are recommendations which involve principles universally acknowledged; if the appointments are not made to accord with these principles, it will not be from ignorance that such a course should be followed.

But alas, academical appointments, like those in political life, are often influenced by "interest" and at some times by "expediency." As an instance of the latter class, we may refer to a case in which a most eminent American Naturalist was a candidate for the chair of Natural History in a Northern University. His high claims over other candidates for the position were admitted, and he was told informally that he had been appointed. The professor was preparing for his new home, when he received the very sudden announcement that another of the candidates had been finally selected for the position. The explanation of the mystery was very simple. The University, or College, was supposed to be filling the chair of Natural History, as Professor Cooper would desire, "with the best man without regard to creed or section," but unfortunately there was a want at the establishment for a man to do ministerial duties; the result was that the trustees, in filling the chair of Natural History, rejected the eminent Naturalist, and selected from among the candidates the one who had the greatest capacity for prayer.

For our part, we believe that such complications suggested by Professor Cooper, are not to be anticipated; when the buildings are ready, the right men to fill the positions in the faculty will be forthcoming. The establishment of a University in America, is no new experiment, and the experience of the past will be a valuable aid to those who will organize the University in Texas.

As a rule, the management of the Universities and Colleges in the United States, is one of the redeeming points which has done much to restore confidence in the institutions of this country; the selection of Professors is also usually judicious, and among the corps of instructors, the number of those who do honor to the position they occupy is fortunately great, and no American now has need to leave his native shores to obtain a thorough knowledge in any department of science.

TYCHO BRAHE'S NEW STAR.

On November 11th, 1572, Tycho Brahe noticed a new and very bright star in the constellation Cassiopeia. Afterwards it appeared that this star had been seen before at various places in Europe, and Tycho, in order to fix its position, and to determine whether it moved, began a series of measures with his sextant, by which he connected the position of the new star with nine known stars in the same constellation. The new star shone with a wonderful brightness, being brighter than the planet Jupiter, and, according to some reports, it was visible in full daylight. In January, 1573, its brightness began to wane, and in May of the same year it was only of the second magnitude, or as bright as Polaris. It remained visible to the naked eye, however, until March, 1574.

This star was also remarkable for the changes of color that it exhibited. At first it was white, then it became yellow, and, finally, red. But in May, 1573, it was again of a dull white color, and remained so until it disappeared.

Although many cases have occurred of new stars blazing out for a short time, and then fading away beyond the sight of the naked eye, such as those of 1866 and 1876, yet Tycho's star, on account of its brilliancy and its long duration, is the most remarkable of any star of this kind of which we have any authentic record; and his observations of it have been carefully reduced and discussed by several astronomers. Professor D'Arrest, of Copenhagen, made a very complete catalogue and chart of 212 stars, which are within a distance of ten minutes from the position of Tycho's star. This catalogue is for the year 1865, and it will serve for a standard of reference in case Tycho's star should again blaze out. Mr. J. R. Hind, of England, by the reduction of a part of Tycho's observations, found the position of the new star to be for 1865,

A.R. = 4° 16′ 48″: Decl. = + 63° 23′ 5″.

(Monthly Notices, Royal Astronomical Society, Vol. 21, p. 233.) From a more complete reduction of Tycho's observations Argelander found for 1865,

A.R. = 4° 19′ 58″: Decl. = + 63° 23′ 55″.

(Astronomische Nachrichten, Band 62, p. 274.) This position agrees very well with that of a small star of the 10½th magnitude, which is No. 123 of D'Arrest's catalogue. The position of this small star for 1865 is,

A.R. = 4° 19′ 30″: Decl. = + 63° 22′ 54″.

When we remember that Tycho's observations were made without the aid of telescopes or of any magnifying power, we may consider the difference of these positions as within the limits of the probable error of his determination. We conclude, therefore, that Tycho's star is still visible in our telescopes, and that its brilliant appearance in 1572 was only an extreme case of the variations of light that are frequently happening among the stars.

John Goodricke, an English astronomer, who in 1782 determined the period of the variability of the famous star Algol, thought that Tycho's star might be the same as the new stars reported to have been seen in the years 945 and 1264. This would make the period of its variability between 300 and 320 years, and hence this star should re-appear in the latter part of the present century. Goodricke's conjecture seems to be very uncertain, since the reports for the years 945 and 1264 are extremely vague. It will be seen that if we assume the period of the variability of Tycho's star to be 315 years, five such periods would carry it back to near the beginning of the Christian era. Astrologers and others have not been slow to catch at such analogies, and to base predictions on these uncertain data; and thus we have it asserted that Tycho's star is identical with the star of Bethlehem, and that it will re-appear in the year 1887, with wars and social revolutions. Of course it is impossible to reply to such assertions. Wars and social revolutions are continually going on, and such grim predictions are as safe therefore, as it is to say, that to-morrow the winds will be variable, or that we shall have "rain in areas;" or snow next January. The only wonder is that intelligent people are imposed on by such assertions.

At the present time more than a hundred variable stars are known to astronomers, and every year increases their number. Many of their periods are well determined, but what causes the variations of light we do not know. The so-called new stars may be only extreme cases of the variable stars, and the appearance of one is an interesting astronomical phenomenon which should be carefully observed. There is a rich field for observation and for study.

A. HALL.

WASHINGTON, D.C., Nov. 29, 1880.

NEW YORK ACADEMY OF SCIENCE.

We direct special attention to the excellent course of lectures provided by the New York Academy of Sciences, to which non-members are admitted free, on making application to the proper authorities.

The lecture for Monday next, December 6th, will be delivered by Professor W. Boyd Dawkins, F. R. S., of Owens College, Manchester, England, the subject being "The Man of the Caves."

We understand the present will be the only opportunity for hearing Professor Dawkins lecture in this country on a subject on which he is a specialist. We anticipate a large attendance.

The present efforts of the executive of the New York Academy of Science, under the presidency of Professor Newberry, to provide a course of free lectures of the highest order, should be fully recognized by all interested in Science and we advise those who would avail themselves of the opportunity to address Professor D. S. Martin, of 235 West Fourth street; or Professors W. P. Trowbridge and Alexis A. Julien, both of Columbia College, N. Y., as these gentlemen constitute the Committee on Lectures.

HISTORICAL NOTES ON GAS ILLUMINATION.

At the present moment when the public is all impatience to see the electric light perfected for general illuminating purposes, it may be interesting to note a few particulars descriptive of the early days of gas, when it struggled into existence for the same purposes.

In looking over a few somewhat ancient scientific papers I found much relating to the subject, and will now reproduce these historical facts in the order in which I found them.

It appears that in the British Museum there is preserved a paper (Ascough's Cat. 4437), entitled "Experiments Concerning the Spirit of Coals, in a Letter to the Hon. Mr. Boyle, by the late Rev. James Clayton, D.D., B. Mus." These experiments were undertaken by him in consequence of his having observed that the gas, issuing from certain fissures near a coal pit at Wigan, in Lancashire, took fire when a burning candle was presented to it. He therefore distilled coal, and obtained first "phlegm," afterwards a black oil, and then "an inflammable spirit," which he collected in bladders. By pricking these bladders he was able to ignite the gas at pleasure. Hence it is evident that the discovery of the carburetted hydrogen gas took place previous to the year 1664.

So states a paper, No. 66, in the *Philosophical Journal*, by Mr. John Webster, "*On the Discovery of the Inflammable Gas from Coals;*" the date of the paper is not before me, but its republication, in the form I found it, was in 1807.

In the *Phil. Jour.*, No. 67, the subject is again mooted by a Mr. Hume, who states that in the forty-first volume of the Philosophical Transactions, p. 59, is a "sheet-paper," which appears to have been read before the Royal Society in January, 1739, as "A Letter to the Hon. Robert Boyle, from the late Rev. John Clayton, D.D., in which is described how the discovery originated, and also some of the effects produced by this gas or 'spirit' of coal."

Mr. Hume further draws attention to the difference in the Christian names given to Mr. Clayton, in the first instance "James" and the second "John," and draws the very probable conclusion that the same person is referred to in both papers, and states, "At any rate, the merit of this discovery can be no longer claimed by any living person."

This remark was called forth on account of the public papers of that day, 1808, being much taken up with the proposal of a Mr. Winsor to light cities with gas. It appears that Mr. Edward Heard also obtained a patent in June, 1806, for "Obtaining inflammable gas from pit coal, in such a state that it may be burned without producing any offensive smell."

There was money in this patent, for Mr. Winsor was organizing a large company, which was not to buy the patent, but to pay a royalty as a license for the exclusive right to make use of it. As usual in such cases there was a great outcry, and the attempt was made to break down the patent by asserting that the invention was not new, one Nicholson taking the ground that the patent was invalid, because the inflammable nature of coal gas was demonstrated by "Boyle" before 1691; and he further stated that Lord Dundonald used gas from coal to give light many years ago, and that a Mr. Murdock also put it in practice upon a large scale in 1792 and 1798, so that it was absurd for Mr. Winsor to claim the invention for the public use of gas.

To parry these attacks Mr. Winsor published a small pamphlet, and boldly asserted that it was true that the inflammability of coal gas had been long known, but that no one *had purified gas*, and thus made it fit for general illuminating purposes, until he took out his patent in 1804. He also accused others, who were in the field, of having obtained their knowledge from him.

Mr. Winsor had to contend against other difficulties; for, at that date, the statute law of the realm prohibited more than five persons holding a patent as joint property, and it was held that as the shareholders of the proposed company would share the profits, they would be joint holders of the patent. To this Mr. Winsor replied that he retained the patent himself and merely sold the right to use it. To show the poor prospects for gas illumination entertained in those days, the remark of the editor who published these papers is significant, for he says he "regards the whole scheme as a bubble."

The next paper before me "On the Application of the Gas from Coal to Economical Purposes," by Mr. William Murdock—*Phil. Trans.*, 1808, shows the question to have advanced to the stage when a large building had been illuminated by gas; this building was the cotton manufactory of Phillips and Lee, of Manchester, England, the whole of which, together with the dwelling house of Mr. Lee, was lighted with gas.

This was thought, at the time, to be a great feat, and shows by what slow degrees the process of gas illumination was developed; the idea of a central manufactory of gas, and that of carrying it by pipes throughout a district, never entered the minds of the most advanced advocates of the system, but that each house or establishment should manufacture its own gas and use it, was considered the perfection of gas lighting.

Mr. Lee distilled the coal in large iron retorts, and the gas was conveyed into large gasometers, where it was washed and purified, and then conveyed to the burners. There were 271 burners on the principle of the Argand lamp, each of which gave a light, as measured by means of shadows, equal to four mould-candles of six to the pound; and 633 burners, called cockspurs, having three apertures only of 1-30th of an inch, and of which the light was equal to two and a quarter of the same candles; so that the whole of the lights were equal to 2,500 candles of that size, each of which consumed 4-10ths of an ounce, or 175 grains of tallow in an hour. Mr. Murdoch continues, "the quantity of gas required by this number of burners was 1250 cubic feet in an hour. In some mills where the work is light, the average time required will be three hours, but in this manufactory the yearly averages is two hours a day, or 2,500 cubic feet of gas. This quantity of gas required the distillation of 7 cwt. of cannel coal." The expense of the lights used in this manufactory may therefore be stated thus:

Cost of 110 tons of best Wigan cannel, at 22s. 6d. is		£123
Cost of 40 tons of common coals to heat the retort at 10s. is		20
		£143
Interest on capital, and wear and tear of apparatus		550
Attendance, the same as when candles are used, therefore need not be stated		0
		£693
Deduct value of 70 tons of coke.	£93	
Value of 1250 ale gallons of tar not yet sold	0	£93
		£600

"The expense of candles to give the same light would be, at 1s. per lb., nearly £2,000. The light is peculiarly soft and clear, and of almost unvarying intensity, so as to be very pleasant to the workmen. It is also free from the danger of spark."

This will give an idea of the method of making comparative calculations then used to determine the merits of gas as against the use of candles. The editorial remarks on this paper, might, if the word gas be substituted for electricity, be taken for one of the criticisms so lately in fashion, and now a little obsolete.

"The present paper furnishes the necessary data for calculating the quantity of coals that would be required to yield a light equal to that of a given number of candles; and it affords an easy means of investigating the economical advantages of this process, which seems well adapted to the illumination of public buildings, large manufactories, and generally speaking, all establishments where a great number of lights are required; but we fear the *expense of the apparatus will always be against its introduction in domestic establishments* on a small, or even middling scale."

The last paper I notice is "*On the Advantage* of Employing Coal-gas for Lighting Small Manufactories, and for Other Purposes," by Mr. B. Cook, *Philosophical Journal*, No. 94.

Mr. Cook in this paper drew attention to the increased price of tallow, on account of the "rupture with Russia," so that the advantage of using coal-gas becomes evident. It is true, he says, that coal itself might increase in value, but, as he suggests, it might lead to an increased search and greater production.

Mr. Murdock explains the method of making gas for large manufactories, and Mr. Cook in his paper describes his plan for making gas for dwelling houses. "Such an apparatus," he says, "should be an 8 gallon iron pot, with a cover of the same metal luted on with sand. About 20 to 25 pounds of coal are put into the pot, and distilled, which requires the combustion of about 25 pounds more of coals. The quantity of gas varies with the quality of the coals, it is passed through water into the reservoir, which only holds about 400 gallons, but in general more is produced; so that the overplus, perhaps 200 gallons, is wasted. From the reservoir the gas is conveyed round the house by means of old gun barrels, used as tubes, and coated once a year, or seldomer, with the produced tar.

"The gas flame is found superior to that of a lamp for soldering with the blow-pipe. The moment the stop-cock is turned on, the frame is ready for use, while with oil or cotton wick, the workmen are forced to wait until the lamp is sufficiently on fire."

The expense of this apparatus was £50, but he thought others could be put up for £40.

In regard to the light produced, Mr. Cook offers the following facts: "The lights employed in the manufactory are 18 or 20, equal to eighteen shillings a week for candles, for 20 weeks, which amounts to £18. It used to cost £30 a year for oil and cotton for the soldering lamps; and the coke is certainly worth £2, 10*s.* a year, so that, setting the tar against any little accident that may happen, the whole produce may be taken at £50. 10*s.* a year.'

Supposing 50 lbs. of coal are used daily, the weekly expenditure on that head will be 2 shillings, and allowing part of a man's time to attend to making the gas to be worth 5 shillings, the whole will be 7 shillings per week or £18, 4 shillings a year; this however is one fourth more than it ought to be, because for 25 to 30 weeks the gas will not be required for lights. And adding to this expense £2 a year for interest on the cost of the apparatus, there will remain a saving of £30. 6s. in the year.

For a family using only six candles and one lamp, a gas apparatus would cost from £10 to £12, the cost of which will be saved during the first year.

The critical remark of the editor of this paper is truly amusing, for, by a train of reasonining, he states that he is compelled to oppose the introduction of gas, because *it will raise the price of butchers' meat.* The editor argues that if gas supersedes candles, the price of tallow will fall; therefore, as the fat of animals will be reduced in value, butchers will have to charge a higher price for the leaner portion of the meat, so as to realize the value of the beasts. "*Therefore, as food is of more consequence than artificial light, it is rather to be depreciated.*"

A year later Mr. Cook read a second paper "On the Advantages of Coal Gas Light" (*Phil. Trans.* 98), which shows that the methods of preparing the gas was very imperfect, and an unpleasant oder was given off when it was used. In regard to this, Mr. Cook says, in reference to this objection that the smell occasioned by the gas is injurious to health, and that "it rather tends to preserve health by destroying contagion, *and purifying the air.*"

This absurd statement appeared to give satisfaction to the editor, who in his observations on the paper states: "Information of this kind has long been wanted, and those who have made the greatest bustle on the wonderful advantages attending the use of the gas light have, in this respect, been deficient." Possibly public opinion was leaning towards the introduction of gas, for the same editor, who in 1809 observed, on Mr. Murdock's paper, that "the expense of the apparatus will always be against its introduction on a small or middling scale," now observes, in 1810, "The statement of Mr. Cook clearly proves the great advantages connected with those lights, *even on a small scale.*"

It is not intended that the foregoing represents the history of the introduction of gas for illuminating purposes, but it gives phases of the question which are of interest at this moment, and shows that, as in the introduction of the electric light for the same purpose, its development was very gradual. It will be seen that the economy of both gas and electricity for lighting purposes was at first disputed, both were afterwards considered only adapted for large buildings, then came the time when each was shown to be fitted for domestic purposes. The introduction of gas was considered "*a bubble,*" and when all other objections had been exhausted, scientific testimony of that day finally stated that gas lighting would raise the price of beef. When gas lighting was first introduced, the idea of a great central manufactory for a city was not even dreamed of; possibly at that time the mere suggestion of such a design would have caused a panic; but that it was successfully accomplished we all know. Gas was also first used for lighting large buildings, but it required the genius of one man to invent a process for its purification, so as to make it practical for general illuminating purposes.

The reader, with a knowledge of recent events, can easily compare them with the facts here recorded respecting the early days of gas, and notice how history has again repeated itself.

First the possibility of using the electric light for general illuminating purposes was denied, then its adaptability for large buildings was admitted, and now finally its use for domestic purposes is unquestioned.

The economy of electric lighting was also assailed, but the arguments are now getting stale. As each consumer had at first to make his own gas, so the first idea of electric lighting was coupled with the necessity on the part of each consumer to own his own electric generator and it was reserved for Edison to reform the whole system, and put it on a practical footing. He first publicly exhibited an electric lamp, that could compete with gas, and that was adapted for the general illumination of houses by electricity; he first subdivided the electric current, and thus demonstrated that its economic use was a possibility, and he will be the first to achieve the final triumph of establishing a central station for the manufacture of electricity and conducting it to the houses of the people.

Capitalists combining with scientific experts and patent pirates may endeavor to strip Edison of the honors due to him, earned by patient and exhaustive study of the question. That the electric light would eventually supersede gas for general illuminating purposes, no one doubted, but that Edison by bringing to bear upon it his great inventive powers, combined with almost unlimited resources, has advanced the time for accomplishing the result by at least fifty years, will be admitted by all unprejudiced persons. J. M.

THE DISTRIBUTION OF TIME.

By Professor Leonard Waldo.

From time to time within the last twenty years there have appeared articles in the public prints which indicated an awakening and growing interest in the practicability of having wide sections of our country transact its business and govern its social duties by a common time. Within the last few years official reports from various observatories, departments of the Government, scientific societies and telegraph companies, have shown so considerable a progress in the introduction of uniform systems of time, and these systems have been so cordially received by the communities interested, that there can be no doubt that the country is ready to be divided into a few great sections, each of which shall be governed by its own standard, which shall bear some simple relation to the standards governing the neighboring sections.

The principal systems now in operation comprise the United States Naval Observatory system, which extends its distribution of Washington time to Chicago and the West; the Harvard and Yale systems, which distribute, respectively, Boston and New York time over New England; the Alleghany Observatory system, which is concerned chiefly with the Pennsylvania Railroad; and the more local services emanating from the observatories at Albany, Chicago, Cincinnati, and St. Louis. Unfortunately, except in New England, the distribution of the time of an observatory has not always resulted in the adoption of that time for general use, and it is often the case that the local jewelers who are guardians of town clocks, and local time as well, will convert the time received by telegraph into their own local time, and thus make it inconveniently different from the time in use in any other city of their region.

A railroad may or may not secure the adoption of its own time in the cities along its route. It is generally a question as to which is the most important, the railroad or the town. But certain it is that there is not an important railroad in the country, outside of New England, along which the commercial traveler may go without having to compute the discrepancy between his watch and the time kept by the business men at one-half of the stopping-places. Thus it happens that, even where cities are closely connected by large railroads, the people have been dictated to by their jewelers regarding their standard of time, when a little reflection shows that there is only a very questionable advantage arising from having a local time simply because the jewelers of the city insist on a time which shall appeal to the local pride of their customers.

On the other hand, the disadvantage of having the factory operatives begin work on railroad time and stop on local time, because they gain ten minutes a day by that sharp practice; the jostle and inconvenience in the commercial interchange between two neighboring cities, because the stock exchanges, business offices and the banks, close with a difference of ten minutes; the thousand engagements broken by the discrepancies of time—all indicate the need of the adoption of such a common time as already exists in the European countries.

The writer has always felt that the railroads ought to be the most influential means in securing uniformity. They can be successfully appealed to for the financial support which any accurate system demands, because they have a direct and strong interest in the use of the same time at every office and by every employee of their roads. The superintendents, too, with whom the decision of such matters generally rests, are keenly alive to anything which lessens the risk of accident, and they at once appreciate the advantage of having the clocks of intersecting roads, and of the towns through which their roads pass, all indicate the same time. The control of a telegraph wire for railroad business gives them the means of transmitting time-signals, and in New England it is the railroads which have virtually caused the all but universal acceptance of the Boston and New York standards referred to. Outside of New England there has been scarcely any concert of action among the railroads, and there are about seventy different standards of time in use. The result of the experiment in New England fairly justifies the belief that, were the railroads in the rest of the United States approached on this question, they would combine to adopt the standards of time now used by a few of the great centres of population. Thus, while it was found quite impossible to unite the New England roads upon Boston time, and while it would have been equally impossible to cause the Boston roads to run on New York time, it has proved highly satisfactory to allow the current of travel, which always drifts toward the nearest centre of population, to decide the matter. To bring into use in a large section of the country two standards, where before there has been a dozen, is the first step toward uniting the two into one; and, in the writer's opinion, it is only by a gradual amalgamation of different local times that the final adoption of a few standards for the whole country can be effected. As a rule, railway corporations are more intelligent on this subject than the town councils which are elected by popular suffrage. They are also urged to encourage uniform time by their own interests. They are under the direct influence of State legislation, and the agreement of a number of railroads can be made to influence the communities of the regions traversed to use the railroad standard. Whether the pressure of State legislation ought to be used is an open question. It has been the writer's experience that the railroads are quite willing to do their part without recourse to any such means; and with the average railroad official the fact that a service is to be enforced by legislation prejudices him against it.

The difficulties in the way of introducing a new standard would still further be reduced if the observatories universally took care to distribute a time which should be as accurate as human art could make it, and use only such simple means of rendering it available as could allow of no vitiation of the message over the time-telegraph wires. By so doing the observatories would, so to speak, have a monopoly of the best article in the market, for no private jewelers could hope to furnish the local time with the precision obtained in a first-class observatory, where every means is taken to insure accuracy. There is, however, little use in trying to supplant a local time which is furnished by a respectable jeweler who takes good care of a good clock, and who has acquired the art of determining his time carefully, if the new system of signals is not to be relied upon within a single second. Unfortunately, the example set the time-services of the country, by that under the direction of the Naval Observatory at Washington, is not of the best; and, until it is realized by the proper officers that a division of responsibility in the charge of delivering time-messages results in the inaccuracy of the service to the public, the services organized under the control of universities will occupy the first place for accuracy.

The best, because the most unmistakable in its indications, of the means yet proposed for the distribution of a public time consists in the ordinary telegraph receiving-instrument, which is brought into circuit with the observatory clock at stated intervals. The clock then automatically beats in such a manner as to indicate the beginning of the minute, or of the five minutes, which have been agreed upon for the reception of the time by telegraph.

Experience has shown that the average railroad employee or telegraph operator very quickly apprehends this method of transmission, and, since the clock effects the distribution automatically, if the signals are received at all they must be exact. The very tempting method of propelling the hands of clocks by electricity has never been successfully applied over extended areas; and the nearest approach to an accurate service from a distant observatory takes place when the pendulum of the clock at a distance from the observatory is moving in sympathy with the observatory clock, through the action of induced electrical currents. A very good example of this kind may be seen in the Treasury clock, at Washington, where one of the Observatory clocks controls it, beat by beat, through the intervention of a mile of telegraph-wire. In this system, which is commonly known as Jones's system, the interruption of the telegraphic circuit, by storms or otherwise, does not cause the controlled clock to stop, as in the systems above referred to; but one can never be sure, when the current is restored, that the controlled clock will not have deviated during the stoppage of its control; and this method has not proved successful where high accuracy is demanded, or the telegraph lines are liable to such interruptions as are common in our climate. This method, however, has found considerable favor in England, and the writer had little difficulty in using a clock, so controlled, at the end of a well-protected wire four miles distant from the Observatory of Harvard College. It was not, however, perfectly reliable, and errors of from two to ten seconds were sometimes found to exist in the controlled clock.

Of the new method, which originated, we believe, in Vienna, and has made its way as far westward as Paris, of setting clocks by means of pneumatic tubes, there can be a great deal said on the score of economy, when the system is applied to large cities. It certainly would be a popular idea to have the time laid on, as the water or gas is, from a small pipe passing the door. The special clock needed would be furnished and kept in order by the small payment of a small annual rental. The expense would be trifling as compared with any system yet suggested of equal accuracy, and the field is so promising that it would be strange if attempts were not soon made in our large cities to occupy it. But such or any similar systems for the local distribution of time will depend upon the accurate and regular reception of the standard from an observatory which may be several hundred miles distant; and for this principal service, as well as for the railroads, the writer has already expressed the opinion that the transmitting and receiving apparatus of the telegraph companies, in connection with an observatory clock, affords the best, as well as the simplest, means.

So much for the public distribution for commercial and social purposes. There is another and extremely important service, too much neglected in our country, in behalf of the merchant marine. The Royal Observatory at Greenwich justly considers the accurate dropping of the time-balls on the English coast of almost equal importance with the transmission of time over England. A similar service should be undertaken by our own Naval Observatory, and the suggestions embodied in Professor Holden's report to the Secretary of the Navy*, on this subject, receive the cordial support not only of the officers of the navy and of the merchant marine, but of those men of science whose attention has been called to the lack of such a service at the important ports of Philadelphia, Baltimore, and San Francisco.

Such a service is performed for the port of New York, though not with the assurance of accuracy we have a right to expect in such a Government work. The Observatory of Harvard College, in connection with the United States Army Signal Service, drops a time-ball for the benefit of Boston Harbor, and perhaps there is no one public signal of the Harvard Time Service which is received with more public favor than this, not only by the commanders of vessels lying in the harbor, but by the people living on the surrounding highlands, and numerous factories and institutions from which the signal is visible. This signal owes its existence to the public spirit shown by the Equitable Life Insurance Company, of New York, in erecting the apparatus necessary upon the top of their magnificent building. The time-balls in Boston, New York, and Washington, have thoroughly ingratiated themselves in the public favor.

The cost of the construction of a time-ball of the

* Report of the Secretary of the Navy, Second Session, Forty-fourth Congress.

best materials and of sufficient size, with the electrical apparatus necessary, is about a thousand dollars, and, although it is the most accurate signal for popular use, yet the time-gun has many advantages, on the score of economy and convenience, over the more exact time-ball. The time-gun could be extemporized from one of a battery, at any place where there is a detachment of the artillery service permanently located. Of course, there is an error owing to the time required for the sound to traverse the distance from the gun to the hearer, but this is insignificant for ordinary purposes, and it is not necessary to take any other trouble than to merely listen for the report of the gun which is known to be discharged by an electrical current from some observatory at an arbitrary instant. The time-guns have shown themselves to be very popular in Great Britain and on the Continent; and if our army, either through its Signal Service or the artillery, could act in concert with observatories in different parts of the country, the discipline necessary for the efficient performance of such a service would be obtained, and the service would be extremely popular among the people.

Doubtless the Naval Observatory could assist in distributing the time to the whole country, but there are several reasons why it would be inexpedient for many years to come. That observatory has a legitimate sphere in fostering astronomical science throughout the country, and in performing such services as are directly for the benefit of the navy and other Government offices.

There are several observatories, particularly in our Western cities, which rely for a large share of their hold of the popular sympathy upon the public time-signals which they furnish. So long as they are strongly interested in the growth of their local service, they will do missionary work for science by interesting the people in the observatory which gives them their time.

Now, let these communities be approached through the offices of the telegraph companies acting as the agents of the Naval Observatory, and the majority will at once feel, with some truth, that the matter is no longer one of science and the patronage of a local or State institution, but that the telegraph companies are urging for their own profit the introduction of a service for which the people have not sufficient need to pay the price charged. In support of this view it might be mentioned that under date of April 2, 1877, our most prominent Telegraph Company issued an official circular through the agency of its principal local offices throughout the United States, which urged the importance of accurate time, and made financial proposals to furnish the Naval Observatory time to seventy-eight cities of the United States once a day, at a charge varying from seventy-five to five hundred dollars per year for each place. So far as the writer knows, there has not been a single acceptance of these proposals, and even one or two acceptances might be considered exceptions to a rule. Another difficulty is the cost of the service to cities which are far distant from the distributing office. The telegraph companies justly claim that this service ought to be paid for at a higher rate than ordinary business messages because it is preferential, and all other business must cease at a given time. This arbitrary stoppage may sometimes prove highly inconvenient, and presupposes a thoroughness of discipline among employees which it is difficult to maintain over the long lins of our Western country. The service to be popular must be quick to redress grievances, and accommodating in the details of its work, particularly at its initiation. It is evident that these agencies are best insured by having the friendship toward the observatory of an important class in the community somewhat dependent on the efficiency of its time-service.

The furnishing of correct time is educational in its nature for it inculcates in the masses a certain precision in doing the daily work of life which conduces, perhaps, to a sounder morality. and this idea will not seem far-fetched if we consider how strikingly indicative of the character of a people in the scale of civilization is the promptness with which they transact their business. It is felt, therefore—and particularly in New England—that the university does a creditable action when it directly encourages the distribution of time from its observatory. This view will be adopted by the Wstern institutions of learning as they gradually rise to the dignity of having distinct observatories connected with them.

At the last meeting of the American Association for the Advancement of Science, in Boston, a committee was appointed to urge the adoption of uniform systems in various parts of the country. This committee includes the representatives of the observatories which have dode most in this cause.

The American Metrological Society, through a committee, have presented a carefully prepared report on the present condition of this question in the United States.* It is the opinion of that committee that the standards of time for the various parts of the country should differ by even hours, beginning with the meridian which is just four hours west of Greenwich, and designating the systems as in the last column of the following table:

PROPOSED SCHEDULE OF STANDARDS OF TIME.

Geographical Section.	Standard Meridian.	Time Slower than Greenwich.	Designation.
Newfoundland, New Brunswick, Nova Scotia, etc.	60° west.	4 h. o m. o s.	Easern time.
Canada, Atlantic States, Ohio to Alabama, Lower Lakes	75° "	5 h. o m. o s.	Atlantic time.
Mississippi Valley, Missouri, Upper Lakes, Texas	90° "	6 h. o m. o s.	Valley time.
Rocky Mountain regions	105° "	7 h. o m. o s.	Mountain time.
Pacific Slope, British Columbia, Vancouver's Island	120° "	8 h. o m. o s.	Pacific time.

The constitution of both of these committees is such that they would favor the distribution of standards of time according to any such scheme as the preceding, rather than the distribution of a single time from the Naval Observatory. The above scheme, in the opinion of those who have given much thought to the subject, is the best one so far presented. It was due originally to Professor Benjamin Peirce, and its great merit consists in there being no greater difference than half an hour in any part of the country between the true local time and the arbitrary standard—an amount but slightly greater than exists between Greenwich and the west of England. In passing from Ohio into the Mississippi Valley, for instance, the traveler merely changes his watch by one hour; and the merchant, remembering that Pacific time is three hours slow of Atlantic time, knows that it is half-past two in San Francisco when it is half-past five in New York.

Any scheme which proposes the adoption of a uniform time from one extremity of the country to the other must be looked upon as chimerical for a century to come. Ten o'clock in the morning at once conveys to our minds an idea of the average occupation of our people at that time; it is associated with a certain brightness of daylight; it means that the working classes have been occupied with their daily task about three hours; we expect to find the majority of banks and shops open; and any disturbance of these traditional times would be received with marked disfavor. To learn, for instance, from the morn-

* Proceedings of the Metrological Society, vol. ii. New York; Published by the Society.

ing paper that a distinguished public man had arrived in San Francisco late in the evening, and, fatigued with his journey, had retired at seven o'clock, would give the Eastern reader a sense of the utter strangeness of keeping a time three hours different from local time.

Any action for the establishment of standards of time over the country must begin by securing the active coöperation of the telegraph companies. The most influential of these companies has been traditionally public-spirited in allowing the use of its wires for scientific purposes, often at considerable expense to itself. The service of transmitting time occupies at present such an extremely small proportion of its ordinary business that the company has not as yet an officer of its service empowered to carry out the details necessary for such time-distributions as have been already discussed. If, however, the committees referred to could prepare a scheme that was thoroughly practical, and agree upon a uniformity of details which should not seriously interfere with the ordinary business of this or any other company, it is believed that the company would find it to their own interest to establish a regular system of procedure to govern their action in the case of observatories in different parts of the country which desire to secure their services in transmitting time-signals. In consideration of the assumption of responsibility and the efforts at introduction made by the observatory, the company would probably be found willing to so adjust their charges that it would prove to be entirely practicable for the various observatories to secure a large patronage for the services emanating from them without the financial burden seeming an undue amount.—*North Am. Rev.*

[*Continued from page* 270.]

THE UNITY OF NATURE.

By the Duke of Argyll.

III.

ANIMAL INSTINCT IN ITS RELATIONS TO THE MIND OF MAN.

All the knowledge and all the resource of mind which is involved in these instincts is a reflection of some Agency which is outside the creatures which exhibit them. In this respect it may be said with truth that they are machines. But then they are machines with this peculiarity, that they not only reflect, but also in various measures and degrees partake of, the attributes of mind. It is always by some one or other of these attributes that they are guided—by fear, or by desire, or by affection, or by mental impulses which go straight to the results of reasoning without its processes. That all these mental attributes are connected with a physical organism which is constructed on mechanical principles, is not a matter of speculation. It is an obvious and acknowledged fact. The question is not whether, in this sense, animals are machines, but whether the work which has been assigned to them does or does not partake in various measures and degrees of the various qualities which we recognize in ourselves as the qualities of sensation, of consciousness and of will.

On this matter it seems clear to me that Professor Huxley has seriously misconceived the doctrine of Descartes. It is true that he quotes a passage as representing the view of "orthodox Cartesians," in which it is asserted that animals "eat without pleasure and cry without pain," and that they "desire" nothing as well as "know" nothing. But this passage is quoted, not from Descartes, but from Malebranche. Malebranche was a great man; but on this subject he was the disciple and not the master; and it seems almost a law that no utterance of original genius can long escape the fate of being travestied and turned to nonsense by those who take it up at second hand. Descartes' letter to Moore, of the 5th February, 1649, proves conclusively that he fully recognized in the lower animals the existence of all the affections of mind except "Thought" (*la Pensée*), or Reason, properly so called. He ascribes to them the mental emotions of fear, of anger, and of desire, as well as all the sensations of pleasure and of pain. What he means by thought is clearly indicated in the passage in which he points to Language as the peculiar product and the sole index of Thought—Language, of course, taken in its broadest sense, signifying any system of signs by which general or abstract ideas are expressed and communicated. This, as Descartes truly says, is never wanting, even in the lowest of men, and is never present in the highest of the brutes. But he distinctly says that the lower animals, having the same organs of sight, of hearing, of taste, etc., with ourselves, have also the same sensations, as well as the same affections of anger, of fear, and of desire—affections which, being mental, he ascribes to a lower kind or class of Soul, an "âme corporelle.' Descartes, therefore, was not guilty of confounding the two elements of meaning which are involved in the word machine—that element which attaches to all machines made by man as consisting of dead non-sentient matter—and that other element of meaning which may be legitimately attached to structures which have been made, not to siumlate, but really to possess all the essential properties of Life. "Il faut pourtant remarquer," says Descartes, emphatically, "que je parle de la pensée *non de la vie, ou de sentiment*."[1]

The experiments quoted by Professor Huxley and by other Physiologists, on the phenomena of vivisection, cannot alter or modify the general conclusions which have long been reached on the unquestionable connection between all the functions of Life and the mechanism of the body. The question remains, whether the ascertainment of this connection in its details can alter our conceptions of what Life and sensation are. No light is thrown on this question by cutting out from an organism certain parts of the machinery which are known to be the seat of consciousness, and then finding that the animal is still capable of certain movements which are usually indicative of sensation and of purpose. Surely the reasoning is bad which argues that because a given movement goes on after the animal has been mutilated, this movement must therefore continue to possess all the same elements of character which accompanied it when the animal was complete. The character of purpose in one sense or another belongs to all organic movements whatever—to those which are independent of conscious sensation, or of the will, as well as to those which are voluntary and intentional. The only difference between the two classes of movement is, that in the case of one of them the purpose is wholly outside the animal, and that in the case of the other class of movement the animal has faculties which make it, however indirectly, a conscious participant or agent in the purpose, or in some part of the purpose, to be subserved. The action of the heart in animals is as certainly "purposive" in its character as the act of eating and deglutition. In the one the animal is wholly passive—has no sensation, no consciousness, however dim. In the other movement the animal is an active agent, is impelled to it by desires which are mental affections, and receives from it the appropriate pleasure which belongs to consciousness and sensation. These powers themselves, however, depend, each of them, on certain bits and parts of the animal mechanism; and if these parts can be separately injured or destroyed, it is intelligible enough that consciousness and sensation may be severed for a time from the movements which they ordinarily accompany and direct. The success of such an experiment may teach us much on the details of a general truth which has long been known—that conscious sensation is, so far as our experience goes, inseparably dependent upon the mechanism of an organic structure. But it cannot in the slightest degree change or modify our conception of what conscious sensation in itself is. It is mechanical exactly in the same sense in which we have long known it to be so—that is to say, it is the result of life working in and through a structure which has been made to exhibit and embody its peculiar gifts and powers.

Considering now that the body of man is one in structure with the body of all vertebrate animals—considering that, as we rise from the lowest of these to him who is the highest, we see this same structure elaborated into closer and closer likeness, until every part corresponds, bone to bone, tissue to tissue, organ to organ—I cannot doubt that Man is a machine, precisely in the same sense in which animals are machines.

[1] "Œuvres de Descartes," Cousin, vol. x. p. 205 *et seq.*

If it is no contradiction in terms to speak of a machine which has been made to feel and to see, and to hear and to desire, neither need there be any contradiction in terms in speaking of a machine which has been made to think, and to reflect, and to reason. These are, indeed, powers so much higher than the others that they may be considered as different in kind. But this difference, however great it may be, whether we look at it in its practical results, or as a question of classification, is certainly not a difference which throws any doubt upon the fact that all these higher powers are, equally with the lowest, dependent in this world on special arrangements in a material organism. It seems to me that the very fact of the question being raised whether Man can be called a machine in the same sense as that in which alone the lower animals can properly be so described, is a proof that the questioner believes the lower animals to be machines in a sense in which it is not true. Such manifestations of mental attributes as they display are the true and veritable index of powers which are really by them possessed and enjoyed. The notion that, because these powers depend on an organic apparatus, they are therefore not what they seem to be, is a mere confusion of thought. On the other hand, when this comes to be thoroughly understood, the notion that Man's peculiar powers are lowered and dishonored when they are conceived to stand in any similar relation to the body must be equally abandoned, as partaking of the same fallacy. If the sensation of pleasure and of pain, and the more purely mental manifestations of fear and of affection have in the lower animals some inseparable connection with an organic apparatus, I do not see why we should be jealous of admitting that the still higher powers of self-consciousness and reason have in Man a similar connection with the same kind of mechanism. The nature of this connection in itself is equally mysterious, and, indeed, inconceivable in either case. As a matter of fact, we have precisely the same evidence as to both. If painful and pleasurable emotions can be destroyed by the cutting of a nerve, so also can the powers of memory and of reason be destroyed by any injury or disease which affects some bits of the substance of the brain. If, however, the fact of this mysterious connection be so interpreted as to make us alter our conceptions of what self-consciousness, and reason, and all mental manifestations in themselves are, then indeed we man well be jealous—not of the facts, but of the illogical use which is often made of them. Self-consciousness and reason and affection, and fear and pain and pleasure, are in themselves exactly what we have always known them to be ; and no discovery as to the physical apparatus with which they are somehow connected can throw the smallest obscurity on the criteria by which they are to be identified as so many different phenomena of mind. Our old knowledge of the work done is in no way altered by any new information as to the apparatus by which it is effected. This is the error commttted by those who think they can found a new Psychology on the knife. They seem to think that sensation and memory, and reasoning and will, become something different from that which thiterto we have known them to be, when we have found out that each of these powers may have some special "seat" or "organ" in the body. This, however, is a pure delusion. The known element in psychology is always the nature of the mental faculty; the unknown element is always the nature of its connection with any organ. We know the operations of our own minds with a fullness and realty which does not belong to any other knowledge whatever. We do not know the bond of union between these operations and the brain, except as a sort of external and wholly unintelligible fact. Remembering all this, then, we need not fear or shrink from the admission that Man is a reasoning and self-conscious machine, just in the same sense in which the lower animals are machines which have been made to exhibit and possess certain mental faculties of a lower class.

