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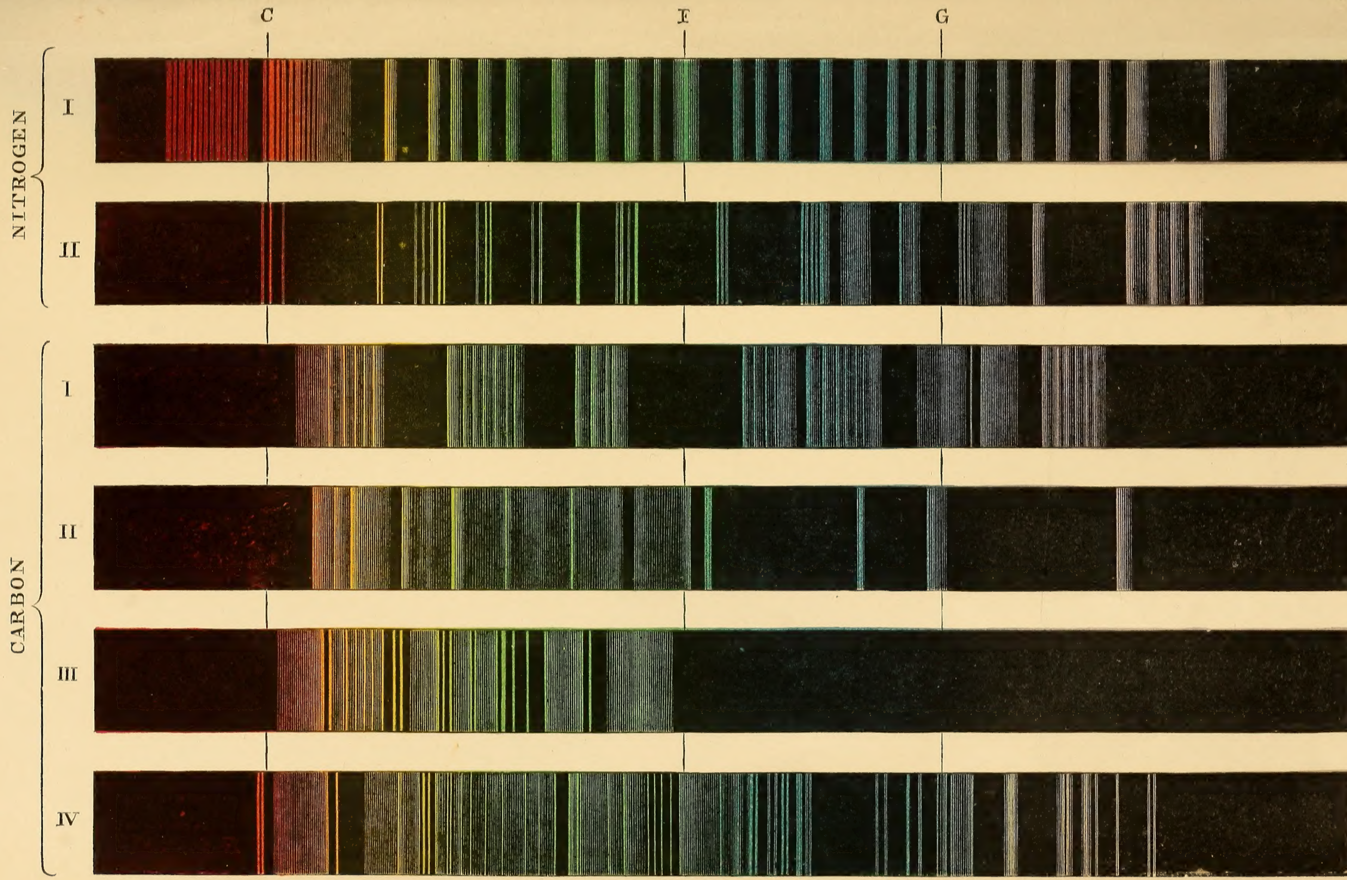

DOUBLE SPECTRA BY W. MARSHALL WATTS, D.Sc.

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JANUARY, 1871.

I. ON DOUBLE SPECTRA.

By W. Marshall Watts, D.Sc.

It is now just ten years since the first principles of spectrum analysis were established by Bunsen and Kirchhoff. It would probably be difficult to find any ten years in the history of science which have yielded such a glorious harvest of results. In these ten years we have not only become acquainted with four elementary constituents of the earth's crust, whose existence was previously not even suspected, but we have been enabled to extend our analysis beyond the earth, to far distant stars, and to learn the composition of our sun and of other suns, whose distance from us is so great that, in comparison with it, the immense space which stretches between the sun and the earth sinks into insignificance; nay, more, we have been able to detect star-motions which it is beyond the power of the telescope to reveal,—have gained extensive and important insight into the physical constitution of our great luminary, the sun, since the spectroscope enables us to observe any day, at leisure, phenomena which, without the spectroscope, can be studied only during the rare occurrence of a total eclipse, and are even able to look back into past time and to trace the history of the system of which our planet forms a part.

But we have in these ten years not only learnt new modes in which this mighty weapon of research may be employed, but have also made not less important progress in other directions, viz., in the establishment on sure ground of the principles on which the analysis rests, and in the definition of the precise conditions under which it is applicable. It must not be forgotten that every conclusion to which experimental science comes is a result of induction, and the danger of too hasty generalisation is manifest when we come to work with principles so potent as those of spectrum analysis. If, for example, we conclude because the spectrum of solar light contains a dark line of the same refrangibility as the dark line which can be obtained artificially in the spectrum of incandescent sodium vapour, that the sun's atmosphere


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It is proposed in this paper to describe some of the exceptions to first-enunciated broad principles which, though unsuspected at first, have been shown to exist since spectrum analysis has been known as a separate method of research.

Professors Bunsen and Kirchhoff, in their first memoir on spectrum analysis, describe the spectra of the metals of the alkalies and alkaline earths. They endeavour to establish firstly, that the spectrum is the same whatever compound of the metal is employed. If a bead of sodium chloride be brought into the Bunsen flame the spectrum will consist of two yellow lines only, and will remain the same if the sodium chloride be exchanged for sodium iodide, or sodium sulphate, or sodium carbonate. A second conclusion arrived at in the same memoir is that the position of the bright lines is independent of the temperature to which the vapour of the substance is heated. Professors Bunsen and Kirchhoff found differences of intensity only when they employed, instead of the ordinary Bunsen flame, the flames of sulphur, carbon disulphide, carbonic oxide, hydrogen, or the oxyhydrogen flame. They also compared the flame-spectra of sodium, lithium, potassium, strontium, and calcium with those obtained when the spark from an induction coil was taken between wires formed of the respective metals, and convinced themselves that the bright lines of the flame spectra were present in the same position, although other lines were seen which they supposed to be due to foreign metals present in the electrodes, and to the nitrogen of the air. Professor Kirchhoff expresses his opinion that, although the appearance of the spectrum may be very different under different circumstances, yet the position of the lines does not depend on the temperature.

"Even the alteration of the mass of the incandescent gas is sufficient to effect a change in the character of the spectrum. If the thickness of the film of vapour whose lines are being examined be increased, the luminous intensities of all the lines increase, but in different ratios. The intensity of the bright lines increases more slowly than that of the less visible rays. The impression which a line produces on the eye depends on its breadth as well as its brightness. Hence, it may happen that one line being less bright although broader than a second is less visible when the mass of incandescent gas is small, but becomes more distinctly seen than the second line when the thickness of the vapour is increased. Indeed, if the luminosity of the whole spectrum be so lowered that only the most striking of the lines are seen, it may happen that the spectrum appears to be totally changed when the mass of the gas is altered. Change of temperature appears to produce an effect similar to this alteration in the mass of the glowing vapour, no deviation in the maxima of light being observed, but the intensities of the lines increasing so differently that those most visible at a high temperature are not those most readily seen at a low temperature."

These conclusions of Bunsen and Kirchhoff are now known to be true only within certain limits. The spectrum of a substance may be very considerably altered by change of temperature, and these changes in the spectrum do not consist merely in the alteration of the relative intensities of the lines, but are caused both by the addition of new lines and by the actual disappearance of lines present in the spectrum produced at the lower temperature. We have in the lithium and sodium spectra examples in which the change caused by increase of temperature consists simply in the addition of new lines, and the higher the temperature the greater becomes the complexity of the spectrum. A bead of lithium chloride in the Bunsen flame gives a spectrum consisting of only one red line, whose wave-length is about 6684 ten-millionths of a millimetre, which corresponds to 32 of the scale to which the spectra accompanying this article are drawn. If the temperature be slightly raised by employing the blowpipe, an orange line at 44½ (wave length, 6107) makes its appearance. At the higher temperature of the oxyhydrogen jet a blue line at 105 (wave length, 4605) is added, while at the intense temperature obtained by using the electric light the spectrum gives a fourth line at 86. The sodium spectrum at the temperature of the Bunsen flame consists only of the double yellow line of the same refrangibility as the solar line D, but if the sodium compound be ignited in the electric arc the spectrum contains four other lines, each also double.

The high temperature spectrum of sodium is represented in Fig. 1, on the plan proposed by Bunsen. The position of the bright bands on the illuminated millimetre scale of the spectroscope is shown by the position of the black lines, while the intensity is indicated by the relative height of the lines. In regard to the other conclusion of Bunsen and Kirchhoff, that all compounds of a metal give the same spectrum, we now know that this is true only if the metal is one whose compounds are decomposed even at the low temperature of the flame. It is well known that a sufficiently high temperature causes the decomposition of many chemical substances into their elements, and that these re-combine when the temperature is allowed to fall again. Thus, sodium carbonate and sodium nitrate give the same spectrum, because each is decomposed in the flame yielding metallic sodium by the incandescent vapour of which the yellow line is produced.

But we have compounds which do not split up into their elements in the flame, although they are decomposed in the intense heat of the electric spark. Such a substance is copper chloride, which volatilises in the flame without being decomposed, and gives a spectrum which is altogether different from the true spectrum of copper obtained when the electric spark is allowed to pass between copper poles. There is no doubt that the spectrum is that of the compound copper chloride. Fig. 2 shows this spectrum compared with that of copper.

The spectra of barium, strontium, and calcium show differences at different temperatures, which are probably to be explained in the same way. A reference to Fig. 3 will show the great difference observed when the calcium spectrum is produced by taking the electric spark in an atmosphere containing calcium, and when it is produced by bringing a bead of calcium chloride into the Bunsen flame. Figs. 4 and 5 show the spectra of strontium and barium respectively under the same circumstances. It is supposed that in the flame spectrum the incandescent substance to which the lines of the spectrum are due is calcium or strontium oxide, so that we have the spectrum of a compound, but when the electric spark is used, the oxide is decomposed, and we have the true spectrum of the metal. It should be remarked here that the true metal spectrum consists invariably of sharply defined lines, while a compound gives a spectrum containing broad bands, which show a family resemblance amongst themselves and are often repetitions of each other. This is seen in Figs. 2, 3, 4, and 5. The spectra of those metals whose salts are easily decomposed in the flame—for example, sodium, lithium, and thallium—give spectra containing only lines, and the only change producible by increase of temperature is the addition of new lines, while, in the case of metals whose salts are not so easily decomposed, the increase of temperature not only adds new lines, but also splits up the bands into groups of fine lines.

We observe the same thing amongst the non-metallic elements in the case of cyanogen. If the flame of cyanogen burning in air be examined with the spectroscope, a magnificent spectrum is seen, which is obviously made up of two different spectra; one stretching from the light green into the blue exhibits a series of groups of lines, in each of which the brightest line is towards the red, and each group fades away on the side towards the blue; but the red end of the spectrum shows a series of groups of lines of exactly the opposite character—the brightest line of each group being on the side towards the blue, and each group fading away on the red side. We shall see afterwards that the blue portion of the spectrum is due to the element carbon, but the red end is produced by the compound cyanogen; and if the cyanogen be burnt in oxygen instead of air, the two of these groups most towards the blue become replaced by carbon lines, while if the gas be ignited by the electric spark, the whole of the cyanogen bands disappear, and the spectrum consists altogether of carbon lines.

It is a coincidence which doubtless has its own significance that, in all cases where by increase of temperature the bands of the compound are made to give way to the lines of the element, the change takes place earliest in the blue end of the spectrum, and proceeds gradually towards the red. The flame-spectrum of strontium contains one line Srδ of the metal which is seen also in the spark-spectrum. The calcium flame-spectrum also contains one line (135) which remains unchanged on increasing the temperature to that of the spark, and in both cases these lines of the metal terminate the spectrum towards the blue end.* The flame-spectrum

* I owe to my friend Mr. Aldis the probable explanation of this peculiarity as also of the way in which the carbon bands shade off uniformly towards the blue.

of barium contains no line of the metal, since barium oxide is less easily decomposed than either calcium oxide or strontium oxide.

But amongst all the additions made to our knowledge of spectrum analysis within these ten years, none is so startling as the discovery, which we owe to Plücker, that a substance may give two totally different spectra which have no line or band in common. In a paper published in the "Philosophical Transactions" for 1865, Plücker and Hittorf describe double spectra of nitrogen, sulphur, selenium, hydrogen, and iodine. Nitrogen exhibits this peculiarity in a marked manner. In order to obtain its spectrum it is necessary to employ electricity, as no flame is hot enough. If an ordinary vacuum-tube containing nitrogen have the current from an induction coil sent through it, the narrow part of the tube gives out a purple light, which is resolved by the prism into the spectrum represented in the chromo-lithograph—a spectrum consisting of an immense number of shaded bands.

If, instead of using nitrogen at low pressures, we let the spark pass in the gas at the ordinary pressure, and intensify it by connecting the two wires with the outer and inner coatings of a moderate sized Leyden jar, we obtain an intensely bright light, which gives a spectrum also represented in the chromo-lithograph. This second spectrum is entirely different from the other, consisting only of sharply defined bright lines. Plücker terms these spectra, spectra of the first order, and of the second order, respectively. It will be observed that these spectra possess, respectively, the

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The vibrations which produce light depend, so far as we can see, on the manner in which the atoms of a molecule are in equilibrium. We see from the occurrence of lines in all parts of the spectrum, that there are in the molecule several different vibrations executed simultaneously, and these correspond to the greater or less intensity of the force by which the atoms are maintained in their position of equilibrium. The more intense the force by which an atom is held in equilibrium, the faster it will vibrate when set in motion.

When cyanogen is moderately heated its molecules vibrate in the regular way indicated by the cyanogen spectrum; but when the temperature is raised the compound is dissociated, and the carbon atoms vibrate uninfluenced by those of nitrogen. Now, when this takes place, the vibrations of the cyanogen which first disappear must be those due to the closest intimacy—that is, the most rapid. Hence the carbon spectrum comes in at the *blue* end.

Further, the vibrations of an atom about its position of equilibrium will not all be of equal length, and so will produce light of varying *intensity*. If the vibrations are quite cycloidal, all will be executed in the same time, and we shall have a *sharp bright line*, but if the vibrations are like those of an ordinary pendulum, the smaller vibrations will be performed in less time than the larger, and the result will be a *band fading off into the blue*. If, on the other hand, the vibrations of small amplitude are executed more slowly than those of larger amplitude, the result will be a band fading off *into the red*.

characteristics of those produced by compounds and by elements. It remains for future experiments to confirm or modify the indication thus given of the compound nature of nitrogen. Similar results were obtained by Plücker with sulphur.

In order to experiment with sulphur, a tube of difficultly fusible glass was employed. Sulphur was introduced into the tube, which was then completely exhausted. When the narrow part of the tube is gently warmed by a spirit-lamp, and the platinum wires are connected with the induction coil, a spectrum of the first order is obtained. It consists of thirty-seven well-defined bands extending from the red into the extreme blue. Seven lie between the solar lines C and D, eighteen between D and F, and eleven between F and G. On heating the tube still more a quite different set of bright lines makes its appearance, and on introducing a Leyden jar into the circuit the second spectrum becomes fully established and no trace of the spectrum of bands remains. This second spectrum consists entirely of sharply defined bright lines—two red lines are especially noticeable, each of them triple.

It is thus established that certain gases may, under altered circumstances, vibrate in an entirely different manner, and Plücker believes that the necessary difference of circumstance is simply difference of temperature, the spectra of the first order belonging always to the lower temperature. The Leyden jar increases the temperature of the gas, for it necessitates the accumulation of a larger quantity of electricity preparatory to each discharge, so that the temperature of the spark with the Leyden jar is much higher than that of the simple discharge. Thus we see that, on heating the sulphur-tube and employing the Leyden jar, the low temperature spectrum gives way to the high temperature spectrum.

The spectrum obtained from an ordinary vacuum-tube containing hydrogen (under a pressure of 5 to 10 millimetres), consists of three lines only—$H\alpha$, coincident with Fraunhofer's line C in the red; $H\beta$, coincident with F in the blue; and $H\gamma$, nearly coincident with G in the violet.

Plücker, who first observed this spectrum (Fig. 6), described, in the paper already referred to, a second spectrum of hydrogen, corresponding to a lower temperature. This observation has been abundantly confirmed by important experiments, made since the date of Plücker's paper, by Prof. Wüllner, of Bonn; and his results are so remarkable that it will be well to describe them somewhat at length. His apparatus consisted of a vacuum-tube of the ordinary shape, which formed the upper portion of the shorter leg of a U-tube; the long leg of which was about eight feet high. The spectral tube was connected by one side-tube with the apparatus for preparing pure hydrogen, and by a second side-tube with a Geissler's air-pump, by means of which any degree of exhaustion could be obtained, while, by pouring mercury into the long leg of the U-tube, the pressure could be increased up to about three atmospheres.

Prof. Wüllner found that, using the discharge from an induction coil, the spectrum obtained varied essentially with the pressure. With the lowest pressure which could be obtained, the light in the tube is of a splendid green, like a thallium flame, and the spectrum does not contain either the red line or the violet line of Plücker's spectrum, but consists of six groups of very brilliant green lines (Fig. 6, 3). At pressures of one, two, or three millimetres the spectrum is the well-known one consisting of the three lines without any of the green lines; and upon increasing the pressure still further, another spectrum makes its appearance which contains $H\alpha$ and β, but not $H\gamma$, and the space between $H\alpha$ and $H\beta$ is filled up by a number of beautifully shaded bands, which also extend somewhat beyond $H\beta$; this spectrum persists till the pressure rises to about 400 millimetres. It is represented in Fig. 6, 2. Prof. Wüllner thinks that these different spectra of hydrogen are to be explained by differences of temperature, and that the spectrum last described is due to a higher temperature than that which produces the spectrum of three lines.

The results obtained with oxygen were quite similar. At the lowest pressure it gave a spectrum consisting of five groups of fine lines in the green and blue; at pressures of about one millimetre a second spectrum of broad bands; and at higher pressures a third, consisting of a great number of fine lines.

Nitrogen, on the other hand, only gave the two spectra already described.

The same memoir of Prof. Wüllner contains a description of two different aluminium spectra. Both were obtained by letting the spark strike between wires of aluminium, but the spectrum varied with the distance between the electrodes. With a distance of about two millimetres the spectrum (Fig. 7, 2) consists of four green splendidly shaded bands, brightest on the side towards the blue, and each traversed by fine bright lines. When, however, the spark distance exceeded ten millimetres, a quite different spectrum was obtained, which is no doubt produced at a higher temperature. This spectrum, which is represented in Fig. 7, 1, consists of a number of bright lines and groups of lines standing out from a feebly illuminated background.

The changes of spectrum shown by the element carbon are, perhaps, as curious and interesting as any. At first sight it would appear that carbon is an element unlikely to yield a discontinuous spectrum, inasmuch as it is not known in the gaseous condition; and, that if we obtain discontinuous spectra from carbon compounds, they must be due to some compound of carbon. Thus the bright blue lines observed by Swan (1856), in the spectrum of the Bunsen flame, might be supposed to be more probably due to carbonic oxide or carbonic acid than to carbon itself. But we find that these same lines occur not only in the spectrum of the flame, but also in the spectra obtained by passing the electric spark either through carbonic oxide, or olefiant gas, or cyanogen, and the lines thus found to be common to compounds of carbon with different elements must of course be due to carbon itself. Whether they are really produced by carbon in the gaseous state is a question which cannot yet be certainly decided. If the carbon is in the solid state we shall then have an exception to the law that incandescent solids give continuous spectra, of which we have only one other example, viz., the spectrum of bright lines obtained by Bahr and Bunsen from glowing erbia. In the case of erbia it is not impossible that the bright lines are really produced by a gas (Huggins and Reynolds, Proc. Roy. Soc., June 16th, 1870), and it is by no means improbable that, when a hydrocarbon is burned, it is first of all decomposed into its elements, which then combine with oxygen. If this be so the carbon may exist for the moment in the gaseous state.

There exist no fewer than four spectra which are all probably due to incandescent carbon vapour. The first carbon spectrum is obtained when olefiant gas and oxygen are burnt together in an oxyhydrogen blowpipe-jet. The flame thus obtained exhibits a central cone of intense green, which, examined by the spectroscope, gives the spectrum first obtained by Swan, and ascribed by Attfield to the vapour of carbon. This spectrum, which is represented in the chromolithograph, No. 1, is one of the most beautiful which can be imagined, and consists of five groups of lines—α in the red, γ in the greenish yellow, δ brilliant emerald-green, ϵ in the blue, and f violet.

Group a contains five lines, of which the third is the brightest. γ contains seven, of which the least refracted is the brightest, and each succeeding line is less brilliant than the one before; so that the group rises sharply out of darkness on the left, and fades gradually away on the right. The group δ, which contains four lines, presents the same gradation of intensity; ϵ contains four lines of nearly equal intensity, the fourth being double; and f consists of a broad band, then a fine bright line, and then a band fading away on the most refracted side. When the spectrum is obtained very brightly, there may be observed in addition six very fine bright lines of equal intensity, which gave the readings 86, 87·5, 89, 91, 93, 95. The band 128—133 is also seen to be shaded by a large number of nearly equidistant fine dark lines; and the least refrangible band of the group f (121—126) is resolved into lines.

This spectrum may be obtained from the flame of any hydrocarbon, though in many caess, owing to the faintness of the spectrum, only some of the groups can be recognised. In the flame of an ordinary Bunsen burner δ and ϵ are easily seen, γ and f are much fainter, and the red group cannot be detected.

When the temperature is sufficiently high, we have, instead of the group f, two other groups, ζ and θ, which are also represented in the chromolithograph. Group ζ contains seven lines (105—113) and group θ contains six (136—142).

The lines of the second carbon spectrum were first observed by Plücker in 1859, and supposed by him to be the lines of the compound carbonic acid, but the fact that they really constitute a spectrum of carbon, since they can be obtained either from carbonic oxide or from olefiant gas, was first noted by Roscoe in 1864. This spectrum is represented in Fig. 2 of the chromolithograph.

The third carbon spectrum is that of the flame which issues from the converter in the Bessemer steel process, in which air is forced through molten iron. It is represented in the chromolithograph, Fig. 3, and is remarkable because it consists of groups of lines, in each of which the brightest line is the most refrangible—an aspect which is exactly the reverse of that so noticeable in the ordinary carbon-spectrum, where each group has its strongest line on the left hand. These lines are unquestionably produced by carbon in some form or other. They disappear when all the carbon has been burned out of the iron, and their disappearance forms the most delicate test by which to determine the right point to stop the blast.

The fourth carbon spectrum is obtained when the spark from an induction coil is taken in carbonic acid, and a

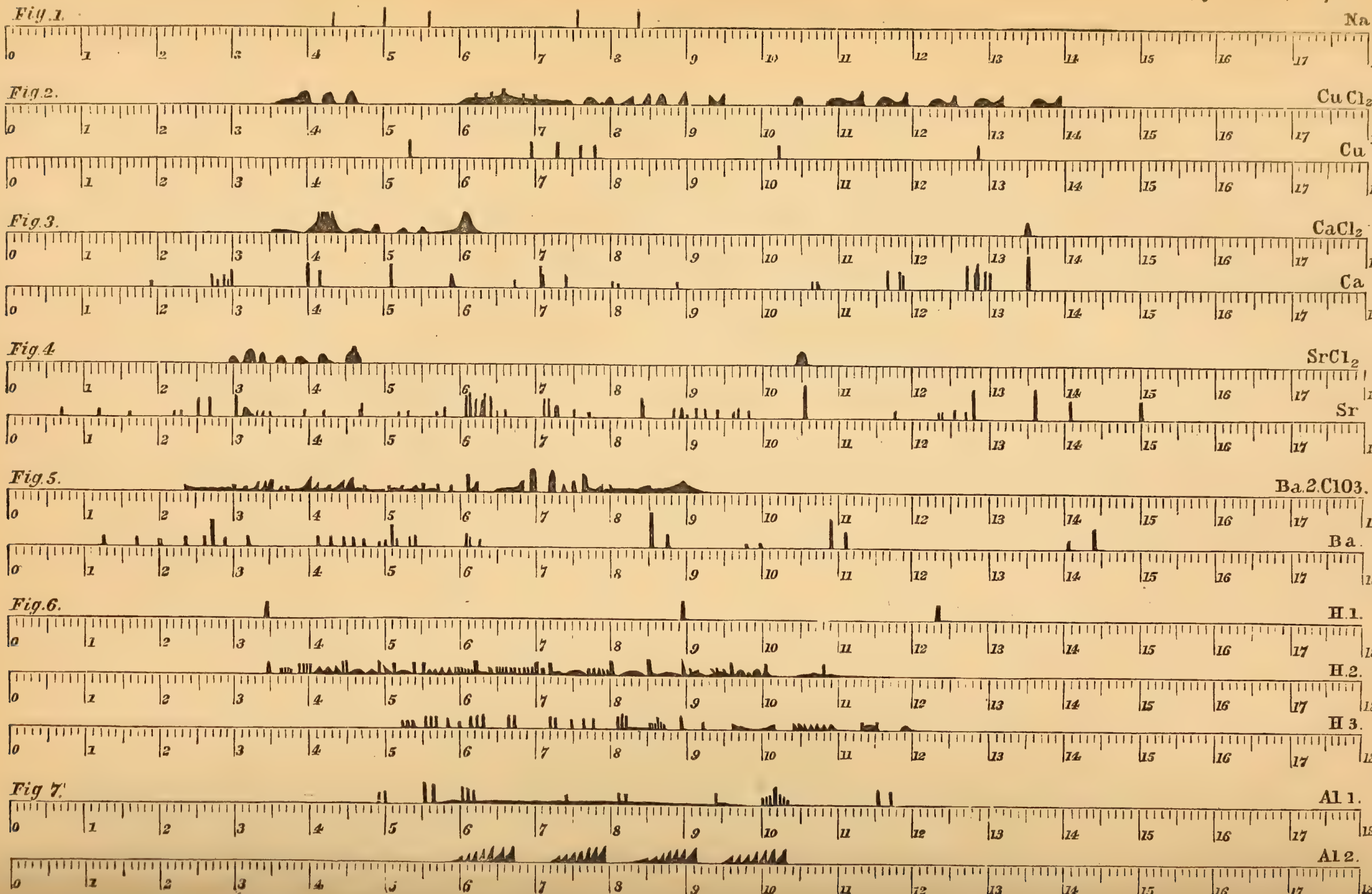


Fig. 1.
Na
Fig. 2.
Cu Cl₂
Cu
Fig. 3.
CaCl₂
Ca
Fig. 4.
SrCl₂
Sr
Fig. 5.
Ba.2.C103.
Ba.
Fig. 6.
H.1.
H.2.
H 3.
Fig. 7.
Al 1.
Al 2.

Leyden jar is included in the circuit. The spark without the Leyden jar is not hot enough to decompose carbonic acid, and gives in that gas only a continuous spectrum. In carbonic oxide (which seems to be more easily decomposed) it yields the first carbon spectrum, but when the Leyden jar is employed, both gases are decomposed with deposition of carbon, and give the same spectrum. This fourth carbon spectrum (represented in the chromolithograph, Fig. 4) is one of Plücker's "spectra of the second order," consisting of sharply defined lines, often in pairs, and is no doubt produced by carbon at an extremely high temperature.

The attempt to ascertain the circumstances under which these four carbon spectra are produced, has led to a result which, if it can be maintained, is of the greatest interest.

We have seen that the explanation given of the different spectra of hydrogen is that they are produced by the gas heated to different temperatures, and there is no difficulty in conceiving that the particles of a gas may vibrate differently when the gas is differently heated. We have only very rough means of measuring high temperatures, and the determination of the temperature to which the gas in a flame is heated can only be approximate. It is true that we can calculate the temperature of a flame from the known amount of heat given out by the gas in burning, but these results are always too high, in consequence of conditions which it is impossible to take into account in the calculation. We have, however, one or two experimental determinations of value. Deville and Debray have determined the temperature of the oxyhydrogen flame by heating a mass of platinum to the highest possible temperature in a crucible of lime, and then plunging it into water, and determining the temperature to which the water is heated, and find in this way that the temperature is not higher than 2500° C. Bunsen, by a quite different method has found for the same number 2800° C., which, considering the difficulty of the determination, agrees satisfactorily with Deville's result. Bunsen has also found that the temperature of the flame of hydrogen in air is 2024°, of carbonic oxide in air, 1997°, and in oxygen, 3033°, and of cyanogen in air, 3297° C. Deville and Debray have also determined by their method the fusing point of platinum, which they find lies between 1800° and 2000° C.

The first carbon spectrum is obtainable from flames which cover a considerable range of temperature. It is given by the blue cone of a Bunsen flame, the temperature of which is not high enough to melt the finest platinum wire, and must therefore be less than 2000° C. It is also given (with the addition of the groups ζ and θ) by the flame of cyanogen in air, the temperature of which we have seen to be 3297°, and by the flame of cyanogen in oxygen, which is the hottest flame known, and is probably at least 5000° or 6000° C. We see thus that the lines of the first spectrum are given by carbon heated (at any rate) between 2000° and 3000° C. Now it can be shown that the second carbon spectrum can also be produced by carbon heated within the same range.

This second spectrum is not obtainable from any flame at all, but is produced by the electric discharge in either carbonic oxide, or olefiant gas, or carbon disulphide.

We have seen that the sodium spectrum contains, when the temperature is sufficiently high, besides the well-known double line, four other lines, each of them double; Naβ at 56, Naγ at 75·5, Naδ at 83·2, and Naϵ at 43. In the Bunsen flame, the D lines only are obtained; but, if the temperature of the flame be increased, these other sodium lines come out one by one, and in the order given above, Naβ becoming visible almost precisely at the temperature at which platinum melts—that is, 2000° C.; so that, if a bead of sodium carbonate be brought into any flame incapable of fusing platinum, only the D lines will be seen, but, if the flame be hot enough to fuse platinum, Naβ will also be visible.

For example—

The Bunsen flame gives the D lines only, and is incapable of fusing platinum.

The flame of coal-gas, fed by a jet of air mixed with a little oxygen, gave Naβ, but not Naγ. The flame fused platinum easily.

The flame of carbonic oxide in air (temperature, 1997° C.) gave only D; it is incapable of fusing platinum. Carbonic oxide fed by oxygen (temperature, 3033° C.) gives Naβ and Naγ.

We conclude, therefore, that Naβ indicates a temperature of at least 2000° C., and that Naγ comes out about 3000° C.

If now the discharge of an induction coil be passed through a Geissler's tube containing carbonic oxide into which some pieces of sodium have been introduced, the carbon spectrum No. 2 is obtained; and if the sodium be volatilised, the sodium lines come out one by one as the tube gets hot, and the carbon lines are seen simultaneously with Naβ, and (at first) without Naγ. Hence we conclude that this second spectrum is also produced by carbon between the temperatures of 2000° and 3000° C., and we are therefore unable to ascribe the differences observed to difference of temperature.

The conclusion drawn from this experiment appears to open up new views of the cause of double spectra, and to demand further investigations which may throw light on the molecular constitution of gases. We know so little about the mode in which those vibrations of the atoms of a gas take place by which light is produced, that it is not of much use to speculate on the causes of the difference of light emitted at the same temperature. We know, indeed, that heat is caused by the motion of the molecules of a gas, and that the faster they move the higher is the temperature of the gas. If the vibrations which constitute light are executed by the constituent atoms of the molecules, it is possible that the atoms may vibrate differently, although the motion of the molecules is the same—that is, the light emitted may vary, although the temperature is the same.*

Further experiments may enable us to distinguish between the effect of change of temperature and change of pressure. It may be noted that the second carbon spectrum has been obtained only from gases at pressures less than 100 m.m., although the first carbon spectrum (which is produced at the same temperature) can be obtained either from gases at low pressure or at high pressure. It is understood that Messrs. Frankland and Lockyer are engaged in experiments which will throw light on this subject.

* If this supposition is correct, increase of temperature ought to cause the lines of a spectrum to expand, since, in the motion of a molecule towards the observer, the refrangibility of the light it emits ought to be increased, and in the opposite motion ought to be as much diminished, The expansion which the lines of hydrogen undergo under increase of pressure is, however, too great to be accounted for by any possible consequent increase of temperature.

II. THE GREAT PYRAMID OF EGYPT,

FROM A MODERN SCIENTIFIC POINT OF VIEW.

By C. Piazzi Smyth, Astronomer Royal for Scotland.

Part I.

Hearing, discussing, and disposing of the older claims, of History (as hitherto written), Architecture, Archæology, Egyptology, Progressive development, "Theotechny," and the British Museum, to explain any of the chief data of the Great Pyramid.

1. *Touching the General Status of both the Structure and its Primeval Authors.*

SELDOM does a practical astronomer occupy himself much with matters of very high antiquity; and why should he, or what improvement is he likely to gain in his own art by studying what has descended from those dim, primeval times?

Truly great may the Greeks, so very generally taken as types of all that is grandly intellectual in antiquity, have been in æsthetic appreciation, or as ethical writers; supreme, too, in that deceptive art, which, in the eyes of more than one of our living statesmen, has elevated Homer to a greater height than all his unrivalled poetry, viz., his "Theotechny;" but who amongst *those* ancients, whether Pelasgic or Hellenic, had any notion of measuring an angle in the sky with what we should call now even decent accuracy?

They are said, those Greek philosophers (and of course in the present enquiry mere Romans are too modern to appear at all), after their civilisation had gone on growing for several centuries, to have ascertained, about the date of 330 B.C., that the then Pole star, some 6° from the Pole, was not in the very Polar point. But as to measuring its distance therefrom down to minutes and seconds of angle, that was totally beyond their powers of comprehension, or even their very imagination of the possible. Not that they were necessarily inferior by nature to the men of the present day, in the improvable faculties to be exercised in that science; but that that science or art, viz., angular measure, does not, and cannot spring into life full and complete at once like a Divine gift, for it is, *par excellence*, the choicest fruit of man's own progressive development of his own powers through long succeeding ages of steady, undeviating, unwearied toil.

Hence, if we should desire to see with exactitude what really has been accomplished in the world of man, by that sometimes overpraised, sometimes under-valued, "progressive development," no better example can be studied than the history of practical astronomy from the date of Pytheas, of Marseilles, 2200 years ago, with his infantine tale about the Pole star, down to the present time, when—though even the most accomplished observer is still separated from absolute perfection by difficulties which are always found to underlie, with even increasing pertinacity, every successive fraction of a second that man may be enabled to reach in his results,—just as invincibly, indeed, as a boundless ocean separated Newton and his philosophical discoveries among the mere pebbles of his beach from the end of discoverable things,—yet a well-taught schoolboy may now measure an angle in the heavens to a degree of accuracy that the whole Grecian race, even in its day of plenitude, was perfectly incapable of.

Quite certain, therefore, may we be, on continuing the observed rate of progress inversely backwards, that amongst all the Greek, and Latin races too, we need not look for anything respectable in practical astronomy, at or about 3000 years ago; less still at any earlier dates; and yet my task is now to direct my reader's attention to a monument far older still than anything yet mentioned, or of more than 4000 years ago; and to show that it is one where practical performance was carried out with a degree of excellence, which may form a cynosure for emulation to some of the most skilled workers and learned thinkers amongst us still, even in astronomy!

In such case, of course this monument, the Great Pyramid of Egypt, is not Greek, nor Latin, nor Phœnician, nor even Assyrian. And if we were to add that it has nothing to do either with ancient idolatry in any shape, or the worship of false gods, or the vain glorification of any mortal man,—the results of the closest scrutiny yet made would entirely justify the assertion; at the same time that it brought to light also overwhelming proof of an abiding and all-directing belief in the immortality of the soul, and a future judgment having then firmly obtained.

Most essential is it to be warned on these points, because, while the Great Pyramid does in so far stand just within or upon Egypt; it is therefore by one class of unthinking persons at once confounded with and attributed to the inventions of those long subsequent, but still so-called "ancient Egyptians," but of "the New Kingdom," who formed the well-known Pharaonic despotisms of history, and who erected innumerable temples—marvels no doubt of colossal art, but teeming over all their surfaces with engravings and pictures of every kind of abominable animal-headed idol-god; and by another class it is equally, but erroneously, attributed to early ages of the world, when, according to the doctrine of progressive development rather strongly applied, it is believed that man could then nowhere have been in possession of any clear ideas of his soul's life, of an eternal and all-wise Creator, and future bliss or pain transcending everything in this present world of trial.

Wherefore, whoever the builders of the Great Pyramid were, and whenever they lived,—and it will be our business presently to endeavour to find out,—let no one fancy that with them he is in the presence of none but necessarily either rampant idolaters or miserable men entirely atheistic.

2. *The Great Pyramid the Oldest Monument in Egypt.*

Visiting in the present day the land of Egypt, that country so inimitably prepared by nature, with the mildest of climates and driest of atmospheres, to be both the perpetual storehouse of human monuments and faithful keeper of historical records,—all travellers and all writers declaim on the almost perfect preservation of every document, even down to the seals impressed by many centuries of kings on sun-dried bricks of Nile mud. Indeed there, with ease, backward you may step across the ages that are gone, and read on the later monuments records of the first wild rush of Mohammedan conquerors, issuing, 1200 years ago, locust-like from their Arabian home to purge out the idolatries of the Greek Christian Church degraded; then beyond that you may arrive at proofs of the Roman rule; beyond that, at Macedonian Ptolemies, enervated and debauched in a southern climate; then the cruelties of Persian despots; then Egyptian kings, who fought over Jerusalem with Nebuchadnezzar and monarchs of Mesopotamian line; then the Theban kings, who revelled through long ages in idolatry carried to its worst culminations; then to Sesostris and his supposed wars, but really oppression of Israelites in Egypt; then to Memphian kings who ruled the Delta-land mildly in the times of Joseph; then to older Dynasties among whose tombs some Pyramids are found; then up through many Pyramid-building kings to him at last, King Cheops, Suphis or Shofo, who built, or rather during whose reign *was* built, that Pyramid which all the world still calls the Great Pyramid. The greatest, too, it is of all the series of Egyptian pyramids; and, most passing strange to say, when with that quality, *the first.* Not only, too, the first Pyramid of all known Pyramids, but the first example of all known architecture in *stone.*

Before that remarkable building, as we shall presently show from independent and impartial authority, there is no known monument in all Egypt; no material existing proof of the handy-work of man, unless it be some flint chips. *After* that building comes everything of the hosts of acknowledged Egyptian remains, but nothing equal to it. The Pyramids subsequently built were all smaller, less perfect in mechanical construction, without science in design, and tending to meretricious in taste. The Great Pyramid is alone a perfect example in design and execution, both æsthetically and mechanically; and, as we are to show, it is the largest; and it was the first.

3. *The Superior Antiquity and Constructive Excellence of the Great Pyramid as Testified to by Egyptologists.*

Now, each and all of these features of the Great Pyramid are totally opposed to the favourite modern doctrine of "progressive development;" and, whereas the methods of "scientising" in the present day have been for long almost confined to looking out for some minute discrepancy, just perceivable in telescope or microscope, between theory and observation, and following up that little difference until its reason has been discovered,—what amount of attention ought not to be given by all engaged in any branch of anthropological science, to such an entire and confounding difference from theory as this; viz., that the first stone building ever erected by man, instead of being the smallest and worst, is actually the best and highest of all that have ever been erected on the face of the earth!

My readers will require some proof of these things; and though to set forth full proofs of the greatest marvel in all the general history of man, within the compass of one or two short papers, is totally beyond me, I will yet attempt something in the direction of at least showing what kind of data there are from which proofs may be extracted, if men would only apply themselves heartily to the task. And it may, perhaps, expedite our progress if I quote, whenever printed facts allow of it, not so much from my own observations, as from those authors who hold opposite opinions to myself touching explanations and conclusions, but who are allowed by all the world to be deservedly great in practical Egyptology, or a knowledge of the monuments still standing in Egypt, and by means of their own examinations, readings, and measures.

Thus, as regards that strange priority of the Great Pyramid to all other monuments of Egypt (and if of Egypt then of the whole world), what says Dr. Lepsius, the most experienced hierologist of the day; but one who really cares little or nothing about the Great Pyramid on all the counts of the new *scientific* theory, because regular Egyptian hieroglyphics of the late Theban date are his *forte*, and the most fearful pictures of revolting animal gods, the study of all others that his soul delights in; and these things he does *not* find in the Great Pyramid. Thus, then, on the one required point of superior antiquity, writes the famous Prussian *savant*, "the hope of Egyptology," as he was begun to be designated even in his youth.

"Nor have I yet found a single cartouche that can safely be assigned to a period previous to the fourth dynasty. The builders of the Great Pyramid seem to assert their right to form the *commencement of monumental history*, even if it be clear that they were not the *first* builders and monumental writers."

And again,—

"The Pyramid of Cheops, to which the *first* link of our whole monumental history is fastened immovably, not only for Egyptian, but for universal history."*

And still again, but this time to come closer home, take the most literary architect of the day, or that has yet appeared in our literature,—James Fergusson,—whose soul likewise delights in the horrible details of such things as "Tree and Serpent worship" by immortal man, and in degrading forms of idolatry which he persists in declaring, on mere *hypothesis*, to have been *necessarily* the earliest forms of worship for all men on their gradually emerging, according to that hypothesis, by their own powers alone, out of a lower than merely savage condition. Even he then, our great æsthetical historian of the absolute and material, after confessing that—

"No one can possibly examine the interior of the Great Pyramid without being struck with astonishment at the wonderful mechanical skill displayed in its construction."

And even adding to that,—

"Nothing more perfect mechanically has ever been erected since that time."

* Dr. Lepsius's "Letters from Egypt in 1843." Compare also the chronological arrangement of Lepsius's unrivalled collection of folio Egyptian lithographs, in the *Denkmaeler*.

Yet even he is fain to allow that—

"Stretch the history of architecture how we will, we cannot get beyond the epoch of the Pyramid builders (of Lower Egypt)."

Or, as another learned writer in the 60th edition of a British Museum catalogue prints it—

"The earliest known architecture, the Pyramids of the fourth Dynasty."

A few years ago, certain ruined masses amongst them were thought to be older than the Great Pyramid, the chief work of the said so-called or reputed fourth dynasty, and that Cambridge Humboldt of his day, the travelled Dr. Clarke, at the beginning of the century pronounced from his boat on the Nile that he could distinguish a long succession of earlier ages to that of the Great Pyramid, by the continued approach in *their* pyramids to a mere mound of soft earth. But as all the more signal of his examples have since then been positively proved by the disciples of Champollion and his post-Clarkian science of hieroglyphical interpretation to have been built by monarchs of dynasties long subsequent to King Shofo of the fourth, and to owe their mouldering forms and subsiding masses to their having been built of perishable brick in place of lasting stone, the claim of the Great Pyramid to be the earliest builded monument that man can put his hand upon in the present day, is now almost, if not quite, universally allowed.

This claim, too, is equally good, no matter whether a long or a short chronology for Egypt and the East be adopted; for, in so far depending, as we are now doing, upon, and giving all honour to, the disquisitions of *literary* Egyptologists, it is *differential* date only. The Great Pyramid itself, questioned on the scientific theory presently to be expounded, can declare its own *absolute* date with admirable exactness; but in this first part of my paper a differential date is enough for our purpose, and I am anxious to make full use of whatever standard points have been ascertained in past years by the literary gentlemen, acknowledged leaders of Egyptology—good men undoubtedly, and very able, some of them, too, gifted with actual genius.

Wherefore *their* conclusion as to the earlier *comparative* date of the Great Pyramid to that of any other known building, whether in Egypt or elsewhere throughout the known world, is a fact of the first magnitude, to be kept vividly before us during the whole of our present inquiry.

4. *The Egyptologists* versus *the Ordinary Arguments of Progressive Development.*

In fact, the priority of date (whether it should eventually be proved absolute and entire, or just short thereof by a very few years, a mere nothing at this distance of time, and equally opposed to progressive development) is the beginning of the new theory of the Great Pyramid, the very key-note of that grand and solemn story which its component stones have, and are able, to tell. For even thereby that building stands unique upon earth; there is not anything architectural so old elsewhere, and yet by modern architects, upon mere ordinary, superficial, but sound, architectural grounds, it is pronounced to be the best built monument ever yet erected, even down to these times in which we live.

What conclusions, then, are we thence to draw?

What, indeed! For precisely at this point begin the utter antagonisms between the advocates of the new theory and the older Egyptologists.

They, the Egyptologers, say (see Bunsen, see Fergusson, see Renan) "*because* the Great Pyramid is so big and so admirably built, *therefore* infinitely long ages of slowly growing, improving, developing, architecture *must* have preceded it."

Yet why *therefore*, and why *must*? Those phrases depend merely on the human hypothesis of progressive development, combined with the gratuitous assumption of its having been the only nursing-mother or foster-father of the human race during all that primeval period of unknown length, which preceded the first of our veritable historical records, and for, or of, which we have no mundane material data.

The new theorists, on the other hand, following out the ideas of the late John Taylor, of London, feel compelled to assert that, "if such long-continued ages of progressive architecture *had* preceded the Great Pyramid, the remains thereof, especially in such a monument-preserving climate as that of Egypt has already been proved to be, ought to, and would, have been found most extensively and abundantly, even to far outnumbering all the builded monuments that have been erected since. Yet the actual and admitted fact by every able man who has searched Egypt and the adjacent countries is,—*after* the Great Pyramid comes the date of everything of the architectural known; *before* it there is nothing."

The Egyptologers are perfectly aware of this profound difficulty, and how do they treat it? Some of them with imperious coolness disdainfully ignore it altogether, and go on undisturbedly detailing to their willing readers long lists of paper kings, and the length of the years of their imagined reigns over a period of more than 10,000 years before the Great Pyramid. But a fully honest man, like James Fergusson, freely confesses the tightness of the case he finds himself shut up in, and ingeniously puts forward two attempted explanations—as thus:—

1st. He hopes that the architecture of those very early and most lengthy theoretical periods *will* be discovered some day. And,—

2nd. He has a theory based on facts in his own science, that the first example of any new idea in architecture is always the finest!

The former reason, so little complimentary to the present state of our knowledge of the surface of the earth, may be safely left to itself: but the latter one touches a point wherein no one can cross or neglect Mr. Fergusson with impunity; and he lays down, not only that the earliest stone dome, as that of the Pantheon of Rome, is still the finest and largest of its kind existing, but that the first of the rock-cut temples of India is also the grandest of the whole series since excavated, and so with many other varieties of shrines. On which doctrine, if it really applies to the case, Mr. Fergusson would evidently be entitled to claim that the *first* Pyramid in Egypt ought also to be the grandest and best of the whole subsequent line.

If it applies, I say; for, while even the Pantheon case (small as it is in the amount of invention or degree of superiority implied, seeing that the Romans had been for ages largely given to introducing stone *arches* into their buildings, and were at the height of their imperial power and world-drawn wealth when the Pantheon dome was erected, as one only among many other large buildings, so late as about 300 A.D.), is a direct negation that progressive development (usually supposed, even defined, to be supreme within historical times for all teachable arts) applies in this way to architecture,—still it must be evident to everyone that the gulf is almost infinite which separates that little latter-day invention of a stone dome based on a wedged arch, from the untold pre-eminence of the *highest* of all stone buildings yet erected, when it was also the *first*, not only of its own kind, but of every kind of building in stone; and was reared, moreover, in days of the paucity of the human race, in a small country, where *no soldiers* appear among its pictured denizens to uphold the oppression of, or insist on obedience to, the mandates of a tyrant king.

This Great Pyramid case is indeed in *every way* a challenge from primeval time to the whole world since then, past and present, and to every nation that has yet borne empire-rule in the earth, from Nebuchadnezzar's, of Babylon, to that of the present King William of Prussia. But the *one* element, however, of *height* alone, in stone architecture, may form our best and speediest ground of comparison, because height immediately tests the strength of foundations, goodness of material, correctness of work, and steady heads of the builders; while it makes short work, too, of all historical architecture, for there is nothing left behind by any recorded nation of antiquity that can compete with our modern Christian cathedrals. Therefore let us compare the chief of them in height with the Great Pyramid.

	Inches.
St. Paul's in London has a height of .	4322
St. Peter's at Rome	5184
Strasburg Cathedral	5616
But the Great Pyramid, either, . . .	5819
,, ,, or, . . .	5835

Had the Cathedral of Ulm or that of Cologne ever been finished, according to the plans drawn out for them on paper, they would have been higher than the Great Pyramid; but their architects were unable to finish them, and they are actually very much lower. And again, had the steeple of old St. Paul's, in London, *not* been made of wood, it would both have competed for height of *stone* architecture with the Great Pyramid successfully, and would not have been burned to ashes by lightning in so few years after it was erected.

But Cologne Cathedral, I am told rather angrily by Prussians, *is* going to be completed; for their hero king, anxious to have a grand Roman Catholic Cathedral in the Germany of which he expects presently to be crowned Emperor, and able now at the sword's point to extract as large requisitions as he pleases from prostrate France, will not stop short of the original design: and then, how will it fare with the Great Pyramid? Will not Cologne Cathedral of the German Fatherland, they ask, then step into the Pyramid's place as the greatest architectural wonder on earth?

Not in any degree, unless it shall also exceed every other building of its own day, by as large a *proportionate* amount as the Great Pyramid did its contemporaries.

That is, the Prussians, if they *will* enter the competition, must not stop until they have made Cologne Cathedral exceed every other building of the world infinitely, both in height and every other good mechanical quality, and can likewise guarantee that it will be lasting and in sound condition 4000 years after everything that is at present standing has returned to dust or mud.

But as it is evident they will never do that, the Great Pyramid remains untouched in its isolation as the most solemn of all architectural works; and if, before many more years pass by from the present date of the world, some modern building shall at last be raised to the same, or even a greater height in inches, it will be merely to show all men, and in the most practical and lasting of all the arts and sciences, that human progressive development in mind and education, combined with national growth in population and wealth, after a painful schooling of full four thousand years, has at last only just succeeded in enabling man, slowly and with the utmost difficulty, to creep up to again, and touch once more, that towering height from which he once fell morally; when refusing to profit by knowledge as it was then communicated to the patriarchs of his race, full, complete at once, and perfect at all points,—he went off on his own foundation, and to indulge in his own inventions.

5. *The Argument of the Early* WOODEN *Theory.*

There is, indeed, another architectural argument occasionally resorted to, in order to tone down the almost evidently supra-natural manner in which the Great Pyramid takes its place suddenly and absolutely in the history of man, as thus:—

"The previous architecture *may* have been of wood, and has therefore perished."

"Well!" we may answer, indulging in our turn for a while in using as history what some term only human fable, "very likely it was of wood:" for not only must all men's minds in that early day have been still most notably impressed with the majesty of that gigantic work of naval carpentry, whose dimensions to be were given to Noah by Divine inspiration (and which has never been successfully exceeded since); and of course weaker minds were always showing their admiration by imitating its style, though in ever so distant a degree;—but the earliest existing stone architecture of most countries *does* imitate forms and constructions of pure wood.

Nay, indeed, round about the Great Pyramid itself there are tombs of the same or closely following ages, where you may still see lime-stone door-posts and ceilings of rock-excavated tombs carved in imitation of palm-tree trunks, while the basalt sarcophagus of the *third* Pyramid was elaborately chiselled in imitation of a very neat piece of "joiner's" work.

Still, however, and even allowing a general wooden architecture to have prevailed amongst all men previous to the date of the Great Pyramid, there is left to the startlingly new-light invention of that building's unknown architect, the mighty change, the *bizarre* revolution from slight, tough, easily-worked, perishable wood, to the use of hard, brittle, heavy, lasting stone; and he, too, who made that invention then and there, made it suddenly, at once and perfectly, for there is no trace throughout all the details of that monument of any dependence on the previously customary forms when men were working in a totally different material. And yet precisely such a slavery of thought and poverty of invention did prevail everywhere else, even into long subsequent ages. Thus the finest Grecian temples may be objected to æsthetically for their frequent imitation of soft, pliant forms of vegetable origin in stone material. The earliest Buddhist temples, also, for their circular enclosures of stones cut most expensively into the shape of mere wooden posts and rails; while idolatrous Egyptian buildings, at Thebes and Philæ, were positively heinous in their persistent use of stone pillars carved and painted in imitation of the most succulent and squashy of all watery reeds.

But the Great Pyramid is in this æsthetical respect as faultless in its whole as in its parts; for what is not its whole but a reproduction on the grandest scale of the form, true and simple through its whole extent, of a crystal; or of the very ideal of mineral substance refined and perfected; the emblem, too, of light, splendour, method, purity, power, eternity!

Again, therefore, a new test, not very scientific perhaps, but brought up by others specially to lower the Great Pyramid, is rather found, on close examination, to leave it with an additional claim to unique distinction.

6. *The Lepsius-Wylde "Growing" Tomb Theory.*

And now I would gladly go on to set before my readers the modern scientific theory of the Great Pyramid and what it rests on, but that with a monument which has looked down on the whole course of known human history, from before Nineveh to besieged Paris, and of which we have literary notices so old as 2300 years, *i.e.*, older than of any other still existing building on earth, there are of course many senior theories already in the field, and if any one of these theories explains all the essentials of the case, far be it from me to attempt anything contrary. Nay, indeed, would my readers tolerate it?

For their sakes, then, and the subject's sake, it is necessary to examine the sufficiency of at least the best and most popular of these older theories—say the tombic.

Egyptologists in general are rather loud—some young men amongst them even intolerant—in insisting on all the Pyramids having been tombs, and having, too, been built for the tombic purpose, and for that alone; while Dr. Lepsius a few years ago published a theory of Pyramid building based on such special view.

The real credit of constructing his theory is, indeed, said by some to be rather due to the English architect Wylde, who, with the London painter Bonomi, was hired by Lepsius with Prussian gold to assist in his magnificent Royal Expedition to Egypt.

Which of these eminent men was the chief, or sole, inventor of that theory, I do not pretend to discriminate; but the fact of its being disputed, sufficiently attests that the theory was thought much of in the Egyptological world, and is therefore worthy of our attention.

This Lepsius-Wylde theory, then, shortly is,—that each Egyptian king of the early time, on his first accession to the throne, immediately began the construction of his future tomb, and in the form of a pyramid. The commencing operation in the first year was to execute a subterranean chamber and sloping entrance passage in the rock, and put a few squared stones, as one layer of masonry, over it. The next year, stones of another layer were added and the lower one extended; and so on for every successive year of the king's reign until he died. On which principle the size of a pyramid always shows the length of the reign of its king.

Now this theory may explain the pyramids in general, *i.e.*, the pyramids subsequent to the Great Pyramid, for many, if not all of them, were undoubtedly used for sepulture, but how does it explain, or apply to, the Great Pyramid?

That pyramid being the greatest of all the pyramids should therefore have been the longest in building, and ought, according to human nature and the general march of things, to exhibit a similar tendency, but in more conspicuous degree, to a sort of progressive variation, such as all of our more tardily built cathedrals do—begun perhaps in sturdy Norman and ending in flamboyant Gothic. But instead of anything of that kind, behold one, and one only, style both of building and quality of material reigns throughout the Great Pyramid from top to bottom, and from side to side.

The surest, too, of the local traditions collected by Herodotus on the spot 2300 years ago, declare emphatically that the structure was begun from the first as a great building with enormous subterraneans, which occupied the workmen ten years, and a large part of which excavations may still be seen descending into the rock far deeper and further than those of any other pyramid; also that the entire building of the structure was fully prepared for beforehand, and then carried out energetically during only twenty very hard working years, and with well organised bands of workmen relieving each other every three months; and finally, that it was finished by its founder and cased definitely outside when the original finite design was completed, not when he could no longer, on account of death, go on extending it.

And what *was* the design according to the same Herodotus,—not, indeed, by any means a perfect authority on all Egyptian matters, but as the "father of history," duly to be noted and commented on?

A duplex design. Partly tombic—though he says that that failed, for the king, after all, was *not* buried in any part of the Pyramid—and partly mathematical, in a manner too which has been tested by modern scientific mensuration, and found true to within so small a quantity for extensive subaërial masonry as two minutes of angle.

Hence, however well the Lepsius-Wylde tombic *growing* theory applies to other pyramids, it is totally opposed to the manner and history of construction of the *Great* Pyramid.

7. *The Universal Egyptological Tombic Theory.*

Nothing daunted, however, by that failure, and nothing interested in the recovery of the old mathematical problem by modern measures, too many of the regular Egyptologists still go on asserting, that the Great Pyramid could never have been intended for anything else than a tomb, *because*, say they, the preparation of a tomb, or the "good house" for a man's soul to revisit in its future long and thousands-of years-between returns from the other world, was the *grand occupation* of every Egyptian's life while in the flesh: the only object, indeed, in whose cause that people cared to "build for eternity," or in so perdurable a manner as the pyramids; of which it is now an old remark that "everything else fears time, but time fears the pyramids."

Let us come to close quarters, then, with these Egyptologists, and on their own Egyptological grounds. What they say about Egyptians in general, and their terrific and undying propensities for lasting tombs, is perfectly true; but there are certain turning points in the modern Egyptological doctrine where the pundits thereof are exceedingly hazy, and yet these are the very corners at which clear light is needed to explain the Great Pyramid.

Thus, *why* the form of a square-based pyramid, such as the Egyptian (the subsequent Greek pyramids were triangular based), was first chosen for a tomb, the hierologists do not say a word upon; though they are eloquent enough on the form having been enthusiastically used by the Lower Egyptians for tombs, after its shape had once been invented and practically exemplified amongst them. They also agree that pyramid building for king's tombs went on only during the early dynasties of the "Old Empire"—the dynasties of the pyramid builders as they are often called—and that it was replaced long before the culminating period in population, power, and wealth of the Egyptian monarchy by a totally different method; viz., by the subsequent kings excavating their tombs in the gorges of rocky hills, as in the now well-known valley of the tombs of the kings at Thebes.

"Why, then," I asked recently of a most astute Egyptologist, "why did pyramid building cease so early in the history of ancient Egypt; in fact, before the nation had arrived at its full maturity?"

"Ah!" said the semi-Coptic philosopher, with a deep-drawn sigh, "men began to be frightened at the facility with which the pyramids were broken into, and their contained mummies extracted or destroyed."

A rather clever suggestion was this, but not altogether sufficient; because, although there *are* evidences that the pyramids were broken into during days of Egyptian civilisation, still there is no document in existence showing that that violation took place so early as the close of the Pyramid builders' era. In fact, the suspicion is with many persons that the violence was done a thousand years afterwards; and, if not by orders of Cambyses, by some of the same Ethiopian fanatics shortly before his period, who also broke open the valley tombs of the kings at Thebes, and robbed *them* of all their Royal remains just as completely as the Pyramids are found to have been likewise harried.

Egyptian kings, then, acquired no safer style of burying by abandoning the antique local example of the Great Pyramid; but they gained something else which, as Egyptologists themselves attest, they loved dearly; for by shaking off the fetters of the Pyramid's puritan rule,—they at once acquired full license to expand their burial chamber into whole suites of apartments, each one carved, painted, and inscribed more gorgeously than another, both to the glorification of the tomb-maker and the immortalisation of his idol gods.

Even in the age of the Great Pyramid, the tombs of subjects round about it show an inveterate taste for emblazonry and self-glorification to have prevailed amongst all classes of the people; wherefore I put again a most crucial question to the distinguished Egyptologer; as thus—

"When such self-glorification on the interior walls of the tomb was the besetting idea of all Egyptian peoples in all ages, and was never carried out with more artistic excellence than in the very period of the Great Pyramid,—why in that tomb, if it was the tomb, of their king—the very man of all others who could have best paid for any amount of glorification—is there not a particle of such praise of him, neither in sculpture, nor painting, nor writing, on any of the internal finished walls; not even his name on his, in modern times, so-called sarcophagus? No pictures either of the animal gods of Egypt are there; no representation of lands being ploughed, cattle numbered, crops reaped, and the produce brought to the greatest man of the realm, as seen with the non-regal owners of smaller tombs. Nothing but plain geometrical surfaces of exquisite workmanship, and of certain measured lengths, breadths, and angles."

At this question the eminent hierologist became pale, and confessed that he could offer no explanation of the huge and mighty antithesis. There it was, he allowed that, but he could say nothing as to how or why it came there.

Neither could he give any exact information as to what precisely those geometrical surfaces in the Pyramid were or are, in number, weight, and measure. No hierologist, he said, cared about such things, not having to use them in their science. Lepsius, for example, when encamped with a large party for months together at the Great Pyramid, made not a single measure of the monument, either in line or angle, but spent his and their time in nothing else than copying inscriptions, and pictures in the neighbouring tombs; nor did Champollion or Rossellini pay the Great Pyramid's geometrical qualities any more respect; while Caviglia, even when engaged by Howard Vyse expressly for Great Pyramid investigation, would pertinaciously always go off in any other direction hunting for mummies and "little *green idols*."

8. *The Mud Foundation Theory.*

If such, then, be the final and sorry result of all that modern Egyptologists and hierologists can help us to, as to what really constitutes and did originate the Great Pyramid,—who will necessarily blame some *scientists* for taking up that primeval monument just at the point where the hierologists have left it; and, after studious toil for years in applying to it instruments for mathematical measure of unwonted precision, think they can now begin to see a connected purpose and clear meaning—consistently, too, with that geometrical key-note gathered by Herodotus—running through all the great building's dimensions?

No positive blame, perhaps, will be extended openly; but not a few insinuations are indulged in privately, deriding the very idea of so old a building possessing any features on which accurate measure can still be made.

Thus, as touching the very first step which would have to be taken in such an enquiry, it is asserted in the "Record," (newspaper) of February 7th, 1868—that no less a personage in modern society than the existing President of the British Association for the Advancement of Science, declared, in a lecture before the British Clergy at Sion College, that the Great Pyramid, as included amongst "the oldest Pyramids of Memphis," is founded on alluvial mud; "built," he declares "on the site of the great valley of the Nile;" and the conclusion is added, that "these monuments evidently existed after this great deposit of mud upon which they stand."

Had such a state of things been the case, what sinkings and tiltings of the Great Pyramid's floors would have taken place through long ages. Would they even have remained to be measured at all? Would they not rather have gone down altogether out of sight, as the once famous walls of Babylon very speedily did in similar soil?

But, lamentable to say, either for the "Record," if it has reported the lecture wrongly; or for the British Association President, if reported correctly,—there is not a word of truth in the statement. The Great Pyramid is in reality (and I declare it on the strength of nearly four months' residence at its foot) founded on a hill of compact nummulitic lime-stone, at a level of about 100 feet above the alluvial soil of Egypt, and to one side of it.

The building is indeed an admirable example of a proverbially surely *rock*-founded one; for the following can still be made out as the mode of proceeding adopted. The whole of the original rugged top of the hill was cut away and lowered symmetrically until sound rock was everywhere arrived at; and on that firm material, exquisitely levelled, the first of the outer component stones of the Pyramid were laid; every stone being several feet long, broad, and high, but admirably worked by grinding processes, after the ruder cutting, to true mathematical figures; and with their joints cemented, but almost inconceivably fine and close, or no thicker than the vanishing thickness of "*a sheet of silver paper*." While at the four ground corners of the whole structure, shallow, but broad and level-floored sockets were cut in the rock, anciently to receive the lower corner stones of the outside casing; and now, by the accomplished fact, to form the fiducial reference-points for the original length of the four base-sides of the building.

In connection with one of these sockets nearly 12 feet square, —said to me one morning at the place, Mr. Inglis, the working engineer for Mr. Aiton—who had, with Arab help, just cleared away all the rubbish and brushed out the last particle of dust from the fair white floor of the ancient work,—"I have tested the whole of the socket's floor with my spirit-level, and can find no error in it." Remarkable testimony, surely, to come 4000 years after the date of the work's execution, and upon its being specially examined with "instruments of precision" invented in so long after a day.

9. *The Testing Angle of the Great Pyramid.*

But how is the whole building—anyone may very properly ask—as to subsidence of foundations or original errors?

The answer cannot be given so positively at present as it may be on a future occasion, because the greater part of every base side of the Great Pyramid is now covered up by huge heaps of loose rubbish. The closest approach, perhaps, yet made, was when I measured the angle of elevation of each of the arris lines of the building with a large altitude-azimuth instrument, the most powerful angular measurer ever taken to the Pyramid: and while, too, this instrument was plumbed accurately over the outer corner of each of the fiducial corner sockets already described, its upper observing signal was a specially prepared staff held at the top of the

Pyramid, where modern breakages are at their smallest limit, by the same Mr. Inglis just mentioned.

The result then arrived at, after several hours' work, was, that the correction to reduce each arris line to the mean of the whole amounted to—on a run, be it remembered, of no less than 700 feet of sub-aërial masonry—

			Min.	Sec.
For the N.E. arris line	=	−	1	15
„ N.W. „	=	+	1	45
„ S.E. „	=	−	1	15
„ S.W. „	=	+	0	45

While the mean absolute angle was 41° 59′ 45″, which gives for the angle of the middle of any side with the base, 51° 51′ 11″.

And as this angle, or something a few seconds only therefrom, and which was repeated in every stone anciently forming the flanks of the Great Pyramid, is never attained within a quarter of a degree, and sometimes not within several degrees, by any other Egyptian Pyramid (though there are three which come to within a single minute of *each other,* so truly could, and did, the builders work when they saw occasion for it), it may be looked on as the characteristic angle of the Great Pyramid, enabling anyone at once to distinguish any of its outer bevelled casing-stones from those of all its neighbours.

Now, in the British Museum, it is stated that there are three casing-stones, presented to the nation by that *vir incomparabilis* in Pyramidology, the late Colonel Howard Vyse. I rushed, therefore, eagerly, in a passing visit last year, and tried their angles of slope with a pocket clinometer, when, Oh! horror! one of them gave 48·5°; another, 58·8°; and another, 46·5°.

At such angles they could *not* have belonged to the Great Pyramid, and it was a serious libel on that exactest of buildings to say so. But these angles, as measured, included, with the angle of level cut by the primeval masons, the angle of level arranged by the officers of the British Museum. Wherefore, in a second attempt, eliminating their recent handiwork by measuring the *difference* of angle of the bevelled face of any stone from its own next worked surface of a horizontal joint, behold, the angles came out, 52°, 52° 30′, and 51° 30′; or, considering the fragmentary state of the stones and the imperfections of my mere pocket clinometer, quite close enough to show that they might all have come from a pyramid of 51° 51′ + x″; and to prove that, the ancient builders must have been at least five times, and probably twenty times, more accurate in their outside masonry than the modern Museum officers have been in *their* indoor adjustments.

Wherefore, *then* I looked around at the locality to which these precious fragments of earliest intellectual antiquity have been dragged, far from their true place in the world; and for what ?

A grand gallery of the British Museum, architecturally, is the Egyptian; but, as to its contents, almost wholly given over to idolatry. "No, no," says one apologist, "only to the fine arts." Well, then, to that most subtle and truly fine of all the fine arts, "Theotechny;" for hardly is there a single statue of a cat-headed, or baboon-headed, or unclean dog-headed idol, recovered from any part of idolatrous Egypt or sensual Nubia, but it has had a magnificent pedestal of Aberdeen granite prepared for it, at an expense rarely sanctioned by Government for anything scientific, and has then been set up under an imposing light, to be the gazing-stock and cynosure of a supposed admiring and Christian public; while those far earlier works of man, the casing stones of the Great Pyramid, which have no idolatry, nor human theotechny either, about them, but only the perfection of mechanical execution, and the exact embodiment of special angles in geometry and astronomy, in a noble cause and for a purpose recently supposed by some to be in direct connection with *Revelation—they* are shoved away contemptuously into an outer passage under a low shelf of a sort of cupboard; and even there are destroyed in their teaching by atrocious modern blunders in levelling.

Knowing the priority of the Great Pyramid to all their other architectural remains (for they have themselves published it), what a magnificent beginning to their Egyptian gallery,—and with that, to their microcosmal exhibition, of all human architecture—might not the officers of the Museum have made by erecting there, in addition to their fragments of actual Great Pyramid casing-stones—*properly levelled*,—full size models, both of Colonel Howard Vyse's two complete casing-stones, with their infinitely fine joints and admirable surfaces as he found them there *in situ*, and also of one of the outer corner-stone sockets of the whole building—accompanied by modern levels on a large scale and refined angular instruments to test the position, and exhibit the ancient angles so clearly to all comers that those who ran might read.

Then would there have been visible proof before the British public, that the earliest known material remains of intellectual man are not false gods and animal idols, nor trophies of bloody wars, nor gew-gaw adornments of tyrant kings; but were, on the contrary, works of pure science and unalloyed knowledge, under a peaceful reign where the soldier class does not seem yet to have been invented or required, and the accompaniments are such as neither a modern Christian nor an ancient Job, fleeing from idolatry even in momentary thought, could find any fault with.

Yet in place of that, see what our custodiers have done! Not, however, that the officers of the British Museum, so far as I know, are more idolatrously inclined than other men; but that, being amazingly wise in their own generation, they know perfectly well both what pleases the modern British public, and what also they do not care to be troubled to look at.

We have, however, fortunately, to deal here only with that awakened and enlightened section of Britons which reads the *Quarterly Journal of Science*; and then comes the question—Will *its* members care to hear more about the Great Pyramid, when they now know for certain that it deals only, and under circumstances of peace and purity, with such innocent things as number, weight, and measure; and what may be typified thereby? And will they, in the interests of true history and the primeval condition of man, ever join in representing to Her Majesty's Government, that if the Egyptian gallery in the British Museum has been built so gloriously at the expense of the nation to do honour to, and illustrate the progress of, the *antiquity* of intellectual man,—the oldest remains there, viz., those of the Great Pyramid, ought, in simple justice, to occupy the highest and fairest place; while the idols of subsequent times, the fruits of man's wandering from his first estate into the dangerous and unhallowed mazes of "theotechny," should rather have a lower place assigned to them, if not also a veil of shame drawn over their hideous and repulsive countenances.

III. ON THE THEORY OF IRRIGATION.

By Frederic Charles Danvers, A.I.C.E.

THE great importance which the subject of irrigation has recently attained to in this country is no doubt primarily due to the forced necessity of utilising our town sewage, and devoting it to profitable purposes, instead of, as has become the general practice, emptying it into our streams and rivers, polluting their waters and destroying their fish. The advisability of adopting a system of irrigation in England has, however, of late years, become a necessity, and one which must annually increase in urgency, entirely irrespective of the subject of sewage utilisation ; and it is only by a proper appreciation of this fact, and of the causes to which it is due, that we can expect the subject will receive attention. In order to convey the full meaning of the foregoing remarks, it is necessary that we should consider, somewhat in detail, the true theory of irrigation, which will be found, as we proceed, to owe its origin to a disturbance, by the works of man, of the balance originally prescribed by nature between evaporation and precipitation.

A careful study of the works of nature, in their primitive state, cannot fail to show the beautiful harmony of creation, and the perfect economy of its arrangements whilst contributing only to the support of brute creation. To man, however, in his more elevated sphere, has apparently been given a certain power over the elements, by means of which he can disturb that harmony of existence, which not only is not violated, but is actually promoted by the lower orders of creation. The student of history, by applying this test in his researches into the records of past ages, will find that to man alone may be attributed such a disturbance of the balance of forces as, in progress of time, has led to serious convulsions of nature, affecting not only the geography of the earth, but also many atmospheric and climatic changes in different parts of the world, the occurrence of which there can be no difficulty in establishing. "If* we compare the physical condition of certain ancient countries at the present day with the descriptions given by old historians and geographers, of their fertility and general capability of ministering to human uses, it will be found that more than one-half of their whole extent—including the provinces most celebrated for the profusion and variety of their spontaneous and their cultivated products, and for the wealth and social advancement of their inhabitants — is either deserted by civilised man and surrendered to hopeless desolation, or at least greatly reduced in both productiveness and population."

There are two great primary causes which, above all others, may be said to have led to these remarkable changes ; and these are, first, the destruction of forests ; and, secondly, surface and subsoil drainage. We shall consider briefly these two subjects in the order in which they are mentioned

* "Man and Nature," by George P. Marsh ; 1864, p. 3.

above, and then proceed to show in what manner their evil effects may best be remedied.

It has been stated above that in the absence of human interference the natural law of consumption and supply keeps the forest growth, and the wild animals which live on its products, in a normal state of equilibrium,—and the perpetuity of neither is endangered until man interferes to destroy the balance, and this he does, not wilfully, but in order to contribute to his necessities. Thus, when the means of subsistence began to fail on such ground as had been left open by nature—and which must first have been subjected by man for the supply of his necessities—and as population increased, recourse was necessarily had to the removal of a portion of the forest that stood in the way of further extension of cultivation. A small quantity of wood only being required for fuel and buildings, fire was most probably resorted to in order to clear lands for agriculture, which method, as is well known even at the present day, renders the ground beneath especially suited for vegetation. Such indiscriminate destruction of forests necessarily caused a disturbance in the economy of nature by affecting the temperature and humidity of the atmosphere, thus causing considerable climatic changes; by influencing the local distribution of rainfall; and by its effect upon the flow of springs.

"Forests," says Becquerel,* "act as frigorific causes in three ways:—1. They shelter the ground against solar irradiation and maintain a greater humidity. 2. They produce a cutaneous transpiration by the leaves. And—3. They multiply, by the expansion of their branches, the surfaces which are cooled by radiation. As these three causes act with greater or less force, we must, in the study of the climatology of a country, take into account the proportion between the area of the forests and the surface which is bared of trees and covered with herbs and grasses. We should be inclined to believe *a priori*, according to the foregoing considerations, that the clearing of woods, by raising the temperature and increasing the dryness of the air, ought to react on climate. The observations by Boussingault leave no doubt on this point."

With regard to the influence of forests upon humidity, it must be remarked that the vegetable mould, resulting from the decomposition of leaves and of wood, whilst it helps to obstruct the evaporation from the mineral earth below, absorbs the rains and melted snows that would otherwise rapidly flow away. This moisture it subsequently parts

* "Des Climats et de l'Influence qu' exercent les Sols Boisés et Non-Boisés."

with gradually by evaporation and percolation. The water absorbed by the roots of a large tree has been found to be greatly in excess of the weight of that fluid which enters into new combinations resulting in its growth, and the superfluous moisture must somehow be carried off almost as rapidly as it flows into the tree. "Recent experiments* on this subject by Von Pettenkofer were made with an oak tree, extending over the whole period of its summer growth. The total amount of evaporation in the year was estimated at 539·16 c.c. of water for the whole area of its leaves. The average amount of rainfall for the same period was only 65 c.c.; and the amount of evaporation was thus 8½ times more than that of the rainfall." This evaporation of the juices of the plant, by whatever process effected, takes up atmospheric heat and produces refrigeration, increasing, at the same time, the humidity of the air by pouring out into the atmosphere, in a vaporous form, the water it draws up through its roots.

Although the destruction of forests can hardly be said to influence the total amount of rainfall, it has no doubt, owing to the circumstances above mentioned, no small effect upon its distribution. The most obvious argument in favour of this supposition is that the summer and even the mean temperature of the forest is below that of the open country adjoining. This must reduce the temperature of the atmospheric stratum immediately above it, and, of course, whenever a saturated current sweeps over it, it must produce precipitation which would fall upon or near it.

The manner in which forest destruction has most directly led to the necessity for irrigation, is, perhaps, the effect which it has upon the flow of springs. The roots of forest trees penetrating far below the superficial soil conduct the water accumulated on its surface to the lower depths to which they reach, and thus serve to drain the superior strata and remove the moisture out of the reach of evaporation. This ensures the permanence and regularity of natural springs, not only within the limits of the wood, but at some distance beyond its borders, and so contributes to the supply of an element essential both to vegetable and animal life. As the forests are destroyed, the springs which flowed from the woods, and, consequently, the greater watercourses fed by them, diminish both in number and in volume. Boussingault, in his "Economie Rurale," remarks that, "since the clearing of the mountains in many localities, the rivers and the torrents, which seemed to have lost a

* "Quarterly Journal of Science," October, 1870, p. 524.

part of their water, sometimes suddenly swell, and that, occasionally, to a degree which causes great disasters. Besides, after violent storms, springs which had become almost exhausted have been observed to burst out with impetuosity, and soon after to dry up again." Arguing from the basis of facts already established, he draws the conclusion that forests have a special value—" that of regulating, of economising in a certain sort, the drainage of the rain-water."

To sum up the results consequent upon the clearance of forests, as already set forth, it may briefly be stated that any undue extent of interference with the economy of nature in this respect cannot but be followed by the drying of the vegetable mould on the surface of the ground affected by the clearance; and it soon becomes removed by the alternate action of wind and rain, leaving behind a sterile soil, possessing none of the properties necessary for cultivation; but not, fortunately, beyond the power of man to restore, in course of time, to its former powers of reproduction. The means for effecting this are the same which, if adopted earlier, would have prevented its falling into a state of sterility, viz., the artificial application of water to the soil, so as to counteract, in some measure, the consequences necessarily arising from an interference with the proper proportion prescribed by nature of forest to open land.

Having now considered the effects caused by the destruction of forests, we have, in the next place, to trace, in a similar manner, the probable evil consequences of land drainage.

Surface-drainage is a necessity in all newly-reclaimed lands, and probably dates its origin from the commencement of agriculture; but the construction of subterranean channels for the removal of infiltrated water, marks ages and countries distinguished by a great advance in agricultural theory and practice, a large accumulation of pecuniary capital, and a density of population which creates a ready demand and a high price for all products of rural industry. Under-drainage being most advantageous in damp and cool climates, where evaporation is slow, and upon soils where the natural inclination of surface does not promote a very rapid flow of the surface waters, it is not surprising to find that this practice has been carried further, and a more abundant pecuniary return obtained from it, in England than in any other country.

By removing water from the surface of the soil, however, the amount of evaporation is necessarily lessened, and the refrigeration which accompanies all evaporation is diminished in proportion. Accordingly it is a fact of experience (as stated by Marsh in his "Man and Nature" previously referred to) that, other things being equal, dry soils, and the air in contact with them, are perceptibly warmer during the season of vegetation, when evaporation is most rapid, than moist lands with the atmospheric stratum resting upon them. Under-drains, also, like surface-drains, withdraw from local solar action much moisture which would otherwise be vapourised by it, and, at the same time, by drying the soil above them, they increase its effective hygroscopicity, and it consequently absorbs from the atmosphere a greater quantity of water than it did when, for want of under-drainage, the soil was always humid, if not saturated. Under-drains, then, contribute to the dryness as well as to the warmth of the atmosphere, and as dry ground is more readily heated by the rays of the sun than wet, they tend also to raise the mean, and especially the summer, temperature of the soil.

Although the immediate improvement of soil and climate, and the increased abundance of the harvests have fully testified to the advantages of surface and subsoil drainage as adopted in England; its extensive application appears to have been attended with some altogether unforeseen and undesirable consequences, very analagous to those resulting from the clearing of the forests. The under-drains carry off very rapidly the water imbibed by the soil from precipitation, and through infiltration from neighbouring springs or other sources of supply. Consequently, in wet seasons, or after heavy rains, a river bordered by artificially drained lands receives in a few hours, from superficial and from subterranean conduits, an accession of water which, in the natural state of the earth, would have reached it only in small instalments, after percolating through hidden paths for weeks or even months, and would have furnished perennial and comparatively regular contributions, instead of swelling floods to its channel. By thus substituting swiftly acting artificial contrivances for the slow methods by which nature drains the surface and superficial strata of a river basin, the original equilibrium is disturbed; the waters of the heavens are no longer stored up in the earth to be gradually given out again, but are hurried out of man's domain with wasteful haste; and while the inundations of the river are sudden and disastrous, its current, when the drains have run dry, is reduced to a rivulet.

It has thus been shown that a great similarity exists in the consequences arising from the destruction of forests and from land-drainage, both as they affect the temperature and humidity of the atmosphere and soil; which, in their turn are, with a good show of reason, supposed to have a considerable effect upon the distribution of rainfall, though not, perhaps, upon the actual amount of it. It is impossible to restore the harmony of nature thus once disturbed, without allowing the lands, cleared and improved, to revert to their original state; but as this would be detrimental rather than conducive to man's interests, it is more desirable that the balance should be restored in other ways, and by other means, which, whilst counteracting the evil effects above referred to, admit of the retention of the land in its improved state of productiveness. Thus, by the artificial production of moisture in the soil, by means of irrigation, the equilibrium may be restored; whilst the subsoil drainage which has in many cases rendered a resort to irrigation necessary, is in itself essential to the proper development of cultivation by irrigation; otherwise the land, especially in heavy soils, is liable to become waterlogged, to the injury alike of the crops and the health of the neighbourhood. This latter is clearly proved in the case of rice crops, which are so notoriously injurious to health that no European can with safety sleep in their vicinity. "Not only does the population decrease where rice is grown," says Escourron Milliago, "but even the flocks are attacked by typhus." This is happily not the case where simple irrigation is adopted for the growth of grass, cereals, vegetables, and other crops required in European countries generally, where proper attention is paid to subsoil drainage. The reason why land will not produce good crops in the absence of a sufficient amount of water, even though it be highly manured and otherwise well cultivated, is that moisture is essentially necessary for the admixture with the soil of those invigorating properties existing in manures, which, in the absence of that agency, would, though mechanically mixed with the earth, remain chemically separate and distinct from it, and, therefore, not in such a state as to be in any way beneficial for the development of growth in herbage or plants. With the assistance of water, however, the salts contained in manure are set free and eagerly unite with the soil, by which they may be said to be digested and prepared to become fit food for the nourishment of vegetation; but, even when so taken up, these salts are, during seasons of drought, held from vegetation with an iron grasp by the soil, from which moisture alone can again loosen them. Thus we see that, whilst moisture is required in order to cause a chemical combination between the constituents of the manure and the soil, it is also further required before that soil will yield up the properties thus obtained for the purposes of vegetation.

Having now considered in what manner irrigation has been rendered a necessary adjunct to cultivation, it remains but to state briefly what steps are required for the conservancy of rainfall in order to render it most conducive towards a restoration of that balance in nature which previous operations of man have tended so seriously to disturb. These are two; namely, the prevention of waste by storeage, and the construction of channels for the proper distribution of water so collected, properly fitted with mechanical appliances for the regulation of the supply to different fields or districts as it may be required.

It is not the object of the present paper to enter into any account of the works or contrivances necessary for the collection and distribution of rainfall and drainage water; some brief allusion to what has been done in this respect, in former ages and in other countries, has already been made in the pages of this journal, but a complete study of the history and engineering nature of such works would occupy more space, and deserve more attention, than could be given to the subject in the concluding part of an article which has already occupied so much space; it may, however, be considered of sufficient importance to form the theme for a separate article upon some future occasion. In conclusion, it may be remarked that the question of sewage irrigation is one entirely distinct from that of simple irrigation by means of water alone; the purposes of the one being but the application of moisture to the soil, it in no way supersedes the necessity for manuring, whilst the former combines the application of manure together with irrigation. It does not seem at all probable that the two systems will ever be carried out in conjunction with each other, neither is it necessary that they should be combined. It is also clear that, whereas sewage irrigation is only practicable to a certain limited extent, and in localities bordering upon towns or places where a number of human habitations are congregated together, irrigation in its simple form may be adopted, to a greater or less extent, wherever land is brought under cultivation.

IV. WAR SCIENCE.

By H. Baden Pritchard.

THE solicitude exhibited just now in all directions on the subject of our army and national defences arises, as we all know, from one of those panics induced from time to time by the occurrence of certain stirring events. Periodical alarms of this kind,—causing us with fear and misgivings to pass in review our preparations, and to take stock, as it were, of the wares in which we have been liberally investing our money,—are thus fraught with a great deal of good; we are led thereby to examine minutely into our own system and organisation, and do we but fulfil this task honestly, we learn to profit by the misfortunes of others and to accumulate knowledge which otherwise is to be gained only as the result of dearly bought experience. Thus, a wise merchant seeing his neighbour's business go to ruin, through defective management, will turn a more watchful eye to his own concerns, to assure himself from the occurrence of a similar danger.

The subjects of manning our army, of mobilising our reserves, and of devising a simple and trustworthy method of organising our entire forces, have of late much occupied public attention, and been freely discussed in the columns of the press, and so in like manner, perhaps, we may be allowed to turn to that portion of the question in which we are more nearly interested and consider the extent to which science has been taken advantage of by the military authorities of this country; glancing briefly over those branches of the service where its applications have been the most important, so as to afford the reader some idea of the marked progress effected by its aid in many directions.

There prevails a wide-spread impression in this country, and more especially, we believe, among scientific men themselves, that sufficient attention is not paid by the soldier to the many important improvements and discoveries that are every day made around us whose application to warlike purposes would often be practicable and useful. This idea results, however, simply from entire ignorance of our military system; for, truth to say, there is scarcely any government to be mentioned that pays such strict attention to the advancement of military science, whether it be in one department or another, and certainly none other by which investigations in connection with novel warlike materials and methods of warfare are so carefully and completely conducted. As an instance of this we may mention the subject of gun-cotton, which has only recently, after many weary years of experiment and research, been accepted into the service as a reliable explosive. Austria, it is true, gave the matter some consideration, and contributed, indeed, much towards its further trial and adoption as a military element, but the French and Prussian Governments contented themselves with making a few hasty experiments therewith, and in abandoning the material without the semblance even of a fair probation; in England, on the contrary, our authorities perceiving the matter to be one of some promise, ordered a thorough and exhaustive investigation to take place, and the consequence is that, after a study of some seven years, a most valuable war agent has been secured, which when employed in torpedoes for harbour defence, as also in mines and other engineering works, is simply without a rival.

This, however, only by way of example; and that the reader may form an adequate idea of the great assistance and support which modern warfare really derives from science, we cannot do better than pass in review some of the matters in which it plays an important part in the various branches of the army. It is well known that there exist among our troops two scientific corps, the Royal Artillery and Royal Engineers. The duties of the former body are at once clearly defined, for although the calculations and problems arising in the elaboration of gunnery science are occasionally of an abstruse and profound nature, still they always lie within well marked boundaries. It is different, however, with the Royal Engineers; with them it is no longer one particular science that requires to be mastered, but rather half a dozen, a tolerable acquaintance with almost every applied science being insisted upon in all officers of the corps.

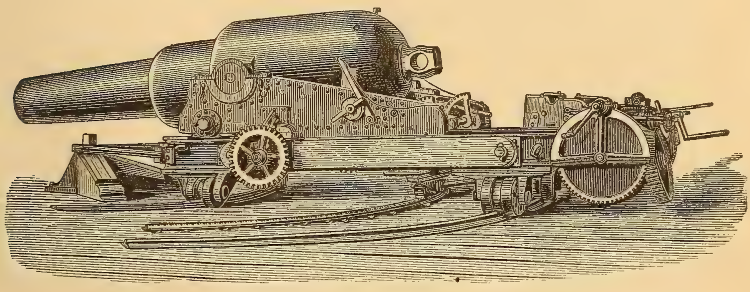
For the purpose of imparting this knowledge of the many duties devolving upon the Royal Engineer, a special college has been founded at Chatham, under the name of the School of Military Engineering. At this establishment professors, all of whom are senior officers of the corps, are engaged in the tuition of juniors in mathematics, chemistry, physics, mineralogy, fortification, &c., together with the practical application of such sciences as telegraphy, photography, topography, and other kindred subjects. A course of instruction at this school invariably forms the prelude to an officer's career, who, although very well educated before obtaining his commission, must furthermore go through this prescribed training. The advantages of this proceeding need scarcely be pointed out, but the striking illustration of the value of an educated body of men like the Royal Engineers afforded by the successful Abyssinian campaign—which has been frequently designated a gigantic piece of road-making, and which was in every sense an unqualified triumph over engineering difficulties in a wild and unexplored country, rather than a victory over a half-civilised nation—may be cited as a proof of the wisdom of such policy, if, indeed, any were wanted. During that campaign a wild and unknown country was surveyed and accurately mapped out, four hundred miles of road were constructed, a telegraphic system was established, wells were sunk, photographic records were secured, and, moreover, the geological formation of the mountains studied by the engineer officers of the staff entrusted with these multifarious duties. Of course, so much engineering skill is not habitually required in ordinary campaigns, but when this is unnecessary, other calls of a different nature are made upon the Royal Engineer, whose services are continually required for designing earthworks, stockades, balloon equipments, &c.

The improvements and applications of one kind or another in connection with telegraphy, signalling, surveying, and other engineering matters, which are frequently being made at the School of Military Engineering, is a further evidence of the interest taken in science by this intelligent branch of the army, and it is, indeed, not only in the officers, but also in the non-commissioned officers and men, that this superior ability is apparent, many of the latter frequently giving proof of their skill in the elaboration of novelties connected with galvanic batteries, signalling methods, photographic manipulations, &c.

Turning from Chatham to Woolwich, we find science as well cared for on the banks of the Thames as on the Medway. At the Royal Arsenal, at Woolwich,—the chief source of our military supplies—not only are all the manufacturing establishments under the charge of men chosen specially from the Royal Artillery and Engineers, by reason of their ability and attainments, but their doings are, moreover, controlled by an experimental committee, appointed solely for the purpose of watching the progress of war science, and investigating such inventions and modifications as are brought to their attention from time to time. At Woolwich there exist three vast factories or departments, occupied respectively with the manufacture of ordnance, ammunition, and military carriages, and the marked progress recently perceptible in all these establishments bears testimony to the desire on every hand to utilise new data and theories contributed by competent men. And no better proof of this can be shown than by referring to the improvements which have just now been effected in the construction of gun-carriages and slides, an illustration being here afforded of the manner in which necessities are met, and designs carried out in those cases where modifications are urgently called for.

It will be remembered that for the past ten years efforts have been continually made by those learned in gunnery to increase to the utmost the weight and calibre of modern ordnance, until at the present moment we possess weapons capable of throwing projectiles weighing as much as five and six hundredweights. In regarding such monster productions the casual observer is so occupied with the grandeur of their proportions and capabilities, that he is apt to overlook altogether the question of mounting and working them, for to his mind probably, the whole of the difficulty to be overcome lies simply in the manufacture of the gun. This is, however, a great mistake, for without a suitable carriage and machinery, it would simply be a matter of sheer impossibility to work the gun at all; the difficulty of handling or training a mass of metal weighing some five-and-twenty tons, or more,—irrespective of the circumstance of its powerful recoil on firing,—rendering the employment of ordinary appliances useless; this is more particularly the case when such guns are worked on board ship (indeed, these larger cannon are mostly for the navy) and require to be run in and out of the portholes, when, mayhap, the vessel is rolling and pitching in a heavy sea, and the gun-slides are inclined at a considerable angle. Under these circumstances, it will be easily understood that, unless a ready means were devised for working such heavy guns securely and rapidly in unfavourable weather, their employment in the navy would be altogether impracticable. When these big Woolwich cannon, then, were first designed, a serviceable slide or carriage became an imperative necessity, and the authorities cast about among professional men to obtain a solution of the difficulty. Many plans and propositions were brought forward, and, eventually, Captain R. A. E. Scott, R.N., whose name as a military inventor was already well known, communicated a method of construction which has since proved so efficient and simple that it has been almost universally adopted, and, in truth, there is now hardly a gun in the Royal Navy which is not worked by the aid of that officer's invention. Without the assistance of diagrams it would be difficult to convey a perfect idea of this clever system, but it may be briefly described as a low frame or gun-carriage, planted upon a long metal slide, so that the force of recoil being placed exceedingly low, it is the more readily overcome when the gun slides back on being fired. An ingenious tackle arrangement allows of the weapon being readily trained and directed, and a break weighing certainly not more than twenty pounds suffices to check the stupendous mass of metal in any desired position. So easily may a 600-pounder gun be worked by the aid of the Scott gun-carriage, that its loading, running up, and firing, actually requires less time than was formerly necessary in

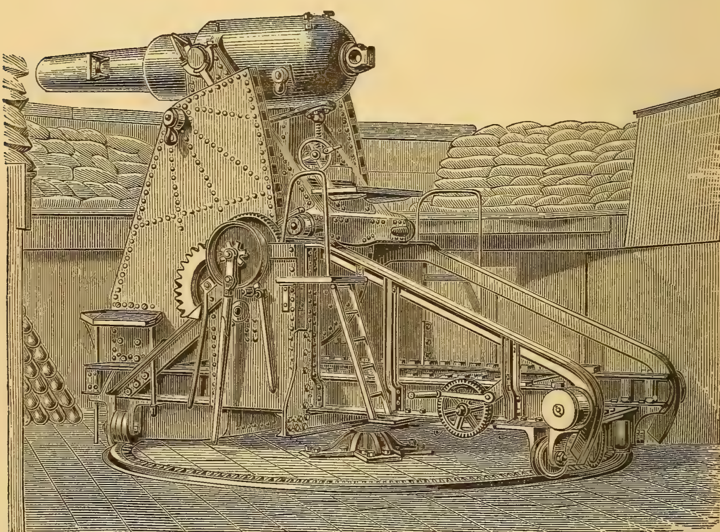
Fig. 8.



manipulating the old 32-pounder smooth-bore upon a block carriage. Thus, in squally weather, when the gunners actually find it a difficult matter to move about with safety, heavy cannon can be fired in this way at the rate of one round a minute, and, withal, as steadily and truly as if the weight to be handled amounted but to a few pounds.

Another invention, of perhaps equal importance, deserves mention while on the subject of gun-carriages. Everyone has heard of the Moncreiff carriage, and the great saving that the same will possibly effect to the country, in the way of rendering unnecessary the construction of fortifications and outworks. The invention represents, in fact, an elaborate mathematical problem successfully solved, the abstruse character of which can scarcely be understood without minute inspection of the details which go to make up the ingenious piece of mechanism. The carriage consists of a platform, supporting upon a movable pivot a strong gun-rest, which may thus be moved easily up and down with a rocking-horse sort of motion, so that the gun is sometimes raised to a height of twelve or fifteen feet, and sometimes lowered almost to the ground; the entire structure is placed in a deep trench or pit dug in the earth—a circular tramway being laid down in the first instance to allow of the carriage and gun being moved in a half circle so as to sweep the horizon for a considerable distance—and located so far below the level of the ground that, when "run down," the gun is hidden altogether from view. While in this position the operation of loading is completed, the machine being afterwards "run up" by means of a counterweight, which swings the gun aloft until the same is raised some inches above the earth level, when the sighting and laying of the weapon are proceeded with by the aid of a mirror fixed on the forepart of the carriage. On the gun being fired, the recoil is sufficient to send the arm down again under cover, to the loading position, not, however, by a sharp irregular jerk, as might be supposed, but in an exceedingly firm and gentle manner. The advantages of such a system must be

Fig. 9.



Moncrieff Gun-Carriage "run up" ready for firing.

at once obvious to all; the solid earth itself is made to take the place of the fortifications or parapet, and forms in this way the best protection possible, for, excepting just at the moment of firing, there is positively no portion of the armament capable of being hit or damaged by an enemy. Indeed, so promising has the Moncrieff invention appeared to Government, that a large number of the carriages are already in course of construction, and the possibility has, moreover, been suggested of employing the same in turret vessels with low freeboards, where it is of great importance that the weighty armament should be for the most part below the water-line. Had, for instance, the unfortunate *Captain* been able to lower her guns some ten feet below the sea-level, instead of their being mounted up on deck, there would have been, in all probability, no chance of her heeling over or becoming topheavy in a gale of wind. Whether these particular carriages can, however, be rendered available for the navy remains to be seen, but in any case their value for field works alone is incalculable.

We could, if necessary, easily multiply instances of this kind to prove that the manufacturing departments of the Royal Arsenal are fully alive to progress in matters of a warlike nature; the improvement effected in rifling ordnance, in the construction of cannon, whereby the strain is more equally distributed throughout the metal, in the shape and hardness of iron projectiles, &c., all betoken that in this branch of the War Department much attention is given to the advancement of military knowledge. A more direct proof, however, of the value and reliance placed upon science by the War Office, is that offered by the existence of an establishment devoted exclusively to the investigation and elaboration of novel implements of warfare, and to the control of army supplies of every description. This establishment, termed the Chemical Department of the War Office, is in itself a modest and insignificant institution, and appears to the visitor to be almost lost among the busy factories that are crowded together within the limits of the Royal Arsenal. Established some fifteen years ago as a simple chemical laboratory, under the direction of Professor Abel, it has steadily increased in size and importance, forming at the present moment a general reference department to which all matters bearing upon scientific questions are submitted. The chemical establishment of the War Department fulfils, indeed, as it now stands, the part of adviser and judge in regard to all supplies necessary for the *personnel* and *matériel* branches of the service, and upon the dictum of the chemist alone is the fitness, or otherwise, of army stores decided; so successfully has this method been carried out through many years, that the reliance now placed upon the judgment of the gentlemen forming the scientific staff is almost unbounded, and no step of importance is ever taken in these matters without their opinion and sanction being first obtained.

To enumerate completely the multifarious duties which constitute the work of the war chemists would require more space than we have here at our disposal, but we shall be able, at any rate, to give some idea of the important nature of their labours, and to demonstrate the profit which inevitably results therefrom, not only to the nation at large, but to the *personnel* of the army itself. In the first place, then, we may mention the subject of gunpowder as one of those which has recently received a large share of attention, and which, furthermore, still seems to need very much solicitude. Such a compound as this, whose composition and nature have been known to us through many centuries, should, it might be inferred, have been by this time thoroughly discussed aud ventilated in all the various phases of its behaviour, but, truth to tell, despite what has already been done in the matter, its perfect investigation would seem to have been but just commenced. In the days of the Old Brown Bess, and the times when fire-arms of any kind were considered serviceable weapons as long as they could be discharged without injury to their owners, the manufacture of explosives was studied to a very limited degree indeed ; and beyond the circumstance of paying attention to the constituent proportions of its elements, no further care was taken in its preparation. At the present moment, however, when we expect our weapons to serve for long ranges, and to perform their functions with truth and accuracy, gunpowder must be looked upon from a very different point of view. No improvement in its chemical composition has, indeed, recently been made, for truly we use the same proportions of sulphur, charcoal, and saltpetre, as at the period when the "villainous saltpetre" of old was first concocted by our ancestors ; but although similarly constituted, the gunpowder of to-day is as different from that used in the days of Cressy as if the two products were obtained from totally different sources. The operations of mixing, pressing, and granulating, impart to the material various specific qualities which alter greatly with the manner in which the above manipulations are conducted, and for this reason it is not only necessary for the chemist, in order to ensure supplies of uniform quality, to analyse the product and determine the percentage of its elements, but it is, furthermore, of equal importance to arrive at a knowledge of the hardness, density, and hygroscopic nature of the grains. The question of a gunpowder's density or compactness (which, by the way, is totally distinct from hardness) exercises, perhaps, the greatest influence upon the burning of a charge, for unless the grains are always manufactured of a uniform character in this respect, results of an equal and reliable nature must not be anticipated.

The method of testing this particular quality in gunpowder is at once so simple and interesting that we do not hesitate to describe it here in detail; an instrument, termed a densimeter, is used for the purpose, consisting of an oval glass bulb, of which the upper end is in connection with an air-pump, while the lower one terminates in a tube dipping into a bowl of mercury. Into this glass globe, which is furnished, we should mention, both at the top and bottom, with stop-cocks, is placed a certain quantity, say 1000 grains, of gunpowder, which fills up, perhaps, one-half or two-thirds of the available space; the vessel is screwed into connection with the air-pump, and the lower stop-cock having been closed, the air is exhausted from the interior of the bulb. When this has been effected, the upper stop-cock is closed and the lower one opened, upon which the mercury from the reservoir placed underneath rushes into the tube and completely fills up the vacuum, so that nothing is actually contained in the glass vessel but powder and quicksilver. In this condition the bulb is disconnected from the air-pump and accurately weighed, and the heavier the result, or, in other words, the more mercury there is present, the denser must be the powder, for the latter, if of a compact nature, takes up but little room and leaves a larger space for the mercury; if, however, the gunpowder under examination happens to be of a light, porous description, the 1000 grains of material will of itself have filled up the greater part of the bulb, and the quicksilver in that case has found but limited space therein. Of course, when the weight of a standard powder has been fixed upon, it is a very easy matter to institute a comparison between it and the product under examination, and an accurate result is thus readily obtained. This, then, in a few words, is the method adopted for examining all supplies of powder made in the Government Mills, at Waltham Abbey, or sent in by contractors, for the War Department factory is, by itself, incapable of turning out the large quantity of gunpowder expended annually by our army and navy, even in times of peace.

Passing to other questions referred to the Chemical Department, perhaps the most interesting are those affecting the supply of stores to the army. It forms, in fact, one of the principal duties of the Government chemists to examine rigorously into the nature of all military necessaries, and upon the chemical report alone depends, in great measure, if not altogether, the acceptation, or rejection, of supplies. Thus, contracts for such articles as soap, candles, bees'-wax, oils, paints, tallow, and other necessaries too numerous to specify, all pass through the hands of a chemist, who carefully examines specimens of the goods before they are definitely accepted from the contractors. In short, the mode of purchasing stores adopted by the war authorities has now been reduced to so perfect a system, that it is simply impossible for dishonest firms to send in defective or inferior goods. The method of proceeding is this: Tenders are, in the first instance, submitted by such persons as may be willing to contract for the various stores, and accompanying the offers made to the War Office are sent samples of the goods firms are willing to supply; these samples are all tested in the Chemical Department, and the house who offers the most suitable article at the lowest price forthwith gets the contract. As a matter of course, when the goods themselves are sent in, a further critical examination of them is made, and if this proves satisfactory, then, but not till then, is the bargain completed and the bills paid. Should the actual supply be inferior to the pattern sent to the War Office in the first instance, the agreement is not only cancelled, but the order is given to the tenderer whose price was the next highest, and the difference in money must then be paid by the house which has failed to fulfil the conditions of its bargain. In this way a very sound method of dealing is established, one that is alike simple in its nature and not easily abused, for the Chemical Department, being again responsible to the authorities for the genuine nature of all purchases made through its instrumentality, must necessarily exercise an impartial selection, and perform its functions justly and fearlessly.

We cannot obviously describe, in all its particulars, the course pursued by the chemists in examining into the quality of the various stores coming under their notice, but to afford some idea of the way in which the business is carried on, we will select at random two or three instances to indicate the searching manner in which these trials and tests are applied. The question of candles, for instance, will suit our purpose admirably; here there are many points of a various nature to be considered, such as the time of burning, the photometric power, the melting-point, the disposition to soften and bend under the influence of heat, and other matters; the most important of all these being, however, the luminosity of the flame. The value of a candle in this respect is estimated by means of a photometer, of which there exist many well-known descriptions, but, nevertheless, we will, at the risk of being tiresome, describe the one just now used at Woolwich for these experimental purposes.

The instrument, as our readers will suppose, is fitted in a darkened chamber, and, moreover, screened with black curtains, to shelter it from any stray beams of light; at one end of a horizontal bar, some three or four feet in length, is planted the standard light with which the candles are to be compared, and which may itself consist of a candle of known luminosity or of an oil-lamp of uniform light-giving qualities. At the other end of the beam are one or more candlesticks, for the reception of the specimens, fitted upon a stand, which forms, in fact, one side or scale of a balance, the functions of which shall presently be explained. The horizontal beam is marked off into inches or degrees, and acts in the capacity of a tramway, upon which a little waggon bearing the photometer runs freely to and fro when moved by the hand; the essential part of this instrument is a paper disc, of which the centre has been stained with oil so as to appear transparent, whenever there happens to be a greater amount of light behind it than before it. This paper disc mounted upon the waggon, separates, as it were, the two flames from each other (that of the standard light and that under examination), and is moved to and fro along the beam, sometimes towards one flame and sometimes towards the other, until a spot is found where the paper diaphragm appears opaque all over and the transparency of the centre is invisible, showing, therefore, that the amount of illumination proceeding from each source is at that point identical. This result obtained, the degree marked upon the beam is carefully noted; if the photometer is exactly midway between the two lights, when equal illumination is shown, then we know that the standard and experimental flames are identically alike in intensity; but if, on the contrary, the diaphragm happens to be farther from the standard than the other, then we know that the light under examination is the weaker of the two, for it has been necessary to approach nearer to it to obtain the same amount of illumination as that afforded by the standard at a longer distance off. The degrees marked upon the bar will give the precise result of the investigation, showing at once the comparative power of the experimental flame, and in what respect or degree it is greater or less than the standard; but the photometric test does not end here, for it not unfrequently happens that superior illumination is simply due to an extravagant rate of burning of the material of which the candle is composed; and a second question thus arises, as to how much of the candle has been consumed during the period of the experiment. This is ascertained by the balance arrangement above referred to, the same being so adjusted that as soon as a certain amount of candle has been consumed, say a hundred grains or so, and the one scale has become lightened to this extent, the heavier scale bears up the lighter one, and, at the same time, a small gong proclaims the termination of the experiment; a stop clock records at once the exact duration of the trial, and the twofold result—luminosity and time of burning—thus obtained, goes far to establish the actual value of the light examined.

The above serves as a good illustration of the nature of the investigations undertaken by the Chemical Department, but the variety of its duties are, as may be supposed, very great. Thus, all water supplies made to barracks and hospitals are analysed periodically as to purity and fitness for domestic purposes, and, in the same way, is a wholesome supply of ration bread ensured to the troops, from contractors at the different stations. All the soldier's requisites, down even to the blacking for his boots and the brass paste for his buttons, are tested, for the purpose of seeing whether they fulfil to the utmost the purposes for which they are required, and the welfare of the troops is in this way narrowly watched wherever at all possible. We see, therefore, that the existence of a department of this nature is not only of vital importance to the Government, in checking the quality of the enormous supplies which are made from time to time, but it also stands in the light of a true friend to the soldier, who profits largely by its kindly offices. Indeed, ever since the time the war chemists were first enabled to exercise an efficient control, the complaints of commanding officers, respecting bad stores and bad provisions, have almost entirely ceased, and such questions as a loaf of bread being adulterated with alum, or a ground-sheet being imperfectly waterproof, are, at the present day, quite unknown. The influence of sound and genuine supplies on the health of troops is, in truth, very great, for if we take, just by way of example, the subject of ground-sheets, to which we have just alluded—an article consisting simply of a stout sheet waterproofed with india-rubber, for stretching upon the bare ground to prevent the damp from penetrating the blankets composing the soldier's bed when under canvas—the necessity of ensuring a perfect and reliable protection is of the greatest importance. Should the thin india-rubber coating be composed of an inferior material, or should the same be but imperfectly attached to the fabric, the ground-sheet would obviously, soon be rendered useless, and the result would certainly be an increase in rheumatism and sickness among the troops. It is to make sure, therefore, that manufactured articles of this nature are in no way of a doubtful character, but that they may be relied upon in all climates and under all conditions, that very severe chemical and physical experiments have to be carried out, and considerable expense is not unfrequently incurred in the purchase of necessaries as shall be absolutely beyond suspicion.

Again, such stores where it is a fact of every day notoriety that adulteration exists to a greater or less extent, as, for instance, in soap, paints, emery, serge, white-lead, &c., special analyses must, in every case, be made; in soap, the amount and nature of the fatty matter has to be considered; in paints, the basis thereof to be ascertained; in emery, the genuine corundum requires to be separated from the magnetic oxide or other earths usually found in admixture; the serge must be made up entirely of wool, and absolutely free from cotton threads, which in cartridge bags would retain fire and smoulder in the guns, thus igniting subsequent charges rammed into the piece; white-lead contains as an adulterant a greater or less proportion of sulphate of baryta, and so on with very many other army stores.

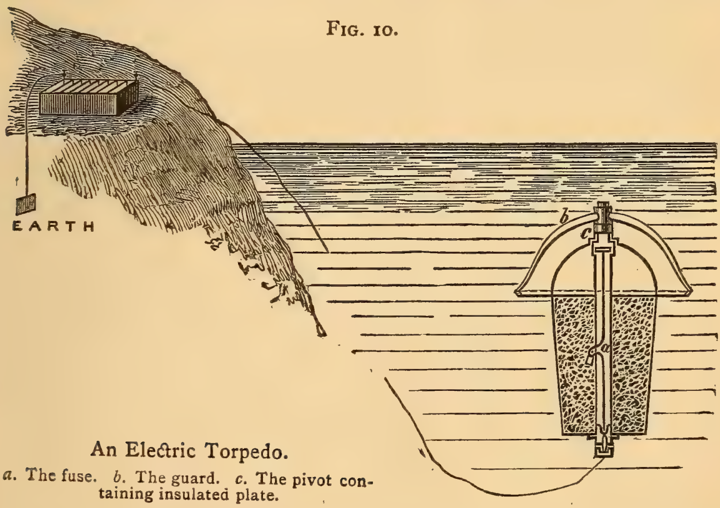
Beyond the examination of military supplies, there exist, besides, many other subjects of a scientific nature to be considered by this busy little department at Woolwich. The wants of military men, in regard to new compositions, explosives, applications, and improvements, require to be satisfied as they arise, and these necessarily originate lengthy experimental investigations. The employment of gun-cotton in warfare, which has just been decided upon, shows the care and attention which many of these subjects entail. It is well known that for some time after Schönbein's discovery of this explosive, all attempts to utilise it in the same way as gunpowder proved unavailing, and chemists and military men, one and all, found themselves compelled to abandon the task. A fresh impetus was, however, given to the subject some twelve or fifteen years since by an Austrian officer, Captain von Lenk, who, by manufacturing the material in the form of twist or yarn, instead of wool, as was formerly the case, and by making certain other minor improvements, was enabled to produce a material which burned steadily and uniformly, and not with that ungovernable violence which had hitherto characterised its combustion. Despite these valuable modifications, however, the Austrians, in 1863, abandoned altogether what appeared to many a most promising investigation, and the English Government thereupon took up the matter where it had been left by

Captain von Lenk. A committee was appointed to work out the question, among whom was Professor Abel, who subsequently made a thoroughly exhaustive study of the subject; and the interesting discoveries made therewith in the Chemical Department during a period extending over seven years, testify to the skill and ability with which the matter was handled. We have not here opportunity of doing justice to these improvements, which appear so full of value and promise, and must content ourselves, therefore, with merely making this casual mention thereof. For field engineering purposes, for demolishing stockades, for mines, for blasting operations, and especially for torpedoes, the new explosive will be simply invaluable, the destructive effect of gun-cotton being almost without parallel when ignited, or rather detonated, by means of a charge of fulminating powder; while, in other respects, it is, when dry, not more dangerous to store or manipulate than gunpowder, and when wet or damp, not only non-explosive, but actually uninflammable, although subsequent drying will restore to it all its valuable qualities intact. The use of gun-cotton, then, whether for military or industrial purposes, is in great measure due to the labours of the scientific staff attached to the War Department, and adds one more weighty proof, were any additional evidence required, of the intrinsic value of scientific aid in military establishments.

But we must hasten on. Further mention of the duties which come within the scope of the Chemical Department need not be mentioned, as the reader will by this time have formed some idea of their very extended and multifarious character. As demands for assistance arise, so it is afforded to almost every branch of the service, and, perhaps, in bringing to a close our imperfect sketch of the applications of science in this direction, we cannot do better than describe briefly the principles of a system of electric torpedo defence which has, in truth, only been decided upon within the last month or so.

This particular mode of warfare is certainly one of which the value is now placed beyond all doubt, for, when we remember that in the case of the present German war, a hostile fleet of the most formidable character, consisting of some twenty iron-clads and rams, of exceedingly modern construction, has been completely paralysed, or at any rate rendered harmless by the presence of a well-organised defensive system of this kind, it is really difficult to overrate the importance of such protections. Of course the employment of submarine mines by the Germans in this case is, as our readers very well know, by no means the first instance of torpedoes proving of value, for in the Russian war, our own vessels were sometimes considerably embarrassed by their presence in the Baltic, and, more recently still, the Confederates employed them to good effect against Federal shipping; but certainly no such conclusive argument has yet been advanced in their favour as the circumstance of the perfect safety enjoyed by the Prussian Ports in the North Sea, in the presence of the overwhelming naval force of the French. As regards our own Government, practical steps to elaborate a good system of defence has only recently been taken, but thanks to the energetic labours of the Woolwich and Chatham professional authorities, a method of some ingenuity has at last been hit upon, which bids fair to prove both efficient and reliable. It was decided after many experimental trials that choice should be made

FIG. 10.



An Electric Torpedo.
a. The fuse. *b.* The guard. *c.* The pivot containing insulated plate.

of two electric systems, one of which may be termed the electric-closing and the other the electric-breaking arrangement. Of these two, the former, or Woolwich system, will probably be selected, it being found by experience that the arrangements for breaking the circuit and the employment of a platinum wire fuse to effect ignition of the charge are open to several objections of a practical nature. The principles of an electric torpedo are easily explained; a large metal case, somewhat in the form of an Italian oil jar, is rendered buoyant enough to float in mid water, and moored at a suitable distance below the surface, so as to come into contact with any vessel passing over it. These machines are placed in lines or groups in the channel to be defended, and are capable of being exploded either by a sentinel on shore at a particular moment or by concussion against any floating mass or obstruction. Each torpedo contains, or is in communication with, a large charge of gunpowder or gun-cotton, and in the centre of this is placed the igniting fuse, an instrument composed of two fine insulated wires, whose poles—imbedded in a very sensitive explosive composition—almost touch one another; an electric current of but slight intensity passing from one of these poles to the other is sufficient to set fire to this composition and thus to explode the charge, rendering necessary, therefore, the employment only of batteries of but small dimensions. An insulated wire leads from the battery on shore to one pole of the fuse, while the other pole is connected with an insulated metal plate located in the head of the machine. We should state that the top of the torpedo has spread over it an iron cage or guard, something in the form of an open umbrella supported on a central pivot, and the former, on being struck by any floating obstruction, swerves bodily round, and for the moment comes into metallic contact with the insulated plate just mentioned, completing in this way the electric circuit through the fuse; for the second pole of the latter has thus been placed momentarily in contact with the battery on shore through the medium of the earth, or, more strictly speaking, of the water. An explosive machine of this nature is termed a self-acting torpedo, and will probably be that most suitable for warfare, but a modification of it, also fitted with this electric fuse, will be likewise employed for ignition from the shore.

The great advantage of these torpedoes lies in the employment of the Abel fuse just described, which, besides possessing the qualifications of an ordinary electric fuse, allows of a current being sent through them at any time to test their efficiency without the slightest risk of explosion, provided, always, the battery employed for the purpose is a comparatively weak one; signals may also be passed through the fuse in like manner, should the charges happen to be planted midway between two stations, and a torpedo system may thus be used in the absence of other telegraphic communication.

Contrived upon this plan we see at once that a system of torpedo defence becomes one of the most effective and controllable means of harbour protection that can possibly exist; until the actual approach of an enemy the batteries may be kept entirely disconnected from the machines, and the latter thus preserved in a perfectly harmless condition, so as to be capable of being taken up and examined, re-distributed, and handled without the semblance of danger. But on the presence of a hostile fleet in the neighbouring waters, in chase, may be, of friendly vessels, it is necessary merely to allow one's own ships to pass over the line, and when these are safely beyond the boundary, the turn of a switch, or depression of a key, suffices instantly to set up an impassable barrier, and to convert the series of sunken buoys into active submarine volcanoes of the most deadly and destructive nature.

V. SPECTRA OF METALLIC COMPOUNDS.

DURING the last few years the constant employment of the spectroscope in qualitative analysis has become so general, that it is of great importance to detect and carefully remove any attendant sources of error. Now, there are many other compounds besides those of the alkalies and alkaline earths which afford spectra, and a similarity in the position of their spectral lines and bands causes them to be mistaken on cursory examination for spectra of certain of the alkaline elements. An examination of all the more important metallic compounds, and their faithful mapping, so far as they differed from each other, would eliminate this source of error. Such maps, moreover, would enlarge the field of application of the spectroscope, and enable us to detect the presence of many other bodies than those to which its use is at present restricted. A comparison of these spectra would set at rest many interesting points of inquiry and speculation. As, for example, the amount and kind of alteration which takes place in the position, number, and relative intensity of spectrum lines at various temperatures. For the variable influence—the temperature—being but one function of the spectrum, it is by no means to be concluded, without experimental inquiry, that the less and more refrangible parts of spectra alter *pari passu.* The fact that at high temperatures decomposition takes place has already led (see memoir of M. Diacon, Ann. de Chim. [4], iv., 5) to a variety of interesting results. He found that in certain cases, when mixtures of volatile compounds were examined in the spectroscope, the spectrum obtained was not that of the compounds previously existent in the mixture, but that of the compounds which had been formed from their decomposition and subsequent re-combination, according to the strength of their affinities at elevated temperatures. For example, a mixture of baryta and calcic chloride gave not only the spectra proper to these two compounds, but that of barytic chloride as well. The careful study of these changes would remove a source of embarrassment in spectroscopic analysis. Moreover, it would probably furnish some information upon the chemistry of compounds, which we are wont to study in the solid state or in solution, when converted into vapours, and upon the phenomena of disassociation.

We already possess a very laborious and extensive series of determinations of the spectra of compounds by Prof. A. Mitscherlich (Pogg. Ann., No. 3, 1864, and Phil. Mag. [4], September, 1864). He found that compounds of the first order, in so far as they are volatile and remain undecomposed when adequately heated, always exhibit spectra which differ completely from those of the metals. He obtained the spectra in a variety of ways. 1st. By evaporating solutions in a narrow flame of coal-gas or hydrogen. 2nd. By bringing the substances into the flame of an oxygen-coal-gas burner. 3rd. By bringing them into a hydrogen-chlorine burner. 4th. Evaporating bromine and iodine in hydrogen, and volatilising the substance in the flame produced by the burning of this mixture in air or oxygen. 5th. By passing the gas to be examined either alone, or, in case it is not combustible, along with carbonic oxide or hydrogen, through the middle aperture of an oxyhydrogen burner, and burning the mixture in air or hydrogen. 6th. By volatilising the substance in a current of hydrogen, and igniting the jet thus charged with the substance for examination. 7th. By passing the electric spark between electrodes of the metals or of their salts when surrounded by an atmosphere of various gases. 8th. By using solutions of metallic salts as electrodes, and passing the spark from liquid to liquid.

It will be seen, by a comparison of these methods, that they differ greatly with regard to the temperature at which the spectra are formed. It is much lower when, as in the first method, the solutions of the salts are volatilised than when the fused salts themselves are used. In the latter case, the spectra are much more brilliant and persistent, and the lines are more numerous.

The third method was improved by Diacon, in that he surrounded his hydrogen-chlorine burner with a hood in such a way as to prevent the vapourised substance from coming into contact with the air. He thus obtained the spectra of the chlorides unmixed with the spectra of the corresponding oxides.

The feeble illumination in the green part of the spectrum, when hydrogen is burned in chlorine, may fairly be attributed, according to the same interpretation which we apply to the spectra of the metals when burned in chlorine, to the chloride of hydrogen or hydrochloric acid; and the broad bluish-green nebulous band when hydrogen is burned in air or oxygen, to the spectrum of aqueous vapour. The spectra of metals as obtained by Mitscherlich with the electric spark have been re-determined by Huggins and Miller with great care, and the lines obtained have been referred to a scale in which the atmospheric lines form fiducial points.

The maps given by Mitscherlich and Diacon, being referred to an arbitrary scale, are intelligible with difficulty. This difficulty applies not only to the position of the lines, but in a still greater degree to their relative intensity and brightness. It was very much to be desired that the benefits of their labours should be made available to us in the present advanced state of spectroscopy, and an attempt was made to reduce their measurements to normal wave-lengths according to Angström's tables. But this was altogether impossible with Diacon's map, since the intervals had been micrometrically determined, and no comparison has been made with the solar or other standard lines. In Mitscherlich's maps, the lines *a*, α, D, E, *b*, and F are marked, but when a graphical construction was attempted, by making the values of these points, as given in the maps, the ordinates, and their corresponding wave-lengths the abscissas, of a curve, the curve was so irregular that the attempt had to be abandoned.

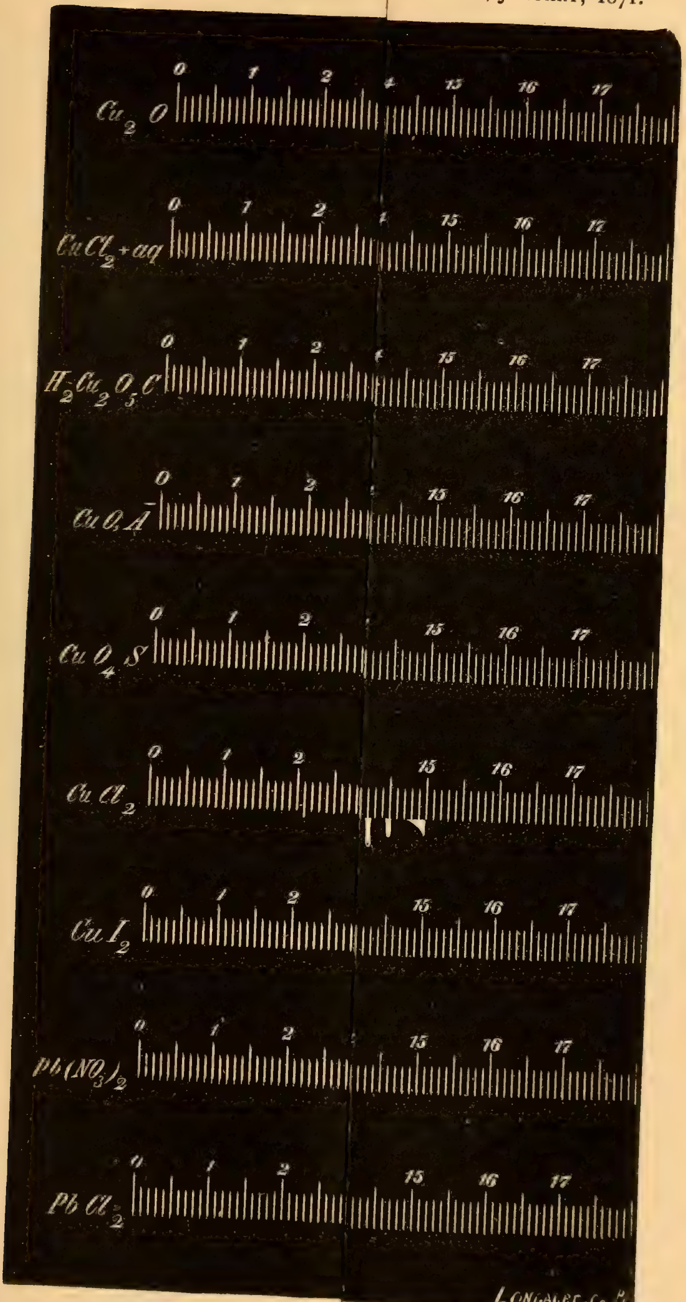
Professor A. R. Leeds has instituted some interesting experiments on the spectra of the metallic compounds. He employed the flame of a Bunsen burner, since it is to this source of heat that the spectra in ordinary laboratory work is referred. The resulting spectra are of two different kinds. In the case of oxygen salts, the spectra of the oxide of the metallic radical is obtained, the lines and bands being more or less broadened and brightened according to the degree of volatility of the salt. With haloid salts, the spectrum proper to the compound and also the spectrum of the oxide of the metallic radical is obtained. Two instruments were employed in the observations; one, a single prism spectroscope made by Desaga, of Heidelberg; the other a five-prism direct-vision spectroscope made by Hofmann, of Paris. Both were provided with arbitrary photographic scales. The numbers obtained in both cases were reduced to the scale accompanying the coloured drawings of the metallic spectra, by Bunsen and Kirchhoff,* a dash between two numbers indicates a continuity of the spectrum between the points corresponding to them.

TABLE OF SPECTRAL LINES AND BANDS.

Substance.	Position of Lines according to Bunsen and Kirchhoff's Map.
Cuprous oxide	37·8—44·8 60·8—72·4 78·2 84·1
Cupric chloride with water	60·8—72·4
Cupric carbonate ..	39·5—43·8—46—47·6 60·5—62·7 67·5—74·6
Cupric acetate	34·6 43·8—46 58 60·5—62·7 67·4 68·9—69·4 70·3—72·2
Cupric sulphate ..	36·6 39·5—46 60·5 67·4 69·8 72·2 81·5 84—84·5
Cupric chloride ..	31·8 37·8 40·2 41·4 45 52 60·8 65·5 68·9—73·6 75·9—78·2 81·7 84·1—86·4 88·7—91 92·2 93·4 94·5 95·7 96·9 98—102·7 105 108·5 110·8 115·5 117·8 121·3 127·1 127·1 129·4 130·6 134·1 136·4 142·2 144·5
Cupric iodide	33·6 34·6 36·6 39 41 42·1—43·2 45·5—46 62·7 65 66·5 68·9—69·9 71·8 73·2 75·1 76 77·5 79 81·3 82·2 83·2 85·1—86·5 89·8—91 94·2—95·7 98·5—100 102—103·5 104·8 107·3 109·1—111·1 114·1—116·1 119·1—120·6
Plumbic peroxide ..	55·5—58·5 59·5—64·2 66·5—68·4 69·9—71·8 73·6—76 80·3—83·2
Plumbic carbonate..	39—43·8 46—48·6 53—56·5 58·5—59·5 61·5 63·7 66—67·9 69·4—70·8 72·7 76 79·3—82·3 84·1 86·5
Plumbic nitrate ..	34·6 37·6—40·5 41·1—43·8 46—48·1 49·1—49·6 54·5 55·5—57·58—59 60·5—64·2 66·5—67·9 69·9—72·2 74·6 76·5 79·3—82·7 88·8—91·5
Plumbic chloride ..	35·6—40·5 41·6—42·7—43·8 47—48·1 55—57 58·5 60·5—63·7 66—66·5—68·4 69·4—71·3 72·2 72·7 74·1 76·5 77 81·8 82·2 83·2 84·6 86·5 90·3 92 96·2 101 105·8 108·6
Manganous chloride	40·2—42·6 47·4—49·8 57—59·4 62—64·3—65·5—66·5 68·9—74·8 75·9—84·1
Cadmic nitrate ..	71·3—72·2 75·5—78 80·3—81·8 88·5—89·8

In the accompanying map of the spectra of metallic compounds the distances on the horizontal divisions of the scale are taken as abscissas, and the relative intensities of the lines and bands as ordinates. It will be seen by comparison of the spectra of the oxygen salts of copper that there is a close similarity between them. Indeed, it is probable that if the drawing had been made directly from the instrument, instead of from notes taken of observations and used in drawing afterwards, the spectra would have been almost identical. The differences would have consisted merely in the breadth and brightness of the lines. But between the

* $K\alpha = 17·5$, $Na = 50·4$, $Li' = 31·8$, $Ca\alpha = 42$, $Ca\beta = 60·8$, $Sr\delta = 105$.


Cu_2O
$CuCl_2+aq$
$H_2Cu_2O_5C$
$CuO,\bar{A}$
CuO_4S
$CuCl_2$
CuI_2
$Pb(NO_3)_2$
$PbCl_2$
0
1
2
15
16
17
LONGACRE-Co. Ph.


Spectra of certain Metallic Compounds
0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17
Cu_2O
$CuCl_2 + aq$
$H_2Cu_2O_5C$
$CuO,\bar{A}$
CuO_4S
$CuCl_2$
CuI_2
$Pb(NO_3)_2$
$PbCl_2$
LONGACRE-Co.

spectra of the oxygen and haloid salts, and between the haloid salts themselves, the differences are numerous and striking. In addition to the lines in the less refrangible portion of the spectrum, which are common to all, and which belong to the metal as oxide, a great number of lines in the green, blue, indigo, and violet are seen, whose form and grouping are peculiar to the haloid salt under examination. In the spectrum of cupric chloride, the most noticeable feature is the grouping of the lines, in the more refrangible end of the spectrum, into pairs, in which the broader and more conspicuous lines are separated by an interval of about six degrees, while to their right, at a distance of about one degree, another but much feebler line in each case is seen. In the spectrum of cupric iodide no such symmetrical arrangement is evident. With plumbic chloride the same extension of the lines into the upper end of the spectrum takes place. Many of the bands in the spectra of the plumbic salts are beautifully shaded, and commence with a feeble illumination on the side toward the less, and increase to a line of maximum brightness on the side toward the more, refrangible end of the spectrum, where they abruptly terminate.

Without detailing in this place what takes place when the various metallic compounds are examined, it will be interesting to note briefly the deportment of one of them—cupric chloride. When a mass of this salt, which has not previously been freed from water of crystallisation, is heated on a platinum wire in the flame of a Bunsen burner, it imparts in the first place a greenish illumination to a large portion of the flame. On examining the flame through dark blue glass, it is seen that the part immediately above the heated substance is of a deep blue colour. This becomes tinged with violet, and later a tongue of reddish flame rises in the centre of the blue. If the substance be pushed into the hotter part of the burner, this flame changes to a bright white light, which at its upper edge becomes lurid again. The spectrum in this case is continuous throughout the middle and lower portion, the separate bands of violet still remaining distinct.

These phenomena are evidently of a mixed character. When a mass of this salt is carefully heated, so that it is slowly volatilised along with aqueous vapour at the outer edge of the flame, a green band, extending from 60·8 to 72·4, alone makes its appearance. On heating a concentrated solution of cupric chloride, the red lines from 37·8—44·8 appear synchronously with the green from 60·8 to 72·4. It is only at higher temperatures that the great number of lines in the blue and violet make their appearance, and it is not until the salt is fused that the spectrum becomes continuous. In this case the continuity of the spectrum is not attributed to the diffusion of incandescent particles of the solid substance throughout the flame, but to the widening out of the bands in every part of the spectrum until their fusion produces white light.

We have to express our thanks to the Editors of the "Journal of the Franklin Institute" and Professor Leeds for this article. At a future time Professor Leeds hopes to replace this preliminary essay by more carefully prepared drawings, and by a more extended table of the lines referred in position to normal wave-lengths, and in intensity to the solar spectrum taken as a standard. These further researches, we trust, will, without delay, be communicated to our readers.

VI. ON THE VARIOUS TINTS OF AUTUMNAL FOLIAGE.

By H. C. Sorby, F.R.S., &c.