But what of this? What is the value of this conclusion? Its value would be small indeed if this conception of ourselves as machines could be defended only as a harmless metaphor. But there is far more to be said for it and about it than this. The conception is one which is not only harmless, but profoundly true, as all metaphors are when they are securely rooted in the Homologies of Nature. There is much to be learnt from that aspect of mind in which we regard its powers as intimately connected with a material apparatus, and from that aspect of our own bodies in which they are regarded as one in structure with the bodies of the brutes. Surely it would be a strange object of ambition to try to think that we are not included in the vast system of adjustment which we have thus traced in them ; that our nobler faculties have no share in the secure and wonderful guarantee which it affords for the truthfulness of all mental gifts. It is well that we should place a high estimate on the superiority of the powers which we possess ; and that the distinction, with all its consequences, between self-conscious Reason and the comparatively simple perceptions of the beasts, should be ever kept in view. But it is not well that we should omit from that estimate a common element of immense importance which belongs to both, and the value of which becomes immeasurably greater in its connection with our special gifts. That element is the element of adjustment—the element which suggests the idea of an apparatus—the element which constitutes all our higher faculties the index and the result of a pre-adjustment harmony. In the light of this conception we can see a new meaning in our "place in Nature;" that place which, so far as our bodily organs are concerned, assigns to us simply a front rank among the creatures which are endowed with Life. It is in virtue of that place and association that we may be best assured that our special gifts have the same relation to the higher realities of Nature which the lower faculties of the beasts have to the lower realities of tne physical world. Whatever we have that is peculiar to ourselves is built up on the same firm foundation on which all animal instincts rests. It is often said that we can never really know what unreasoning instinct is, because we can never enter into an animal mind, and see what is working there. Men are so apt to be arrogant in philosophy that it seems almost wrong to deprecate even any semblance of the consciousness of ignorance. But it were much to be desired that the modesty of philosophers would come in the right places. I hold that we can know, and can almost thoroughly understand, the instincts of the lower animals ; and this for the best of all reasons, that we are ourselves animals, whatever more ;—having, to a large extent, precisely the same instincts, with the additional power of looking down upon ourselves in this capacity from a higher elevation to which we can ascend at will. Not only are our bodily functions precisely similar to those of the lower animals,—some, like the beating of the heart, being purely "automatic" or involuntary—others being partially, and others again being wholly, under the control of the will—but many of our sensations and emotions are obviously the same with the sensations and emotions of the lower animals, connected with precisely the same machinery, presenting precisely the same phenomena, and recognizable by all the same criteria.

It is true that many of our actions became instinctive and mechanical only as the result of a previous intellectual operation of the self-conscious or reasoning kind. And this, no doubt, is the origin of the dream that all instinct, even in the animals, has had the same origin ; a dream due to the exaggerated "anthropomorphism" of those very philosophers who are most apt to denounce this source of error in others. But man has many instincts like the animals, to which no such origin in personal experience or in previous reasoning can be assigned. For not only in earliest infancy, but throughout life, we do innumerable things to which we are led by purely organic impulse ; things which have indeed a reason and a use, but a reason which we never know, and a use which we never discern, till we come to "think." And how different this process of "thinking" is we know likewise from our own experience. In contemplating the phenomena of reasoning and of conscious deliberation, it really seems as if it were impossible to sever it from the idea of a double personality Tennyson's poem of the "Two Voices" is no poetic exaggeration of the duality of which we are conscious when we attend to the mental operations of our own most complex nature. It is as if there were within us one Being always receptive of suggestions, and always responding in the form of impulse—and another being capable of passing these suggestions in review before it, and of allowing or disallowing the impulses to which they give rise.

There is a profound difference between creatures in which one only of these voices speaks, and man, whose ears are, as it were, open to them both. The things which we do in obedience to the lower and simpler voice are indeed many, various, and full of a true and wonderful significance. But the things which we do and the affections which we cherish, in obedience to the higher voice have a rank, a meaning, and a scope which is all their own. There is no indication in the lower animals of this double Personality. They hear no voice but one: and the whole law of their Being is perfectly fulfilled in following it. This it is which gives its restfulness to Nature, whose abodes are indeed what Wordsworth calls them—

"Abodes where Self-disturbance hath no part."

On the other hand, the double Personality, the presence of "Two Voices," is never wholly wanting even in the most degraded of human beings—their thoughts everywhere "accusing or else excusing one another."

Knowing, therefore, in ourselves both these kinds of operation, we can measure the distance between them, and we can thoroughly understand how animals may be able to do all that they actually perform, without ever passing through the processes of augmentation by which we reach the conclusions of conscious reason and of moral obligation. Moreover, seeing and feeling the difference, we can see and feel the relations which obtain between the two classes of mental work. The plain truth is, that the higher and more complicated work is done, and can only be done in this life, with the material supplied by the lower and simpler tools. Nay, more, the very highest and most aspiring mental processes rest upon the lower, as a building rests upon its foundation stones. They are like the rude but massive substructions from which some great temple springs. Not only is the impulse, the disposition, and the ability to reason as purely intuitive and congenital in Man as the disposition to eat, but the fundamental axioms on which all reasoning rests are, and can only be, intuitively perceived. This, indeed, is the essential character of all the axioms or self-evident propositions which are the basis of reasoning, that the truth of them is perceived by an act of apprehension, which, if it depends on any process, depends on a process unconscious, involuntary, and purely automatic. But this is the definition, the only definition, of instinct, or intuition. All conscious reasoning thus starts from the data which this great faculty supplies; and all our trust and confidence in the results of reasoning must depend on our trust and confidence in the adjusted harmony which has been established between instinct and the truths of nature. Not only is the idea of mechanism consistent with this confidence, but it is inseparable from it. No firmer gronnd for that confidence can be given us in thought than this conception—that as the eye of sense is a mechanism specially adjusted to receive the light of heaven, so is the mental eye a mechanism specially adjusted to perceive those realities which are in the nature of necessary and eternal truth. Moreover, the same conception helps us to understand the real nature of those limitations upon our faculties which curtail their range, and which yet, in a sense, we may be said partially to overpass in the very act of becoming conscious of them. We see it to be a great law prevailing in the instincts of the lower animals, and in our own, that they are true not only as guiding the animal rightly to the satisfaction of whatever appetite is immediately concerned, but true also as ministering to ends of which the animal knows nothing, although they are ends of the highest importance, both in its own economy and in the far-off economies of creation. In direct proportion as our own minds and intellects partake of the same nature, and are founded on the same principle of adjustment, we may feel assured that the same law prevails in their nobler work and functions. And the glorious law is no less than this—that the work of instinct is true not only for the short way it goes, but for that infinite distance into which it leads in a true direction.

I know no argument better fitted than this to dispel the sickly dreams, the morbid misgivings, of the Agnostic. Nor do I know of any other conception as securely founded on science, properly so called, which better serves to render intelligible and to bring within the familiar analogies of Nature those higher and rarer mental gifts which we know as genius, and even that highest and rarest of all which we understand as inspiration. That the human mind is always in some degree, and that certain individual minds have been in a special degree, reflecting surfaces, as it were, for the verities of the unseen and eternal world, is a conception having all the characters of coherence which assure us of its harmony with the general constitution and the common course of things.

And so this doctrine of animal automatism—the notion that the mind of man is indeed a structure and a mechanism—a notion which is held over our heads as a terror and a doubt—becomes, when closely scrutinized, the most comforting and re-assuring of all conceptions. No stronger assurance can be given us that our faculties, when rightly used, are powers on which we can indeed rely. It reveals what may be called the strong physical foundations on which the truthfulness of Reason rests. And more than this—it clothes with the like character of trustworthiness every instinctive and intuitive affection of the human soul. It roots the reasonableness of faith in our conviction of the Unities of Nature. It tells us that as we know the instincts of the lower animals to be the index and the result of laws which are out of sight to them, so also have our own higher instincts the same relation to truths which are of corresponding dignity and of corresponding scope.

Nor can this conception of the mind of Man being connected with an adjusted mechanism cast, as has been suggested, any doubt on the freedom of the Will,—such as by the direct evidence of consciousness we know that freedom to be. This suggestion is simply a repetition of the same inveterate confusion of thought which has been exposed before. The question of what our powers are is in no way affected by the admission or discovery that they are all connected with an apparatus. Consciousness does not tell us that we stand unrelated to the system of things of which we form a part. We dream—or rather we simply rave—if we think we are free to choose among things which are not presented to our choice,—or if we think that choice itself can be free from motives,—or if we think that we can find any motive outstde the number of those to which by the structure of our minds and of its organ we have been made accessible. The only freedom of which we are really conscious is freedom from compulsion in choosing among things which are presented to our choice,—consciousness also attesting the fact that among those things some are coincident, and some are not coincident, with acknowledged obligation. This, and all other direct perceptions, are not weakened but confirmed by the doctrine that our minds are connected with an adjusted mechanism. Because the first result of this conception is to establish the evidence of consciousness when given under healthy conditions, and when properly ascertained, as necessarily the best and the nearest representation of the truth. This it does in recognizing ourselves, and all the faculties we possess, to be nothing but the result and index of an adjustment contrived by and reflecting the Mind which is supreme in Nature. We are derived and not original. We have been created, or—if any one likes the phrase better—we have been "evolved:" not, however, out of nothing, nor out of confusion, nor out of lies,—but out of "Nature," which is but a word for the sum of all existence—the source of all order, and the very ground of all truth—the fountain in which all fullness dwells.

ASTRONOMICAL NOTES.

ON THE DETERMINATION OF THE VALUE OF ONE REVOLUTION OF A MICROMETER SCREW, ETC.

To determine the value of a revolution of a micrometer screw, it is desirable to use several different methods. The most common and least accurate is by the observation of the transits of stars over two wires of the micrometer, set at a known distance (in revolutions) apart. Mechanical measures, depending upon the measurement of the length of the screw, of the dimensions of the objective, and of the principal focal length of the telescope come next. The measures in arc of terrestrial objects of known linear dimensions come next. Bessel's triangulation of the Pleiades was made, in part, so that the distances of any pair of these stars might be used as a known celestial arc to be determined in terms of the screw revolution. Dr. Vogel, of Potsdam, determined the value of the screw of the Leipzig refractor by measuring the difference of declination between two stars with the micrometer, and afterwards using the divided declination circle of the equatorial to determine the whole arc. This method was improved in the determination of the value of the screw of the Washington equatorial, by measuring with the micrometer the difference of declination of two *standard* stars (ι and *c Orionis*) a degree apart. In these last methods the value of the known arc in the sky depends upon our knowledge of the positions of its two terminal points. Dr. Winnecke, of Strassburg, has recently employed an ingenious way, which is even more simple. The distance between some asteroid (whose orbit is well known) and any star near it, is measured on several nights, as the asteroid passes from north to south of the star (let us say). Then, although the absolute position of the asteroid is not known, its daily motions are well determined, and the arc moved over may be used as a known distance from which the value of the screw may be determined.

The following complete list of asteroids (21 in all) discovered by the late Prof. JAMES C. WATSON, Director of the Washburn Observatory, Madison, Wis., has been compiled by the aid of the list of "Minor Planets," published by Mr. A. N. Skinner in the *American Journal of Science and Arts*, Vol. XVIII, *Dec.*, 1879. All of these asteroids, with one exception, were discovered at the Ann Arbor Observatory, Michigan. Juewa was discovered at Peking, China, where Prof. Watson was in charge of one of the Transit of Venus parties.

NUMBER.	NAME.	DATE OF DISCOVERY.
79	Eurynome	September 14, 1863.
93	Minerva	August 24, 1867.
94	Aurora	September 6, 1867.
100	Hecate	July 11, 1868.
101	Helena	August 15, 1868.
103	Hera	September 7, 1868.
104	Clymene	September 13, 1868.
105	Artemis	September 16, 1868.
106	Dione	October 10, 1868.
115	Thyra	August 6, 1871.
119	Althæa	April 3, 1872.
121	Hermione	May 12, 1872.
128	Nemesis	November 25, 1872.
132	Aethra	June 13, 1873.
133	Cyrene	August 26, 1873.
139	Juewa	October 10, 1874.
150	Nuwa	October 19, 1875.
161	Athor	April 19, 1876.
168	Sibylla	September 28, 1876.
174	Phædra	September 3, 1877.
175	Andromache	October 1, 1877.

The report of the *Telegraphic Determination of Longitudes on the East Coast of South America*, by Lieutenant Commanders F. M. GREEN, and C. H. DAVIS, and Lieutenant J. A. NORRIS, U. S. N., has been issued recently from the Hydrographic Office. This work embraces the meridians of Lisbon, Madeira, St. Vincent, Pernambuco, Bahia, Rio de Janeiro, Montevideo, Buenos Ayres and Para, and is designed to supplement the work done in 1877, under the direction of Lieutenant Commander Green, in the West Indies and Central America, by connecting important points in South America, whose longitudes have always been exceedingly uncertain, with well-known places in Europe.

Having made arrangements with the French *Bureau des Longitudes* to furnish the party with the difference of longitude between Lisbon and Paris, the work was begun in December, 1877, by connecting Lisbon, Portugal, with Funchal, Madeira, by means of an intervening station at Carcavellos. This "transmitting" station was found necessary in order to connect the submarine cables with the land lines; a direct connection endangering the safety of the cables. Partly by cables, and partly by the overland wires, the stations from Lisbon to Buenos Ayres were connected in the order named above, with the exception of a break between Pernambuco and Rio de Janeiro caused by a defect in the cable. These two stations were connected with Bahia, and Pernambuco with Para in 1879; and as the French Government had failed to communicate to the Hydrographic Office the longitude of Lisbon, it was determined to connect Lisbon with Greenwich, in order to make the chain complete. This last connection was effected by means of transmitting stations at Porthcurnow, Lands End, and Carcavellos on the coast of Portugal. The reduction of comparisons of the Lisbon and Greenwich clocks "gives the somewhat startling result that the longitude of the observatory at Lisbon, has, up to the present time, been in error more than two miles." The American determination of the difference of longitude between these two places being 9° 11″ 10.2″, while that heretofore accepted has been 9° 9′ 2.1″.

Of the instruments used, the Transit Instrument was of what is known as the "broken transit" pattern (the eye-piece being at one end of the horizontal axis), especially designed for this work by Mr. J. A. Rogers, and fitted to be used as both transit and zenith-telescope. It was of 2.5 in. aperture and 30 in. focal length—made by Kahler, It seems to have combined considerable steadiness with great portability, as it weighs in all but 125 lbs. In speaking of the performance of this instrument, the report says: "The results of the observations have demonstrated that the reversal of the axis is almost inevitably attended with a slight change of azimuth, and that a correction must always be introduced for flexure of the axis," and adds further on, that these effects "are probably unavoidable in portable instruments of this pattern."

In the reductions, no correction has been applied for personal equation of the observers, either in noting transits of stars, or in receiving the deflections of the galvanometer needle from the cables. After careful experiment, it was found that the correction would be quite small, and in view of the uncertainty involved in its determination, it was decided to take no account of such error, but to eliminate it, as far as possible, by placing one observer alternately east and west of the other, commencing at Lisbon. Advantage was taken of every opportunity to make latitude determinations with the zenith-telescope, and the results in both latitude and longitude show that nearly all of the stations occupied have been up to this time considerably in error.

The spectrum of Hartwig's comet has been observed by Konkoly and Backhouse, and by Young in this country. It gives four bright lines, whose wave-lengths are respectively 5609, 5492, 5169, and 4859 tenth-meters, and a faint continuous spectrum. W. C. W.

Washington, D. C., November 30, 1880.

SWIFT'S COMET.

Swift's comet is a faint object, and its distance from the sun is so great, never less than 1.102, and therefore always outside the earth's orbit, that no great changes of form are to be expected, such as we see in comets that pass near the sun. A. HALL.

To the Editor of Science:

Several interesting observations have been made by me of Swift's latest comet. The last observation was made on the evening of November 26th, at 7.20 P.M.T., being then by estimation in about A. R. 2 hours 30 minutes, north declination 53 degrees 45 minutes. It was quite a conspicuous object in the 5-inch Newtonian Reflector, being fully as bright on that date as at any previous observation, although its theoretical brightness is decreasing. It is a faint, diffused object, but to show that it is within the range of quite moderate telescopes, I would say that I first picked it up on the evening of November 5th, with a refractor of only two inches aperture. In my last observation two faint stars were seen shining through the comet.

The comet's position for the 10th of December will be A. R. 4 hours 40 minutes, Dec. + 44 degrees 47 minutes. On December 14th it will be about 5 degrees south of Capella. WILLIAM R. BROOKS.

Red House Observatory, Phelps, N. Y.,
November 30th, 1880.

MICROSCOPY.

Dr. Carpenter, the well-known English microscopist, occupied the attention of the Royal Microscopical Society, on the 6th instant, by describing the "Student Microscope," recently designed by Mr. George Wale, of New Jersey. The instrument in question was highly commended for its efficiency, and English opticians were advised to consider the practical improvements it suggests.

Mr. James Swift exhibited and described an improved form of *Calotte* diaphragm, consisting of a series of small circular apertures, to be applied above the achromatic condenser immediately beneath the object, and on a level with the surface of the stage.

A binocular eye-piece, by Professor E. Abbe, was described as consisting of two uncemented prisms (together forming a thick plate of glass) in the direct tube; the adjacent diagonal surfaces of the prisms being both cut at the calculated angle of 38° 5′, which angle was computed to allow precisely one-half of the light to be transmitted, and to reflect the other half; the latter half fell upon a total reflecting prism, whence the rays emerged through the diagonal tube to the left eye. Another point was the mechanism by which the *diagonal* tube attached to the direct tube by a box-fitting, was moved to accommodate the width of different observer's eyes, a screw motion causing the tube, with eye-pieces above and reflecting prism below, to travel smoothly nearer to or further from the direct and stationary tube.

The application of the eye-piece to the left tube at such a distance as to compensate for the extra distance travelled by the pencil of light, and thus render the images seen by both eyes of equal magnitude.

Lastly, the application of two semi-circular caps, one over either eye-piece; in one symmetrical position of these apertures the effect produced was *pseudoscopic* vision, by another arrangement of them stereoscopic vision was obtained.

This form of binocular is said to be specially applicable to the short tubes of Continental microscopes and some of American make,

A new fluid for writing the names of objects on glass slides is sold by Mr. Browning, of London. It is more active than hydrofluoric acid, and has an immediate action on the surface of glass.

Dr. Günther, of Berlin, has made photographs of *Frustulia Saxonica.* These and a micro-photograph by Mr. S. Wells, of Boston, were compared with the photograph by Dr. Woodward, produced in 1875. The latter showed no trace of beaded resolutions, whereas both the former showed the resolutions remarkably well. Mr. Mayall asks if Dr. Woodward still maintains his opinion of the unreality of the longitudinal lines.

Mr. Crisp mentions that Professor Abbe has found great advantage in mounting diatoms in monobromide of naphthaline, by which they were rendered far more visible than when mounted on Canada balsam.

BOOKS RECEIVED.

THE NATURALIST'S DIRECTORY FOR 1880. Edited by SAMUEL E. CASSINO, 299 Washington street, Boston. May, 1880.

This useful work will be welcome in scientific circles; it contains the names, addresses, special departments of Study, of Naturalists, Chemists, Physicists, Astronomers, etc., etc., etc. It also gives a list of scientific societies, of scientific periodicals, and the titles of scientific books published in America from July 1, 1879 to October 1, 1880.

The arrangement of the names in this edition of the directory is by States, and was adopted after repeated requests, though not, as the publisher admits, without misgivings on his part as to the convenience of the list thus arranged. On this point we are glad to notice that what we consider to be an error is acknowledged, and that in future the alphabetical order will be resumed. For our purposes the directory thus arranged is almost useless, as the loss of time in searching 45 separate lists for an address, is a great drawback to the use of the work.

We are also at a loss to know on what principle the list has been constructed, as the omission of the names of well-known scientific men is quite incomprehensible; as examples we fail to notice Professor John Le Conte, of California; Professor W. H. Brewer, of Yale; Professor Jas. D. Dana, of Yale; Professor Simon Newcomb, of Washington; Col. J. J. Woodward, M. D., Washington; Professor Asaph Hall; Professor Julius E. Hilgard, Washington; Professor C. Y. Young, of Princeton; Professor C. F. Chandler, of New York City; Professor Henry Draper, of New York City; and Professor Jno. W. Draper, of Hastings-on-Hudson, or Mr. Edison. We have had no time to make a systematic search for omissions, but the above names which are household words in scientific circles do not appear.

As we find some of these names have already appeared in previous editions, the present omission would not appear to be altogether accidental.

As this directory is the only one of its kind published, we suppose these errors will not effect its sale, but we regret that a more perfect work was not produced.

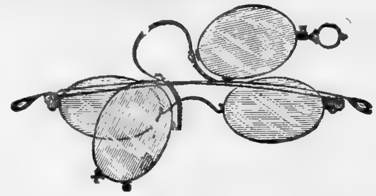
Since writing the above notice, we have heard from the publishers of the Directory; they state that the arrangement of the work is acceptable to a majority of the subscribers, and that the cause of the omission of names was due to their failure to receive responses to printed circulars which were forwarded to all known scientists.

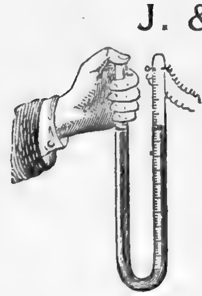
The readers of this journal must be familiar with the efforts we have made to secure a perfect register of the scientific men of the United States. Our intention in this respect was also made known by an editorial notice in the New York *Times*, and in the *Medical Record* of last week.

The *Times* pointed out the value of such a perfect list, and the little trouble it entailed on scientific men. So far the response to our appeal has been very partial. We therefore again request those who have hitherto failed to forward their names and addresses, with speciality of study, to do so at once, and if the heads of Universities and Colleges would make up lists, considerable help would be rendered.

We also suggest that those interested in scientific pursuits make up lists of scientific men in their neighborhood, and of amateurs following a particular line of scientific investigation.

As we stated lists of names will be forwarded to the Smithsonian Institution, and Messrs. Cassino and others will have the full benefit of it for future use.





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Engineering News	3.00	6.25	Scribner's Monthly	4.00	7.20
Engineering and Mining Journal	4.00	7.00	Science Observer	.50	4.40
Education	4.00	7.00	SCIENTIFIC AMERICAN	3.20	6.56
Engineering Magazine	5.00	8.25	" " SUPPLEMENT	5.00	8.00
Harper's Magazine	4.00	7.00	" " WITH SUPPLEMENT	7.00	9.60
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JOHN MICHELS, Editor.

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NOTICE TO SUBSCRIBERS.

We consider it due to those subscribers who have favored us with their subscriptions, previous to the publication of our club rates, that they should have the privileges of the list. They can therefore send us subscriptions for any or all of the publications named at the reduced double rates, less $4, the subscription price of "SCIENCE."

Since the publication of the club rates last week, we have received rates from the proprietor of *The American Journal of Science and Arts*, the terms of which are $6 a year. The club rate with SCIENCE will be $8.50 per annum.

THE Report of the United States Commissioner of Education, for the year 1878, has just reached us, and as but twenty days intervenes before 1881 will make its debut, the first impression on opening the volume is that it is already somewhat out of date. We believe that the cause of delay in printing this and other reports is attributable to the slow action of Congress in making the appropriations for printing, and we trust that in future the Commissioner may have facilities for publishing his report at an earlier date, as both its value and interest are much diminished by its being circulated two years after the facts recorded have transpired.

Thus, the first line of the report lamenting the existence of the financial depression, is read with impatience in these booming times. We congratulate the Commissioner on the fact that "the assault on the bulwarks of society, by ignorant, unfortunate or unprincipled persons," has not been so destructive as was anticipated. Society at least survives, notwithstanding the action of those "who would modify our present freedom of conscience, and of those who would establish a distinction of classes with a view to a permanent aristocracy, or practice some form of destructive communism." These gloomy political forebodings, which hardly appear to come within the range of Educational Statistics (in the absence of the catastrophy indicated), may now be read without alarm, and we feel tempted to suggest the propriety of publishing official prophetic utterances, while anticipations may yet "lend enchantment to the view."

The Commissioner of Education makes a strong appeal to public opinion, that Congress may be influenced to place more adequate means at his disposal to carry out the duties of his office. "Called upon by thoughtful educators in anticipation of perils, from which it was hoped he might afford relief or safety, and in the midst of ignorance on the one hand and indifference or opposition on the other," he complains that he is not furnished with either the quarters, the assistants, or the money necessary to do the work required.

To enable the Bureau of Education to perform its national functions satisfactorily, without the co-operation of volunteer aid, which has in the past enabled it to accumulate information, the Commissioner wishes Congress to comply with six requests, which he makes in the following order: *First*, a sufficent force of competent and trained men and women; *Second*, proper quarters; *Third*, a library having everything printed on the subject of education; *Fourth*, a collection of educational appliances, the character of which is described; *Fifth*, appropriate means of receiving and collecting information in regard to educational systems, institutions and methods; *Sixth*, means to arrange all this information, publish it, or communicate it to the educators of the country.

We fear the Commissioner has somewhat weakened his case by showing his ability to present so ample a report with the means already at his command, but we trust that any substantial aid that he really stands in need of will not be withheld. The concessions he calls for appear quite reasonable and essential to his office, and his success in obtaining them will probably be controlled by his ability to prove that such is the case.

This Journal, representing one of the highest branches of education, naturally desires that a National Bureau, for collecting educational statistics, should be properly supported by the nation, so that no lack of means at the command of the Commissioner should justify an inadequate administration of the office.

We have made a few selections from this report, chiefly relating to scientific schools, and a few facts that appear of special interest. These will be found in another column.

THE EPSOM MINERAL WATER OF MISSOURI.

BY PROF. CHAS. E. WAIT.

A shallow well recently sunk within three miles of this place yields a mineral water which promises to be a valuable addition to the list of saline purgatives. A sample of this water was taken

October 23, and was examined, with the following interesting results:

ANALYSIS.

Temperature.... = 58° F.

Specific Gravity, = 1.006819.

SOLIDS.	GRAINS PER GALLON.
Sodic Carbonate	4.160
Calcic Carbonate	23.616
Magnesic Carbonate	.569
Ferrous Carb nate	.081
Sodic Chloride	27.312
Sodic Sulphate	4.844
Potassic Sulphate	9.730
Calcic Sulphate	67.231
Baric Sulphate	trace.
Magnesic Sulphate	264.505
Aluminic Oxide	.034
Ammonia	trace.
Silicic Oxide	.038
Organic Matters	1.178
	403.298

GASES.	CUBIC INCHES PER GALLON.
Carbonic Anhydride	23.178
Nitrogen	4.330
Oxygen	1.493
Hydrogen Sulphide	trace.
	29.001

Not enough thus far is known of the water to enable me to present any reliable data concerning its therapeutic value; but physicians here and elsewhere, who have tried it, pronounce it an exceedingly valuable water.

MISSOURI SCHOOL OF MINES,
ROLLA, *November* 26, 1880.

THE ANTHROPOLOGICAL SOCIETY.

The Anthropological Society, of Washington, met on Tuesday evening, December 7, in the Smithsonian Institution, Professor Otis T. Mason in the chair. The following papers were announced: "Superstitions," by Mr. A. S. Gatschet, and "Savage and Civilized Orthoepy," by Professor Lester F. Ward. Mr. Gatschet, after giving the definitions of different authors and finding them too narrow, ascribed to superstition the following meaning: A belief in a physical power operating either within or without us, acting miraculously to affect our bodies or our minds, and which can be influenced to grant our requests. The word is derived from *super stare*, to survive. There are two kinds of superstition, the religious, relating to the world of spirits, and that of the physical nature, relating to all the phenomena of sense. It is hard to draw the line where religion ends and superstition begins, but the latter most generally represents the forces of nature as deified or anthropomorphic. The existence of superstition is manifested in names of gods, those of the American gods representing the sun, moon, and forces of nature.

Symbolism plays an important part in this connection, as well as the cultus of dreams, augurytaboo, omens and prognostics; such as cheiromancy and fortune-telling, hunting and fishing signs, witchcraft, medical jetishes, meteoric showers, comets, amulitism, etc.

The causes of superstition are mental inertia and ignorance of the real causes of things, coupled with the insatiable desire to account for phenomena. Isolation is also a very fruitful source of these beliefs. They are valuable to us only when we can trace their origin; then they lead to a knowledge of savage psychology, and are of very great use. The author of the paper illustrated the various points taken up by many myths and superstitions from our Indians and other sources.

Mr. Gatschet, having spent several years in personal contact with the aboriginal mind, is very competent to form an opinion as to the rationale of our Indian superstitions.

Dr. Morgan took the ground that superstition is natural to our race, having found in his practice that few of his patients were free from it.

Mr. Mason drew attention to the worthlessness of these innumerable stories unless they are brought together in classes, so that out of them some clue may be found to their origin. Every intelligent mortal passes his life between two worlds, the known and the unknown. Between these two is a border land, where superstition dwells. Its inhabitants are different for different individuals or tribes, and vary with our growing years. For Mr. Haeckel it is peopled with atom-souls, and, for the savage, with the concrete souls of things.

NEW YORK ACADEMY OF SCIENCES.

THE MAN OF THE CAVES.

BY PROFESSOR W. BOYD DAWKINS, F.R.S., Owens College, England.*

The questions which we have to put to ourselves are these: At what time in the geological history of the earth did man appear? and what manner of man was he? The answers to these questions are to be found in the recent discoveries, in the deposits of ancient rivers, and in the accumulations in caverns, which have been explored in the Old World during the last 60 years. Inquiry into the antiquity of man falls within well defined limits in point of time. Since there were no living species of the higher mammalia in the earlier stages of the tertiary period, the Eocene and the Miocene, it is hopeless to look for a highly specialized being such as man, nor in the succeeding Pliocene is it likely that he will be discovered, since but very few of the living, higher mammalian forms were then on the earth. When we examine the next stage, or Pleistocene, a period characterized by the presence of numerous living mammalia in both the New and Old Worlds, the field is fairly opened before us for our inquiry. The conditions of life at that time were precisely those in which man would be expected to exist, and it will be my object to put before you the evidence as to the earliest man of which we have any certain knowledge.

In the Pleistocene period the physical conditions of Europe were wholly unlike those which it now presents. The sea-board of the Atlantic reached to the 100-fathom line, or 100 miles to the west of the coast of Ireland. The British Isles formed a part of the Continent of Europe, and the area of the North Sea formed a shallow valley, abounding in mammalia of various kinds. The Mediterranean Sea also was much smaller than it is now, a land barrier extending North into Spain by the way of Gibraltar, and another passing in the direction of Malta, Sicily, and Italy, while what is now the bed of the Adriatic Sea was dry land, and most of the islands in the Ægean Sea were the tops of ranges of hills overlooking rich and fertile valleys. The living mammals appearing on this tract of land consisted of Southern species—the hippopotamus, spotted hyena and others—which ranged as far north as Yorkshire.

A second division is composed of the Northern animals, such as the reindeer, the musk sheep, and the like, which ranged as far to the South as the Alps and the Pyrenees, while yet a third division, such as the stag, bison, and horse, ranged over nearly the whole of Middle and Southern Europe. The remains of these animals, lying side by side with extinct species, such as the mammoth and the woolly rhinoceros, characterise the Pleistocene deposits of Europe. There were great climatal changes in Europe during the Pleistocene age. The temperature gradually lowered, and in the North large masses of ice spread over certain regions. When the temperature was lowest the Northern animals advanced furthest to the South, and when the temperature was warmest the Southern animals advanced furthest to the North, and from the intimate association of their remains in ancient river deposits and in caves may be inferred that the Winters were very cold and the Summers very warm

* Lecture delivered before the Academy, December 6, 1880.

Besides the seasonal variations, there was a gradual lowering of the temperature which produced the phenomena known as Glacial, and which characterized the Glacial period, as it is generally termed. The appearance of man at this stage may be conveniently studied from the point of view of the river deposits of Crayford, in Kent, a place remarkable for the large number of mammoths, bisons and horses, which have there been exhumed. Numerous flint splinters of unmistakeable human workmanship were discovered in the Spring of the present year, under conditions which indicated the exact spot on which an ancient hunter sat and chipped them, and these chips being so little disturbed that it was found possible to put together several large masses, and to restore some of the original nodules from which the implements were made. In one case I was fortunate enough to discover an implement rudely chipped all around which indicated that the primeval hunter of the mammoths, bisons and horses of that neighborhood was in the same state of culture as the man who hunted reindeer in the valley of the Thames in the next or the latest stage of the Pleistocene period. The river valleys of the south of England are covered with sheets of gravel termed river drift, and these contain vast numbers of reindeer, as well as bisons and horses, and were accumulated at a time when the climate was severe. In these, numerous implements were discovered, extending from Peterborough, in the north, as far as the channel. Similar implements are also met with in France, and occur .n Spain, Italy, Greece, Northern Africa, and Egypt; they also occur in Asia Minor, and have been found throughout the peninsula of India. They indicate a primeval condition of savagery from which mankind has emerged, which was uniform over the whole of this area. It is not a little strange that the river-drift hunter should have used implements of precisely the same shape and material in the Indian jungles, in the forest-clad shores of the Mediterranean, and in the wilds of Middle and Northern Europe. No human remains assignable to this age are sufficiently perfect to allow of our passing opinion of man's physique, but they tell us that he was a man and not a "missing link." The researches of Dr. Abbott on the river gravels of Trenton appear to establish the fact that the river-drift man was an inhabitant of America during the time when the mammoth was living in the valley of the Delaware. The paleolithic implements of the ate Pleistocene river beds are rude and simple, although they show a considerable advance from the simple flake, which is the only trace left by the man of the middle Pleistocene. As regards the man of that period, it is probable that the plateau of Central Asia was the centre from which the race diverged.

On the bottom of the caves of Creswell, in Yorkshire, were found river-drift implements in association with vast numbers of gnawed bones of both living and extinct animals, brought in by hyenas, while in the upper portions were found implements of a higher type, composed of flint and carved bone. Among these was the incised figure of a horse; these imply a higher type than that of the river-drift, and belong to a state of culture known as that of the cave man. It seems to be unquestionable that the cave men were preceded in their habitations by the river-drift men, in some places at least, and that of the two sets of implements now found the ruder belongs to the latter race. It has been a debated question whether the civilization of the cave man was the outcome of the development of that of the river-drift man. The evidence seems to indicate that they must be classed either as two distinct races or as two sections of the same race, which found their way into Europe at widely different times—the river-drift men being of far greater antiquity in Europe than the others. The discoveries of late years tend to confirm the identification of the cave men with the Esquimaux. We infer that the cave men clothed themselves with skins, for instruments for dressing skins are found precisely like those now employed for that purpose by the Esquimaux. That they wore gloves is shown by carvings which represent them, and there is reason to believe that they were in the habit of decorating their persons in various ways. The art of representing wild animals in carvings and by sculpture was carried to a high stage of excellence by the cave-dwellers, and it is doubtful if an artist of the present time could do better work, or even as good, with the rude instruments used by them. One of the most interesting examples of their skill is shown by representation of a mammoth, and we know that the extinct creature is faithfully por-trayed, because its remains have come down to us perfectly preserved in the ice of the northern latitudes. In various ways the habits of the cave men correspond to those which now prevail among the Esquimaux.

NATURAL SELECTION.

A curious instance has occurred showing the difficulty of explaining the true theory of "Natural Selection," even to scientific men; it is therefore not surprising to find that those who are opposed to the principle from religious motives, fail to realize what is understood by the term. In a letter to *Nature*, Mr. Charles Darwin states he is sorry to find Sir Wyville Thompson does not understand this principle of natural selection as explained by himself and Dr. Wallace, as, if he had done so, he would not have wrttten a sentence found in his introduction to the voyage of the *Challenger*, as follows; "The character of the abyssal fauna refuses to give the least support to the theory which refers to the evolution of species to extreme variation, guided only by natural selection." This, says Mr, Darwin, is a standard of criticism not uncommonly reached by theologians and metaphysicians, when they write on scientific subjects, and asks, "can Sir Wyville Thompson name any one who has said that the evolution of species depends only on natural selection?" and continues, "as far as concerns myself, I believe no one has brought forward so many observations on the effect of the use and disuse of parts, as I have done in my 'Variations of Animals and Plants under Domestication,' and those observations were made for that special object. I have also there adduced a considerable body of facts, showing the direct action of external conditions on organisms, though, no doubt, since my books were published, much has been learnt on this head."

PROPAGATION OF SOUND BY LIGHT IN 1811.

In searching a volume, dated 1811, for papers relating to the introduction of illuminating gas, we noticed a paper by Modeste Parolette, entitled "Inquiries Concerning the Influence of Light on the Propagation of Sound," taken from the *Journal de Physique*, Vol. LXVIII.

Although Parolette cannot be said to have anticipated those physical facts, the knowledge of which enabled Edison to design that wonderful instrument, the *Tasimeter*, and since developed by Bell in his *Photophone*, still Parolette seemed to be on the right track.

In opening his subject, Parolette states that the object of his inquiry was the relation which subsists between the action of light and the vibrations of sonorous bodies, and he actually made an instrument for measuring the effect of light on sound-vibrations, and called it the *Phonometer*.

Parolette's experiments were rude compared with those of more recent date, but it most be remembered that they were made seventy years ago. He used no mirrors for concentrating a beam of light, but relied merely on the natural properties of light without such aids. He says, "As it is known that the vibrations of elastic fluids are always analogous to those of the particles of the sounding body, and that if two strings, belonging to two instruments, be in unison, when one is touched the other will vibrate and emit a perceptible sound; I availed myself of these properties in the construction of my apparatus, and in determining the object of my inquiry.

The *Phonometer* consisted of two violins placed on a horizontal plank ten feet long and eight inches wide. Having tuned these instruments to the Paris diapason, he fixed a piece of paper to the second string of one of them to serve as an index during the course of his experiment—one violin being fixed and the other moving in a grooved sliding rest. The second string was then vibrated in a uniform manner, which produced an oscillatory motion, which was heard on the corresponding string of the other violin. The paper on the string showed the vibration at a distance, and the violins were separated from each other until the agitation of the paper ceased. This point was marked as the limit of the vibrations and marked 100, the intermediate portion being marked off to represent the one thousandth part of the distance.

Experiments made at noon with this instrument, and often repeated, indicated the same distance within a few thousandths. The whole extent of the scale was seven feet, and this distance was the limit of the greatest propagation of sound under the influence of light in the apparatus. Parolette further states that experiments in darkness gave, as a result, a mean temperature of 0.98, and that the mean difference of this propogation at noon and midnight was two degrees on the scale. In conclusion, Parolette tries to explain the results arrived at by stating that during the day, the atmosphere is more nearly saturated with oxygen than in the night, but he says it remains to be proved that this excess is sufficient to cause such a difference in the propagation of sound during the two periods and adds, "rather, may not light be the true cause of this increased propagation in oxygen and nitrous gas ; as it is known that the former has a great capacity for light, and the latter cannot be formed without its presence." As the velocity of light is 900,000 times greater than that of sound, it does not appear unreasonable to explain, in this way, its effects on the vibrations which proceed from sonorous bodies. J. M.

THE NATIONAL ACADEMY OF SCIENCES.

As the meeting held on the 16th of November last, and those of the three following days, were devoted to the reading of scientific papers only, little executive business was transacted and no new members were elected.

At the meeting of the Council the following deaths of members were announced :

J. Homer Lane, of Washington, in May. S. S. Haldeman, of Chickies, Pa., in September, and Count L. S. Portalès, of Cambridge, Mass., in October.

The decease of Professor Benjamin Peirce, of Harvard College, one of the original active members of the Academy, but whose connection with it had been severed, was also announced.

Resolutions, thanking the Trustees of Columbia College for providing rooms for the meeting, and to President Barnard and officers of the college and other members of the Academy in New York for liberal entertainment of its members, were adopted.

THE FOLLOWING PAPERS WERE PRESENTED :

1. On the Basin of the Gulf of Mexico.—J. E. Hilgard.
2. On the Origin of the Coral Reefs of the Yucatan and Florida Banks.—Alexander Agassiz.
3. Observations on Ice and Icebergs in the Polar Regions.—F. Schwatka.
4. On the Duration of the Arctic Winter.—F. Schwatka.
5. Mineralogical Notes.—Benjamin Silliman.
6. The Relationship of the Carboniferous Euphoberia to living and extinct Myriapods.—Samuel H. Scudder.
7. Report on the Dredging Cruise of the U. S. Steamer *Blake*, Commander Bartlett, during the Summer of 1880.—Alexander Agassiz.
8. On Some Recent Experiments in Determining the Electro Motive Force of the Brush Dynamo-electric Lamps operating by Incandescence.—Henry Morton.
9. On the Intimate Structure of certain Mineral Veins.—Benjamin Silliman.
10. On the Ellipticity of the Earth as Deduced from Pendulum Experiments.—C. S. Peirce.
11. On an Improvement in the Sprengel Air Pump.—O. N. Rood.
12. On the Thermal Balance.—S. P. Langley.
13. On the Measurment of Radiant Energy.—S. P. Langley.
14. Causes which Determine the Progressive Movements of Storms.—Elias Loomis.
15. On the Antimony Mines of Southern Utah.—J. S. Newberry.
16. On the Conglomerate Ore Deposits of the United States and Mexico.—J. S. Newberry.
17. On Photographing the Nebula in Orion.—Henry Draper.
18. On Condensers for Currents of High Potential.—George F. Barker.
19. On Sigsbee's Gravitating Trap.—Alexander Agassiz.
20. On the Deposits of Crystalline Iron Ores of Utah.—J. S. Newberry.
21. On the Origin of Anthracite.—T. Sterry Hunt.
22. On the Star-List of Abul Hassan.—C. H. F. Peters.
23. Dimensions of the Brain and Spinal Cord in some extinct Reptiles.—O. C. Marsh.
24. On the Rimravidæ.—E. D. Cope.
25. On the Miocene Canidæ.—E. D. Cope.
26. On a New General Method in Analysis.—Wolcott Gibbs.
27. Note on the Relations of the Oneonta and Montrose Sandstones with the Sandstones of the Catskill Mountains.—James Hall.

ON THE MEASUREMENT OF RADIANT ENERGY.*

By Prof. S. P. Langley.

Sir William Herschel showed that a thermometer indicated more heat beyond the darkest red of the spectrum of a prism than in the brightest part of the color ; therefore, he concluded that light and heat were essentially different things. This view has apparently been confirmed by numerous other European experiments, and has been set forth in all but the most recent text-books, where different curves are drawn to exhibit the light and the heat of the sun. Of late years many leading minds have recognized that these were only different manifestations of radiant energy. Prominent among these is Dr. John W. Draper, who asserted this principle long ago, and who has always maintained that if the heat in a pure diffraction spectrum could be accurately measured, its distribution would be found almost identical with that of light. This was an experiment, which, however, could never have been satisfactorily performed had it not been for the skill of Lewis M. Rutherfurd, Esq., of this city, who has made at his private expense the exquisitely delicate apparatus which can produce pure spectra, with a success far greater than any attained by the most skillful professional artisans of Europe.