IN the following paper I shall endeavour to explain the production of all that variety of colour which imparts such a charm to woodland scenery in autumn. I must, however, frankly admit that very much yet remains to be learned. The complete study of the question would involve very much research, and I see the importance of examining the colouring matters in spring and summer now that it is too late. Still, however, I trust that I shall be able to give a tolerably satisfactory general account of the subject, and perhaps little more is desirable on this occasion, since that may interest many who would not care to occupy their time in studying in detail the optical characters of the colouring matters found in leaves. These are certainly very numerous, and I have even so far established the existence of about a score, though I have examined only a few dozen plants with the requisite care. These have, however, been chosen in such a manner as to show the more striking phenomena, and probably a more extended examination would merely reveal a greater number of different colours, without materially altering the general results.

In the first place, I must say that it appears very desirable to divide the various colouring matters into different groups or genera, each of which includes a number of distinct substances or species, having some well-marked peculiarity in common. I shall not attempt to give anything like a complete account of the characteristic difference of the various species, since that would involve a long and tedious description of minute particulars, and shall confine my remarks to such prominent facts as are of importance in the subject more especially before us, and can be described without illustrations or very technical notation. I scarcely need say that such an inquiry could not possibly be carried out by any other than the spectrum method. Chemical analysis would be of very little use, and might easily lead us to conclude that different substances were the same, and the same different. It also is especially useful in studying the complicated mixtures with which we have to deal, since particular substances can be easily recognised when it would be quite impossible to obtain them in a separate state. For a more complete description of this method of research I beg to refer to what I have already published on animal and vegetable colouring matters,* and on some technical applications of the spectrum microscope.† I may also say that, spending, as I do, several hours nearly every day amongst the woods, fields, and moors, I have had good opportunity for studying the application of such inquiries to the subject before us.

The group of colouring matters which first of all claims our attention is that which may be distinguished by the term *chlorophyll*. It has often been treated as if it were one simple substance, but optical examination proves the existence of a number of separate species. The leaves of most plants are coloured green by a mixture of two or more of these. One kind occurs in a state of comparative purity in the small aquatic plants allied to *Oscillaria*, and the green leaves of trees appear to contain this along with one which gives special absorption-bands. Another is the product of the action of acids on these, and occurs naturally in some leaves, especially when turned brown in autumn, and this gives rise to a very special spectrum with numerous bands. A fourth, found in faded *Confervæ*, is closely related to the last, but differs in gradually turning to a deep blue colour, when hydrochloric acid is added to the alcoholic solution. All these have the following peculiarities in common—they

* "Proceedings of Royal Society," vol. xv., p. 433.

† "Quarterly Journal of Microscopical Science," New Series, vol. ix., p. 358.

are insoluble in water, but soluble in alcohol or bisulphide of carbon; the spectra have all a very well-marked absorption-band in the red, but the green more or less completely transmitted, so that the prevailing tint is a more or less modified green.

The second class of colouring matters may be described as the *xanthophyll* group. These are characterised by being insoluble in water, but soluble in alcohol and in bisulphide of carbon; the spectra show absorption at the blue end, often with more or less well marked narrow bands, but the red, yellow, and yellow-green are freely transmitted, so that the general colour is clear yellow or orange. The different species are distinguished by the character and position of the absorption-bands, which are best seen when the colour is dissolved in bisulphide of carbon. A considerable number are found in various fruits, flowers, and roots, but only two are so commonly met with in leaves as to claim attention in this paper. These appear to be the same as the two which give rise to the difference in the colour of the yellow interior and the orange exterior of some carrots. They may be obtained by dissolving in hot alcohol, and agitating the cold solution with excess of bisulphide of carbon, which subsides to the bottom with more or less of the colour, and leaves in the alcohol all other substances soluble in water. Both give spectra with two rather obscure absorption-bands, which lie further from the blue end in the case of colour from the external layer of the carrot, and the colour of this is orange, and of the other yellow. This latter is the kind most commonly met with in yellow leaves, from which it may be obtained in the manner just described, and when nearly pure it is of the same tint as gamboge. The orange colour is more rare, but occurs in leaves fading to a deeper and more orange-yellow, as, for instance, in those of the India-rubber tree, to which it gives a tint closely corresponding to that of Indian-yellow. It also occurs in a more pure state in the ripe envelope of the fruit of the common winter cherry (*Physalis Alkikengi*), to which it gives a still more orange-coloured tint, approximating to that of the exterior layer of the carrot. There may be some other colours besides these having bands in intermediate situations, but, on the whole, I am disposed to regard them as variable mixtures of the two just described.

Since the name of *erythrophyll* has been already applied to the red colour of leaves in autumn, it will be best to adopt it as that of a group containing a number of different species. These may be said to be characterised by their more or less red colour, which is made more intense by acids, and more purple, blue, or green by alkalies. This is because there is strong absorption in the green part of the spectrum, and the broad band is raised towards the blue end by acids, and lowered towards the red by alkalies, which also often increase the absorption at the blue end, so as to make the colour green, though I am much inclined to believe that in most cases this is due to the presence of a second yellow-coloured substance, so that a mere difference in colour is no proof that the red colours differ. Usually, but not invariably, they are soluble in water and aqueous alcohol, but not in bisulphide of carbon. Very many species are met with in fruits, flowers, and roots, distinguished by their spectra, either in their natural state or when acted upon by various reagents, and so far I have found at least six in leaves. That which gives rise to the red patches in the beautiful, variegated leaves of some of the geraniums of our gardens, is the same as that met with in the flowers of particular species. The purple colour of the leaves of turnips is the same as that of the purple flowers of the common garden stock. The colour of red cabbage has well-marked peculiarities, and so has that of the root and leaves of the beet. The dark leaves of *Tamus communis* contain another distinct colour, and so do those of the purple beech, but all these are normal constituents of the young leaves of particular varieties of the plants, and not simply developed towards autumn. It is, however, impossible to draw a line between the two cases, since the colour which gives rise to the dark brown tint of heath in autumn appears to be the same as that of the purple beech, and that which occurs in the dark leaves of ivy seems to correspond with the fine deep pink colour developed in many leaves only in autumn, so as to give rise to the splendid red and scarlet, which produce such a fine effect on certain kinds of scenery.

In order to obtain these red colouring matters in a satisfactory state for experiment, the leaves should be boiled in alcohol, which dissolves chlorophyll, xanthophyll, and the reds; but, as I have already described in previous papers, the alcoholic solution of most of them rapidly fades, so that the solution is only of a dirty green or yellow tint. On evaporating it to dryness, the splendid red colour chiefly collects round the edges, and the chlorophyll and xanthophyll are deposited more in the centre, so that we can immediately see that there is a mixture. By re-dissolving in water, the chlorophyll and xanthophyll are left insoluble, and the erythrophyll is obtained in solution, and on gentle evaporation is left in the state of a dry gum, which in some cases remains almost unchanged for months or even years. It must, however, be borne in mind that, as thus prepared, the erythrophyll must necessarily contain a variable amount of the colours described below, and it is no doubt to their presence that some of the reactions are due, which both myself and others have referred to the red colour itself. Thus, for instance, when they are slightly oxidised, the broad absorption-band is lowered towards the red end, and, by further oxidation, the colour becomes more or less orange-yellow, just in the manner that the colour of dark grapes is changed into that of new wine, and this in time to that of very old, as described by me in a paper already cited, but I am now inclined to believe that this further oxidisation, which destroys the main absorption-band, extending over the yellow and green, does really completely destroy the colour of the red substance, and that the more or less orange-yellow is due to the oxidisation of a pale yellow colour previously obscured by the deeper red. This fact is of considerable importance in the subject before us, since it explains why intensely red leaves fade to almost, if not exactly, the same tint as those of the like kind which were previously not at all red; that colour being so completely destroyed as to produce no effect on the tints subsequently developed.

The fourth group of colours is composed of those soluble in water and aqueous alcohol, but insoluble in bisulphide of carbon, which have a sufficiently decided gold-yellow colour to justify my distinguishing them by the term *chrysophyll* group. They vary somewhat in tint from a little more yellow, to a little more red, than yellow ochre. They are made darker and more orange by oxidisation, and thus are in an unoxidised condition as compared with the colours of the next group. In order to prepare them, the leaves should be boiled in alcohol, and after evaporation to dryness at a gentle heat, the soluble portion re-dissolved in water. I have so far met with at least four different species, distinguished by the spectra which they yield on partial oxidisation. The most satisfactory method is to dissolve some of the colour in a small quantity of water, dilute this with alcohol, and then to add a little nitrite of potash and hydrochloric acid. In some cases this gives rise to one or more well-marked absorption-bands, and changes the colour from yellow to pink. In others no bands are developed, but the colour is altered from pale yellow to deep orange-red. On evaporating to dryness, we obtain colours which vary in tint from that of "light red" to those of burnt umber and raw sienna.

The fifth group of colours consists for the most part of various browns, and therefore I propose to distinguish it by the term *phaiophyll* group. In most cases they are due to the oxidisation of chrysophyll or other previously-existing soluble compounds, as may be proved artificially. There must be, at all events, several colours of this group, but their accurate determination is difficult, because they do not give well-defined absorption-bands. On the whole they may be said to be soluble in water and not in bisulphide of carbon, but in some cases water alone dissolves them very sparingly, and they are more soluble in dilute alcohol, along with an acid.

When leaves pass into complete decay they turn dark brown, and ultimately become nearly black. This is evidently due to the formation of dark coloured substances allied to humus, but their accurate determination would be very difficult, and I have not yet studied them very much. Though it may be convenient for our present purpose to separate these more or less black colours from the brighter browns of the phaiophyll group, yet I am by no means convinced that there is any actual distinction between them. They are no doubt produced by the decomposition of most varied compounds, both soluble and insoluble; and since it is perhaps impossible to obtain these in a pure state, it is difficult to ascertain the exact connection between the various unaltered and altered products.

Having given a general account of the various colouring matters met with in foliage, I will now proceed to show how they serve to give rise to the almost endless variety of autumnal tints. These are usually due to varying mixtures of colours belonging to two or more groups. It is very doubtful if any leaves are coloured by one single substance, and generally they contain not only colours belonging to several groups, but even more than one of the same group.

Unfaded green leaves are coloured mainly by chlorophyll, but the tint is very much modified by xanthophyll, and by colours of the chrysophyll group. The presence of these, in varying relative and absolute amount, explains in a most satisfactory manner all the various brighter and darker greens met with in different leaves in different conditions. It is doubtful if chlorophyll has ever been seen free from xanthophyll. On heating green leaves with alcohol, a bright green solution is obtained. On agitating with bisulphide of carbon, this sinks to the bottom with the greater part of the chlorophyll and some xanthophyll in solution, whilst the alcohol retains most of the xanthophyll and some chlorophyll. After agitating this with a little fresh bisulphide, evaporating to dryness, and dissolving out the chrysophyll by water, when dry, the impure xanthophyll may be dissolved in bisulphide of carbon. The solution of chlorophyll in the bisulphide first obtained may be somewhat purified by agitating with fresh alcohol, but even then the spectrum clearly shows the absorption-bands due to xanthophyll. Still, on comparing the two different products, we can see at once that an approximate separation has been effected, and that for an equal amount of chlorophyll one contains six or eight times as much xanthophyll. This is a mere green-yellow, and the other a bright green; but, since it must be made considerably brighter by the xanthophyll, pure chlorophyll is no doubt of a darker and bluer green colour. The tint of many green leaves is also much modified by various colours of the erythrophyll group, which give rise to more or less green browns, and in some cases almost to black; for the green chlorophyll absorbs the blue and red rays, and the erythrophyll the green, so that all light is extinguished. If the erythrophyll preponderates over the chlorophyll, we have a red or even purple green, as in the case of the copper and purple beech; and thus, independent of any change, there is a considerable variation in the tints of normal growing leaves. It is, however, in autumn, when the chlorophyll has disappeared, that the brighter and more definite colours are produced. The amount of xanthophyll which is found in green leaves is so considerable, that probably the yellow colour of faded leaves is quite as much, or even more, due to that which previously existed than to any specially developed in the change, and the alteration may be said to consist chiefly in the disappearance of the chlorophyll. The result of this is that a deep green is changed into a bright yellow, and the general change in the spectrum is that there is no longer any absorption at the red end. Probably, however, few yellow leaves are coloured merely by xanthophyll, and the tint of many depends quite as much on the chrysophyll, and is also very much modified by colours of the erythrophyll and phaiophyll groups.

As I have already named, many leaves contain colour of the erythrophyll group, even when young and healthy, but the production of a red colour is more common in autumn, when their vitality is diminished. In some cases it takes place whilst the chlorophyll is unchanged, or only partially altered into the browner modification, and then its production merely gives rise to a dark brown which does not attract the eye. The deep brown colour of heath in autumn is an example of this, and on careful examination it may be seen that the brown shade is almost entirely confined to the side of the plant which is exposed to strong light. The red colouring matter is so disguised by the green chlorophyll that one would scarcely expect to extract, by the method already described, a colour quite equal in beauty to carmine, and of almost exactly the same tint. Very many other illustrations might be given of the same general fact, but the colour does not attract attention until the chlorophyll fades, and then the mixture of the previously existing red with a more or less pure yellow gives rise to scarlet. This may be seen to great advantage in the leaves of the common bramble and many other plants. It may then be asked why we never see a fine scarlet in the case of heath or purple beech. The explanation seems to be that in them the red colour is not the same as in the case of the numerous plants which turn scarlet, and is so much more easily decomposed that it entirely fades before the chlorophyll is altered. The spectrum method indicates that the colour of these two plants is the same, but differs from that red colour which occurs in most of those turning to a fine scarlet, as, for example, in the leaves of the bilberry, bramble, hawthorn, Berberis, cherry, apple, and guelder-rose. In other plants the red colour is not specially developed, whilst the chlorophyll is unchanged, but is produced at the time of that change, as if in some way dependent on the same cause. I have especially studied this point in the case of the leaves of the common sorrel, and find that the production of the red colour depends in some way on exposure to light, and on loss of perfect vitality. I had long known that most of the bright red leaves met with in the fields were those which had been broken off from the plant, and yet when dried in the house, or even kept in the dark with their stalks in water, the green leaves fade to dull yellow. I therefore placed in my garden detached leaves with their stalks in the earth, some with the upper, and some with the lower surfaces exposed to the light, some in the sun, and some in the shade, and I found that those which turned to a fine red were those exposed to the sun with the lower surface upwards. I have also noticed that the red colour is often produced in spots where the leaf has been injured by insects; and in the case of other plants I have remarked that the leaves on partially broken twigs show this colour to unusual advantage. I am therefore disposed to attribute the formation of the red colouring matter to some change which takes place when the leaves are not actually dead, but in a state of very low vitality. Of course this is scarcely applicable to those in which a red colour is a normal constituent; but, at the same time, even then it may indicate more or less of the same kind of condition; for I have remarked that in those branches of the bilberry which are of a fine scarlet in the early part of the year, the form of the leaves departs considerably from the usual type, as if they were not altogether in a healthy state. Perhaps the fading of green leaves to yellow, and the normally yellow state of some leaves may be referred to a somewhat similar low vitality, which either permits the chlorophyll to become changed or prevents its formation, as in the case of plants growing in the dark. I may here say that there seems every reason to conclude that on further change the red colour so completely fades away as to produce little or no effect on the general tint, since faded red leaves cannot be distinguished from those of the same kind which were not red, and the artificial oxidisation with nitrite of potash, of the substances soluble in water, extracted from scarlet leaves of the bilberry, gives rise to exactly the same colour as in the case of the yellow or green leaves. I may also here say that when scarlet leaves are digested in hot water the red colour is easily removed, and the green, yellow, or brown colours left as the case may be.

In studying the further changes which occur in leaves in autumn, it is most important to understand the properties of the various colours allied to the chrysophyll group, since it is to them that we must attribute a great part of the more prevailing tints. On the whole, clean scarlets are uncommon in this country in the case of large trees, and simple bright yellows are not very abundant, or only last for a short time; since the chlorophyll seldom disappears entirely before the chrysophyll is more or less changed. Much remains to be learned with respect to the various kinds of chrysophyll, and the connection between each and the species or special variety of the plant, and the circumstances in which it is placed. As far as my present knowledge enables me to judge, there is some decided connection between the kind of colour and the species of tree, but, at the same time, I have met with entirely different colours in the same species growing in other situations, and I am even disposed to think that there may be individual differences, analogous to what is so common in the colour of the hair of animals. It is this complication of facts which makes it very difficult to explain the cause of some of the results, but, at the same time, this variation is in complete agreement with the varied tints of different trees of some species.

One of the most striking kinds of chrysophyll which has much influence on autumnal tints is that contained in yellow beech leaves. It is of pale yellow colour, but when dissolved in alcohol and oxidised by means of nitrate of potash and hydrochloric acid, it is changed to deep pink-red. When dissolved in water the oxidisation gives rise to a very copious precipitate of the same colour, which is, therefore, apparently only imperfectly soluble in water, but more easily in acid alcohol. This kind of chrysophyll appears not to be formed till the time when the leaves begin to turn yellow, since I found that green beech leaves contained much of another, which is often associated with the one just described in other trees. The yellow leaves contain the yellow xanthophyll, whereas the orange-brown leaves contain the orange modification, as though, perhaps, derived from the other by partial oxidisation. I have met with this kind of chrysophyll in the leaves of the bilberry, and in some varieties of plane, and in a less pure state in many others. Another species of chrysophyll found in many leaves may be procured from some varieties of the common elm. I have found that leaves of large size and loose texture give it in the most pure state, but in some it is mixed with much of the kind found in the beech, and in others is almost entirely replaced by a third colour, which is but little altered by nitrite of potash. The colour to which I wish, however, to call especial attention turns to a pink-orange when thus oxidised, and gives a spectrum with a sufficiently well-marked narrow absorption-band, in the centre of the green, and a fainter nearer to the blue; but after a while these bands fade, and the colour becomes orange-yellow. I have met with this colour in the leaves of the poplar, Spanish chesnut, alder, apple, and oak; and, as far as I am able to judge, there are very many trees whose leaves contain variable mixtures of this with that found in yellow beech.

I have met with a very special kind of chrysophyll in the leaves of a plane tree, which turns to a very fine yellow. This gives, on oxidisation by nitrite of potash, a pink colour, with a very well-marked absorption-band in the green, nearer to the red end than in the case of that met with in the elm. In the green leaves of other planes of the same species I found only that colour so common in yellow beech leaves, and I have noticed that such plane leaves do not turn yellow, but to an orange-brown, modified by the continued presence of chlorophyll, though, when this has been dissolved away by means of alcohol, the former colour can be very clearly seen.

It is very probable that other kinds of chrysophyll will be found on more extended examination, and that a more complete knowledge of their properties will serve to explain many facts which are still obscure. I have, indeed, even now good reason for believing in the existence of some others, but their characters do not differ sufficiently from those described to materially modify the general results.

The alcoholic solutions of all these various kinds of chrysophyll resemble one another in being changed by oxidisation with nitrate of potash and hydrochloric acid to pink-red substances, which alter more or less slowly into orange colours. When thus changed, and kept dry, or in solution with a slight excess of ammonia, they are further modified into various brown substances. This is well seen in the case of the red colour obtained from beech leaves, which, on the addition of ammonia, shows a well-marked absorption-band in the orange. This slowly disappears, and, after keeping for awhile, when evaporated to dryness, it is nearly as brown as burnt umber, and the addition of hydrochloric acid to the solution does not restore it to the original fine pink-red, but we have a decided brown colour, to some extent absorbing the red end of the spectrum, which was previously quite clear. All these modified colours, when dissolved in sulphuric acid diluted with an equal bulk of water, and still further oxidised by means of chlorate of potash, merely fade, and are thus in that state of oxidisation which is characterised by a maximum depth of colour. These different changes may be simply due to an alteration of the chrysophyll, but, at the same time, there are cases which seem to indicate that almost, or quite, colourless compounds may contribute to the production of deep colours. For example, when the fresh leaves of *Acuba japonica* are digested in cold alcohol, the solution evaporated to dryness and treated with water, a clear yellow solution is obtained, which, when evaporated at a gentle heat, turns dark brown, on account of the formation of an insoluble substance of that tint. Water then extracts the apparently unaltered yellow substance ; and, though these facts are an exception to the general rule, they seem to show that a dark colour may be formed independent of the previous existence of a colouring matter of the chrysophyll group.

Though some of the various phaiophyll colours are soluble in water, the proper solvent to extract them from the leaves is moderately strong alcohol, to which a few drops of hydrochloric acid have been added. Since they are almost insoluble in hot neutral alcohol, it is well to digest the leaves first in it, to remove some of the xanthophyll, and any other colour soluble in the liquid. After evaporating to dryness the solution in hot acid alcohol, any colour soluble in water should be dissolved out, and the insoluble portion digested in a cold mixture of alcohol and water, in equal parts, with a little hydrochloric acid, which dissolves much of the phaiophyll, and leaves an impure xanthophyll. After evaporating the clear solution to dryness, strong neutral spirit dissolves the more pure colour, which is always darker and browner when dry than when in solution. As thus obtained, it may be, and often is, a mixture of various coloured substances, and it is only by comparing those from different specimens of leaves that we can arrive at a satisfactory conclusion respecting them. For example, beech trees are occasionally found whose leaves turn in autumn to a very deep colour, almost exactly like burnt sienna. These yield a fine red phaiophyll colour, which, when dry, is redder than burnt sienna, and almost exactly like so-called "light-red." When dissolved in alcohol it is of a fine pink-red colour, and corresponds in every particular with that obtained by oxidising the chrysophyll of yellow beech leaves as described above. The leaves of other beech trees turn to an orange-colour, like burnt sienna mixed with raw sienna, and these yield what appears to be a mixture of the above red colour with the browner modification into which the red passes, as already explained; and those leaves which have remained some time on damp ground contain still less of the red and more of the brown, which, when approximately pure, has a tint like that of a mixture of burnt sienna with burnt umber. Besides these, neutral alcohol or water extracts from the leaves a colour which closely corresponds with the orange modification already mentioned; and thus it will be seen that the actual tint of the leaves is the result of a mixture sometimes of at least six different colouring matters.

I have not been able to obtain from the leaves of the elm, chesnut, poplar, or oak, any pink-red colour corresponding to that first formed when the chrysophyll is artificially oxidised, but only the brown modification of burnt umber tint which corresponds almost exactly with what is formed on keeping the artificially oxidised in a dry state. This absence of the redder colour appears to be because it passes into the brown modification much more rapidly than the analogous colour in beech leaves, so that whilst they remain comparatively unaltered for weeks, those of the other trees just named often turn from light yellow to brown in the course of a day or two. The correspondence between the artificial and natural colours in these various cases is extremely close; and though the tint of many leaves is very much modified by the presence of other colours, yet I trust that the examples I have described will sufficiently well illustrate the fact that the most characteristic brown or orange shades are mainly due to the various kinds of phaiophyll, derived from the oxidisation of previously existing soluble compounds more or less characteristic of particular species or varieties of trees. The two principal kinds are those seen to advantage in the beech and in the Spanish chesnut or elm, but it seems as if they very commonly occurred mixed in very variable proportion, not only in different sorts of trees but in different leaves of the same, so as to give rise to every shade of redder or yellower brown, made more or less dull by the presence of more or less of the dark humus. Another very important modification of the tints is that dependent on the continued presence of chlorophyll, after the chrysophyll has been completely altered. This entirely prevents the development of any brilliant tints, since it makes what would otherwise be a fine orange-brown merely a dull brown-green, like that commonly met with in the faded leaves of the alder. In such cases, the chlorophyll is sometimes found to have been completely changed into the dull green modification produced by the action of acids on the brighter green kind. I need scarcely say that many leaves are variably and sometimes very curiously mottled, on account of the changes I have described having occurred partially and in patches. There seems to be a connection between some of these facts and the conditions under which the trees grow, and we cannot therefore be surprised that differences in climate, and variations in the weather of particular years, very materially modify the character of the prevailing tints. On this account, perhaps, some of the illustrations I have chosen may not be appropriate in all cases, being chiefly derived from the district with which I am most familiar.

The following table will serve to show the general relation of the various groups of colouring matters, their prevailing tint when alone, and the varying shades which result from the mixture of varying quantities of any of the two connected by brackets. I have also inserted on the left hand side the condition of the leaves of which, on the whole, the colours may be considered characteristic, commencing with perfect vitality, when carbonic acid is decomposed and oxygen set free, and ending with death and decomposition when the opposite change occurs.

Complete vitality and growth ..	Chrysophyll (gold-yellow) Chlorophyll (deep green)	More or less bright green.
		More or less green-brown.
Low vitality and change	Erythrophyll (crimson-red) Xanthophyll (bright yellow)	More or less red-scarlet.
		More or less bright orange-brown.
Death and decomposition	Phaiophyll (brown-orange) Humus (brown-black)	Less or more dull brown.

According to these principles, we must look upon the production of the fine tints of autumn as evidence of the diminished vital powers of the plants. I presume that this can admit of no doubt, and it agrees with the fact of unhealthy branches of a tree turning yellow whilst the rest remain green. The subsequent development of more sombre tints is evidence of more complete death. Perhaps some of my readers may think that such an explanation robs the fading leaves of autumn of much of their poetry, but, at the same time, I trust that the facts I have described may tend to explain many of the beautiful and varied tints which delight us so much in autumn, and that a knowledge of such general laws will compensate for any loss of poetic sentiment.

VII. ON THE RELATIONS BETWEEN CHEMICAL CHANGE, HEAT, AND FORCE,

WITH A SPECIAL VIEW TO THE ECONOMY OF ELECTRO-DYNAMIC ENGINES.

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CHAPTER I.

I THINK I may say, first, that the theory at present generally accepted, and which it would be considered somewhat heretical to deny, is this; that a certain amount of chemical change corresponds, and is interchangeable with a certain amount of heat and electric force; and that this heat again corresponds and is interchangeable with a certain amount of work or mechanical energy. This is, no doubt, a very pretty, plausible, and apparently philosophical theory; but, is it true? or, how far is it true? This is the question I now propose to consider, with a special view to the further question of the economy of electro-dynamic engines.

Let us first examine the first part of the theory—namely, that a definite amount of chemical change is interchangeable with a definite amount of heat and of electric force. I think we may safely grant that the doctrine of the equivalence of definite amounts of chemical change, heat, and electric force, is true when the chemical change takes place under precisely the same circumstances. This may be true; but it is a truth practically useless.

2. But does a definite amount of chemical change correspond with the same definite amount of production of heat and electric force under different circumstances, say of atmospheric pressure, surrounding temperature, contiguity of different substances, and other external influences? There may, or may not, be *a priori* reasons why it should, and such have been urged as conclusive reasons derived from the axiom of conservation of energy; but I think I shall show that there are practical reasons for doubting the universal applicability of this principle except in mere mechanics.

3. But, before doing so, let me quote an instance which shows the great practical importance of the question. Take the case of the production of iron by the hot and cold blast. With the cold blast certain quantities of ore, flux, and fuel are mixed together; combustion is induced, cold air passed through the mixture, and the various chemical compositions and decompositions take place. With the hot blast, part of the fuel is used first to heat the air which passes through the furnace, and part is placed as before in the furnace. It is found that in this way a much greater effect is produced by a smaller quantity of fuel. This, of course, does not prove that more total heat is thus produced, but it shows that it is produced in a more effective manner.

4. Now let us take the case of the production of heat in a galvanic battery. The theory at present generally accepted is this:—

(1). That the total amount of heat produced in a circuit depends upon the amount of zinc or other metal oxidised, though the distribution of the heat in this circuit depends upon the resistance of the several parts; being directly proportioned to this resistance. Thus, if H be the amount of heat and nM the units of weight of the metal consumed, $\mathrm{H} \alpha n\mathrm{M}$. And if I be the quantity of electricity circulating in each section of the circuit and r_1, r_2, the resistance of two different portions of the circuit, the heat produced in each of these portions in a unit of time, H_1 and H_2, will be

$$H_1 = I^2\,r_1$$
$$H_2 = I^2\,r_2.$$

(2). If R be the total resistance of the circuit, and H the total heat in a unit of time, $H \alpha I^2 R$. Now if these equations be true, they must be consistent with each other; if inconsistent with each other, one or all must be untrue. In order to show that I am not mis-stating the theory, let me quote from an able article in Watts's "Dictionary of Chemistry." "The development of heat in liquids by the electric current is regulated by the same law as in metals, the quantity of heat evolved in a given time being proportional to the resistance of the liquid and to the square of the strength of the current (E. Becquerel, Ann. Ch. et Phys., [3], ix., 21). Moreover, Joule has shown (Phil. Mag., [3], xix., 210), that the evolution of heat in each couple of the voltaic battery is subject to the same law, which, therefore, holds good in every part of the circuit, and, therefore, also for the entire circuit, including the battery."

"With a current of given strength the sum of the quantities of heat evolved in the battery, and in the metallic circuit joining its poles is constant, the heat actually developed in the one part or the other varying according to the thickness of the metallic conductor; this was first shown by De la Rive, and has been confirmed by Favre (Ann. Ch. Phys., [3], xl., 393)."

Let us now test these laws and see when they are consistent with each other and when inconsistent.

5. We know that I, or the quantity of electricity circulating in each section of a battery circuit in a given time, is expressed as follows:—$I = \dfrac{nE}{nRb + rw}$, E being the electro-motive force of the metals used, n the number of similar cells in the battery, Rb the resistance of each cell, and rw the resistance of the rest of the circuit. Take a battery of this kind and we shall find the total heat evolved in it in a given time will be

$$H = \left(\frac{nE}{nRb + rw}\right)^2 \times (nRb + rw)$$
$$= \frac{n^2E^2}{nRb + rw}$$

Now double the resistance in each cell, by using plates of half the size, or in any other manner, and double the resistance of the wire; then we get—

$$I = \frac{nE}{2(nRb + rw)}$$

Or the circulating quantity of electricity is half what it was, and consequently only half the metal is consumed, but as $H = I^2R$, this equation will now become—

$$H = \left\{\frac{nE}{2(nRb + rw)}\right\}^2 \times 2(nRb + rw)$$

$$= \frac{n^2E^2}{2(nRb + rw)}$$

That is, the quantity of heat is now half what it was, and only half the metal is consumed.

In this case the theory is consistent with itself.

6. Let us take one more case in which it is so. Double the number of cells and double the resistance in the conducting wire; then—

$$I = \frac{2nE}{2(nRb + rw)} = \frac{nE}{nRb + rw}$$

but now H becomes—

$$H^1 = \left(\frac{nE}{nRb + rw}\right)^2 \times 2(nRb + rw) = 2\frac{n^2E^2}{nRb + rw}$$

or double what it was before; that is to say, we have doubled the circulating electricity, and consequently the consumption of zinc, and also doubled the heat.

Here then, again, the theory is consistent with itself, and we may accept it as partially true when the circumstances do not vary more than in the manner we have described. That is to say, *in some cases* the theory is partially true, and it will be found that the general theory itself has been erroneously deduced from an experimental examination of such particular cases. I say partially true, because we have as yet spoken only of the total heat produced, but not of its distribution in the various parts of the circuit. Further on I think it will appear that the laws which are supposed to regulate this distribution are not true, except in particular cases.

7. But now let us vary the circumstances in another manner.

Take a galvanic couple of (say) zinc and platinum, having an electro-motive force, E, a battery resistance, R*b*, and a conducting wire with a resistance, *rw*. Then, as before—

$$H = I^2R = \left(\frac{E}{Rb + rw}\right)^2 (Rb + rw) = \frac{E^2}{Rb + rw}$$

Next, instead of the single galvanic couple of zinc and platinum, take two couples of zinc and (say) copper or other metal having an electro-motive force $=\frac{E}{2}$; take the plates of such a size that the battery resistance of the two couples together shall be exactly equal to the battery-resistance of the former single couple, and use the same conducting wire; then the quantity of electricity circulating is exactly the same as before, and H becomes—

$$H=\frac{\left(2\frac{E}{2}\right)^2,}{\left(\frac{2Rb}{2}\right)+(rw)}=\frac{E^2}{Rb+rw}$$

That is, the heat and circulating electricity are exactly the same as before, and yet as there are two couples instead of one, and the circulating electricity is the same, exactly double the amount of zinc is consumed.

The theory, then, is here inconsistent with itself. If the zinc consumed be doubled, the heat produced should be double, and the amount of electricity circulating in the circuit should be double. Half the zinc, therefore, is wasted, and the oxidation of a given weight has either produced only half the heat, or if it has produced an equal amount of heat, only half of it has been put into circulation; and if the magnetical and dynamical effects be proportional to the heat circulating, or to the electricity circulating, or to any power of these, we get from the same quantity of zinc only half of the effect which we got in the first case.

It is evident, again, that by taking a single couple of zinc and of some other metal whose electro-motive power is half of E (that of zinc and platinum), and halving the total resistance of the circuit, we should get the same quantity of electricity circulating, and an equal quantity of zinc consumed as in the first circuit, but only half of the heat developed; for the equation would now become $H=I^2\frac{R}{2}$, R being the resistance of the original circuit. So that we can construct different batteries in which respectively the ratio of the zinc consumed to the heat produced shall be the same or shall vary in any proportion.

8. Now take another instance of the same kind. First take as before a single pair of zinc and platinum. Then we get the same equations and results as before; namely—

$$H=\frac{E^2}{Rb+rw}$$

Next, insert in the circuit a cell with two plates of zinc, and make the total resistance the same, either by shortening the conducting wire, or by enlarging the zinc and platinum plates, or in any other manner. We now get, as before, exactly double the consumption of zinc, but the quantity of electricity circulating and the heat evolved in the circuit (if the accepted formulæ be right) the same.

How is this? Is it again that the heat evolved by a given weight of zinc is only half under this arrangement, or is only half of it put into circulation? In either case the ordinarily accepted principles are hopelessly wrong.

It is obvious that we might in a similar manner, by the use of zinc with different metals as a negative, multiply to any extent the cases to which the formulæ will not apply. Let us take one more of a different kind.

9. But before describing it let me observe that, in speaking of the heat produced by the oxidation of a given weight of zinc, we speak of what is left of this heat after deducting the amount of cold produced by the evolution of the corresponding amount of hydrogen at the opposite pole of each cell. On the principles ordinarily accepted, if this credit balance of heat were *nil*, the electricity and heat evolved in the circuit should be *nil*, and if it were a minus quantity, cold instead of heat should be produced in the circuit.

But now take a couple consisting of copper and platinum. The heat produced by the oxidation of an equivalent of copper is said to be 21,885 units, but the cold produced by the evolution of an equivalent of hydrogen 34,462 units. Hence, if the amount of heat evolved in a circuit be equal to that produced by the oxidation of the metal, minus the cold evolved by the hydrogen, the wire should be cooled and not heated; and yet copper is universally recognised as positive to platinum. Or take again an alloy of zinc and copper or other metals, whose heat-equivalent is the same as that of hydrogen, or 34,462. There can be no doubt that this would be positive to platinum, and would produce a current of electricity, though the heat evolved would be *nil*; and we should have the anomaly of an electric current passing through a homogeneous wire without heating it.

10. We conclude, therefore, that the whole subject requires a fresh, strict, and full experimental investigation. What we want to discover is how much heat a certain consumption of zinc and other metals produces when used in different electro-motive combinations with other metals, and what becomes of it; how much circulates through the circuit, and according to what laws. An investigation of this kind would probably show either that a

different amount of heat is evolved in different circumstances, or that the distribution of it is regulated, not according to the resistance of the different parts of the circuit, but partly according to these and partly according to the electro-motive power of the metals used in the cells; and that the total heat circulating in the circuit is not equal to the heat produced by the chemical changes taking place.

11. Let me add that the laws regulating the amount of heat produced in each part of the *external* conducting-wire of a battery seem to be tolerably well established, both by the experiments of Müller, on which they are based, and by corresponding laws, regulating the heat produced in various parts of a circuit by the discharge of Leyden jars; but that where they utterly break down, is when we go on to extend the same laws to the liquid cells of a galvanic battery and to the whole galvanic circuit.

NOTE. After the above had been placed in the printer's hands, I discovered the true law which generally regulates the distribution of heat in a galvanic circuit, and published it in the "Chemical News," of Nov. 4th and 11th, vol. xxii., pp. 224, 238. The reasonings by which this law is established being too late for the present number of the "Quarterly Journal of Science," will be published either in an early number of the "Chemical News," or in the next number of the "Quarterly Journal of Science." I will merely add that the law is as follows:—The heat produced in a battery is divided into three parts; (1) That arising from local action which is confined to the battery; (2) A given portion of the residue also retained in the battery; and (3) The remainder which is transmitted through the circuit. Calling these H_1, H_2, and H_3; $\frac{H_3}{H_2+H_3}$ depends upon, and represents, the electro-motive force of the negative element in respect to the positive element.

Indeed, the difference between one negative element and another consists in the property they have of transmitting different amounts of the heat produced in the battery. Portions of H_3 are evolved in each part of the circuit, including the battery, in proportion to the resistance of each part. Hence if R be the battery resistance, and r the exterior resistance of the circuit, HR and Hr the heat evolved in the battery and in the external part of the circuit—

$$HR = H_1 + H_2 + \frac{H_3 R}{R+r}$$

$$\text{and} \quad Hr = \frac{H_3 r}{R+r}$$

I may add that this law is proved mathematically by some of the considerations given above, and confirmed by comparing various experiments of M. Favre with others of M. Raoult. Curiously enough M. Favre gives only the heat which is found by experiments to be evolved in a battery, while M. Raoult only gives that evolved in the exterior circuit. Putting the two together, the truth of the above law becomes abundantly confirmed.

Chapter II.

1. What is the mechanical equivalent of heat? That is to say, what weight will be raised a metre (or foot, or any other unit of length) by the heat which will raise an unit of weight of water from the temperature 0° to 1° C., and, *vice versa*, what mechanical energy will produce this amount of heat? Many distinguished physicists fix the number at about 430, taking any unit of weight and a metre as the unit of length; M. de la Boulaye in several papers published in the "Comptes Rendus" argues for about 180, or less than half that number; Weber and Kohlrausch conclude from their experiments on the mechanical value of electric force, that the oxidation of a milligramme of hydrogen, which produces about 34 gramme-units of heat, will raise 226,800 kilogrammes through 1000 metres with a constantly accelerated velocity. Of course, with such enormously discrepant results, there must be a great error somewhere. One calculation makes the equivalent of heat about 430, another about 180, another about 6,000,000,000! For a milligramme of hydrogen produces about 34 gramme-units of heat, and 226,800 kilogrammes × 1000 metres = 226,800 × 1000 × 1000 grammetres, which divided by 34 gives more than 6,000,000,000 grammes raised one metre high by each gramme-unit of heat. I find it stated that Joule himself, the great authority on the subject, has at different times, and judging by different experiments, varied between the numbers of 80 and 1300 grammetres as the heat equivalent.

2. But, in the first place, it may be as well to inquire whether there is such a thing as a mechanical equivalent of heat. There may be, or there may not, but we venture to say it has never been proved; and why are we forced to suppose that the same quantity of heat must always produce the same mechanical effect, whether applied by means of the dilatation of different kinds of gases or of solids, or liquids, or in the many other ways in which it can be applied, and *vice versa*? That the same amount of fuel produces the same amount of energy, whether it is consumed in the steam

engine, the horse, the dog, the swallow, the wasp, the gnat? At any rate, we may observe that the very phrase is certainly a misnomer, and a misnomer of such a kind as to have a fatal effect in producing a false conception of things. For mechanical energy just as often produces cold as heat; it may produce either heat, or cold, or neither. In fact, as a general rule, though with notable exceptions, every pushing or compressing force produces heat, and every pulling or expanding force cold. Place a weight on a pillar, and the weight produces heat in the pillar; hang it on a wire, and it cools the wire. Place it on a pillar, which pillar is itself hanging by its lower end on wires, and it will produce neither heat nor cold. The heat produced in the pillar may be made exactly to counterbalance the cold produced in the wire. In the same way, in a fire-syringe, use force to press down the piston, it produces heat—heat enough to kindle tinder; but use the same force to pull up the piston, and it produces cold. Combine two fire-syringes together, one within the other, or in any other way, and let equal forces push one piston down and pull the other up; neither heat nor cold will be the final result. So, also, put a pressure on water at a temperature above its greatest density, and it produces heat. Below that temperature it produces cold. At that temperature it produces neither heat nor cold. Hence we see the same pushing force produces at one time heat and at another cold; and, similarly, a pulling force, tending to expand water, may produce either heat or cold, according to the temperature of the water. The phenomena which point to -273° C. as the absolute deprivation of all heat possibly only tend to show that at that temperature a further condensation of air would produce not heat but cold, and that further cold would expand, not condense, air. There is just as much mechanical energy in a lump of ice which will produce 100 units of cold as there is in a lump of coal which will produce 100 units of heat; there is as much stored-up power in a glacier as in a coal-mine. When our coal is exhausted we may quarry the icebergs of the poles and make them do the work which coal now does for us. No amount of heat in a body can produce any effect till that body comes into contact or communication with some other body hotter or colder than itself. So that, in reality, force is produced, not by heat or cold, but by the restoration of the equilibrium of two bodies or parts of bodies unequally heated, and mechanical energy produces neither heat nor cold (except accidentally), but simply a disturbance of the equilibrium in the heat of two bodies or parts of a body. This disturbance or restoration of equilibrium might be so contrived as to produce no outward effect at all recognisable by our instruments. Take, for instance, a cylinder, supported at the lower extremity, and pierced with a number of vertical holes passing through it, through which holes pass wires fastened to the upper surface of the cylinder. Now a weight or weights hung to these wires would cool the wires and heat the cylinder, and by increasing indefinitely the number of wires and perforations of the cylinder, the heat and cold produced would be so blended as to be incapable of being detected. This is exactly the condition in which water exists at its maximum point of density. The heat and cold produced exactly balance each other. Now let us take Joule's famous experiments, on which, one may almost say, the doctrine of the mechanical equivalent of heat is founded. He churned various liquids in a calorimeter and measured the increase of temperature. But in this kind of motion, as, perhaps, in all cases of friction, there is a pulling exertion of force, as well as one of pushing. Behind the arms of the paddle-wheel in the churn the liquid is pulled, and is pushed before them. Hence we might expect cold to be produced as well as heat, but the thermometer will only show the balance of heat over cold. From these and such like experiments, therefore, we can draw no trustworthy conclusion whatever as to the amount of disturbance of equilibrium which has taken place.

3. Now take again M. Favre's elaborate experiments with a galvanic battery. He formed a galvanic circuit, in which he placed an electro-dynamic engine. He placed the battery in one calorimeter, and the engine in another. He found that the battery working alone without the engine produced 18,682 units of heat for every gramme of hydrogen evolved. When the battery worked the engine, but without raising any further weight, he found that the battery calorimeter produced 13,888 units of heat, and the calorimeter in which the engine was placed, 4679 units, making together 18,657 units; whence he concluded that the other 25 units were absorbed and disappeared in working the engine. He next made the engine raise 131·24 kilos. a metre high, and found then that the battery calorimeter showed 15,427 units of heat, and the engine calorimeter, 2947, making together 18,374 units. Hence he concluded that the remaining 308 units were absorbed in the 131·24 kilogramme-metres of work. He consequently deduced that the mechanical equivalent of heat was about $\frac{131240}{308}$ or 426. Now let me give reasons for

thinking that this experiment is wholly inconclusive. First, is it not exceedingly strange that when the engine did no work the battery calorimeter absorbed only 13,888 calories, but when the former raised 131·24 kilogramme-metres, the latter should register 15,427, thus showing that when the engine did no work, it (the engine) exercised a much greater comparative resistance and absorbed much more heat than when it did the work? Then, can any serious conclusion be built on a difference of 300 units out of 18,682? Is not this difference quite within the limits of accidental error? Indeed, the difference is much less than differences shown in other experiments of M. Favre where he had no engine to do any work. But there is a very much more serious objection than either of these. Supposing the numbers to be strictly reliable, is there not a much simpler explanation of the phenomenon? M. Favre does not tell us how the magnetic engine worked, but doubtless it worked as most of such engines do (chiefly at least) by pulling iron keepers to the electro-magnets. Now, by this action the iron is expanded, and this pulling or expanding action, as we have shown, usually produces cold, and hence the disappearance of 300 units of heat. If the engine had worked by pushing instead of pulling, that is, by repulsion instead of attraction, should we not have had an increase of heat instead of a decrease? We have every reason to conclude that we should. Unfortunately the apparatus required for repeating these experiments is so very costly and delicate that very few persons are in a position to repeat them, and M. Favre himself has either never repeated them, or if he has, as he seems to have done, he has never given us the full results. This one single experiment is *the only one of the kind* of which he has *published* the result. In the accounts of his later experiments he has never published the number of the calories evolved in both the battery calorimeter and the engine calorimeter, but only the former; and a calculation of what the latter *ought to be*, but not what they actually *were*.

4. And to set against this single experiment of M. Favre, we have numerous experiments of M. Soret, in which he finds results totally discordant with those of M. Favre. In the "Comptes Rendus," xlv., 301, 380, M. Soret gives us the result of his experiments. He placed an electro-dynamic engine in a calorimeter to ascertain the effects of its working. Unfortunately, he gives us very few details, but he says (as we should expect from what we have just said) that the results were very discordant with each other. When using a brass calorimeter he found that the effect of

his engine working was to *produce* instead of *absorbing* heat. This he attributed to induced currents in the brass; but using a glass calorimeter, the conclusion to which he came, on the whole, was that it made no difference whatever in the calorific effects, whether the engine produced work or not. Such are also the conclusions to which he afterwards came after some years of further experiments. At any rate, the only experiments he gives us in which the working of the engine made any difference tend to show that the engine *produced* not *absorbed* heat. We can only say, then, that the whole subject is at present in a state of chaos, and that no legitimate conclusion can be drawn without a new and careful experimental examination of the whole of the facts.

5. Let me point out next how eminently unsatisfactory are the conclusions drawn from the experiments of Weber and Kohlrausch. As we said before, they drew from these the inference that a milligramme of hydrogen produced electricity enough to attract 208 tons at a distance of 1000 metres in opposition to gravity—that is, to raise anything less than 208 tons 1000 metres, with a constantly increasing velocity. But I venture to say their experiments were wholly inconclusive. For how did they operate? They first measured the attractive force of the electricity contained in a Leyden phial. They then examined what effect this had in moving a magnetic needle, placed in a galvanometer. Next they tried what was the quantity of water which, in its decomposition in a circuit, corresponded with the same motion of a magnetic needle. Comparing the two they drew the deductions I have just mentioned. But here was the fallacy. In order to prevent a spark passing, and to enable the electricity in the Leyden phial to move the magnetic needle, they passed the current through a long column of water. But they seem to have forgotten that if, instead of the column of water, they had substituted a great length of wire of corresponding resistance, and had formed that wire into a number of galvanometers, the electricity in the Leyden phial would probably have given the same amount of motion to many thousands of magnetic needles, instead of to one, and, consequently, that these conclusions were probably wrong many thousands of times over. I ought to say that I have not read the account of these experiments in the original, but only the description of them given in Watts's "Dictionary of Chemistry;" so that it is possible I may have misunderstood them.

6. Other philosophers have been as much out in their calculations. Règnault calculated how much was the

utmost force which could be got out of steam, supposing there were no waste; but, unfortunately, on examining the work actually done by existing steam engines, it was found that they produced two or three times as much work as he had calculated to be within the limits of possibility. I find, also, that Sturgeon states that Professor Page in America has produced from a galvanic battery nine times as much work as Joule and Scoresby had proved to be the utmost possible, according to their computation of the mechanical equivalent of heat. And I much doubt whether any philosophers have yet properly laid down the very first principles of the question involved. No doubt there are many cases in which, where the circumstances do not vary much, it may be convenient to ascertain the *usual* amount of energy derivable from a given amount of heat, and the provisional assumption of the rule may be practically useful; but to proceed beyond this, and to lay down an universal law, that heat has a definite and invariable mechanical value is unphilosophical, and, to my own mind, inconsistent with known facts.

A given amount of heat applied to expand air will raise ten times the weight that it will if applied to expand vapour of turpentine, and one-and-a-half times as much as if it were applied to expand steam. It may be answered, "Yes; but it also expands the vapour of turpentine or of water, as well as raises the weight!" True, but this is not mechanical energy as measured by foot-pounds raised; and to assume that it is equivalent to it is to beg the question at issue.

7. I have shown that the very term "mechanical equivalent of heat" is in itself fallacious. If there be any mechanical equivalent of the kind it is not an equivalent of heat, but of the disturbance or restoration of the equilibrium of heat. But the question now arises, is there such a maximum equivalent? Or can we, by skill and contrivance, increase indefinitely the amount of work to be got out of a given disturbance or restoration of this equilibrium? Take two separate pounds of water, differing from each other and from the temperature of the air by a given number of degrees of temperature; mix them together, and you get no work out of them; but connect them together by a bar of copper of a temperature between the two, and you get one end of the copper enlarged and the other diminished, and a series of changes and motions going on, till all parts of the water and copper at length arrive at an equal temperature. We have now got some work out of them. Now put one at one pole of a thermo-electric battery, and the other at the other

pole, and we get a current of electricity, magnetic, and other forces, all brought into play, and we get a good deal of work out of them. Next put them at the poles of another thermo-electric battery composed of metals differing in thermo-electric power from the former battery, and we accordingly get more or less work out of them, as the case may be. Is there any limit to this? The answer to this question requires proof.

8. Now to turn to the aspect of the question as presented in a galvanic circuit. Is there any necessary connection, under all circumstances, between the heat evolved and the magnetic force produced? Do they bear any proportion to each other? It is often assumed and stated that they are the measures of each other. Let us show that they are not. It is pretty well agreed that i, being the intensity, and r the resistance of the external part of a circuit, H (the heat of the exterior circuit) $= i^2 \times r$. But though many persons have asserted or assumed that other forms of energy vary as the heat, yet I think no one has ventured to maintain in so many words that the force or energy varies as $i^2 \times r$. Almost any experiment would instantly show that this was not true. What is the mathematical expression for the energy is by no means determined. *Quot homines tot sententiæ.* It is pretty well agreed that the action of a wire on a magnetic needle varies as $l \times i$, l being the length of wire. But when we come to an electro-magnet this will not hold. Some say its attractive force on soft iron varies as $l^2 \times i^2$; this seems the prevailing opinion, but it is certainly not true; or, at least, true in exceedingly few cases.

I have examined and compared more than 1200 experiments by different persons; and though it certainly varies as some function of l and some function of i, yet, under different circumstances, it seems to vary almost as any function of l less than l^2, and any function of i less than i^2. In fact, it seems to vary under different circumstances, according to laws which are as yet almost wholly unknown. But this we may, I think, safely say that it does not vary as the heat. Again, the law which connects the portative power of a magnet with its attractions at various distances is by no means uniform. To show clearly that there is no connection between the heat and other forms of energy in a wire, take the very instructive experiments published by Mr. Gore in the "Philosophical Magazine," of October, 1870. He took two helices of the same length; one made of platinum wire, the other of copper wire; he placed them at equal distances from a magnetic needle, and so arranged them in the circuit as to act on the needle in opposition to one another. The platinum evolved very much more heat than the copper wire, and, indeed, soon became red hot; but they both acted on the needle with exactly equal forces. So that clearly heat is not necessarily any measure of magnetic force, though, under certain accidental circumstances, it may be so.

9. The state of things revealed by comparing M. Soret's experiments with M. Favre's is something very remarkable. M. Soret has confirmed (by numerous experiments) M. Jacobi's conclusion that when a magnet is doing actual work it increases the resistance, and consequently diminishes the consumption of zinc; while M. Favre's experiments clearly show that a magnet whilst doing work absorbs less of the total heat of the circuit than whilst it is doing no actual work. These two facts, apparently both thoroughly well established, seem utterly inconsistent. How are they to be explained? Is it that M. Favre's magnets by working produced cold, and so diminished the calories shown in their own calorimeter, whilst, at the same time, by the repeated approach of the magnetised soft iron armatures to the magnets they produced counter currents of electricity, and so produced heat in the circuit, of which the battery, by its higher resistance, took the lion's share, and consequently exhibited in its own calorimeter? At any rate, I think it is quite plain that we have not yet got to anything like the bottom of the subject, and that our present theories cannot account for the facts revealed.

10. Take another case in which we make a battery do an unlimited amount of work. Put in the circuit a cell or voltameter, having no electro-motive power of its own, such as a solution of nitrate of silver with silver poles. Then, for every equivalent of zinc consumed, an equivalent of silver will be carried from one pole to the other. Now put two such cells in the circuit, and then every equivalent of zinc will convey two equivalents of silver the same distance; and by repeating the process we can make an equivalent of zinc move any weight of silver a certain distance.

It may be said we lose time: Yes; but the only time we lose is the time which it takes for the electric force in the first instance to traverse the circuit. The current once established, equal amounts of silver are conveyed an equal distance in equal times, by a consumption of zinc which may be diminished to any extent. Does not this prove, then, that we may make a certain amount of zinc do any amount of work, with this only condition, that it takes so much longer before it begins its work; but the work, once begun, goes on at an equal pace? If this be not so, then all the long-established elementary laws of definite electrolytic action are wrong. And so, also, if, instead of inserting more voltameters, we increased the distance of the poles in a single voltameter, so as to increase the resistance, we should in the same way increase the distance to which an equivalent of zinc could convey an equivalent of silver, and so make every equivalent of zinc do any number of times the amount of work it did before. And we have nothing to do but to increase the size of the electrodes and dimensions of the vessels containing them to make an equivalent of zinc convey any number of equivalents of silver any distance in any time. Electrolytic experiments have usually been carried on with the poles on a horizontal level. It would be curious to try what effect would be produced if one pole were placed over the other, so that the silver had to be lifted up, instead of conveyed horizontally. Would the power of the electric force be as indifferent to the force of gravity as it is to the resistance of the liquid to the horizontal motion of the silver? Where there is no opposing electromotive force at work, no resistance or length of circuit, less than infinite, can reduce the electrolytic or magnetic force to *nil;* and at every point in the circuit, however long the circuit be, this force is equal, and equal in equal lengths.

Even supposing the poles in a voltameter to be placed one over the other, vertically, one would say, judging from all analogy, that the only result would be that it would merely give an additional resistance to the circuit, and so diminish the intensity and rate of working; but that still an equivalent of each element of an electrolyte would be conveyed in each cell according to Faraday's law. And to counteract this additional resistance all we have to do is to enlarge our cells and plates. The experiment of placing the electrodes in a voltameter of nitrate of silver or sulphate of copper vertically over one another would seem to be almost an *experimentum crucis* of the truth of the theory of a mechanical equivalent of heat.

11. Does there not, then, exist a power in nature for force to multiply force—even in the same way as life is multiplied by life through successive generations, and one living being may in due time become a thousand without losing its own vital energy? Or, again, as one magnet may make a thousand other magnets, and yet all the while rather increase than diminish its own strength? Conservation of energy is true in mechanics. A pound weight at a metre of height from

the ground can at most, as we find by experience, only lift by its own descent another pound to a metre of height. And again, all experiment tends to show that multiplication of *matter* is impossible. But it is not proved that skill cannot get more and more mechanical force out of a given amount of chemical change or heat. This I venture to say is a question still at issue. In multiplying an electrical or magnetic or even a caloric power we are creating nothing; we are simply disturbing or restoring to a greater extent an equilibrium. Whatever amount of electrical, magnetic, or caloric power we get, there is still the same quantity of electricity, magnetism, or heat in the world; all we do is to disturb an equilibrium; the more positive electricity we get, the more negative we get, *pari passu*, and so in magnetism, so in heat; we cannot get north polarity without getting at the same time south; nor produce heat without at the same time producing virtually in some shape or other an equivalent of cold. The *total amount*, reckoning the plus to credit and the minus to debit, remains still the same. In mechanics we are allowed to balance plus motion in one direction against minus motion in an opposite direction; and the slightest preponderance on one side or another will set in motion an illimitable amount of mass. Why may not the same principle be applied to electrics, magnetics, and calorics? Cannot skill, mere skill, produce a less or greater disturbance and restoration of equilibrium, and so more or less force? What an enormous generation of force and disturbance of chemical and calorific equilibrium can we produce by the mere application to a forest of a lucifer match or spark, which having once commenced the motion, this constantly reproduces itself, till in a few hours carbon and hydrogen have all become carbonic acid and water, and a grand disturbance of calorific equilibrium has been produced which takes ages to restore itself, till at last, again, in due time, by the re-growth of the vegetation, carbon, hydrogen, oxygen, caloric have recovered their original state; and all this grand revolution and restoration of forces—this regular round of chemical activity—has been produced by a lucifer match or spark? Nothing has been gained or lost; the heat produced by the combustion has again been absorbed by the growth of vegetation; and the carbonic acid, water, and other compounds have again resolved themselves into the shape of wood and leaves. And cannot we by skill, without force, induce more or less of the combined electricities plus and minus, or of the combined heat and cold in matter, so to separate themselves that, by their re-combination, they may work for our benefit? This is the great physical problem of problems for science to solve. But if this is a problem beyond our power to solve, the next best thing to be attempted is to harness in our service the great powers of nature, to catch and force to our own use the circuits of electricity which are for ever circling the earth, and which hitherto we have only used to direct our magnets, and guide our ships; in fact, to mount, as skilful and well-taught Phaetons, the chariot of the sun, and force its four mighty steeds, the strongest and mightiest of all steeds, heat, light, electricity, and chemical force, to work for us our ploughs, and looms, and engines, and drag our railway trains. Twice, indeed, every day as I sit and watch the Thames, I see the moon drawing up and down along its silent highway trains of barges; for does she not as she goes round the earth, pull with her its tidal waters, with barges and all else that floats on them? I should like to see the sun put to the same kind of work, and as he rejoices like a strong man to run his race, to yoke him by the harness-cords of induction and conduction, and radiation, so as to force him to do for us more work, and to act more at our will and guidance, than we have hitherto forced or persuaded him to do.

VIII. OUR PATENT LAWS.

THIS subject so closely connects itself with the progress of scientific civilisation and the rights and comforts of many scientific men, that it cannot but command our attention; and the perusal of a recent book entitled "Abolition of Patents—Recent Discussions in the United Kingdom and the Continent," has much interested us. As to the laws themselves, we believe them to be unmanageable by lawyers, and they reflect no honour on legislation. They are consequently, and without blame to individuals, carried out in such a way as to corrupt all that are concerned, deceiving and robbing both the inventor and the public, and inducing all to deceive in return. The inventor pays for a patent, and obtains literally no protection unless he can pay for that also. One shilling would register his invention as fully as his present fee, and if the invention is printed in a journal many would be purchasers and it might pay itself.

In the present state of things patentees with no inventions come with crushing weight of gold, and prove to the satisfaction of ignorant juries that they have the truth, whilst the real inventor has a mere imitation. For this the legal profession are to blame for not inventing shorter processes of eliciting truth. We do not blame the men but rather the technicalities of statutes and the antiquated habits of thought among lawyers. It is a symptom of decay when men think long over trifles and give themselves to hair splitting. It is a symptom of vigour when the hairs are made into a rope and cut through with one stroke. We know the value of details in proper places, but life is short, and he who does not know how to dash through the spray of trifles may sit long in the wet before he lands on the shore.

We cannot by a statute law destroy the power of money, and the influence of cunning and falsehood. It is not sound reason which throws the blame of such proceedings as we have alluded to on inventors, on lawyers, or any one class. Government may not know in all cases to whom a patent is due, but it will know in very many cases, and the incredible proceeding of selling a patent to twenty individuals more than one to have it solely, which we are told has been done, ought to be quite impossible among us. Some officers have an interest in giving patents, but an officer who can act thus does not fulfil a useful function. If it is a rule of his office, and not his personal fault, the rule should be rescinded. This, surely, must be an infringement of right. It is no law of nature or mind; it must at best be some poor paper law that usurps that lofty name.

The lawyers are a powerful race; their minds are acute and their impetus is great. They are respected for many reasons, and one is because they are believed to carry, and do carry, within them the wisdom of the ages regarding the various relations of society; we know much of this is true. Still, as a rule, the lawyer in a court is merciless and not refined. He probes feelings, he exposes private relations, he hurts tender hearts, he is determined to win, fighting over the dead and dying. Men with delicate feelings and keen sense of justice are outraged in courts and leave as soon as possible. Inventors, real inventors, being often of this class, seeing no hope for themselves unaided, give part of their rights to others, who, being determined to make as much money as possible, begin the crushing, pounding work of a trial. The invention may be a real one, but in such rude hands it is soon made to assume all the offensive appearance possessed by the unreal. No distinction is made—truth-loving scientific men are ill-treated by men accustomed to deal with law-breakers, and the result is the same to a spectator as if no real invention or inventor was ever connected with the question. The character is often crushed in this way, and the man feels as if his moral nature were injured along with his property. No wonder, then, that we find so many of the best of men refusing to fight their inventions before a court and refusing to become patentees. Circumstances have made it appear as if the law and the lawyers had conspired with the wealthy to turn them aside by ridicule, by force, by disgust, by anything that will rid the court of their presence.

It is not wonderful, then, that the word "patentee" is not necessarily applied to a respectable man only. If the Government gave a patent to an honest man for an honest invention, the name of patentee would be courted; a patent would be exactly like a grant of land for services.

All inventors, however, are not thin-skinned and weak in fighting with the world. There are some who can defend themselves, but to do so must demand much vigilance. It requires wealth to carry out their ideas, and wealth to make them of benefit to the whole world. There are now living patentees who have altered the manufactures of Europe and America, and developed resources that would make a nation rich, and who have been able to fight their own battles without fear of lawyers or of courts. Inventions such as those of Bessemer have made the patent list stand respected by the pirates; but there are still men amongst us who have devoted years of their life to an object, hoping to gain at least a living by making their work useful to their fellow men. The result—they become hopeless, and in a sense paupers. Give patents under better laws.

There is much in this age to contend with. The old rights, and, indeed, the only known rights, of landed property seem to many to be unjust, and communism is making way among the nations of Europe. It would seem as if a certain mode of thought pervaded a whole generation. Whilst the poorer classes seek the re-distribution of land in some districts, the richer classes seem to think it right to demand a certain community of property in the thoughts of others. We are sorry, therefore, to find single-minded and unselfish men, in Parliament and out of it, opposing this latter class of property. They are doing work from which they will shrink when they see its real meaning. It is a higher aim, but no less a piratical aim. In France a leader of temporary fame said that all property was theft. In England we find members of Parliament saying that there can be no property in ideas. If a man spends all his life in digging land and growing corn he may sell it, but if he spends his life in making a machine to cut the corn, he is to give the benefit of his labour to mankind. A speaker at the Social Science Congress tells us that if a man discovers anything of benefit to mankind he ought to give the result of his discovery as an act of benevolence. Will any one give his land or his crops out of benevolence? And yet these come to many men without labour. People imagine that discoveries are floating fancies and are caught by some men as readily as such, whereas they are almost invariably the result of much labour—to some more and to some less, exactly as property falls to some men more and to some less easily.

Since certain of the speakers on patents have ventured to bring them in as opposed to natural law, they must be contradicted. Arguments they do not give, so we are saved much trouble. There is a property in ideas; and if we consider for a moment we find there is no perfectly stable property except in ideas. You may have property in land, but some one may come and dispossess you. Your property in ideas, however, you have power to retain against kings, armies, and barristers. We do know, and it is sadly true for humanity, that such property is so entirely in the power of a man that he can take it with him out of the world. This he cannot do with land, and yet a man is found to say that there is no property in ideas.

Now this is no quirk or fancy. The property is real in the fullest sense; and it is valuable; it can be exchanged for all the various goods of the world, and it can be made by labour whilst it cannot be made without it. The property in ideas has all the essential characteristics of other property. And the world must not give up its rights; it must hold good the title-deeds to ideas, because without such justice it will suffer more than is consistent with prosperity. Knowledge must have its rights as well as the mere possession of matter, and the world must agree to this as a law.

The reasoning adopted for free trade seems to solve the question to some minds; they do not seem to consider that neither Adam Smith, Cobden, nor Bright ever defended the right of a man to trade with another's property. These thinkers object to protection, but they never object to protect a man in the possession of his own house, and certainly never advise anyone to thrust his hand into another's pocket and take value from it, still less to take the very secrets of another's mind and sell them for selfish purposes. Honour forbids it; when, however, men are all honourable neither patents nor policemen will be required.

We were very much surprised to see that even a law officer of the crown confounded patents with monopolies. The natural law regarding patents is so simple that those who see it can answer the question regarding monopolies at once. Is a man to have no monopoly of his own house or his own lands, or will you so grind him down that he will have no monopoly of his own brains?

Natural monopolies are established facts; artificial monopolies for the interest of individuals not conferring a proportionate benefit on the state it is now agreed are unjust. The great East Indian Company was a monopoly, and is put down after a long and prosperous life. It is a natural monopoly when a man has the sole right to use his own wit, and for convenience sake we give the same to him who first takes possession of land. It is wonderful what may be done with a word. Where is logic in the application of the same word to the exclusive use for fourteen years of a man's own invention? How unfair the comparison! Fourteen years is seldom enough for the perfecting and introduction of an invention. To gain a reward, an inventor must work very hard and spend much money, and he gives all his knowledge to the public whether he gains anything or nothing. The state takes freely, but it gives nothing. The public takes and buys perhaps from the inventor, but only if it can profit by the purchase. If an inventor is allowed no time to be the sole user of his discovery, he is treated as a useful but despised tool that takes out the chestnut from the fire and gets burnt for its pains.

One of the greatest difficulties with which inventors must contend is the occult change in the mode of thinking, produced by the study of law and the study of science. It will be long before scientific men will be able quite clearly to make their distinctions felt before lawyers. The scientific man has his ideas of law and sequence founded on natural phenomena. So far his ideas are imperfect, because founded on imperfect appreciation and mixed with the ordinary prejudices of education, society, and personality. But the lawyer has his ideas of law from books written by lawyers, and containing the imperfect reasons, and the traditional errors, as well as wisdom of many generations. He has his mind framed in a faulty model. Of course we know the reply. These model thinkers have studied carefully, and the wisdom of ages is in their books, but at best it is a wisdom altered every year and condemned by scores of parliamentary bills, whilst the acts of natural law may be seen by everyone in unchanging order and beauty. The two laws produce two classes of mind. This is known in daily life. A barrister, an advocate, or judge, unless after special study, never fully comprehends a scientific idea; there is a *quasi* intelligence which passes before the multitude and amongst themselves, but scientific men always retire from them in a hopeless state.

The battle between lawyers and scientists is only beginning. It will be fought by lawyers as men fight who have hitherto had no opponents but of their own class. They are already preparing a somewhat inferior place for the scientific man, who with his natural laws has inaugurated a mode of reasoning diverging from that of the bar, and has in this beginning of the revolution stood out as all reformers do as a lawless person, whose character must be carefully scrutinised. There can be no doubt of the struggle. We cannot doubt the end. Natural law will rise and statute law will fall—how far we cannot tell, perhaps, till it agrees with natural law. This time may be distant, but the approach of it has begun, and barristers now beginning life ought to prepare for it by a sound drilling in scientific knowledge, and, if possible, by a little hand labour in the workshops of mechanicians or physicists.

The inevitable tendency of the mind of the bar, caused as it is by education and habit, must not lead us to speak evil of the men. How could they do otherwise? Neither do we suppose they deny what we say. We all resemble that which teaches us. A similar result occurs among scientific men. They are not supposed to be perfect, even if they do study natural law; no, on their side is too often shown too much ignorance of the social aspects of a question, and these can be best seen by the barrister. They are also too hard and self-sufficient, receiving a habitually unyielding trait from the necessarily unyielding character of natural law—a characteristic which constant study has transferred to the man. The two must learn to work amicably, but there is a large district now in the occupation of the law which must be taken by scientific men.

Some of the arguments used against patents are to us very amusing. The speaker, who says that a patent is a monopoly, nevertheless adds that, in early times, "if new kinds of business were to be established, it was not unreasonably thought safe, or even needful, to allure by promise of exclusive privileges." So it appears that even monopolies might be given in the old way to men who did not invent but who did what was equal, namely, brought new manufactures from other countries. We think so, too, under certain circumstances, and as a patent is for a description of a new manufacture, the right cannot be given more fairly than to him who describes it, but we would add that he must carry it out or attempt to do so within a given time. We know as a certainty the history of several patents, and we know of none which were successful without labour. But even if a patent were for some idea that suddenly struck a person, why should we refuse to reward him for giving it to us. If a man finds a diamond we do not steal it from him. Are not men's minds rushing through the regions of thought to discover the good and the useful? Why should we steal what they find?

We agree with M. Renard—"Common sense and equity would join to say that when a scientific man indicates a discovery or an invention, that discovery or invention remains at the disposal of every one if the finder does not claim the exclusive right to work it;" supposing it to belong to the class to be made useful in the arts.

How different this clearness from the wanderings of M. Chevalier and others, who are not sure if a man can claim an idea, because many can hold it at the same time. The patentee does not attempt to keep others from the idea; he wants only his reward for teaching it. Surely this is agreeable to all law and justice.

Mr. Scott Russell is quoted in the recent discussions as giving an objection to patents. It is that the moment anything is patented everybody avoids it, lest he be subjected to an expensive prosecution. Surely it is the same thing with your purse—everybody, except the thief, avoids taking that, lest he be subjected to crime and disgrace.

Mr. Grove's objection took us by surprise. He says that it is natural that people should yield to the patentee for fear of prosecution. Perhaps this is the same as Mr. Scott Russell's without the comedy. We do not question Mr. Grove's experience, and we say, what a serious responsibility it is, then, to give a patent to a man.

But the law seems to sell patents to anyone. It says, like all hucksters, "*Caveat emptor.*" To us these patents have a character not different from forged notes if given to more than one individual, or useless to him.

Mr. Grove makes another objection. It takes too much time to try the patent cases. On this, also, we have to quarrel with the management of patent law affairs. The power of arriving at the main point in a discussion seems to be lost; we drivel for days over trifles.

Perhaps we have not legal men enough. We have known twenty men brought two hundred miles and sent away in an hour, because the barristers were busy with other matters. The law itself being bad, the results of course are saddening. We have heard that mercantile men have sometimes set about conducting arbitrations in order to diminish the evil, but the result has been that they have tried to imitate barristers, and have of course been still more unfortunate.

The "Journal of Jurisprudence" calls patents "trading monopolies," quite forgetting, we think, the characteristics and fine distinctions. Mr. Webster, Q.C., says, truly, that people rush after patents for applications of some new idea or invention. Yes, unfortunately; and it is for lawyers to give the true limits, if possible.

Mr. Meadows has a horror of patent laws, and says "one evades it (a patent) by designing something else, perhaps as good in itself, but giving one infinite trouble without any advantage to the holder of the patent." The patent here must have been doubly good. So good that it was necessary to imitate it, and two inventions were obtained for the public instead of one.

Mr. Hale has an objection to patents because so many useless ones are obtained and they obstruct the trade. Why use them if they are useless? If you do not meddle with such useless things they will not hurt you.

Many known men are brought forward as being much troubled by patents. Probably people had invented things before them, unfortunately taking advantage of their priority of existence by making prior inventions.

One says that considerable inconvenience and great obstruction to the trade are caused by patents.

One authority finds that many people are thinking of the same thing, and it is not fair to give the patent to him that first runs to the office.

There may seem a hardship here; we have known not a few cases. We have picked many a much desired book from a catalogue and sent for it. But some one had it before us. Is there a scientific man, thirty years of age, who has not had nine-tenths of his best ideas anticipated? Did your own friend not catch that fine trout which had run off with your hook half an hour before? Were there not a hundred applicants for that situation and only one got it? We must not sink into helpless weakness. He who wins must enjoy. Do not let us seek impossibilities or grow sick with sentimentalism.

A witness from Liverpool finds it very hard to move in any direction without treading on a patentee's toes. The meaning of this we suppose to be that he cannot improve his business without the assistance of patented inventions, as he cannot invent sufficiently for himself. Surely he ought to thank the patentees for helping him.

Mr. Montague Smith, the judge, has found great inconvenience from the multiplicity of patents which the inventor has had to wade through to see that he has not been anticipated. The meaning of this seems that the inventor was obliged to see if the Government had not deceived him by selling the rights to some one else before him. The law again failing to come up to our expectations.

Sir W. Armstrong then says, You cannot give a monopoly without excluding other persons who are working on the same subject: That is a truism. Let him who works and finds first be rewarded. Will you reward those who find after him ?

Mr. Platt gives a good objection to patents as they are, but it applies wholly to the law or to that law-officer who allows patents, or both, and we again say that no one has sufficiently explained how thoroughly unjust are the workings of our patent law.

Mr. Woodcroft, to our mind, settles the question against the mode of giving patents. "I know of existing patents which are but old inventions as old as the hills."

Another objection is that so few are remunerative. We need not answer this. With such laws it requires great power to fight your way. Inventors are not rich men, and the power of wealth and legislation may crush them still further. If lawyers were to invent such laws as would protect these men the generation would bless them, and true invention would increase.

Mr. Richard Roberts is quoted as saying that a quart of ale will bring out secrets of trade, so impossible is it to keep a process without a patent. We know that the system of bribery is common enough. There are men who have laboured for years, and discovered something, and desire to give it to their children. When it can be carried on by one man he will have no patent. There is even one business which has had a secret for three generations. If patents are not given men will resort to this mode of secrecy, as they do now in Prussia. In that country patents are so few that it seems the belief that only bribery or friendship can gain one, and works are closed up to visitors to a great extent. We cannot doubt that the till of late slow development of industry in Germany has been owing to the want of decision in giving patents; want of openness, and, so far as we can make out from Germans, want of official honesty, and want of communication among manufacturers, are productive of the same stagnation as the want of roads in a country.

Some persons are quoted as saying that if there were no artificial rewards the inventors would be as numerous as at present. That is only half true. Poems would be as numerous; mathematicians would be, and scientific men generally might be, but even Sir W. Armstrong forgets the expense of bringing out an invention. A man might give his idea for the honour, but would he give the time and expense of trying it, and, until he does so, the idea is valueless to the public. The public gives the patentee nothing for his idea. They pay for the soap he makes, or the pens, or the cannons; and the country knows to its cost how much money experiments on the latter require. Not only would real finished inventions be fewer, but it is a necessity of the case that it should be so. The inventor obtains money on loan frequently, to be able to complete his invention. It is only because he can afterwards make money by having the exclusive use of the patent for a while that he can borrow money. If no patent, then no experiments. There would be as many ideas, perhaps—that is to say, the brains of men might wander as much—but a finished invention is a serious and expensive affair. We have too often seen the inventor in the throes of his study and struggle of experiment to doubt this, and we have seen it last too many years to be led by the fancies of men who seem never to have lived at the heart of the nation, nor seen the method in which the blood moves.

But do the patent laws present to us no difficulty? Do we pretend to see all clearly where others have been perfectly blind or given to ghost-seeing? We have no such satisfaction. Still, without pretending to know how to settle the question, we have confidence that every difficulty will be cleared away regarding English patents by a broad, common-sense view of an invention. We should like to get rid of the exclusively lawyers' mode of viewing facts, and of his humiliating and time-wasting formalities. When we look abroad we have no difficulty in seeing how some countries can get on without patents. Switzerland has none. It obtains inventions by taking from other countries such as suit it. Being small, it has the best of it. So practically does Prussia to a great extent. Nations are selfish, and if they can gain by robbery, they will find a new name for the offensive act.

The opinion of Count Bismarck on patent laws is to be seen at page 185 of the work quoted. He, like others, sees "no natural claim on the part of an inventor to be protected by the state." In other words, there are men the fruits of whose industry he will not protect; this is simple monopoly and despotic respect of persons. We can easily see why an old aristocracy ignores the value of invention, just as monied men dislike it. It raises the inventor to power as well as wealth. After this, the opinion of Count Bismarck can be of no value in our eyes. He thinks that "the remarkably developed system of communication and conveyance now-a-days, which has opened a wide field to real merit, and enables industrial men promptly to reap all benefit of production by means of enlarged outlets for their articles will, generally speaking, bring those who know how *to avail themselves before others of useful inventions* to such an extent ahead of their competitors that, even when no permanent privilege is longer admissible, they will make sure of a temporary extra profit in proportion to the service rendered to the public." This is not to the purpose. The sharpest man who can first come forward is to be the gainer. The inventor is not the sharpest, is never the sharpest man, and can never make such gains as we have spoken of.

We can imagine a statesman asking himself, by what means can he evolve most invention out of the nation? whether by wringing it out unjustly or acting with kindness? Rulers have tried the first in all nations, but of late they are finding out that honesty is the best policy. We can best benefit a nation by letting every individual feel that he is governed with justice—that he and his property are free. We have been told of inventors who had rich men looking in at their windows to see the new machines; these were the sharp fellows that knew how "to avail themselves of discoveries before others," and which are especially favoured by Count Bismarck. One suggestion, however, strikes us as good for a distant day. It is to have an European and American system. "The parliament of man, the federation of the world," will rule in time, and it cannot be begun so well by any as by the authors and inventors. Until that is fully developed we cannot hesitate in saying that he who destroys patent right has begun a movement which will do incalculable evil. He, however, who teaches us how to remodel the laws will benefit us in a way such as he did who first brought law among mankind; he will be to us as a "Rhadamanthus, Ruler of the Blest."

We cannot see M. Renard's difficulty, who thinks that, because coal is a natural product, no one can claim a reward for discovering it. We say the discoverer deserves a reward, and gladly would we give one to him who gave us a new fuel.

Inventors are expected to do all the benevolence, and to give away their lives for the world, whilst men with "real property" enjoy it themselves. If you destroy the property of inventors, the time may come when the title-deeds of other property will be asked for. We fear such a time, and such men as will make the demand; and we prefer to see property safe, and to respect the feelings of man as fully as we would respect the cannon or the bayonet. Or is this opposition to the property of thinkers the new form that a material philosophy is taking? Is it denying the existence of all property that is not made of solid matter; determined to enjoy the material reward of thoughts without itself having the trouble of thinking?

We are told that only three per cent of patents are very successful, but if 5000 are taken out annually, that makes 150 very successful patents every year, and as they go on for 14 years we have 2100 of this higher class in action at a time, making money, and, of course, giving occupation to many individuals. The owners are all busy men, stimulated by the love of honour and money, and all that money brings. We can scarcely imagine the amount of work to keep 2100 successful patents in motion. They have a power extending to the most distant places, and their influence at home is felt in every household without exception.

If three per cent be really the amount of very successful patents, let us ask if it is a small amount. If 100 young men entered Cambridge with the hope of being mathematicians, do three per cent obtain any honours whatever? Of 5000 men who write poems, do three per cent ever become known? Of 100 men in any profession are three per cent made public characters by their talents? Of 100 men that become medical practitioners, how few are ever known to publish a book, or to advance the science? Of the thousands who have begun chemistry within the last 20 years, scarcely five can be got to fill a professor's chair when demanded. Do three per cent of those who enter Parliament ever become distinguished? To stop short. Three per cent of mankind do not rise above their fellows so as to become prominent, then why should we expect more from patentees? But the world does expect more, and we believe that even more would be found if the laws were just, for at present they do not elicit inventions from that class of mind well able to invent but afraid to fight before lawyers.

However, there is still the real difficulty—what is an invention? One man makes an invention as described in the volume; another crushes him by manufacturing 300 specimens at a time instead of one. If the first man's patent is held good, the delay of the second for 14 years may be a great loss to the country. Would it not, in such a case as this, be fair to compel the second to pay a sort of royalty to the first?

Many of these difficulties that have oppressed the bar would be easily removed by a tribunal of common sense. Could the lawyers not invent such a tribunal?

We admire a judge against whose opinion an Act of Parliament was brought. He replied, "What! am I to put the young man in a position that will almost certainly ruin him merely because of an Act of Parliament." It is such judges as this last that we think able to do justice by the use of common sense. Is there no hope of obtaining for them freedom of action.

We must now for a moment revert to that frequent assertion that inventions rise up in various places at the same time; the argument seems to be that nothing is lost to humanity by the loss of an individual mind. This, we think, would not be asserted by any of the men who have spoken on patents. They would be afraid to see the actual meaning of their own words. But if men will rush upon great problems of psychology and history without consideration, we can expect only a superficial result. If we look at the subject from the narrow field of the daily patent lists only, we may imagine that so many minds are occupied that nothing can be lost; but if we look on the history of invention, we come to a different opinion.

There are obvious conclusions drawn by several individuals at a time, many of such patents we should be glad to get rid of entirely, but good sound inventions come rarely to more than one mind at a time.

It is to the individual that Nature and Providence give the rich, rare gifts that advance humanity. The history of the few has been the history of human progress, and a lost thought may roll for ages through creation without finding a mind to comprehend it or a brain to make it useful to society.

PROGRESS IN PHYSICS,

(Including Light, Heat, Electricity, Meteorology).

LIGHT.

Mr. C. B. Boyd has devised a novel form of telescope. It consists of a plane speculum, having an equatorial motion, which reflects an object to a conclave speculum at a distance; this in turn reflects the image to the eye-piece situated behind the plane mirror; a hole in the centre of the latter allowing this to be done. This arrangement brings the mirror and eye-piece close to the observer, while, at the same time, it enables one to have a very long telescope, without any great extra expense, for an observatory: it combines, in fact, both observatory and telescope in one construction. A thirty-foot telescope needs an observatory that will cost £10,000; while in this arrangement the observatory proper needs only be sufficient to cover the equatorial, the instrument itself being 50, 100, or 500 feet long.

In a letter from Professor Young, of Dartmouth College, N.H., we learn that he has succeeded in obtaining photographs of protuberances on the sun's limb. They were obtained by attaching a small camera to the eye-piece of the telescope and opening the slit somewhat widely, working through the hydrogen line near G. Three-and-a-half minutes' exposure was required, and the double-headed form of the prominence is evident.

Professor Young has also designed a new form of spectroscope for observations upon the solar spots and protuberances, a detailed description of which appeared in the "Chemical News," vol. xxii., p. 277. Although the instrument has the dispersive power of 13 prisms of heavy flint, each with an angle of 55°, it weighs less than 15 pounds, and measures only 15 inches in length, 8 in breadth, and 4½ in height. The collimator and observing telescope have each an aperture of ⅞ths of an inch, and a focal length of 7 inches. The light from the slit, after passing the collimator, is transmitted through the lower portion of a train of six prisms of heavy flint glass, each 2¼ inches high, and having, as stated above, a refracting angle of 55°. A seventh *half-prism* follows, and to the back of this is cemented a right-angled prism, by which, after two total reflections, the light is sent back through the upper part of the same train of prisms, until it reaches the observing-telescope. This is placed directly above the collimator, and firmly attached to it. A diagonal eye-piece brings the rays to the eye in a convenient position for observation.

Observations of the Solar Protuberances.—With this instrument Professor Young has observed about forty different prominences. Fig. 11 represents a small one, which was observed upon the E. limb of the sun on September 14th, about 4.30 p.m. From the point marked A, which was very brilliant, a small fragment detached itself and rose towards A′, enlarging in size and growing fainter as it rose. It disappeared (from faintness) in about twelve and a half minutes, at a distance of 2′ 30″ above the limb of the sun, as determined by the time, 8·5″, which was occupied by the intervening space in passing over the slit of the spectroscope. Allowing for the obliquity of the motion to the parallel of declination, the length of

Fig. 11.

path passed over by this cloud was more than 90,000 miles, and the velocity above 120 miles per second. Fig. 12 represents a prominence observed September 20th, at 4 p.m., on the S.E. limb. (Pos. S., 60° E.) It was a nearly vertical stream, made up of spindle-formed filaments, and had attained the enormous height of 3′ 20″, or 90,000 miles (determined, as in the case above mentioned, by a time observation, corrected for inclination). It was very brilliant near the base, and at two or three other points along its length. At 4.30 it was nearly gone, only a few faint wisps of cloud remaining. Another observed on September 27th, at 4.10 p.m., and situated on the W. limb of the sun, is represented in Fig. 13. It was formed of separate, well-defined narrow streamers, which appeared to consist of matter, first violently ejected, and then as violently deflected, by some force acting nearly at right angles. The altitude of the highest point was 1′ 25″, the length of the whole about 3′ 30″.

FIG. 12.

Bright Lines.—In the spectra of different protuberances, the following bright lines have been observed, the numbers referring to Kirchhoff's scale: C; D_1; D_2; D_3; 1474; 1515; b_1; b_2; b_3; b_4; 1990; 2001; 2031; F; 2581·5; 2796; *h*—17 in all. On one occasion, September 27th, the base of a prominence on the N.W. limb, close to a spot just leaving the limb, exhibited as many as twelve or fifteen short bright lines between E and F, which are not included in the above enumeration. The line, 2581·5, which was conspicuous at the eclipse of 1869, seems to be *always present* in the spectrum of the chromosphere, and shows the form of its upper surface or of a protuberance nearly as well, though not so brightly, as the 2796 line. It has no corresponding dark line in the ordinary solar spectrum, and, not improbably, may be due to the same substance that produces D_3. The reversal of the sodium and magnesium lines is not at all uncommon. In some instances these lines were so bright that, on opening the slit, the form of the prominence could be made out through them. This was the case with a small hand-shaped prominence observed on September 27th. Comparing the form thus seen through D_1 and D_2, with that given by D_3, it appeared that the sodium line was sufficiently developed for observation only along the edge and at one or two bright points in the prominence, most brilliantly neither at its summit nor its base. Fig. 14 represents the appearance (the slit was perpendicular to the sun's limb). The case was similar with the magnesium lines.

FIG. 13.

FIG. 14.

Spectrum of Solar Spots.—The most interesting phenomena were exhibited by a large group, which was first observed near the E. limb on September 19th. Changes of wave length were frequent in its neighbourhood. Figs. 7 and 8 represent the appearances assumed by the F and C lines respectively, at the times indicated below each figure, during an observation on the afternoon of September 22nd. The point where these changes of wave-length occurred was at the western edge of the penumbra. The C and F lines were reversed in some portion or other of the group nearly every time Professor Young observed it. On September 22nd, the sodium lines were both reversed for several hours, while D_3 appeared as a dark shade. On September 28th, again, at 4 p.m., the southern nucleus of the group (which at this time contained four large umbræ,

FIG. 15.

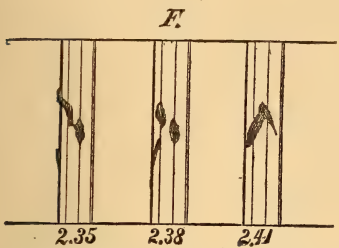
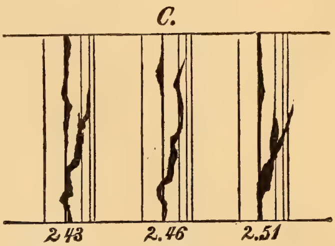


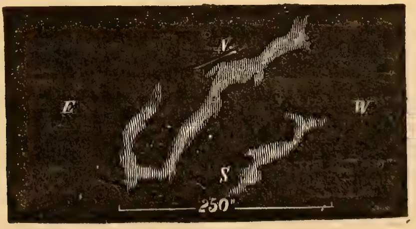
FIG. 16.



besides many small ones), reversed all of the following lines, viz.: C; D_1; D_2; D_3; 1474; b_1; b_2; b_3; b_4; F; 2796; and *h*. All of these were *conspicuous*, except 1474; D_3 and b_3 especially so, and the latter (a nickel line) showed considerable changes of wave-length, alternate increase and diminution, which were not shared by its magnesian neighbours, b_1, b_2, and b_4. At 4.05 p.m. the brilliance of the F line increased so greatly that it occurred to Professor Young to widen the slit, and to his great delight he saw upon the disk of the sun itself a brilliant cloud in all its structure and detail identical with the protuberances around the limb. Indeed, there were two of them, and there was no difficulty in tracing out and delineating their form. Fig. 17 represents them as they were from 4.05 to 4.10; Fig. 18 gives the form at 4.15-20. They were then considerably fainter than at first. During the intervening ten minutes the other lines of the spectrum were examined, and it was found that the form could be distinctly made out in all the hydrogen lines, even in *h*; but that the reversal of the other lines, including D_3, was confined to the region immediately over the spot-nucleus, where the smaller but brighter cloud terminated abruptly; or, rather, originated. The larger one faded out at both ends. When the clockwork of the equatorial was stopped,

FIG. 17.

FIG. 18.

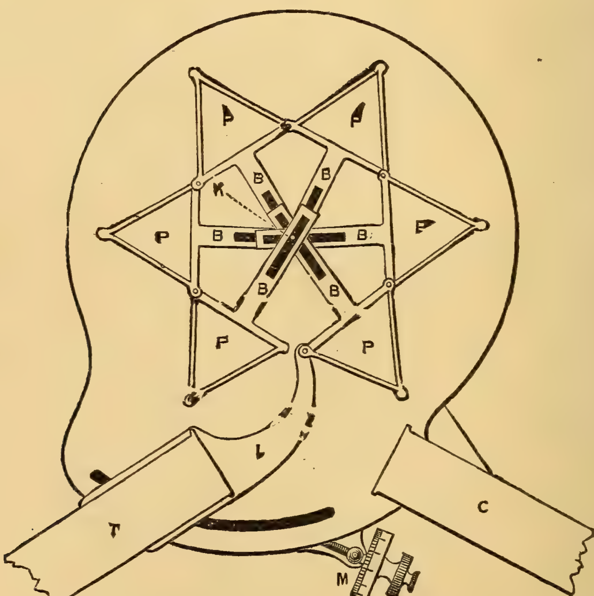


the luminous cloud took 16·7 seconds of time to traverse the slit which was placed parallel to the hour-circle. This indicates a length of at least 130,000 miles, without allowing anything for the foreshortening resulting from the nearness of the sun's limb. By 5 o'clock the clouds had nearly disappeared; a little rack alone remained. In the telescope this group of spots, from their first appearance, exhibited a strong yellowish tinge, which appeared to overlie all the central portion of the cluster. So conspicuous was it that several persons, unaccustomed to astronomical observation, noticed it at once before their attention was called to it. The penumbra of the group was unusually faint.

At a late meeting of the Manchester Photographic Society, Mr. Kershaw showed some very interesting photographs, illustrating one of the many uses made of photography in the war raging on the Continent. The subject consisted of the important portions of Paris newspapers cut out, arranged in columns, and photographed on talc. What would form nine columns of a newspaper was copied on a space about two inches by one inch and a half, and was perfectly legible with a glass of moderate magnifying power. These talc photographs left Paris by balloon post.

Mr. Browning, the eminent optician, has arranged a spectroscope in which the prisms are automatically adjusted for the minimum angle of deviation for the particular ray under examination. In spectroscopes of ordinary construction, when several prisms are employed, a great deficiency of light will be

FIG. 19.



noticed towards the more refrangible end of the spectrum. This arises from the fact that the prisms are adjusted to the minimum angle of deviation for the most luminous rays which occupy the middle of the spectrum. Fig. 19 shows the method in which the change in the adjustment of the prisms to the minimum angle of deviation for each particular ray is made automatically. In this diagram, P P, &c., represent prisms; all of which prisms, with the exception of the first, are unattached to the plate on which they stand, the triangular stand on which the prisms are hinged together at the angles corresponding to those at the bases of the prisms. To each of these bases is attached a bar, B, perpendicular to the base of the prisms. As all these bars are slotted and run on a common centre, the prisms are brought into a circle. This central pivot is attached to a dovetail piece of two or three inches in length, placed on the under-side of the main plate of the spectroscope; which is slotted to allow it to pass through. On moving the central pivot, the whole of the prisms are moved, each to a different amount in proportion to its distance in the train from the first or fixed prism on which the light from the slit falls after passing through the collimator, C. Thus, supposing the first prism of the train opposite C, represented in the diagram, to be stationary, and the second prism to have been moved through 1° by this arrangement, then the third prism will have moved through 2°, the fourth through 3°, the fifth through 4°, and the sixth through 5°. As these bars are at right angles to the bases of the prisms, and all of them pass through a common centre, it is evident that the bases of the prisms are at all times tangents to a common circle. Now for the contrivance by which this arrangement is made automatic. A lever, L, is attached to the corner of the triangular plate of the last prism. This lever by its further end is attached to the support which carries the telescope through which the spectrum is observed. Both the telescope and lever are driven by the micrometer-screw, M. The action of the lever is so adjusted that when the telescope is moved through any angle it causes the last prism to turn through double that angle. The rays which issue from the centre of the last prism are thus made to fall perpendicularly upon the centre of the object-glass of the telescope, T, and thus the ray of light travels parallel to the bases of the several prisms, and ultimately along the optical axis of the telescope itself, and thereby the whole field of the object-glass is filled with light. Thus the apparatus is so arranged that on turning the micrometer-screw, so as to make a line in the spectrum coincide with the cross wires in the eye-piece of the telescope, the lever, L, attached to the telescope and prisms, sets the whole of the prisms in motion, and adjusts them to the minimum angle of deviation for that portion of the spectrum.

The Paris correspondent of the "Engineer," in an interesting letter sent by balloon post, speaking of the use of the electric light in war, says:—"War certainly does something for science. The use of the electric light is common to both sides in the present struggle, but the French have used it largely. The apparatus set up on Montmartre is arranged by M. Bazin, and is electro-magnetic. The central cylinder supports four series of double coils, covered with copper wire, enveloped in silk; the cylinder is rotated by a small steam engine of three-horse power, making 400 revolutions per minute. The lamp used is of the ordinary form, with the Foucault-Duboscq regulator. The reflector is parabolic in form, and the whole is surrounded by a shield to hide it from the enemy. This light, from its elevated position, commands the whole of Paris and the plains around. A spectator on Montmartre sees distinctly the details of the *façade* of a building which stands 2600 metres off; at 2900 metres a man may be seen standing at a window; at 3000 metres a mass of cavalry or infantry is distinguishable; and at 4000 metres the dome of the Invalides, with its bands of gold, is brilliant. A man cannot be seen on the dome at that distance, but on walking towards the building all soon becomes clear. On the ramparts, at 3800 metres from Montmartre, the light is sufficient to read an ordinary newspaper. Thus, though the practical effect of the lamp only extends about 300 metres from its position, the field is illuminated to the extent of 700 metres, for the benefit of all placed between the light and the object. Thus a sentinel on the ramparts can see about 3000 metres from the *enceinte*, and by this means strict watch is kept upon the plains around the city at night, as far, in one direction, as 1000 metres beyond St. Denis. M. Bazin is now occupied in applying his apparatus to the purposes of night telegraphs, by the adoption of the system of flashes, and with the aid of coloured lenses. A corvette, the *Coligny*, already possesses such a signal apparatus, and the signals are distinctly visible at more than eight miles distance."

The spectrum of the light during the magnificent Auroral display of the 24th and 25th of October was obtained by several observers. From the following abstract of their accounts it will be seen that there is a general uniformity in the results; the presence of hydrogen appearing to be established. Professor A. H. Church, of the Royal Agricultural College, writing to the "Chemical News," says that he was able to observe the spectrum of the aurora as seen at Cirencester. He saw a steady yellowish green line, and frequently a brilliant red line near that of calcium. A pale line was also seen in the green, and a more definite one in the blue—these last not being constant in occurrence.

A correspondent of "Nature," "T. F.," observing at Torquay, saw four lines in the rosy portion, and one in the greenish; one strong red line near C, one strong pale yellow line near D, one paler near F, and one still paler beyond. The C line was very conspicuous, and was intermediate in position and colour to the red of the lithium and calcium. His opinion is that there were two spectra superposed; the red portions showing the four lines (probably hydrogen) and the greenish only one, near D.

Mr. W. B. Gibbs saw two bright lines; one, a red, looking like the C hydrogen line, and one a greenish gray.

Mr. Elger saw a red band near C, a bright band near D, a faint and rather nebulous line, supposed to be near F, and another very faint line between the latter two.

Mr. H. R. Proctor saw a brilliant red line more refrangible than the hydrogen C line.

Mr. J. R. Capron saw a line in the red very much like the lithium line, and a line in the light green like one of the lines from the larger nebulæ.

It may be of interest to compare these observations with those of other observers made previously. J. A. Angström found the light of the aurora to be almost monochromatic, consisting of a single bright line less refrangible than the red calcium lines. With a wide slit, traces of three other bands were seen. Professor Winlock found the spectrum to consist of four green lines and one blue line. Three of the green lines coincided with lines seen in the spectrum of the corona as observed by Professor Young during the total solar eclipse of August, 1869.

Microscopy.—A correspondent of the "American Naturalist" makes the old complaint as to the insecure mode by which dry objects are commonly mounted. Paper, paste, and gum are, as usual, the origin of all the mischief, affording no protection against damp, scarcely keeping out dust, and favouring the growth of numerous fungi, which, sooner or later, spoil the preparation. He suggests, as the only remedy, a steady refusal to purchase objects so mounted. This state of things is just as common in England; the majority of dry objects being mounted in the above well-known manner. To those who merely require a few showy preparations to gratify the taste of the moment for sensational objects, these defects may be of little importance, but to the student the storing of objects for reference at, possibly, a very distant period, is a matter of the utmost consequence. No dry preparations should be tolerated unless secured in air-tight cells, and every precaution should be taken not only to exclude damp, but also not to include it; the object should not only be thoroughly dried, but the mounting should be done in a dry atmosphere; there is no reason why as much pains should not be taken to keep the damp out of dry objects as there is to retain the liquid in the cell in the case of fluid mountings. Our microscopical cabinets ought to be available for the use of the observers of at least the next century.

Mr. T. Greenish has examined microscopically the various articles sold as lint. The result appears to be, that those labelled "Lint" consist entirely of cotton, while, with one exception, those described as "*Flax* lint" contain more or less cotton, but by an ingenious arrangement of the materials the linted surface is composed almost entirely of flax. There is an idea prevalent that cotton forms an extremely bad application to sores, causing great irritation, and linen is generally preferred in dressing: should this prove to be the case any adulteration of flax lint would be an undoubted evil, but lint wholly of cotton is used extensively in hospitals, and had it been productive of any bad consequences its use would soon have been discontinued. The peculiar lint known as *charpie* used in continental hospitals and the marine lint were also noticed. Microscopical examination fails to throw any light upon the alleged irritating properties of cotton, as, although, the fibres are flat, they have a perceptible thickness at the edges, and are, moreover, rounded; the section of a cotton filament being somewhat like that of a thin tube flattened. It was suggested that the cause of irritation was not owing to the form of the fibre, but to its hygroscopic power of twisting when moistened and causing irritation by its movement.

Mr. J. W. Stephenson describes a new Erecting Binocular Microscope, which he has had constructed for the purpose of carrying on dissections under moderate powers. The pencils transmitted by the object-glass are equally divided and reflected by a pair of prisms, and again reflected by another prism into the two bodies, which are placed at a very convenient angle for dissecting operations; the image is, as the name indicates, non-inverted, and the two fields equally illuminated. For ordinary observations the Wenham binocular will be found preferable, not only on account of one-half of the pencil being transmitted without reflection, but also from the facility with which it may be converted into a monocular instrument, But for the especial set of purposes for which it is designed nothing has yet equalled Mr. Stephenson's instrument, either for comfort or convenience; the definition is also remarkably good when it is used with the low powers for which it is intended.

In the "Transactions of the Devonshire Association for the Advancement of Science," Mr. E. Parfitt describes and figures twenty-seven sponge spicules from the greensand of Haldon and Blackdown, near Exeter. The specimens, which were obtained by Mr. Vicary, are in remarkably perfect condition, being imbedded in a very friable rock, from which they separated with the greatest ease. Many of the spicules are identical with those of recent species of sponges, described by Dr. Bowerbank, "British Spongiadæ." One, Fig. 13, closely resembles the spicules of *Euplectella aspergillum* (Venus's flower-basket). Another, No. 26, is much like the spicules of *Pheronema* (*Holtenia*), dredged by Dr. Carpenter, in the "Lightning" and "Porcupine" expeditions.

Spontaneous generation still furnishes matter for discussion; it is a good subject for controversy, and from the nature of the evidence procurable, is not likely to be satisfactorily settled one way or the other. Mr. B. T. Lowne states that he has boiled spores of *penicillium* in sealed tubes, in a solution of acetate of alumina, and that within twenty-four hours many of them had germinated. This goes very far to prove that some germs cannot easily be killed. If such is the case with spores known to exist, how many included germs that have escaped observation may retain their vitality, in spite of the severe treatment to which they have been subjected.

The subject of high power definition, especially with regard to the nature of the markings on the scales of *Thysanuridæ* (*Poduræ*) and other insects, is now being actively discussed, and will probably give rise to as long a controversy as that on the *Diatomaceæ*. Mr. J. Beck believes, from the manner in which fluids diffuse themselves on the scale, that one of the surfaces (the outer when *in situ*) is smooth, the other, the inner one, corrugated, or strengthened by longitudinal ribs running from the base to the apex of the scale ("Monthly Micro. Journal," Nov. 1870). Mr. S. J. McIntyre has examined not only the scales of most of the *Thysanuridæ*, but also those of several butterflies and other insects; the result is very fully described in his paper ("Monthly Micro. Journal," Jan.). Mr. Slack, from evidence derived from torn and damaged scales, considers that the beaded markings shown by high powers under certain arrangements of the illumination are realities, the tear never passing through but between the beads. Dr. Piggott discusses the subject from an optical point of view, and calls attention to the means by which false images are produced; he has carefully studied the shadows formed when light is passed through arrangements of intersecting glass rods, spun glass, and other substances, and endeavours to explain the phenomena of the podura and other unknown markings by reference to the curious appearances so produced. Mr. Slack, as an example of deceptive appearances, cites an instance of minute cracks in a film of silica, which when focussed in the manner most satisfactory to the eye resemble tubes instead of fissures in the thin layer of silica.* Professor Huxley considers that, at present, we have arrived at the end of our optical resources, so far as histology is concerned, and does not believe that more can be seen by the aid of a 1-50th than with Ross's 1-12th. Dr. Beale invites Professor Huxley, or other skilled microscopist, to examine a preparation to be made by himself, first with a 1-12th or 1-16th, and to make a careful drawing of all that he can discover, and afterwards to continue the observation with a 1-25th or 1-50th, and then to decide whether these higher powers do or do not reveal new structural details. Prolonged as the controversy will be, there can be but little doubt that it will ultimately tend to the further improvement of the microscope, by showing where high power definition is defective, and pointing to the means of eliminating errors which possibly may exist even in the best of our present objectives.

Referring to the notice in a recent number of this journal, of the remarkable magnifying power of 100,000 diameters having been obtained by a New York pseudo-microscopist, an American correspondent says, "It was done by eye-piecing and you know what that amounts to. But it was said that with this power they photographed the *Pleurosigma angulatum*, showing dots two inches in diameter. This is hardly a correct statement. Looking at the *angulatum* through their microscopes, they made drawings of what they *thought* they saw; then made a pine model and photographed that! It was all done to advance the interests of a well-known manufacturer of cheap microscopes, but the parties have no scientific reputation with us. The photograph is now on exhibition in this country, but the true facts (which were related to me by one of the assistants who knew the facts but did not appreciate the rascality) are not generally known." As we have had no means of judging for ourselves, or of making the necessary examinations, we do not accept the responsibility of our correspondent's statements, but merely quote them from his letter.

The death is announced of the President of the Royal Microscopical Society, the Rev. Joseph Bancroft Reade, M.A., F.R.S. The deceased was one of the founders of the society, also the author of many improvements connected with the microscope and microscopical research, and also a Fellow of the Royal Astronomical and Meteorological Societies.

HEAT.

M. G. Salet has described some peculiarities of the blue flame of sulphur and some of its compounds. This blue flame is not hotter than red-hot iron, and contains reduced sulphur; but, at the perisphere of the flame, very active oxidation takes place, and sulphuric acid is formed. By the same arrangement the author has shown that the zone of aqueous vapour which envelopes the flame of burning hydrogen, contains nitric acid and binoxide of hydrogen.

M. Toselli, the inventor of a very ingenious ice-making machine, states that experience has proved that ice made by his process withstands the influence of a high temperature better than natural ice, or than any other ice obtained by artificial means. To illustrate this fact, he adduces the instance that a block of ice weighing 20 kilos., made by the author's process in 18 minutes, and sent off from Paris to Algiers on the 30th of June last, arrived, properly packed, at its destination late in the afternoon of the 5th of July, the block of ice then remaining still weighing 10 kilos. The loss, by fusion, on a journey through a warm climate in the middle of summer was, therefore, only 84 grms. per hour. 100 kilos. of ice made by the author's process would require, under the same conditions of temperature, 49½ days to liquefy it; whereas

* A specimen in illustration was exhibited at the November meeting of the Royal Microscopical Society.

100 kilos. of the ice artificially made by another process has been found to melt in six days under the same conditions. The secret of this appears to reside in the high degree of compactness and freedom from fissures in the ice produced by M. Toselli's machine.

A short time ago, a fire broke out at the premises of MM. Behague and Paxer, wholesale silk mercers; the fire was, however, very quickly discovered, and this gave rise to the discovery that it originated inside a large parcel of black-dyed silk which had been returned from the dye-house only 24 hours previously. That black-dyed silk is somewhat liable to spontaneous combustion has been a well-known fact for years, but, notwithstanding the researches of Persoz and others on this subject, the real cause is not quite elucidated.

In some researches on "The Simultaneous Boiling of Two Liquids which are not Miscible," M. A. Kundt gives an account of a series of experiments made with water and benzol, water and oil of cloves, water and sulphide of carbon. The chief point of interest in his results is that two liquids, not miscible with each other when in contact, boil at a lower temperature than when the most volatile of these liquids is brought to ebullition by itself.

Mr. M. G. Farmer, of Boston, has fused the native iridosmine by placing the natural grains in a groove in charcoal, and subjecting them to the action of a current of voltaic electricity from sixty large Bunsen cells, using large platinum wires to make contact with the ends of the groove. He obtained in this manner bars of perfectly compact metal, brittle and very hard. The operation was anything but pleasant, on account of the intense light emitted and the fumes of osmic acid, which attacked the eyes and nostrils, producing the phenomena of rose or hay fever, and sunburning the face. Mr. Farmer estimates the temperature of the fusion at about 10,000° F. The object of the experiment was to prepare a bar of the alloy for the purpose of electric illumination. On rendering it luminous by an electric current, he found that when near the melting-point one square inch of surface evolved light equal to 2800 candles, which threw shadows in broad daylight at noon, and produced excellent photographs. The same battery converted solid bichloride of iridium into fused metal as soft and ductile as platinum.

Messrs. Mottershead and Co. have just brought out an improved automatic regulator, maintaining constant temperatures in laboratory operations, which is applicable to any operation in which gas is used as a source of heat. When fitted to copper drying closets or air-baths the temperature may be limited to any degree from 75° F. upwards. This apparatus acting automatically, the temperature is unaffected by varying pressure of gas, the common source of accident in ordinary drying closets. Drying closets and evaporating basins are also fitted on a slightly modified plan, in which a reservoir of air placed *inside* the oven is made to act on the regulator, thus avoiding the necessity of a double copper chamber, and at the same time forming a sufficiently sensitive apparatus for many practical purposes.

ELECTRICITY.

Dr. J. G. Fischer has described some curious effects caused by lightning striking his house, a detached residence situated near Hamburg; the lightning first struck and demolished a stack of chimneys, and next found its way to the soil along a zinc pipe for conveying the rain-water from the roof downwards; the pipe alluded to, previously sound, was perforated in three places in a very curious manner; at one of the holes the metal was forced outward, while at the two other holes the metal had been forced inward in such a manner as to close the tube for the passage of water, at the point where the tube reached at the bottom the earthenware drain-pipe; the latter was smashed, the soil which covered it having been scooped out; no fire ensued by the striking of the lightning, nor was fusion of metal anywhere perceptible.

The substitution of aluminium for platinum in Grove's batteries has been successfully tried by Mr. Nettleton. Two small cells, the size of the aluminium plates, being 4 inches by 1¼, decompose water very energetically. The metal aluminium possesses the advantage of costing (for equal surfaces) about one-tenth the price of platinum, being about 5s. per ounce, cut to size.

The subject of the electro-deposition of brass is one of some commercial importance; it has been recently studied with considerable success by Mr. W. H. Walenn. The history of electro-metallurgy, as set forth by Smee, in 1851, comprises only that part of the subject which relates to the electro-deposition of metals from their neutral or acid solutions. With some isolated exceptions, no alkaline solution is there treated of. Mr. Walenn uses potassic cyanide and neutral ammonium tartrate, when mixed with water, to form the solvent solution for brass. The quality of brass (yellow or red) depends upon the heat of the solution. Acid solutions, in general, give a spreading, or matted deposit; alkaline solutions, a bristling one. The contact of the coating is promoted by working the solution hot. The article should be pickled, scrubbed with sand, washed, scrubbed with a portion of the depositing solution, and then placed in the depositing trough; after deposition the article is washed, and dried in hot mahogany sawdust. Complete protection from rust, and a satisfactory coating for any purpose, is given by the use of an acid-depositing bath subsequent to that of the alkaline bath.

The self-recording instruments at the Kew Observatory, showed a large amount of disturbance of the magnetic declination and horizontal force during the progress of the Aurora of the 25th of October. Dr. Joule also observed the changes which took place in the magnetic dip, at Broughton, near Manchester. The most remarkable variation occurred during the interval from 6h. 15m. to 6h. 23m. G.M.T., when the dip increased from 69° 8′ to 70° 30′.

Apropos to a statement which appears to be authentic, that there has been in Versailles during the investment of Paris a telegraphist who has secretly rendered most important services to his country, by transmitting information from the head-quarters of the German army to the imprisoned authorities in Paris, the "Electric Telegraph Review" enters into some interesting speculations on the subject of secret telegraphy. It is considered by most persons impossible that telegraphic communication could be carried on between Versailles and the besieged capital without being discovered; for, they argue, even if we could not hit upon the line wire, we should obtain speedy information of the locality where it terminates, and where this ingenious individual manipulates his instruments. Is a secret telegraphic circuit, hidden from all ordinary means of search, and capable of being worked from *one* extremity without the exhibition of batteries, signalling instruments, or even a contact key—nay more, without any appearance of "line" and "earth" terminals, beyond the resources of modern telegraphic ingenuity, to say nothing of modern electrical science? Our contemporary thinks not. It appears certain that the subject of secret subterranean telegraphs is one which has from time to time received some attention in France; and it is one which, sooner or later, would probably well repay a little investigation on the part of our own Government. The data, even as at present understood, seems to be nearly, if not quite, sufficient to realise the object in view. *Employés* in the secret service would need to be trustworthy and thoroughly efficient telegraph clerks, trained to send signals rapidly and accurately without a "key"—or with merely a common-place latch-key, bringing into interrupted contact an innocent-looking bell-wire and a harmless gas pipe. They would require also to be thoroughly conversant with the art of converting the *tongue* into a delicate receiving instrument. The lines must be buried deeply, and excavations for other purposes, such as gas and water pipes, and sewers, should be utilised when possible. The choice of the locality for one terminal is of importance; an out-of-the-way bedroom in a public building, or in premises adjoining it, is generally to be preferred. The outward appearance of the terminal has been already hinted at; it may be a *real* bell wire,—the point where this is connected with an insulated wire, led below the foundations of the building, being of coure carefully concealed. "Earth" of course can be obtained wherever there is a gas or water pipe. Devices such as these are very simple; but it does not seem to occur to the mystified authorities at Versailles that, by means of them, telegraphic communications may be sent, and even received, without any of the paraphernalia of an ordinary telegraph office, provided some of the latter are at the *other extremity* of the line-wire.

If the French telegraphists have outwitted the Germans at Versailles, the latter have retaliated at Meudon, where a complete underground electric telegraph was discovered in one of the cellars. For days the unconscious French were sending messages, which were "tapped" by the Prussians to no great purpose, however, as they were generally in cypher.

Varley's Singing Telegraph.—Mr. C. F. Varley, C.E., of Fleetwood House, Beckenham, Kent, has just invented some telegraphic apparatus, whereby two, three, or more messages can be sent on one line wire at the same time, and without interfering with each other. This invention, therefore, is likely to prove of considerable value, since it will so largely increase the transmitting power of line wires at present overcrowded with work, and will save the cost of hanging additional wires between London, Liverpool, Manchester, and other large centres of industry. Another remarkable feature of the invention is, that the instruments sing, or rather, "hum" their messages. The Morse alphabet is used; a loud, long hum is given for a dash, a quieter and a shorter hum is given for a dot. When one of the instruments is at work it sounds as if a big bee were teaching a little bee to exercise its voice, because, as a general rule, the loud and softer sounds are given alternately. The sounds are caused by the vibrations of a thick iron wire, the instrument being something like a violin, five feet long, with one thick string. The first instruments, made for experimental purposes, worked well; the writer read off messages by them, which Mr. Varley sent from another part of the house, through a hundred miles of wire, of the electrical resistance of that ordinarily used on land for telegraphic purposes.

By the new apparatus, waves are superimposed upon the currents ordinarily used in working a Morse circuit, and the receiving instruments respond to the action of these small waves. Suppose a long rope, with a weight at each end, were passed near the ends over two pulleys; signals might be transmitted by pulling the rope at either end, and lifting the opposite weights up and down; if now, at the same time, vibrations or waves were set up in the rope by striking it with a stick, and an observer could read off messages sent by these small undulations, we should have an effect analogous to the new system of telegraphing.

Let A B, Fig. 20, be the line wire; Mr. Varley attaches a condenser, D, to it; the other side of the condenser is attached to the sending apparatus, which

Fig. 20.

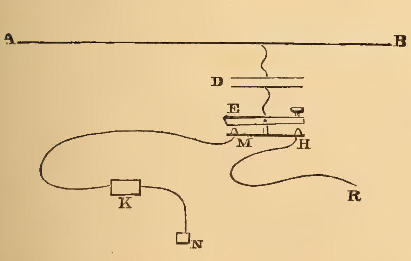
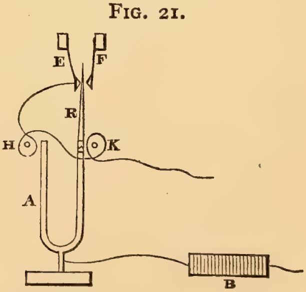


Fig. 21.

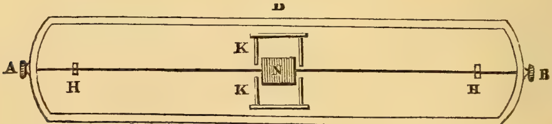


charges and discharges the condenser with great rapidity; consequently, these impulses add to and take from the line a number of small waves, without affecting the mean magnetising power of the ordinary Morse current. The diagram shows how the apparatus is connected for receiving messages. The "key" or commutator, E M H, when not in use for sending messages, keeps the condenser, D, in contact with the receiving instrument, K, through the metallic stud, M; the other side of the receiving instrument is connected with the earth at N. When the key is depressed, contact is broken at M, and the condenser, D, is placed in communication, through the stud, H, with the wave-generating apparatus beyond R. A convenient condenser to use is one of three microfarads.

The vibrations of the tuning-fork, A, Fig. 21, are employed to set up the waves, and the fork is caused to vibrate incessantly by power obtained from the battery, B, one pole of which battery is connected with the fork; the two light springs, E F, have small platinum studs at the end, and when the fork is at rest, the light piece of metal, R, attached to one of its legs, is in contact with the spring, E. A current then passes from E, round an electro-magnet, the poles of which are shown at H K. Instantly, of course, the prongs of the fork are pulled outwards by the attraction of the magnet, contact with E is broken, and with F established; the fork, however, at once springs back again, and thus continuous vibrations, at the rate of about 200 per second, are produced. The battery power used for generating the waves is ten-quart Daniell's cells. Some electro-magnetic apparatus is fixed between the tuning-fork and the condenser, and this apparatus sends the impulses into the condenser when the key for sending signals is depressed.

The principle of construction of the receiving instrument is shown in Fig. 22. A D B E is the iron frame of a sounding-board, which board is 4 feet 6 inches long between the bridges, H H. The thick iron wire, A B, is No. 14 gauge, and its tension is regulated by the two screw nuts shown at the ends of the iron frame. This wire passes through the hollow helix, N, which helix is 3 inches

FIG. 22.



long and $\frac{1}{8}$ inch internal diameter. This coil is attached to the iron frame, and does not touch the sounding-board. Two horse-shoe magnets, K K, are placed one on each side of the wire. One end of the coil, N, is connected with the condenser while a message is being received, and the other is connected with the earth. The rapid currents from the condenser magnetise the wire within the helix, so that it is attracted or repelled by one of the two magnets, K K. By this means rapid vibrations are set up in the wire after the wire has been "tuned," by turning the screw A or B, so that it has a tendency to vibrate synchronously with the pulsations of the fork at the sending station. When it is properly tuned, very feeble currents will give a distinctly audible sound.

There are various ways of augmenting the sound if desired, by means of vibrating tongues, strained diaphragms, the use of the stethescope, and other appliances; the signals can also be made visible by optical apparatus, or be made sensible to the touch. In the experiments above mentioned the sound was full and clear, without such additional aids. There are also various ways of originating waves in the sending portions of the instrument.

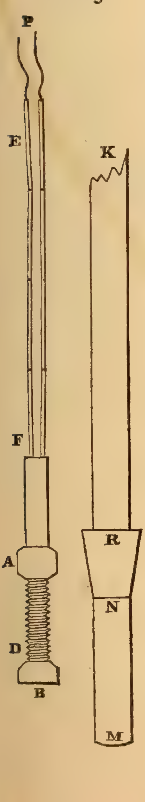
By this apparatus, while messages are being transmitted from London to Brighton by the ordinary Morse apparatus, through one line wire, the singing instruments may be placed at half a dozen intermediate stations on the same wire, and each station may communicate with its next neighbour, without interfering with the main line work. The singing instruments on one length of the wire are prevented from interfering with those on the next length by the interposition of an electro-magnet, for an electro-magnet offers at the first moment considerable opposition to the passage of a current of electricity, so may be considered to be opaque to rapid waves. By the arrangement just described, half a dozen or more messages may be traversing the same wire at the same time. Or, if there be no intermediate stations, two messages at the same time may be traversing the wire from end to end, and even here the limiting power of transmission of the wire is not reached. Two sets of wave-signals may pass through the wire independently of each other, if a sufficient difference be made in the periods of the two sets of waves; and Mr. Frederick Varley, who made much of the apparatus, informs the writer that he has very readily sent three messages from end to end of the same length of wire at the same time.

Where the distance between any two singing instruments exceeds 100 or 150 miles, the difficulties in signalling with them are increased; transmitting intermediate stations may therefore be necessary for very long distances. The instruments have also to be tried on ordinary telegraphic lines, to find out their real practical value. Whether the commercial value of Mr. Varley's plan of telegraphing be very great or very small, it is certainly one of the cleverest scientific inventions of the present age.

Siemens's Electrical Pyrometer.—Mr. C. W. Siemens, C.E., F.R.S., of Great George Street, Westminster, has recently invented an ingenious pyrometer, the principle of which is, that as the electrical conductivity of platinum, iron, and other metals decreases as they rise in temperature, their increase of resistance to the passage of the current is a measure of the heat to which the metals are subjected.

The principle of construction may be explained by the aid of Fig. 23, in which F A B is a tube of pipe-clay, and the length between the projections, A and B, has a screw-shaped spiral groove cut on its outer surface; the length of this part of the tube is about 3 inches. A spiral of fine platinum wire lies in the groove, each turn of the platinum spiral being thus protected from lying in contact with its neighbour by the projecting edges of the groove, by which plan of insulation the current is forced to pass through the whole length of the fine wire. D is a little platinum clam, connected with one pole of the battery, and the position of this clam on the spiral regulates the length of platinum wire through which the current shall pass. By this plan of adjustment, all the pyrometers constructed by Mr. Siemens are made to agree with each other.

FIG. 23.



At F, the ends of the thin platinum wire are connected with very thick platinum wire, and higher up, near E, where the heat of the furnace is less felt, the thick platinum wires are connected with thick copper wires, shown at P; from E to F, these connecting wires are protected by clay pipes, as shown in the cut.

When this arrangement has to be used, the whole of it is dropped into a thick metal pipe made of iron, copper, or platinum, according to the heat of the furnace to be tested. The lower end of this outer pipe is shown at K M, and when it is used, the spiral A B, lies inside it at N M. At R there is a very thick collar of metal in which the heat accumulates, and this prevents the cooling action of the length K R (most of which does not enter the furnace), from interfering with the accuracy of the indications. The ends of the wires, P, are connected with suitable and very delicate electrical apparatus, by which the increasing electrical resistance of the hot spiral is measured.

To measure low temperatures on the same principle, several miles of insulated iron wire are enclosed in a tube containing dry air, and hermetically sealed. This instrument is very useful to measure underground temperatures, as the indicating part of the apparatus may be far away from the thermometric portion. Dr. Carpenter used this apparatus, with some modifications, in ascertaining deep sea temperatures.

Thompson's Syphon Recorder.—Among the inventions of Sir William Thompson, is one, which has recently come into use for registering feeble indications, made by weak currents of electricity, such currents, in fact, as are used in working Atlantic cables.

The principle of the invention may be explained by the aid of Figs. 24, 25, and 26. In Fig. 24, N S are the poles of a powerful permanent magnet, capable

FIG. 24.

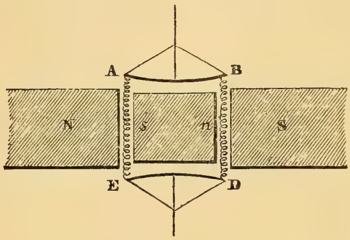
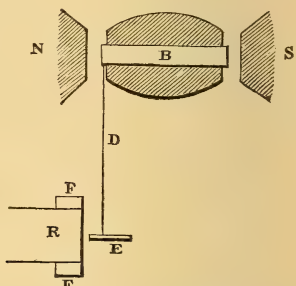


FIG. 25.



of lifting several pounds. To still further increase the power of the magnetic field Mr. Varley's plan of placing a piece of soft iron, *s n*, between the poles without touching them is adopted. A B D E is a very light coil of a small number of turns of fine wire, through which the feeble current from the line-wire passes. This coil is kept stiff by being stretched over pieces of any light stiff material, like the boom in the rigging of a ship. By this arrangement, whenever the current from the line passes through the coil, the coil will move in or out according to the direction of the current.

FIG. 26.

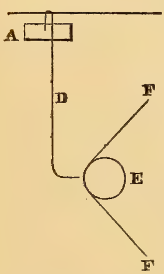


Fig. 25 is a diagram of the same arrangement viewed from above, N S being the poles of the magnet, and B the coil. A silk thread, D, connects one side of the coil with one of the bends of the syphon, E, so that as the coil, B, moves to and fro, the leg of the syphon, E, moves to and fro near the surface of the slip of paper, R, which is drawn by clockwork over the brass roller, F F. This roller is insulated with vulcanite supports, and is connected with an electrical machine, in consequence of which the liquid or ink in the capillary syphon made of glass spirts out against the paper, and thus the motions of the syphon and coil are recorded. Fig. 26 is another view of the same apparatus, A being the vessel containing the ink, D the syphon, E the brass roller, and F F the paper.

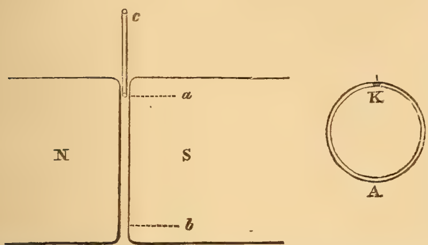
New Experiments in Diamagnetism.—The following experiments were made with a powerful electro-magnet belonging to Lord Lindsay; the iron horse-shoe is four inches in diameter, and the bent bar is seven feet long. The two helices are of copper wire nearly a quarter of an inch in thickness, and a very powerful Grove's battery is employed to excite the magnet. The experiments hereinafter described were devised by Mr. C. F. Varley.

Some rings were prepared, consisting of pieces of bent copper wire, soldered together at the ends; the wire was $\frac{1}{16}$ inch in diameter, and the ring $1\frac{1}{4}$ inch. When the ring, A, Fig. 27, was inserted between the movable poles, N S, of the electro-magnet, each pole 2 inches square, the ring fell gradually and slowly until the upper part of the ring, *c*, was level with *a*, or about $\frac{1}{10}$ of an inch below the upper surfaces of the two poles. It then fell rapidly until the lower portion of the ring arrived at *b*, when it commenced to fall slowly once more, and soon afterwards it passed out of the magnetic field, when it fell at the ordinary rate.

While the ring was going into the magnetic field it experienced great resistance to its fall; the moment it passed into a sensibly uniform magnetic field, it fell unimpeded; on attempting to get out of the magnetic field it experienced the same resistance as in the first part of its fall. The ring was seven seconds in falling through the two inches of magnetism.

But on breaking the soldered junction, K, of the ring, A, and thus preventing the formation of electric currents therein, the ring, to all appearance, fell between the poles as rapidly as if there had been no magnetism there at all. Therefore, electricity, which has no weight, actually helped to keep the heavy metal copper in suspension in the air, notwithstanding the attraction of gravitation. Mr. Varley concludes from this experiment that "a current of electricity is as solid and material to a magnetic field as is a plate of iron to a bar of copper."

FIG. 27.



The following is another curious experiment made with Lord Lindsay's large magnet. A horizontal piece of vulcanite, *v*, was placed between the square poles, N S, Fig. 28. An end view of one of the poles, with the vulcanite, *v*, is shown in Fig. 29. When one of the copper rings was placed rather more than half way in between the

FIG. 28.

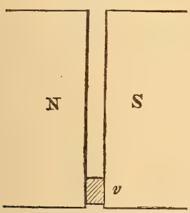
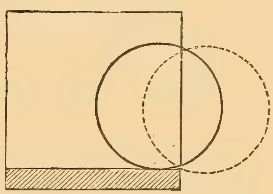


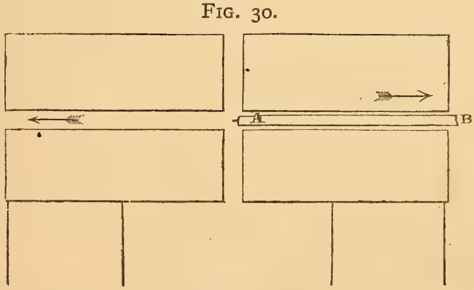
FIG. 29.



poles, as in the cut, the magnetisation of the magnet drew the ring inwards. When the ring was a little less than half way in, as shown by the dotted lines, the magnetisation of the magnet repelled the ring. Although the movement of the ring was not more than $\frac{3}{8}$ inch, yet there was power enough to roll it up an incline of thirty degrees.

Another experiment made by Mr. Varley is shown in Fig 30. Two hollow

FIG. 30.



poles were placed upon the magnet; when the magnet was not magnetised, and a burning taper, A B, was thrust in until the burning wick was opposite the opening between the poles, sufficient air did not pass between them to support combustion, and the flame was extinguished after burning a few seconds. On the other hand, when the poles were magnetised, and the taper was thrust in as before, the burnt air was driven out in the direction shown by the arrows, and the flame continued to burn. On demagnetising the magnet the flame went out, and on immediately magnetising again, the wick, which had become nearly black, began to glow again, and clouds of smoke poured out at each end of the hollow poles in the directions shown by the arrows in the cut. This smoke was of course driven out by the diamagnetism of the heated vapour. On one occasion the heat produced in the wick was sufficient to re-light the taper.

Some, but not all, of the experiments herein described were shown a few months ago by Mr. Varley and Lord Lindsay, to the Prince of Wales, at a *soirée* held at the house of Mr. John Pender, 18, Arlington Street, London.

The Deposition of Iron on the Poles of a Magnet.—At the last meeting of the British Association, a paper was read by Mr. Frederick Varley, F.R.A.S., on "The Deposition of Iron on the Poles of a Magnet," which paper, it will be remembered, attracted considerable attention. He has favoured us with the following details of his plan of coating the magnet; these details may be of interest, as it is difficult to get a satisfactory deposit, notwithstanding the discoveries of Jacobi in this direction.

The electro-deposit of iron was thrown down on the poles of a permanent horse-shoe magnet, composed of several bent bar-magnets. Each of the poles was $\frac{7}{8}$ of an inch square. This magnet was made the electrode of a battery, and the poles were immersed about $\frac{3}{4}$ of an inch in the solution hereinafter described. A Daniell's battery of one cell with large plates, and having a resistance of 0·33 Ohms was used. The plating solution consisted of equal volumes of saturated solutions of sulphate of iron and sulphate of magnesia mixed together, after which sufficient water was added to make the sp. gr. of the liquid 1400. The plating was effected in a large cell holding about 3 pints of solution, and from the size of the cell the resistance was practically equal in all directions. The cathode consisted of two small pieces of iron, placed at opposite sides of the cell, the magnet being over the centre; about 1 inch of iron surface was exposed on each side of the cell opposite to the poles of the magnet. The magnet was allowed three months in the solution to receive its electro-deposit of iron, and during that period the mean temperature was 60°.

At the end of this time it was found that the iron instead of being uniformly deposited on the surfaces of the poles, as in the case of ordinary electrotyping, was deposited chiefly on the sharp edges of the poles. A bridge of iron was not made from pole to pole as might have been expected, or as would have been the case had iron-filings been scattered over them, but, on the contrary, the growth was equal on all sides of the square poles. The greatest deposit was at the corners of each of the poles.

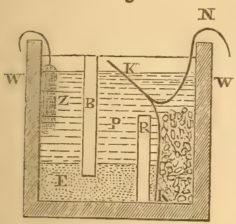
This experiment opens the way to a new field of research. For instance, experiments may be made on the deposition of iron inside helices, and the plan shows how magnetic rays may be stereotyped. It also suggests that experiments should be made with thallium, bismuth, and other diamagnetic metals which are easily electrotyped; the cells in which the work is carried on should be placed between the poles of powerful electro-magnets. Under these conditions, it is reasonable to suppose that the form of the deposit will be modified by the action of the diamagnetism.

An Improved Daniell's Battery.—The Daniell's battery now commonly used for telegraphic purposes has held its ground so long against all competitors, that at first sight there would seem to be little scope for inventive power applied to its improvement, yet a considerable improvement has just been made by Mr. Octavius Varley, and it is patented by Messrs. O. and F. Varley, of 11, Poultry, London, E.C.

The following are the defects of the ordinary Daniell's battery:—The sulphate of copper solution diffuses through the porous diaphragm, so that it reaches the zinc plate and covers it with copper; thus the surface of the zinc which should be positive is made negative, the action of the battery is paralysed, and the zinc plate has to be scraped before the proper electrical action is set up once more. Owing to this defect the full potential of the battery is scarcely ever obtained. Moreover, there is a great waste of the copper salt, for whether the battery be in or out of use, the consumption of the sulphate of copper goes on very nearly the same. When one firm alone sends out £10,000 worth of sulphate of copper per month for telegraphic use at home or abroad, the value of any invention which will prevent waste of this salt in the batteries may very readily be judged.

The improved form of battery is shown in Fig. 31. The great object to be achieved is to stop the sulphate of copper, K, from reaching the zinc plate, Z, by diffusion. This is effected by placing the copper salt in a compartment, K, which is watertight at the sides and bottom of the partition R. Thus the strongest part of the sulphate of copper solution lies at the bottom of the compartment, K, and only the weakest part diffuses over the top of the partition R. The level of the solutions in the battery is represented by W W. Another watertight partition, B, is so fixed that the electrical current has to pass through the diaphragm of wet sawdust, E. This sawdust is well soaked in sulphate of zinc before it is placed in the cell, therefore, it remains at the bottom of the cell, and does not float on the surface of the liquid. In charging the battery the compartment Z is filled with weak sulphate of zinc, and the compartments P and K with water. The copper plate, K K, has a band of copper, N, attached to it; the electro-deposit of copper begins on the top of this plate, and very slowly spreads downwards.

FIG. 31.



When the battery is not in action, the weak solution of sulphate of copper diffuses into P, but when the battery begins working again, the copper is driven back, and the liquid in P becomes once more colourless. Although there are convection currents in the liquid, the fact that the cell is divided into three parts prevents these currents from carrying the sulphate of copper to the zinc plate.

The result of all this is, that the zinc plates are found to work away till they become as thin as paper, and drop off the connecting band. The battery will go on working for nine or fourteen months when employed in ordinary telegraphic purposes, and it requires no attention beyond charging afresh about once in three months. Some of these batteries have just been ordered by the Government for trial.

METEOROLOGY.

Meteorology was fairly well represented at the British Association. The Report of the Rainfall Committee contained a notice of some experiments carried out at Calne by Colonel Ward, with the object of determining the difference in the amount of rain collected at various heights above the ground. The results of these are not in accordance with former theories. For instance, we may take the comparison of the quantities collected on the ground, and at a height of 20 feet above it, from which it appears that the difference is nearly three times as great in winter as in summer, so that the mean annual correction is only applicable to the total yearly fall, separate coefficients being required for the several months.

A paper was read by Mr. Charles Chambers, of Colaba Observatory, Bombay, on the cause of the variations of rainfall just alluded to, which he thinks is to be found in electrical action. He supposes that the globules of water are polarised by induction from the ground, and that according as they coalesce drops are formed. This action would be most rapid close to the ground: it would also be more active over vegetation than over bare earth, and it would be stronger on hills than over plains, owing to the greater electrical tension on the summits.

Mr. F. Galton read a very ingenious paper on "Barometrical Predictions of Weather," based on a comparison of the continuous records from Falmouth, published in the "Quarterly Weather Report of the Meteorological Office." The result at which he arrived was, that if we use the ordinary rules given in the text-books for barometrical fluctuations, and the indications of weather they afford, "judging by the experience of 106 well-marked instances of change occurring at Falmouth during the first quarter of 1869, it is more unwise in the ratio of 10·0 to 7·7 to be guided by the barometer, than to say off-hand that the weather will continue as it has been."

The interpretation to be placed on this apparently paradoxical statement is clearly this. When we consult a barometer, we involuntary interpret its changes with reference to the present appearance of the sky, the direction and force of the wind, and ordinary local signs of weather. It is perfectly evident that the use of the barometer *by itself* could scarcely fail to mislead us.

Mr. Laughton read a *resumé* of his work on "Physical Geography," noticed in our last number. We did not then explain the agencies to which he attributes the cause of atmospherical circulation. His views are concisely stated in the following sentence:—

"All observation shows us that there is not a permanent current (of wind) towards the west, but that there is one towards the east; and although we are unable at present to master all the details of the manner of the motion, the evidence of geographical fact, combined with that of astronomical possibility, justifies us in inclining towards the belief that the motive force for which we are seeking is really the disturbing force of the attraction of the heavenly bodies."

It will be seen that the proofs he adduces for the existence of the celestial influence are not of a very convincing nature.

"The Journal of the Scottish Meteorological Society" contains a continuation of Mr. Buchan's discussion of the "Rainfall of Scotland." The district treated of is that of the basins of the Forth and Tay, up to the line of the Grampians. The average fall in the upper valleys of these rivers, in the West of Perthshire, amounts to about 90 inches per annum, being 20 inches or so more than that collected at Ettrick Pen, the wettest station in the south-west of Scotland. This result appears to be attributable to the fact that the mountains of Ulster drain the south-westerly winds which come to the south of Scotland, while the Perthshire hills receive the air from the open Atlantic.

The evidence against the dependence of the amount of rain upon mere height, and in favour of its relation to local conditions, is very clear. Thus, for the region about Loch Katrine, we find that "the absolutely largest average obtained is 91·90 inches at Glengyle, which is only 580 feet above the sea. At Loch Dhu, 325 feet high, the quantity is 87·62 inches; whereas, at the Head of Duchray Valley, 1800 feet high, the annual average is only 91·50 inches. Even more striking is a comparison of the rainfall at Leng with that on the west shoulder of Ben Ledi. At Leng, 345 feet in height, the annual quantity is 66·37, whereas on Ben Ledi, at a height of 1800 feet, the amount is only 58·43 inches."

Owing to the war, the foreign periodicals have not come in regularly. We have only received the "Austrian Journal" up to July 15. The papers of most interest in the latest numbers have been those on the climates of the Straits of Magellan and of Bear Island. The first has been extracted from the "Anales de la Universidad de Chile," and great credit is due to Dr. Hann for having rescued it from its comparative oblivion. The observations were taken at Punta Arenas, about the middle of the Straits, by Senor Jorje Schyte. The following table of mean temperatures for the station, compared with those for Dublin and Barnaul, in the corresponding latitude in the Northern Hemisphere, gives a fair idea of the climate:—

	Punta Arenas, 53°2′ S.	Dublin, 53°3′ N.	Barnaul, 53°3′ N.
Winter	35·42	41·36	0·14
Spring	43·52	46·22	32·72
Summer	50·54	56·84	64·04
Autumn	42·62	48·92	32·18
Year	42·98	48·38	32·36
Range	16·90	17·60	72·00

The climate is therefore distinctly "insular," but cooler than that of these islands. It is also slightly cooler than that of the Falkland Islands.

The rainfall is not so heavy as at most of the stations on the West Coast of Patagonia. In connection with this statement, Dr. Hann cites Darwin's remark that the eastern entrance of the Straits is barren and rainless, while at the other extremity the hills are well wooded and watered.

We are glad to say that this paper is only the precursor of others on the climate of South America which Dr. Hann promises to give us.

The notice of Bear Island is based upon observations made by a Norwegian, Captain Tobiesen, who spent the winter of 1865-6 on the island. The chief point to which we should draw attention is the extreme lateness of the winter; the coldest month is March, the climate being mainly regulated by the sea surface temperature, which reaches its lowest point in that month.

Professor Mohn, Director of the Meteorological Institute of Norway, has made a most valuable contribution to the science of weather in his Storm Atlas, which has just appeared. This contains an account of several storms felt in Norway in the years 1867-8. One of these, that of March 31, 1868, was chiefly felt at the Loffoden Islands, and was in a more northerly locality than any storm which has hitherto been studied.

Each period is copiously illustrated by charts. We have first a chart of the entire disturbance, with the paths of the several centres of depression which may have been noticed. For each day four charts are given: one large one, showing the observations of the barometer, wind, and weather, for 8 a.m.; a second smaller one gives the same information for 8 p.m. The other two charts show the variations of the barometer and thermometer respectively for the twenty-four hours succeeding the date of the large chart. Thermometrical readings are not given, except in the form of the variation chart just referred to.

On one chart, as a specimen, the tension of aqueous vapour is given. Professor Mohn prefers this mode of representation of moisture to charts of either humidity or the hygrometrical state of the air.

The charts show a number of barometrical maxima and minima, each surrounded by isobaric lines. The minima are usually indicative of storms. The form of the innermost curve is generally oval, its major axis lying—E. and W. seven times; N.W. and S.E., twice; N. and S., eleven; N.E. and S.W., five; and indeterminate, seven times.

In the cases in which the direction of advance of the minima was clearly marked, it was found to be, on the average, E9°S, and its mean rate 26 miles per hour.

All the storms travelled fastest over the ocean, slackened in speed when crossing Scandinavia, but increased again over Russia. In all cases the easterly component of this motion diminished regularly as the storm advanced from the sea into the interior of the continent.

The mean barometrical reading at the centre of the depression was 28·84 in. over the Arctic and Atlantic Oceans; 28·68 in. over Scandinavia; and 29·14 in. over Russia. The storms, therefore, increased in violence when passing over Norway, and died out gradually on reaching Russia.

The mean direction from which the wind blew during the storms was S.E. on nine occasions; S.W. on sixteen; N.W. on twenty-two; and N.E. on thirteen.

Aqueous vapour is always in greatest abundance on the southern side of the path of the centre of depression. It does not, however, travel onwards with the advance of the storm. There appear to be certain districts where moisture is constantly present in quantity. These are Portugal, the Mediterranean, South of Sicily, and the Atlantic, off the coast of Ireland.

The variation charts show some very interesting facts. Thus, the probable advance of the centre of depression on any day is shown by the next succeeding variation chart. However, on this entire question, which is quite novel, we must look to Professor Mohn to explain his views more thoroughly, for his present statement of them is not at all clear.

The second portion of the work is a theoretical account of the origin of storms in general, which, on the whole, agrees with that given by Mr. Buchan, as it assumes a spiral motion of the air in the storm; an ascending current at its centre, and a descending counter current outside. The admission is made that the relative influences of the respective causes of these motions cannot be exactly determined. The explanation given of the effect of aqueous vapour is, however, very good. The precipitation of this vapour as rain is acknowledged to be a very active agent in depressing the barometer, and, according to M. Mohn, the direction and rate of advance of the storm area is determined mainly by it. He illustrates this statement by the following instance:—

When a storm is advancing across the Atlantic its velocity is great, and its motion is towards the east, owing to the fact that the vapour is transferred over the sea surface so rapidly from the great reservoir of this element, lying to the south of the trajectory of the centre, that it sweeps round in the spiral to the front of the storm, and falls there with a S.E. wind.

When the storm reaches Norway the condensation is accelerated by the action of the coast, the barometer at the centre sinks lower, and the rain falls principally with S.W. winds, as mechanical obstacles prevent the free passage of the moist air to the point where it would blow as a S.E. wind. This change in the conditions causes the greatest barometrical variation to be very local, and to be situated to the south-east of the centre. The depression at the centre becomes more serious, the storm slackens in its rate of progress, and begins to move towards the south.

When the disturbance has crossed into Russia the supply of vapour is drawn from the Mediterranean. The air which brings it is partially dried in its passage over central Europe; so that by the time it arrives at the storm region we find that the rain falls with westerly winds, the barometer falls fastest to the south of the centre, and the easterly component of the advance of the depression is entirely cancelled. The rain is less in absolute quantity, so that the depression is not "fed" with moisture, and the whole storm gradually disappears.

Similar principles are applied to explain the phenomena of the storms of North America, and of tropical cyclones.

It is stated that since the opening of the present war in Europe that part of Germany in the vicinity of Frankfort has been almost constantly visited by rain and thunder storms, a most unprecedented thing at this season of the year. In the light of other recorded facts, the German press has almost unanimously attributed these unusual storms to the firing of cannon and small arms in Alsace and Lorraine. The attention of scientific men is, however, so well directed to observation of the meteorological events succeeding heavy cannonading that the question of the correlation of artillery discharges with rainfall will eventually be settled. The "Ungarische Lloyd," in an interesting article on this subject, says that the history of the wars of the last eighty years are full of accounts of the great meteorological changes which have followed violent engagements in war. In 1861 Lewis, in an article in Silliman's "American Journal," said:—"In October, 1825, I observed a very plentiful rain immediately after the cannonading which took place in celebrating the connecting of Lake Erie with the Hudson. I published my observations on this event in 1841, expressing the opinion that the firing of heavy guns produces rain in the neighbourhood. After the first battle in the last war between France, Sardinia, and Austria, there followed such important rains that even small rivers were impassable, and during the great battle of Solferino there broke out such a violent storm that the fighting was interrupted. In July, 1861, M'Clellan's troops on the Upper Potomac had four separate engagements on four days, and before the close of each violent rains fell. On the 21st of July Bull Run was fought in Virginia, and on the 22nd rain fell the whole day till late at night. Under the heading, 'Can we Produce Rain when and where we like?' the Cincinnati 'Woechtenliche Volksblatt' for the 10th of July, 1862, remarked:—'The cannonading (during the war) on the York River and James River, as well as the cannonading of Corinth and on the the Mississippi, were followed by such fearful storms that the land was inundated.' The Bohemian campaign of 1866 was accompanied during the whole course by violent rains."

PROGRESS IN CHEMISTRY,

(Including Chemical Science, Technology, and Mineralogy).

CHEMICAL SCIENCE.

To the list of earth-eating people the Javanese must be reckoned; and Professor C. W. C. Fuchs has given a full account of the edible earth in use by this people. One deposit possessing an intensely red colour, exists in the neighbourhood of Sura Baja, between strata referable to the time of the latest tertiary. This earth is formed into thin cakes, having a diameter of from 1 to $1\frac{1}{2}$ inches; it is then dried over an open fire, and in this condition is brought into the market. It is perfectly smooth to the touch, and is composed of materials in the finest state of subdivision. By a chemical analysis it is found not to contain the slightest trace of an organic substance. It is apparent that the earth consists of a clay rich in iron, in which is still retained small quantities yet undecomposed, of the minerals from which it derived its origin. Upon rubbing it, not the slightest grittiness is perceptible, and on being moistened with water it forms a smooth and unctuous mass. The enjoyment derived from eating it seems to reside in the similarity of the sensations it produces with those derived from the eating of fatty substances. In many parts of Würtemburg the quarrymen have the habit of eating the smooth, unctuous clay which collects in the fissures of the rocks. The term "Mondschmalz," which they apply to it, would seem to refer to the enjoyment they experience in the process of eating.

Dr. Hofmann has discovered a most delicate test for chloroform, based upon the fact that, when chloroform is mixed with aniline and an alcoholic solution of caustic soda, a very strong reaction takes place, and isonitrile is generated, which is readily recognised by its peculiarly characteristic smell; this reaction is so delicate that, when 1 part of chloroform is mixed with from 5000 to 6000 parts of alcohol, the first-named substance is readily detected.

Professor Seely has made the discovery that anhydrous liquid ammonia has a solvent power upon certain metals, and he has actually succeeded in obtaining a solution of sodium in liquid ammonia. This solution presents all the physical characteristics of a true solution. On evaporation, the sodium is gradually restored to the metallic state in the same continuous manner in which the solution has been affected. The colour of the solution is a very intense blue, of high tinctorial power.

A very simple and powerful method of resolving minerals in analysis has been devised by Dr. Schonn. A steel crucible is heated over a lamp; into this is projected a few pieces of metallic sodium, and afterwards the finely-divided and dry mineral is added. The crucible is then covered and heated to redness. As soon as the the reaction is finished the contents of the crucible are allowed to cool, and water is cautiously added, sufficient for the purpose of filtration. The fused mass is then thrown upon a filter and thoroughly washed. In the filtrate will be found the electro-negative constituents of the mineral combined with the sodium, such as sulphur, cyanogen, chlorine, chromic acid, silica, molybdic and tungstic acids, and such oxides as are soluble in soda-lye. On the filter will be found the metals and their oxides, also the lower oxides of titanium, molybdenum, tungsten, and possibly silica and alumina. The contents of the filter and the solution in the filtrate can be further treated according to the order of analysis. In this way all minerals can be readily resolved, and their constituents determined either qualitatively or quantitatively.

In a paper on organic matter in water, Mr. C. Heisch indicates a very simple test for the presence in water of organic matter arising from sewage contamination, which consists in adding ten grains of pure loaf sugar to six ounces of the suspected water, and setting the bottle aside for a few days in a warm place exposed to light, when, in the event of a sensible quantity of this unwholesome form of organic matter being present, the germs become developed with generation of small spherical cells and strings, which are easily discoverable under the microscope, and, in extreme cases, cause a general turbidity, with production of a butyric odour. Filtration through the finest quality of Swedish paper was found to be ineffectual in keeping back this kind of impurity; but treatment with animal charcoal and subsequent filtration proved much more efficacious in removing this source of contamination.

Recent events on the Continent have rendered all subjects connected with the economy of food or of counteracting the effects of insufficient food of the highest importance. A French experimenter, M. Rabuteau, suggests that a man may live for several months, and keep active, strong, and healthy, by consuming daily 150 grms. of a mixture (dry) of powdered cocoa, 1000 grms.; infusion of coffee, 500; infusion of tea, 200; sugar, 500 grms. The two infusions, as strong as can be made, to be evaporated to dryness previous to being mixed with the rest of the substances, The whole weight of this 2220 grms. would be only about 1600, and would be sufficient food for ten days; when taken it is to be mixed with some boiling water. The author states that it is highly agreeable, he having purposely experimented with this mixture upon himself while abstaining from other food.

Speaking of the great tinctorial power of some of the aniline dyes, Dr. Hofmann quotes the following:—The solution of a salt of rosaniline, mixed with a few drops of acetic acid, and so diluted with water as to have 1 part of the rosaniline salt to 1,000,000 of water (1 milligrm. to 1 litre of liquid) is deeply carmine-coloured, and yields a fluid capable of dyeing silk thread, previously moistened with dilute acetic acid. When the coloured liquid is diluted with water, so that 25,000,000 parts of that liquid are present (1-25th of a milligrm. of the salt to the litre), the liquid is yet distinctly coloured; and silk, immersed in this bath for a quarter of an hour, is dyed a pale rose-colour when removed from the liquid. Even 1 part of the rosaniline salt in 100,000,000 parts of water is visible, if the layer of liquid seen through is about half a metre thick.

Dr. Klein, a pupil and collaborator of Professor Jacobi, of St. Petersburg, states that the iron obtained by electrolysis is not, as has been often thought, the pure metal, but, on the contrary, a compound of iron and hydrogen, which, when heated to redness, gives off an enormous amount of that gas, and becomes, while greatly increasing in bulk, a silver-white, very soft, ductile, and malleable metal, which decomposes water readily below its boiling-point and oxidises most rapidly.

A lengthy memoir of a series of experiments on the venom of the *Scorpio occitanus*, has recently been published by Dr. Jousset. The author draws, from his experiments, the following conclusions:—The venom of the *Scorpio occitanus* acts directly and solely upon the red blood globules. This action consists in withdrawing from the globules their property of gliding over each other. By losing this property, the blood globules become glued together, and, by thus becoming an adhesive mass, obstruct the circulation of blood in the capillary portion of the vascular system, thereby causing a stasis which is altogether incompatible with the proper conditions of life. Since the action of the scorpion's venom is purely chemical, and a certain quantity of it (the venom) is required for exerting its action, it essentially differs from virus, which acts as a ferment.

Dr. W. Wenzell finds that a solution of 1 grain of permanganate of potash in 2000 grms. of sulphuric acid is, *par excellence*, the test for the successful demonstration of traces of strychnine. The limit of positive recognition by the bichromate of potash and sulphuric acid test may be placed at 1-100,000th, that of the chromic acid test at 1-600,000th, and that of the permanganate at 1-900,000th. The discovery of this use of permanganate is due to Dr. Guy, of London.

TECHNOLOGY.

To separate or distinguish the fibres of silk, wool, cotton, jute, &c., when interwoven or composing the structure of mixed fabrics, Mr. Spiller employs concentrated hydrochloric acid, which has the property of dissolving silk immediately and completely, without appreciably affecting any woollen or lignine fibres (cotton, linen, jute, &c.) with which the silk may have been interwoven. Having submitted the material to the action of hydrochloric acid and noted any indications of rupture or solution of the fibres, the residual fabric or loose filaments may be washed and collected, and will usually be destitute of colour. The presence or absence of wool in that portion of the fabric which resists the attack of hydrochloric acid is then determined. A warm aqueous solution of picric acid instantly imparts a full yellow tint to wool, but does not in the least affect cotton, linen, jute, or China grass, so that it is only necessary to immerse the fabric or fibres in the dye, wring out, and wash well with water in order to remove the adhering yellow solution, and note any indications of the existence of wool. In the examination of ribbons and some other stiffened goods it is often necessary to immerse them for a few minutes in boiling water, to dissolve out the starch or size prior to applying the hydrochloric acid test, for, by this simple expedient, the results are rendered much more decisive.

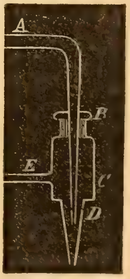
A new sulphur deposit has been discovered in the Island of Saba, a Netherlands' possession; it was accidentally discovered by an enterprising New Yorker, who visited the island in search of health. With the aid of some of the natives, he succeeded in quarrying about two sloop-loads of the crude mineral, which, on being brought to New York, was found to yield, on an average, rather over 60 per cent of sulphur, while the Sicily mineral only yields about 30 per cent. The deposit is, considering the small size of the island, very extensive, and will be of great value for the manufacturing interests of the United States, being distant only about 1500 miles from New York.

When glue, in thick solution, is mixed with tungstate of soda, and hydrochloric acid is added, there is thrown down a compound of tungstic acid and glue, which, at from 30° to 40° C. is so elastic as to admit of being drawn out into very thin sheets. On cooling, the mass becomes solid and brittle; but, on being heated, it becomes again soft and plastic. This material has been successfully employed, instead of albumen, in calico-printing, in order to fix the aniline colours upon cotton.

M. Mène has effected some considerable improvements in the treatment of wood for paper manufacture. The wood, previously reduced to the state of shavings or sawdust, is placed for a time in water, and is left there to rest. The rotting in water has the effect of disintegrating and partly decomposing the nitrogenous matter of the wood, and the fibre is also afterwards more readily bleached, not becoming yellow by the use of chlorine, as is the case where these matters have been left in the wood. The rotted wood is thoroughly washed with boiling water and steamed, and next treated with an alkali.

Experiments with gun-cotton have recently been made at Chatham on an unprecedented scale of magnitude, and in connection with a system of electric-torpedo defence. Charges of 432 lbs. weight have, upon two separate occasions, been fired by Messrs. Abel and Brown, with results which are stated in the official reports to be of the most decisive character, and such as would undoubtedly have sent to the bottom the largest iron-clad vessel in the world, had she been within range.

FIG. 32.



There are many locations in which the Bunsen filter pump cannot be employed on account of a deficiency in the water supply. For such cases a plan has been devised by Dr. Walz, of New York, which has been proved in practice to be efficient. The accompanying cut shows the outline section of the most important part of the apparatus. A is a tube supplied with steam from a flask or boiler; E is connected with the exhaust of the filter. By the action of the steam jet identical with that known as the "exhaust" in a locomotive, a vacuum is produced in B C D. By sliding the tube, A, back and forward in the cork, B, an adjustment can be given to the outlet within D, so as to secure the best effect. The conditions of best effect here are identical with those in the inner nozzle of the Giffard injector when it is starting its water supply from a lower level, and no doubt the proportions found most efficient in that instrument will prove also in this.

According to Dr. Wiederhold, genuine Chinese lacquer-work is done over tin-foil, and consists of a mixture of 2 parts of copal and 1 of shellac, melted together. When fluid, there are added 2 parts of boiled linseed oil; and, after the vessel containing this mixture has been taken from the fire, there are gradually added 10 parts of oil of turpentine. If colour is required, gummigutta, dissolved in oil of turpentine, yields yellow, and dragon's blood, dissolved in the same liquid, yields red.

A plastic material of great resistance, suitable for a variety of uses, is prepared by M. Rost, as follows:—He mixes litharge and glycerine, so that they may form a creamy liquid. The mixture becomes, in a short time, a hard, homogeneous mass, which readily adheres to metals, resists the action of water and steam, and a temperature of 275° C. In many instances, this paste is preferable to red-lead cement; and this glycerine-litharge paste may be even used, when in very fluid state, for galvano-plastic copying, since the material preserves even fine engraved lines.

Dr. Böttger prepares a glue which stands moisture without softening, by dissolving in about 8 fluid ounces of strong methylated spirit ½ an ounce of sandarac and mastic, and next adding ½ an ounce of turpentine. This solution is then added to a hot, thick solution of glue to which isinglass has been added, and is next filtered, while hot, through cloth or a good sieve.

MINERALOGY.

It must be confessed that when the discovery of diamonds in South Africa was first announced in this country, some three or four years ago, there was a tendency among many men of science to indulge in a little incredulity as to the genuineness of the reputed discovery. But, whatever may have been the doubts which were conscientiously entertained at that time, they have assuredly been long since dispelled by the glowing accounts which have since reached us—substantiated as those accounts have been by the arrival of larger consignments of the veritable gems, especially by the Cape Mails during the past quarter. Whilst the majority of these stones are, as might be expected, of only small size, there are, nevertheless, an unusually large number of them which exceed the average weight. One South-African diamond weighs 56 carats, and a second stone—now in the hands of Messrs. Hunt and Roskell* —reaches 83 carats, while a correspondent of "The Times"† has recently alluded to a fine octahedron of not less than 107 carats. Of course it is not to be expected that all the stones are of fine quality, and, indeed, it appears that a large proportion of the Cape diamonds are very defective. As to the conditions under which the gems are found in these new districts—a subject of the first importance to all who purpose adventuring forth to the diamond-fields —it appears that they are commonly found, not in the beds of rivers as is elsewhere generally the case, but rather on the summits or on the slopes of the little *kopjies* or hillocks, where they are sparsely disseminated amid detrital accumulations, which consist of water-worn pebbles of quartz, agate, cornelian, jasper, iron-ore, itacolumite, and basalt; while in many places they occur in a ferruginous gravel associated with a conglomeratic rock. We understand that many practical men at the diggings believe that the distribution of the diamonds bears some relation to the occurrence of certain trap rocks in the district, and even suspect that in or near such rocks they may find the original matrix of the gem. Without admitting for a moment that the enigma of the genesis of the diamond is yet completely solved, it may be well to

* Journ. of the Soc. of Arts, Nov. 25, 1870.
† Nov. 11, 1870.

remember that we are in possession of a large body of evidence tending to show that the diamond is probably of vegetable origin, or has at least been formed by the wet way, and that, therefore, its occurrence is not to be expected in an eruptive rock, although the presence in such a rock may have influenced its development. It may not be amiss to direct the attention of diamond-seekers at the Cape to the probable occurrence of the substances known as *bort* and *carbonado* in association with the true diamonds. These minerals are, from their unattractive appearance, likely to be overlooked by those who are absorbed in looking for the more precious gem, and yet they possess, in consequence of their extreme hardness, a very high value for polishing purposes. The black metallic-looking carbonado accompanies the diamond in Brazil, where it was entirely overlooked until a comparatively recent date, and we believe that it has already been found in South Africa. Whatever may be the future prospects of the South-African diamonds-fields, it is certain that we must henceforth register in our mineralogical text-books such parts of the valley of the Vaal as Pniel and Hebron as important diamond-bearing localities.

In New South Wales the search for diamonds has also been prosecuted with some measure of success, and according to Mr. John Hunt,* who has acted as Manager of the Australian Diamond Mining Company's Works, there have hitherto been found in the colony about 5500 diamonds, averaging one grain each. He believes that an excellent clue to the presence of diamonds, there and elsewhere, is afforded by the presence of a certain green mineral containing phosphate of iron. The diamonds of New South Wales are found, accompanying gold, in the alluvial drifts, and especially in the neighbourhood of basaltic rocks.

Some time ago we hinted that diamonds had been reported from Bohemia. The report was circulated in many of the German and Bohemian journals, but according to Professor Zepharovich,† it rests upon a very insecure basis. Indeed, it appears that only a single stone has been found, and this under conditions far from satisfactory. As is well known, the pyropes, or "garnets," found in the sands of some of the Bohemian rivers are extensively cut and polished for the purposes of the jeweller; and it was among a number of these stones at the polishing-works of Dlaschkowitz that the diamond in question was found. That a diamond has been discovered among Bohemian garnets is, therefore, certainly a fact; but that it was derived from the same deposit with the garnets is, at present, merely an assumption. When we remember that diamonds are in daily use in the workshop for the purpose of boring the garnets, a suspicion arises that after all the so-called "Bohemian diamond" may be merely a stray stone from the lapidary's stock. At least it behoves us to be silent upon the subject until we shall hear that diamonds have actually been found in the garnet-bearing sands.

More than twenty years ago the Italian mineralogist, Professor Scacchi, described a new arsenical sulphide from the solfatara in the Phlegrœan fields, where it occurs in the form of minute orthorhombic crystals difficult of measurement, but still sufficiently perfect to exhibit, in different specimens, two distinct sets of axial relations, whence the mineral received the name of *Dimorphine.* Dr. Kenngott has recently made some observations on the crystalline forms of this so-called species,‡ and finds that not only are the two types of form closely related to each other, but that they stand extremely near to the crystalline forms of orpiment. Dimorphine appears, therefore, to be crystallographically nothing more than orpiment, but while orpiment contains As_2S_3, it appeared from Scacchi's imperfect examination that dimorphine had the composition of As_4S_3. Nevertheless, the specific gravity and the colour of the two minerals are almost identical, and, on the whole, Kenngott is disposed to lay aside Scacchi's formula, and to believe that dimorphine must be united with the species orpiment.

* Mining Journal, Nov. 12, 1870.
† Poggendorff's Annalen, 1870, No. 8.
‡ Leonhard and Bronn's Neues Jarbuch für Mineralogie, 1870, Heft 5, p. 537.

Some simple experiments of considerable interest to the mineralogist have been made by Herr Credner, in Kolbe's laboratory at Liepzig,* with the view of determining what influence is exerted upon the crystalline form of carbonate of lime by the addition of certain substances to the solution from which it crystallises. It is generally believed that *aragonite* is deposited at high and *calcite* at low temperatures, but Credner finds that aragonite may be obtained from a cold solution of bicarbonate of lime, if a small quantity of bicarbonate of strontia be present; and that on adding very slowly the strontium bicarbonate to the lime solution, the rhombohedra of calcite which are deposited are accompanied by short acicular crystals of aragonite. The dimorphism of calcic carbonate is seen, therefore, to depend not only on temperature, but also on the presence of foreign substances in the mother-liquor. Again, while calc-spar crystallises in rhombohedra from a pure solution, these simple forms are much modified by the presence of a small proportion of nitrate or carbonate of lead, or of silicate of potash or of soda.

A specimen of obsidian, or volcanic glass, from Hecla, in the mineralogical collection of Zurich has recently been made the subject of careful microscopic examination by Professor Kenngott.† These observations may be said to supplement those of Zirkel on a like subject. Kenngott's specimen exhibits a brown vitreous base, in which may be detected a number of dark-coloured concretions, each surrounded by a pale ring formed of yellow acicular crystals arranged in a radiate form, whilst certain other smaller concretions are surrounded by fine black hair-like bodies. A few minute crystals of *belonite* were observable, and throughout the whole base were abundantly strewn certain small black particles, which are probably crystals of magnesian mica. Two parallel layers run across the specimen, and exhibit oblique striation; the striæ may be resolved by a high power into a system of markings, the ultimate lines being merely rows of points which are really small crystals.

The joint work of Professor Maskelyne and Dr. Flight contains much that is valuable, both chemically and crystallographically; but as it has been submitted to the Chemical Society, we need do no more in this place than call the attention of the crystallographer to the interesting example of hemimorphism—not hemihedrism—presented by the crystals of cronstedtite which have recently been found in Cornwall.

A number of newly-described species and varieties, as usual, call for attention, but must be noticed as briefly as possible. *Rionite* is the name of a bismuthic variety of fahl-ore from near Cremenz, in the Einfischthal, in Switzerland, where it is worked for the sake of the bismuth which it contains and which amounts to upwards of 13 per cent.‡ A manganese ore containing lithium was discovered some time ago by Herr Frenzel, and has been lately described under the name of *Lithiophorite*.|| It occurs in some of the iron-lodes of the Erzgebirge, in Saxony, and is merely a product of alteration, derived probably from psilomelane. Professor C. U. Shepard calls attention to some new minerals from the guano deposits in the Guanape Islands.§ One of these, termed *Guanapite*, contains sulphate of potash with sulphate and oxalate of ammonia; whilst a second species, named *Guanoxalite*, contains sulphate of potash, oxalate of ammonia, and water. *Ambrosite* is Shepard's name for a resin resembling amber, and occurring in the phosphatic formation near Charlestown, South Carolina. In compliment to the Tyrolese geologist, Herr Trinker, a new mineral resin, allied to Professor Church's Tasmanite, has been described by Tschermak under the name of *Trinkerite*.¶ It occurs in red or brown masses in the lignite of the fresh-water beds near Carpano in Istria, and has the following percentage composition:—Carbon, 81·1; hydrogen, 11·2; sulphur, 4·7; and oxygen, 3. *Uranotile* is Boricky's name for a new mineral

* Leonhard's Jahrbuch, 1870, p. 603.
† *Ibid.*, 1870, p. 529.
‡ *Ibid.*, 1870, p. 590.
|| Journ. f. Prakt. Chemie, 1870; Bd. 2 (N.S.), p. 203.
§ "The Rural Californian," Silliman's Journal, Sept., 1870, p. 273.
¶ Jahrb. d. Geol. Reichsanstalt, xx., p. 279.

from Wölsendorf, in Bavaria,* where it occurs in the form of yellow acicular crystals in druses of quartz encrusting fluor-spar. The crystals, which belong to the rhombic system, contain—Silica, 13·781; phosphoric acid, 0·448; peroxide of uranium, 16·752; alumina and ferric oxide, 0·511; lime, 5·273; and water, 12·666.

Excellent information on the mines and minerals of Elba—an island to which we are indebted for those wonderful specimens of specular iron-ore, iron-pyrites, and lievrite, which grace the cabinet of every mineralogist,—will be found in Von Rath's recently-published mineralogico-geological essay on that island.†

Gustav Rose has shown‡ that in iron-pyrites and cobaltine a close relation exists between the hemihedral forms of the crystal and their thermo-electric properties. The same author announces the discovery of zircon in the hypersthenite of the Radauthal in the Hartz.|| Dr. Scharff has published a crystallographic paper in which he seeks to determine what influence twin-growth exerts upon crystals of calcite.§

So full of interest are the volcanic rocks which occur in the neighbourhood of the Laacher See, in the Eifel, that they have often courted investigation by the mineralogist and geologist. We need not, therefore, do more than call attention to a series of papers in course of publication by Herr Dressel, whose long residence at the Abbey of Laach has given him more than usually good means of observation. His first contribution deals with the trachyte of Laach.¶

The Townshend Jewels.—Much scientific as well as artistic interest centres about many of the minerals which have been used for purposes of ornament. The study of the intruding substances and the cavities often occurring in quartz, ruby, and topaz, has led to the discovery of many singular facts, and has partially lifted the veil which shrouds from curious eyes the origin of some of the most beautiful products of the earth. So also the crystalline forms and optical properties of precious stones, together with the action of heat and other forces upon them, have afforded ample and profitable materials of research for the mathematician and the physicist. Thus it would be easy to vindicate the claims on scientific consideration which jewels possess. Were we to travel for a moment out of the path which we intend to follow in the present notice, and to speak of the artistic qualities of precious stones, we should have a rather difficult task to accomplish, for strange to say, it has become usual, amongst a certain clique of artists and connoisseurs, to depreciate their beauty. Professor Ruskin,** for example, talks of the colours of gems as "entirely common and vulgar;" he calls the green of the emerald as "vulgar as house-painting," and states (what is absolutely incorrect) that "no diamond shows colour so pure as a dewdrop." "The ruby," he adds, "is like the pink of an ill-dyed and half-washed-out print compared to the dianthus." Other writers on art, ignorant of those wondrous properties of jewels which are developed only by judicious cutting, would not allow a specimen to be facetted, but simply rounded and polished in the way known as *en cabochon*. Our present purpose, however, is not to offer a logical justification of the fondness for precious stones, which most people exhibit to some degree, but to direct the attention of our readers to the superb suite of specimens which has been recently bequeathed to the nation by the late Rev. C. H. Townshend. The collection is to be seen in the South Kensington Museum, and is, or was recently, in one of the picture galleries. A catalogue†† of the specimens has been published under the auspices of the Science and Art Department, but,

* Leonhard's Jahrbuch, 1870, p. 780.
† Zeitsch. d. Deutsch. Geolog. Gessell, xxii., 1870, p. 591.
‡ Monatsbr. d. k. Akad. d. Wissensch., Berlin, 1870.
|| Zeitsch. d. Deutsch. Geolog. Gessell, xxii., 1870, p. 754.
§ Leonhard's Jahrbuch, 1870, p. 542.
¶ *Ibid.*, p. 559.
** Ruskin's Lectures on Art, pp. 176 and 177. (1870).
†† Catalogue of Gems and Precious Stones [Townshend Bequest], by J. Tennant. (1870).

unfortunately, it is in many points an inadequate production: for ease of reference it will, however, be expedient to follow the order in which the specimens are therein described.

A fine large crystal of diamond (No. 1172) affords an example of the peculiarly brilliant lustre which distinguishes even the natural surfaces of this stone, and permits any one who appreciates its character to single out a rough diamond from all other stones. The specific gravity of the diamond (3·53) does not enable us to distinguish it from glass or white topaz with certainty, though it separates it from white quartz, which is only 2·65, and white beryl, which is only 2·71. There is a black diamond (No. 1173) in the collection, which displays on its facets the characteristic lustre of the stone. A colourless diamond of pure water, and about $\frac{5}{12}$ of an inch across, is a splendid brilliant from the Hope collection. A lustrous yellow diamond of the same size should also be noticed. Of other coloured diamonds there are two green specimens, one of a pale blue hue, one of a curious and very rare puce tint.

It will not be necessary to dwell upon the peculiarities of any of the many forms of silica represented in the collection. Almost all the ordinary and most of the rare varieties may be here seen mounted in rings, and in some cases set off by borders of diamonds. One amethyst (1187) is remarkable for containing several cavities partially filled with fluid. Some of the translucent specimens known as *cat's-eye*, of a yellow or brown colour, are good examples of the peculiar "chatoyant" effect produced by regularly disposed asbestos filaments penetrating the quartz. Several of the specimens catalogued under the heading Silica belong, however, to far more *récherché* species. One, for instance (No. 1188), is probably a pink topaz, and not an amethyst; another, called Burnt Quartz (No. 1194), appears to be a splendid chrysoberyl, the dispersive power of the stone being far too high for silica. Burnt quartz is the name given (after strong heating) to a dark variety of rock crystal from Portugal and Brazil, which assumes, after having been submitted to a high temperature, a sherry-brown or red colour.

Of opals there are more than a dozen in the Townshend collection; many of them exhibiting the most superb play of prismatic colours. One is pale grey, and shows a fine blue iridescence.

The sapphires and rubies include a series of fine specimens of the highly-prized deep velvet blue, and the pigeons'-blood red colours. There are also some violet and salmon-coloured corundums of great rarity and beauty. A golden-yellow one, of great brilliancy (No. 1312) is included with the topazes. The star-rubies and star-stones are well represented. These stones somewhat resemble the cat's-eye, but they show a six-rayed star, which is best seen when the gem, cut across its principal axis, and left with its top *en cabochon*, or rounded, is viewed in sunlight or in the focus of a condensing lens. In the case of these star stones, the internal reflection is due, not to intruding substances, but the mode in which the crystal has been built up of symmetrically disposed but not perfectly regular layers.

The turquoise comes next in order. The true turquoise is a phosphate and hydrate of aluminium coloured by a little phosphate of copper. The false, or bone turquoise, is merely fossil ivory tinted with phosphate of iron, not phosphate of copper, as stated in the catalogue.

Several of the most beautiful varieties of garnet are seen to perfection in the Townshend series. Two of the specimens (1306, 1307) are wrongly labelled "jacynth," and included under the heading Zircon in the catalogue. One of the so-called garnets, of an amethystine colour, seems likely to belong to another species.

A fine inscribed emerald, from the celebrated collection of the late H. P. Hope, is the first beryl in the list. It is of rich colour, and nearly $\frac{7}{8}$ inch by $\frac{5}{8}$ inch in dimensions. Another stone (No. 1284), though smaller, is a still finer specimen, faultless in cutting, shape, and colour, and almost half an inch across. The aquamarines in the collection are of immense size and rich hues. One of them presents almost the exact tint of the more valuable blue topaz.

Nine specimens are catalogued as chrysolites, but at least four of these are incorrectly assigned to this species. Nos. 1297 and 1304 are chrysoberyls, while 1304 is a zircon of the variety known as jargoon.

The white topaz is not well represented in the collection, and there is great confusion amongst the coloured varieties of this stone. No. 1309 is much more like a spinel than a topaz, while No. 1312 is a splendid yellow sapphire. No. 1318 seems only quartz.

Of the five stones labelled as tourmalines, one, No. 1322, is certainly a jargoon. The green tourmaline (Nos. 1321, 1323) is a beautiful but not brilliant stone. A peculiar interest attaches to this species, owing to its optical characters. It is a striking instance of dichroism, the rich grass-green specimens being perfectly opaque when viewed along instead of across the principal axis of the crystal.

Among the spinels is one of an indigo-blue colour (No. 1325), and two fine pink-coloured specimens.

There is a splendid suite of chrysoberyls, although some of them are incorrectly included under quartz and chrysolite. The precious cat's-eye, or cymophane, is a variety of chrysoberyl. Its opalescence is due to its intimate structure, and only appears when the light is incident at a particular angle. The line of light should be straight and clear as silver wire, and shows to the best advantage when the stone is of a clear green tint.

We have not space to record the peculiarities of the remaining species represented in the Townshend gift. There are moonstones and sunstones, with examples of labradorite, kyanite, and pyrite, all gorgeously mounted in gold coronet rings, and in many instances set off by rows of diamonds. We have merely noticed the existence of this very interesting and valuable collection in the hope that it may receive that attention at the hands, both of artists and mineralogists, which it deserves. The names assigned to about twenty of the specimens should be revised, and then we should possess in London three fine suites of authentic precious stones, from which their characteristics might be readily learnt. The other two public collections are in the British Museum and the Geological Museum of Jermyn Street. It is greatly to be desired that the specific gravity, hardness, crystalline system, and refractive and dispersive power should be noted on the label of each specimen.

A. H. C.

PROGRESS IN MECHANICS,

(Including Mining, Metallurgy, and Engineering).

MINING.

Although the Statistical Returns which for many years past have issued with great regularity from the Mining Record Office, under the direction of Mr. Robert Hunt, F.R.S., have this year been delayed in publication, owing to the illness of their respected editor, they have appeared sufficiently early to enable us to publish in this number our usual summary of their contents. The following statement shows the quantity and value of the several minerals raised in the United Kingdom during the year 1869* :—

	Tons.	£
Coal	107,427,557	26,856,882†
Iron ore	11,508,525	3,732,560
Tin ore	14,725	1,027,805
Copper ore	129,953	519,912
Lead ore	96,866	1,189,030
Zinc ore	15,533	49,366
Iron pyrites	75,948	41,023

* "Mineral Statistics of the United Kingdom for 1869." By Robert Hunt, F.R.S., Keeper of the Mining Records.

† Calculated before any charges for movement are added.

	Tons.		£
Arsenic ores	2,561	..	11,464
Gossans, ochres, &c. (returned)	709	..	943
,, ,, (estimated)	5,000	..	4,000
Wolfram and tunsgate of soda	25	..	323
Maganese..	1,558	..	7,897
Barytes	5,987	..	3,415
Clays, fine and fire (estimated)	1,200,000	..	450,000
Earthy minerals, various (estimated)..	—	..	670,000
Salt	1,250,000	..	687,500
Total value of the minerals produced in the United Kingdom in 1869			£35,252,120

On the most careless comparison of these returns with the corresponding summary of the previous year's productions,* one cannot fail to observe the extraordinary increase in the amount of coal which has been raised—an increase of considerably more than *four millions of tons* over the quantity quoted last year. Let it not, however, be supposed that this advance can be referred to any increased exportation of our coal. Mr. Hunt is indeed careful to point out that the advance is due to our own home-consumption, and is mainly referable to the renewed activity of many of our manufactures. Whatever may be the cause, this additional evidence of the constantly-growing drain upon the resources of our collieries, naturally leads us to look forward with much anxiety to the forthcoming Report of the Royal Coal Commission—a report which, if not actually presented at the commencement of next session, will, we hope, be in a fair way towards completion.

Of late considerable attention has been directed to the desirability of esta-lishing mining schools in the more important mineral districts. It is to be hoped that these efforts may be attended by a greater measure of success than fell to the lot of the mining schools of Truro, Wigan, and Glasgow—which are all now defunct—or even to the Bristol School, which is still lingering. And, indeed, better results may fairly be anticipated; for whatever may be the working of "The Elementary Education Act," it cannot fail to be of great benefit to the young miner, by putting him in possession of an amount of primary education which will enable him the more readily to receive and appreciate that higher technical education which it is the function of a mining school to dispense.

At the Meeting of the Social Science Congress at Newcastle, a paper "On the Scientific Education of Miners," was read by the Very Rev. Dean of Durham. The author argued strongly in favour of the establishment of a mining school in the north of England, and suggested that the University of Durham might institute a department of practical science with especial reference to mining.

At the same meeting, the Rev. W. A. Scott brought forward a communication "On the Education of Miners." This gentleman's information was derived chiefly from an acquaintance with the collieries of Durham, and he was led to complain bitterly of the long hours of work imposed upon boys working in mines,—a single shift of boys working with two shifts of men, and consequently each lad having to work from 12 to 14 hours daily. After the physical exertion inevitably consequent upon this length of labour, it can hardly be expected that a boy should avail himself of any means of instruction offered in the evening.

A paper "On Mining Schools," by Mr. T. Comber, was brought before the South Wales Institute of Engineers, at their late meeting at Cardiff. Following the plan of some of the continental establishments, the author suggests the propriety of having two classes of schools—a higher and a lower grade—and he believes that such schools, in order to be successful, must be self-supporting, since experience has shown that, however freely subscriptions may at first be given, it is futile to place reliance upon such a source of income for continued support.

‡ See Quarterly Journal of Science, January, 1870, p. 128.

One of the most effectual means of ultimately ameliorating the condition of the miner will be found in the gradual substitution of mechanical power for manual labour in underground work. It is therefore with much interest that we watch the progress of coal-getting machines. Messrs. Winstanley and Barker, of Manchester, have patented an improved mode of constructing machinery for holing or cutting coal. In this apparatus, a greater or less number of cutters or knives are securely fixed, by means of wedges and screws, to the circumference of a horizontal toothed wheel. Motion is imparted by the action of a pair of oscillating cylinders, worked either by compressed air or by steam : the piston-rods are connected to the crank-shaft, at the lower end of which is keyed a toothed pinion gearing with the spur-wheel or cutter. As the machine is slowly moved along the working face of the coal—for example, by means of a crab in front—the knives cut into the coal, and when the whole face has been holed, the cutting wheel may, by an appropriate arrangement, be brought under the body of the apparatus so as to facilitate the transit of the machine along the narrow passages of a colliery.

Mr. Mark Fryar suggests the use of simple hand-worked machines for coal-getting in the Indian collieries, where labour is cheap and complicated machinery expensive.

We are glad to hear that the hydraulic wedge, invented some time ago by Mr. J. Grafton Jones, has lately been working with considerable success at the Kiverton Park Collieries, near Sheffield. A hole having been drilled into the coal by means of a special drilling apparatus, the hydraulic wedge is inserted : by the action of a small screw-pump, a strong ram is powerfully forced against the steel wedge, which is thus driven between the inclined faces of two steel pressing blocks, and these by their lateral motion break the coal along its plane of cleavage. The use of gunpowder in blasting coal, which so often leads to disastrous results in fiery pits, is thus avoided.

An improved safety-lamp has been patented by Messrs. Teale and Co., of Manchester. The body is constructed on the principle of a sponge or portable gas-lamp, and thus a light is obtained at once more brilliant and less expensive than when oil is used. This sponge-lamp is screwed to the cage of an ordinary Stephenson or Clanny, which carries upon the inside two horizontal hinges working upwards : these allow the body to be readily screwed on, but on unscrewing the lamp they force a sliding tube over the wick, and thus extinguish the flame. The lamp is also furnished with a lock and stop, by which the miner is prevented from disconnecting the reservoir from the cage.

METALLURGY.

From the "Mineral Statistics," recently issued for 1869, we learn that from those minerals which are smelted the following quantities of metals have been obtained—the quantities being coupled with their respective values :—

		Tons.	£
Pig-iron		5,445,757	13,614,397
Tin		9,760	1,201,456
Copper		8,291	644,065
Lead		73,259	1,397,415
Zinc		4,500	92,400
Silver	ozs.	831,891	207,972
Gold	,,	18	62
Other metals (estimated)		—	500,000
Total value of metals produced in the United Kingdom in 1869			£17,651,767

An attempt has been made by M. Le Brun-Virloy to smelt iron ores in horizontal, inclined, or vertical furnaces, and yet to expose them to conditions similar to those which obtain in the blast-furnace. The ore is reduced to small fragments, which are mixed with such an amount of carbonaceous matter as is needful to effect their reduction, and with the proper proportion of fluxing material. In some cases it is considered desirable to form the mixture into hollow cylindrical blocks in order that it may be exposed to the most favourable conditions. These prepared blocks are introduced through the charging door, and are gradually passed through the furnace; the molten mass falling into the hearth at one end while a fresh charge is introduced at the other, so that the furnace is constantly kept filled. It is said that the ore is first "reduced and carburetted"; then as it passes onwards and is exposed to a higher temperature it melts, and at length flows into the melting-hearth, near which the combustible gases and heated air gain access to the furnace.

We learn from "The Glasgow Daily Herald" that a blast-furnace has recently been erected at the works of the Monkland Iron and Steel Company on a new principle patented by Mr. Ferrie. The furnace is 83 feet high, and is closed at the top by a bell-and-cone arrangement. On lowering the cone the raw materials fall into four separate retorts at the top of the furnace, where the coal is coaked before passing down into the body of the furnace, whilst the other constituents of the charge become strongly heated. The combustible gases evolved are used partly for heating this coking apparatus and partly for heating the blast.

A new lead-smelting furnace has been invented by Mr. George Metcalf, of the Pertusola Foundry, near Spezzia, in Italy, and is said to be working with the most satisfactory results. The furnace is divided longitudinally into two chambers by a vertical partition, which does not, however, reach to the grate or fire-bars. These passages may be placed in communication with the chimney at will, and as the draught is shut off from each compartment alternately, it follows that the charge on one side will be subjected to the free current of heated products of combustion, whilst the other side will be exposed merely to a dead heat. The charge is worked gradually over the curved bed of the furnace towards the fire-bars, being subjected to a preparatory roasting on one side of the partition and finished on the other. The slag collects in the chamber in front of the grate, whence it is run off and subjected to treatment in a blast-furnace, whilst the metal on the bed of the furnace is drawn off through the tapping-door. Each charge consists of about 1½ tons of ore, and occupies 24 hours in its passage through the furnace; four charges are in work at the same time, one being drawn every 24 hours.

The third volume of Dr. Percy's Metallurgy*—a volume so long expected and so often promised—has at length appeared. Instead, however, of completing the treatise, as originally intended, or even embracing the three metals lead, silver, and gold, as subsequently announced, the present volume deals only with the metallurgy of lead, including, however, the processes of desilverisation and cupellation. The volume forms a goodly octavo of nearly 600 pages.

It will probably be remembered that in the course of last summer Mr. Fremantle, the Deputy Master of the Mint, in company with Mr. W. Chandler Roberts, the chemist, and Mr. J. M. Napier, as engineer, made a rapid continental tour with the view of inspecting the chief European Mints, and of reporting upon their organisation. The report of this tour of inspection has just been published, and contains much that is of interest to the metallurgist. It is satisfactory to learn that the "waste" in our English Mint compares well with that of other establishments, and that the loss on gold coinage is estimated not to exceed in future 0·2 per mille; that is to say, there will not be more than £200 lost on a coinage of one million. Mr. Fremantle sees no necessity for altering our standard of fineness for gold coin (11 Au to 1 Cu), and believes that the "remedy of fineness"—that is, the margin allowed on the legal standard in the manufacture of gold coin—ought not to be reduced below 2 parts per mille.

In that portion of the report which has been prepared by Mr. Roberts we observe many excellent suggestions relative to improvements in the metallurgical operations of the Mint. Already he has introduced Miller's chlorine process for the treatment of gold too brittle for coinage—a process which was

* The Metallurgy of Lead, including Desilverisation and Cupellation, by John Percy, M.D., F.R.S.; Murray, 1870.

described at length some time ago in the "Chemical News." In selecting the copper which is to enter into the composition of "standard gold," it is of course needful that metal of considerable purity should be employed, and Mr. Roberts proposes to test the copper by drawing it to fine wire, and then comparing its electric conductivity with that of copper of known purity—the presence of a small amount of foreign metals having a great effect upon this physical property of copper. During the process of "blanching" or pickling the annealed blanks for gold coin, the copper is dissolved from the surface and a thin film of pure spongy gold produced, but during circulation this superficial film wears off, and the standard thus becomes deteriorated. Mr. Roberts therefore proposes to abolish the blanching of gold coin, and believes that this may be done without affecting the beauty of the surface. In future the "sweep" of the Mint—by which we understand the dust from the floors, ashes, and old crucibles—will not be sold, but will be treated in the Mint, and it is proposed to extract the precious metals from this refuse by the aid of Mr. Crookes's sodium-amalgam.

ENGINEERING—CIVIL AND MECHANICAL.

Paris Defences.—The present war has, to a great extent, directed the attention of engineers to the subject of offensive and defensive works, in supercession of those of progress and improvement. Naturally, all thoughts are directed towards the beleaguered city of Paris, and speculation is rife as to her chances of a successful resistance to the armed band by which she is encircled. The water-gates of the city have been protected by movable dams and pontoons, leaving just room enough for the passage of barges. The pontoons are composed of iron plate, and pierced with loopholes for riflemen; they are moored to anchors in the bed of the river, and similar defences are placed under the arches of the bridges. A new kind of defensive armour has been invented for the use of riflemen, called a *pelle cuirasse*, which consists of a light steel shovel, with a handle of strong but light wood; the tool is used for digging a pit where the soil is suitable, and this done, the rifleman may stick the handle in the ground and protect himself behind the shovel. Two or three great locomotive turrets for mitrailleuses have been constructed for the defence, and rails have been laid down within the ramparts for moving them wherever they may be required; and MM. Cail and Co. are now employed in the construction of an immense armour-plated locomotive to carry three guns, as well as in the manufacture of Chassepot barrels out of steel rails and axles which were in store at the depot of the Western Railway. The iron gun-boats on the Seine each carry a piece of 24 cwts. in the stern, and two mitrailleuses forward, and are pierced in the sides with fifty loopholes for fusiliers on each side of the vessel below deck. Admiral Labrousse has produced a modified form of the Moncrieff gun-carriage, substituting springs for the counterpoise, which has been tried with successful results. A corps of Engineers has been formed under the direction of M. Tresca, Director of the Conservatoire des Arts et Metiers, to form what are called casemented shields in all the sections of the fortifications, for which purpose a sum equal to £9000 has been granted. In the forts round Paris there were, until quite recently, neither *abris*, platforms, magazines, casemates, embrasures, nor any of the necessary accessory works without. All these were, however, rapidly supplied by the military Engineers. Besides such works, the sixty-nine gates of the *enciente* had to be closed and furnished with bridges. Four canals had to be barred, and the Seine stockaded. The military zone had to be cleared of houses, portions of the Bois de Boulogne and Vincennes cut down, the forts supplied with palisades, extending in all to 61,000 metres, and, finally, three new batteries were formed at St. Ouen, Montmatre, and the Buttes Chaumont. In 1840 the armament of Paris was fixed at 1824 pieces, with a reserve of 540; now there are 2190 guns in battery, with a reserve of 350. The bronze field guns cast in Paris during the siege are composed of a mixture of 90 parts of copper and 10 of tin; the lining of brass consists of about 62 parts of copper, 35 of zinc, and 3 parts of tin and lead.

Balloon Navigation.—Since the investment of Paris the only communication with the outer world has been carried on by means of balloons. Almost as soon as these aërial carriages were invented they became applied to the purposes of war. In the early campaigns of the French Republic a company of ballooners was organised under a young doctor named Coutelle, which performed good service; and again, in the late civil war in America, they were employed as aërial posts of military observation. At the present time a balloon *captif* floats over Montmatre as a post of observation, and a regular system of balloons has been established for postal purposes. At first letters were sent off in passenger balloons, one of which consisted of three balloons united together, and carrying 2 cwts. of letters. Another mode is now in operation, and postal cards are sent off sometimes in free balloons. The Brothers Godard have an *atelier* for the construction of balloons at the Orleans Railway Station, whence several have already been made and sent off; these balloons contain 2045 cubic metres; but it is said that they are about to construct a gigantic aërostat to carry from twelve to fifteen passengers.

M. Dupuy de Lôme, formerly Chief Constructor of the French Navy, has taken up the subject of aërial navigation. He posposes to form a balloon in the shape of an elongated egg, the main axis to be horizontal and 130 feet long, the transverse diameter 46 feet, and the total volume 3860 cubic metres. The car is to be ovoid in form, and fitted with two spars or bowsprits, the extremities of which will be 100 feet apart. The car is to be attached to the balloon by means of shrouds fixed to these two spars, and to be suspended about 50 feet below the balloon. A small triangular sail at one end of the car is to perform the function of a rudder. The gas to be employed is the ordinary carburetted hydrogen, and the ascensional force will be equal to nearly three tons. The Government of Paris has voted a sum of 40,000 francs to enable M. Dupuy de Lôme to carry out his proposed plan of navigable balloons. He proposes to work them by manual power. Another inventor is engaged in the construction of an aërostat to be worked with steam or other power. The Academy of Sciences of Paris has expressed a very decided opinion against aërostats heavier than the air, moved by steam or other power; but it has given some countenance to a proposition by a M. Sorel, who uses a sail as a rudder, and a screw simply to produce a difference between the velocity of the machine and that of the wind.

Loss of the "Captain."—The foundering of H.M.S. *Captain*, off Cape Finisterre, early on the morning of the 7th September last, has revived the question of broadside *versus* turret ships. This ill-fated vessel, it appears, was constructed in direct opposition to the expressed opinions of Mr. E. J. Reed, the late Chief Constructor of the Navy, who has always been opposed to vessels having a low free-board. The Captain was originally to have had a free-board of 8 feet, but, owing to some alterations in her construction, possessed a considerably increased immersion, the danger of which was publicly pointed out by Mr. Reed in his letters to the "Times" about the period of her sailing on her last voyage. The naval court-martial, before which the few survivors of the vessel were arraigned, found in their verdict that the *Captain* was capsized "by pressure of sail, assisted by the heave of the sea, and that the sail carried at the time of her loss (regard being had to the force of the wind and the state of the sea) was insufficient to have endangered a ship endued with a proper amount of stability." They further remarked that the *Captain* was built in opposition to the views and opinions of the controller and his department, and that the evidence all tends to show that they generally disapproved of her construction. It further appearing in evidence that, before the *Captain* was received from her contractors, a grave departure from her original design had been committed, whereby her draught of water was increased about two feet, and her free-board was diminished to a corresponding extent, and that her stability proved to be dangerously small, accompanied with an area of sail, under these circumstances, excessive." This loss cannot fail to have the effect of, for a time, bringing into disrepute a most valuable class of war vessel, and it is to be feared that this calamity will prejudice the minds of Englishmen against turret ships long after the real cause of its occurrence has been forgotten.

The Solar Engine.—Captain John Ericsson, the well-known American engineer, has recently been engaged in the construction of an engine to be driven by means of solar heat, concentrated by the means of certain mechanism for the purpose of generating steam. The inventor, in communicating the subject to a weekly contemporary, states as follows:—"The several experiments that have been made show that the mechanism adopted for concentrating the sun's radiant heat abstracts, on an average, during nine hours a day, for all latitudes between the equator and 45°, fully 3·5 units of heat per minute for each square foot of area presented perpendicularly to the sun's rays. A unit of heat being equivalent to 772 foot-pounds, it will be perceived that, theoretically, a dynamic energy of 2702 foot-pounds is transmitted by the radiant heat per minute for each square foot; hence 270,200 foot-pounds for an area of 10 feet square. If we divide this sum by the adopted standard of 33,000, we ascertain that 100 square feet of surface, exposed to the solar rays, develope continuously 8·2 horse-power during nine hours a day, within the latitude before mentioned." Hence, he argues, "that those regions of the earth which suffer from an excess of solar heat will ultimately derive benefits resulting from an unlimited command of motive power, which will, to a great extent, compensate for evils hitherto supposed not to be counterbalanced by any good." The solar engine is composed of three distinct parts:—The engine, the steam generator, and the mechanism by means of which the feeble intensity of the sun's rays is augmented to such a degree that the resulting temperature will exceed that of the lowest pressure of steam admissible in an efficient engine. The motor itself is essentially a modern steam-engine, utilising, to the fullest extent, the mechanical energy of the steam generated by the concentrated solar rays. Mr. Ericsson withholds from the public, at present, any description of his steam generator and concentration apparatus, and until he shall have brought them to a more perfect state.

Railway Gauges.—A most important question has lately been engaging the attention of the Government of India, both in this country and subsequently in India, namely, as to the gauge to be adopted for the railways of the future. The existing standard gauge in India is 5 feet 6 inches; but it was considered that with a narrower gauge the requirements of the country might equally well be met, and at a less cost per mile. The subject was accordingly referred to a committee, consisting of Colonels Dickens and Strachey, and Messrs. Fowler and Rendel. The result of their inquiries has been the submission of two reports, the one by Mr. Fowler, wherein a 3 feet 6 inches gauge is recommended, and the other by the remaining members of the Committee, who advocate a gauge of only 2 feet 9 inches. The general opinion of the profession appears to be in favour of the wider gauge, some leading members being even averse to any deviation from the existing standard gauge. The final decision rests with the Governor-General in India, and that decision has not yet come home.

Plain Cylindrical Boilers.—At the recent meeting of the Iron and Steel Institute in South Wales, Mr. Jeremiah Head, of Middlesborough, read a paper "On the Efficiency and Durability of Plain Cylindrical Boilers." Mr. Head, after giving certain statistics relative to these boilers, remarks that their simplicity of construction is greater than in any other class of boilers, and that in them an ordinary quality of plate and ordinary workmanship are alone necessary. The diameter being moderate a high pressure may safely be maintained without the use of thick plates, and without the expense of double rivetting and drilled holes, and that the risk of overheating for lack of water is much less than with internally-fired boilers. The cost of such boilers varies with the locality, but taking the rates at present ruling in the Cleveland district, plain cylindrical boilers, with fittings, all of best material and workmanship, cost £18 10s. per ton delivered. Comparing these with internally-fired boilers, Mr. Head shows that the cost of the latter is three times that of plain cylindrical boilers, whilst their saving in fuel amounts only to about 9 per cent. The objection to these boilers—in common with all others externally fired—is that they undergo a succession of expansions and contractions, and ultimately break their backs, generally through a line of rivets. This drawback, however, it is proposed to remedy by hanging them to rods and nuts, the latter resting upon volute springs instead of upon rigid bearers, by which means they are enabled to adjust themselves to each change of form of the boiler without materially increasing or relaxing their hold.

QUARTERLY LIST OF PUBLICATIONS RECEIVED FOR REVIEW.

Advanced Text-Book of Zoology, for the Use of Schools. By H. Alleyne Nicholson, M.D., &c. *William Blackwood & Sons.*

The Proceedings of the Canadian Eclipse Party, 1869. By Commander Ashe, Director of Observatory, Quebec. *Middleton & Dawson.*

Results of the Magnetic Survey of the Colony of Victoria, executed during the years 1858—1864. By George Neumayer, Ph.D. *Mannheim: J. Schneider.*

Comparison of the Mean Daily Range of the Magnetic Declination with the number of Aurora observed each year, and the Extent of the Black Spots on the Surface of the Sun. By Elias Loomis. *New Haven: Tuttle, Moorhouse, & Taylor.*

Geology. By John Morris, F.G.S., and T. Rupert Jones, F.G.S. First Series. *J. Van Voorst.*

Lessons in Elementary Physics. By Balfour Stewart, LL.D., F.R.S. *Macmillan & Co.*

Handbook of the Telegraph. By R. Bond. Third edition. *Lockwood & Co.*

Handy Book of Small Farm Management. By Thomas Baldwin. *Dublin: Browne & Nolan.*

A Manual of Zoology, for the Use of Students. By Henry Alleyne Nicholson, M.D., D.Sc., M.A., &c. *William Blackwood & Sons.*

Patents and Patentees, from 1854 to 1866, and 1867, 1868. Compiled by W. H. Archer, Registrar General of Victoria.

Abstracts of Specifications of Patents Applied for from 1854 to 1866. By same Author.

Abstracts of English and Colonial Patent Specifications relating to the Preservation of Food, &c. By same Author. *Melbourne: John Ferres.*

PAMPHLETS AND PERIODICALS.

On Ocean-Currents. By James Croll.

Reports of the Mining Surveyors and Registrars of Victoria. *Melbourne: John Ferres.*

Biology *versus* Theology, or Identity of the Cosmical and Vital Forces, according to Dr. Lewins. By Julian.

On Recent Improvements in the Cultivation and Management of Hops. By Charles Whitehead.

The Education and Status of Civil Engineers in the United Kingdom and in Foreign Countries.

The Technologist. *Industrial Publication Company, New York.*
The Popular Science Review.
The Geological Magazine.
The Food Journal.
The English Mechanic.
The Armourer.
The American Naturalist.
North British Agriculturist.
The American Chemist.

PROCEEDINGS OF LEARNED SOCIETIES, &c.

Proceedings of the Academy of Natural Sciences of Philadelphia.
Monthly Microscopical Journal. *Robert Hardwicke.*
Journal of the Royal Historical and Archæological Association of Ireland. *Dublin: McGlashan & Gill.*
Proceedings of the Bristol Naturalists' Society. (Vol. iv., 1869).
Seventh Annual Report of the Belfast Naturalists' Field Club. (1869-1870.)
Monthly Notices of the Royal Astronomical Society.
Proceedings of the Royal Society.
" " Cotteswold Naturalists' Field Club. (1869.)

ERRATUM.—In No. 26, April, 1870, page 230, line 9 from top, for "*there is no reason*," read "*there is reason.*"

NOTICE TO AUTHORS.

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THE QUARTERLY

JOURNAL OF SCIENCE.

APRIL, 1871.

I. A PAGE OF SCIENTIFIC HISTORY:

REMINISCENCES OF THE EARLY DAYS OF THE ROYAL COLLEGE OF CHEMISTRY.

By A. W. HOFMANN, Ph.D., LL.D., F.R.S., &c.

THE Royal College of Chemistry owes its origin to the grand development of chemical science which took place towards the middle of this century, more especially due to the labours of Professor Liebig, then teaching chemistry in the small University of Giessen. At that time Liebig had already written his "Chemistry Applied to Agriculture," in which he, for the first time, had laid the true scientific foundation of this most important of all human trades. His work on "Animal Chemistry," which gave a new start to physiological and pathological researches, had just appeared, and his "Familiar Letters on Chemistry" had opened to the many a field of scientific instruction which hitherto had been accessible but to few. But Liebig had done more than this. He had founded a school of chemistry—indeed, the first school of chemistry. Between the years 1835 and 1845, the University of Giessen possessed the only laboratory in which not only chemical analysis, but also the art of performing chemical researches, more especially in the department of organic chemistry, was systematically taught. Young men from all quarters of the globe flocked to Giessen for the purpose of studying under Liebig's auspices, and the number of important chemical investigations which at that period emanated from the Giessen laboratory might with truth be said to form an era in the history of scientific discovery.

It was more especially in England that the progress of Liebig's school had been watched with deep interest. English agriculture and English industry had obviously received a powerful impulse from the researches carried out by Liebig and his pupils, and the idea very naturally suggested itself that the establishment of a similar school in England would materially accelerate their further progress.

The utility of founding a chemical school upon the model of Liebig's, in Great Britain being once admitted, it was not long before a number of public spirited men formed an association for the purpose of calling such a school into existence. To Dr. John Gardner, the translator of Liebig's "Familiar Letters," belongs the merit of having agitated the project with great ardour and indefatigable perseverance. Temporary premises were taken in St. Martin's Place, where a public meeting was held on the 29th of July, 1845, at which a definite form was given to the new institution by the election of a council, with power to add to their number, and by the appointment of certain executive officers under the control of the council.

Sir James Clark had joined the movement from its very beginning. He soon became deeply interested in the new institution, the staunchest and most faithful friend of which he remained as long as he lived.

The first step the council had to take was to appoint a fit professor capable of organising the school to be established, and, from this early period, the influence of Sir James began to be exerted. He at once most clearly recognised what was actually wanting. There was no lack of most excellent chemical lecturers in England; indeed, the style of experimental illustrations, then quite general in England, was infinitely superior to that which at that period prevailed in Germany and on the Continent in general. It was easily accessible instruction in chemical analysis, and more especially in the art of carrying out experimental inquiries, that had to be provided; the few laboratories at which one or two young men were received as practical students being altogether inadequate to supply the daily increasing demand for instruction on moderate terms, such as was offered by the chemical laboratory of the University of Giessen. After mature consideration, the council of the new college, chiefly at the suggestion of Sir James, agreed that Liebig should be called upon to recommend one of his assistants for the new professorship. The council did not conceal to themselves the difficulties involved in the appointment of a foreigner, but none of the distinguished young English chemists of that period, though nearly all pupils of Liebig, had had the advantage of being thoroughly acquainted with the method of teaching introduced by Liebig—in one word, of having taught under the great master.

Prof. Liebig recommended to the council three of his former assistants, viz., in the first place, Dr. Will, then Assistant Professor in the University of Giessen; in the second place, Dr. Fresenius, Professor of Chemistry in the Agricultural School of Wiesbaden; and, lastly, the present writer, then one of the junior teachers in the University of Bonn. Drs. Will and Fresenius declined the professorship offered to them in the first instance, but the writer of these lines could not resist the temptation thus thrown in his way.

At an interview with Dr. Gardner, who had become secretary to the college, and who came over to Bonn for this purpose, the preliminaries of an agreement were settled. But some very important difficulties had still to be removed. The council being unable to offer more than a definite appointment for two years, the writer, who had never been in England, was unwilling entirely to break his connection with the University of Bonn. It was but natural that he should have wished to retain a hold of his most modest position in Germany in case his experimental mission to England were to fail. No one, except a man in Sir James's influential position, could have overcome this obstacle.

In the latter part of the summer of 1845 the Queen and Prince Albert visited Germany. They were in Bonn on the day the monument of Beethoven was inaugurated. It was on the very same day that I met Sir James Clark for the first time. Our interview did not last more than ten minutes, but they were sufficient to shape the course of my life. The assurances Sir James gave me removed the last hesitation which I felt. I was introduced to the Prince, who confirmed every word Sir James had said. To bring matters to a conclusion, Sir James asked me to come next day to the castle of Brühl, where the English Court was residing with the King of Prussia. At this interview the Chevalier Bunsen was present.

This interview is a bright spot in my recollections, and I may perhaps be excused for entering into some details. Twenty-five years have elapsed, but I could almost repeat the words which were spoken. "After a short stay in London," Sir James said to me, "you will probably be amused at your having been clinging with so much tenacity to your private docentship at Bonn; still, we find this feeling perfectly legitimate, and we think that only with your mind perfectly at rest as to the future, can you devote your whole energy to the task, by no means easy, which you are going to undertake. You ask that we may procure for you from the Prussian Government a leave of absence for two years, that you may be enabled, after the lapse of this term, to resume the position you now hold in case you should not succeed in establishing a school of chemistry in

London. This we are most willing to do, but we believe that we ought to do more for you—we ought to obtain a promise from the Prussian Government that, should you feel disposed to return to Germany after two years, you should be allowed to join the University of Bonn with such promotion as you would in all probability have attained by remaining in Bonn. What would your position probably be after two years?" I said that if I had good luck, I should then hold an Extraordinary Professorship. "This is exactly what the Chevalier Bunsen says," replied Sir James, "and we must take care to make them promise that they will appoint you Professor Extraordinary when you return." The Chevalier Bunsen said that it was a very uncommon thing to give promises of this kind, but that if Prince Albert were to speak upon the subject to the King, the demand might after all be granted. He himself meanwhile undertook to submit the case to the minister." I said that if they succeeded in carrying out this scheme I should be ready to go to-morrow. This was really a day of good luck. The great state transaction which was to accomplish so important a result was carried on with unheard-of despatch. The King, the Prince, and the minister were, in fact, close at hand; they all, together with my two new protectors, being inmates of the castle of Brühl. After two hours, during which I took a charming morning walk in the beautiful gardens of the castle, we met again. When I entered Sir James's room, I found him beaming with delight. His mission had been perfectly successful. The Prince and the King had discussed the question, and the latter agreed to everything the former had demanded. The Chevalier Bunsen in his interview with the minister had found more difficulties. "The minister," he said, "cannot promise to make an appointment after the lapse of two years, because he does not know whether at that time he may be still in office. All he can do for Dr. Hofmann will be this. He can at once appoint him to an Extraordinary Professorship in the University of Bonn, and then give him leave of absence for two years." I had no objection to this mode of getting over the difficulty.

In October, 1845, the analytical course of the College of Chemistry commenced, a temporary laboratory having been established in Great George Street, Hanover Square. A considerable number of students (about twenty) at once entered, and afforded sufficient evidence that an institution like the one projected was really wanting in England.

The opening of the school in these temporary laboratories having been effected, the council proceeded to look forward to a more convenient and permanent habitation for the college.

Encouraged by the promises of support they had received, and backed by the guarantee of several noblemen and gentlemen, the council took premises in Hanover Square. This site not only possessed the advantages of a good and ostensible position, affording all necessary accommodation for the official purposes of the college, the residence of the professor and secretary, and the meetings of the members, but supplied the desideratum of a large piece of ground, with a frontage in Oxford Street, whereon convenient and well arranged laboratories could be erected.

To the construction of these laboratories the most anxious consideration of the council was next directed. After lengthened consultations with their architect, Mr. James Lockyer, a plan was ultimately adopted to which all the parties concerned had given their adhesion, and on the 16th of June, 1846, the council had the gratification of seeing the first stone of the building laid by H.R.H. Prince Albert, who had never ceased to lend his most cordial support to the new establishment. In the short period of three months the new building was completed, so that as early as October, the operations of the school, to which in the meantime the patronage of the Prince had procured the title of the Royal College of Chemistry, could be commenced in the new premises.

As might have been expected, in the endeavour to institute such an establishment as the Royal College of Chemistry, serious outlay had been unavoidable. Here the difficulty of the task commenced. In building the laboratories and furnishing them with the necessary apparatus, the responsibility of a heavy debt was incurred. A building fund had been opened for this purpose, and contributions at first flowed in with great liberality, but long before the buildings were completed the current slackened, and at last a debt of £2000 remained, without apparent means of defraying it. The pressing demand was met by the noble generosity of the council, who, by a *pro rata* contribution among themselves, succeeded in extinguishing this heavy debt. But this effort was by no means sufficient to clear the difficulties. It was found that the ordinary expenditure of the institution exceeded, and, indeed, very considerably exceeded, its income. The annual subscriptions declined, and notwithstanding the constantly increasing receipts from the fees of the students, the council of the college became more and more embarrassed for the means necessary to carry out the noble work so well begun. The most conflicting proposals were made to restore the financial equilibrium of the institution.

It was at this dangerous period, when the very existence of the college seemed imperilled, that Sir James stood forth its most enlightened and its most devoted champion.

It had been the ambition of the council to establish in England a school of chemistry on the model of the chemical laboratories connected with the German universities. They were well aware that not one of those institutions is self-supporting. All these institutions receive very considerable support (varying, of course, with the importance of the university) from the respective states to which they belong.*

The British Government of that period did not yet recognise the claims of science, and it was therefore necessary to look towards other sources from which to defray the expenses of the college. For this purpose the system of life subscriptions and annual subscriptions had been resorted to. But since it was not likely that many would be found willing to support on public grounds alone an institution designed for the diffusion of chemical knowledge and for the advancement of science in the country, it became necessary to hold out some prospect of personal and private advantage to the subscribers. Various proposals were made for this purpose. One of the privileges which from the very commencement it had been thought possible to grant, was the analysis of substances sent by subscribers. Some of the members of the council were most anxious to establish in the college an analytical department undertaking private investigations on a very large scale. They hoped to secure in this manner ample means for carrying on the scientific school. Others, again, imagined that an institution midway between the Royal Institution and the Polytechnic would attract crowds of members, and that the income derived from brilliant exhibitions, evening lectures both instructive and amusing, and, lastly, from frequent conversaziones, would be more than sufficient to defray the expenses of the school. All this stupendous machinery was to be put in motion by the Professor of the College with his two assistants, who were, moreover, expected to instruct some thirty or forty students in analysis, and to contribute to the progress of science by their researches. Happily, there were men on the council who clearly perceived the folly of

* The annual grant of the Prussian Government to the chemical laboratories of the University of Berlin amounts to 7100 thalers (£1065).

these proposals, which, if they had been entertained, would have altogether changed the nature of the institution, and thus have deprived the country of the only school of practical chemistry then in existence. If these several proposals, which were urgently pressed by some well-meaning but ill-advised members, were rejected, it was due to the clear-sightedness of Sir James, who, aided by some other influential members, more especially Lord Ashburton, Mr. John Dalrymple, and, at a later period, by Mr. Warren de la Rue and Dr. Bence Jones, succeeded in convincing the council that the only way of saving the college would be by confining themselves to the principal object contemplated in its foundation, viz., the advancement of science by means of practical instruction in the laboratories and by researches. From this moment Sir James became the very soul of the school. By his advice all unnecessary expenditure was at once avoided; the large premises in Hanover Square were given up, the laboratories only being retained; lastly, the office of secretary was established. It was the confidence in Sir James's friendship that induced the Professor to give up half his emoluments.

By this re-constitution of the College the expenditure was at once very considerably diminished, and since the income from students' fees was still increasing, the council found no difficulty in defraying the small deficiency by an annual donation which the College owed to the liberality of Lord Ashburton, and by the subscriptions of the friends who had remained faithful to the institution. A series of prosperous years now ensued: the College gained more and more the estimation of the public and of the leading scientific men, and Sir James had the satisfaction of seeing that the time and labour which he had so largely devoted to the institution did not remain without reward. But Sir James was not one of those who are satisfied with a partial success. He perceived that the financial balance, which had been happily restored in the College, was very precarious, and easily disturbed by fluctuations in the attendances of pupils or by falling off of subscriptions. The more the College began to fulfil his expectations as a teaching institution, the more was he anxious to establish it on a more permanent foundation. His aim was to obtain a Government grant for the school, and no pains were spared on his part, no means neglected which his high and influential position could suggest to attain this important object. Several attempts failed, and it was not until 1852 that a happy combination of circumstances enabled Sir James to achieve a plan which he had so long cherished. It was in that year that Dr. Lyon Playfair resigned the Professorship he then held in the School of Mines connected with the Museum of Practical Geology in Jermyn Street. Sir James at once placed himself in communication with Sir Henry de la Beche, then Director of the Museum, and by his exertions, in which he was most generously supported by Lord Ashburton, he succeeded in bringing about a fusion of the two institutions. The Royal College of Chemistry was adopted by Government as the chemical department of the Museum of Practical Geology, the Professor of the College becoming the successor of Dr. Playfair as Director of the Laboratory and Chemist to the Museum. The Royal College of Chemistry became the property of Government, under whose auspices it has since been carried on in a most prosperous manner.

Of the further development of the Royal College of Chemistry in connection with the Royal School of Mines, and of the services which it has rendered to the cause of science, nothing need be said here, except that Sir James never ceased to take the most lively interest in its progress. Even long after my return to Germany—indeed, within the last twelve months—I received two letters from him, which showed that he was still watching the welfare of this College with unabated solicitude.

I must not, however, conclude this sketch without alluding in terms of the deepest gratitude to the, I might almost say, paternal kindness with which Sir James overwhelmed me from the moment when first we met at Bonn until when, but shortly before his death, I saw him last. During the many years that I have directed the laboratories of the Royal College of Chemistry, often, and more especially in the beginning, under difficulties which appeared insurmountable, Sir James's ever-ready counsel, his most active support, his warm-hearted sympathy, have never failed me on any occasion. Not without profound emotion can I think of the time which most unsparingly he devoted to my instruction ; indeed, so far went his anxiety to promote my progress, that, tired as he often was from the exertions of his professional life, which at that time had reached its culminating point, he did not shrink from giving up the late hours of the evening to correct my early attempts at English composition. The evenings which, twice a week and often more frequently, I spent at that time with Sir James and Lady Clark in Brook Street, belong to the most charming recollections of my life. Indeed, if I have not altogether failed to fulfil the expectations which the originators of the College of Chemistry entertained when they placed the new institution under my direction, it is due to the powerful influence which the friendship of Sir James exercised upon my career at that most important period of my life.

POSTSCRIPT. Having in the preceding sketch pointed out the early exertions which Sir James Clark made to procure a Government grant for the College of Chemistry, it is certainly not without interest to contemplate that at present a Royal Commission is sitting in London charged to inquire in what manner science can be best promoted in England by the grant of Government subvention, and that the author of this sketch, whilst writing it, has received from that Commission an invitation to proceed to England for the purpose of giving evidence on the mode in which scientific institutions are supported by Government in Germany.

II. THE THEORY OF ATMOSPHERIC GERMS.

By ARTHUR ERNEST SANSOM, M.D.,
Member of the Royal College of Physicians.

ONE of the most celebrated of the Deputies lately commenced a speech before the Austrian Reichsrath with the words :—"The great question before us is,—Is Charles Darwin right or wrong?" Probably this sounded strangely, for the world is not accustomed to be told suddenly that the solution of a scientific problem is material to its position or its progress. There is another great question which has been recently debated with a considerable amount of warmth, and has seemed to evoke a very considerable interest. Mighty issues are involved in it, and yet its problems are interwoven with the most ordinary processes of domestic life. It has to do with the most abstruse speculations as to the origin of living things in the kingdom of nature. It concerns the art of the brewer and the maker of wine. It is linked with the processes which preserve food from decomposition and sewage matters from being hurtful to mankind; with the treatment of wounds and with the arrest and prevention of pestilence. Complex in its relations, the question can nevertheless be curtly stated—Do living things of necessity spring from pre-existing living things or no?

Those who debate this question range themselves in two classes, with defined lines of demarcation between them. The ones say: living things do *not* necessarily arise from pre-existing bodies, which possess or did possess attributes of vitality; but they may originate from non-living matter in connection with the ordinary forces of nature. The others say that living things invariably are the progeny of living things; that in nature only vital material is capable of endowing non-vital matter with the special properties which itself possesses. After a protracted debate in the learned societies of France, the upholders of each side of the question appear to have arrived at that stage at which each prefers to remain unconvinced. More recently in this country the ardour of debate has been displayed, and yet there are strong forces in either camp.

The following is a brief outline of the debateable ground. It is well known that it is the tendency of moist organic matter, under certain physical influences, to undergo certain processes known as decay or decomposition. Either it disintegrates, evolving nauseous odours, and giving rise in the course of the disintegration to a multiplicity of chemical products organic or inorganic. Such is known by the term putrefaction. Or, the organic material being less complex in the beginning, the process runs a more definite course and the products are less varied. Such is fermentation.

In both instances there is observed intimately connected with the process, the occurrence, growth, and multiplication of living organisms, either amid the particles of the moist decomposing substance or upon the surface, where these frequently make their appearance as ordinary mildew. How are these results explained by the two theories? According to the one, the molecules of a putrefying or a fermenting body are in a state of motion, tending to the disruption of their elements. The living particles observed are the results of the communion of certain non-living elements with the physical forces with which they are in relation. Thus there is a strict analogy between crystallisation and creation. As in the one case certain molecules under certain conditions assume definite crystalline forms, so certain molecules under other certain conditions assume the appearances and the attributes of vitality.

According to the other theory, there is a single cause for all the phenomena. This cause is the presence of living matter. The organic elements of a putrescible or fermentescible compound undergo disruption by no inherent tendency of their particles to motion, but by the influence upon them of living, growing, and multiplying organisms, which, by their very acts of life and struggle for existence, superinduce this disruption. The living beings which are acknowledged to be present are the intimate causes and not the adventitious signs, nor yet merely intermediate agents of the decomposition of the material.

If we investigate the question from the stand-point of the second theory, we shall have to inquire concerning the living organisms declared to be the *prima moventia*, how they are brought into relation with the decomposable matters. The close relation between the presence of atmospheric air and the occurrence of the phenomena of putrefaction has been constantly admitted. It is a matter of common knowledge and common practice that to expel the air from a putrescible substance is a powerful means of preserving it from putrefaction. The presence of air is one of the conditions insisted upon by the supporters of the theory of spontaneous generation as essential to the production of living forms. No other gas can be substituted for atmospheric air except oxygen.

Upon what element of the air does its influence upon putrefaction and upon the appearance of living forms depend? The nitrogen may be at once dismissed, as direct experiment shows that it prevents putrefaction and is fatal to living things. The influence might appear with greater show of probability to be due to the oxygen. This was the hypothesis of Gay-Lussac, but experience soon showed that in many instances putrefaction was prevented when oxygen had free access to putrescible solutions; and when the progress of chemistry allowed Gay-Lussac's experiments to be conducted with greater precision, it was found that in cases wherein putrefaction was arrested by the attempted expulsion of air, oxygen, instead of being invariably absent from the gaseous residuum, was very generally present. Furthermore, according to Dr. Bastian's late experiments, the development of living forms may take place though all air may have been excluded as rigidly as possible.

Schwann concluded from his experiments that it is not oxygen, at least the oxygen of the air, which occasions putrefaction, but *a principle* contained in ordinary air which heat can destroy. This same principle could also, according to Schroeder and Dusch, be arrested by the meshes of cotton-wool. And it could be arrested by flexures made in a fine glass tube which admitted the air to a putrescible fluid, according to observations made by Pasteur and more lately repeated by Lister. Now, it may be urged that these results are not the *invariable* teachings of experiments. In case of certain putrescible fluids, and in certain conditions, putrescence has not been prevented by the means employed; but it must be recollected that, in all the instances quoted, parallel experiments were carefully made. In the one case, wherein the air was uncalcined or unfiltered, putrescence took place and organisms appeared; in the other, wherein the air was calcined or filtered, there were no putrescence and no organisation. The first step in the inquiry is the determination of the substances besides the gaseous constituents which are present in atmospheric air. The suspended matters are proved to be—(1) A number of fine particles of inorganic matter; chloride of sodium derived from the sea is proved by spectrum analysis to be present almost constantly; fine particles of the metals are present, especially in localities where are prosecuted trades, wherein they are manipulated. (2) A quantity of organic *débris*; starch cells and fragments of vegetable tissue are common; cotton and wool fibres are found in certain localities besides many kindred dry organic substances. (3) Organic *débris* derived from the animal body is also abundantly found. Epithelium cells have been frequently observed in ill-ventilated rooms; Eiselt found pus cells in the air of an ophthalmic hospital; organic matter, moreover, is given off from the lungs, for sulphuric acid is darkened, permanganate solution is decolourised and pure water rendered offensive by it. (4) Lastly, organised bodies, spores of fungi, have been seen by a host of observers. The appearance of organised bodies in ordinary atmospheric air was first satisfactorily established by Pasteur (see fig. 1). Since his observation the microscopic examination of air has been made in many ways by different observers, and all have agreed as to the presence in it of sporules of cryptogams, and of bodies possessed of vitality.

FIG. 1.

Corpuscles obtained from atmospheric air, treated by (*a*) solution of potash, (*b*) aqueous solution of iodine. (After Pasteur.)

The fact of the presence of organised bodies in the air being to all intents and purposes uncontested, we have to consider their possible relations in the causation of the development of vital forms in putrescible solutions. On this point there is wide difference of opinion. M. Pasteur, with the Panspermatists, holds that they are the sources of all the organisation; M. Pouchet and the heterogenists, that they are far too infrequent and insufficient to account for the phenomena.

The question, "Are the organisms which have been observed and described sufficient to account for the multitudes of living forms which are met with in the course of putrefaction?" is of the first importance. If so, they ought to be observed upon or within the putrescible solution, and their mode of multiplication should be distinctly made out. It is, however, certain that this cannot be established. When an organic solution is examined, prior to the commencement of putrefaction, the highest powers of the microscope fail to detect the ova or spores of the future minute organisms. There remains, therefore, this dilemma: either the germs are invisible or else the primigenetic molecules are self-formed in the fluid. We should then inquire if there is anything in analogical evidence which could tend to the conclusion that the germs of living things are so minute that the microscope cannot detect them. Dr. Beale, using the highest powers, figures extremely minute masses of germinal matter which develop into perfect fungi, yet which might elude very close investigation; and it is far from certain that these are not the indices of still more minute particles of living matter. It is surely unfair to stay further investigation by asserting at once that, because the germs cannot be demonstrated, therefore the inquiry is at an end, and the question is judged. The heterogenists have constantly urged this: they say "Show me a specimen of these creatures and I will admit them; otherwise I shall declare that they do not exist." The eye alone is not the test absolute of the cause of phenomena. Would a philosopher assert that a man could safely enter a poisonous atmosphere because he could see nothing wrong even with the highest aid to his visual powers? Would it be fair to conclude after the inoculation of vaccine virus into an animal that no virus exists in its blood because none can be detected? Would it be fair to infer that the weeds which appear in such abundance in our gardens spring up spontaneously because we cannot trace their seeds as they are wafted by the air? The method which Professor Tyndall has popularised has shown that the apparently homogeneous atmosphere which we breathe is dense with floating particles; the minuteness of these particles is extreme. It has been asserted that though many filters fail to intercept them, a stratum of cotton-wool is a perfectly efficient filter; but we must not entirely accept this dictum, for Pasteur long ago found that if strata of cotton-wool were arranged in series and air were drawn through them, fine particles were absorbed by many of the successive layers. As far, then, as the direct examination of the air, microscopic or otherwise, is concerned, we may conclude that there is at least as much evidence in favour of the germ theory as of the spontaneous generation theory.

It is at least as easy to conceive the existence of germs so minute that vision, however aided, fails to detect them, as it is to conceive the undemonstrable union of ordinary matter and ordinary forces to produce a living thing which, when produced, overrides the laws of both. Two objections, however, are urged against the germ theory which are of high importance. The first difficulty may be stated thus: the spores or germs of definite fungi are themselves definite: they are not of infinite minuteness; these fungi nevertheless spring up when it is absolutely certain that their ordinary spores cannot be discovered. The second difficulty is this: the organisms found in putrescible solutions are of many varieties and species; there must be definite and distinct germs for each of these. You must, therefore, concede that the air contains multitudes of them, that every square inch of it is a magazine of innumerable varieties. The air would be then encumbered with germs. These questions may be styled those of the specificity and the plurality of germs.

According to the germ theory the essential cause of each variety of fermentation is a cellular fungoid organism. The objective phenomena of fermentation are but the results of the development, growth, and acts of life of the organisms. How, then, does this theory explain the apparently spontaneous fermentations, *i.e.*, when no determining cellule has been added to the organic solution? It is proved that a yeast cell can form in a saccharine fluid precisely as other varieties of organisms arise in other putrescible fluids. The fluid in which yeast cells float, and which contains no formed cells whatever, is exceedingly efficient in inducing saccharine fermentations. It follows either that those cells arise from molecules infinitely more minute than the cells themselves, or else the germ theory cannot stand. The careful researches of Dr. Beale have shown that molecules capable of development into perfect yeast cells are of extreme minuteness, many of them being much less than 1-100,000th of an inch in diameter. Such germs as these are readily capable of being wafted by the air and could defy the closest means of detection.

It was long ago ascertained that other fungoid spores than those peculiar to yeast could, in a manner precisely similar to the latter, induce fermentation.

Penicillium cultivated upon lemon gave rise to alcoholic fermentation, with the production of yeast globules, and, conversely, yeast globules were found to produce *Penicillium* as well as *Mycoderma*. It is obvious that this observation of the mutability of species in fungoid organisms is of the highest importance. The apparent structural characters of the spores and the subsequent developments and fructification of *Mycoderma cerevisiæ* differ widely from those of *Penicillium*. If they are thus mutually convertible, it would annihilate the argument advanced against the existence of atmospheric germs on the score that, to explain the different varieties of forms produced, one must concede the existence of an immense number of germs in the air of different kinds. This would tend to show, on the contrary, that the same germ under different conditions could assume the appearances and the functions of a different organism.

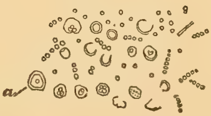
The most elaborate investigation of recent date bearing upon this subject has been made by Professor Hallier, of Jena.

According to Hallier, the same germinal molecules develop according to the nature of the fermentescible substances in which they are deposited into the fungoid forms peculiar to each fermentation. The forms inducing putrefaction, fermentation, and mildew are all varieties of one another. As they develop within fluids they are cellular formations. When they grow upon the surface only do they present fructification (*Schimmel*). Hallier endorses Pasteur's view that the germs of all are carried by the air. The most abundant source of germs appears to be *Penicillium crustaceum* (see fig. 2), whose spores are universally

Fig. 2.

Fructification of *Penicillium crustaceum*. (Hallier.)

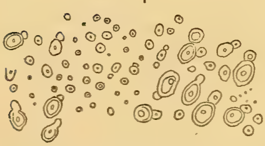
Fig. 3.



Spores of *Penicillium crustaceum* bursting in water and setting free their contained particles, which unite in rows or chains. (Hallier.)

spread because it is more hardy, more fertile, develops at lower temperatures, and grows and fructifies more rapidly than others of its kind. It will accomplish its growth and will bear fruit in forty-eight hours. A so-called spore of *Penicillium* falling into watery fluid bursts into a multitude of particles, each of which may be the radicle of a living fungus ; the minute particles approximate and unite in twos, forming a double cell ; moreover, they subdivide excessively rapidly, "so that the number produced can scarcely be expressed" (see fig. 3). The minute particles then unite in chains, constituting *Leptothrix*, which is not a species but a form of vegetation common to many species. In pure water development can go no farther, nor after a few hours do the organisms continue to be formed; for further development the presence of a nitrogenous substance is necessary. The minute spherules (*micrococci*) are the special ferments of putrefaction ; in the presence of sugar the spherule enlarges and becomes a nucleated cell (*cryptococcus*) identical with the yeast-cell (see fig. 4).

Fig. 4.



Cryptococcus in various grades of development from *Penicillium*. (Hallier.)

Fig. 5.

Milk ferment.

A very similar change takes place in oil fermentation. In milk the *micrococcus* elongates and forms parallelopipeds or staff-like cells (see fig. 5) ; in acetic fermentation the cells assume a lancet shape (see figs. 6 and 7). These ferments increase by division, and are classed by Hallier under the term *arthrococcus*. If we can accept the teaching of the foregoing evidences, fermentation and putrefaction are both due to the influence of a single agent —vitalised matter—which is transported from place to place by the air. We must believe that ordinary air contains minute masses of living matter—call these particles germs, germinal matter, protoplasm, bioplasm, or what we will—that they are derived from the fructification of fungi which can spring up wherever nitrogenous organic matter is in contact with air ; that from such fructification it is not the visible spores but minute fragments of these which are the first causes of the subsequent decompositions ; that these living molecules grow wherever they find a soil meet for them, and in different soils develop into different forms, and produce by their vital acts different effects.

It now appears that none of the objections hitherto urged are fatal to the germ theory. Chief, however, amongst the arguments adduced against it has been that deduced from the results of destructive agencies.

At a very early period in the history of the controversy concerning the origin of life, investigations were made with the intention of determining this question. Can vital organisms be found to develop under physical influences which are, under ordinary circumstances, fatal to any living thing? If so, there is a great difficulty in the way of accepting the germ theory. The heterogenists have argued that living bodies *are* discovered after processes

FIG. 6.

Acetic ferment in course of development. (Hallier.)

FIG. 7.

Acetic ferment from the surface of stale beer.

of physical destruction which it is *impossible* for life-possessing matter to withstand; therefore, by exclusion, spontaneous generation must occur.

The necessary conditions for experiments of this nature are—(1) the subjection of fermentescible matter to such destructive agencies that vitality in it is impossible; (2) the prevention of any subsequent contact with it of an agent which may be a possible vehicle of vitality.

The agency usually employed for the destruction of all possible life in fermentescible fluids has been heat; the precautions against the admission of possible germs or germ-laden air have been various. The most simple of these processes has been the subjection of the fermentescible fluid to heat, varying in intensity in different experiments, and its preservation in sealed flasks whence air was as rigidly as possible excluded. The originator of this method of investigation was Needham. Since his time it has been put in practice with varying methods for the exclusion of possible germs after the heating of the fluid. The temperatures employed have varied in different experiments from boiling for periods ranging from a few minutes to six hours (Pouchet, Mantegazza, Child, Bastian) to 153° C., or 307·4° F. (Bastian). The means of excluding possible vitality subsequently have been passing the air through a tube raised to a red or white heat (Pouchet, Wyman, and others), or through a tube containing strong sulphuric acid (Pouchet, Joly, Musset), or supplying only oxygen (Pouchet, Child, Mantegazza), or artificial air (Pouchet, Child), or by preserving the boiled putrescible fluid *in vacuo* (Pouchet, Bastian). In all these cases organisms have been described as resulting. In these experiments it would appear that the conditions would be fulfilled with the least liability to objection in the cases wherein the solutions were exposed to very high temperatures *in vacuo*. M. Pouchet filled a flask with a decoction of malt which had boiled for six hours, and hermetically sealed it. After six days a deposit, apparently of yeast, took place in the flask; on the seventh day the external temperature having been suddenly raised the flask burst with a loud report. There had been fermentation, and the microscope disclosed that yeast corpuscles had been formed. Dr. Bastian has also performed analogous experiments with great care. In his first series, the fluids were boiled in small flasks for ten to twenty minutes, and then by means of the blowpipe hermetically sealed; after periods varying in the different experiments from five to thirty-nine days, the flasks were opened, and the contents were examined with the result of the discovery of many lowly-endowed organisms. Living bodies were also found when solutions of tartrate and phosphate of soda and some other saline solutions were in like manner made the subject of experiment. It has been supposed that these observations are very antagonistic to the germ theory, and lend colour to the views of heterogenists. In briefly considering this conclusion we may divide the arguments into two classes, the first being derived from the evidence of the supposed destructive influence of high temperatures on living matter, the second from the supposed impossibility of the pre-existence of germs in the fluid employed. It must be recollected that by the argumentation in favour of heterogeny the method of exclusion is employed, and this requires the most prolonged and patient employment before its conclusions can be said to approach finality. Furthermore, its upholders must be prepared to substitute for the explanation of phenomena which may be derived from any other theory another which shall be more satisfactory.

Let us, in the first place, accept as proved, by the results of the experiments, that living forms have been discovered after the subjection of the fluid containing them to a temperature of 307·4° F.

The affirmative proposition necessary to establish the heterogeneous evolution of these organisms is that no living thing can in any state and under any conditions withstand a temperature of 307·4° F., without annihilation of its vitality.

It is probable that this would, according to their preconceived notions of the properties of living bodies, be at first sight adopted by most people.

It is difficult, *à priori*, to believe that living matter could resist such a seemingly powerful cause of destruction; but it behoves us to consider the subject, not solely according to our preconceived ideas, but to inquire as to the properties and the behaviour of living matter from observation below the surface of ordinarily visible nature. And here we find many seeming paradoxes: we find that it possesses powers of persistence and of resistance, which would certainly, *à priori*, appear impossible.

First, as to persistence. We know that vitality may lie dormant for a period which is almost inconceivable. Stramonium seeds, according to Duhamel, can develop after remaining twenty-five years under ground. Friewald observed the germination of melon seeds after they had been kept more than forty years. Pliny asserted that corn grew after it had been kept 100 years. Desmoulins obtained plants from seeds found in a Roman tomb of the third or fourth century. Finally, it is well known that corn found in some of the tombs of ancient Egypt has germinated and grown to perfection, and that a squill-bulb, found in the hands of a mummy has, when planted in this age, and in this country, grown and blossomed. In these cases it can scarcely be questioned that, remarkable as it may seem, the vitality (or term it what we will) of the various germs has slumbered during the protracted periods indicated.

The proposition that there have been in these cases actual death and subsequent reviviscence cannot be seriously sustained, least of all by those who uphold the heterogeneous evolution of living things according to definite and progressive stages by which the identical original form could only by a miracle be obtained. Concerning lowly-endowed organisms, Claude Bernard taught that "infusoria carefully dried lose all vital property, at least in appearance, and can remain thus for whole years; but when water is restored to them they re-commence their life in the same manner as formerly, provided a certain degree has not been overpassed in the desiccation."

These facts alone are sufficient to show that we must rely on no *primâ facie* or surface ideas for our conception of the nature and properties of those bodies which possess vitality.

Then, as regards resistance on the part of living organisms, especially with regard to temperature.

First of all, we have *direct* evidence as to the influence of temperatures on low organisms. We have abundant evidence that they can support very low temperatures, very high temperatures, and very rapid alternations of temperature. Bacteria and monads survive a cold of 23° F. for an hour, and often 5° F. for a few minutes. The lowest organisms "possess for the most part," says an ardent supporter of heterogeny, "a resistance often surprising to heat and cold." An experiment of M. Pouchet shows even that certain of them can easily support sudden changes of temperature, even a rapid transition of 100° C. In the hot Geyser springs which reach nearly to the boiling temperature, unicellular plants have been found growing. Practically, however, the extreme limit of heat which it is found that *developed* organisms can bear in the presence of water has been fixed at 100° C., *i.e.*, the temperature of boiling water, and this by the upholders of either side. In dry air, organisms are capable of withstanding a considerably higher temperature than when they are contained in fluids.

In the ovum and spore condition, life-possessing matter, according to all the evidence, possesses a higher power of resistance than obtains in case of the developed organism. The zoosperms of the frog can retain their vitality in a cold not exceeding 24° below freezing. M. Payen determined that the sporules of the *oidium aurantiacum* resisted a moist heat of 248° F., and only lost their faculty of germination at a heat of 284° F. Pasteur asserted that spores of mildew *in vacuo* or dry air were fertile after exposure for twenty minutes to half an hour to a temperature of from 248° F. to 257° F., but he concluded that exposure for more than twenty minutes to 260° F. to 266° F. completely destroyed their vitality. It is thus seen that organised material in the embryonic condition is capable of resisting temperatures which seem at first sight almost impossible. An argument to the contrary has, however, been urged from the behaviour of known and recognised spores under such conditions. Pouchet observed that the spores of *Ascophora*, *Penicillium*, and *Aspergillus* were completely *disorganised* by being boiled for a short time in water. So, also, Bastian showed that a fungus and spores heated in a sealed flask for four hours to 153° C.; that is to say, treated in precisely the same manner as the solutions which yielded him, in the case of infusions, evidence of life and organisation, were completely disorganised. "Not a single entire spore could be found; they were all broken up into small, more or less irregular, particles." But it behoves us to inquire further as to the signification of this disruption. We can quite agree that by the influence of the heat the spore is torn asunder and dissipated into fragments, but it is another thing to assert that such fragments are bereft of all vital property. Observations have all tended greatly to show that our notions concerning a "spore" must be modified. It is not, like the seed of a phanerogamous plant, the nucleus of a single organism, but a collection of extremely minute individual particles, each of which may become a definite organism. Though, therefore, we may agree that there is an apparent disintegration of the visible spores, we need not subscribe to the view that every one of the individual particles succumbed to the destructive influence. Disruption need in no way connote destruction. The divided polyp is not destroyed, but its fragments grow into fresh organisms, and a dismembered portion of a plant can become an individual tree. We may agree that the spores have lost the power of reproducing the plant whence they were originally derived, but we know also that the surroundings and the conditions of pabulum are greatly changed by the influence of the heat, and this may be a sufficient explanation. One observes when mildew grows upon organic matter, that though a certain species (say *Aspergillus*, for instance) may be shedding its spores in all directions, these do not spring up as successive crops of *Aspergillus*—a species totally distinct in form succeeds it, and so on through the generations following. We are not bound to believe, therefore, from the apparent evidences of physical destruction, that every particle is rendered lifeless; certainly we cannot conclude this from *à priori* grounds. To say that because higher forms would have lost vitality, therefore lower or embryonic forms must lose it likewise, would be equivalent to estimating the power of resistance to physical influences of a spermatozoon from the power of resistance of a developed animal.

Dr. Bastian asserts that the vitality of vibrios and bacteria is destroyed by the boiling temperature: when infusions containing active bacteria and vibrios are boiled, the result is the disruption of the vibrios and the disappearance of all signs of life in the bacteria. "All their peculiarly vital acts have at once ceased, and they have henceforth displayed nothing but mere Brownian movements." It must, however, be remembered that such diagnosis of vitality is purely arbitrary. In the course of putrescence, no one can tell when movements cease to be Brownian and commence to be vital; conversely, when the special bacterian movements have been caused by heat to cease, no one could assert that the movements of the *débris* were purely mechanical; or, if all movements had ceased, it could not be positively stated whether vitality had been annihilated or only paralysed.

In face of all the facts it cannot be said that the heterogenists have proved their case, that subjection to a heat of 153° C. is an absolute test of the absence of vitality. When we see the extraordinary powers of persistence and of resistance of life-possessing matter, any single test which we may impose, unaccompanied by collateral evidence, cannot satisfactorily prove the absence of vitality. The very experiments themselves, which are supposed to prove the impossibility of vitality by the stringency of the adverse influences employed, demonstrate how vital matter defies such adverse influences. Could one predict that, even if low forms could originate, complete fungi could grow and fructify in the conditions of vacuum and of pabulum which would be profoundly altered by the exceedingly high temperature? But not only so; in some of Dr. Bastian's experiments the growth of organisms seems to have been *favoured* by the conditions. As, therefore, with phanerogamous plants there is a wide range of temperature-conditions most befitting the perfection of different species, so there is no reason for denying to lower forms a wider range than our *à priori* views would have led us to imagine.

We will now turn to the second part of the argument advanced in favour of the heterogeneous evolution of the organisms found in these conditions, viz., the impossibility of the pre-existence of living matter in the materials employed in the experiments. The chief evidence in this direction is adduced by Dr. Bastian from his experiments with saline substances. In examining crystals of the neutral tartrate of ammonia, Dr. Bastian found in their interior positive evidences of fungoid germs. Far from this being an argument in favour of the transformation of crystalline into living matter, many will consider that it lends weight to the germ theory. Upholders of the latter assert the universal presence of germinal molecules. Dr. Bastian shows them to exist in crystals by physical demonstration. That once in a saline solution such vital organisms can grow and propagate is no matter of doubt—it is admitted by either side. Pasteur showed that a salt of ammonia and the phosphates could become a perfect pabulum for torula cells. The oxalates and phosphates have been shown by Hallier to be excellent media for the development of fungoid fibres, oidium forms, mycoderms, and fructifying organisms. The whole difficulty rests, therefore, with the earliest phenomena.

It behoves us to inquire into the teaching of the negative as well as the positive results. That the proliferation of forms of life under restrictive influences such as have been mentioned is exceptional is taught by the results of every labourer in the field. Privation of air is admitted to be a most potent cause in prevention of the appearance of organisms: or to put it in another way, "experiments in closed vessels are quite unfavourable to the demonstration of heterogeny, because the natural and regular progress of the phenomena is paralysed." Increment of heat is another adverse influence, "because the destructive agent impairs without destroying that organising force which is an essential property of organic matter." These are the modes of expressing the facts adopted by the heterogenists; and MM. Joly and Musset, by actual experiment, prove that the organisms in a putrescible solution submitted to ebullition "are more simple and less numerous accordingly as the ebullition is prolonged." In fact, we must conclude with M. Pennetier "that the phenomena of spontaneous heterogenic generation, intense while their regular course is respected, are nevertheless manifest, though in successively lessening degrees, as causes of difficulty are increased, to finally cease when the phenomena of fermentation and putrefaction are themselves prevented." This as an *explanation* of the position to which science has arrived in this question it is scarcely needful to say is feeble in the extreme; it is merely a categorical expression of results which can be explained at least as well by the one theory as by the other.

From the foregoing data it appears that none of the arguments adduced are sufficient to invalidate the germ theory. There is a mode of putting the question which must appear to many to be unfair. It is alleged that the onus of proof rests entirely with the Panspermatists. It is not for the heterogenists to prove that germs do not exist, but for the Panspermatists to prove that they do exist. "The charge of proof in science rests with those who allege a fact." Surely the facts support precisely the contrary view.

The positive proposition '*omne vivum e vivo*' is asserted by all the evidence of visible nature, and by microscopic research, to those confines beyond which human powers cannot reach. That there is an exception to the seemingly universal law in the case of those organisms which are invisible it must be the duty of those who embrace the theory of spontaneous generation to prove, or else to tolerate becomingly the scepticism of others. It behoves us seriously to weigh the only real objection to the reception of the germ theory—the resistance to the destructive agency of heat, and to inquire as to the effects of other physical agencies which may contribute to a solution of the question. It is not by the results of a single method of investigation that this question is to be judged, but rather by the collective evidence of many methods.

Heat is not the only destructive agency which may be employed in the inquiry: others, fraught with much valuable teaching, may be put in force, though these have been apparently in the recent controversies entirely ignored. Such are the evidences derived from the destructive influences of chemical and of poisonous agents. It has been known from time immemorial that the addition of certain compounds prevents both putrefaction and fermentation. The belief being that these processes were essentially chemical, it was naturally probable that the agents which suppressed them should be susceptible of a chemical classification: but the infinite variety and opposite properties of the various agents precluded this classification. If the processes were, as asserted, those of oxidation, it would surely be not unreasonable to expect that the agents which arrest them should also arrest oxidation; but common experience taught an absolutely contrary lesson—that *oxidising* agents were the most efficient in arresting the processes. Again, on the chemical theory there ought to be some quantitative relation between the amount of a chemical agent employed and the degree of its influence; but the fact is that an agent present in such feeble quantity as to be capable of no appreciable chemical effect on a mass of putrescible material is yet capable of stopping all putrefaction. Furthermore, agents, such as carbolic acid, which are proved to exert no influence whatever on processes purely chemical, are among the most efficient of all means for preventing putrefaction and fermentation.

A large series of observations shows, on the other hand, that the agents arresting these processes exert their influence precisely in so far as they are *poisonous* agents to low organisms. If at any stage of the process these microscopic organisms are rendered lifeless, the process, with all its attendant phenomena, ceases: on the other hand, the overt signs grow with their growth, strengthen with their strength, subside when they languish, and cease when they die.

Hitherto experiments have usually been made with the view of ascertaining the effects of antiseptic agents when mixed with putrescible material; but the author has attempted to ascertain the results which occur when the air alone is influenced by certain agents, the materials being left intact. In this way the evidence of the results obtained from experiments with *heat* may be tested by the evidence of other agents of vital destruction.*

The results obtained may be thus briefly summarised:—

1. Putrefaction, mildew-formation, and the appearance of organisms can be checked or absolutely prevented by the existence of certain agents in the air supplied to a putrescible body.

2. The power of such agents can in no sense be measured by their chemical constitution or characters. From many experiments the following expresses their order of efficiency from weakest to strongest:—(1) chloride of lime; (2) sulphurous acid, ammonia, sulphuric ether; (3) chloroform; (4) camphor; (5) iodine, phosphorus, creosote, carbolic acid.

3. The agents which stop fermentation are vegetable, not animal, poisons. Fungi will grow in the presence of hydrocyanic acid and of strychnia.

4. Comparative experiments show that a given volatile agent is far more efficient when it is contained in the air supplied to a putrescible solution than when an equal quantity is mixed with the solution itself.

(5) All fungoid organisms can be prevented by the presence of a minute proportion of creosote, carbolic acid, ammonia, hydrochloric acid, or sulphurous acid in the air, though beneath the surface of the fluid are found numerous bacteria and vibrios.

There seems to be no escape from the conclusion that the germs of fungi exist in the air and are destroyed by the volatile poisonous agent.

* See paper by the Author in the "Chemical News" (vol. xxii, pp. 241, 254), "Evidence concerning the Germ Theory of Fermentation afforded by the Action of certain substances when Suspended in the Air."

III. MOLECULES, ULTIMATES, ATOMS, AND WAVES.

By MUNGO PONTON, F.R.S.E.

PART I.

CHEMISTS have, for a considerable number of years, recognised the convenience of distinguishing the particles of chemical compounds by the term "molecules," reserving the term "atoms" for those of the chemical elements; but the time seems now to have arrived when it has become needful to make a further discrimination. This need has been rendered manifest by the results of spectrum analysis. The word "atom" conveys the idea of a particle incapable of being analysed; but the spectrum has shown the particles of the chemical elements to be otherwise constituted. Had they been simple homogeneous masses of definite size and weight, each element, when thrown into vapour and rendered incandescent, would have exhibited in the spectrum only a single bright line; because every particle of the element being precisely alike would, when in the vapourous state, and freed from all extraneous influences, have vibrated in exactly the same periods of time, and so have originated luminous waves of only one definite length and period. The discoveries of Messrs. Bunsen and Kirchhoff, however, and of other labourers in the same field of research, have shown that the chemical elements, when in the state of glowing vapour or gas, exhibit more than one bright spectral line—some of them, indeed, a large number of distinct lines of various degrees of brightness, in widely different regions of the spectrum. Even hydrogen, the element which is at once the lowest in specific gravity, and the lightest in the scale of chemical equivalents, exhibits four bright spectral lines, while iron presents a large number.

The law of chemical combination by equivalent weights appears to exclude the supposition that the ultimate particles of any element differ in size and weight—the equivalent being merely an average. For the smallest quantity of the vapour of any chemical element shows the same number and kind of lines as any greater quantity; while there is an extreme improbability that this should be the case, did the ultimate particles differ in size and weight, and were the combining proportion merely a general average of those diverse weights. The only alternative conclusion appears to be, that each ultimate particle of the element consists of numerous more minute atoms, differing in their inertia, and held together by a force too great to be overcome by any chemical means which can be brought to bear upon them for the purpose of effecting their separation. This view at least possesses such an amount of probability, that it may be fairly assumed as a basis of argument and of further investigation.

Making this assumption, then, it will be found convenient to designate the ultimate particles of the chemical elements by the term "ultimates," reserving the term "atoms" for their assumed constituents. We shall thus have the three terms, "molecules," "ultimates," and "atoms;" the first denoting the particles of chemical compounds, the second those of the chemical elements, and the third the assumed constituents of those ultimates, the atoms being themselves incapable of further analysis. The term "particle" may be applied generically to embrace all the three.

So far as observation goes, the only way in which vibrations can be excited in the infinite luminiferous ether is by corresponding vibrations in the particles of ponderable matter. On the other hand, these latter may always be excited by pre-existing vibrations in the ether itself—the one set of vibrations acting and reacting on the other. We are ignorant, however, of the manner in which vibrations may be excited in the particles of ponderable matter, apart from any pre-existing vibrations in the ether, or from the conversion of progressive into vibratory motion by collision. Electricity is ostensibly one kind of force which may be available for this purpose; but then we know not to what extent the ether associated with the ponderable particles may be needful to the existence of the electricity, although we do know that it can neither exist in the free ether, nor pass through it without the help of ponderable matter. Waiving this question, let it be assumed that the vibrations in the particles of ponderable matter are excited by some adequate force—some kind of motive energy.

The amount of vibratory motion assumed by the particles will depend—(1st) on the degree of motive energy applied, and (2nd) on the inertia of the particles—their resistance to the applied force. This inertia is of two sorts, (1st) intrinsic, (2nd) adventitious. The intrinsic is that possessed by each particle in itself, independently of its relations to other particles with which it may be associated. The adventitious inertia arises out of those relations. It is the immobility which any particle possesses in virtue of the forces exerted on it by other neighbouring particles. The intrinsic inertia predominates in gases and vapours, the adventitious in solids and liquids.

Incandescence begins in all solids and liquids at precisely the same temperature. A red heat or a white heat indicates the same temperatures whatever be the nature of the incandescent substance, provided it be either in the solid or liquid condition. But then it requires very different amounts of applied motive energy to raise different substances to the temperatures indicated by those phenomena of incandescence. This amount depends on the specific heat of the substance, or the inertia of its particles. Thus the motive energy required to raise water to the temperature of a red heat is about 33 times the amount requisite to raise an equal weight of mercury to the same temperature; yet both substances would exhibit the same tint of redness. Part of this difference may be due to the greater space over which the motive energy is diffused in the case of the water, which has upwards of 13 times the bulk of an equal weight of mercury; but for equal volumes the water still requires about two and a half times more applied energy than does the mercury. Both substances being in the liquid condition, this difference can be most readily explained on the supposition that the molecules of water have a greater intrinsic inertia than have the ultimates of mercury.

It is the adventitious inertia, however, that is most prominently displayed in the case of incandescent solids and liquids. The difference in the colour of the light which they exhibit is an exact measure of the degree of rapidity of the vibrations communicated to the luminiferous ether by those of the ponderable particles. The two sets of vibrations must be synchronous; and as we can determine the rate of the ethereal vibrations appertaining to each tint, so we can ascertain the rate of the corresponding vibration in the particle by which the ethereal vibration is generated.

In all solids and liquids the force of cohesion by which the particles are held together must act on them very unequally, according to their position in the mass. Those at and near the surface are almost entirely freed from the operation of that force in one direction, while on those within the mass the cohesive attraction acts in all directions. The latter must, therefore, have by virtue of their position a greater amount of immobility than the former, and they will remain at rest after the former have been set in motion by the applied force, or at least they will more slowly acquire their proper rate of vibration. When incandescence begins, it is only the most movable particles—those at and near the surface—that vibrate with sufficient energy to generate luminous vibrations in the ether. They are driven to a comparatively considerable distance from their points of rest, and the force by which they are dragged back again being comparatively weak, they take a longer time to perform their excursion. They accordingly generate only red waves in the ether. The light is feeble, and the spectrum it exhibits is chiefly confined to the region of the fixed lines A and B. As the motive energy increases, those particles situated a little farther in from the surface begin to vibrate at their proper rates. Their excursions are performed in a more limited space. The force by which they are dragged back to their points of rest is greater, consequently their periods of vibration are quicker. These give rise to orange and yellow waves in the ether, and the spectrum gradually extends towards the line D. A still greater energy sets more of the particles into violent motion. These perform still quicker vibrations, in virtue of their greater adventitious inertia. At last the light becomes quite white, and the spectrum extends continuously from the extreme red to the extreme violet. The particles of the substance are then vibrating at every rate of rapidity embraced within the limits of the visible spectrum, and also at various other rates both slower and quicker than these. This variety of rate cannot be due to diversities in the intrinsic inertia of the particles themselves, and can be explained only by differences in their adventitious inertia, arising from their position in the mass and the various degrees in which they are influenced by the force of cohesion.

When a substance is thrown into vapour, the operation of the cohesive force is suspended, and the molecules or ultimates are held together merely by the force of gravity. They are free to perform larger excursions; consequently, while the amplitude of their vibrations is increased, their rapidity is so much lowered that they cease to propagate through the ether vibrations of those rates which fall within the limits of the visible spectrum. It is not till the applied motive energy becomes greatly augmented that a vapour begins to propagate luminous vibrations, and these are then found to be no longer capable of exhibiting a continuous spectrum, but only definite bright lines—showing that there is an entire alteration in the mode of their production. The same remarks apply to permanent gases.

In some few instances incandescent elemental vapours exhibit, in addition to their characteristic bright lines, a diffuse, weak, more or less continuous spectrum. The most probable explanation of this phenomenon is an imperfection in the vapourisation. Minute incandescent solid or liquid particles may be carried up in intermixture with the vapour, and give rise to a weak continuous spectrum. At least it would not be easy to find any other more feasible explanation.

Confining attention to the definite bright lines, and assuming these to be due to the vibration of the atoms constituting the chemical ultimates of the gas or vapour, it is plain that these atoms, while very close to each other within the limits of the ultimate, cannot be in absolute contact, otherwise they would be incapable of separate individual vibration. They must be held together by a very powerful atomic attraction ; but they must be simultaneously kept apart by a correspondingly powerful elastic or repulsive force which prevents their passing into absolute contact. This elasticity can be no other than that of the luminiferous ether, which, probably in a compressed state, must intervene between the atoms and keep them asunder, balancing the atomic force at a certain minute distance.

The nearest analogy to such a condition is that furnished by the extreme points of two similarly magnetised fine sewing needles. Suppose the points to be both north poles, and one of the needles to be fixed in an upright position with its point uppermost, while the other is suspended over it with its point downwards. In the absence of the magnetism, the suspended needle would, by the force of gravity acting alone, be attracted into a position exactly over the other, so that the two needles would form a straight vertical line. But the magnetic repulsion subsisting between the two points will prevent their assuming this position. The upper needle will become slightly inclined to the other, the two points being maintained at a certain distance apart, depending on the strength of the magnetism. In this case, the magnetic repulsion acts the part of the elastic ether, while the terrestrial gravitation performs the office of the atomic attraction in the other case. But the atomic attraction must in its intensity approach very nearly to that of the repulsion exerted by the ether, so that the two forces may balance each other. Were the repulsion exerted by the ether much in excess of the mutual attraction of the atoms, these could not approach so near one to another as they do. Were the attraction in excess of the repulsion, the latter could not prevent the atoms from passing into absolute mutual contact.

If such be the constitution of the chemical ultimates, it may not be improbable that, in those cases in which the bright spectral lines are very numerous, part of the effect may be due to the various degrees in which the atoms composing the ultimate are influenced by the atomic attraction, instead of its being wholly due to differences in the intrinsic inertia of the atoms. The diverse rates of vibration may be partly owing to mere differences in the position of the atoms within the limits of the ultimate, as in the case of incandescent solids and liquids. Among atoms having the same intrinsic inertia, those at and near the surface of the ultimate will have slower rates than those in the interior. Indeed, each ultimate may be regarded as a minute incandescent mass, whose atoms are vibrating at different rates—only the rates are much less various than in the case of solids and liquids; so that only vibrations of certain definite rates are communicated by the atoms to the ether—giving rise to the definite spectral lines. Doubtless, however, where these lines are few in number, differences in the intrinsic inertia of the atoms themselves are probably more efficient in causing diversities in the rates of vibration, than mere differences in the position of the atoms relatively to each other, within the limits of the ultimate which they unite to constitute.

When the elementary gases or vapours act as absorbents on white light passing through them, they produce in the continuous spectrum of the transmitted light dark lines, exactly corresponding in position to the bright lines which they themselves generate. This effect cannot be explained otherwise than by the transference of the vibratory motion passing through the ether to the particles of the gas or vapour. But were it transferred to the chemical ultimates in their integrity, they being all of the same weight in the same element, could take up only the vibrations performed at one single rate or some octave of that rate. The circumstance, however, that they absorb definite lines in diverse and distinct parts of the spectrum, shows that the motion must be taken up by different atoms having in virtue of their intrinsic inertia a tendency to vibrate at those different definite rates. For it seems highly improbable that one and the same individual atom, or that atoms all exactly alike in their intrinsic inertia, could select and assume different definite rates of vibration from among the vast number of rates presented in the continuous spectrum.

The appearance of the lines, whether bright or dark, is much influenced by the temperature and pressure to which the gas or vapour is subjected. Increase of temperature augments the brightness or blackness of the lines, and in some cases adds to their number, but it does not affect the breadth of the individual lines. Where it adds to their number, the new lines are always developed towards the violet end of the spectrum. They correspond to quicker rates of vibration, the same order being followed as in the case of incandescent solids and liquids. A diminution of pressure narrows and weakens the lines, sometimes extinguishing them altogether. An increase of pressure brightens or blackens the lines, and adds considerably to their breadth. When the increase of pressure becomes very great, the breadth of the bright lines becomes so augmented that they ultimately blend and form a continuous spectrum, as if the substance had become liquid or solid, although it nevertheless continues in the gaseous condition. This last phenomenon indicates a development of adventitious inertia, in virtue of which the atoms that, under a more moderate pressure, vibrated at only certain definite rates, are, under the influence of the augmented pressure, compelled to vibrate at various rates, and behave like the particles of solid and liquid bodies. The most obvious explanation of this fact is, that the chemical ultimates are really very complex in their structure. While they consist of atoms having different degrees of intrinsic inertia, in virtue of which they tend, when freed from all adventitious inertia, to vibrate at only certain specific rates, nevertheless even the lightest ultimates consist of a considerable number of such atoms; so that, when they are forced into greater mutual proximity by strong pressure, they act and react on each other by their atomic attractions in such a manner as to develop within the limits of the ultimate an inertia of position, in virtue of which the constituent atoms vibrate at a much greater variety of rates.

IV. THE GREAT PYRAMID IN EGYPT,

FROM A MODERN SCIENTIFIC POINT OF VIEW.

By C. Piazzi Smyth, Astronomer Royal for Scotland.

(Continued from page 35.)

Part II.

Collecting Observations on the Present, from which to Deduce the Past, Phenomena of Appearance and Fact.

1. *Principles of Observing.*

WHAT a relief is it always to the over-strained soul of man,—after being driven to and fro between the storms and counterblasts of opposing theories of causation, on subjects, too, where mere theories of the present day can avail but little,—when the appeal to observation begins! That is, at least, if the appeal be conducted more or less, and *mutatis mutandis*, on the same observing principles which are now so well followed out in most astronomical observatories.

I mention *them* so prominently here, because nowhere else, perhaps, among all the sciences, is the arena of pure and direct observation so far removed from that of theoretical investigation, in all that concerns its time, place, manner, means, and often the men to be employed. On all which accounts, as well as for the never ending multiplicity of things to be observed, and the readiness with which the *apparent* phenomena lend themselves as so many convenient hooks whereon the observer may at once hang and duly arrange all the numerical measures that he can ever make, a different world of thought and exertion is opened up for a time to the earnest student.

A world, too, where such person may safely let every regard for the ultimate conclusions which may be educed from all his present work remain in perfect rest, while he attends, not only to procuring the data of observation, but indelibly recording them also; with full accompaniments, too, as to their limits of probable error, from whatever cause arising. Above everything, moreover, endeavouring to free himself from past prejudices dependent on mere opinion; while seeking rather to cultivate a teachable, inquiring, learning frame of mind, as well as sufficient of the spirit of humility to perceive, that though we may be able to stand so easily on the dead of ages ago, it is a *lion* dead that we are standing on in the Great Pyramid case, and through no superior innate virtue of our own.

All this may haply form but a dull occupation to many minds; yet is it a positive duty to be performed. And if I spent five *months* in carrying it out to the best of my ability in Egypt, my readers will hardly complain if five *minutes* of their time be now occupied in hearing little more than details of numerical measures. Fully convinced, too, as they should be in their own minds, that such work is an absolutely necessary *Red Sea* of difficulty, toil, and trouble which all must wade through if they would fain reach eventually the promised land of sound theory, and thence behold some glorious rays of primeval truth concerning man, and even haply ascertain *why* these early things which we behold were *so* made and fashioned.

2. *Of the Things to be Observed.*

After the traveller has enjoyed his first and very distant view of the Pyramids of Jeezeh, from the deck of his Nile boat, or in modern times from the window of a railway carriage, as both the one and the other conveyance are nearing Cairo, and are passing a particular neck of flat, open land, just where the grand African river, which has rolled its floods from the South through so many thousands of miles of a single continuous channel, suddenly parts them into a fan-like form of many branches, producing thereby the Delta shaped land of Lower Egypt; the said traveller, I say, is rather bewildered than enlightened, and astounded more with confusion than sublimity when he *afterwards* of set purpose reaches the pyramids themselves, and actually treads the ground on which they stand.

In the distant view, both the great and the second great, and sometimes even the third great pyramid, stand up sharp and clear on the horizon line, with their triangular sides glittering towards the sun like the facets of a regular crystal, and you could then fancy any amount of refinement and finish of their surface; while the country between shows you only calm reaches of blue water, and wide expanses of greenest corn-land, with the occasional waving fronds of a gracious palm-tree, crowning a village knoll or adorning a garden grove in the nearer distance that intervenes.

But, on reaching the very pyramids by the usual road from Cairo, you have already turned your back on everything that is green throughout all the land of Egypt; the silent, and white and yellow desert is about you on every side; and those stupendous monuments of early man seem, if anything more than mouldering heaps of rudest stones, to have been built somewhat in cyclopean sport, as flights of audacious, mocking steps, some three to four feet high, each of them.

To describe these appearances fully, and, for their own sake alone, would be merely to deal in the poetic, or picturesque, and such telling effects as ruin and decay invariably import into the architectural works of man. But this sort of thing has been done often enough before; unfortunately *proves* nothing; and is not our true business here. We have come, if for anything, to search for traces of the *original* forms before all this dilapidation began. Such traces, too, there are; many, perhaps, still entombed in the rubbish of latter ages, and only to be opened up to the attentions of the scientific measurer by extensive excavations, too costly for *him* to undertake; but numerous are the traces of exactness still remaining above ground, if men would only seek them out earnestly. This, however, a few persons *have* of late begun to do; whence it has already arrived, that our knowledge of the original condition of the pyramid is much more advanced now than it was only a few years ago.

Every one knows the usual mathematical definition of a pyramid, viz., a solid whose base is a regular rectilinear plane figure, and whose sides are plane triangles, having all their vertices meeting together in a point above the base, called the *vertex* of the pyramid."

Give, then, to the base of such a conception *four*, rather than any other number of sides, and you have at once (with only a possible variation in the angle of slope of said sides) the very figure which you saw so bright, so clear, so inimitably defined, so blocked out in the solid, and yet so delicately brought up to a summit point, in the distant view. But now that you are close to it, on the hill of Jeezeh, the scientific question that arises is, to what degree of accuracy was the practical realisation of such theoretical idea really carried out, when on the vast scale of the Great Pyramid? And our answer must evidently depend, not on the general ruined heaps lying about, but on those exact traces of the original which, as I have said, *are* to be found in no inconsiderable number by those who seek for them aright.

3. *Situation of the Great Pyramid.*

Perhaps the first point in the general problem to which we should direct our attention is that of location or situation.

I have already, in Part I., set forth that the Great

Pyramid is not founded clumsily and boorishly on mere alluvial mud low down in the Nile valley, as taught by some, but is raised on the top of a broad and lasting hill of nummulitic limestone, more than a hundred feet above the level of either the Egyptian valley or the highest range of its river's floods; or more exactly, for a purpose which will afterwards appear, 1780 inches above the well-water level, and 2580 inches (nearly) above the Mediterranean Sea-level. But there is a notable manner about its emplacement even there, indicating forethought and preparation on the vastest scale; well illustrated, too, by a spontaneous and irrepressible remark of Mr. Aiton (professionally a railway-cutting contractor) when he visited my wife and self, then living on the Jeezeh hill; for he would insist, in telling workman language, on the patent evidence to *him*, after walking about every side of the Great Pyramid, that all the ground around it was "excavated ground."

Such was his technical phrase for describing what had previously struck me, viz., that the whole of the original top of the hill was artificially removed, or had in a manner been sliced away horizontally, slice after slice, until at length an admirable quality of dense, sound rock had been everywhere obtained, on which to lay the foundations of the new building with security, and to form a level area of lasting character round about it.

Within the intended compass indeed of the building itself, certain portions of the living rock had been left standing, so as to save masonry, but were only so left when of good quality, and after having been *cut* into regular horizontal steps suitably to joining on with the courses of the masonry. Even the topmost surfaces, therefore, of these internal rock portions of the pyramid, are not of the original surface material of the top of the hill before the pyramid builders began their work.

What was that original surface then?