By the use of one of these "gratings," made on Mr. Rutherford's engine by Chapman, and the employment of the thermal balance described in another paper, I succeeded in obtaining for the first time full and exact measurements of the distribution of energy in a pure spectrum, where no lens or prism had been used, and of fixing its relative amount, as determined accurately by the wave-lengths of light in all parts of the visible spectrum and in the ultra red. It remained to make some minute corrections for the selective absorbtions of the reflecting apparatus employed. The essential result, however, is of high theoretical interest ; it is, that heat and light as received from the sun are now experimentally proved, so far as such measurements can prove it, to be in essence the same thing. The old delineations of

* Read before the National Academy of Sciences, N. Y., 1880.

essentially different curves representing heat and light must be banished hereafter from text-books. The old views on this subject can no longer be maintained even by European men of science, who are prepossessed in their favor. This result, fulfilling what was almost a prophecy when made, a quarter of a century ago, by the elder Draper, and, being due largely to means which science owes to Mr. Rutherfurd, may, if obtained, be most fairly claimed as largely due to the two Americans whose names have just been cited.

ON THE INTIMATE STRUCTURE OF CERTAIN MINERAL VEINS.*

PROF. BENJAMIN SILLIMAN.

Dr. Sorby, of England, in his classical paper "On the Cavities and Fluidal Inclusions found in Certain Varieties of Quartz," made the sagacious suggestion that certain fluidal inclusions observed by him in quartz consisted of two fluids, viz., water and probably a liquified gas also. An examination has recently been made of a remarkable vein stone from a gold vein nown as "Hunter's Rest," Arizona. This vein was capped by a black uncrystalline rock resembling somewhat hornblend in a compact form. But it was seen under the microscope with polarized light to be compact tourmaline, a mineral never found associated with gold. This black rock which is common enough in connection with tin ore, is here abundantly coated with gold. But beneath this black capping at a very moderate depth, occurs the usual quartz filling of gold-bearing veins—the quartz in this vein showing free gold in brilliant points, and stains of copper green with some pyrite, galena, etc. This quartz seen in thin section under a high power, showed a multitude of fluidal cavities, and among them were some which under a high power (⅛ to 1-15-inch) showed distinctly two fluids, one of which existed as an inner bubble, and which displayed almost constant activity of motion. This second liquid was liquified carbonic acid. Thin sections of the veinstone were placed upon a slide for examination. When warmed, the carbonic acid expanded and the motion ceased, bnt when permitted to become cold, it became as active as before. Quartz with gold found in Southern California near the Nevada line, is entirely destitute of sulphurets, showing that the intervention of iron salts as a solvent agent was not necessary in the formation of the deposits of gold.

THE TURQUOISE OF NEW MEXICO.*

PROF. BENJAMIN SILLIMAN.

A number of domestic articles have recently been found in excavations at Mount Chalchuitl, in Los Cerillos, about twenty-two miles southwest of the ancient town of Santa Fe. Among these are a large stone hammer of the hard hornblendic Andalusite of which the mountains of the country are largely formed; a lamp, a pottery idol, such as are manufactured to this day; a spoon made of shell; a perpect specimen of a pottery dish, and some of the bones of the Pueblos or Indian miners, who were killed in 1680 by the fall of a large section of Mt. Chalchuitl, which had been undermined by them. These articles had been covered in the caverns for 200 years when found. The rocks which form Mt. Chalchuitl—the Indian name of the turquoise—are distinguished from those of the surrounding and associated ranges of the Cerillos by their white color and decomposed appearance, closely resembling tuff and kaolin, and giving evidence of an extensive and profound alteration, due, probably, to the escape through them, at this point, of heated vapor of water and perhaps of other vapors or gases, by the action of which the original crystalline structure of the mass has been completely decomposed or metamorphosed, with the production of new chemical compounds. Among these the turquoise is the most conspicuous and important. In the seams and cavities of this yellowish-white and kaolin-like tuffaceous rock the torquoise is found in thin veinlets and little balls or concretions called "nuggets," covered on the exterior with a crust of the nearly white tuff, and showing on cross fracture the less valued varieties of the gem, more rarely offering fine sky-blue stones of higher value for ornamental purposes. It is easy to see these blue stains in every direction among these decomposed rocks, but the turquoise in masses of any commercial value is extremely rare, and many tons of the rock may be broken without finding a single stone which a jeweller or virtuoso would value as a gem.

That considerable quantities of the turquoise were obtained can hardly be questioned. The ancient Mexicans attached great value to this ornamental stone, as the Indians do to this day. The familiar tale of the gift of large and costly turquoise by Montezuma to Cortez for the Spanish crown, as narrated by Clavigero in his history of Mexico, shows the high value attached to this gem. It is not known that any other locality in America has furnished turquoise in any quantity. The origin of the turquoise of Los Cerillos in view of late observations is not doubtful. Chemically, it is a hydrous aluminum phosphate. Its blue color is due to a variable quantity of copper oxide derived from associated rocks. The Cerillos turquoise contains 3.81 per cent. of this metal, Neglecting this constituent the formula for turquoise requires: phosphoric acid, 32.6; alumina, 47.0; water, 20.5. Total, 100.1. Evidently the decomposition of the feldspar of the trachyte has furnished the alumina, while the phosphate of lime, which the microscope detects in the thin sections of the Cerillos rocks, has furnished the phosphoric acid. A little copper is diffused as a constituent also of the veins of this region, and hence the color which the metal imparts. The inspection of thin sections of turquoise by the microscope, with a high power, shows the seemingly homogeneous mass of this compact and non-crystalline mineral to consist of very minute scales, nearly colorless, and having an aggregate polarization, and showing a few particles of iron oxide. The rocks in which the turquoise occurs are seen by the aid of the microscope and polarized light in thin section to be plainly only the ruins, as it were, of crystalline trachytes showing remnants of feldspar crystals, decomposed in part into a white kaolin-like substance, with mica, slag and glassy grains, quartz, with large fluidal inclosures, looking like a secondary product. There is a considerable diversity in their looks, but they may all be classed as trachyte-tuffs, and are doubtless merely the result of the crystalline rocks of the district along the line of volcanic fissures.

ON A NEW GENERAL METHOD OF ANALYSIS.*

BY PROF. WALCOTT GIBBS.

The process consists essentially in passing a galvanic current through the solution in such a manner that a surface of metallic mercury forms the cathode, and a plate of platinum the anode. Under these circumstances the metal in the solution combines with the mercury to form an amalgam. What is new in this process is the fact that a number of metals, as for example, iron, cobalt, nickel, zinc, cadmium, tin, mercury, etc., may be *completely* removed from the solution so that the electro-negative constituent of the roll may be determined in the solution by ordinary methods, while the metal itself is found by the increase in weight of the mercury. The extent of the applications of the method and its limitations remain to be determined.

* Read before the National Academy of Sciences, N. Y., 1880.

* Read before the National Academy of Sciences, N. Y., 1880.

NOTE UPON THE RELATIONS OF THE ONEONTA AND MONTROSE SANDSTONES OF VANUXEM, AND THEIR RELATION TO THE SANDSTONES OF THE CATSKILL MOUNTAINS.*

By Prof. James Hall.

Great difficulty has been experienced, from the time of the New York Geological Survey, in reconciling the observations made upon these sandstones in their various localities. Mr. Vanuxem indicated the upper formation of the third geological district as the "Montrose sandstone, or sandstone of Oneonta," and described it as occurring in Otsego, Chenango and Broome counties, New York, and as covering the whole of the upper part of Susquehanna county in Pennsylvania. Oneonta, Gilbertsville and Mount Upton were regarded as typical localities, the latter affording remains of both animals and plants. Mr. Mather described the "Catskill Mountain series" as occupying the county of Delaware and the greater part of the counties of Sullivan, Ulster, Greene and Schoharie; but in this description he included the olive slates and shales above the Helderberg series, which have since been separated as the Hamilton and Chemung groups. In the final arrangement of the nomenclature of these rocks, the observations of Mr. Mather in Delaware and Ulster counties led to the adoption of the term Catskill sandstone, or Catskill Mountain sandstone, for the whole, including the Oneonta and Montrose sandstones of Vanuxem, under the belief that the rocks as exposed in the several localities constituted parts of, or different exposure of, a single formation. This view has been accepted in all subsequently published observations, and universally believed to be the true one.

My first observations in this part of the country, previously to 1870, were made in 1844, but at that time only for the collection of fossils. In 1863 I made a section across the formations from Schoharie to Oneonta and thence to Franklin and to the South-westward of that town, and across the country to Delhi in Delaware county, returning to Schoharie by a more Eastern route. The results proved unsatisfactory from the fact that crossing from Oneonta and approaching Franklin over red and mottled shales and sandstones with an apparently southwest dip, these were succeeded by gray and greenish shales and sandstones carrying Chemung fossils; and again, on the road to Delhi, these latter were succeeded by red rocks.

Although, in the mean time having visited Montrose and some other localities of these sandstones, it was not until 1869 and 1870 that I was able to give any special attention to the relations of these formations on the Western slope of the Catskills, in the towns of Oneonta, Guilford, Sidney Plains and the adjacent country, still finding myself quite unable to parallelize the formations as there existing, with the sandstone of the Catskills. In the latter year Mr. George B. Simpson and Dr. J. W. Hall were employed in this region, and directed to make cross sections of the country in different directions; and their observations, after having reviewed the principal localities in company with myself, gave the same result, viz.: that the extensive formation of red and greenish mottled shaly sandstones, with brownish red and gray diagonally laminated sandstone, in the localities of Oneonta and Mount Upton and other places in the same region, were succeeded by sandstones and arenaceous and argillaceous shales, carrying great numbers of marine fossils known as belonging to the Chemung group, together with some bones and teeth of fishes of a peculiar character. To the latter again succeeded red and greenish gray or brownish gray beds, which in one locality in the town of Andes had already furnished scales of *Holoptychius*, and a nearly entire specimen of that fossil fish.

Notwithstanding the clearly ascertained order of succession among the members of the higher formations of the State, I have hesitated to publish results in opposition to the conclusions of my former colleagues, believing that I might possibly have been mistaken in my interpretations of the geological structure of the country.

About the same time, I employed Mr. Andrew Sherwood to work out the geological structure of the Catskill mountain region, and in 1875, after four years of investigation, I was able to present to the American Association for the Advancement of Science, and subsequently to the Academy, a large geological map, showing the general structural features of the Catskill region. In this work upon the structural character, in regard to the anticlinal and synclinal arrangement of the strata, the question of a subdivision of the formation has not been presented; and it was only in the present year, 1880, that Mr. Sherwood was again employed to complete investigation for a final geological map. In this work it became necessary to review the section along the Schoharie creek, which had previously been left at the commencement of the red rocks; and also of the country about Oneonta, Mount Upton, Guilford, Sidney Plains and Franklin.

The result of these observations has been entirely confirmatory of the results brought out by Messrs. Simpson and Hall in 1870. In accordance with our present knowledge therefore, we are compelled to adopt the view that the red and gray rocks of Oneonta and Mount Upton, beginning at the latter locality, with shaly beds containing large numbers of a single fossil species described by Mr. Vanuxem as *Cypricardites Catskillensis* and *C. Augusta*, and supposed to be the equivalent and actual continuation of the Catskill red sandstone of Delaware county, are in fact succeeded by rocks carrying large numbers of Chemung fossils.

The fossil shell described by Mr. Vanuxem has the form and character of an *Anodonta*, and is apparently a fresh water form, and occurs in association with large numbers of fragmentary and drifted land plants. The formation consists of red marls, red and gray sandstones in alternating bands, the whole diagonally stratified, and attaining, in this region, a thickness of at least 500 feet.

The fossiliferous beds of the Chemung are found lying upon that formation between Norwich and Oneonta, and to the east of Sidney plains, and at or near Franklin, where they apparently pass beneath the great red sandstone formation of the Catskills, which is characterized by the presence of bones and scales of *Holoptychius*.

From all these facts it would appear, that some time after the Hamilton period, the open sea was cut off from this area during a long period, that dry land producing abundant vegetation with estuary and fresh water conditions ensued; and that at a later period the subsidence of the coast allowed the influx of the ocean which spread over the area westward, giving beds of shale, sandstone, etc., charged with marine fossils of the Chemung period. That again, the open sea was invaded by an elevation of the littoral line, and then followed the great accumulation of red and greenish marls, brown sandstones and conglomerates, terminating above by a heavy formation of gray sandstone, the whole forming the great mass of the Catskill mountains; and to this formation only should the name of Catskill sandstone be properly applied.

This conclusion, which is sustained by our present knowledge, suggests some very important considerations concerning the relations of the Hamilton, Portage and Chemung groups, which will be discussed at some future time, and which, when investigated under the present phase of our knowledge, may solve some existing problems regarding these formations.

* Read before the National Academy of Sciences, N. Y., 1880.

AN IMPROVED METHOD OF OPERATING THE SPRENGEL AIR-PUMP.*

By Professor Ogden N. Rood.

Professor Rood's paper gave an account of his experiments with the pump for the purpose of obtaining the highest possible vacua. He first experimented with an arrangement similar to the ordinary form of the Sprengel pump, and reduced the pressure to one three-millionth. The exhaustion went on very rapidly at first and then very slowly—slower than the increased rarefaction seemed to call for. This indicated a leakage, and it was found that this leakage amounted in one minute to one-eighty-seven millionth of an atmosphere. The form of the pump was modified to correct the leakage, and a vacuum was obtained with a pressure of one-sixty-millionth. It was impossible to get beyond that point, and it occurred to Professor Rood that the potash he used might have given out moisture. He therefore substituted sodium, and the pressure rose only to one-four-millionth. Anhydrous phosphoric acid was substituted for the sodium, and the pressure fell to one-millionth. It finally struck the experimenter that the trouble was in the gauge, and when a correction was applied to the gauge, vacua were obtained with pressures of one ninety-four-millionth and one-hundred-and-ten-millionth. Higher vacua even can be obtained.

It had recently been stated in *Nature*, said Professor Rood, that his arrangement was exhibited four years ago at the Kensington Garden, and he would not, therefore, call it new. But the best result obtained in England was one-twenty-millionth, and the best result reached by an eminent French chemist was one-seventeen-millionth. He, therefore, thought there must be at least something new in his method of using the Sprengel air-pump.

* Read before the National Academy of Sciences, N. Y., 1880.

REPORT OF THE COMMISSIONER OF EDUCATION, FOR 1878.

(*Extracts.*)

TEXT-BOOKS AND COURSES OF STUDY.

The lack of uniformity in the conditions of public education in the different States is illustrated in the report on text-books and courses of study.

Returns from 31 States present the following information:

The State board is empowered to decide these matters in California, Connecticut, Delaware, Louisiana, Nevada, and Oregon. In Kansas, Nebraska, New York, and Rhode Island, the State superintendent or commissioner has authority to recommend the text-books to be used, but their adoption and the course of study are finally decided by the school committee or district boards. In Iowa and South Carolina these matters have been decided by a commission appointed for the purpose. In Maine, authority in these matters is delegated to the town supervisor or school committee; in Maryland, to the county commissioners; in Massachusetts, to the school committee; in New Jersey, to school trustees of districts acting with the county superintendents; and in Pennsylvania, to the directors and controllers of each school district, acting with the teachers. District or local boards either solely or acting in concert with superintendents and teachers decide these matters in Michigan, Mississippi, Missouri, Ohio, and Wisconsin.

In the following States—Minnesota, New Hampshire, Tennessee, Texas, Virginia, and West Virginia—the course of study is prescribed by law, but in the application discretion is given to superintendents, local boards, teachers, &c.

In Indiana, North Carolina, and Vermont no definite provision with reference to these matters has been made.

UNIVERSITIES AND COLLEGES.

The total number of universities and colleges reported is 358, with 3,885 instructors and 57,987 pupils. In the preparatory departments of these institutions were 682 instructors and 26,266 students; in the collegiate departments, 3,203 instructors and 30,368 students: unclassified, 1,353. They had 2,187,932 volumes in their libraries, and the value of their buildings, grounds, and apparatus was $36,871,213; their productive funds, $37,071,958; income from these funds, $2,548,324; receipts from tuition, $1,555,484; receipts from State appropriations, $622,577; aggregate amount of scholarship funds, $1,719,426.

Of the students in the preparatory departments, 18,481 are males and 6779 females; 6,576 are preparing for a classical course and 5,621 for a scientific course. In the collegiate departments, 15,803 (14,152 males and 1,651 females) are in classical course, and 3,893 (2,724 males and 1,169 females) are in scientific course.

The summary of college entrance examinations gives the following facts: Total number of candidates, 5,297; admitted without conditions, 2,553; conditioned in Latin, 822; in Greek, 577; in mathematics, 1,068; in history and geography, 585; rejected for deficiency in Latin, 84; in Greek, 70; in mathematics, 66; in history and geography, 22; in two or more subjects of examination, 424.

There are also statements of the numbers preparing for college, classical, and scientific courses, as follows: number preparing for classical course in academies, 6,206; in preparatory schools, 4,195; in universities and colleges, 6,576; preparing for scientific course: in academies, 2,167; in preparatory schools, 1,107; in universities and colleges, 5,621; in preparatory departments of scientific schools, 1,550; total, 27,422.

Students in institutions for superior instruction are distributed thus, viz.: in colleges, 30,368; in schools of science, 11,603; in schools for the superior instruction of women, 18,115; in all 60,086.

The Commissioner presents a brief outline of the movement in colleges to satisfy the demand that the study of science and sociology be advanced to an equality with the classics and mathematics. Without sacrificing anything of the former curriculum, temporary provision for the new studies has been made in most instances by a system of electives. The action is traced through the record of Harvard and Yale Colleges, and the views of Dr. McCosh, president of Princeton College, Dr. Peabody, of Harvard University, and Prof. B. L. Gildersleeve, of Johns Hopkins University, with reference to the most important conditions of the change, are cited.

Some have feared that in this readjustment of college courses the classics would be sacrificed, but the present tendency is toward greater thoroughness and a more extended range in classical studies; nor under the elective system is the number of students who take the modern in place of the classical course sufficiently large to create any apprehension as to the future influence of classical study.

The prevalent views on this subject are well represented in letters from Professor Hæckel of Jena and Professor Zarncke of Leipzig, which are given in full in the report.

SCHOOLS OF SCIENCE.

Of this class 76 schools, including the United States Naval and Military Academies, were reported to the Bureau. They numbered 809 instructors and 13,153 students. The comparative table for the years from 1870 to 1878, inclusive, shows this to be an increase in all particulars over the figures reported for any previous year. The increase above 1877 was in number of schools, 2; instructors, 28; students, 4,594. The number of students in preparatory departments was 1,436, viz.: 1,153 males and 283 females; the number in scientific departments was in regular course, 4,806; in partial course, 772; number of graduate students, 97. The number of volumes in general libraries was 119,164, an increase in the last school year of 3,543; the number in society libraries was 7,737. The value of grounds, buildings, and apparatus reported, was $7,587,421; productive funds, $5,020,446; income from the same, $319,503; receipts from tuition fees, $68,660; from State appropriations, $484,742.

With reference to schools of science the Commissioner observes:

"By the act of 1862 donating public lands to the several States and Territories which should provide colleges for the benefit of agriculture and the mechanic arts, the movement toward scientific training became national, the prospective institutions were sufficiently endowed for the initiatory stages, and each was free to suit its organization to the wants of its locality; the scientific schools previously established had been organized and developed in accordance with strict scientific principles, and their example afforded a powerful opposition to the influences which tended to hold the new schools to a lifeless routine of mechanical exercises on the one hand or to a feeble modification of the methods of classical colleges on the other. The reports of the year indicate that the future of these institutions as schools of applied science, conducted according to the laws of intellectual progress and directed 'to the liberal and practical education of the industrial classes' is assured, and that in the main the character of each school is to be determined by the material condition of the section in which it is placed. Thus, in the East, the tendency is to the training of engineers and scientific experts; in the great agricultural section of the West and South, agriculture and horticulture receive most attention; while in the mineral region of the Pacific section mining and metallurgy are made prominent; but even where these special tendencies are marked, other branches of scientific and industrial instruction have received attention proportionate to the demand."

Interesting facts are presented illustrating the practical advantage of these institutions to our industrial progress. The Commissioner adds that there has been marked advance in the general organization of these schools and in their preparation for efficient work in science and mechanics.

SCHOOLS OF MEDICINE.

The number of schools of medicine, dentistry, and pharmacy reported to the Bureau during the year was 106. These had 1,337 instructors and 11,830 students. The regular school of medicine and surgery reported 64 institutions, 915 instructors, 8,279 students, 2,506 graduates, 46,065 volumes in libraries, $1,685,250 in grounds, buildings, and apparatus, $214,347 of productive funds, yielding an income of $13,186, and tuition receipts to the amount of $289,398. The eclectics reported 6 institutions, 51 instructors, 448 students, 211 graduates, 3,000 volumes in libraries, $161,000 in grounds, buildings, and apparatus, and $8,960 receipts from tuition. The homœopathists reported 11 schools, 158 instructors, 1,215 students, 363 graduates, 39,800 volumes in libraries, $349,000 in grounds, buildings, and apparatus, and $95,471 receipts from tuition fees.

The dental schools report as follows: number, 12; instructors, 161; students, 701; graduates, 218; volumes in libraries, 505; value of grounds, buildings, and apparatus, $68,000; receipts from tuition fees, $60,734.

The pharmaceutical schools number 13; instructors, 52; students, 1,187; graduates, 380; volumes in libraries, 5,175; value of grounds, buildings and apparatus, $155,000; receipts from tuition fees, $25,487.

COLLEGIATE AND PROFESSIONAL DEGREES.

"This Office," says the Commissioner, "is informed that the better colleges and universities of the country are becoming increasingly careful in the bestowal of honorary degrees. At the same time it is well known that the sale of diplomas by persons who have obtained control of collegiate and university charters by purchase or fraud is still going on. This disgraceful proceeding has already injured the reputation of American learning and the value of American degrees in other countries; but the Federal Government did not create the corporations which are causing this scandal and has no power to cancel their charters. It is for the authorities of the State to move in the matter and thus vindicate the honor of the nation and of American scholars."

The following summary of degrees in course and honorary conferred by reputable institutions of learning needs no further explanation:

The number of degrees of all classes conferred was, in course, 9,999, honorary, 396, divided as follows: letters, in course, 3,631, honorary, 114; science, in course, 990, honorary, 6; philosophy, in course, 222, honorary, 31; art, in course, 46; theology, in course, 222, honorary, 159; medicine, in course, 3,814, honorary, 4; law, in course, 1,000, honorary, 78. Of these degrees, classical and scientific colleges conferred 6,367 in course and 388 honorary; colleges for women, 674 in course and 1 honorary; professional schools, 2,958 in course and 7 honorary.

EDUCATIONAL BENEFACTIONS.

The total amount of educational benefactions is $3,103,-289, which is distributed as follows: universities, and colleges, $1,389,633; schools of science, $49,280; schools of theology, $397,852; schools of law, $100,000; schools of medicine, $18,562; institutions for the superior instruction of women, $241,820; preparatory schools, $97,191; institutions for secondary instruction, $759,817; institutions for the deaf and dumb, $49,134.

EDUCATIONAL BENEFACTIONS.

During the year 1878 the sum of $3,103,298 was presented to various educational establishments in the United States by private individuals.

Of this sum $1,389,633 were placed at the disposal of universities and colleges. We regret to find that while Theology received nearly $400,000, but $49,280 were devoted to Science, and $18,562 to Medicine. Schools of Law received $100,000. The deaf and dumb received about the same amount as Science.

The University of California received $125,000, $25,000 to build a library building, and $50,000 to purchase books. This amount did not include a collection of works of art and a library valued at $50,000.

Yale College received $189,590. Boston University $30,-000 towards the purchase of the Shepard Collection of minerals. From various sources Harvard University received $177,207; Dartmouth College, $35,000: Cornell University, $27,663; Union College, N. Y., $84,000; Oberlin College, O., $25,000; University of Virginia, $50,000 to endow School of Geology and Natural History; Wellesley College, $155,000; Thayer Academy, Mass., $417,000; Deerfield Academy, Mass., $88,000; Dean Academy, $38,000.

PALÆONTOLOGY.

THE DEVONIAN INSECTS OF NEW BRUNSWICK.

In a memoir, on the Insects in the Devonian of New Brunswick, Mr. S. H. Scudder draws the following conclusions in regard to the earliest known insects:

"It only remains to sum up the results of this re-examination of the Devonian Insects, and especially to discuss their relation to later or now existing types. This may best be done by a separate consideration of the following points:

"There is nothing in the structure of these earliest known insects to interfere with a former conclusion that the general type of wing structure has remained unaltered from the earliest times. Three of these six insects (Gerephemera, Homothetus, Xenoneura) have been shown to possess a very peculiar neuration, dissimilar from both Carboniferous and modern types. As will also be shown under the tenth head, the dissimilarity of structure of all the Devonian Insects is much greater than would be anticipated; yet all the features of neuration can be brought into perfect harmony with the system laid down by Heer.

"The earliest insects were Hexapods, and as far as the record goes, preceded in time both Arachnids and Myriapods.

"They were all lower Heterometabola.

"They are all allied or belong to the Neuroptera, using the word in its widest sense.

"Nearly all are synthetic types of comparatively narrow range.

"Nearly all bear marks of affinity to the Carboniferous Palæodictyoptera, either in the reticulated surface of the wing, its longitudinal neuration, or both.

"On the other hand they are often of more and not less complicated structure than most Palæodictyoptera.

"With the exception of the general statement under the fifth head they bear little special relation to Carboniferous forms, having a distinct facies of their own.

"The Devonian Insects were of great size, had membran ous wings and were probably aquatic in early life. The last statement is simply inferred from the fact that all the modern types most nearly allied to them are now aquatic.

"Some of the Devonian Insects are plainly precursors of existing forms, while others seem to have left no trace. The best examples of the former are Platephemera, an aberrant form of an existing family; and Homothetus which, while totally different in the combination of its characters from anything known among living or fossil insects, is the only Palæozoic insect possessing that peculiar arrangement of veins found at the base of the wings in Odonata typified by the arculus, a structure previously known only as early as the Jurassic. Examples of the latter are Gerephemera, which has a multiplicity of simple parallel veins next the costal margin of the wing, such as no other insect ancient or modern is known to possess; and Xenoneura, were the relationship of the internomedian branches to each other and to the rest of the wing is altogether abnormal.

"If, too, the concentric ridges, formerly interpreted by me as possibly representing a stridulating organ, should eventually be proved an actual part of the wing, we should have here a structure which has never since been repeated even in any modified form.

"They show a remarkable variety of structure, indicating an abundance of insect life at that epoch.

"The Devonian Insects also differ remarkably from all other known types, ancient or modern; and some of them appear to be even more complicated than their nearest living allies.

"We appear, therefore, to be no nearer the beginning of things in the Devonian epoch than in the Carboniferous, so far as either greater unity or simplicity of structure is concerned; and these earlier forms cannot be used to any better advantage than the Carboniferous types in support of any special theory of the origin of insects.

"Finally, while there are some forms which, to some degree, bear out expectations based on the general derivative hypothesis of structural development, there are quite as many which are altogether unexpected, and cannot be explained by that theory without involving suppositions for which no facts can at present be adduced."

MICROSCOPY.

Mr. W. H. Bullock, of Chicago, the maker of the microscope for lithological work described by us in Vol.. I. No. 21 of SCIENCE, writes to us, objecting to an editorial remark, that the arrangement of the polariscope for instant use, claimed as a novelty by Mr. Bullock, had been used in the same position by Swift, of London, for many years.

Mr. Bullock admits the accuracy of this statement, but now sends details, as evidence, that he has shown considerable ingenuity in arranging his analyzing prism, "mounting it in such a manner, that it can be turned round 90 degrees, so that when the lower prism is at the spring stop or zero point, and the upper prism is pushed into position with the indicator forward, the prisms are parallel, and upon its being turned back or revolved 90 degrees the prisms are crossed." "The lower prism is also arranged differently to that used by Swift; it can be fitted either to the sub-stage or used in the supplementary sub-stage, and thus used close under the stage, so that no light can reach the object under observation, except that which passes through the lower prism." Mr. Bullock also notices other improvements which must render the instrument very perfect for the purposes for which it was designed, namely, lithological work.

Mr. Bullock sends a photograph of this microscope and we readily admit that it appears to be an excellent instrument; of the workmanship we are, of course, unable to speak, but probably the reputation of Mr. Bullock is sufficient guarantee in this respect.

NEW YORK ACADEMY OF SCIENCES.—Section of Chemistry.—Monday Evening, December 13, 1880, at 8 o'clock, the following paper, by Dr. HENRY A. MOTT, is announced:— Chemical Decomposition incited by a Cold Fluid Stratum floating on a Warm Liquid,

ASTRONOMY.

JUPITER.

MOTION OF SPOTS ON HIS SURFACE.

Jupiter, always enigmatical, has, since the appearance of the great red spot in his Southern hemisphere, become more and more perplexing. It was supposed this object would afford a ready means of determining Jupiter's true period of rotation. It has not done this, but has certainly led to the development of many interesting facts, one of which is that no period can be determined, because there are not two parts of the planet's visible surface which rotate in equal times. It would seem reasonable that any two points on the same parallel of latitude and in the same hemisphere must necessarily rotate with equal velocities; this does not even hold good. Could we be placed in such a position that the rotation of the planet would not visibly change the position of objects on his surface, we should still see the spots moving not only with different velocities, but in contrary directions. Spots very rarely change their latitude, as the very great axial rotation of Jupiter confines their motion to a parallel with his equator. In Jupiter's Southern hemisphere are two or three small dusky oblong spots. The most distinct of these I first observed on the morning of July 25, 1880, (see *English Mechanic*, No. 804, where an engraving showing its position is given). This group of small spots lies on a parallel of latitude about even with the Southern edge of the great red spot. On July 25, the centre of the first observed of the spots preceded the centre of the large spot by 1h. 35m. Since that date the red spot has been observed constantly, and the small one frequently. Up to November 23, thirty-five transits of the great spot across the central meridian, and nine of the smaller have been carefully observed. On November 22, the small spot preceded the greater by 3h. 17m. The interval between their transits having increased 1h. 42m. since July 25. The large spot has moved backward, compared with the direction of rotation, making its transit on November 22 occur 49m. later than on July 25, while the small spot came to its transit 53m. earlier than on July 25, showing that the two are moving with nearly the same velocity, but in opposite directions. The mean daily drift backward of the great spot since July 25 has been 0.40245m, while the forward motion of the small spot has been, during the same period, 0.43948m per day. It will be seen from this that a rotation derived from the small spot would indicate a quicker period than that derived from the large red spot.

From the observations of July 25 and Nov. 22, the great spot rotates in 9h. 55m. 37.065s., and the small one in 9h. 55 m.16.176s. The mean rotation of the two is 9h. 55m. 26.621s. A reduction of all the observations on hand will, doubtless, slightly change these figures. It would be well for observers to watch this small spot, as it may last as long as the large one. If it should continue permanent, it will eventually make the circuit of Jupiter and meet the red spot; this would occur about the middle of February, 1882.

But the motion of these two objects is very slow compared with the rapidly moving black spots which appeared just north of the equatorial belt on the last of October. But as attention has already been called to these remarkable objects by Messrs. Dennett, Williams and Denning, in *English Mechanic*, No. 816, I will not refer to them here, further than to say that they have been observed and sketched as often as the weather would permit since their first appearance. The region occupied by the great equatorial belt is subject to constant and quite rapid change, being filled at times with the most delicately soft plumey forms. Brilliant white spots are not unfrequent in this zone. These bright spots generally appear as intensely white heads, followed by a light, diffused and fainter train. Sometimes this train is composed of light, tufty balls, resembling cumulous clouds. These white heads are invariably bent or turned, as if slightly doubled

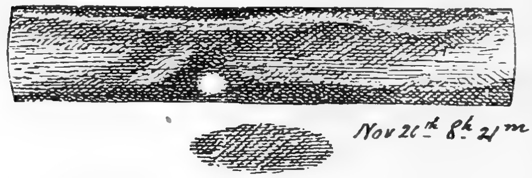

FIG. 1.

under, toward the south, and are generally partially or wholly imbedded in the inner edge of the south portion of the equatorial band. These heads soon become isolated into a regular white spot, the train gradually fading out. All the objects in the equatorial zone move with a very great velocity in the direction of rotation, invariably n a contrary direction to that pursued by the slowly moving red spot, which is really the only object that has a backward motion on the planet. Indeed it would not be a bad comparison were we to compare the red spot to a mighty city built on the shore of a vast and swiftly flowing river, which is constantly being filled with drift, and an occasional glistening mass of ice, tearing its way past the city with a velocity of not less than six thousand

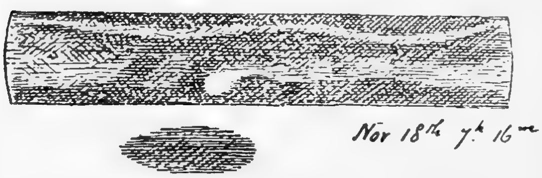

FIG. 2.

miles a day. In such a comparison the city would need be as great in area as three-fourths that of our entire earth, and the river fully sixteen thousand miles in breadth!

One of these swiftly moving bright spots was observed on Nov. 18th (Fig. 1). It had probably existed some few days before that date, but bad weather had prevented observations of the planet.

As it passed very close to the red spot that object afforded a capital means of illustrating its motion.

On the 18th it was situated on a meridian with a part of the red spot about ⅓ its length preceding the following end.

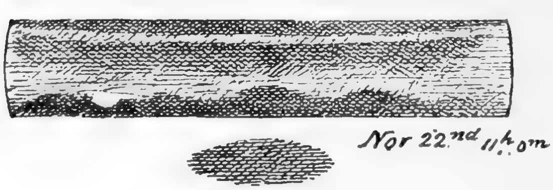

FIG. 3.

This spot was bright with the fainter train following in its wake. On the 20th it was near the preceding end of the red spot (Fig. 2), and had isolated itself more from its train, being partially imbedded in the inner edge of the south band. On the 22d it had left the red spot far behind (Fig. 3), and was smaller and paler, apparently the size of satellite I, then nearing transit. By the 23d it had advanced still further (Fig. 4), and was nearing the west limb when the red spot was central in transit. It was smaller and appeared to vary in brightness.

Bad weather since the 23d has prevented any further observations of this remarkable object.

The pen and ink drawings show the rapid progress of the spot The first sketch was made when the red spot's following end was in transit; the three others when the

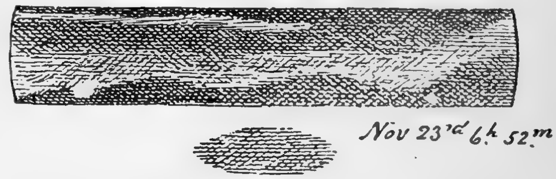

FIG. 4.

spot was central. To save space the sketches only show the great equatorial band and the red spot.

E. E. BARNARD.

Nashville, Tenn., Nov. 29.

THE NOVEMBER LEONIDS, 1880.

BY EDWIN F. SAWYER.

In the years 1846–47 and 1849, at the November 11–15 epoch, meteors were recorded in considerable numbers, doubtless representing the perihelion passage of a minor cluster of meteors in the cometary-meteor orbit. Last year, both in Europe and America, these meteors were found to be unusually numerous from the 11th to the 15th of November, and the earth probably encountered the minor cluster of 1846 at its return to perihelion. In anticipation that the shower would, this year, at the nodal passage, be of some little intensity, preparations were made for observing the same, but owing to cloudy weather observations could only be obtained on the 11th and 12th; but the indications, at these early dates, were that a large number of shooting stars would be recorded on the 13–14th, and as observed elsewhere such proved to be the case. At Cambridgeport on the 11th, during a two hours watch, from 14h. 30m. to 16h. 30m., 14 meteors were recorded, of which 6 were Leonids. On the 12th, during an hours watch only, from 16¼h. to 17¼h., in a sky more than half overcast, 6 others were noted, equal to at least 15 Leonids per hour for one observer in a clear sky. At the Haverford College Observatory, Penn., Mr. Isaac Sharples, assisted by three other observers, recorded 52 meteors in about an hours watch on the 13th from 3h. 30m. to 4h. 20m., of which 28 were Leonids. Mr. Sharples says, that at the end of the watch, when the sky become overcast, meteors were falling at the rate of two a minute and promised much.

From W. F. Dunning, Esq., F. R. A. S., we learn that the weather was generally unfavorable for observing purposes in England at the November epoch, so that the observations as recorded in this country have a special value, being, so far as heard from, the only ones obtained during the dates on which the Leonid shower is in play. As in the year 1849, meteors were also numerous at this epoch, we may expect a return of the Leonids as a minor shower during the next two years.

Cambridgeport, Dec. 5, 1880.

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 WHITE SPOT ON JUPITER.

To the Editor of SCIENCE:

The white spot seen passing the great red spot on November 18, 20, etc., and situated on the inner edge of the south equatorial band, was observed again on December 2, the first night for observing since November 23. The white spot was in mid-transit some time before the red spot had begun to appear at the east limb. It has kept up its rapid motion with probably no particular change in appearance.

From the observed transits of November 22 and December 2, its rotation period is 9h. 50m. 19.4s. It gains 5 m. 18 s. on the red spot at each rotation, or 12 m. 48 s. per day. Should this spot continue permanent for one month longer it will have made the circuit of Jupiter and again be seen passing the great red spot on the night of January 4. On that night the white and red spots will transit together at 11 h. 39 m.

E. E. BARNARD.

NASHVILLE, TENN., *December* 6.

To the Editor of SCIENCE:

Last night I viewed Swift's Comet, and found that the Ephemeris computed by Mr. Upton, of the Naval Observatory, Washington, answered quite closely, and was from note book as follows:

SWIFT'S COMET.

Northfield, Minn., Mean Time, 10h. 52m.
R. A. 3h. 32m.
Dec. 51° 28′.

The Comet was well seen, having about the brightness of a seventh magnitude star. Our clock refractor, of aperture of 8¼ inches, was used with power of 50.

Latitude of Observatory, 44° 27′ 40.77″. Longitude from Washington, 1h. 4m. 23.02s., which has been recently determined telegraphically by aid officers of the Coast Survey. Respectfully yours,

WM. W. PAYNE.

NORTHFIELD, MINN., Dec. 2, 1880.

ASTRONOMICAL MEMORANDA.—(Approximately computed for Washington, D. C., December 13, 1880):

	H.	M.	S.
Sidereal time of Mean Noon	17	31	2
Equation of time		5	14

mean noon *following* apparent time.

The *Moon's* phases for the month are:

	D.	H.	M.		D.	H.	M.
New Moon	1	9	48	First Quarter	8	1	30
Full Moon	15	22	28	Last Quarter	24	1	49
New Moon	30	20	48				

We have the somewhat unusual occurrence of two new moons in the month, and both of them cause partial eclipses of the sun. The eclipse of the first was invisible in the United States; that on the morning of the thirty-first is partially visible. The sun rises eclipsed and remains so until a little after nine o'clock, nearly three-quarters of its disk being covered at the time of greatest obscuration. In addition to these solar eclipses there will be a total eclipse of the moon December 15–16, invisible in the United States, but visible in Central Asia.

Mercury may be seen during the week rising about an hour before the sun, and 5° farther north.

Venus now crosses the meridian nearly three hours after the sun, and is steadily growing brighter as she approaches the earth.

Mars has reached a sufficiently great distance from the sun to be readily seen about an hour before sunrise close to the eastern horizon. It is 2° farther north than the sun, and is easily recognized by its red color. Mars and Mercury are in conjunction on the 23d.

Jupiter and *Saturn* have changed but little their relative positions, Saturn crossing the meridian at 7h. 53m., a few minutes after Jupiter. The brilliant markings upon Jupiter's belt have been attracting universal attention.

Uranus, crossing the meridian about 5 o'clock in the morning, is in R. A. 11h. 1m. 26s. decl. + 7° 6.2′.

Neptune rises about midnight and reaches the meridian at 9 o'clock, at an altitude of 48°.

The Great Nebula in Orion situated around the small quadruple star θ Orionis (the central one of three stars which form Orion's sword-hilt) will be found of great interest to all possessing good telescopes. It rises at 6 P. M., and is just visible as a nebulous mass to the naked eye.

In a communication to the Colorado Academy of Science, Prof. George Davidson, of the U. S. Coast and Geodetic Survey, has placed upon record the somewhat unusual occurrence of a naked eye observation of one of Jupiter's satellites. The station was Monticello, overlooking the Sacramento Valley, 3,125 feet above the sea level. Jupiter, at an elevation of 8°, was slowly rising through a smoky atmosphere, without the least radiation. The third satellite was noticed first by Prof. Davidson, below the disk and somewhat to the left, and was readily seen by four other persons, when attention was called to the phenomenon. Its position was afterwards confirmed by the aid of a field-glass. The satellite remained visible for about twenty minutes, and was finally rendered invisible by the moonlight. On subsequent nights with much clearer sky and no moon, no satellites could be made out with certainty by the unassisted eye.

There is a very ingenius instrument in use at the Greenwich Observatory to record automatically the duration of sunshine through the day. It consists of a glass globe hung within a hemispherical cup of slightly greater diameter. This cup is lined with a strip of paper covered with stencil ink. While the sun is shining, the globe, which is entirely exposed upon the roof, acts as a burning glass, and causes a continuous line to be made upon the paper. This line will be broken, however, as often as the sun's light is obscured by clouds, and thus a determination of the amount of sunshine for the day will be obtained.

M. Martin is engaged in polishing the object glass of the large refracting telescope now building at the Paris Observatory. The diameter of this exceptional lens is 73 centimetres, and its weight 200 kilograms. The quality of the glass having proved defective, it has already broken twice, and the operation is now being made on the third casting. –*Nature*.

We learn from *Nature* that Prof. Bell, together with M. Janssen has been making some experiments at Meudon, upon the application of the photophone to the study of sounds which occur on the sun's surface. "A solar image 0.65m. in diameter" was explored with the selenium cylinder, but no very marked results were obtained.

Schmidt calls the attention of observers to a sharp black spot in the northern part of Jupiter's belt, which gives a time of rotation=9h. 55m., while the heavy white clouds in the middle of the belt give 9h. 50m.

W. C. W.

WASHINGTON, D. C., December 8, 1880.

A new optical milk test has been invented by Messrs. Mittelstrap, Magdeburg. A given quantity of milk, and also of water is examined by looking through different thicknesses until opacity is reached. The vessel holding the liquid has a glass bottom, and in its cover a vertical graduated tube in a slide, with glass closing its lower end. Light is thrown up from below by means of an oblique mirror, or from a direct source. The tube (through which one looks) is moved in the slide until the light disappears, and at this point the scale is read off. Professor Maercker has made experiments with this apparatus, and states it to be very accurate; the greatest difference between the determination of fat in milk, with it, and by chemical analysis, being an average of 0.1 per cent. The usefulness of the instrument applies only to fresh milk, and for skim-milk a special tube is prepared.

BOOKS RECEIVED.

A General Description of the State of Indiana, extracted from the First Annual Report of the Bureau of Statistics and Geology for 1879, re-published by authority of his excellency, James D. Williams, Governor.

This is a small pamphlet of 16 pages, containing information of an industrial rather than of scientific character. A map of Indiana is given, the typographical imperfections of which render it a useless addition.

The Scientific English Reader. Englisches Naturwissenschafltich-Technisches Lesebuch für höhere technische lehranstalten und zum selfstudium für studirende, lehrer, techniker, industrielle. Mit sprachlichen und sachlichen erläuterungen. Von Dr. F. J. Wershoven: I. Theil—Physik, Chemie, Chemische Technologie, by F. A. Brockhaus, Leipzig, 1881.

This work is intended to place before the German student specimens of the best literary productions of English scientists. The present volume, treating of Physics and Chemistry, gives selections from the works of Lardner, Maxwell, Roscoe, Lockyer, Wilson, Smiles, Grover, Ure, and others who have treated on technical subjects within range of the present work.

To aid those who desire to make translations from this book to the German language, an appendix of German equivalents of English technical words has been given at the end of the work.

Dr. Wershoven's work will also be useful to the English student, "who desires readings in Science." The selections are made with good judgment, and they will be read with profit by those who desire a general idea of English scientific literature, carried well up to date.

The Student.—A Monthly Journal devoted to the interests of Education.—Haverford College, Montgomery County, Pa., $1 per annum, 10 cents single number.

The number of periodicals devoted to education is increasing rapidly. "The Student," published by Haverford College, and edited by Professor Isaac Sharpless and Professor Watson W. Dewees, appears to advocate a return to what the editors term old-fashioned studies—classics and mathematics—believing they have made many a sturdy man in the past, and that their influence is as potent for the future as ever. A strictly practical education, meaning such an one as can be directly used in business, the editors consider extremely limited and fruitless of disciplinary value.

If Professor Sharpless has no faith in a "practical education," he appears to believe in making "The Student" a practical educational journal, and we are agreeably surprised to find the subject handled in such an attractive manner.

Griffen's Chart of Animal Classification—adapted to Steele's Zoology. By A. B. Griffen, 641 Broad street, Newark, N. J. Price, 15 cents.

This Chart shows, in an admirable manner, the relations of the various divisions of the Animal Kingdom. The six great sub-kingdoms, Vertebrata. Articulata, Mollusca, Echinodermata, Coelenterata, are represented as the trunks of as many "Zoological trees," whose branches and twigs are the Classes, Orders, Families, etc. It is of quarto size, and so arranged that it may be folded conveniently and without injury. As a systematic synopsis for convenience of reference we heartily recommend it to the students of Zoology

CHEMICAL NOTES.

Characteristic Distinctions between Human Blood and that of other Animals.—Dr. Vincenzo Peset y Cervera has found that on mixing the blood of different animals with a little bile there are formed in the mass, crystals not exceeding 0.003 metre in size. These crystals may be distinguished thus:—Those of man are right rectangular prisms; those of the horse, cubes; of the ox, rhombohedrons; of the sheep, rhombohedric tablets; those of the dog, rectangular prisms; those of the rabbit, tetrahedrons; of the squirrel, hexagonal tables; of the mouse, octahedrons; of common poultry, cubes modified at their angles, &c.

On some Causes which Hinder or Facilitate the Precipitation of Manganese Hydrate by Ammonia.—Giulio Puliti finds that the precipitation of manganese from its solution by means of ammonia may be partially or totally hindered by sal-ammoniac. Heat renders the sal-ammoniac more efficacious. In hot liquids the precipitation of manganese may be completely prevented if the metal meets with this reagent in the proportion of 1 : 150. He also finds that iron, aluminium, and chromium facilitate the precipitation of manganese.

Behavior of Carbonic Acid with Nessler's Reagent and Ammonia.—A solution of acid ammonium carbonate or a dilute solution of sal-ammoniac mixed with water containing carbonic acid or with sodium bicarbonate, if mixed drop by drop with Nessler's reagent gives a yellow precipitate, which disappears on agitation without imparting the slightest coloration to the liquid. Not until the free carbonic acid has been saturated by the addition of caustic potassa or of an excess of the reagent, is a permanent yellow coloration produced.—Th. Salzer. *Bul. de la Soc. Chim.*

Perforation of Zinc Cisterns and Corrosion of Lead Pipes by Water.—X. Rocques has observed that the plates of zinc cisterns are corroded, not uniformly, but in certain well-defined places. The cause of this inequality is the electric current, which is set up between the purer portions of the metal and those more alloyed. Zinc, lead, and copper are attacked very slowly by ordinary water and by saline solutions in general (chlorides, bicarbonates). The corrosion is more rapid if there are several metals in contact. The presence of nitrogenous matters and ammonia accelerates the action, especially in case of zinc. The phenomena display their greatest activity in presence of oxygen. This is the case at the surface where the metal is alternately in contact with water and air. The deposits formed are chiefly silicates and carbonates of lead, zinc and copper.

Detection oe Picric Acid in Beer.—Dr. H. Fleck evaporates 500 c.c. of the beer to a syrup, mixes with ten times its volume of absolute alcohol, filters off the precipitate, washing it as well as possible, and evaporating the alcoholic filtrate to dryness. The residue is extracted with water at the boiling point as often as the liquid becomes colored, evaporates to dryness, and extracts the residue with ether. The ethereal extract contains the picric acid almost pure.

Determination of Theine in Aea.—Fifteen grms. tea are repeatedly extracted with boiling water till completely exhausted; the liquid is filtered, evaporated to the consistence of an extract, mixed with 2 grms. calcined magnesia and 5 grms. powdered glass and completely dried.

Use of Bromine in the Analysis of Sulphides.—Bromine oxidizes sulphur and sulphides very rapidly. Iron pyrites require to be very finely pulverized and a prolonged action is required. Copper pyrites are dissolved very rapidly if an excess of bromine is used, which is easily expelled by a gentle heat. The sample is placed in a small flask, covered with a little water, and the bromine is added. A gentle heat is sometimes necessary towards the end. One part of sulphur requires about 15 parts of bromine. Bromine water is especially adapted for destroying sulphuretted hydrogen and dissolving recently precipitated sulphides.—E. Reichardt.

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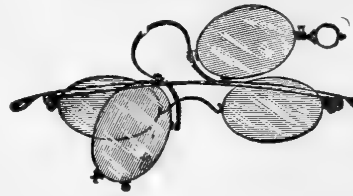

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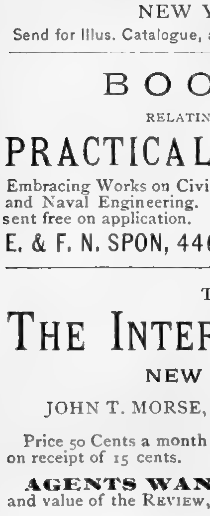

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SCIENCE:

A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.

Vol. I, No. 24. - - - - - - - December 11, 1880.

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SCIENCE

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

ILLUSTRATED.

Entered in the Office of the Librarian of Congress, at Washington, D. C.

Vol. I—No. 25. December 18, 1880. Price 10 Cents.

ON THE SUPERFICIAL VISCIDITY OF LIQUIDS.

Translated for "SCIENCE" from the French of J. Plateau,
By THE MARCHIONESS CLARA LANZA.

In the year 1638 Descartes affirmed that the surface of water presented a resisting tendency as though it were covered with a thin pelicle. Several other learned men have asserted the same fact and sought to verify it by various experiments. Some limited their researches to water alone; others maintained that the tests were applicable to all liquids. In my own observations I have described experiments and facts which, in my opinion, at once remove all doubt as to the reality of a peculiar resisting force manifested by the surface of water, solutions, etc., and I have attributed this resistance to a characteristic viscidity or glutinous matter pertaining exclusively to the outer coating of these liquids. Nevertheless, I have demonstrated that certain other fluids are totally exempt from this peculiarity, and I hope I have fully established the fact, heretofore, that in several among them, such as alcohol, spirits of turpentine, sulphuric ether, etc., the invisible particles of the outer layer offer, on the contrary, less resistance in regard to relative displacement than those within the mass.

Permit me to recall the facts of which my principal experiments consist. A magnetized needle is placed upon a pivot in the centre of a glass cylinder; the liquid to be tested is then poured into the vessel—just enough to come in contact with the needle; the latter is then turned to a meridian of about 90°, then in a few moments is left to itself, and the time which it takes to travel over a determined angle must be correctly ascertained. In my experiments the angle was 85°. More liquid must then be introduced into the cylinder so that the needle is completely covered, the liquid rising at least two-thirds of an inch above it. The needle is then again turned to a meridian of 90°, and one must remark the time taken to describe the preceding angle. Now, for example, when the liquid is distilled water, my needle took precisely twice as long when upon the surface as when beneath it, notwithstanding that in the first case the under surface of the needle alone came in contact with the water, while in the latter it was completely immersed. When the liquid employed was alcohol or turpentine the time required by the needle, when upon the surface, was less by half than when beneath it.

I must add that in those liquids on whose surface the magnet moves more slowly than when beneath it, the entire outer coating moves also, although somewhat less rapidly.

Two liquids, one a solution of albumen and the other of a saponaceous consistency, have exhibited superficial viscidity in an extremely forcible manner. After having moved with the utmost slowness, the needle stopped at an angle of 35° on the surface of the former. It did not move at all upon the latter liquid.

I omit purposely the various details relative to those experiments, as well as other facts belonging to the subject in question. I shall mention them further on as the reasons present themselves, and at this time merely confine myself to the special object of these remarks, that is to say, the cause and nature of these phenomena.

In a notice published in 1870, M. Luvini expressed doubt in regard to the superficial viscidity of liquid matter[1]. He presumes that the effects I myself have observed are due to some alteration in the outer surface caused by the contact of the liquid with the air, or else by particles of dust floating about.

In 1872 M. Marangoni published a paper,[2] in which he seeks to prove that the viscidity upon the outer portion of the liquid is identical with that which is beneath the surface. According to him, in such liquids as water, for instance, which does not produce bubbles, the resistance is increased by a capillary action exercised by the glass upon the needle; while, when liquids which bubble easily are used, the resistance springs from a thin cuticle in coating of a nature peculiar to the liquid itself.

I replied to both these articles[3]; but M. Marangoni attacked me again last year[4]. In his second work he substitutes, for some unknown reason, *particles of dirt* for the word cuticle. When speaking of the saponaceous solution, he states that the carbonic acid in the air decomposes the soap and produces an alkaline carbonate which removes the fatty acids and forms a kind of emulsion upon the surface. As to the solutions of albumen he thinks probably that the coating of dust is produced by the evaporation of the water.

He does not positively deny that the surface of liquids cannot possess a viscidity of its own apart from that which is in the mass; but he is persuaded that the influence of the viscidity peculiar to the surface is very small indeed when compared with that which effects the final results. The following is the substance of his theory:

We all know that if we place upon any liquid a drop of another possessing less external elasticity, the drop will spread itself in a thin coating upon the surface of the former. Consequently, when a liquid is covered with a layer of dirt, we may reasonably admit that this layer possesses an elastic force much inferior to that which belongs to the pure, fresh surface of the underlying liquid. Now proper experiments show us, first of all, that the tension of this coating is effectively much less forcible than that of the liquid beneath; secondly, that if the coating becomes sufficiently thick, the elasticity disappears entirely, or very nearly; thirdly, that in any saponaceous solution the film can be accumulated upon certain portions of the surface and removed upon others.

When a bubble is blown from one of these liquids the layer of dirt extends in both sides of it and thus prevents its breaking. Liquids such as alcohol, ether, turpentine, etc., cannot, owing to the slight elastic force they possess, be covered with a coating of dirt, and for this reason they are unable to produce bubbles.

The retarded movement of the needle upon the surface of the liquid does not arise from any viscidity of the outer layer, for, in the saponaceous solution at least, this coating is very movable, as the two following facts will show:

In the first place, when a large soap bubble is blown, reflecting various colors, the slightest breath of air will cause it to whirl rapidly backwards and forwards. In the second place, if a certain amount of soap-suds be put into a horizontal brass tube sufficiently large for the purpose, and a magnet be placed inside upon a pivot, directed toward the magnetic meridian, and then left to oscillate at will, you will perceive that the vibrations are very nearly as rapid as when the magnet moves freely in the air, notwithstanding the fact that it has to overcome the resistance offered by the two outer coatings of the liquid.

When a coating of dirt exists, the somewhat retarded motion of the magnet upon the surface, together with the rotating movement of the entire mass, can be explained in the following manner: The magnet itself tends to remove the dirt which is behind it and accumulate it all in front; this produces an excess of elasticity along the posterior contour, directly opposed to the natural motion, and at the same time a diminutive expansive force along the anterior contour. Furthermore, behind each half of the magnet, the superfluity of expansive force on the fresh surface draws together the edges of that portion which is already freed from dirt as though to close the rent, and, at the same time, as in each of the anterior parts, the portions far removed from the edge of the magnet possess a weaker expansive force than those nearer to it; the former attract the latter and thus determine the rotation of the entire mass.

[1] *Alcune sperienze considerazioni intorno all' adesione tra solidi e liquidi.* Turin, 1870.

[2] *Sul principio della viscosita superficiale dei liquidi stabilito dal Signor J. Plateau.*

[3] *Réponse aux objections de M. Marangoni contre le principe de la viscosité superficiale des liquides.*

[4] *Difesa della teorica dell' elasticita superficiale dei liquidi.* 1878.

If the outer layer of the liquid should resist the movement of the magnet from any viscosity of its own, it would pucker perceptibly; moreover, viscous bodies propagate motion with difficulty from any distance.

The coating of dirt imitates closely an elastic body, inasmuch as it tends to return to its primitive state when broken; however, it substitutes, in place of superficial vsicosity, *superficial elasticity*.

In regard to those liquids of strong expansive force which do not produce bubbles, such as water, the greater portion of briny solutions, etc., liquids upon whose surface a layer of dirt cannot easily be attested, the retarded movement of the magnet upon the exterior is hardly due to the changes which occur in the cavities of the meniscus, terminating the magnet at each end, partly, also, to the beginning of a layer of dirt, M. Hagen having discovered that the surface of water undergoes modifications by exposing the liquid to the air. But the principal cause may reasonably be said to be the fact which M. Van der Mengsburgghe has so well described, namely, that when the surface of any liquid is augmented, or, in other words, when any diffusion of the pure exterior takes place, a sudden cooling, followed by an increased tension, ensues, and, reciprocally, a warmth and decrease of tension correspond to any diminution or contraction of the surface.

This then is the main substance of the theory proposed by M. Marangoni in compensation for mine. Let us endeavor to examine it. First of all, it would seem, according to his doctrine, that it is merely necessary to add to any liquid of strong expansive force which does not bubble, a drop of another liquid of weak tension in order to produce large bubbles from the former. Now if a drop of olive oil or spirits of turpentine be placed upon distilled water, the liquid will rebel strongly against the formation of bubbles. Should the water be covered with a thin coating of either of the above mentioned liquids, you will find that it bursts in the bowl of the pipe before you have even commenced to blow the bubble. We must admit therefore, in the first place, that the supposed coating of dirt must have close connection with the liquid beneath it. The author also assigns an additional and indispensible cause for the production of bubbles which he describes as the *superficial elasticity*, or in other words, the facility with which the dirt spreads itself over the liquid, so that the latter is always covered. Nothing however, goes to show us that a thin coating of olive oil or turpentine does not possess the same elasticity.

The author, in fact, describes two circumstances in which foreign substances produce a coating upon distilled water which is more or less effectual. First of all, if the pollen of flowers is spread upon the surface and air blown from above within an hour or two, the little apertures formed will remain for a long time; but the liquid refuses to form bubbles when blown from a pipe or tube. In the second place, they can be produced, nevertheless, by means of pure distilled water, if the tube is partially filled with small particles of camphor. The diameter of these bubbles may reach an inch and more. But we can readily see that these facts are only the beginning of success. However, they are in no wise opposed to the theory of superficial viscidity, since in both cases the outer layer of the water undergoes modifications.

According to the author, the superficial elasticity is estimated by the difference which exists between the tension of the pure surface and that of the dirty surface, and he determines this by means of a small apparatus which he calls a *capillary balance*. In his opinion, as we have already seen, when a bubble is blown the coating of dirt prevents its being broken. In his statement he gives no reason for this but in a preceding work he explains himself clearly on this point. He says that if the coating of dirt should become disunited, the excess of tension upon the under layer, or in other words, the superficial elasticity, would instantly close the aperture. Hence the facility for the formation of bubbles, or as the author calls it, the pompholygenic power, should decrease with the superficial elasticity. Now M. Marangoni is led to the conclusion that all causes which tend to diminish this elasticity without removing the dirt, render the development of bubbles much easier. Further on, he returns to this proposition and says that "all those conditions which diminish this elasticity to the advantage of the plasticity increase the pompholygenic force." If we examine closely his ideas, we can understand that an increase of plasticity favors considerably the generation of bubbles; but how is it possible that a diminution of elasticity can lead to the same result?

Let us return to the first of the two facts quoted above. The author finds, by means of his capillary balance, that the superfine elasticity of the distilled water, covered with pollen, may become doubly as great as that found in the saponaceous solution. Now, inasmuch as the latter produces large bubbles while the former gives none at all, it is necessary, according to M. Marangoni's proposition that the plasticity of the saponaceous solution should be much superior to that of the distilled water, which is rather di ꞏcult to admit owing to the peculiar rigidity of the surface of the former; indeed there are two totally different liquids in question; nevertheless, the author's statement seems to apply equally to both in this case.

In order to show that the layer of dirt can be accumulated upon one portion of the surface of a liquid and diminished upon another, M. Marangoni describes the the following curious experiment.

He plunges, into a soapy solution, a ring made of iron wire about seven and a half inches in diameter and fastened to the end of a fork which serves as a handle; when the ring is immersed he draws it out again, holding it in a horizontal position; he then raises it until the catenoid wave, which unites it to the surface of the liquid, separates into two portions, one of which forms an even layer within the ring, while the other produces a spherical cavity upon the liquid; now, if the temperature is low enough (from 12 to 14 degrees), this cavity is very hollow, the radius of the base measuring 48 *millimetres*, while the height is only 27. M. Marangoni began this experiment four times, always breaking the cavity before again immersing the ring, and by this means he obtained the maximum of depression in which the depth was exactly half of the radius of the base. While the ring is being raised the circumference, in accordance with which the catenoid lamina unites with the surface of the liquid, contracts, and as M. Marangoni affirms, condenses the coating of dirt on the interior and dilates it on the exterior. Hence, when the cavity is once formed a diminution of tension takes place in the space limited by its base, and an increase of tension occurs on the outside; this excess of tension consequently aids the basis of the cavity to enlarge, and results in the depth being diminished.

According to my theory, the superficial layer of the liquid contracts, as above stated, on the interior of the opening, and dilates on the exterior; but its consistency does not undergo any modification. The portion which contracts forces a part of its molecules into the mass beneath, and the dilated portions attract these atoms. Now, according to M. Van der Mensbrugghe's theory which I have mentioned already, these effects cannot be produced unless a diminution of tension takes place within the contracted portion and an augmentation of the same in the dilated part. This phenomenon, however, can only occur in a very low temperature, and when, in consequence, the cavities manifest a certain viscidity. When the temperature is notably higher the cavities are smaller and their depression less. At 26 degrees hardly any effect is visible. The radius of the base at this temperature was 23 *millemetres*, and the height 20; but I have shown that all cavities formed upon the surface of liquids are never complete hemispheres. M. Marangoni thinks it probable, as I have said before, that the coating of dirt on the saponaceous solution is due to the action of carbonic acid contained in the air.

I have ascertained that carbonic acid actually decomposes the solution inasmuch as it removes all fatty acids; but does the formation of the layer really arise from this cause? In order to discover this the following experiment has been made:

A certain amount of a concentrated solution of caustic potash was placed within a bottle holding almost a quart, then, after tightly corking the latter it was violently shaken so that the liquid swept over every part of the interior. The greater portion of the liquid was then poured out and the bottle instantly re-corked. In the meanwhile a funnel provided with a plug was procured and the interior of its neck moistened with the solution of potash; it was then placed in the neck of the bottle and wax applied at the junction. This done, almost 300 *grammes* of a solution of Marseilles soap previously rendered clear by means of filtration was poured into the funnel and left there for one hour. At the end of that time the wax was removed and the funnel gradually lifted, the plug being opened simultaneously, and, as the liquid flowed into the bottle the funnel continued to be slowly raised until the extremity of the neck was about on a line with the top of the bottle; the latter was then rapidly corked, some of the liquid remaining in the funnel.

The potash necessarily absorbed the small quantity of carbonic acid contained in the bottle, and at the moment when the funnel was removed no exterior volume of air could possibly penetrate within the bottle, because the stream of liquid flowing in must have expelled much more air than could possibly have found its way in to replace the neck of the funnel. Finally, as merely a portion of the liquid escaped into the bottle, and that at a distance far above the free surface, it could absorb nothing from the superficial layer. Now, with this liquid merely united with air deprived of carbonic acid, transverse waves of a very persistent character were easily developed (the bottle measured three and a quarter inches in diameter), which could evidently not have occurred had the liquid been without an efficient coating. It is quite impossible, therefore, for me to accept M. Marangoni's explanation. Besides, the effectual coating upon the saponaceous solution does not arise from the evaporation of water; for a fatty liquid like soap-suds, for instance, which produces bubbles in consequence of this consistency, does not evaporate at all, but, on the contrary, attracts the dampness in the air. In order to assure myself that the effectual coating of the saponaceous solution does not proceed from the evaporation of water as M. Marangoni thinks it does, I added two parts of Price's glycerine to three parts of the solution, about the proportions generally used to produce a liquid glycerine, and the two substances were thoroughly mixed together. This compound, in consequence of the glycerine, should absorb moisture instead of losing it; now, by means of a pipe it produced bubbles at least two inches and a half in diameter. I then increased the quantity of glycerine, so that the two substances were about equally divided, and even then bubbles two inches in diameter were obtained. Thus, the effectual coating of the solution is not due to the loss of water by evaporation.

As to the solution of albumen, inasmuch as its properties are analagous with those of the soapy solution, although less pronounced, I considered it useless to make the same experiments in reference to it.

Now, if the cause which originates the formation of the effectual coating upon the saponaceous solution is due neither to the action of carbonic acid contained in the air, nor to the evaporation of water, whence does it arise? Must we have recourse to Dupré's somewhat unacceptable idea, which holds that in certain solutions the substance dissolved rises abundantly to the surface? Is it not much easier to admit, as I do, that the superficial coating of liquids forms itself spontaneously into a particular condition, which results in a greater or less difficulty in regard to the relative displacement of the molecules than could occur in the interior of the mass? Does not the fact that tension exists suffice to show that this coating possesses an especial character in reference to the action of molecules?

The experiment which originated Dupré's singular idea mentioned above, is based upon the fact that the height of a fine stream of liquid precipitated from a certain distance must be considerably diminished by the tension of its surface, and Dupré, therefore, concludes that in a little stream of soap-suds the tension is sensibly identical with that of pure water, while we all know that when a solution of soap is in a state of repose its tension does not approach that of water by two-thirds. Dupré concludes that in the stream of saponaceous solution, where the surface is constantly renewed, the soap itself has no opportunity of coming to the outside. But in my theory—a remarkable fact which I have myself confirmed by an entirely different process which it is useless to refer to here—proves that the superficial coating of liquids requires a certain amount of time, however short, to assume its proper atomical condition.

"But," says M. Marangoni, "the superficial coating of the saponaceous solution has no extraordinary viscidity; on the contrary, it is very susceptible of motion." I acknowledge that it does in fact possess great mobility, which proceeds from the extreme thinness of its consistency. Also, it is capable in itself of making but slight resistance towards the movements of the magnetized needle. Still, as it adheres in its fullest capacity to the underlying liquid, and should therefore attract a certain amount of the latter as it rotates, a greater part of the resistance must necessarily be due to this fact. Moreover, we observe, nothing goes to show us that the superficial layer, although very mobile, is less so than the underlying liquid if both are of an equal consistency. We can reasonably admit this after an experiment with the magnetized needle placed within the liquid. Indeed, as the number of oscillations performed by M. Marangoni's needle when in the liquid and when removed from it were respectively five to six, the governing powers of the needle in these two conditions are in proportion to the square of the above numbers, as, for instance, thirty-six to twenty-five, or about three to two. The resistance of the liquid robs the needle of nearly one-third of its governing force; only as we require which part the two superficial coatings play in this resistance, nothing prevents us from attributing it to the principal one of them.

Finally, the resistance in regard to the displacing of molecules cannot be denied as far as the superficial layer of saponaceous solutions is concerned, consequently we should admit this fact, although in a much less degree, in reference to solutions of soap itself. In one of my papers, and also in paragraph two hundred and seventy-eight of my book, I have described a certain number of facts which prove the rigidity existing in the effectual elevating of the saponaceous solution. I will confine myself to one of them as follows:

A bubble about an inch and a half in diameter is blown and placed upon the surface of the liquid; now, holding the mouth of the pipe in close contact with the hemisphere into which the bubble is transformed, you blow gently, increasing its dimensions until it bursts. The spray immediately spreads itself upon the liquid in several parts, each, however, being separated from the surface by a small quantity of air, and gradually disappears as though sinking into the mass, the contraction occupying several seconds.[1]

M. Marangoni, although maintaining perfect silence in

[1] In order to make this experiment successful, it is necessary to use a perfectly pure solution.

regard to this powerful viscidity, relates several experiments which make the fact of its existence very perceptible. Let us quote the following which is merely the continuation of one I have already drawn attention to :

A bubble is blown from a moderately wide tube which, however, has a broad mouth, and the other end is then left perfectly free. The bubble decreases gradually in size, but not in a perfectly systematic manner. On the contrary it elongates and at the same time contracts transversely, assuming a series of longitudinal folds or wrinkles. M. Marangoni explains this fact by stating that owing to the diminution of the surface, the coating of dirt becomes supersaturated and consequently the tension is annulled or reduced almost to nothing, inasmuch as the thin layer forming the bubble thus wrinkled and of a nearly conical shape does not show any tendency towards the minimum of the surface. But, he adds, if the unoccupied end of the tube should be corked so that the bubble would not decrease in size, the form of the latter would grow gradually round, and at the same time it would expel from the bottom certain drops of frosty moisture which forms in the little folds or wrinkles we have already mentioned ; then the coating of dirt would resume its normal condition, and the bubble assume, once more, a spherical shape.

M. Marangoni supposes that apart from the wrinkles on the bubble, the tension is utterly null or very nearly so: Now, the existence of any liquid utterly devoid of tension would be very extraordinary and we may say hardly probable. Moreover, the drops of moisture in the interior of the bubble, being the liquid which constitutes the outer coating of dirt, should possess little or no tension. I have collected these drops upon the crystal of my watch, and after repeating the experiment a number of times, I finally procured enough of the liquid to attempt the formation of bubbles by means of it. (I must state here that these drops were purely liquid and not at all frothy like those M. Marangoni describes.) Now, bubbles were formed from this liquid, some of them extending three inches in diameter, that is to say, they were similar in proportion to those obtained by means of the saponaceous solution ; only, with the liquid collected from the drops in the crystal, this maximum was much more difficult to reach. In a word, I modified M. Marangoni's experiment in a manner calculated to render his explanation of it still more improbable. A bubble about two inches in diameter was blown from the pipe and the drop suspended from the bottom removed ; then, inasmuch as the tube was expressly narrow, the wrinkled and cone shaped form was produced by inhaling through it, and before the drop produced at the extreme point of the cone could fall, the pipe was turned upside down in such a way that the liquid forming the drop ran along the surface of the bubble and separated itself as much as possible on the exterior. Now, although the superficial coating thus conserves very nearly its former consistency, and as consequently (according to M. Marangoni), the tension becomes, so to speak, annulled, the bubble instantly resumed its spherical shape while the pipe was being turned upside down, the time thus occupied not being more than one second. This experiment was repeated several times and always with the same result.

In my opinion these facts can be explained very simply. When you breathe through the pipe, should it be moderately wide or even narrow, the bubble necessarily contracts. It consequently becomes of a thicker consistency and a surplus amount of liquid flows towards the lower extremity; but the strong viscidity of the superficial coatings renders the general augmentation of density, and the equal contraction on all sides, very difficult during the short interval of reduction. The surface wrinkles in very much such a manner as a small bladder would should the air within it be inhaled, and at the same time it elongates into a conical form from the weight of the liquid which accumulates at the bottom. Nevertheless, this liquid arising from the increased density of the bubble does not notably diminish the tension, as is shown by the fact that when the pipe is held upside down and the liquid rests upon the bubble sitelf, the latter regains its spherical form immediately.

In regard to the superficial coating of the solution of soap, M. Marangoni observed that if this coating was viscous it should wrinkle when before the needle, which, however, does not occur at all. In order to discover what really takes place in reference to this circumstance, I began the experiment once more by sprinkling the surface of the solution with pollen[1] just before liberating the needle. If attention is then drawn to the tension of the needle, it will be seen that on the side toward which this half advances, and until a moderate distance is reached, the dispersion of the pollen is diminished, while on the opposite side—that is to say, behind this particular half—it is considerably increased. Thus, the superficial coating in front of the needle, instead of puckering, contracts, and dilates behind it. Now, if we reason in accordance with my theory, and consequently do not admit the existence of a coating of dirt, we should acknowledge that in the portion constructed the molecules pertaining to the superficial coating have left it and entered the interior of the mass, and also that in the dilated portion the molecules belonging to the interior have annexed themselves to the superficial coating in order to maintain the density ; these two effects could not be produced, moreover, unless a certain amount of resistance existed. They have necessarily developed also a difference of tension ; but, in the second of the two series of estimates which I effected in reference to the duration of the needle's movements on the surface and in the interior of the solution, the temperature was about 21°, and from M. Marangoni's observations upon the spherical cavities before mentioned, it follows that at this degree of temperature the differences of tension should possess but slight influence. However, the ratio concerning the duration of these movements upon the surface and beneath it have been found to about equal 1–78. Besides, these experiments seemed to result in showing that the effect arising from the difference of tension is not altogether to be overlooked, for in the first series in question when the temperature was but 18°, the ratio of duration was somewhat increased ; that is to say, about 1–87.

At the beginning of these remarks it was seen that M. Marangoni explains the retarded motion of the needle upon the solution of soap by the difference existing between the tension of the dirty coating and that of the liquid beneath. We have also seen that in regard to liquids such as water, saline solutions, etc., which also retard the needle's movements, he seemed somewhat embarrassed. At the commencement of his work, he insists upon the capillary action of the meniscus, then further on, he appears to attach but little importance to it, and invokes a small quantity of dirt ; further on still, he takes refuge in M. Van der Mensbrugghe's theory.

As far as the capillary action of the meniscus is concerned, I have endeavored to make it thoroughly understood that if we admit it at all, we should consider it as being probable the very reverse of what M. Marangoni supposes. He knows, moreover, that the action of a meniscus would not be sufficient in itself to satisfactorily explain the existence of any phenomena ; for example, it could not account for the rotation of the entire surface of the liquid. M. Marangoni, therefore, only ascribes a partial *rôle* to it, and at the same time seeks protection under a coating of dirt and the ideas expressed by M. Van der Mensbrugghe. But, you will ask, where then does this coating of dirt come from ? Does it arise from particles of dust floating about in the atmosphere ? In his first work M. Marangoni says that water which has

[1] In order to do this, the pollen must be spread upon the surface of the liquid by means of a small paper tube held at a certain distance above the solution. Care must be taken to do this as quickly as possible, as the soap moistens the particles and causes them to sink rapidly.

been distilled several times can remain exposed to the air for six or eight days without the slightest augmentation of resistance, in regard to the needle, being apparent. Besides, in the measures taken with distilled water, the entire preparation of the experiment from the moment when the liquid was poured into the capsule until the needle, was left to itself, occupied but five minutes; then during the ten partial measures afterward effected, no increase of resistance was observable. Could particles of dust floating about in the atmosphere produce an effect during those five minutes? Is it admissible? Indeed, M. Hagen has shown us conclusively that the superficial tension of distilled water decreases perceptibly when the liquid is exposed to the air; but this diminution is gradual and continued, and in order to produce any visible effect requires several hours. The peculiar fact M. Hagen describes, therefore, appears to me to bear no relation whatever to the resistance shown to the needle's movements; and inasmuch as air on the other hand, exercises no chemical action upon distilled water, and moreover as we are unable to invoke the influence of particles of atmospherical dust, we are led to attribute the fact established by M. Hagen to a cause arising from the interior of the liquid.

Now, in reference to the actual state of the case, I shall say again that it is useless to have recourse to a coating of dirt whose existence we cannot account for, and also that it is much more simple to admit the presence of an atomic organization peculiar to the superficial layer of the liquid.

As far as M. Van der Mensbrugghe's theory is concerned, M. Marangoni expresses himself in the following manner:

"The mass of the liquid effectually diminishes the variations of temperature produced upon the surface, which, in its turn, also decreases the variations of tension; in ordinary cases the latter are but trifling when compared with the variations attributed to dirt."

According to this remark, we should believe that the surface of the saponaceous solution, which, M. Marangoni states, possesses an undeniable coating of dirt, resists the movements of the needle more forcibly than the distilled water which could have hardly any dirt on its surface. In my experiments however, directly the opposite of this has occurred. The ratio of time required for the needle to describe an angle on the surface and beneath it when distilled water was used was, 1, 92, while when soap was used it was but 1, 82.

M. Van der Mengsbrugge's theory certainly deserves some attention in regard to the phenomena in question; but owing to the above remark of M. Marangoni, and the considerable dimension of the needle, relatively speaking, we may be permitted to doubt that any notable effect can result from it. Besides, if it did, we should find it again in those liquids of weak tension which do not produce bubbles, that is to say, alcohol, spirits of turpentine, olive oil, etc.; at least we should be able to observe a feeble rotation of the entire surface; now, this is by no means authenticated.

Finally, before attributing these phenomena to any other cause than that of a peculiar viscidity of the outer coating, it would be necessary to refute those arguments which have led me to the conclusion that the superficial coating of liquids possesses more atomic mobility than the interior portion. M. Marangoni is perfectly silent in regard to this part of my work.

After this examination of M. Marangoni's theory however, I consider myself justified in maintaining my opinion; but I forego all ulterior discussions referring to the subject, and leave all those physicists who may be interested in the question, to compare for themselves M. Marangoni's writings with mine, and to try to discover, if possible, which of us is right.

ON THE STRUCTURE OF THE ORANG OUTANG.

By Henry C. Chapman, M. D.

From the paper on this subject in the Proceedings of the Academy of Natural Sciences, of Philadelphia, we take the following facts:

The subject dissected was a young male Orang Outang (*Simia Satyrus*), about three years old. The first thing to strike Dr. Chapman was the length of the upper extremity, it being three inches longer than the lower one, agreeing nearly in this respect with the Gorilla, the difference in the extremities of that animal being 3½ inches, whereas in the Chimpanzee a difference of 1¾ inches only was found. The foot in the Orang, however, was ½ inch larger than the hand, whereas in the Gorilla the hand was ½ inch larger than the foot; in the Chimpanzee the difference in this respect was ⅜-in. in favor of the foot. Indeed, the distinctness of hand and foot superficially is more marked in the Gorilla than in the other anthropoids. The same facial muscles are found in man and the Orang Outang, with the exception that there is but one zygomaticus, possibly corresponding to the zygomaticus minor of man. The facial muscles, however, are not differentiated as in man, rather hanging together. The upper extremity of the Orang, in its muscles, differed essentially from that of man in the absence of the flexor longus pollicis, and extensor primi internodii pollicis and in the presence of the additional tendons to the ring and middle fingers.

The Orang agreed with the Gorilla in not having a flexor longus pollicis, but disagreed with it in having the pronator radii teres, arising by two heads in the presence of a palmaris longus, in the additional tendons for ring and middle fingers, and in not having the extensor primi internodii pollicis.

As compared with the Chimpanzee, the Orang agreed in reference to the pronator radii teres and palmaris longus, but in the absence of the flexor longus pollicis as a slip from the profundus, and in the presence of the additional extensor tendons it differed.

Dr. Chapman differed from Bischoff, Owen, Huxley and others, in seeing no essential difference between the *scansorius*, of Traill, and the glutæus minimus in man, an opinion, it appears, which had been previously expressed by Prof. Barnard in 1876.

The leg and the foot of the Orang, as compared with man, differed in the absence of the peroneus tertius, plantaris, flexor longus hallucis and transversus pedis, in the fibular origin of the soleus, and in the presence of the external origin of the accessorius only, in the distribution of the perforating and perforated tendons of the toes, in the interossei, and in the presence of an opponens for the big toe. In this latter respect, the Orang differs not only from man, but from all the other monkeys and anthropoids, the foot having a very hand-like appearance, as compared with that of the Gorilla and Chimpanzee. The foot of the Orang differs further in the absence of a special flexor for the big toe. This is supplemented, to a certain extent, by the opponens, and in a partly developed accessorius.

If Professor Huxley's canon can be accepted that the distinction between a hand and a foot consists in the latter possessing tarsal bones, the peroneus longus and brevis, the short extensor and short flexor muscles, then the posterior extremity of the Orang terminates in a foot.

Dr. Chapman, however, appeared to think that the difference between the hand and the foot in Man, the Gorilla, and Chimpanzee, and the lower monkeys, is greater than that observed between the corresponding members of the Orang.

It is usually stated that the uvula is absent in the Orang, and on looking into the mouth, at first sight this appears to be the case, as it does not hang down as in man, between the pillars of the fauces. Nevertheless, Dr. Chapman found it to exist. Professor Bischoff, however, mentions finding the uvula in the Orang.

The stomach of the Orang is not so human in its form as that of either the Gorilla or the Chimpanzee. Nothing peculiar was noticed about the spleen or pancreas.

The quadrate lobe of the liver was absent. In the small intestine five fine specimens of the *Ascaris lumbricoides*, and one in the large, were found, and in the cæcum a *Trichocephalus dispar*, perhaps the first time these entozoa have been found in the same anthropoid. Dr. Chapman did not notice anything special about the heart different from the human.

The brain was examined and described, but as the researches of Dr. Spitzka in this direction have been published in "SCIENCE," we need not here state the peculiarities which exist.

Dr. Chapman draws the following general conclusions respecting what can be inferred from the general organization of the Orang as to its relation to the primates.

The Orang, like man, has twelve ribs, whereas the Gorilla and Chimpanzee have thirteen ; on the other hand the carpal and tarsal bones are nine in number in the Orang, while the Chimpanzee and Gorilla agree with man in having eight. The Chimpanzee and man are alike in this respect, at least the slip from the flexor longus digitorum in the former is functionally a flexor longus. In the absence of a flexor longus hallucis, and in the presence of an opponens hallucis, the Orang differs from man, the anthropoids and all monkeys. The great blood vessels arise from the arch of the aorta in the Gorilla and man in the same way ; the same disposition is usually seen in the chimpanzee, rarely in the Orang. The lungs in the Orang are not divided into lobes as in the Gorilla, Chimpanzee and man. The stomach in the Gorilla and Chimpanzee is human in its form ; in the Orang, however, it is quite different. The peritoneum in the Gorilla, Chimpanzee and Orang is like that of man ; in the lower monkeys it is different. The brain of the Orang in its globular form, in the cerebellum being usually covered by the cerebum, and in the development of the first occipital gyrus, resembles man more than that of the Gorilla and Chimpanzee. On the other hand, the frontal and temporal lobes in the Orang are not as mnch convoluted as in the chimpanzee, and still less than in man, and the Island of Reil is not convoluted at all, at least in the Orang here described. It will be seen that from the above illustrations, of which many others might be given, that the gorilla and man, in some respects, agree with and differ from the Chimpanzee and Orang, while from other points of view the Orang approaches man more closely than either the Gorilla or Chimpanzee, and that as regards certain muscles, man and the lower monkeys agree in having them, while they are absent in the anthropoids.

From these facts we may reasonably infer that the ancestral form of man was intermediate in character as compared with the living anthropoids or lower monkeys, agreeing with them in some respects, and differing from them in others. The Orang is closely allied to the Gibbons, the Chimpanzee to the Macaques, and the gap between these and the *Semnopithecus* is bridged over by the *Mesopithecus* of Gaudry. Until, however, the paleontologist will have procured more material like that from Pikermi, and interpreted it as ably, it seems to Dr Chapman premature to offer any detailed genealogical tree of the Primates.

Mr. A. D. Anderson, author of "The Silver Country or The Great South-West," has prepared a brief narrative of all efforts since the time of Cortez to effect interoceanic transit across the Isthmus of Tehuantepec. The book will be published at once by Messrs. A. S, Barnes & Co., of New York,

ON THE ORIGIN OF ANTHRACITE.*

BY T. STERRY HUNT, LL.D., F.R.S.