There is a hill about a mile to the south which answers the question admirably. Its middle height is composed of horizontal limestone strata, whose transverse sections, one over the other, form a cliff towards the north; a cliff, too, so solid, so regular in its constructive layers, that one has to look long and attentively to be quite sure that it is not a vast Cyclopean wall of art and man's design. But no! it is Nature's own building, the edge only of one of her vast platforms, whose surface in this case is covered and hidden by the top of the hill, and that top is angular, rough, ragged and horribly rotten, with a geologically later limestone belonging to the tertiary period.

That then, as the near approach to horizontality in all the geological strata hereabouts shows, must have been the sort of stuff which the Great Pyramid builders had to clear away in vast quantities from the top of *their* hill also. Yet, truly vast as must have been the amount of it then removed—"excavated" Mr. Aiton would say—it was cleared away by them so thoroughly that neither Strabo, Diodorus Siculus, nor others among "the curious" of the classic ancients who went prying about precisely on such a quest, ever obtained a glimpse of, or even suspected, the real state of the case. They were, indeed, mightily troubled about certain other rubbish which their theories taught them must have existed once, viz., that arising from the chipping and squaring of the innumerable stones used in building the Great Pyramid; and they, viz., Strabo and his near contemporaries, have left us this invaluable note of the state of things in their day, that they walked all around and about the Great Pyramid, actually *looking for* the said heaps of chips and mason's rubbish, but could find none. The building seemed to them "as if it had descended from heaven complete at once, and without the agency of man,"—certainly, we might add, without the agency of the *modern Egyptian* man; for, go where you will now about Cairo, you see rubbish wholesale wherever there is, or has been, any building.

Each age of the world seems to have selected some special feature of the Pyramid to wonder at, and in every case the real full fact has subsequently been found to be still more wonderful than the portion that had first been recognised on the surface. Thus with Strabos's little wonder; so utterly incomprehensible was the complete removing of all the mere mason's rubbish at the Pyramid to him and other archæologists of 2000 years ago, that they invented the most startling accounts of the water of the Nile having been led *uphill* to wash away heaps supposed to be impossible to man.

Yet 2000 years more in the passage of time have revealed not only where all that rubbish was neatly stowed away by the hand of the builders, but the much larger amount also which arose from their masterly preparation of the summit of the hill for the honourable burden it was in future to bear. For both supplies may now be seen—detected by the differential effect of occasional rain through so many ages on their loose heaps, as compared with the compact natural rock—banked away against the northern cliff of the hill. Banked up too, so firmly against it, and so nicely levelled and smoothed on the top, as to have made for 2000 years at least an undistinguished extension towards the north of that flattened area of worked rock surface on which the Pyramid immediately stood.

An extension, however, that was in so far decidedly the right thing in the right place; for, so close is the Great Pyramid to the northern brink of the hill, as almost to threaten danger to its foundations on that side. A safer site might have been found further south and west on the same hill, or on the southern one, or on the so-called Arabian hill on the eastern side of the Nile,—whence, too, the builders had, even as it was, to derive all their best qualities of stone for facing the outside of the Pyramid and lining its interior passages. But they preferred that spot, whereon the Great Pyramid now stands, with its one only passage of communication with the exterior air looking right away northward from the very brink of the cliff, without any interruption, over the fertile delta land of Lower Egypt.

Now this opportunity of free and open view in that direction would not have been commanded from any of those other sites; and the Great Pyramid has wholly, if not in a manner visibly, arrogated this particular and *unique* site to itself after a most remarkable fashion. For all the other pyramids of Egypt are, by their places, kept away southward of it, or behind it, in its lee as it were, forming a ruck of lame and impotent ones; or a servile suite of attendants waiting silently on their Lord, while he advances in calm and single majesty to indulge in his own thoughts as he gazes northward from the end of the high rock, where it looks down into, upon, and over the river-formed and populous, triangular plain. One unfortunate, ill-omened, pyramid was indeed begun in advance of the imperial monument of the land, or about six miles to the N.N.W. at a place now called Aboo-Roash. But never did exception better prove a rule; for *that* pyramid has remained ever since miserably unfinished; and, whatever the reason was, all men may go to the place now,—trace by the large ground plan, and steep angle, the ambition of the design,—but on looking at the few feet of height accomplished, and the small portion of building material lying about, they may say with supreme significance, and with as much certainty as though they had been eye-witnesses of what took place 4000 years ago, "This man began to build and was not able to finish."

Thus much any one can see for himself with his own natural eyes, standing in front of the Great Pyramid and looking northward over the flat country which is spread out diverging before it, as if for its pathway to larger power. But the good fortune was reserved to Mr. Henry Mitchell, Chief Hydrographer on the United States Coast Survey, to show further, from maps and charts, that the position of the Great Pyramid is at the very centre of origin of both the mathematical figure, and even physical formation, of the whole delta-land as an angular 90° *sector* in shape; also that the Great Pyramid's central meridian line, or the plane of its one entrance-passage continued, forms the meridian line of all Lower Egypt, marking precisely the longitude of the northernmost and culminating point of its regularly curved northern coast; while the N.W. and N.E. diagonals of the building produced, similarly define the two sides of the delta, stretching though they do far beyond eye-shot from even the summit of the building; and all the fan-like system of streams which irrigate that most fertile of level lands, all, when viewed from without, point convergingly towards the Great Pyramid as their one centre of command.

Mr. Mitchell, indeed, working from the convex curve of the northern coast-line on the charts to find the centre of formation thereof, some 120 miles inland, confesses that his arcs and radii do not pretend to distinguish between the Great Pyramid and its immediate fellows on the hill of Jeezeh, though that general group they do determine. But the moment we visit the hill itself, thus centralised, there is not a particle of doubt left then as to which is the master building there, viz., the Great Pyramid,—for it alone, as just described, stands over the sector's circle's centre, and possesses the one commanding prospect, extending, without interruption, from its very foot illimitably away towards the north, and north-east, and north-west.

Even further still, too, as Mr. William Petrie has lately shown by careful calculation,—the actual, *i.e.* the adjusted, height of the hill around the base of the Great Pyramid, *also* marks for Physical Geography that precise hypsometrical level to which the whole ocean surface would rise, if every particle of presently sub-aërial land were to be distributed over the bottom of the sea; and which surface of such terraqueous globe is that to which geodesists should refer all their measures for the true and full size of the earth.

"Never," therefore most truly may Mr. Mitchell exclaim, "never was there any building founded by man on so important a physical site," or with the whole surrounding country serving in all its natural features merely as a chorographical pointer, on the grandest dimensions, to the said building's eternal place of standing on "the utmost bound of the everlasting hills," during all human history.

Nay, indeed, there is even yet *more* of hard scientific fact to distinguish the position; for, on recently summing up the areas and positions of all the land-surface of the entire globe,* it has been found that the general superficial centre thereof (*i.e.*, of the *land surface* or that which mainly constitutes the greatness of human empire and the numerical strength of nations), such centre,—when measured both in latitude and longitude, and defined also by the crossing of the longest line of land surface in latitude and the longest in longitude,—comes, in either way, close to the same identical Great Pyramid site. No other natural position, therefore—if required now to be sought for whereon to establish a central anthropological monument for all the human race through all historical ages—could be found, other or better than this remarkable one in Lower Egypt on which the Great Pyramid is already, and has been so long, established with such care and precision in all particulars.

4. *Shape of the Great Pyramid.*

The levelling of the rocky hill top and the production thereby of a white and smooth flat surface of a half chalky but compact limestone, was of course a famous preparation for laying off any mathematical figure with exactitude, even when of the grandest dimensions; and it was done.

At the north-east, south-east, and north-west angles of the huge square base of this pyramid, the corners are marked by flat-floored, shallow, rectangular sockets to hold the colossal corner foundation-stones of the whole building. But at the south-west angle, where, too, the socket, though dug for through the modern superincumbent rubbish in vain by Mr. Inglis (Mr. Aiton's assistant) and his Arabs, including amongst them two "sheiks of the pyramid," was only at length discovered when I lent them the aid of the late John Taylor's theory, by measuring his computed angle from the less dilapidated summit of the building, and showing them thereby on the ground below, where the outside corner of ancient times *ought* to be. There, then, when that long-lost socket was thus re-discovered in April, 1865, after a repose of 900 years, was its floor, cut methodically into the fair white rock, and levelled so accurately as

* See "On an Equal Surface Projection for Maps of the World," by C. Piazzi Smyth. Published by Edmonston and Douglas: Edinburgh, 1870.

to be nearly without error for placing a modern altazimuth instrument upon; and there, too, were the chiselled-out, elevated borders of the socket on three sides, but on the fourth, only a line, an indented line about 0·08 inch broad and 0·02 deep, ruled on the flatted rock surface: but a line so straight, so true, so steady, and so firm, that it proved itself at once the work of a master hand.

We could, moreover, hardly fail to recognise whose hand it was, so precisely similar appeared the line in all its finer graphiological features to two other lines which I had already discovered on the walls, far down the dark vault of the entrance passage of the Great Pyramid; lines introduced there, too, in a manner that showed they must have been the veritable work of the original builders, and were well adapted to call attention to a singular change required at that point, for apparently symbological purposes, in the angular direction of the joint lines on either side.

Thus closely, then, have we of the present day all four corners of the ancient square base still accurately defined; though, alas! for modern science and scientists too, not yet measured by them with sufficient exactness. For, although the greatest linear error, in terms of one of the sides as unity, has been given by the only series of socket measures yet published in full,* as being not more than 0·001, still the greater part of that quantity may be shrewdly assigned as the "probable error" of the observations themselves. And similarly with the only observation yet published of the angle at one corner included between two adjacent sides, and which came out within 0·0002 of the required right angle.

The base, therefore, of the Great Pyramid is, to fully as close as modern measures have yet been carried, a sensibly perfect square; and it only remains to ascertain, for the due definition of "shape," what is the angle of rise of the triangular sides, or flanks, of the building, from the four edges of the said base.

This angle, the builders could make anything that they pleased under 90°, and their structure would have still been a true mathematical pyramid. Or, again, they could also, if they pleased, though in that case to the destruction at once of the pure mathematical idea, have begun by raising up from the base an ornamental rectangular pedestal, with vertical sides, fluted mouldings, and other trickery of the

* See "Life and Work at the Great Pyramid," by C. Piazzi Smyth. 1867, vols. ii. and iii.: Edmonston and Douglas, Edinburgh.

art architecture of subsequent days and human schools; as in fact, too, some latter-day writers, thinking no grand architecture possible without ornament for ornamentation's own sake, have evolved out of their own internal consciousness and applied to the Great Pyramid. But Colonel Howard Vyse's happy discovery of two of the lowest row of that pyramid's original outside casing-stones still *in situ* near the middle of the northern side of the base, and beginning their angular slope at once from the broad flat "pavement" there, has demonstrated the Great Pyramid's general form to be decidedly such as to illustrate an everlasting geometric truth, rather than merely to adorn an architectural fancy of the passing hour.

The Colonel's discovery gave also an excellent opportunity, so marvellous were both the original exactness of the workmanship and the perfect preservation of those great bevelled blocks of stone, to determine their precise angle of shape; and he made it by two different methods,—not, however, without a certain rather puzzling small error therein,—to be somewhere between 51° 51′ and 51° 52′.

This was, neverthless, as compared with all his predecessors, most respectable work; and yet when its mean result was employed soon after, as giving the Great Pyramid's whole angle,—certain London critics exclaimed, and not without some wit, that that was rather too exaggerated a case of deducing the *ex pede Herculem*, for them to swallow. But they need not have disturbed themselves: for, as I have shown in Part I., p. 33, my own long subsequent observations of all four of the grand arris lines, comprising the entire Pyramid within them, gave for the mean sides the equivalent of 51° 51′, and some seconds.

Or, if other proof of more multitudinous character be required for the universal public, they have only to dig in the heaps of broken material encumbering now every side of the Pyramid, to find innumerable fragments of the casing stones which once covered the whole of the tall triangular flanks: and among these fragments they will often find precious examples with remnants of the outside bevelled slope in connection with either the upper or lower horizontal worked surfaces. I myself found no less than nineteen such pieces, or two from the south, five from the west, five from the east, and seven from the north side, and they gave the mean angle as between 51 °49′ and 51° 58′. While a more remarkable and unexpected testimony still was derived from measuring the azimuthal angles of an otherwise incomprehensible system of far separated and huge trenches cut anciently into the rock of the hill before the eastern face of the Pyramid, so as to have a joining of all their axes *when produced* in one central point, and there giving up, to the alt-azimuth instrument which interrogated them in 1865 A.D., the angle 51° 51′ 35″ for the foot, and 76° 18′ 37″, or within a minute or two thereof, for the summit angle of the Great Pyramid.

5. *Size of the Great Pyramid.*

What so easy, at first sight, as to determine the lengths of the four sides of a square base, marked off neatly by corner sockets, or even by ruled lines on a flat and almost polished surface of white rock; and yet it has not been done by modern men in this case even up to the present time?

But let the reader please to remember, that though two of the sockets were indeed discovered so long ago as in the year 1799, by the French *savants* under Bonaparte, the other two were only brought to light by Messrs. Aiton and Inglis, and partly by myself, in 1865. And, even then, though all the four sockets themselves were, at that time, for some days, bared at once, and brushed quite clear and clean of all dust; yet, when a would-be measurer stood at, or upon, or within one of them, instead of finding the line from that socket to the next one smooth, level, and proper for laying his measuring rods upon,—behold a long hill of rough, ruined, stone rubbish near sixty feet high, occupying the middle part of the line, and a similar one on each of the other three lines.

Who, especially if limited in time and means, could measure accurately over such obstacles as those? Certainly the men who have tried it have not covered themselves with glory. For Mr. Inglis's measure of the mean of the four sides in 1865 came out 9110 British inches; and that of the ordnance officers in 1869 came out 9130 British inches; while the much more careful and elaborate measure of the French *savants* of 1799,—on only one side, indeed, but of the assumed square, and where they had two terminal sockets to measure between,—was 9163 inches; and the repetition of the same by Colonel Howard Vyse and Mr. Perring in 1837 gave 9168 inches.

Now, all these numbers cannot be correct, nor even each of them for its own date; for base lines, so marked in solid rock, are not elastic and variable through enormous limits. What, then, is the one and only real, true, and definite length of the mean of the four sides, as it is in nature and fact, to an accuracy let us say of a tenth of an inch?

Unfortunately, the whole modern world can give no satisfactory answer to this simple question. Our own country, even by itself, has spent millions of money on their Home survey, the Indian survey, and surveying in the Colonies. It has spent largely, too, on surveying the archæology of various lands, and is spending most liberally at this very moment on that of India; but on the measurement of the earliest archæological monument of the whole world, and that which lends itself at once, by the whole nature of its form and construction, most absolutely and completely to become the subject of exact mensuration, viz., the Great Pyramid, our rulers have not thought fit to spend a farthing; unless, indeed, some of them are responsible for that mere spurt of an effort made in 1869 by the officers who were returning from the survey of Sinai, and whose probable error in one of their base-side measurements was apparently something more than 300 times as large as what was incurred on a base line measured in India, about the same time, by the officers of the trigonometrical survey there.

But what are we, who desire to theorise on the Great Pyramid, to do at the present moment with these conflicting modern numbers before us, for the true length of the Pyramid's ancient base-side?

The simple mean of the whole, or 9143 inches, might be a fair approximation; or, again, weighting each result according to the care and means employed in obtaining it, perhaps 9160 inches might be closer to the truth. But on neither of these numbers, nor any neighbouring ones to them obtained by similar judgment processes, do I venture to decide absolutely. I only insist that we are bound to hold that the true quantity must be somewhere between—perhaps not far from midway between—the extreme limits of the widest observers; such conclusion being, moreover, also borne out by a comparison of all the best measured heights of the Pyramid, as determined by various methods, and used, in combination with the angle of slope, for deducing the length of the base side.

And then, in conclusion of this part of the subject, to show that we have, after all, some valid grounds for confirming the popular feeling of all the world throughout all ages, for the so-called "Great Pyramid" being really such,—I subjoin a list of all the Pyramids of Egypt, similarly measured—and it will be found that there is not one which contests the Great Pyramid's superior size, within many times even the most extravagant estimate yet made of the probable error of the measures.

THE PYRAMIDS OF EGYPT;

ALL STANDING IN THE LIBYAN DESERT, ON THE WESTERN SIDE, AND ABOVE THE LEVEL, OF THE NILE VALLEY; THE MEASURES BEING APPROXIMATE ONLY, AND CHIEFLY BY COLONEL HOWARD VYSE AND MR. PERRING.

Modern name of each Pyramid.	Latitude. North.		Angle of rise of the faces to horizon.		Length of side of base, always four-sided.		Central axis, or vertical height.	
					Present.	Ancient.	Present.	Ancient.
	Deg.	Min.	Deg.	Min.	Brit. Inches.	Brit. Inches.	Brit. In.	Brit. In.
Great Pyramid of Jeezeh ..	29	59	51	51	8950	9142 / 9168	5410	5819 / 5833
Second Pyramid of Jeezeh ..	29	59	52	20	8290	8493	5370	5451
Third Pyramid of Jeezeh ..	29	58	51	0	4200	4254	2436	2616
Fourth and minor Pyramid of Jeezeh	29	58	in steps		1230	2200	834	1440
Fifth ditto ditto	29	58	52	15	1656	1749	1000	1119
Sixth ditto ditto	29	58	in steps		1230	2200	834	1440
Seventh ditto ditto	29	59	52	10	1500	2070	540	1332
Eighth ditto ditto	29	59	52	10	1500	2070	660	1332
Ninth ditto ditto	29	59	52	10	1440	1920	960	1221
Pyramid of Aboo-Roash.. ..	30	4	no casing		3840	x	480	x
Pyramid of *Zowyet* el Arrian	29	57	ruins only		3600	x	730	x
Pyramid of Reegah	29	56	75 / 50	20 / 0	1200	1480	500	1150
Northern Pyramid of Abooseir	29	54	51	43	2600	3084	1400	1953
Middle Pyramid of Abooseir	29	54	51	(?)	2560	3288	1284	2056
Great Pyramid of Abooseir ..	29	54	52	(?)	3900	4317	1970	2734
Small Pyramid of Abooseir ..	29	54	50	(?)	650	905	216	564
Pyramid 1 of Saccara	29	53	rubbish only		2500	x	700	x
Pyramid 2 of Saccara	29	53	52	(?)	2150	2775	1300	1758
Pyramid 3, or Great Pyramid of Saccara	29	53	73 / in steps	30	3700 N. to S. / 4200 E. to W.	4214 N. to S. / 4727 E. to W.	2200	2405
Pyramid 4, at Saccara	29	53	ruined		2640	x	740	x
Pyramid 5, at Saccara	29	53	ruined		3000	x	480	x
Pyramid 6, at Saccara	29	53	ruined		3240	x	960	x
Pyramid 7, at Saccara	29	53	ruined		1680	x	330	x
Pyramid 8, at Saccara	29	53	ruined		2880	x	1044	x
Pyramid 9, at Saccara	29	53	ruined		2940	x	900	x
Pyramid base of Mustabet el Faraoon	29	53	in steps		3500 / 2300	3708 N. to S. / 2604 E. to W.	650	720
Northern *Brick* Pyramid of Dashoor	29	49	51	20	4500	4200	980	2586
Northern Stone Pyramid of Dashoor	29	49	43	36	8400	8633	3918	4111
Southern Stone Pyramid of Dashoor	29	48	54 / 42	15 / 59	7400	7400	3834	4029
Small Pyramid of Dashoor ..	29	48	50	12	1700	2172	816	1281
Southern Brick Pyramid of Dashoor	29	48	57	20	4800	4110	1872	3208
Northern Pyramid of Lisht ..	29	38	ruinous		4320	x	1080	x
Southern Pyramid of Lisht ..	29	37	ruinous		5400	x	822	x
The False Pyramid, or Pyramid of Meydoom	29	27	74	10	2388	x	1494	x
Pyramid of Illahoon	29	17	ruinous		4320	x	1580	x
Pyramid of Howara..	29	18	ruinous		3600	x	1270	x
Pyramid 1 of Biahmoo	29	26	63 / 50	30 / 0	360	1440	360	x
Pyramid 2 of Biahmoo	29	26	63 / 50	30 / 0	360	1440	360	x

6. *Internal Arrangements.*

Had the above tabular statement been extended so as to contain descriptions also of the interior passages and chambers of the Pyramids, more striking diversity still would have been found between the great monument and its companions. For though all of them have a descending entrance passage with a chamber at the further end, *i.e.*, the simple tombic arrangement known to all men, none but the former has an *ascending* passage and *other* chambers high up in the sub-aërial masonry of the building.

That ascending passage, with its consequences, is indeed the chief constructive secret, as well as peculiarity, of the Great Pyramid; and a secret well kept too, for with the exception of its place having been broken into once, apparently by some very ancient fanatics of the native race, the mystery remained unknown to all other men, from the building's primeval birth down to the year 800 A.D., when an accident, often described in books since then, disclosed the beginning of the upward ascent to the Khaliph Al Mamoon, who was quarrying at the moment close by.

Yet was the arrangement not *intended* to escape posterity for ever; seeing that so soon as I began to measure the important, and otherwise regular, joint lines in the floor of the descending passage both in mathematical manner and in a respectful frame of mind for the original designer (a simple combination, but which seems to have escaped previous travellers), than I stumbled on, nay, started at, rather than merely discovered, two lines that were altogether anomalous in their angle, as well as peculiar in the extra hardness of their stone material; and behold! they pointed to the very place where, behind an apparently ordinary part of the ceiling, and totally unsuspected by later Egyptians, Ptolemæan Greeks, and Imperial Romans (who *did* pass up and down the more ordinary descending passage) the hidden *ascending* tube begins to take its mysterious rise. More yet, too, of such hints had been left behind; for only a little way beyond the system of horizontal-angle trenches, already alluded to, in front of the Eastern Side of the Great Pyramid, there is a full sized model, in everything but length, of the said Pyramid's unique system of both descending and ascending passages, cut into the rock in a vertical plane; and with even a beginning of the greater breadth and peculiar ramps of its grand gallery.

But if the Great Pyramid has these *additional* hollow spaces over and above what other Pyramids possess, must it not thereby be just so much further off from the ordinary mathematical definition of a Pyramid, viz., a *solid* of such and such a shape?

True is the objection in principle, though in practice not felt to any sensible extent; and because, in the first place, all the hollow portions of the Great Pyramid put together barely amount to 1-2000th of its whole bulk; and in the second place, so far as specific gravity is concerned, the effect is in part made up for by some of these same interiors being lined, and others even filled, with *granite*, a far heavier material than the very light nummulitic limestone of the general mass.

And here let it be noted for a moment, *how* the Great Pyramid makes use of that remarkable material, granite; viz., in the very uniform temperature of its *interior*, and there only; where too it has lasted and preserved its anciently polished surface admirably. When my friend St. John Day, C.E., in a lecture to the Philosophical Society of Glasgow, was recently describing the Mokattam limestone which the Pyramid architect had chosen for its *external* covering, where the changes of temperature between night and day, under the radiations of sun and sky, are excessive—a practical constructor rose with exceeding positivism to say:—"Now, I think that the architect was wrong there, and I am sure that I could have advised him better; for I have come to believe that all the strength of the earth consists in granite Divinely put into it; and when the town council the other day took up a pavement that I had laid for them ten years before with granite out of the quarries of his grace the Duke of Argyle, it was found perfectly sound and good after all that wear and tear."

Mr. Day's answer, by merely relating facts from the Pyramid, was as ready as if prepared beforehand: viz., that the architect of the subsequent third Pyramid (decidedly, too, an idolatrous and anti-great Pyramid man) *had* used granite for the external casing of a large part of his Pyramid; and with the result now, or after the more certain and crucial test of 3500 years, that the exposed granite surface is so disintegrated or exfoliated, especially at the corners and edges, that the once smooth-planed bevelled casing blocks are now in shape like so many puddings, or even embryo boulders; and fail, in fragments, to tell the original angle they were cut to, within many whole degrees. While the peculiar limestone of the similarly *external* coating but of the still *more* ancient Great Pyramid has had such a special power of resisting atmospheric effects, that you may occasionally, among the later ruins of it caused by man, pick up specimens where the meeting of two of the worked surfaces is preserved sharp up to their mutual corner line, without a deficit of more than one or two hundredths of an inch broad.

Whereupon another C.E. expressed soon afterwards the effect produced on *his* mind, by the ultimately right rock for the place and purpose, though at the moment, from its softness, so utterly unlikely to ordinary apprehension (and there were *then* no previous stone buildings in the land to refer to for experience), having been so unerringly chosen by the designer of the Great Pyramid. But we can hardly delay now for mere mental testimony, seeing that details of numerical mensuration are our true subject in this place, and are quite as much as we can pretend to overtake.

Nay, indeed, even in that department also, the editor's limits of space begin to warn me to contract the fuller account that I had once hoped to give; and perhaps I may be allowed all the more readily by the reader thus to do, seeing that in so far as the *interior* of the Pyramid is concerned, the battle of the different mensuration testimonies extant, was in reality fought out by me in 1865, at the place itself; and by the only unexceptionable method, viz., that of measuring the parts disputed again and again; by taking fifty measures where my predecessors had taken only two or three; by working sometimes six hours a day in the solitary interior of the dark monument with only my candles, measuring rods, and note-books about me; and by continuing such studies day after day through several months, instead of paying the place a single visit of only a few minutes' duration, and then, either oppressed by Arabs, or thronged by "troops of festive friends."

Wherever the subject of measurement on those occasions exhibited to me original worked surfaces of stone in tolerable preservation, I attempted to make all linear measurements to tenths of inches; but if the ancient surfaces were superfine, then to hundredths of inches; while the angles of the descending and ascending passages, together with others for testing the reputed horizontality or verticality of floors and walls, were examined by three different classes of instruments, of which, if one was characterised chiefly by portability, the other two were splendid examples of their kind for accuracy: viz., first, a clinometer, presented to me for the occasion by A. Coventry, Esq., of Edinburgh, with its base, 120 inches long, and a central complete circle for the angles, reading by three pairs of opposite verniers to ten seconds each; and second, an altitude-azimuth circle, donated years ago to the Royal Observatory, Edinburgh, by the famous Professor Playfair, and reading off both its circles by micrometer microscopes *to tenths of seconds.* In order to convey these large instruments into the Pyramid's interior, I had of course, then, to hire several Arab assistants; but the days for their use came few and far between; partly, too, because I had, as my experience advanced, to prepare with my own hands wooden stands and other helps to observation adapted in shape and size to the strange corners, slopes, projections, &c., where the instruments required to be placed.

But all these painful and laborious steps having been duly gone through, and the separate measures, whether accordant or discordant, printed in full elsewhere,* the reader may not care for more now than merely to gather some of the cream of the results, and hear what they seem to tell.

Part III.

Of Why These Things were thus Made of Old.

1. *Wherefore the Position of the Great Pyramid.*

Perhaps the title of this third part is a shade too presumptuous; at least, for the safe and cautious mode on which alone I propose to proceed.

For example, touching our very first topic, or the *position* of the Great Pyramid, have we not already shown that that site is in the very centre of all the land surface, or man-producing and man-supporting regions of the whole globe; and is not that quite a sufficient reason, for *there,* rather than anywhere else, having been placed of old,—if sufficient wisdom presided over its birth,—that one vast monument which is full of instruction to man, and not only saw the beginning of human history, but bids fair to be a witness also of its termination.

True that such central position, though in so far a fact, *may* be only an accidental coincidence; especially as it requires a knowledge of Australia and America to make it even approximately true. But then there is a sort of triplicity in the fact itself which gives a certain *small* probability in favour of intention, from knowledge derived somehow or from somewhere, to begin with, in rolling up our ball of pyramid scientific ideas, purposes, and intentions.

* Vol. ii. of "Life and Work," by C. Piazzi Smyth. Edmonston and Douglas, Edinburgh.

For, if geography thus assigns the general central position of the land-surface of the world roughly to Lower Egypt, chorography assigns a particular physical centre of figure and formation to a certain part of Lower Egypt, and locates that more precise point upon the hill of Jeezeh; while minute topography there shows the Great Pyramid arrogating peculiarly to itself the one only exact standpoint on that hill which distinctly commands the beginning or central origin of the peculiar physical domain just mentioned.

2. *What Decided the Shape of the Great Pyramid.*

Who does not give precedence to German minds, and especially to their greatest leaders in philosophy, in every question of deepest mystery. Can we then pass over what the illustrious Schelling has written on the identical subject of this section. Certainly not, for it is exemplary in every way.

"The Great Pyramid of Cheops," says Schelling,* was the symbol of the higher Monotheism, in which way we can account for its being constructed on a quaternary principle, viz., on the three powers of Typhon, Osiris, and Horus (below), and on the fore-mentioned one-only-Godhead (above). (The three powers form the basis, the one God above them the apex.) For the quaternary is also the ruling principle in this theistic system, viewed as intelligible (although as yet we have only developed it into the threefold), as already eight gods have come into view out of Herodotus, which, when one understands the half to be female, shows that the fundamental number is the fourfold. The pyramid is the first body, the first solid, and since, in the old philosophy of arithmetic, the point stood for unity, as the line appeared to be produced out of the number two, and the plane superficies out of the number three, so the grand meaning of the number four was seen in this, that it would show itself as the first solid-like number, since the pyramid, the first of the five regular bodies is produced in it with the four given points."

Now there is a great deal in this essay most admirable as to some general principles of philosophising, though in the particular instance before us, failing miserably in practice. Not only, too, because we have no proof that the myths of Typhon, Osiris, and Horus were invented until several centuries after the Great Pyramid, but because we do know positively, even from our own careful, clear, distinct, and

* "Philosophie der Mythologie," pp. 405, 406. From J. T. Goodsér's "Homilies," p. 260.

broad day-light measures, that the Great Pyramid's base, which these hands have handled and these eyes have seen, has not three, but four, sides; and that the whole Pyramid has therefore of necessity not four, but five, points or corners, and five bounding sides or surfaces, the base being mathematically and crystallographically included as one.

Hence springs up not a quaternary, but a quinary, arithmetic; and a quinary, too, so combining two sets of five into one whole, as to lead on just as naturally to a complete decimal system, as the two so-called five-fingered hands belonging to one body of a man are usually held to *have* led to the world's present decimal arithmetic. The parallel, too, is all the closer, because, if the human hands contain really not five, but four tapering fingers and one broad thumb, does not the Pyramid exhibit four taper triangles and one broad square. Or, again, to take it in a higher manner by its points or corners, are there not four lower, earthly, ordinary foundation corner-stones, and one upper, topmost, skyward, unique head-corner-stone,the very "head of the corner," and in which, as we read in a higher than any German authority, "all the building fitly framed together, groweth unto an holy temple in the Lord."

Poor Schelling was, therefore, in a manner not so very far out after all, at least as to having a vision of certain principles; but he should assuredly have tabled the number five in place of four, and ought to have been thinking about the one true God of Scripture, and not of the unlawful theotechnic inventions of later idolatrous Egyptian minds. Had he only so acted, he could at once have counted St. Paul, St. Peter, David, and Isaiah (themselves lower foundation corner-stones) on his side,* and would have enjoyed a priority over the late John Taylor† in giving the modern world their first full interpretation and only perfectly satisfactory mechanical explanation of the figurative expression both of the Old and the New Testament "head-corner-stone;" for it can be exactly paralleled nowhere else than in the capping-stone of a pyramid similar to the Great one.

There yet remains, however, the question as to the *angular* slope of the Great Pyramid's sides, for that forms equally a part in the decision of *shape*.

* Psalm cxviii., 22; Isaiah xxviii., 16; Zechariah iv., 7; Job xxxviii., 6; Ephesians ii., 20, and iv., 15, 16; 1 Peter ii., 4, 5, 6, 7; Luke xx., 17, 18; Matt. xxi., 42, 44.

† Long the publisher to the London University; died in 1864. The book here referred to is by name "The Great Pyramid; Why was it Built, and Who Built it." Longmans and Co., London.

That angle, too, might have been varied at the pleasure of the architect (with no precedent all the world over to control his proceedings), from the almost flat appearance of a mere "buckled" plate, or of King Alyattes' low and broad tumulus mound in Asia minor, up to a needle-shaped spire to compete in subsequent days either with Pagan Egyptian obelisks or modern Christian church spires, for delicate slimness of figure. But the said architect chose to give his building the special form due to an angle of 51° 51′ and some seconds; and kept to that one angle with singular care and success.

What virtue, then, if any, was there in that particular angle as introduced in that place?

To the same venerable John Taylor, in his 80th year, we owe also the happy suggestion, that it was because that angle, in such a quinary pyramid, is demonstrative of the true "squaring of the circle," known to mathematicians as "the value of π," a subject which is at the base of all practical calculations throughout the whole of the exact sciences; and though the quantity be styled an "incommensurable" (that is, an unlimited eternal fraction), yet has its true value, approximated to only by the Greeks, been obtained at last correctly by the learned of modern Europe to at least thrice as many decimal places as are ever required in practice. Their result, too, stands perfectly firm, though it is still ever and anon being radically disputed through its whole extent by those strange enthusiasts who never die out amongst men, but are continually reappearing in almost every month's serial literature that so quickly passes on its inevitable road to oblivion.

According to John Taylor's supposition, 51° 51′ 14·3″ is the precise *computed* angle which such a π pyramid ought to possess, and is just about as close to a mean of the best *modern measures* yet made of the Great Pyramid, as is possible for *their* residual errors. Closer, in face of the growing dilapidation, no future measures can well be expected to come. Hence, any further proof that π and not some other idea, differing in its angle by a few seconds only from the π quantity, was intended, must be sought elsewhere than in practical re-measurements of the simple angle. Nor have we very far to seek.

On the π principle, the continued length of the four sides of the base is equal to the straightened-out length of the circumference of a circle whose *radius* is the vertical height of the pyramid. If such a circle be drawn concentrically on a plan of the Great Pyramid, its circumference will be found to lie outside the base when near the middle of the sides, and inside at the corners. By just such a quantity, moreover—if the pyramid be taken in progress of construction, or without its final sheet of casing stones—that the projection of the circle at the middle of the side has been shown by W. Petrie to be coincident with the common axis of two of that remarkable system of azimuth trenches just mentioned; while rectangular offsets from the places near the corners, where the circle's circumference enters the square base, define the very precisely cut and broad rectangular ends of the two self-same trenches.

After this, it is needless to add that Herodotus's puzzling statement of the height of the pyramid being equal to the length of the side of the base, when interpreted by John Taylor as referring to *square* measure, and meaning that the area of one of the sides was equal to the square of the height, does really give as its angle 51° 49′, or closer than many of the modern measures; and that a very lately started idea, or that a rise of nine for every ten of distance in the diagonal of a pyramid's base, would give a quantity differing only a few seconds from 51° 51′ 14″—all this I say is needless, because these ideas with merely closely similar but not identical angles, and many other ideas which are vastly simpler—but vastly further off, too, from the angular truth as measured—have no such additional testimony of intention from the builders themselves, as the π theory has. *Inside* the pyramid, too, we may truly say, as well as *outside*, for a corollary of the same theory assigns the angle for both the descending and ascending passages; and when the longest length and most carefully constructed part of any of them, viz., the Grand Gallery, came to be measured by myself,—with instruments, both mechanical and astronomical, the like of which for power of accuracy had never been taken inside the Pyramid before,—the mean result, though in the hands of some previous explorers differing several degrees, was 26° 17′ 32″, the computed angle being 26° 18′ 10″. While the closely approximate π value itself is found again, permeating both the peculiar arrangements of the antechamber* and the proportions of the *coffer*,† that ultimate treasure of the final and innermost "King's Chamber" of the Great Pyramid.

Let it be further noted, too, by the assistance of the

* As shown by Captain Tracey, R.A., in a paper about to be read before the Royal Society, Edinburgh.

† St. John Vincent Day, the discoverer. See his published "Papers on the Pyramid;" also his folio book "Plates and Notes on Pyramids."

tabular view of pyramids already given, that not one of the others has this angle or anything near it; no angle either for which any superior mental idea has ever been proposed. The builders of those other and subsequent pyramids copied the Great Pyramid in all that *they* saw or *they* thought important; but plainly the scientific secrets, as reasons of its construction, were not known to the ordinary idolatrous Egyptians of even the most ancient days. Of course they could not fail, if copying at all, to reproduce the *square* base and *four* triangular flanks, whence they may lay claim to quinary arrangements and decimal arithmetic, which, in fact, the old Egyptians had. But the *angle* of the Great Pyramid! No, they never hit that! Nor even imagined that there was anything important therein! For over and above the utterly diverse *built* angles of so many of the subsequent pyramids, there may be seen still, even in the best cut hieroglyphics and Royal cartouches of pyramid-building kings of ancient Memphis, little figures standing for pyramids and most ridiculously out in their angles; being, generally, tall and slim or anywhere between 60° and 80°, and this by artists who could touch off the precise outline of any of a hundred varieties of animals to most speaking likeness and almost microscopic exactitude.

So far as *shape* is concerned, then, all these other pyramids came no nearer to the Great Pyramid than do monkeys to man made in the image of God. Monkeys have at least as good, if not better, five fingered hands than man; but soul and immortal spirit and capacity for religious impressions—where are they?

Besides, however, its correct solution of the universal π problem, and its claim, considering also its primeval date, to school respect on that account—the Great Pyramid is further, and even thereby, an instrument for holding up to man, through every age, a certain thing in creation which he, with all his powers, cannot evenly decimalise, strive he though never so wildly. And yet, viewed submissively according to nature's laws,—the very self-same principle gives the building a claim to distinguish and typify some other numbers, especially 3 and 7,* besides the 5's and 10's of its radical formation, which lead to some singularly instructive consequences.

* See postscript by W. Petrie to vol. iii. of my "Life and Work at the Great Pyramid."

3. *Why of that Particular Size.*

If we next inquire why the Great Pyramid was made so truly great as modern measure proves it to be, the ready answer has too often risen up in polite society, from the time of the elder Pliny to our own,—"Because the man who built it was a brute; he appreciated nothing but big, burly, bullying size, and he tyrannised over his people and drained his country of its resources, merely to make a vulgarly grosser tomb for himself than his neighbours had."

A very pretty theory, too, is this, as it stands; but then it does not explain in any degree either the remarkable abnegation of praise, vain-glorying, and self which was alluded to in Part 1, page 30, or the already proved fact of the recondite and most meaning *angle* that was adopted and rules the whole building from bottom to top, and from side to side. Now so important and all-pervading a feature of shape as that, not only cannot be overlooked, but we ought to take it along with us when seeking an explanation of size also. And how shall we consider the size chiefly exemplified? By one of the four base sides? Or by one of the four longer diagonal arris lines? Or by one of the four slanting heights from the middle of a base side up to the apex? By none of these mutually confusing and competing measures; but, inasmuch as the shape-theory has already attached such importance to the *one* and only line representing vertical height, and made that to be radius,—why we will take it for size also as a radius; but a radius of what?

Next to land surface for man to dwell on, what is there so important to his physical well-being on earth as sunshine. "The Sun; Ruler, Light, Fire, and Life of the Planetary System," is the well thought out title of Mr. Proctor's recent solar book, and applies most eminently to the earth and man's corporeal existence thereon. So, too, had also considered, before that book was written, my friend W. Petrie, and had convinced himself from other sources as well, that in so anthropologically directed a monument as the Great Pyramid, there *should* be an expression of the sun's mean distance from the earth as a radius, and that the numbers 10 and 9, or rather 10 and 3 × 3, as he preferred to write it, should play the chief part in the proceeding. Wherefore he took the vertical height of the pyramid as a radius, and multiplying that by 10 raised to the power of 3 × 3 (*i.e.*, 1 with nine o's after it), got 92,080,000 miles for the result. But he regarded that, at the time, as a failure; for all his astronomic books told him that the true distance was 95,293,056 miles, and he had in consequence almost thrown the whole thing overboard, when he heard accidentally one day that within the last ten years, astronomers have entirely quitted their former trust in 95 millions and so many odd miles (though they publicly crowned with exceeding honour the German astronomer and computer who brought it out for them), and are now close upon 92 millions instead, but with no very great certainty either. In fact, at this moment the astronomical world has split into two opposite camps, one of which, championed chiefly by foreign astronomers, makes the true solar distance very sensibly greater than the other, though both of them much less than the old 95 millions. Each party is perfectly certain that they are right and the others wrong, and in their eagerness to demolish their opponents, overlook the remarkable fact that the pyramid sun-distance comes *between them*; for even with all its present needless amount of probable error as depending on the bad measures for the pyramid's size taken by modern science, both limits of this anciently defined quantity come into the vacant space between the presently opposing parties in practical and physical astronomy. They fight; but until one of them has entirely annihilated the other, the friends of the pyramid may look on perfectly unmoved.

If, then, the vertical height of the Great Pyramid was really regulated so as to be a certain round fraction of the sun's mean distance from the earth, we need not expect any other cosmical quantities to be expressed by the size of other features such as the slant height, arris line, &c., &c.; for they can only be necessary mathematical accompaniments of the settled vertical height in a π pyramid. But if we introduce a new idea, or unit of measure, something may come of it, especially if we have pyramid authority for such additional unit.

Now, in the northern hemisphere of the earth, the semi-axis of rotation stands up in the midst of the circle of the equator, something after the fashion of the pyramid's vertical height to its square base with the circular proportion. Let us take the length, then, of that semi-axis as determined by modern science and divide it by 10 raised to the 7th power,—that number being chosen partly because seven is one of the extra numbers peculiarly given to the Great Pyramid by means of its π proportion, and partly, *and* still more pointedly, because the length so obtained, or 25·025 British inches is, in the first place, signally within the limits which Sir Isaac Newton settled a century and a half ago as being the length of the sacred cubit placed before the children of Israel by Moses as the cubit "of the Lord their God;" but totally without the limits of the cubits of profane Egypt, Babylon, Nineveh, and all known idolatrous countries of the ancient world; and, in the second place, because such a length-standard is very remarkably introduced into the chamber called the Queen's Chamber of the Great Pyramid, by a massive and most unique, as well as striking, arrangement of a portion of the architecture there, which can have been brought about by no others than the original builders.

Taking, then, that 25·025 inch rod, and applying it to the base side of the Great Pyramid, it shows no less appropriate a quantity than the number of days in the year; or another most solar, most anthropological datum in astronomical science; seeing that it is the number of times the earth revolves on its axis while it circulates round the sun, with a radius vector already typified in the pyramid. At present, indeed, the roughness of modern measures of the pyramid prevents us saying with certainty whether siderial days or solar days of the earth are indicated; but both come so decidedly within the limits of the best modern measures, such as they are, that it would be hardly fair to hide what seems so close to proof in the ancient building, merely because modern measurement of it is disgracefully far behind.

Again, let us take a new unit still, *i.e.*, divide the 25·025 rod by 5, and by 5 again. A most eminently pyramid division abstractly, and practically it is justified by the pyramid itself; (1) because the chamber containing the larger unit stands on the 25th course of masonry constituting the whole pyramid; and (2) because a division of 5 by 5 again, of that very length, is shown in the so-called ante-chamber to the King's Chamber. Each of these chambers devotes one or more of their walls to memorialising a division into 5; and it is in one of them that we see first, the fifth part of 25·025 inches, and then the fifth of that again, in the outside breadth, and then the thickness of the so-called boss on the granite leaf.

Now a little mark only five inches long and one inch thick, would be a curiously difficult thing for total strangers to find in a general way throughout the Great Pyramid; it would be the prototype, indeed, of the confounding needle in the farmer's bottle of hay. But there is no such difficulty at the pyramid with this little boss; for it is taken away from the Queen's Chamber, whose walls, being only in limestone, would be too soft to retain so small a fiducial mark, and is introduced into the ante-chamber at the commencement of the *granite* constructions of the interior; and there, too, it is found projecting from the polished surface of a remarkable granite block, which is not only theoretically important from being on the junction point of several of the most signal construction lines of the pyramid, but practically is singularly prominent there, because it stands right across the one and only passage way to the further interior; and in a manner which nothing yet known fully explains, though every traveller or explorer is painfully aware of the fact, that no man can enter the ultimate and so-called King's Chamber without bowing his head low to pass under this said stone.

Armed, then, with this new unit, or the twenty-fifth part of the sacred cubit of Israel and the Great Pyramid, we measure the diagonals of the base of the whole structure, and find them amount to 25870, more or less; *i.e.*, at the rate of such unit to a year—the number of years, as well as it is known at present, in the grand celestial cycle called the "Precession of the Equinoxes."

Yet what has the Great Pyramid to do with the Precession of the Equinoxes, our readers may fairly ask; and we are enabled to answer them instantly, "A very great deal—both constructively, astronomically, and anthropologically; or as follows."

4. *Of the Chronology of the Great Pyramid.*

Not unfrequently may be traced two ideas dominating so nearly the same parts of the pyramid, that at first sight anyone might well pause before accepting either one or the other, and certainly not both of them, as having been really *intended*. Yet a closer examination usually shows, that while both ideas belong appropriately to successive steps in the same line of thought, the practical influences of each on the building remain totally distinct in their mechanical realisation.

Thus the pure geometry of the pyramid's figure, if proved at all, remains just as completely proved, after there has also been subsequently shown to have been a practical astronomy at work there likewise, settling the orientation of the self-same figure; *i.e.*, directing the sides of its base upon the cardinal points of the earth and sky.

That such geographical astronomy was no accident is abundantly shown from the excellence of the resulting emplacement; the mean error coming out, when measured by myself with the Playfair alt-azimuth instrument—(1) on the socket line of the east side of base; (2) on the same line of the north side; and (3) on the axis of the entrance passage, at something less than five minutes of angle: and these limits will probably be smaller when the chief line on which the architect relied, viz., the axis of the "Grand Gallery," can be measured in azimuth as I did measure it in altitude.

Again, my suggested geometrical diagram depending on the π proportion, and producing the passage-angle already alluded to, requires for some of its further developments a standing point in 30° N. latitude.

Is the Great Pyramid, then, so placed?

It is, and it is not, I may perhaps be allowed to answer. At all events, it is *nearer* than any of the other pyramids; for, while the Aboo Roash abortion is several whole miles too far north, and the Great Pyramid (being, by my measures, in 29° 58′ 51″ latitude) is more than one mile too far south, all the other known pyramids are further south still.

Indeed, the exact position of the parallel of 30° latitude in that meridian is right down in the hollow alluvial plain in front, or northward, of the Great Pyramid; a region impossible for secure foundation. Rather, therefore, than incur that danger, would it not be better to put up with the error of 1′ 9″ in the angle of some of the lesser features of the building? Perhaps so; though meanwhile it may be doubted whether the error on the original intention really amounts even to the third part of 1′; for there are two ways of viewing the introduction of 30° latitude, or the symbolical elevation by so much of the polar point of the sky.

Shall it be, for instance, in a perfectly abstract manner, and as though the earth with all that is thereon were merely a mathematical point in space: or shall it be as the earth is in nature, where man can only view the polar star by gazing through the intervening medium of a refracting atmosphere, without which he could not live?

The former method would perhaps be preferred by the scientific few, but the latter is more adapted to the apprehensions of the greater number of the multitudes of mankind. Neither, therefore, can be altogether omitted in a monument both scientific and anthropological. Wherefore, as the former principle requires 30° 0′ 0″, and the latter 29° 58′ 23″ (or a latitude where the polar point, raised by refraction, would become 30° to the eye), we take a mean between the two, viz., 29° 59′ 12″, and find the Great Pyramid so very near thereto as necessarily to throw us back on that never-ending subject, the probable error of even the best modern determination; and also on that long felt physical doubt, as to the want of perfect constancy in relative position through long ages, between the crust of the earth and the direction of its axis of rotation.

But there is more important astronomy in the Great Pyramid than merely for geographical position.

The one only entrance passage, as already stated, looks out northward in the plane of the astronomical meridian and at the height almost of the polar point itself. Now certain well-meaning naval officers, about fifty years ago announced that, having descended that passage a certain distance at night, they saw, on looking up towards its mouth, the polar star, viz., *a* Ursæ Minoris, most clearly; whence they inferred, that the passage must have been built for the purpose of observing that same star in ancient times.

On this statement being submitted in 1839 by Col. Howard Vyse, when he had returned from North Africa, to Sir John Herschel, then just returned likewise from his celebrated astronomical sojourn in South Africa, he at once said,* These gentlemen have failed to take into account the *Precession of the Equinoxes*, which, in 4000 years, would have displaced every star in the heavens, from its then apparent position on the sphere by no less a quantity than 55° 45′ of longitude, and would have changed all the relations of the constellations to the diurnal sphere; whence it comes that the present pole star could by no possibility have been seen at any time in the twenty-four hours through the entrance passage into the Great Pyramid. But there was another star, he added, then almost exactly in that line, viz., *a* Draconis, and that must therefore have been the polar star of reference for the pyramid builders; the passage being built at such a vertical angle, in the meridian, as to point to that star at its transit beneath the pole, and at that precise date when Precession had made the star's distance therefrom to amount, for the time, to 3° 42′, or whatever the observed deficit of the passage angle below 30° might be.

This suggestion of Sir John Herschel's certainly introduced another, and in so far a dangerous, mode of explaining one passage angle which we have already got approximately from a very different consideration. In this latter case, however, the quantity was merely an auxiliary, while in Sir John Herschel's explanation we have the ultimate natural

* See p. 107, vol. ii., of Vyse's "Pyramids of Gizeh."

phenomenon itself. I freely, therefore, give his conclusion the preference; and even consider his contribution thereby to the astronomy of the Great Pyramid, as singularly important in its way, as John Taylor's π hypothesis has proved to be for the geometry.

And yet this pole-star-*cum*-Precession theory, though so admirably started, was eventually left by its brilliant author only half worked out. For α Draconis was not *once* only, but *twice*, in early history, at a distance of 3° 42′ from the polar point of the sky; viz., first in 3400 B.C. (nearly), and afterwards in 2170 B.C. (nearly). Wherefore all the world is surely entitled to ask, *which* of these two occasions was intended to be memorialised in the Great Pyramid?

At the time of Sir John Herschel's writing, viz., in 1839 A.D., a date of about 2100 B.C. was largely believed in as that in which the Great Pyramid was really built; so there seemed nothing to be done then, but to accept the *last* star distance, or that of 2170 B.C., as the one intended. But since then, learned Egyptologists have been so perseveringly stretching their early chronology, that the chief geniuses among them, such as Bunsen, Brugsch, Lepsius, Renan, and Mariette Bey, are all for dates of either 3400 B.C. or something earlier still. Whence it comes that the self-same species of testimony which led Sir John Herschel in 1839 to decide for 2170 B.C., ought now to lead both him and everyone else also to prefer 3400 B.C.,—equally appealing all the time for proof to the same polar star, α Draconis, and the same entrance passage in the Great Pyramid.

Such, then, is the condition in which the brightest light of modern astronomy left this ancient question, or with an uncertainty of 1200 years (*i.e.*, almost a quarter of the whole time concerned) hanging over it. And if *that* were all the accuracy which the primeval architect cared to reach, I, for one, would cease to advocate his claims to attention from the learned in the present day. But let us try to inquire, and in something of that respectful frame of mind previously enlarged on as requisite, what *he*, the designer of the Great Pyramid, thought about this matter.

One of the very first features in Sir John Herschel's theory—which I, though beginning with the utmost desire to find it complete and faultless, could *not* admit; *i.e.*, after I had once acquired at the Pyramid by painful daily toil innumerable proofs of the wisdom, foresight, economy, and constant calculation of its primeval designer—was the attempt to father on *such* a man, and as a method in chronology, the taking a transit of a *polar-star!*

Why, does not the very youngest assistant in every modern observatory know, and know it as well as he knows black from white, that a *pole star* transit by itself is not the way to get an exact knowledge of the *time!* No, indeed, he seeks the very opposite, or an equatorial star, for such a purpose. And this broad principle is equally applicable to any tube, whether erected for actual observation, or merely for memorialising purposes, as was indeed the case in the Great Pyramid; where the passages, after being grandly constructed in such a totally diverse manner from the rest of the building, that they would be identifiable while any of it lasted—were immediately filled up with close fitting plugs of stone the whole way, and had their outer ends concealed until the days of dilapidation began.

Then, again, there chimed in with this pole-star-for-time difficulty—at least in my eyes and on the score of the respect due from every one living to the dead lion we were all standing upon—the further anomalous query, viz., that even *if* so bad a star for time purposes as a pole star *had* been adopted by the Pyramid architect, why did he take even that star in its weaker position, or at its lower, instead of its upper and brighter, culmination?

I do not pretend to say how other persons have contrived, with a clear conscience, to get over this double difficulty with merely an expression of contempt for all the knowledge of the best men in the world 4000 years ago, and the assertion that such fellows, barely developed in that early age out of primitive savagedom, were only thinking about "the best angle for sliding stones down the passage." But the rule of conduct which my Pyramid investigations had long since forced on me suggested, first the simple and very different answer,—"Because when that α Draconis star was crossing the meridian below the pole, another and more important star must have been crossing the same plane above the pole," and then set me inquiring about it.

Now the more important star for chronological purposes could be only an equatorial, or at least a zodiacal, star; a meridian combination of which, with a pole star for position reference, would be as powerful a method for fiducial time observation as any one could devise in the present day. But was there anything of the kind available at either of those two epochs, viz., 3400 B.C. or 2170 B.C.?

At 3400 B.C. there was no such combination, and α Draconis had to pass the meridian below the pole in barren state. But in 2170 B.C., see, and as the exact companion required, that zodiacal cluster, so dear to human sympathies throughout all post-diluvian ages, "the leader of the heavenly host" to the ancient Hindoos, and the type of Halcyon, *i.e.*, Alcyone-led, days to the classic peoples of Europe, viz., the Pleiades. Yes, indeed, no other star-group than the Pleiades, and at a distance *then*, as little removed from the equator as was α Draconis from the pole.

That typical polar distance has already been stated as 3° 42′; now the same star is 25° away; and the difference, or 21° 18′, exhibits something of the grand scale on which "Precession" allows the time elapsed to be computed; though for the further and best method we must rather look to what takes place simultaneously on the equator.

The moment, too, that we do look there, behold! there was something else on the meridian simultaneously with the Pleiades and α Draconis, viz., no less admirably suited and most rare a phenomenon,—for it occurs only once in 25,000 years,—than the vernal equinox. So that the Pyramid's precessional interval in chronology is measured off, on no accidental part of the general circumference, but from the very time-zero of the chief time-circle in the sky, viz., from one of the equator's intersections with the plane of the Ecliptic. 4040 years ago the stars of that vernal equinox, viz., then the Pleiades and their enclosing constellation Taurus, crossed the meridian at midnight, in the autumn portion of the year, say, on the 22nd of September. Now, the same stars are not seen in a similar position until the 19th of November: wherefore the number of days elapsed from Sept. 22nd to Nov. 19th, at the approximate precessional (really *re*cession of the equinoxes) rate, of 70 years to a day, gives at once the absolute date of the Great Pyramid we are searching for. The only absolute date, too, that has yet been discovered in any of the early monuments of the Egyptian land, and where, as well as in all that concerns the early history of man, it is so much required.

Most remarkably and completely, therefore, is the Great Pyramid, by the facts of its primitive construction, the special monument of "the Pleaides year;" wherein the year begins on that night when the Pleiades "are most visible," *i.e.*, when they are seen uninterruptedly from sun-set to sun-rise, and that is again, when they cross the meridian at midnight. An inimitable primeval method of measuring time over long intervals is thus procured, and which the researches, chiefly of Mr. R. G. Haliburton, of Halifax, Nova Scotia (its modern discoverer it may be said), have tended to show was universal among mankind before they left patriarchal rule and went off on their own inventions of "theotechny," war, chronology by their supposed even number of days in a year, and astronomy, not by the sure method of meridian passages, but by the fallacious risings and settings of stars. But this unexceptionable Pleiades and pyramid method, long running in men's minds, though ill understood, as among the ancient Romans,* is found even yet in chronological life and activity among the entirely unchanged representatives of extreme antiquity, such as Australians, and other stationary races: in these days, invaluable representatives; for they have never, since their first supranatural dispersion, had the then imparted mental heirlooms of their race interfered with by the substitutions of other men's progressive development, and the artificial wonders,—changing from age to age like the patterns of a kaleidoscope,—of their school civilisation.

5. *The Interior of the Great Pyramid in its relation to the Interior of the Earth.*

When studying the *linear* dimensions of the Great Pyramid at p. 199, we found its one, all compelling feature for whole size, viz., vertical height, alluding in even terms of the Pyramid numbers $10^{3 \times 3}$ to that much-meaning symbol of *external* creation, the sun's mean distance from the earth.

But if we now commence an enquiry touching the next chief feature of physical description after size, viz., *weight*, our ideas are immediately turned towards our own *earth*, and there to its *interior;* which, if less extensive, is a vastly more mysterious region than the exterior creation around it. For the distant sun, in spite of his distance, we may both see and optically measure,—but who can look through the solid depths of the earth, right down to the very centre, and tell all the various products of which its mighty opaque mass is composed, and on whose multifarious chemical qualities the chief success of modern civilisation depends?

No man, of course, can do so, or knows, or need ever expect to know, how much gold, silver, iron, and other important elementary substances there may be still stored away within "the deep of the earth that lieth under." But taking the question in another way,—scientific measure

* "Candidus auratis aperit cum cornibus annum Taurus."—*Virgil.* A passage on which many commentators have long since remarked, "that it does not apply with astronomical truth to Virgil's own day, but to a period two thousand years earlier:" though why Virgil should have gone back to that previous time they had no idea.

can approximate to the *mean specific gravity* of the whole contents of the earth taken faithfully and on trust together; and can say with confidence, that the all-important quantity in physics thence arising, is close to 5·7 times the specific gravity of that ever present, almost universal, surface fluid of the earth, water. Whence, having already from the linear measures of modern science, the outside size and shape of the globe, we may readily compute its weight (here in a particular species of tons), and shall find it to amount to 5,271,900,000,000,000,000; (the numbers, however, not being trustworthy to more than the three, possibly four, first figures, and the remaining o's indicating uncertainty rather than exactness, in everything beyond "place.")

Now, between the *bulk* of the earth and the bulk of the Great Pyramid, there is no even numerical relation. Nor should there be, if the size has been settled upon a different feature of the Kosmos. But between the above *weight* of the earth and the *weight* of the Pyramid there may, or may not, be something of the kind; because, without in the slightest degree interfering with shape, size, or astronomical position, the architect might use a material of any given density.

Now, the substance which that primeval designer did choose, is very peculiar in respect of weight for a given bulk. For whereas, in terms of the earth-globe as unity, the specific gravity of granite is 0·479, and of what is usually called by builders in this country "common stone," 0·442, —that of the chief material of the Great Pyramid is only 0·412, and a portion so low as 0·367. Whence, calculating the separate weights of each portion, we obtain for the final sum of the Pyramid's weight (in terms of the same tons as before), 5,273,000 nearly;* or a quantity which may be practically regarded as being to the whole earth's weight, in the grandly round, even, and truly Great Pyramid proportion of $1 : 10^{5 \times 3}$.

The *linear* relations, however, of the Great Pyramid did not stop at a single cosmical equivalent of the whole, but proceeded, after the introduction of a subsidiary idea, to another and more practical relation on a very much smaller scale, and more fitted for everyday use among men. Is there, then, anything of the same kind in the *weight* relation of the Great Pyramid?

*See "Antiquity of Intellectual Man," by C. Piazzi Smyth, p. 475.

There is, indeed; and though the instrument thereof be shrouded in the eternal darkness of the most central interior, yet it signifies far and wide its ruling importance by spreading a certain type of its nature throughout the entire substance of the whole building, even to its very exterior, and equally on the four sides; accomplishing it thus.

In the highest, innermost, most granite-built, most regularly fashioned, and final chamber of the Great Pyramid, there is a long, hollow, lidless, rectangular box admirably carved out of a single block of hard red granite. It is the only piece of furniture in the room, and is quite loose, or stands freely, and may be pushed about hither and thither on the flat granite floor; and has been known to the world sometimes as a "sarcophagus" for burial purposes, and sometimes as a "vase," "coffer," "laver," "font;" and at last as a grand standard of capacity and weight measure,—the cubic contents of the interior showing the former, and the weight of water, at a certain temperature, required to fill it, the latter. John Taylor, too, went even a step further than his predecessors, in proving that the contents of the vessel were equal to those of the *laver* of the Hebrews, and its 4th part the same as the so-called *quarter* of early Saxon metrology,—but a quarter of *what* the oldest existing tradition had allowed to escape.

There is much in all this requiring independent mechanical proof before its full force can be appreciated.

First, for instance, if the vessel be loose, and the door high enough to allow of its passing in and out (and as the said door does by just a fraction of an inch), how are we to feel assured that such a portable thing has not been brought in at some long subsequent time, and not by the original builders?

In this way:—

The height of said coffer is to the length of two sides of its base as 1 to 3·141, &c.,* *i.e.*, it is a π proportioned coffer in a π proportioned Pyramid; a shape which applies to the Great Pyramid alone: while further still, the cubic contents of the interior, most carefully measured,† are 1-50th of those of an anciently and accurately marked off portion of the lower granite course of the chamber, itself containing the said coffer. That vessel, therefore, being found,—when tested by ideas which were never ventilated until the last few years,—to be so eminently appropriate in shape, as

* See St. John V. Day's "Papers on the Great Pyramid," p. —; also p. 13 of his "Plates and Notes on Structures called Pyramids:" Edmonston and Douglas, Edinburgh.

† See "Life and Work," vol. iii., p. 154 and 167.

well as size, to the very chamber and of the identical Pyramid in which it stands—cannot be other than the very original vessel for which the chamber was anciently built.

And what was it introduced there for?

"To serve as a sarcophagus," says the general Egyptological world. Wherefore, as we have always shown gentlemen of that creed so much respect, let us seriously examine their idea in this case also.

The sides of a granite sarcophagus in ancient Egypt were just the very places of all others, *teste* Sir Gardner Wilkinson, where the prospective occupier,—if rich enough,—or his friends after him, invariably caused his name, titles, wealth, and glory to be most signally and abundantly engraved. But there is not a shadow of carving, painting, or writing here, on what some persons will persist in calling "King Cheops's coffin." *

Again, *if* a mummied corpse used to be deposited in its sarcophagus at the usual place of abode amonst the living, and the lid of said vessel then fastened on securely before the transport to the tomb began,—such lid, if made according to any and every example of the period,† would add so many inches to the height of the *coffer of the Great Pyramid* (which is already higher than any known sarcophagus of the Old Empire), as totally to prevent it getting into that building at all, much less coming through the low doorway of its so-called King's Chamber.

There is, indeed, a certain partial ledge cut out in the top of the sides of the coffer, *almost* as if for fastening on a lid; but it would not have held, if tried; and is found, on measurement being applied, to supply a peculiar and, in the

* Though that King did not glorify *himself* by inscriptions either on the Coffer, or any of the finished surfaces of the vast monument in which it stands,—yet his name and memory have not perished amongst men, as some of the detracting class are fond of insisting on, when denouncing the building of the Great Pyramid as a piece of sheer barbarism and utter folly. For his true name, in the original Egyptian form or *Shofo*, was discovered by Colonel Howard Vyse when he excavated his way into certain hollow spaces of masonry above the King's Chamber, and found it painted on some of the stones for quarry marks, evidently by the working men of the period: a discovery which at once vivified and gave tangible substance to the Grecian form preserved in Herodotus, viz., *Cheops*.

True, indeed, that the name is preserved there, only as accompanied by a most odious tradition as to moral character; but that, as John Taylor showed, is rather to be taken as a measure of the *purity* of that King's religion, whose chief end and aim, according even to his enemies, was to root out idolatry from the land. And that beginning of the removal of the aspersions of 4000 years from the memory of one who worked not for himself but for God,—may, D.V., go on increasing until King Shofo shall become one of the best known names and most highly appreciated characters in primeval history.

† See the oldest granite sarcophagi at the Museum of the Egyptian Government at Boolak.

present day of mischievous dilapidations by travellers, a very much needed confirmation of the original full *depth* measure of the coffer. In fact, the whole structure teems, not with mortuary remains, but with practical geometry: for, in addition to the above, the thickness of sides and bottom have been made such, that the cubic contents of the bottom are half those of the sides; and the cubic content of both sides and bottom together are equal to that of the interior;* or the interior is just half the exterior. These proportions, however, are only obtained by theoretically filling up the said "ledge cut out;" but being so exactly obtained then, prove the said ledge to be adventitious. Taking the coffer, then, without that later infraction into its integrity of figure,—let us enquire, what determined its most important feature,—if a measurer of capacity—and that by which it is so largely distinguished from *bonâ fide* sarcophagi from other pyramids and neighbouring tombs,—viz., its *internal cubic contents?*

This. The 10^7th part of the earth's axis of rotation—cubed to represent solidity,—and then multiplied by the earth's mean specific gravity 5·7, divided by 10.

Now such 10^7th part amounts to 50 of the same Pyramid units, whereof we took 25 in the linear investigations: wherefore—

$$50^3 \times \frac{5{\cdot}7}{10} = 71{,}250.$$

or as close as the best measures in the same units have yet made the cubic contents of the coffer to be, or rather to have been, anciently. While again—

$$50^3 \times 5{\cdot}7 \times 5 = 3{,}562{,}500,$$

or the cubic contents, within a few inches, similarly measured, of the marked off portion of the lowest granite course of the chamber walls holding the said coffer.

The number 50, then, plays a very important part in this investigation; and appears here first, in terms of previously known linear Pyramid units, whereof 500 millions make the length of the whole axis of rotation of the earth. Now the chamber itself contains, or is made up of, as to its walls, 5 grand, even, regular courses of polished red granite, which courses run round and round the room in uninterrupted horizontal lines of remarkable mechanical perfection,—while the physical nature of their surfaces, glittering, characteristically for granite, with innumerable distinct

* See "Antiquity of Intellectual Man," pp. 295 to 301; also Mr. Henry Perigal's published "Original Dimensions of the Coffer."

and parti-coloured little crystals, beyond any other species of stone there, might be taken almost as emblematic of some mighty and extensive number, though not necessarily 100,000,000. But this is only a beginning; for next, in a far more remarkable and certain manner, the floor of this chamber (on which the coffer, so eminently depending on the standard of 50 of those units long, actually stands) is found to be coincident with the 50th course of colossal, wide-spread, horizontal masonry which forms the structure of the entire Pyramid:* and marks off outside, but for those only who know them by measure, the levels of the scientific chambers within.

Plainly, therefore, by this constantly increasing number of consilient and allied coincidences, we are dealing here with numbers and proportions arising from no mere accident. And when it has been still further and variously shown that the cubic contents of this unique Great Pyramid coffer were precisely those of the most sacred "Ark of the Covenant" of Moses, under a symbology, too, also indicating the solid earth as a whole: and when the marked portion of the chamber course is found equal in cubic contents to Solomon's Molten Sea; † and when the exact cubic contents of one-fourth of the coffer,—or equally the Hebrew chomer and Anglo-Saxon quarter,—have been found more recently ‡ to be expressed by the lower stone of that granite leaf, of which the upper one expresses the division of the sacred and Pyramid cubit into 5 and 5 again, or into the very units employed—we are plainly on a threshold where the primeval architect drew from the same eternal well of truth as did Noah, Moses, and David at almost equal intervals with him, through the ages; and where the material in which this first of architects in stone worked, has enabled his own *ipsissima verba*, as it were, to be preserved for our reading, testing, and guidance down to this very day. And if so, to raise this still further question above even all others, viz., if or rather when all these things were thus evidently "prepared from the beginning of the world," why have they been allowed only just now to be in part rightly apprehended?

Perfectly inexcusable, however, would it be in me, at this most extended point of a repeated paper, to crave a little more, and then, doubtless, more space still after that from our most complaisant and intelligent Editor; for there *now* rises in view really only the beginning of the end; or a vast,

* See "Life and Work," vol. iii, p. 172.
† See "Life and Work," vol. ii, p. 466.
‡ By Captain Tracey, R.A.

unsounded subject, and which, when it comes to be fairly worked out in its several applications to anthropology, past, present, and future, can hardly fail to transcend infinitely all that mere physical science will ever be able to accomplish for the final amelioration of the human race in its higher attributes and ultimate destinations. Yet is it only by physical science rightly applied that this ancient and mighty message of old, to the present day, can be read.

What crusade, then, is there of our times so important as, that of, or for, a correct, complete, and exact Great Pyramid investigation at the place? A few individuals—and by no means rich ones—have done their little in the cause most heartily: but the next steps are for kings, princes, and nations; and oh! that *they* would heedfully bear in mind that it is written,—"It is the glory of God to conceal a thing; but the honour of kings is to search out a matter."

V. STEAM BOILER LEGISLATION.

By Sir William Fairbairn, Bart., LL.D., F.R.S.

A QUESTION of great public interest has arisen touching the prevention of Steam Boiler Explosions. The desire for increased pressures of steam in order to save fuel, has led to steam boilers in many cases being taxed beyond their strength; and, as under the present system of obtaining motive power, these important vessels cannot be dispensed with, it has become necessary that some steps should be taken to protect her Majesty's subjects from the danger both to life and limb to which they are now constantly exposed. The number of fatal explosions which occur from year to year, and from month to month,—one explosion taking place every week, and one life being sacrificed every fourth working day in the year,—has called forth a spirit of inquiry into the causes of these unfortunate catastrophes, and to a certain extent has led to the adoption of preventive measures.

The Manchester Steam Users' Association was founded 15 years ago under the original title of "The Association for the Prevention of Steam Boiler Explosions, and for effecting Economy in the Raising and Use of Steam." It was established by a few philanthropic and disinterested gentlemen connected with manufactures, and carried out by the writer and owners of mills for self protection, and the reduction of those calamitous events, steam boiler explosions.

Such was the origin of the Association. It has ever since been in active operation, and while increasing in the number of its members, has proved not only of advantage to them but to the public generally. The investigations and experiments conducted by the Committee, have also conduced to improved forms and secure principles on which boilers should be constructed.

After the above Association had been in work for some years, several joint stock companies were established for carrying out inspection accompanied with insurance. It will be readily understood, however, that the object of a joint stock company must be the promotion of dividend; and as it has been proved by experience that inspection is very costly, while insurance is very cheap, it is evident that the introduction of the joint stock principle will militate against the efficiency of the inspections, and that it will tend to promote insurance rather than inspection. Insurance of boilers will not restore life after a fatal explosion has occurred, while the companies make no provision for the families and representatives of those killed and injured. They profess only to compensate the owner of the boiler, though it may have been old, worn out, and quite unfit for use, while they do nothing for the poor stokers who may have been killed or injured, or for their families who may have lost their bread-winners. It is not intended by these remarks by any means to condemn the principle of insurance generally, as it may advantageously be adopted as a provision against the loss of shipping at sea, the destruction of property by fire, or in the event of death—whether accidental or otherwise,—certain sums having been paid for the securities offered. This system of insurance is satisfactory where no inspection is required, but it is not satisfactory in its application to boilers with the view of preventing explosions, since this end can only be obtained by constant and costly inspection, which the demand for dividend on the part of the shareholders will tend to diminish to the lowest possible point. It is on these grounds that inspection carried out by joint stock insurance companies cannot admit of comparison with that of voluntary honorary Associations, whose sole object is—careful inspection, for the prevention of steam boiler explosions, and the saving of human life.

During the early days of the steam engine it was considered unsafe to work steam at more than 7 lbs. on the square inch, and no difficulty existed in making boilers to sustain that pressure. The first in respect of form was what was called the Haycock boiler; next came the waggon boiler, by Watt, which was the favourite for more than forty years; again followed the circular Cornish boiler used for pumping engines, which for a number of years was worked at a pressure of 30 to 40 lbs. on the square inch. These were the pressures at which Cornish boilers were worked forty years ago, but the extension of trade, and the demand for increased pressure and increased power have produced great changes. It was found that, in order to make an engine do double the work, it was only necessary to double the pressure and increase the velocity or number of strokes until the required force was obtained. This was done with great advantage as regards a saving of fuel, but not without risk; and thence ensued a continued series of break downs and boiler explosions. These at one time became so frequent that the writer was for several years summoned by coroners' juries to investigate the causes of these disasters, and to suggest remedies such as appeared necessary to reduce the risk, and, if possible, to prevent the lamentable results of boiler explosions. These enquiries ultimately led to the formation of the Association above described, which since that time to the present has been of great benefit to the public.

From the greatly increased and still increasing pressure of steam at which engines are worked will be seen the importance of having perfectly secure and suitable vessels to contain a force of so destructive a power as steam. It does not appear necessary to adopt a retrograde process, and reduce the pressure to 7 or 10 lbs. as that of former days. On the contrary, it is desirable to prepare the public for a still further increase of the working pressure if we are to secure a satisfactory result of economy in the use of fuel.

All these elements connected with steam, will, therefore, have to be carefully considered in order to acquire a motive power calculated to cheapen production in every stage of our manufacturing process. This, it will be observed, can be accomplished with perfect safety provided that boilers are made on sound principles of construction, and accompanied with the use of sound material and good workmanship. It must be borne in mind that the practice of boiler construction, which at present exist cannot, in many cases, be too severely censured, as these constructions demands the skill of our best engineers, and the exercise of definite laws founded on experimental research.*

* It not unfrequently happens that some of the mill owners in the manufacturing districts are their own engineers, in which they frequently stipulate for having their own proportions of steam engines, such as the diameters of the

It has been stated, and that correctly, that boiler explosions may be reduced to a minimum, if not entirely prevented, by competent periodical inspection; and since steam users neglect so simple a precaution, and persist in sacrificing 75 lives every year, it must clearly be the duty of the Government to interfere on behalf of those whose lives are jeopardised, and to enact that no boiler shall be worked unless periodically examined and certified. To arrive at the best plan, however, of administering this inspection so as to prevent explosions on the one hand, without unduly interfering with the liberty of the steam user on the other, is a problem of some difficulty. It received the most careful attention of the Committee of the Manchester Steam Users' Association, and after much consideration they drew up a report on the subject, giving the conclusions at which the Association had arrived after fifteen years' experience. These conclusions are as follows:—

Proposition No. 1.

That the use of steam, &c., as at present conducted, entails great suffering from the destruction of life and property occasioned by the constant recurrence of boiler explosions.

That boilers are now to be found under the pavements over which the public walk, behind walls close to which they pass, in the basement of buildings crowded with busy workpeople; and, that, in short, they are to be found everywhere. That many of such boilers have given rise to the most disastrous explosions, so that the lives of all those living near so dangerous an instrument as a boiler, or even casually passing by, are seriously jeopardised unless suitable precautions are adopted to ascertain whether the boiler be safe and trustworthy; and, if not, to render it so. That most of those who have suffered from these explosions have had no voice in the management of the boilers, and thus were helplessly victimised, some being women in their own houses, and others, children at play. Further, that in the generality of cases, those injured by the explosions of boilers at the works at which they earn their livelihood are in a similarly helpless position.

That the subject, therefore, becomes one of general and public interest demanding immediate investigation, more especially since the use of steam is daily on the increase,

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cylinder, air pump, length of stroke, &c. They also in some cases stipulate for the dimension, form, and construction of boilers; but it must be admitted that in many instances their knowledge is not inferior, and in some cases superior, to the engineer they employ.

and notwithstanding any precautionary measures at present adopted, explosions still recur with the most persistent regularity and frequency.

The following list exhibits the numbers of killed and injured during the last 16 years.

Table showing the Number of Explosions Recorded by the Manchester Steam Users' Association from the commencement of 1855 *to the* 31*st of December,* 1870.

Year.	Number of explosions.	Persons killed.	Persons injured.	Total.
1855	10	10	1	11
1856	31	58	70	128
1857	27	67	58	125
1858	33	78	100	178
1859	26	57	65	122
1860	22	45	36	81
1861	22	30	51	81
1862	28	87	89	176
1863	48	76	80	156
1864	33	65	91	156
1865	48	46	79	125
1866	72	87	108	195
1867	36	60	67	127
1868	45	57	60	117
1869	58	86	126	212
1870	51	78	106	184
Total, 16 Years	590	987	1187	2174

From this Table it will be seen that 590 explosions have occurred since the foundation of the Association, killing 987 persons, and injuring 1187 others, while to this it may be added, that many thousand pounds' worth of property has been destroyed. This return, however, does not include the total number of explosions, or the total number of persons killed and injured during the period referred to, since in the early years of the Association's operations such complete records were not kept as is now the case, so that many escaped its notice. At the present time there occur on an average 50 explosions a year, killing about 75 persons and injuring as many others, so that an explosion occurs every week, and one person is killed every fourth working day in the year.

Proposition No. 2.

That boiler explosions are not a necessary consequence of the use of steam, but that they are preventable. That though complicated in result, they are simple in cause; arising in the main from bad boilers, bad either in construction, or bad in condition. That six explosions are due to bad boilers, through the neglect of the boiler maker or boiler master, for every one due to the neglect of the boiler minder. That competent inspection is adequate to detect the badness of the boilers, and thus to prevent by far the greater number of the explosions now occurring.

In support of the statement that explosions arise from simple and preventable causes, the following Table may be adduced.

Table showing the Causes of the Steam Boiler Explosions that occurred from the 1st of January, 1861, to the 18th of June, 1870.

Cause of explosions.	Percentage of whole number.	Number. of explosions.	Persons killed.	Persons injured.
Malconstruction of the boilers, either in the shells or fittings	40	120	216	268
Defective condition, either in the shells or fittings	29	88	188	239
Failure of the seams of rivets at the bottom of externally fired boilers	15	44	64	67
Overheating of the plates from shortness of water	10	30	23	23
Overheating of the plates	—	—	—	—
From incrustation.. .. .. (6) From the use of boiler compositions .. .. (1) From causes requiring further investigation .. (1)	3	8	14	12
Excessive pressure through attendants tampering with safety-valves	2	5	10	12
Explosion of economiser, but whether from gas or overpressure of steam is uncertain	½	1	—	—
Cause entirely independent of the condition or construction of the boiler, and may be termed accidental	½	1	1	1
Number of cases in which the cause has been investigated	100	297	516	622
Number of cases in which the cause could not be investigated for want of sufficient particulars	—	114	123	160
Total number of explosions	—	411	639	782

In the records which the Association has filed, minute particulars and illustrations are detailed of the majority of the explosions narrated in the above Table. From this it will be seen that the greater number of the whole arose from malconstruction and defective condition, the danger arising from which competent inspection is able to detect in time to prevent explosion. It will also be seen that adding the number of explosions that arose from overheating of the plates, whether from shortness of water, or from incrustation to those arising from the attendants tampering with the safety-valves and attributing them all to the neglect of the boiler minder, that they do not exceed 15 per cent of the whole number,—or, in round numbers, one in seven, so that, as stated above, where the stoker or boiler minder has to answer for one explosion, the boiler master and boiler maker have to answer for six. It is important that this should be kept in view, as the attempt is constantly made to attribute the majority of explosions to the fault of the boiler minder, and thus to show that, after all, inspection is unable to grapple with steam boiler explosions. A review, however, of the facts of the case shows how ill founded these assertions are.

To this it should be added, in justice to the poor boiler minder, so often abused, that it is by no means clear that he should be made accountable for the whole of the 15 per cent of the explosions referred to above, and they were only tacitly admitted in the course of argument that it might be apparent that the case in his favour was not over-stated but under-stated. In many of the cases of explosions from shortness of water, the boilers have been lamentably deficient of suitable mountings, a condition for which the engineer and employer are exclusively responsible.

Proposition No. 3.

That notwithstanding the proved efficiency of competent boiler inspection, and the publicity constantly given to the subject, yet that steam users refuse to protect the lives of their workpeople, or those residing near to their works, by having their boilers inspected. That it appears approximately that out of more than 100,000 boilers in the country, only 20,000 are enrolled either with inspecting associations or insurance companies, so that out of every five boilers one only is enrolled. That a great number of boiler owners are totally ignorant of the risk to which they expose their own lives and those around them, and in many cases are only undeceived by the shock of explosion. That, judging from experience, there can be no doubt that there are now a number of dangerous boilers on the very verge of explosion being worked on at the risk of all those living near them. That under these circumstances the public safety demands that competent periodical inspection should be enforced by law, and that it should be rendered as illegal for the lives of the public to be jeopardised by dangerous boilers as by the storage of gunpowder, petroleum, or other explosive materials.

The committee have most reluctantly arrived at the conclusion that it is necessary to enforce inspection by law, but the inexorable logic of facts has left them no alternative.

This Association, as already stated above, was founded fifteen years ago for the administration of a system of voluntary periodical inspection with a view to the prevention of steam boiler explosions. The experience of that term of years has shown that inspection is adequate, but that voluntaryism is not. Inspection has succeeded, voluntaryism has failed. The difficulty is not with the boilers, but with the boiler owners. Inspection will prevent explosions, but the owners will not have their boilers inspected.

The cause of this neglect of inspection may in many instances be simple ignorance; and it is thought that the recent explosion at Liverpool may be taken as a representative one. Here was an old second-hand worn-out boiler eaten away by corrosion till reduced to about a sixteenth of an inch in thickness, yet worked at a pressure of 60 or 70 lbs. on the square inch. It was situated in a populous part of the town, and surrounded with dwelling-houses. It was only cut off from a public thoroughfare by a 9-inch wall, which afforded concealment but no protection. The boiler exploded; the sanctity of the dwelling-houses all around was invaded; the greater part of the boiler was hurled into one house, and a fragment into another, while the ruins of the boiler setting and other *débris* were scattered in every direction. Two children in one house were killed, and a woman injured; another child was killed in the public street, and a lad at the works scalded and crushed to death. Yet the owners appear to have been ignorant of the danger. The boiler had but a few months before received a home-spun inspection, and been declared perfectly sound, and equal to last the owners' lifetime. The owners trusted it; both of them were within a few yards of the boiler at the moment it burst, and narrowly escaped being killed themselves. The destruction of their premises exhausted their resources, and thus left them unable to repair the ruin their bad boiler had caused. The public outside had no voice in the management of this boiler, yet they had to suffer the consequences of its explosion. All this suffering may be fairly attributed to ignorance on the part of the owners, or they would not have jeopardised their own lives in the manner they did. Nothing but coercion in some form or other will meet such cases as this.

Proposition No. 4.

That although it is necessary in the interest of the public that inspection should be enforced by law, it is not advisable, either in the interest of the steam user or the public at large, that that inspection should be undertaken by the Board of Trade or any other department of the Imperial Government, as such a course would, it is feared, be a source of annoyance to the steam user, and hamper progress.

The above proposition needs but little to be said in its support, as the view is so generally entertained that an enforced system of inspection administered by the Government would be irksome and obstructive. Without any disrespect to the Board of Trade or any other department of the Government, it is thought that such a system would have a tendency to be always behind the times. As improvements were made from day to day, the system would require to be constantly modified, which it is thought with a Governmental administration it would be difficult to accomplish.

Proposition No. 5.

That while the administration of a system of enforced inspection should not be committed to the Imperial Government, neither should it be committed to municipal governments, nor to private inspecting associations, insurance companies, boiler makers, or others, as there would then be no guarantee for the virtue of the inspection. That conducted by town councils would be liable to be biassed by party interests, while the practice of one municipality would vary from and contradict that of another. That conducted by private associations and companies, and more especially by private individuals, would be irresponsible; and as those who gave the greatest license would get the greatest number of fees, the certificates would soon become worthless and degenerate into a sale of indulgences, so as utterly to defeat the object in view of promoting the public safety.

In support of the proposition that the practice of one municipality would vary from and contradict that of another, a brief extract may be given from the report presented to the last meeting of the British Association for the Advancement of Science on the subject of Steam Boiler Explosion Legislation. "The science of boiler making is progressive; it is in a transition state, and in spite of the amount of information constantly disseminated, great ignorance prevails with regard to it. In consequence of this, one corporation would declare a boiler safe which another corporation would declare unsafe; so that a boiler carried by rail from one part of the country to another might be counted safe at the beginning of its journey, and unsafe at the end. For instance, in Lancashire, the practice of strengthening flue tubes at the ring seams, with flanged joints, or hoops of T iron, or other suitable section is highly approved.* In fact, it is thought that no high-pressure boiler should be constructed now-a-days without these appliances. In Cornwall, however, nothing can convince steam users of their necessity, and Cornishmen persistently adhere to the idea which the Franklin Institute of Pennsylvania endeavoured to dispel thirty-four years ago, viz., that a boiler cannot explode as long as it is properly supplied with water. They appear to believe that furnace tubes, though of great length and diameter, and though worked at high pressures of steam, can only collapse from the neglect of the water supply, or in other words, from the neglect of the attendant, and not of the owner or the maker. In Cornwall, boiler flue after boiler flue collapses, simply from weakness, till the Cornish boiler stands in the return of explosions as one of the most dangerous."

Proposition No. 6.

That while it is advisable that inspection should be enforced by law, though not administered by the Government, whether imperial or local, nor by private inspecting associations, insurance companies, or boiler makers, lest the administration should be arbitrary in some cases and contradictory and inefficient in others, yet that a system of administration, competent to secure the interests of the public on the one hand, without detriment to the steam users on the other, might be framed on the following basis:—

To secure the efficiency of the inspection, let the administration be national, so as to be above all local party or private interests, and let it be undertaken not for profit, but to promote the public safety. To prevent the administration becoming arbitrary, stereotyped, and old-fashioned, and to render it capable of adaptation to the constantly-altering and growing requirements of the boiler owner, let it be administered by a series of district Boards elected by the steam users themselves; the Boards having the power of making such laws, rules, and regulations from time to time as might be found necessary for conducting the service. Such a mode of administration would, it is thought, not only secure efficient inspection adequate to prevent explosions, but also be considerate to individual steam-users, and be found not to hinder but to assist engineering progress. On this system the inspection would be enforced by law to render its adop-

* For the experiments connected with this subject—*the Collapse of Tubes*—see "Philosophical Transactions."

tion universal; it would be administered by the steam-users themselves to prevent its becoming arbitrary; and, finally, it would be founded on a national basis to secure its accomplishment.*

Such are the conclusions at which the Manchester Steam Users' Association has arrived after mature consideration. It will be understood that the district Boards proposed, are to cover, in their operations, the entire country. Also, that they are to be of mixed constitution, most of the members being men of commerce, *i.e.*, employing steam power for mercantile purposes, and others, men of science, *i.e.*, engineers, chemists, and others, competent to advise on matters affecting the inspection of boilers, or the interests of steam users generally, to assist at the counsels of the Board, and to add weight to their decisions. These Boards to be empowered to impose such a boiler-rate as might be found necessary to ensure competent and thorough inspection. Monthly Reports very similar to those of the Manchester Steam Users' Association to be issued, these reports giving the number of inspections, the state of the boilers, &c. It is further proposed that the monthly reports, or the substance of them, should form the basis of an annual report, to be presented to the members of both Houses of Parliament.

On this plan, it is thought that a system of periodical inspection would be insured, which would be competent to prevent steam boiler explosions, and thus save the lives now sacrificed, without in any way incurring unnecessary interference with the steam user, but protect alike the interests of manufacturers and the public. Ultimately it would prove a help and not a hindrance to scientific and commercial progress.

On further inquiry, other suggestions and improvements will no doubt present themselves which might be adopted without detriment to the fundamental principles of the system proposed, which are, firstly—that steam boiler inspection should be rendered compulsory, and, secondly, that it be allowed to be administered by a national series of district Steam Boards, appointed in the main, if not entirely, by steam users themselves.

It may be interesting to append, in illustration of the foregoing remarks, an engraved photographic view of the

* For somewhat fuller details of the proposed National Steam Boards, see Report on "Steam Boiler Explosion Legislation," presented to the last meeting of the British Association for the Advancement of Science.

FIG. 8.

EXPLODED BOILER NOT UNDER THE INSPECTION OF THE MANCHESTER STEAM USERS' ASSOCIATION.

The Explosion occurred at Messrs. Chapman and Holland's, Ashley Lane, Manchester, on Monday, December 23rd, 1867.
Six persons killed, and four others injured.

destructive effects of boiler explosions. It is taken from some hundreds in the possession of the "Steam Users' Association for the Prevention of Boiler Explosions." It does not exhibit one of the most fatal and disastrous occurrences to life and property as some others; but the catastrophe is sufficient to claim the sympathies of the public, and to enlist the interference of the Government, and that more particularly when we are assured that immunity from these disasters may be obtained from careful periodical inspection. It is not high pressure steam that we have to fear, but unsuitable vessels, designed and constructed by ignorance, and want of proper inspection we have to guard against. These defects remedied, by sound principles of construction in the first instance, and careful inspection in the second, would go far to make boiler explosion the exception and not the rule.

"This explosion, which took place a few minutes after one o'clock on the afternoon of Monday, December 23rd, was of a very disastrous character, as many as six persons being killed, and four others injured, while the dye works at which it occurred were levelled to the ground.

"The boiler, which was set immediately under the drying room, was of the ordinary Cornish construction, being internally fired, and containing a single furnace tube. Its length was 18 feet, its diameter in the shell 6 feet, and in the furnace tube about 3 feet 2 inches, while the thickness of the plates was three-eighths of an inch, and the blowing-off pressure about 25 lbs. on the square inch.

"The boiler failed at the bottom of the external shell, rending longitudinally from one end to the other, when the whole was lifted from its seat, and the entire works laid in ruins. It was to this that the loss of so many lives was due, and not to scalding, or injuries received directly from the boiler, but to the fall of the works upon the poor fellows who were crushed and buried in the ruins.

"The cause of the explosion was but too apparent on examination. The boiler, which was set on a midfeather wall, had been shamefully neglected; the external brickwork flues were very damp, leakage had occurred at many of the seams, the boiler had not been properly examined, and external corrosion had been allowed to go on eating into the plates till they were wasted away at the bottom of the shell to the thickness of a sheet of brown paper, almost from one end of the boiler to the other. The boiler was totally unfit for work, and it is a matter of surprise how it withstood the steam pressure at all. The explosion is a striking instance of the folly of neglecting periodical inspection.

"At the coroner's inquest the jury returned a verdict of 'Accidental Death,' adding that they considered great neglect had been displayed by the conduct of the employers, and urging the coroner to call the attention of the Home Secretary to the fact that it is considered desirable that the periodical inspection of boilers should be compulsory."

VI. THE ECLIPSE OF LAST DECEMBER,

WITH SPECIAL REFERENCE TO THE INTERPRETATION OF THE SOLAR CORONA.

By Richard A. Proctor, B.A., Cambridge.

SIX months ago, I dealt in these pages with the circumstances of the eclipse to which astronomers were at that time looking hopefully forward. The plate which illustrated my paper showed the course which would be followed by the moon's shadow as it swept across the earth's surface. In that plate, the chief observing stations were also indicated; while in the body of the paper I discussed the modes of observation which astronomers and physicists thought of applying. I have now to exhibit the results which have rewarded the exertions of those who have taken part in the various expeditions.

There were so many observers, however, and the reports already to hand are so voluminous, that it seems advisable to make a selection of those results only which throw a new light on problems of solar physics. Indeed, it chances unfortunately that too many of the observations which were made seem to have been intended to resolve problems already long since disposed of. Precisely as we find that the American observers in 1869 were for the most part little familiar with the history of former eclipses, so during the late eclipse the larger proportion of the observers would appear to have been quite unprepared for phenomena which have been described over and over again by the observers of past eclipses. It would seem as though that preliminary examination which the Astronomer Royal so earnestly advocated as respects modes of observation, might also have been well extended to the historical details of eclipses.

Broadly stated, the object of the expeditions was the determination of the real nature of the corona—that crown of glory which never fails to make its appearance when the moon has concealed the whole of the sun's disc. The views of astronomers and physicists respecting this striking appearance had been growing gradually clearer, one theory after another being disposed of until certain very definite issues alone remained to be considered. The theory that the glory is a lunar phenomenon had been rejected because modern observations prove so conclusively that the moon has not an atmosphere of appreciable extent. So, also, the theory that the corona is due to the diffraction of the solar rays as they pass by the moon, had been shown to be erroneous. But another theory which seemed much more worthy of support had also been successfully oppugned: the fact that all round the sun a glory of bright light appears when the sun's disc is concealed, suggests, on the face of it, that the sun has an atmosphere of great extent, either self-luminous or brilliantly illuminated by the sun himself. This view had been supported by astronomers of eminence, despite certain difficulties which it undoubtedly presents; but recent researches had caused grave doubts to be cast upon this imagined solar atmosphere. Let us briefly consider the nature of these researches.

The coloured prominences which are seen around the sun during total eclipse had been shown to be gaseous, a chief element in their constitution being the gas hydrogen. The coloured *sierra*, too, which appears at a lower level—that is, closer to the sun—had been found to be as its appearance implied, almost identical in structure. During the spectroscopical examination of these objects, peculiarities of appearance had been noted in their characteristic bright lines which seemed to throw important light upon their nature, and, indeed, on the general habitudes of the solar atmosphere. The peculiarity which now specially concerns us is *this*, that the spectrum, though undoubtedly indicating an increase of pressure with approach to the sun's surface, yet by no means indicated such an increase as was to have been expected if the sun has an atmosphere of considerable extent. The researches of Plucker, Hittorf, Frankland, and Huggins had shown that the spectrum of hydrogen varies according to the circumstances of pressure and temperature under which the gas subsists; and the widening of the hydrogen lines had come to be regarded as in a special manner indicative of increase of pressure. Mr. Lockyer to whom Frankland exhibited the spectrum of hydrogen under varying conditions, found thus a new significance in the observations made at the telescope. He found that, so far as the evidence of the laboratory experiments could be depended upon for determining pressure, the glowing hydrogen even at the base of the *sierra** exists at a pressure considerably less than that of our own atmosphere at the sea-level. Wüllner assigned the limits of pressure at the sun's surface at two inches and twenty inches of the mercurial barometer (under the earth's attraction).

This seemed to dispose conclusively of the theory that the corona is a solar atmosphere properly so-called. Without accepting the somewhat *bizarre* notion put forward by Mr. Lockyer, that the *sierra* is itself the solar atmosphere, we seem yet fairly led to believe that that atmosphere into which the glowing hydrogen which forms the prominences has been flung, and in which smaller prominences and the remains of larger ones probably commingle to form the sierra, can extend but to a moderate distance above the summits of the loftiest prominences. For undoubtedly an atmosphere extending even to but 80,000 miles from the sun (and many prominences are far higher) and subjected to his enormous attractive power, could not fail to exert a pressure enormously exceeding that of our own atmosphere at the sea-level, unless this solar atmosphere were of extreme tenuity. I know not, indeed, whether even our so-called vacuum-tubes ought not to be regarded as densely filled with matter, by comparison with the outer regions of a solar atmosphere, if, with a height of some 100,000 miles at the very least, such an atmosphere is to exert at the sun's surface no greater pressure than our own.†

It was probably such considerations as these which led to the adoption by Mr. Lockyer of Faye's idea that the corona is a phenomenon due merely to the passage of the solar rays through our own atmosphere. It seems at a first view, more especially to those unpractised in considering the

* By *sierra* I mean the coloured layer or envelope of prominence matter to which the name of *chromosphere* has been applied. *Sierra* harmonises so well with the word *prominences*, that it is to be hoped this term, employed so many years before by Airy, Leverrier, and others, will before long dispossess so questionable a title as "the chromosphere."

† There are few problems more difficult than those suggested by the subject here touched upon. An atmosphere of hydrogen 80,000 miles deep, and at the upper limit existing at the very lowest pressure which our physicists can be assumed to have attained by any process of exhaustion, would yet at a depth of 60,000 miles, under the action of solar gravity, exert a pressure exceeding fully a hundredfold that of our own atmosphere at the sea-level. Nor is the difficulty removed by supposing, as I have done in my Treatise on the Sun, that the real pressure close by the photosphere exceeds manifold Mr. Lockyer's estimate. This supposition, shown by Professor Young's observations last December to be correct, yet leaves the pressure at a depth of 60,000 miles below the summits of the loftiest prominences, or at a height of twenty or thirty thousand miles above the photosphere, altogether less than can be explained by any physical relations we are familiar with at present.

relations of tridimensional space, that when the sun is directly behind the moon, as in a total eclipse, his rays passing grazingly by the moon, must so illuminate our own atmosphere as to exhibit a glory of light—a real radiated corona—around the dark disc of the moon; and it was this consideration—plausible, but beyond question erroneous—which was the original basis of Faye's "atmospheric glare" theory. Few but mathematicians would have been prepared to hear—as Baxendell, Curtis, Harkness, and others at once pointed out—that the illumination by direct solar light could not in any ordinary total eclipse cause a ray of light to come from a space of many degrees in width all round the place of the eclipsed sun. A negative corona, that is, a corona of blackness softening off all round towards a background of considerable brightness, would be the real appearance resulting from the effect of direct sunlight.*

This having been proved, the atmospheric glare theory was modified by the introduction of a supposed action exerted by the moon on the solar rays. The nature of this action was not definitely (or indeed at all) stated; but the theory thus modified agreed, so far as it went, with La Hire's, which had been long regarded as untenable. As a matter of fact, also, while introducing fresh difficulties, it removed none of those urged by Baxendell and others.

It is bare justice, however, to the supporters of the atmospheric glare theory to point out that, as the eclipse of last December approached, the objectionable features of the theory were removed, and views were put forward which undoubtedly agreed much better with observed facts. Let me not be misunderstood, as respects my use of the word objectionable. To an erroneous theory, in the abstract, no objections need be made; since the enunciation and demonstration of the true theory disposes of the incorrect one without occasion for a special refutation. But it sometimes happens that the enunciation of an erroneous theory at a particular time may interfere with the progress of observation—either by distracting the attention of observers, or by causing them to under-estimate the interest of the object which they are to study. And beyond question, if the corona might by any possibility be no more noble a phenomenon,

* On this subject, Sir John Herschel writes to me—"Placing the limits of what may properly be called the earth's atmosphere at 100 miles high, then at the borders of a circular section subtending an angle of about 23 degrees in diameter every trace of light reflected in our atmosphere would be quite evanescent." This precisely accords with the objections I urged last year against the atmospheric glare theory as originally propounded.

no better worthy of study, than the radial beams of light which extend from between clouds over large arcs on the heavens, the enthusiasm of eclipse observers might well have been diminished. It was, indeed, on this account that, more than a year ago, I earnestly and persistently dwelt upon the objections, simple and obvious though they are, against the atmospheric theory, pure and simple, or modified so as to include lunar action. The new theory, fortunately adopted early enough to prevent the mischief I had feared—was not only far more accordant with observed facts, but even if erroneous could in no way interfere with the work of observers.

It was now urged that the bright light seen close around the moon during total solar eclipse may be the real cause of the atmospheric glare which the theory requires in explanation of the fainter light seen farther from the moon.* This, in fact, corresponds with what I had myself pointed out in oppugning the atmospheric glare theory ("Monthly Notices of the Royal Astronomical Society" for March, 1870, p. 141), when I wrote that "the only light which can reach this part of the atmosphere" (the part towards the moon's place) "is that from the chromosphere and the coloured prominences, or from the earth and surrounding illuminated air." It is

* As it has been denied that the atmospheric theory had been in any way modified, I may as well remark at this point that my only means for determining what the original theory was, had been the words used in its enunciation. They asserted that "the corona is nothing else than an effect due to the passage of sunlight through our own atmosphere," and they were understood by Baxendell, Curtis, Harkness, myself, and others, to mean that the air seen towards the moon's place is lit up by direct sunlight. Nor was exception taken for several months to the fact that our objections were founded on this supposition. Last June, however, it was said that the theory had been misrepresented, and in *this* way, that no notice had been taken of "a possible lunar action" really included in the theory. Here, it should seem, was the opportunity for introducing as a part of the theory the possible chromospheric action now believed in; yet nothing was said of it. In writing on the late eclipse, however, Mr. Lockyer says of his own views, after the eclipse of 1869:—"I thought the explanation still possible which regarded the corona as of terrestrial origin; that is, which assumed it to be an appearance due to the presence of light" (no longer sunlight, it will be noticed), "in our own atmosphere. The problem was one of such difficulty that there seemed a possibility that, by some unexplained cause, some of the solar light might be diffused and beat out of its course, and then, mixing up with the light of the chromosphere, give us a sort of continuous spectrum; in other words, that as the eye perceives a bright irregular region or glare round the uneclipsed sun, an effect due to our atmosphere, so also the eye might perceive a bright irregular region or glare round the *uneclipsed chromosphere during eclipses*, due also to our atmosphere." These are, indeed, other words, for they have another meaning. In fact, if the atmospheric glare theory had originally been propounded in the vague form here assigned, it would have escaped attack. The "possibility" that "a sort of" effect might result from "some unexplained cause" would be difficult to disprove.

obvious that the air above and around the observer of a total eclipse must be lit up by the prominences and sierra, since these are actually seen; and, *à fortiori*, it must be lit up by that intensely bright inner portion of the corona which has so often led the observers of total eclipses to suppose that a ring of real sunlight has, in fact, remained unconcealed; though it must be noted at the same time, as I pointed out in the paper just quoted, that the light from this source "would extend over the moon's disc, since it would illuminate the air between the observer and the moon's body." This, indeed, as we shall presently see, has been abundantly confirmed by the observations made during the recent eclipse.

It is clear that, even though the corona thus accepted as real by the supporters of the glare theory, should, in fact, be but a portion of the complete corona, the enunciation of the theory could in no way interfere with the observations to be made by the eclipse expeditions. On the contrary, the very attempt to distinguish where the real corona ended and the atmospheric corona began, could scarcely fail to be rewarded by results of extreme importance.

Now, so far as the evidence available before the late eclipse was concerned, we may select direct observations formerly made, and the spectroscopic observations made during the Indian and American eclipses (of 1868 and 1869) for consideration. Photographic evidence was not wholly wanting, but so far it was certainly unsatisfactory. Polariscopic evidence, again, had seemed (and still seems) most vague and contradictory.

But direct observation had already revealed many facts bearing most importantly on the question of the corona's real extent.

To begin with, on the very point which was assigned strangely enough by the organising committee as the principal object of the expeditions, "the differentiation of the outer layers of irregular outline, and a stratum, say some 5′ or 6′ high close around the sun," the history of coronal research is decisive. Plantade and Capiés, in 1706, had already detected the difference, which 164 years later has been called "the key-note of all the observations" made last December. In 1842, Arago had dwelt specially on the same point. In fact, nothing in astronomy has been more thoroughly determined than the general uniformity of the coronal light up to a certain moderate distance from the sun, where a rapid but not sudden degradation takes place.

Yet, again, on the question whether the fainter and radiated part of the corona, even to its extremest extent, actually belongs to a solar appendage, the history of former researches speaks very plainly. These radiations have been seen to remain unchanged in position, not only during totality, but for twenty, thirty, even forty seconds afterwards. They have also presented the same figure as seen from distant stations. So that, setting aside, as we justly may, all the accounts of seeming changes in certain radiations (since such changes may so easily be explained, or are, rather, on any theory to be expected), the positive evidences, which there is no misinterpreting, force on us the conclusion that the true solar corona has radiations extending to enormous distances from the sun.

As respects the spectroscopic evidence, there seemed room for doubt. Lieutenant-Colonel Tennant had in 1868 obtained from the corona a faint continuous spectrum, without either dark lines or bright lines.. The American astronomers in 1869 had seen different spectra: in some cases a single bright line on a continuous spectrum, in others a bright line and two fainter ones, while in yet others a continuous spectrum was alone discerned.*

There was little room for doubt as to the significance of these observations. In the "Quarterly Journal of Science" for October last, I pointed out reasons for believing that all the observations should be accepted, the apparent difference between the results being regarded as due in all probability to a difference in the opening of the slit of the spectroscope.

This view,—abundantly verified by the observations made during the late eclipse, leaves us a double spectrum to interpret,—a continuous spectrum and a spectrum of bright lines. Assuming that the background of the spectrum really is continuous—that is, that no dark lines exist in it—we should infer that the source of light is matter in the solid or liquid form, and incandescent through intensity of heat. It is possible, however, that dark lines exist, but are unseen on account of the faintness of the spectrum; in which case

* I may take this opportunity of correcting a mistake which has been made—in these pages and elsewhere—with respect to Professor Young's observations of the coronal spectrum in 1869. He saw, indeed, three lines, but he was not sure that more than one of these—the line 1474 Kirchhoff—belonged to the corona. "I have felt," he writes, "somewhat annoyed by finding the other two lines put on the same footing as 1474." This is a point requiring correction, though surely not calculated to annoy. The mistake arose from the inexact wording of the American reports; and may be compared to the erroneous account given by the American observers (including Professor Young) in 1869 of the observations made by Tennant in 1868—his continuous coronal spectrum appearing in their accounts as a spectrum showing the solar dark lines.

we may regard this portion of the light as reflected sunlight. As respects the part of the light which gives the bright line spectrum, we should infer that the source of light is luminous gas or vapour,—with this *proviso*, that we need not necessarily have to deal with masses (still less with a complete atmosphere) of such gas, but possibly with the light due to such electric discharges as are assumed to cause the appearance of our own auroras. This view seems at least far from improbable, and it is supported by the fact that the conspicuous bright line of the coronal spectrum is one of the principal lines of the auroral spectrum. Assuming it to be true, we should remain in doubt whether the vapours corresponding to the lines exist as vapours in the space traversed by such electric discharges, or whether those lines indicate the nature of the particles or bodies between which the discharge passes.

But doubts still rested on the observations made by the American astronomers. Observers in Europe seemed disposed to question the accuracy of their American fellow-workers. "The evidence furnished by the American observers," wrote Mr. Lockyer, "is *bizarre* and puzzling to the last degree;" and he referred to observations of his own, causing him to "hesitate to regard the question as settled." The fact, also, that a double spectrum had been seen, was referred to by Mr. Lockyer as "hard to understand, unless we suppose the slit to have been wide, and the light faint, in either of which cases final conclusions can hardly be drawn either way."* With a caution not very complimentary to American men of science, their results were relegated to the eclipse of 1870, for confirmation or disproval by European observers.

Fortunately, in this respect, the observations of last December were most satisfactory and conclusive. One of the first telegrams received from the shadow-track was one in which Mr. Lockyer admitted that the observations made by the Americans in 1869 had been confirmed. And the first detailed account published in England, in the form of a letter addressed to the Editor of the "Daily News," by Father Perry, the head of one of the observing parties sent out to Spain, exhibited very decisively the same result. The following extract from Father Perry's narrative may fitly introduce the more detailed evidence to be subsequently given. Knowing that the unfavourable sky would render observations with larger spectroscopes quite impracticable,

* From an article in the first number of "Nature," (Nov. 4, 1869.)

"I desired Captain Maclear," writes Father Perry, "to observe with a small direct-vision Browning spectroscope, attached to a 4-inch telescope." "Immediately totality commenced, the ordinary solar spectrum was replaced by a faint diffused light, and bright lines." "There were no dark lines—that is to say, none of those lines which are present in the solar spectrum. When the spectroscope was directed to a distance of about 8′ from the edge of the moon's disc, the same lines remained visible. The centre of the moon was then tried, and the bright lines were still seen, but only half as strong as before." *

But let us turn to Professor Young's account of his own observations, and those made by his fellow-workers.

First of all, and perhaps scarcely inferior in interest to anything learned during the recent eclipse, there is Professor Young's observation of the reversal of all the dark lines of the solar spectrum close by the limb at the commencement of totality. I had, at page 295 of "The Sun," ventured to express my belief that the Fraünhofer dark lines would be found to have their birth-place here (and not, as surmised by Mr. Lockyer, beneath the visible limits of the photosphere); and it was, therefore, with some pleasure that I read the following confirmation of my views on this point:—"With the slit of Professor Young's spectroscope placed tangentially," writes Professor Langley, "at the moment of obscuration, and for one or two seconds later, the field of the instrument was *filled with bright lines*. As far as could be judged during this brief interval, every non-atmospheric line of the visible spectrum showed bright; an interesting observation confirmed by Mr. Pye." "From the concurrence of these quite independent observations, we seem to be justified in assuming the probable existence of an envelope surrounding the photosphere and beneath the chromosphere, usually so called, whose thickness must be limited to two or three seconds of arc, and which gives a discontinuous spectrum consisting of all or nearly all the ordinary lines, showing them, that is to say, *bright* on a dark field." On this point Professor Young remarks, that "the sudden reversal into brightness and colour of the countless dark lines of the spectrum at the commencement of totality, and their gradual dying out, was

* It will be noticed that this result exactly accords with my anticipations in the above quoted passage from the Monthly Notices of the Royal Astronomical Society. No stronger proof can be required of the fact that the illumination in our air is not the "sunlight" of the atmospheric glare theory, than this circumstance, that towards the moon's disc, where we have this illumination pure and simple, we get a spectrum of bright lines (instead of a faint solar spectrum).

the most exquisitely beautiful phenomenon possible to conceive, and it seems to me to have considerable theoretical importance. Secchi's *continuous spectrum* of the sun's limb" (the existence of which had been so strenuously denied) "is probably the same thing modified by atmospheric glare,—anywhere but in the clear sky of Italy so much modified, indeed, as to be wholly masked."

Now, with respect to the lines seen in the spectrum of the corona, it is to be noted that Mr. Abbay, "observing at Xeres with a spectroscope of two prisms of 45° belonging to Professor Young, saw the bright lines C, D, and F, and afterwards another line rather more bright than F on the less refrangible side of B." But it is worthy of notice, as pointed out by Professor Young, that with a hazy sky as at Xeres, and, indeed, all the Spanish stations, it is impossible to feel sure from the mere appearance of a line that the part of the corona towards which the spectroscope is directed really has a spectrum showing that line. In fact, the appearance of the C line of hydrogen as far as 6′ or 7′ from the sun, "far above any possible hydrogen atmosphere," says Young, effectually demonstrates this, even if it had not happened that Mr. Abbay had seen the C and F lines on the moon itself.

Fortunately, however, Professor Young has been able to establish in a highly satisfactory, or rather demonstrative, manner the fact that light coming from a region much more than 6′ or 7′ from the sun's limb gives the bright line 1474 Kirchhoff,—the very line be it noted respecting which the gravest doubts had been expressed. Of the four spectroscopes employed by his party, "two," he writes, "were what might be called *analysing*, and two *integrating* instruments." The former gave the relative brightness of the several lines in the spectrum from a definite portion of the corona or prominences; the other showed the relative quantity of light corresponding to different bright lines when the whole of the corona supplied light to form the spectrum. Now the analysing spectroscope showed the line 1474 very much fainter than the C line of hydrogen. "Even during totality," says Professor Young, "1474 can hardly be called conspicuous in an analysing instrument, while C blazes like a red Sirius;" and he concludes that C is at least 25 times and perhaps 50 times as bright as 1474. Now in the integrating spectroscope the order of brightness is reversed. The line 1474 is appreciably brighter than the line C, insomuch that Professor Young assigns as the numbers indicating the relative brightness of these lines, 100 for the line 1474 and 72 for the line C. Only one explanation is available, namely, that the area of the region supplying light whose spectrum shows the 1474 line must exceed the area of the region whose light shows the C line, in the proportion which 100 × (from 25 to 50) bears to 72, or from 35 to 70 times. Professor Young, assigning to the coloured sierra and prominences an apparent area equivalent to a ring a quarter of a minute high round the sun, concludes that the self-luminious corona would be "equivalent to another ring from 8′ to 14′ high." * The irregular nature of the inner part of the corona would also seem to leave no escape from the conclusion that this portion cannot suffice to give all the light showing the 1474 line, unless we suppose the inner corona to extend in places considerably more than 8′ from the sun. Professor Young, therefore, concludes that "we have surrounding the sun, beyond any farther reasonable doubt, a mass of self-luminous gaseous matter, whose spectrum is characterised by the green 1474 line. The precise extent of this it is hardly possible to consider as determined, but it must be many times the thickness of the red hydrogen portion of the chromosphere; perhaps on an average 8′ or 10′ with occasional horns of twice that height. It is not at all unlikely that it may even turn out to have *no upper limit,* but to extend from the sun indefinitely into space." †

As respects other spectroscopic evidence, little need be said. None of the other observers save Lieutenant Brown obtained results which differ essentially from those already recorded. Lieutenant Brown was unable to detect the 1474 line, but as he used a single-prism spectroscope, whereas the invisibility of the line depends on a certain amount of dispersion being obtained, his failure can be readily interpreted. The Italian observers at Agosta saw two very brilliant bands. "Father Denza," writes Secchi, "executed the part assigned him to admiration. He saw the protu-

* In "Nature" this is written 8′ to 16′ high," but this is obviously due to a mistake; and Professor Young probably wrote the passage as I have put it. A ring 16′ high round the sun's disc would exceed a ring one quarter of a minute high, about 80 times in extent, instead of about 70 times.

† In one of the most humorous paragraphs I ever remember to have read in a scientific paper, Professor Young assures the advocates of the atmospheric glare theory that "in the main" he concurs with their views "*with the exception* that he is disposed to" negative the only remaining special point which they had not conceded. The corona had been gradually admitted to be solar as respects a greater and greater extension from the sun; but for all above 6′ or 7′ from the sun, and specially for the radiations, a contest was still maintained with the object of proving these parts to be terrestrial. Professor Young placidly hands over to the sun these last remaining portions of the corona, assuring the advocates of the terrestrial theory that *in all other respects* he agrees with them.

berances clearly, and choosing the most brilliant part of the corona, he directed the spectroscope towards it; and whilst the assistant kept it fixed, he was able to distinguish plainly the spectrum of the protuberances from that of the corona. This last was continuous, and had two very brilliant bands, one in the green near Fraünhofer's E, and another in the yellow-green. . . . . The shortness of the time, and the difficulty of calculating the scale would not allow us to fix the lines in question with more precision."

As regards the agreement of the line which has hitherto been spoken of as Kirchhoff's 1474, with that well-known iron line, Professor Young remarks, "the difference, if any (and I have not found the slightest reason to suspect the least want of coincidence in observations with the whole dispersive power of thirteen prisms) is less than 1-10th of one division of Kirchhoff's scale. There is no solar dark line but 1474, therefore, with which this bright line can be supposed to agree.

There seems to be no valid reason for regarding this line as belonging to a new element, as has been suggested. In fact, without denying the possibility that elements unknown on earth may exist in the interplanetary spaces, or elsewhere, it could only be on the score of very striking evidence that terrestrial physicists would admit the actual existence of such elements in any special instance. In this case, we have a line which appears to be absolutely identical with a known iron line, and is also recognisable in the spectrum of the terrestrial aurora. We may reasonably inquire, therefore, under what conditions the metal iron can supply this line without exhibiting the other lines belonging to the iron spectrum; but we certainly seem to have no evidence favouring the supposition that a new element is in question. On the contrary, the evidence, as far as it goes, is opposed to such a supposition.*

But we must now turn to the evidence afforded by direct observation, supplemented as such observation has in this case been by the evidence of photographic records.

I lay no stress on the recognition of a very striking difference between the luminosity of the inner part of the corona and that of the outlying and seemingly radiated portion. For this difference is a phenomenon which has

* We now have Zöllner's study of the auroral spectrum to show that in all probability the extreme tenuity of the medium, through which the electrical discharges causing the auroral light take place, is to be regarded as the secret of the nature of the auroral spectrum. The presence of one line of iron both in the auroral and coronal spectra, while all the remaining lines are absent, may thus come one day to be satisfactorily interpreted.

been known for more than a century and a half, and only that forgetfulness of past labours and their results which has so constantly characterised eclipse observation, could have caused any stress to be laid on the recognition of so well-worn a fact.

For a similar reason, I should be disposed to lay little stress on the evidence obtained respecting the coronal radiations, conceiving as I do that all that could be learned respecting their general characteristics had been long since ascertained. But as to many, unaware of the immense mass of evidence already available, this feature of the corona has seemed worthy of special study, it will be well to consider what fresh evidence has been obtained during the recent eclipse. Fortunately, I am able to do this without altogether repeating an oft-told tale,—partly because photography has supplied a new kind of evidence on the point, and partly because a new fact has been brought out in connection with it.

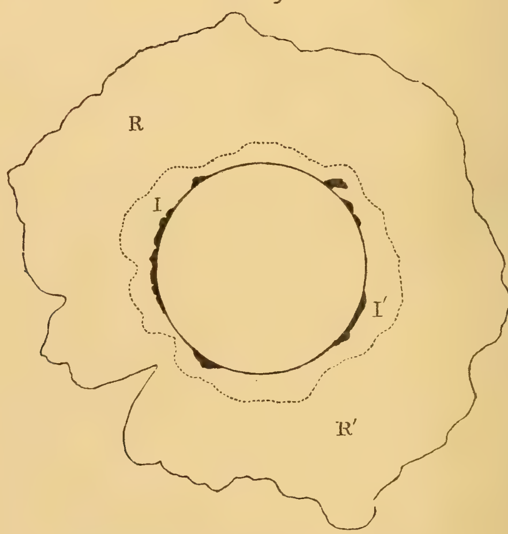
Considering the unfavourable circumstances under which the recent eclipse was viewed, and that the eclipse itself was much less important than many during which the corona has been studied, it may be regarded as fortunate that the corona presented one feature which could scarcely escape the notice of any moderately attentive observer. Opposite the south-eastern quadrant of the moon's limb, there was a well-marked gap—V-shaped—whose apex reached close up towards the moon. So close, indeed, that, as depicted in figure 9, the distance of the apex from the sun was actually less than that of the highest parts of the inner corona. *Under this gap the inner corona sinks lowest, while on each side it rises quickly up to about its mean level.* Still referring to Lieutenant Brown's picture, let it be noticed that the V-shaped gap is the most striking feature of the corona. It is the sort of feature which might well be expected to afford a crucial test of the vexed question whether the radiations really belong to some solar appendage or are merely phenomena of our own atmosphere. If it shall appear that this V-shaped gap was seen at other stations, and more particularly at stations far removed from Lieutenant Brown's, then all further question on this point becomes impossible.* It

* Note, however, in passing, that if the depression of the brighter part of the corona beneath the great gap be admitted, on the strength of Lieutenant Brown's drawing, then the matter is disposed of at once; for the connection between the matter in which the gap exists and the inner corona is at once established. It need hardly be said that the shape of the inner corona could not possibly be reflected—as it were—in the illumination of our own atmosphere.

happens fortunately that, notwithstanding the unfavourable weather generally experienced, the evidence on this point is unmistakable.

In the first place, we have the evidence respecting the corona as seen at other Spanish stations. I extract the following account from a paper in the "English Mechanic," for January 27th, 1871, which, though anonymous, shows obvious signs of having been written by a practised mathematician and a well-informed physicist. I note, also, that the account accords exactly with statements addressed to the January meeting of the Royal Astronomical Society,

FIG. 9.



The Corona as drawn by Lieut. Brown.
R, R′ the outer radiated corona.
I, I′ the inner and brighter corona.

and with accounts I have received from different members of the expedition:—"The *corona proper*, or *glory*, or *radiated corona*—as it is variously called, extended a distance of almost the moon's diameter from the moon's limb, but not equally in every direction. It had a greater extension in four directions, at the extremities of two diameters at right angles, so as to give it the shape, roughly speaking, of a square with rounded corners. It was broken in parts, and notably by one decided V-shaped gap. This was observed not only by one party, but at three stations, San Antonio, Xeres, and La Maria Louisa, which form a triangle each of whose sides is five or six miles in length." Mr. Hudson, Fellow of St. John's College, Cambridge, referring (at the January meeting of the Royal Astronomical Society) to the V-shaped gap exhibited in Lieutenant Brown's drawing (of which figure 9 is a reduced outline-view) remarked that the gap appeared to him somewhat larger; and Lieutenant Brown agreed that his drawing did not quite adequately represent the dimensions of the gap.

So far, then, the evidence is most satisfactory. It may be indeed regarded as already sufficient; since no peculiarity of our own atmosphere could possibly explain the similarity of the views seen at stations so far apart; while the fixity of the gap, a fixity respecting which all accounts agree, disposes of the theory (untenable also on other accounts), that the bounding radiations which formed the gap were due to matter near the moon illuminated by the solar rays.

But Lord Lindsay, and Mr. Willard (a member of the American observing party), had photographed the eclipse at stations near Xeres; and here was a further means of testing the matter. If the photographs showed the V-shaped gap, the evidence would indeed be irresistible.

Now, as respects Lord Lindsay's photographs, I am able to say but little. He had with great spirit undertaken his share of the observations at a time when it seemed likely that Government aid would be refused altogether. He had furthermore taken out at his own expense a fine 12¼-inch reflector by Browning. The ingenious contrivances by which the production of the successive pictures was mechanically provided for, enabled Lord Lindsay to take no less than nine photographs. But those I have seen suggest the belief that, so far as the corona is concerned, the results would have been more satisfactory had fewer pictures been taken, the exposure of each being correspondingly increased. The very clearness with which, in a most beautiful photograph of the series, now lying before me, the prominences are shown, leads me to believe that the exposure was altogether too short. We have evidence, both in the American photograph and in that by Mr. Brothers, that the actinic energy of the prominence light is much greater than that of the corona, "the light of which," says Mr. Brothers, in a letter to me, "rather resembles moonlight in this respect." A photograph, therefore, which shows the corona well cannot possibly show the prominences clearly—though the *effects* of the prominences may be very marked indeed; and, *vice versâ*, a picture in which the prominences are well-defined cannot be expected to show the details of the corona.

The American photograph is shown in the second figure of the illustrative plate. It will be seen that the extent of the field is limited, as if by a circular stop, to a space of about 12′ wide all round the moon's body. Yet the V-shaped gap is shown very distinctly indeed. It is to be noted, also, that there are obvious traces of two other gaps, one about 40° round towards the left, and the other about 70° round towards the right. Yet, again, it is to be noted that the depression of the inner corona beneath the great V-shaped gap is very distinctly indicated; and in other respects the agreement of the part of the corona shown with the indications in figure 9 is most remarkable.

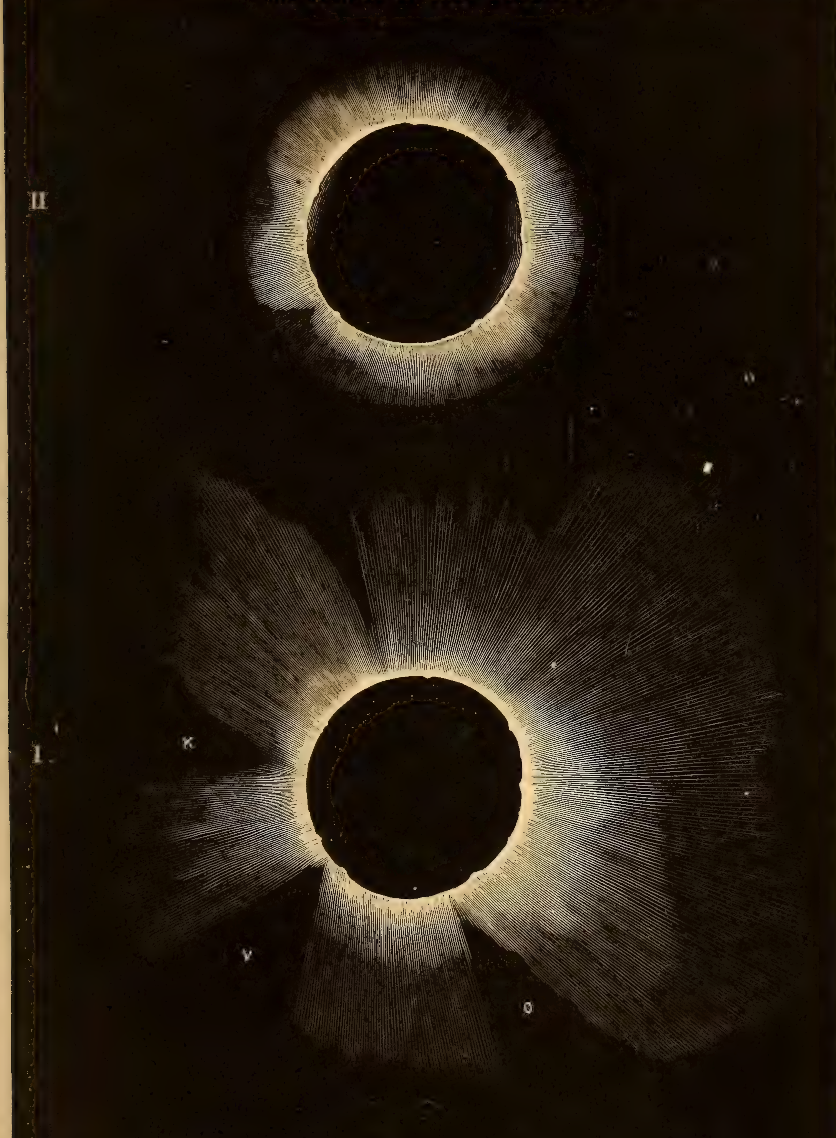
But now another circumstance invites our attention. The American photograph shows the position of the apex of the gap to have been exactly midway between two prominences. The prominences are, indeed, not actually seen; but the notching of the moon's disc (caused by the eating away due to their actinic energy) leaves no question as to their existence there. I find two prominences indicated also in this position, not only in Lord Lindsay's photographs, but in some excellent drawings of the eclipse by Lieutenant Watkin, R.A., Lieutenant Sidney G. Smith, R.N., and Mr. Abbatt, F.R.A.S. Now here we have the means of ascertaining the real existence of this gap as a true solar phenomenon beyond all possibility of question,—if only another photograph taken at a distant station shall show the gap similarly placed.

This brings me to what I take to be the crowning achievement of the eclipse expeditions—an achievement well worth all the labour and cost which those expeditions may have involved to the American, British, and Continental Governments and students of science. I refer to the admirable photograph of the corona by Mr. Brothers, F.R.A.S., taken at Syracuse, during only the last few seconds of totality—for clouds rendered the first exposures relatively ineffective. Those eight seconds, however, during which this photograph, the fifth of the series, was exposed, sufficed to give a picture whose scientific value is only surpassed by the interest which attaches to the view as the first really successful photographic delineation of the solar corona.

This photograph, of which the first view in the illustrative plate is a copy,* is well worth careful study.

* Mr. Brothers writes to me that there are strange details in the negative which he cannot reproduce, either in photographic copies or by draughtsmanship. Their extreme delicacy defeats all attempts to reproduce them. Even the extensions seen in the plate have had to be copied directly from the negative, for they are too delicate to show in ordinary photographic copies.

THE ECLIPSE OF LAST DECEMBER



I. MR BROTHERS' PHOTOGRAPH, TAKEN AT SYRACUSE.

II. MR WILLARD'S PHOTOGRAPH TAKEN NEAR XERES.

In the first place, the situation of the great V-shaped gap is clearly defined, and unmistakably agrees with what is seen in the American photograph. *There* are the notches showing the places of the two prominences equally distant from the apex of the gap. And if any doubt could exist as to these two notches—or rather the prominences they indicate—being the same as those in the American photograph, that doubt would be removed by the agreement of the other prominence-notches—all round the moon's disc.

Again, the two other gaps or rifts, which are but faintly indicated in the American photograph, are plainly shown in Mr. Brothers's. We see also *why* they are less clearly seen in the former. They do not extend so far in towards the sun's globe, and are banked up, as it were, all round by masses of the bright inner corona. Their agreement in position with the indications of the American photograph, as also with what can be recognised of these delicate phenomena in Lieutenant Brown's drawing, cannot be questioned.

Let it further be noted that the agreement of the features of the inner corona as shown in the two photographs is even greater than was to have been anticipated when the undoubted effect of atmospheric illumination, in blurring and even in part modifying the details, is carefully attended to.* Here again, too, the accuracy of Lieutenant Brown's drawing is *surprisingly* confirmed. I use the italicised word advisedly, for it has very seldom happened that the corona has been satisfactorily delineated even when practised draughtsmen have attempted the work.

But two features remain to be specially noticed.

In the first place, the disc of the moon is not perfectly round but obviously compressed from east to west.† If the moon in the American view were similarly distorted, we might be led to ascribe this to some peculiarity arising from the moon's motion from west to east across the sun's face. But as the American drawing shows only a glare of light on the moon's eastern and western limbs, this explanation

* Mr. Brothers having kindly sent me copies both of his own photograph and the American one (the former enlarged to the scale of the latter) I had hoped to provide chalk drawings for the engraver, and so to have preserved the originals for my own use. But I was foiled in this, by the fact that I found myself quite unable to reproduce the close resemblance which exists between the two photographs. Even when I made both drawings from one photograph, the resulting views were not so similar as the two originals. As I write, I have not seen the proofs; but I fear that no engraver, however skilful, can make the two pictures so like as to give a sufficient idea of the evidence they really afford.

† The drawings are placed about as the sun actually appeared from the Syracusan station; so that east and west correspond respectively to the left and right sides of the drawings.

is not available. Fortunately we *have* an explanation, and, carefully considered, it throws some light on an interesting question of solar physics. Mr. Brothers tells us that there was a good deal of wind during totality. Now the image of moon, prominences, and corona being more swayed in a horizontal than in a vertical direction, we can understand that the moon's horizontal diameter would be more affected than the vertical diameter. The actinic energy of the prominences far exceeds that of the corona, so that the prominences would leave their track (as it were) unmistakably impressive on the eastern and western limbs of the moon. But it needs only a brief study of the picture to see that the western limb has suffered much more than the eastern. At first sight this seems perplexing, especially when we note that the *whole* of this limb has suffered, not such parts only as were opposite lofty prominences. But when we consider that the totality was drawing to a close at the time, we see that this is precisely what should be expected; for the sierra would have come into view on the western side, while the *sierra* on the eastern side would be concealed by the advancing moon. It was the image of the *sierra*, then—swayed backwards and forwards by the wind across the western limb of the moon—which caused the remarkable flattening of the moon on that side.*

The second feature is the great extent both of the inner and outer corona on the western side. Now, as respects the inner corona, a portion of the excess of width on the western side must, without doubt, be ascribed to the flattening of the western side of the moon's disc. We have only to conceive the full dimensions of the moon indicated on that side to see that the inner corona there will be greatly reduced. Since, also, the moon was as far as possible towards the east—for it is to be remembered that $3^{s.}$ after the exposure was completed totality ended—we seem to have a sufficient explanation of the eccentricity of the ring-formed corona. As regards the great extension of the outer corona towards the west, it is necessary to note that the outer corona even in the negative is of extreme delicacy,—very much more

* We have corroboratory evidence on this point in Father Secchi's account of the eclipse. For he tells us that towards the close of the totality he was able to see the protuberances (at first there had been clouds); and he remarks that the protuberances formed a beautiful small semicircular corona. "It was full of the well-known rosy jets, of admirable shape and beauty," confirming the view that "they extend all round the sun, differing in height in different places." It will be seen that Father Secchi considers the evidence altogether unfavourable to the existence of a real envelope such as that conceived in the theory embodied in the title *chromosphere* (for the *sierra*.)