From my comparative studies of carbonaceous minerals I, as long ago as 1861, reached the conclusion that petroleum and anthracite form the extremes of a series, all of which may have been derived from organic matters, by natural processes at ordinary temperatures.†

To this is opposed the ordinary view that anthracite, on the one hand, and petroleum on the other, result from the action of heat on matters of intermediate composition, the one being a distillate and the other a residuum. Late geological studies, however, show that such an hypothesis is untenable for petroleum, and the author, while not denying that a local coking of bituminous coals must naturally result from the proximity of igneous rocks, has long taught that it is equally so for our anthracite fields. The prevalent notion has hitherto been that the difference between these and the bituminous coals farther West is in some way connected with the mechanical disturbance of the strata in the former region ; but to this is opposed the fact that, while the undisturbed coals of Arkansas are anthracite, the highly disturbed coals of northeastern America, Belgium, and other regions are bituminous.

These considerations I have for many years presented to my classes in Geology, and have maintained that the change which results in the conversion of organic matters into anthracite was effected before the disturbance of the strata ; that the hydrogen was removed, as ordinary vegetable decay, in the forms of water and marsh-gas ; and that differences in aeration during the process of change and consolidation of the carboniferous vegetation are adequate to explain the chemical differences between anthracite and bituminous coals.

Prof. J. V. Lesley, to whom I have explained my views, has pointed out that there is an apparent connection in the great Appalachian coal-basin, between the more or less arenaceous and permeable nature of the enclosing sediments and the more or less complete anthracitic character of the coal, while Principal Dawson informs me that he has observed similar facts in the coal-measures of northeastern America. Inquiries which promise to throw farther light on this question are in progress, and the present note to the Academy is to be considered as only preliminary to a farther discussion of the subject.

NIMRAVIDÆ AND MIOCENE CANIDAE.*

PROF. EDWARD D. COPE.

The *Nimravidæ* is a new group resembling the cats, but differing fromthem in the presence of six pairs of foramina which are characteristic of other families of *Carnivora*. They are older than the *Felidæ* occuring in Miocene formations commencing with the lowest horizons. Some of the species are supposed to occur in the upper Eocene. The family includes the primitive cats, the false sabretooths, and the primitive sabre-tooths, which correspond respectively with the true cats, and the true sabre-tooths, forming heterologous terms of two homologous divisions.

The genera of Miocene *Canidæ* in North America are *Amphicyon Tennocyon*, and *Galecynus*, all distinguished by the presence of the epitrochlean foramen. Other genera are *Enhydrocyon* and *Ieticyon*.

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* Read before the National Academy of Sciences, N. Y., 1880.

†Canadian Naturalist, July, 1861, and Report Smithsonian Institution for 1862 ; also Chem. and Geol. Essays.

ON PHOTOGRAPHING THE NEBULA IN ORION.*

By Professor Henry Draper.

The gaseous nebulæ are bodies of interest because they may be regarded as representing an early stage in the genesis of stellar or solar systems. Matter appears to exist in them in a simple form, as indicated by their simple spectrum of three or four lines. It is desirable, therefore, to ascertain what changes occur in the nebulæ, and determine, if possible, the laws regulating their internal movements. Drawings by hand have been made of some of the nebulæ, and especially of the nebula in Orion, for upwards of 200 years. But drawings are open to the objection that fancy or bias may distort the picture, and it is therefore difficult to depend on the result, and to compare the drawing of one man with that of another. To apply photography to depicting the nebulæ is difficult, because these bodies are very faint, and, of course, owing to the earth's motion and other causes they seem not to be at rest. They require a large telescope of special construction, and it must be driven by clock-work with the greatest precision. All such difficulties as those arising from refraction, flexure of the telescope tube, slip of loose bearings, atmospheric tremor, wind, irregularities of clock-work, foggy or yellow state of the air, have to be encountered. The photographic exposure needed is nearly an hour, and a slip or movement of a very small fraction of an inch is easily seen in the photograph when it is subjected to a magnifier.

The means I have used to obtain the picture are as follows: A triple achromatic objective of 11 inches aperture made by Clark & Sons, according to the plan of Mr. Rutherford, for correcting the rays especially for photography. This telescope is mounted on an equatorial stand and driven by a clock that I made myself. The photographic plates are gelatino-bromide, and are about eight times as sensitive as the wet collodion formerly employed.

As to the picture itself the nebula is very distinct in its bright portions. The stars of the *Trapezium* and some others are so greatly over exposed that under the magnifying power employed they assume a large size, partly from atmospheric tremor and partly from other causes. It is probable that much more of the nebula will be obtained in pictures taken in the clear winter weather. This photograph was made at the end of September when there was some fog in the air; but nevertheless, the original shows traces of the outlying streamers seen in the drawings of other observers. A series of photographs taken at various times of the winter season and in different years will give us the means of determining with some precision what changes, if any, are taking place in this body.

ELECTRO-MOTIVE FORCE OF THE BRUSH DYNAMO-ELECTRIC MACHINE.*

Professor Henry Morton.

Some recent experiments, which I made in determining the electro-motive force of the Brush dynamo-electric machine, and various instruments for the accurate measurement of electric currents of great strength, show that each pair of coils on the armature of the machine develops a fluctuating electro-motive force, the projection of which gives a kind of oval curve around the centre of a diagram.

When these curves for each pair of coils are combined, it is found, that they show a kind of eight-lobed figure with intersecting lines in certain places. These intersections, if compared with the positions of the commutator, are found to coincide exactly with the points at which rupture of circuit occurs, and thus show that each pair of coils is thrown out, not at the point where its force is least, but at that at which its electro-motive force is equal to that from which it breaks; thus suppressing a spark, but only at a certain sacrifice of theoretical efficiency.

* Read before the National Academy of Sciences, N. Y., 1880.

APPLICATION OF THE PHOTOPHONE TO THE STUDY OF THE NOISES TAKING PLACE ON THE SURFACE OF THE SUN.

On visiting the Observatory of Meudon, at the invitation of M. Janssen, Mr. Graham Bell examined with much care the large photographs which are being made there for the study of the solar surface. M. Janssen having informed him that he detected movements of a prodigious rapidity in the photospheric matter, Mr. Bell had the idea of employing the photophone for the reproduction of the sounds which these movements must necessarily produce on the surface of the sun. M. Janssen approved of the idea, and requested Mr. Bell to attempt its realization at Meudon, placing all the instruments of the observatory at his disposal. The weather being very fine on Saturday last (October 30), Mr. Bell came to Meudon to attempt the experiment. A large solar image of 0.65 metre in diameter was examined with the selenium cylinder. The phenomena were not sufficiently decided to be regarded as successful, but Mr. Bell does not despair of succeeding on further examination. M. Janssen suggested that the chance of success would be much greater if in place of directly interrogating the solar image where the variations are produced, though responding to considerable changes on the sun's surface, are not sufficiently rapid even in the most powerful instruments to cause the production of sounds in the photophone, a series of solar photographs of one and the same spot, taken at sufficient intervals to obtain well-marked variations in the condition of the spot, might be passed with a suitable rapidity before an object glass, which would give conjugated images upon the selenium apparatus. This would be a means of condensing into a time as brief as could be desired the variations which in solar images are much too slow to give rise to a sound. M. Janssen has placed himself at Mr. Bell's disposal to provide him with solar photographs suitable for carrying out this idea, and the latter has sent M. Janssen the photophonic apparatus requisite. It has appeared to M. Janssen that the idea of reproducing on earth the sounds caused by great phenomena on the surface of the sun was so important that the author's priority should be at once secured.

LECTURE PHOTOPHONE.

A simple form of Photophone, which is sufficient to show the principle of the instrument, and may be used for lecture purposes, has been arranged by Mr. Shelford Bidwell, and exhibited before the Physical Society of London.

The reflector for receiving the light is discarded, and the beam focussed on the selenium by the lens.

The two lenses used cost only about six dollars, and the beam is sent fourteen feet.

The selenium cell was made by spreading melted selenium over sheets of mica, and then crystallized by heat. For mica Professor Bell recommended microscopic glass.

The resistance of the cell was 14000 ohms in the dark and 6500 in the light. Speech was distinctly transmitted by this apparatus.

ASTRONOMY.

SWIFT'S COMET.

Swift's periodic comet, which has now become quite faint, was observed on December 10th and 11th, with the 26 in. equatorial of the Naval Observatory, and it is hoped that more observations will be obtained as soon as the moon has passed. On account of an elliptic motion, it has been slowly departing from the ephemer's computed by Mr. Upton with parabolic elements and published in Vol. I, No. 21, of "SCIENCE."

The following is a continuation of Mr. Upton's ephemeris, which he has correc ed, however, from the most recent observations:

EPHEMERIS.—WASHINGTON MEAN MIDNIGHT.

DATE.	R. A. H.	M.	S.	DECL.	
1880, December 18	5	14	44	+38°	5.2′
" " 20	5	20	40	36	45.1
" " 22	5	25	53	35	29.9
" " 24	5	30	35	34	19.6
" " 26	5	34	47	33	14.0

A NEW Astronomical Journal, *Urania*, edited by Dr. Ralph Copeland and Mr. J. L. E. Dreyer, is to appear early in January. It will be published in numbers of from 16 to 24 quarto pages, as material can be accumulated. The names of the editors, Mr. Dreyer as a former assistant to Lord Rosse, and Dr. Copeland as Lord Lindsay's assistant, are sufficient assurance that this will meet the want long felt in England and Ireland of a journal, published at frequent intervals, especially devoted to the interests of astronomers.

Lieut. S. E. Tillman, of the Corps of Engineers, whose name is well known in connection with the American Transit of Venus Expedition to Tasmania, has been appointed Professor of Chemistry, Mineralogy and Geology, at West Point, in the place of Professor H. L. Kendrick, who has voluntarily retired in order that this appointment might be made. Professor Tillman has had a very varied experience as an officer of engineers, his duties having led him to astronomical and geodetic work in the field and to geographical and geological explorations. The military academy may be congratulated upon having secured a valuable addition to its present strong academic staff.

The volume of Washington Astronomical Observations for 1876, containing, in an appendix, the reports on the total solar eclipse of 1878, is expected, in a few days, from the Government printing office.

Mr. S. C. Chandler, Jr., publishes, in *Science Observer*, a description of an instrument, the "Almacantar," which he has invented for determining time and latitude. The instrument is designed for the observation of "Equal Altitudes," the principle upon which it is made being that of Kater's floating collimator. The Y's, in which the pivots rest are secured to opposite sides of a hollow iron rectangle which floats in a rectangular basin of mercury. The telescope can be clamped in altitude and the whole instrument rotated about a vertical axis. The float is allowed to seek its level, and thus the telescope will indicate equal altitudes on either side of the meridian. The probable error of a clock correction, as determined from a series of observations with this instrument, is about ± 0.05 sec.

W. C. W.

P. S.—For notice of a new comet see page 297.

To the Editor of SCIENCE:

I observe what appears to be some errors in dates in the list of minor planets discovered by the late Prof. J. C. Watson, mentioned by your correspondent.

(133) Cyrene was discovered August 14, 1873. (*American Journal of Science*, III., vi, 296).

(174) Phædra was discovered August 8, 1877. (*American Journal of Science*, III., xiv, 325).

(175) Andromache was discovered September 2, 1877. (*American Journal of Science*, III., xiv, 325).

He also discovered, October 20, 1857, the planet observed a few days before by Luther, and since named *Aglaia;* also, October 9, 1865, the planet seen by Peters a few days previously, and since named *Io;* also, July 29, 1873, a planet which on account of cloudy weathe, eluded his subsequent observation. (*American Journal of Science*, III., vi, 296).

A. WINCHELL.

University of Michigan, Ann Harbor, Dec. 11, 1880.

MICROSCOPY.

In the American Monthly Microscopical Journal for December, Dr. J. J. Woodward claims for Professor J. L. Riddell, M. D. of the United States, the priority in inventing at least two forms of Binocular Microscopes, since introduced by Beck of London, and Nachet of Paris.

This communication of Dr. Woodward appears to prove beyond a doubt that to an American, Dr. Riddell, then of New Orleans, is due the credit of first demonstrating and publishing the optical principle, on which all the most successful binoculars, made prior to the present year, depend. He first showed that the cone of rays proceeding from a single objective may be so divided by means of reflecting prisms, placed as close behind the posterior combination of the objective as possible, that orthoscopic binocular vis on can be obtained both with the simple and compound microscope.

While giving full credit to Dr. Riddell for all that is due to him, we think, in justice to Mr. Wenham, the fact should be admitted that he was the first to produce a binocular arrangement for the microscope, so simple and perfect in its form, as to render its general use possible. We once asked a London microscope maker, why the Stephenson form of binocular was only adopted by a very few microscopists, and were informed, in reply, that the expense was great in constructing microscopes on this model, and on that account they were not popular.

As we find from Dr. Woodward's paper that the improved form of Dr. Riddell and that of Stephenson were practically alike, it may be that for this the reason neither received the attention anticipated.

An interesting paper on *Cercaria hyalocanda*, by Herman C. Evarts, may be found in the same Journal. This larval form of a trematode was observed to come from the common pond snail (*Physa heterostropha*) when placed in a shallow dish containing water.

In form, the body when contracted was globular, and this form was maintained by the animal while actively swimming about; at rest it would extend its tail, and then assume a somewhat triangular form.

They were sufficiently large to be seen by the naked eye, and were observed to encyst themselves, contracting during the process to a globular form, around which was secreted a glutinous mass. A few seconds after the cyst commenced to form, the tail detached itself and swam away.

We are also indebted to this journal, for the description of the following method of mounting opaque objects, contributed by Mr. A. H. Chester:—

"The object is first fastened to the slide, which is centered on the turn-table, by means of a weak solution of gelatin, gum water, or Brunswick black. For very small objects a small circle of the gelatin is turned in the centre of the slide, and then allowed to dry. The objects are arranged on the spot, and then, by carefully breathing on the slide, they are fixed in position. If larger objects are to be fixed to the slide a spot of gelatin or gum that the object will entirely cover is put on, and after drying, the object is fixed in the same way. For larger and heavier objects a circle of Brunswick black is turned, and after it has been thoroughly hardened by heat, so that when cool a needle point will not mark it easily, the object is arranged on the spot and fastened by warming again.

In whatever way the object is fastened, the next thing to be done is to lay the slide on the plate and heat it until it is perfectly dried and ready to be covered.

The slide is then centered on the table and a circle of shellac, which has been thickened and colored with Chinese vermillion, is run around the specimen, at such a distance from it that its inner edge is just larger than the cell to be used. The cell is then laid on, centered, and pressed hard to set it. If the slide is slightly warm and the cement thick, it will not run at all, but will hold the cell firmly in place, so that the cover can be put on at once. If it is thin it must first be allowed to harden somewhat. When ready, as it will be in a few moments if properly managed, a ring of the same cement is run on the cell and the cover is then laid on, pressed down, clipped in position, and the mount laid aside to harden. It is well in an hour or so to remove the clip and run cement in the joints between cover-glass cell and slide, in order to be certain that no air-holes remain. It can then be reclipped, and set aside until the cement is perfectly hard. The mount is complete and will last a long time if proper care is taken of it. I think for security it is well to put on additional rings of cement more elastic than the shellac, and to make a final finish for the sake of appearance. I, therefore, put on a ring of white zinc cement which completely fills up the joints, and makes a smooth surface from cover-glass to slide. This must harden several days, and the slide is then complete, unless additional rings are run on for a finish.

In making the rings on slides it is not always easy to make the edges true, and sometimes the cement spreads too far. In such cases I turn them down with the point of a knife until they suit. If the cement is taken just at the right time this is easily done, and it improves the appearance very much."

BOTANY.

THE COLOR OF FLOWERS.—At a recent meeting of the Vaudois Society of Natural Science, Prof. Schnetzler read an interesting paper on the color of flowers. Hitherto it has generally been supposed that the various colors observed in plants were due to so many different matters—each color being a different chemical combination without relation to the others. Now, however, Prof. Schnetzler shows by experiment that when the color of a flower has been isolated by putting it in alcohol, one may, by adding an acid or an alkali, obtain all the colors which plants exhibit. Plants of Pæony, for example, yield, when macerated in alcohol, a violet-red liquid. If some acid oxalate of potassa be added, the liquid becomes pure red; while soda changes it, according to the proportion used, into violet, blue or green. In the latter case, the green liquid appears red by transmitted light, just as a solution of chlorophyll does. The sepals of Pæony, which are green bordered with red, become wholly red when placed in a solution of acid oxalate (binoxalate) of potassa. These changes of color, which may be obtained at will, may quite well be produced in the plant by the same causes; since, in all plants, there always exist acid or alkaline matters. Further, it is stated that the transformation from green into red, observed in the leaves of many plants in autumn, is due to the action of the tannin which they contain, on the chlorophyll. Thus, without desiring to affirm it absolutely, Prof. Schnetzler supposes, *a priori*, that there is in plants only one coloring matter—*chlorophyll*, which, being modified by certain agents, furnishes all the tints that flowers and leaves exhibit. As for white flowers, it is well known that their cells are filled with a colorless fluid, opacity being due to air contained in the numerous lacunæ of the petals. On placing the latter under the receiver of an air-pump, they are seen to lose their opacity and to become transparent as the air escapes from them.

PROFESSOR W. W. Bailey, of Brown University, states that the herbarium of the late Col. Stephen T. Olney, of Providence, R. I., was left by his will to Brown University, on condition that it be placed in a fire-proof building. It is probably known to the readers of "SCIENCE" that Col. Olney was an invalid and incapacitated for business during the last years of his life. At that time the herbarium, which had been stored in Butler Exchange, was transferred by the trustees to the fire-proof library building of Brown University, the only edifice possessed by the college which would fulfil the requirements. Professor Bailey was requested to examine and arrange the collections, which he did in connection with Mr. James L. Bennett. He is greatly indebted to this gentleman for valuable suggestions and assistance which his natural neatness of method and mature experience rendered easily possible. He it was who arranged the *Carices* (which were Col. Olney's specialty), together with the lower Cryptogams, many of which he had himself collected.

They found this elegant herbarium, one hardly surpassed by any private collection in America, badly injured by insects. The first work, then, was to poison what could be saved. It is not an exaggeration to say that one-third of the Phanerogams had suffered. In places a whole genus would be riddled by the *Anthrenus*. It was a sad sight; for the specimens had been prime, were superbly mounted, and many of them impossible to replace. Col. Olney was so neat in his methods that he disliked to see a blemish on any paper; hence his very sense of order was perhaps a means of loss. Every plant had to be thoroughly poisoned, Now that the college has come into possession, it will be necessary to throw out mutilated specimens and replace them by others. Mr. Bennett and Professor Bailey stand ready to fill the vacancies from their own herbariums.

The collection is a fine one in every way. In Rhode Island plants it is only equalled by that of Mr. Bennett. It is very rich in Southern and Western plants of Hale, Chapman, Curtis, Ravenel, Fendler, Parry, Thurber and many other well-known collectors. There is a fine set of Wright's Cuban plants, of Robin's *Potamogetons*, of Sullivant's and Austin's Mosses, etc., etc. Indeed the owner spared no expense (and he was a wealthy man) to build up his herbarium. In the genus *Carex* it must long remain unique and classic. There is much raw material and many duplicates in *Carex*. As Colonel Olney's correspondence shows him to have been in debt as regards exchanges, Professor Bailey, who now has charge of the herbarium, would be pleased to communicate with such botanists as have not received returns. He will then, acting under the direction of the college authorities, endeavor to discharge all such obligations. Col. Olney bequeathed a fund of $10,000 for the increase of his herbarium and library. The latter, containing 712 volumes also comes to Brown University, together with his Chevalier and Smith & Beck's microscopes and much valuable apparatus and material. With another $25,000 left by the deceased Colonel, a professorship of Natural History has been created. One of the duties of the professor is to give lectures on Botany.

We are under obligations to the *Bulletin* of the Torrey Botanical Club, for occasional botanical notes. This Journal has now been published for ten years, and was established as a means of communication for botanists. The address of the editor is, W. H. Leggett, 54 East 81st street, New York City. The rates are one dollar per annum, so that its cost will hardly be a bar to its use by botanists. We can probably arrange club rates for our subscribers.

CHEMICAL NOTES.

ADULTERATIONS OF SAFFRON.—Saffron is sophisticated with muscular fibre, the flowers of *Calendula officinalis*, safflower, *Crocus vernus*, *Punica granatum*, fragments of sanders-wood, glucose, glycerin, oil, chalk and heavy-spar.

PREPARATION OF ASHES DESTINED FOR THE EXTRACTION OF IODINE FROM SEA-WEEDS.—The most advantageous weeds for this purpose are the two varieties of *Fucus digitatus*. Dr. Thiercelin states that he has succeeded in extracting from the plant 3 per cent. of iodine.

MANUFACTURE OF PHOSPHORIC ACID.—Natural phosphates, unground, are dissolved in dilute hydrochloric acid. When the acid has ceased to act the clear solution is run off from the insoluble matters and mixed with sulphuric acid enough to saturate all the dissolved lime, leaving a mixture of hydrochloric acid, dilute phosphoric acid, and calcium sulphate. This mixture is submitted to pressure to separate the sulphate from the free acids, which are then concentrated, and the hydrochloric acid is condensed and collected for use by means of ordinary columns.—M. A. COLSON.

COMPLEX ACIDS CONTAINING BORIC ACID.—Dr. F. Mauri has formed boro-tungstic acid by dissolving tungstic anhydride in a solution of borax, and continuing to add the former until the liquid is no longer rendered turbid bd hydrochloric acid. He is engaged with the formation any the study of the boro-molybdic acid and its salts

COMPOUND OF TITANIUM TETRA-CHLORIDE AND OF PHOSPHORUS PROTO-CHLORIDE.—The composition of this compound is represented by the formula $TiCl_4PCl_3$.—M. ARMAND BERTRAND.

COMPOUND OF TITANIUM TETRA-CHLORIDE AND ETHYL OXIDE.—If the vapors of these two bodies are brought in contact, fine crystals of a greenish yellow color are produced.—M. ARMAND BERTRAND.

REDUCTION OF ETHYL NITRATE BY ALCOHOL.—Nascent ethyl nitrate is reduced in presence of alcohol, yielding ethyl nitrite and aldehyde.—M. ARMAND BERTRAND.

PRODUCTS CONTAINED IN THE COKE OF PETROLEUM.—Experiment shows that the accumulation of the carbon is effected with an increasing rapidity, and the weight of the molecule rises to a limit still little known, but which can be no other than the formation of insoluble bodies whose richness in carbon is equal or even inferior to that of the bodies which have remained soluble. We reach thus, by a progression easy to conceive, the term of the series which must equally include crystalline bodies such as graphite and diamond. It is known, on the other hand, that the higher polymers, when submitted to very high temperatures, seem to depolymerise themselves (as happens with metastyrolene), yielding gaseous carbon compounds.—MM. L. PRUNIER AND EUG. VAREUNE.

ACTION OF MONO-BROMATED DIPHENYL-METHAN UPON AMMONIA.—If the ammonia is in alcoholic solution, ammonium hydro-bromate is deposited; and the alcoholic liquid, if precipitated with water, yields, as a principal product, a mixed ethyl-benzhydrolic ether. Concentrated aqueous ammonia acts differently; the crystalline bromine is gradually transformed, and in twenty-four hours the mass becomes liquid. In forty-eight hours more it becomes solid, and then it no longer contains bromated diphenylmethan.—MM. C. FRIEDEL and M. BALSOHN.

SYNTHESIS OF CHINOLINE.—The alizarin blue of Prud'homme has the composition $C_{17}H_9NO_4$, and is probably a dihydroxylised quinon of anthrachinoline. It is formed from nitro-alizarin and glycerin, with the elimination of water. Chinoline is actually obtained on heating together nitro-benzol, glycerin, and sulphuric acid.—Z. H. SKRAUP, *Wiener Anzeiger*, 1880, 69.

FUNCTION OF LIME IN THE LIFE OF PLANTS.—E. v. Raumer and Ch. Kellermann assert that lime is absolutely necessary for the life of plants, and its function is most closely connected with the utilization of the carbohydrates.

CHEMICL INVESTIGATIONS IN THE BOHEMIAN CENTRAL MOUNTAINS.—J. Stocklasa has recently made an examination of the marls and clays of Priesen.—*Listy Chem.*, 4, 135.

BÖDECKER'S METHOD OF DETECTING ALBUMEN IN URINE.—The urine is slightly acidified with acetic acid, and a few drops of a solution of potassium ferrocyanide are added. In presence of even very slight traces of albumen a turbidity at once appears, and in a short time there is deposited a flocculent sediment. The test is exceedingly sensitive.

CHLORALUMINIUM USED AT CLOTH WORKS.—A sample contained 15.49 per cent. Al_2Cl_6, 1.13 Al_2O_3, 2.59 NaCl, 0.14 Na_2SO_4, 80.65 H_2O. Apparently formed by decomposing aluminium sulphate with barium chloride. F. STOLBA.—*Listy Chem.*, 4, 193.

CRYSTALLINE PRUSSIAN BLUE.—W. Gintl states that if recently precipitated Prussian blue be treated with a moderate excess of hydrochloric acid at a gentle heat, it dissolves to a slightly yellowish liquid, which, on exposure to the air, gradually deposits Prussian blue as a crystalline sediment, which displays a splendid coppery lustre by reflected light. So-called Turnbull's blue dissolves in hydrochloric acid in the same manner as ordinary Prussian blue, and yields similar crystals—a further evidence of the identity of the two compounds.

TRUE CLAY IN SO-CALLED CLAY SOILS.—A. Funaro has shown that the highest proportion of clay does not exceed 33 per cent.

PHYSICAL NOTES.

CERTAIN MODIFICATIONS UNDERGONE BY GLASS.—J. Salleron often meets with well made thermometers, the indications of which are erroneous to 8° or 10°, or more. Such changes occur at printing ink works, where oils are heated for several days to 270°; in glycerin works, and with rectifiers of benzol. Glass is not merely modified when heated to 300°; it undergoes true deformations at far lower temperatures. Thus the hydrometers used in sugar works, which are often exposed for a considerable time to temperatures of 95°, are affected. After an immersion of some days they are completely modified; their weight decreases, and they become erroneous to the extent of 7° or 8° B.

THE MAGNETIC APPARATUS OF M. EDARD.—Among other electric or magnetic appliances for the treatment of various diseases is mentioned a magnetic sand, which M. Edard imports from the Isle of Bourbon, and which has been subsequently found near Morbihan. Its application is said rapidly to revive diseased plants.

SPECTROSCOPIC STUDIES OF THE SUN, CONDUCTED AT THE OBSERVATORY OF PARIS.—L. Thollon asserts that the sun has entered on a period of activity, and he has described and figured certain luminous protuberances, to one of which he ascribes a height of more than 100,000 kilometres.

Dr. J. H. Gladstone read a paper "On the Specific Refraction and Dispersion of Isomeric bodies" before the Physical Society of London. He concluded that the dispersion of a body containing carbon of the higher refraction, is very much greater than that of a body containing carbon of the normal refraction (5), and that isomeric bodies which coincide in specific refraction coincide also in specific dispersion.

ULTRA-VIOLET RAYS.—J. L. Shönn has ascertained the position of the ultra-violet rays of the spectra of cadmium, zinc, thallium, calcium, indium, magnesium, iron, and aluminium. His apparatus is well adapted for the study of absorption spectra; a column of water of 10 centimetres contained between two plates of quartz absorbs the greater part of the ultra-violet rays, whilst a block of very pure ice of 21 centimetres does not sensibly absorb the rays of cadmium in this portion of the spectrum.

BOOKS RECEIVED.

ZOOLOGY FOR HIGH SCHOOLS AND COLLEGES, by A. S. Packard, Jr., M.D., Ph.D. Second edition revised. Henry Holt & Company. New York. 1880.

The second edition of Professor Packard's manual of Zoology supplies a want that has been long felt among Naturalists, for a work of convenient size and form on this subject, with ample illustrations judiciously selected.

In compiling his book the author has freely used the larger works of Gegenbaur, Huxley, Peters and Carus, Claus, Rolleston and others, and even paraphrased or adopted the author's language *verbatim* when it suited his purpose.

In order to secure a greater accuracy of statement, and to render the work more authoritative as a manual of Zoology, Professor Packard has submitted the manuscript of certain chapters to naturalists distinguished by their special knowledge of certain groups. Thus the manuscript of the Sponges was read by Professor A. Hyatt; of the Worms and Mollusca, by Dr. Charles S. Minot; of the Echinoderms, by Mr. Walter Faxon; of the Crustaceæ, by Mr. J. S. Kingsley; of Fishes, by Professor T. Gill, whose classification, as given in his "Arrangement of the Families of Fishes," has been closely followed, his definition being often adopted word for word. The manuscript of the Batrachians and Reptiles was read by Professor E. D. Cope; of Birds and Mammals, by Dr. Elliott Coues, U. S. A.

The work being thus the joint production of so many eminent naturalists, it may be considered a thoroughly reliable guide to the advanced student who desires a general review of the animal kingdom, covering the most advanced teachings up to the date of publication.

The illustrations to this work, five hundred and fifty in number, is one of its most attractive features, and the author acknowledges his obligations to the publisher for his liberal co-operation in producing them. A fair proportion are original. We notice that Dr. C. S. Minot has drawn the full-page illustrations of the typical vertebrates, and that Mr. J. S. Kingsley and Professor W. K. Brooks contributed drawings of the nervous system and otocyst of the clam, while acknowledgment is made to Professor F. V. Hayden, Professor S. F. Baird and others for assistance given.

The work is presented in a handsome, large 12 mo. volume of over 700 pages, printed in large type and on excellent paper.

In regard to the manner in which the subject is treated and the general scope of the book, Professor Packard has designed a work to be used quite as much in the laboratory, or with specimens in hand, as in the class-room. He states that if Zoology is to be studied as a mental discipline, or even if the student desires simply to get a genuine knowledge at first hand of the structure of the leading types of animal life, he must examine living animals, watch their movements and habits, and finally dissect them, as well as study their modes of growth before and after leaving the egg or the parent, as the case may be. But the young student in a few weeks' study in the laboratory cannot learn all the principles of the science. Hence he needs a teacher, a guide, or at least a manual of instruction. This work, which is an expansion of a course of lectures for college students, has been prepared also to suit the wants of the general reader who would obtain some idea of the principles of the science as generally accepted by advanced zoologists, in order that he may understand the philosophical discussions and writings relating to modern doctrines of biology, especially the law of evolution and the relations between animals and their surroundings.

Such is the programme of the author of this book, and we congratulate him on the practical and exhaustive manner in which he has carried it out. The inductive method has been selected, and the student is first presented with the facts; is then led to a through study of a few typical forms, taught to compare these with others, and finally led to the principles or inductions growing out of the facts. He is not assailed with a number of definitions or diagnoses applicable to the entire group to which the type may belong before he has learned something about the animals typical of the order or class; but these are placed after a description of one or a few examples of the group to which they may belong. The simplest, most elementary forms are first noticed, beginning with the Protozoa and ending with the Vertebrates, believing that this is the more logical and philosophical method, and that in this way the beginner in the science can better appreciate the gradual unfolding of the lines of animal forms, which converge towards his own species, the flower and synthesis of organic life.

Professor Packard concludes the above explanation of the general plan of the work by advising the student to commence with Chapter VIII., on Vertebrates, and to master, with specimen in hand, the description of a frog, in order that he may have a standard of comparison, a point of departure, from which to survey the lower forms.

The concluding chapters of the work relate to the comparative anatomy of the organs, the development and metamorphoses of animals, the geographical distribution and geological succession of animals, the origin of species, man's place in nature, instinct and reason in animals. These subjects are lightly touched on, and the problems involved sketched in outline only, the author referring the reader to the works of specialists who have given these matters exhaustive consideration.

Professor Packard has been long prominently identified with practical scientific work covering this department of science, and his present work can be accepted without hesitation as an authoritative manual on the subject. We have read this manual of Zoology with peculiar satisfaction, because it is illustrated by our own more familiar natural objects. The first steps of the student of Zoology are plainly set forth, and by the aid of excellent wood engravings and intelligent descriptions, the various forms of life from the lowest to the highest are made clear to his understanding.

We take pleasure in advising students of Zoology to make use of this work as the best guide that can be secured, and the general reader may study it with advantage, for it treats of a subject of interest to us all, and deals with problems of the highest importance to mankind.

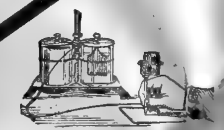
We have received a copy of the Proceedings of the Iowa Academy of Sciences, which covers a report of the work done from its organization in 1875 up to June, 1880.

The President of the Academy is Professor Charles E. Bessey, M. Sc., Ph. D., author of the recently published Manual of Botany, and a Professor in the Iowa Agricultural College.

We recognize among the list of Fellows, many names which are familiar to us, as specialists in various departments of Science, and we regard the Academy as being fortunate in possessing so strong an organization.

At the annual meeting in June last, a series of valuable papers were read, and we regret that the abstracts presented in the report are too brief to enable us to reproduce them with advantage.

We hope to be able to publish later, comprehensive abstracts of papers read before this Academy of Sciences.

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COOLER

SCIENCE:

A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.

Vol. I, No. 25. - - - - - - - - December 18, 1880.

CONTENTS.

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SCIENCE

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

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A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS

ILLUSTRATED.

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SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

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NOTICE TO SUBSCRIBERS.

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PROFESSOR TAIT, in a recent number of *Nature* (Nov. 25, 1880), directs attention to the necessity of perfect definiteness of language in all scientific work. "Want of definiteness," he says, "may arise from habitual laziness, but oftener indicates a desire to appear to know, where knowledge is not."

It is also claimed that scientific writers, even of the present day, have not that clear comprehension, which is essential, of what is subjective and that which is objective, and thus much confusion arises. To use Professor Tait's own language, our only source of information in physical science is the evidence of our senses. To interpret truly this evidence, which is always imperfect and often wholly misleading, is one of the tasks set before reason. It is only by the aid of reason that we can distinguish between what is physically objective and what is merely subjective. Outside us there is no such thing as noise or brightness; these no more exist in the aerial and ethereal motions, which are their objective cause, than does pain in the projectile which experience has taught us to avoid. To arrive at the objective point of Professor Tait's article, we may state that it involves a disagreement between himself and Mr. Herbert Spencer, as to the real meaning of certain words, and the propriety of making use of them on occasions which are mentioned.

In one of his works, Mr. Spencer states that, 'Evolution is a change from an indefinite, incoherent homogeneity, to a definite heterogeneity, through continuous differentiations and integrations."

Mr. Kirkman translates the foregoing into "plain Enlish," or as Professor Tait rather profanely asserts, "strips it of the tinsel of high flown and unintelligible language," thus:

"Evolution is a change from a nohowish, untalkaboutable, all-alikeness, to a somehowish and in-general talkaboutable not-all-alikeness, by continuous somethingelsifications and sticktogetherations."

Mr. Spencer claims that the explanation of the meaning of the word "Evolution" is a formula. Professor Tait calls this "a definition;" hence the difference of opinion, the latter asserting it to be not a mere quibble of words, but that an important scientific distinction is involved, to which the attention of the scientific world is directed.

The perusal of a communication from Professor Asaph Hall, of Washington, which will be found in this column, will greatly assist those who desire to solve the question. Professor Hall does not enter into any details of the controversy, but offers "*an illustration*" which appears to strike at the root of the matter in dispute.

We think that Mr. Spencer may rest satisfied with applying the term "*definition*" to his form of words, for by the rule presented by Professor Hall, it is evidently straining a point to assert that in them we find "a *formula*," using that word in the same sense as when we speak of the law of gravitation.

By the law of gravitation astronomers are able to predict the positions of known celestial bodies four years before the event, and Professor Tait asks if Mr. Spencer, with his "formula," can predict, four years before hand, the political and social changes which will happen in the history of Europe.

AN ILLUSTRATION.

In regard to the controversy between Professor Tait and Mr. Herbert Spencer, I beg to offer the following illustration. If we take by chance the three numbers 11, 12, 13, and form their squares, we have

$$(11)^2 = 121$$
$$(12)^2 = 144$$
$$(13)^2 = 169$$

Now take the numbers with the figures in an inverted order, and we have,

$$(11)^2 = 121$$
$$(21)^2 = 441$$
$$(31)^2 = 961$$

We see that the figures of the squares are also inverted; and this holds in the case of three consecutive numbers. We infer therefore that this is a general law in the formation of square numbers. Arguments of this kind might have an extended application in various branches of science; but if we make further examination we soon find numerous exceptions to our law, and we conclude finally that, although in the common phrase there may be something in it, yet our assumed law is in fact no law at all.

Again I examine my table of squares, and I find a rule of this kind: The second differences of the squares are constant, and equal to 2. I make many trials of this rule and never find an exception. Others do the same and always the same result is found. We conclude therefore that we have at length discovered a real law that exists in the formation of squares; but at the same time we invite every one to make the examination for himself, and if possible to find an exception. A. HALL.

Washington, D. C., December 17, 1880.

PROFESSOR TAIT AND MR. HERBERT SPENCER.

In another column we have referred to the controversy between Professor Tait and Mr. Spencer. Since this was put in form we have received a copy of Mr. Spencer's reply and, with pleasure, give his own explanation, which appears in *Nature* of the 2d instant:

"I pass now to his implied judgment on the formula, or definition, of Evolution. And here I have first to ask him some questions. He says that because he has used the word 'definition' instead of 'formula,' he has incurred my 'sore displeasure and grave censure.' In what place have I expressed or implied displeasure or censure in relation to this substitution of terms? Alleging that I have an obvious motive for calling it a 'formula,' he says I am 'indignant at its being called a *definition*.' I wish to see the words in which I have expressed my indignation; and shall be glad if Prof. Tait will quote them. He says—'It seems I should have called him the *discoverer of the formula!*' instead of 'the inventor of the definition. Will he oblige me by pointing out where I have used either the one phrase or the other? These assertions of Prof. Tait are to me utterly incomprehensible. I have nowhere either said or implied any of the things which he here specifies. So far am I from consciously preferring one of these words to the other, that, until I read this passage in Prof. Tait's lecture, I did not even know that I was in the habit of saying 'formula' rather than 'definition.' The whole of these statements are fictions, pure and absolute.

"My intentional use of the one word rather than the other, is alleged by him *àpropos* of an incidental comparison I have made. To a critic who had said that the formula or definition of Evolution 'seems at best rather the blank form for a universe than anything corresponding to the actual world about us,' I had replied that it might similarly be 'remarked that the formula—"bodies attract one another directly as their masses and inversely as the squares of their distances," was at best but a blank form for solar systems and sidereal clusters. Whereupon Prof. Tait assumes that I put the 'Formula of Evolution alongside of the Law of Gravitation,' in respect to the definiteness of the provisions they severally enable us to make; and he proceeds to twit me with inability to predict what will be the condition of Europe four years hence, as astronomers 'predict the positions of known celestial bodies four years beforehand.' Here we have another example of Prof. Tait's peculiarity of thought. Because two abstract generalizations are compared as both being utterly unlike the groups of concrete facts interpreted by them, *therefore* they are compared in respect to their other characters.

"But now I am not unwilling to deal with the contrast Prof. Tait draws; and am prepared to show that when the conditions are analogous, the contrast disappears. It seems strange that I should have to point out to a scientific man in his position, that an alleged law may be perfectly true, and that yet, where the elements of a problem to be dealt with under it are numerous, no specific deduction can be drawn. Does not Prof. Tait from time to time teach his students that in proportion as the number of factors concerned in the production of any phenomenon becomes great, and also in proportion as those factors admit of less exact measurement, any prediction made concerning the phenomenon becomes less definite; and that where the factors are multitudinous and not measurable, nothing but some general result can be foreseen, and often not even that? Prof. Tait ignores the fact that the positions of planets and satellites admit of definite prevision, only because the forces which appreciably affect them are few; and he ignores the fact that where further such forces, not easily measured, come into play, the previsions are imperfect and often wholly wrong, as in the case of comets; and he ignores the fact that where the number of bodies, affecting one another by mutual gravitation, is great, no definite prevision of their positions is possible. If Prof. Tait were living in one of the globular star-clusters, does he think that after observations duly taken, calculations based on the law of gravitation would enable him to predict the positions of the component stars four years hence? By an intelligence immeasurably transcending the human, with a mathematics to match, such prevision would doubtless be possible; but considered from the human standpoint, the law of gravitation, even when uncomplicated by other laws, can yield under such conditions only general and not special results. And if Prof. Tait will deign to look into 'First Principles,' which he apparently prides himself on not having done, he will there find a sufficient number of illustrations showing that not only other orders of changes, but even social changes, are predictable in respect to their general, if not in respect to their special characters."

REVERSION IN FLORAL PARTS.

By WILLIAM A. BUCKHOUT.

One of the best plants for showing the reversion of floral parts to the form of leaves is the common red field-clover (*Trifolium pratense.*)

It is always easily obtained, and during the fall of the year these heads of reverted flowers are quite common. The pedicels of the flowers are much elongated, and somewhat reduced in number; hence the heads have a loose appearance, which, with their very leafy look and absence of color, makes them conspicuous among

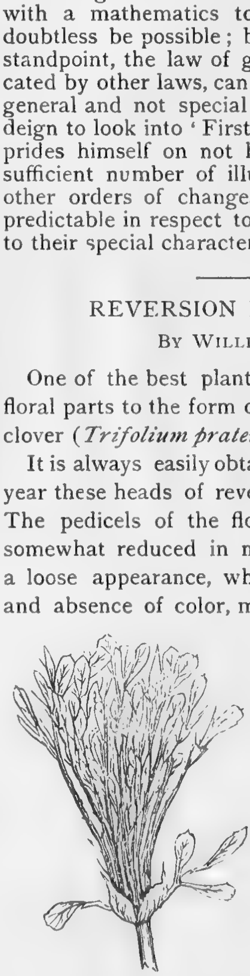

FIG. 1.