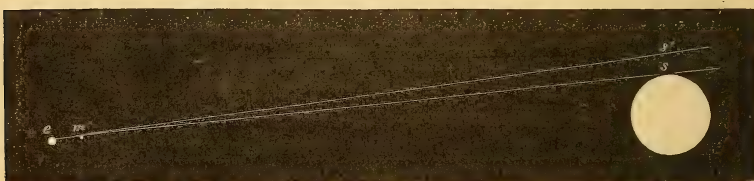
delicate, in fact, than any tint which the most skilful engraver could produce; and a very slight additional atmospheric illumination would suffice to add to the apparent extension of the corona. Now we have only to conceive the actual nature of the illumination of our atmosphere during totality by the bright inner corona to see that towards the end of totality the light thus arising would be much stronger on the west than on the east,—*so* much stronger, we may readily believe, as to veil in some sort the features of the actual radiated corona on that side, and seemingly also to increase its extension.

This, indeed, seems the only available explanation. For though Dr. Oudemann has suggested that matter between the earth and moon may explain the peculiarity—which was first observed in the eclipse of 1715, and has since been recognised in nearly all total eclipses—there are overwhelming objections against his theory. Those I am about to urge do not, indeed, apply to the theory as understood by Professor Young, who in the paper from which I have already quoted, remarks that a light cloud of cosmical dust, one or two hundred thousand miles above the earth's surface, and of great thickness, would account for observed peculiarities otherwise not easily interpreted. This is just, but nothing could account for such a cloud as a regular attendant of the earth and always lying towards the moon's place in total eclipses. Dr. Oudemann's theory is very different, and is also in itself far more plausible. For he supposes the cosmical dust to occupy the whole space between the earth and the sun,—to form, in fact, such a lenticular disc around the sun as astronomers have long been familiar with in theories explaining the zodiacal light. Not only is there no improbability in the general supposition, but, on the contrary, it may be regarded as absolutely certain that space around the sun, as far as our earth and farther, is in reality so occupied.

But to Oudemann's theory, propounded by way of explaining the peculiarities of the corona, there is an overwhelming objection. If we suppose that *e* (fig. 10), is the earth, *m* the moon, and *s* the sun, (the dimensions of these bodies, and the distance *e m* being enormously exaggerated compared with the distance *m s*), then whatever light might be supplied during total eclipse from a space *e m* full of the supposed cosmical dust, would be altogether surpassed by the light coming from the prolongation of the space *e m* towards *s*, up to, and *past s*. And *this* not only because the latter regions of space are so much more extended, but because the matter occupying a large portion of them must be so much more brilliantly illuminated. The obliteration of the stars by the light of the sun cannot be more complete than the obliteration of the light from the region *e m* by that from the region beyond *m*, up to, and past *s*.

The polariscopic observations, as I have mentioned, are perplexing, and in some respects seem contradictory. They seem, however, to establish these results, that (1) a considerable proportion of the coronal light is reflected; (2) that

FIG. 10.



Illustrating the main objection to Oudemann's Theory of the Coronal Radiation.

all the reflection has not taken place in our own atmosphere; and (3) that it is not such as would result if the sun's light were reflected from mere amorphous dust, but rather as though the reflection took place from surfaces of a crystalline nature.*

And now to sum up—very briefly, for already I have exceeded my allotted space—how does our knowledge respecting the corona now stand, and in what respects has it been increased? It will be gathered from what I have already said that I attach no great weight to certain results which have elsewhere been dwelt on as the most important fruits of the expedition,—for this reason, simply that those results had been obtained long before the expeditions set forth, and

* The account of the eclipse would be incomplete without a reference to the remarkable observations made by Sig. Diamilla Muller of the variations in terrestrial magnetism during the eclipse. He says in the "Gazzetta Ufficiale del Regno d'Italia," January 17:—"On the 22nd December the needle followed its usual course till the commencement of the eclipse. At that moment it ought to have continued its usual motion from east to west. Instead of this, soon after the first contact, its westerly motion stopped, and it retraced its steps till it reached its minimum declination at 1 hr. 58 min. (Terranova mean time) exactly at the instant of totality. From the moment of totality to that of last contact, as the disc of the sun gradually reappeared, the ascending motion towards the west began anew; and at the end of the eclipse the declination needle had returned to the precise position which it had quitted at the beginning of the phenomenon." This observation merits the most careful study; but I have not here space to indicate the conclusions to which it would seem to point.

were well known to all familiar with the history of physical astronomy. I take it that the fruits of the expeditions may be thus summarised :—In the first place the corona has at length been photographed, so that its peculiarities may be studied at our leisure without fear of mistakes arising from inexact delineation. Secondly, the connection between the ring-formed and the radiated corona has been demonstrated by the photographic and other evidence showing how the height of the bright inner corona corresponds with that of the outer corona. (This is a most important discovery.) Thirdly, the fact that one of the lines of the corona spectrum is identical with Kirchhoff's 1474, a line seen in the spectrum of our own aurora, has been abundantly demonstrated. Fourthly, the region in which the Fraünhofer lines have their origin has been ascertained and shown to be an atmospheric envelope (which may be some two or three hundred miles deep) lying immediately above the photosphere. Fifthly, the theory that the *sierra* is of the nature of an atmosphere has been invalidated, and the earlier opinion (which Professor Respighi had supported on the evidence of his spectroscopic observations) has been confirmed if not demonstrated,—the view, viz., that the sierra consists of multitudes of the rosy prominences resembling the large ones in all other attributes except size.

I shall not venture to theorise in this place respecting the real nature of the corona; for the present we may remain satisfied by having learned beyond all question its real position in space, and something of its physical constitution. There are problems of extreme interest concerning it which still await solution. There are difficulties, also, which must by no means be overlooked if we would explain its phenomena aright. To future observation and research these problems and difficulties may not unsafely be relegated.

NOTICES OF BOOKS.

The Descent of Man, and Selection in Relation to Sex. By CHARLES DARWIN, M.A., F.R.S., &c. In 2 vols.; with Illustrations. London: J. Murray. 1871.

THE first impression with many on opening this work,—the announcement of which has for the last year and a half stimulated the curiosity and excited the most intense interest of the whole scientific world,—will possibly be one of disappointment, that so small a portion of it is devoted to the main subject, an attempt to trace the descent of man from those lower forms of animal life which present the closest relationship to him in structure and in mental development. The book is divided into two parts, of which the first, "On the Descent of Man," occupies only rather more than half the first volume; the second part, "On Sexual Selection," comprising fourteen out of the twenty-one chapters of which the work consists; eleven of these fourteen chapters treating of sexual selection in relation to the lower animals, the last three again applying the principles which have been arrived at to the case of man. No one, however, will read beyond the first chapter without recognising many of those qualities which secured for the author's work "On the Origin of Species" a reception almost without a parallel, and enabled it to effect a revolution all but unexampled on the current of thought in the scientific world. The lapse of twelve years has abated none of Mr. Darwin's industry in collecting a prodigious array of facts from all conceivable sources in support of every proposition which he brings forward; nor has it deprived us of that wonderful combination of humility, confidence, and appreciation of the labours of others, which makes him ever ready even to magnify the importance of facts which appear to tell against his own peculiar views, and never backward to acknowledge when he himself sees reason to change some previously expressed opinion.

Commencing with a detailed account of the homologous structures in man and the lower animals, especially in their rudimentary organs and embryonic development; and with a comparison between the mental powers of the human race and those of the remainder of the animal kingdom, Mr. Darwin then proceeds to discuss the questions of the manner in which man has been developed from some lower form, and of the particular form which has established the best claim to be considered in the light of our remote ancestor. The question naturally arises in the outset,—granted the hypothesis that man has sprung by a process of gradual evolution from some lower form, and does not owe his origin to a separate act of creation,—are we to attempt to trace this descent in one or more lines? In other words, are all the various races of mankind to be included in one species or in several? It must be borne in mind that this question is not the same as whether mankind are sprung from one original pair. When the evolutionist supposes that one species of animal or plant has been evolved out of a pre-existing closely allied form, it is not necessary to assume that all the existing individuals are descended from some one aberrant off-shoot of the original stock; the whole race may have gradually changed by the operation of the law of the Survival of the Fittest, owing to some alteration in its external environment, so that it may be impossible to draw a line of demarcation between the earlier and the later form. This is doubtless the manner in which all evolutionists must hold that man gradually arose by continuous modifications from his nearest structural relatives, the anthropoid apes; but whether in one or several lines of descent is an open question. Mr. Wallace, in his essay on "The Development of Human Races under the Law of Natural Selection," while stating the arguments on both sides of the question, sums up on the whole in favour of the primitive diversity of man; Mr. Darwin we understand to hold a different opinion. We could have wished that his views on this point had been more explicitly stated; we draw our conclusion rather from his mode of expressing himself than from any definite statement, and from the absence of any allusion to more than one such line of descent. Let us examine his arguments a little more in detail.

Starting with the main principle of the theory of Natural Selection, that all organic species tend spontaneously to vary from the parent form, generally to a very small extent, and that those variations only survive and become hereditarily perpetuated which present some advantageous point of structure in comparison with their fellows, Mr. Darwin applies this principle to the case of man, stating that, "in order that an ape-like creature should have been transformed into man, it is necessary that this early form, as well as many successive links, should all have varied in mind and body. It is impossible to obtain direct evidence on this head; but if it can be shown that man now varies—that his variations are induced by the same general causes, and obey the same general laws, as in the case of the lower animals—there can be little doubt that the preceding intermediate links varied in a like manner. The variations at each successive stage of descent, must, also, have been in some manner accumulated and fixed." One of the earliest changes must have been in the shape of the hands and feet. The hands and feet of the anthropoid apes are admirably adapted for climbing trees and obtaining their food. Baboons, however, which frequent hilly and rocky districts, and only from necessity climb up high trees, habitually use their feet for walking along the ground, and have acquired almost the gait of a dog. In order to enable man to obtain mastery over those arts which have raised him so far above even the most intelligent of his nearest relatives, it was necessary that his hands should not be used for the purpose of locomotion, and hence that he should acquire the habit of walking erect on his hind legs. This great change having become effected in the course of countless generations, the rest followed more easily, and probably more rapidly. "The pelvis would have to be made broader, the spine peculiarly curved, and the head fixed in an altered position, and all these changes have been attained by man. Professor Schaafhausen maintains that 'the powerful mastoid processes of the human skull are the result of his erect position;' and these processes are absent in the orang, chimpanzee, &c., and are smaller in the gorilla than in man. Various other structures might here have been specified which appear connected with man's erect position. It is very difficult to decide how far all these correlated modifications are the result of natural selection, and how far of the inherited effects of the increased use of certain parts, or of the action of one part on another. No doubt these means of change act and react on each other; thus, when certain muscles, and the crests of bone to which they are attached, become enlarged by habitual use, this shows that certain actions are habitually performed, and must be serviceable. Hence the individuals which performed them best would tend to survive in greater numbers." The increased use of the hands and hand-made weapons, and the consequent decreased use of the jaws and teeth in fighting, would tend at once by disuse to a reduction of the great development of the jaws, and especially of the canine teeth of the males, which is the most conspicuous element in the facial difference between the higher apes and man. The absence of hair on the back and other parts of the body, which presents so great a difficulty to Mr. Wallace, arose probably from that sexual selection which Mr. Darwin enters into so largely in the latter part of the book, originating in the female from a dawning sense of beauty in the male, and hence becoming transmitted by inheritance to both sexes. With the increased use of the hands and gradual discovery of the arts, the social instincts were developed, man became more and more dependent on his fellow, the brain was brought more and more in requisition, and hence, together with the skull, increased in size; the intellect became increasingly developed, and hence half-savage man gradually emerged into definite existence.

To return to the question of the single or plural origin of man; there is much to be said in favour of the former idea. The fertile interbreeding of all races with one another; the discovery of the use of fire and of other arts in pre-historic times, and many other circumstances, point to this conclusion. On the other hand, there are many grave difficulties in the way, if we look to natural or sexual selection as the only means by which man has raised himself above the level of the brutes. Our nearest living relatives, the anthropoid apes, belonging to the section *Simiadæ*, include the orang, the gibbons, the chimpanzee, and the gorilla. Of these the gibbons, or *Hylobates*, comprise half-a-dozen species scattered over the Islands of Java, Sumatra, and Borneo, the Malayan Peninsula, and a portion of the Continent of Hindostan; the true *Simia*, or orang, is found only in Sumatra and Borneo; while the *Troglodytes*, or chimpanzee and gorilla, belong to North Africa. Fossil remains of other anthropoid apes have also been found in South Europe; so that we may consider the family to have extended at one time over the whole of the warmer portions of the Old World. Now, seeing that the raw material, so to speak, of the human race had this wide distribution, it is difficult to explain the fact,—if we suppose with Darwin that no internal predisposing cause has been at work,—that in one spot only in this vast region have the circumstances been sufficiently favourable to evolve from the pre-existing materials the more highly developed form. The ordinary course of nature would have been for one race of men to have become developed in the Islands of the Indian Archipelago, another in Africa, and another possibly in South Europe, so distinct that they could not be confounded with one another, and each adapted to the circumstances in which he was placed. We may take a similar instance from the equine tribe, which, until the discovery of the fossil *Hipparion*, was considered to be a family without near relatives in past or present times. The equine progenitor, however, the *Hipparion*, has become developed into the horse, the ass, the zebra, and some other forms, forming species so absolutely distinct that they either refuse to interbreed or produce only sterile hybrids.

Man presents a very singular exception to the general law, that widely distributed species belong to genera which include a large number of species; in other words, have many very near relatives. A case of extreme differentiation, similar in some respects to that of man, is furnished by the giraffe; but Mr. Mivart has shown in his "Genesis of Species" the difficulties in the way of the theory that the giraffe has been developed from other African genera of *Ungulata* by the operation of natural selection alone.

Again, the gap between the higher apes and man is, by the admission of all who have studied the subject, so enormous, that we might fairly expect that geological researches would have laid bare some of the intermediate links. Remains of anthropoid apes have been discovered in Greece, but they are manifestly those of anthropoid apes and nothing else. Remains of man have been found of enormous antiquity, contemporary with the mammoth and the woolly rhinoceros, but we have the authority of Professor Huxley (who has probably given the subject more attention than anyone else, and who is assuredly not biassed against the developmental hypothesis) for asserting that the Engis and the Neanderthal skulls "can in no sense be regarded as the remains of a human being intermediate between man and the apes," and that "they do not seem to take us appreciably nearer to the lower pithecoid form." Nor does the ordinary reply of the imperfection of the geological record seem to us to apply here. The remains of the animal or animals which formed the link between man and the apes would be preserved in the most recent formations, nearest the surface, where they would have been subjected to the least destructive influences; and it is strange that no trace of them has yet rewarded the labours of the many diligent searchers in this field.

It may be some comfort to sensitive persons to hear that we need not look in the Zoological Gardens or elsewhere for any one species of ape to which we are bound to offer the homage of paternity, that the gorilla, chimpanzee, orang, and gibbons are, after all, nothing more to us than very remote cousins of the same generation, but deprived of the same advantages of circumstances or of education. Mr. Darwin believes that "man is descended from a hairy quadruped, furnished with a tail and pointed ears, probably arboreal in its habits, and an inhabitant of the Old World;" though again we are not specifically informed whether this creature is the missing link between some extinct anthropoid ape and ourselves, or the common ancestor of the whole of the *Simiadæ*. If the former, how do we arrive at the development of the tail? the suppression of which Geoffroy St. Hilaire believed to be indispensable to the enlargement of the opposite extremity of the spinal cord.

The subject of Sexual Selection is treated at great length, and with a most instructive wealth of illustration, in the volumes before us. In the lower divisions of the animal kingdom sexual selection appears to have done little or nothing; it commences its operation apparently with the lowest classes of the *Arthropoda* and *Vertebrata*, and its development runs to some extent parallel with that of the intellectual faculties. "In the most distinct classes of the animal kingdom, with mammals, birds, reptiles, fishes, insects, and even crustaceans, the differences between the sexes follow almost exactly the same rules. The males are almost always the wooers; and they alone are armed with special weapons for fighting with their rivals. They are generally larger and stronger than the females, and are endowed with the requisite qualities of courage and pugnacity. They are provided either exclusively or in a much higher degree than the females, with organs for producing vocal or instrumental music, and with odoriferous glands. They are ornamented with infinitely diversified appendages, and with the most brilliant or conspicuous colours, often arranged in elegant patterns, whilst the females are left unadorned. This surprising uniformity in the laws regulating the differences between the sexes in so many and such widely separated classes, is intelligible if we admit the action throughout all the higher divisions of the animal kingdom of one common cause, viz., sexual selection." Some of the most interesting chapters in the whole book are those in which Mr.

Darwin details the varied contrivances which are found in different sections of the animal kingdom, by which the male is enabled to please or to charm the female by superiority in colour, in adornment, in form, or even in voice; and shows that the female does exercise a power or choice in selecting the male which pleases her best. He follows Montague and Bechstein in affirming that "the males of song-birds and of many others do not in general search for the female, but, on the contrary, their business in the spring is to perch on some conspicuous spot, breathing out their full and amorous notes, which, by instinct, the female knows, and repairs to the spot to choose her mate;' and that "the female canary always chooses the best singer, and that in a state of nature the female finch selects that male out of a hundred whose notes please her most." With an admirable method and logical sequence, Mr. Darwin traces his phenomenon through the animal kingdom, and points out the effect it must have had in gradually improving the race by giving the more fortunately endowed males a preference as the parents of the next generation. Many of the peculiarities of the human species are traced to the same cause, and especially the gradual diminution in the amount of hair in both sexes, through the development of the sense of beauty.

But, granting the establishment of this principle, what do we gain by it? It seems to us, indeed, to throw the difficulty of accounting for the origin of the higher forms of life only one step backward. The best favoured males are selected in preference by the females; but whence comes the power of the female to discriminate between her rival wooers? It is obvious that for a hen canary to distinguish between the song of one bird and another, which even to our ears present only a slight shade of difference, or for the turkey hen to pick out her partner who struts in the most fascinating style or displays the most gorgeous plumage, requires the assumption of the possession on her part, not only of powers of observation of a very high order, but also of a not contemptible æsthetic principle, which must gradually have been produced by insensible accumulations, and cannot have been, according to Darwinian principles, an innate gift or power. Do we, then, arrive any nearer to a solution of the principle which lies at the base of a continuous organic improvement of the race, when we carry back our position from a gradual advance in external characters in the male to a gradual advance in the female of a mental power of appreciating these external characters? We think not. While sexual selection appears abundantly sufficient to account for the one, to what cognate principle can the followers of Mr. Darwin point to explain the other? Is not the mental development of the female, in fact, a harder problem to solve than the physical development of the male?

Darwin's "Descent of Man" is a work that will long hold a place in our literature as a monument of patient and laborious research, and of great impartiality and candour in the results adduced. As the basis from which other inquirers will start their labours, it will always be invaluable. The line of argument pursued is throughout rigid and consistent; and if the conclusions arrived at are ever shown to be erroneous, it will be not so much by discovering any flaw in Mr. Darwin's line of argument, as by a substitution of other premisses for those on which he founds his hypothesis; in other words, by the discovery of some organic law or laws governing the evolution of organic forms, of which we are at present ignorant.

Whether the dogma that Natural and Sexual Selection are of themselves sufficient to account for the evolution of the lower forms of animal life, and of man from those orders which are most nearly allied to him, the progress of future research only will show.

General Outline of the Organisation of the Animal Kingdom, and Manual of Comparative Anatomy. By THOMAS RYMER JONES, F.R.S.; 4th Edition; Illustrated by 571 Engravings. London: Van Voorst, 1871.

A *Manual of Zoology for the Use of Students, with a General Introduction on the Principles of Zoology.* By HENRY ALLEYNE NICHOLSON, M.D., D.Sc., &c. Edinburgh and London: Blackwood and Sons, 1870.

OF the two books which we have here bracketed together, the first is a new edition of a well-known and deservedly popular text-book of Comparative Anatomy. Since the first edition of the work was published, great advances have been made in the study of zoology; the improvements in the construction of microscopes has much advanced our knowledge of the *Infusoria;* the researches of Van Beneden and Siebold have opened new fields in the embryogeny of the *Tæniadæ*; while the discovery by Steenstrup of the alternation of generation of the *Hydrozoa* has thrown a new light on many organic problems. More recently the investigations of Huxley and others have necessitated, in the present edition, a re-arrangement of the lower divisions of the animal kingdom; the separation of the *Protozoa* from the ciliated *Infusoria;* the abolition of the *Radiata* of Cuvier as an independent sub-kingdom, and establishment of the *Cœlenterata;* and the transference of the classes *Rotifera* and *Cirripedia* into close proximity with the *Crustacea*. For the general reader, as well as the scientific student, Professor Jones's book is perhaps the very best general hand-book for study or for reference; and for the benefit of those who do not already know it, we may briefly indicate its general plan. Commencing at the bottom of the scale, the author takes each class in succession, and after a general description of the class, describes in detail the anatomy of some typical species, including, in the case of the higher forms, the structure of the muscles and nerves, the circulatory and respiratory systems, the generative apparatus, male and female, and the embryology or metamorphoses. With the more important classes, the classification into orders is also given. The illustrations are abundant and really beautiful. To illustrate their completeness, we may mention that in the class of *Annelida*, we have drawings of the dental system, digestive organs, circulatory apparatus, respiratory organs, and generative apparatus of the leech: of the viscera, circulation, sexual organs, and eggs of the earth-worm; besides others to illustrate other families of the order. These engravings would have been still more useful had they been accompanied with some explanation of the scale to which they had been enlarged or reduced; it is somewhat perplexing to find facing one another on opposite pages drawings of *Pulex irritans* and the stag-beetle occupying just the same portion of the page; the beautiful drawing, too, of the head of a flea loses half its value from having no indication of its scale.

Dr. Nicholson's Manual is of considerably smaller size, and is more exclusively a book for the class-room. Pursuing the same general plan, a considerably larger proportionate space is devoted to the higher classes of animals, nearly one-half the volume being given up to the *Vertebrata*. Useful elements which are not found in the larger manual are an account of the distribution of each class in area and in time, together with brief descriptions, in some case accompanied with drawings, of those extinct orders the fossil remains of which so often fill up the gaps between widely differentiated existing forms. The drawings are numerous and good, though not of the same high degree of finish. As a student's book it may be safely recommended.

Notes on the Natural History of the Strait of Magellan and West Coast of Patagonia. By ROBERT O. CUNNINGHAM, M.D., &c.; with Map and Illustrations. Edinburgh: Edmonston and Douglas, 1871.

THE voyages of Mr. Darwin and Dr. Hooker have made us familiar with the main features of the natural history of the southern extremity of the American Continent, to our knowledge of which Dr. Cunningham now adds a useful and interesting contribution. An accomplished naturalist spending the greater part of four years attached to an expedition to that seldom-visited land, could not fail to make many valuable observations and some interesting discoveries, which are here recorded in the form of a continuous narrative, though without much attempt to generalise from them. It is very singular to find in that remote land plants not differing specifically from some familiar European and even British forms, as for instance the sub-alpine *Primula farinosa*, and the celery, *Apium graveolens*, though Dr. Cunningham confirms the observation of Dr. Hooker that the latter loses in Patagonia the acrid pungency which is characteristic of the wild European plant, and becomes mild and agreeable to the palate. The illustrations, on stone, add much to the value of the work, which is a very interesting record of conscientious zoological and botanical labour.

The Students' Elements of Geology. By Sir CHARLES LYELL, Bart., F.R.S., Author of "The Principles of Geology," "The Antiquity of Man," &c. With more than 600 Illustrations on Wood. London: John Murray, Albemarle Street, 1871.

IN 1830, 1832, and 1833 appeared the first edition of "The Principles of Geology," in three volumes, comprising a fourth book, "in which some account was given of systematic geology, and in which the principal rocks composing the earth's crust and their organic remains were described." This fourth book remained a portion of the original work, "The Principles," up to the fifth edition, published in 1837, but was omitted in the following edition, and published by itself in an enlarged form in 1838, under the title of "Elements of Geology." It was subsequently re-cast and enlarged again, and then published in 1851, under the designation of "A Manual of Elementary Geology." After six editions of "The Elements," or "Manual," has been sold, it has seemed good to Sir Charles Lyell again to alter the name of his work, and he now gives forth to the world "The Students' Elements of Geology," which is really the original "Elements" re-written, put into a more compact form, and better adapted to the beginner of the science. By alterations of type and shape, the book is made more handy for frequent reference, whilst space is gained by omitting discussions on theoretical matters, and in some cases by a diminution of the size of the woodcuts. These curtailments, however, are certainly counterbalanced by the addition of new matter, for in place of 520 woodcuts, the new edition contains 636, and for every page of disquisition omitted, certainly a page and a half of new facts is added.

It is, indeed, unnecessary for us at the present time to recommend any work of Sir Charles Lyell's, still less one so thoroughly well known as "The Elements." We need only say that the present edition is specially designed for the student.

Professor Huxley, in speaking of the obligations that the present generation are under to various geological writers of the uniformitarian school, speaks especially of the debt that we owe to Hutton, Playfair, and Lyell; but that, whilst the highly valuable works of the two former are read by but few in the present day, that every page of the latter writer is thumbed by all who wish to make any progress in geological thought. The present edition will go far to increase this inequality—for the readiness of reference and the handiness of this volume will make it, what it ought to be, the constant companion of the working geologist.

There are, besides modifications and additions, improvements in the present volume. Sir Charles Lyell is a man great enough to be able to say, "I have been wrong and others have been right"—in consequence, in deference to the French geologists, he now classes as Lower Miocene what he formerly called Upper Eocene. How many Frenchmen dare make a like concession to an Englishman?

A Dictionary of Science; comprising Astronomy, Chemistry, Dynamics, Electricity, Heat, Hydrodynamics, Hydrostatics, Light, Magnetism, Mechanics, Meteorology, Pneumatics, Sound and Statics; preceded by an Essay on the History of the Physical Sciences. Edited by G. F. RODWELL, F.R.A.S., F.C.S. London: Moxon, Son, and Co., 1871.

"THE number of dictionaries professing to comprehend a general view of the arts and sciences, in a condensed form, already extant is very great; yet great as it is, it is every year rapidly increasing." Thus wrote the editor of a so-called portable encyclopœdia in the year 1826, and verily since his day the number of books of reference has not decreased. One great advantage, however, has resulted from the increased number of topics on which universal dictionaries have to treat; and that is, that no one man can any longer attempt to inform the world upon every subject. It was Dr. Wm. Smith, we believe, who first adopted the plan of inducing a number of other well-known men to write each on the subject with which he was best acquainted, and then the whole were amalgamated into a complete lexicon. The system was found to be very successful, and not only has the original editor continued a series of such works, but others have imitated the design in other branches of knowledge. Among these we may class the work edited by Mr. Rodwell. He has embraced chemistry, astronomy, and the sciences usually called physical. These subjects have been assigned to the following well-known gentlemen:—Messrs. J. T. Bottomley, M.A.; W. Crookes, F.R.S.; F. Guthrie, B.A., Ph.D.; R. A. Proctor, B.A.; C. Tomlinson, F.R.S.; R. Wormell, M.A., B.Sc.; and the Editor; each taking two or three distinct subjects.

The articles in a very brief space give a thorough history of our knowledge on each subject, and bring that knowledge down to the latest discoveries; and besides this they direct the student to the works and papers where fuller information can be obtained. Altogether we can recommend the book very highly both to the student who is desirous of gaining some general information about other sciences than the one to which he especially applies himself, and to the general reader who wishes to keep *au courant* with the scientific knowledge of the day. It is not, of course, from such works as these that the man of science learns the depths of what he requires to know, but there are few so thoroughly well read in all the branches of physical science that they will not find here some knowledge they have partially acquired brought to a focus, and the means of increasing that knowledge suggested to them.

We hope that the somewhat enigmatical notice facing the title page, "that this series is published under the sole direction of J. Bertrand Payne," may mean that the other sciences may obtain like attention to that bestowed upon them by Mr. Rodwell and his coadjutors.

Geology; By JOHN MORRIS, F.G.S., Professor of Geology and Mineralogy, University College, London, &c., and T. RUPERT JONES, F.G.S., Professor of Geology and Mineralogy, Royal Military College, Sandhurst, &c. First Series. London: John Van Voorst, Paternoster Row, 1870.

WE learn from the preface that the intention of the authors is that this work should be considered as a concise guide book, so arranged that it may be useful both to students and teachers of geology. This first series consists of heads of lectures and synopses, with a table of the geological formations in the British Isles. Small as this book is, it is a real *multum in parvo*. It is excellently arranged, thoroughly methodical, and its contents may be readily retained in the memory. On this account it will become an important and valuable assistance to the study, not only of geology, but of what we may term the natural history and physiognomy of the entire mineral kingdom.

Civil Engineers' and Contractors' Estimate and Price Book for Home or Foreign Service. By W. DAVIS HASKOLL, C.E., Author of "Railway Construction," &c. Blackwood and Co.

THIS work is compiled to assist the engineer in preparing estimates for tenders for public works, one of his most important duties, and which affects his professional reputation most directly. It is in two parts, one giving the particulars of price, &c., in matters relating to the estimate, the second part being a priced list of contractors' machinery, plant, and tools. Even the experienced engineer or contractor may have his attention called, upon a glance at this book, to matters which in the haste of estimating might otherwise be forgotten.

The Student's Guide to the Practice of Measuring and Valuing Artificers' Works. By E. Wyndham Tarn, M.A., Architect. Lockwood and Co.

This work, re-edited from Mr. Dobson's "Student's Guide to the Practice of Measuring and Valuing," contains a great deal of fresh matter explanatory to the student of the technicalities and modes of construction employed in the several trades. Mensuration, well-sinking, excavating, bricklaying, carpentry, and masonry; and indeed all the branches of the building trade are fully gone into. The work is well indexed, and arranged to facilitate reference. Tables of constants of labour render the valuing of work done easy to the inexperienced in the trade, and make the book of exceeding use to all who have to deal with their own workmen.

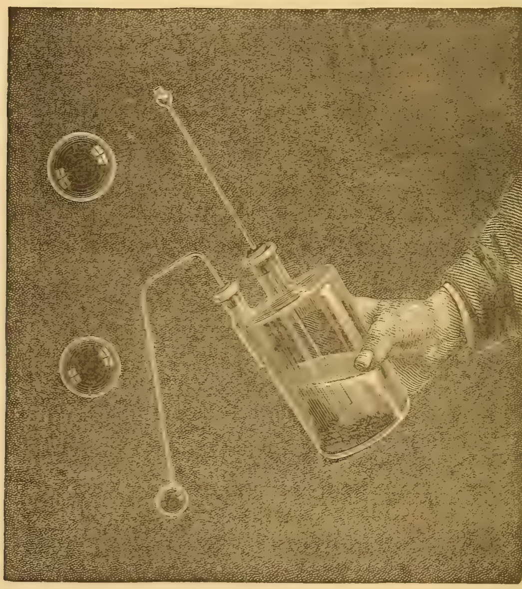
Microscopic Objects Figured and Described. By John Martin, Honorary Secretary to the Maidstone and Mid-Kent Natural History Society. Pp. 114; 97 Plates. Van Voorst, 1870.

This work consists of a series of 194 lithographs, interleaved with short descriptions of the objects represented, and occasional hints as to mounting, illumination, &c. The objects are represented as filling the field of the microscope, and occupy circles of $2\frac{1}{2}$ inches diameter. The figures are evidently the work of one accustomed to microscopical observation, but the execution is, with very few exceptions, extremely coarse (Fig. 46, seed of *Eccremocarpus*, for instance). The insect illustrations are evidently drawn from preparations made after the usual manner of those who mount objects for sale in balsam, in which muscles, viscera, &c., are carefully removed by alkaline maceration, and little else left but chitinous and membranous structures. In Fig. 129, tongue of the house cricket, the beautiful delicacy of the pseudo-tracheæ is wholly wanting. The proboscis of the blow-fly (Fig. 151) is taken from the popular mutilated and compressed specimen, and for the purpose of perpetuating this object, which has done much to give false views respecting the structure of the oral appendages of the insect, careful directions are given for preparing it. The most useful portion of the work is the short appendix, which contains some plain and simple directions for mounting and preserving objects.

Elementary Treatise on Natural Philosophy. By A. Privat Deschanel, formerly Professor of Physics in the Lycée Louis-le-Grand; Translated and Edited, with extensive additions, by J. D. Everett, M.A., D.C.L., Professor of Natural Philosophy in the Queen's College, Belfast. In four parts: Part I. Blackie and Son, London, Glasgow, and Edinburgh, 1870. 239 pp. Medium 8vo.

This work is the first portion of the translation of M. Deschanel's "Traité Elementaire de Physique," and includes Mechanics, Hydrostatics, and Pneumatics. The complete work was first published by Messrs. Hachette, in 1868, and it has since been adopted by the Minister of Public Instruction as the text book for Government Schools. We have no work in our own scientific literature to be compared with it, and we are glad that the translation has fallen into such good hands as those of Prof. Everett. The type is large and clear, and the woodcuts really admirable, and quite a pattern to scientific works in general, as will be seen from the woodcut given below (Fig. 11), which shows the ascent

FIG. 11.*



of soap-bubbles filled with hydrogen. We are glad, moreover, to notice that the most recent discoveries have been introduced.

This portion of the book is divided into eighteen chapters, the first seven of which relate to mechanics, the succeeding four to hydrostatics, then five to pneumatics, and the two last to hydrodynamics.

The work commences with some remarks on the origin of Natural Philosophy. The latter is defined as "the study of the material world, including the phenomena which it presents to us, the laws which govern them, and the applications which can be made of them to our various wants."

Now, we think this definition a little too general, for surely pure physics does not concern itself at all with the applications which may result from the study of its natural phenomena. We think we

* We are indebted to Messrs. Blackie and Sons for this woodcut.

should carefully distinguish between pure and applied science. Natural science began with the introduction of the experimental method by Galileo (?), and this is defined as a method which consists "in observing facts instead of trying to divine them; in carefully examining what really happens, and not in reasoning as to what ought to happen."

In the dynamics we may specially notice the very full and accurate treatment of the "laws of falling bodies," and the detail descriptions of Attwood's machine, Bourbourze's modification of it, and Morin's apparatus. The subject of capillarity receives an unusually full treatment. The book is a valuable contribution to our scientific literature; it will form an admirable text-book for special science classes in schools, and we look forward with pleasure to the appearance of the remaining portions of the work.

Lessons in Elementary Physics. By BALFOUR STEWART, LL.D., F.R.S., Professor of Natural Philosophy, Owen's College, Manchester. 372 pp. London: Macmillan and Co., 1870.

THIS work forms the seventh of a series of elementary class books for schools, which already numbers among its authors the Astronomer-Royal, and Professors Huxley, Roscoe, Lockyer, and Oliver.

For the first time, we have an elementary text-book on what we may call *the new physics*. When Francis Bacon introduced his system of philosophy in place of the many middle age systems, which had all, more or less, sprung from Aristotelianism, it was called "the new philosophy." We may, with almost equal justice, speak of the natural philosophy of Thomson, Tait, and a few others, as the new physics; for, compared with the physics of even ten years ago, it is, indeed, in many respects, a new science. A science of units, potentials, energies, and vortices; infinitely more philosophical and absolute than the physics of any previous age.

The arrangement of this work is both original and philosophical. It is distinguished by the lucid style and strong, even grasp which is associated with all Professor Stewart's work and writings. In reading it we have the satisfaction of knowing that it is the work of a man who is very familiar with the experimental portion of the various sciences which he describes, and who is well versed in the more refined methods of research, and in the higher flights of pure inductive reasoning.

We must express some regret that a series of questions is not appended to the work, as it would by this means be rendered more useful as a school text-book. This matter can, however, be easily remedied in a second edition, the speedy appearance of which we may safely predict.

Introduction to the Study of Inorganic Chemistry. By WILLIAM ALLEN MILLER, M.D., D.C.L., LL.D. London: Longmans, Green, and Co., 1871. 282 pp. Small 8vo.

A SHORT time ago Messrs. Longmans commenced a series of elementary works on mechanical and physical science, "adapted for the use of artisans and of students in public and other schools." This work is the last published of the series, and we are glad to notice that works on the "Theory of Heat," by J. Clerk Maxwell, and on "Sound and Light," by Professor Stokes, are soon to follow.

More than half the work before us is devoted to the chemistry of the non-metallic bodies (ten chapters), while the remaining seven chapters relate to the metals.

There is nothing special or very noticeable in the arrangement or treatment of the subject matter of this work. We all know the thoroughness and completeness of the writings of the late Professor Miller, and to enlarge upon this here is needless. This work is necessarily, to a greater or lesser extent, a condensation of Professor Miller's large "Elements of Chemistry," which is a universally used text-book of the science, and needs no praise. The author was engaged in reading the proof sheets of the work before us at the time when he was seized with the illness which has deprived science of one of its greatest lights, and all who knew Professor Miller of a genial, sincere friend.

A Treatise on Magnetism; designed for the use of Students in the University. BY GEORGE BIDDLE AIRY, M.A., LL.D., Astronomer-Royal. 220 pp. Macmillan and Co., 1870.

THIS is essentially a work for university teaching, the subject being treated mathematically, and the mathematics being of the higher order. It was written as one of a series designed to promote and exalt the study of mathematical physics in the University of Cambridge.

The work is divided into twelve sections. The first treats of the dissemination of magnetism through the universe, and it is herein stated that the sun and moon, in all probability, like the earth, act as magnets, although our knowledge of magnetism is limited to the magnetism of iron, steel, &c., and of the earth. A steel magnet is defined as a "bar of steel which, when so suspended or so mounted on a fine point that it can vibrate freely in the horizontal plane, will take a definite direction; and, if disturbed from that direction, will return to it by a series of vibrations gradually diminishing in extent, from the effect of atmospheric resistance, &c." Reasons are adduced for believing that terrestrial magnetism is not produced by magnetic forces external to the earth, and that it does not reside in the earth's crust; three theories are then brought forward to account for the magnetic action of the earth; first, by the action of a small magnet of great power near the centre of the earth (*Mayer's theory*, adopted by Biot and Humboldt); secondly, by the action of two magnets within the earth *(Hansteen's theory)*; thirdly, by supposing that the different kinds of magnetism are distributed irregularly throughout the earth *(Gaus's theory)*. The latter, which is based on very elaborate mathematical calculations, is spoken of as "one of the most beautiful and the most important investigations that has appeared for many years in physical mathematics."

A very interesting account is given under the tenth section of the magnetism of iron ships and the means of obviating its effect upon their compasses. The researches of Mr. Archibald Smith on this subject are here introduced, together with a mathematical investigation of the effect of the ship's heeling.

The importance and value of the series of works on mathematical physics to which this belongs, has been so fully recognised by the universities, that we need say nothing to commend it. Moreover, the Astronomer-Royal—who, be it remembered, has been both Lucasian Professor of Mathematics and Professor of Experimental Philosophy at Cambridge—is capable of treating physical subjects in a manner which is possible to few, and which renders any work from his pen a valuable contribution to the scientific literature of the world.

The Sun: Ruler, Fire, Light, and Life of the Planetary System. By R. A. Proctor, B.A., F.R.A.S. 8vo., 480 pp. London: Longmans, Green, and Co., 1871.

The interest which the sun possesses for us is manifold. Apart from the fact that he is necessary to our life, and the source of all energy—*i.e.*, of all the work done (save tidal energy) in the world; that in fact, as Mr. Proctor has it, he is "ruler, fire, light, and life of the planetary system," there is a special reason why we should regard our great luminary with the profoundest interest. We may remember that the sun has even been the symbol of the Deity, and (the symbolism being forgotten) has often been himself worshipped. Agni, the god of light and fire, was placed first in the Hindu Trinity, and many of the most charming hymns of the Rig Vedu Sanhita are addressed to him. Then the ancient Persians introduced fire as the symbol of this deity, and Mr. Palgrave, during his late visit to Arabia, found in certain fastnesses traces of the old Sabæan worship still remaining among the inhabitants.

But all this was before man had asked "proud philosophy" to teach him what the sun is; and since nature has been of late communicating her secrets pretty freely in this direction to some well-known astronomers, and the sons of science,—Herschel

Schwabe, Carrington, De la Rue, Balfour Stewart, Huggins, Zöllner, Respighi, Lockyer, Young, &c.,—we must leave the fairy land of a primitive world, peopled by primitive men, with fresh Nature-worshipping intellects, and plunge into the more practical details of spiral protuberances, faculæ, chromospheres, and coronæ.

We are to regard the sun, says Mr. Proctor in the introduction, "as the recognised centre of the solar system, ruler over a scheme of worlds, on which he pours forth abundant supplies of heat and light." The first chapter treats of "the sun's distance and diameter."

The second chapter is entitled "The Sun as Ruler," and herein is discussed the action and influence of the sun as the controlling power of the motions of celestial bodies. At equal distances the sun exerts 315,000 times the attractive power of the earth. "So that if the earth's mass were as great as the sun's, her dimensions remaining unchanged, a mass which now weighs one pound would weigh more than 14½ tons. . . . A body, if raised but a single inch and let fall, would strike the ground with a velocity three times as great as that of the swiftest express train." The mean velocity of the earth around the sun is 18·2 miles per second. At a point as close as possible to the surface of the sun a body would possess a velocity of no less than 378·9 miles per second, while at the distance of Neptune, 2,745,998,000 miles (measured from the centre of the sun), he can control and also generate a velocity of only 4·7 miles per second. We can but be struck here with the wonderful richness of expression which Mr. Proctor possesses, and which he exercises so admirably and so judiciously. Astronomy, which has been ever admitted to be the grandest and sublimest of the sciences, requires such language to give full force to her wonderful results and deductions.

At the close of this chapter we have an example of graphic diction which reminds us somewhat of poor Hugh Miller's description of the six periods of creation, save that the following is more calm and indulges in less lofty flights; it is also less rhapsodical than the generality of Tyndallics.

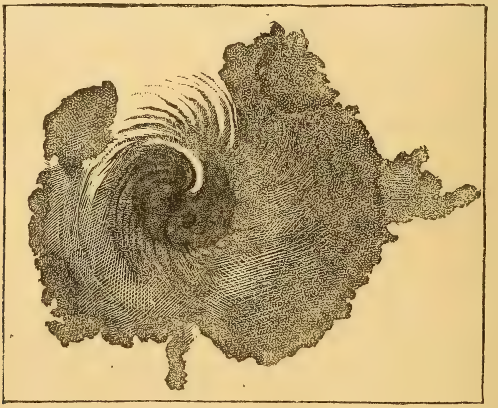
"Tracing back the history of that system, we seem to recognise a time when the sun's supremacy was still incomplete, when the planets struggled with him for the continually inrushing materials from which his substance, as well as theirs, was to be recruited. We can see him by the mighty energy of his attraction clearing a wide space around him of all save such relatively tiny orbs as Venus and the Earth, Mars, Mercury, and the asteroids. With more distant planets the struggle was less unequal. The masses which flowed in towards the centre of the scheme swept with comparatively slow motion past its outer bounds, so that the subordinate centres there forming were able to grasp a goodly proportion of material to increase their own mass or to form subordinate systems around them. And so the giant planets, Jupiter and Saturn, Uranus, and distant Neptune, grew to their present dimensions; and became records at once of the sun's might as a ruler—for without his overruling attraction the material which formed these planets would never have approached the system—and of the richness of the chaos of matter from which his bulk and theirs were alike evolved. Nor is the consideration without a mysterious attraction that, in thus looking back at the past history of our system, we have passed after all but a step towards that primal state whence the conflict of matter arose. We are looking into a vast abysm, and as we look we fancy we recognise strange movements, and signs as if the depths were shaping themselves into definite forms. But in truth those movements show only the vastness of the abysm; those depths speak to us of far mightier depths within which they are taking shape. 'Lo! these are but a portion of His ways; they utter but a whisper of His glory.'"

The third chapter treats of "Analysing Sunlight." The certainty with which we may assert the presence of this or that element in the sun or other heavenly body, is well shown by the fact that no less than 450 iron lines have been recognised in the solar spectrum. Now, the possibility of any such relationship being the result of *chance*, "is less than a fraction whose numerator is unity, and whose denominator consists of no less than 136 figures." The rest of the chapter treats of the application of the spectroscope to the examination of various portions of the sun's surface,—of the faculæ, prominences, &c.

The next chapter gives an account of the various observations which have been made with a view of studying the surface of the sun. Firstly of sun spots, to illustrate which we have two coloured plates, showing, respectively, a portion of the sun's disc observed in May, 1870, and the sun as seen by the author on September 25th, 1870. Faculæ (*facula*, a small torch) were first studied by Herschel, in 1792; he borrowed the name from Hevelius, and applied it to elevated bright places on the sun's surface, an excellent representation of which is given on p. 180. An account is given in some detail of the researches of Schwabe and Carrington on sun spots, and the more recent papers of Stewart, De la Rue, and Loewy. By the application of photography, a record of the appearance of the sun's surface on every clear day is now obtained. No less than 1137 sun spots have been detected. The above-mentioned physicists have, in summing up their recent researches, expressed the following opinion:—"Solar faculæ, and probably also the whole photosphere, consist of solid or liquid bodies of greater or less magnitude, either sinking slowly or suspended in equilibrium in a gaseous medium. A spot, including both umbra and penumbra, is a phenomenon which takes place beneath the level of the sun's photosphere." Several admirable woodcuts of sun spots are given in this chapter, notably one (Fig. 48) which represents a group of spots and the solar willow leaves. Mr. Dawes was the first to detect the appearance of cyclonic motion in a spot, as if a solar storm were taking place there; this is shown in the following figure (fig. 12).

The dimensions of sun spots are enormous. In 1858 a spot was noticed which was 107,520 miles long; and, in the same year, the largest spot ever recorded was first noticed:—"It had a breadth of

FIG. 12.



A spot presenting the appearance of cyclonic motion. (*Secchi.*)

no less than 143,500 miles; so that across it no less than eighteen globes as large as our earth might have been placed side by side."

The prominences and the chromosphere form the subject of the following chapter. The former were first examined during the total solar eclipse of 1842, but were perhaps detected in 1733, by Vassenius, of Gottenburg. Several subsequent observations were made; the most important, until recently, being those of Mr. De la Rue and Father Secchi, in 1860. The prominences were then proved to be real phenomena belonging to the sun, and to possess great luminosity, brilliant colour, and considerable photographic power.

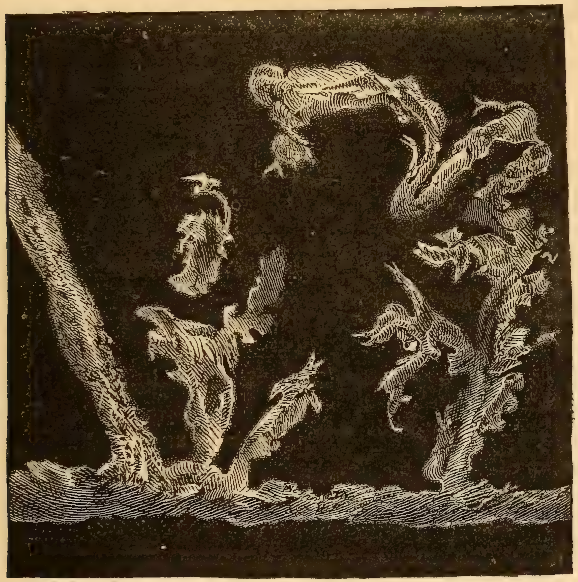
FIG. 13.

Illustrating the vast scale of the larger prominences.

Secchi further noticed that there were "masses of prominence matter suspended and isolated like clouds in the air." The height of the solar prominences is enormous:—130,000 miles is considered by astronomers no exaggerated amount. The following figure illustrates the great altitude of the larger prominences very strikingly.

The large disc on the left of the prominence represents the comparative dimensions of Jupiter, while the uppermost of the small discs within the prominence represents Mercury, the next Venus, then the Earth and Moon, and the lowest Mars. A coloured

FIG. 14.



A group of solar prominences.—March 14, 1869, 11h. 5m. (*Lockyer.*)

plate (p. 272) represents the prominences as seen during the total eclipse of August, 1868. The following figures represent a group of prominences drawn by Mr. Lockyer, and the group only ten minutes later. Dr. Zöllner has also figured a number of prominences, the most interesting of which are represented in beautifully coloured plates. Professor Respighi has studied the form and nature of the prominences even more fully than Zöllner.

Chapter 6 treats of "The Corona and Zodiacal Light." The corona is the crown of light which surrounds the black disc of the moon during a total eclipse; it is first mentioned by Apollonius, and later by Plutarch. A detail notice is given of eclipses in which remarkable coronæ have been observed, the most interesting observation having perhaps been made in 1842 by Arago.

In the next chapter the "Physical Condition of the Sun" forms the subject of study, and this is followed by a short chapter entitled "The Sun, our Fire, Light, and Life." The heat, light, and chemical activity which we receive from the sun has of late been often and accurately estimated. As to the heat, it has been calculated that if the sun's heat were distributed uniformly over the surface of the earth it would in one year suffice to liquefy a layer of ice 100 feet thick; and this amount of heat, be it remembered, is only the $\frac{1}{2,138,000,000}$th part of the heat emitted by the sun in the course of one year. The absolute

FIG. 15.

The same group as shown on the last page ten minutes later.

luminosity of the sun's surface is more than 146 times the luminosity of the lime light. The subject of the dissipation of energy and the maintenance of the sun's heat finds ample discussion in this chapter.

The last chapter is entitled "The Sun among his Peers." The sun, as is well known, possesses proper motion of his own; he is carried forward in space, and with him all the members of the solar system which revolve around him. We do not know, however, whether any of his brother suns accompany him. As the earth is carried along with the sun, while it rotates around him, the path followed by it is a helicoidal path.

"Jupiter is carried some 1,700,000,000 miles onward with the advancing sun, while he circuits once around his orbit of less than 1,000,000,000 miles in diameter; Saturn sweeps on through some 4,400,000,000 miles while circuiting his orbit, less than 1,850,000,000 miles in diameter; and the paths traversed by Uranus and Neptune amid the depths of sidereal space are even more remarkably drawn out, regarding them in their helicoidal character."

The work is terminated by two appendices; the one relating to "the approaching transits of Venus, and the best means for observing them;" the other to Eclipses. By means of the transits of Venus we can obtain, with greater accuracy than by other means, knowledge as to the sun's distance from us,—it is needless to say a matter of the extremest importance. The next transit will take place in 1874, and the Government has voted a sum of £10,000 for the purposes of the observing expeditions. Two maps accompany this appendix, and it contains many very useful hints as to the most effective method of observing it completely. There will be a second transit in 1882.

We have endeavoured to give some insight into a book which commends itself both to the man of science and to the general reader. The complete astronomer must revel in this book, not less than the man who, when he opened it, did not know the meaning of parallax or proper motion, and who was all ignorant of solar willow leaves and spiral prominences. The most recent results have been introduced, yet the book is popular in style. Popular scientific books are seldom the work of the real *savant*, yet Mr. Proctor is an accomplished astronomer and sound mathematician, and scarce a number of the "Journal of the Royal Astronomical Society" appears without a paper from his pen. We have long wanted books in this country of the *Le Ciel*, and *Phenomênes de la Physique* of the Guillemin class: popularly written, well printed, profusely illustrated, and accurate withal; instructive without being dull; stimulative and of a nature to lead the reader deeper and deeper into the depths of science. Mr. Proctor has supplied us with such a book as regards astronomy, and we cannot be too grateful to him.

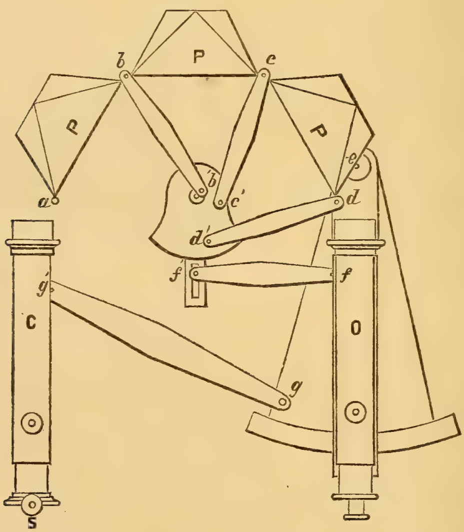
PROGRESS IN SCIENCE.

LIGHT.

Grubb's Automatic Spectroscope.—P, P, P are three prisms placed, we will say, in the position of minimum deviation for some mean ray—O being the observing telescope, and C the collimator carrying the slit at S. The prism tables are jointed together in a species of chain; the first point, *a*, of the chain working on a fixed stud let into the base of the instrument; the other three joints, *b*, *c*, and *d*, are connected by steel levers with three studs, *b'*, *c'*, *d'*, in a central disc attached at different distances from the centre, and in such positions that the levers form tangents in their mean positions.

If the distances of the studs from the centre be properly proportioned, the rotation of the disc draws the three joints, *b*, *c*, *d*, in or out as the deviation is required less or more, in just such a ratio as will preserve the position of minimum deviation in all the prisms. So far for the prisms; and now as to the telescopes. The collimator is stationary, pointed at the centre of the first surface of the first prism, which hardly moves at all. The observing telescope is attached to an arm which is centred on a point, *e*, in the third prism table, which corresponds

FIG. 16.



to the centre of the last surface of the third or last prism, so that in whatsoever manner the telescopes or prisms are turned, this telescope always points towards the centre of that surface, and consequently takes in the whole pencil of light emerging from that surface. On the same pivot is centred also a sector to measure the angle through which the observing telescope is turned.

The arm to which the observing telescope is attached is coupled to the central disc by a connecting rod, *f*, *f'*, of such proportions that the movement of the telescope round its pivot for the purpose of viewing the different parts of the spectrum will also rotate the disc and cause the prisms to assume their proper positions for that particular refrangibility of light under examination.

And, finally, a point, g, in the limb of the sector is coupled to a fixed centre, g', in the bed plate by another connecting rod, g, g'. This is for the purpose of compelling the sector to travel in a direction parallel to itself; that is to say, if in any position of the instrument a line were drawn upon the sector parallel to the collimating telescope, it would also be parallel to it in every other position. Hence, a little consideration will show that the readings of the sector give actual differences of deviation.

In fact, if the two telescopes were placed parallel to each other, and that point marked 180° on the sector and the other parts numbered up and down from that, the instrument would then read actual deviations.

In practice, however, such a system of division will probably not be found as convenient as some others, as it is only differences that are required; but these differences, it will be seen, will not be correctly given unless the sector has that particular motion imparted to it described above.

Two-Prism (*Compound*) *Spectroscope on Automatic Principle.*—The two-prism spectroscope for Dr. Huggins's star observations is constructed exactly on this same principle, modified, of course, to suit the two instead of three prisms. The prisms are compound, and admit of a pencil of light, $1\frac{1}{4}'' \times 1''$ to pass to objectives of telescopes which are $1\frac{1}{4}''$ diameter. The power of the spectroscope is, therefore, equal to that of four large prisms of 60°. There is one common collimator used for several spectroscopes; this collimator is $1\frac{1}{4}''$ aperture and $4\frac{1}{2}''$ focus. The great shortness of focus is obtained by making the object-glass of that quadruple form which Mr. Grubb applied to the 7-inch circle telescope in Armagh. The advantage of having the collimator of large angular aperture is that a more powerful cylindrical condensing lens can then be used and greater brilliancy obtained.

Compound Prisms.—The compound prisms devised by Mr. Grubb, F.R.S., for spectroscopic investigation are composed of three elements. The centre one being of dense flint glass of an angle varying from 90° to 100°, according to the exact nature of the glass, and crown prisms cemented on each side of about one-fourth that angle reversed in the direction of their apices. By cementing these crown prisms on the centre flint, the light which otherwise would not be capable of passing either in or out of the prism is enabled to do so, and a dispersion is by this means obtained equal to about two 60° prisms, and with a deviation equal to about one 60° prism. Various prisms have been made from time to time on the compound principle, and by various artists, but in most cases they have been tied with the condition of "direct vision," which is by no means necessary, except for special purposes, and sometimes even undesirable.

In other cases the centre flint has been made of extravagant angle; this is also useless, as crown prisms of great angle must then be attached, which again brings down the dispersion; and the inside surface being so much inclined to the course of rays much light suffers internal reflection. In the proportion arrived at by Mr. Grubb, the required qualities have been found both theoretically and practically to be the best balance. The advantages in using them can be summed up as follows:—Double the dispersion is obtained by them as compared with an equal number of ordinary 60° prisms with the same number of surfaces (the inside not counting, being cemented), a less absorption from the glass, one-half the complication of mechanism (particularly if automatic spectroscopes be used), and freedom from danger of injury to surfaces of flint glass.

Mr. Browning has contrived a direct vision spectroscope, for use in the Bessemer process. It has great dispersive power, with very low magnification, the result being a very luminous spectrum, which fills the whole field of view. The jaws of the slit are opened or closed by turning a milled ring just behind them. There are no adjusting screws or movable parts to this instrument. It is made with what are known as fixed adjustments, so that in an ordinary workman's hands there is nothing to get out of order. The instrument contains ten prisms, and will divide the D lines in the solar spectrum quite easily.

Mr. Browning has recently published some chromo-lithographs of spectra for the special purpose of keeping records of the position of absorption-bands as observed with his micro-spectroscope. The size of the paper is the same as that of the present journal, and each plate contains seven coloured figures on a black ground, with the places indicated of the principal solar lines; the memoranda can be made with pencil, or, still better, with a brush, and a tolerable representation of the observed spectrum obtained. This mode of registration will be preferred by many to Mr. Sorby's ingenious notation as adapted to his interference spectrum, an instrument somewhat difficult to procure, on account of the trouble involved in making the plate of quartz of the proper thickness to produce the exact number and position of the bands.

Dr. Huggins has devised a registering spectroscope, by means of which the positions of lines observed in the spectrum may be instantly registered without removing the eye from the instrument, so as to avoid the loss of time and fatigue to the eye of reading a micrometer-head, or the distraction of the attention and other inconveniences of an illuminated scale. In this instrument the small telescope of the spectroscope is fixed, and at its focus is a pointer which can be brought rapidly upon any part of the spectrum by a screw-head outside the telescope. The spectrum and pointer are viewed by a positive eye-piece which slides in front of the telescope, so that the part of the spectrum under observation can always be brought to the middle of the field of view. The arm carrying the pointer is connected by a lever with a second arm, to the end of which are attached two needles, so that these move over about 2 inches when the pointer is made to traverse the spectrum from the red to the violet. Under the extremity of the arm fitted with the needles is a frame containing a card, firmly held in it by two pins which pierce the card. This frame containing the card can be moved forward so as to bring in succession five different portions of the card under the points of the needles; on each of these portions of the card a spectrum can be registered. The mode of using the instrument is obvious. By means of the screw-head at the side of the telescope, the pointer can be brought into coincidence with a line; a finger of the other hand is then pressed upon one of the needles at the end of the arm which traverses the card, and the position of the line is instantly recorded by a minute prick on the card. From ten to twelve Fraunhöfer lines can be registered in about twelve seconds, and when the same lines are recorded five times in succession on the same card, no sensible difference of position can be detected between the pricks registering the same line in the several spectra.

A most ingenious application of the spectroscope, and one likely to be of considerable use in many enquiries, has been made by Professor Church. On one side of a crowded court several cases of typhoid fever had been developed. The water used by the inhabitants of these houses was drawn from a rather shallow well, and was highly charged with various unoxidised compounds of nitrogen. It was suspected that the drain from a public urinal might be defective and have allowed egress of its contents into the well. This notion was confirmed by the quantity of common salt contained in the well-water, namely, seven times as much as that in the normal waters of the neighbourhood. But it received an absolute proof in the following novel manner. Two grammes of a *lithium* salt were introdued into the urinal. Two hours afterwards lithium was detected spectroscopically in a litre of the well-water before alluded to. A quantity of this water, ten times as large, showed no trace of lithium previously.

Microscopy.—Dr. Ormerod, of Brighton, uses a new material for grinding sections of bone, tooth, and similar hard tissues. The bone is first cut into slices with a saw in the usual manner, and rubbed down to an even surface on a file or coarse stone; then, using a piece of flat pumice stone as a pad, it is rubbed down with water to the necessary degree of thinness on a piece of coarse ground glass about 6 inches square; the glasses, when the first roughness is worn off, may be used for giving a still finer surface to the section. Ground glass will be found to act rapidly and efficiently as a grinding agent upon tissues of the hardness of those mentioned. The section, when dried with blotting paper, but *still moist*,* is to be mounted upon a slide on which Canada balsam has been hardened, and covered with a thin glass on which balsam has been similarly hardened, the moist surface prevents the balsam penetrating the lacunæ and canaliculi and obliterating the structure. With practice, from the time of cutting the slice till the slide is ready for the cabinet, about half an hour will have elapsed.

A machine for grinding sections of hard substances for microscopic purposes is described by Mr. C. Sellers, in the "Proceedings of the Academy of Natural Sciences of Philadelphia." It consists of an emery or corundum wheel similar to those employed by dentists, over which is placed a plate having a circular aperture, through which a portion of the edge of the wheel projects; the amount of this projection is capable of being altered by means of a screw adjustment. The substance to be ground is attached by a suitable cement to a glass slide and moved over the aperture while the wheel is rotating until a level surface is obtained. The contrivance is ingenious, but for producing perfectly plane surfaces nothing can equal the lap or horizontal wheel commonly used by opticians and lapidaries; the edge of a wheel has a constant tendency to grind a hollow place, and most careful shifting of the object is required to secure any approximation to truth.

Dr. Barker brings before the Royal Irish Academy an adaptation of the immersion principle for the illumination of microscopic objects. The good qualities of hydro-objectives or immersion lenses, with respect to improved definition, increase of light, working distance, and magnifying power, are already well known; corresponding advantages are to be obtained by the use of illuminators constructed on a similar plan. Dr. Barker has at present only experimented on the principle as applied to Wenham's paraboloid, which he constructs with a flat top (Fig. 17), instead of the usual hemispherical cavity, and introduces a film of water between it and the under surface of the slide, thus securing optical contact between the paraboloid and the slide; the film of fluid will also act as a water joint, and allow free action to the stage movements, so that any part of the slide can be easily examined. The oblique rays are thus economised; little dispersion takes place if the object is mounted in fluid or balsam, and there is sufficient brightness for all powers. Another advantage arises unexpectedly, for if the focus of the paraboloid be made a little higher, or if a slide of extra thinness be used, the

FIG. 17.

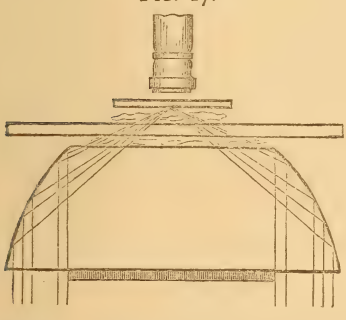
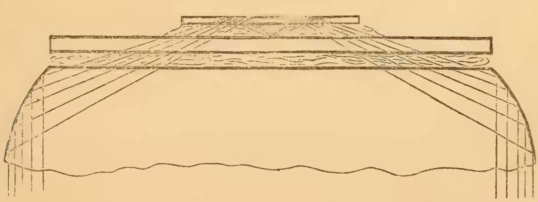


FIG. 18.



oblique rays will undergo total reflection from the upper surface of the covering glass and be sent down on the object so as to illuminate it by reflected light

* Mounting a specimen damp in balsam must sooner or later give rise to a cloudy appearance in the surrounding medium; if the balsam is well hardened and used with the smallest necessary amount of heat, there is but little fear of the canaliculi being filled.

(Fig. 18), in a nearly similar manner to Mr. Wenham's combination of paraboloid and truncated lens,* but of course with less loss of light. The following constructive details are given. If the focus be chosen $\frac{1}{12}$th of an inch above the upper surface, the equation of a perpendicular section $y^2 = ax$ will become $y^2 = a\left(\frac{a}{4} + \frac{1}{12}\right)$; and solving the equation for a, we shall have $a = -\frac{1}{6} \pm \sqrt{4y^2 + \frac{1}{36}}$; and if the upper surface of the paraboloid be made $\frac{3}{4}$ths of an inch in diameter (a size most convenient in practice), a becomes $=$ to $\frac{3}{4}$, and the equation $y^2 = \frac{3}{5}x$ is that of the paraboloid form. Should the focus be taken at a distance of $\frac{1}{10}$th of an inch above the truncated paraboloid, and its upper surface be 1 inch in diameter, then the equation becomes $y^2 = \frac{3}{7}x$ *quam proxime:* this latter form will admit of a hollow cone of light of 120° to 185°, and will almost give a dark ground illumination for a $\frac{1}{8}$th immersion object glass. A modification of the same principle is also given by which objects may be illuminated from above and the use of the highest powers permitted. Dr. Barker further considers that the immersion principle is applicable to all kinds of condensers placed in the axis of the microscope.

The thinnest kind of glass used for covering microscopical objects is very troublesome to clean on account of its brittleness; when slight friction will remove the dirt it is best rubbed between two discs of wood or metal covered with wash leather, but if cleaning with alcohol or other fluid is needed, accompanied with somewhat hard rubbing, the breakage is considerable. The fluid used by photographers for cleaning glass plates, composed of sulphuric acid 1 oz., bichromate of potassium 1 oz., water 1 pint, will be found very effective. The thin glass is to be left in this solution for some hours,—a day or two will do no harm; it can then be poured off and the glass washed with several waters to remove all traces of the preparation, the last washing being with distilled water. The thin glass is placed upon a soft porous cloth† spread upon a table and carefully rubbed dry with another cloth; it will be found that very little breakage takes place, as the glass is so perfectly cleaned even when extremely dirty or covered with *bloom*, that very little pressure or hard rubbing will be needed. This process may be used with advantage for cleaning slides as well as cover glass; the solution is cheap, and effectually cleanses all kinds of glass with the minimum amount of trouble.

Dr. Ward, at the American Association for the Advancement of Science, remarked "that the production of a beaded appearance on the Podura scale as a purely optical effect should be considered no longer doubtful, but rather as an occasional accident to persons using high powers." As an extreme instance in the case of a coarse and familiar structure, he related that, while experimenting upon an elater of *Marchantia polymorpha*, that beautiful double spiral was resolved into three rows of "beads" or "hemispheres," perfectly distinct and unmistakable, which occupied of course the position of the middle and edges of the spiral. They were illuminated by parallel light, very oblique, under a $\frac{1}{15}$th objective of 175°, worked at a power of 3000 diameters.

According to the "American Naturalist," October, 1870, Colonel Dr. J. J. Woodward has succeeded in photographing the beaded appearances in the scale of *Degeeria domestica*, from specimens supplied by Mr. S. J. McIntire. They are plainly seen in a series recently received by the Royal Microscopical Society: also the lines of *Amphipleura pellucida* (*Navicula acus*), and the hemispheres of *Surirella gemma*. Mr. Wenham considers the beads as "ghost beads," and evidently caused by intercostal corrugations of membrane.

Mr. T. H. Hennah, of Brighton, has succeeded in procuring a series of photographs of arrangements of intersecting glass rods, as suggested by Dr. Pigott: the results are curious in the extreme; they were placed before the writer without his being informed of their nature, and were at once pronounced to be diatom hemispheres, insect eyes with diminutive images of objects, and

* Monthly Microscopical Journal, 1869, vol. ii., p. 28.

† Old cotton stocking, carefully cleaned with soda and hot water to remove the soap from former washings, answers well for this purpose.

something very like the ! markings of the Podura test scale, *Lepidocyrtus curvicollis*. The appearances in the photographs are much more deceptive than those seen in the glass rods themselves, as the observer is aware of what he is looking at. The effect is most conveniently produced by mounting a number of small glass rods placed in contact in two circular frames, which are caused to revolve over each other by the contrivance used in chromatrope slides. The deceptive appearances are very well shown by using the slide as a magic-lantern object.

The fossil sponge spicules obtained by Mr. W. Vicary, of Exeter, from the green-sand of Blackdown and Haldon, and described by Mr. E. Parfitt in the "Transactions of the Devonshire Association for the Advancement of Science," have been further examined by Mr. H. J. Carter, F.R.S., and form the subject of a paper in the "Annals of Natural History," vol. vii., p. 112. The spicules are embedded in quartz sand, and are, as Mr. Carter believes, the remains of dead and disintegrated sponges, consisting chiefly of the larger spicules, more or less in a fragmental condition, and altered in shape by trituration; the minute forms of spicules are generally absent. The surface of the spicules is considerably eroded, and presents the peculiar form of chalcedony. The triturating and solvent influences attending petrifaction have probably obliterated any spinous or tubercular processes which might have been expected to have been found on spicules of so large a size, and which exist on recent sponge spicules with which they are identical. Besides sponge spicules, the deposit contains a few minute bivalve shells and *Foraminiferæ*, but no remains which could be identified with the calcareous spicules of *Echinodermata*, *Alcyonidæ*, *Gorgonidæ*, or *Ascidiæ*. Owing to the tolerably perfect condition of many of the spicules, Mr. Carter has succeeded in identifying a large number of them; but owing to the almost entire absence of the smaller spicules, it has only been possible to make the comparisons with the larger spicules of recent sponges. The four plates which illustrate the paper contain seventy-six figures of spicules from the localities named.

Mr. H. J. Slack has noticed the unusual form of microscopic crystals produced by dissolving various salts in a colloid solution of silica obtained by dialysis. Sulphate of copper afforded the following results:—First, what may be termed a "pavement pattern," in which the silica cracks divide the film into a great number of compartments, of three, four, five, or more sides, which may be roughly likened to the appearance of vegetable ivory, or a section of pinna-shell. In each of these divisions crystallisation takes place, usually radiating from the centre, and sufficiently diversified in thickness to produce polariscope patterns. The crystals thus produced have a tendency to exhibit Maltese crosses, of colour changing with the positions of the polarising and analysing apparatus. In these "pavement patterns," the force exerted by the silica film in contracting and cracking causes the crystalline force to operate in limited compartments without change in the linear direction common to radiating groups. In other cases, the contractile force acts tangentially, or in curves, while the crystalline force operates radially, and then exquisite spiral and turbinate forms, similar to those described by Mr. R. Thomas (Quart. Journ. Micr. Sci., 1866, p. 177). Hippuric acid, salicine, and tartaric acid, all yielded patterns differing from the ordinary forms of crystallisation from aqueous solutions. Mr. Slack remarks, in conclusion, that it is impossible to view a number of slides prepared in this way without being struck with the resemblance of many of the patterns to those of sections of various structures in organised beings, and we are led in the direction long since indicated by Mr. Rainey, to extend our notions of the functions performed by the chemical and physical forces at work according to their own methods in living things. The action of the silica appears to be chiefly physical and mechanical; but the appearance of the crystals leads to the belief that some portion of silica is present in them, and is not eliminated as the process of crystallisation goes on. (Monthly Micro. Journ., vol. v., p. 50, and pl. 77 and 78).

A new microscrope lamp, designed by Mr. Fiddian, of Birmingham, has recently been brought forward by Mr. Browning. One of its merits is great portability, the size of the cylindrical case, Fig. 19, in which it is packed being only 6 inches by 3 inches, and its disposition when put away will readily be understood by Fig. 20. The chimney is of metal, and to enable it to be placed in so small a case, the upper part consists of two joints sliding into each other like the drawers of a telescope; the aperture in the bulb is furnished with white and tinted glasses, and also with a plano-convex lens for obtaining parallel rays. The general form of the lamp is shown in Fig. 21. An arrangement is made which permits the lamp to be tilted to a slight extent when

FIG. 19.

FIG. 20.

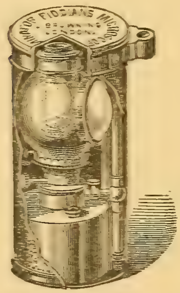
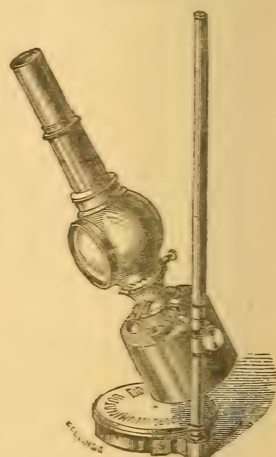


FIG. 21.



required. The burner is small and of good quality, and the intensity of the light much increased by the white lining of the interior of the chimney, which can easily be renewed whenever it becomes discoloured by the deposition of soot. Mr. Browning is engaged in making provision against the liability of spilling the paraffin in case of the accidental inversion of the lamp during conveyance from place to place, an improvement that will be fully appreciated by microscopists who are in the habit of using their instruments away from home.

Captain F. H. Lang, President of the Reading Microscopical Society, communicates his experience, and that of Captain Haig and Mr. Tatem, respecting the selection and mounting of diatoms. The instrument preferred by Captain Lang for picking up diatoms is a finely pointed badger-hair, whipped on to a light handle, so that the hair does not project more than about ⅜ths of an inch. This is dipped into a weak solution of gum and allowed to dry, and, when breathed upon, the most delicate form from a dried gathering may be taken up, and will remain on the hair whilst the slide containing the general gathering is shifted, and that on which it is to be placed substituted. Captain Haig uses a thin cell, formed of gold-size, baked until it is partially carbonised after the manner of japanning, to prevent the diatoms being crushed by pressure. The diatoms are to be mounted on the cover in preference to the slide, on which is placed a smear of glycerine, containing a minute quantity of gum to keep the diatoms in their places. The glycerine is then disposed of by evaporation on a hot plate, and the diatoms mounted either dry or in balsam, or the solution of balsam in benzol or chloroform. Mr. Tatem places his cover, on which the diatoms have been arranged, on the cell which has been filled with chloroform, and places a drop of balsam in contact with the edge of the cover, allowing it to run in as the chloroform evaporates. For keeping the diatoms in their places when gum is necessary, a grain and a half in an ounce of distilled water is generally sufficient. A small drop of this fluid is preferred by some to arrange their diatoms in instead of glycerine.

Mr. F. H. Wenham contributes a valuable paper on "Object-Glasses and their Definition," to the January number of the "Monthly Microscopical Journal." It is an admirable sequel to his series of papers on the construction of object-glasses, vol. i., p. 111, &c., of the same journal. After criticising the papers of Dr. Pigott,* and calling attention to the fact of the reduction of angular aperture of the objective, when used on objects mounted in Canada balsam, demonstrated by himself and Professor Robinson in 1854,† he proceeds to explain, by the aid of a diagram on a large scale, the course of the rays through an eighth objective of 130° aperture of his own construction, and giving reasons for the various combinations of lenses employed in it. Some remarks are made respecting the vexed question of the structure of the Podura scale, and the appearances shown in Colonel Dr. Woodward's photographs defended against Dr. Pigott. Mr. Wenham bears testimony to the extreme value of the "mercury globule" as a test for ascertaining the nature of the aberrations of object-glasses during their construction, and gives some details of the mode of reading its delicate indications. The subject of immersion lenses is treated in a very plain manner, and their effects illustrated by large diagrams. Of the one described, he writes as follows:—"The effect of this immersion lens is to give greater clearness and brilliancy to the object, and render markings more distinct that were before scarcely visible with the dry lens. This is, in fact, attributable to the saving of light and comparative absence of refraction and reflection from the top surface of cover and front of lens; but the great merit consists in the perfect correction that the adjustable thickness of the water stratum affords in compensating for every thickness of cover. Nor is the thickness of an immersion front a matter of particular nicety, for it can be made as thin as desirable; the water will occupy the place of the deficiency." The whole paper is one of great interest at the present time, when the subject of high power definition is being actively discussed.

HEAT.

The following criticism of a paper by the Rev. H. Highton has been forwarded to the Editor by John Hopkinson, D.Sc.:—Mr. Highton's first chapter is devoted to the consideration of the heat produced in the various parts of a conducting circuit by the passage of an electric current. The paradoxes there proposed seem to be founded on a misconception as to what the received views on the subject are. He assumes that the heat generated in a voltameter, or battery cell, follows the same law as in a metal wire—that it is proportional to the resistance and square of the intensity, and is independent of the chemical decompositions which may take place; from this, by reasoning which involves further assumption, he shows that the energy produced by a given consumption of zinc is variable. To show that he is mistaken about the facts, I will quote from Jamin's "Cours de Physique," vol. iii., p. 173:—"M. Becquerel admits that it (Joule's relation between heat and intensity of current) is applicable when we electrolyse sulphate of copper with copper electrodes, because, though on the one hand, there is solution of metal, and consequently absorption of heat at the positive pole, on the other there is a deposit of an equal quantity of copper and a disengagement of the same quantity of heat at the negative pole. Everything goes on as if there were no decomposition, and the law, heat $= Kri^2$, applies to this liquid as to a solid. The circumstances are totally different when the voltameter considered contains water with platinum electrodes. The gases, as they are disengaged, absorb all the heat which they produce in combining; it is proportional to their quantity; that is, to the intensity, i, and a constant, E. Consequently, the heat found in the voltameter ought to be equal to $Kri^2 - Ei$. Experiment confirms this explanation." It will be seen, then, that the fundamental assumptions of the first chapter are contradicted by the facts of the case, as explained in an elementary manual. It

* Monthly Microscopical Journal, July and September, 1870.
† Quart. Journ. Micr. Sci., July, 1854, p. 212; and January, 1855, p. 165.

is, therefore, useless to discuss the reasoning until these are proved false. The rest of the paper refers more immediately to the conversion of heat and chemical action into mechanical effect. It will be amusing to anyone conversant with the subject to observe how, in the second paragraph, Mr. Highton gravely sets forth the elementary deductions of thermodynamics as anomalies. That stretching a wire, within the limits of its elasticity, cools it, follows from the fact that heat expands it. A little further on, we find a confusion of *energy* with the *availability of energy*, or, as Tait calls, the *eutropy*: "There is just as much mechanical energy in a lump of ice which will produce 100 units of cold as there is in a lump of coal which will produce 100 units of heat; there is as much stored-up power in a glacier as in a coal-mine." . . . "No amount of heat in a body can produce any effect till that body comes into contact or communication with some other body either hotter or colder than itself." If Mr. Highton will refer to Professor Tait's "Thermodynamics," or Professor Balfour Stewart's "Heat," for the second law about reversible engines, he will see in what way a cold refrigerator is not exactly a source of mechanical energy, but a means of converting more energy already existing as heat into the form of visible motion than we otherwise could; the glacier Mr. Highton proposes to use would be not a source of energy itself, but might serve to render the energy existing as heat around us available. He proceeds: "So that, in reality, force is produced, not by heat or cold, but by the restoration of equilibrium in the heat of two bodies, or parts of bodies, unequally heated, and mechanical energy produces neither heat nor cold (except accidentally), but simply a disturbance of the equilibrium in the heat of two bodies, or parts of a body." I may just point out that we are not talking about force, but about energy or work. I admit we cannot transform heat into force any more than into yards; there is no more an equivalent of heat in pounds weight than in pints, though there is in foot-pounds; but a foot-pound is not a force. This confusion of work and force is common in Mr. Highton's writings. It produces a serious error in his paper in the "Chemical News," of January 27th, which is repeated in the note at the end of the paper in last quarter's "Journal." He then assumes that, if the forces in operation in an engine are greater, the engine will necessarily produce more work from the same quantity of fuel. But I presume that, in this case, though the reverend gentleman says force, he means work. Now, though to change heat into work, we need to have a source and a refrigerator of different temperatures, yet, of the heat which leaves the source, only part ever reaches the refrigerator, the difference disappears as heat, and is changed into the equivalent amount of work. What is meant by mechanical energy producing heat *accidentally*, I fail to comprehend. Possibly, when the theory which shall replace the science of Thermodynamics is set forth, all cases of failure will be referred to the chapter of accidents, and the theory will have mainly to be proved by its exceptions. But the fact that mechanical energy may be transformed into heat was proved long ago, beyond dispute, by Davy's well-known experiment, in which he melted two pieces of ice by rubbing them together in an atmosphere itself below the freezing point. In paragraph 7, we have—"A given amount of heat applied to expand air will raise ten times the weight that it will if applied to expand vapour of turpentine, and one and a quarter times as much as if it were applied to expand steam. It may be answered, 'Yes; but it also expands the vapour of turpentine or water, as well as raises the weight!' True; but this is not mechanical energy as measured by foot-pounds raised; and to assume that it is equivalent to it, is to beg the question at issue." I think Mr. Highton makes more than one mistake here. His premises are loosely stated; and even if they had been right, his conclusion would not follow from them. By proper mechanical appliances, such as hydraulic presses, we may make a given amount of heat raise almost any weight we please, whether it be applied to heat air, steam, or vapour of turpentine; but let us suppose that instead of "raise ten times the weight," it were written "do ten times the work." The specific heat of vapour of turpentine is just ten times that of air; so that we may conclude that Mr. Highton means that it will require ten times as much heat to expand a given volume of turpentine by a certain amount against a given pressure as would be required if the turpentine were replaced by air. This will be a little inaccurate, as the coefficient of expansion of a vapour is always greater than that of a permanent gas. Now, this heat thrown in has these effects: a portion, H_1, is used simply to heat the gas to raise its temperature; a second part, H_2, does internal molecular work, by separating the particles against their own cohesion; whilst a third, H_3, expands the gas against the external pressure, and produces the work which we measure in foot-pounds. It is easy to see that Mr. Highton compares these foot-pounds with the whole heat thrown into the gas, $H_1+H_2+H_3$, instead of the heat, H_3, used in producing them. In a gas, H_2 is very small, as the experiments of Joule and Thomson show; but in a vapour it is considerable. Mr. Highton's answer to the supposed objector is altogether irrelevant. The objector might have added that the heat also raises the temperature of the gas, and he does not assume that this is equivalent to work measured by foot-pounds, though it is only that some of the heat is used to effect the expansion and the heating, whereas Mr. Highton unwittingly assumes that none of the heat is so applied, but that all goes to lift the weight.

ELECTRICITY.

The Passage of Electrical Currents through Rarefied and other Atmospheres.—Mr. C. F. Varley, the electrician, has recently made several new discoveries in relation to the passage of electricity through rarefied gases, and through the atmosphere at its normal pressure. Some of these discoveries were made known in papers read before the Royal Society a few weeks ago, and several were shown at General Sabine's *soirée* at Burlington House a fortnight since. In his experiments he uses Geissler's vacuum tubes, which have been exhausted by chemical means to a degree ten or fifteen times as attenuated as the vacuum which can be produced by the best made air-pump. Such a tube shows stratifications in the light when an electrical current is passed through it. He has discovered that four distinct kinds of luminous discharge may be produced by graduating the strength of the current flowing through the tube, by introducing various amounts of resistance into the circuit. The results may be summed up as follows:—1. With an extremely feeble current of high intensity and small quantity, the luminous positive pole is alone visible. This light, although scarcely visible to the eye in the darkest room, photographs itself perfectly on a photographic film of wet sensitised collodion; an exposure of thirty minutes with a double-combination portrait lens is, however, necessary. 2. On diminishing the resistance so as to increase the quantity, a tongue of light projects from the positive towards the negative pole, and the two poles become luminous. 3. On still further augmenting the quantity, the negative pole only becomes luminous. 4. Lastly, on passing a momentary current of great quantity and intensity through the tube, the positive pole alone is luminous, but in spots only, and not all over as in the first case. All these experiments have been photographed by Mr. Varley.

A bell struck in a vacuum produces no noise. In the performance of the fourth of the foregoing experiments, the momentary discharge of electricity through the vacuum produces a sharp distinct "click" of a peculiar sound.

Mr. Varley has also discovered that four different kinds of discharge may be obtained through air, between the two conductors of a powerful Holtz's electrical machine, capable of giving sparks from eight to eleven inches in length. In the course of his experiments, he observed one kind of discharge, which seems to clear up the mystery hanging over that very rare phenomenon—ball lightning.

When a small strip of paper, pointed at both ends, is attached to the knob of the negative conductor of the Holtz's machine, the two ends of the paper being bent, so as to point towards the knob of the positive conductor, and when the two conductors, fully and continuously charged, are placed rather too far apart for the spark to pass, two bright spots are seen upon the negative conductor. The first impression with regard to these spots is that there are two particles of dust on the positive pole, acting as points to throw off electricity. That such is not the case is easily shown by rotating, first the negative, and then the positive pole. When the positive pole rotates, the luminous spots do not rotate with it; but when the negative pole rotates, and the positive is stationary, the two luminous spots on the positive pole rotate with the negative. If, therefore, a negatively charged cloud should be passing over the earth, and one corner of the cloud be nearer to the earth than the remainder, a luminous ball might possibly be seen running over the surface of the ground, and following the movements of the cloud above. When the cloud came sufficiently near the earth for the spark to pass, there would be a flash of lightning and a crash of thunder. Mr. Varley cautiously observes that this *may* explain the phenomenon known as ball lightning, which is too well attested to be doubted, but which, from the absence of explanation, has been hitherto unbelieved to be a reality by many men of science. This experiment was shown by Mr. Varley, for the first time in public, at the *soirée* previously alluded to.

On the same occasion, another interesting experiment was shown by Mr. Varley, in confirmation of an hypothesis which he mooted at one of the *soirées* last year, namely, that the stratifications in vacuum tubes are partially or wholly due to the presence of particles of matter detached from the negative pole. Plücker has shown that when a current of electricity is passed from the positive to the negative pole of a Geissler's tube, and the tube is placed over the poles of a very powerful magnet, the light is gathered up, and follows the course of the magnetic rays, no matter what be the direction in which the electric current is flowing. When the electric current and the magnetic rays run parallel to each other, a magnificent arch is produced, which extends on each side of the negative pole, beyond where there is any electric current flowing. Last year Mr. Varley stated, that from the nature of this phenomenon, he believed the arch to consist of incandescent particles detached from the negative pole, and controlled as to their course by the magnetic rays. The truth of this conjecture he has since supported by the following simple experiment:—A thin slip of talc, 1 inch in length, $\frac{1}{10}$th inch broad, and about $\frac{1}{10}$th grain in weight, is mounted on a single fibre of silk, and suspended inside the Geissler's tube. When this tube is placed over the poles of a powerful electro-magnet in such a position that the luminous arch shall not touch the talc, the passage of the electric current does not put the talc in motion. If the tube be so shifted that the arch plays upon the lower portion of the talc, it repels it. If it be still further shifted, so as to throw the arch against the upper portion of the talc, it is repelled, although in each case, from the position of the poles, there is no electric current whatever passing in that part of the tube which contains this delicate tell-tale. When the arch is allowed to play against the almost invisible fibre of silk which suspends the talc, the fibre is in no way injured. A still further confirmation of the truth of the hypothesis is, that that portion of the arc which strikes the talc produces a luminous cloud brighter than the rest of the arch, indicating that the matter is arrested and condensed there; the remaining portion of the arch is cut off by the intervention of the strip of talc.

The most singular thing, perhaps, in this experiment is the fact that this transfer or projection of matter from the negative pole, is in the opposite direction to that in which, *à priori*, it might be expected to pass. It is in the reverse direction to that in which particles of carbon are carried between the carbon points of the working electric lamp. Mr. W. R. Grove noticed, many years ago, when experimenting with the voltaic arc in air on a very large scale, that if the negative pole were of platinum, while the positive pole was carbon, there was a transfer of carbon from the positive pole to the negative; but, in addition to this, particles of platinum appeared on the carbon, they having been transferred from the negative pole to the positive, in obedience to some law at present unexplained.

METEOROLOGY.

The Meteorological Office has published Parts II. and III. of its "Quarterly Weather Report for 1869," containing the lithographed curves and explanatory chronicle for the six months from April to October.

We have also to welcome a new and revised edition of the "Board of Trade Barometer Manual," which has just appeared. This little book, which runs to the length of 70 pp., contains a good deal of new matter compared with preceding editions. Among other points, we may notice a chapter on "The Present Condition of our Weather Knowledge in Connection with Meteorological Telegraphy," by Mr. Scott; a chapter on "The Use of the Barometer to Seamen," by Captain Toynbee; and a short description of "The Most Usual Forms of the Barometer, with some Tables for Barometrical Reductions." The text is illustrated by a few woodcuts and some plates, and the whole pamphlet forms a much more complete and satisfactory manual than those formerly issued by the department.

We are glad to notice that since the beginning of the year the "Shipping and Mercantile Gazette" has commenced the issue of a daily wind chart for these islands, the information for which is supplied by the Meteorological Office. The chart is prepared on a plan devised by Captain Charles Chapman. The publication of these charts is a great step in advance, and we learn that they have been received with very general approval by the subscribers to the "Gazette."

The last number of the "Proceedings of the British Meteorological Society" contains a paper by Mr. Dines, "On Evaporation and Evaporation Gauges." The experiments seem to have been very carefully carried out; but as the author of the paper himself remarks, a much more thorough investigation of the subject is requisite before laws can be laid down. The mode of testing the evaporation, on a small scale, was to place a vessel of water on one scale of a delicate balance, and to counterpoise it exactly with weights. If then evaporation be taking place, the weight of the water will decrease; if, on the contrary, condensation be going on, the weight will increase. In either case, the rate at which the weight changes can be measured, and also the temperature at which condensation ceases and evaporation begins. Theoretically this temperature should be the dew-point as given by independent hygrometrical observations, and, practically, Mr. Dines found that in the saturated atmosphere of a greenhouse the two temperatures nearly agreed; but that in the ordinary atmosphere of a room the temperature at which the water on the scale of his balance ceased to increase in weight was sometimes as much as 3° or 4° below the dew-point, as given by the wet and dry bulb hygrometer. Mr. Dines's evaporating gauge is a large cistern, with a smaller cistern beside it, connected with it by a pipe at the bottom, so that hydrostatic equilibrium is always preserved. The level of the water in the small cistern is observed by means of a ball which floats on it, and is attached to an arm hinged at the bottom of the cistern. The arm may be prolonged beyond the ball, so as to describe a large arc and give an open scale. The remainder of the number is mainly taken up with accounts of the Auroras of October 24, 25, as observed by the several assistants at Greenwich Observatory. As might be expected, the individual observations correspond very well with each other, so that the notices might fairly have been condensed from 16 pages into three or four.

Mr. Blanford has published a paper "On the Normal Rainfall of Bengal," in the "Journal of the Asiatic Society." Dove's notice of this region, in his "Rain Tables for the Globe," contained in the first part of his "Klimatologische Beiträge," is confined to the figures for 12 stations. The number of stations included in Mr. Blandford's paper is 47, and the Presidency is divided into ten districts.

The principal results of the discussion are:—

1. The rainfall of Eastern begins earlier, and is on the whole heavier than that of Western Bengal, at stations equally distant from the sea, and at equal heights above its level.

2. The south-west monsoon of Eastern Bengal is probably induced by the rarefaction over Thibet; that of Western by rarefaction over the Punjaub.

3. Western Bengal receives, in addition to the normal rain of the south-west monsoon, a slight precipitation during the cold season, and also some irregular spring rains.

From the other side of the Atlantic we learn that in the United States a system of telegraphic weather reports and of storm warnings is in process of organisation, under the superintendence of General A. J. Megens, the chief signal officer. This is a most important measure, for hitherto the meteorological observations taken in the United States, though in themselves copious and valuable, have been of an isolated and, therefore, desultory character, as was unavoidable, owing to the vast extent of territory covered by the stations. It is, however, to be regretted that, in drawing up the report which announces the proposed scheme, care has not been taken to resort to the latest sources of information in Europe. The state of things described as existing in this country and in France in 1870 is that which prevailed in the years 1862-3. No notice whatever has been taken of the discontinuance of weather forecasts, or of the other changes which occurred on the death of Admiral Fitzroy.

While speaking of the development of meteorology in the States, we should not omit to notice that the government of Canada are also organising a general system of observation for the several provinces of the dominion. Among the proposals for storm-warnings in America, there is one by a Mr. Watson which is rather amusing. He suggests that notice of storms should be given by the discharge of artillery!

The Common Council of the City of New York have established a very complete meteorological observatory in the Central Park, under the management of Mr. Daniel Draper. This observatory is fitted with self-recording instruments, somewhat similar in principle to those at the observatories in connection with our own Meteorological Office. Experience will show whether the alterations introduced by Mr. Draper are improvements or the contrary. Among other instruments there is a self-registering rain-gauge. The description of the instruments will be found in the "Thirteenth Annual Report (1869) of the Committee of the Central Park."

In Holland, the Marine Department of the Meteorological Institute, after eight years of nearly total silence, has resumed the publication of results. The present issue, edited by Lieutenant Comelissen, consists of "Sailing Directions from Java to the Channel," and is a new edition of the work published by Lieutenant Audran in 1858. It is in two parts. Part I. is in the form of an atlas, and exhibits the actual crossings of each meridian and the length of voyage for each ship. The epoch chosen for classifying the passages is the month in which they cross the parallel of St. Helena, being about the middle of the voyage. Part II. contains a discussion of the results, with sailing directions based upon them. Of the two Appendices, the first is "On the Storms of the Southern Hemisphere;" the second consists of "Hydrographical Notices."

Herr von Freeden has brought out No. III. of the "Mittheilungen of the Norddeutsche Seewarte," which is a discussion of the tracks of the North German Lloyd's steamers from Bremen to New York—374 passages in all. Unfortunately, as the logs date from the period prior to the establishment of the Seewarte, they contain no barometrical or thermometrical observations, referring exclusively to wind and weather, distance made good every day, and amount of coals expended. The specially meteorological portion of the work consists of remarks on Ice, Storms, and Wind.

It appears that ice is principally met with between the meridians of 46° and 51°, and is rarer to the westward of that district than to the eastward.

The storms are analysed carefully, and it would be impossible for us to give even a satisfactory sketch of the discussion. The direction from which they most frequently blow is, on the whole, between W. and N.N.W. As regards the time of their occurrence, 50 per cent of the entire number are recorded during November, December, and January; 26 per cent during February, April, and October; 12 per cent during March and September; and the remainder are distributed over the four months from May to August. The examination of the storms with reference to longitude shows that they reach a maximum for 30° W., and maintain it to 45° W., their direction being north-westerly, *i.e.*, between W. and N.N.W. West of the meridian of 55° W., the majority of the storms are north-easterly. From this relation Herr von Freeden concludes that the storms have their origin not far from the Banks of Newfoundland, where the cold Arctic current meets the warm waters of the Gulf Stream, and that they are not West India hurricanes crossing the Atlantic from shore to shore.

The general winds of the North Atlantic for the ships' tracks are also discussed, and the results given in the form of a chart.

The author concludes by expressing a conviction, in which we thoroughly agree with him, that the cause of meteorology will be more benefitted by the careful investigation of observations taken over limited areas of the earth's surface than by all the theoretical disquisitions on general atmospherical circulation which are constantly emanating from the "depths of the self-consciousness" of German Professors, and are invariably so attractive to the non-scientific public.

We have received a number of Annual Reports from the Continental Meteorological Observatories. That from Saxony is for the year 1868. It contains complete monthly means, &c., from 25 stations, with five-day means of pressure and temperature. In addition there are a series of tables of earth temperature at various depths, and also of the depth of water in wells at Dresden, Leipzig, and Zwickau, and of the levels of the Elbe and Moldau, and the other rivers of Saxony. Professor Bruhses has also brought out the Report of his own Observatory at Leipzig for the year 1869. In Wurtemburg Dr. Schoder has printed a brief paper on the meteorological conditions of that kingdom, as determined by several years' observations, taken at 22 stations in connection with the Statistical Bureau. His Report for 1869 contains full tables for all the stations for the year, and a general notice of the special phenomena of the several months.

The fifth volume of the "Jahrbuch of the Central Austalt in Vienna," contains the usual tables of mean results, but instead of the daily deviations of the instrumental readings from their mean values, Dr. Jelinck has reprinted the daily telegraphic reports from 15 stations.

We are glad to learn that a New Meteorological Institute has been established in Hungary. Its head-quarters are at Pesth, under the directorship of Dr. G. Schenzl. It commenced work at the beginning of the present year.

Dr. A. von Dettingen has brought out the Third Report of the Observatory of Dorpat. It does not contain much that is new, excepting a method of traversing ordinary observations of wind as to direction and force. A dial is fitted to the indicator of the vane; and the different quadrants of the circle are divided so as to show the north (or south) and the east (or west) components at a glance. It is announced that the form of the Report will be altered in future, as Dorpat is to be the central observatory of one of the Departmental Meteorological Organisations of the empire, on the plan sketched out by Dr. Wild, (Quart. Journ. Science, vol. vii., p. 415).

Dr. Jelinck has published a short paper in the "Proceedings of the Vienna Academy," "On the Annual Distribution of Thunderstorms in the Austrian Empire." He only deals with the days on which electrical phenomena were recorded, and not with the individual storms. He finds that the storms are almost exclusively summer storms, excepting at the Adriatic stations, such as Trieste and Lesina, where, however, the winter storms form only a small percentage of the whole. This result shows the marked contrast which exists, as regards thunderstorms, between the conditions of Central and Southern Europe, and those of North Scotland, as stated by Mr. Buchan in his paper ("Journal of the Scottish Meteorological Society," vol. ii., p. 344).

Another paper, by Dr. Jelinck, in the same journal is on "The Annual Range of Temperature at Trieste, Klagenfurt, and Arvavakalja.

We have nine numbers of the "Journal of the Austrian Meteorological Society" to notice, but they do not contain many papers of much importance.

Dr. Hann gives us two more papers on the climate of South America, in continuation of that noticed in No. 29. The first is on the "Southern Stations of Chili, Puerto Moult, Valdiera, and Conception;" the second on Central Chili, Santiago, and Valparaiso.

We have already spoken of the contrast, as regards climate, between the eastern and western extremities of the Straits of Magellan; differences quite as remarkable as these exist between the meteorological conditions of Patagonia and those of Central Chili. On the southern part of the coast the glaciers, even in 46½° S., come down to the sea, and the snow-line between 41° and 43° is at the level of 6000 feet above the sea. Ten degrees further to the north the snow-line rises to the level of 14,500 or 15,000 feet. Southern Chili and Patagonia are among the wettest districts of the globe, while Central Chili is rainless for seven months in the year, and its soil is at times a desert, unless where irrigated. The Isotherm of 50° cuts the west coast of South America in about the same latitude (south), as the corresponding Isotherm cuts the East Coast of Asia and America in the Northern Hemisphere. The summer is on the whole about 10° or 15° cooler than that of corresponding places at the other side of the equator, and the climate is, therefore, pre-eminently insular. For Central Chili Dr. Hann gives us observations for Santiago and Valparaiso, and bases his remarks chiefly on the accounts furnished by Gillis in the Report of the United States Exploring Expedition. The climate is, on the whole, subtropical, but excessively dry, excepting during the rainy season. Thunderstorms are very rare and excite no less astonishment than earthquakes. The range of temperature is moderate, much more so than in corresponding latitudes in the Northern Hemisphere. The average temperature of Santiago is 13° lower than that of Beirat. The low summer temperature on the coast is due to the proximity of the cold water of the Humboldt current.

Another paper is on "The Climate of Mesopotamia," based principally on the observations of Dr. Schläfli and Lieut. Collingwood. This region is one of the hottest in the world, as although it is in latitude 35° N. the Isotherm of 95° passes over it in July. In summer, life in Bagdad is entirely regulated by the temperature, for from 10 a.m. to 5 p.m. the heat above ground is unendurable, and the whole population descends into cellars called "surdabs." On emerging from these in the evening the rooms and bazaars are still too hot to be habitable, having been exposed to the sun all day long; supper is accordingly taken on the flat roofs of the houses, where everyone sleeps, in the open air, without fear of injury, owing to the non-existence of dew.

Raulin's paper on "The Rainfall of Algeria," which appeared in the "Comptes Rendus," is also reproduced in abstract.

Dr. Hann gives a notice of the remarkable phenomenon of increase of temperature with the height, so well known in the Alps. He traces out the actual fact very carefully, but does not attempt to assign a cause for it.

Dr. Wojeikoff gives a most interesting note on "The Ice and the Water Level of the Volga, in Relation to the Clearing of the Country," based on the observations taken at Astrachan, for the years 1830-67. The following table shows the means for the four nearly equal divisions of that period:—

	Froze.	Thawed.	Flood began.	Highest level.	Flood ended.	Greatest depth of water.	Difference in days.			
	(*a*)	(*b*)	(*c*)	(*d*)	(*f*)	Eng. ins.	*a-b*	*b-c*	*b-d*	*c-f*
1830-37	Dec. 18	Mar. 22	Ap. 30	June 16	Aug. 17	104	94	39	86	109
1838-47	„ 14	„ 23	„ 28	„ 18	„ 31	90	99	36	87	125
1848-57	„ 22	„ 26	„ 29	„ 14	Sept. 12	114	95	33	79	136
1858-67	„ 10	„ 29	„ 22	„ 13	Oct. 31	118	108	24	76	192
Average	16	25	26	15	Sept. 16	106	99	30	78	143

The duration of the ice has somewhat increased, the river freezing earlier and thawing later, but these changes are immaterial. The alteration in the time, extent, and, above all, duration of the floods, is most striking. The fact that the flood comes down eight days earlier is attributable to the great clearing of the forests on the banks of the Volga itself, which has the effect of allowing the snow to thaw sooner. The highest flood is noticed when the northern tributaries yield their quota; and as in their basins no clearing of consequence has as yet taken place, their floods have not become much earlier in date, and the highest level is only reached at Astrachan three days earlier than was formerly the case. The duration of the floods, which in the last 10 years is nearly double what it was at first, is probably to be accounted for by the fact that clearing of woods promotes more rapid drainage, and so the floods in the rivers and the desiccation of the soil proceed *pari passu.* The continuance of the flood is therefore due, not to the melting of the snow, but to the rainfall, which was formerly retained on the land and is now discharged more rapidly.

In connection with these changes it is very interesting to learn that the level of the Caspian has risen very materially of late years, as is proved by the soundings in the port of Baku. M. Wojeikoff promises to return to the subject at a future date.

The journal also contains full descriptions of the auroras of October 24 and 25, from several European Stations.

MINERALOGY.

Although it is only within the last few years that attention has been forcibly directed to the importance of studying the minute structure of minerals and rocks, sufficient work has nevertheless been already accomplished to encourage the belief that the microscope will eventually become almost as important an aid to the mineralogist and petrographer as it has long been to their biological brethren. Among the most successful workers in the department of micro-minerology may be mentioned Professor Zirkel, of Kiel, whose recently published researches* it becomes our duty to notice.

Whilst it is well known that crystals of quartz frequently contain many microscopic cavities charged with certain fluids, such enclosures are but rarely found in crystals of felspar. It is, therefore, not without interest that Zirkel announces the discovery of a very large number of fluid cavities in the felspar of a coarsely crystalline rock from the Isle of Mull, consisting of plagioclase, diallage, and olivine. It is found, too, that the crystals of labrador-felspar from the olivine-gabbro of the Isle of Skye are rich in similar fluid cavities. In many of the liquid-bearing hollows with which quartz is frequently charged, may be detected minute cubic crystals, usually clear and smooth faced, but sometimes having their planes striated in squares. Zirkel finds that many of these cubes consist of sodium-chloride, and believes that the fluid which encloses the crystals is a solution of the same salt—results which corroborate the conclusions advanced many years ago by Mr. H. C. Sorby.

Attention is directed by the same author to the wide distribution of microscopic crystals of apatite, and a long list is published of many eruptive rocks—diorites, melaphyres, and diabase—in which this mineral may be detected. The author also describes some curious crystals of leucite which enclose foreign particles disposed in a concentric radiate form distinct from the zonal arrangement which often obtains in this mineral. The microscopic structure of elæolite from the zircon-syenite of Laurovig and Frederiksvärn, in Norway, has likewise been studied by the author, who shows that the mineral from these localities is never a pure substance, but consists of a colourless mass in which are disseminated microscopic crystals of hornblende, to which the green colour of the elæolite may be referred. But perhaps the most interesting of Zirkel's results is the discovery of microscopic tridymite—Vom Rath's new species of silica—widely distributed through trachitic rocks. In the rock of San Cristobat, near Pachuca, in Mexico, the mineral occurs in colourless six-sided plates, having a length and breadth rarely exceeding 0·02 m.m., the outlines being somewhat rounded, and the crystals grouped together in a way which seems to be characteristic of this species. Tridymite is also found in crystals, both microscopic and macroscopic, in the sanidine-trachyte of the Drachenfels and Perlenhardt in the Siebengebirge on the Rhine.

* Leonhard und Bronn's Jahrbuch für Mineralogie, U.S.W, Heft 7, 1870, p. 801.

Zirkel shows that the small rounded granules of serpentine so widely disseminated through certain crystalline limestones have in many cases resulted from the alteration of olivine, and it seems likely that his researches in this direction may eventually shed some light on the origin of the so-called eozoonal structure exhibited by many serpentinous limestones.

We are glad to learn that one of our English chemists—Mr. John Arthur Phillips—is busying himself with the chemical and microscopic study of some of our British rocks and minerals.* Many of the old ore-bearing rocks of Cornwall have received Mr. Phillips's attention; but so far as they are rock-masses and not minerals, any notice of them scarcely falls within the scope of this chronicle. One of the subjects of our author's study is, however, a felspathic mineral which is quarried at the glass mine, near Roche, and exported to the potteries. It is a yellowish white crystalline mineral, enclosed in a schorlaceous granite, and traversed by veins of milky quartz. On analysis it was found to be an orthoclase containing 10·37 per cent of potash, and 2·4 of soda, with about 1·6 of lime; whilst its monoclinic form was determined by Professor Miller's crystallographic measurements. Analyses are also published of the beautiful serpentine of the Lizard—a substance which may be regarded with equal propriety as a mineral or as a rock. The specimen analysed by Phillips presented a dark green colour, thickly spotted with red, and exhibited under the microscope a crypto-crystalline base, with spots of oxide of iron, accompanied by indistinct crystals of a green or yellowish brown colour, which it is suggested may be pseudomorphs after pyroxene.

An interesting essay on the relation subsisting between chemistry and mineralogy has been published by Professor Rammelsberg.† At once a distinguished chemist and an excellent crystallographer himself, he maintains that every mineralogist should be equally familiar with both these sciences, and points out the mischief which must necessarily result from any attempt at divorcing two branches of science so closely cognate as chemistry and mineralogy proper. Indeed, our author is bold enough to repeat the question which had previously been mooted, "Might it be allowed to call chemistry and mineralogy one science?" Believing that it is the duty of all who are interested in mineralogy to keep pace with the progress of modern chemistry, he denounces the persistence with which many mineralogists cling to the old-fashioned atomic weights and rational formulæ. In these respects Rammelsberg's views are directly opposed to those of Von Kobell, or at least to those which he held a year or two ago.‡ We believe, however, that anyone who follows up mineralogical literature cannot fail to observe that the conservatism which has long been imputed to the mineralogist is gradually yielding, and that at any rate most of the younger men cannot be accused of lagging behind their age.

At the request of Professor Weisbach of Freiberg, Herr Frenzel has analysed the two minerals which were recognised as distinct many years ago by Breithaupt under the names of *Plumbostib* and *Embrithite.*‖ These new analyses show that the two minerals in question have identically the same chemical composition, both being represented by the formula $10PbS + 3SbS_3$. It is probable that they should be consolidated into one species; and if this be done, our author suggests that of the two names in vogue "Plumbostib" should fall to the ground.

O'Reileyite is the name which Mr. D. Waldie proposes§ for a new Burmese mineral obtained from the late Mr. O'Reiley, of Martaban. It contains arsenic, iron, and copper, approximately in the proportions of two equivalents of As, six of Fe, and one of Cu.

Mr. J. S. Adam has analysed some specimens of the rare mineral *Gahnite*, or zinc-spinel, found by Professor Brush, at Mine Hill, Franklin Furnace, New

* Philosophical Magazine, Feb., 1871, p. 87.
† Chemical News, Feb. 10, 1871, p. 64.
‡ See Quart. Journ. Science, July, 1868, p. 416.
‖ Journ. für prakt. Chemie, No. 18, 1870, p. 360.
§ Chemical News, Jan. 6, 1871, p. 4.

Jersey.* In affecting a cubic instead of an octahedral form the specimens are crystallographically unique, whilst they are notable chemically for the large proportion of zinc which they contain—40 per cent of zinc oxide, and 50 of alumina being the main constituents.

An analysis of the Stewart-county meteorite has been published by Mr. J. Lawrence Smith.† The stone fell at about 11.30 a.m., on October 6th, 1869, in Stewart Co., Georgia, U.S.; and the details of observations relating to its fall have been collected by Professor Willet. A single stone has been found weighing 12½ ozs., and covered externally with a dull black coating. It presents an irregular conical shape, has a specific gravity of 3·65, and contains 7 per cent of nickeliferous iron, 6·1 of magnetic pyrites, and 86·9 of bronzite, olivine, albite or oligoclase, and chromic iron.

It has been shown by Professor Church that many of the hyacinth-coloured gems usually known to collectors and jewellers as *jacinths* or *hyacinths* are nothing more than garnets of the variety termed *essonite.* Indeed, the true red zircon, or hyacinth proper, is much rarer than has generally been imagined, and hence it is interesting to learn from Professor Church‡ that certain rolled pebbles lately brought to this country from Mudgee, in New South Wales, are veritable hyacinths, and agree in their pyrognostic characters with the well-known zircons of Expailly, in France.

About three years ago attention was directed to the occurrence of gold on the Cudgegong River, in New South Wales, and a great rush was accordingly made to a locality known as the Two-mile Flat. It was not long, however, before diamonds were detected by the gold-diggers; and although the discovery attracted at first but little notice, workings in quest of the precious gem were eventually set on foot by the Australian Diamond Mining Company. A visit to the washings on the Cudgegong, near Mudgee, has enabled Mr. Norman Taylor and Professor A. M. Thomson, of Sydney, to prepare some interesting notes on the occurrence of the diamond in this locality.

The diamonds are distributed somewhat sparingly and irregularly through certain outlying patches of an ancient river-deposit, similar to what is known in Victoria as the older Pliocene drifts.

They are for the most part transparent and colourless, but many present a pale straw tint; whilst some have been found of green, brown, and even black colours. One opaque black diamond has been recorded. The average size of the stones is small, and the largest hitherto discovered weighs only 5⅝ carats. Many of the diamonds are well crystallised, and it is notable that their faces and edges have suffered little or no abrasion, whilst the accompanying minerals have been much water-worn. The most characteristic of these minerals are a vesicular black pleonaste, or spinel; brookite, or oxide of titanium, mostly in flat reddish transparent plates; and a bluish-white opaque corundum in six-sided slightly barrel-shaped prisms, with flat terminal planes. In addition, however, to these minerals, which are especially characteristic of the diamond-bearing deposits of the Cudgegong, there are found also topazes, generally white, in water-worn fragments and sometimes in crystals; quartz in ordinary double-hexagonal pyramids, and in pebbles including varieties of cornelian, jasper, and agate; sapphire—blue, green, yellow, or parti-coloured; ruby occurring but sparingly, and in small flat grains; Barklyite, or an opaque magenta-coloured corundum; zircons in brown, red, and colourless fragments, and also as a sand; tourmaline in rolled black prisms; black magnetic and titaniferous iron-sand; garnets; wood-tin; and native gold. The authors enhance the value of their paper by publishing analyses of the pleonaste and of two of the varieties of corundum which occur in association with the Cudgegong diamonds.

Those who are interested in the diamonds of South Africa, and the geological conditions under which they are found, may consult with advantage an

* Silliman's American Journal, Jan. 1871, p. 28.
† *Ibid*, Nov., 1870, p. 339.
‡ Chemical News, Feb. 17, 1871, p. 78.

excellent paper on this subject recently published by Professor T. Rupert Jones.*

MINING.

A new self-extinguishing safety-lamp has been devised by Mr. W. Simpson, of Battersea. It is constructed for burning paraffin oil, so that the collier may be furnished with a more brilliant light than usual, and therefore be less disposed to remove the wire cage. Any attempt to do this, however, will immediately bring into play the automatic extinguisher. This consists simply of two small metal plates, which are caused to fall upon the wick and wick-holder by means of a spring that is released on unscrewing the cage from the body of the lamp.

Attention has also been lately directed to the magnetic lock applied to the safety-lamp some short time ago by Mr. S. P. Bidder, jun. In this lamp the bolt of the lock can only be withdrawn by the action of a powerful magnet, and a strong electro-magnet is, therefore, permanently fixed in the lamp cabin. As an instrument of the requisite strength could not be carried about, it appears that the illegitimate opening of the lamp is placed beyond the power of the working collier.

A simple form of safety apparatus for preventing accidents in shafts from breakage of the ropes or from over-winding, has been invented by Messrs. Turner, Grey, and Brydon, of Barrow-in-Furness. As in many other forms of safety machinery, the cage is sustained in its descent by the action of chisel-pointed pawls which are caused to clutch the wooden guides. These catches are connected by means of levers with a bar which passes through a vertical cylinder fitted with india-rubber springs, and is attached at its other end to the rope. In the ordinary action of the cage the springs are retained in a state of contraction, and the clutches glide freely up and down quite clear of the guide-rods; but on rupture of the rope the springs expand and the clutches are brought into play. In favour of this apparatus, it may be said that the safety arrangement comes into action every time the skip completes its journey up or down the shaft; and being, therefore, in constant use, there is no fear of its being out of order at the fatal moment when an accident occurs.

To obviate the inconvenience of having the apparatus which develops the motive power connected directly with the cutting-tool, Mr. George Simpson has invented coal-cutting machinery by which the moving power may be long retained in one position, whilst the tool alone moves with the advance of the work. Different forms of cutter for excavating the fuel are employed; but it would be difficult to describe the mechanical details without the aid of diagrams. They may all, however, be brought into relation with the apparatus for developing motive force—which may be steam, compressed air, or manual labour—by means of pulleys and belts, or other modes of transmitting motion.

METALLURGY.

It is matter of familiar observation that certain metals—especially iron and steel—are more readily broken in cold than in warm weather. The inconvenience arising from this apparent increase in brittleness is especially felt by the railway engineer, who well knows that more accidents occur from fracture of rails, tyres, and axles in winter than in summer, or in a severe climate than in a mild one. But while none will probably deny this fact, opinion is strangely divided as to its explanation. On the one hand it is affirmed that the tenacity of the metal is actually lowered by a reduction of temperature; and this seems to be the common opinion entertained by practical men, who constantly speak of iron becoming brittle when "the frost is in it." On the other hand, there are not wanting many eminent men of science who maintain that the strength of the metal is not reduced by lowering the temperature; it is true, they admit, that a rail, for example, is broken more readily during frost, but this is not because the metal is really more brittle, but simply because the road on which

* Geological Magazine, Feb., 1871.

the rail rests becomes rigidly frozen, so that the permanent way loses its elasticity, and becomes subject to a series of sharp concussions every time the rolling load passes over it. This subject is one not only of paramount interest to the metallurgist and engineer, but also deeply concerns the public at large. It is not, therefore, surprising that the severity of the past winter, and the consequent accidents, should have led to the discussion of the question anew. A batch of papers has indeed been presented to the Manchester Literary and Scientific Society, an abstract of which it is our duty to lay before our readers.

Mr. W. Brockbank contributed some "Notes on the Effect of Cold upon the Strength of Iron." Some of these experiments relating to the resistance of cast-iron to a transverse strain were performed at the beginning of the year at the works of Messrs. Jackson and Co., of Salford. Good castings were obtained by the judicious admixture of several suitable forms of pig-iron, and the experiments were thrice repeated in order to obtain fair results. These results show that bars of cast-iron suffered a considerable diminution of strength and elasticity when the temperature was reduced below the freezing point.

A paper "On the Properties of Iron and Steel as Applied to the Rolling Stock of Railways," by Sir W. Fairbairn, was read at the same meeting, and took an opposite view of the subject. The author believes that temperature has little or nothing to do with the strength of the metal. He finds, for instance, that the tensile strength of wrought-iron plates is as great at zero as at a low red heat; indeed, the breaking-weight was slightly greater at the lower temperature. At a red heat, however, one-half of the strength was lost.

Some notes "On the Alleged Action of Cold in Rendering Iron and Steel Brittle," were communicated by Dr. Joule. His experiments were made upon a dozen darning needles, one-half of this number being tested in a freezing mixture, and the other half at the ordinary temperature. The test was applied by attaching a wire to the middle of the needle and pulling it by a spring weighing machine. The breaking-strain was found to be rather greater in the cold than in the warm needles, and Dr. Joule believes that accidents arise mainly from badly-selected metal, and not from the reduction of temperature. Some experiments were also performed on cast-iron garden-nails cooled in a refrigerating mixture.

Another paper on the same subject, by Mr. Peter Spence, details some experiments on certain cast-iron bars which were subjected to transverse strain when cooled artificially. These experiments tend to show that reduction of temperature actually increases, *cœteris paribus*, the strength of cast-iron.

Reviewing all these experiments from a metallurgical stand-point, it is to be regretted that no chemical examination of the metal accompanied the mechanical tests. Everyone who knows the effect exerted on the physical properties of iron and steel by the presence of certain elements, such as phosphorus and sulphur, will feel that no general conclusion should be deduced from such experiments unless the chemical composition of the metal under trial has been determined.

We cordially welcome the appearance of a new technological periodical in the shape of "The Journal of the Iron and Steel Institute." The first number contains the proceedings of the Merthyr Tydvil meeting, held last September, under the presidency of the Duke of Devonshire.* His Grace opens the "Journal" by an introduction, in which he explains the object and scope of the periodical. It will, in short, contain a complete record of the progress made from time to time in the manufacture of iron and steel. In addition to the "Proceedings of the Institute," the number before us contains, among other original papers, the first part of an elaborate essay on the "Chemical Phenomena of Iron Smelting," by Mr. I. Lowthian Bell—an essay which gives the results of an experimental and practical examination of the circumstances which determine the capacity of the blast furnace, the temperature of the air, and the proper condition of the materials to be operated upon. A forerunner of this paper appeared in the shape of a lecture delivered before the Chemical Society some two years ago. One of the most valuable features of

* See Quart. Journ. Science, Oct., 1870, p. 561.

the new journal is the "Quarterly Report on the Iron and Steel Industries of Foreign Countries," prepared by the Foreign Secretary, Mr. David Forbes, F.R.S. In addition to Mr. Forbes's able report, some notes on the British iron and steel trades are contributed by the general secretary, Mr. J. Jones.

Mr. George Fowler, a mining engineer of Batsford, purposes to effect an improvement in the smelting of iron ores by reversing the conditions which obtain in the blast furnace. It need hardly be said that in the ordinary process the air which is forced in through the tuyeres near the bottom of the furnace forms carbonic acid (carbon dioxide) by the union of its oxygen with the carbon of the fuel; but that this gas in its ascent through the incandescent mass rapidly becomes reduced to the condition of carbonic oxide (carbon monoxide), which then deoxidises the ore, and is, in fact, the true reducing agent in the furnace. Inverting this action, Mr. Fowler injects a current of carbonic oxide at the top of the furnace, which, in its descent, permeates the ore and effects its reduction. He generates the carbonic oxide in a separate furnace, by the passage of a limited amount of air over ignited fuel, and then forces this heated gas into the furnace near the top, whence it descends through the mass of iron-ore, which it deoxidises, whilst it becomes itself partially converted into carbonic anhydride. That portion of the carbonic oxide which remains unaltered suffers combustion by contact with the blast from the tuyeres, and the heat thus generated suffices to fuse the metal and slag, which, therefore, fall into the hearth in a molten state. The heated carbonic acid and nitrogen are then passed through a Siemens's regenerator, where they are made to give up their heat before finally escaping.

The analysis of a genuine specimen of Wootz steel—the celebrated steel of native Indian make—by Professor Rammelsberg, has shown that it contains 0·867 per cent of carbon, 0·136 of silicon, 0·009 of phosphorus, and 0·002 of sulphur, in addition to iron. It may be remembered that in Faraday's analysis of wootz, a small quantity of aluminium was detected, and it is to the presence of this element that the excellent quality of the steel has often been attributed. Rammelsberg, however, has failed to find aluminium, thus corroborating the results obtained by Karsten and Henry.

A new mode of treating copper-pyrites, so as to form a rich matt previously to smelting, has been devised by M. C. M. Tessie du Motay. After fusion in a cupola, the pyrites is run into a peculiar roasting apparatus, which consists of two chambers or vessels connected below by means of a wide tubular channel. Air or oxygen is injected through a number of orifices into the fused sulphides, by means of which the roasting is effected; whilst by varying the pressure in the two chambers the material is forced alternately from one to the other, and is thus subjected to a mechanical stirring, which greatly facilitates the reaction. By repeatedly fusing and re-roasting the product, if needful, with the addition of silica, a rich matt is finally obtained, and this is then readily reduced and refined.

MILITARY, CIVIL, AND MECHANICAL ENGINEERING.

The defensive works carried out for the protection of Paris occupied by no means an unimportant part of our notes on engineering which appeared in the last number of this journal. The capitulation of that city in January last prevented any opportunity of fairly testing the success of such works, but the lessons to be learned from this war between France and Germany have been the means of causing other nations—and ourselves amongst the rest—to direct their attentions to the attainment of weapons of larger calibre, superior efficiency, and in far greater numbers than had heretofore been considered necessary. We purpose, therefore, to give a brief account of some of the principal arms now in course of adoption.

Rifles.—From a description by a military correspondent of "The Times," on the subject of French and German small arms, it appears that the chassepot is of a longer range, inflicts a more dangerous wound, and has proved a more effective weapon in every respect than the Prussian needle gun. The new

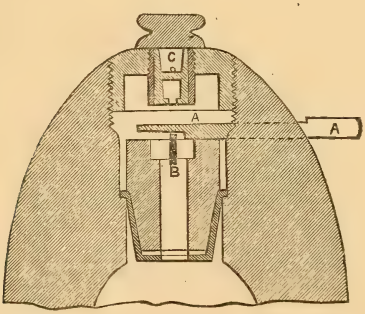
Bavarian rifle, the Werder, is better than either the chassepot or the needle gun; and its action as a breech loader is thought to be quicker even than that of the Martini rifle. It is a small bore, and the barrel has a sharply twisted rifling. With regard to our own army, the special committee appointed some time back to report upon the subject of small arms, has recommended the introduction of the Martini-Henry rifle throughout the service. In 1869, 200 of these rifles were distributed to the army for trial, with a view to their being subjected to exhaustive tests as to their suitability for hard usage, and the results appear to have fully borne out the recommendations of the Committee.

Artillery.—The principal French Artillery consists of breech-loading guns of 16, 19, and 24 centimetres. These guns are of cast-iron, strengthened by hoops, the trunnion ring being one of them. The vent is bushed with steel lined with copper. The rear end of the chase is closed by a screwed breech piece and gas ring. The dimensions, weight, and charges of projectiles for these guns are as follows:—

Projectiles for guns of	16 c.m.	19 c.m.	24 c.m.
Mean calibre (millimetres)	162·3	191·5	237·00
Weight of elongated shell (loaded)	31·5 k.	53·0 k.	100·00 k.
Weight of solid shot (steel or cast-iron) ..	45·0 k.	75·0 k.	144·00 k.
Charge for firing elongated shell	5·0 k.	8·0 k.	16·00 k.
Charge for firing solid shot	7·5 k.	12·5 k.	20·24 k.

Some of the shells fired by the Prussians into Paris during the bombardment measured 223 millimetres, or very little under nine inches in diameter at the base; were about twenty-two inches long, and weighed 88 lbs. Their percussion fuses failed to explode upon falling on soft ground; but they were causes of serious accident in more than one instance when carelessly handled, and the Government accordingly issued instructions regarding their treatment, in connection with which the following sketch was given of the arrangement of the percussion fuse, a copy of which appeared in "The Engineer" of 13th January last: "The arrangement will be seen almost at a glance, but it must be mentioned that the hole at the bottom of the fuze is covered with a piece of muslin kept in its place by a flat ring or washer of brass. The pin or plug, A, prevents

FIG. 22.



accidental discharge by keeping the discharging point, B, from touching the fulminant in the button, C. This pin disappears on the firing of the gun."

With regard to English guns, some very interesting particulars are given in a pamphlet reprinted from the Proceedings of the Royal Artillery Institution, and entitled "English Guns and Foreign Critics," by Captain Vivian Dering Majendie, R.A. The future armament of our field artillery has also recently formed the subject of a highly interesting lecture delivered at the same institution by Lieutenant C. Jones, R.A., Instructor Royal Gun Factories, Royal

Arsenal, from which we have extracted the following particulars. It appears that the controversy of muzzle *versus* breech-loading guns has been practically determined in favour of the former. Bronze has proved too soft, and steel by itself too brittle, being liable to burst at any moment without warning, whilst wrought-iron alone does not possess the necessary hardness for the interior of the barrel, but a steel barrel strengthened with wrought-iron coils furnishes the very best construction yet discovered. Experience has shown that every campaign from the Peninsula War down to the Crimea has ended in a heavier projectile being adopted than was used at its commencement. The war on the Continent has proved this to have been true at the present day; and accordingly a new gun has recently been adopted for our field artillery. It is a 16-pounder shell gun, having a bore of 3·6 in., and a powder charge of 3 lbs.; it is built upon the Fraser principle of steel and iron, being rifled with three grooves, and its weight is 13 cwts. At the same time that a new pattern of field gun is being introduced, a new 35 ton gun for the navy has been manufactured and proved with 130 lbs. of powder. Twelve more of the same calibre have been ordered to be at once put in hand.

Some months ago experiments were conducted with different patterns of mitrailleurs against field guns with a view to ascertain their efficiency, and as to which was the best adapted for introduction into the British army. The results of these experiments were that the Gatling gun fired 492 lbs. of ammunition and scored 2803 hits; the Fosberry mitrailleur with 472 lbs. scored 1708 hits; the 12-pounder gun with 1232·5 lbs. scored 2286 hits; and the 9-pounder gun with 1013 lbs. scored 2207 hits. The whole series of trials form the subject of an elaborate report which has been made by the committee to the War Office. Twelve more of the Gatling guns have since been ordered to be made and issued to the troops for trial, in order to confirm the experiments which have already been carried out at Shoeburyness.

Torpedoes.—Our military authorities have decided upon forthwith preparing a large number of torpedoes, and Messrs. Spencelayh and Archer, iron-founders and engineers, of Chatham, have received orders from the War Office to prepare the cases for nearly 1500 of such weapons. On 7th December last Mr. Robert Weir, formerly of the United States Navy, read a paper before the New York Society of Practical Engineering on "Submarine Torpedoes." After giving some notice of what has already been accomplished in the way of submarine warfare, Mr. Weir proceeded to explain his device, which is in reality a submarine rocket, a slow burning composition being used as the propelling power; its whole construction and action is plain and simple, being made up of but two parts, the torpedo or exploding shell, and the rocket or propelling shaft. It is proposed to discharge them from guns or guides of simple construction secured in the vessel's sides or bows, from 8 to 10 feet, or even more, below the water line. They are made as nearly as possible of the same specific gravity as water, and are discharged by a friction primer. The best torpedo yet introduced into this country is one designed by Captain Harvey, R.N. This torpedo is proposed to be towed against an enemy's vessel, the torpedo ship, from which its movements are controlled, being a small quick-speed craft, from whose line of progression the torpedo has a divergence of 45°, which is due to the vertical plane of the torpedo being thrown at that angle by the manner in which it is slung. The casing of the torpedo is made of stout timber strengthened with iron straps at the ends and sides. The charge is, of course, dependent upon the size of the casing, and it is fitted with top and side firing levers, either of which, when pressed, acts upon another lever which presses down the exploding bolt, and thus fires the charge. It can also be adapted for firing by electricity, a special circuit closing apparatus having been designed for that purpose by Captain Harvey.

A committee has recently been appointed in this country with Captain Beaumont, R.E., M.P., as president, and Lieutenant Grove, R.E., and Mr. Abel, F.R.S., as members, to carry out experiments on the utilisation of balloons for reconnoitring purposes.

Tunnels.—On the 24th of last December the boring of the Mont Cenis

Tunnel was so far completed that the miners from the French and Italian sides of the mountain met in their respective underground passages. The advanced heading has been now effectually completed, and it remains but to complete it to the full necessary section in order to prepare it for the laying of the railway. A provision in the original contract established the obligation of the contractor to complete the contract by September next; but now, should the premium demanded by the contractor be granted, Signor Galery will undertake to complete the work by March, which would permit the tunnel line to be opened to Modena by the end of April. The tunnel is 12,200 metres, or about 7 miles 1020 yards in length. Boring operations were systematically commenced at both ends on 25th January, 1863, and the work will thus have occupied about seven years and two months. The cost of the tunnel was originally estimated at £2,600,000, but it is now believed that the outlay will not exceed £2,400,000.

Ship Canals.—The Portage Lake and Lake Superior Ship Canal has been cut through, and it is announced that it will be ready for the passage of the largest steamers on the northern lakes in the course of the present spring. This canal passes along the base of the promontory of Keweenaw, which is situated on the southern shore of Lake Superior, about midway between the eastern and western extremities, and extends in a north-easterly direction nearly one-third of the way across the lake. At the lower part of the promontory is a bayon called Portage Lake, which extends from Keweenaw Bay on the east nearly across to the west side. To complete the navigation, a canal two or three miles long had to be cut through a sand-ridge, and this part of the work has been accomplished at a heavy expense. It is probable, however, that the navigation of Portage Lake will have to be improved, and the channel deepened, before the largest lake steamers can pass through. The new route, it is stated, will shorten by 100 miles the length of the voyage between the Sault Ste. Marie Canal and the towns on the western coast of Lake Superior.

Railways.—The principal improvements of the present day with regard to permanent way, consist in the introduction of iron sleepers in the place of wooden ones. One great objection to cast-iron sleepers has always been their uncertainty to preserve the gauge. In order to obviate such defects, an iron permanent way has been designed by Mr. J. Cockburn-Muir, of Westminster. The sleepers in this system have been specially desined for the Vignoles rail, to which no altogether satisfactory application of a cast-iron support has hitherto been made. The rail is held without bolts or nuts, so that it is not weakened by punching. The form of the sleeper in plan is a parallelogram, to insure the largest amount of bearing surface, while the corners are rounded sufficiently to insure free flow of the metal in casting. The rail is held in position on the inner side by two lips cast on the sleeper, and on the outer side by a gripping-piece dropped in between the jaw on the sleeper and the foot of the rail, the gripping piece, or clip, being brought home on the rail by a taper-key of peculiar form, which is driven between the jaw and the back of the clip. The transverse bar is of T-iron, the dimensions of which are regulated according to the gauge and the weight of the engines to be used.

With the view of obviating railway accidents, Mr. William Naylor has recently introduced a continuous railway brake, which may be briefly described as an adaptation of the ordinary lever waggon brake to the purpose of working sets of brakes on a train of carriages. The levers which actuate the brake blocks are connected by other short levers to a spiral spring, which is kept in tension by means of a chain running under the entire train. When this tension is released, which can be done at pleasure, the spring presses the brake-blocks, through the action of the levers, against the wheels. Several arrangements have been designed by Mr. Naylor for tightening the chain, but want of space prevents further reference to the subject upon the present occasion.

A continuous railway brake, designed by Mr. E. D. Barker, of Weston-super-Mare, is, we understand, about to be tried upon the Great Eastern Railway. Mr. Barker employs hydraulic power to apply the brake-blocks to the wheels, and according to his plans each carriage has a main water-pipe running its entire length, and which is provided with branches leading to small hydraulic cylinders, a length of india-rubber tubing being used to connect each of these cylinders with the corresponding branch pipe. There is an hydraulic cylinder to each wheel, to which brake-blocks are applied, the ram with which the cylinder is fitted being connected to the brake-block nearest to it, while the cylinder itself is coupled, by a pair of rods, to the brake-block on the opposite side of the wheel. The cylinder is supported by the brake-hangers, and the water under pressure being admitted to the cylinder at the end furthest from the wheel, acts upon that end and upon the ram, thus forcing the brake-blocks into contact with the opposite sides of the wheels. This arrangement ensures uniformity of pressure on the blocks, and the thrust of one block being resisted by that of the other, no strain is thrown upon the axle.