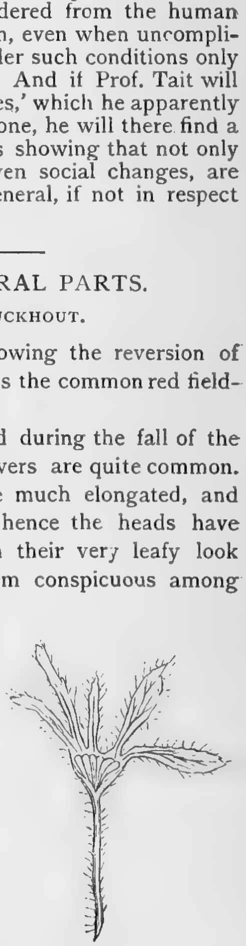

FIG. 2.

hose having well developed flowers. Fig. 1 gives a fair idea of one of these heads. A dissection of a flower shows that all parts have changed, and are decidedly leaf-like, though not to the same extent. Of the sepals (Fig. 2.) two are larger than the others, are very distinctly veined, and have a few small teeth near their tips; the remaining sepals are narrow, elongated, and have only the midrib without any lateral veins. The petals have lost their papilionaceous character entirely, though the vexillum may be recognized by its larger size. Each petal (Figs. 3, 4.) is leaf-like in shape, veining, and especially in the possession of a pair of stipules which are fused with its base precisely, as are the stipules of the leaf proper. The petals project but slightly from the tube of the calyx.


Fig. 3.


Fig. 4.

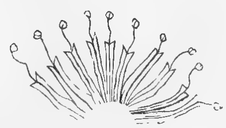
Fig. 5.

The stamens (Fig. 5.) are not diminished in number, but are separate, and each filament bears the stipules distinctly. They are joined with it nearly to the anther. This would seem to indicate that the sheath of united stamens in the *Leguminosæ* is made by the fusion of the stipular elements of the leaf alone.

Within the stamens, and occupying the centre of the flower, is a single, rather long-stemmed leaflet, apparently the middle one of the three so characteristic of the trifoliums. It is unmistakably a leaf in its veining, outline, color, etc., and upon its petiolar portions are borne —as might be expected—the stipules; in this case as plainly stipules as those which are borne by the true foliage leaves. No trace of a pistilline nature is to be seen. The reversion has been complete. All the parts, except the stamens are exceedingly hairy.

The peculiar feature in this case is the retention of the stipules as separate parts in all the whorls, excepting the calyx, where they are undistinguishably fused to form the cup-like portion of that organ.

The ease with which these reverted flowers can be obtained and studied, and the light which they throw upon the morphology of the parts of the flower make them worthy the attention of students who ought, as soon as possible, to gain a practical knowledge of the real nature of floral parts.

A demonstration in mathematics could not be more conclusive than this lesson from *Trifolium pratense*, our familiar red clover.

Pennsylvania State College, Dec. 20, 1880.

THE CLASSIFICATION OF SCIENCE.

By Rev. Samuel Fleming, LL. D., Ph. D.

I.

DEFINITIONS.

The term science has been variously defined. It is from the Latin *scientia* (from *scio*, I know,) which is defined as "a knowing, or being skilled in anything; generally, knowledge, science." The original sense of the term *scientia* involves the twofold conception, of the *thing*, or fact itself, which is the subject of knowledge, and the *knowing* the fact. The former is the *objective* signification, the latter the *subjective*. In defining the term, therefore, diverse forms of expression have been used, and different senses conveyed. In the edition of Webster's Unabridged Dictionary, published in 1878, modified definitions are given as follows: "Knowledge; the comprehension of truth or fact; truth ascertained; that which is known; hence, specifically, knowledge duly arranged, and referred to general truths on which it is founded." By some, the definition given is "systematic knowledge"; by others, "what is comprehended by the mind"; another definition is in the following language: "Science is the name for such portions of human knowledge as have been more or less generalized, systematized and verified." Herbert Spencer gives the following, corresponding with the general divisions of his "Classification of the Sciences": 1. That which treats of the forms in which phenomena are known to us; 2. That which treats of the phenomena themselves. Prof. Tice, after stating that "there is a broad distinction between knowledge and science," gives this distinction in the following terms: "Knowledge is a clear and certain perception of that which exists, or of truth or of fact. Science is a body of general principles: particular truths, and facts, arranged in systematic order."

The terms science and knowledge have sometimes been used as synonymous; frequently without due discrimination. It is evident that the facts of science, if not science itself, exist prior to, or irrespective of the mind which acquires the knowledge of them, if we except the science of the mind itself. Existence is one thing, the knowledge of such existence is radically another thing. Hence the propriety, and often great importance of recognizing this distinction, and of discriminating in the use of the terms. Scientific terms should be used with definiteness of meaning, for clearness and conciseness of written or oral instruction. If science and knowledge are synonymous terms, if the definition "science is knowledge" is the same with the terms transposed, thus "knowledge is science," every child and uneducated person who knows that "fire burns," is a scientist, without, it may be, knowing what fire is, or its causes. Then science would signify no more than knowledge. But all fundamentally distinctive ideas are appropriately expressed by different terms. And it is desirable that the demands of language be recognized, and this practical rule for the use of discrimniating words be observed. Synonymous words are properly those which are derived from different languages, and are used for euphony, or variety.

Further, there is a legitimate distinction between common, obvious, or non-scientific knowledge, and scientific knowledge. And this is not a distinction in respect to certainty; for common knowledge is often as certain as scientific knowledge, as in reference to the fall of a body to the earth: while much that is called scientific knowledge is far from being exact in its complete sense, as in respect to the nature of the ultimate cosmic forces, the aurora borealis, and other phenomena. Nor is it a difference simply in degree of knowledge, but a difference also in respect to kind and quality. Thus two persons may observe an eclipse of the sun or moon; one may know only that one body intercepts the light of another body; the other person may know the causes, the sizes, the distances, orbits, periodic times, laws of motion, and many other elements whose knowledge is essential to the determination of the phenomenon. The attainments respectively differ,—the former having only the knowledge of a single fact, the latter the knowledge of the whole system of facts, principles and laws pertaining to the phenomenon; the former possessing ordinary knowledge, the latter scientific knowledge. The distinction is therefore fundamental, and should be recognized as really as other differing facts.

These may be regarded as extreme cases, and it may be said that the point of transition, or the boundary line between the non-scientific and the scientific may not be clearly determined. Be it so; the claims of science require exactness of knowledge to the extent to which the exactness may be obtained by observation or reasoning, and to which the facts themselves fix the standard. And though perfection is the standard and aim, the knowledge of a sufficient number of related facts constituting a systematic knowledge, or knowledge sufficiently "generalized, systematized, and verified," for the comprehension of the relations and laws pertaining to such facts, may be received as evidence of scientific attainment, and capacity for intelligent progress. A man may possess a practical knowledge of carpentry, by which he may perform work, when a frame is "laid out," without the scientific knowledge of the principles, rules or methods by which such work is planned; much less without the higher mathematical and mechanical knowledge of architecture.

Another point of distinction claims attention. The term science is used in both a general, and a special or restricted sense. Either the whole body or aggregate of facts throughout the whole range of phenomena, relations, laws and applications, is referred to comprehensively, as "The classification of all science"; or, a branch or sub-science is referred to specially, as "The Science of Chemistry." Frequently a special science is recognized by the form of statement implying that to which reference is made, the term science being used by metonomy for *a* science, or a particular branch of general science, thus: "Science [chemistry] teaches that all masses of matter are made up of elements which had previously been isolated or separate." Or this: "Science [the science of the conservation of the forces] teaches that a certain quantity of heat may be changed into a definite quantity of mechanical work; this quantity of work can also be re-transformed into the same quantity of heat as that from which it originated." It may be added that the term science is sometimes used in an indefinite sense, or without precision, as "a man of science",—one who possesses a wide range of knowledge.

These distinctions between knowledge and science, between non-scientific and scientific knowledge, and between the special and general significations of the term science, being recognized, it remains only to give such forms of definition as shall meet the requirements of the case. The following are believed to be sufficiently precise:

1. Science (special or particular) is a system of phenomena, principles, relations and laws pertaining to a special subject.

2. Science (general or universal) is the aggregate of special sciences.

Many attempts have been made to classify the various sciences. The conception that they are naturally related, intimately, or more remotely, having general or special connections, has led to such arrangement of these in departments and groups as has accorded with the fundamental principles upon which they have been conceived to be allied. And since science consists chiefly of the facts, phenomena, laws, and principles, material or immaterial, which pertain to being, or the forms in which being is known, it is evident that schemes of classification will be adopted according to the systems of philosophy maintained by those who construct them. All classification will hence be observed to conform in general principles of structure to one or another of the three following systems of philosophy with respect to existence, or entities, viz.: Spiritualism, Materialism or Dualism. The first, which includes Idealism, rejects the doctrine of material essence, mind only being held to be fundamental and real—the outer world only phantasmic or apparent, or as held by some, matter being a mode or manifestation of mind. The second rejects the doctrine of a spiritual entity—the mind or spirit being held to be a phenomenon of matter; force, life and mind being but properties, or special manifestations of matter. Both the above systems are monistic, one substance, or essence, only held to exist. The third maintains the real existence of both matter and spirit in essential connection, yet distinct and unlike, not only in essence, but in their laws of development and modes of action—two related yet diverse processes. This may be termed *Dualistic Realism*, in contradistinction to the *Monistic Realism* predicated of each of the two former systems above mentioned.

But so diverse and even contradictory, in important respects, are many of these schemes of classification, that the question may be asked with pertinence, is any unexceptionable classification possible? Indeed, it has been admitted by men of high scientific standing that the most perfect classification will contain some incongruities and minor imperfections; and that a system substantially correct may, notwithstanding, contain something which is artificial, or merely theoretic. An apparent incongruity may be explained by the fact that several of the sub-sciences bear relations to different and widely separated sciences as to their fundamental characteristics, as will be observed in the scheme of the writer of this article.

A few diverse schemes are here given to illustrate the fact that one's philosophy will determine his principles of classification.

The fundamental principle of Oken, a German philosopher, is, that "Mathematics is the universal science," and holding the transcendental idea that Mathematics is zero, equal to nothing (0), has constructed his scheme to embrace three general classes, viz.: 1. *Mathesis*, the doctrine of the whole; 2. *Ontology*, erroneously defined to be "the doctrine of the *phenomena of matter*," or what seems to be, consistently enough with his doctrine of Idealism; 3. *Biology*, all orders of life and mind. Included in class first he has two groups: 1st. *Pneumatogeny*, the doctrine of immaterial totalities; subjects arranged in the following order: Primary Art, Prim. Consciousness, God, Prim. Rest, Time, Polarity, Motion, Man, Space, Point, Line, Surface, Globe, Rotation. Group 2d, under the term *Hylogeny*, defined to be "the doctrine of material totalities," includes the following: Gravity, Matter, Ether, Heavenly Bodies, Light, Heat and Fire. Included in Ontology he has Rest, Centre, Motion, Line, Planets, Form, Planetary Systems, Comets, Condensation, Simple Matter, Elements, Air, Water, Earth, Mineralogy, Geogony, etc. Other divisions of this anomalous system are here omitted. The author has conceived of a phenomenal process, which is given under the term Ontology, but which, so far as it represents the facts, pertains to cosmogony. It will be observed, moreover, that the place of geogony, to represent a consecutive order, is at the point where the genesis of the earth is given, if it can be found. But this system is based upon the fundamental principle of mathematics, which, according to the author, is zero = 0; for, as it is assumed, "Mathematics is the universal science of forms without substance." Such a system of nothings, consisting of terms, names and propositions, without realities, may well be termed Idealistic Nihilism!

The philosophy of Hegel is founded upon the theory that the essence of the universe is a process of thought from the abstract to the concrete. His classification is based upon Logic, as its fundamental principle, instead of Mathematics, which is Oken's, with which it otherwise well corresponds. A quotation from President Hopkins, that "Classification is a law of *forces*, not a law of logic," may here be given as a sufficient answer to Hegel's principle.

The method of M. Compte, the author of "The Positive Philosophy," gives what he calls "The one rational order," as follows: Mathematics (including mechanics), celestial and terrestial physics, chemistry, physiology and social physics. In its general outlines, it is a near approach to the proper order; but, in its special application and interpretation, it is a statement of the philosophy contained in his celebrated work just referred to. In that he gives his theory in the following statements: "Our study of nature is restricted to the analysis of phenomena, in order to discover their laws, and can have nothing to do with their nature, or cause, or the mode of their production." The question is suggested, What is the province of philosophy, if not to explain such nature, cause, and mode of production? He opposes "all inquisition into the essence of things;" rejects all hypotheses of "electric fluids and luminous ethers which are to account for the phenomena of heat, light, electricity, and magnetism." He denies that there can be any such thing as internal observation of the mind, or any knowledge of the causes of phenomena. What does he mean by mind? and how does he know that there are other minds than his own, or what is so called, to study his Positive Philosophy! He defines *law* to be "a constant relation of succession or similitude," and ignores all causes operating in matter, and o course there are no such entities as force, life or mind, human or divine.

In his subdivisions and groups, many incongruities are found, the statement of which must here be omitted. The subject matter of concrete mathematics, which is composed of plane geometry and rational mechanics, he has stated to consist of space, time, motion, and force, whose nature, indeed, may not be inquired into. He undertakes to classify the science in the order of historic development, or progress, which cannot be substantiated. Thus, historically, geometry had advanced to a considerable degree of perfection before the invention of algebra; and chemistry had made considerable progress before geology and mineralogy had become strictly sciences; while many of the facts of zoology had been arranged in systematic order more than two thousand years before the laws or methods of the stratification of the rocks, including immeasurable periods of time, had come to be accepted, as against the almost universally received doctrine of a miraculous creation of "the heavens and the earth," in six literal days about six thousand years ago.

The method of Herbert Spencer, while ostensibly based upon the distinction between the abstract and the concrete sciences, really precedes in development upon the hypothesis of Materialistic Evolution. He classifies the sciences under three tables: 1. *Abstract Science*, which includes mathematics and logic. 2. *Abstract-Concrete Science*, which includes mechanics, meteorology, chemistry, heat, light, electricity, and magnetism. 3. *Concrete Science*, which includes astronomy, astrology and geogony. Evolved from the latter are those subjects which are contained in the two following branches; 1. Mineralogy, meteorology, and geology; 2. Biology, out of which evolves morphology, physiology, psychology, and sociology. It will be seen that the distinction between the abstract and the concrete sciences has involved inconsistencies and confusion. While mathematics is appropriately placed first in the order, inasmuch as its principles apply to the measure of content, which belongs to all things susceptible of measurement, especially to the physical, mechanical and chemical departments of science; and also, as numerical mathematics applies to organic being, social statistics, etc., logic pertains to the rational nature and cannot with propriety be placed below both inorganic and organic nature without involving the necessity of separating subjects which are necessarily affiliated, as empirical psychology and rational psychology are. Further, both mathematics and logic are both abstract and concrete, being founded in principles which are applied practically both to forms and things. The term abstract, which means to draw from, or separate, or that which is considered part from its related subjects, is more appropriately applied to some other sciences than those assumed; thus *Kinematics* is an abstract science, inasmuch as it is "motion considered apart from its causes."

In the second table, the sciences of the laws relative to bodies are given before the recognition of such bodies, as if anticipating them; yet these are given under a twofold term "abstract concrete," instead of being given as abstract. Thus, in giving the mechanical laws of solids and fluids before the supposed existence of these, is presumption, and we may well ask, how can there be laws of entities which as yet do not exist? for it should be observed, these material entities are expressed in the third table, and as being evolved from terrestrial elements, and included under the term theology. The scheme betrays the design of the classification. It seems evidently devised to exhibit, under the term "concrete," the evolution from matter and motion, of all the "totalities" included in this branch. According to this, matter and motion, in their redistribution, evolve the phenomena of force, life, and mind, while these entities, held as real by a true dualism, are regarded by Mr. Spencer as having no substantive existence, but only modes of motion manifested by matter, the only real existence, according to his philosophy. The author of this scheme proceeds upon the postulate that "The second and third groups supply the subject matter to the first, and the third supplies the subject matter to the second." Why not, then, begin with the subject matter, not simply including material phenomena, but the inherent force, and the laws of manifesting phenomena? He abhors a "serial" order, upon whatever scheme of philosophy, and combats M. Compte on this ground, yet has conveniently adopted it for his main purpose, as betrayed in his third table.

An extended criticism of his system of philosophy, and his classification of the sciences, is not intended in this paper. Such has been given by M. Lettre, Prof. Bain, and others.

Only one other scheme of classification by other persons than the writer of this, will here be given; it is that of Prof. Laurens P. Hickok, D. D., LL. D., who is the author of several profound philosophical works. He gives what he designates a "Rational Method of the Classification of all Science." His method includes two general branches or divisions: 1. Empirical or Inductive Science; 2. Rational or Transcendental Science. These fundamental divisions are clearly defined. The first is limited to *facts* or phenomena; the second to *laws* and *principles*. The first embraces "what is given in experience," using the terms empirical and inductive to include observation and experiment. It is divided into two parts: 1. *Qualities* given in Perception; 2. *Things* given in Reflection; the former grouping external phenomena, as optics, acoustics, etc., the latter grouping things in space and time, including mensuration, substance, cause, counter-cause, chemistry, magnetism, mechanism. The second or rational branch is divided into, 1. Intuitive (all mathematics); 2. Discursive (all philosophy). "Mathematics deals only in forms; philosophy deals only in existences." Discursive science is divided into two parts. 1. *Ontology*, which includes cosmology, psychology, and theology. 2. *Deontology*, defined to be the rule of speculation, includes the canons of taste, (esthetics), politics, ethics, and religion. Cosmology is treated as including not only material nature, but physiology, now classified under biology. According to this scheme, therefore, man's physical nature belongs to cosmology, the term anthropolgy not being given as it is common with systems of philosophy.

The subdivisions of Dr. Hickok do not appear to be systematically arranged. His special field of thought does not embrace the sciences pertaining to inorganic matter, nor indeed to biology, but lies in the profound depths of transcendental philosophy held to be consistent with christian theism.

PERSONAL DANGER CONNECTED WITH ELECTRIC LIGHTING.

In a recent paper Mr. Swan, of Newcastle, says:

"While on the subject of alternating currents, I take occasion to remark on a letter of Mr. Preece in the *Times*, referring to the death of two persons, said to have occurred through their taking hold of the wires in connection with an apparatus supplying the current to Jablochkoff's candle. One of these cases occurred some time ago; the other was more recent. Now, admitting for the moment that these deaths occurred directly from the shock (which I consider by no means proved), I do not think that the extreme views put forth by Mr. Preece as to the dangers consequent on electric lighting in general can be supported, and for this reason:—The machine which supplies a Jablochkoff s candle gives alternating currents; the machines which supply the ordinary electric arc, which supply my lamps, and which are more *generally* used for lighting, give a current constant in one direction. Now, although the physiological effect of the alternating currents is undoubtedly severe, yet the effect of touching the wires from a direct-current machine is merely that you feel at the moments of making and breaking contact a slight shock, but while you have hold you feel almost nothing. [Mr. Swan afterwards demonstrated practically the harmlessness of the current by taking hold of the wires from the dynamo-electric machine for some minutes.] I think Mr. Preece, knowing how many real difficulties are connected with electric lighting, should hardly have added to these by magnifying to so great an extent the dangers which in some cases *may* accompany it."

REPORT OF THE DREDGING CRUISE OF THE U. S. STEAMER *BLAKE*, COMMANDER BARTLETT, DURING THE SUMMER OF 1880.*

By Alexander Agassiz.

The cruise was undertaken with the object of determining the exact relation of the fauna of the Atlantic Ocean to that met with in the Gulf of Mexico, and in the Caribbean Sea. In the Atlantic and Pacific oceans, deep-sea soundings have generally been made to a depth of 1500 fathoms; in the Gulf of Mexico, to a depth of 450 fathoms. Work was begun in June last, south of Cape Hatteras, on a line parallel to the coast, and at an average distance of about 120 miles from it.

Instead of finding a gently sloping sea-bed, as has heretofore been supposed to exist in these latitudes, the dredgers discovered, what proved to be, a continuation of the plateau, of which the northern portion is known to extend as far as Cape St. George, and of which the south-easterly limit is supposed to rest on the Bahama Banks. The western ledge on this plateau, was examined during last summer's cruise, and proved very interesting from a geological point of view. The eastern slope has not been traced as yet. Its exact limits is a matter of conjecture, but are to be determined in next year's cruise. The sides of this plateau are steep. Three ship's lengths from a point where a depth of 100 fathoms was reached, the sounding apparatus did not strike bottom until 450 fathoms of the line had been paid out. More animal life is found on the edge of the plateau than elsewhere. The character of the animals is, on the whole, the same as that of the species found in the Gulf of Mexico and the Caribbean Sea. The edges are composed of rich deposits of alluvia and mud, washed from the top of the plateau by the action of the Gulf stream, the course of which extends over the entire length of this Atlantic plateau. The deposits of numerous rivers flowing into the Atlantic Ocean serve to enrich the western slope. These conditions are all favorable to the preservation of animal life on the edges of this sub-marine highland, while on its top no animal life is to be met with, a certain species of coral formation excepted. Altogether the success, obtained by this expedition, was great. The same set of officers has served for three consecutive seasons. The same amount of work, which, in the course of the first year's cruise, required three months' time, during the past season has been accomplished in seven weeks. Work was continued day and night. The rapidity with which the soundings were made enabled eight dredges, each of them to the depth of 800 fathoms, to be made every twenty-four hours. Formerly, one deep-sea sounding was considered a good day's work.

* Read before the National Academy of Sciences, N. Y., 1880.

THE DURATION OF THE ARCTIC WINTER.*

By Leutenant F. Schwatka, U. S. N.

The generally received opinion, that the Arctic winter, especially in the higher latitudes, is a long dreary one of perfectly opaque darkness, is not strictly correct. In latitude 83° 20′ 20″ N., the highest point ever reached by man, there are four hours and forty-two minutes of twilight on December 22, the shortest day in the year, in the Northern Hemisphere. In latitude 82° 27′ N., the highest point where white men have wintered, there are six hours and two minutes in the shortest day, and it is in latitude 84° 32′ N., 172 geographical miles nearer the North Pole than Markham reached, and 328 geographical miles from that point, that the true Plutonic zone, or that one in which there is no twilight whatsoever, even upon the shortest day of the year, must be found. Of course, about the beginning and ending of this twilight, it is very feeble and easily extinguished by even the slighest mists, but nevertheless it exists, and is quite appreciable, on clear cold days, or nights, properly speaking. The North Pole itself is only shrouded in perfect blackness from November 13 to January 29, a period of seventy-seven days. Supposing that the sun has set (granted, the existence of a circum-polar sea, or body of water, unlimited to vision) on September 24, not to rise until March 18, for that particular point, giving a period of about fifty days of uniformily varying twilight, the Pole has about 188 days of continuous daylight, 100 days of varying twilight, and 77 of perfect inky darkness (save when the moon has a Northern declination) in the period of a typical year. During the period of a little over four days, the sun shines continuously on both the North and South Poles at the same time, owing to refraction parallax, semi-diameter and dip of the horizon.

SIGSBEE'S GRAVITATING TRAP*

By Alexander Agassiz.

Lieutenant-Commander Sigsbee devised this trap to ascertain the depth to which the animal fauna of the ocean descends. The extisence of animal life at great depths is extremely doubtful and this belief is confirmed by the fact that, whether dredging in 50 or 2000 fathoms of water, there is always brought to the surface the same species of animals. To secure water from different depths, Lieutenant-Commander Sigsbee constructed cylinders with traps, which could be opened from on board the vessel by lines, and which closed with the pressure of the surrounding water as soon as filled. They were found to sink 50 fathoms in 45 seconds. At the depth of 50 fathoms the trap brought to the surface the animals that usually float on the surface. At the depth of from 50 to 100 fathoms the number of animals decreased and only five species of pelagic forms were found, while seventeen species had been discovered at the former depth. Using every possible precaution the apparatus was next sunk in from 100 to 150 fathoms of water, but no animal life was found. The water was perfectly clear. The dead bodies of pelagi require from three to four days to sink in 1000 fathoms of water.

* Read before the National Academy of Sciences, N. Y., 1880.

DR. SIEMENS' ELECTRICAL FURNACE.

At a meeting of the Society of Telegraphic Engineers, Dr. Siemens gave the following description of his electrical furnace :

Amongst the means at our disposal for effecting the fusion of highly refractory metals, and other substances, none has been more fully recognized than the oxy-hydrogen blast. The ingenious modification of the same by M. H. Ste.-Claire Deville, known as the Deville furnace, has been developed and applied for the fusion of platinum in considerable quantities by Mr. George Matthey, F. R. S.

The Regenerative Gas Furnace furnishes, however, another means of attaining extremely high degrees of heat, and this furnace is now largely used in the arts—among other purposes, for the production of mild steel. By the application of the open hearth process, 10 to 15 tons of malleable iron, containing only traces of carbon or other substances alloyed with it, may be seen in a perfectly fluid condition upon the open hearth of the furnace, at a temperature probably not inferior to the melting-point of platinum. It may be here remarked that the only building material capable of resisting such heats is a brick composed of 98.5 per cent. of silica, and only 1.5 per cent. of alumina, iron, and lime, to bind the silica together.

In the Deville furnace an extreme degree of heat is attained by the union of pure oxygen with a rich gaseous fuel under the influence of a blast, whereas in the Siemens furnace it is due to slow combustion of a poor gas, potentiated, so to speak, by a process of accumulation through heat stores or regenerators.

The temperature attainable in both furnaces is limited by the point of complete dissociation of carbonic acid and aqueous vapor, which, according to Ste.-Claire Deville and Bunsen, may be estimated at from 2500° to 2800° C. But long before this extreme point has been reached, combustion becomes so sluggish that the losses of heat by radiation balance the production by combustion, and thus prevent further increase of temperature.

It is to the electric arc, therefore, that we must look for the attainment of a temperature exceeding the point of dissociation of products of combustion, and indeed evidence is not wanting to prove the early application of the electric arc to produce effects due to extreme elevation of temperature. As early as the year 1807, Sir Humphrey Davy succeeded in decomposing potash by means of an electric current from a Wollaston battery of 400 elements ; and in 1810 the same philosopher surprised the members of the Royal Institution by the brilliancy of the electric arc produced between carbon points through the same agency.

Magneto-electric and dynamo-electric currents enable us to produce the electric arc more readily and economically than was the case at the time of Sir Humphrey Davy, and this comparatively new method has been taken advantage of by Messrs. Huggins, Lockyer and other physicists, to advance astronomical and chemical research with the aid of spectrum analysis. Professor Dewar, quite recently, in experimenting with the dynamo-electric current, has shown that in his lime tube or crucible several of the metals assume the gaseous condition, as demonstrated by the reversal of the lines in his spectrum, thus proving that the temperature attained was not much inferior to that of the sun.

My present object is to show that the electric arc is not only capable of producing a very high temperature within a focus or extremely contracted space, but also such large effects, with comparatively moderate expenditure of energy, as will render it useful in the arts for fusing platinum, iridium, steel or iron, or for affecting such reactions or decompositions as require for their accomplishment an intense degree of heat, coupled with freedom from such disturbing influences as are inseparable from a furnace worked by the combustion of carbonaceous material.

The apparatus which I employ consists of an ordinary crucible of plumbago or other highly refractory material, placed in a metallic jacket or outer casing, the intervening space being filled up with pounded charcoal or other bad conductor of heat. A hole is pierced through the bottom of the crucible for the admission of a rod of iron, platinum or dense carbon, such as is used in electric illumination. The cover of the crucible is also pierced for the reception of the negative electrode, by preference a cylinder of compressed carbon of comparatively large dimensions. At one end of a beam supported at its centre is suspended the negative electrode by means of a strip of copper, or other good conductor of electricity, the other end of the beam being attached to a hollow cylinder of soft iron free to move vertically within a solenoid coil of wire, presenting a total resistance of about 50 units or ohms. By means of a sliding weight, the preponderance of the beam in the direction of the solenoid can be varied so as to balance the magnetic force with which the hollow iron cylinderis drawn into the coil. One end of the solenoid coil is connected with the positive, and the other with the negative pole of the electric arc, and, being a coil of high resistance, its attractive force on the iron cylinder is proportional to the electromotive force betweeen the two electrodes, or, in other words, to the electrical resistance of the arc itself.

The resistance of the arc was determined and fixed at will within the limits of the source of power, by sliding the weight upon the beam. If the resistance of the arc should increase from any cause, the current passing through the solenoid would gain in strength, and the magnetic force, overcoming the counteracting weight would cause the negative electrode to descend deeper into the crucible; whereas, if the resistance of the arc should fall below the desired limit, the weight would drive back the iron cylinder within the coils, and the length of the arc would increase, until the balance between the forces engaged had been re-established.

The automatic adjustment of the arc is of great importance to the attainment of advantageous results in the process of electric fusion ; without it the resistance of the arc would rapidly diminish with increase of temperature of the heated atmosphere within the crucible, and heat would be developed in the dynamo-electric machine to the prejudice of the electric furnace. The sudden sinking or change in electrical resistance of the material undergoing fusion would, on the other hand, cause sudden increase in the resistance of the arc, with a likelihood of its extinction, if such self-adjusting action did not take place.

Another important element of success in electric fusion consists in constituting the material to be fused the positive pole of the electric arc. It is well known that it is at the positive pole that the heat is principally developed, and fusion of the material constituting the positive pole takes place even before the crucible itself is heated up to the same degree. This principle of action is of course applicable only to the melting of metals and other electrical conductors, such as metallic oxides, which constitute the materials generally operated upon in metallurgical processes. In operating upon non-conductive earth or upon gases, it becomes necessary to provide a non-destructible positive pole, such as platinum or iridium, which may, however, undergo fusion, and form a little pool at the bottom of the crucible.

In this electrical furnace some time, of course, is occupied to bring the temperature of the crucible itself up to a considerable degree, but it is surprising how rapidly an accumulation of heat takes place. In working with the modified medium-sized dynamo-machine, capable of producing 36 webers of current with an expenditure of 4 horse-power, and which, if used for illuminating purposes, produces a light equal to 6000 candles, I find that a crucible of about 20 centimetres in depth, immersed in a non-conductive material, is raised up to white heat in less than a quarter of an hour, and the fusion of one kilometre of steel is effected within, say, another quarter of an hour, successive fusions being made in somewhat diminishing intervals of time. It is quite feasible to carry on this process upon a still larger scale by increasing the power of the dynamo-electric machine and the size of the crucibles.

By the use of a pole of dense carbon, the otherwise purely chemical reaction intended to be carried into effect may be interfered with through the detachment of particles of carbon from the same; and although the consumption of the negative pole in a neutral atmosphere is exceedingly slow, it may become necessary to substitute for the same a negative pole so constituted as not to yield any substance to the arc. I have used for this purpose (as also in the construction of electric lamps) a water pole or tube of copper, through which a cooling current of water is made to circulate. It consists simply of a stout copper cylinder closed at the lower end, having an inner tube penetrating to near the bottom for the passage of a current of water into the cylinder, which water enters and is discharged by means of flexible india-rubber tubing. This tubing being of non-conductive material, and of small sectional area, the escape of current from the pole to the reservoir is so slight that it may be entirely neglected. On the other hand, some loss of heat is incurred through conduction in the use of the water pole, but this loss diminishes with the increasing heat of the furnace, inasmuch as the arc becomes longer, and the pole is retired more and more into the crucible cover.

To melt a gram of steel in the electric furnace takes, it is calculated, 8100 heat units, which is within a fraction the heat actually contained in a gram of pure carbon. It results from this calculation that, through the use of the dynamo-electric machine, worked by a steam engine, when considered theoretically, 1 lb. of coal is capable of melting nearly 1 lb. of mild steel. To melt a ton of steel in crucibles in the ordinary air furnace used at Sheffield, from 2½ to 3 tons of best Durham coke are consumed; the same effect is produced with 1 ton of coal when the crucibles are heated in the Regenerative Gas Furnace, whilst to produce mild steel in large masses on the open hearth of this furnace, 12 cwts. of coal suffice to produce 1 ton of steel. The electric furnace may be therefore considered as being more economical than the ordinary air furnace, and would, barring some incidental losses not included in the calculation, be as regards economy of fuel nearly equal to the Regenerative Gas Furnace.

It has, however, the following advantages in its favor: 1st. That the degree of temperature attainable is theoretically unlimited. 2d. That fusion is effected in a perfectly neutral atmosphere. 3d. That the operation can be carried on in a laboratory without much preparation, and under the eye of the operator. 4th. That the limit of heat practically attainable with the use of ordinary refractory materials is very high, because in the electric furnace the fusing material is at a higher temperature than the crucible, whereas in ordinary fusion the temperature of the crucible exceeds that of the material fused within it.

Without wishing to pretend that the electric furnace here represented is in a condition to supersede other furnaces for ordinary purposes, the advantages above indicated will make it a useful agent, I believe, for carrying on chemical reactions of various kinds at temperatures and under conditions which it has hitherto been impossible to secure.

DESILVERIZATION OF LEAD BY THE ZINC PROCESS.*

By J. E. Stoddart.

The treatment of argentiferous leads with zinc, for the purpose of extracting the silver and refining the lead, is by no means a novel process. About twenty years ago a metallurgist named Parks took out patents for desilverizing rich leads by means of zinc, and a manufacturing firm adopted his process. They were, however, subsequently obliged to abandon it, in consequence of the difficulty experienced in the separation of the zinc from the concentrated silver, to admit of the cupellation of the latter metal. A German chemist named Flach afterwards took up the subject, and by running the alloy of zinc, silver, and lead along with iron slag, through a peculiarly constructed blast-furnace, was enabled to free the concentrated silver-lead from zinc. He also proposed the use of this furnace for removing of traces of zinc from the desilverized lead, but this was abandoned in favor of the ordinary improving or calcining pan. The operation with the blast-furnace was found to be very troublesome, and as the greater portion of the zinc was entirely lost, was by no means economical. M. Manes, of Messrs. Guillem & Co., Marseilles, who were the first to work Flach's process, found out and patented a simple means of treating the alloy, and recovering the zinc by distillation. This is the process now in use and known as the Flach-Guillem process, and which is carried on in the following manner:— About 18 tons of "rich lead," containing generally from 60 to 70 ounces of silver per ton, are melted in a large cast-iron pot, to which 1 per cent. by weight of zinc is added, and the whole well stirred for twenty minutes. The fires are drawn, and the contents allowed to settle and cool until the zinc rises to the surface, and forms a solid ring or crust containing the silver and other foreign metals. This alloy is removed to a small pot at hand, where part of the lead is sweated out, and the alloy thoroughly dried. The large pot with the lead now partially desilverized is again heated up, and treated in the same way as before, but with the addition of only a half per cent. of zinc, which when it has risen to the top is removed as before, and dried. A third addition of a quarter of per cent. of zinc is found necessary to take out the remainder of the silver, care being taken, on the cooling of this zincing, that all the crystals are cleanly skimmed off. The lead in the large pot is assayed, and found almost always to contain less than 5 dwts. of silver to the ton of lead; if it should happen to contain more, it is due to carelessness on the part of the workmen. The pot is now tapped, and the lead run down into an improving pan, where it is kept at a high heat for nearly eight hours, for the purpose of oxidising or burning off the small percentage of zinc which is left in it from the zincing process; after seven or eight hours' firing in this pan it should contain no trace of zinc. It is then tapped and run into moulds for market lead, or for the manufacture of lead products. The old improving pans were made of cast-iron, placed on a bed of sand, with a groove in the upper sides, which groove was filled with bone-ash to prevent the action of oxide of lead on the iron. These pans, from the giving way of the bone-ash, and the great wear and tear on the iron from the high heats necessary, were found to be both troublesome and expensive; they were very often under repair, and seldom lasted more than six or eight months. They have been superseded by an improving pan of cast-iron lined with brick inside. This pan, instead of being placed on a bed of sand, as was the case with the old improving pan, is hung on brick walls, and is quite open both below and round the outside. This new pan has been working in the patentee's works, Marseilles, for some years without any break down. It burns no more coal, and can be as economically worked in every way as the old pans. The zinc and silver alloy, after being dried, is melted in a plumbago crucible, covered on the top, well luted with fire clay, connected with a small cast iron receiver by means of a plumbago pipe, and fired up with coke. The zinc, distils over, and is condensed in the iron receiver. After all the zinc has been distilled, the pipe is disconnected, the cover removed, and the lead and silver, left in

* Read before the Philosophical Society of Glasgow, Nov. 8, 1880.

the crucible, is ladled out into moulds. Thence it is taken to the refinery, where it is cuppelled in the usual way. The block of metallic zinc recovered in the condenser is removed, and used over again in the first part of the process. All the oxide of lead and dross formed in the different processes are taken to the reducing-furnace, mixed with coal-dross, and reduced to the metallic state. The refuse from this furnace still contains some lead, and is put through the slag hearth, a blast furnace fired with coke, the fumes of lead oxides from which are condensed in what is known as Johnson's patent condenser, and are all recovered. The lead from the slag hearth, containing a number of impurities, as copper, antimony, iron, or sulphur, is taken to the improving furnace—a furnace built in exactly the same way as the dezincifying pan. About 20 tons of this lead are heated for a period generally from four or five days, but the time varies according to the amount of impurities present. The oxidised impurities, as they are formed, float to the surface, and are skimmed off by the workman, who is made to keep the lead perfectly clean, so as to have a fresh surface always exposed to the action of the flame. The dross skimmed off is first of a black color, but gradually becomes lighter as the operation goes on, until it shows nothing but yellow oxide of lead. When this appearance is noted the pan is tapped into moulds, or into the desilverizing pot, where it is treated with zinc, and the silver extracted as in the manner before described. By this process the lead can be desilverized and turned out in the shape of market lead in thirty hours from the time it is put in process ; the loss in working being not more than 1¼ per cent., and the amount of oxide of lead formed is very much less than that formed in any of the other processes, thereby effecting a very considerable saving in the working expenses. It makes an excellent quality of sheets, pipes, red-lead, and litharge, and has even been used for the manufacture of white-lead. There is, however, one product it cannot be used for by itself, and that is the manufacture of chemical lead. Your President gave us a very interesting paper on this subject last session, showing that the reason of its not being suitable for this was on account of its extreme purity. I understand that Mr. James Napier, Jr., of this Society, has made a number of experiments in the same direction, and found that by adding to it a small percentage of copper or antimony, or both, a good chemical lead can be obtained. That all the silver is thoroughly taken out may be seen from the fact that there is an excess of silver obtained on the large scale to the extent of nearly 2 per cent. over the assays. An analysis of the market lead gave—Antimony, 0.0015, and silver 0.0004 per cent., a trace of copper, but no iron or zinc ; from which it will be seen that the lead refined by the zinc process is almost chemically pure, and to this is due the finer quality of the products manufactured from it.

THE TERRESTRIAL PROGRESSION OF THE BRAZILIAN "CAMBOTA," *CALLICHTHYS ASPER.*

To the Editor of SCIENCE :—

Letters from Mr. John C. Branner, who was engaged upon the geological survey of Brazil under the late Prof. C. F. Hartt of Cornell University, contain extracts from letters to him from Mr. Joseph Mawson, Bahia, describing some habits of the siluroid fish, *Callichthys Asper*, there known as "*Cambota.*" These habits have probably been observed and described already, but as they are not referred to in Günther's Catalogue of the Fishes in the British Museum the account of a recent observer may be interesting to the readers of "SCIENCE."

"During the rainy season the fish live in fresh water pools. When the pools dry up in the dry season, they bury themselves in the mud and remain there until the rains return the following year. They are noted for overland excursions. It is said that they are often met with going from one pool to another.

I have had six of the fish in a narrow-necked tin of water, with some sand and mandioca meal at the bottom, for five days, and they continue active and vigorous, especially the smaller ones. These examples measure from 5 to 10 cm. in length, and I have seen them much larger. I have had them out in the garden several times. I find that they move best on smooth damp ground, and are embarrased by sticks or other inequalities. They can jump a little vertically, but their progress on land is effected entirely by a quick wriggling motion of the body which is nearly flat upon the ground. The paired fins (pectorals and ventrals) are extended laterally, and seem to bear little if any weight ; but they move slightly, and appear to serve to steady the body.

Last night I heard a peculiar sound, and on looking around I saw one of the fish travelling about the room. He had escaped from the tin which was in my bed-room, had fallen from the table to the floor, and travelled along the corridor, about 12 meters (about 40 feet) to the *sala*. I watched him travelling for two hours, during which time I estimate that he moved at least 90 meters. Toward the end of the two hours he seemed to flag a little, but in the earlier part his method and speed were fairly seen. He seemed to start with a sudden movement of the head or the barbels, then wriggled briskly for 5 to 10 seconds, advancing about a meter. Then he would rest for about 10 seconds, and start as before. This was kept up during the whole two hours, and I left him still moving. This morning, five hours later, I found him dead. While he was moving I spilled some water on the floor, but he crossed it ; hence I concluded that it was mud rather than water of which he was in search. The fish are eaten and considered good food."

It may be added that some examples of these fish were brought me by Mr. Branner, and found to be the *Callichthys asper*. The species of the genus are easily recognized from the fact that the trunk is covered by only two rows of large scales, a dorsal and a ventral series.

The ability of *Callichthys* to withstand a somewhat protracted deprivation of water, which it shares with other fishes of South America and India, with the North American Ganoids *Amia* and *Lepidosteus*, and with some other Ganoids and Dipnoans, is probably accounted for by the observations of Prof. Jobert of Rio Janeiro, published in the *Annales des Sciences Naturelles*, sixth series, V. and VII.

ITHACA, Dec. 21, 1880. BURT G. WILDER.

ASTRONOMY.

A PROBABLE VARIABLE STAR.—On Nov. 25, Swift's Comet was compared with the star No. 4339 of Lalande, by Mr. Talmage at Mr. Barclay's Observatory, Leyton, the magnitude of the star being estimated 8, as it was also by Lalande. Argelander in the *Durchmusterung* gives it 6.4 and Heis made it a naked eye star 6.7, but erroneously identifies it with Lalande 4359. It escaped observation in the Bonn Zones and may be worth occasional examination as likely to prove an addition to our variable star list.—*Nature*.

WINNECKE suggests that Hartwig's Comet is identical with the comets of 1382, 1444, 1506, 1569 and that it therefore has a period of 62⅓ years.

THE asteroid picked up by Peters on Oct. 10, is identical with that discovered by Palisa on Sept. 30.

M. TRIPIER is expected to take charge of the Observatory of Algiers in April, 1881.

DR. COPELAND at Dunecht, using Prof. Pickering's device of a prism introduced between the eyepiece and objective of his telescope, discovered a small binuclear, planetary nebula. Its position for 1880 is R. A. 21h. 2m. 11.8s, Dec. 47° 22.2′ N.

Washington, December 23, 1880. W. C. W.

SWIFT'S COMET.

The following are two more positions of this comet. These were obtained by the aid of a ring micrometer. Nov. 20, 1880, R. A. 1h. 6m. 24s. : Dec. +54° 22′ 39″ : Time is 10h. 49.1m. Washington *m. t.* Dec. 5, 1880, R. A. 4h. 7m. 49.2s. : Dec.+48° 30′ 10″ Time is 9h. 49m. Washington *m. t.* I have also an observation of position for Nov. 7, which has not been reduced as I have not yet managed to find the position of a fifth magnitude star, to which the comet's position was referred. The star's position will soon be obtained.

Nashville, Tenn., Dec. 21, 1880. E. E. BARNARD.

NEW COMPANION TO γ FORNACIS.

Sir John Herschel entered this as No. 2161, of his Fifth Catalogue of Double Stars, by reason of a distant eleventh magnitude which he detected, at an estimated distance of 45″, in the direction of 169.4°. This star was measured by me in 1879 in connection with a series of observations of a class of stars given in "Smyth's Bedford Catalogue." Since then, in repeating the measure of the Herschel Star, I have discovered a much nearer component, which fairly entitles the large star to be classed as double. The new star is very faint, and a rather difficult object with the 18½-inch refractor of the Dearborn Observatory. This, however, is partly due to its low altitude in this latitude, it being 25° south of the Equator. The mean result of my measures of these companions on four nights is :—

A and B	P=144.4°	D=11.53″	1880.93
A and C	157.0°	48.85″	1880.68

I have estimated the new companion as thirteenth magnitude. This, it will be remembered, is in the Struve scale of magnitudes, which would make it very much smaller than Herschel's twentieth magnitude.

The place of the principal star for 1880 is :—

R A. 2h. 44m. 33s.
Decl. —25° 3″

S. W. BURNHAM.

CHICAGO, Ills., December 21, 1880.

To the Editor of SCIENCE :

Professor Winchell, in the last number of "SCIENCE," refers to what he supposes "to be some errors in the dates in the list of minor planets discovered by the late Professor Watson," viz. :

(133) Cyrene, discovered Aug. 14, 1873, *Am. Jour. Sci.* III., VI., 296.
(174) Phaedra, " " 8, 1877, " " " III., XIV., 325.
(175) Andromache, " Sept. 2, 1877, " " " III., XIV., 325.

In correcting these supposed errors Prof. Winchell has fallen into more grievous ones.

Owing to a misprint in the *Astronomische Nachrichten* I was led to record the date of the discovery of (133) as August 26; it should be August 16, vid. *Astron. Nach.* 82,241 *Am. Jour. Sci.* III., VI., 296.

(174) Phaedra was discovered September 2, 1877, vid. *Am. Jour. Sci.* III., XIV., 325. This date is given September 3 in *Circ. Berl. Jahr.* No. 76. September 2 is undoubtedly the correct date. The object discovered August 8 turned out to be (141) Lumen, vid. III., XIV., 429, *Circ. Berl. Jahr.* No. 76.

(175) Andromache was discovered October 1, 1877, vid. *Astron. Nach.* 91-127; also *Circ. Berl. Jahr.* No. 81. The object called (175) in *Am. Jour. Sci.* III., XIV., 325 was really (174) Phaedra, as is explained in *Circ. Berl. Jahr.* No. 81. AARON N. SKINNER.

U. S. NAVAL OBSERVATORY,
WASHINGTON, D. C., Dec. 22, 1880.

BOTANY.

PILOSITY AS A TERATOLOGICAL PHENOMENON.—Hitherto teratologists have considered undue pilosity, or the adventitous production of hair in plants, as a matter of minor importance, but M. Ed. Heckel, in a recent note to the French Academy, (*Comptes Rendus*, xci., p. 349), insists that there are certain phases of this sort of change in plants which have a higher significance than that of a simple variation. He proposes to divide the phenomenon into the following three categories :

(1.) *Physiological Pilosity*, which includes the formation of hairs, or the increase in number of these, on the parts of plants where they are normally present, or even entirely wanting. Such cases are oftenest seen when plants change their habitat from a wet to a dry soil. This sort of physiological adaptation takes place within quite narrow limits ; and it varies from glabrousness to pilosity unaccompanied by any alteration of specific characters.

(2.) *Teratological Pilosity*, which begins at the moment the specific habit is altered, and acquires its maximum when the modifications are profound enough to suggest the idea of a new species. A large number of conditions capable of producing nutritive troubles in plants may give rise to this peculiar phenomenon, which M. Heckel proposes to introduce into teratological literature under the distinctive term of "Deforming Pilosity" (*Pilosisme deformant*).

(3.) *Pilosity due to the Sting of Insects or to Organic Variations*, which is clearly distinguished from the former in being very localized (e. g. certain galls, the filaments of *Verbascum* with aborted anthers, etc.,) and which cannot change the habit of the species.

Of changes due to *deforming pilosity*, M. Heckel gives two prominent examples which he has studied, *Lilium Martagon*, L., and *Genista aspalathoides*, Lam. The alterations in the last named plant are so profound that its monstrous state has even been described by De Candolle as a species, under the name of *G. Lobelii;* while by Morris it has been regarded as a marked variety, and named by him var. *confertior.*

MICROSCOPY.

The remarks of the "Fellow of the Royal Microscopical Society," who so ably advises *The English Mechanic* on Microscopy , on the faulty construction of many forms of "Student's" microscopes, is well timed.

In regard to the system of getting as much as possible for the money, he says : "It is just this petty economy in the original outlay on a practical stand that cramps the student when he has acquired some manipulative dexterity. Dealers and manufacturers are, of course, driven to supply what is recommended by the 'authorities.' The continued refrain of 'cheapness, cheapness,' brings down the construction of the microscope until it has become (in far too many instances) the baldest tube, stage, mirror, objective, and eyepiece with which it is possible to view a speck of saliva on a slip of glass. This perpetual reduction of the finish and design of the microscope tends to exclude all the better opticians from supplying students' microscopes, for they cannot do justice to themselves when the price is to be cut down as it has been during the last few years. The consequence is that an enormous number of common French or German instruments has been imported into this country and America ; students have been 'set up' with these things, to discover later on, when they have become experienced enough to judge of such matters, that they have no market value except as lumber.

The severe competition, lately, has been mainly confined to the production of *low-priced* microscopes, *not* the production of an *efficient* instrument at a moderate cost ; the consequence appears to have been that manufacturers whose appliances are about equal to the task of making gas-fittings have been induced to enter the competition ; a model of stand has been placed before them which they have copied 'more or less ;' at any rate, the market has been glutted with what appears to be lacquered brass-work."

This is well expressed, and needs but one word in addition, as to the remedy: On this point we advise the microscopist to recur to the good, but old-fashioned plan, of gradually building up his microscope and its accessories. Let his money accumulate until he can purchase a first-class stand of a reliable maker, the adjustments of which will be reliable, and arranged to receive all necessary accessories as they are added. With such a base of operation, he will have nothing to retract, and every step will be one of progress.

In justice to some makers in America, it must be admitted that they have produced, recently, some moderately-priced instruments which are well finished; but there are also some students' microscopes, on the market, carelessly made, badly constructed, and unfit for scientific work.

As to objectives the writer in the *Eng. Mech.*, above referred to, says: "Large firms abroad, who are not opticians at all, and whose appliances are suited to the production of bull's-eye lenses, &c., have been induced to 'take up' with the microscope, and thus thousands of things called objectives have been floated that are a disgrace to microscopy. Here and there an advertiser of microscopes obtains these things, patches on some trumpery adapter that conceals the original make, and disposes of the wares as 'our own first-class manufacture;' the unwary student finds out how he has been imposed upon only when experience has taught him the meaning of good optical appliances, among which those he is unhappily possessed of take no rank whatever."

We have no doubt the writer has good reason for making this exposure of the tricks of opticians. The practice of importing objectives and, after remounting, passing them off as "*our own first-class manufacture*," is not confined to Europe. When in London, on one occasion, we were shown a written order from a well-known American objective maker, for a quantity of objectives, to be used for this very purpose.

It is certainly a disgraceful state of things that a microscopist, who purchases an objective of a reputable maker, should receive a glass manufactured by an inferior house, whose prices are probably 50 per cent less.

Purchasers of microscopes and objectives in the United States, who endeavor to steer a course between exorbitant charges and inferior workmanship, have need of much caution, and if inexperienced, should not rely on their own judgment.

The number of microscopists in this country appears to be on the increase if we may draw conclusions from the statement of a maker, who asserts that he has orders in hand which will keep him employed for four months.

PHYSIOLOGY.

Mr. Simon H. Gage has just been appointed Assistant Professor of Physiology, and Lecturer on Microscopical Technology in Cornell University. While a student in the Natural History course at that institution, Mr. Gage acted as laboratory assistant, and since his graduation, in 1877, has been Instructor in Microscopy and Practical Physiology. He has published several papers, mostly microscopical, some of which have been copied into European Journals. In addition to the supervision of other laboratory work, Mr. Gage gives practical lectures upon Microscopical Technology, in all its branches, and upon Microscopy in relation to Medical Jurisprudence. His deserved appointment will not only strengthen the general Natural History instruction, but greatly aid Professor Wilder's efforts to provide preliminary medical education.

The following list of the published papers of Mr. Gage will give some idea of his scientific activity, and indicate his special line of research:

1. Plaster of Paris as an Injecting Mass.—*American Naturalist*, November, 1878, pp. 717-724.
2. Notes on the Cayuga Lake Star Gazer.—*Cornell Review*, November, 1878, pp. 91-94.
3. The Ampulla of Vater and the Pancreatic Ducts in the Domestic Cat, Felis Domestica.—*The American Quarterly Microscopical Journal*, January, 1879. pp. 123-131, and April, 169-180.
4. Laboratory Notes in Microscopy.—*Am. Q. M. Jour.* Vol. I., pp. 71, 160, 166. Part of these were copied in the *Journal of the Royal Microscopical Society*, of London, 1879, p. 191. and also in the *American Journal of Microscopy and Popular Science*, 1879, p. 176.
5. The Inter-Articular Ligament of the Head of the Ribs in the Cat.—*Proc. of the Am. Association for the Advancement of Science*, Saratoga Meeting, 1879, pp. 421-424.
6. A New Method of Demonstrating the Thoracic Duct in Animals.—*Proc. A. A. A. S.*, 1879, p. 425.
7. An Apparatus for Photographing Natural History Objects in a Horizontal Position. Read before the A. A. A. S., at Saratoga, and published by title in the proceedings for 1879, p. 489.
8. Preparation of Ranvier's Picro-Carmine.—*American Monthly Microscopical Journal*, 1880, pp. 22-23. Copied in the *Jour. of the Royal Mic. Soc. of London*, 1880. pp. 501-502.
9. Permanent Microscopical Preparations of Amphibian Blood. Read at the Boston Meeting of the A. A. A. S., and published in the *American Naturalist*, October, 1880, pp. 752-753.
10. Permanent Microscopical Preparations of Plasmodium. Read at the Boston Meeting of the A. A. A. S., and published in the *Am. M. Mic. Jour.*, October, 1880, pp. 173-174.
11. A supplement to the article on calcareous crystals in Amphibia, by Professor Bolton, of Trinity College. This supplement was prepared at his request, and published with his paper in the *Proc. of the A. A. A. S.*, 1879, p. 418.

Finally Dr. Wilder and Mr. Gage have been preparing a laboratory manual for the last two years, which will be published next fall.

For an opinion as to the value of the laboratory notes, etc., mentioned above, see the Proceedings of the New York Microscopical Society, as published in the *Am. Jour. of Mic. and Pop. Science.* Feb., 1880, p. 51.

CHEMICAL NOTES.

ULMIC MATERIALS PRODUCED BY THE ACTION OF ACIDS UPON SUGAR.—The formulæ ascribed by Mulder to the ulmic products which had been dried at from 140° to 165° before being submitted to combustion are not a dmissible, since, at temperatures above 100°, these bodies lose a notable quantity of volatile matter, and in particular of formic acid. The ulmic substances obtained by the action of dilute sulphuric acid upon sugar, and which may be called sacchulmine, appear in the form of minute yellowish brown globules. On treatment with a cold aqueous solution of caustic potassa, sacchulmine gives off an acid principle derived from the action of sulphuric acid upon glucose. The ulmic matter (sacchulmine), insoluble in cold alkaline liquids, is derived directly from saccharose. In the ulmification of sugar there is evolved a considerable quantity of volatile acids, especially formic acid.—F. SESTINI.—*Gazzetta Chimica Italiana.*

THE DIFFUSION AND THE PHYSIOLOGICAL CONDITION OF COPPER IN THE ANIMAL ORGANISM.—Prof Giovanni Bizio has attempted to prove that his father, Bartolomeo Bizio, was the original discoverer of the normal occurrence of copper in the animal economy.

CHEMICAL CONSTITUTION OF MILK.—Caseine is not a homogeneous albuminoid, but a mixture of albumen and protalb-bodies which appear as transition stages in peptonisation. In the milk globules has been found an albumenoid which constitutes the serum. In the curd are met with an albuminous body identical with the stromæi alb-compound of the globules, a body which Danilewsky and Radenhausen name orroproteine and two series of peptones. Hence it is no longer proper, in milk-analysis, to speak of caseine and albumen, but rather of albuminates.—DR. N. GERBER.—*Correspondenz-Blatt.*

OCCURANCE OF COPPER.—Dr. W. Hadelich has detected and determined copper in the soil of a churchyard, and in portions of exhumed bodies.

SIMPLE METHOD OF OBSERVING THE PHENOMENA OF DIFFRACTION.—The rays reflected by a heliostat are concentrated by two lenses. In the focus is placed a diaphragm with a very small aperture, and the luminous glass is received on a screen. In this glass are placed the bodies whose shadows are to be studied.—V. D. VORAK.

BOOKS RECEIVED.

FOUR LECTURES ON STATIC ELECTRIC INDUCTION, by J. E. H. Gordon, B. A., Assistant Secretary of the British Association—16mo, price 80 cents. D. Van Nostrand, New York, 1881.

These lectures, which were delivered before the Royal Institution of Great Britain during the early part of 1879, convey, in simple and clear language, an explanation of the laws of the induction of electricity, pointing out the problems connected with it, which have been solved, and what remains to be done in this direction.

About forty illustrations take the place of the lecturer's apparatus, and will be found a great aid to the reader in following the text. As a popular guide to a subject of great present interest, this little work, from so reliable a source, should be welcome. As the author admits, our knowledge of electricity is very incomplete; the question, What is electricity? still remains unsolved. Of the phenomena considered in these lectures, a few only can be explained, the experimental facts standing out alone and disjointed.

Many lines of reasoning and research open out a little way and then are lost in the darkness through which, as yet, human sight cannot pierce.

The magnitude of the experiments and the exhaustive researches of Edison are making these difficult ways clear and trodden paths, utilizing the disjointed facts and weaving them into one perfect and harmonious whole.

NATUREN.—Et illustreret maanedsskrift for populær Naturvidenskab, udgivet af Hans H. Reusch, cand. real.—Assistent ved den geologiske Undersögelse—Kristiania—Trykt hos A. W. Brögger. Vol. I, No. 2, 1880.

The gratification which attends success, must, in the case of the Editors of *Nature*, have been increased by finding that their journal has become the model for scientific weekly journals in other countries.

France, Germany and Italy have each their *Nature* published in their respective languages, and we have now to congratulate Norway on possessing an excellent scientific journal on the some model.

The cultivation of science in Norway is of recent date, the first efforts in this direction being contemporaneous with the foundation of the present constitutional monarchy in the year 1814, when the separation from Denmark took place. About this time also the first Norwegian University was organized.

The short time the constitution of Norway has existed appears sufficient to prove that political freedom and independence—if not absolute conditions—are at least powerful vehicles for the intellectual development of an energetic people.

As might be expected the strong and impulsive enthusiasm which arose from this political regeneration was not at first concentrated on the solution of scientific subjects, but the intellectual life thus created found expression in a more æsthetic tendency, and poets who then and later arose are remembered and appreciated, while the Norwegians still treasure the names of Welhaven, Wergeland, Björnson and Ibsen.

Of those Norwegians who have established a reputation in the field of science may be mentioned Professor Christopher Hansteen, known by his researches in Magnetism, and as an eminent mathematician. He died in 1873, and may be said to have been succeeded by Professors O. J. Broch, Sofus Lie and Bjerknaes.

Professor Michael Sars has done excellent work on the lower fauna of the country, and his son, Professor G. O. Sars, has written several important works on the subject.

In Botany honorable mention may be made of Professor N. M. Blytt, and in Geology we refer to Professor Sjur Saxe, who is the author of some admirable works on the glaciers and snowfields. Professor Th. Kjerulff is also a high authority on the same subject.

Among those who have contributed to the literature of Medicine we may name Professor W. Boeck, who died in 1873, and Dr. D. C. Danielsen.

Professor P. A. Munch, who died in 1863, established a high reputation by his historical works, and Professor Sofus Bugge's researches in respect to the ancient languages have been recorded in works which are much esteemed.

The present number of "*Naturen*" now before us, which was the second issued, is printed on good paper, and is well printed. The contents are somewha popular in character, the first article being one of a series on the five senses, entitled "*Synet*" [sight] with ten illustrations. The second article on "*Lungefiske*," [Lung fishes] is also illustrated with drawings of the *Lepidosiren paradoxa* and allied forms. The number concludes with minor articles of interest.

We understand "*Naturen*" will be well patronized and we wish the promoters of the paper every success.

NOTES.

A PATENT has been granted for an electro-magnetic rock-drill. A drilling tool is directly attached to the core of axial magnets and arranged to impart to said core a reciprocating motion. The current is shifted alternately to the coils.

AN application for a patent for the photophone was filed at Washington on the 28th of August, 1880, by Bell. The Patent Office *Gazette* of the 7th of December shows that the patent has been granted.

PHYSICO-CHEMICAL ANALYSES OF SOILS.—M. Pellegrini has compared the methods of Schlœsing, Nœbel, and Masure, and obtained such differences as clay, 37 and 87; sand, 1.5 to 28. He considers Schlœsing's method the most satisfactory.

The conclusion arrived at by G. Hauser, in regard to the organ of smell in insects is as follows: The organ of smell, in all the Orthoptera, Pseudoneuroptera, Diptera and Hymenoptera, also in a large part of the Lepidoptera, Neuroptera and Coleoptera, consists: 1. Of the antennal nerve. 2. Of a terminal perceptive apparatus consisting of rod bearing cells arising from hypodermic cells, with which a nerve-fibre connects. 3. Of an apparatus consisting of a pit or a cone filled with serous fluid which may be considered as simple infolds and projections of the epidermis.

Considerable encouragement to naturalists living in cities should be afforded by the amount of botanical work executed by Mr. L. P. Gratacap, on a few vacant lots, in the City of New York, known as Manhattan Square. A short time since the inequalities of the ground were filled up by earth which was carted in, the result being the introduction of an army of plants which soon covered the ground with a mantle of waving weeds. A careful examination of these plants showed them to be composed of 35 orders, of 99 genera, and 117 species.

M. Levoiturier, an entomologist, of Elbeuf, has communicated to the Société d'Acclimation the result of an enquiry as to Coleoptera found in wools from different parts of the world. The author's list is quite a long one, and it is stated that by its inspection the origin of a particular sample of wool may be ascertained, which knowledge is important, as the net return from wool, after scouring, varies greatly. The list comprises, for Australia, 47 species of insects; Cape of Good Hope, 52; Buenos Ayres, 30; Sapin, 16; Russia, 6.

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A WEEKLY JOURNAL OF SCIENTIFIC PROGRESS.

Vol. I, No. 26. - - - - - - - - December 25, 1880.

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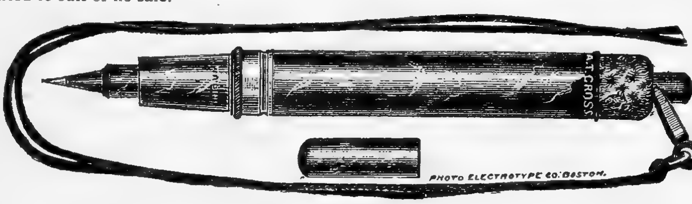

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We have studiously avoided occupying the pages of this journal with personal reference to its progress, but the conclusion of the first volume appears to be an appropriate moment to offer a few words on this subject, and to acknowledge our obligations to those, who, by their subscriptions or literary contributions, have aided in making "SCIENCE" a success.

A selection from the letters addressed to us by scientific investigators, approving of the management of the journal, or containing congratulations for the future, would fill the pages of this number; having only a column at our service, we make but one reference, which appears to epitomize all previous communications.

In a letter recently received from the last President of the Association for the Advancement of Science, Professor GEORGE F. BARKER, are these words:

"I take this opportunity to congratulate you on the success of "SCIENCE." The numbers which I have seen have been creditable to all concerned."

With this testimony to the standing of the journal from so severe a critic and one so eminently capable of forming an opinion of what a scientific journal should be, we might be well content to rest, but being fully aware that better results may be attained, our unremitting efforts in the future will be directed to secure a still higher standard, and more perfect development of the various departments.

We would remind specialists, who desire to see certain branches more fully represented in "SCIENCE," how much may be accomplished by individual efforts, As an instance, we may refer to our astronomical department, which already reflects the high attainments and *esprit de corps* of those following this line of research.

Twenty-six weeks only have elapsed since our first number was issued, of which period the first half was a time of recreation and rest, when few were within reach of our announcements; it is therefore a subject of congratulation that in so short a time our pages show a vitality indicative of a journal which has existed years rather than a few weeks.

It is a pleasure to find on our subscription list the names of so many representative scientific men, both of the United States and Canada, while all the leading Universities and Colleges of this country receive "SCIENCE" through members of their faculties. This journal is also now circulating in England, France, Germany and Italy.

We have placed several pages weekly at the service of the Smithsonian Institution, and are willing to increase the number of our sheets as the matter awaits publication. This offer has been so far accepted, that arrangements are in progress, by which the difficulty of compilation may be overcome.

We have demonstrated that a really scientific weekly journal can be conducted without conflicting with the old established monthlies, which have their special field of usefulness, and when the editor of the AMERICAN JOURNAL OF SCIENCE (which may be considered one of the institutions of this country) sent us his good wishes for success, we accepted his greeting in all sincerity.

England, France, Germany and Italy, have each their representative weekly scientific journal. Is there an American scientist with so poor an opinion of the scientific work of his country, or of his fellow workers, as to think the same convenience for the United States superfluous?

"SCIENCE" has received a welcome and strong support far beyond the expectations of the editor; but it must be apparent to all that the personal co-operation of every scientific man, in the future, will not only insure the permanency of the publication, but increase its usefulness and value; let all, then, who are interested in the promotion and advancement of science in the United States, whether professionally engaged or as student, or amateur, feel that one and all are invited to assist in the work.

Those who are acquainted with journalism must be aware that the capital employed in this enterprise has been advanced for the best interests of science rather than for speculative purposes. The editor, therefore, in making this appeal for general co-operation, feels that he may do so without danger of misinterpretation of his motives,

SOME RECENT AMERICAN PAPERS IN COMPARATIVE ANATOMY.

Marsh, O. C.—The limbs of *Sauranodon*, with notice of a new species. *American Journal of Science*, Feb. 1880, pp. 169–171; 1 figure.

Morse, E. S.—On the Identity of the Ascending Process of the Astragalus in Birds with the Intermedium. Anniversary Memoirs of the Boston Society of Natural History, 1880; pp. 10, 1 plate, 12 figures.

Chapman, H. C.—The Placenta and Generative Apparatus of the Elephant. Journal of the Academy of Natural Sciences of Philadelphia, VIII, 1880. 4 plates, 1 figure, 11 pages.

Chapman, H. C.—On the structure of the Orang Outang. Proceedings of the Academy of Natural Sciences, 1880. 16 pages, 7 plates.

Among the many surprises which science owes to the paleontological discoveries of Professor Marsh, few are more notable than the condition of the limbs in *Sauranodon.* In the present paper Professor Marsh describes the limbs with some detail, and gives a figure of the left hind paddle of *S. discus.* In each limb the proximal segment consists of a single bone which undoubtedly represents the humerus or femur. The following four segments consist respectively of three, four, five and six approximately discoid pieces, which are interpreted as representing the bones of the antebrachium or crus, the two rows of the carpalia or tarsalia, and the metacarpalia or metatarsalia of the ordinary vertebrate limb.

Regarding the carpalia or tarsalia as constituting a single segment, Professor Marsh suggests the following general names for the corresponding segments in the two limbs: propodial, epipodial, mesopodial, metapodial, and phalangial; since the latter two terms have already been employed there seems to be no reason why the other three should not be accepted.

The figure seems to demonstrate the normal presence in this fossil reptile of six distinct digits or dactyls. "This is a character not before observed in any air-breathing vertebrate. Some of the Amphibians retain remnants of a sixth digit, and *Ichthyosaurus* often has, outside of the phalanges, one or more rows of marginal ossicles that probably represent lost digits. With these exceptions, the normal number of five is not exceeded."

This condition of things in *Sauranodon* is worthy of consideration in view of the not infrequent occurrence of *sexdigitism* with man and others of the higher vertebrates. Darwin had suggested that this anomaly might be due to reversion, but (The Descent of Man, 1, 120, note) afterward reluctantly abandoned the hypothesis in consequence of Gegenbaur's denial of the existence of more than the regular number of digits in the Ichthyopteriga. His original view is now strengthened by Professor Marsh's account of the limbs of *Sauranodon*, but does not yet serve to explain the occurrence of more than six digits with man, the cat, and perhaps other mammals.

The other striking peculiarity of the sauranodont limb is the presence of *three epipodial* elements. All of them articulate with the humerus or femur, and Prof. Marsh suggests that the intermediate one represents the *os intermedium* which, in most air-breathing vertebrates, is more closely associated with the mesopodial bones. He thinks its proper place is indicated in *Sauranodon*, but that, "in the process of differentiation this bone has been gradually crowded out of its original position."

In the paper cited Prof. Morse offers a different interpretation; "That the bone which he (Marsh) indicates as the intermedium is really the fibula, and the bone which he represents as the fibula is an outer tarsal bone which, with its metatarsal and phalangeal bone in series becomes obliterated in time; that, in the process of differentiation, the intermedium is as likely to be partially compassed by the distal extremities of the tibia and fibula, as that a third bone of this (epipodial) segment had been crowded down into the tarsal series." Pending the discovery of new facts in paleontology, embryology, or comparative anatomy, it is probable that most anatomists will be predisposed toward the view of Professor Morse.

Those who are interested in the general morphology of the vertebrate limb should not fail to read the suggestive facts and considerations presented by Prof. Huxley in his paper on Ceratodus, Proc. Zool. Soc. of London, Jan. 4, 1876.

Most of Professor Morse's paper consists in the presentation of facts in corroboration of the opinion advanced by him in 1872, that the intermedium is represented in most birds by the so-called "ascending process of the astragalus" which, in an embryo heron, had been found by the late Professor Jeffries Wyman to have a separate centre of ossification. Figures are given of the parts as they exist in several aquatic species, and there seems to be no reason for doubting the correctness of Professor Morse's conclusions. Our author also reproduces Cuvier's figure of the tarsal region of the "Honfleur Reptile," afterward named by Cope *Lælaps Gallicus*, and Cope's figures of the same parts of *Lælaps* and *Ornithotarsus.* He considers that the intermedium is distinctly represented as an ascending process with *Lælaps*, but is in doubt as to *Ornithotarsus*, whether it is "represented by the enlargement of the tibiale in front, or was a separate bone which occupied the fossa on the anterior face of the tibia." The manus of the sea-pigeon (*Uria grylle*) is figured to show the interesting presence of "rudimentary nails on the second and third fingers, (index and medius)."

Dr. Chapman has profited by the unusual opportunities afforded to a zealous anatomist by the extensive zoological garden of Philadelphia, and by the large menageries which sometimes have their winter-quarters in the same city, and the papers here cited contain important contributions to our knowledge concerning two forms whose structures and functions are far from thoroughly known.

A young Indian Elephant was born on the 9th of March, 1880, the gestation having lasted either twenty months and twenty days, or twenty-one months and fifteen days, according as it is dated from the last or the first of the seven observed opportunities for its commencement. "Immediately after birth the mother rolled the young one in the straw. The young elephant, a female, stood 30 inches (about 75 cm.) in height, measured from base of trunk to root of tail 35 inches (about 88 cm.), and weighed 213½ pounds (about 97 kilograms). It was perfectly formed and well-developed; it was noticed immediately that it sucked with the mouth, and not with the trunk, as Buffon reasoned it must do—an error so often repeated in works on Natural History.

Dr. Chapman was fortunate enough to obtain the fresh membranes, and to have them well injected. The figures and descriptions indicate that, as Turner had concluded from less perfect materials, the placenta of the elephant is *deciduous* as in the Primates and Carnivora, and *zonular* as in the latter group.

The generative apparatus of the female elephant presents some peculiar features, and although our author begins his concluding paragraph by saying, "it appears to me that there can be little doubt now that the generative organs in both species of elephant are understood," yet his admission, in a foot note, that what he had called vagina may be really an elongated *cervix uteri*, will lead other anatomists to avail themselves of any opportunity that may present in itself for further study of this portion of the proboscidean structure.

The anatomical account of the Orang is full of interesting facts and ideas, but most of them have been outlined already in No. 25 of this Journal. Like nearly all of the Orangs, whose brains have been examined, this example was young, estimated to be about three years old. The immaturity of the brain, together with the probability of considerable individual variation in the details of the cerebral fissures, should be taken into account in estimating the resemblances and differences with respect to man and the other anthropoids. Possibly these considerations may apply also to the somewhat mooted question as to the extent to which the occipital lobes of the cerebrum project over the cerebellum. Here, however, there enters another element than the distortion or displacement to which Dr. Chapman very properly refers, namely, the *position in which the brain is held or placed.* In Dr. Chapman's figures, the organ rests upon the medulla, and upon the ventral aspects of the frontal and temporal lobes; were it brought into something approximate to its natural position, or to the position of the human brain, the occipital lobes would surely project beyond the cerebellum to an appreciable extent. It would be well if the next Orang's head should be sawn into sections parallel with the mesial plane, and the brain figured *in situ.*

It is gratifying to find Dr. Chapman, like Humphrey and Barnard, insisting that the "scansorius" muscle of Traill is really the entoglutæus. But Dr. Chapman does not seem to have observed the curious little muscle passing over the capsule of the acetabulum which Prof. Barnard has called "ilio-femoralis subrectus," and which, in the opossum, Coues seems to have mistaken for the unlucky "scansorius."

A novel and significant suggestion is that, "morphologically speaking, the laryngeal pouch of the anthropoids would be homologous with and replace the two layers of the cervical fascia in man."

This otherwise very commendable paper is marred by some important misspellings, as of *ilium* which is made *ileum* twice on page 4, and by an occasional obscurity of style which sometimes renders the author's meaning doubtful. B. G. W.

ON THE CONSTITUTION OF THE NAPHTHALINES AND THEIR DERIVATIVES.

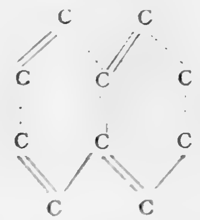
(Translated from the German.)

By M. Benjamin, Ph. B., and T. Tonnelé, Ph. B.*

Among the many aromatic hydrocarbons, naphthaline is one of the most interesting. The causes and laws of isomerism may be studied from the numerous isomeric compounds on one hand, while on the other, much information is derived from the consideration that many of these have acquired a great importance in the technical arts. In consequence of this, a great number of memoirs exist on this subject, and they are scattered abroad among the numerous scientific journals. It is, therefore, no simple matter for one to obtain a clear survey of the naphthaline question. We hope that the following pages, comprising material originally collected for our own information, will be welcomed by such of our professional colleagues as may have occasion to study this subject, for we feel assured that by consulting this article much of their time and labor will be economized.

CONSTITUTION OF NAPHTHALINE.

Naphthaline was discovered in 1826, by Garden, and subsequently widely studied by many investigators; yet its constitution remained undetermined for more than forty years. In the year 1866, Kékulé[1] published his ingenious and fertile theory of the aromatic compounds, considering them as derivatives from a single hydrocarbon, benzol. Soon after Erlenmeyer[2] so extended this theory as to include naphthaline, which he considered as having been derived from two benzol rings possessing two carbon atoms in common.



Graebe[3] was the first to demonstrate the correctness of this theory, in the course of his remarkable researches on the chinones of benzol and naphthaline. Since then this theory has been sustained by a large number of facts, while no satisfactory objections have been brought forward against it.

Other formulæ have been proposed by Berthelot[4], and later by Ballo[5], and also by Wreden[6], but none of these have received the approval of chemists.

The following are the principal facts which support the formula given by Erlenmeyer and Graebe:

I. The bichlornaphtochinon $(C_6 H_4)(C_4 Cl_2 O_2)$ yields on oxidation phthalic acid $C_6 H_4 \begin{cases} COOH \\ COOH \end{cases}$ and it is also transformed by the action of pentachloride of phosphorus into the pentachloride of naphthaline $(C_6 H_3 Cl)(C_4 Cl_4)$ and this on oxidation produces tetrachlorphthalic acid $C_6 Cl_4 \begin{cases} COOH \\ COOH \end{cases}$.

These facts prove that naphthaline is composed of two symmetrical rings, and that it can only have the formula of Erlenmeyer by the acceptance of Kékulé's benzol scheme.

On the other hand, the ortho- (1, 2) position of phthalic acid is likewise shown which was corroborated by the examination of the benzol bi-derivatives (Graebe).[7]

Ladenburg[8] and Wreden[9] have objected to Graebe's method of proof, on account of the derivation of tetrachlorphthalic acid from phthalic being uncertain. It can just as well be obtained from tere or isophthalthic acid. This is improbable, because the tetra-chlorphthalic acid used, agrees in all its properties (formation of anhydrides, etc.), with phthalic acid, and not with its two isomers.*

II. Naphthalinetetrachloride $(C_6 H_4)$. $(C_4 H_4 Cl_4)$, by oxidation gives phthalic acid $C_6 H_4 \begin{cases} COOH \\ COOH \end{cases}$ (Laurent). On submitting it to dry distillation it becomes converted into the α and β dichloronaphthaline $(C_6 H_4)(C_4 H_2 Cl_2)$ and the latter (β) produces, on oxidation, dichlorophtalic acid $C_6 H_2 Cl_2 \begin{cases} COOH \\ COOH \end{cases}$ (Atterberg).[10]

III. Monochloronaphthalinetetrachloride $C_6 H_4 (C_4 H_3 Cl Cl)_4$ may be converted into ordinary phthalic acid by oxidation. (P. and E. Depouilly[11] and Widman[12].) Monochloronaphthaline, $C_6 H_4 (C_4 H_3 Cl)$ which is the basis of the above compounds may be converted into

* Note.—The following memoir, written by MM. F. Reverdin and E. Nötting, was published in Geneva early this year. In addition to the text herewith given, the original pamphlet was supplemented with several valuable tables. These showed the derivation and behavior with reagents of the various substitution products. It is with regret that we are obliged to omit them. The space which they would occupy, together with the fact that they are not of general interest, does not seem to warrant their insertion. The entire article is undoubtedly the best resumé of the different theories concerning the formation of the naphthalines in existence. (*Translators.*)

1 Annalen der Chemie und Pharmacie, vol. CXXXVII, p. 129 (1866).

2 Ann. Chem. Pharm., CXXXVII, 346 (1866).

3 Ann. Chem. Pharm., CXLIX, 1 (1869).

4 Ann. Chem. Pharm., CXLII, 251 (1867). Comptes Rendus, LXIII, 788 and 834.

5 Das Naphtalin und seine Derivate. Braunschweig (1870).

6 Deutsche Chem. Ges., IX, 590 (1876.)

7 Ann. Chem. Pharm., CXLIX, 1 (1869.)

8 Theorie der aromatischen Verbindungen Braunschweig. 1876, p. 36.

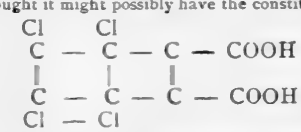
9 Deutche Chem. Ges., IX. 547 (1877).

10 Deutsche Chem. Ges. IX, 547, (1877).

11 Soc. Chim., Paris, IV, 10, (1865).

12 Om Naftalins Klorföreningar, Upsala, 1877, p. 16. Soc. Chim. Paris, XXVIII, 505, (1877).

*Wreden thought it might possibly have the constitution,



that is to say, be derived from an isometric hypothetic benzol. This is however, extremely improbable.

chloronitrophthalic acid $C_6 H_2 Cl (NO_2)$, $\left\{ \begin{matrix} COOH \\ COOH \end{matrix} \right.$ (Atterberg).[10]

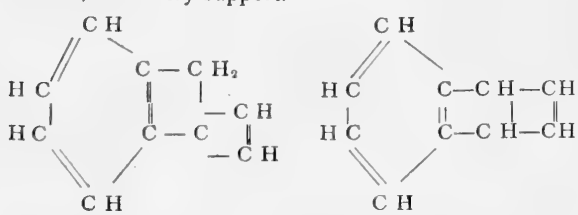
IV. This same monochloronaphthalinetetrachloride yields on saponification with alcoholic potash the *α* trichloronaphthaline $C_6 H_4 (C_4 Cl_3 H)$ melting at 81°, and this, by oxidation, produces trichloronitrophthalic acid. $C (NO_2) Cl_3 \left\{ \begin{matrix} COOH \\ COOH \end{matrix} \right.$. (Widman.[13])

V. Mononitronaphthaline $C_6 H_3 (NO_2) (C_4 H_4)$ produces, by oxidation with chromic acid, nitrophthalic acid $C_6 H_3 (NO_2) \left\{ \begin{matrix} COOH \\ COOH \end{matrix} \right.$ melting at 212°, which can also be obtained directly from phthalic acid by the action of nitric acid. (Beilstein and Kurbatow.[14])

On the other hand, naphthylamine $C_6 H_3 (NH_2) (C_4 H_4)$ which corresponds to this nitronaphthaline may be oxidized, by means of permanganate, into ordinary phthalic acid [15] (Graebe.[16]) $C_6 H_4 \left\{ \begin{matrix} COOH \\ COOH \end{matrix} \right.$.

Binitronaphthol $C_6 H (OH) (NO_2)_2 (C_4 H_4)$, produced from naphthylamine, also yields, by oxidation, phthalic acid. (Liebermann and Dittler.[17])

This last proof is indisputable, as it shows conclusively that no matter which half of the naphthaline ring is oxidised the same phthalic acid results. Naphthaline must therefore possess an absolutely symmetrical structure, and hence the following formulæ, proposed by Wreden, lose every support.



Berthelot,[18] and latter Ballo,[19] deduced from the formation of naphthaline from benzol or styrol and aethylene the formulæ :

CH—CH—C—CH—CH
‖ | ‖ | ‖
CH—CH—C—CH—CH

The above formula explains very clearly, the corresponding syntheses and is also symmetrical but it cannot be accepted because it would give the following formula for benzol:

CH—CH—CH
‖ | ‖
CH—CH—CH

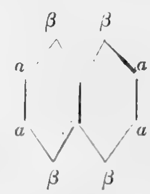
in which the six hydrogen atoms have not all the same value. The univalence of the same is positively shown from the examination of Ladenburg, Hübner and Petermann, Hubner and Wroblewsky.

ISOMERISM OF THE NAPHTHALINE DERIVATIVES.

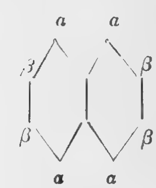
Faraday[20] observed while investigating the sulpho-acids of naphthaline that two isomeric mono derivatives were formed. Since then an entire series of others have been discovered. Almost all of the biderivatives exist in two modifications, and the number of isomers among the higher substitution products is very numerous. The formula now used to represent naphthaline explains this fact in a very satisfactory manner.

One observes, therefore, that it is not indifferent whether the hydrogen consecutive to the four combining carbon atoms are replaced or whether the four combining positions furthest removed from them, are replaced. On the other hand it is evident that the hydrogen atoms in groups of four possess equal values. In order to distinguish between these two varieties of hydrogen atoms Merz[21] has designated them by the letters *α* and *β*, and distinguishes the isomeric series as the *α* and *β* derivatives. He did not, however, state to which of the positions of the hydrogen atoms belonged the *α* and which the *β* series.

Wichelhaus,[22] soon after, forming a theory on the analogy between the *α* naphthol with phenol, assumed that the substituting hydrogen atoms were combined with the carbon atoms in both compounds alike (*i. e.* in a similar manner) and hence the *α* position must correspond to the combining place furthest removed from the carbon atoms. Especially as they are more like the benzol carbon atoms. Subsequently when it was shown that naphthachinon was an *α*—*α* derivative (Liebermann and Dittler [23]) and the para (1, 4) position of ordinary chinon was definitely settled, the notation was changed.



FORMER.

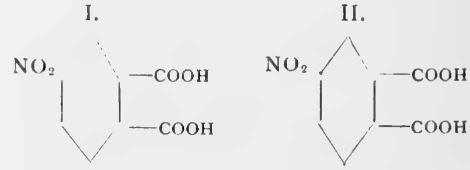


PRESENT.