GEOLOGY AND PALÆONTOLOGY.

Eozoön Canadense.—The genuine organic character of this oldest known fossil having recently been attacked, has brought a *resumé* of the grounds for so considering it from Dr. Carpenter and Professor Dawson, of Montreal. To the objection that it has hitherto only been found in metamorphic and structurally altered rocks, in which it would scarcely be possible for an organic structure to be preserved, Dr. Carpenter replies that the eozoönal structure is most characteristically displayed in those portions of the serpentine limestone of the Laurentian formation which have undergone the least metamorphic change. Principal Dawson also states that several other *Foraminiferal* forms allied to *Eozoön* which have recently been discovered in the Laurentian rocks of Canada, will shortly be described.

Duration of the Cretaceous Epoch.—Professor Wyville Thompson has recently propounded the theory that "we may be said to be still living in the cretaceous epoch," a view to which Dr. Carpenter has also lent his support. The arguments in favour of this theory rest, not only on the deposition over a large part of the North Atlantic sea-bed, at the present time, of a sediment closely corresponding with the chalk, and of the occurrence in it of a few types of life like the *Lingulæ* and *Terebratulidæ* belongingto the older formation; but by the pesistence of those which constitute the formation itself, viz., the *Globigerinæ*, the coccoliths, and the coccospheres; as also of numerous types of *Echinodermata* that were formerly considered essentially cretaceous, and of a great variety of those sponges (including *Zanthidia*) and *Foraminifera*, whose abundance in the white chalk is one of its most important features.

The exact part played by the Gulf-stream in the heating of the North Atlantic Ocean and the western shores of Europe above the normal temperature of their latitude, though belonging more strictly to Physical Geography than to Geology, has yet an important bearing on the above subject. Dr. Carpenter believes that the direct influence of the Gulf-stream terminates with the latitude of the Bay of Biscay, and considers it an open question, whether the super-heating of the surface-water observed on a hot midsummer day beyond the northern border of the Bay of Biscay is not as probably due to the direct influence of the sun as to the extension of the Gulf-stream to that locality. This current he does not believe to extend to the Channel between the North of Scotland and the Faroe Islands. Professor Wyville Thomson, on the other hand, adheres to the older belief of the direct influence of the Gulf-stream on the climate of north-western Europe; an opinion which is shared by the eminent geographer Dr. Petermann, who maintains that there is no longer a shadow of doubt as to the existence of a movement of warm water, be it called a drift or a stream, from the Tropics obliquely across the Atlantic Ocean towards the Arctic regions. Dr. Carpenter states that the temperature-soundings taken in the "Porcupine" expeditions of 1869 and 1870, conclusively show that a temperature as low as 36·5° F. prevails over the deeper parts of the North Atlantic sea-bed; in connection with which the statement becomes of great interest which was made by Mr. Gwyn Jeffreys, at the recent meeting of the British Association, that the species of mollusca dredged up from great depths in the Mediterranean in previous expeditions are identical with Arctic species. An admirable *resumé* of the present condition of the Gulf-stream controversy, by Mr. Keith Johnston, jun., will be found in the "Academy" for Nov. 15th.

Principal Dawson, of Montreal, in the Bakerian Lecture delivered before the Royal Society, in May, 1870, described, under the name of *Prototaxites Logani*, what he believed to be the oldest known tree. The remains of this organism are abundant in the Lower Devonian or "Erian" rocks of Canada, and were described by Principal Dawson as "a simplification of the coniferous structure in the cylindrical cells marked only with spiral threads. They sometimes attain to a diameter of five feet, and though of a structure so lax as to be comparable only with the youngest stems of ordinary conifers, these trees must have been durable, as they are furnished both with medullary rays and rings of annual growth." More recently, however, Mr. Carruthers has made a minute microscopical examination of the structure of specimens of this wood, and has discovered that they are really specimens of hugh *Algæ*, belonging to at least more than one genus. They are very gigantic when contrasted with the ordinary *Algæ* of our existing seas; but some approach to them in size is made in the huge and tree-like *Lessonias* which Dr. Hooker found in the Antarctic Seas, and which have stems about 20 feet high, and with a diameter so great that they have been collected by mariners in those regions for fuel, under the belief that they were drift-wood. From a paper published in the October number of the "American Naturalist," it would appear, however, that Principal Dawson still holds to the coniferous character of *Prototaxites*.

In the recently published 2nd volume of Schimper's "Traiteé de Palæontologie Végétale," that eminent palæontologist divides the order *Lycopodiaceæ* into two families, *Lycopodieæ* and *Lepidodendreæ*, the latter entirely extinct, the former abounding at the present time and inhabiting all latitudes from the equator to the Arctic regions. Of this family Professor Schimper recognises only seven fossil species, all congeneric with *Lycopodium* itself, and, strange to say, confined, with the exception of one doubtful species, to the coal-measures. Dr. Hooker, on the other hand, considers that, after making every allowance for the imperfection of the geological record, it appears impossible to admit that a group so well represented now-a-days should be absent from all intervening beds, including the most modern Tertiaries; and is, on the whole, disposed to doubt the fossils being *Lycopodiums* at all.

The "Report on the Exploration of Kent's Cavern," presented to the recent meeting of the British Association, showed that during the past year the committee had investigated the only portions of the eastern division of the cave which had remained unexplored, called the North and South Sally Ports, in the belief that they led to external openings. The diggings yielded a large number of bones, including several birds and a few fish, portions of antlers, and about 1400 fragmentary and perfect teeth, some of them still attached to the jaw-bones. The teeth belonged to the following animals:—Horse, hyæna, rhinoceros, bear, sheep, badger, fox, rabbit, elephant, deer, lion, ox, hare, and pig. Agglutinated lumps of wings and elytra of beetles were also found; and about twenty-one flint implements and flakes.

The Geological volume of the scientific results of Professor Agassiz's Thayer Expedition, by Professor Hartt, of the Cornell University, discusses the origin of the layer of clay, or loam, varying in thickness from a few feet to one hundred, and wrapping in its folds the hills and valleys of Brazil, over vast tracts of country, including the steep slopes and summits of some of the highest mountains. It has been observed as far north as the Amazon valley, covering alike the gneiss and the Tertiary formations. It is of a red colour, evidently formed of the materials of the adjacent and underlying rocks, and without the slightest sign of stratification. Professor Hartt always speaks of this formation as "drift," and agrees with Professor Agassiz that its peculiarities are such as unmistakably to indicate its glacial origin.

BOTANY AND VEGETABLE PHYSIOLOGY.

Transpiration of Aqueous Vapour by Leaves.—Professor McNab, of Cirencester College, has recently published the results of an important series of experiments on this subject, confirming in a remarkable manner the statement made some time since by MM. Prillieux and Duchartre, that plants absorb no moisture whatever in the state of vapour through their leaves. The following are the more important results obtained :—1. Quantity of water in the leaves. The mean of several experiments gave 63·4 per cent. 2. Quantity of water removable by calcium chloride or concentrated sulphuric acid. This was found to be from 5·08 to 6·09 per cent, while the sun caused about the same quantity —5·8 per cent—to be transpired. The remainder, from 56 to 57 per cent, was therefore determined to be fluid in relation to the cell-sap of the plant. 3. Rapidity of transpiration in sunlight, diffused light, and darkness. The results given are—in sunlight, 3·03 per cent in an hour; in diffused daylight, 0·59 per cent; in darkness, 0·45 per cent. 4. Amount of fluid transpired in a saturated and in a dry atmosphere in the sun and in diffused daylight. In sunshine the experiments gave 25·96 per cent in an hour in a saturated atmosphere; 20·52 per cent in a dry atmosphere; in the shade, none whatever in a saturated, 1·69 per cent in a dry atmosphere; thus again confirming the earlier observations of M. Dehérain, that in sunlight evaporation goes on equally in a saturated as in a dry atmosphere. 5. Quantity of water taken up by leaves when immersed in it. The mean of several experiments gave 4·37 per cent in $1\frac{1}{2}$ hours. 6. Quantity of aqueous vapour absorbed by leaves in a saturated atmosphere. None whatever. 7. Differences in the amount of fluid transpired by the upper and under side of leaves in the sun and in diffused daylight. Under both circumstances the amount was found to be much larger from the under than from the upper surface. 8. Rapidity of ascent of fluids in plants. From $4\frac{7}{12}$th inches in 10 minutes to $8\frac{7}{12}$th inches in 70 minutes. In all these experiments the plant operated on was the common cherry-laurel (*Prunus laurocerasus*), and the fluid used for testing the rapidity of the ascent was lithium citrate, the presence of a very small quantity of which can be readily detected by the spectroscope.

The phenomena of cross-fertilisation and self-fertilisation are still attracting much attention among botanists. Mr. A. W. Bennett details in the "Journal of Botany" for October, a series of observations on British plants. Two sets of facts have been especially observed, in particular by Darwin in this country, Hildebrand in Germany, and Delpino in Italy, to favour cross-fertilisation in hermaphrodite flowers; the phenomena of dimorphism and trimorphism, and the special arrangements which render it easier for the pollen to be brushed off by insects visiting the flower than to fall on its own stigma. But, besides these, another arrangement exists by which self-fertilisation is hindered, the single fact that the stamens and pistil belonging to the same flower are frequently not ripe, so to speak, at the same time. The terms *protandry* and *protogyny*, used by Hildebrand, are adopted by Mr. Bennett; but instead of non-dichogamy, he prefers *synacmy* for the simultaneous maturing of the two sets of organs. The most frequent arrangement appears to be that the pollen commences to be discharged from the anthers at a longer or shorter interval before the maturing of the stigma. In some cases there still remains a certain quantity of pollen in the anthers when the stigma is ready to receive it; in other cases the anthers have either withered up or entirely dropped off before fertilisation of the ovules can possibly take place. Synacmy is nearly as frequent as protandry; while protogyny is of far less common occurrence. The two extremes among the species observed may be stated to be *Campanula rotundifolia* and *Scrophularia aquatica*.

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I. THE DAWN OF LIGHT PRINTING.

IF the question were asked—Who discovered photography? —in very few instances, do we think, would the reply be a correct one. It would not, of course, be just, as we all know very well, to attribute to any one individual the invention of the art as practised at the present moment, for many minds and many hands had to be engaged on the subject before the methods we are to-day so well acquainted with were elaborated; and obviously, therefore, to no one of the illustrious names connected with the early history of photography can the undivided honour of being sole discoverer be assigned. Wedgwood and Davy were undoubtedly among the very first to show that prints could be obtained by the action of light, and to demonstrate how the solar camera might be employed to secure images in this manner. Niepce, Fox Talbot, and Daguerre followed these philosphers, and with the further aid of Sir John Herschel, Archer, and others, established photography as a practical art such as we now know it.

Of these names it has usually been the fashion to select those of Daguerre and Fox Talbot as being the most entitled to rank as the discoverers of photography; in many popular works, however, and especially those of foreign origin, Daguerre receives the lion's share of the honour for having in 1839 brought forward Daguerreotype, a process at once practical and efficient. Some have gone even further and have stated that Daguerreotype was the true germ of photography, and that before Daguerre made his important discovery no reliable results at all of light-printing had been obtained. Now, we do not for one instant wish to detract from the merits of Daguerreotype, which was, and indeed still is, one of the most beautiful and delicate methods we possess of reproducing images in the camera; but at the same time we must heartily protest against the same being considered the first step in photography. Fox Talbot's inventions, which date from the same period as that of Daguerre, although by no means so finished and complete as the beautiful method of his French rival, have been far more fruitful in their subsequent career, and form assuredly the true basis of the processes of to-day. Among other discoveries, that of producing numberless positives from one negative may be cited as of exceeding importance, a circumstance which shows, moreover, that Talbotype is quite dissimilar from Daguerreotype (where only one image can be produced for each exposure in the camera), and that it is in principle identically the same as our present mode of proceeding. But we shall endeavour to show, and we hope also to prove most undeniably, that Niepce, whose name is generally but incidentally mentioned as a participator in the discovery, ranks, if not above, certainly upon an equal footing with Talbot and Daguerre as an early traveller along the road of photographic science. Indeed, if we take photography to mean simply the reproduction of an object or landscape in nature, caught up in the camera upon a sensitive screen upon which it remains after the action of light has ceased, we cannot but regard Nicephore Niepce as the first accomplished photographer, an inventor, moreover, who had attained his end, not by any stroke of good fortune, but as the result of patient and untiring investigation. Unfortunately for him, as in the case of many other inventors and discoverers, past and to come, he never reaped in any way the glory and advantages of his labours; but, on the contrary, has been during the past few years in a fair way of losing almost entirely the bare honour to which he is so justly entitled. Any contribution to the true history of so great an art-science as photography must, we feel sure, be acceptable to all students of science, and for this reason we make no apology for introducing to the readers of the "Quarterly Journal of Science" a few facts tending to place Niepce in a more favourable position than he has hitherto held as one only among many rivals.

Of the invention of Daguerreotype we know comparatively little. Daguerre himself never gave any explicit account of his early researches, with the exception of the manner in which the development of the latent image first became known. This, our readers will remember, was an accidental circumstance, and happened in this wise. After many fruitless exertions to secure an image upon a silver plate treated with iodine, he was examining on one occasion some of his plates, taken from a cupboard which served him in the double capacity of laboratory and store-room, when to his unbounded amazement he found a half-developed picture upon one of them. His surprise was the greater from the fact that he was confident no such image existed upon the metallic surface at the time of its being experimented with, and that, therefore, to whatever cause the phenomenon was due, it must have occurred subsequent to the plate's entry into the closet. The mystery was hard to unravel, and Daguerre for some time was at a loss for a solution thereof. A second plate, prepared in the same manner and exposed to light in a camera, was put into the mysterious closet, and after a short sojourn therein the same startling result appeared. At last, hidden away in a corner of the cupboard, where it had long remained unheeded and forgotten, was found a bowl of mercury, the vapour of which condensed in a more or less concentrated form upon the isolated plate, and thus developed the latent image.

This was, perhaps, the first discovery of a photographic latent image;* but in no sense can it be considered the foundation of photography, and for two reasons; in the first place, as we shall presently see, Daguerre was anticipated by at least twenty years by Niepce in securing a photograph from nature in the camera; and, secondly, it is not Daguerreotype, but Talbotype, which is, as we have said, the basis of photography now-a-days. Daguerreotype was already a complete art when first made known by its discoverer, and could not, therefore, be improved upon to any marked extent, while Talbot's process, less elaborated possibly, was the stepping-stone to further progress. But it is not for the discussion of the relative merits of these two inventions that we have here taken up the pen, but rather to assert the claims of Nicephore Niepce, to whom we shall now turn without further ado.

M. Victor Fouque, a man well known in France, both in literature and science, has recently collected together and published the letters and papers of Niepce, and with these before us, a very clear estimate of that philosopher's doings may be formed. The letters are of an exceedingly interesting nature, and written for the most part to a brother between whom and the writer there existed a kind of partnership. Nicephore, it seems, had been brought up to the profession of a soldier; but after serving in the South of France and Sardinia for some years as sub-lieutenant, his health and failing eyesight obliged him to relinquish military life. His elder brother retiring about the same time from the French

* In all probability the honour of discovering the latent image belongs to the late Rev. J. B. Reade, F.R.S., who as early as 1837 developed by means of gallic acid some images of insects secured by means of the solar camera upon salted and silvered paper.

navy, the two brothers settled down together at the little town of St. Loup de Varennes, there to earn their bread in the best way they could. During the first years, being both of an ingenious nature, they turned their attention to mechanical propelling contrivances, and invented a description of velocipede, termed by them a pyreolophore, and this appeared to them so successful that Claude was despatched to England in 1807 for the purpose of selling the design. Left to himself, Nicephore continued some experiments he had been making with lithography, then a novelty brought forward by Senefelder, in Germany, and was seized with the idea of elaborating a plan by means of which a design reflected upon a stone would be permanently fixed upon the surface. This idea led to the institution of researches with the camera, so as to obtain the actual result given by the retina of the eye, and his exertions in this direction are detailed with much precision in a letter to his brother Claude, dated 12th of April, 1816, of which the following is a translation:—

"I have profited by the short time we remain here to construct a kind of artificial eye, which is simply a square box, measuring six inches each way, and furnished with a telescope-tube containing a lens. Without this apparatus it would be impossible for me to judge of the merits of my process."

Here we have a description of a camera with movable focus clearly described. On the 5th of May, Nicephore writes:

"You heard in my last letter that I had broken the lens of my camera, and that I had another one which I hoped to make use of. In this I was disappointed, for the focus of my other lens was shorter than the diameter of the box . . . . As I could not use my camera on account of the broken lens, I constructed another artificial eye with a ring-box, measuring from sixteen to eighteen lines square. This was fitted with a lens from my solar microscope, which, as you know, formerly belonged to our grandfather Barrault, one of the glasses possessing precisely the requisite focus. By means of this little apparatus I obtained images most clearly and sharply defined, reflected upon a field thirteen lines in diameter. I placed my apparatus where I operate, in front of the pigeon-house, and made an experiment in the usual manner; I obtained on the white paper the whole of that portion of the pigeon-house which is seen from the window, and a faint image of the casement, which was less brilliantly lighted than the exterior objects . . . . I am well aware that there are many difficulties in my path—above all, the problem of fixing the colours—but with steady perseverance and much patience I hope to succeed in working out my designs. That which you foretold has happened; the ground of the picture is black, and the images are white; that is, lighter than the ground. I believe a method of obtaining pictures by this means might be made use of, and I have seen engravings of a similar description. It may not be impossible to change the disposition of the colours, and I have made some experiments in this direction which I am anxious to verify."

From the above we see that Niepce had formed a very excellent idea as to how an image could be secured, and was truly in a fair way to solve the mystery of printing by light. At the same time he was aiming at far too much when he sought to secure the colours themselves, and not merely their impression upon some sensitive material. His progress in heliography was, however, extraordinarily rapid, for we find from a letter written on the 19th of May, 1816, that he had undoubtedly succeeded in securing an image of some kind in his tiny apparatus. He does not, however, seem to have been much struck with the wonderful nature of his results, for he alludes to them almost incidentally:—

"I hasten to reply to your letter of the 14th, which was received the day before yesterday, and with the contents of which we were much pleased. I write on half a sheet of paper only, as mass this morning, and a visit to be made this evening, leave me but little time; besides, I have to enclose two pictures taken by my process, and it is necessary, therefore, that I should not increase the weight of my letter too much. The smallest image was obtained in the ring-box, and the other from the medium-sized camera of which I spoke some time since. You will be able to judge of the effect better by placing yourself somewhat in the shade, and holding up the pictures placed upon an opaque body against the light. A picture of this description is, I believe, subject to alteration after a time, as although proof against the action of light, the reaction of the nitric acid in its composition will have the effect of destroying it; it is possible, also, that the prints may become damaged by shaking and jolting during their transport. This is, of course, merely an experiment, but if the materials were rather more sensitive (as I hope to obtain them), and, above all, if the order of the shadows were inverted, the illusion would, I think, be quite complete. The two pictures were made in my operating room, and the field is merely the diameter of the casement. I read in the Abbé Nollet* that in order to reproduce a larger number of distant objects, a lens possessing a very long focus must be used, and a second glass must be fitted into the tube carrying the lens. If you desire to preserve the impressions, although they are scarcely worth the trouble, you have merely to place them between two sheets of grey paper and put them into a book. My future investigations will be conducted with a three-fold object—

"1. To obtain a clearer definition of the reflected image.

"2. To transpose the tints or shadows.

"3. To fix the tints, an operation which is by no means an easy one."

It is not definitely stated what is the nature of the exciting chemical employed, but from the hint given there can scarcely be a doubt of its being nitrate of silver. In any case, however, we have here absolute proof that Niepce, working alone in a small out-of-the-way provincial town, far from all scientific resources, and ignorant, may be, altogether of the preliminary researches of Wedgwood and Davy, made several years previously, succeeded in catching up and securing the images reflected in the camera. Of course, as we know very well, such images were not permanent, and required to be fixed before their durability for any lengthened period could be ensured; but, after all, it must be remembered that even at the present day the imperishability of silver prints is still an unsolved problem. But Niepce's success was not yet complete; from the very first it is apparent that he entertained a remarkably clear notion of what was necessary to elaborate a process of this nature, and the precise and orderly manner in which he arranged his ideas and conducted his research, catching hold of his subject as it were at the right end, could not but fail in bringing him to sound and speedy conclusions. After having succeeded so far as to hold fast the shadow, the next task he set himself to accomplish could scarcely have been better chosen as a sure means of progressing on his way. In his next letter, dated but nine days afterwards, he tells how fortunate he has been in his later efforts; and, indeed, this commuication reads more like the words of an enthusiastic amateur photographer of the present day, rather

* L'Abbé I. Antoine Nollet, Member of the Académie des Sciences; born 1700, died 1770. He occupied himself chiefly with the study of electricity, and was distinguished as an accomplished lecturer and exponent of the physical sciences. His principal work was entitled "Leçons de Physique Experimentale," in six volumes, published in 1743.

than those of a pioneer in science of some half century ago. He writes to his brother Claude in England:—

"I send you herewith four new prints—two large and two small ones—which I have obtained of sharper definition by the adoption of a very simple process, which consists in contracting by means of a disc of cardboard the diameter of the lens. The pigeon-house is reversed on the pictures, the barn (or rather the roof of the barn) being on the left instead of the right. The white mass which you perceive to the right of the pigeon-house, and which appears somewhat confused, is the reflection upon the paper of the pear tree, which is some distance farther off; and the black spot near the summit is an opening between the branches of the tree. The shadow on the right indicates the roof of the bake-house, which appears somewhat lower than it ought to be, because the cameras were placed about five feet above the floor. Finally, those little white lines marked above the roof of the barn are the reflection upon the sensitive film of some trees in the orchard. The effect would be more striking if, as I told you, the lights and shadows could be inverted. I shall confine myself to remedying this defect previously to endeavouring to fix the colours."

Another letter, written on the 2nd of June, contains further proof of the successful nature of the photographic results, in these words:—"The colour of the pigeon-house near the yard is of a brownish tint, but above the doorway, and near the pig-stye, there is a white patch, which will be found distinctly reproduced on the specimens."

We see, therefore, that beyond all doubt Niepce in 1816 produced a real photographic landscape, as perfect in truth, from a scientific point of view, as any of the negatives taken at the present day. But his researches did not by any means end here; Niepce knew almost as well as we do now what were the principal defects of his process, and laboured diligently to overcome them. Our limited space precludes us, unfortunately, from making any further extracts from these highly interesting letters, which form so perfect a history of his experiments; but within a month of the last communication we find our hero hard at work to discover some chemical agent capable of being bleached, upon which the solar rays might be impressed, so as to obtain a picture in its natural lights. One after another were all the then known compounds, that he thought likely to suit his purpose, experimented with, but none were found sufficiently sensitive for manipulation in the camera. Many, also, were the ingenious devices attempted to promote the bleaching action of the light, supplies of carbonic acid, chlorine, and hydrogen, being at times introduced into the camera during the period of exposure, but all to no purpose, and Niepce thereupon wisely abandoned this hopeless problem, and addressed himself to the question of producing permanent photographs. In this he was more successful, albeit the research was a long and tedious one, for we find that in the years 1823 and 1824 he had obtained permanent pictures upon metal, glass, and paper surfaces, by employing as the sensitive agent bitumen of Judea, which is rendered by the action of light insoluble in certain essential oils that otherwise readily dissolve it. So permanent, indeed, are these early examples of light-printing, that there exist even at the present moment at the Museum at Chalons, as likewise at our own British Museum, several specimens of Niepceotype executed about this time.

We must pass rapidly over Niepce's further experiments to elaborate a photo-engraving process, the results of which still form the basis of several well-known photo-lithographic methods, as likewise his essays with silver plates and iodine, which seem to point so curiously to the subsequent invention of Daguerreotpye. Our readers probably remember the circumstance of Niepce's journey to London in 1827, to visit his dying brother at Kew (a photograph of Kew church taken about this time is, we believe, in the British Museum at the present moment), and how he afterwards remained in London in the hope of profiting by his invention; also the fact of his wishing to bring forward the results of his labours before the Royal Society, which body, however, refused to see his productions unless the whole history of the affair was divulged. After this period poor Niepce seems to have lost heart in his cherished undertaking, and returned the following year to France dissappointed and sadly broken down. It was on his way back through Paris that he first heard of Daguerre, then famous as a skilful painter, to whom he was introduced as one who had also worked in the same direction as himself. After some correspondence a deed of partnership was eventually drawn up between the two in December, 1829, from a perusal of which it would seem that Daguerre was by far the chief gainer in the contract; in this document, of which a *fac simile* has been published, Niepce describes in detail the whole of his experiments and results from first to last, while Daguerre discloses nothing whatever, but agrees simply to contribute his labours towards the further elaboration of the process. The position of the parties is, indeed, very clearly explained at the commencement thus:—

"Art. 1. A partnership will be established between MM. Niepce and Daguerre for the purpose of working out the said discovery invented by M. Niepce, and improved by M. Daguerre."

We all know how Niepce died a few years after this agreement was made, while the world was yet unacquainted with the works he had so patiently and modestly accomplished, and how Daguerre in 1839 communicated to the Paris Academy the wonderful process of Daguerreotype, a method which, while dissimilar and superior to that of Niepce, was nevertheless due in some measure to that philosopher's discoveries. All honour and fame to Daguerre for making one mighty stride of progress in the beautiful art of photography, but we should doubtless have admired him more if, when publishing his invention to the world, instead of ignoring Niepce's early aid, he had generously made some mention of the labours of his dead partner.

II. PNEUMATIC TRANSMISSION.

By Frederic Charles Danvers, A.I.C.E.

THE practical application of air to the transmission of carriages on land dates only from the commencement of the present century. The first idea, however, of transmitting power to a distance by means of pneumatic pressure appears to have originated with the celebrated Denys Papin, a Frenchman, who, in 1688, described an apparatus in which a partial vacuum produced in a long tube, by air pumps fixed at one end, caused the motion of pistons at the other end; but no record remains to prove that any steps were taken by Papin to carry his suggestions into effect so as to derive any useful practical advantage from them. The introduction of the locomotive engine naturally directed the attention of engineers and others to the subject of the provision of improved means of communication, and this desire was doubtless stimulated by the acknowledged defects of the locomotive at that time, and its reputed inapplicability for lines with gradients exceeding 1 in 100. The first person to introduce the atmospheric system of propulsion was a mechanical engineer named George Medhurst, who, in 1810, published a pamphlet on the subject entitled "A New Method of Conveying Letters and Goods with

Great Certainty and Rapidity by Air," in which may be recognised the first practical suggestions for the introduction of what is now known as the "Pneumatic System;" and it is not a little surprising to find that in this, and two subsequent pamphlets by the same author, are foreshadowed almost everything that has hitherto been discovered in connection with this subject—all subsequent inventions having reference merely to the detailed means for carrying that system into effect.

George Medhurst's first idea clearly was to employ the pneumatic system for the conveyance of small parcels only, but he subsequently suggested its application for the transport of goods of a more bulky nature. It is perhaps a pity that he did not confine his attention in the first instance to the development of his earliest ideas on the subject, and which has subsequently been proved to be the most practical method of applying his inventions, viz., for the transmission of letters and small parcels. The rage of the day being, however, for improved means of communication, it is not surprising that his own ambition and the popular clamour should have caused Medhurst to endeavour to apply his invention to a purpose for which it was ill-suited. We shall not now follow the progress of the gradual rise and fall of the atmospheric railway, from the time when John Vallance, in the year 1826, constructed a model tunnel in Devonshire Place, Brighton, 120 feet long, and nearly 8 feet in diameter, through which a carriage was propelled by means of air pumps worked by two steam engines, which was the first of its kind ever constructed, to the abandonment of the atmospheric principle upon the Paris and St. Germains line for the last mile and a-half of its length, which was taken up in the year 1860, after having been in successful operation for about 15 years. From this last-named circumstance it is clear that the atmospheric system is not wholly unsuited for railways under certain circumstances, the chief ground of its applicability being upon very steep inclines, such as were unsuited for locomotives. The inconvenience, however, of having different systems of propulsion upon the same railway has been the cause which has led to the abandonment of the atmospheric principle upon every railway where it has ever been tried; the general advantages, greater speed, and undoubted superiority of the locomotive gaining for it, in every case, the preference over the latter.

Thus ended all attempts to introduce atmospheric railways; but a few years before their final abandonment the adaptation of the principle for the transmission of small parcels was again revived, and it now seems likely to come into very general use, especially in connection with the Post Office and the Telegraphs. The want of some means of speedy communication between the offices of the Electric and International Telegraph Company in London, in addition to that afforded by their lines of wire, probably led to the invention of a pneumatic tube for that purpose by Mr. Latimer Clark, and the first tube was laid down by that company in 1855. This tube was of lead, and the carrier (in which messages were placed) nearly fitted the bore, and was covered with felt. It turned out to be a complete success, and a similar principle was adopted by the Prussian Telegraph Administration in Berlin, between the Telegraph Office and Exchange, in the year 1863; in 1866 it was also adopted in Paris in connection with the Electric Telegraph stations in that city. Later still the principle has been introduced into New York, and quite recently a line has been laid down by Messrs. Siemens, in connection with the General Post Office in London, for the conveyance of messages in original between Telegraph Street and Charing Cross and on to the House of Commons, instead of sending them by Telegraph.

There are two methods which have at different times been adopted for impelling carriers through pneumatic tubes, the one being by creating a vacuum in front of the carrier, which is then impelled forward by the atmospheric pressure with a force equal to the difference between the latter and the vacuum. The other is by creating a "plenum" behind the carrier, or, in other words, increasing the pressure behind the carrier beyond that of the atmosphere, the difference between these two forces in pounds or ounces per square inch representing the force expended in driving the carrier through the tube. Medhurst, in his first invention, adopted the latter principle, and the introduction of the vacuum for the purpose is ascribed to John Vallance, who proposed it in a pamphlet published by him in 1824. Experience has shown that the vacuum is the far preferable manner of working pneumatic tubes, and it is also more economical than working with a plenum.

The difference between the effects of compression and exhaustion would appear, so far as recorded experiments upon the subject show, to vary in the cases of tubes of different diameters; but as a general rule it has been observed that when a carrier is inserted into a tube it is driven forwards with a mean velocity corresponding to that with which the air at the higher pressure is introduced behind it, or that at the lower pressure is exhausted in front of it. In a paper read before the British Association at Liverpool last year by Mr. Robert Sabine, that gentleman has worked out a number of formulæ for calculating the work performed in pneumatic tubes, and the result of his investigations on this subject cannot fail to be of great value, as it is one upon which very little of scientific value has hitherto been published. "The problem of a successful pneumatic system," says Sabine, "is simply this: To make a given quantity of air expand from one pressure to another in such a way as to return a fair equivalent of the work expended in compressing it. It is obviously impossible to regain the full equivalent of the work, because the compression is attended with the liberation of heat, which is dissipated and practically lost to us. Therefore, in designing a pneumatic system, that which we have to do is first to contrive means of compressing the air as economically as possible; secondly, to get back as much as we can of the mechanical effect stored up in our already compressed air, irrespectively of the work which was employed in compressing it. The utmost theoretical work which a given quantity of air can be made to perform is evidently that of expanding from the higher to lower pressure; and the mechanical effect employed in propelling a carrier and air through a given tube is therefore equivalent to that due to the expansion of a tubeful of air from the higher to the lower pressure." The speed at which a carrier travels in a horizontal tube has been worked out by Sabine, and is expressed by the following equation :—

$$s = \sqrt{2g \frac{vf - Wl\mu}{W + \frac{w_1 + w_2}{2} v \left(1 + \zeta \frac{l}{d}\right)}} \text{ feet per second.}$$

But when going up or down an incline—

$$s = \sqrt{2g \frac{vf - Wl(\sin\alpha + \mu \cos\alpha)}{W + \frac{w_1 + w_2}{2} v \left(1 + \zeta \frac{l}{d}\right)}} \text{ feet per second.}$$

In these equations the volume of the tube in cubic feet is represented by v; l represents the length of the tube in feet, and d its diameter, also in feet; W is the weight of the carrier in pounds, and g the accelerated motion due to gravity; f represents the mechanical effect performed by one cubic foot of air; μ, the coefficient of friction of motion of the carrier in the tube; w_1 the weight in pounds of one cubic foot of air at the higher pressure; w_2 the weight of a cubic foot at the lower pressure, and α the angle made by the tube with the horizon, and which is + when the carrier ascends, but − when it descends. ζ is an empirical constant;

experiments to determine its value have been made by Girard, D'Aubuisson, Buff, Pecqueur, and others, who give a mean value for it of 0·02.

Dr. P. Brix, Professor at the Bau-Akademie, has published in the German "Telegraph Journal" particulars of experiments made by him upon velocity with a tube $2\frac{1}{2}$ inches in diameter, laid down some years ago, by Messrs. Siemens at Berlin, between the Exchange and Central Telegraph Station, the results of which were that when working with compression the tension of the air at either end of the tube was 19·31 lbs. and 14·75 lbs., and with exhaustion, 14·75 lbs. and 10·19 lbs., respectively; the mechanical effect produced by one cubic foot of air in each case was 512·17 lbs. in the former, and 520·44 lbs. in the latter; and the weight of the air at the two extreme tensions was, in compression, 0·1099 and 0·0753 foot-pounds, and in exhaustion 0·0752 and 0·0447 foot-pounds. For each case the frictional resistance of the carriers in the tube averaged 0·1 lb.; the length, 2920 feet for each half of the tube; its diameter, 0·193 feet; and its volume, 85·49 cubic feet. With these values, the formula worked out by Sabine gives the calculated speeds in these two experiments as follows:—With compression, 34·1 feet, and with exhaustion 43·2 feet per second, or that the carrier should have occupied in the transit from station to station, in the former case 86 seconds, and in the latter 68 seconds, differing from the observations made by Dr. Brix 9 seconds in the one case, and only 2 seconds in the other. This difference, Sabine thinks, may possibly be due to an error of observation of the pressure, or possibly to the fact that the constant ζ may not be the same for small welded iron tubes as for a large cast-iron tunnel.

Mr. Sabine has also made some experiments with the tube of the Pneumatic Company, between Euston Station and High Holborn, which was some years ago designed by, and carried out under the engineering superintendence of, Mr. Rammell and Mr. Latimer Clark. This tube is ◠-shaped, $4\frac{1}{2}$ feet broad and 4 feet high. The trains used were each made up of three trucks, and these were loaded with an average weight of 6 tons, making, with the carriages, a gross load of 9 tons. The average time occupied in running through the tube from Euston Station to Holborn was $7\frac{1}{2}$ minutes, with a partial vacuum of 5 ozs. per square inch, whilst the empty trucks were returned to Euston Station with a compression of 5 ozs. per square inch in $6\frac{1}{2}$ minutes. Assuming the temperature of the air to have been 20° C., and its mean pressure 14·75 lbs., it is calculated that in drawing the loads through to Holborn the air was exhausted to 14·44 lbs., and in sending back the empty carriers it was compressed to 15·06 lbs. per square inch. From these data the mechanical effect due to the expansion of one cubic foot of air in the two experiments has been deduced to have been 31·964 foot-pounds with exhaustion, and 31·945 foot-pounds with compression.

In employing the same amount of mechanical effect, and the air remaining of the same mean specific gravity, Sabine finds "that the mean speed of transmission varies inversely with the length, and inversely also with the square root of the diameter of the tube. Thus, with an equal mechanical effect expended upon it in each case, a very light piston would travel through a tube of one mile long with exactly twice the speed with which it would travel through a similar tube two miles long. And further, if we had two tubes, each a mile long, one having a diameter of 4 feet and the other a diameter of 1 foot, the air in the larger tube would only travel half as fast as that in the smaller one, assuming, of course, the total work performed during the transit to be in each case equal. The cause of this is simply that the greater portion of the mechanical effect which in the larger tube is used for moving the greater mass of air, is, in the smaller one, converted into speed. If the case arose, therefore, that a pneumatic transit had to be made with a stated expenditure of work, we should proceed economically by adopting a tube of small rather than one of large sectional area. With an equal utilised engine power in each case, the mean speeds of transit of air through two tubes are inversely as the cube roots of their diameters and lengths. For instance, with a utilised effect of 10-horse power, the velocity of transit in a tube eight miles long, being 20 feet per second, that attainable with the same power in a one mile length of the same tube would be 40 feet, and if we had two tubes of equal length—one eight times the diameter of the other—the speed attained in the larger tube would be only half that attained in the former. To obtain the same speed of transit of a very light piston in two tubes of equal length and different diameters, other things being equal, the utilised horse-power must be directly proportioned to the diameter, whilst to produce the same mean speed of transit of very light pistons in tubes of equal diameter but different lengths, other things being equal, the utilised horse-powers of engines may be taken as directly proportional to the lengths. Similarly, when the lengths and diameters are equal, but the mean specific gravity of air in the two operations are different, the mean speed of a very light piston being the same in its transits through the same tube, or through two tubes of equal dimensions, the utilised engine power is directly proportionate to the mean specific gravity of the air on the two sides of the piston. It follows from this, therefore, that in working by exhaustion less engine power is required, other things being equal, than in working through the same tube by means of compression. And it would also follow that in hot weather, and when the barometer is low, the working of a pneumatic tube should be less costly in engine power than in cold weather and when the barometer is high."

"With given utilised horse-power operating upon a given line, the velocity of a very light carrier would be reciprocally proportional to the cube root of the mean specific gravity of the air moving in it. Mr. Siemens has proposed to take advantage of this fact by the employment of hydrogen gas for propulsion in letter tubes instead of atmospheric air. The specific gravity of hydrogen is 0·07; that of air being 1. The speed attainable, therefore, by the substitution of this gas would be as—

$$1 \;:\; \frac{1}{\sqrt[3]{0{\cdot}07}}, \text{ or as } 1 \text{ to } 2\tfrac{1}{2}, \text{ nearly.}$$

This plan would be easily practicable with Messrs. Siemens's system of complete circuit tubes, in which the same air is pumped round without being changed. With any of the ordinary systems by which the tube is open at one end, of course only the atmospheric air could be used in practice."

A by no means unimportant matter in connection with the working of pneumatic tubes is the mechanical means employed for producing the vacuum or plenum, as the case may be. Several methods have been introduced with this object. The first system adopted by Vallance in his model at Brighton, in 1828, was to produce the vacuum by means of air pumps worked by two steam engines, and this was the plan afterwards most generally employed on the several experimental lines of pneumatic railway laid down in different parts of the United Kingdom and elsewhere. In the Prussian pneumatic dispatch tube both compression and expansion are employed. The tube itself consists of two tubes of welded iron, $2\frac{1}{2}$ inches in internal diameter, laid parallel to one another beneath the pavement. A transverse coupling connects them together at one end, whilst at the other end they terminate in two reservoirs, between which an air-pump exhausts the air from one and compresses it into the other, thus keeping up a continual circuit of current within the tubes.

The pneumatic system in use in Paris differs from the foregoing principally in the use of water power instead of steam-engine power for working the tubes, each station being supplied with an arrangement for compressing air. Until recently the transmission of carriers between stations was effected by means of compression alone, produced by the action of water upon a chamber full of atmospheric air. This water is obtained from the River Ourcq, and is employed in the following manner :—Three wrought-iron cylindrical vessels are erected at each station, one of which is large and the other two smaller, and of the same size as one another. The larger vessel is connected by means of a pipe with the water mains of the town, and an exhaust pipe leads from the same vessel into the sewers; each of these pipes is fitted with a valve to enable the communication to be opened and closed at pleasure. From the top of this vessel a pipe leads into the first of the two smaller reservoirs, and these again are connected together by a pipe fitted with a cock, whilst the second smaller reservoir is in communication with the pneumatic transmitter. In order to obtain a supply of compressed air, the valve is closed in the tube communicating with the sewers, and that leading to the water mains is opened, allowing the supply water to rush into the larger vessel, and to displace the air which previously filled it. This displaced air is compressed into the two smaller reservoirs until the water has risen nearly up to the top of the larger vessel. A cock, in the pipe communicating between that and the smaller reservoirs, is then closed to prevent the air returning. The water is then run out of the larger vessel into the sewers, and it again becomes filled with air, which is in turn compressed into the reservoirs as before, as soon as it is required.

The Pneumatic Dispatch Company in London employ a pair of horizontal engines to drive a fan 22 feet in diameter, by means of which the air can either be exhausted from, or forced into, the tubes at pleasure.

The latest form of pneumatic tube is that recently laid down by Mr. Siemens, from the Post Office in St. Martins-le-Grand towards Westminster, and which will probably be extended, in course of time, in other directions. Several entirely new features have been introduced in the construction of this tube, which we shall describe more fully at some future time in our chronicles of engineering, when the system has become more perfected than it is at present. One new feature which Mr. Siemens proposes to introduce is a novel kind of blower, which he has designed, to be worked direct by the steam from a boiler, without the intervention of machinery, and which will not only effect a considerable saving in the first cost of the requirements of each station, but it will also be much more economical in working and for maintenance.

The methods adopted at the different stations on any pneumatic line differ, of course, with the various methods employed for transmission, and it is in this that very great improvements have recently been introduced by Mr. Siemens. We have stated that in the Berlin pneumatic despatch system two tubes are placed parallel to each other, and connected together at one end by a transverse coupling, whilst at the other they terminate in two reservoirs, between which an air-pump exhausts the air from one and compresses it into the other, by which means a continual circuit is maintained, and provision is made for the dispatch of messages in either direction. The tube for transmitting a carrier from any station is connected with the pressure reservoir, beyond which connection it is continued at a slight incline in which are placed two cocks, at an interval equal to the length of the carrier. Beyond the second cock the tube is open at the top in the form of a trough, into which the carrier is first placed. The first cock is then opened, allowing it to pass down the tube as far as the second cock. The former is then closed again and the latter opened, whereupon the carrier descends the inclined tube until it passes the pipe communicating with the pressure reservoir, whereupon it is caught by the current of air and blown to its destination. These carriers are thin metal cylinders, nearly filling the tube, supported by four wheels, two at each end, and alternately at right angles to each other. The receptor consists of a square box placed in continuation of the tube connected with the exhaust reservoir, with which it communicates by means of a cock bored to the same diameter as the tube itself. This box is lined with brushes, through which the carrier forces its way, and its impetus is thus checked, whilst at the extreme end is an india-rubber buffer. The Exchange station, at the other end of the tube, is supplied with similar apparatus, only, of course, without the engine, pump, &c.; instead of which the tubes are connected together by a short coupling tube. In some experiments which were made to determine the relative pressures in the two reservoirs, it was found that with equal differences of 9 inches of mercury above and below atmospheric pressure, the transit time of a carrier the whole distance of 3000 feet was 95 seconds from the station to the Exchange, whilst it only occupied 70 seconds in returning. The pressures now employed are, in the one reservoir, 7 inches of mercury over, and in the other 6 inches under atmospheric pressure. With this arrangement the transit times were, from station to Exchange 1 min. 30 sec.; and from Exchange to station 1 min. 20 sec.

In the system in use in Paris the same apparatus constitutes both the receptor and transmitter for the carriers. It consists of a cast-iron stand or pedestal, surmounted by an air-tight box, in front of which is a lid or door. Two tubes enter this box from opposite sides; one leading to the pressure reservoirs and communicating through a cock or valve with a branch below it with the box, whilst the other branch from the box is connected with a tube open to the air at the top, and also provided with a cock. A central vertical tube closed at the top is used when carriers arrive, and acts then as an air buffer, against which they expand their force. Beneath the box another tube leads to the next station. In sending a message it is placed in a box and the door shut, the cock communicating with the compression reservoirs is then opened, and the pressure of air blows the carrier through the tube. At this time the cock communicating with the open tube is kept closed, but when a message is to be received this cock is opened and the other kept closed—the open tube admitting the escape of the air in front of the carrier.

According to Mr. Siemens's new method, a complete circuit is formed by the current of transmission, with which several stations may be brought into communication with each other. The transmitting and receiving apparatus is extremely simple, and consists of two short pieces of tubing the same diameter as the main tube, and out of the latter a piece is removed of equal length. By means of a crank, or rocking shaft movement, either of these short tube pieces may be connected with the main tube by a simple movement of a lever, and thus brought into circuit. One of these short tubes is open throughout. This is the transmitter. It is ordinarily kept in circuit, so that messages to other stations beyond may pass through. When it is desired to send a message the circuit is broken by moving the transmitter a little to one side; the carrier with its message is then placed inside, and after communicating by signal with the station for which it is intended, the transmitting tube is once more brought into circuit, when the current of air immediately catches the carrier and hurries it on to its destination. Upon receipt of the signal at the further station for which the message is intended, the person in charge brings the receiver into circuit. This consists of a tube similar to the transmitter, with the exception that it is partially closed at one end so as to catch the carrier as it arrives. Its arrival is ascertained by the click caused by its striking the partially closed end of the receiver, which is then drawn back to extract the carrier, and the open transmitter is at the same time thrown into circuit so as to allow any through messages to pass. As each through message passes a station it causes a small bell to ring, as a signal to the superintendent, and to enable him to count when any message is due for his station. By this means a continual service of messages may be carried on in any circuit, the amount of business transacted being limited only by the means of the superintendents at each station to keep pace with the arrival and dispatch of messages.

AN ESSAY TOWARDS ESTABLISHING A "SCIENCE OF MONEY."

By A LONDON MERCHANT.

WHILE engaged with some views on the origin, nature, and use of money a few years ago, I happened to read Professor Müller's new work on the Science of Language; and from certain similarities apparent in the two cases, it then occurred to me, after his example, to entitle my subject the Science of Money. The Professor had satisfactorily established that language was not born of antecedent design, but rather of men's needs and capacities, in contact with external objects and events. As he had felt hesitation at the novelty of his term, much more had I at the unwonted apparition of a Science of Money. I did not therefore venture on the term on that occasion. Seeing, however, that the Science of Language is already a familiar phrase to us, I am encouraged to do so now. But I am still content with the introductory form of my title, hoping that on the next occasion with the subject I may at length venture to call it simply the Science of Money.

"As civilisation advanced," says Professor Price in his late lecture on Money, "an effective contrivance was invented in coin, which every one consented voluntarily to take in exchange for the goods he had to sell, because he knew that when he himself required to buy he would be able to get other property of the same value as that he had sold for coin." If this be an accurate or probable account of the origin of our subject, there can be no Science of Money. Our chief question then is: Did money come into existence by antecedent design or otherwise?

To aid our illustration, let us take a few early historical data on the subject. They will present money to us in a simpler condition than at present, and in a state that may help us in tracing it still nearer its source. In early Greece iron was used as money; in early Rome rings of copper. Silver money came subsequently to Greece in her trading with Asia, along with the weights and measures and other commercial facilities of the teeming and busy East. Gold money came generally at a later time. The Greek trading communities—Argos, Ægina, Athens—readily adopted the silver; the interior and non-trading, as Sparta, clung longer to the iron. Lastly, the Lybians were reputed to have invented money—a question that will be estimated further on.

This rude money of early Europe, then, has developed into the vast and highly artistic money system of after times, concurrently with the great commerce of these times. But this advance has not been uniform over the world; and that world still presents to us even to-day, among Australian tribes, for instance, as well as other populations, a primitivism as free from industry in the commercial sense, from exchange of products and from money, as any that preceded Greece or Rome. We have ascended a step when, quitting our modern Australian, we come to Mr. Brookes's paterfamilias of Borneo, who, gathering a load of bees'-wax, perambulates the country till he has exchanged it for other things his household needs. But our Bornean showed readiness to advance when our countrymen, and others from without, introduced him to larger trading and the use of money. Commerce and civilisation seem born of such mutual intercourse. The steps of human progress are generally taken with a difference due to places and circumstances, and by intercourse these differences are mutually appropriated towards further progress. Civilisation, therefore, with its indispensable commerce, has chiefly flourished along the world's great streams of human intercourse, shaped, as these are perhaps chiefly, by physical and climatic features. The peoples who have remained outside, shunted from the active life of the world, have more or less preserved to us the primitivism to which we can still to-day refer in illustration of our subject.

Let us now take a further step; let us, in short, go as near to the threshold of money as may still indubitably leave us outside of it. We shall suppose a society where labour is organised into a daily vocation; the society tolerably compact—not scattered and merely pastoral. As a whole it is fairly well off, with a permanent stock of means. In a society thus conditioned we should find that industry had taken the direction of the subdivision of labour. Each household would be busy over something special, instead of trying to produce directly everything it required. Each thus acquiring a special skill and knowledge in its own department, the productive power of society at large is greatly increased. Under this system, into which an industrious people, when sufficiently aggregated, instinctively adjust themselves, owing to practical appreciation of its advantage, there is concurrently, of course, a system of mutual exchange. These exchanges, which would assume at first only a casual and relatively unimportant aspect, will gradually by their universality come to be only second—if, indeed, second—to production itself.

Let us follow the probable course of the exchanges in such an early community. Each trader, we may suppose, will first supply his family from his own special wares, to the extent of their suitability and of his own means. He will next effect exchanges of his wares for his family's further wants; and lastly, he will aim at maintaining a stock adequate to his trade.

Now, if there were no diversities of personal business qualities, or such things as good and bad fortune, communities might plod along in this simple way for very lengthened terms without any development beyond the few and direct exchanges required. But surplus means gradually come into various hands, and when the immediate wants of the family and the trade are provided for, what is to be done with what is over—with a surplus always increasing in a vigorous and advancing society? We know that society does not relax its industry when entering on this phase, and therefore that a solution goes on concurrently from the first. The tendency will be to take opportunities of investing such surpluses in some value outside one's own trade. A clothier will not, as his productive power increases, pile up garments indefinitely, nor a butcher or baker mere additional heaps of his own kind of food. The surplus means will go to some variety of investment that commends itself, in addition to the distinct advantage of variety, by superior keeping qualities, and especially by being an article readily realisable when the value, thus, as it were, safely but temporarily stored up, is wanted for some actual—some permanent—investment. In this way arises a system of intermediate or temporary holding of a kind of value that is considered most convenient and suitable. These instinctive efforts to provide for surpluses initiate the system of wholesale dealing. Practically the surpluses are offered at so much discount; and out of this dealing, as we shall see further on, emerge loanable capital and banking.

In a considerable aggregate of a market such as we are supposing, a good many different kinds of articles may at first seem, to the free fancy of the different traders, about equally suitable, on considerations special and general, for this temporary holding. But practical experience gradually dispenses with many of these as less generally suitable than others. The tendency is ever to a limitation of choice, and the action in this direction is the more decisive with the additional demand for these articles—their additional negociability, as the result of this extended business. In short, the drift of the market is towards some one kind of article, which commending itself previously by being in constant and general use, has now such a further market as to be the object of purchase and sale much more than any other, and thus to present quite a surpassing negociability. Let me add, from historical fact, that a society arrived at this commercial stage has always come into the use of the commoner metals—iron and bronze, or copper. These metals, in their wide-spread use and with their qualities in other respects, are so markedly superior to most, or indeed to all other articles alluded to, that one or other of them has generally come into the prominent position in question; and this position, we shall presently find, is no other than that of money.

Let us now turn to the early Greeks and realise how iron came to be used by them as money. Iron had come into general commercial use; it was the raw material of two chief trades, that of the fabrication of arms, and that of agricultural implement making. For both purposes the manufacture of the metal would go on in common up to a certain stage, whence there would be a divergence respectively to the sword and the ploughshare. Comprising at this stage the material of the two chief vocations, there would be relatively a great trade in the iron, which would consist most probably of bars conveniently portable and of moderate value, and as much alike as possible. Such a kind of value, at once durable for keeping purposes and readily realisable, would be specially attractive to those having a surplus they were not otherwise prepared to invest. This practice become general, the bars are of course the subject of a much more extended market than before. Constantly bought and sold, not merely for the original manufacturing but for intermediate holding purposes, they thus come to be by far the most negociable of all articles.

A result of this practice, and one prompt to show itself, is that every other article comes to be valued in these iron bars. The needs of commerce will early jump at this obvious facility. Then, as every article has its value in bars, that indispensable basis of business—a price current of the market—becomes possible, because the bars are a common measure of value. The price current quickly makes its appearance. Further, the growing facilities of dealing in bars lead eventually to every exchange, as a rule, being effected by the medium of the bars. Both the facility and the accuracy of dealing in this way are so obvious that direct exchange or barter, as a rule of dealing at least, definitively dies out.

Let us realise clearly the position we have now reached. At first the bars were selected as a suitable kind of value for a temporary investment. This operation was simply one variety of the general barter system then going on. At last we find that exchanges generally, not merely those for a distinctly purposed although temporary holding, are effected in the first instance for bars. In fact, as the practical facility from accurate valuation and ready dealing gives a most decided superiority to this mode, seemingly round-about although it be.

Everything thus comes to be bought or sold in bars, whether for immediate exchange or upon a time contract. Some traders might still barter direct or bargain in kind. They might, for instance, exchange so much wheat for a sheep, or deliver at once a ton of lead in consideration of a ton and something more of the same three months hence. Most persons, however, will gradually prefer to contract value for value, which they are enabled to do by contracting for payment in bars. Bars are the most accurate and indisputable value, as well as the most realisable of all articles. Eventually, the custom is universal, at least in any considerable market. All bargains, then, as a rule being for bars, every trader has his stock of bars to accurately balance as well as otherwise facilitate his dealings. Lastly comes a trader who deals only in bars, and who yet has no connection with the special manufacturers the bars were originally intended for. He receives balances of bars from the other traders for safe keeping until wanted; and, as this gives him an average available stock of them on hand, he can lend the bars to others, or discount the time-contract notes expressed in bars. This trader is no other than a banker—the rude prototype, indeed, of the dignified personage of to-day, who deals only in a highly artistic value derived from the subsequently distinguished "precious metals," or in his own not less artistic bank-notes. But our original is none the less a banker, and none the less is he dealing with money, which has now come into veritable existence in the community.

We of to-day, indulging in the facilities of *ex post facto* views, can perceive that, rude as the materials are to our highly-educated sense of money, these bars are nevertheless being used as money, for they have evidently a sphere of use of their own that is quite distinct from and independent of their original use for manufacture. Indeed, so evident is this to us, that we are apt to take for granted that our early community see it also. But this by no means follows. We must bear in mind that they have had no help whatever to such an idea, because the bars are still in the form in which they go direct to the forge or the factory. No doubt, some reflecting banker of that time may have noticed that the stock of bars in existence seemed greatly more than the manufacturer could want. But it must also strike him that the whole stock goes eventually, sooner or later, to the factory. "To-day in the till, to-morrow in the furnace," might be his concluding reflection as he dismissed this part of the question. Like some theorist of our own time who wondered at the huge stocks of modern trade, he would simply conclude that somehow the modes of business required them all. I do not hesitate, therefore, to assert that the probabilities are decisively against the view that at this stage money was detected, even after it had come into full existence.

But the banker, although he has as yet failed to perceive money, can from his vantage ground see clearly some of its effects: he sees that these bars repeatedly come back upon his hands. Perhaps out of mere curiosity on this point he has marked a number of them; and although he learns, as of course, that some of these marks have yielded up a brief life at the factory, yet others, probably much the larger proportion, go for some time at least to and fro between himself and the other traders. A convenience therefore suggests itself to his mind: he doubles the bar into a ring for the sake of easier carriage. Hesitatingly he does so, because he has no other idea as to the bars than that of their use in "The Arts," as we would now phrase it. Every article, in fact, still belongs to the arts; and we can best realise our early banker's case by supposing some modern ironmonger to be bent on lightening work to his shopmen by doubling up some portion of a large stock of brass rods or iron crowbars, which he expected to remain long on hand, and whose frequent shifting he wished to facilitate. Even with more hesitation might the customers of our innovating banker receive these altered bars, which, with the sole idea of the manufacturer before them also, they would regard at first rather as things tampered with and depreciated than as made more convenient to their business. But the edge of the novelty gone, habit and convenience would steadily plead for the rings, and eventually they would penetrate everywhere by preference, except to the manufacturer's forge. Thus there will at last be two quite distinct articles—the bars and the rings, and it can now be seen that they have respectively quite different uses.

We have already traced the probable origin of iron money in Greece, and we have just seen how most likely arose the copper-ring money of early Rome. Thus we reach a further and most important stage of our subject, for money now stands forth confessed, instead of being indiscriminately mixed up in the general market. It is still, indeed, as much as before, one among the articles of that market; its use is still as before for temporary or intermediate holding; but having now its own distinctive pattern, the mind can now follow it into its own distinctive function. In short, although money had previously come into actual existence, it has only now come into differentially visible being.

Money thus distinctively standing forth, one early consequence is a distinctive name. A new idea occupies the mind, and a new, or at least a distinctively applied, name will be forthcoming for its recognition. This completes the separation of the rings from the bars, or, in more customary language, of money from merchandise. Let us attentively follow the further effects. At the bar stage, it will be remembered, we had, equally as at the ring stage, the reality of money, only that money had not become distinctively visible—had not, as it were, emerged from merchandise. We have seen that, even at the earlier stage, exchanges, as a rule, had come to be for bars, and all values to be expressed in bars; but that the mind had not parted, in all this, from the actual market of commodities. The bars themselves were undistinguishably a portion of these commodities, used for convenience to value and exchange the rest. But now that money has shown itself and has acquired its own special name—money let us call it—there arises in the mind a new and irresistible tendency. A spade is no longer a spade. All dealings have been in bars or rings; now they are in money. What had been a loan of bars, or of other wares, repayable in bars, with some few additional bars as consideration, is now, with all attractive simplicity, a loan of money, repayable with interest. Business in this way is indeed marvellously simplified, and hence the irresistible tendency; but the popular mind has drifted into a misconception, from which, with every successive step of departure from the plain-speaking age of the bars, it is less and less able to free itself. Everywhere money crowds up to the business surface, obscuring the view of everything else in the market. As the banker turns over countless contracts, he meets with nothing but money. No doubt the consideration for all these engagements has been as a rule merchandise of some sort, and he is not and cannot be ignorant of that fact; but as the consideration is all expressed in money, the banker has nothing before him but money; while all this floating value that is so represented is to him "the money market." When his customers have large balances of this value, money is plentiful; when otherwise, money is scarce; and the consideration for the loan or use of the value is small or great accordingly.

But what is the Money Market, and what are those balances of value the banker is dealing with? They are simply those surplus means that we have seen gradually arising in society, and compelling their owners, by the force of their own interests, into that course of advantageous investment that has thus resulted in the use of a "currency," a "circulating medium," a "common measure of value"—in short, in the use of money. Our great modern money markets are mainly the result of the increasing productiveness of labour in its further and further adaptive subdivision in the home and foreign trade, with all the exchange system of that trade, and the huge stocks required to conduct it effectively. The Money Market is all this product, including such a proportion of money—usually but a small proportion—as the modes of business and the means and money-carrying habits of society happen to require.

Let me notice here in passing that the confusion here alluded to in the popular mind has given us two quite different meanings of the term "value of money." The value of money, as of any other commercial article, should mean the exchangeable value, that value, namely, which places gold above an equal quantity of silver, and silver above as much lead or copper. But this proper sense of the term is almost lost sight of in the general prevalence of quite another sense, which, erroneously as we have seen, associates it with rates of interest and discount. Interest and discount refer to surplus means generally, to the capital of the community, or, more strictly, to the floating or circulating portion of the capital—the part of it that people have to spare, and that is thus available for loaning.

And now we return to our first and principal question, upon which we are, I hope, in a position to decide—Did money originate by design or otherwise? But the question has been already virtually answered when we found reason to conclude that even after money had come into being through the natural tendencies or "drift of the market," it had not been recognised, even in the act of using it, until the subsequent accidents of its further course had given it a distinctive unmistakable aspect of its own. Of course, therefore, it did not owe this existence to anticipation and design.

Nevertheless, there is so strong a general prejudice towards the opposite view that it may well be asked—What has caused and what sustains such an opinion? This bias has arisen chiefly, I think, from an exaggerated idea of the difficulties of early trade, under the barter system, prior to the use of money—difficulties so great, as we are apt to suppose, that our predecessors in trade must have been impelled into the invention of money. Indeed, volumes have been written upon the unbearable inconveniencies and obstacles of these pre-money times. But these difficulties are entirely of our own imagining, because, in the effort to realise them, our minds are prepossessed by a condition and scale of trade that had no existence in the times in question. The true case is, that each successive age has found its own facilities for all it had to do, and was no more under a sense of inconvenience than any people of to-day less advanced than ourselves, and with wants as limited as their commerce. Even the bees'-wax bartering of our Borneo Dyak would not have ceased upon a mere explanation of the use of money, unaccompanied by the additional commerce that required money's facilities. Money could not live in his thin atmosphere of trade, any more than double-entry book-keeping in the simple dealings of a costermonger. In some of our more sequestered villages, if, indeed, any such still remain in this railway age, the use of money even now may hardly be considered general—a state of things that we, with our modern prepossessions, are apt to associate, although not always correctly, with the extremity of poverty. Money was not an invention, but a growth—a development arising gradually out of the needs of society. We have seen how it arose out of the Drift of the Market, and therefore there is a Science of Money.

Here my subject properly ends, because the further history of money has reference to the adaptations of an intelligent design—adaptations which, so soon as there is a clear view of money's existence and uses, are all more or less obvious—more or less readily seen to be advantageous or necessary. But in order to complete our question it is necessary still to follow the principal of these changes, because to most of us of to-day money has become almost indissolubly associated with the Mint and the precious metals. We have hardly even alluded to this part of the money question, and therefore to most people's ideas we have not even yet reached the recognisable domain of money. From our stage of the primitive bars and rings, therefore, let us, in deference to popular prepossessions, descend for a brief space into the modern arena.

The intervention of the State is at no great distance after the recognition of money. At first this may be limited to the mere guarantee by an official stamp of an article so generally used and trusted in; but eventually the State appropriates the whole question. A minuter subdivision of value is, no doubt, the very first of the secondary steps of money's subsequent progress. The ring—holding still to our representative and very illustrative ring—is broken into pieces to suit the wants of small dealings. For the opposite reason, there is next felt the want of the expression of high value in reasonable bulk, and silver and gold are successively brought in to supply the need. Gradually the monetary idea has become a subject so familiar and plastic that the State can venture on materials for its money without reference to any other consideration than their special suitability for money, and the cost of getting them. In this crisis the iron is liable to be summarily dismissed from office. Money takes to itself a further distinctive existence, and the materials of which it is chiefly composed are specially honoured as "the precious metals." But long prior to this result—perhaps concurrently with even the earliest of these various secondary steps—the artistic idea has been struggling to the front. It has never, indeed, been relatively behind in the early history of those intelligent races among whom money has first appeared; and it finds its expression here in a shapely form given to the metallic pieces. In short, the rude fragments of the ring become coins, and the ambitious taste of a ruler is not long behind with the addition of his name and effigy. Such must be the true gage and the true importance, or rather unimportance, of the Libyan discovery.

Conventionality is now supremely enthroned in the domain of money. It is readily apprehended that money is not wanted in any sense of actual consumption applicable to other articles. While, therefore, a half ration of food could not be reasonably issued as a whole ration, yet adequate authority might command a half pound of money to circulate as a whole pound. Hence we have mintage and seigniorage rates, and depreciations and debasements of currency in all degrees. Finally, paper money dispenses with the cost of any value whatever. In its usual and regular form, paper money is the promise to pay the actual money whenever demanded; and where the issuing party is fully trustworthy, the sign is as good as the thing signified, and often a greatly more convenient instrument. To this "convertible paper" the inconvertible is only the too evident alternative.

These are all contrivances for supplying money below the cost to the issuing party. Many quarters may be simultaneously at work with these cheap contrivances, the banker as well as the government, and the forger, too, in his smaller, but not less economical, way. Can all this go on indefinitely, and has money at last emancipated itself from those laws of supply and demand that apply to other articles, and that evidently applied to money also as one of these articles before all this artificiality was introduced? Has money in short become a thing "*sui generis?*" In no wise whatever. Society's modes of business, and the individual habits as to money-carrying, settle themselves into the need of a certain amount of money, in whatever of money's forms this supply may come; and if this supply is exceeded, there is simply the effect due to a relative excess in the supply of anything else in the world of trade materials. The excess is returned to that world of materials whence it came, and if through seigniorage or other obstacles it cannot be so got rid of, the money is depreciated until it can.

We know that money has not in all cases arisen and been developed in the way here stated, namely, by undesigned tendency out of the most commonly used metals, and afterwards by conventional transfer to higher-value metals. There is allusion in early history, for instance, to live stock being used as money. In modern times the early Virginian settlers used tobacco for the like purpose, and the eastern Asiatics still use brick tea, and the Abyssinians cakes of salt. And again we have the notable case—solecism among exceptions we may call it—of the cowrie money. But these exceptional moneys are always the product of exceptional circumstances. They arise, excluding for the moment the special case of the cowries, only in thin, scattered, or merely pastoral communities. Such money could find no life in any ordinary aggregate of intelligent industry, with its conspicuous factories of the common metals, and even, failing them, of many other articles more suitable for a temporary holding and for exchange purposes, than a sheep or a piece of tobacco. The drift of the market, as I have termed it, would be peremptory, decisive in this respect. Where there is no market in this sense of industrial human aggregation, this regular effect is not of course forthcoming. But all these articles of irregular money, so to call them, have ever had the main requisite of a general appreciation and use as articles of trade, giving the users the idea of realisableness or ready negociability, and thus meeting for each person the convenience of the hour or of the special case in circumstances probably of very limited choice. Such money, however, has generally at best held but a locally partial or a divided sway; and it is only in some secondary sense of this kind that we can interpret the old classic allusions to sheep and cattle that have come down to us. Cowrie money stands on a different ground. Its origin as money is doubtless attributable to the fancies of children, protracted into the later life of simple peoples, whose minds never rise greatly beyond those of children. The case is as though our schoolboys had preserved into later years their strongly impressed notions of the exchangeable value of their marbles, so that this money of boyhood's transactions had passed into those of mature life.

Lastly, let me advert to yet another subject, fraught, like some of the preceding, with popular misconception. I might entitle this closing paragraph, "the cost of a currency." People have not usually thought of money, indispensable as all admit it to be, as none the less a costly instrument to society. We have seen, however, that the tendency in each individual is to economise money—to find his advantage in doing with as little of it as possible; and hence the whole community is gainer by this course.

Countries the most commercially advanced have the greatest facilities in this respect; and hence arises the sort of popular paradox just hinted at, namely, that such countries possess, relatively to the amount of business done, the least amount of money. In other words, countries which, as having the greatest commerce, are the richest, require, and do actually possess relatively, the least money. In ancient times when there was comparatively little of commercial credit and the modern banking facilities, there must always have been a large amount of money relatively to the other wealth of society. We conclude that such a condition was not to the enrichment but the impoverishment of society, much as any other of its indispensable habits would be if gone about after a like costly fashion.

Money may be aptly compared to a road through a field. The field at first, we will suppose, showed no tracks, as every traveller in the business of passing through was finding his own. By-and-bye some definite paths appear, and eventually one or other of these becomes the common highway, broad and well-beaten, because used by every one. The passengers' object or design had not been to make a road, but to get through the field; nevertheless the road is the result. The road we perceive, indeed, to be indispensable for the object in view, but the illustration may help us to apprehend as readily, that the less of the field we must surrender to the road, the more of it will remain to us for other uses.

"WHERE ARE THE BONES OF THE MEN WHO MADE THE UNPOLISHED FLINT IMPLEMENTS?"

By WILLIAM PENGELLY, F.R.S., F.G.S.

WHY don't you find the *bones* of the men, as well as their *implements?* is a rejoiner constantly made, and with an unmistakable air of triumph, by those who hear with disbelief the statement that human implements have been found with remains of extinct animals, in cavern deposits and river gravels. It is unnecessary to say that the question thus put is based on the following assumptions:—

1st. That, under all conditions, the bones of man are as conservable as those of other mammals.

2nd. That no portion of the human skeleton has ever been found in association with the extinct cave animals.

3rd. That until they have been so found, the doctrine of "Man amongst the Mammoths" remains non-proven.

We purpose devoting this paper to a consideration of these assumptions, and in the order in which they stand.

I. The proposition that, "under all conditions, the bones of man are as conservable as those of other mammals," assumes a very plausible aspect, no doubt, when we are told that their composition is the same; that in Egypt no difference is observed between the condition of the mummies of men and those of quadrupeds; that, on ancient battle-fields, the bones of the warrior and of his horse are equally well preserved; and that Cuvier, who gave much attention to the question, did not believe in the contemporaneity of man with the mammoth.

Let it be supposed to be the fact that, in a general way, the bones of man and the lower mammals *have* the same composition. Since as much may be said in the case of plants, it might be thence argued that had there been in the old Carboniferous Period the same variety of plants as we have in the present day, representatives of all of them would have occurred as fossils in the coal and associated beds. The fact, however, is otherwise; for certain groups of the vegetable kingdom are totally unrepresented in the beds in question; and hence, if the argument is worth anything, they could have formed no part of the flora of the coal period. To this, however, it is sufficient to reply that in March, 1833, the late Dr. Lindley placed in water, in a tank, 177 specimens of various plants belonging to all the more remarkable natural orders, including representatives of all those which are constantly present in the coal measures, and also those which are universally absent. The uncovered vessel was exposed to the air, and left untouched, further than filling it up as the water evaporated, until April, 1835, or upwards of two years. At the end of that time it was found that certain kinds had entirely disappeared; others had left some more or less recognisable traces; whilst others —especially fungi, ferns, and coniferous trees—were comparatively well preserved. In short, the plants remaining and the plants which had disappeared were respectively of the same groups as those which are and those which are not present amongst the coal fossils. *

Again, it is well known that certain shells, such as oysters and limpets, are more frequently met with in a fossil state than others are, such as cockles. Now, both groups are

* See Lindley and Hutton's Fossil Flora, vol. iii., pp. 4—12.

formed of the same materials—carbonate of lime and animal matter. If, therefore, the argument of similarity of chemical composition is to be trusted, it follows that a rock rich in fossil oysters but poor in fossil cockles must have been formed at a time when, or in a locality where, the former abounded, but the latter were scarce. In 1862, however, Mr. Sorby brought the subject under the notice of the British Association, and pointed out that the carbonate of lime in the shells of limpets, oysters, and some other mollusks, took the form or condition of *calcite*, but in cockle shells and their allies, the form of *arragonite;* the difference being one, not of chemical composition but of physical arrangement, whereby the molecules were in stable equilibrium in the former, but unstable in the latter, thus giving the arragonite shells a liability to disappear from which those of calcite were exempt.*

It is obvious, therefore, that as it is unsafe to assert that a deposit poor in arragonite shells was formed at a time when, or in an area where, few such shells existed, and as it is not necessary to suppose that there were no lichens, or mosses, or grasses, or sedges in a given period of the past, simply because its deposits contain no fossils of these groups, it may be very far from certain that there were no men during what is called the cavern era, even if it be a fact that no human bones have ever been found in the "cave-earth."

Whilst it is true that the bones of the human and of the infra-human mammals consist of the same materials, it must not be supposed that the different bones of even the same animal, or different parts of one and the same bone, or corresponding bones in individuals of different species, are *identical* in composition. They differ in the proportions in which the components are mixed; or, in the language of the chemist, though they agree *qualitatively*, they differ *quantitatively*. We must refer our readers desirous of full information on this point to the analyses of Von Bibra,† from which we have compiled the annexed table (see next page), showing the composition of the thigh bone in the human subject at different ages, and in different species of the lower mammals.

Though it may be true that in Egypt no difference is observed between the condition of the mummies of men and of quadrupeds, it does not appear that the fact has much, if any, bearing on the question before us, for the human bones we are supposed to be in quest of could not have been under conditions at all similar. They were neither placed

* See Report of British Association, 1862; Proc. of Sections, p. 95.
† See Day's Simon's Chemistry, vol. ii., Pt. ii., pp. 396 *et seq.*

Table showing the composition of the thigh bone in the human subject at different ages, and in different species of the lower mammals :—

	Phosphate of lime, with a little fluoride of calcuim.	Carbonate of lime.	Phosphate of magnesia.	Salts.	Cartilage.	Fat.
Human, 5 years	59·96	5·91	1·24	0·69	31·28	0·92
,, 19 ,,	54·78	10·90	1·34	0·83	31·15	1·00
,, 25 ,,	57·02	8·92	1·70	0·61	29·58	2·00
,, 25 or 30 years	59·63	7·33	1·32	0·69	27·70	1·33
Horse, 6 ,,	54·37	12·00	1·83	0·70	27·99	3·11
,, 14 ,,	54·63	11·28	1·50	0·40	27·98	4·21
Boar	58·88	9·02	1·17	0·92	28·00	2·01
Bull	54·07	12·71	1·42	0·82	29·09	1·91
Goat	55·94	12·18	1·00	0·50	29·68	0·70
Hare	58·45	9·07	0·99	0·82	29·60	1·07
Squirrel . . .	57·03	10·45	1·36	0·90	29·46	0·80
Rat	60·38	6·72	1·91	0·91	28·98	1·10

in well-formed tombs, nor swathed in protective envelopes; nor had the countries in which it is said they should be forthcoming the dry atmosphere of Egypt. They must have been buried by the action of water in a deposit which, in all probability, would constantly allow water to have access to them, in at least small quantities.

We have seen it stated that on some ancient battle-fields, in South America in particular, bones of men and of their horses have been found equally well preserved, and have no intention of calling it in question; but, before its bearing on the present subject can be admitted, we must know something about the atmospheric conditions of the districts in which the phenomenon has been observed.

The opinion of Cuvier, or any one else, on the question of the contemporaneity of man with the mammoth, is of much less importance to us than the facts and reasons on which the opinion was based. Authority is but little likely in the present day to be admitted in a scientific argument. Be this as it may, the large body of fact which has been carefully collected since Cuvier's time may be believed to be well calculated to have changed his opinion, as it has that of many eminent men of science now living. Thus M. A. de Quatrefages, Member of the Institute of France, speaking on this point, says, "I should state that, after having for a long time shared the belief of Cuvier, I have arrived at a contrary opinion."*

It is much to be desired that some one would do for bones, including those of man, what Dr. Lindley did for plants, and ascertain by experiment how long-continued immersion in water would affect them. In the meantime, however, we are not without reason for believing that human bones soon disappear when thus exposed. In 1853 the Dutch Government succeeded in converting the Lake of Haarlem into dry land, and thus added 45,000 acres to the soil of Holland. There had been many shipwrecks and naval fights on this sheet of water; from thirty to forty thousand souls had their homes on its borders, and hundreds of men had found a watery grave in it. The canals and trenches dug to a considerable depth through the rescued land must have had an aggregate length of thousands of miles; yet not a single human bone was exhumed from first to last. Some arms, a few coins, and one or two wrecked vessels alone rewarded the antiquaries, who watched the operations in the hope of a rich harvest. Here, as in cavern deposits and river gravels generally, works of art alone furnished evidence of the existence of man, even though no part of the deposit could be more than three hundred years old, as the lake was formed by an inundation towards the end of the sixteenth century.†

II. If it be true that no portion of the human skeleton has ever been found in association with the extinct cave animals, the literature of the question bristles with unmistakable and grave errors, as we shall now proceed to show:—

In 1824, the Rev. Dr. Fleming published a paper, in which he objected, apparently on good grounds, to some of the speculations of Dr. Buckland, in his "Reliquiæ Diluvianæ, published the year before, and distinctly stated that "Man was an inhabitant of this country at the time these animals, now extinct, flourished, his bones and his instruments having been found in similar situations with their remains.‡

Mr. W. L. Wrey, F.G.S., who during many years resided in South Wales, between Llanelly and Llandeilo, recently informed us that in 1831 he heard of the discovery of a cavern in his immediate vicinity, in the ordinary course of

* See Anthropological Review, vol. i., p. 333, 1863.

† See Lyell's Antiquity of Man, p. 147 *et seq.*

‡ See "Remarks illustrative of the Influence of Society on the Distribution of British Animals," in the Edinburgh Philosophical Journal, vol. xi., pp. 287—305.

quarrying the lime-rock. On visiting it he found the roof beautifully studded with stalactites, whilst the floor was a thick stratum of stalagmite extending from side to side. This floor rested on a mass of dark red very tenacious clay, throughout which bones of extinct and recent animals were mixed indiscriminately. Mr. Wrey obtained a large number of these remains, including an undoubted human skull, very perfect and in good preservation, found with the other relics. Believing the case to be unique and important, he at once wrote to Dr. Buckland, who lost no time in repairing to the spot, where he spent three days as Mr. Wrey's guest. For the explication of the facts, Dr. Buckland suggested that a human body had been buried in the clay, and thus the remains of man and of the much older extinct mammals had become mixed, after which the sheet of stalagmite had been formed over the whole. "This hypothesis," said Mr. Wrey, "was never satisfactory to my mind, because the human bones should have been lying together in the form of a skeleton, but instead of this they were mixed through the clay with the other bones." Many of the remains, including the human skull, were presented to Dr. Buckland, who expressed his intention of placing them in the Museum at Oxford.

The late Dr. Schmerling, of Liège, having carried on extensive researches in the numerous caverns in the Valley of the Meuse, published the results of his labours in 1833-4,* and stated that the deposits in many of the caverns were covered with a floor of unbroken stalagmite, and contained the commingled remains of extinct and recent animals, including man; that the human relics were of the same colour and in the same condition as those of the lower animals; and that they were so rolled and scattered as to show that they were not intentionally buried there.

Amongst the human relics found by Dr. Schmerling were several skulls, the most perfect of which, known as the Engis skull, from the cavern in which it was found, has attracted much attention, and according to Professor Huxley, is "a fair average human skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage."†

In 1840 Mr. Godwin-Austen read to the Geological Society of London a paper on "The Bone Caves of Devonshire," which, with other papers on the same district, was published

* Recherches sur les Ossemens Fossiles Découverts dans les Cavernes de la Province de Liège. See also Lyell's Antiquity of Man, pp. 63—74.

† Man's Place in Nature, p. 156.

in 1842 under the general title of "The Geology of the South-East of Devonshire."* In his description of Kent's Cavern, Torquay, he says, "Few, I imagine, who are acquainted with the facts which the labours of MM. Schmerling, Marcel de Serres, and others have established, entertain any doubts as to the fact that the bones of man have been found in caves; what I wish to state distinctly is, that they occur in Kent's Cavern under precisely the same conditions as the bones of all the other animals. The value of such a statement must rest on the care with which a collector may have explored; I must, therefore, state that my own researches were constantly conducted in parts of the cave which had never been disturbed, and in every instance the bones were procured from beneath a thick covering of stalagmite; so far, then, the bones and works of man must have been introduced into the cave before the flooring of stalagmite had been formed. It may be suggested that this cave was used as a place of sepulture by some early inhabitants of this country, and that bones of the other animals occupied the lower parts of the cave when such sepulture took place.

"In this case our researches should expose the human skeletons entire, as in the Paviland Cave; or at least the bones should occur in some sort of mutual relation to each other, but no such thing has ever been observed by any explorer in Kent's Hole; so that as far as the evidence from this case is to be our guide, ... there is no ground why we should separate man from that period, and those accidents, when and by which the cave was filled."†

In a discussion at the Plymouth meeting of the British Association, in July, 1841, the same geologist, according to the "Athenæum,"‡ said, "at Kent's Hole, near Torquay, arrows and knives of flint, with human bones, in the same condition as the elephant and other bones, were found in an undisturbed bed of clay, covered by 9 feet of stalagmite." Dr. Buckland, in reply, "contended that human remains had never been found under such circumstances as to prove their contemporaneous existence with the hyænas and bears of the caverns. In Kent's Hole the Celtic knives and human bones were found in holes *dug by art*, and which had disturbed the floor of the cave and the bones below it."

Dr. Buckland's reply is chiefly valuable as showing that Mr. Austen was correctly reported. The "holes dug by

* Trans. Geol. Soc., 2nd Ser., vol. vi., part 2, pp. 433—489.
† *Op. cit.*, p. 446.
‡ Athenæum, Aug. 14, 1841, p. 626.

art" in Kent's Cavern, of which the former spoke, were purely hypothetical, and invented solely in defence of a foregone conclusion. Dr. Buckland's knowledge of the phenomena of the cavern was entirely derived from the researches of other persons, chiefly the Rev. Mr. MacEnery, who spent several years in the exploration of Kent's Hole, and declined the acceptance of the hypothesis, as the following passage shows;—"Dr. Buckland is inclined to attribute these flints to a more modern date by supposing that the ancient Britons had scooped out ovens in the stalagmite, and that through them the knives got admission to the diluvium. . . . . . Without stopping to dwell on the difficulty of ripping up a solid floor, which, notwithstanding the advantage of undermining and the exposure of its edges, still defies all our efforts, though commanding the apparatus of the quarry, I am bold to say that in no instance have I discovered evidence of breaches or ovens in the floor, but one continuous plate of stalagmite diffused uniformly over the loam."*

The late Colonel Hamilton Smith devoted a section of his "National History of the Human Species" (1848), to the question of "Bones of Man among Organic Remains," of which the following is a brief summary:—In a conversation with the author in 1824, Cuvier admitted that the opinions then in vogue on the point would require considerable modification. Donati, Germer, Rasoumouski, and Guetard, maintained that human bones had been found intermixed with those of lost species of mammals in several places; they had been detected in England in caves and fissures; they were found at Meissen in Saxony, and at Durford in France, by M. Firmas. A fossilised skeleton found in the schist at Quebec, and in part preserved at the seminary, excited no attention; and the well-known Guadaloupe skeletons had been pronounced recent upon hypothetical reasoning. Those discovered by M. Schmerling in the Liège caverns were similarly disposed of, and Dr. Lund's reports respecting partially petrified human bones, found by him in the interior of Brazil, in the same condition with those of numerous animals now extinct, which accompanied them, attracted no more than incredulous attention. In the caverns of Bizé, in France, human bones and shreds of pottery were found in red clay mixed with the *débris* of extinct mammalia; a similar collocation was soon after detected by M. de Serres, in the caverns of Pondres

* See Trans. Devon. Assoc., vol. iii., p. 334, 1869.

and Souvignargues; and Dr. Boué found human bones mixed with others of extinct species at Lahr. In 1833 human bones were found, together with several species of the well-known extinct cave mammals, in caves near Liège, beneath a thick coat of stalagmite; and about the same period, the Rev. Mr. MacEnery collected from the caves of Torquay, human bones and flint knives, amongst a great variety of extinct species, all under a crust of stalagmite, upon which the head of a wolf reposed. Amongst the bones of the mammoth and his contemporaries, found at Oreston, near Plymouth, at different times before and after that period, the upper portion of a humerus of man was detected, and immediately thrown away as valueless on being pointed out to the possessor. About the end of the last century, gypsum quarries were opened in the Vale of Kostritz, in Upper Saxony. The gypsum was intersected in every direction by caves and fissures, which were filled with red clay containing clusters of bones of mammalia, including man, elephant, rhinoceros, horse, ox, elk, deer, reindeer, a great felis, hyæna, hare, and rabbit. A fragment of an arm and a thigh-bone of a man were dug out of the clay at a depth of eighteen feet; and eight feet below two phalanges of a rhinoceros. The facts were carefully observed and first described by Baron von Sclotheim, who remarked of the human bones, that they were few, completely detached and isolated, always found with the other animal remains, under the same relations, not constituting connected skeletons, but gathered in various groups.*

In 1861, M. E. Lartet, the eminent French palæontologist, published a highly interesting description of a cave or grotto at Aurignac, in the south of France.

It appears that some years before the paper was written, a man breaking stones for road repairs, observed a rabbit run into a hole in a steep talus of *débris*, and was led to thrust his arm in after it. He was not a little surprised to find that he had seized and drawn out a large bone. Prompted by curiosity, he cut a passage through the talus until his progress was stayed by a large upright slab of stone, which, on being removed, was found to have closed the entrance of a small cavern, previously unknown and unsuspected, and in front of which it had certainly been artificially placed. In the grotto he found a quantity of human bones and skulls, which naturally attracted a

* *Op. cit.*, pp. 93—98.

large amount of attention in the neighbourhood, and were the theme of so much conjecture, that the mayor of Aurignac deemed it prudent that the human remains should be collected and re-interred in the parish burial ground. Fortunately the mayor was a medical man, and before his directions were carried out, he had ascertained that the bones represented seventeen human bodies.

In 1860, M. Lartet, passing through Aurignac, visited the cave, of which he had previously heard, and proceeded to a full personal investigation of the deposit, both within and immediately without the cavern. In the soil which the human bones had occupied, and in its immediate prolongation outside, he found the remains of from 80 to 100 individuals, belonging to nineteen species of mammals, some existing and some extinct, including the cave bear, cave lion, cave hyæna, *Elephas primigenius*, *Rhinoceros tichorhinus*, Irish elk, reindeer, and aurochs. With them were tools of flint and of deer's horn—chiefly reindeer, and an article made of a canine of a young cave bear, the use of which was unknown, but was perhaps interred with one of the bodies as a token of affection. The marrow-bones of the aurochs, reindeer, and rhinoceros had been split by man, and it was observed that some of those of the last named species which had been thus treated, had subsequently been gnawed by the hyænas, as had many others. Outside the grotto, under the talus of fallen *débris* but on the ossiferous earth, was a layer composed of ashes and charcoal, from six to eight inches thick, and covering an area of several square yards. It contained a very great many teeth, principally of herbivorous animals, together with many hundreds of fragments of their bones, and coprolites of the hyæna. By chemical examination it was found that the bones of the reindeer, aurochs, rhinoceros, &c., had retained precisely the same proportion of nitrogen as the human bones mixed with them.

The interpretation which the facts obviously suggested and required, was that the grotto was an ancient burial-place, closed ordinarily with the heavy slab found at the entrance, but opened from time to time for the introduction of another corpse; that symbolical and votive objects, as well, perhaps, as such as were supposed to be useful in another state of being, were interred with the bodies; that after each burial a feast was held immediately without the sepulchre; that the hyænas devoured the remnants of the feast after the departure of the relatives; and that the era of the interments was that of the great cave bear and his contemporaries.*

The Committee appointed by the British Association in 1864 to explore Kent's Cavern, Torquay, reported in 1867, that in the stalagmitic floor overlying the cave earth, they had found a human tooth and part of an upper human jaw, containing four teeth; and, in 1868 as well as 1869, that in the same continuous sheet of stalagmite they had detected remains of the cave bear, cave hyæna, and rhinoceros. Moreover, that whilst the human remains were deeply imbedded in the stalagmite, where it was 20 inches thick, some of those of the extinct mammals were not only near its upper surface, but were partially exposed.†

The case of the famous human jaw of Moulin Quignon, near Abbeville, will probably be remembered by all our readers; and were we to pass it by in silence, we might be suspected of acquiescing in the opinion that it was an unquestionable forgery. The facts are very briefly as follow:—In March, 1863, a gravel-digger informed the late M. Boucher de Perthes, the distinguished Archæologist of Abbeville, that at 15 feet below the surface a bone was to be seen projecting about an inch from the face of a cutting then in progress, in a gravel pit at Moulin Quignon. M. de Perthes, accompanied by a friend, proceeded at once to the spot, where they witnessed the extraction of the bone, which proved to be an entire half of a human jaw, containing one tooth. The discovery at once attracted the attention it deserved; an Anglo-French scientific commission was appointed to investigate it, and after much experimentation and discussion, the opinion of a large majority that the jaw was found in an undisturbed portion of the lowest of five superimposed layers of gravel and clay, containing unpolished flint implements, and half a yard below fragments of the tooth of an extinct mammal, was embodied in a report drawn up by Professor Milne Edwards, and presented to the Academy of Sciences of Paris in May, 1863. The conclusion, however, was that of a majority only, for a few of the English Commissioners dissented; and this being the case, though by no means convinced that the minority was right, we doubt the wisdom of an unqualified acceptance of the authenticity of this famous jaw.‡

Such are the statements respecting the inosculation of the

* See Lyell's Antiquity of Man, pp. 181—193; or, Natural History Review, vol. ii., pp. 53—71.

† See Reports British Association, 1867, p. 28; 1868, p. 52; 1869, p. 204.

‡ See Anthropological Review, vol, i., pp. 166—168, 177—179, 312—335.

bones of man, and of the extinct cave mammals in undisturbed deposits, which, without special search, we have from time to time encountered and noted in the course of reading. Compared with the number of infra-human remains supposed to be contemporary which are piled up in numerous museums, the human relics are no doubt but few; yet we cannot but think that, without insisting on each individual case—though we know not why any should be excepted—they are sufficiently numerous and sufficiently well-attested to constitute a full and complete answer to the question, "Why don't you find the *bones* of the men as well as their implements?"

III. It may be questioned, perhaps, whether the acknowledged discovery of a portion of man's osseous system, in the very places occupied by the implements, would, or, indeed, ought to produce any change in the opinion of those who propose the sceptical interrogation. To doubt the human origin of the implements, of bone and stone, is utterly beyond the power of any one. They do or they do not prove that man was the contemporary of the animals with whose bones and teeth and horns they are found. If they do, his bones could do no more. If they do not, they must have found their way into the deposits since the relics of the extinct animals were lodged in them; and if this is possible in the case of human *tools*, why not also in the case of human *bones*?

But is the interrogation we have heard so often always proposed with perfect ingenuousness? Does it not, at least, sometimes remind us that "Drowning men catch at straws?" Do the proposers ever act similarly in the common affairs of life? Does any one ever think that De Foe's Crusoe was illogical, when he inferred that a man had been on his island simply because he saw a human foot-print on the sand? Or suppose that he should have waited until he had seen the *man* before he took the trouble to make himself secure?

Though by no means essential as evidence of the existence of man when the deposits in question were laid down, there can be no doubt that the exhumation of human bones would be a source of much pleasure; for if they did no more, they might teach us whether our early ancestors belonged to an extinct or to an existing branch of the human family, and what were their mental capabilities.

When we remember, however, that the implements which have been found imply that the intellectual position of their makers was low *actually*, whatever it may have been *potentially;* that in a climate like ours a savage population must also be very sparse; that so far as we know savages either burn their dead or bury them, and that any not recovered by their friends would probably have been speedily disposed of by the hyænas and other beasts of prey which then abounded, we are by no means sanguine that many bones of the men of the period under consideration will ever be found, unless it be in an ancient cemetery, such as that at Aurignac, already mentioned; or that those who wish to retain it will speedily be deprived of the privilege of asking, "Why don't you find the *bones* of the men as well as their implements?"

V. EXPERIMENTAL INVESTIGATION OF A NEW FORCE.

By WILLIAM CROOKES, F.R.S., &c.

TWELVE months ago in this journal* I wrote an article, in which, after expressing in the most emphatic manner my belief in the occurrence, under certain circumstances, of phenomena inexplicable by any known natural laws, I indicated several tests which men of science had a right to demand before giving credence to the genuineness of these phenomena. Among the tests pointed out were, that a "delicately poised balance should be moved under test conditions;" and that some exhibition of power equivalent to so many "foot-pounds" should be "manifested in his laboratory, where the experimentalist could weigh, measure, and submit it to proper tests." I said, too, that I could not promise to enter fully into this subject, owing to the difficulties of obtaining opportunities, and the numerous failures attending the enquiry; moreover, that "the persons in whose presence these phenomena take place are few in number, and opportunities for experimenting with previously arranged apparatus are rarer still."

Opportunities having since offered for pursuing the investigation, I have gladly availed myself of them for applying to these phenomena careful scientific testing experiments, and I have thus arrived at certain definite results which I think it right should be published. These experiments appear conclusively to establish the existence of a new force, in some unknown manner connected with the human organisation, which for convenience may be called the Psychic Force.

* See Quarterly Journal of Science, vol. vii., p. 316, July, 1870.

Of all the persons endowed with a powerful development of this Psychic Force, and who have been termed "mediums" upon quite another theory of its origin, Mr. Daniel Dunglas Home is the most remarkable, and it is mainly owing to the many opportunities I have had of carrying on my investigation in his presence that I am enabled to affirm so conclusively the existence of this Force. The experiments I have tried have been very numerous, but owing to our imperfect knowledge of the conditions which favour or oppose the manifestations of this force, to the apparently capricious manner in which it is exerted, and to the fact that Mr. Home himself is subject to unaccountable ebbs and flows of the force, it has but seldom happened that a result obtained on one occasion could be subsequently confirmed and tested with apparatus specially contrived for the purpose.

Among the remarkable phenomena which occur under Mr. Home's influence, the most striking as well as the most easily tested with scientific accuracy are—(1) the alteration in the weight of bodies, and (2) the playing of tunes upon musical instruments (generally an accordion, for convenience of portability) without direct human intervention, under conditions rendering contact or connection with the keys impossible. Not until I had witnessed these facts some half-dozen times, and scrutinised them with all the critical acumen I possess, did I become convinced of their objective reality. Still, desiring to place the matter beyond the shadow of a doubt, I invited Mr. Home on several occasions to come to my own house, where in the presence of a few scientific enquirers, these phenomena could be submitted to crucial experiments.

The meetings took place in the evening, in a large room lighted by gas. The apparatus prepared for the purpose of testing the movements of the accordion, consisted of a cage, formed of two wooden hoops, respectively 1 foot 10 inches and 2 feet diameter, connected together by 12 narrow laths, each 1 foot 10 inches long, so as to form a drum-shaped frame, open at the top and bottom; round this 50 yards of insulated copper wire were wound in 24 rounds, each being rather less than an inch from its neighbour. These horizontal strands of wire were then netted together firmly with string, so as to form meshes rather less than 2 inches long by 1 inch high. The height of this cage was such that it would just slip under my dining table, but be too close to the top to allow of the hand being introduced into the interior, or to admit of a foot being pushed underneath it. In another room were two Grove's cells, wires being led from them into the dining-room for connection if desirable with the wire surrounding the cage.

The accordion was a new one, having been purchased for these experiments at Wheatstone's, in Conduit Street. Mr. Home had neither handled nor seen the instrument before the commencement of the test experiments.

In another part of the room an apparatus was fitted up for experimenting on the alteration in the weight of a body. It consisted of a mahogany board, 36 inches long by $9\frac{1}{2}$ inches wide and 1 inch thick. At each end a strip of mahogany $1\frac{1}{2}$ inches wide was screwed on, forming feet. One end of the board rested on a firm table, whilst the other end was supported by a spring balance hanging from a substantial tripod stand. The balance was fitted with a self-registering index, in such a manner that it would record the maximum weight indicated by the pointer. The apparatus was adjusted so that the mahogany board was horizontal, its foot resting flat on the support. In this position its weight was 3 lbs., as marked by the pointer of the balance.

Before Mr. Home entered the room the apparatus had been arranged in position, and he had not even had the object of some of it explained before sitting down. It may, perhaps, be worth while to add, for the purpose of anticipating some critical remarks which are likely to be made, that in the afternoon I called for Mr. Home at his apartments, and when there he suggested that as he had to change his dress, perhaps I should not object to continue our conversation in his bedroom. I am, therefore, enabled to state positively, that no machinery, apparatus, or contrivance of any sort was secreted about his person.

The investigators present on the test occasion were an eminent physicist, high in the ranks of the Royal Society, whom I will call Dr. A. B.; a well-known Serjeant-at-Law, whom I will call Serjeant C. D.; my brother; and my chemical assistant.*

Mr. Home sat in a low easy chair at the side of the table. Close in front under the table was the aforesaid cage, one of

* It argues ill for the boasted freedom of opinion among scientific men, that they have so long refused to institute a scientific investigation into the existence and nature of facts asserted by so many competent and credible witnesses, and which they are freely invited to examine when and where they please. For my own part I too much value the pursuit of truth, and the discovery of any new fact in nature, to avoid enquiry because it appears to clash with prevailing opinions. But as I have no right to assume that others are equally willing to do this, I refrain from mentioning the names of my friends without their permission.

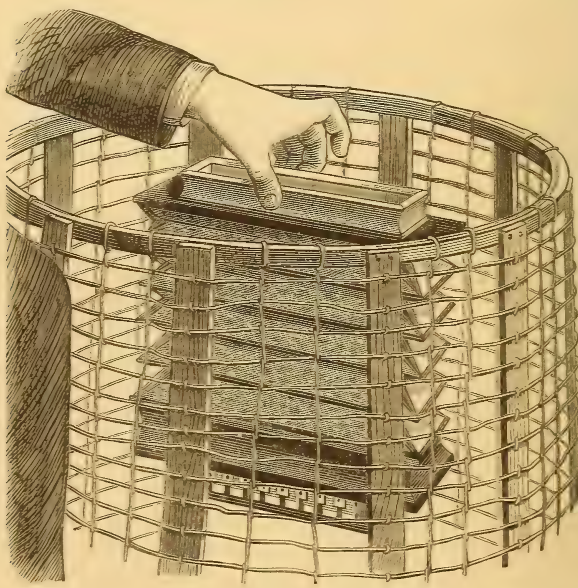
his legs being on each side of it. I sat close to him on his left, and another observer sat close on his right, the rest of the party being seated at convenient distances round the table.

For the greater part of the evening, particularly when anything of importance was going forward, the observers on each side of Mr. Home kept their feet respectively on his feet, so as to be able to detect his least movement.

The temperature of the room varied from 68° to 70° F.

Mr. Home took the accordion between the thumb and middle finger of one hand at the opposite end to the keys (see woodcut, Fig. 1), (to save repetition this will be subsequently called "in the usual manner.") Having

FIG 1.

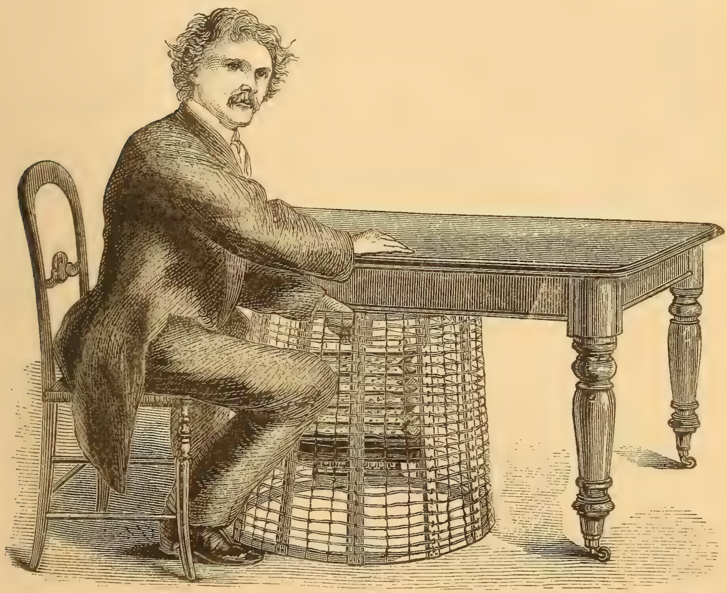


previously opened the bass key myself, and the cage being drawn from under the table so as just to allow the accordion to be passed in keys downwards, it was pushed back as close as Mr. Home's arm would permit, but without hiding his hand from those next to him (see Fig 2). Very soon the accordion was seen by those on each side to be waving about in a somewhat curious manner; then sounds came from it, and finally several notes were played in succession. Whilst this was going on, my assistant got under the table, and reported that the accordion was expanding and contracting; at the same time it was seen that Mr. Home's hand which held it was quite still, his other hand resting on the table.

Presently the accordion was seen by those on either side of Mr. Home to move about, oscillating and going round and round the cage, and playing at the same time. Dr. A. B. now looked under the table, and said that Mr. Home's hand appeared quite still whilst the accordion was moving about emitting distinct sounds.

Mr. Home still holding the accordion in the usual manner in the cage, his feet being held by those next him,

Fig. 2.



and his other hand resting on the table, we heard distinct and separate notes sounded in succession, and then a simple air was played. As such a result could only have been produced by the various keys of the instrument being acted upon in harmonious succession, this was considered by those present to be a crucial experiment. But the sequel was still more striking, for Mr. Home then actually let go the accordion, removed his hand quite out of the cage, and placed it in the hand of the person next to him, the instrument then continuing to play whilst no one was touching it.

I was now desirous of trying what would be the effect of passing the battery current round the insulated wire of the cage, and my assistant accordingly made the connection with the wires from the two Grove's cells. Mr. Home again held the instrument inside the cage in the same manner as before, when it immediately sounded and moved about vigorously. But whether the electric current passing round the cage assisted the manifestion of force inside, it is impossible to say.

The accordion was now again taken without any visible touch from Mr. Home's hand, which he removed from it entirely; I and two of the others present not only seeing his released hand, but the accordion also floating about with no visible support inside the cage. This was repeated a second time, after a short interval. Mr. Home presently re-inserted his hand in the cage and again took hold of the accordion. It then commenced to play, at first chords and runs, and afterwards a well-known sweet and plaintive melody, which it executed perfectly in a very beautiful manner. Whilst this tune was being played, I took hold of Mr. Home's arm, below the elbow, and gently slid my hand down it until I touched the top of the accordion. He was not moving a muscle. His other hand was on the table, visible to all, and his feet were under the feet of those next to him.

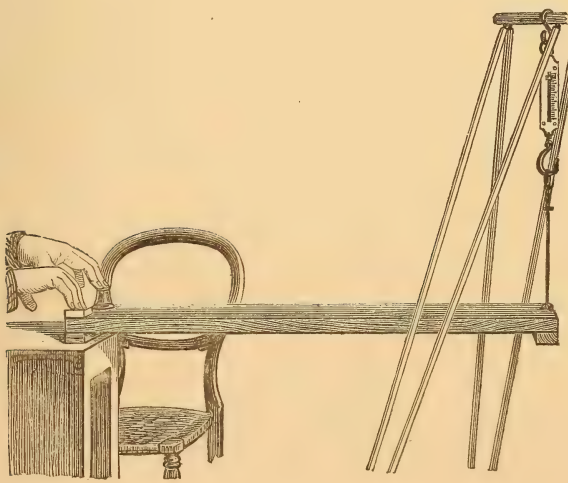
Having met with such striking results in the experiments with the accordion in the cage, we turned to the balance apparatus already described. Mr. Home placed the tips of his fingers lightly on the extreme end of the mahogany board which was resting on the support, whilst Dr. A. B. and myself sat, one on each side of it, watching for any effect which might be produced. Almost immediately the pointer of the balance was seen to descend. After a few seconds it rose again. This movement was repeated several times, as if by successive waves of the Psychic Force. The end of the board was observed to oscillate slowly up and down during the time.

Mr. Home now of his own accord took a small hand-bell and a little card match-box, which happened to be near, and placed one under each hand, to satisfy us, as he said, that he was not producing the downward pressure (see Fig. 3). The very slow oscillation of the spring balance became more marked, and Dr. A. B., on watching the index, said that he saw it descend to $6\frac{1}{2}$ lbs. The normal weight of the board as so suspended being 3 lbs., the additional downward pull was therefore $3\frac{1}{2}$ lbs. On looking immediately afterwards at the automatic register, we saw that the index had at one time descended as low as 9 lbs., showing a maximum pull of 6 lbs.

In order to see whether it was possible to produce much effect on the spring balance by pressure at the place where Mr. Home's fingers had been, I stepped upon the table and stood on one foot at the end of the board. Dr. A. B., who was observing the index of the balance, said that the whole weight of my body (140 lbs.) so applied only sunk the index $1\frac{1}{2}$ lbs., or 2 lbs. when I jerked up and down. Mr. Home had been sitting in a low easy-chair, and could not, therefore, had he tried his utmost, have exerted any material influence on these results. I need scarcely add that his feet as well as his hands were closely watched by all in the room.

This experiment to me appears, if possible, more striking than the one with the accordion. As will be seen on referring to the cut (Fig. 3), the board was arranged perfectly horizontally, and it was particularly noticed that Mr. Home's fingers were not at any time advanced more than

Fig. 3.



$1\frac{1}{2}$ inches from the extreme end, as shown by a pencil-mark, which, with Dr. A. B.'s acquiescence, I made at the time. Now, the wooden foot being also $1\frac{1}{2}$ inches wide, and resting flat on the table, it is evident that no amount of pressure exerted within this space of $1\frac{1}{2}$ inches could produce any action on the balance. Again, it is also evident that when the end furthest from Mr. Home sank, the board would turn on the further edge of this foot as on a fulcrum. The arrangement was consequently that of a see-saw, 36 inches in length, the fulcrum being $1\frac{1}{2}$ inches from one end; were he therefore to have exerted a downward pressure, it would have been in opposition to the force which was causing the other end of the board to move down.

The slight downward pressure shown by the balance when I stood on the board was owing probably to my foot extending beyond this fulcrum.

I have now given a plain unvarnished statement of the facts from copious notes written at the time the occurrences were taking place, and copied out in full immediately after. Indeed, it would be fatal to the object I have in view—that of urging the scientific investigation of these phenomena—were I to exaggerate ever so little; for although to my readers Dr. A. B. is at present represented by incorporeal initials, to me the letters represent a power in the scientific world that would certainly convict me if I were to prove an untrustworthy narrator.

I confess I am surprised and pained at the timidity or apathy shown by scientific men in reference to this subject. Some little time ago, when an opportunity was first presented to me of examining into the subject, I invited the co-operation of some scientific friends in a systematic investigation; but I soon found out that to obtain a scientific committee for the investigation of this class of facts was out of the question, and that I must be content to rely on my own endeavours, aided by the co-operation from time to time of the few scientific and learned friends who were willing to join in the inquiry. I still feel that it would be better were such a committee of known men to be formed, who would meet Mr. Home in a fair and unbiassed manner, and I would gladly assist in its formation; but the difficulties in the way are great.

A committee of scientific men met Mr. Home some months ago at St. Petersburg. They had one meeting only, which was attended with negative results; and on the strength of this they published a report highly unfavourable to Mr. Home. The explanation of this failure, *which is all they have accused him of*, appears to me quite simple. Whatever the nature of Mr. Home's power, it is very variable, and at times entirely absent. It is obvious that the Russian experiment was tried when this force was at a minimum. The same thing has frequently happened within my own experience. A party of scientific men met Mr. Home at my house, and the results were as negative as those at St. Petersburg. Instead, however, of throwing up the inquiry, we patiently repeated the trial a second and a third time, when we met with results which were positive.

These conclusions have not been arrived at hastily or on insufficient evidence. Although space will allow only the publication of the details of one trial, it must be clearly understood that for some time past I have been making similar experiments and with like results. The meeting on the occasion here described was for the purpose of confirming previous observations by the application of crucial tests, with carefully arranged apparatus, and in the presence of irreproachable witnesses.

Respecting the cause of these phenomena, the nature of the force to which, to avoid periphrasis, I have ventured to give the name of *Psychic*, and the correlation existing between that and the other forces of nature, it would be wrong to hazard the most vague hypothesis. Indeed, in enquiries connected so intimately with rare physiological and psychological conditions, it is the duty of the enquirer to abstain altogether from framing theories until he has accumulated a sufficient number of facts to form a substantial basis upon which to reason. In the presence of strange phenomena as yet unexplored and unexplained following each other in such rapid succession, I confess it is difficult to avoid clothing their record in language of a sensational character. But to be successful an enquiry of this kind must be undertaken by the philosopher without prejudice and without sentiment. Romantic and superstitious ideas should be entirely banished, and the steps of his investigation should be guided by intellect as cold and passionless as the instruments he uses. Having once satisfied himself that he is on the track of a new truth, that single object should animate him to pursue it, without regarding whether the facts which occur before his eyes are "naturally possible or impossible."

Since this article was in type, the Author has been favoured with the following letters from Dr. Huggins and Mr. Serjeant Cox—the Dr. A. B. and Serjeant C. D. therein referred to:—

Upper Tulse Hill, S.W.,
June 9, 1871.

Dear Mr. Crookes,—Your proof appears to me to contain a correct statement of what took place in my presence at your house. My position at the table did not permit me to be a witness to the withdrawal of Mr. Home's hand from the accordion, but such was stated to be the case at the time by yourself and by the person sitting on the other side of Mr. Home.

The experiments appear to me to show the importance of further investigation, but I wish it to be understood that I express no opinion as to the cause of the phenomena which took place. Yours very truly,

WILLIAM HUGGINS.

WM. CROOKES, Esq., F.R.S.

36, Russell Square,
June 8, 1871.

MY DEAR SIR,—Having been present, for the purpose of scrutiny, at the trial of the experiments reported in this paper, I readily bear my testimony to the perfect accuracy of your description of them, and to the care and caution with which the various crucial tests were applied.

The results appear to me conclusively to establish the important fact, that there is a force proceeding from the nerve-system capable of imparting motion and weight to solid bodies within the sphere of its influence.

I noticed that the force was exhibited in tremulous pulsations, and not in the form of steady continuous pressure, the indicator moving and falling incessantly throughout the experiment. This fact seems to me of great significance as tending to confirm the opinion that assigns its source to the nerve organisation, and it goes far to establish Dr. Richardson's important discovery of a nerve atmosphere of various intensity enveloping the human structure.

Your experiments completely confirm the conclusion at which the Investigation Committee of the Dialectical Society arrived, after more than forty meetings for trial and test.

Allow me to add that I can find no evidence even tending to prove that this force is other than a force proceeding from, or directly dependent upon, the human organisation, and therefore, like all other forces of nature, wholly within the province of that strictly scientific investigation to which you have been the first to subject it.

Psychology is a branch of science as yet almost entirely unexplored, and to the neglect of it is probably to be attributed the seemingly strange fact that the existence of this nerve-force should have so long remained untested, unexamined, and almost unrecognised.

Now that it is proved by mechanical tests to be a fact in nature (and if a fact, it is impossible to exaggerate its importance to physiology and the light it must throw upon the obscure laws of life, of mind, and the science of medicine) it cannot fail to command the immediate and most earnest examination and discussion by physiologists and by all who take an interest in that knowledge of "man," which has been truly termed "the noblest study of mankind." To avoid the appearance of any foregone conclusion, I would recommend the adoption of some appropriate name, and I venture to suggest that the force be termed the *Psychic Force;* the persons in whom it is manifested in extraordinary power *Psychics*; and the science relating to it *Psychism*, as being a branch of *Psychology*.

'Permit me, also, to propose the early formation of a *Psychological Society*, purposely for the promotion of the study by means of experiment, papers, and discussion, of that hitherto neglected Science.—I am, &c.,

EDWD. WM. COX.

To W. CROOKES, Esq., F.R.S.

VI. MOLECULES, ULTIMATES, ATOMS, AND WAVES.

By MUNGO PONTON, F.R.S.E.

(Continued from p. 176).

PART II.

THE better to understand this subject it is well to have before the mind some definite conceptions respecting the luminiferous ether and its waves. The phenomena of heat, light, actinism, and fluorescence render it needful to assume the existence of an infinite ocean of ether, in which all ponderable bodies subsist. Each particle of this ethereal medium must be supposed to have a normal position in space from which it never departs, beyond the minute distance involved in a vibration. The ethereal particles must also be supposed to repel each other with an enormous energy, which, when they are disturbed, restores them in an inconceivably short space of time to their points of rest. Nevertheless, as this energy must act on every particle equally in all directions, any one or more of them may be moved from their points of rest by the application of a very slight extraneous impulse, so that the medium is perfectly elastic. Minute waves of various degrees of length are supposed to be propagated through this elastic ether, when any of its particles are disturbed. These waves manifest their existence in the phenomena of heat, light, actinism, and fluorescence —the longer waves in heat, the shorter in actinism and fluorescence, the intermediate in light.

According to the most recent determinations, the speed with which these waves traverse the ether is 185,000 miles in a second, or nearly 11,721-millionths of an inch in the billionth of a second—this last being the most convenient form in which to state the speed.

Great labour and skill have been bestowed on the measurement of the lengths of those minute waves. Fraünhofer, who first discovered the fixed lines which bear his name, measured the wave-lengths corresponding to the lines which he named B, C, D, E, F, and H, leaving behind him two sets of measurements, differing slightly from each other. More recent observations with more delicate appliances than were at his command, have shown the lines D, E, and H to be double. Professor Ångström of Upsal, availing himself of the best instrumental means that could be obtained, has re-measured the wave-lengths of all the principal lines from A to the more refracted H, inclusive. The results he has published along with an elaborate atlas of the spectrum, in which the lines are laid down according to their wave-lengths. The spectrum represented in this atlas is the diffracted or normal solar spectrum, obtained from a system of very fine equidistant lines. In all spectra produced by prisms the fixed lines are displaced relatively to each other, so that their true wave-lengths cannot be determined from any such spectra. This displacement is termed the irrationality of the spectrum—a phenomenon discussed at large in a paper inserted in the "Philosophical Magazine" for 1860, pp. 165, 263, and 364. This displacement is different in every different medium, and even in different sorts of glass; while it is more or less affected also by the temperature of the medium. Thus the fixed lines will have different positions relatively to each other in different spectroscopes and at different temperatures, while the greater the number of prisms the greater will be this source of error—a circumstance which has been rather overlooked in recent observations on spectrum analysis.

A plan of a portion of the solar spectrum has also been published by M. Kirchhoff, and of the remainder by Messrs. Hofmann, Ångström, and Thalén—all drawn to the same scale—in which the lines are laid down as they appear to the eye when viewed through the spectroscope. But from the circumstance above explained, these positions are true only for the particular instruments employed in the observations. The positions given in this plan are also very diverse from those laid down in M. Ångström's atlas, which is constructed on a totally different principle, the lines being represented, not in the positions in which they appear to the eye, but in positions corresponding to their wave-lengths in ten-millionth parts of a millimetre. It would be desirable that a fresh plan were constructed combining both principles. The plan and the atlas both give the names of the vapours and gases by which the lines are supposed to be produced, in so far as these have been ascertained; but there is great difficulty in identifying any of the lines in the atlas with those on the plan, except in the case of the most conspicuous lines.

In 1859, when only the measures of Fraünhofer were known, it was shown in a paper inserted in the "Philosophical Magazine" for 1860, p. 437, that, assuming those measures to be approximately correct, it is probable that the wave-lengths of the principal lines are so connected by a law as to render their observed values a check one upon another. It has accordingly become interesting to ascertain how far this probability is confirmed by the more recent observations of M. Ångström. On investigation it is found that the measurements of the Swedish philosopher do follow such a law, but one differing from that which appeared deducible from those of Fraünhofer. A careful analysis of M. Ångström's measurements has proved the wave-lengths to be so mutually related that from the one corresponding to the more refrangible E, all the others may be accurately calculated by simple formulæ; while of the seven equations by which those values may be determined, the sum of the first three is equal to that of the remaining four, thus showing that all the wave-lengths are inter-dependent, each one upon the whole. These curious relations have been already enunciated in a memorandum added to a work recently published.* Craving reference to that memorandum for the formulæ by which they are expressed, it will suffice here to give the general result, by exhibiting in the following table the values of the wave-lengths, as calculated from those formulæ, and as given by M. Ångström from his observations,

* The Beginning; Its When and its How. Longmans and Co.

the unit being the ten-thousandth part of the millionth of a millimetre.*

	Observed.	Calculated.	Differences +.	Differences −.
A.	7,604,000	7,604,017	000,001,7	—
B.	6,867,100	6,867,098	—	000,000,2
C.	6,562,100	6,562,093	—	000,000,7
D.	5,895,130	5,895,117	—	000,001,3
E.	5,268,670	5,268,670	—	—
F.	4,860,740	4,860,747	000,000,7	—
G.	4,307,250	4,307,235	—	000,001,5
H.	3,933,000	3,932,995	—	000,000,5
			000,002,4	000,004,2

Thus the sum of the differences plus and minus is only 66-tenths of the thousandth of the millionth of a millimetre; while the greatest difference is only 17 of those decimal parts. This quantity would affect the position of the lines to an extent so minute as to be wholly inappreciable to the eye, either on the atlas of M. Ångström or on the map of M. Kirchhoff. The following are the calculated wave-lengths stated in billionths of an inch:—

A.	29,957,600	B.	27,054,360	C.	25,852,720
D.	23,225,300	E.	20,757,000	F.	19,149,930
G.	16,969,250	H.	15,494,850		

The extreme smallness of the differences between M. Ångström's measurements and the values calculated from the formulæ seems to establish both the accuracy of his observations and the truth of the relations which the formulæ express. The wave-lengths, as calculated from those formulæ, may accordingly be regarded as approximating very closely to the truth.

The relations thus shown to subsist among the wave-lengths, corresponding to the above eight principal fixed lines, will appear all the more remarkable when it is borne in mind that the waves themselves are due to the action of diverse substances. The A and B waves are produced by the action of atmospheric air; the C and F waves by hydrogen gas; the D wave by the vapour of sodium; the E, G, and H waves by the vapour of iron.

The marvel of the double coincidence, moreover,—the circumstance that not only are the other seven capable of being

* It is the longer of the two D waves, the shorter of the two E waves, and the shorter of the two H waves that are given in the table.

calculated from the wave-length of E alone, but that of the seven equations by which these are determined the sum of the first three is equal to that of the remaining four,—will be more fully appreciated by those who are able to estimate the vast amount of the probabilities against the existence of such a coincidence. It is also a curious, though accidental, coincidence that the wave-length of E expressed in ten-thousandths of the millionth of a millimetre is almost exactly equal to the tangent of an angle of 27° 47′, namely, 5268685—the difference, 0000015, lying much within the limits of probable errors of observation, which are more than sixty times greater.* This tangent might accordingly be assumed as the value of E without affecting the relations of the wave-lengths to each other.

Having thus obtained the wave-lengths of the principal lines, it is easy to calculate the periods of time which the wave motion takes to traverse each wave-length in parts of the billionth of a second. For, seeing the motion is propagated at the rate of 11,721 millionths of an inch in the billionth of a second, we have only to divide this number by each wave-length, as given in the last table, to obtain the times. The following table exhibits the results in parts of the billionth of a second :—

A. One 391st.	E. One 565th.
B. One 433rd.	F. One 612th.
C. One 453rd.	G. One 691st.
D. One 505th.	H. One 756th.

The wave-length being the space traversed by the wave motion travelling onwards in a right line, while each ethereal particle embraced in it is performing a single vibration in a transverse direction, the above times are also the periods of vibration of the individual particles involved in each wave. It remains to ascertain what amount of motion the particles perform during the above minute fragments of time.

By comparing the speed of sound in traversing the air with that of light in passing through the ether, we learn that the elastic energy, operating in the latter case, exceeds in intensity that of terrestrial gravitation, operating in the former case 1,137,156 millions of times—the forces being to each other as the squares of the speeds. Terrestrial gravity being capable of dragging a particle through 193 inches in a

* The wave-lengths of F, when stated in hundred thousand millionths of an inch, makes a still nearer approach to being equal to the versed *sine* of an angle of 36° 3′ 1″ = 1914962, which might be adopted as the value of the wave-length of F without affecting any of the calculations.

second, a force of the above number of times greater intensity would, in the same time, drag or drive it through nearly 219 billions of inches, or 219 billionths of an inch in the billionth of a second—the spaces being as the squares of the times.

The extreme distance to which a vibrating particle will depart from its point of rest will be that through which it may be dragged or driven by the returning force in the fourth part of the period of vibration. In the case of the wave A, it will be that through which it may be dragged or driven in the 1564th part of the billionth of a second. This gives the 11,169th part of the billionth of an inch for the departure of the particle from its point of rest. The length of the wave A being 29,956,800 billionths of an inch, this length is to the departure of each particle involved in that wave from its point of rest in the ratio of 334,600 millions to 1.

Seeing the spaces vary as the squares of the times, the ratio which the wave-lengths bear to the departure of the particles involved in them from their points of rest will be inversely proportional to the wave-lengths themselves. The shorter the wave the higher the ratio. The following table exhibits the ratio for the wave-lengths corresponding to the eight principal lines :—

A. 334,600	millions to	1.	E. 482,885	millions to	1.
B. 370,470	,,	1.	F. 523,390	,,	1.
C. 387,708	,,	1.	G. 590,647	,,	1.
D. 431,555	,,	1.	H. 646,881	,,	1.

A better notion of this immense excess of the length of the wave over the departure of the individual particles from their points of rest may be obtained by imagining both to be magnified a million of billions of times. The length of the A wave would then be nearly 473 millions of miles, while the departure of each particle involved in that wave from its point of rest would be only about 7½ feet. In the case of the H wave, its length would be about 245 millions of miles, while the departure of each particle involved in that wave from its point of rest would be only about 2 feet.

This great excess of the wave-length over the departure from the point of rest shows the latter motion to be rateably very much slower than the speed with which the motion of translation runs along the wave-lengths from particle to particle—in other words, than the velocity of light. Thus in the case of the A wave the particles move the 11,169th part of the billionth of an inch in the 1564th part of the billionth of a second. Supposing the motion to be uniform, this rate is only about a seventh of an inch in a second, during which period of time the wave motion travels 185,000 miles. The rate of motion of the particles is the same for all the waves, and the amazingly short period of time in which the vibration is accomplished results merely from the extreme minuteness of the motion performed.

The foregoing estimates apply to only that portion of the spectrum which is visible. But the thermal effects of the invisible ultra-red waves can be traced to a distance beyond A, which is equal to nearly five-eighths of the visible spectrum. As the wave-lengths increase by a geometrical progression, this distance would give for the longest wave-length whose existence can be ascertained nearly 4797 hundred millionths of an inch. Although this is only a rough approximation, it may for the present purpose be assumed as correct. Owing to the constitution of the ether, no particle can by a vibration be moved from its own point of rest so far as to become nearer to that of any other particle. There is thus a natural limit to the excursion of each individual particle—consequently to the length of the wave. Assuming the above to be the longest possible wave, the ratio which its length bears to the departure of the individual particles involved in it from their points of rest is 208,926 millions to 1; and about half this quantity, or, say, in round numbers, 100,000 millions to 1, will be the ratio which the longest wave-length will bear to the normal distance between the particles, the number of which in the longest wave will also be about 100,000 millions. This estimate may serve to convey some notion of the extreme smallness of the intervals between the particles of the luminiferous ether. What, then, must be the minuteness of the particles themselves?

From the nature of the wave motion and of the vibrations of the individual particles, the direction of which is across that in which the wave motion is propagated, it appears probable that, in any given line of propagation, or ray, the individual particles must depart wholly out of the line, so as to produce a complete interruption of its continuity, in order that the existence of the movement may be traceable by any physical effects;—in other words, the departure of the particles from their points of rest in all likelihood slightly exceeds their own radius. Could we, therefore, ascertain the probable length of the shortest wave whose existence can be traced by any physical effects, and the corresponding probable smallest departure of any particle from its point of rest, we might obtain a rough approximation to the probable diameter of the particles, which would be a little less than double that departure.

The late Professor W. A. Miller, of King's College, London, in an elaborate paper "On the Photographic Transparency of Various Bodies, and on the Photographic Effects of Metallic and other Spectra, obtained by means of the Electric Spark" ("Philosophical Transactions," 1862, p. 861), has shown how far the actinic effects of the spectrum, obtained from silver electrodes, can be traced beyond the line H. Dividing the portion of the spectrum between B and H into 16 equal parts, Professor Miller found that the greatest distance beyond H at which any photographic image could be produced with silver electrodes was 70·5 of these parts, or about 4·4 times the distance between B and H. This result could be obtained only when the light passed through a medium exerting little or no absorptive action on the most refrangible part of the spectrum, such as ice, water, white fluor spar, quartz, atmospheric air, hydrogen, carbonic oxide and carbonic acid gases. In all these media the actinic spectrum extended to the same limit beyond H. With zinc electrodes, however, an image was obtained at a point as far beyond H as 83 of the scale. This may be regarded as the limit of the actinic powers of the spectrum.

Professor Stokes, again, in a paper "On the Long Spectrum of Electric Light" ("Philosophical Transactions," 1862, p. 599) has shown that, with aluminium electrodes, a line can be traced a good way beyond this limit. What is remarkable, however, is that Professor Miller with aluminium electrodes could not obtain a photographic image at any point beyond 55 of his scale, much within the limit of the most refrangible zinc line. The very highly refrangible aluminium lines, discovered by Professor Stokes, render their existence manifest only by their effects on a strongly fluorescent salt of uranium, employed as a screen for their reception—quartz being used for the prism and lens. From a comparison of the diagrams given by Professor Stokes with those given by Professor Miller, it appears that the most highly refrangible wave whose existence is thus manifested by a bright line on the fluorescent screen is very nearly at 113 beyond H on Professor Miller's scale, or about 30 beyond the last zinc line.

No attempt appears to have been hitherto made to determine by actual measurement the wave-length corresponding to either the extreme zinc or the extreme aluminium line. Our only resource, therefore, is to make a rough estimate, founded on the supposition that the wave-lengths continue to follow a geometrical progression. Thus estimated in hundredths of the millionths of an inch, the shortest actinic wave would be about 71, and the shortest wave capable of exciting fluorescence about 23·4—almost exactly 128 times, or 7 octaves shorter than the length of the wave A.*

If this be the correct length of the extreme aluminium waves, it will show that the fluorescence which it produces is analogous to a resonance in the case of sound, where a vibrating musical string establishes sympathetic vibrations in another string 7 octaves below it in the musical scale. The very short wave in the ether produced by the aluminium electrodes is not itself visible, but by acting on the molecules of the uranium salt, or on the chemical ultimates composing those molecules, or, perhaps, on the atoms constituting the ultimate of the metal uranium, they establish in them vibrations synchronous with themselves. These very rapid vibrations give rise by sympathy to others of a lower rate, an octave or more below their own—passing, perhaps, through several successive octaves until there become established in the fluorescent substance vibrations which are synchronous with some of those in the ethereal waves of the visible spectrum. There are thus produced in the ether return waves, which are of a length sufficient to affect the optic nerve.

On the supposition that the extreme aluminium wave is exactly 128 times shorter than the wave A, the proportion which the length of the wave bears to the departure from their points of rest of the individual particles involved in it will be 128 times greater than it is in the case of the A wave. This would make the proportion nearly 43 billions to 1, while the probable number of ethereal particles included in the length of this wave would be about 488 millions.

To convey a more precise idea, let us again apply a magnifying power of a million of billion times. The length of the shortest wave would then be about 3,702,300 miles, while the departure of each particle from its point of rest would be only 0·00547 decimal parts of an inch.

We are now in a position to form a pretty accurate

* Might not these invisible ultra-violet waves be turned to some useful purpose? Might not the aluminium waves be used for secret telegraphy by night, these invisible rays alone being sent forth in parallel lines by means of a parabolic reflector, while at the receiving station they might be concentrated by another parabolic reflector into a focus on a highly fluorescent surface, by which they would be rendered visible? Might not also the invisible zinc rays be employed to photograph an object secretly in the dark, by directing them on the object, and receiving them into the camera through a quartz lens?

conception of a luminous wave as it exists in the free ether, if attention be confined to a single line of ethereal particles lying in the direction in which the wave motion is travelling. Take for example the wave F, one of those produced by hydrogen gas. Its length in hundredths of the millionth of an inch is about 1915, the probable number of ethereal particles embraced in this length is about 40,000 millions, all of which are more or less moving simultaneously in a direction transverse to that in which the wave is advancing. The foremost—that at the point of the wave—is just beginning to move. At a quarter of the length of the wave from this point, there is a particle which has performed the first quarter of its motion, and has departed to the farthest limit from its point of rest on one side. It has been in motion one 2448th part of the billionth of a second, and in that interval has travelled one 523,390th part of the millionth of the length of the wave, moving at the average rate of about one-seventh of an inch in a second. At the middle point of the wave is a particle which is passing through the line of propagation on its way to the other side of that line. It has been double of the above period of time in motion, and has travelled double the space. At three quarters of the wave-length from the front is a particle which has performed three-fourths of its movement. It has gone as far to the other side of the line of propagation as the limit of departure from its point of rest, having been in motion one 918th part of the billionth of a second. At the extreme rear of the wave is a particle which has just returned to its point of rest, having entirely completed its excursion in one 612th part of the billionth of a second. The particles intervening between each pair of the five above specified are, according to their position, in intermediate phases of their movement.

The perfect elasticity of the ethereal fluid leads to the inference that the rearmost particle, having regained its point of rest, will remain there until disturbed by a fresh impulse travelling along the same line of propagation. It will have delivered over to those in front of it the whole of the motive energy which it had acquired from the primary impulse. Owing, moreover, to the circumstance that all ponderable particles are in a continual state of rapid motion through the ether, it is improbable that any individual line of ethereal particles will be agitated twice in succession by one and the same ponderable particle, although it may be agitated by one of the same kind, and thus have transmitted along it a succession of waves of the same length. The waves, however, travelling along any single line of propagation may be separated from each other by long intervals. The greater or less rapidity with which they succeed one another, will merely give rise to a greater or less intensity of effect at the recipient surface; a greater or less brightness in the case of the luminous waves; a greater or less heating effect in the case of the thermal waves; a greater or less photographic effect in the case of the actinic waves; a greater or less amount of fluorescence in the case of those waves which stimulate that property into action. In the case of luminous waves, they might be separated by intervals of 18,500 miles, and yet produce continuous vision; but waves separated by intervals of only a mile would produce a very great amount of intensity. It is owing to this largeness of the interval between the waves travelling along any individual line of propagation, that rays from different sources are found to cross each other in all directions without interfering one with another.

(To be continued).

VII. A NEW MECHANICAL AGENT.

A JET OF SAND.

HOW to cut or carve, mechanically, hard substances, such as stone, glass, or hard metals, in an expeditious, accurate, and economical manner, has always engaged the attention of engineers. At the present time the rapidly increasing cost of manual labour makes improvements in this direction more needful. The discovery and utilisation of opaque crystallised carbon, cheaper than transparent diamonds, but perhaps equally durable, has gone far in this direction. Now, Mr. B. C. Tilghman, of Philadelphia, comes forward, and shows that a jet of quartz sand thrown against a block of solid corundum will bore a hole through it $1\frac{1}{2}$ inches in diameter, $1\frac{1}{2}$ inches deep, in 25 minutes, and this with a velocity obtainable by the use of steam as a propelling power, at a pressure of 300 pounds per square inch—a remarkable result, when we consider that corundum is next to and but little inferior to the diamond in hardness.

At the meeting of the Franklin Institute, held February 15th, 1871, the resident secretary, Dr. W. H. Wahl, introduced this invention, illustrating his description of it by practically cutting or depolishing the surface of a plate of glass by a sand blast of very moderate intensity. Various examples of hard substances cut, depolished, and carved into shape, were displayed. In the discussion which followed the presentation of this very remarkable discovery, Mr. Robert Briggs, in his interesting remarks on the subject, took occasion to say that it had been long remarked that window glass, exposed to the wind-driven sand, near the sea-shore, soon loses its polish, and cited some other well-known examples of the erosion of surface when exposed to a continued stream of moving particles. When we think of the many such examples, and consider that engineers have had continually to make provision against this well-known cutting effect, it seems surprising that it should not have been turned to some good account before this.

The following description of the process is taken from a paper by Mr. Coleman Sellers communicated to the "Journal of the Franklin Institute," advance sheets of which we owe to the courtesy of the editor, Dr. W. H. Wahl.

Mr. Tilghman's attention seems first to have been directed towards cutting stone or hard metal by a jet of sand impelled by steam escaping under high pressure. His early experiments were with very high pressure, but as he progressed in the knowledge of the results obtainable with various velocities, a great use for this process seemed to develope itself in sand driven by moderate air blasts, and applied to grinding or depolishing glass for ornamental purposes.

For grinding glass he uses a common rotary fan, 30 inches in diameter, making about 1500 revolutions per minute, which gives a blast of air of the pressure of about 4 inches of water, through a vertical tube, 2 feet high by 60 inches long, and 1 inch wide.

Into the top of this tube the sand is fed, and falling into the air current and acquiring velocity from it, is dashed down against the sheets of glass, which are slowly moved across, about 1 inch below the end of the tube. About 10 or 15 seconds' exposure to the sand blast is sufficient to completely grind or depolish the surface of ordinary glass ; so that sheets of it carried on endless belts may be passed under this 1-inch wide sand shower at the rate of 5 inches forward movement per minute. In the machine in use for this purpose the spent sand is re-conveyed to the upper hopper by elevators, and the dust made by the sand blast (which might otherwise be a source of annoyance to the workman) is drawn back into the fan, and thence passes with the wind into the blast tun, and again mingles with the shower of sand upon the glass.

By covering parts of the glass surface by a stencil or pattern of any tough or elastic material, such as paper, lace, caoutchouc, or oil paint, designs of any kind may be engraved.

There is a kind of coloured glass made by having a thin stratum of coloured glass melted or "flashed" on one side of an ordinary sheet of clear glass. If a stencil of sufficient toughness is placed on the coloured side, and exposed to the sand blast, the pattern can be cut through the coloured stratum in from about 4 to 20 minutes, according to its thickness.

The theoretical velocity of a current of air of the pressure of four inches of water, he calculates, is (neglecting friction) about 135 feet per second; the actual velocity of the sand is doubtless much less.

If a current of air of less velocity is used, say about 1 inch of water, very delicate materials, such as the green leaves of the fern, will resist a stream of fine sand long enough to allow their outlines to be engraved on glass. By graduating the time of exposure with sufficient nicety, so as to allow the thin parts of the leaves to be partly cut through by the sand, while the thicker central ribs and their branches still resist, the effect of a shaded engraving may be produced.

The grinding of such a hard substance as glass by an agent which is resisted by such a fragile material as a green leaf, seems at first rather singular. The probable explanation is, that each grain of sand which strikes with its sharp angle on the glass pulverises an infinitesimal portion, which is blown away as dust, while the grains which strike the leaf rebound from its soft elastic surface.

The film of bichromatised gelatine used as a photographic negative may be sufficiently thick to allow a picture to be engraved on glass by fine sand, driven by a gentle blast of air.

For cutting stone the inventor uses steam as the impelling jet; the higher the pressure, the greater is the velocity imparted to the sand, and the more rapid its cutting effect.

In using steam of about 100 pounds pressure, the sand is introduced by a central iron tube, about 3-16th inch bore, while the steam is made to issue from an annular passage surrounding the sand tube.

A certain amount of suction of air is thus produced, which draws the sand through the sand tube into the steam jet, and both are then driven together through a tube about 6 inches long, in which the steam imparts its velocity to the sand, and finally strikes on the stone, which is held about an inch distant from the end of tube.

At the spot struck a red light is visible, as if the stone was red-hot, though really it is below 212° F. The light is probably caused by the breaking up of the crystals of the sand and stone.

The cutting effect is greatest when free escape is allowed for the spent sand and steam. In making a hole of a diameter but slightly greater than that of the steam jet, the rebounding steam and sand greatly interfere with and lessen the efficiency of the jet.

Under favourable conditions, using steam which was estimated as equal to about 1½ horse-power, at a pressure of about 125 pounds, the cutting effect per minute was about 1½ cubic inches of granite, or 3 cubic inches of marble, or 10 cubic inches of soft brown sandstone.

By means of flexible or jointed connecting tubes, the blast-pipe is made movable in any direction: grooves and mouldings of almost any shape can thus be made; or by means of stencil plates, letters or ornaments can be cut either in relief or intaglio with great rapidity in the hardest stone.

At a high velocity, quartz sand will cut substances much harder than itself, as before stated. With a steam jet of 300 pounds pressure, a hole 1½ inches in diameter was cut through a piece of corundum, 1½ inches thick, in 25 minutes.

A hole 1 inch long and ¼ inch wide was cut through a hard steel file ¼ inch thick, in 10 minutes, with a jet of 100 pounds steam.

A stream of small lead shot, driven by 50 pounds steam, wore a small hole in a piece of hard quartz; the shot were found to be only very slightly flattened by the blow, showing their velocity to have been moderate.

Among the curious examples of glass cut by this sand blast was shown a piece of ordinary window-glass, which, having been partially protected by a covering of wire gauze, had been cut entirely through, thus producing a glass sieve, with openings of about 1-12th of an inch, the intervening glass meshes being only 1-16th of an inch wide. This seems to have been produced more as a curiosity than for any practical purpose. Should such a sheet of perforated glass be required, it is questionable if it could be produced from a solid sheet by any other method.

A microscopic examination of the sheet glass depolished by this process shows a succession of pits formed by the blows of the impinging grains of sand, and looks more uniform than do surfaces ground by any rubbing process.

This steam sand jet has already been introduced to clean cast-iron hollow ware previous to tinning the interior. Heretofore the interior surface has been turned, it having been found necessary to remove a thin shaving in a lathe to obtain a clean surface. The surface is cleaned more rapidly by the sand blast, and even more perfectly, because it penetrates into any holes or depressions which the turning tool could not reach. It is also probable that the sand striking the particles of plumbago, which separate the particles of metallic iron in ordinary grey cast-iron, will remove them, and thus expose a continuous metallic surface to take the tin.

In this relation Mr. Sellers notes that about twenty-five years ago, some experiments were made in Cincinnati, at the establishment of Mr. Miles Greenwood, by his brother, Mr. George Escol Sellers, with a view to making tinned hollow ware of ordinary grey iron. He made a machine for scouring the inside of the pots and kettles with sand and water; afterwards the still wet scoured surfaces passed into the chloride of zinc solution, and thence into the molten metal, and were uniformly tinned. For some reason, the process was not continued, and now it is only recorded as an abandoned invention, never before made public. The wet sand grinding could not, in this case, have been so efficient as Mr. Tilghman's sand blast. To speculate on the various uses to which this process may be applied, would not serve any good end, and would take up too much space. With this discovery we can hardly help recurring to the works of the ancients, and wondering if some such process could have aided the workers in the stone age, or could have been used in carving the Egyptian hieroglyphics. It has been noted by those familiar with the cutting or dressing of stone, that some materials, such as granite, are very much injured, or "stunned," by the blows of the cutting tool, and after being hand dressed a thickness of perhaps from $\frac{1}{8}$th inch to $\frac{1}{4}$th inch has to be ground away, to produce a solid uniform surface. By this sand cutting process the surface is not injured, is not "stunned," and is ready for polishing at once.

One curious fact connected with its use is, that when a surface to be cut in intaglio or otherwise is partially protected by templates of metal, these templates curl up under the blows of the sand, so that paper patterns are really more durable than patterns cut from brass. Sheet steel, cut into shape and then hardened, will also curl up under the blows of the fine particles of sand, unless protected by sheets of yielding material. Fine lace will protect glass during the depolishing process, and leave its designs in polished lines on a ground surface.

At a recent meeting of the Photographic Society of Philadelphia, Mr. Tilghman submitted some specimens of engraving on glass, prepared by this beautiful sand process. A bichromatised gelatine negative is taken on glass from an engraving. This is then subjected to a stream of sand under a pressure of 1 to 4 inches of water. The gelatine film protects the glass, the parts not covered by it being cut by the sand. The process is complete in from three to ten minutes. The finest specimens are produced by using fine sifted sand at about 1 inch pressure, and a longer time of exposure.

VIII. BRITISH SERVICE ORDNANCE, 1870, 1871.

By S. P. Oliver, Lieut. Royal Artillery.

THE sixth annual "List of Service Ordnance and Ammunition" for 1871 has just been published by the Royal Artillery Institution, and by comparing it with the list of June, 1870, we are able to note the progress and change in the ever steadily advancing history of British gunnery.

The following abstract of changes effected during the last twelve months therefore may be worth noting.

To the superficial observer, indeed, the two lists mentioned above would appear almost identical, but the initiated soon detects important changes.

To commence with the rifled guns. In June, 1870, we had, and still retain in our service, two natures of 7-inch breech-loading screw polygrooved guns (the old Armstrong 100-pounder), two 40-pounders, three 20-pounders, one 9-pounder, and one 6-pounder, all Armstrong guns; the only other breech-loading guns in the service were, and now are, the 64-pounder and 40-pounder wedge guns. As regards all descriptions of breech-loading guns there has been no change, and this section of the list for 1870 remains unaltered in that for 1871.

It is in the section relating to muzzle-loading rifled guns alone that any change is to be noted. Of this description there are two bronze guns, the 9-pounder and the 7-pounder, the latter grooved on the French principle, and the former on the same modified. The first gun was intended for Indian service only, and so marked in No. 5 list, but the note to this effect has been removed in this year's list. We now come to the first change, viz., in the cast-iron guns with an inner barrel of coiled wrought-iron. In No. 5 list there was only one in the service, viz., a 64-pounder of 71 cwt. (the old 8-inch smooth-bore altered). This year's list gives two more 64-pounders as added to our service ordnance, viz., an altered 68-pounder for Indian service, and a bored-up 32-pounder, both land service guns; all the 64-pounders are plain grooved, and also take the 32-pounder smooth bore ammunition.

The great change of the year, however, is undoubtedly the next item on the list; not as regards diameter of bore, but as regards weight and strength, an immense stride has been made. The list of wrought-iron guns last year was headed by the 12-inch gun of 25 tons weight, and perhaps more popularly known as the 600-pounder. The 12-inch is now preceded by the apparently reduced diameter of 11·6 inch, but under the heading of nominal weight and in the column which represents tons appear the ominous figures 35. These figures may well cause dismay to those gunners who, knowing the difficulty they have experienced in mounting 25-ton or even 12-ton guns, look forward to the questionable pleasure of having to place a "*Woolwich Infant*" in position; and considering that whole pamphlets have been written on the mere mounting of 12-ton guns at Malta,* we may expect large volumes on the handling of this infant monstrosity. Fortunately only one appeared at birth, although it is possible that Mr. Cardwell ardently desired twins, but the Woolwich factory is again in travail, and we may shortly expect an addition or two to the wrought-iron family. This latest gun, the gun of the period, is at present intended for sea service only, and will not improbably be placed on board the *Devastation* now building at Portsmouth.

After the 12-inch gun another 25-ton gun; the 11-inch is an addition to the list; the 10-inch, 9-inch, 8-inch, and two 7-inch guns remain in *statu quo*. All the above wrought-iron guns are rifled on the Woolwich system. To the three sea service 64-pounder shunt guns which succeed next in order, there is added this year a fourth plain grooved for land service. The remaining additions to last year's list of

* Proceedings of the Royal Artillery Institution.

wrought-iron guns consist of a 40-pounder, a 25-pounder, a 16-pounder, and two 9-pounders; the three first of which are not, indeed, yet *officially* introduced into the service, but are practically so. The steel 7-pounder gun completes the tale of the rifled ordnance at present in our service.

A short summary of the well known old smooth bore ordnance still in use will complete our brief survey of our modern British cannon.

Of bronze we have the following natures, viz., 12-pounder, 9-pounder, 6-pounder, and two 3-pounder guns; together with 32-pounder, 24-pounder, 12-pounder, and 4·4 inch howitzers; also two bronze mortars, the *Royal* and *Cœhorn*.

The after-mentioned pieces are all cast-iron. Of these there are still four carronades not yet obsolete, the 68-pounder, 32-pounder, 24-pounder, and 12-pounder. Of guns, we have two 68-pounders, of 112 and 95 cwt. respectively, one 10-inch Dundas shell gun, and three 8-inch (two of Millar's and one of Dundas's construction); the 56-pounder and the two patterns of 42-pounders are only retained in the service until replaced by rifled guns.

Next in order come the 32-pounder guns, of which there are more varied patterns than of any other gun in the service, there being no fewer than eleven descriptions, varying in weight from 63 cwt. to 25 cwt.; comprising one Millar, two Dundas, three Blomefield's (two of which are bored-up 24-pounders), three of Monk's (A, B, and C), one Dickson, and one Congreve; certainly a very miscellaneous but useful array of weapons: their diversity of size and pattern originally arose from the varied classes of vessels in the navy for which they were introduced or adapted from time to time.

Three 24-pounders, and four 18-pounders, two 12-pounders, and three 9-pounders, finish the list of cast-iron guns. The three howitzers of 10-inch, 8-inch, and 5½-inch, need no comment beyond noticing that the last will be replaced by a rifled gun. There now remain only the mortars to be disposed of; and here we may remark that Mr. Mallet is confident of producing a 36-inch mortar weighing 35 tons, which will meet the requirements and tests which his last mortars, built under the auspices of Lord Palmerston, failed to stand. Of our present mortars the 13-inch sea service is by far the most formidable; it is a grand weapon. Besides it we have two other 13-inch mortars. Those who wish to see what a heavy mortar battery is like should visit the fine battery at Puckpool, not far from Ryde, in the Isle of Wight. This apparently (from the sea) innocuous looking earthwork, hides one of the most terribly destructive batteries in the world. After the 13-inch mortars we have two 10-inch and one 8-inch mortar. Such, according to the authority we have quoted, is the "*story of the guns*" at present, and we venture to hope that no other military state could show a list to surpass our actual supply of guns. The battle of Dorking will not be fought this year anyhow.

To conclude our notice of the sheet in question, we may mention another innovation in the right direction; this is a short table giving the muzzle velocities calculated from the latest experiments with our rifled breech and muzzle-loading ordnance, from which we may quote one illustrative example. For instance, we learn that the 600-pounder Palliser large capacity shell leaves the muzzle of the 12-inch 25-ton gun with a velocity of 1168 feet per second, when the full battering charge of 67 lbs. of the "*brutal*" R.L.G. powder is fired; on the other hand, when the milder pebble powder is used, a charge of 85 lbs. can be fired, giving the same projectile a muzzle velocity of 1300 feet per second. The greatest muzzle velocity given is that of the 7-inch gun of 6½ tons, the 115 lbs. shell of which is projected by 30 lbs. of pebble powder, with the tremendous velocity of 1525 feet per second.

It may also be remarked that this year's list contains besides the "Marks on cast-iron ordnance referring to their vents," &c., some additional useful memoranda as to the "Chief points to be remembered in considering whether a given fuze will suit any shell." In these days, when varieties of shell and fuzes are multiplied daily, and when many varied descriptions of each are found in a single battery, these last instructions should be conspicuously placed in every shell-room.

NOTICES OF BOOKS.

A Memoir on the Indian Surveys. By CLEMENTS B. MARKHAM. Printed by order of Her Majesty's Secretary of State for India in Council. W. H. Allen and Co., 13, Waterloo Place. London, 1871.

THE object of this work is to furnish a general view of all the surveying and other geographical operations in India from their commencement. It is divided into seventeen sections, comprising the Indian Marine Surveys; route surveys; the trigonometrical, topographical, geological, and archæological surveys; meteorological and tidal observations; astronomical observations; and the physical geography of India. It would be impossible to give a comprehensive review of a volume embodying so much information, hitherto contained only in official records, or scattered amongst the pages of almost innumerable publications, within the scope of which we are limited; a few remarks only to show the nature of the book before us must therefore suffice. The records from which information has been gleaned date back over 250 years. "The Surveys of India began along the coasts, and the sailors preceded the shore-going surveyors by nearly 200 years. Before India could be measured it was necessary to get there; and the history of Indian Surveying takes us back to the day when James Lancaster's fleet of four ships and a victualler got under way from Torbay, on the 2nd of May, 1601." Richard Hakluyt, it appears, was appointed historiographer of the East Indies in the same year; to him was entrusted the custody of all the journals of the East India voyages, and he gave lectures relating to them to the students at Oxford. The records of early voyages and of the surveys made by the Bombay Marine reached from 1601 to 1830. These latter were continued by the Indian Navy from 1832 to 1862, when it was arranged that all further surveys that might be required should be conducted by the Royal Navy.

The land survey and mapping of British India have advanced with the acquisitions of territory; they were commenced when the first battles were fought and the first provinces gained. Rennell, the father of Indian geography, served under Clive, the conqueror of Plassy. At that time all existing knowledge of India, derived from routes of solitary travellers and rough charts of the coasts, had just been collected and utilised by the great French geographer, D'Anville. His map of India appeared five years before the date of the battle of Plassy; and eight years afterwards Rennell was at work in the newly acquired territory of Bengal and Behar, laying the foundation for the construction of a map which was destined to succeed the admirable work of D'Anville. Brief accounts of the Trigonometrical, Topographical, Revenue, and Geological Surveys of India appeared in the number of this Journal for October, 1870, and we shall not, therefore, further refer to those subjects beyond stating that they are given at much greater length and completeness in the volume now before us.

The Archæological Survey of India was for a long time carried on by independent antiquarians, who published their labours in the journals of different Indian societies, and they appear to have acted without any system and but little communication with one another. General Cunningham published his views on archæological investigation in 1848, but it was not until 1860 that the Government of India instituted an archæological survey, with the object of preserving ancient monuments, rendering them easy of access, obtaining correct copies of inscriptions and pieces of sculpture, and thus facilitating the studies of future antiquarians and historians.

Meteorological observations appear to have been taken in India so far back as the year 1785; and the earliest recorded tidal observations were taken at the Kidderpore Dock-head, on the Hooghly, by Mr. James Kyd, in 1806. Astronomical observations were commenced by the Government at Madras, in 1787, but the results of similar observations made by native astronomers so long ago as the 5th century, are preserved to the present day.

The physical geography of India is to a great extent written in the histories of travellers, and Mr. Markham has extracted from the works of numerous authors some very interesting information on this subject. Indeed, throughout the entire volume dry facts are interspersed with interesting historical information; and although its title would not probably commend itself to any but scientific readers, there is nevertheless much in it which will well repay others for the trouble of reading it, whilst the style in which it is written is easy and fluent.

Classical and Prehistoric Influences upon British History; our Philanthropy from of Old, our ever struggling Past, and our Future. By SAXE BANNISTER, M.A., formerly Attorney-General of New South Wales. Second Edition. London: Longmans, 1871.

MR. BANNISTER has a theory to uphold, and a very good theory it is, viz., that our peaceableness, our consideration for our colonies, and our elevation of the native nations which impinge upon our possessions rather than a desire to dispossess or exterminate them, as is openly proclaimed by one class of politicians as a decree of inevitable Fate, will tend to the prolonged existence of the nation as a ruling power, whereas the opposite course will bring upon us a destruction as complete and probably as speedy as that of the Roman Empire, produced as it was by the wanton conquests and tyrannical oppression of conquered nations. This theory plainly stated and supported by the simple arguments that naturally arise, would no doubt find a response in the mind of every thoughtful man whose passions were not inflamed by preconceived notions or interested motives. But we hardly think that the pamphlet of Mr. Bannister, in which he endeavours to show that this theory was the ruling principle of the ancient Britons or other inhabitants of this land before the Roman conquests, and that the reverse was the guiding motive of the Romans during the whole period of the empire, a motive which led directly to their downfall, will greatly advance the end he has in view. The references to early writers, without quoting their actual words and without discussing the weight of authority that each deserves, will scarcely influence the critical minds of the present day, and it can only be to minds thoroughly imbued with an historical tone that such an argument can be addressed. We would rather meet with Mr. Bannister as an advocate than as an historian.

An Introductory Text-Book of Zoology, for the use of Junior Classes. By H. ALLEYNE NICHOLSON, M.D., D.Sc., &c., &c., Lecturer on Natural History in the Medical School of Edinburgh, &c. Wm. Blackwood and Sons, Edinburgh and London, 1871.

THIS work is just what it professes to be. In plain and simple language it describes the characteristic differences between the various sub-kingdoms, classes, and orders of the animal kingdom. The technical terms introduced are not only explained as they occur, but their derivations are given, and a glossary at the end enables the student to refer again to the meaning of the word should it occur a second time. The illustrations are numerous, and are chosen, where possible, from subjects of common occurrence, and include both external appearance and internal structure. The book is well suited to become the text-book for schools, and contains nothing that an ordinary schoolboy of 13 or 14 could not understand. We hail such a work as tending to the introduction of Natural History teaching in schools, a subject which has hitherto been neglected owing greatly to the want of such text-books as the one before us.

Romance of Motion, or a Mode of Motion of the Planetary Bodies in Space; also a Hypothetical Analysis and Synthesis of Nitrogen. By ALEC LEE. London: Longmans, Green, and Co., 1871.

THIS work is the result of Imagination applied to the Physical Sciences. Starting from the hypothesis that the ether, which pervades space as demonstrated to us by the passage of light and heat, must be resistant to the motions of the heavenly bodies, Mr. Lee would find a counteractant force in the magnetism of the sun acting on the planets as diamagnetic bodies, and he then suggests that as magne-crystals they would in certain positions retard their velocities, thus causing the varieties of speed at various portions of their orbits. Neither for this nor for his theory that nitrogen is a compound of C_3H_4O does he give the slightest proof beyond the mere fitness of things. We must, therefore, decline to consider either proved, although the atomic weight of the elements divided by the reduced volume of the gas after combination does give the specific gravity of nitrogen.

The Moabite Stone, a fac simile of the Original Inscription, with an English Translation, and a Historical and Critical Commentary. By CHRISTIAN D. GINSBURY, LL.D. London: Longmans, 1870.

THIS monograph not only contains, as its title-page describes, the original inscription in *fac simile*, a translation, and a commentary, but it also gives a transliteration of the inscription in modern Hebrew characters, so that any one but moderately instructed in that language can follow the text; and it gives also a history of the discovery and destruction of the stone itself, together with six translations besides that of the editor, viz., those of Professors Noeldeke, Haug, and Schlottman, and those of MM. Ganneau, Neubauer, and Derenbourg.

With the historical and theological value of this monument, believed to be the most ancient extant inscription in this branch of the Semitic languages, we have here nothing to do; but we may call attention to the extreme value of the inscription from an alphabetic and linguistic point of view. Though it has been long known from internal evidence as well as from tradition that the main portion of the Greek Alphabet was derived from the Phœnician, *i.e.*, the old Hebrew; tradition, which had been right in the main outline, declared that some few of the letters were the addition of the Greeks themselves. This the inscription proves to be untrue, for not only are the earliest forms of these doubtful letters found in the Moabite alphabet, but they correspond in position to the letters supposed to be peculiarly Greek. Thus we can complete the pedigree of all our letters from the present day back to the Syrian races, at least as early as the 10th century, B.C.

Linguistically, also, the stone is valuable. It shows that the language of Moab was less distinguished from that of Judea than was that of Phœnicia—a very moderate Hebrew scholar could read the inscription in many parts without difficulty. It shows also that the traditional vowel points, which are known to have been inserted at a late period, have some amount of authority, and that the *matres lectionis*, which some have supposed to be late additions to the Hebrew text, were then, as now, sometimes used and sometimes omitted.

The Year Book of Facts in Science and Art, &c.. By JOHN TIMBS. London: Lockwood and Co., 1871.

THE Year Book of Facts is a compilation consisting almost exclusively of cuttings from various newspapers, from periodicals—more or less scientific—and from the proceedings of learned societies. A portrait and life of Professor Huxley begin the book, and these are accompanied by an abstract of the President's address to the British Association at Liverpool, and some account of his other works. The various "facts" are arranged under the heads of the sciences to which they principally refer, but there is no sequence in the arrangement, nor is there added the few words of explanation, the date, or the circumstance, that in many cases would make the events more interesting. An obituary at the end of the book gives the names of some great men and the lives of others, of whom we confess never to have heard before. We have been unable to discover whether distinction in art, science, or literature has opened the portals of this Walhalla, in which, whilst others have a page or two, Maclise, Chas. Dickens, the Earl of Clarendon, and Professor J. Redtenbacher obtain one line each.

Report to the Board of Visitors of the Royal Observatory, Edinburgh, at their Visitation, held on Wednesday, the 29*th of June*, 1870, *at Three, p.m.* By C. PIAZZI SMYTH.

Papers on the Great Pyramid, including a Critical Examination of Sir Henry James's "Notes on the Great Pyramid of Egypt." BY ST. JOHN VINCENT DAY, C.E., F.R.S.E, &c. Edinburgh: Edmonston and Douglas.

Plates and Notes relating to some special features in structures called Pyramids. By ST. JOHN VINCENT DAY, C.E., F.R.S.S.A. Edinburgh: Edmonston and Douglas.

IT needs no argument to prove to the scientific mind the necessity of accurate standards of measurement, and that even popular feeling is strongly enlisted in this matter is demonstrated by frequent Royal Commissions issued by the Government in answer to the call from without. Nor is this feeling of modern growth. The allusions to justness of weight and measure are frequent in the early books of the Jewish people, especially in the Book of Proverbs. One of the main claims on which the soul of the dying Egyptian rested its hope of happiness in a future state of existence was, that he had "not shortened the cubit," whilst in that future state he expected to encounter as one of his first sights "the God Thoth with the cubit in his hand." Could one of the numerous mummies resting in the museums on British soil now come to life again he would no doubt recognise his god Thoth under the guise of the Astronomer-Royal for the northern part of this island, who has so manfully stood up for the accurate measurement of the old Egyptians' monument of metrology, the Great Pyramid of Cheops.

After all the time, trouble, and expense devoted by Professor C. Piazzi Smyth to determining the minutest details of this marvellous and mysterious monument of early engineering skill, and his persevering efforts to read the riddle of its construction, it must, to say the least of it, have been disappointing that when the Lords of the Treasury had refused "to entertain the question of the measurement of the Great Pyramid" suggested to them in a formal report, the work should have been undertaken by the Department of the Ordnance Survey as a *parergon*, a bye-work, a matter of secondary importance to the main object of the expedition which was to survey the Sinaitic Peninsula, and this without previous information to the man who had done most to draw public attention to the desirability of this enquiry. When, however, it appeared that the Director-General of the Survey had published notes and opinions upon the Pyramid itself, in which there was a considerable want of accuracy both of calculation and of statement of lengths, it was scarcely to be hoped that the results of the survey itself would be satisfactory to those most anxious for it, since those results were not the actual details of each individual measurement, but means arrived at by calculations made by the department and not fully explained.

Both Professor C. P. Smyth and Mr. Day devote a considerable portion of their papers to the refutation of Sir Henry James's calculations and deductions about the Pyramid, and as it seems to us convict him of inaccuracy in his earlier remarks, and thus throw a doubt over the results of the last survey. We hope this may lead only to a fuller publication of all the data upon which these results have been arrived at.

The Report of Professor C. P. Smyth contains other interesting matter, amongst which we would especially draw attention to the determination of the causes of variation in the transit observations at the Carlton Hill Observatory. It was for some time supposed that these originated in contractions and expansions of the rock upon which the piers of the instrument rested; but on comparing the mean variations of the instrument for ten years, with the mean variations of the temperature of the atmosphere, and also of the earth at various depths, it appeared that the extreme errors of the instrument preceded the supposed cause. Experiment then showed that the piers themselves were liable to considerable and irregular expansion, thus vitiating the most careful observations and corrections. The Report on this subject is most interesting, and is accompanied by plates showing the different variations. It is a striking example of careful observation and tabulation of facts leading directly to the discovery of the cause of the disturbance.

Mr. Day, besides his successful onslaught on Sir Henry James, gives an account of the various attempts to measure the Pyramid, and shows why the results of the French *savans* and of Colonel Howard Vyse were, until the last few years, the only ones on which reliance could be placed. In a paper printed at the end of his book, but written previously, Mr. Day attacks "the 'development' band of Darwins, Crawfurds. Lubbocks, *et hoc genus omne*" with language which is somewhat stronger than his arguments. It seems strange that people cannot carry on investigation in their own line without branching off into topics with which the one in hand has no clear connection.

The plates in Mr. Day's other work are most elaborately and carefully engraved, and no doubt will be exceedingly useful to those for whom they are intended, viz., those who are already tolerably well acquainted with the literature of the subject; and we agree with the author's opinion that the volume will be of little use "to those uninitiated in the peculiarities of that class of ancient structures which are in most cases erroneously called 'pyramids.'" For four of the fifteen plates we are indebted to Professor Piazzi Smyth, from whom the world has received an example rarely equalled of a devotion of extraordinary powers to the drudgery of scientific verification. Whilst theories of such momentous importance are in the balance, every contribution towards arriving at well-founded conclusions must be hailed with gratitude, and we cannot but thank those two writers for clearing away some of the difficulties in this remarkable investigation.

The Natural History of Commerce, with a copious list of Commercial Terms and their Synonyms in several languages. By JOHN YEATS, LL.D., &c., assisted by several scientific gentlemen. London: Cassell, Petter, and Galpin.

MUCH has been written and done in regard to the education of the poor—the Education Bill, like most Acts of Parliament, following upon greater activity outside the walls of the Legislature, merely supplements what it appears to have created. Much, also, has been done for the education of the rich, as is testified by deep and radical changes in our Universities, our Public Schools, and our Grammar Schools. At both ends of the scale of society endowments more or less permanent assist the work that parents themselves do not value sufficiently to pay for at the current market rate. But in the meantime the middle classes are left without the excitement of discussion or the assistance of external provision to seek the education required as much for their sons as for those of the other classes of society. It is not surprising, therefore, if in their natural ignorance of the essential qualifications of teachers and teaching they should have suffered unduly from what, being put forward as education, has turned out to be an unreal imitation of mental training and a useless accumulation of disjointed facts. We hail, therefore, as a harbinger of better things such a work as the one under review. Written by a schoolmaster, it is intended not only for his boys whilst under instruction in school, but the information collected is especially designed to be carried away with them into their after work, and to be consulted and referred to when the *schooling* is over, but whilst the education is still going on. It is one of the main faults of much that is called education or instruction that it has no reference to the future pursuits of the learner, and, in consequence, the child which has an unreceptive brain loses that learning from never employing it. Whilst strongly deprecating a mere technical education, we still hold that the only way in which to make a liberal education act upon the whole character of the individual is to connect the general with the particular pursuits of the individual. We hope to see the new School Boards bearing this in mind, and providing that the children of the poor shall not only be taught those subjects which would fit them for a higher station in society than their fathers held, but that they shall also learn the principles of those labours to which their future life will be devoted. The present work brings before us this connection. It shows the application of geography, geology, meteorology, as well as the various branches of natural history, to the work of the counting-house and of the shop. Whilst, therefore, interesting the mind of the lad whose work is concentrated upon the supply of a certain class of productions, it leads him on to the circumstances attendant upon the growth and supply of these and similar substances, to the noting of differences, and to accounting for their causes; and whilst not distracting him from the work that is to furnish his bread, it enlarges his mind to the embracing of new facts, which will enable him to recognise and provide for new wants and new productions.

The plan of Dr. Yeats's book, of which the idea has been taken from the school books in use in the commercial schools of Holland, Belgium, and Germany, is as follows:—After showing the diversity of our own land and of its productions, he points out what countries are analogous to it in various points of temperature, soil, &c., thus leading on to the question, Whence are our raw materials imported? Then taking in turn the various classes of raw materials derived from the Vegetable, the Animal, and the Mineral Kingdoms, the locality of each, and the means adopted for its production, with an outline of the process of manufacture are described. To this is added a table of the names of materials in a great variety of languages. The work seems to have been thoroughly done, and the information is up to the present day, whilst the style is readable and agreeable. We congratulate Dr. Yeats on having produced a book really useful for educational purposes,—adapted, too, for consultation when the work of life has begun, and also suited for more general reading. We are happy to see that the same author intends to carry on the work thus begun by an Industrial and Political History, and by a Technical History of Commerce, and thus to complete a series of works which will enable the young man engaged in commercial pursuits to connect his daily life with the literature which may really enlarge and inform his mind. Men thus trained would do much to wipe away the stigma so often cast at them by foreigners, that the British commercial man exists for the sole purpose of money getting without any higher or intellectual life.

Electrical Tables and Formulæ. By LATIMER CLARK and ROBERT SABINE. E. and F. N. Spon.

THE paucity of English telegraph literature gives rise to the greatest anticipation when any new work is announced, and when proceeding from the pens of two so well-known electricians and authors as Mr. Latimer Clark and Mr. Robert Sabine many a hope is expressed that it may be found adequate to the wants of the telegraph engineer. The existing literature would seem to be composed of two classes—works requiring a more than ordinary knowledge of elementary mathematics to render their application useful, and those in which the mere principles of the science are stated. The book under consideration is of the former class, though much useful information unembarassed by mathemetical formulæ will be met with in the sections devoted to the Laws of Magnetism, Electro-Magnetism, and Induction. It will be more in demand in the office of the chief-engineer than of use to the essentially practical operator. Exception must be made to the employment of the cube knot as a fundamental unit of resistance. A knot is a term occurring in but one branch of telegraphy, and even then the assumption prevents the verification of the formulæ by an individual inquirer. On the other hand, the insertion of the formula

$$I = \frac{E_1 + E_2 + E_3 + \ldots\ldots + E_n}{G + r_1 + r_2 + r_3 + \ldots\ldots + r_n},$$

as determining the current from a battery of n elements of differing electromotive forces and resistances connected in series, goes far to show that the authors have not been bound down by any erroneous ideas, however conventional. The joining-up of such a series would be considered by some rule-of-thumb operators to produce a resultant electromotive force equal to that of the lowest cell only: it is these men who, from their deficiency in the mere elements of mathematics, will still be debarred the proof presented to them. The various units of electromotive force and resistance that have been or are extant are given with their equivalent British Association measures. The term *veber* is used to express the unit quantity of electricity, or that which passes through one *ohm*, in one *second*, with a difference of tension of one *volt;* while the *micro-farad* has been assigned as the unit of capacity. The tests given are from all sources and of all degrees of delicacy; the most delicate operation being, perhaps, that of eliminating the error due to the unknown temperature of the leading-in wires. Even though a new branch of engineering, telegraphy can boast in this work of a most compact collection of formulæ and tables. Every subject coming under the cognisance of the telegraph engineer has its chapter, whether it be the best method of winding galvanometer wires, or the course to steer a cable-laying ship, to take a set of soundings for a submarine cable, or to measure the timber for an overland line—in a word, the work is a dictionary for telegraph inspectors.

On some Disorders of the Nervous System in Childhood: being the Lumleian Lectures delivered at the Royal College of Physicians of London, in March, 1871. By Charles West, M.D., Fellow and Senior Censor of the College; Physician to the Hospital for Sick Children. London: Longmans, Green, and Co.

The foundation of the Lumleian Lectures is intrinsically interesting in so far that they were founded by men who, in a remarkable time, were themselves most remarkable. In 1572, when Shakespeare and Bacon were boys, when Raleigh was serving his first apprenticeship to arms with the troops of the heroic Queen of Navarre, when religious dissensions were highest, and Europe struck aghast by the massacre of St. Bartholomew—Richard Caldwell, and John, Lord Lumley, executed a joint deed laying a perpetual rent-charge on their lands for the foundation of these lectures. Not only historical worth, but the fact that from the Lumleian chair William Harvey first publicly taught his doctrine of the circulation of the blood, will make the post one to be held only by the distinguished in the art of medicine. Dr. West has been the lecturer elected for this year, and, though he has no discovery to bring forward, the subject chosen will find favour with every parent. His book is the work of one who has passed much of his life among the young; to him their wants and sufferings are more familiar than those of grown-up people. The disorders of the nervous system have been selected, both because they are the most frequent and the most fatal, as also because their peculiarities are more remarkable than those of any other class of diseases; for the nervous system is more informed, its functions more rudimentary, its condition one of change and development, the like of which does not take place in the organs of respiration, circulation, or even of digestion. In infancy and childhood we are told that pain referred to any part signifies almost without exception that disease of some sort or other is going on there, or near at hand. And then Dr. West proceeds to treat of neuralgia and epilepsy, chorea and paralysis, in a way that conveys information both to the medical man and to those having care of children.

Light Science for Leisure Hours. A Series of Familar Essays on Scientific Subjects, Natural Phenomena, &c. By Richard A. Proctor, B.A. Camb., F.R.A.S., Author of "The Sun," "Other Worlds than Ours," "Saturn and its System," &c. London: Longmans, Green, and Co., 1871.

This is a collection of essays selected from the author's contributions to serial literature during the past three or four years. Mr. Proctor tells us that his chief object has been to present scientific truths in a light and readable form—clearly and simply, but with an exact adherence to facts; and most strictly has he borne this view in mind. Devoid both of technicality and excessive simplicity, there is conveyed to the reader an immense amount of information. It is certainly a matter for most earnest congratulation that scientific men of the present day have lost that conservatism in their knowledge formerly so rampant; there is a desire to make known to others what is likely to interest, keeping back those dry details through which men, commercially engaged, have no time to wade. And this desire to extend the knowledge of others must in its reflex action bring beneficial results to the teacher, enlarging his auditory and perhaps his means of yet further instruction. It is such works as these from the pen of Mr. Proctor that silence the cry of *Cui bono*, unhappily so prevalent. We cannot all be scholars in the limited meaning of the word, but we may all be scholars in the school of Nature, learning to read what is present to all our senses. It is this that Mr. Proctor does—brings a cultured brain to sift those questions that others else have not the opportunity of even meeting with —a true interpreter, who learns Nature's language not for himself alone. Most of these essays have been before the public in another form, but there is not one that would pall upon a second perusal, while some, published in a college magazine, are new to the general reader. Beyond this the subjects are so various that he must be hard to please indeed who does not find sufficient to interest him.

Iron and Heat. By JAMES ARMOUR, C.E. London: Lockwood and Co. 163 pp.

THIS manual may be divided into two sections. The first comprises the fundamental principles concerned in the construction of iron beams, pillars, and bridge girders; the second deals with the action of heat upon the different materials of iron-smelting. The language is simple, and the mathematical illustrations in the first part of the work, as well as the definition of the laws of chemical combination concerned in the second, are such as will be readily understood by the practical workman. The use of logarithms in calculating strains by Hodgkinson's formulæ is shown with great clearness and brevity, while with equal terseness the combinations taking place chemically in the interior of the furnace during the smelting of the iron are explained by measure instead of by the atomic theory. Mr. Armour will no doubt find many readers among the practical men for whom he writes, and with whom his own experience will have its weight.

Fragments of Science for Unscientific People. By JOHN TYNDALL, LL.D., F.R.S. London: Longmans, Green, and Co.

THESE fragments are in reality solid intellectual repasts provided at different times for large audiences; but the illustrations are so fresh that the repetition consequent upon the aggregation of articles written on the same subject for distinct purposes does not cloy. The first essay is a very happy exposition of its title, "The Constitution of Nature." It presents to the unscientific reader the most important physical laws, pictured in that lucid manner which the author advises as the only successful method of study. Perhaps no other writer has been able to make the conception of an interstellar medium so comprehensible to the student. The present theory of Heat and Light is easily enough understood, once the imagination has accepted the existence of an interstellar luminiferous æther; but it is this endeavour to grasp the intangible that appears the stumbling-block of the tyro in science. How Dr. Tyndall makes this the starting-point from which he tends to the consideration of an extra-ætherial sun is best told in his own words.

"Imagine a paddle-wheel placed in water and caused to rotate. From it as a centre, waves would issue in all directions, and a wader as he approached the place of disturbance would be met by stronger and stronger waves. This gradual augmentation of the impressions made upon the wader's body is exactly analogous to the augmentation of light when we approach a luminous source. In the one case, however, the coarse common nerves of the body suffice; for the other we must have the finer optic nerve. But suppose the water withdrawn, the action at a distance would then cease, and as far as the sense of touch is concerned, the wader would be first rendered conscious of the motion of the wheel by the actual blow of the paddles. The transference of motion from the paddles to the water is mechanically similar to the transference of molecular motion from the heated body to the æther; and the propagation of waves through the liquid is mechanically similar to the propagation of light and radiant heat."

But the power of this work most certainly culminates in the essay on the Scientific Use of the Imagination. Such an article is a true index of the liberality of present scientific investigation, and a true growth of the grand scheme of inductive logic. No reader of this essay can predict a limit to experimental philosophy, can say how soon electricity and magnetism may have supplied by a thoughtive imagination the missing links in the chain that will some day assuredly show their connection with the wave-theory of Light and Heat, binding the phenomena of nature in a comprehensive whole. And when it is seen what imagination has done for science, we cannot wonder that the pioneers of science acquire fresh energy to strike out new paths in the wilderness of fact. What the compass is to the explorer, Imagination is to the scientific investigator; both direct, but neither limit the distance, to be found only by successive trials. Most noticeable of all is the poetic feeling which the author brings to bear upon his subject. There is an emotional under-current that imperceptibly draws the student on to the more fatiguing paths of inquiry: a vividness of mental vision well illustrated, where it says, "The mind is, as it were, a photographic plate, which is gradually cleansed by the effort to think rightly, and which when so cleansed, and not before, receives impressions from the light of truth."

The Meteoric Theory of Saturn's Rings. By AUGUSTUS MORSE DAVIES, B.A., F.R.A.S., &c. London: Longmans, Green, and Co.

THIS consideration of the Meteoric Theory results from seed sown by Mr. Proctor's "Saturn and its System," which induced the endeavour to think out the question of the accumulation of such an assemblage of minute attendants on this one member of the solar system. The work deals shortly and decisively with those elements of Saturn distinguishing it from the other planets, showing that, Jupiter excepted, Saturn from its extremely low velocity is most favourably circumstanced for attaching meteors in its train as satellites, particularly when the velocities of the meteors are considered. Then the distance of Saturn from the Sun exceeding that of Jupiter, Saturn's influence on passing meteors would be greater than that exerted by Jupiter, the solar attraction varying according to the law of inverse squares. It does not appear strange that the meteoric theory has met with but few adherents amongst scientific men, when it is considered that the rings have been revealed by modern science alone. The fact that the god Nisroch, associated with Saturn, should be represented as a man encircled with a belt, does not go far towards an argument that the rings were known before the time of Galileo. The surprise would be why so few sound theories have been propounded to explain so unusual an attendance. But in this book, including, as it does, an epitome of the theories gone before, there is not much left to be desired. The meteoric theory can now most certainly boast of a clearly written manual, in which the argument is aided by sound analytical expressions and remarks drawn from the works of Sir W. Thomson, Herschel, Lyell, Arago, and Proctor. Appended is a paper on the Meteoric Theory of the Sun, in which the solar centre is considered to consist of a central globe, hollow or continuous, surrounded by a liquid envelope of molten metals. On this ocean rests an atmosphere of flaming gas, and this atmosphere is composed of volatilised substances from the liquid surface and the smaller meteors. In it the meteoric flights first become visible to us, in part, adding to its temperature by the resistance it offers to their motion, and showering down their substance in it. The pains taken to impart clear ideas certainly inspire the hope that the theory will be well accepted.

Select Methods in Chemical Analysis (*Chiefly Inorganic*). By WILLIAM CROOKES, F.R.S., &c. London: Longmans, Green, and Co., 1871.

IT will be perceived from the title of this work that the author has not intended to provide the student with a complete text-book of analysis, but rather with a laboratory companion, containing information not usually found in ordinary works on Analysis. The book is essentially a reprint of the important articles on inorganic chemistry in the first twenty volumes of the "Chemical News," verified, condensed, and arranged in proper order; and as some of these have proved to be of great value, it was thought that a service would be rendered to analytical chemistry if these trustworthy methods of analysis were systematically arranged in a convenient form for laboratory use. In some instances the descriptions are given in the language of the original writer, but in all cases where the author has improved the processes, the necessary modifications have been introduced.

It is strange that modern works on analysis should ignore about twenty of the elements. Even Fresenius gives only a separate form for their detection. Were investigators more in the habit of looking for the "rare" elements, they would no doubt turn up unexpectedly in many minerals. In this work equal prominence is given both to the rare and to the ordinary elements.

The order in which the analytical separation of the metals is carried out will be readily understood. Take, for instance, the case of copper. After giving the best method for the detection and quantitative estimation of this metal, comes a description of the processes for separating it from those metals which have been previously passed under review, as mercury, silver, and zinc; but no attention is paid to the separation of copper from such metals as lead, tin, &c., which have not previously been treated of. Under the respective headings "Lead" and "Tin," the separation of these metals from copper is described.

A complete list of separations has not been attempted. Where no process of separation or estimation is given, it may be inferred that the author has had no experience in any but the well-known methods employed in most laboratories; and to have introduced these ordinary processes into the work simply for the sake of filling up gaps would have largely increased its bulk without adding materially to its value. To save space, the description of a process is frequently discontinued at the point where the substances under separation are brought to such a state that the concluding steps are obvious.

No special system of weights and measures has been employed; many of the descriptions having been condensed from the original memoirs, it was thought better to retain the system therein adopted, so as to have simple numbers to deal with, instead of having to convert them to one common scale and to introduce decimals; thus—when an author says take 8 grains of a substance, 0·51816 gramme has not been substituted; and where 10 grammes are mentioned, he has not put 154·3840 grains. When not otherwise expressed, all degrees are according to the centigrade scale. Formulæ have been avoided as far as practicable.

A Series of Chemical Problems, for use in Colleges and Schools; adapted for the Preparation of Students for the Government Science and Society of Arts Examinations. By T. E. Thorpe, Ph.D., Professor of Chemistry in Anderson's University, Glasgow. With a Preface by Professor Roscoe, B.A., Ph.D., F.R.S. London: Macmillan and Co. Manchester: James Galt and Co., 1870.

In a short preface to this little book, Dr. Roscoe says that it will prove a great boon to those engaged either in teaching or in learning the science; and on going over the contents we perceive that the questions have been prepared with great skill, and with special reference to junior classes in which elementary chemistry is taught. In order to give our readers some idea of the contents and manner of treatment, we quote the headings of the sections of this little work:—Chemical Calculations; French System of Weights and Measures; Conversion of Thermometric Scales; Correction of the Volume of Gases for Temperature and Pressure; Specific Gravity of Solids, Liquids, and Gases; To Calculate the Percentage Composition of a Compound from its Formula; To Calculate the Amount of Material required to Produce a Given Weight of any Substance: or the Quantity of the Substance produced by the Decomposition of a known weight of the Material; Combination and Decomposition of Gaseous Bodies; Calculation of the Results of Atomic Weight Determinations; Deduction of the Empirical Formula of a Body from its Percentage Composition; Calculation of the Formula of a Body from the Results of its Analysis; General Analytical Questions; Exercises on the Specific Heat, Latent Heat, Calorific Power, and Calorific Intensity of Substances. The Appendix contains in tabulated forms:—(1.) Combining weights and symbols of the elements; (2.) Weight of 1 c.c. of atmospheric air at different temperatures, from 0° to 300°, at 760 m.m. pressure; the weight of 1000 c.c. of water of $t°$ C., when determined by means of brass weights in air of 0° C., and of a tension 0·760 m.m., is equal to $1000-x$ grms.; volume and density of water at different temperatures (Kopp); for the conversion of the degrees (T′) of a mercurial thermometer into the corresponding values (T″) of an air thermometer; portions of tables of logarithms and antilogarithms. We sincerely wish that this excellent book, which is to be shortly followed by a key to the questions contained therein, will prove an assistance to teachers as well as pupils, and become a great impulse to a more general learning of a science which is, on account of its utility, of the greatest national importance to the whole community.

The Sub-tropical Garden, or Beauty of Form in the Flower Garden. By W. Robinson, F.L.S.; with Illustrations. London: J. Murray, 1871.

The term "sub-tropical" is scarcely a happy expression, and if this work had reference only to plants which required a sub-tropical climate to bring out their beauties, it would be of little use except to the very wealthy. This term is, however, here used in a wider sense, and it means the culture of plants with large and graceful or remarkable foliage or habit, and the association of them with the usually low-growing and brilliant flowering plants now so common in our gardens, and which frequently eradicate every trace of beauty of form therein, making the flower garden a thing of large masses of colour only. The author informs us that his guiding aim has been the selection of really suitable subjects, and he has rejected several that have been recommended and tried for this purpose. Many have been excluded which have been long acclimatised in this country; likewise some very tender stove plants, which require too much care. But no tropical or sub-tropical subject that is really effective has been omitted. The illustrations are very numerous, and give a striking idea of the ornamental value in gardens of plants having large and handsome leaves or graceful port. Through the courtesy of the publisher we are enabled to illustrate our remarks with some of the cuts decorating this handsome volume.

One of the greatest mistakes ever made in the flower garden was the adoption of a few varieties of plants for culture on a vast scale to the exclusion of interest and variety, and too often of beauty or taste. To quote the words of our author, "We have seen how well the pointed, tapering leaves of the *Cannas* carry the eye upwards; how refreshing it is to cool the eyes in the deep green of those thoroughly tropical Castor-oil plants with their gigantic leaves; how grand the *Wigandia* with its wrought-iron texture and massive outline looks after we have surveyed brilliant hues and richly painted leaves; how greatly the sweeping Palm leaves beautify the British flower garden; and in a word the system has shown us the difference between the gardening that interests and delights all beholders as well as the mere horticulturist, and that which is too often offensive to the eye of taste."

It is a mistake to imagine that this kind of gardening can only be indulged in by those who possess conservatories, hot-houses, or other means of preserving tender plants in the winter. What, for instance, can be more beautiful than the Pampas grass, which, when well grown, is unsurpassed by anything that requires protection. There are the *Yuccas*, noble and graceful in outline, and thoroughly hardy, and which if planted well are scarcely to be equalled by anything of like habits we can preserve indoors. There are the *Arundos conspicua* and *Donax*, things that well repay for liberal planting; and there are fine hardy herbaceous plants, like *Crambe cordifolia*, *Rheum Emodi*, *Ferulas*, and various graceful umbelliferous plants, that will furnish effects equal to any we can produce by using the tenderest exotics. The *Acanthuses*, too, when well grown are very suitable for this use. Among the *Yuccas* the *Y. pendula*, considering its graceful and noble habit, is simply invaluable in every garden, and when once planted, may be left year after year without protection. The accompanying illustration shows the grace and symmetry acquired by old and well established specimens standing alone on the grass, from the lower leaves which sweep the ground, to the central ones that point up as straight as a needle.

But if the garden is furnished with conservatories properly heated for the winter, there is scarcely any limit to the beauty

FIG. 4.

YUCCA PENDULA.
Hardy evergreen, fine foliaged type.

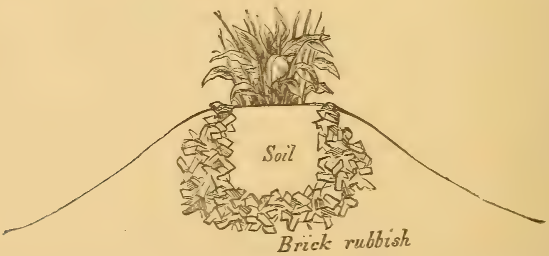
which may be attained. What, for instance, can be more beautiful than the shady and sheltered dell figured on the next page, with tree-ferns and other stove plants placed out for the summer.

The author speaks with approval of a peculiar mode of preparing the beds for the finer sub-tropical plants as practised in Battersea Park. Here many of the beds are raised above the level of the ground, and underneath and around the mass of light rich soil is a good layer of brick rubbish, as shown in the accompanying engraving (Fig. 6). The soil is first excavated, and thrown round the margin of the bed, then the brick-rubbish is put in on the bottom and around the sides also, raising the bed somewhat above the level of the ground; the cavity in the centre is then filled up generally with fine light rich soil, and the outside is arranged in a gentle slope, and covered neatly

FIG. 5.

with turf. The soil may vary in depth from 3 feet to 18 inches, according to the kinds of plants to be grown in it. The chief advantage which this method insures is that of good drainage, but it renders efficient protection against cold winds somewhat difficult. This latter shelter is a most essential requisite for

FIG 6.



all the more delicate kinds of plants. Mr. Robinson's remarks on this subject are very judicious, and will well bear quotation:— "Warm, sunny, and thoroughly sheltered dells should be chosen where convenient; and in any case positions which are sheltered should be selected, as the leaves of all the latter kinds suffer very much from strong winds, from which they will be protected if judiciously planted near sheltering banks and trees. Even in quite level districts it will be possible to secure shelter by planting trees of various kinds; among which such graceful conifers as *Thujopsis borealis*, *Thuja gigantea* (true), *Cupressus macrocarpa*, *Cryptomeria elegans*, &c., should be freely used in the foreground, as in beauty of form they are unsurpassed by any short-lived inhabitants of the summer garden. Except, however, in the case of the Tree-ferns and various other things not grown in the open air, but simply placed there for the summer, it is very desirable not to place the plants in the shade of trees. All the things which have to *grow* in the open air should be placed in the full sun. Not a few hardy subjects will thrive very well without any but ordinary shelter, as, for example, the *Yuccas* and *Acanthuses*; but judging by the remarkable way in which the hardy Bamboo thrives when placed in a sheltered dell, shelter has a considerable influence on the well-being even of these, as it must have on all subjects with large leaf surfaces. But it should not be forgotten that shelter may be well secured without placing the beds or groups so near trees that they will be robbed, shaded, or otherwise injured by them."

The book is divided into three parts; the first contains Introduction and General Considerations; the second gives the Description, Arrangement, Culture, &c., of suitable species, hardy and tender, alphabetically arranged; whilst the third part contains Selections of Plants for various purposes in the Subtropical garden. The second part occupies far the largest portion of the book, and is of the greatest use, as it gives full descriptions of all (frequently illustrated with woodcuts) the plants which the author has selected, the kind of care they require, and the most suitable position in the Subtropical garden for them to be displayed to the best advantage. As an illustration of the method adopted by the author in this part of the work we cannot do better than give a quotation:—

"*Dicksonia antarctica.*—A very noble evergreen tree-fern, with a stout trunk, which varies considerably in thickness, and attains a height of 30 feet or more. The fronds, which form a magnificent crown 20 feet or 30 feet across, are lancet-shaped, much divided, of a shining dark green on the upper surface, and paler underneath, from 6 feet to 20 feet long, beautifully arched, and becoming pendulous with age. Perhaps the hardiest of tree-ferns, and therefore most suitable for placing in the open air in summer in sheltered shady dells, from the middle of May to the beginning of October." (See Fig. 7, on the next page.)

It is with mingled feelings of pleasure and regret that we close this work—regret that those who have had the laying out of some of our parks and public gardens have not adopted more freely the judicious views here expressed, in preference to fatiguing the eye and offending the taste by the sight of hard, geometrical patterns, and glaring chromatic made up masses of blossom, variegated gravel, and box edging ; and pleasure to

FIG. 7.

TREE-FERN.

For half-shady sheltered dells, in warmer and milder districts during the summer months.

think that the publication of such a book as this cannot fail to produce a beneficial effect, contributing as it does so many valuable suggestions in connection with an art for which Englishmen have long taken the highest rank.—that of landscape gardening.

PROGRESS IN SCIENCE.

MINING.

THE recent development of the extraordinary mineral districts in Nevada and Colorado, now rendered accessible by the Pacific Railroad, has led to an official exploration of the chief ore-bearing regions in these States. In 1867 Mr. Clarence King was appointed Geologist in charge of the expedition, and the volume of the Report which relates to Mining, prepared chiefly by Mr. J. D. Hague, has recently been published.* The complete report will comprise five volumes—two on Geology, one on Mining, and the others on Zoology and Botany. The third volume—that on Mining—has been issued first in consequence of its great practical importance. It forms a magnificent quarto volume amply illustrated, and accompanied by a large folio atlas of maps, plans, and sections, printed in colours.

Although professedly an exploration of the fortieth parallel, the survey has been extended to a belt of country stretching for about 100 miles from north and south, and included between the meridians of 109° and 120°; moreover, any important mining districts lying adjacent are also noticed.

Over the whole Cordilleras deposits of metallic ores are more or less abundant; these deposits usually range in longitudinal zones parallel to the general trend of the rocks, which is nearly north and south. The Pacific Coast Range upon the west carries ores of mercury, tin, and chrome-iron ore; the next belt —that of the Sierra Nevada and Oregon Cascades—bears on its western slope copper and gold, while along the eastern base is a chain of silver mines; these silver deposits extend into Mexico, whilst the gold ores—both auriferous veins and placer workings—run far into Alasca. Through Middle Mexico, Arizona, Middle Nevada, and Central Idaho is another range of silver-mines; and through New Mexico, Utah, and West Montana a zone of lodes containing argentiferous galena; whilst on the east a belt of gold deposits runs through New Mexico, Colorado, Wyoming, and Montana. For a proper understanding of these mineral deposits, it is of course needful to study the geology of the far West. A series of conformably stratified beds, varying in age from early Palæozoic to late Jurassic times, appear to have been at the latter period upheaved and corrugated, whilst masses of granitic and porphyritic rocks were injected into the fissures formed by this disturbance. Deposits of cretaceous and tertiary age were then laid down on the flanks of these rocks, and at a period subsequent to the miocene epoch they were folded into mountain-chains parallel to, but outside of, the earlier system. This second upheaval was accompanied by volcanic disturbances, the lavas overflowing and covering the earlier deposits. The mineral lodes of these regions consequently resolve themselves into two distinct sets, corresponding to the two periods of disturbance—firstly, those of Jurassic age, consisting of veins of granite, or in more or less metamorphic strata, varying in date from Jurassic to Devonian; and, secondly, veins of tertiary age in volcanic rocks. A large number of lodes are referable to the first date, but many of the richest, such as the Comstock lode, are referable to the second disturbance.

As might be expected, a large proportion of the volume is occupied by a description of the celebrated Comstock vein. This lode courses nearly N. and S., and has been traced for a distance of four miles, the part already developed occupying about two miles. The masses of ore occur in disconnected groups, and the width of the vein varies considerably, sometimes containing masses of vein-stuff 100 ft. in width. The report contains a minute description of the lode, with details of the many mines opened upon it, and of

* Report of the Geological Exploration of the Fortieth Parallel; vol. iii., Mining Industry, by J. D. Hague, with Geological Contributions by Clarence King. Washington, 1870.

the metallurgical treatment to which the Comstock ores are subjected. The mines of Central and Eastern Nevada, and of Colorado, also receive ample notice.

During the last few years the great development of the steel trade, due to Mr. Bessemer's improvements, has led to the extensive working of our deposits of red hæmatite, an ore which by its freedom from phosphorus yields an iron well adapted for conversion into Bessemer steel. Our deposits of this ore are, however, extremely limited, being confined chiefly to the mines in the carboniferous limestone of the neighbourhood of Ulverstone, in Lancashire, and Whitehaven, in Cumberland; and hence apprehension has not unnaturally been entertained that these workings may soon become insufficient to meet the ever-growing demand for hæmatite. Accordingly the Iron and Steel Institute, some time ago, appointed a committee to report on the distribution of our iron ores, with special reference to deposits of hæmatite, and at a recent meeting of the Institute this committee presented its preliminary report. It appears that an application was made by the Secretary to the Director-General of the Geological Survey, requesting that the officers serving under him should be instructed to report upon this subject. Whilst we feel assured that the Survey has ever been willing to further the mining interests of the country, it is obvious that the progress of a great national work of this kind can be interfered with only under very special circumstances. Hence we are hardly surprised that the Director-General does not feel justified in immediately acquiescing in the scheme of the Institute, although willing to do so at the earliest opportunity. A slight misunderstanding, however, appears to have been entertained on this subject, which elicited some explanatory remarks from Mr. Robert Hunt.

Those who are interested in the promotion of mining education in this country will have learnt with surprise that the Royal Commission on Science has suggested in its Preliminary Report, that the Royal School of Mines and Mining Record Office shall be separated from the Museum of Practical Geology, and that the School be removed to South Kensington. However ardently we may desire to see the establishment of a grand central College of Science comparable with some of the continental institutions, the policy of the proposed step with reference to the Mining School may be fairly questioned. Such technical subjects as mining and metallurgy should obviously be taught in an institution where the student may readily gain access to good collections of minerals, metallurgical products, and mining models. Nowhere can these conditions be better fulfilled than in Jermyn Street, where for the last twenty years valuable collections have been gradually accumulating. Indeed, it is certain that many of the objects—such as the grand series of vein-stones—could not possibly be obtained in duplicate for Kensington. Nor must we ignore the convenience to the public arising from the concentration of the Mining School, the Geological Museum, the Mining Record Office, and the head-quarters of the Geological Survey under the same roof.

Three valuable memoirs on mining subjects, by Mr. W. Jory Henwood, of Penzance, form the eighth volume of the "Transactions of the Geological Society of Cornwall," recently published—a volume which is divided into two parts, each forming a stout octavo. Mr. Henwood's first memoir occupies between 700 and 800 pages, and describes in detail more than 130 mines in India, Chili, Brazil, North America, Jamaica, Spain, France, the Channel Islands, and Great Britain and Ireland. These lengthy "Observations on Metalliferous Deposits" are followed by two shorter papers—one giving the result of certain "Observations on Subterranean Temperature," and the other "On the Change of Temperature which takes place, at the same and at different times, on the Surface and at Depths of Three, Six, and Nine feet in the Canga at Agoa Quente, in Brazil." The present volume is a befitting companion to the fifth volume of the same series, which is devoted to Mr. Henwood's celebrated "Memoir on the Metalliferous Deposits in the West of England."

We have before us the last Report of "The Miners' Association of Cornwall and Devonshire," which shows what useful but unpretending work the association continues to effect. In an introductory notice, the Honorary Secretary, Mr. R. Hunt, F.R.S., sketches the rise and progress of the association, and shows the part which it has played in supplying the working miner with sound, scientific education. It is really vexing to learn that such good work should be impeded by insufficiency of funds. The present report contains papers on boiler explosions in Cornwall, by Mr. C. E. Martin; on the use of Haupt's improved rock-drill in Swaledale, by Sir G. M. Denys, Bart.; and on a new buddle for dressing gold, tin, copper, lead, or other ores, by Mr. R. H. Williams.

A paper on the iron-producing districts of Central Sweden has been communicated to the Iron and Steel Institute, by Mr. David Forbes, F.R.S. The disadvantageous inland position of these mineral deposits has hitherto impeded their development, but the construction of a railway traversing the heart of these iron-mining districts will probably render them available to the English iron-master, whilst a considerable impetus may be given to the iron-trade of Sweden, which of late years has been decidedly on the wane.

METALLURGY.

Considerable discussion has lately arisen as to the value of the "Sherman Process" for purifying cast-iron. The application of very small quantities of iodine, or some compound of this element—such as potassic iodide—is said to effect the removal of sulphur and phosphorus from the crude metal, so that good wrought-iron or steel may be obtained from inferior brands of pig-iron. Mr. Snelus, of Dowlais, has, however, examined samples of steel treated by this process, and fails to detect any appreciable chemical difference between this steel and that made by the ordinary method. In defence, Mr. Sherman has published an analysis of a cast-steel said to have been made from Cleveland pig by his process. It is well known that the Cleveland iron contains a large proportion of phosphorus, but this is reduced to 0·04 per cent in the steel. The following analysis is due to Mr. Barker, of the Atlas Works, Sheffield:—Combined carbon, 0·67; graphite, traces; phosphorus, 0·04; sulphur, traces.

Mr. Bessemer suggests that if iodine is so efficacious in refining iron, it would certainly be more economical to employ crude substances containing iodine, such as vitrified sea-weed, than to use the element itself or its pure compounds.

Under the name of "Burnt Iron," our workmen recognise iron which has been damaged by re-heating or by exposure to excessive heat after balling in the puddling-furnace. Such iron is brittle, and having lost its fibrous character, breaks with a short crystalline fracture. The cause of this change has not hitherto been satisfactorily explained, but Mr. Mattieu Williams has presented to the Chemical Society a paper on this subject, in which he shows that the burning of iron is simply a process of oxidation, which, however, affects the whole substance of the metal. By working the iron in a reducing atmosphere, this internal oxidation is prevented, and the various devices employed by workmen to prevent burning are based upon this principle.

Steel raised to a yellow or white heat and suddenly cooled, is rendered permanently hard and brittle, and incapable of being tempered by simple re-heating. This "burnt-steel" presents a coarse granular fracture, and in addition to a crystalline structure shows certain rounded facets, with concavities known as "toad's-eyes." The carbon in steel prevents the oxidation which produces burnt iron; but it is known that a slow combustion of the carbon in steel may take place even at a low red heat, and that this action gradually proceeds inwards. Mr. Williams believes that the carbon-monoxide produced by interior oxidation may remain in occlusion in the iron, and that if steel be suddenly cooled, so that this occlusion is prevented, the minute bubbles of carbon-monoxide and oxygen thus arrested would render the steel porous, and so affect the cohesion of its molecules in such a way as to produce the characters of burnt steel.

Herr H. Scheerer publishes in the "Berg- und Huettenmaennische Zeitung" the results of his examination of a crystallised Bessemer slag from Hoerde. Drusy cavities in the slag run out from the converters, often contain well-formed crystals isomorphous with rhodonite, pajsbergite, and babingtonite, and certain triclinic minerals of the augite group. The slag is a bisilicate of iron and manganese, and contains—Silica, 44·73; ferrous oxide, 20·59; manganous oxide, 32·74; lime, 1·53; and magnesia, 0·17. Its specific gravity was 3·08.

ENGINEERING—MILITARY, CIVIL, AND MECHANICAL.

Military Engineering—Field Guns.—The report has recently been published of a committee which was appointed in July last for the purpose of carrying out certain competitive trials, with the view of testing the relative efficiency of muzzle and breech-loading guns for field service. Two batteries were placed at the disposal of the Committee for the purposes of their experiments, one consisting of three 12-pounder and three 9-pounder breech-loading wrought-iron guns, and the other of six 9-pounder muzzle-loading bronze guns. Each gun was provided with 240 rounds, consisting of a certain number of rounds of shrapnel, segment, and common shell, of case shot, and of percussion and wood time fuzes. From the results of these experiments it appears, that in respect of shooting, the 9-pounder muzzle-loader and the 12-pounder breech-loader are about on a par, whilst the 9-pounder breech-loader is inferior to both. The advantages of simplicity, facility of repair, ease of working, rapidity of fire, original cost, and cost of maintenance, are all in favour of the muzzle-loader, whilst the breech-loader possesses the advantage of affording a superior amount of cover to the detachments when entrenched and in the open. The conclusion of the Committee is that, on the whole, a muzzle-loading gun is more efficient for war purposes, and they recommend that, if adopted for home service, they be made of wrought-iron with steel tubes. The report has been adopted by the Secretary of State for War, and it may therefore be concluded that the contest, so far as this country is concerned, has, for the present, been definitely settled in favour of the muzzle-loader.

Marine Heavy Ordnance.—The following description of our new 35-ton gun is taken from "Engineering." The great gun consists of six distinct parts; First, the inner or A tube, which is of Frith's steel, and is 13 feet 6½ inches in length, internal measurement. The thickness of the tube is 3½ inches at the breech, and tapering down to 2½ inches at the muzzle. Next this is the muzzle or B tube, which is of wrought-iron, and is shrunk on to the steel tube. The third piece forms the first-stepped joint, about 6 feet from the muzzle, and is known as the intermediate coil; it holds the front end of the coiled breech-piece which forms the fourth part. The fifth part is the outer or C coil, with its trunnion band, which is shrunk on to the breech portion of the gun, binding the rear end of the intermediate piece. The sixth and last piece is the cascable, which is made button-shaped instead of the ordinary form, in order to obtain the greatest possible length of bore in the gun within a restricted dimension, the Admiralty having limited the length over all to 16 feet 3 inches. The vent is placed on one side of the breech piece in order to render it easily accessible for firing. The gun is rifled to a calibre of 11·6 inches, and the length of the bore is 13 feet 6½ inches. The rifling consists of nine grooves, and in twist uniformly gaining from zero at the breech to 1 turn in 35 diameters at the muzzle. The weight of the projectile which this gun is intended to carry is 700 lbs.; the length of the solid shot being 30 inches, and that of the common shell 40 inches; the shell carries a bursting charge of 38 lbs.

Breech-loading Rifles.—On the 27th April last, a paper was read before the Institution of Mechanical Engineers, by Mr. W. P. Marshall, on "The Principal Constructions of Breech-loading Mechanism for Small Arms, and their Relative Mechanical Advantages; Illustrated by Specimens of Breech-loading Rifles." Mr. Marshall stated that breech-loading guns were first adopted by the Prussian army: the celebrated needle gun having been in use for more than 20 years, while for many years it has been the universal arm used by that army. Although breech-loading carbines had been in use for 14 years, to a limited extent, for the cavalry of England, it was only since 1866 that the principle had been adopted for the army generally. Breech-loading carbines were adopted for the cavalry solely for the purpose of overcoming the difficulty of loading whilst on horseback. The writer after having described the various breech-loading guns, including the needle-gun, the Chassepôt, the Albini-Brændlin, the Werndl, Henry's, Soper's, Westley-Richards's, Sharp's, the Snider, Remington's, the Berdau, &c., went on to consider the Martini-Henry breech-loading gun, which has been selected for adoption in the British army after a long investigation and practical trial. We shall give a full description of this weapon upon a future occasion.

Marine Engineering—Docks.—The Somerset Dock at Malta was officially opened on the 16th of February last. Shortly after Malta came into the possession of the British Government in 1814, steps were taken to provide dock accommodation there. The site chosen was the Dockyard Creek, a small inlet of the great harbour. In the commencement a small graving dock was started on the south side of the creek, but the fissures in the rock permitted so much infiltration of water that it had to be abandoned. In 1840 the so-called old dock was commenced. It was completed in 1848, and in 1857 it was lengthened. The accommodation available at the dockyard was, however, at this time, very inefficient, and it was ultimately determined to construct a new dock, which should be of sufficient dimensions to receive the largest ironclad. The site chosen for the dock is immediately under St. Michael's bastion, where in 1565 the Turks conducted the obstinate and long-maintained siege of the towns of Senglea and Borgo. Before the works were commenced about 300,000 cubic yards of old materials, buildings, earth, and rock had to be removed. This occupied a considerable time, and it was the end of 1866 before the dock itself was commenced. The dimensions of the old and new docks are as follows:—

	Somerset Dock.	Old Dock.
Depth to floor	34 feet	25 feet.
Width between copings ..	104 ,, ...	82 ,,
Length on floor	430 ,, ...	301 ,,
Width of entrance ..	80 ,, ...	73 ,,

Considerable difficulties were experienced in making the excavations, numerous fissures having been encountered through which flowed large quantities of water. The smaller of these were stopped before the masonry was laid; in the larger ones cast-iron pipes were laid and the water carried off until the masonry was completed, after which the pipes were closed. The stonework of the dock is laid throughout upon the solid rock, the backing being of an inferior quality of the crystalline limestone of the island, and the floor and sides of a superior quality of the same stone. The thickness of the floor is 7 feet 6 inches. Of the first quality of stone 340,000 cubic feet were employed, and of the second quality 190,000 cubic feet. The entrance to the dock is closed by a sliding caisson, which runs back into a tunnel excavated in the rock beneath the Senglea bastion. The engine house contains two 60-horse power engines, which drive three centrifugal pumps, each of which throws a stream 3 feet in diameter.

A paper containing "An Account of the Basin for the Balance Dock and of the Marine Railways in connection therewith, at the Austrian Naval Station at Pola, on the Adriatic," by Mr. Hamilton E. Towle, of New York, U.S., was recently read before the Institution of Civil Engineers. It was at first intended that excavated docks should be formed, but in consequence of the volcanic and treacherous nature of the ground, this idea was found to be impossible of execution. A floating dock, basin, and railway system was therefore decided upon, the dock adopted being that known as Gilbert's Balance Floating Dock. The dimensions of the Basin are as follows:—

	Feet.	Inches.
Width inside the enclosing walls	211	6
Length	311	6
Depth from the top of the walls to the stringers in the floor of the dock	17	$1\frac{1}{4}$
Depth from the level of ordinary high water to the top of the stringers	13	0
Depth from the level of ordinary low water to the top of the stringers	11	0

The material selected for the wall of the basin was Santorin béton, composed of Santorin earth, a volcanic product from the Greek island of Santorino, and lime paste, in the proportion of 7 to 2, forming the hydraulic mortar; to this was added 7 parts of broken stone, the mixture being made into a conical heap, and tempered by exposure in the open air from one to three days, when it was ready for use. Of this béton extensive wharves and moles had already been constructed at Trieste, Fiume, and Pold, and as it had been found durable and efficient, was moderate in cost, and obtainable in any quantity, it was considered that no better material could be determined upon for the walls of the Pold basin.

Propellers.—On the 18th of April a paper was read at a meeting of the Institution of Civil Engineers, by Sir C. F. Knowles, Bart., M.A., F.R.S., "On the Archimedean Screw Propeller, or Helix of Maximum Work." In considering the defects of existing screw propellers, the author was led to propose to himself the problem, "What is the form of the surface of the screw propeller of which the *work done* is the greatest possible?" Referring the required surfaces to three rectangular co-ordinates, x, y, and z, one in the axis of rotation, the other two in the plane of rotation, the author first obtained a general expression for the total "work done" by the blade in propelling the ship, in the form of a double integral in terms of the co-ordinates x and y, and of the partial differentials of z with respect to each of them, of the speed of rotation of the blade, and lastly, of the speed of the ship. As this integral was to be a *maximum* for all points of the surface sought, it must be treated by the known methods of the calculus of variations. This done, an equation of condition was obtained, which, by the performance of the operations indicated by the symbols, led to an equation involving two factors, each factor being a partial differential equation between the three co-ordinates of the surface. The first of these being integrated gave for its solution the whole family of ordinary helices which were the surfaces of *least* work. The second factor was the differential equation of the required surface, the treatment of which was given in the paper *in extenso*. It led at once, and very simply, to an equation analogous to that of the common helix $\left(\tan.\ \theta = \frac{a \tan. \alpha}{r}\right)$ viz., $\tan.\ {}^2\theta = \frac{a \tan. 2\alpha}{r}$. From this it was at once deducible that the surface of the blade at the axis cut the plane of rotation at an angle of 45°, while the common helix cut it at 90°, and therefore acted powerfully in the dead water to propel the ship, just where the common helix had no propelling power. It was proposed to call this surface the hemi-helix, or hemi-angular helix. The paper then proceeded to determine the pressure of this blade upon the vessel in the direction of the keel, and thence the whole circumstances of the ship's motion.

Civil Engineering.—The Kistnah Viaduct, now in course of construction in connection with the Great Indian Peninsula Railway, consists of 36 spans of 100 feet clear at the top of the columns, and is 3848 feet long from centre to centre of the end columns, which are built into the abutments. The piers are arranged for carrying a double line of railway, although at present girders for a single line only are erected. The section of the river is irregular, the height of the piers varying from 34 feet to 76 feet 6 inches from base of pier to rail level. The river bottom is hard rock, into which the cylinders of the piers are sunk, and to which they are securely bolted. The piers each consist of two columns formed of wrought-iron cylinders, averaging 10 feet in diameter at the base, and tapering to 7 feet in diameter at the top. The vertical joints are formed internally of T irons, which are continuous from top to bottom, and are rivetted through to outside vertical strips. For the horizontal joints the ends of the cylinders are planed, the joints being butt-joints with strips inside and out. The cylinders are filled in with cement concrete, and finished with stone caps, which form the bases on which the bed girders rest, the caps being placed directly on the concrete. The bottom lengths of the cylinders are held down to the rock by eight bolts let head-downwards into holes drilled in the rock, which are afterwards filled in with Portland cement. Each pair of columns which form one pier are connected at the top by two transverse bed girders, on which the main girders rest on chairs. The main girders are on the Warren principle, each 103 feet long, and have a vertical depth of 9 feet 11 inches from centre to centre of pins. These girders are each fixed at one of their ends over alternate piers, the other ends being free to move on Bessemer steel rollers. The total weight of the ironwork in the superstructure of the Kistnah Viaduct is 2500 tons, whilst that in the cylinders is 1200 tons.

The *Czernowitz Bridge* crosses the river Pruth at the town of Czernowitz, the capital of the district of Bukowina, which forms the easternmost portion of the Austrian dominions. The bridge carries a roadway and two side footpaths, and measures 762 feet 6 inches in length, and 25 feet in width from centre to centre of girders. It has six openings over the river, each 126 feet between centres of piers; the five piers and two abutments are of masonry, resting on concrete foundations. The main girders are continuous, of the single lattice type, and are 11 feet 10 inches deep. The flanges are boxes composed of two large channel irons and a flange plate which connects them. The diagonals are placed at an angle of 45°, and consist of a pair of flat bars which form the ties, and a pair of channel irons braced together, which form the struts. Except at the piers, the main girders have no verticals, nor are they anywhere braced across the top flanges. The method adopted in order to prevent the top flanges from collapsing has been to carry them at every pier in two strong U-shaped frames, which keep the top flanges steady at this point, whilst the stiffness of the flanges between the piers is secured by their great width. The two main girders rest upon roller bearings at each of the piers except the centre one, so that any expansion from increase of temperature radiates outwards from the centre, and extends the bridge equally at each end. The roadway of the bridge consists of a timber platform carried on the cross girders, and supporting longitudinal timbers, upon which is laid transversely 4½-inch planking, and upon this again rest oak blocks 5 inches thick. The footpath is laid with 3-inch oak longitudinal decking, upon which the wearing planks are spiked.

Gas Engineering—Cleland's Steam Jet Gas Exhauster.—At a recent meeting of the Liverpool Polytechnic Society, Mr. William Cleland, Manager of the Lindere Gas Works, read a paper descriptive of the system of exhausting and washing gas by steam which he has adopted. This invention is named the direct action method of exhausting and washing gas by steam, because it aims at superseding in these operations the use of steam engines, and of exhausting and pumping machinery, by bringing the steam into actual contact with its work. The retorts used in the destructive distillation of coal and cannel being made either of coarse fire-clay or of iron, and both kinds becoming very leaky by use, there would thus be a serious loss of gas, if means were not used to prevent it. One of these means is exhausting, pumping, or sucking the gas from the retorts. It is well known that a current or stream of any fluid, moving through any fluid medium, produces, or tends to produce, around itself a current in the fluid medium. For instance, if we blow from the mouth through a small tube which projects into a larger tube open at both ends, the interior stream of breath sets in motion the whole body of air in the outer tube, causing at the further end a copious outflow, and at the near end an inflow. This effect has been termed lateral induction. The outer current is named the induced current, the exterior tube the induction tube, and the interior tube the projector. Substitute for the exterior tube filled with air a gas main of the proper diameter filled with gas, communicating through the outlet with the gasholder, and through the inlet with the retorts; substitute for the central jet of breath a jet of high pressure steam, and you have at once a working gas exhauster complete in all its essentials. In practice, a projector with orifice $\frac{3}{8}$ inch diameter, and a cylindrical induction pipe 3 feet long and 4 inches diameter, have been found sufficient to pass 50,000 cubic feet of gas per hour against a back pressure equal to 13 inches of water, and to maintain a steady vacuum of 2 inches in the hydraulic main.

Mechanical Engineering—Testing Rails.—The importance of instituting an efficient test for rails has been brought before the notice of the Institution of Civil Engineers by Mr. James Price. There are certain causes of increased wear in rails which can be avoided; first, the difference in the forms of rail-tops on lines worked as one system, the tyres which run on one portion not fitting the form of rail-top on other parts; and, secondly, the super-elevation of the outer rail on curves not being properly attended to. To facilitate calculation in the latter case, the author had invented a rule which left out the term radius—a chord being found, the versed sine of which was the correct super-elevation for any curve. This chord, for a speed of 40 miles per hour, was 64 feet for the English gauge, and 67 feet for the Irish gauge, a chain length (66 feet) being sufficiently near for either. For any speed and any gauge the rule was—"Length of chord whose versed sine equalled super-elevation $= \frac{1}{2}$ velocity in feet per second $\times \sqrt{\text{gauge}}$."

The qualities sought in a rail are fourfold:—1. Strength as a girder, to sustain a moving load; 2. Toughness, to resist sudden strain or impact; 3. Solidity to resist separation under pressure; and, 4. Hardness, to resist wear of the surface. The flange, or Vignoles rail, stands first as to form, for strength, and the double-headed rail comes next, while the bridge rail is a bad girder form, and 30 per cent of such rails break before they are worn out.

Existing modes of testing rails may be divided into—1. Dead weight test, which is valueless as not being analogous to actual work; 2. Falling weight test, which is unfair, serviceable rails being rejected by it, as it goes as far beyond the requirements of practice as the former test falls short of it; 3. The examination of fracture to ascertain structure is a matter of too great nicety for general use. The machine used by the author for testing rails subjects them to wear analagous to actual use. It consists of a pair of metal rollers, 5 feet in diameter, 16 inches wide, and weighing $2\frac{1}{2}$ tons each, supporting a circular frame or beam weighing $6\frac{1}{2}$ tons; this frame being connected by radii with a centre boss, through which passes a vertical axis. The circle traversed is 40 feet in diameter, and the pressure borne by the rollers is 6 tons and 5 tons respectively. Motion is communicated by a steam-engine, and the machine can be worked at about 20 miles per hour; but it is generally run at a speed of about 13 or 14 miles per hour. The rollers are caused to revolve over a ring or polygon of the rails to be tested until they are broken or worn out, and they bear with a weight equal to that of the driving wheel of a locomotive.

Sea Waves.—At the Friday evening meeting of the Royal Institution of Great Britain, May 26, 1871, Dr. W. J. Macquorn Rankine, C.E., F.R.S., delivered a most interesting lecture on this subject. In the first place he gave a summary, illustrated by diagrams and machines, of existing knowledge of the mode of motion of water in waves, and of the geometrical and dynamical laws which govern the relations between the depth of disturbance of the water, the velocity of advancc of waves, their periodic time, and their length. He referred to the experimental and theoretical researches of previous authors on the subject, such as the Webers, Airy, Scott Russell, Caligny, &c. He then explained the principle, of which Mr. Froude was the first to point out the importance, that the action of water agitated by waves upon a ship tends to make her perform the motions which would have been performed in her absence by the mass of water that she displaces. In still water, the forces of gravity and of buoyancy tend to keep the ship upright, and if she has been heeled over, to restore her to the upright position, and that tendency constitutes the *statical stability* or *stiffness* of the ship. Amongst waves the same forces, combined with the reactions due to the heaving motions of the water and of the ship, tend to place her in the position called *upright to the wave surface;* that is, with her originally vertical axis normal to the wave surface. If the ship yielded passively to that tendency, like a broad and shallow raft, she would accompany the waves in their rolling; and thus, a ship having great *stiffness* may be very deficient in *steadiness.* Every ship has, like a pendulum, a natural period of rolling, depending on her stiffness, or tendency to right herself, and her moment of inertia, being a quantity depending on the distribution of her mass. Stiffness tends to shorten, and inertia to lengthen, the period. It was shown in 1862, by Mr. Froude, that the greatest unsteadiness and the greatest danger of being overturned take place when the periodic times of rolling of the ship and of the waves are equal; for then each successive wave adds to the extent of roll; and if the coincidence of the periods were exact, the ship would inevitably be overturned in the end.

In the course of the present spring it has been pointed out that in well-designed ships a safeguard exists against the occurrence of such disasters. It is well known that no pendulum is absolutely isochronous; but great oscillations occupy a longer time than small oscillations. In like manner, no ship is absolutely isochronous in her natural rolling; but great angles of roll occupy longer periods than small.* Hence, supposing a ship to encounter waves of a period equal or nearly equal to her own natural period for small angles of roll, her angle of rolling is at first progressively increased; but at the same time her natural periodic time of rolling is increased, until it is no longer equal or nearly equal to the periodic time of the waves; and thus she in a manner *eludes* the danger arising from coincidence of periods. In order, however, that this safeguard may act efficiently, it is essential that the natural period of the ship for the smallest angles of roll should not be less than the period of the waves; otherwise the first effect of the progressive increase of angle will be, not to destroy, but to produce coincidence of period; and the result will be great unsteadiness of motion, and possibly great danger.*

The speaker described the above principles as being the latest additions to our knowledge of the theory of the relations between ships and sea-waves; and he illustrated them by means of experiments on a machine so constructed as to imitate the dynamical condition of a ship rolling amongst waves.

Animal Mechanics.—A few weeks ago the Rev. Samuel Haughton, M.D., D.C.L., F.R.S., of Dublin, delivered a series of three Tuesday Afternoon Lectures at the Royal Institution, on "The Principle of Least Action in Nature, illustrated by Animal Mechanics."

Dr. Haughton said that Alphonso Borelli taught Mathematics in the University at Naples towards the close of the seventeenth century; he was also Professor of Anatomy at the University, and his book shows that his scheme of uniting Anatomy with Geometry had the honour of being approved by the Pope, and pronounced to contain nothing dangerous to faith or morals. This book was published in 1680 under the title of "De Motu Animalium." It is full of mistakes because of the want of knowledge of the author of the composition of forces discovered by Newton; but with all its defects it is the only book which can be called a systematic scientific treatise on Animal Mechanics. Two Germans, Edward and Wilhelm Weber, of Göttingen, tried in later times to unite Anatomy and Geometry, and between them produced a very good

* An exception to this rule exists in the case of that form of ship known as the "Symondite," in which the sides flare out at and near the water-line, so as to make the stiffness increase faster than the angle of heel. In such ships the period of rolling *shortens* when the angle increases; and thus the well-known unsteadiness of large vessels of that model is accounted for. In a small boat, whose natural periodic time for the smallest angle of roll is shorter than that of any of the waves she encounters, the Symondite model does not promote unsteadiness; for the shortening of the natural period of rolling removes it farther from coincidence with the period of the waves.

treatise on the motions of the human body, but the work is a less systematic and complete one than that of Borelli.

The lecturer said that he would explain what he meant by "the Principle of Least Action in Nature," by the aid of a few illustrations. Let the earth be supposed to be a lazy animal swimming round the sun in a curve which would enable it to complete the journey with the least trouble and exertion to itself; on this assumption, if two certain points in the path of the planet be known, also the position of the sun, the whole of its path can be accurately calculated and predicted. If a ray of light on striking and being bent from its original path by a piece of glass be supposed to be a living intelligent animal trying to perform its journey through air and glass at the least trouble to itself, on this hypothesis its path can be calculated and predicted, just as easily as by the laws of refraction and reflection. Some time ago he saw some oyster women at the Mumbles Harbour, near Swansea, carrying their oysters along a road which consisted of two parts; there was the slippery shingle of the beach, and beyond that the smooth common. The velocity of these poor women on the shingle and on the common was different. They did not go straight from the shingle to the common as he would have done, but "made a tack;" he afterwards measured the angles made by their path, and made a calculation which proved to him that they had unconsciously taken the path of minimum trouble. Lastly, he cited the instance of the cell of the bee, in which the largest quantity of cell-space is obtained with the minimum quantity of wax.

Dr. Haughton then stated that before proceeding to apply this principle of least action in nature he found it to be necessary to obtain the coefficient of muscular force. What is known to engineers as the coefficient of a rope, means the number of pounds or tons weight necessary to break it across. If a rope of muscle one square inch in cross section were hanging from the roof of the theatre of the Royal Institution, and that muscular rope were acted upon by the will, its coefficient would be the weight it would lift from the ground by contraction. It cost him twelve years of hard work to obtain this coefficient in pounds per square inch for human muscle. He had not succeeded in obtaining it for any other animal but man, the hairy quadrupeds with long tails not being intelligent enough to submit to the necessary experiments. He had determined that 94·7 lbs. per square inch of muscular fibre was the weight which the arms of a young man accustomed to athletic exercises was capable of lifting; 110·4 lbs. was the coefficient of the muscular fibres of the legs; and 107 lbs. of the muscles covering the abdomen; consequently 104·04 lbs. was very nearly the coefficient of muscular force.

He had obtained this information by dint of hard study of cases of cholera, hydrophobia, and lock-jaw in hospitals, in which very painful contortions sometimes occur, but which threw light upon some of the problems in animal mechanics. He had also learnt the secret of working the treadmill in a lazy manner, and could get through the work with the least trouble to himself as well as the most expert burglar in London; he also knew where to place himself to the best advantage on the wheel. Some of these trade secrets he unlocked in the first instance from the hearts of their possessors by means of an ounce of tobacco; he must say that burglars and thieves were much better people than he once believed them to be, and he thought that more good would be done by treating them with kindness than with severity. He had not only to ascertain the power of each particular muscle during life, but to measure its dimensions after death; but if he measured the cross sections of the muscles of aged people after lingering illness, false results would have been obtained. The cross section of a muscle during life is much larger than after death. So he had to watch for cases of severe accidents in the hospitals, but if a man died a violent death the cause was usually so plain, that the coroner would not order a medical examination of the body, and the friends of the deceased were always in a hurry to "wake" him. Those in Dublin who were executed by the hands of the law were nearly always patriots who had shot their landlords, so they had a vast amount of popular sympathy on their side, and it would have been dangerous for any scientific man to dissect their bodies. All these things placed many difficulties in his path. At last a brilliant idea flashed across his mind, which he was compelled reluctantly to set aside, namely, to take a farm in Westmeath, refuse to pay his rent, shoot his landlord, and dissect him at his leisure. He believed, upon his honour, that public opinion in Ireland would not sanction the shooting of a landlord in order to obtain the coefficient of muscular force. But he had overcome the difficulties in his investigation, and had hoped to show that the tendons of the legs and arms of animals were constructed on the principle of least action in nature, as if every one of the muscular organs were itself instinct with life and reason. He added, "It is not the instinct of the planet, or of the oyster woman, or of the bee, that guides them in their path. There is instinct, there is knowledge, there is foresight, there is calculation; it is the wisdom, the knowledge, the foresight, and the calculation, of the great Architect and Geometrician of the Universe."

Dr. Haughton next called attention to the amount of friction in the hands and feet of various animals, and the strength of the various tendons. He said that the animals whose tendons suffer least from friction at the wrist and ankle are the goat and kangaroo. In the wrist of the goat no force whatever is lost, and the most perfect organ of locomotion he was acquainted with among animals was the hind leg of the boomer kangaroo. He also explained why, in certain cases, there must be friction, and how this friction was utilised in the animal economy. He also described the powers and forms of the various muscles, and gave special attention to the most wonderful triangular muscle in the world, the biceps femoris muscle, or the flexor of the thigh of the tiger. While speaking of the strength of tigers, he narrated how he cut the claws of a tiger in the Zoological Gardens of Dublin, because the claws were growing into the body of the foot. Eight men held the tiger close to the bars of the cage by means of ropes. They kept one foot of the tiger off the ground, and this was a great point in the operation, because then the tiger was obliged to keep the other foot on the floor to keep from falling, so could not use it very offensively. As Dr. Haughton was on the point of cutting the claws, the tigress came up to see what was going on. She put out her paw, laid hold of Dr. Haughton by the hat, and in doing so, lifted up the door of the cage, leaving him face to face with the animals. The eight men resolved themselves into their component elements; all of them but one ran away. The tiger bent itself for a spring, Dr. Haughton pulled down the door of the cage, and the animal broke its teeth against the bars. The seven cowards were then collected, and the claws of the tiger were cut, after which the animal began to purr, and licked the hand of the operator.

The muscle in the leg of a man is like a rope of parallel prismatic fibres; in most other animals it is arranged as a prismatic muscle. He asked, "Why has nature deliberately sacrificed a certain amount of force by putting a triangular muscle into the leg of a tiger to do the work which she does so effectively in my leg by a straight rope or muscle?" The answer was that the weight of muscle necessary to enable the tiger to make his great springs was so enormous, that if it had been placed as a straight rope from point to point, it would not only be a great deformity in his appearance, but would seriously impede him in his progress through the jungle. "No, I cannot do that," says Nature, "I must preserve beauty of form," making the tiger the most beautiful creature which God has created. Therefore the tiger was given a triangular muscle, with a certain amount of loss of force; but there is a gain by spreading the muscle over a great surface—a gain in the packing and shape of the leg. The lecturer added that he was sorry to upset the superstitions of childhood, but the tiger is a much more powerful animal than the lion, and will always beat the latter in a fight. The lion had a great mane and looked big; still he was a humbug, like a good many people he knew.

Dr. Haughton said much about the wings of birds, and the muscles which govern their motions, after which he spoke of the force with which the muscles of the heart pump the blood through the veins of animals. He said that it had been proved in the early part of this century, that the muscular force exerted by the living heart of a cow will raise a column of fluid blood nine feet high in a tube. The experiment cannot be made with the heart of a living man, as death would be the result, but the height to which the blood will spout when the artery of a man is cut is known; also the height to which the blood of a cow will spout under like conditions. By comparing the two phenomena, the hydrostatical pressure exerted by the muscles of the heart of a man has been approximately ascertained, as well as by another method. The blood of a horse or cow will spout about 2·53 feet high when an artery is cut, and the blood of a man 2·58 feet high; the blood would rise much higher than this in tubes, because, when an artery is cut, Nature makes an effort to close the orifice, to stop the bleeding. By various methods of experimenting he had come to the conclusion that the muscular force constantly exerted by the heart of a man is competent to raise between twenty and twenty-one pounds one foot high in a minute of time.

He also gave some little attention to the low musical note given by a muscle when it contracts. When a muscle contracts it gives a low sound, like the distant roar of cabs rattling along a rough street. There are several ways of hearing this sound. One is, when lying in bed at night, to lay one ear on the pillow, so that the pillow shall act as a sounding-board, and then to clench the teeth. The contraction of the muscle which acts upon the jaw, will then give the low roaring noise just mentioned. By means of tuning-forks, he and others had determined that this musical sound corresponded to $35\frac{1}{3}$ double vibrations per second.

He said that when muscular fibre contracts it contracts to $\frac{8}{9}$ of its length. On measuring the muscular fibres of the heart, he found that they were so arranged that they could all contract to $\frac{8}{9}$ of their length, and some of them in so doing gave the interior of the heart a squeeze, which expressed the last drop of blood from the pumping apparatus, so that no force was lost. The human heart beats 57 times in a minute. He closed his lectures by speaking of the perfection of the muscle which gives birth to the young of animals, and with a few remarks to the effect that science was not opposed to theology.

CHEMICAL SCIENCE.

Further particulars have now reached us as to the remarkable discovery by Professor Seely of the solubility of metallic sodium and other metals in ammonia. He prepares the ammonia gas in an iron-generator, to which is attached a stout glass tube containing the metal to be subjected to the ammonia. When sodium is subjected in this apparatus to the condensing ammonia, before any ammonia is visibly condensed to the liquid state, it gradually loses its lustre, becomes of a dark hue, and increases in bulk. The solid then appears to become pasty, and at last there is only a homogeneous mobile liquid. During the liquefaction, and for a little time after, the mass is of a lustrous, copper-red hue; the condensation of the ammonia and its mingling with the liquid steadily goes on, the liquid is progressively diluted, and passing through a variety of tints by reflected light, at last it becomes plainly transparent and of a lively blue, as well by reflected as by transmitted light; the liquid now closely resembles a solution of aniline blue or other pure blue dye-stuff. On reversing the process by cooling the ammonia generator, the ammonia gradually evaporates out of the liquid, and the changes observed during the condensation re-appear in the reverse order, till at last the sodium is restored to its original bright metallic state. If the evaporation be conducted slowly and quietly, the sodium is left in crystals of the forms seen in snow. The formation of the transparent blue liquid and the restoration of the sodium are steadily progressive, and the repeated and closest scrutiny of the process has failed to reveal the slightest break or irregularity in its continuity. The inevitable conclusion from such facts is that the blue liquid is a simple solution of sodium in ammonia, not at all complicated or modified by any definite chemical action. The brilliant and varied colours exhibited in the experiment may seem anomalous to some, when, in fact, a closer scrutiny of the case will show that they might have been predicted. Sodium appears white to the eye, but with the white light reflected from its surface to the eye there are always mingled red rays. If most of the incident white light were normally decomposed, sodium would appear as a brilliant red metal. Ammonia favours such a decomposition probably by reducing the density and opacity of the surface, and thus the concentrated solution of sodium is lustrous copper-red by reflected light. What should be the colour by transmitted light? Not red, for the red rays do not penetrate the substance; the colour must be looked for in that which is complementary to red; it must be blue or yellow, or combinations of these. A continuance of the argument will bring the conclusion that the colour by transmitted light will be blue. Intense blue tinctorial substances, like aniline blue, indigo, and Prussian blue, all illustrate the phenomena of colour of the sodium solution; they are metallic red when concentrated, and if the solvent be applied in vapour as in the sodium-dissolving experiment, there will be the same modification of colour exhibited. Sodium has a remarkable tinctorial power, which seems not to be surpassed by that of any of the aniline colours.

Referring to what was said in our January number, p. 127, about the earth-eating of the Javanese and the miners about Wurtemburg, a correspondent gives us the following interesting particulars:—He writes that an old gentleman, a friend of his, who was born and bred at Dunsire, near Dolphinton, in Lanarkshire, said that when a boy at school there, he and his schoolfellows used to eat a sort of clay which they found somewhere near the banks of the little river Medwin. He described it as of a light brown colour; it broke into thin plates about like thin oat-cake, was of a sweetish taste, and melted in the mouth. Perhaps it is yet to be found in that neighbourhood; and, if so, we should be very glad to have a sample for examination.

Gunning has discovered, in acetate of zinc, a reagent that precipitates the slightest traces of the colouring matter of blood from solutions, even where the liquids are so dilute as to be colourless. Blood, washed from the hands in a pail of water can readily be detected in this way. The flocculent precipitate, thrown down by acetate of zinc, must be washed by decantation, and finally collected on a watch-glass, and allowed to dry, when the microscope will readily reveal hæmin crystals if any blood be present.

The artificial preparation of coniine, an oily liquid, highly poisonous, and closely resembling the nicotine obtained from tobacco, has more than ordinary interest, as it suggests the possibility of our being able to make other alkaloids, such as quinine, morphine, and the like; and if we can succeed in this, why not prepare the less complex compounds, sugar, starch, &c.? Coniine has been artificially prepared by Hugo Schiff, by heating alcohol and ammonia at 210°, together with butyraldehyd, precipitating with a platinum salt, and distilling the product. The artificial alkaloid exhibits the same properties as the native, and is a violent poison. As the first step in the synthesis of vegetable alkaloids, the discovery of Professor Schiff is one of the most important in modern chemistry.

Recent occurrences have rendered anything connected with the consumption and production of food a matter of great interest. In a public lecture delivered at the Medical School at Paris, for the purpose of imparting some sound knowledge on the quantity of food required to keep man in a vigorous and healthy state, Dr. Sée said that the daily ration for an adult might be enumerated as follows:—100 grms. of meat, 20 grms. of salt fish, 750 grms. of bread, 50 grms. of lard, and 50 grms. of dried and compressed vegetables—a total of 970 grms. of solid food, containing 88 grms. of albuminous matter. Incidentally, the author observed that crust of bread contains just twice more nutrimental value than crumb, which contains 44 per cent of water. The highly nutritive value of wine was specially alluded to, and illustrated by the fact that, in some districts of France and Spain, men live on bread and wine only for many weeks together in a healthy and vigorous state.

Some experiments have been made with dogs on the influence which coffee and cacao exert as food, in which Dr. Rabuteau gave diets in one case consisting daily of 20 grms. of bread, 10 grms. of fresh butter, and 10 grms. of sugar; in the other case, 20 grms. of cacao, 10 grms. of sugar, and an infusion of 20 grms. of well-roasted coffee. From these experiments the author draws conclusions leading him to consider coffee and cacao as simply preventing de-nutrition. This view has been objected to by MM. Payen, Dumas, and Chevreul. As regards cacao (commonly, but erroneously, in this country named cocoa), there can be no doubt that, containing as it does from 17 to 20 per cent of albuminous matter, with from 10 to 12 per cent of starch, from 40 to 50 per cent of fat, and among its mineral matter phosphates, it is food. M. Chevreul very properly observes, among other matters, the existence of idiosyncracy, and its influence on the individual tastes, and hence also more or less on the action of various alimentary substances, pointing out that he himself has, from his earliest years, an invincible repugnance against wine, milk, fish, and various vegetables, none of which he ever partakes of, but for all that it would, of course, be absurd to deny the nutritive properties and value of these substances.

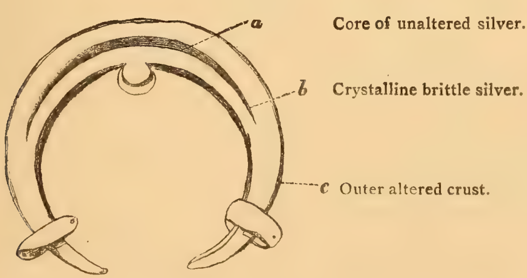
The occurrence of the metal manganese in the mineral kingdom has been a matter of discussion and some little doubt. Dr. J. E. De Vry states that, while studying in Germany, thirty years ago, he collected beech-nuts in the vicinity of Giessen, and found the ashes of these nuts to contain a rather large percentage of manganese, which was readily explained by the fact of that mineral being present in that locality rather abundantly. In 1847, being at the meeting of the British Association at Oxford, the author gathered some unripe beech-nuts in Blenheim Park, which, on being afterwards tested, were found to contain a relatively large amount of manganese, although grown in a very different soil. A third analysis of the ashes of beech-nuts, collected in the wood of the Hague, confirmed the same fact.

Dr. A. Vogel has described a simple experiment to illustrate the action of dilute sulphuric acid upon starch. Since nearly all kinds of writing paper are nowadays so very largely sized with starch that they are coloured blue by a dilute solution of iodine, the following experiment may be made to illustrate visibly the change which starch undergoes when acted upon by dilute sulphuric acid, for which purpose writing or other figures are traced on the paper with very dilute sulphuric acid, and a gentle heat applied, care being taken not to char the paper. When next a dilute solution of iodine is painted over this paper, the portion acted upon by the dilute acid remains white, while the rest becomes blue; but since, in course of time, this blue colour disappears altogether, the same piece of paper may be repeatedly used for this experiment.

The true constitution of artificial ultramarine has long been a matter of doubt. Dr. W. Stein has written a long essay on the mode of combination in which the sulphur is present in it. The author states that his aim is to prove that blue ultramarine contains sulphurous, not hyposulphurous acid, which former, however, is not an essential constituent; and further, that ultramarine contains only sulphuret of aluminium, there being no sulphuret of sodium at all. The conclusions drawn by the author from his experiments are—Ultramarine consists mainly of a white mass, through which and with which black sulphuret of aluminium is most intimately and molecularly incorporated; the blue colour of ultramarine is, therefore, not due to its chemical composition, but to the optical relation of its component substances. The essay winds up with some observations on white and green ultramarine; the latter contains less soda than the blue-coloured pigment, and that, again, less soda than the white pigment. The quantity of sulphur contained in blue-coloured ultramarine is less than that contained in green-coloured ultramarine.

The examination of some ancient silver ornaments has led Professor Church to a curious result, which may prove of interest to some of our readers. The silver objects referred to were obtained lately by General de Cesnola in the Island of Cyprus. He opened several hundreds of tombs, specially at Dali, the site of the ancient city of Idalium, and obtained large quantities of specimens of antique glass and metal-work. The silver specimen with which the present note is concerned cannot have an antiquity of less than 1500 years, but the date of its manufacture may prove to have been even more remote. It was shaped like a fibula. It resembled a crescent in that the ends of the bow were small and pointed and its middle much stouter; sections throughout were nearly cylindrical, and in the widest part about ¼-inch in diameter. The specimen was nearly covered with a dark sub-metallic crust, and underneath this a soft powdery grey substance was observable. Both these layers, which together presented an average thickness of 1-30th of an inch, could be readily removed by the finger-nail. Below, the substance appeared white, metallic, and uniform, but yet was remarkable for its extreme brittleness, the object being easily snapped in numerous pieces by a very slight pressure. If the brittleness had pertained to the outer crust or layer, obviously altered as it was, it would not have been surprising. But to find that an apparently continuous metallic substance of great lustre, and, certainly once, at the time of its manufacture, in a perfectly ductile condition (for portions of it had been twisted into fillets), to find this metallic substance as brittle as a stick of chalk was certainly difficult to explain. And a further difficulty was offered by the observation that in the thicker part of the crescent, and only there, a kind of core existed similar in colour to the main mass of the object, but possessing great tenacity and ductility. This core could not be broken short off, but might be wrenched out of the surrounding brittle mass, though with some difficulty. It tapered off towards the narrower ends of the crescent, and terminated where those parts did not exceed ⅛th of an inch in thickness. The outer crust consisted of the corroded and chemically altered silver-alloy. It was made up of finely-divided metallic silver, with traces of the sulphide and chloride of that metal, and it gave also a faint trace of iodide; copper, apparently in the form of a basic carbonate, was likewise present, with a trace of gold. The main part of the specimen consisted of the second and extremely brittle layer referred to before. On analysis it was found to have the composition:—Silver, 94·69 per cent; gold, 0·41; copper, 3·43; lead, 0·28; antimony, with trace of arsenic and bismuth, 1·21. The central ductile and malleable core had *precisely* the same composition as this brittle layer. To what, then, are the physical differences between them to be attributed? Not to chemical differences or chemical changes, but to a *molecular change* which, in the course of ages, has occurred throughout all the thinner parts of the piece of silver, but had left unchanged in the thickest part a central fusiform core. A crypto-crystalline structure had been produced in the previously homogeneous alloy, which caused the peculiarly

FIG. 8.

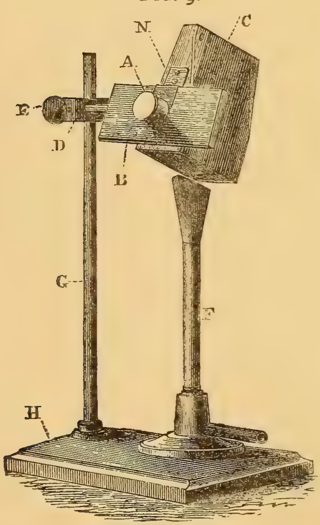


Section (in the plane of the paper) of the silver object referred to.

easy fracture of the metal. A smart blow with a hammer was capable of shattering it to powder, while by rolling or gentle hammering, the brittle mass could be restored readily to its pristine condition. The density of the brittle silver was 9·06, but, by rolling, it became 10·20. There was not, therefore, a chemical change, but a physical one. However produced, whether by minute and long-continued alterations of temperature, or by other agencies, this molecular change of silver may possibly throw some light upon the similar change observed in the case of iron.

A very convenient arrangement for heating crucibles by gas is described by Professor J. Lawrence Smith, in Crookes's "Select Methods in Chemical Analysis," p. 408. It has been found of the greatest use in the analysis of silicates, but it will be equally useful in other branches of analysis where fusions at a high temperature are required. The figure here given illustrates the stand, burner, crucible, &c., and is about one-sixth the natural size. H is the stand with its rod, G. D is a brass clamp, with two holes at right angle to each other; having two binding screws, it slides on the rod, G; the second hole is for a round arm attached to B, the binding-screw, E, fixing it in any position. B is a plate of cast-iron, 5 to 6 m.m. thick, 10 to 11 centims. long, and 4½ centims. broad, having a hole in its centre large enough to admit the crucible to within about 15 m.m. of the cover without binding. A is the crucible, which is made to incline a few degrees downwards by turning the plate of iron that supports it. C is a chimney of sheet-iron, 8 to 9 centims. long, 10 centims. high, the width at the bottom being about 4 centims. at one end, and about 3 centims. at the other end. It is made with the sides straight for about 4 centims., then inclines towards the top, so as to leave the width of the opening at the top about 1 centim. A piece is cut out of the front of the chimney of the width of the diameter of the hole in the iron support, and about 4 centims. in length, being semicircular at the top, fitting over the platinum crucible. Just above this part of the chimney is rivetted a piece of sheet iron in the form of a flattened hook, N, which holds the chimney in place by being slipped over the top of the crucible support; it serves as a protection to the crucible against the cooling effects of the currents of air. F is the burner, the upper opening of which is a slit from 1½ to 2 m.m. in width, and from 3 to 4 centims. long, and when used is brought within about 2 centims. of the lowest point of the crucible, the end of the flame just playing around the lower end of the crucible. The gas enters the lower part of the burner by two small holes of 1-16th of an inch, furnishing at 1-inch pressure about 5½ cubic feet of gas per hour. It is surprising to see the effect produced by this simple burner as here used; 8 grms. of precipitated carbonate of lime can be decomposed to within 2 or 3 per cent in one hour, and when mixed with silica or a silicate, in a very much shorter space of time. A method invented by Thiercelin has been successfully used in extracting iodine from crude Chili saltpetre. The mother-liquors resulting from the manufacture of saltpetre are treated with a mixture of sulphurous acid and sulphite of soda, and the iodine will be precipitated as a black powder. This is placed in earthen jars, on the bottom of which are layers of quartz-sand, fine at the top and coarse at the bottom; from these jars it is removed by earthen spoons lined with gypsum, and the greater part of the water is thus separated. It is further purified by sublimation, but is often sold before undergoing the last-named process. The amount of iodine thus reclaimed from Chili saltpetre already amounts to 30,000 pounds per annum.

FIG. 9.



Up to a few years ago indigo was considered to be insoluble in almost all agents which did not decompose it. Now several agents are known in which it can be dissolved and from which it crystallises on cooling. So far back as 1861 an editorial article in the "Chemical News" contains the following passage:—"Hot aniline dissolves indigo, forming a solution having almost exactly the tint of the solution of azuline in methylated spirit. On cooling, the indigo crystallises out in beautiful coppery spangles. This method of crystallising indigo from a solution in boiling aniline does not appear to be generally known to chemists." Dr. V. Wartha now states that Venetian turpentine, heated to its incipient boiling-point, is a solvent for indigo, which, after cooling, is readily purified by the aid of ether or alcohol. Boiling paraffin also dissolves indigo-blue, and the paraffin can be removed, after cooling, by means of benzol. Spermaceti, stearic acid, and chloroform, all at a high temperature, are stated to be more or less good solvents for indigo.

According to the German chemist Kopp, a mixture of two liquids boils at a lower temperature than either of the component liquids separately. Mr. D. Howard has verified this law in the case of a mixture of amylic alcohol and water, which boils at a lower degree than pure water.

M. A. Valenciennes, of the chemical works at St. Denis, near Paris, has lately prepared metallic manganese and several of its alloys. The metal was obtained by the reduction of pure binoxide of manganese in a magnesia crucible, and formed a brittle and very hard button. Immediately after breaking it, the pieces were as white as cast-iron, but more rapidly oxidised by the air. Manganese shows great affinity for copper. Valenciennes prepared alloys of copper and manganese, containing from 3 to 20 per cent of manganese, all of which resemble very much the copper-tin alloys (bronzes), being, like these, hard, sonorous, and easily fusible. The alloy, containing 15 per cent of manganese, is grey, very hard, brittle, fuses like bronze, is easily cast into moulds, and remains unchanged for some time. The alloy with 12 per cent of manganese is also brittle and very hard, grey after being turned, but soon becomes yellow as brass.

A cold solution of bichromate of potassa in nitric acid is, according to Dr. Böttger, an excellent test for the genuineness of silver plating on metals. The metallic surface to be tested is first of all cleaned with strong alcohol, in order to remove dirt, fatty matter, and especially any varnish. A drop of the test fluid is then applied to the metallic surface by means of a glass rod, and immediately afterwards washed off with some cold water. If pure silver is present, there will appear a blood-red coloured mark (chromate of silver). Upon German silver the test liquid appears brown, but after washing with water the blood-red coloured mark does not appear; the so-called Britannia-metal is coloured black; on platinum no action is visible; metallic surfaces coated with an amalgam of mercury yield a reddish speck, which, however, is entirely washed off by water; on lead and bismuth the test liquid forms a yellow-coloured precipitate; zinc and tin are both strongly acted upon by this test liquid; the stain as regards the former metal is entirely removed by water, while, as regards the latter, the test liquid is coloured brownish, and addition of water produces a yellow precipitate which somewhat adheres to the tin.

A method of imparting a yellowish hue to white marble is sometimes wanted. Dr. R. Weber has made known the fact that alcoholic solutions of perchoride of iron are not precipitated by carbonate of lime, and may therefore be applied in different degrees of concentration to impart a more or less deep yellow hue to white marble. The converse of this—the removal of yellow stains from white marble—is still a desideratum.

In experimenting upon the mother-liquors of the manufacture of sulphate of quinine, Mr. D. Howard has occasionally been perplexed by an unusual loss in re-crystallising, which the mechanically adhering mother-liquor did not seem to account for. A more careful examination showed that the cause was the presence of an alkaloid hitherto undescribed, the extreme solubility of the salts of which both distinguishes it at once from the chinchona alkaloids already known, and renders it very difficult to separate from the uncrystallisable quinoidin. The platino-chloride is almost insoluble in water or in cold hydrochloric acid; it forms a crystalline powder by precipitation, and well-defined crystals by solution in acid. It is isomeric with the platino-chloride of quinine, but anhydrous. The author has investigated the sulphate, tartrate, citrate, hydrochlorate, phosphate, and sulphocyanide of the new base. He has, however, not yet found out whether this alkaloid is contained in all the species of cinchona, or, if not, in which of them.

Dr. H. Fleck has published the account of a series of experiments made with the view to ascertain how far it is possible to substitute for the ordinary process of malting the method of steeping the grain (barley or any other) in weak acids, to obtain thereby the same effect as produced by germination, but in a far shorter period of time. It appears that, provisionally, the author has succeeded in his attempt, but is engaged in further experiments. Dilute nitric acid, containing 1 per cent of acid, yields excellent results.

It is well known that, when glycerine, subjected to the ordinary atmospheric pressure, is heated so much as to cause ebullition, it is more or less rapidly decomposed by repeated distillations. This decomposition may be, however, entirely prevented by a reduction of the pressure in the apparatus employed to 12·50 m.m. Mr. T. Bolas, who has worked for some time on this subject, has determined the boiling-point of glycerine, by effecting the distillation in a long-necked flask, having a supplementary neck attached at right angles to the principal one. In the principal neck the thermometer was fixed by the aid of a caoutchouc cork, while the smaller neck was connected in a similar manner with a two-necked receiver. The glycerine, together with a few fragments of tobacco-pipe (this latter being required to prevent the bumping which would otherwise occur), being placed in the retort-flask, the receiver was connected with a Sprengel's mercurial pump and a manometer, the caoutchouc joints being made air-tight with glycerine in the usual way. Unless the glycerine distilled had been dehydrated by previous distillation in a vacuum, the first portion of the distillate consisted principally of water; afterwards, when the glycerine in a pure state came over, the temperature indicated by the thermometer was 179·5° C., the pressure on the liquid being 12·5 m.m. Under a pressure of 50 m.m., glycerine distils without change at about 210° C. Glycerine, dehydrated by distillation, absorbs water from the atmosphere to the extent of about 50 per cent of its weight.

It is a striking lecture experiment in illustration of the fact that the terms combustible and supporter of combustion are only conventional, to show that in an atmosphere of hydrogen, oxygen burns with a flame, just as hydrogen does in air or oxygen. Professor Thomsen, of Copenhagen, has refined upon this experiment by making oxygen burn with a sooty flame. Take a long-necked flask; pour into it some benzol or oil of turpentine; close the flask with a doubly-perforated cork through which two short glass tubes are passed, one of which should be of about 1 centimetre internal diameter, the other narrower and somewhat bent sidewards. Let the liquid in the flask be boiled, and, as soon as the vapours issue from the wider tube, ignite them; and, this having been done, there is passed through that tube another narrower glass tube connected with a suitable gas-holder containing oxygen. This tube should be provided with a platinum burner bent upwards and fitted with a cork, which closes the opening of the wider tube. This oxygen-carrying tube is made to pass deep into the flask; and immediately after the closing of the 1 centimetre wide tube, the oxygen begins to burn with a sooty flame, while the excess of the vapours of the boiling liquid are discharged by the narrower glass tube above alluded to.

Dr. E. von Gorup-Besanez has given an account of the very serious effects of the explosion of only 10 drops of nitroglycerine, which were put into a small cast-iron saucepan by one of the pupils of the author, in his laboratory, and heated with a Bunsen gas-flame. The effect of the explosion was that forty-six panes of glass of the windows of the laboratory were smashed to atoms, the saucepan was hurled through a brick wall, the stout iron stand on which the vessel had been placed was partly split, partly spirally twisted, and the tube of the Bunsen burner was split and flattened outwards. Fortunately, none of the three persons present in the laboratory at the time were hurt. The author takes the opportunity of referring to a communication of Dr. E. Kopp, bearing upon the conditions under which nitroglycerine explodes or burns off quietly. When nitroglycerine is caused to fall drop by drop on a thoroughly red-hot iron plate, it burns off as gunpowder would do under the same conditions; but if the iron is not red-hot, but yet hot enough to cause the nitroglycerine to boil suddenly, an explosion takes place.

Dr. Von Kobell gives an account of the testing of various minerals containing lithia with the spectroscope. The author puts the pulverised minerals on a piece of platinum foil formed like a channel, perforated with small holes, and held by means of a pair of platinum forceps. The author observes that, unless the mineral to be tested for lithia is previously well moistened with hydrochloric acid, the detection of lithia may fail, since only the spectrum of the flame tinged by chloride of lithium exhibits the characteristic line. Several minerals have also been tested by this author for thallium, and more especially some zinc ores. Most of these minerals gave a negative result; but the sphalerite (a kind of zinc blende) from Geroldseck, and a similar mineral from Herbesthal (a small Prussian village close to the Belgian frontier), gave very evident indications of the presence of thallium.

The popular toy, "Pharaoh's Serpents," has gone almost out of use, owing, probably, to the poisonous nature of the ingredients and the danger of inhaling the mercurial fumes they evolved. Dr. Puscher has published the recipe for a mixture for producing so-called Pharaoh's Serpents which are not attended with injurious fumes. Mix intimately together 2 parts of bichromate of potassa, 1 part of nitrate of potassa, and 3 parts of white sugar. This mixture should be pressed in paper or tinfoil cones, and, if intended to be kept for any length of time, the paper should be varnished over with sandarac varnish. A small quantity of balsam of Peru may be added to perfume the mixture, so as to cause its combustion to be attended with a pleasant odour. The greenish coloured very porous mass, which assumes the serpent shape, is a mixture of carbonate of potassa, oxide of chromium, carbon, and a small quantity of neutral chromate of potassa. The author states that this mass is an excellent substance for polishing all kinds of metals. The mixture above specified should be kept in a dark place.

Dr. Gräger has proposed a new method for the reduction of chloride of silver dissolved in ammonia by means of metallic zinc. This process, according to the author, succeeds very well, and yields a silver of greater purity than is obtained by the process for reduction of silver by the moist way now in use, provided the operation be carried on in closed bottles. The silver, after complete reduction, is first thoroughly washed with concentrated hydrochloric acid, next with water, and, lastly, for some moments with dilute ammonia, and then again with distilled water. It is clear that this method of reduction involves the use of a considerable quantity of ammonia; but this, the author states, can in great measure be recovered by distillation.

A mode of distinguishing the deposit of arsenic obtained by Reinsch's process from salts of mercury has been published by Mr. James St. Clair Gray. It is for mercury a test of even greater delicacy than that afforded by sublimation of metallic globules from the coated copper foil, and is possessed also of the advantage that the result can be seen by the unaided vision, while its application is extremely simple. It is founded on the great affinity which exists between mercury and gold. One of the copper slips, coated in the ordinary manner by Reinsch's process, is first washed in pure distilled water and then thoroughly dried; when thus prepared it is rubbed with a flattened bead of pure gold, or, should this not be at hand, a flat signet ring will suffice. The result, if the coating be mercurial, is that a portion of the mercury, whose affinity for gold is greater than for copper, is transferred from the copper to the gold, appearing on its surface as a clear, white, shining, metallic crust, this being more conspicuous the more highly coloured the surrounding gold is. The stain is at once removed by pure strong nitric acid. This is of itself perfectly conclusive of the presence of mercury in the metallic coating or deposit on the copper, and is equally applicable should there exist in that deposit a combination of mercury with any or all of the other three metals which yield by Reinsch's process a metallic deposit on copper foil.

MINERALOGY.

At present we are in great measure ignorant of the conditions which are necessary for determining the appearance of certain faces in a crystal. Chemical research has, indeed, afforded some insight into this subject; we know, for instance, that alum may be made to crystallise in cubes by the presence of alkaline carbonates; in octahedra by the presence of sodic nitrate; and in a combination of the two forms by the presence of cupric nitrate. In the case of most native crystals, however, the subject is still so obscure that any attempt at its elucidation must needs be welcome. Dr. A. Stelzner, of Freiberg, who has, we believe, been recently appointed Professor of Mineralogy in the University of New Cordova, publishes a memoir, in which he seeks to discover what natural causes have induced the presence of trapezohedral planes in certain crystals of quartz.* Compared with the ubiquity of quartz the appearance of these faces is extremely rare. The author collects all the known instances of their occurrence, and records the accompanying minerals, with their relative age. These paragenetic studies show that the minerals associated with trapezohedral quartz are chiefly apatite, axinite, datholite, fluor-spar, mica, topaz, tourmaline, beryl, scheelite, iron-glance, anatase, rutile, brookite, sphene, wolfram, and cassiterite; and that the quartz must in general have crystallised nearly contemporaneously with these substances. As many of these associated minerals contain compounds of fluorine, chlorine, or boron, the author deduces the conclusion that the presence of these elements must be connected with the trapezohedral habit of quartz. Moreover, it is found that small faces of this form may be developed on quartz by etching the surface with hydrofluoric acid. The author's studies lead him to the general conclusion that the trapezohedral form of quartz is restricted to those cases in which the crystals have been formed contemporaneously with certain minerals of the tin or titanium formation, with development of hydrochloric or hydrofluoric acid.

Mr. Robert H. Scott, of the Meteorological Office, has presented to the Geological Society some notes on the interesting group of minerals found in the mines of Strontian, in Argyllshire. This locality, which gives its name to the element *Strontium*, contains several lead-mines, which yield many rare zeolitic minerals, especially harmotome and Brewsterite. The former has been found chiefly at Bell's Grove Mine, and the latter at Middle Shap Mine, whilst both of them have been obtained from Whitesmill Mine. At the newly-opened workings at Corrantree, fine crystals of calcite occur in association with well-twinned crystals of harmotome. The mine is opened in gneiss, whilst the old mines were worked at the junction of the gneiss with the granite. In all these workings the gangue is remarkable for the absence of fluor-spar, and the comparative rarity of zinc-blende and barytes; whilst the galena is but slightly argentiferous.

Eosite is the name which Dr. Schrauf has applied to a new mineral from Leadhills, in Scotland. It occurs in small aurora-red octahedra, sparsely scattered over the surface of a greenish-yellow cerussite, and accompanied by very small yellow needles of pyromorphite. The new species crystallises in the pyramidal system, and contains molybdenum, vanadium, and lead, forming a connecting-link between wulfenite and vanadinite, and yet essentially different from the well-known forms of red wulfenite. The name has reference, we presume, to the aurora-red colour of the crystals.

A new mineral discovered by the distinguished geologist, Dr. Gümbel, has very properly received the name of *Gümbelite*. It is a hydrous silicate of

* Quarz und Trapezoëderflächen: eine paragenetische Skizze. Leonhard u. Geinitz's Neues Jahrbuch, Heft i., 1871, p. 33.

alumina, potash, ferric oxide, &c., and occurs at Nordhaben, near Steben, in Upper Franconia. The mineral forms thin shortly-fibrous layers on clay-slate and on iron pyrites, and presents a greenish-white colour, coupled with a pearly lustre.

The well-known locality, Lüneburg, in Hanover, has given its name to a new species termed *Lüneburgite*. It is a boro-phosphate of magnesia, probably deposited from the mother-liquor of sea-water.

Under the name of *metacinnabarite*, a native amorphous sulphide of mercury has been described by Mr. G. E. Moore. The mineral was found by Professor Whitney in Lake Co., California, and contains—sulphur, 13·82; mercury, 85·79.

According to the Abbé Moigno, a valuable deposit of native phosphate of lime has been discovered near St. Petersburg. The layer occurs at a depth of about 1·8 metre below the surface, and was brought to light during some recent excavations for drainage.

Von Kobell has detected thallium in the zinc-blende, or sphalerite, of Geroldseck and Herbesthal; whilst the blende of many other localities was examined for this element with only negative results.

It is so long since Dr. Schrauf issued the first part of his fine "Atlas of Crystal-Forms," that we had almost ceased to expect a continuation of this valuable work. It appears, however, that the delay has arisen from causes which neither author nor publisher could control, and we may now hope that the succeeding parts will follow in rapid succession. The second instalment just issued* contains clear drawings of crystals of anglesite, anhydrite, anorthite, antimony, antimonite, apatite, and aphanesite. It may be well to note that this last name is applied to the arseniate of copper, so well known as clinoclase—a name which, in spite of its priority, has been transferred from this species, and applied to some of the felspars—clinoclase being of course the proper correlative of orthoclase and plagioclase.

Many attempts have from time to time been made to represent mineral specimens by plates painted in natural colours, but such attempts have rarely been successful. A second edition of Weber's work "On the Minerals of Bavaria," has now appeared under a new title,† and contains 64 plates of minerals tinted after nature. The present edition has been prepared with the assistance of Dr. Haushofer.

A work on the mineralogy of volcanoes, by Dr. Landgrebe,‡ gives in alphabetical order descriptions of all minerals found in rocks of volcanic origin, whether these minerals are original constituents of the rocks or merely subsequent products. Its especial value lies in notices of minerals occurring in the two Hesses and the adjacent countries, where the author has done original work. For example, he has discovered graphite in the columnar basalt of the Lammsberg.

Naumann's "Elements of Mineralogy" has now reached an *eighth* edition.§ It is only two years since the last edition was issued, but the form of the work has since been much improved, and formulæ are now given on the modern atomic weights.

TECHNOLOGY.

The first report of the Royal Commission on Scientific Instruction and the Advancement of Science has just been published. From it we make the following abstract:—The commissioners consider that there is no necessary con-

* Atlas der Krystall-Formen des Mineralreiches. Zweite Lieferung. Tf. xi.—xx. Wien, 1871.

† Die Mineralien in 64 coloriten Abbildungen nach der Natur, von J. C. Weber. Zweite Auflage: verbessert und vermehrt unter Mitwurkung von Dr. K. Haushofer. 8vo. München. Pp. 99.

‡ Mineralogie der Vulcane, von G. Landgrebe. 8vo. Cassel und Leipzig. Pp. 396.

§ Element der Mineralogie, von C. Naumann. Achte vermehrte und verbesserte Auflage. Mit 836 figuren. 8vo. Leipzig. Pp. 606.

nection between the direction of the Geological Survey of Great Britain and Ireland and the government of the Royal School of Mines. The Royal School of Mines and the Royal College of Chemistry, which practically constitute one school of pure and applied science, are not organised in such a manner as to enable them to perform efficiently the work for which they were originally, or are, at present, intended. This conclusion is based upon three grounds: (*a*) the absence of a chair of mathematics; (*b*) the absence of physical or biological laboratories in which students can receive practical instruction; (*c*) the insufficiency of accommodation in the Royal College of Chemistry. After enumerating the various inconveniences attending the present arrangement, the commissioners recommend: (*a*) That the building in Jermyn Street be given to the Survey and to the Museum, with the reservation that the lectures to working men be delivered as heretofore in the theatre; (*b*) That the building in Oxford Street be vacated by the Royal College of Chemistry; and (*c*) That the Mining Record Office be lodged with the statistical department of the Board of Trade, or, failing accommodation there, in the building now occupied by the Royal College of Chemistry. The commissioners also recommend that the Royal School of Mines and the Royal College of Chemistry be consolidated; that mathematics be added to the courses of instruction now given; and that sufficient laboratories and assistance for giving practical instruction in physics, chemistry, and biology be provided. The institution thus formed (hereinafter called the "Science School") may be conveniently and efficiently governed by a council of professors, one of that body acting as dean. Attention is drawn to the buildings at South Kensington, now nearly completed, and intended for the reception of a projected school of naval architecture and science; and a recommendation is made that the science school should be accommodated in these buildings. Concerning the Royal School of Naval Architecture and Marine Engineering, now conducted at South Kensington, they recommend that the theoretical instruction of that school should in future be given in the science school, the general instruction in mathematics, physical science, and mechanical drawing thus becoming common to both schools. They also recommend that no additional buildings, and no reconstruction of the temporary buildings at present occupied by the Royal School of Naval Architecture and Marine Engineering, should be undertaken until a further report has been received from this commission. Referring to the system of teaching elementary science under the science and art department, the commissioners are of opinion that the quality of the instruction given under this department would be greatly improved if the teachers received practical instruction in elementary science. Such instruction has, indeed, already been given with marked advantage, although only to a limited extent. The science school will be available for the instruction of many science teachers throughout the country.

From a lengthy series of researches by Dr. A. Schwarzer on the conversion of starch into sugar by the action of the diastase of malt, it appears that—(1) the conversion of starch is quickened by an increased quantity of diastase, as well as a higher temperature; (2) that when starch fails to be detected by iodine, the formation of sugar is nearly finished; (3) at all temperatures from 60° C. to 0° downwards, and the application of varying quantities of diastase, there are formed always from 50 to 53 per cent of sugar; (4) at temperatures above 60° less sugar is formed than at temperatures below that degree; (5) a temperature of 70° impairs, and in some instances destroys, the activity of diastase.

Vulcanised rubber has been long an object of study and experiment, to see what use could be made of the waste pieces and cuttings; after the sulphur had been added, it was thought that it could not be worked over again, and this being the case, the price was likely to remain high for many years. Fortunately, the difficulty has yielded to the researches of practical men, and a process has been discovered by which the old rubber can be mixed with the fresh in certain proportions, and thus changed to a useful article.

Dr. Gräger has proposed a new method for the regeneration of waste nitrate of silver solutions used in photography. After first referring to the generally applied and well-known means now in use for this purpose, the author states that the best plan to treat these solutions is the following:—They are boiled either in a porcelain basin or glass flask, and while boiling there is added to them recently precipitated, well washed, and moist oxide of silver, the boiling being continued for some time. The liquid is next filtered, and then evaporated to dryness, the heat being increased to fusion, so as to destroy ammoniacal salts; the residue is pure nitrate of silver. The sediment on the filter contains some oxide of silver, which must be added in excess; and, therefore, in order not to lose that, the filter is preserved, and the contents worked up at a subsequent operation. The nitrate of silver thus obtained is, by practical photographers, pronounced to be of excellent quality.

For coating brass with copper, Dr. C. Puscher dissolves 10 parts, by weight, of sulphate of copper, and 5 of sal-ammoniac, in 150 parts, by weight, of water. Place the brass, well cleaned and free from grease, into this mixture, leave it in it for a minute, let the excess of liquid drain off first, and heat the metal next over a charcoal fire, until the evolution of ammoniacal vapours ceases and the coppery film appears perfect. Wash with cold water, and dry. The coating of copper adheres firmly.

The best varnish to protect polished metals from rusting is, according to Dr. C. Puscher, a solution of paraffin (1 part, by weight, in 3 parts of petroleum). This varnish may be usefully applied to polished metals, especially as, after having brushed the liquid over the surface of the metals, they may be gently wiped clean with a soft piece of flannel, so as to leave only a very thin film of the varnish, yet sufficient for the protection of the polish.