This demonstration is untenable however, for Stenhouse and Groves, [24] have discovered a second napththa chinon, viz.: the *β*, which likewise contains the two atoms of oxygen in the same ring ; hence it follows that there are chinons which do not have their oxygen atoms in the paraposition (1, 4). Consequently the constitution of ordinary naphthachinones, as well as the position of the *α* and *β* atoms again becomes doubtful. Fortuately however other experiments, made under different conditions, permit the final answering of this question.

We have seen how Beilstein and Kurbatow [25], by the oxidation of nitronaphthaline, which is an *α* derivative, have obtained ordinary nitrophthalic acid, melting at 212°. Theoretically, there are but two isomeric nitrophthalic acids possible, both of which have been prepared.

I. II.



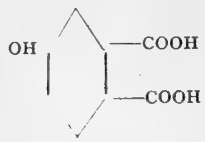
The first melts at 212° ; the other, discovered by O. Miller, melts at 165°.

The latter, according to the exact researches of this investigator, corresponds to the oxyphthalic acid of Baeyer. On the other hand, Schall, [27], in the course of his researches on the hydroxylated benzoldicarbon acids, demonstrat-

[13] Loc. Cit., p. 59. Soc. Chim., Paris, XXVIII, 505, (1877).
[14] Deutsche Chem. Ges. XII, 688, (1879).
[15] Naphthylamine gives, with potassium chromate and sulphuric acid, phtalic acid and naphtachinon, (R. & N.)
[16] Private papers.
[17] Ann. Chem., CLXXXIII, 228, (1876).
[18] Ann. Chem. Pharm. CXLII, 251 (1867.) Comptes Rendus LXIII, 788 and 834.
[19] Das Naptalin und seine Derivate.
[20] Ann. Chim. Phys. XXXIV, 164.

[21] Zeitschrift für Chemie, NF. IV, 399 (1868).
[22] Ann. Chem. Pharm. CLII, 311 (1869.)
[23] Ann. Chem. CLXXXIII, 228 (1876.)
[24] Ann. Chem. Pharm. CLXXXIX, 145 (1877.)
[25] Deutsche Chem. Ges. XII, 688 (1879.)
[26] Deutsche Chem. Ges. XI, 1191 (1878.)
[27] Deutsche Chem. Ges. XII, 816 (1879.)

ed that the corresponding oxyphthalic acid has the following constitution.



Nitrophthalic acid, melting at 165°, has the second (II) formula, while the one melting at 212°, has the first (I) formula. As stated above, these are both produced from nitronaphthaline, which is itself an α compound, and so it is demonstrated that the α position is the one next to the two common carbon atoms. The hydrogen atoms in naphthaline are combined in groups of four, each of which is equivalent; this follows naturally from the observed facts in benzols.

Atterberg, in his masterly researches on the chlorinated naphthalines, found that in naphthaline, the four α positions are of equal value without any reference to the benzol formula. According to de Koninck, Marquardt and Atterberg, nitronaphthaline may be converted into α monochloronaphthaline. Therefore, in these compounds, the nitro and chloro groups hold the same position. The monochloronaphthaline may, however, be converted into a nitro compound and that into a β dichloronaphthaline. Nitronaphthaline may be converted into two different dinitronaphthalines, and those into two different dichlornaphthalines γ and ζ. Hence all three dichlornaphthalines β, γ, ζ, contain a chlorine atom in the position of the nitro group of the nitronaphthalines. The three remaining chlorine atoms of the three compounds must take different positions with reference to the first, since otherwise the three compounds could not be different. All of the chlorine atoms of these compounds possess an α position, consequently the naphthaline molecule must possess four α positions of equal value.

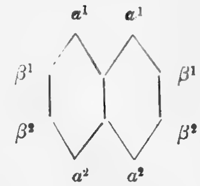
DETERMINATION OF THE CONSTITUTION OF THE NAPHTHALINE DERIVATIVES.

The constitution of naphthaline derivatives is ascertained by converting them by a simple reaction into another of known position. The nitro derivatives may, for instance, be converted into the chlorine or bromine derivatives by the chloride or bromide of phosphorus, and then by reduction into the amido derivatives. These latter may, by means of their diazo-compounds, be converted into phenols, chlorine, bromine (and perhaps iodine) derivatives, and by means of formic acid into nitriles, and consequently into carbon acids. The bromine derivatives produce, with ethyl and methyl iodide, ethyl and methyl compounds, and with chlorcarbonic acid ether carbon acids are produced. The sulpho-acids give with potassium cyanide, cyanates. With penta chloride and bromide of phosphorus, chlorine and bromine derivatives are obtained with sodium formate, carbon acids; and with sodium at a high temperature phenols are formed. On the other hand the oxidation often shows whether the substituting groups are in the same ring, or are divided among both; in the first case phthalic acid is formed, and in the second substitution products of phthalic acid are formed.

CONSTITUTION OF THE NAPHTHALINE DERIVATIVES.

The mono substitution products exist in but two modifications, and it is easy, therefore, to determine their constitution. When in the bisubstitution products, the two substituting groups are equal, ten different isomeric compounds are obtained. If, however, they are unequal, the number is increased to fourteen. The constitution of a given number of the same is exactly known, while with others it is only known that the substituting groups are contained in the same or in two different rings, that they possess an α or a β position, or a similar position.

In the case of the higher substituted naphthaline derivatives, the number of possible isomers is considerably increased, especially when the groups are unequal. When, however, the groups are equal, fourteen tri-derivatives, twenty-two tetra-derivatives, fourteen penta-derivaties, ten hexa, two hepta, and a single octo-derivative, in which all the hydrogen has been replaced, are obtained. There are, for example, seventy-five possible chlorine naphthalines; of these, however, only twenty-four have been prepared. In order to simplify the nomenclature of these numerous compounds, we will distiuguish the two from each other by designating the same position in each ing, as α^1, α^2, β^1, and β^2.



When a compound contains both of its substituting groups in the same ring, we will combine the latter after Jolin's method, that is, by a simple line, as for example, α_1-β^1, α^1-β^2, α^1-α^2, etc. When, however, the groups are divided between the two rings, then they are combined by double lines, thus: $\alpha^1{=}\alpha^1$, $\alpha^1{=}\beta^1$, $\alpha^1{=}\beta^2$, etc. The same method of lettering may be used in the higher substituted compounds; thus the compounds $\alpha^1-\beta^1-\alpha^2$, $\alpha^1-\beta^2-\alpha^2$, $\alpha^1-\beta^1-\beta^2-\alpha^2$, have their groups in the same ring. The compounds $\alpha^1-\alpha^2{=}\alpha^1$, $\alpha^1-\alpha^2{=}\beta^1$, $\alpha^1{=}\alpha^1-\alpha^2$, $\alpha^1-\alpha^2{=}\alpha^1-\beta^1$, have their groups divided between the two rings. We have placed together, in a series of tables, the most important derivatives of naphthaline. In these tables will be found their constitution as far as it is known; some characteristic properties, as their melting point, boiling point, their formation, conversion, and, as complete as possible, a list of the literature.

It is to be hoped that the many vacancies which appear among these tables may soon be filled.

Last of all we would observe that the terms α, β, γ, δ, etc., which we have chosen to represent the naphthaline derivatives have no connection with their constitution with the single exception of the mono derivative. They have been given to the different isomers only in chronological order, and they do not correspond by any means as far as position is concerned to the different α, β, etc., derivatives. This fact is unfortunate, because it may cause confusion. We believe, however, that at present no change should be made in names originally chosen by the discoverers. When the constitution of the napthaline derivatives is better known, a rational nomenclature according to the above principals will naturally be adopted. Thus for instance the present β, γ and ξ dichlornaphthalines will be designated as α^1—α^2, $\alpha^1{=}\alpha^2$ and $\alpha^1{=}\alpha^1$ dichlornaphthaline, the α and δ trichlornaphthaline as α^1—β^1—β^2 and α^1—$\alpha^2{=}\alpha^1$, trichlornaphthaline, the α and β chlorodinitronaphthalines as α^1—$\alpha^2{=}\alpha^1$ and α^1—$\alpha^2{=}\alpha^2$ chlorodinitronaphthalines, and in a similar manner for all other compounds by which their constitution will be immediately recognized.

NATIONAL ACADEMY OF SCIENCES.

The abstracts of the papers read before the recent meeting at New York were, in all cases, either corrected or rewritten by the authors, and we are under obligation to Professors James Hall, Wolcott Gibbs, E. D. Cope, S. P. Langley, Henry Morton, Elias Loomis, B. Silliman, O. N. Rood, T. Sterry Hunt, Henry Draper, for their assistance in presenting correct reports.

The addresses of Professor Alexander Agassiz and Lieut. Shawatka were delivered *viva voce*, and we made use of the stenographic notes made for the New York

Tribune in these cases, which were submitted to the authors on the 4th of December last for correction or rejection; no objection being made we printed them in a recent number. After publication Professor Agassiz now writes that the reports under his name are not satisfactory to him. We therefore request our readers to consider them withdrawn.

Professor George F. Barker, Professor O. C. Marsh and Professor J. E. Hilgard are preparing more elaborate reports of their important papers, and promise them at an early day.

THE BRAIN OF THE ORANG.*

BY HENRY C. CHAPMAN, M.D.

The brain of the Orang has been figured by Tiedemann, Sandifort, Schroeder van der Kolk and Vrolik, Gratiolet, Rolleston, etc. On account, however, of the few illustrations extant, and of the importance of the subject, I avail myself of the opportunity of presenting several views of my Orang's brain (Figs. 1 to 5), which was removed from the skull only a few hours after death. The membranes were in a high state of congestion, and a little of the surface of the left hemisphere had been disorganized by disease, otherwise the brain was in good condition. It weighed exactly ten ounces. The brain of the Orang in its general contour resembled that of man more than those of either of the Chimpanzees which I examined. In these the brain was more elongated. The general character of the folds and fissures in

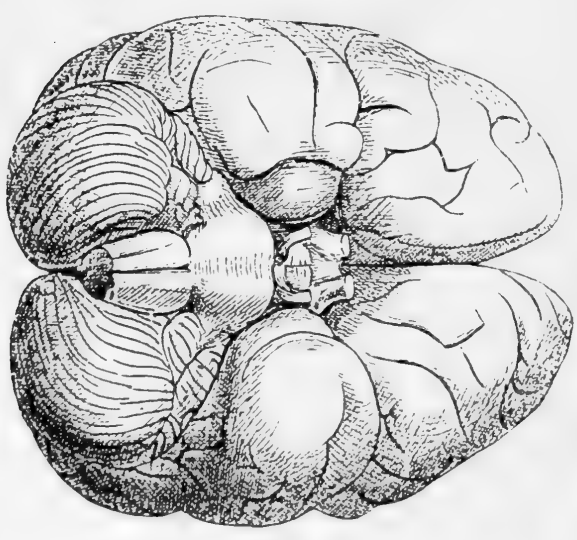


FIG. 1.

the brain of the Orang, Chimpanzee, and man are the same; there are certain minor differences, however, in their disposition in all three. The fissure of Sylvius in the Orang runs up and down the posterior branch pursuing only a slightly backward direction; the anterior branch is small. The fissure of Rolando, or central fissure, quite apparent, is, however, situated slightly more forward in the Orang than in man. It differentiates the frontal from the parietal lobe. The parieto-occipital fispure is well marked; bordered externally by the first occipital fold it descends internally on the mesial side of the hemisphere, separating the parietal from the occipal lobes.

* From the Proceedings of the Academy of Natural Sciences, Phila., 1880.

in the Orang, the parieto-occipital fissure does not reach the calcarine, being separated from it by the "deuxième plis de passage interne" of Gratiolet, or "untere innere Scheitelbogen-Windung" of Bischoff. I have noticed this separation as an anomaly more than once in man.

According to Bischoff, this disposition obtains in the Gorilla, and seems to be usual also in the Chimpanzee. In the female Chimpanzee, however, on the left side I found the parieto-occipital fissure passing into the calcarine, as in man. The frontal lobe is easily distinguished from the parietal by the fissure of Rolando, and from the temporal by the fissure of Sylvius. In the Orang it is higher, wider, and more arched than in the Chimpanzee. The anterior central convolution in front of the central fissure runs into the post-central convolution above and below, as in man. It is difficult, however, to identify the three frontal convolutions seen in man and the Chimpanzee, the frontal lobe of the Orang dividing rather into two convolutions, the middle one being badly defined. This is due somewhat to the length of the precentral fissure, which is as long as the fissure of Rolando, extending farther upward than in man. There was nothing particularly noticeable about the base of the frontal lobe; on the mesial surface it ran into the parietal. The part above the calloso-marginal fissure in the Orang is not as distinctly divided into convolutions as in man, though these are not constantly present even in all human brains. The parietal lobe is separated from the frontal by the central fissure, from the occipital and temporal incompletely, by the parieto-occipital and Sylvian fissures. The posterior-central convolution is well defined. The parietal fissure in the Orang is more striking than that of man, resembling the Gorilla's; it is twice as long as the corresponding fissure in the Chimpanzee, extending from the transverse occipital fissure, as is sometimes the case in man, almost into the fissure of Rolando. It is unbridged and without a break, and divides the parietal lobe completely into upper and lower parietal lobules. The upper parietal lobule is bounded externally by the parietal fissure; posteriorly it is separated from the occipital lobe, internally by the parieto-

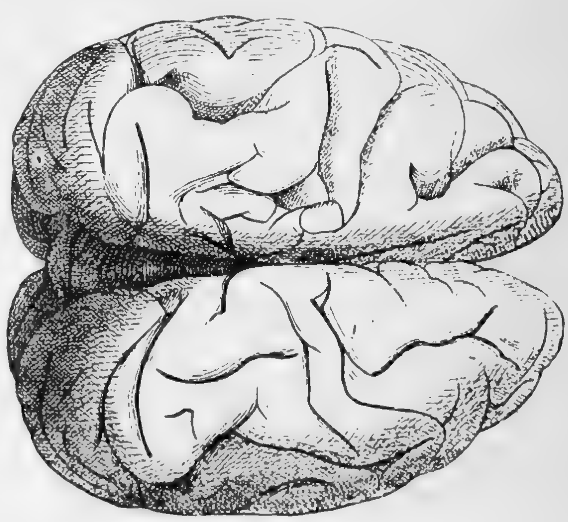


FIG. 2.

occipital fissure; externally it is continuous with the occipital lobe, as the first occipital gyrus, anteriorly it is separated from the posterior central convolution more completely than in man, by a fissure which runs paralled with the central fissure. There is in the Orang, also, a fissure running paralled with the parietal, which subdivides the upper parietal lobule into inner and outer portions. The precuneus, or the space on the mesial side of the parietal lobe between the parieto-occipital fissures and the ascending branches of the calloso-marginal, is well defined. The lower parietal lobule in the Orang divides naturally into the supra-marginal and angular gyri. The supra-marginal fold curves around the upper end of the posterior branch of the fissure of Sylvius and runs into the superior temporal gyrus. The angular gyrus, which is very evident, arches around the first temporal fissure, and becoming continuous with the second occipital fold, passes then into the upper temporal

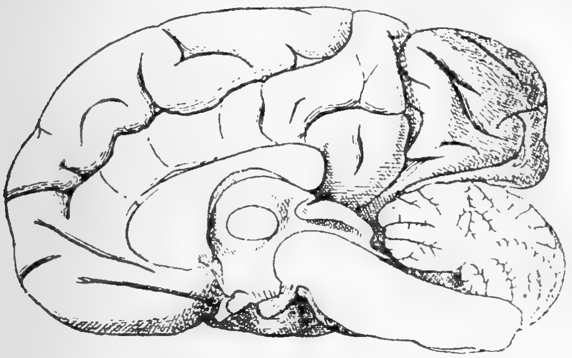


FIG. 3.

gyrus. The occipital lobe, separated from the parietal, internally, by the parieto-occipital fissure, is continuous with the upper parietal lobule through the first occipital gyrus, and by the second occipital gyrus with the angular. There are no sharp lines of demarcation between the occipital and temporal lobes. In the occipital lobe of my Orang the transverse occipital fissure was present, and received the parietal fissure. The calcarine fissure was well marked, but was separated in the Orang from the parieto-occipital fissure by the "deuxième plis de passage interne" of Gratiolet, the "untere innere Scheitelbogen-Windung" of Bischoff. The cuneus of the Orang is therefore somewhat different from that of man. In man I have seen these two fissures separated as an anomaly. The calcarine passed into the hippocampal fissure, so that in the Orang, as in monkeys generally, the gyrus fornicatus was separated from the hippocampal gyrus, whereas in man these convolutions are continuous. This disposition has been noticed in the Hylobates, in Ateles, and in one Chimpanzee, where the calcarine did not reach the hippocampal. The first occipital gyrus is very well developed, and as the late Professor Gratiolet observed, is one of the most striking convolutions

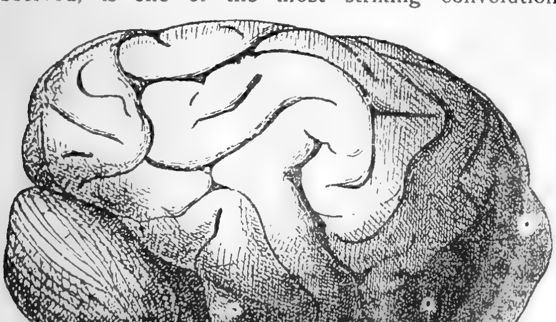


FIG. 4.

in the brain of the Orang. It rises so to the surface that the internal perpendicular fissure or external part of the parieto-occipital fissure is almost entirely bridged over, the operculum so characteristic of the monkey almost disappearing. It is continuous with the upper parietal lobule arching around the parieto-occipital fissure. This convolution comes to the surface in the Hylobates and Ateles almost to the same extent as in the Orang, but it is more developed in the latter than in the Chimpanzee. It is called also the "premier plis de passage externe," by Gratiolet, the "obere innere Scheitelbogen-Windung," by Bischoff, the "first annectant gyrus," by Huxley, and "first bridging convolution," by Turner. The second occipital convolution connects the occipital lobe with the angular gyrus. In my Orang it was partly concealed by the first occipital. It was not as superficial as in man. The third occipital gyrus is continuous with that part of the temporal lobe below the first temporal fissure. I noticed, also, in my Orang the "quatrième plis de passage" of Gratiolet. On the mesial side of the occipital lobe in my Orang was well seen the "deuxième plis de passage interne" of Gratiolet, the "untere innere Scheitelbogen-Windung" of Bischoff, which separates the calcarine from the parieto-occipital fissure; and in both the Orang and Chimpanzee, more especially on the left side, I had no difficulty in recognizing the "premier plis de passage interne" of Gratiolet, its convexity turning inwards, while that of the first occipital gyrus, or the "premier plis de passage externe," turns outward. These two convolutions, the first occipital gyrus and the "premier plis de passage interne," in my Orang were continuous. They are regarded as one by Bischoff, forming his "obere innere Scheitelbogen-Windung," but as two by Gratiolet, constituting his "premier plis de passage externe et interne."

The temporal lobe in the Orang is much less convoluted than in man, or even in the Chimpanzee. The first temporal fissure and first temporal convolution are well marked, but the second and third are badly defined

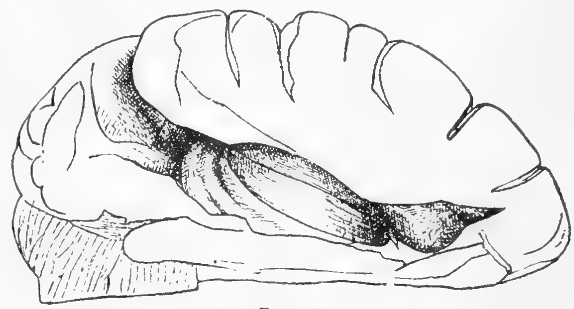


FIG. 5.

The fusiform and lingual lobes are separated by the inferior occipito-temporal fissures, the collateral fissures of Huxley. The Island of Reil was perfectly covered in both the Chimpanzee and the Orang by the operculum, but was not convoluted in my Orang. The surface in places was slightly roughened. I noticed, however, three or four convolutions in the Chimpanzee. On making a section of the left hemisphere of the Orang I noticed that the corpus callosum was relatively smaller than in man, but that the ventricle exhibited an anterior, middle and posterior cornu, the corpus striatum, tænia semicircularis, thalamus opticus and fornix were well developed, the hippocampus major with corpus fimbriatum were perfectly evident, and the hippocampus minor larger relatively than in man. I did not see a trace of the emmenentia collateralis; this is often, however, absent in man.

The cerebellum in my Orang was relatively larger than that of man, but smaller than that of either of the Chimpanzees I have dissected, and was just covered and no more by the posterior lobes of the cerebrum. This relation is still retained in my Orang, though the brain has been lying in alcohol for three months since it was taken out of the chloride of zinc in which it was placed until the pia mater could be removed. During this period it has been subject to the conditions, such as the want of the support of the membranes, the effect of pressure, etc., urged by Gratiolet, Huxley, Rolleston, Marshall, etc., as sufficient to explain why after death the cerebellum is uncovered by the cerebrum in the Orang and Chimpanzee, as held by Owen, Schroder van der Kolk and Vrolik, and Bischoff. Every anatomist knows that the brain, after removal from the skull, especially without the membrane, if left to itself, very soon loses its shape. It is absolutely necessary therefore to examine the brain *in situ*, and after removal from skull to place it in some hardening fluid in which it will float. Even with these precautions, through the change of the surroundings, shrinkage, etc., the brain is always somewhat altered. It happens, however, that I have had lying in alcohol for some years a number of human and monkey brains. Among the latter, examples of the genera *Cebus, Ateles, Macacus, Cynocephalus, Cercopithecus*, etc., taken out of the skull sufficiently carefully, but preserved in the rudest manner without any regard to the above precautions. Now, while all of these brains have somewhat lost their natural contour, they are not so changed that in a single one, human or monkey, do I find the cerebellum uncovered by the cerebrum, and in every instance the posterior lobes overlap the cerebellum to a greater extent than I find is the case in my Orang. If the cerebrum and cerebellum in the Orang and Chimpanzee invariably bear the same proportion to each other as they do in man and the monkeys, why should not the brain of an Orang or Chimpanzee, after lying in alcohol for some years, exhibit the cerebellum covered by the cerebrum as in them? Why should it be necessary to replace the brain of the Chimpanzee or the Orang in the skull, to make plaster casts, etc., if there is no difference between their brains and those of man and the monkeys, for there is no necessity of having recourse to such measures to prove that the cerebellum is covered in the latter?

In the account I gave of the female Chimpanzee,[1] I stated that I found the cerebellum uncovered. I had the opportunity a short time since, of verifying that statement in the male, noticing *in situ* that the cerebellum was uncovered by the posterior lobes. This was found to be the case by Mr. Arthur Browne, the Superintendent of the Phila. Zool. Garden, in a third Chimpanzee which died there. With all deference to Prof. Marshall's[2] photograph of a plaster cast of the brain of a Chimpanzee, and however it may truthfully represent the relations of the cerebellum in his specimen, I must say that it would be simply monstrous if accepted as an illustration of either of mine, and with profound respect for Prof. Huxley's[3] opinion regarding the interior of the skull being a guide for the determination of the proportion between posterior lobe and cerebellum, I find it anything but a safe one as regards the anthropoid apes. For the space between posterior lobes of brain and dura mater and bone, both posteriorly and laterally, I find variable *in situ*, due to the state of the blood vessels and amount of fluid in arachnoid and subarachnoid cavities. In speaking of the Gorilla, Prof. Bischoff[4] observes, p. 100, "Das es bei ersterem am wenigsten von oben Hinterlappen der grossen Hemisphäre bedeckt wird und bei der Betrachtung des Schädel gewiss von oben mit seinem hinterem Rande sichtbar wird." And in reference to the Chimpanzee,[5] p. 95, "Die Hinterhauptslappen des grossen Gehirns bei diesem Affen wie bei dem Menschen das kleine Gehirn überzogen und von oben fast ganz bedecken." And Vrolik[6] states, p. 7, of the Orang: "Ce lobe postérieur ne se prolonge pas autant que chez l'homme; il ne recouvre pas si bien le cervelet du moins il ne cache pas complétement surtout vers les côtés." The fact of the cerebellum being covered by the posterior lobes in my Orang and that figured by Gratiolet, and but slightly uncovered in that of Vrolik's, is no more strange than that Bischoff[7] should find it covered in one Hylobates, and Prof. Huxley[8] having stated it to be uncovered in another.

[1] Proceedings of the Acad. Nat. Sciences, Phila., 1879.
[2] Natural History Review, 1861.
[3] Man's Place in Nature, p. 97.
[4] Das Gehirn des Gorillas, 1877.
[5] Gehirn des Chimpanzee, 1871.
[6] Amsterdam Verslagen, Deel, 13, 1862.
[7] Beitrage zur Hylobates, 1860
[8] Vertebrate Anatomy, p. 411.

CAUSES WHICH DETERMINE THE PROGRESSIVE MOVEMENT OF STORMS.*

PROF. ELIAS LOOMIS.

For the purpose of discovering the causes which determine the progressive movement of storms, I have made an extensive examination of the course and velocity of storm centres in tropical regions, and also of abnormal paths in the middle latitudes of Europe and America. I have examined the courses of all those hurricanes which have originated near the West India Islands, and also all the storm tracks delineated on the maps of the *Monthly Weather Review*. I have examined the courses of all those hurricanes in Southern Asia and its vicinity whose paths have been best determined, and also all the storm tracks delineated on the maps of the International Series of Observation. The following summary presents some of the results derived from this investigation.

1. The lowest latitude in which a cyclone centre has been found near the West India Islands is ten degrees; and the lowest latitude in the neighborhood of Southern Asia is six degrees. Violent squalls and fresh gales of wind have, however, been encountered directly under the equator.

2. The ordinary course of tropical hurricanes is toward the west-northwest. In a few cases they seem to have advanced toward a point a little south of west, and in a few cases their course has been almost exactly toward the north.

3. Tropical hurricanes are invariably accompanied by a violent fall of rain. This rain fall is never less than five inches in twenty-four hours for a portion of the track, and frequently it exceeds ten inches in twenty-four hours.

4. Tropical storms are generally preceded by a northerly wind, and after the passage of the low centre, the wind generally veers to the southeast at stations near the centre, and the southerly wind which follows the low centre is generally stronger than the northerly wind which preceded it. This fact appears to suggest the explanation of the origin of the cyclone, and the direction of its progressive movement. The prevalent direction of the wind in the neighborhood of the West India Islands is from the northeast. Occasionally a strong wind sets in from a southerly quarter. The interference of these winds gives rise to a gyration, and a fall of rain sometimes results. When rain begins the latent heat which is liberated causes an inflow of wind from all quarters, by which the rainfall is increased; and since the winds are deflected by the rotation of the earth, an area of low pressure is produced, and the force of the winds will be maintained as long as the rainfall continues. The effect of this strong wind from the south is to transport the low centre in a northerly direction; and by the combined action of this south wind and the normal wind from the northeast the centre of low pressure is usually carried in a direction between the north and west.

* Read before the A. A. A. S., Boston, 1880.

The electrical blowpipe of M. Jamin consists of a pair of carbon pencils—an electric candle, in fact—surrounded by a coil of insulated copper wire wound a few inches distant from the pencils in the plane of their axes. The current is so led that, in circulating round the coil, it will attract the electric arc formed at the lower end of the carbon pencils, and cause it to flash out almost in the form of a fish-tail gas flame. This spreading out of the arc is the special feature of the action of the apparatus. It facilities the application of the heat of the electric arc to the fusion of refractory substances, and enables us better to take advantage of this little-used means of producing a very high temperature.

LETTERS TO THE EDITOR.

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

To the Editor of SCIENCE :

I was much pleased to see, by an abstract in "SCIENCE," that the opportunity which Philadelphia scientists had of examining into the anatomical peculiarities of the Orang-Outang was utilized, and that the body of the anthropoid, deceased in the Philadelphia Zoological Gardens, fell into the hands of as zealous an anatomist as Dr. Chapman. I have since had access to the original paper*, and would provisionally offer a few comments upon such statements as Dr. Chapman makes with reference to the cerebral relations of his anthropoid specimen.

It is stated that the brain of this Orang resembles that of a man more, as regards its general contour, than that of either of the Chimpanzees which the Dr. examined. It must be borne in mind that the internal dimensions of the cranial cavity of both anthropoid species show a relative excess of the transverse diameter as compared with the average mesocephalic human skull. But the correlated greater breadth of the brain is not due to a general greater breadth of all the lobes, for it is mainly provided for by the immense reduction in mass, and in every dimension of the frontal lobe. If the frontal lobe were relatively as well developed in the anthropoid apes as in man, the general contour of the cerebral hemispheres would be nearly the same in all three species, but more human in the Chimpanzee than in the Orang. Many of the inferences of the writer regarding contours and relations, seem to be based on the hardened and otherwise manipulated specimen, and for reasons which I shall advance are probably faulty. It is further stated, that the fissure of Sylvius runs up and down, "the posterior branch pursuing only a slightly backward direction." On looking at the accompanying plate (Pl. xvii, Fig. 1.),† I perceive the reason for this statement. The Doctor's specimen had been allowed, evidently, to flatten out on its base, for the lower contour of the frontal and temporal lobes as well as of the cerebellum is an accurate straight line. Under such circumstances the fissures must change their natural direction. In both hemispheres of my Orang, the inclination of the Sylvian fissure (horizontal branch) is thirty degrees towards the ideal hemispheral axis. It is owing to the same imperfect manipulation that the author has arrived at the conclusion that the central fissure (Rolando's) is more forward in the Orang than in the other anthropoid. According to some recent writers ‡ on the convolutions, the acuteness of the angle formed by the central fissure and the median fissure separating the hemispheres, forward is an index of cerebral development. It is acuter in both my Chimpanzee brains, then in the Orang in my possession or in any of those figured in plates.

I find the temporal lobe in my Orang well convoluted, showing the same sulci and in about the same degree of complexity as other anthropoid brains.

Dr. Chapman's figures give but a poor idea of the richness in gyri and the proportions of the different parts of the Orang's brain, at least as these are observable in the specimen which I demonstrated before the New York Academy of Sciences. In figure 2 § the frontal lobes are too broad and too long, and the ethmoidal prolongation (*Sieb-bein-schnabel*) is not indicated anywhere. Some of the sulci drawn are not identifiable in any brain that I have seen a record of, and others which are recorded as constant can not be identified at all. It is not difficult to see from the drawings that the cerebral hemispheres were permitted to separate, the whole brain to flatten on its inferior surface, and that no successful attempt was made to retain the natural proportions of any of the parts.

I would add that my observation on the Island of Reil, in the Orang, is distinctly contradictory of that of Dr. Chapman, who states it to be unconvoluted. One of the hemispheres in my possession is so prepared as to show the sulci and *gyri breves* of the Orang's *insula*, which correspond as to their direction and relations to, though less numerous and well marked than, those of man. In every anthropoid dissected by myself I find these gyri and sulci, and one sulcus, is a constant feature of even the Cynocephali. Dr. Chapman has, on a former occasion, asserted the cerebellum to be uncovered by the cerebrum in one of his Chimpanzees. I examined carefully both specimens that were sent to Philadelphia, and of which the Doctor obtained the brains after death. They did not differ in their external cranial configuration from the other Chimpanzees ; they were the healthiest, most active, and most intelligent of the species I have seen, and considering the fact that in both of my specimens the cerebrum clearly overlapped, I was much surprised to find that Dr. Chapman had discovered an exception in one of the two animals* I had myself seen. Subsequently Doctor Parker demonstrated that Doctor Chapman's observation was due to the imperfection of the methods followed. That the writer made erroneous inferences is clear from a statement in the very paper I am now commenting on. Dr. Chapman says, "It happens, "however, that I have lying in alcohol for some years a "number of human and animal brains. Among the latter, "examples of the genera *Cebus, Ateles, Macacus, Cyno-"cephalus, Cercopithecus*, etc., taken out of the skull suf-"ficiently carefully, but preserved in the rudest manner "without any regard to the above precautions. Now, "while all of these brains have somewhat lost their "natural contour, they are not so changed that in a single "one, human or monkey, do I find the cerebellum uncov-"ered by the cerebrum, and in every instance the pos-"terior lobes overlap the cerebellum to a greater extent "than I find is the case in my Orang. If the cerebrum "and cerebellum in the Orang and Chimpanzee invaria-"bly bear the same proportion to each other as they do "in man and the monkeys, why should not the brain of "an Orang or Chimpanzee, after lying in alcohol for some "years, exhibit the cerebellum covered by the cerebrum "as in them ? Why should it be necessary to replace the "brain of the Chimpanzee or the Orang in the skull to "make plaster casts, etc., if there is no difference between "their brains and those of man and the monkeys, for "there is no necessity of having recourse to such meas-"ures to prove that the cerebellum is covered in the "latter?"

The above would be, to say the very least, a novel kind of argumentation, even if its assumptions were true. I have seen hundreds of brains taken out of the skull on *post mortems* of the human subject thrown on a slab, which would, if preserved (and in instances where they were preserved *did*), show an uncovered cerebellum. Why, Benedict of Vienna actually discovered that the cerebellum was uncovered in several criminals ! This discovery was speedily exposed as a crude fallacy by Meynert and Heschl. It is remarkable that Dr. Chapman disposes with such facility, of the exact methods and relies so much on proofs which are, so to speak, the outgrowth of accident. Now, in every instance where the

* On the structure of the Orang-Outang by Henry C. Chapman, M. D. Proceedings of the Academy of Natural Sciences of Philadelphia, 1880, p. 160.

† See page 326 of this Journal, Fig. 2.

‡ Meynert Archiv für Psychiatrie VII. Clevenger. "The sulcus of Rolando an indication of intelligence" *Journal of Nervous and Mental Diseases*, 1880.

§ See page 326 of this Journal, Fig. 1.

* One of these was the black-faced variety which Du Chaillu attempted to make an extra species of (Not the *Tschego*.)

BOOKS RECEIVED.

PROCEEDINGS OE THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA, PART II.—April to September, 1880.—Edited by EDWARD J. NOLAN, M.D., Philadelphia, 1880.

The present volume contains the following papers:

Carcinological Notes,-No. II.—Revision of the Gelasimi. (Continued), by J. S, Kingsley.

Remarks on Pond Life, by Jos. Leidy, M. D.

On the Structure of the Orang Outang, by H. C. Chapman, M. D.

Description of a New Crustacean from the Upper Silurian of Georgia, with remarks upon Calymene Clintoni, by Anthony W. Vogdes.

Carcinological Notes, No. III.—Revision of the Genus Ocypoda, by J. S. Kingsley.

Carcinological Notes, No. IV.—Synopsis of the Grapsidæ, by J. S. Kingsley.

Serpentine Belts of Radnor Township, Delaware Co., by Theodore D. Rand.

On some Homologies in Bunodont Dentition, by Harrison Allen, M. D.

Description of a Partula supposed to be new, from the Island of Moorea, by W. D. Hartman, M. D.

On the Development of Lemna Minor, by Wm. Barbeck.

Description of a new species of Hemitripterus from Alaska, by W. N. Lockington.

Description of a new species of Catostomus (Catostomus chopy) from the Colorado River, by W. N. Lockington.

PROCEEDINGS OF THE MINERALOGICAL AND GEOLOGICAL SECTION OF THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA.

A New Polariscope; A Garnet with Inverted Crystallization; The Minerals of Surry Co., N. C.; A new locality for Lignite; On Serpentine in Bucks Co.; The Iron Ores and Lignite of the Montgomery Co. Valley; An Enclosure in Quartz; On a new Fucoidal Plant from the Trias; The Trenton Gravel and its relation to the Antiquity of man; Note on Philadelphite—a new Mineral; A new locality for Siderite; Magnetic Markings in Muscovite; A new locality for Asbolite; Epidote in Molybdenite; The Optical Characters of some Micas; On the Measurement of Plane Angles; On an Exfoliated Talc; Tin in North Carolina; On Siderophyllite—a new mineral; On Sterlingite and Damourite; Vanadium in Philadelphia Rocks; A new locality for Epsomite; The Surface Geology of Philadelphia and vicinity; On the Bryn Mawr Gravel; On some enclosures in Mica; On Dendrites; On a Jurassic Sand; On Philadelphite (Sp. Nov.); A Potsdam Sandstone Outcrop on the S. Valley Hill of Chester Valley; all by H. C. Lewis.

A new locality for Amethyst; A new Corundum locality; Menaccanite and Talc from Maryland; Sunstone in Labradorite; A new locality for Fluorite; all by W. W. Jefferis.

Fossil (?) Casts in Sandstone; Garnet mistaken for Corundum; by Dr. J. M. Cardeza.

On a peculiar Stratification in Gneiss; The Northern Belt of Serpentine in Radnor Township; Change of Serpentine into Quartz; A new locality for Millerite; A new locality for Gypsum; Chromite near Radnor, Pa.; On Randite; Some Microscopic Enclosures in Mica; Potsdam Sandstone near King of Prussia; all by Theodore D. Rand.

On a probable Pseudomorphism of Gummite and Uraninite; A new locality for Analcite; On Large Sphene from Canada; all by A. E. Foote.

Analysis of of Philadelphite, by Reuben Haines.

The so-called Emery ore from Chelsea, Bethel Township, Del. Co., Pa., F. A. Genth, Jr.

Some new Mineral localities, by Jos. Willcox.

Fresh-water Sponges of Fairmount Park, by Edw. Potts.

Notes on Jarosite, by Geo. A. Koenig.

Rhizopods in the Mosses of the Summit of Roan Mountain, N. C.; Bone Caves of Pennsylvania, by Jos. Leidy, M. D.

On the Timber Line of High Mountains; Dimorphic Flowers in Houstonia; Cleistogamy in Oxalis Acetosella, L., by Thos. Meehan.

On the Timber Line of High Mountains, by J. H. Redfield.

Sexual Variations in Castanea Americana, by Isaac C. Martindale.

We propose to prepare abstracts of the most important of these papers for publication in this Journal. That by Dr. H. C. Chapman, on the structure of the Ourang Outang, will probably be found in another portion of this issue.

HANDBOOK OF SYSTEMATIC URINARY ANALYSIS, CHEMICAL AND MICROSCOPICAL.—BY FRANK M. DEEMS, M. D.—The Industrial Publication Company, New York, 1880.

As a laboratory instructor in the medical department of the University of the city of New York, Dr. Deems must have acquired just the right sort of knowledge to fit him to write such a work.

The aim of the author appears to have been to offer a concise manual on urinary analysis, and to accomplish this end he has tabulated and arranged a large amount of matter, so that within a few pages and almost at one view the essential details of the subject are presented in a form for very rapid reference.

Several small handbooks have been recently published on this subject, which bear a remarkable likeness to one another, and we are glad to see that Dr. Deems has struck out in an original course, and produced a useful book which does not pretend to take the place of the larger works, but will be found of special value to medical students and physicians who require a synopsis of all the facts which it is important to remember in actual practice.

SOCIETY OF AMERICAN TAXIDERMISTS.

The first annual exhibition of the "Society of American Taxidermists," was held at Rochester, N. Y., commencing Dec. 14th and continued eight days. This Society, which is national in its character, was organized in Rochester, March 24th of the current year, with F. S. Webster as President and W. F. Hornaday, Secretary. It now numbers over forty active members besides several honorary members. Its scope is to advance the interest and raise to a higher standard of excellence the work in the line of science which its name indicates.

The exhibit consisted of about one hundred cases of birds in groups and perhaps twice that number of individual specimens ranging from a humming bird to an American Bison.

Perhaps the neatest thing, as "a thing of beauty," was a white heron mounted as a medallion on black velvet in an appropriate frame—the work of Mr. Webster.

The next meeting will be held at either New York or Boston, and as the initiatory one at Rochester was very successful in a pecuniary way, it can hardly fail of success in a large city.

The American Chemical Society will hold its first meeting of 1881 on Monday evening, January 3, at Room No. 1, University Building, Washington Square.

URANIA.

AN INTERNATIONAL JOURNAL OF ASTRONOMY.

Under the above title we intend from January next to publish a Journal for astronomical research, for which undertaking we are anxious to obtain your co-operation.

What chiefly induces us to increase the number of scientific periodicals by a new one is the circumstance that every year many astronomical papers and memoirs of great value are published in Transactions or Proceedings of Learned Societies, and, in consequence, remain unknown to many astronomers. In publishing *Urania* it is our aim to provide a receptacle for papers of this kind, which, we trust, will thereby become more generally known, and by preparing reviews or abstracts of all papers of any importance published in places where they seem likely to be overlooked, we shall endeavor to keep our readers as fully acquainted as possible with the progress of astronomical research. At the same time observations or ephemerides of planets, comets, etc., will not be excluded, and in addition to purely astronomical subjects, the essentially cosmical phenomena of magnetism as connected with Aurorae will also meet with their share of attention.

The journal will be printed in Dublin, and will be issued in numbers of from 16 to 24 pages each, demy 4to, whenever we have material enough for a number. Longer memoirs will not be cut up in several parts, but will be printed together in one number, or carried through two numbers without interruption. Shorter numbers will be issued whenever subjects of more immediate interest shall require it, and in particular ephemerides of comets, etc., will be issued on advance sheets, if necessary. Twelve numbers will contain 240 pages, and will form a volume.

The numbers will be sent by post to all subscribers. The subscription for one volume, including postage to all countries in the postal union, is Sixteen Shillings, payable in advance to the undersigned, J. L. E. DREYER. Post Office Orders to be made payable at the General Post Office, Dublin. Intending subscribers are requested to send in their subscription as soon as possible, in order that the number of copies to be printed may be known at once. No. I. will be published early in January.

We have already secured the co-operation of eminent men of science both at home and abroad. Thus the two first numbers will contain contributions from Lord Rosse, Lord Lindsay, Professor Klinkerfues, Professor Schjellerup, Dr. R. S. Ball, Dr. Börgen, and others. Papers written in French, German, or Italian will be printed in the original language (German MSS. are requested to be written in Roman characters).

RALPH COPELAND,
Observatory, Dunecht, Aberdeen.

J. L. E. DREYER,
Observatory, Dunsink, Co. Dublin.

SCIENCE:

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