The protection of iron water-pipes from oxidation is very necessary, not only to provide against the premature giving way of the metal, but to secure the scarcely less important *desiderata* of avoiding any undue roughening of the internal surface or the diminution of the bore by the accumulation of rust. The most successful means of preventing the results just referred to consists in coating the surfaces with a tenacious varnish. That used on the pipes of the Cochituate Water-works in Boston has withstood a ten years' test, and at the end of that time the pipes "were found to be almost free from rust or ochreous deposits." It was composed of coal-tar, distilled until the naphtha was wholly eliminated, and the tar or pitch brought down to the consistency of wax. The tar reduced to this condition was heated to 300°, a temperature at which it was kept during the dipping operation, and about 8 per cent of linseed oil added to it. The pipes of cast-iron were heated to a degree determined by the judgment of the engineer in charge, and immersed in the prepared tar for from thirty to forty-five minutes, at the expiration of which they were placed in such position to drip that the remaining layer or coating of the material should be of uniform thickness. This method of protecting the pipes is now exclusively adopted in the water-works of most of the cities in the United States, and its advantages have been found to be far in excess of its cost.

For the purpose of obtaining decorative colours on metals, Dr. Puscher employs a solution of hyposulphite of lead in hyposulphite of soda, in the proportion of 3 parts, by weight, of hyposulphite to 1 part of acetate of lead. The clear solution of the salts deposits, when heated to about 100°, a thin layer of sulphide of lead upon any metal—brass, zinc, copper—placed in the solution, and thereby produces a beautiful display of various colours on these objects.

The native Indians of Mexico employ a rapid method of tanning hides and skins, which may prove useful as a basis for experiment in more civilised countries. It appears that they form, with the brains of the animal and certain vegetable products locally called *cascalote* and *huizache* (substances containing a large proportion of tannic acid), an emulsion, which is forced into the skins by means of squeezing between the hands, or, more rapidly, by stamping with the bare feet. In some parts of Mexico, 4000 or 6000 goats are killed at one time, simply for the skin and for boiling down for fat; the blood, flesh, bones, and offal simply being applied as manure.

Many attempts have been made of late years to manufacture artificial stone, some of which have been attended with more or less success, as far as regards the production of large blocks, but have rarely succeeded for the purpose of moulding or making medallions and mosaics, or for emery-wheels, oil-stones, and articles of a similar character. In almost all cases, too, the agency of heat has been required for producing articles in artificial stone, and this has been found very objectionable, as well as expensive. Attention has recently been called to some specimens of artificial stone in the shape of medallions, mosaics, emery-stones, oil-stones, imitation marble, &c., which have been produced in a simple manner, by moulding without the agency of heat, and which appear to partake in every respect of the nature of the stones they are intended to represent. These specimens are formed from natural materials, agglomerated by means of a cement, the invention of M. Sorel, a French chemist, well known in connection with the introduction into this country of the galvanised iron process. This new cement is formed of basic oxychloride of magnesium (oxide of magnesium and chloride of magnesium), either pure or mixed with other substances. The cement may be made in two different ways, viz., either by diluting or tempering magnesia, which may be more or less hydrated and carbonated, with a solution of chloride of magnesium more or less concentrated; or by adding to the magnesia chloride of magnesium in a dry state, and employing water to form the cement. The cement thus produced is specially white and hard, and may with advantage be used in place of some of the best cements. It possesses the same hardness, and will receive the same polish as marble, thus enabling it to be employed in the manufacture of artificial marble, mosaic pavements, and statuary. Imitation ivory can be made from it for making billiard balls and other similar articles, also medallions, buttons, &c. By combination with mineral colours, the cement may be made to assume any desired tint, may be moulded like plaster, and be employed in the manufacture or imitation of innumerable objects of art or ornament. In practice, the cement is never used used in a pure form, but in combination with other materials; which, being incorporated with it while in the moist condition, are in the subsequent setting mechanically bound together into a solid mass. For this purpose, the magnesia, in fine powder, is mixed with mineral substances, such as sand, gravel, dust, or chips from marble or other stones, or with emery, quartz, or other grits of various kinds, in varying proportions, according to the result desired. This mixture is then moistened with a solution of the chloride of magnesium, or with the bittern from salt works. In some cases it is made sufficiently wet to form a mortar, and in others only to produce a condition of dampness like that of moulding sand. The mixture may be effected in troughs, hy hand labour, the materials being worked over with shovels or hoes; or, more expeditiously, in mixing machines designed expressly for the purpose, and worked by horse or steam power. The materials of which this cementing substance is composed are abundantly distributed over the surface of the globe. Magnesia sufficiently pure for the purpose is obtained by simply calcining mineral magnesite, large deposits of which are known to exist in Prussia, Greece, Canada, California, Pennsylvania, and Maryland. Deposits will doubtless be found in other places when the demand is made for the material. The chloride of magnesium is readily obtained by concentrating sea water, the bittern of salt works being sufficiently pure for the purpose. Sea water concentrated at 30° B. precipitates nearly the whole of its chloride of sodium. In the "Sorel" process, it is concentrated to 33° B., when all the chloride of sodium is practically crystallised and precipitated, the mother-liquor retaining, besides the chloride of magnesium, some chloride of potassium and some sulphate of magnesia, which seem to add strength to the cement, as the water in this state makes a stronger stone than the pure chloride of magnesium.

LIGHT.

At the recent *soirée* of the Royal Society, Mr. J. Browning exhibited a spectroscope, in which the rays of light pass through two batteries of prisms, and were then sent back through both trains of prisms, by means of a reflecting prism placed behind the last prisms of the train, to the eye of the observer. In this manner a dispersive power equal to nineteen prisms of flint-glass is obtained. Both trains of prisms, as well as the intermediate prism, are under the control of an automatic movement, which ensures that every ray shall pass at the minimum angle of deviation, for the particular ray under examination. Both the collimator and telescope are fixed; the prisms only are movable. Mr. Proctor suggested this extension of Mr. Browning's automatic spectroscope, and contrived the form of prisms which would enable the automatic action to be carried on, but the mechanical difficulties, in communicating the automatic motion to the second battery were very considerable. These difficulties Mr. Browning has ingeniously surmounted. The D lines in the spectrum of sodium are seen in this instrument, separated by an apparent interval of more than $\frac{1}{8}$ of an inch, and under favourable conditions of the atmosphere, ten or twelve lines are visible between them. We understand that Mr. Spottiswoode, the Treasurer of the Royal Society, has become the possessor of this instrument.

The subject of signalling communications between fortresses and distant armies has occupied the attention of our French neighbours for a considerable time. M. Le Verrier has recently drawn attention to a lengthy paper containing the record of a series of experiments made for the Chief Committee of Defence of the Valley of the Rhône, Lyon, for the purpose of communicating, by luminous signals, at distances of 30 to 100 kilometres. It appears that, under the guidance of the well-known *savant* just named, excellent results have been obtained, and so simple in execution that ordinary workmen can satisfactorily manage the signals. The considerable expenses of these researches have been defrayed by a private individual, M. Maistre, of Villeneuvette, near Clermont, Hérault.

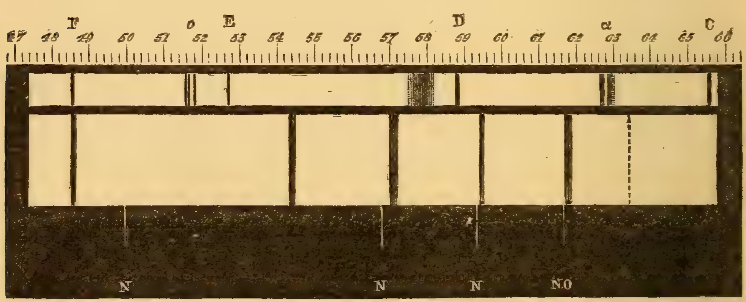
M. Delaurier, of the French Academy of Science, has taken up the proposal of M. Felix Lucas, a Government engineer, to obtain a very powerful electric light for coast service, at a comparatively small cost, by making the light intermittent, with intervals of two seconds; by this means, it is said, one 10,000th part of the electricity expended for a constant light will suffice. M. Lucas proposed to use static, but M. Delaurier prefers dynamic, and especially induced electricity, and suggests the following arrangement:—A battery which is only in action when the circuit is open, the couples united together in series, and the electricity passed through coils of solid iron wire; a very long copper wire, covered with silk, is wound around the iron wire-coil, the copper being sufficiently thick not to be heated by the passage of the current. Each pole is provided with a charcoal point; when these are in contact the current passes, and when they are separated the sparks are excessively brilliant, because both the direct and the extra current are brought into action, resulting from the induction of the current on itself and that by the coil of iron wire. The charcoal points are favourable to a spark of short duration. A very simple arrangement of clockwork causes the points to come into contact every two seconds, and then separates them sharply, so as to break the current instantaneously, It is not possible to obtain by these means a flash of such short duration as with static electricity, because the current passes for an instant through the air when the charcoal points are separated. It is believed that this system will be found useful, not only for piercing fogs at sea, but also for railway signals.

C. Schultz-Sellack has communicated a series of experiments with the object of proving that, while the chloride, bromide, and iodide of silver are acted upon by light, and altered in a well-known manner, this action is due to a phenomenon of dissociation, called forth by the light; but when these salts are put in sealed tubes, and care taken to have an excess of the haloids in free state present, and the tubes exposed to light, a mechanical change is effected, since the previously crystalline salts—obtained in that state from the ammoniacal solutions of the chloride and bromide and hydriodic acid solution of the iodide of silver—become at first dull, and next crumble to powder. This mechanical change of the haloid salts of silver is greatest when the chemical change is smallest.

The change produced in animal and vegetable forms by the influence of varying conditions of temperature, moisture, light, locality, &c., has induced many persons to try experiments on the influence of colour on insects, from which some interesting results have been derived. In some of these experiments, lately published, a brood of caterpillars of the tortoise-shell butterfly was divided into three lots. One-third were placed in a photographic room lighted through orange-coloured glass, one-third in a room lighted through blue glass, and the remainder kept in an ordinary cage in natural light. All were fed with their proper food, and the third lot developed into butterflies in the usual time. Those in the blue light were not healthy, a large number dying before changing; those raised in the orange light, however, were nearly as healthy as those first mentioned. The perfect insect reared in the blue light differed from the average form in being much smaller, the orange-brown colours lighter, and the yellow and orange running into each other instead of remaining distinct. Those raised in the yellow light were also smaller, but the orange-brown was replaced by salmon-colour; and the blue edges of the wings seen in the ordinary form were of a dull slate. If changes so great as these can be produced in the course of a single experiment, it is probable that a continuance of the same upon a succession of individuals would develop some striking results.

By means of the equatorial refractor of 15 inches aperture, by Messrs. Grubb and Son, recently placed in Dr. Huggins's hands by the Royal Society, this eminent physicist has succeeded in making observations described below of the remarkable spectrum which is afforded by the light of the planet Uranus. The spectrum of Uranus, as it appears in this instrument, is represented in the accompanying diagram. The narrow

Fig. 11.



spectrum placed above that of Uranus gives the relative positions of the principal solar lines, and of the two strongest absorption-bands produced by our atmosphere, namely, the group of lines a little more refrangible than D, and the group which occurs about midway from C to D. The scale placed above gives wave-lengths in millionths of a millimetre. The spectrum of Uranus is continuous, without any part being wanting, as far as the feebleness of its light permits it to be traced, which is from about C to about G. On account of the small amount of light received from this planet, a slit sufficiently narrow to bring out the Fraünhofer lines could not be used. The positions of the bands produced by planetary absorption, which are broad and strong in comparison with the solar lines, were determined by the micrometer and by direct comparison with the spectra of terrestrial substances. The spectroscope was furnished with one prism of dense flint-glass, having a refracting-angle of 60°, an observing telescope magnifying $5\frac{1}{2}$ diameters, and a collimator of 5 inches focal length. A cylindrical lens was used to increase the breadth of the spectrum. The remarkable absorption taking place at Uranus shows itself in six strong lines, which are drawn in the diagram. The least refrangible of these lines occurs in a faint part of the spectrum, and could not be measured. Its position was estimated only, and on this account it is represented in the diagram by a dotted line. The positions of the other lines were obtained by micrometrical measures on different nights. The strongest of the lines is that which has a wave-length of about 544 millionths of a millimetre. The band at 572 of the scale is nearly as broad but not so dark; the one a little less refrangible than D is narrower than the others. The measures taken of the most refrangible band showed that it was at or very near the position of F in the solar spectrum. The light from a tube containing rarefied hydrogen, rendered luminous by the induction-spark, was then compared directly with that of Uranus. The band in the planet's spectrum appeared to be coincident with the bright line of hydrogen. There is no absorption-band at the position of the line of sodium. It will be seen by a reference to the diagram that there are no lines in the spectrum of Uranus at the positions of the principal groups produced by the absorption of the earth's atmosphere. On April 7th a faint comet was discovered by Dr. Winnecke. Dr. Huggins observed the comet on April 13 and May 2. It presented the appearance of a small faint coma, with an extension in the direction from the sun. When observed in the spectroscope, the light of the coma was seen to consist almost entirely of three bright bands. A fair measure was obtained of the centre of the middle band, which was the brightest; it gives for this band a wave-length of about 510 millionths of a millimetre. The position of the less refrangible band was only estimated roughly. The result gives 545 millionths. The third band was situated at about the same distance from the middle band on the more refrangible side. It would appear that this comet is similar in constitution to the comets which Dr. Huggins examined in 1868.

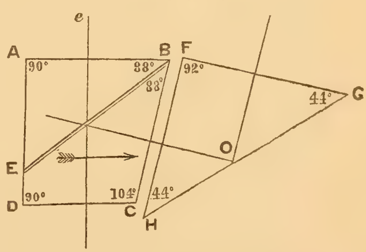
Mr. Rutherford has recently communicated to the Royal Astronomical Society a full description of the apparatus and arrangements he has devised for producing photographs of the sun and fixed stars; and at the last meeting of the Photographic Society Dr. Mann made a communication on the same subject, from which we condense the following particulars:—Mr. Rutherford's success has been mainly due to the photographic excellence of his telescopes—the object-glasses of his instruments having been corrected for photographic work, with peculiar care, under a plan of his own. The instruments, in their photographic equipment, are simply valueless for all ordinary purposes of astronomical vision, but they are as nearly perfect as may be for astronomical photography. The object-glasses of all astronomical telescopes are carefully corrected both for chromatic dispersion and for spherical aberration. But these corrections require a very material re-discussion and modification when the object-glasses are fitted to perform the highest class of photographic work. This further modification, in the case of his instruments, Mr. Rutherford has mainly accomplished by a very elaborate and refined process of step-by-step observation of imperfections in the photographic image, and step-by-step correction of the imperfections as they are noted in the successive observations. The photograph of the sun's disc presented to the Royal Astronomical Society is of exquisitely fine definition. The faculæ are clearly rendered; and a large group of dark spots are shown in their minutest detail, with the incipient bridging across one of them in the process of approaching disruption. The photograph of the Pleiades, however, is a much more remarkable object. This photograph was made with a telescope of 13 inches aperture, with six minutes' exposure, the telescope being, of course, carried during the time of exposure by a very carefully adjusted clock, so as to keep the image of each individual star upon exactly the same point of the sensitive plate during the whole period of exposure; and this service has been so marvellously well performed by the clock that the image of each star is a clear round dot, without the slightest trace of elongation in the direction of the star's movement. There are about thirty stars registered in the photograph, clustered round Alcyone, the chief of the group; and it is very remarkable that the several stars are actually of different magnitudes, from Alcyone, which looks very much like a period of punctuation in printer's diamond type, to vanishing dots that can only be discerned by the help of a lens. The magnitude of the stellar disc is most probably a result of diffraction; but however this may be, there certainly it is. The stars on the photographic plate are not mathematical points, but little dots of different sizes, just according to the notion that the eye forms of them in looking on the group with an aperture of large diameter. There is one very ingenious piece of precautionary contrivance connected with the photographic impression of these stars well worthy of note. Mr. Rutherford's main object, in the labour he has been incurring in perfecting his process of celestial photography, is to afford a ready means of providing registers of the exact positions of the fixed stars which shall be altogether free from the possible errors of personal observation. Now it unfortunately happens that photography has an awkward and mischievous trick of making stars on its own account, which may readily be confounded with the photographic impressions of the proper host of heaven. Any speck of accidental imperfection in a photograph may be readily taken for the image of a star. It therefore becomes a matter of paramount necessity, when groups of fixed stars are to be dealt with for astronomical purposes, that some method shall be devised whereby the portraits of the stars shall be distinguished from the photographic accidents and star-ghosts. This object Mr. Rutherford has most efficiently and satisfactorily accomplished by the simple expedient of covering the object-glass of the telescope, and disconnecting the instrument from the clock-movement for a few seconds, after the six minutes' exposure necessary to give the image of a fixed star, and then again attaching the telescope to the clock and giving a second exposure of six minutes. The immediate effect of this is that every true star image is closely followed by its double—thus . .; in other words, every star photographically pourtrayed is a double star; and this process of re-duplication is obviously one which photographic imperfection, or accident, is quite unable to simulate. When once the photographic images of a group of fixed stars are impressed upon a plate, the measurement of their relative distances becomes a mere matter of mechanical operation. But in Mr. Rutherford's proceeding the relative positions of the stars is also fixed by securing upon the photographic plate a tracing of the direction in which some star moves across the plate, when the telescope is not carried by the clock-movements—that is, a tracing of a line coinciding with a circle of declination. Bright stars, such as Sirius and Vega, leave a distinct trail as they travel across the plate. In the case of fainter stars the same result is secured by giving a second exposure of the star after a brief interval. A line then drawn from the first image of the star to its second image gives the exact direction of the declination parallel. In the photograph of the Pleiades it will be seen that Alcyone has thus been made to leave a position-impression nearly half way across the plate from the re-duplicated image first registered, and that these successive images have been subsequently connected, for purposes of reference and measurements, by a visible pencil-line. Three large telescopes have already had their object-glasses corrected for photographic work under this plan of Mr. Rutherford's. The first on which the correction was made was composed simply of two discs of glass constructed under the ordinary arrangements of the achromatic combination, and had an effective aperture of 11¼ inches. A second, with an effective aperture of 13 inches, is now in use in Mr. Rutherford's observatory; and this large instrument has ingeniously been fitted to be used for ordinary astronomical vision, or for photographic work, by applying the photographic correction through the instrumentality of a separate constituent. The object-glass has such a figure that it is visually perfect, until it is thrown unto photographic equipment by the addition to it of a third lens of glass. It is primarily composed of the ordinary double lens, one of flint- and one of crown-glass. The photographic correction is all concentred in and applied by a third meniscus lens of flint-glass, which is added to two other lenses when the telescope is to be used for photographic work, and which is taken away when the instrument is to be employed in the more usual work of visual observation. This arrangement is found to be so convenient, and to act so admirably, that there is no doubt it is the form which will henceforth be adopted in all important observatories, to enable their large equatorial instruments to be turned to account in occasional photographic work. The third telescope which has been photographically corrected is of the same form of construction, and has an effective aperture of 6½ inches. This instrument was made for the United States Government Eclipse Expedition, and was used by the expedition in Catania in December last.

MICROSCOPY.—Messrs. Powell and Lealand have recently constructed a ⅛th objective, in which, in addition to the usual arrangement for correction of the aberration caused by the glass cover by separating the front lens, another collar is added which acts in a similar manner upon the posterior combination; the result is that the adjustment of the objective can be far more correctly made than by the old plan, and a greater thickness of cover glass can be used. The above object-glass worked well through a cover of 0·01 inch in thickness.

The erecting binocular microscope of Mr. J. W. Stephenson, although originally contrived for the purpose of dissection under low powers, is capable of performing well with objectives up to ⅛th, the definition with this power being but slightly impaired. For this purpose it is found convenient to omit the upper prism, which was added chiefly for the purpose of permitting the stage to remain in a horizontal position, while the bodies were conveniently inclined for observation. A modification of this upper prism also permits the employment of two pairs of bodies, which enables observations to be conducted by two persons simultaneously: the use of this latter form only involves the loss of a small portion of light, which is easily compensated for by increased intensity of illumination. This is decidedly the best binocular introduced since Mr. Wenham's.

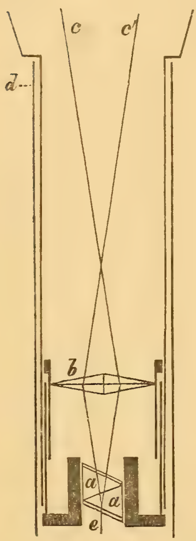
Two binocular microscopes acting upon the principle of dividing the pencil by double refraction by means of a prism of calc-spar have been recently contrived. In one, the invention of Mr. F. A. P. Barnard, President of Columbia College, New York, a prism of calc-spar (Fig. 11), A B C D, cut to the angles indicated in the diagram, is divided at B E; the surfaces are separated by a small interval: in practice they are kept apart by a single thickness of tinfoil introduced at each of the angles. The prism, F G H, is of flint glass, with a refracting index of 1·56 or higher; it is placed with the side F H parallel to B C. A ray incident perpendicularly upon D C and doubly refracted by the prism is resolved into two rays, E and O, of which the first is transmitted and the second reflected by B E, passes perpendicularly through the two surfaces B C and F H, and is a second time reflected by G H, and finally emerges at right angles from the face, F G. The author states, that with a Wales's objective marked 1-30th but more exactly rated 1-25th, with the B eye-pieces, the "Providence" *Grammatophora* is resolved with great facility.

FIG. 11.



The binocular adaptation of Mr. C. D. Ahrens consists of a double image-prism of calc-spar of peculiar construction, *a a*, with a piece of thin parallel glass cemented on each end, which not only preserves the surfaces from injury, but also improves the definition; the ray *e* from the object-glass is divided in each portion of the prism into two rays, but only the extraordinary ones are used, the ordinary being shut off; as there is a good deal of colour in these rays, it is neutralised by the double wedge flint-glass prism, of angle of 25°, *b*, placed over the double-image prisms, which also causes the rays to cross, the separated rays, *c' c*, then proceed to two symmetrically divergent tubes containing the eye-pieces. The tube containing the prisms and eye-pieces fits into the single body in the place of an ordinary draw-tube. The inventor states that the instrument answers well as a micro-polariscope, as the double-image prism takes the place of the analyser, and with much better effect than the usual Nicol prism, as it is worked with greater truth, but only one body can be used at a time unless there is a Nicol prism in one of the tubes, otherwise there will not be the same colour in each field. Mr. Ahrens also suggests that this binocular eye-piece might be used with advantage in the telescope, for by its means we could tell whether the object examined shone by its own or by reflected light.

FIG. 12.



Mr. B. T. Lowne, M.R.C.S., in describing the structure of the so-called "suckers" on the foot of the male *Dytiscus*, demonstrates that the production of a vacuum, as commonly supposed, has nothing whatever to do with their adhesion. Upon causing the insect to attach itself to the interior of an air-pump receiver, the "suckers" did not relax their hold upon the exhaustion of the vessel, but the adhesion was rendered closer by the exudation of the glutinous fluid, which is the real cause of attachment. In cases where the glue has hardened, the suckers are frequently torn away by the insect in its efforts to regain its liberty.

Prof. A. M. Edwards has examined a specimen of marble obtained near Thurman, about twenty-five miles from Saratoga, New York. The whole mass was found to consist of *Eozöon Canadense.* The material is said to occur in large quantities. The discovery is of great geologicial importance, as it indicates that the rocks belong to the Laurentian period, and the more so as the Canadian Geological Survey have only lately traced these rocks into New England —as far, at least, as Salem, Massachusets. Specimens of *Eozöon*, prepared by Dr. Carpenter, are in the cabinet of the Royal Microscopical Society.

Mr. Charles Cubitt, C.E., in the May number of the "Monthly Microscopical Journal," gives a statement of his views respecting the delineation of objects under microscopic observation. After criticising the drawings of several microscopical observers, which he refers to, and in which he detects various inconsistencies, he gives his own mode of proceeding, which is to make rough drawings of the object, then with the eye-piece micrometer to make all necessary measurements. From these data plans and elevations are prepared, and the perspective figure obtained by the process of *linear projection* well known to mechanical draftsmen. He selects as a subject for practical illustration the familiar *Melicerta ringens*, and in the plates accompanying his paper gives the plans and sections and the various lines needed in obtaining his finished representation, which is a marvel of patient application and decidedly accurate, but has, nevertheless, an inartistic stiffness. The process seems far too complicated and tedious for microscopists in general, and only attainable by those who have studied mechanical drawing in an engineer's or architect's office. The whole paper is written from a decidedly engineering point of view, but will do good in causing greater attention to be paid to accurate representation, although few besides its author have time or skill to carry out his views in their integrity.

Mr. H. J. Slack, F.G.S.,* gives an account of the "Optical Appearances of

* Royal Microscopical Society, April 5, 1871.

Cut Lines in Glass." Alluding to his paper on the "Cracks in Silica Films,"* he states that the chances of obtaining perfect illusions are increased by the amount of magnification and the perfection of the objectives employed. The false appearances look more real with well corrected objectives and careful illumination than with bad. Before commencing observations on ruled lines he recommends a careful examination of the edges of a number of thin cover squares, held together and viewed by transmitted light; the observation is to be conducted with a series of objectives commencing with an inch and proceeding upwards to a ¼th or ⅛th. Parts of the glasses' edges can easily be focussed to show the true form, but some portions a little in or out of focus will show beads, appearances like columns of Egyptian architecture, &c. Most of these optical appearances are sufficiently hazy or confused to give warning of their true nature, but some may be found so sharp and clear that they might easily mislead a practised observer. The lines used were ruled at varying distances: 1—2000″, 1—3000″, and 1—4000″. The line cut on glass is defined by Mr. Slack as a furrow, more or less rough at the bottom and sides, and when viewed correctly under the microscope, has the appearance of a narrow depression less transparent than the adjacent spaces. It is difficult to obtain a correct view. The edges of a cut are often apt to appear as two raised lines. The ruled lines, examined in various ways, presented all kinds of appearances; for instance, the cuts appeared as rounded bands, half-round hollows, with rod-like ridges in the middle; flattish spaces, with narrow, raised edges. The true appearance was seen only in a single instance. Much is evidently to be done by careful microscopical examination, under varying circumstances of illumination, &c., of objects the true nature of which is known. The value of such researches is clearly shown in Mr. Slack's paper.

The Royal Microscopical Society has recently acquired an extensive collection of vegetable fibres, chiefly the production of India. The slides are mounted in duplicate, one dry, showing the fibre in its natural condition, the other in balsam, with the ultimate fibres separated, for polariscope observation, by which the structure is better displayed than by any other known means. The collection is similar to one prepared for the India Museum, and is likely to prove of value to those interested in textile materials. Many of the little known Indian fibres are strong, fine, and procurable in large quantities: there exists only the usual prejudice against new materials; many of them have for years been employed in India for fabrics and cordage, paper-making, and many other purposes.

The second part of Vol. 160 of the "Philosophical Transactions" contains the long expected account by Dr. G. W. Royston Pigott, M.A., &c., on "A Searcher for Aplanatic Images applied to Microscopes, and its effects in increasing power and improving definition." The author commences by giving a detailed account of various experiments on the definition of microscopic objectives, which he was led to undertake by an accidental appearance of black beads instead of the usual "! markings" on the *Podura* scale. At an early period of his researches he abandoned the usual test-objects on account of their real structure being unknown, and formed a test by diminishing some known object, such as a thermometer scale, by means of a high power objective placed in the position of the condenser beneath the stage of the microscope. This aërial or aqueous image, as dry or immersion objectives were used, formed a test readily showing the quality of the definition. Some of the results are surprising, and such as would hardly be expected from what the author calls a "*very fine eighth.*" When the draw-tube was used to increase magnifying power by lengthening the body, it was found that the increase of aberration was in much greater proportion than the increase of magnification. Experiments conducted with the view of ascertaining the nature of these aberrations determined that false images, "*eidola*," existed above and below the best focus, and that under certain conditions these *eidola* might become, so to speak, mixed with the true image, giving rise to a great amount of confusion and

* Monthly Microscopical Journal, vol v., p. 14.

necessary liability to errors of interpretation. Another series of experiments on the effects of traversing combinations of lenses placed within the body of the microscope suggested the idea of "searching the axis mechanically for aplanatic foci;" the trials ultimately resulted in the "Aplanatic searcher" thus described by Dr. Pigott:—"A pair of slightly overcorrected achromatic lenses, admitting of further correction by a separating adjustment, are mounted midway between a low eye-piece and the objective, so as to admit of a traverse of 2 or 3 inches by means of a graduated milled head. These lenses are conveniently traversed within the draw-tube, and can be brought to bear within 4 inches of the objective, or at a distance of 10 inches. The focal length of the combination forming the aplanatic image-searcher may vary from 1½ inches to ¾ of an inch. The latter applies more effectively to low objectives, when it is desirable to obtain extraordinary depth of focal penetration and vision through very thick glass,* as with a ½ inch giving 700 diameters with a C eye-piece. It should now be stated that the searcher may be employed with very different intentions. Thus—when it is desirable to view an object through a very thick refracting medium, the searcher is brought as close as possible to the objective, which action lengthens the focus of the objective; and the same thing is necessary when the observer wishes to throw the *eidola* of an upper structure above and away from the true image of the lower but contiguous stratum, as when the lower beads of the *Podura* are required, or when it is required to give additional negative aberration to an objective too positively corrected, in which the front glasses are already forced into a dangerous proximity. On the contrary, when the searcher is traversed the opposite way, the *objective lenses* require to be brought nearer together; the instrument is then more adapted for viewing objects or particles lying in the upper plane of a complex structure, throwing the *eidola* of the lower layer below that layer itself, and so leaving the upper stratum less disguised by the false images of the lower. In intermediate cases, where greater penetration or focal perspective is required, with a thin glass cover, the objective lenses must be proportionately separated by an increased interval, the searcher being traversed towards the objective; and in general confused images of both upper and lower strata can be obtained by opposite arrangements. The most brilliant definition is generally obtained when the searcher (a little more overcorrected) is used as close to the objective as possible. The overcorrection of the searcher is increased by separating its component lenses according to the divisions upon the sliding tubes of the searcher. The use of this instrument will be facilitated by first setting the microscope for ordinary use without the searcher, adjusting an eye-piece, the focus, and screw collar to the most distinct vision, and then applying the draw-tube containing the searcher placed at a point nearest to the eye-piece. As the searcher is traversed towards the objective, the lenses of the objective may *require separation.* The change in the general aberration is shown by the divided index of the milled head actuating the movement of the searcher. The power obtained is in general from two and a-half to four times greater than that given with the third eye-piece, C, of 1 inch focal length; with a very fine eighth of Messrs. Powell and Lealand's *new* construction a clear and satisfactory definition of the beading of the *Pleurosigma formosum* was exhibited to them, by means of the aplanatic searcher, at a power estimated at 4000 diameters. Several inferior objectives have acquired a fine definition by the application of the searcher. This paper, perhaps, will, hardly be complete if I omit to add that the instrument will be most effectively employed by considering it as a conjugate portion or integral part of the objective itself, in which the minute traversing adjustment of the objective lenses finds its counterpart in the more extended, and, therefore, more delicate adjusting traverse of the searcher itself. So that, in short, during minute microscopical research, each adjustment should be intelligently applied, according to the nature of the research in hand. The indications of the one adjustment should be employed to verify those of the other. Correlative movements, by the aid of the searcher, may introduce aplanatic images, whilst a violation of

* Nearly one-fourth of an inch thick.

their correlation will exhibit deformity." These instructions relating to the use of the searcher have been quoted at full length, as some of the failures in the use of the searcher have probably been caused by ignorance as to the mode of its application, and as a natural consequence, observers disagree as to its efficiency. The editor of the "Monthly Microscopical Journal" justly remarks:—"There will, doubtless, be many cheap imitations, which, it may be feared, will only caricature the properly constructed instrument, which must be regarded as a new aid to microscopical research." It is unfortunate that so elaborate a paper should give no constructive details to aid opticians in manufacturing the instrument; the form of the lenses can only be guessed at from the figures, no mention being made of their curves, or the quality of glass employed. This is a great omission, which it is to be hoped that Dr. Pigott will remedy by a future publication. Experiments with a properly constructed searcher, in the hands of microscopists accustomed to high power observations, are needed before the true value of Dr. Pigott's invention can be ascertained.

HEAT.

The re-solidification of broken ice at temperatures above the freezing-point has its parallel phenomena under like circumstances in the cases of many other substances. Some of these are well-known, and at times exceedingly troublesome to workmen in certain manufacturing businesses where rapid liquefaction is a desideratum, but they have not been noticed in any scientific journal. Mr. C. W. Vincent has communicated to the "Chemical News" some instances of these apparent parallels to regelation. Good black rosin, free from turpentine, when subjected to pressure in a mould, or otherwise, at ordinary temperatures, becomes completely pulverised, its particles showing no cohesive power whatever. When the temperature of the mould is raised considerably above the melting-point of rosin, on pressure being applied, a different result ensues; the mass becomes at once solid to the core, the outside alone showing signs of liquefaction. When rosin has to be melted for manufacturing purposes, if even for a few minutes only the workmen neglect to stir, the whole mass becomes completely solidified, and the liquefaction is only carried on at the exterior of the mass, which, when pounded by his stirrer, breaks up without giving the slightest preference to the previous lines of fracture. Pitch, at melting temperatures, gives the same result as rosin, but becomes viscid so much sooner, that though coagulation is readily affected, yet the perfect junction of the broken pieces into a solid mass is not so readily obtainable. The gums used in the manufacture of varnishes, such as dammar, copal, anime, shellac, &c., give similar results. Whilst being run down at high temperatures, they settle together and cohere (if their surfaces be clean) without the possibility of again separating them in the same places; and this while the exterior of the mass is rapidly melting into liquid of almost the fluidity of oil.

Aëronauts have frequently observed a phenomenon which they term as *le ballon fume sa pipe*—that is to say, the gas the balloon is filled with issues from the lower opening of the balloon in the form of a whitish smoke. Dr. de Fonvielle supposes that this phenomenon is due to the cold produced by the increase of the volume of the balloon while rising rapidly upwards. Indeed, for a decrease of pressure of 1 millimetre per second of time, the increase of volume is equivalent to that produced by an elevation of temperature of two degrees for a height where the mean pressure would be 660 millimetres of mercury. A similar phenomenon takes place in every instance of the rapid rising of a mass of humid air, which, while becoming dilated, does as the balloon and *fume sa pipe*—that is to say, leaves behind it, in the shape of more or less dense cloud, the watery vapour which it contained previously in the shape of a diaphanous invisible gas.

A project for obtaining a larger amount of steam from a given quantity of fuel has been patented by Mr. C. F. Varley and T. A. Rochussen. When coal is burnt the solid coal is turned into gas, a large portion of heat becomes latent, and is wasted by volatilising the solid. When zinc, iron, or manganese are burned, the resulting oxide is a dense solid; little or no heat is wasted, as it is not turned into vapour. In addition to this, the inventor obtains the cosmical heat latent in the oxygen of the atmosphere, and the result is that one pound of zinc will evaporate more than four times as much water as one pound of coal, the advantage of which on long sea voyages is obvious. The zinc or other metal thus becomes a vehicle of power, much larger than can be obtained from the same weight or bulk of coal, and the oxide of the metal may subsequently be economically reduced at any convenient place where coal is accessible. The following is the manner in which it is proposed to carry out this invention:—The furnace of the boiler is divided into two or more parts, first, the hearth or grate on which the metal is burned (in this description we will confine ourselves to the metal zinc); secondly, a chamber behind the hearth to collect the oxide. In the case of tubular boilers, the heated gas from this chamber is made to circulate through the tubes. The furnace has the bottom and sides, and sometimes the top also, of brick, fire-clay, or any other refractory substance. The air is admitted over the combustible metal, or by a blast through the same; in the latter case pipes or tuyeres are built in the bottom or sides of the furnace.

The extinction of fire by gas is probably about to be carried out in New York, a company having been proposed for this object. The following is the purport of the scheme:—The company proposes to build, in some central location, a reservoir similar to the gasometer of a gas-house, of sufficient capacity for all emergencies, and run a four-inch pipe therefrom in every direction through the streets, having in front of every door a valve, to which a small rubber hose can be easily attached, by which the stream of gas can be directed to any room on fire. To furnish such a reservoir, with its necessary retorts for making gas, and laying 200 miles of pipe through the city, will come within a cost of 250,000 dollars, and the company claims that the saving of property which water destroys and gas leaves untouched, would more than pay the entire cost in one year, to say nothing of the saving of the buildings. From the reservoir, through the pipes, carbonic acid gas will be forced to the endangered building. Repeated experiments have shown that fire cannot burn in an atmosphere containing one-fifth part of its volume of this gas, and that its presence does not injure the finest fabrics, or discolour the most highly polished plate; consequently it does no damage to goods or furniture, which water destroys. It can be manufactured at so low a price that the flames of a moderate-sized building on fire could be extinguished for a few dollars. Its use has been frequently endorsed by insurance companies. Up to the present time the difficulty in making use of carbonic acid gas for extinguishing fires has been found in the production and application of this gas in sufficient quantities for obtaining a practical advantage at a real fire. Experimentally the presence of this gas in quantity proportionate to a flame has been found to insure its sudden and complete extinction; but until within the last few months no plan has been put in operation to make this power available. In 1851 a coal mine in England, that had been on fire for thirty years, and had extended over twenty-six acres, was completely extinguished by injecting only 8,000,000 cubic feet of carbonic acid gas. Water and chemical solutions had been tried in every way and quantity for years upon these burning acres of coal, without any perceptible effect, for the intense heat would drive them back as a volcano sends out its lava into the heavens. The loss by water, caused by the engines playing after a fire is out, or nearly so, is very great. Once in operation, a steam fire engine is frequently as destructive as the conflagration itself. If the new method succeeds and is generally adopted, many of the calamities and losses that are yearly chronicled will cease. As the pipes will be immediately laid and a reservoir built, it is expected that very soon the new apparatus and plan will be practically tested.

The effect of cold upon iron and steel has formed the subject of a numerous series of experiments which have recently been communicated to the Literary and Philosophical Society of Manchester. The result of these experiments appear to be that these metals were not rendered brittle by low temperatures. Most of the experiments of Joule, Fairbairn, and Spence were made by steady pressure, and therefore cannot be considered reliable when percussive force is brought into play. That there is a difference in these effects, so great that no relation between them can be determined, any one may convince himself by contrasting the tensile strength of glass with its extreme frangibility under percussion. Mr. Brockbank, whose paper drew forth the opinions referred to, took the ground that iron and steel were more liable to break in cold weather, and based his opinion upon percussive experiments. It is obvious, therefore, that his opinion has no weight upon the subject of tensile strength as affected by cold, but it is of great value as confirming experiments previously made to ascertain the effect of cold upon iron and steel subjected to percussion, experiments of which Mr. Brockbank was apparently ignorant at the time his paper was prepared. The "Scientific American" has drawn attention to some previous experiments in this direction. In 1869, a "Treatise on Iron and Steel," by Knut Styffe, was published in London, from a translation by Christer P. Sandberg. The translator, however, took issue with the author upon this very question, and denied the applicability of Styffe's deductions, from tensile experiments, to percussion in cold temperatures, founding his denial upon experiments performed by himself in Stockholm under the authorisation of the State Railway Administration of Sweden, in 1867. The results of his experiments prove that at 10° F. rails will not sustain much more than one-fourth the blow that they will at 84° F. The method of performing the experiments, as well as the details of each, are given in tabulated form in a voluminous appendix to the translation of Mr. Styffe's treatise. Mr. Sandberg concluded from his experiments that the brittleness of iron and steel under low temperatures is due to phosphorus present in the metal, and that with purer metal the results would have been different. It is evident that this subject is imperfectly understood, even by the highest authorities, and further extended investigations, with all kinds of iron and steel, must be made before the general effect of cold, as inducing brittleness under percussion, can be affirmed. Meanwhile, it seems to be well settled that the tensile strength of iron and steel when tested by stretching is not lessened by low temperatures. On the contrary, it would seem from Mr. Spence's experiments to be increased rather than diminished.

Any light that can be obtained on spontaneous combustion adds not a little to the value of property. Many of the fires charged to incendiarism are really owing to spontaneous combustion, so called. The editors of the "Boston Journal of Chemistry" give some cases illustrating this subject which have come under their own experience. Within the past year, twenty-eight rolls of cotton cloth in a large dyeing establishment were dyed black, and were delayed a few days before they could be starched and finished. Two of these rolls were discovered to be on fire—not in flames, but in a smouldering condition, or charred into tinder; a third roll was so hot that hands could not handle the cloth, and the wooden roller upon which the cloth was wound was heated almost to the point of ignition. The rolls of cloth destroyed were the first dyed, and consequently had been longer exposed than the others, which in a measure explains why all the rolls were not in the same condition. In the dyeing, the first rolls were dyed without washing, by an oversight of the dyer. This is the point of importance, as the chemical salts were left in the cloth. Logwood, potash, sulphate of copper, and sulphate of iron constituted the dye, and we suggest this explanation as the probable cause of the fire. The potash and sulphate of iron change to sulphate of potash and protoxide of iron, and by the absorption of oxygen from the atmosphere or from moisture in the cloth, this developes sufficient heat to reach the point of ignition.—A fire was discovered in a silk-mercer's shop in London. The fire originated in a lot of black-dyed silk, and was discovered, as in the first instance, before flame had burst out. The conclusion reached was that it was not safe to have black-dyed silk in large masses, and that each piece ought to be so placed as to allow free circulation of air. It is very probable that the explanation of the combustion is the same as in the preceding case.—In trying to get rid of rats in a dwelling-house, the floors were taken up, in order to cut off their ingress, if possible. The box that held the hot-water pipes was found to be a favourite resort for the vermin, and had actually been on fire. The sides were charred, but there had not been sufficient air to sustain combustion. Upon investigation as to the cause of the incipient fire, we are not long left in doubt, for a store of remnants of greasy cloths used in washing dishes was found, which had been brought by the rats from the kitchen. Some of these were charred, and the others were well saturated with grease and oil. This fire was a distance from the kitchen range of at least forty feet. It would have been natural in all these cases, if the real causes had not been apparent, to attribute the origin of the fire to incendiarism. The introduction of coal oils for the lubrication of machinery has very materially reduced the number of fires from spontaneous combustion, owing to the fact that the coal oils do not absorb oxygen.

The employment of the oxyhydrogen blowpipe for attacking safes has recently been the subject of some careful experiments; the result being that, whilst it is a powerful auxiliary to the drill, it cannot be used alone with success. The temper may be drawn, in time, from a steel plate an inch thick, by the use of the blowpipe, so that the plate may be drilled. It may also be burned quite through when operated upon singly; but it is difficult to do this with iron plates, which burn less easily, and also conduct heat away from the point against which the flame is directed as rapidly as the steel. Spiegeleisen burns with even less facility than ordinary iron. The flame directed against the corner of a fragment of spiegeleisen fused it, but, after continued action, only produced a comparatively small amount of the oxide of iron, which coated the bead formed. The fused metal, on cooling, was as hard as before. This material, in fact, depends for its hardness upon its natural composition, and not upon any process of tempering, so that mere melting does not change its character. It would, however, require apparatus not available to burglars to melt a hole in the centre of a spiegeleisen plate. It follows, therefore, that while iron plates and steel plates may be successively penetrated by the use of the blowpipe, as practically capable of use in the hands of burglars, the spiegeleisen plate, which practically resists drilling, defeats the use of the instrument as an adjunct to the drill. Mr. Dickinson, of New York, manufacturer of carbon points for drills, &c., states that these points will not drill spiegeleisen, except by the use of appliances for obtaining speed, which cannot be used by burglars, and that to drill it at all would be a work of so much time as to prevent its adoption for safe-breaking. The rate at which, by the alternate use of the blowpipe and drill, a hard steel plate can be penetrated, is, according to Mr. Farrell, about one inch per hour; the drawing of the temper in advance of the drill occupying about two-fifths as much time as the drilling. It is found that the alternate use of these instruments enables more rapid progress to be made than when it is attempted to draw the temper entirely through the plate at a single operation.

A discussion on the subject of the congelation of bisulphide of carbon has been going on, for some time past, in the German chemical journals. Dr. Wartha considers that its congelation, which, according to the treatises on chemistry, requires a temperature of $-90°$ for its solidification, may be easily effected by directing a very rapid current of dry air upon the surface of the pure liquid contained in a glass vessel. If a thermometer be plunged into the bisulphide of carbon during this operation, a snowy crust will be noticed covering the sides of the vessel and the thermometer, even before the temperature has become $0°$. The temperature then rapidly descends to $-18°$, and a white mammellated mass rises to the surface, and sometimes even stops up the tube for conducting the air. Soon all the liquid disappears and the thermometer commences to rise again up to $-12°$, where it remains stationary as long as the bisulphide of carbon is solid. In this state it presents the same phenomena as solid carbonic acid. Dr. Ballo has verified all the facts stated by Dr. Wartha, but finds that the solid substance obtained is not really solidified sulphide of carbon (frozen sulphide), but is a hydrate of the body alluded to, containing about 19·14 per cent of water. In a subsequent paper he states that he has obtained what he calls chloroform snow and iodethyl snow, by directing a rapid and moist current of air upon these fluids contained in shallow basins. As regards the real nature of these solid substances, his experiments are not yet quite finished.

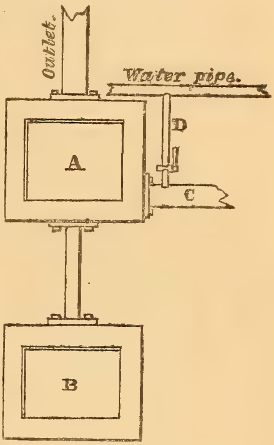
The freezing-point of mixtures of glycerine and water has been examined by Mr. C. Bullock. The glycerine used was common, sp. gr. = 1·250; the quantity of water, in all experiments, 1 gallon. Temperatures Fahr., with ½ pint, fuses at 30°, 1 pint at 24°, 1½ pints at 18°, 2 pints at 10°, 3 pints remain fluid at 3°. These determinations are of considerable value, as mixtures of glycerine and water are frequently used in cases where a mobile liquid is required at temperatures where water would be solid,—*e.g.*, in wet gas meters, ice machines, &c.

An ammonia engine has been designed by Mr. Emile Lamm, in which ammoniacal gas is used as a substitute for steam or air. Ammonia, at the temperature of our atmosphere, is a permanent gas of well-known pungent odour. Its density is 596; air being 1000. It may be liquefied by great pressure, and in the fluid state its density, compared with water, is ·76, or about one-quarter lighter than that liquid. Its vapour, at 60° Fahrenheit, gives a pressure of 100 pounds to the square inch, while water, to give an equivalent pressure, must be heated to 325° Fahrenheit. The volume of ammoniacal gas under the above-named pressure is 983 times greater than the space occupied by its liquid, while steam, under identical pressure, occupies a space only 303 times greater than water. If, therefore, by the application of equal quantities of heat to the specific heat of equal weights of steam and ammonia, such great difference in their relative volume is produced, it is evident that the cost of ammonia, as a motive power, would be one-third that of steam. This immense volume of the gas, compared to other gas or vapours, shows a decided exception to the law of Mariotte, relating to the expansion of gases, greatly in favour of ammonia as a motive power. The latent heat of ammoniacal gas is 880, that of steam being 990. Ammonia is a powerful alkali. Its action upon most of the metals is *nil;* still, strange enough, it acts slowly upon one of the metals of the second class—*i. e.*, copper. Upon metallic iron, which is a metal of the third class, its action is absolutely *nil;* but it readily dissolves carbonated oxide. Ammoniacal gas is absorbed by water with avidity, one volume of water at 70° Fahrenheit absorbing 500 volumes of the gas. The water becomes specifically lighter, whilst its volume is augmented about one-third. As the absorption of the gas goes on, the water becomes heated, and the latent heat of the gas reappears as sensible heat. It is in this property that water possesses of absorbing so large an amount of the gas, and of becoming heated while absorbing it, that the practicability of using ammoniacal gas as a motive power rests, for it must be well borne in mind that the only agency for producing motive power is heat. It matters very little that liquefied ammoniacal gas boils at 40° below zero—72° below the freezing-point; it requires as much heat comparatively as water itself, for its complete and rapid evaporation, when acting as a motive power in an ordinary engine. But ammoniacal gas possesses the remarkable property, from its affinity for water, of being able at any time after its condensation into a liquid to reproduce, at a distance from the receiver where it was condensed, a force equal to the heat which was necessary for its condensation. This reproduction is owing to the fact that the latent heat of the gas appears anew in water of re-absorption, and is re-transferred to the liquefied gas. The re-transfer of the heat to the liquefied gas takes place, through metallic tubes of which the reservoir is composed, from the water of re-absorption which surrounds them, and is similar in its operation to the action of fire in the furnace of a steam boiler. An engine driven by ammonia instead of steam has actually been in use for some time in America, for the purpose of driving street cars. In the last trip of seven miles made by a street car driven by ammonia, the engine used on the car was equal to two-horse power; the ammonia expended during the trip amounted to 1·16 cubic feet. The latent heat of ammoniacal gas being 880, the whole heat expended during the trip would have been sufficient to raise 84 gallons of water from the temperature of 83° Fahrenheit to the boiling-point, 212°. If for supplying this heat it was necessary to rely upon the heat absorbed from the atmosphere, even on the hottest day of summer, it is certain that a car loaded with passengers could not run more than three hundred yards without being obliged to stop, and wait at least fifteen or twenty minutes, in order to acquire sufficient heat from the atmosphere to again run an equal distance. But the mechanical equivalent of heat expended in working the engine by ammoniacal gas, one-seventh of the total heat, according to the best physicist, is fully made up by the extra heat of chemical combination becoming also sensible at the instant of the re-absorption of the gas in the water surrounding the tubes. The register of the pressure-gauge at the moment of starting, and during the trip, shows conclusively that the extra heat compensates all losses. The mean pressure during a trip remains essentially the same, if we take into account the time necessary for the transmission or equalisation of heat between the reservoir containing the liquid gas and the re-absorbing water in which it is immersed. On the last trip made with a street car, the gauge registered at the start 150 pounds; lost in pressure, in making 1½ miles, 30 pounds; gauge then registering 120 pounds. Upon stopping for ten minutes it registered 180 pounds, thus gaining 60 pounds. On stopping at the end of the trip of seven miles, the gauge remained stationary at 176 pounds. The above variations can easily be explained. It is impossible to construct an apparatus in which the transmission of heat would be instantaneous; therefore, in rapid travelling, the reservoir of liquid ammonia becomes cooler than the water of re-absorption outside; but the difference never exceeds a certain point either way, the extreme of which, in the last trip, starting with 150 pounds, was a difference of 45 pounds. The maximum of pressure during one of the warmest days of summer was then 195 pounds. When the reflection is made that this tension was exerted in a boiler, which stood 600 pounds hydraulic pressure, no other commentary is necessary as to its entire freedom from danger; and this is easily explained, for as the temperature between the reservoir of liquefied ammonia and the water of re-absorption diverges, the transmission of heat becomes more or less rapid, in the direct ratio of their degree of divergence,—on this principle, that the production of heat in the re-absorbing water is equal to its reduction by evaporation in the reservoir of liquefied gas. One other advantage which ammonia possesses over steam is the fact that its vapour, not being condensed at the usual temperature of our atmosphere, does not, like steam, at a low temperature, suffer condensation, either in the cylinder of the engine or when used at a distance from the boiler. The cheapness of ammonia as a motive power, when compared with steam, is owing to the fact that one steam-engine, if it could be made to propel one hundred street cars with ease, would be much cheaper than one hundred steam-engines, each requiring a separate fire and an engineer, besides the regular conductor of the car; but the case is far different with ammonia, as a single engineer at the station can superintend the supplying of two hundred cars with liquefied ammonia, in sufficient quantity to run any distance within the limits of a large city, by means of a single fire under the stationary boiler in which the ammoniacal gas is liquefied. Further, liquefied ammonia can be compared to a bottled-up power, which can remain in a reservoir for months, or even years, and be transported anywhere in any desirable quantity; and then at once, without any further preparation, can be used for any purpose desired, and by the simple turning of a tap can be made to act as powerfully as when first liquefied. The other advantages of ammonia as a motive power are: First, its perfect safety—for the reason that its power is exerted at a very low temperature, and consequently its influence in weakening the tenacity of the iron vessel containing it is trifling, when compared with the destructive effects of a high temperature: for it is a fact, known beyond dispute, that the main agency which causes those frequent and deplorable explosions of steam-boilers is the high heat they require for the production of an effective force. Second, for marine purposes there is no doubt that it will conquer for itself a high place before long, for this reason—

that, with an initial pressure of 20 pounds or 25 pounds in a boiler, it would give an effective pressure of 150 pounds on the pistons of the engine.

Mr. J. Sutherland has described an ingenious modification of the common suction pump, to be employed for the pumping of hot liquids where there is considerable difficulty in getting a sufficient flow of the liquid when its temperature is at all near the boiling-point. This result, of course, arises from the well-known fact that liquids boil at lower temperatures exactly as the pressure on its surface is decreased. The pump itself was simply a steam cylinder, having a set of ports opened and closed by slide valves, and producing an exhaust by means both of the up and the down stroke. This arrangement gave excellent results so long as the liquid was comparatively cold, throwing a continuous stream 2½-inch bore or about 10 to 12 tons spent-lye per hour. The cause of failure was due to the exhaust produced by the pump being as rapidly filled up as formed by the vapours generated from the liquid, brought to a state of ebullition by the decreased pressure produced in the pump connection. It then occurred to Mr. Sutherland, that if he could introduce a fine spray of cold water at the inlet of the pump it would condense the troublesome vapour and allow the pump to do its work. A water-pipe passed along the wall within a foot or two of the inlet, and, between the two, a connection was opened with a ½-inch lead pipe having a stop-cock to regulate or shut off the water supply. The end fitting into the cast-metal pipe was fitted with a small rose, in order to spread the water on entering. The accompanying sketch shows the arrangement. A is the pump cylinder, B the steam cylinder, C a 3-inch inlet, D ½-inch pipe introduced on the upper surface of the inlet pipe. It is imperative that it be introduced on the upper surface of a horizontal pipe, as it is there the first vapour will gather and be exposed to the condensing action of the spray. Were the cold water introduced at the lower surface of the pipe it would mix with the hot liquid, and, in all probability, its action would be almost *nil*. The application proved a thorough cure, and not more than about ⅛th of an inch bore of water was necessary to set the 3-inch inlet in full action.

FIG. 13.



ELECTRICITY.

At the meeting of the Royal Society of Edinburgh, held on the 29th May, Dr. R. M. Ferguson described and exhibited a magneto-electric machine of the following nature:—The revolving Siemens's armature consists of one piece of soft iron, with two grooves cut in it, the one at right angles to the other, for the reception of two coils of insulated wire, one of which, on the motion of the armature, gives off a current to excite the electro-magnet, between whose poles it rotates, and the other gives off an external current. It is in effect a modification of Ladd's ingenious application of Siemens's and Wheatstone's principle of magnetic and electric reciprocity, only instead of two armatures placed in length and moving on the same axis, with their coils at right angles, a single double coiled armature in this instance is employed. The grooves are of unequal sizes, the larger being four times more capacious than the smaller, and the larger coil, which furnishes the external current, has its wire twice as long and twice as thick as the wire of the smaller coil and groove. The commutating collars at each end of the armature, against which the springs press, can be moved round and fixed at any angle with the planes of the coils. The core of the electro-magnet, made of boiler plate, is 11 inches long, 9 inches high, and 6 inches between the sides, and the revolving armature within is 11 inches long, and $2\frac{1}{2}$ inches in diameter. The peculiarity of the action is, that the hand in driving feels little or no resistance if the circuit of the larger coil be of small resistance ; but it has to exert great force to maintain the same velocity in a circuit of great resistance. The driving power must therefore be in some measure proportional to the work done in the outer circuit, and when this circuit is broken, and the electric action is limited to the reciprocal action of the small coil and the electro-magnet, it is excessively hard to turn the handle of the machine. With the same rotating speed a current approximately of the same strength can be maintained in any external circuit, provided only the driving power be sufficient. At a certain rate of motion, for instance, the hand feels it has little to do in melting or igniting two inches of a soft iron wire $\frac{1}{63}$ of an inch in diameter ; to effect the same with six inches of the same wire, one hand finds enough to do, and when eight inches are in circuit, both hand are needed to bring it to ignition. Even with the eight inches, little resistance is felt till the heating of the wire ensues. When the external circuit is made through the coils of an electro-magnet a few pounds in weight, it is impossible to keep up a high rate of speed, as the point of saturation of the magnet is never reached. But with a voltameter it is different; the limit of resistance is soon reached, and the driving resistance does not rise beyond a moderate amount, so that the quantity of explosive gas varies with the work of one hand from 3 to 6 cubic inches only, according to the velocity of motion. The principles involved in the action of the machine, which were discussed at some length, seem peculiar and interesting.

Professor A. A. Mayer, of Pennsylvania, has invented a new method of fixing, photographing, and exhibiting magnetic spectra. He first coats a clean plate of glass with a solution of shellac in alcohol, in the same manner as a photographic plate is coated with collodion. After the plate has remained a day or two in a dry atmosphere it is placed over the magnet, so that the under surface of the plate just touches the magnet. Fine iron-filings of Norway iron, which has been repeatedly annealed, are now sifted uniformly over the film of lac. The spectrum is then produced on the vibrating plate by letting a light piece of copper-wire fall vertically upon it at different points. The plate is now cautiously placed on the end of a cylinder of pasteboard, which serves as a support in bringing it quite close to the under surface of a cast-iron plate, which has been heated over a large Bunsen flame. Thus the shellac is uniformly heated, and the iron-filings sink into the softened film. When the shellac is hardened it fixes the spectra. When photographic prints are to be made from the plate, the heat is allowed to act until the metallic lustre of the filings has disappeared, and the film appears quite transparent; but when the plate is to be used as a magic-lantern slide the heating is not carried so far. Many plates have been made by this process, showing the action of single magnets of various forms and of juxtaposed bars, as well as the effects of electric currents led by wires through holes drilled in plates. Among the most interesting are those spectra exhibiting the inductive action of magnets on bars of soft iron and the interaction of magnets and electric currents.

A lecture experiment to prove that mercury is heated while a galvanic current passes through it has been devised by Dr. F. C. G. Müller. Take a glass tube 6 c.m. long and about 6 m.m. diameter ; heat it before the blowpipe, and, while soft, reduce its diameter in the centre to $\frac{1}{2}$ m.m., and next bend it to a U-shape ; fill it with mercury, fasten it in a clamp, and afterwards dip the wires of a galvanic battery in the metal. This having been done, the mercury in the narrowed portion of the tube will be observed to boil rapidly, while a continuous series of sparks will be also exhibited.

The uses to which a clock indicating correctly the same time in as many different places as may be desired can be applied are manifold. In astronomical observatories, public offices, manufactories, institutions, and for railways such a clock would be of extreme utility. Sir Charles Wheatstone's latest patent is for a magneto-electric clock driving sixty or seventy other clocks, and dispensing with all voltaic batteries or other common causes of failure. The system consists of a motor- or driving-clock and as many clocks in circuit as may be desired. The driving-clock has, like an ordinary clock, a weight, multiplying wheels, escapement, and a pendulum. For the ordinary pendulum-bob is substituted hollow coils of insulated wire, oscillating over the poles of two compound permanent magnets. An electric current is produced at each oscillation of the pendulum, which current is in one direction as the coil passes from left to right, and in the opposite direction when it proceeds from right to left. These alternately inverted currents are transmitted by conducting wires to the clocks in circuit, in which the escapements usually employed in telegraphic clocks are dispensed with, and the motion of the hands rendered perfectly continuous. The currents proceeding from the driving-clock are caused to pass through a horizontal coil of wire similar to a galvanometer-coil, in the centre of, and above and below which, is an astatic series of magnetised needles fixed on the same arbor, carrying a pinion in connection with the train of wheels communicating its motion to the hands. The needles are propelled for half a revolution by the current which is produced while the pendulum of the driving-clock is moving from right to left, and another half revolution while the pendulum passes from left to right, thus describing a whole revolution in one second, if half a second is required for each beat of the pendulum. The time when the needles after each half revolution arrive at a position at right angles with the wire of the coil, corresponds exactly with the moment when the pendulum of the driving-clock has arrived at its greatest deflection from the perpendicular. In all former telegraph clocks the current has had to overcome the *vis inertia* of the magnetised needles of the secondary clocks, thus entailing a great loss of power. But in this clock the currents are aided by the momenta of the needles, so that extremely weak currents arriving at the proper period are sufficient to impart motion to the train of wheels. Thus the needles are aided in their rotation much in the same manner that the oscillations of a heavy pendulum can be maintained by well-timed puffs of breath. The driving-clock can be regulated by a standard clock at any distance, it being understood that the regulator possesses one of the usual contrivances for the momentary completion of a voltaic circuit, or, if in proximity to the driving-clock, that a mechanical connection may be established. The regulating is effected by a series of very simple mechanisms, by which the pendulum of the driving-clock is made to shorten or lengthen itself automatically, as the clock has lost or gained. The advantages of employing magneto- instead of voltaic-electricity as the maintaining power of the secondary clocks are obvious. In every case where the source of power is a voltaic-battery, there must necessarily be established a certain number of contacts, and at each of these interruptions of the circuit a spark, no matter how minute, passes between the metallic points. This spark is nothing more nor less than a piece of the metal in a state of incandescence, and the result of this rapid oxidation of the points is the stoppage of the clock in a few weeks. By employing magneto-electricity, Sir C. Wheatstone has obtained the inversion of the current in a closed circuit, and consequently avoided the chief cause of failure hitherto. Moreover, the driving-clock can be made to keep time to any degree of nicety, independently of a regulator.

Another invention of Sir Charles Wheatstone is a magneto-electric counter for registering the number of revolutions or oscillations of any machinery, or the number of visitors to a public building, &c. The instrument consists of a permanent magnet, with soft iron prolongations, on which are wound coils of insulated wire; the currents being induced by the removal and attraction of an armature in connection with the machinery or door whose movements are to be registered. These currents are conveyed to the register, where they act upon suitable electro-magnets, causing the deflection of a magnetised needle. This deflection is continued by a train of wheels to an index-hand, which points successively to the figures on a dial in the usual manner. The register can thus be placed at any distance from the motor-magnets, out of reach of any ill-disposed person. The door, if the counter be so attached, remaining open, the index of the register points to a space intermediate to the two figures, so that the person in charge of the machine can at once see what is wrong. The instrument has been successfully applied to register the number of revolutions of the screw-shafts of some of our largest steam-vessels, and also to indicate in the editor's room the number of impressions of a newspaper press.

It is certainly desirable that such an important art-science as Telegraphy should have its representative institution, and the advent of the Society of Telegraph Engineers will be gladly welcomed by the profession. The Society held its first elective meeting on the 31st ult., when Charles W. Siemens, Esq., C.E., F.R.S., was elected President, and Lord Lindsay, and F. J. Scudamore, Esq., Vice-Presidents. Among the Council are Sir Samuel Canning, Professor Foster, B.A., F.R.S., Major Stotherd, R.E., Captain Webber, R.E., Messrs. Cromwell Varley, Willoughby Smith, and Latimer Clark. Mr. Sabine undertakes the two-fold office of Treasurer and Librarian, and Major Bolton is Secretary. The Society is composed, besides its council, of members, associates, and honorary members, to be elected from those only who have been regularly educated as telegraph engineers, or have acquired a degree of eminence in the profession. An annual provincial meeting is to be held in conjunction with the British Association. If talent will ensure a brilliant career, the association certainly promises well. There will be many who will watch its proceedings with great interest; a helping hand it can hardly want.

Mr. Joel has lately perfected a galvanometer for measuring the relative intensities of induced currents. The instrument is in principle somewhat analogous to the ballistic pendulum. On the axis of a pair of curved magnetised needles working between two vertical electro-magnets, is rigidly fixed a horizontal arm slightly curved upwards at its extremity. The extremity of the axis acts as a pivot to a slight ivory index loosely centred on an agate cup. By means of a micrometer screw, the index is brought against the curved extremity of the arm. If a current is now passed through the coils the index will be deflected, the arc varying as the intensity. The magnetised needles are arranged so as to register only the first current arriving in the coils. This form of galvanometer is simple, and seems calculated to meet with extended practical application. In connection with an induction coil, it would afford to the medical man a surer indication of the strength of the current he is employing than is at present available.

A novel adaptation of Sir W. Thomson's reflecting galvanometer has just been made. As is well known, the signals on a cable of high inductive capacity can be transmitted only with extreme slowness and precision, and even then the interpretation of a skilled clerk is necessary. It has been proposed, and tried experimentally with some success, to place at each end of the cable a system of clockwork to cause the synchronous movements of a fugitive dial, upon which are engraved the letters of the alphabet, appearing in succession, as the dial rotates, at an aperture in a fixed screen. On the same screen, a little lower than the aperture, the ray of light is reflected from the mirror of the galvanometer. The clerks at both stations, by pre-arranged signals, set their clockwork in motion at the same instant, the receiving clerk watching the dial and the light. The transmitting clerk, also watching his dial, immediately on the arrival of the letter required at the aperture, depresses a key, causing a deflection of the light at the distant station. Seeing this deflection, the receiving clerk immediately calls out the letter appearing at that instant on the dial to his assistant. According to experiments made on an artificial cable of extremely high inductive capacity, the rate of signals attained a maximum of 25 words per minute. Other advantages of the system are—that equal and opposite currents can be sent to line, and but a small battery power employed. A further modification is the substitution of type-wheels moving synchronously for the dials. Upon perceiving the deflection of the light the receiving clerk depresses a key, which causes a paper slip to be raised against the letter immediately opposite on the type-wheel. The difficulty of obtaining the perfect synchronism of the dials or type-wheels, the inventor, or rather adapter, M. Poisset, says is apparent only, and has been combated in Professor Hughes's type-printer.

A modification of Steinheil's telegraph has been brought out by Mr. Herring. It is furnished with two keys, one to work a lever carrying a pin, to make a dot, and the other to work a lever carrying a small linear style, to print a dash. Greater accuracy seems likely to be secured, for it takes a long time to acquire the art of releasing or holding down the Morse key the proper interval, and generally learners find it easier to manipulate the single-needle than the Morse. It would also appear that increased speed is obtained, since the dash is printed in equal times with the dot. Mr. Herring suggests that it would be practicable to emboss two slips at the same operation, and to give one to the sender, who would thus know with certainty what messages had been dispatched. The instrument would be especially useful as a record in a single-needle or a bell circuit.

Mr. C. W. Siemens, F.R.S., in an interesting paper, read before the Royal Society, on "The Increase of Electrical Resistance in Conductors with Rise of Temperature, and its Application to the Measure of Ordinary and Furnace Temperatures; also a Simple Method of Measuring Electrical Resistances," showed that, according to the principle of the increase of resistance in a conducting wire with increase of temperature, an instrument could be constructed for measuring with extreme accuracy the temperature at distant or ordinarily inaccessible places, such as the top of a mountain, or the interior of a furnace where metallurgical or other operations are conducted. To measure temperatures not exceeding 100° centigrade, the apparatus is so arranged that two similar resistance coils are connected by a light cable containing three wires insulated from each other. These coils are termed "the thermometer coil" and "the comparison coil." The thermometer coil, carefully rendered impervious to moisture, is placed in the situation of which it is desired to ascertain the temperature; and the comparison coil is plunged into a test-bath, whose temperature is raised or lowered until an electrical balance is obtained between the two coils, as indicated by a galvanoscope. This equality of resistance can only take place when the coils are at the same temperature,—consequently the temperature of the test solution, measured by a delicate thermometer, is the temperature of the situation of the thermometer coil. The temperature of the connecting wires between the coils, which may be some miles apart, would materially affect the correctness of the measurements, if this source of error were not eliminated by means of a third wire connecting both coils of the instrument. The second portion of the paper is devoted to the description of an instrument for measuring electrical resistances without the aid of a magnetic needle or set of resistance coils, such an instrument being much called for on shipboard, where the motion renders delicate manipulation with nicely-balanced needles a matter of great difficulty. It consists of two graduated voltameter tubes, so connected that the battery current is divided between them, the branches including a known standard resistance and the unknown resistance to be measured. From this arrangement an expression is found for the unknown resistance, in terms of the standard resistance, by means of the volumes of gases evolved in n arbitrary units of time. Any change of atmospheric pressure affects both tubes of the voltameter equally; therefore any error from this cause is obviated. The upper ends of the tubes are closed by small weighted valves, which can be raised after each observation to allow of the escape of the gases without taking the instrument to pieces. Mr. Siemens has aptly termed this valuable invention "a differential voltameter."

Referring to Professor A. A. Mayer's method of fixing and exhibiting magnetic curves, Mr. C. J. Woodward writes to say that a somewhat similar method to the one Dr. Mayer describes is in use by Mr. W. F. Barrett, of the International College. Instead of using shellac, Mr. Barrett uses a solution of gum, which is allowed to dry on a piece of glass, and when the figures are obtained they are fixed by breathing on the gummed glass. Mr. Woodward mentions, also, that he has used, at Mr. Barrett's suggestion, the lantern he devised for cohesion figures with very satisfactory results. This lantern allows the slide to be horizontal, so that the actual formation of the curves is seen on the screen when the slide is tapped. One of his pupils, Mr. Anderton, used, some years ago, a similar method to that of Dr. Mayer, only instead of shellac as a cementing material, canada balsam was used.

QUARTERLY LIST OF PUBLICATIONS RECEIVED FOR REVIEW.

The Beginning: Its When and its How. By Mungo Ponton, F.R.S.E. *Longmans & Co.*

An Introductory Text-Book of Zoology for the Use of Junior Classes. By H. Alleyne Nicholson, M.D., D.Sc., M.A., &c. *William Blackwood & Sons.*

Iron and Heat. By James Armour, C.E. *Lockwood & Co.*

The Great Pyramid of Jizeh: The Plan and Object of its Construction. *Cincinnati: Robert Clarke & Co.*

On the Distribution of Rain over the British Isles for the Year 1870. By G. J. Symons, F.M.S. *Edward Stanford.*

Fragments of Science for Unscientific People. By Professor Tyndall, F.R.S. *Longmans & Co.*

Report on the Progress of the Geological Survey of Ohio in 1869. *Columbus: Columbus Printing Co.*

A New Manual of Book-keeping for Wholesale and Retail Traders. By Philip Crellin. *Bell & Daldy.*

Classical and Prehistoric Influences upon British History; our Philanthropy from of Old, our Ever-struggling Past, and our Future. By Saxe Bannister, M.A. *Longmans & Co.*

A Sketch of Romance of Motion, or a Mode of Motion of the Planetary Bodies in Space; also a Hypothetical Analysis and Synthesis of Nitrogen. By Alec. Lee. *Longmans and Co.*

An Illustrated Natural History of British Butterflies. By Edward Newman, F.L.S., F.Z.S. *William Tweedie.*

The Meteoric Theory of Saturn's Rings; also a Paper on the Meteoric Theory of the Sun. By Lieutenant Augustus Morse Davies, B.A., &c. *Longmans & Co.*

Six Titles in Natural Law. By Richard Mansill.

The Subtropical Garden; or Beauty of Form in the Flower Garden. By W. Robinson, F.L.S. *John Murray.*

On Some Disorders of the Nervous System in Childhood. By Charles West, M.D. *Longmans & Co.*

Light Science for Leisure Hours. By Richard A. Proctor, B.A., F.R.S. *Longmans & Co.*

The Technical Educator and Encyclopœdia of Technical Education. Vol. I. *Cassell, Petter, & Galpin.*

Astronomy Simplified for General Reading; with Numerous New Explanations and Discoveries in Spectrum Analysis. By J. A. S. Rollwyn. *William Tegg.*

Introductory Text-Book of Meteorology. By Alexander Buchan, M.A., F.R.S.E. *W. Blackwood & Sons.*

The Antiseptic System: A Treatise on Carbolic Acid and its Compounds. By A. Ernest Sansom, M.D. Lond. *Henry Gillman.*

Elementary Treatise on Natural Philosophy. By A. Privat Deschanel. Translated and Edited, with Extensive Additions, by J. D. Everett, M.A., D.C.L., F.R.S.E. Part II. Heat. *Blackie & Son.*

PAMPHLETS AND PERIODICALS.

Van Nostrand's Engineering Magazine.

Results of Astronomical Observations made at the Melbourne Observatory in the Years 1866, 1867, and 1868, under the Direction of Robert L. J. Ellery. *Melbourne: Mason, Firth, and Co.*

Symons's Monthly Meteorological Magazine. *Edward Stanford.*

The American Journal of the Medical Sciences, Nos. 121 and 122.

The Canadian Naturalist.

Papers on Natural History, read before the Wellington Philosophical Society, by Walter Buller, F.L.S., F.C.S.

Öfversigt ap Kongl. Vetenskaps Akademiens Förhandlingar. *Stockholm: P. A. Norstedt.*

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The American Naturalist.

A Sanitary Enquiry into the Probable Causes of Yearly Epidemics in England, as Observed at Leicester. By Richard Weaver, C.E.

The Geological Magazine.

PROCEEDINGS OF LEARNED SOCIETIES, &c.

Monthly Notices of the Royal Astronomical Society.

Memoirs of the Peabody Academy of Science. Vol. I. No. II.

Proceedings of the Bath Natural History and Antiquarian Field Club. Vol. II. No. II.

Proceedings of the Torquay Natural History Society.

The Thirty-Eighth Annual Report of the Royal Cornwall Polytechnic Society.

Proceedings of the Lyceum of Natural History of New York.

Transactions of the Woolhope Naturalists' Field Club for 1870.

THE QUARTERLY

JOURNAL OF SCIENCE.

OCTOBER, 1871.

I. THE FUEL OF THE SUN.

By W. Mattieu Williams, F.C.S.

At the suggestion of the editor, I offer to the readers of the "Quarterly Journal of Science" the following sketch of the main argument worked out more fully in the essay I have published under the above title, hoping that many who hesitate to plunge into a presumptuous speculative work of more than 200 octavo pages may read this article, and reflect upon the subject.

The book has been handled in a most courteous and indulgent spirit by all the reviewers who have noticed it, but none have ventured to grapple with the argument it contains, although every possible opportunity and provocation for doing so is designedly afforded. It all rests upon the question which is discussed in the first three chapters, viz., Whether the atmosphere which surrounds our earth is limited or unlimited in extent? If my reasoning upon this fundamental question is refuted, all that follows necessarily falls to the ground. If I am right, all our standard treatises on pneumatics and meteorology, which repeat the arguments contained in Dr. Wollaston's celebrated paper, must be remodelled. At the outset, I reprint that paper, and point out a very curious and monstrous fallacy which, for half a century, has remained undetected, and has been continually repeated. As the main point of issue between myself and Dr. Wollaston is merely a question of very simple arithmetic and geometry, nothing can be easier than to set me right if I am wrong; and, as the philosophical consequences depending upon this issue are of vast and fundamental importance, the question cannot be ignored by those who stand before the world as scientific authorities, without a practical abdication of their philosophical responsibilities. Any man who publishes an astronomical or meteorological treatise without discussing this question, which stands before him at the threshold of his subject, is unfit for the task he has undertaken, and unworthy of public confidence.

This may appear a strong conclusion just now, but a few years will be sufficient to graft it firmly into the growth of scientific public opinion.

"The Fuel of the Sun" is simply an attempt to trace some of the consequences which must of necessity result from the existence of an universal atmosphere, and it differs from other attempts to explain the great solar mystery, by making no demands whatever upon the imagination, *inventing* nothing,—no outside meteors, no new forces or materials. It supposes nothing whatever to exist but the known facts of the laboratory—the familiar materials of the earth and its atmosphere. It is shown that these materials and the forces residing within them must of necessity produce a sun, and manifest eternally all the observed solar phenomena, provided only they are aggregated in the quantities which our own central luminary presents, and are surrounded by attendant planets such as his. Nothing is assumed or taken for granted beyond the simple fundamental hypothesis that the laws of nature are uniform throughout the universe. The argument thus conducted leads us step by step to a natural, necessary, and connected explanation of the following important phenomena :—

1. The sources of solar and stellar heat and light.
2. The means by which the present amount of solar heat and light must be maintained so long as the solar system continues in existence.
3. The origin of the general and particular phenomena of the sun-spots.
4. The cause of the varying splendour of the photosphere, including such details as the "faculæ," "mottling," "granulations," &c., &c.
5. The forces which upheave the solar prominences.
6. The origin of the corona and zodiacal light.
7. The origin of the meteorites and the asteroids.
8. The meteorological phenomena of the planets.
9. The origin of the rings of Saturn.
10. The origin of the special structure of the nebulæ.
11. The source of terrestrial magnetism, and its connection with solar activity.

The first and second chapters are devoted to an examination of the limits of atmospheric expansibility. The experimental investigations of Dr. Andrews, Mr. Grove, Mr. Gassiot, and M. Geissler are cited to prove that the expansibility of the atmosphere is unlimited, and other cosmical evidence is adduced in support of this conclusion.

As this, which is really the foundation of the whole argument, is directly opposed to the views expressed by Dr. Wollaston, in his celebrated paper on "The Finite Extent of the Atmosphere," published in 1822, and generally accepted as established science, this paper is reprinted in the second chapter, and carefully examined.

Dr. Wollaston says "that air has been rarefied so as to sustain 1-100th of an inch of barometrical pressure," and further that "beyond this limit we are left to conjectures founded on the supposed divisibility of matter; if this be infinite, so also must be the extent of our atmosphere."

I contend that our knowledge of the whole subject is fundamentally altered since these words were written. We are no longer "left to conjectures founded on the supposed divisibility of matter" to determine the possibility of further expansibility than that indicated by 1-100th of an inch of barometrical pressure, as we now have means of obtaining ten times, a hundred times, a thousand times, or even an infinitely greater rarefaction than Wollaston's supposed limit, an absolute vacuum being now obtainable; and although the transmission of electricity affords a means of testing the existence of atmospheric matter with a degree of delicacy of which Wollaston had no conception, we are still unable to detect any indication of any limit to its expansibility.

The most remarkable part of Dr. Wollaston's paper is the *reductio ad absurdum* by which he seeks to finally demonstrate the finite extent of our atmosphere. He maintains, as I do, that if the elasticity of our atmosphere is unlimited, its extension must be commensurate with the universe, that every orb in space will, by gravitation, gather around itself an atmosphere proportionate to its gravitating power, and that, by taking the known quantity of the earth's atmosphere as our unit, we may calculate the amount of atmosphere possessed by any heavenly body of which the mass is known. On this basis, Dr. Wollaston calculates the atmosphere of the sun, and concludes that its extent will be so great as to visibly affect the apparent motions of Mercury and Venus, when their declination makes its nearest approach to that of the sun. No such disturbance being actually observable, he concludes that such an atmosphere as he has calculated cannot exist. In like manner he calculates the atmosphere of Jupiter, and finds it to be so great that its refraction would be sufficient "to render the fourth satellite visible to us when behind the centre of the planet, and consequently to make it appear on both (or all) sides at the same time."

On examining these calculations, I have discovered the very curious error above referred to. As this is a matter of figures that cannot be abridged, I must refer the reader to the original calculations. I will here merely state that the result of Wollaston's method of calculating the solar gravitation atmosphere and that of Jupiter and the moon is the monstrous conclusion that, in ascending from the surface of the given orb, we always have the same limited amount of atmospheric matter above as that with which we started, although we are continually leaving a portion of it below.

Wollaston's mistake is based on the assumption that, under the circumstances supposed, the atmospheric pressure and density, at any given distance from the centre of the given orb, will vary inversely with the square of the distance. As the area of the base upon which such pressure is exerted varies *directly* with the square of the distance, the total atmosphere above every imaginable starting-distance would thus be ever the same. That this assumption, so utterly at variance with the known laws of atmospheric distribution, should have remained unchallenged for half a century, and that the conclusions based upon it should be accepted by the whole scientific world, and repeated in all our standard treatises, is, I think, one of the most remarkable curiosities presented by the history of science. If it were merely a little cobweb in some obscure corner of philosophy, there would be nothing surprising in its escape from the besom of scientific criticism, but this is so far from being the case that it has hung, since 1822, like a dark veil obscuring another, a wider and most interesting view of the universe which the idea of an universal atmosphere opens out. But I must now proceed to the next stage of the argument.

Starting from the conclusion reached in the previous chapters, that the atmosphere of our earth is but a portion of an universal elastic medium which it has attached to itself by its gravitation, and that all the other orbs of space must, in like manner, have obtained their proportion, I take the earth's mass, and its known quantity of atmospheric envelope as units, and calculating, by the simple rule I have laid down in opposition to Wollaston's, I find that the total weight of the sun's atmosphere should be at least 117,681,623 times that of the earth's, and the pressure at its base equal, at least, to 15,233 atmospheres. What must be the results of such an atmospheric accumulation?

The experiment of compressing air in the condensing syringe, and thereby lighting a piece of German tinder, is familiar to all who have studied even the rudiments of physical science. Taking the formulæ of Leslie and Dalton, and applying them to the solar pressure of 15,233 atmospheres, we arrive, according to Leslie, at the inconceivable temperature of 380,832° C., or 685,529° F., as that due to this amount of compression, or, according to Dalton, at 761,665° F. What will be the effects of such a degree of heat upon materials similar to those of which our earth is composed?

Let us first take the case of water, which, for reasons I have stated, should be regarded as atmospheric, or universally diffused matter.

This brings us to a subject of the highest and widest philosophical and practical importance. I refer to the antagonism between the force of heat and that of chemical combination, to which the French chemists have given the name "dissociation." Having myself been unable to find any satisfactory English account of this subject at a time when it had already been well treated by French and German authors, in the form of published lectures and cyclopædia articles, I shall assume that others may have encountered a similar difficulty, and therefore dwell rather more fully upon this part of my present summary.

It appears that all chemical compounds may be decomposed by heat, and that, at a given pressure, there is a definite and special temperature at which the decomposition of each compound is effected. For the absolute and final establishment of the universality of this law further investigations are necessary, actual investigations having established it as far as they have gone, but these have not been exhaustive.

There appears to be a remarkable analogy between dissociation and evaporation. When a liquid is vaporised, a certain amount of heat is "rendered latent," and this quantity varies with the liquid and with the pressure, but is definite and invariable for each liquid at a given pressure. In like manner, when a compound is dissociated, a certain amount of heat is "rendered latent," or converted into dissociating force, and this varies with each compound and with the pressure, but is definite and invariable for each compound at a given pressure. Further, when condensation occurs, an amount of heat is evolved as temperature exactly equal to that which was rendered latent in evaporation of the same substance under the same pressure; and, in like manner, when chemical re-combination of dissociated elements occurs, an amount of heat is evolved as temperature exactly equal to that which disappeared when the compound was dissociated by heat *alone* under the same pressure.

According to the recently adopted figures of M. Deville, the temperature at which the vapour of water becomes dissociated under ordinary atmospheric pressure is 2800° C., and the quantity of heat which disappears as temperature in the course of dissociation is 2153 *calories*, *i.e.*, sufficient to raise 2153 times its own weight of *liquid* water 1° C.; but, as the specific heat of aqueous vapour is to that of liquid water as 0·475 to 1, the latent heat expressed in the temperature it would have given to aqueous vapour is = 4532° C., or 8158° F.

In order to render the analogy between the ebullition and dissociation of water more evident and intelligible, I will state it as follows:—

To commence the ebullition of water under ordinary pressure, a temperature of 100° C., or 212° F., must be attained.	To commence the dissociation of aqueous vapour under ordinary pressures, a temperature of 2800° C., or 5072° F., must be attained.
To complete the ebullition of a given quantity of water, an amount of heat must be applied, sufficient to have raised the water 537° C., or 968° F., above its boiling-point, had it not evaporated.	To complete the dissociation of a given quantity of aqueous vapour, an amount of heat must be applied sufficient to have raised the vapour 4532° C., or 8158° F., above its dissociation-point had it not decomposed.
In order that a given quantity of vapour of water shall condense, it must give off sufficient heat to raise its own weight of water 537° C., or 968° F.	In order that a given quantity of the elements of water may combine, they must give off sufficient heat to raise their own weight of aqueous vapour 4532° C., or 8158° F.

I have expressed these generalisations and analogies rather more definitely than they have been hitherto stated, but those who are acquainted with the researches of Deville, Cailletet, Bunsen, &c., will perceive that I am justified in doing so.

With the general laws of the dissociation of water thus before us, we may follow out the necessary results of the above-stated pressure and consequent evolution of heat in the lower regions of the solar atmosphere upon the large proportion of aqueous vapour which I have shown that it should contain.

It is evident that the first result will be separation of this water into its elements, accompanied with a loss of temperature corresponding to the latent heat of dissociation. We may assume that in the lower regions of the solar atmosphere the free heat evolved by mechanical compression will be more than sufficient to dissociate the whole of the aqueous vapour, and thus the dissociated gases will be left at a higher temperature than was necessary to effect their dissociation. Their condition will thus be analogous to that of superheated steam, they will have to give off some heat before they can *begin* to combine.

There will, however, be somewhere an elevation where the heat evolved by the joint compression of the elementary and combined gases will be just sufficient to dissociate the latter, and here will be the meeting surface of the combined and the uncombined constituents of water. There will be a sphere containing uncombined oxygen and hydrogen surrounded by an atmospheric envelope containing large quantities of aqueous vapour, and the temperature at this limiting surface will be just equal to that of the oxyhydrogen flame under a corresponding pressure.

What will occur under these conditions? Will the "detonating gases" behave as in the laboratory? Obviously not, as a glance at the third of the above parallel propositions will show. The dissociated gases cannot combine without giving off their 4532° of latent heat as actual temperature. This can only be effected by communication with matter which is cooler than itself.

If a bubble of steam is surrounded by water maintained at the boiling temperature, it will not condense at all, because any effort of condensation would be accompanied with an evolution of heat exactly sufficient to evaporate its own result. If, however, the surrounding water is slowly radiating, or otherwise losing its heat, the enclosed bubble of steam will condense proportionately by giving off to its envelope an amount of its latent heat just sufficient to maintain the water at the boiling-point.

For further illustration, let us conceive the case of a certain quantity of the elements of water heated exactly to the temperature of dissociation, and confined in a vessel the sides of which are maintained externally at precisely the same temperature as the gases within, so that no heat can be added or taken away from them. No sensible amount of combination could now take place, as the first infinitesimal effort of combustion, or combination, would set free just the amount of heat required to decompose its own result. Let us now suppose a modification of these conditions, viz., that the vessel containing the dissociated gases, at the temperature of dissociation, shall be surrounded with bodies cooler than itself, *i.e.*, capable of receiving more heat from it than they radiate towards it; there would then take place just so much combustion as would set free the amount of heat required to maintain the temperature of the vessel at the dissociation-point; or, in other words, combustion would go on to the extent of setting free just so much heat as the gaseous mass was capable of radiating, or otherwise transmitting to surrounding bodies; and this amount of combustion would continue till all the gases had combined.

We have only to give this hypothetical vessel a spherical form and an internal diameter of 853,380 miles,—to construct its enveloping sides of a thick shell of aqueous vapour, &c., and then, by placing in the midst of the contained dissociated gases a nucleus of some kind, we are hypothetically supplied with the main conditions which I suppose to exist in the sun.

A little reflection upon the application of the above-stated laws to these conditions will show that the stupendous ocean of explosive gases would constitute an enormous stock of fuel capable, by its combustion, of setting free exactly the same quantity of heat as had previously been converted into decomposing or separating force; the amount of combustion would always be limited by the possible amount of radiation, and the radiation would again be limited by the resisting envelope of aqueous vapour produced by this combustion.

If these conditions existed in a perfectly calm and undisturbed solar atmosphere, there would be a continually-increasing external envelope of aqueous vapour, and a continually-diminishing inner atmosphere of combustible gases; there would be a gradual diminution of the amount of solar radiation, and a slow and perpetually-retarding progress towards solar extinction.

It should be noted that, according to this explanation, the *supply* of heat is originally derived from atmospheric condensation due to gravitation, that the *storage* of surplus heat is effected by dissociation, and its *evolution* mainly by recombination or combustion.

The great difficulty, that of the perpetual renewal of the solar fuel, still remains unsolved; the fact that during the millions of years of geological history we find no indications of any declining average of solar energy is so far still unexplained by this, as by every other, attempt to account for the origin of solar and stellar light and heat.

In his inaugural address to the British Association Meeting of 1866, Mr. Grove put the following very suggestive question:—"Our sun, our earth, and planets are constantly radiating heat into space; so, in all probability, are the other suns, the stars, and their attendant planets. What becomes of the heat thus radiated into space? If the universe has no limit,—and it is difficult to conceive one,—there is a constant evolution of heat and light; and yet more is given off than is received by each cosmical body, for otherwise night would be as light and as warm as day. What becomes of the enormous force thus apparently non-recurrent in the same form?"

This is a grand question, a philosophical thought worthy of the author of "The Correlation of Physical Forces." Most philosophical thinkers will, I believe, agree with me in concluding that a sound reply to it will solve the great mystery of the everlasting radiations of our sun and all the other suns of the universe. So long as we regard these suns as the *sources* of continually-expended forces of light and heat, their everlasting and unabated renewal becomes a mystery utterly inscrutable to the human intellect, since the creation of new force, or any addition to the total forces of the universe, is as inconceivable to us as any addition to the total matter of the universe. The great solar question assumes a far more hopeful shape when we admit that all the forces of past radiations are somewhere diffused in space, and we ask whether a sun contains any mechanism by which it may collect and concentrate this diffused force, and thus perpetually gather from surrounding suns as much as it radiates towards them.

The next part of my work is an attempt to show that such a mechanism does exist in our solar system, and to explain its action.

We know that if atmospheric air is compressed it becomes heated, that if this heat is allowed to radiate and the air is again expanded to its original dimensions, it will be cooled below its original temperature to an extent precisely equal to the heat which it gave out when compressed. On this principle I endeavour to explain the everlasting maintenance of the solar and stellar radiations.

The sun is attended by his train of planets whose orbital motion he controls, but they in return react upon him as the moon does upon the earth. If this reaction were regular, like that of the moon upon the earth, a regular atmospheric tide would result; but the great irregularity of the dimensions, distances, and velocities of the planets produces a result equivalent to a number of clashing irregular tides in the solar atmosphere; or, otherwise stated, the centre of motion and centre of gravity of the whole system will be perpetually varying with the varying relative positions of the planets, and thus the solar nucleus and solar atmosphere will be subject to irregularities of motion, which, though very small relatively to the enormous magnitude of the sun, must be sufficient to produce mighty vortices, and thus effect a continual commingling between the outer and inner atmospheric strata.

It must be remembered that, according to the preceding, the inner or lower strata of the solar atmosphere should consist of our ordinary atmospheric mixture of oxygen and nitrogen, and the dissociated elements of water and carbonic acid, besides some of the more volatile elements of the solar nucleus. Outside of this there should be a boundary limit where the dissociated gases are combining as rapidly as their latent heat can be evolved by radiation; this will form a shell or sphere of flame,—the photosphere,—and above or beyond this will be the sphere of vapours resulting from this combustion, which, by their resistance to radiation, will limit the evolution of heat and consequent combustion.

Now the vortices above referred to will break through the shell of combustion, and drag down more or less of the outer vapour into the lower and hotter regions of dissociated gases.

As there can be no action without equal and contrary reaction, there can be no vortices, either in the solar atmosphere or a terrestrial stream, without corresponding upheavals. These upheavals will eject the lower dissociated gases more or less completely through the vapourous jacket which restrains their normal radiations, and, thus liberated, they will rush into combination with an explosive energy comparable to that which they display in our laboratories; not, however, with an instantaneous flash, but with a continuous rocket-like combustion, the rapidity of which will be determined by the possibility of radiation. The heat evolved by this combustion, acting simultaneously with the diminution of pressure, will effect a continually augmenting expansion of these upheaved gases, and as the rapidity of combustion will be accelerated in proportion to elevation above the restraining vapours, an outspreading, far in excess of that which would be due to the original upheaving force, it is to be expected.

The reader who is acquainted with the phenomena of the solar prominences will at once perceive how all these expectations are fulfilled by actual observations, especially by the more recent observations of Zollner, Secchi, &c. I need scarcely add that the clashing tide-waves are the *faculæ*, and the vortices the sun-spots.

My present business, however, is to show how these vortices and eruptions—this down-rush in one part of the solar atmosphere and up-rush in another—contribute to the permanent maintenance of the solar light and heat. It must be understood that these outbursts are only visible to us as luminous prominences during the period of their explosive outburst, and while still subject to great expansive tension. Long after they have ceased to be visible to us their expansion must continue, until they finally and fully mingle with the medium into which they are flung, and attain a corresponding degree of rarefaction. This must occur at thousands or tens of thousands of miles above the photosphere, according to the magnitude of the ejection. The spectroscopic researches of Frankland and Lockyer have shown that the atmospheric pressure at about the outer surface of the photosphere does not exceed that of our atmosphere, and as we consider that at 100 miles above this we reach the ethereal regions, I may safely regard all the upper portion of these solar ejections as having left the solar atmosphere proper, and become commingled with the general interstellar medium.

If the sun were stationary, or merely rotating, in the midst of this universal atmosphere, the same material that is ejected to-day would in the course of time return, and be whirled into the great sun-spot eddies; but such is not the case; the sun is driving through the ether with a velocity of about 450,000 miles per twenty-four hours.

What must be the consequence of this motion? The sun will carry its own special atmospheric matter with it; but it cannot thus carry the whole of the interstellar medium. There must be a limit, graduated no doubt, but still a practical limit, at which its own atmosphere will leave behind, or pass through, the general atmospheric matter. There must be a heaping or condensation of this matter in the front, a rarefaction or wake in the rear, and a continuous flow of newly encountered atmosphere around the boundaries in the opposite direction to that of the sun's motion. The result of this must be that a great portion of the ejected atmospheric matter of the prominences will be swept permanently to the rear, and its place supplied by the material occupying the space into which the sun is advancing. We are thus presented with a mighty machinery of solar respiration; some of this newly arriving atmospheric matter must be stirred into the vortices, its quantity being exactly equivalent to that of the old material expired by the explosive eruptions, and left in the rear.

Now, the new atmospheric matter which is thus encountered and inspired, is the recipient of the everlasting radiations whose destination is the subject of Mr. Grove's enquiry; and these, when thus encountered and compressed, will of necessity evolve more or less of the heat which, through millions of millions of centuries they have been gradually absorbing; while, on the other hand, the expired or ejected matter of the gaseous eruptions will, like the artificially compressed air above referred to, have lost all the heat which during its solar existence it had by compression, dissociation, and re-combination, contributed to the solar radiations. Therefore, when again fully expanded, it will be cooler than the general medium from which it was inspired by the advancing sun.

The daily supply of fresh atmospheric fuel will be a cylinder of ether of the same diameter as the sun and 450,000 miles in length! I have calculated the weight of this cylinder of ether on the assumption (which of course is purely arbitrary) that the density of the interstellar medium is one ten-thousandth part of that of our atmosphere. According to this its weight would be 14,313,915,000,000,000,000 tons, affording a supply of 165 millions of millions of tons per second; or, if we assume the interstellar medium to have a density of only one millionth of that of our atmosphere, the supply would be rather more than one and a half millions of millions of tons per second. The proportion of this which is effective in the manner above stated is that which becomes stirred into the lower regions of the sun in exchange for the ejected matter of the prominences.

I will not here dwell upon the bombardment hypothesis, beyond observing that my explanation of solar phenomena supplies a continuous bombardment of the above stated magnitude without adding anything to the magnitude of the sun.

So far, then, I answer Mr. Grove's question, by showing that the heat radiated into space by each of the solid orbs that people its profundities, is received by the universal atmospheric medium; is gathered again by the breathing of wandering suns, who inspire as they advance the breath of universal heat and light and life; then by impact, compression, and radiation, they concentrate and re-distribute its vitalising power; and after its work is done, expire it in the broad wake of their retreat, leaving a track of cool exhausted ether—the ash-pits of the solar furnaces—to re-absorb the general radiations, and thus maintain the eternal round of life.

But ere this, a great difficulty has probably presented itself to the mind of the reader. He will refer to the calculations that have been made in order to determine the actual temperature of the solar surface and the intensity of its luminosity. Both of these are vastly in excess of those obtained in our laboratory experiments by the combustion of the elements of water. Even taking into consideration the dissociated carbonic acid whose elements should be burning in the photosphere with those of water, and adding to these the volatile metals of the solar nucleus whose dissociated vapours must, under the circumstances stated, be commingled with those of the solar atmosphere, and therefore contribute to the luminosity by their combustion, still by burning here on the earth a jet of such mixed gases and vapours we should not obtain any approach to either the luminosity or the temperature which is usually attributed to the sun.

I have made a few very simple experiments, the results of which I think remove entirely these difficulties. They were made with the assistance of Mr. Jonathan Wilkinson, the official gas examiner to the Sheffield Corporation, using his photometric and gas measuring apparatus. We first determined the amount of light radiated by a single fish-tail gas-burner consuming a measured quantity of gas per hour. We found that when another was placed behind this, so that all the light of the second had to pass through the first, that the light of the two (measured by the illuminating intensity of their radiations upon a screen just as the solar luminosity has been measured) was just double that of one flame, three flames (still presenting to the photometric screen only the surface of one) gave it three times the amount of illumination, and so on with any number of flames we were able to test. Mr. Wilkinson has since arranged 100 flames on the same principle, *i.e.*, so that the 99 hinder flames shall all radiate through the one presented to the screen, thus affording the same surface as a single flame, but having 100 times its *thickness* or *depth*, and he finds that the law indicated by our first experiments is fully verified; that the 100 flames thus arranged illuminate the screen 100 times as intensely as the single flame. Other modifications of these experiments described in chapter 7 of "The Fuel of the Sun," establish the principle that a common hydrocarbon gas flame is transparent to its own radiations, or in other words, that the amount of light radiated from such a flame, and its apparent intensity of luminosity, is proportionate to its thickness; therefore the luminosity of the sun may be produced by a photosphere having no greater intrinsic brilliancy than the flame of a tallow candle, provided the flame is of sufficient depth or thickness. I see good reasons for inferring that its intrinsic brilliancy is less than that of a candle—somewhere between that and a Bunsen's burner.

I made a similar series of experiments upon the radiation of the *heat* of flames through each other, and arrived at similar results; but my apparatus in these experiments was not so delicate and reliable as in the experiments on light, and, therefore, I cannot so decidedly affirm the absolute diathermacy of flame to its own radiations. Within the limits of error of these experiments, I found that with the same radiant surface presented to the thermometer, every addition to the thickness of the flame produced a proportionate increase of radiation.

This important law, though hitherto unnoticed by philosophers, is practically understood and acted upon by workmen who are engaged in furnace operations. Present space will not permit me to illustrate this by examples, but in passing I may mention the "mill furnaces," where armour plates and other large masses of iron are raised to a welding temperature by radiant heat, and the ordinary puddling furnace, where iron is melted by radiant heat. In both of these special arrangements are made to obtain a "body" or thickness of radiant flame, while *intensity* of combustion is neglected and even carefully avoided.

According to this there are two factors engaged in producing the radiant effect from a given surface, *intensity* and *quantity*, *i.e.*, *brilliancy* and *thickness* in the case of light, and *temperature* and *thickness* in the case of heat. In the Bude light, for example, consisting of concentric rings of coal gas we have small intensity with great quantity, in the lime light we have a mere surface of great brilliancy but no thickness. If I am right the surface of the moon may be brighter than the luminous surface of the sun, the peculiarities of moonlight depending upon intensity, those of sunlight upon quantity of light.

The flame that roars from the mouth of a Bessemer converter has but small intrinsic brilliancy, far less than that of an ordinary gas flame, as may be seen by observing the thin waifs that sometimes project beyond the body of the flame. Nevertheless, its radiations are so effective that it is a painfully dazzling object even in the midst of sunny daylight; but then we have here not a hollow flame fed only by outside oxygen, but s solid body of flame several feet in thickness. Even the pallid carbonic acid flame which accompanies the pouring of the spiegeleisen has marvellous illuminating power.

The reader will now be able to understand my explanation of the sun-spots, of their nucleus, umbra, and penumbra. From what I have stated respecting the planetary disturbances of the solar rotation, the photosphere should present all the appearances due to the movements of a fiery ocean, råging and seething in the maddest conceivable fury of perpetual tempest. If the surface of a river flowing peacefully between its banks is perforated with conical eddies whenever it meets with a projecting rock or obstacle, or other agency which disturbs the regularity of its course, what must be the magnitude of the eddies in this ocean of flame and heated gases, when stirred to the lowest depths of its vast profundity by the irregular reeling of the solar nucleus within. Obviously, nothing less than the sun-spots; those mighty maelströms into which a world might be dropped like a pea into an egg-cup.

When the photosphere or shell of combining gases is thus ripped open, the telescopic observer looks down the vortex, which, if deep enough, reveals to him the inner region of dissociated gases and vapours. But these have the opposite property to that which I have shown to belong to flame ; they are opaque to their own special radiations, while the flame is transparent to the light of the inner portions of itself. Thus, the dissociated interior of the solar envelope, though absolutely white-hot, will be comparatively dark (direct experiment has proved that the darkness of the spots is only relative).

The sides of the vortex funnel will consist of a mixture of dissociated gases, flaming gases, and combined gases, and will thus present various thicknesses of flame, and thereby effect the various shades of the penumbra. Space will not permit me here to follow up the details of this subject, as I have done in the original work, where it is shown that if the telescope had not yet been invented, all the telescopic details of spot phenomena might have been described *à priori* as necessary consequences of the constitution I have above ascribed to the sun.

Not merely the great spot phenomena, but all the minor irregularities of the photosphere follow with similarly demonstrable necessity. Thus the many interfering solar tides must throw up great waves, literally mountainous in their magnitude, the summits and ridges of which, being raised into a higher region of the absorbing vaporous atmosphere that envelopes the photosphere, will radiate more freely, its dissociated matter will combine more abundantly, and will thicken the photosphere immediately below; this thicker flame will be more luminous than the normal surface, and thus produce the phenomena of the *faculæ*.

Besides these great ground-swells of the flaming ocean of the photosphere, there must be lesser billows, and ripples upon these, and mountain tongues of flame all over the surface. The crests of these waves, and the summits of these flame-alps, presenting to the terrestrial observer a greater depth of flaming matter, must be brighter than the hollows and valleys between; and their splendour must be further increased by the fact, that such upper ridges and summits are less deeply immersed in the outer ocean of absorbing vapours, which limits the radiation of the light as well as the heat of the photosphere. The effect of looking upon the surface of such a wild fury of troubled flame, with its confused intermingling of gradations of luminosity, must be very puzzling and difficult to describe; and hence the "willow leaves," "rice grains," "mottling," "granules," "things," "flocculi," "bits of white thread," "cumuli of cotton wool," "excessively minute fragments of porcelain," "untidy circular masses," "ridges," "waves," "hill knolls," &c., &c., to which the luminous irregularities have been compared.

At the time I wrote, the means of examination of the edge of the sun by the spectroscope was but newly discovered, and the results then published referred chiefly to the prominences proper. Since that, a new term has been introduced to solar technology, the "sierra," and the observations of the actual appearances of this sierra precisely correspond to my theoretical description of the limiting surface of the photosphere, which was written before I was acquainted with these observed facts. This will be seen by reference to chapter 10, the subject of which is, "The Varying Splendour of Different Portions of the Photosphere."

But I must not linger any further upon this part of the subject, but proceed to another, where subsequent discoveries have strongly confirmed my speculations.

The mean specific gravity of the sun is not quite $1\frac{1}{2}$ times that of water. The vapours of nickel, cobalt, copper, iron, chromium, manganese, titanium, zinc, cadmium, aluminium, magnesium, barium, strontium, calcium, and sodium have been shown by the spectroscope to be floating on the outer regions of the sun. None of these could constitute the body of the sun in a solid or liquid state, and subject to the enormous pressure which such a mass must exert upon itself without raising the mean specific gravity vastly above this; nor is there any other kind of matter with which we are acquainted which could exist within so large a mass in a liquid or solid state, and retain so low a density.

I must confess that my faith in the logical acumen of mathematicians has been rudely shaken by the manner in which eminent astronomers have described the umbra or nucleus of the sun-spots as the solid body of the sun seen through his luminous atmosphere, and the solid surface of Jupiter seen through his belts, and have discussed the habitability of Jupiter, Saturn, Uranus, and Neptune always on the assumption of their solidity, while the specific gravity of all of these renders this surface solidity a demonstrable physical impossibility.

If the sun (or either of these planets) has a solid or liquid nucleus, it must be a mere kernel in the centre of a huge orb of gaseous matter, and though I have spoken rather definitely of the solar atmosphere in order to avoid complication, I must not, therefore, be understood to suppose that there exists in the sun any such definite boundary to the base of the atmospheric matter as we find here on the earth. The temperature, the density, and all we know of the chemistry of the sun justify the conclusion that in its outer regions, to a considerable depth below the photosphere, there must be a commingling of the atmospheric matter with the vapours of the metals whose existence the spectroscope has revealed. Some of these must be upheaved together with the dissociated elements of water. They are all combustible, and, with a few exceptions, the products of their combustion would solidify after they were projected beyond the photosphere. Much of the iron, nickel, cobalt, and copper might pass through the fiery ordeal of such projection, and solidify without oxidation, especially when more or less enveloped in uncombined hydrogen.

It is obvious that, under these circumstances, there must occur a series of precipitations analogous to those from the aqueous vapour of our atmosphere. These gaseous metals, or their oxides, must be condensed as clouds, rain, snow, and hail, according to their boiling and melting points, and the conditions of their ejection. We know that sudden and violent atmospheric disturbance, accompanied with fierce electrical discharges, especially favour the formation of hailstones in our terrestrial atmosphere. All such violence must be displayed on a hugely exaggerated scale in the solar outbursts, and therefore the hailstone formation should preponderate, especially as the metallic vapours condense more rapidly than those of water on account of the much smaller amount of their specific heat and of the latent heat of their vapours.

What will become of these volleys of solid matter thus ejected with the furious and protracted explosions forming the solar prominences? In order to answer this question, we must remember that the spectroscope, as recently applied, merely displays the gaseous, chiefly the hydrogen, ejections; that these great gaseous flames bear a similar relation to the solid projectiles that the flash of a gun does to the grape-shot or cannon-ball. Mr. Lockyer says: "In one instance, I saw a prominence 27,000 miles high change enormously in the space of ten minutes; and, lately, I have seen prominences much higher born and die in an hour." He has recently measured an actual velocity of 120 miles per second in the movements of this *gaseous* matter of the solar eruptions, the initial velocity of which must have been much greater. If such is the velocity of the gaseous ejections, what must be that of the solid projectiles, and where must they go? A cosmical cannonade thus follows as a necessary result of the conditions I have sketched, and as prominence-ejections of greater or lesser magnitude are continually in progress, there must be a continual outpouring from the sun of solid fragments, which must be flung far beyond the limits of the gaseous prominences. As the luminosity of these glowing particles must be very small compared with that of the photosphere, they will be invisible in the glare of ordinary sunshine, but if our eyes be protected from this, they may then be rendered visible, both by their own glow and the solar light they are capable of reflecting. They should be seen during a total eclipse, and should exhibit radiant streams proceeding irregularly from different parts of the sun, but most abundantly from the neighbourhood of the spot regions. As these spot-regions occupy the intermediate latitudes between the poles and the equator of the sun, the greatest extensions of these outstreamings should be N.E. and S.W., and S.E. and N.W., while to the N., S., E., and W.—that is, opposite the poles and equator of the sun—there should be a lesser extension. The result of this must be an approximation to a quadrilateral figure, the diagonals of which should extend in a N.E. and S.W., and a S.E. and N.W. direction, or thereabouts. I say "thereabouts," because the zone of greatest activity is not exactly intermediate between the poles and the equator, but lies nearer to the solar equator.

Examined with the polariscope, these radiant streams should display a mixture of reflected light and self-luminosity. Examined with the spectroscope, a faint continuous spectrum due to such luminosity of solid particles should be exhibited, with possibly a few doubtful lines due to the small amount of vapour which, in their glowing condition, they might still give off. Besides this, there should appear the spectroscope indications due to violent electrical discharges, which must occur as a necessary concomitant of the furious ejections of aqueous vapour and solid particles. All these metallic hailstones must be highly charged, like the particles of vesicular vapour ejected from the hydro-electric machine, or the vapours and projectiles of a terrestrial volcanic eruption.

I need scarcely add that this exactly describes the actually-observed results of the recent observations on the corona, and that all the phenomena of this great solar mystery are but necessary and predicable results of the constitution I ascribe to the sun.

There is a method of manufacturing hypotheses which has become rather prevalent of late, especially among mathematicians, who take observed phenomena, and then arbitrarily and purely from the raw material of their own imagination construct explanatory atoms, media, and actions, which are shaved and pared, scraped and patched, lengthened and shortened, thickened and narrowed, till they are made to fit the phenomena with mathematical accuracy. These laborious creations are then put forth as philosophical truths, and, *afterwards*, the accuracy of their fitting to the phenomena is quoted as evidence of the positive reality of the ethers, atoms, undulations, gyrations, collisions, or whatever else the mathematician may have thus skilfully created and fitted. It appears to me that such fitness only proves the ingenuity of the fitter,—the skill of the mathematician,—and that all such hypotheses belong to the poetry of science; they should be distinctly labelled as products of mathematical imagination, and nowise be confounded with objective natural truths. Such products of the imagination of the expert may assist the imagination of the student in comprehending some phenomena, just as "Jack Frost" and "Billy Wind" may represent certain natural forces to babies; but if Jack Frost, Billy Wind, electric and magnetic fluids, ultimate atoms, interatomic ethers, nervous fluids, &c., are allowed to invade the intellect, and are accepted as actual physical existences, they become very mischievous philosophical superstitions.

I make this digression in order to repudiate any participation in this kind of speculation. Though "The Fuel of the Sun" is avowedly a very bold attempt to unravel majestic mysteries, I have not sought *to elucidate the known by means of the unknown*, as do these inventors of imaginary agents, but have scrupulously followed the opposite principle. I have invented nothing, but have started from the experimental facts of the laboratory, the demonstrated laws of physical action, and have followed up step by step what I understand to be the necessary consequences of these. Many years ago, I convinced myself that our atmosphere is but a portion of universal atmospheric matter, that Dr. Wollaston was wrong, and that the compression of this universal atmospheric matter is possibly the source of solar light and heat; but as this was long before M. Deville had investigated the subject of dissociation by heat,* I was unable to work out the problem at all satisfactorily. When I subsequently resumed the subject, I knew nothing about the corona, and had only read of the "red prominences" as possible lunar appendages, or solar clouds, or optical illusions. I had worked out the necessity of the gaseous eruptions, and their action in effecting an interchange of solar and general atmospheric matter, as the means of maintaining the solar light and heat, with no idea of proceeding further with the problem, when the announcement that the prominences were not merely unquestionable solar appendages, but were actually upheaved mountains of glowing hydrogen, suddenly and unexpectedly suggested their identity with my required atmospheric upheavals. It is true that their observed magnitude far exceeded my theoretical anticipations, and in this respect I have made some *à posteriori* adaptations, especially with the aid of a clearer understanding of the laws of dissociation which almost simultaneously became attainable.

In like manner, the necessity of the solid ejections presented themselves before I knew anything of the recently-discovered details of the coronal phenomena—when I had merely read of a luminous halo which had been seen around the sun, and vaguely supposed it to be due to some sort of atmospheric illumination. I inferred that streams of solid particles must be pouring from the sun, and showering back again, but had no idea that such streams and showers were

* My first memorandum on the subject is dated 23rd April, 1840, in a "Register of Ideas," then commenced in very early student days.

actually visible until I was rather startled at finding the corona, instead of being, as I had rather loosely supposed, a mere uniform filmy halo, had been described by Mr. De la Rue in his Bakerian Lecture on the Eclipse of 1860, as "softening off with very irregular outline, and sending off some *long streams*," &c. I was then living on the sides of a Welsh mountain far away from public libraries, and being no astronomer, my own books kept me better acquainted with the current progress of experimental than with astronomical science.

Even when "The Fuel of the Sun" was published I knew nothing of the American observations of the quadrangular figure of the corona, or should certainly have then quoted them, nor of the fact revealed by the Eclipse of December, 1870, that, "wherever on the solar disc a large group of prominences was seen on Mr. Seabroke's map, there a corresponding bulging out of the corona was chronicled on Professor Watson's drawing; and at the positions where no prominences presented themselves, there the bright portions of the corona extended to the smallest distances from the sun's limb;" and that Mr. Brothers's photographs *all* show the corona extending much farther towards the west than towards the east, the west being "the region richest in solar prominences." I am sorry that the limits of this paper will not permit me to enter more fully into the bearings of the recent studies of the corona and the prominences upon my explanations of solar phenomena, especially as the differences between the inner and outer corona which still appears to puzzle astronomers are exactly what my explanation demands. I must make this the subject of a separate paper, and proceed at once to the next step of the general argument.

Assuming that such ejections of solid matter are poured from the prominences, to what distances may they travel? In attempting to answer this question, I confessedly ventured upon dangerous ground, for at the time I wrote I only knew that the force of upheaval of the prominences must be enormous, *probably* sufficient to eject solid matter beyond the orbit of the earth and even beyond that of Mars. Actual measurements of the eruptive velocity of the solar prominences have since been made, and they are so great as to relieve me of my quantitative difficulty, and show that I was quite justified in the bold inference that these eruptions may account for the zodiacal light, the zones of meteors into which our earth is sometimes plunged, and even the outer zone of larger bodies, the asteroids.

But how, the reader will ask, can such solids, ejected from the sun, acquire orbital paths around him. "We have been taught that the parabola is the necessary path of such ejections." Mr. Proctor has evidently reasoned in this manner, for in last April number of "Fraser's Magazine" he says, that some of my ideas are "opposed to any known laws, physical or dynamical," that "there is nothing absolutely incredible in the conception, that masses of gaseous, liquid, or solid matter should be flung to a height exceeding manifold that of the loftiest of the coloured prominences; whereas it is not only incredible, but impossible, that such matter should in any case come to circle in a closed orbit round the sun."

More careful reading would have shown Mr. Proctor that I have considered other conditions besides those of the text-books, that the case is by no means one of simple radial projection from a fixed body into free space and undisturbed return. I have distinctly stated that "the recent ejections may have any form of orbit within the boundaries of the conic sections," from a straight line returning upon itself, due to absolutely vertical projection, to a circular orbit produced by the tangental projection of such curving prominences *as the ram's horn*, &c. The outline of the zodiacal light would be formed by the termination or aphelion portion of these excursions, or of such a number of them as should be sufficient to produce a visible result."

Again, speaking of the asteroids, in Chapter 14 I state that "I should have expected a still greater elongation and eccentricity in some of them, and such orbits may have existed; but an asteroid with an orbit of cometary eccentricity that would in the course of each revolution cross the paths of Mercury, Venus, the Earth, and Mars, in nearly the same plane, and dive through the thickly scattered zodiacal cluster, both in going to the sun and returning from it, would be subject to disturbances which would continue until one of two things occurred. Its tangental force might become so far neutralised and its orbit so much elongated, that finally its perihelion distance should not exceed the solar radius, when it would finish its course by returning to the sun. On the other hand, its tangental velocity might be increased by heavy pulls from Jupiter, when slowly turning its aphelion path, and be similarly influenced by friendly jerks in crossing the orbits of the inferior planets; and thus its orbit might be widened, until it ceased periodically to cross the path of any of the planets by establishing itself in an orbit constantly intermediate between any two. Having once settled into such a path, it would remain there with comparative stability and permanency. If I am right in this view of the dynamical history of these older ejections, all the long elliptical paths of zodiacal particles, meteorites, or asteroids, would thus in the course of ages become eliminated, and the remaining orbits would be of planetary rather than cometary proportions."

A little reflection on the above-stated laws of dissociation will show that the maximum violence of hydrogen explosion will not occur at the birth of the ejections, but afterwards, when the dissociated gases have been already hurled beyond the sphere of restraining vapours. If my explanation is correct, the typical form of a solar prominence should be that of a spreading tree with a tall stem. At first the least resistance to radiation and consequent explosive combination must be in the vertical direction, as this will afford the shortest line that can be drawn through the thickness of the surrounding jacket of resisting vapour; but when raised above this envelope, the dissociated gases cooled by their own expansion and comparatively free to radiate in all directions except downwards, will explode laterally as well as vertically, and thus spread out into a head. My theoretical prominence will be, in short, a monster rocket proceeding steadily upwards to a certain extent, and then bursting and projecting its missiles in every direction from the vertical to the absolutely horizontal. Should the latter acquire a velocity of about 300 miles per second, not merely a closed but even an absolutely circular orbit would be possible. These and the multitude of weaker lateral ejections, reaching the sun by short parabolic paths, explain the mystery of the inner corona.

I need only refer Mr. Proctor to his own recently published book on the Sun, where he will find on plates 4, 5, and 6, a number of drawings from Zöllner and Respighi, which so thoroughly confirm my necessary theoretical deductions that they might be a series of fancy sketches of my own. When we consider that the base of a prominence is only visible when it happens to start exactly from the limb of the sun, while the vastly greater proportion of those which are observed and have been drawn, have much of the stem cut off from view by the solar rotundity, the evidence afforded by such drawings in support of my theoretical deduction, that the typical form of the solar prominences is that of a palm-tree or bursting rocket, is greatly strengthened.*

In a paper by P. Secchi, dated Rome, March 20, 1871, and published in the "Comptes Rendus," March 27, this veteran solar observer speaks of the prominences as composed of jets, which, "upon reaching a certain elevation, stop and whirl upon themselves, giving birth to a brilliant cloud." This cloud is represented as spreading out on all sides from the summit of the combined jets. Again he says, "It is very common to see a little jet stop at a certain elevation above the chromosphere, and there spread itself out into a *wide hat* ('*un large chapeau*') of an absolutely nebulous constitution." This outspreading nebulosity is the flash of the incandescent vapours of the explosion which is theoretically demanded, by my explanation of the constitution of the sun, to occur exactly in the manner and place described. These will be rendered visible by the spectroscopic dilution of the continuous spectrum, while the solid projectiles that must proceed from them in every direction can only be seen during a solar eclipse.

The observations and drawing of Zöllner and Respighi were, for the most part, made while my book was in the press, and like those of Secchi above quoted, were unknown to me when I wrote, and I was then only able to quote, in support of my theoretical requirements, the evidences of actually observed tangental ejection afforded by Sir John Herschel's account of the great solar storm of September 1, 1859; but the special prominence which I have given to this and the rate of *horizontal motion* there measured, should have prevented Mr. Proctor from making the mistake he has in the article above referred to.

Besides this direct tangental projection there are other elements of motion contributing to the same result, such as the whirling of the prominences on themselves, their motion of translation on the sun's disc, and the rotation of the sun itself.

I must now bring this sketch to a close by stating that in order to submit the fundamental question of an universal atmosphere to an *experimentum crucis* analogous to that by which Pascal tested the atmospheric theory of Torricelli, I

* Any reader of "The Fuel of the Sun" will perceive that the vaporous envelope which I have described as "an effectual jacket for limiting the amount of radiation," is a complete theoretical anticipation and explanation of the "solar crust" of Respighi and the "Trennungschicht" of Zöllner. We agree perfectly in our conclusions, though arriving at them by such very different paths, and so independently of each other.

have calculated the theoretical density of the atmosphere of the moon and of each of the planets, and compared the results as severely as I could with the observed facts. As Jupiter is 27,100 times heavier than the moon, and between these wide extremes there are six planets presenting great variations of mass, the probabilities of accidental coincidence are overwhelmingly against me, and a close concurrence of observed telescopic refraction and other phenomena with the theoretical atmospheric density must afford the strongest possible confirmation of the soundness of the basis of my whole argument. Such a concurrence exists, and some new and very curious light is unexpectedly thrown upon the meteorology of Mars and the constitution of the larger planets; the latter, if I am right, must be miniature suns, *permanently* red- or white-hot, must have something like a photosphere, surrounded by a sphere of vapour (the outside of which we see), must have mimic spot vortices and prominences, and in the case of Saturn must eject volleys of meteoric matter, some of which should finally settle down into orbital paths, and thus produce the rings.

These are startling conclusions, and when I reached them they were utterly at variance with general astronomical opinion, but I find since their publication that some astronomers have already shown considerable readiness to adopt them. In my case this view of the solar constitution of the larger planets is not a matter of mere opinion, or guessing, or probability, but it follows of necessity, and as stated on page 200, "the great mystery of Saturn's rings is resolved into a simple consequence, a demonstrable and necessary result of the operation of the familiar forces, whose laws of action have been demonstrated here upon this earth by experimental investigation in our laboratories. No strained hypotheses of imaginary forces are required, no ethers or other materials are demanded, beyond those which are beneath our feet and around our heads here upon this earth; all that is necessary is to grant that the well-known elements and compounds of the chemist, and the demonstrated forces of the experimental physicist, exist and operate in the places, and have the quantities and modes of distribution described by the astronomer; this simple postulate admitted, these wondrous appendages spring into rational existence, and like the eternal fires of the sun, the barren surface of the moon, the dry valleys of Mercury, the hazy equivocations of Venus, the seas and continents and polar glaciers of Mars, and the cloud-covered face of Jupiter, follow as necessary consequences of an universal atmosphere."

If I am right in ascribing a gaseous condition to the sun and the larger planets, and tracing the maintenance of this condition to the disturbing gravitation of the attendant planets or satellites, a solution of the riddle of the nebulæ at once presents itself. We have only to suppose a star cluster or group composed of orbs of solar or great planetary dimensions, and that these act mutually upon each other as the planets on our sun, or the satellites upon Saturn, but in a far more violent degree owing to the far greater relative masses of the reacting orbs, and we obtain the conditions under which great gaseous orbs would be not merely pitted on their surface, but riven to their very centres, moulded and shaped throughout by the whirling hurricane of their whole substance. When thus in the centre of a tornado of opposing gravitations the tortured orb would be twisted bodily into a huge vorticose crater, into the bowels of which the aqueous vapour would be dragged and dissociated, and then, entangled with the inner matter of the riven sphere, would be hurled upwards again to burst forth in an explosion of such magnitude that the original body would be measurably presented as a mere appendage, the rocket case of the flood of fire it had vomited forth.

The reader must complete the picture. If he will take a little trouble in doing so he will find that it becomes a portrait of one or the other of the nebulæ, according to the kind of intergravitating star cluster from which he starts. I have endeavoured to work out some of the details of the nebular conditions in the 20th chapter. In chapter 21st I have concluded by showing the analogy between a sun and the hydro-electric machine, the sun being the cylinder and the prominences the steam jets. If issuing jets of high-pressure steam have the same properties at a distance of 91 millions of miles from the earth as upon its surface, the body of the sun and the issuing steam must be in opposite electrical conditions, and furious electrical excitation must result; and if the laws of electrical induction are constant throughout the universe the earth must be as necessarily subject to solar electrical influence as to his thermal radiations. Thus the same reasoning which explains the origin and maintenance of the solar heat and light, the sun spots, the photosphere, the chromosphere, the sierra, the prominences, the zodiacal light, the meteoric zones, the meteorology of the planets, and the rings of Saturn also shows how the electrical disturbances which produce the aurora borealis and direct the needle may originate.

Electrical theories of the corona and zodiacal light, and their connection of some kind with the aurora borealis, have been put forth in many shapes, but as far as I have learned none afford any explanation of the *origin* of the electrical disturbance. Without this they are like the vortices of Descartes, which explained the movements of the planets by supposing another kind of motion still more incomprehensible.

Explanations which are more difficult to explain than the phenomena they propose to elucidate only obscure the light of true science, and stand as impediments to the progress of sound philosophy.

II. MOLECULES, ULTIMATES, ATOMS, AND WAVES.

By Mungo Ponton, F.R.S.E.

(Concluded from p. 359).

Part III.

In all the foregoing estimates respecting the wave-lengths corresponding to the fixed lines of the spectrum, these have been regarded as they exist in the free ether; but when the luminous waves traverse a portion of the ether, in which any transparent substance temporarily subsists, the progress of the wave-motion is, according to the experiments of M. Fizeau, retarded in exact proportion to the refractive index of the substance. Thus, if the refractive index be 2, the speed of transmission will be halved. The returning energy, however—that by which the disturbed particles are restored to their points of rest, will be only a fourth of that which subsists in the free ether—the forces being as the squares of the speeds of transmission. Hence, for any given period of vibration, the excursion of the individual particles will be only a fourth of what they are in the free ether.

The highest ascertained refractive index is that of chromate of lead (2·974); but it is uncertain whether the extremely short waves produced by aluminium electrodes can be transmitted by that substance. There can be no doubt, however, about quartz, seeing it is by using prisms of this mineral that the existence of those very minute waves has been ascertained. The refractive index of quartz is 1·5484, so that the shortest aluminium wave would, while passing through this medium, be reduced to 151,150 billionths of an inch; while the ratio of the length of the wave to the departure of the individual particles from their points of rest would attain the enormous proportion of between 102 and 103 billions to 1. On the supposition that the departure exceeds the radius of the particle, the diameter of an ethereal particle would be to the length of this shortest wave, while passing through quartz, in the ratio of 1 to upwards of 50 billions. To such an extreme degree do these estimates render it probable, that the divisibility of matter is actually carried in the case of the individual particles of the luminiferous ether—a circumstance rendering it less improbable that even the ultimate of hydrogen, the lightest with which we are acquainted, may consist of a considerable number of more minute atoms.

There is another circumstance tending to support this supposition, namely, the peculiar action which certain bodies exert on the ether irrespective of their specific gravity—an action in which hydrogen is pre-eminent over all other substances. To illustrate this point, take the following six media, the refractive indices, and specific gravities of which are as under—that of hydrogen being adopted as the standard of unity.

	Refractive Index.	Sp. gr.
Hydrogen	1·000138	1·000
Oxygen	1·000272	16·000
Atmospheric air	1·000294	14·500
Tabasheer	1·111450	28,449
Water	1·336000	11,798
Chromate of lead . . .	2·974000	70,829

Since light travels about 11,721 inches in the millionth of a second in the free ether, and as its speed is retarded by each medium in proportion to its refractive index, the spaces through which light would pass in the millionth of a second while traversing the above six media would be as under:—

	Inches.		Inches.
Hydrogen . . .	11,719·38	Tabasheer . . .	10,545·68
Oxygen . . .	11,717·81	Water	8,773·20
Atmospheric air	11,717·55	Chromate of lead	3,941·16

Hence, to traverse a given space, say a million of inches, light would require, for the several media, the following periods expressed in millionths of a second :—

The free ether . .	85·3170	Tabasheer	94·8255
Hydrogen	85·3287	Water	113·9873
Oxygen	85·3402	Chromate of lead .	253·7326
Atmospheric air .	85·3420		

Thus the retardation of the six media is equivalent to the following additional spaces of the free ether :—

	Inches.		Inches.
For hydrogen	138	For tabasheer . . .	111,450
,, oxygen	272	,, water	336,000
,, atmospheric air .	294	,, chromate of lead	1,974,000

That is to say, the wave motion would pass through the above additional spaces of the free ether during the additional time it would require to traverse a million inches of the above refracting media, or, taking hydrogen as the standard of unity, the following numbers will represent the comparative retarding powers of these substances :—

Hydrogen. . . .	1·0000	Tabasheer . . .	812·69
Oxygen	1·9829	Water	2,450·40
Atmospheric air .	2·1367	Chromate of lead .	14,394·00

If, however, we take into account the specific gravities of the substances, the proportions will be very different. Again, making hydrogen the standard of unity, they will stand thus :—

Hydrogen	1·00000	Atmospheric air .	0·147360
Water	0·20770	Oxygen . . .	0·123930
Chromate of lead .	0·20323	Tabasheer . . .	0·028566

Hydrogen and tabasheer thus stand at the head and foot of the list, and, indeed, of all known substances ; the power of the former in retarding the progress of the wave-motion being about 35 times that of the latter, in proportion to their respective specific gravities.

It thus appears that the energy with which any substance retards the speed of the wave-motion of light passing through it, depends only to a partial extent on its specific gravity, and in no inconsiderable degree on a certain special power, in which hydrogen excels all other substances. This power must obviously be due to some peculiar relation subsisting between the particles of the ponderable substance and those of the ether in which they are immersed.

The relation between the ether and the ultimates of any permanent gas, like hydrogen, manifests itself to a certain extent in a tendency to separate those ultimates one from another, in opposition to their mutual attraction and their gravitation towards the earth. It is only by a very considerable amount of pressure that the ultimates of hydrogen are kept in that degree of mutual proximity in which they subsist at the earth's surface, when they exhibit that degree of retarding force which is shown in the preceding tables.

The amount of this pressure is equivalent to that of 30 inches of mercury, corresponding to 4,809,528 inches of hydrogen of uniform density. Thus, in order to maintain the ultimates of hydrogen contained in a length of a million inches, in that degree of mutual proximity which enables them to exert on the wave-motion of light a retarding force equivalent to that which would be exerted by only 138 inches of the free ether, requires the gravitating force of nearly five times that quantity of hydrogen. The force which balances this pressure can be nothing but the repulsion of the particles of the ether for the ultimates of hydrogen. In the length of a million inches, the same number of ethereal particles exist in the absence of the hydrogen, as in its presence, the hydrogen ultimates insinuating themselves between the particles of the ether. Their doing so is facilitated by the extreme minuteness of the probable diameter of the ethereal particles, in comparison with the size of the intervals between them. Of the ratio which the former bears to the latter, a fair estimate may be formed from the proportion which the departure of the particles from their points of rest, in the case of the shortest wave, bears to what it is in the case of the longest wave—this proportion being as 1 to 42,000. The effect produced by the hydrogen is the same as would follow the introduction of an additional quantity of 138 inches into the million inches of the ether—equivalent to an addition of 1-7246th part of the number of ethereal particles embraced in the latter space, or to a corresponding diminution in the length of the intervals between them.

Were the ultimates of hydrogen homogeneous simple masses, they might be supposed to be interspersed along the line of ethereal particles at comparatively considerable intervals; while the ethereal particles intervening between each pair of hydrogenous ultimates might be supposed to be forced into greater mutual proximity—so having their mutual distances diminished by 1-7246th part. But according to this view, the compressing force could be only the mutual gravitation of the hydrogenous ultimates and their general gravitation towards the earth, aided by the superincumbent pressure. When it is borne in mind, however, that the mutually repellent force of the ethereal particles is more than a billion of times greater than the force of terrestrial gravitation as it subsists in the lower strata of the atmosphere, it will be perceived how inadequate the latter is to produce the observed amount of compression, and that some other force must be looked to, as that which countervails the repulsive power of the ethereal particles to so great an extent as 1-7246th part of its amount.

The phenomenon might be explained by supposing the existence of some special repulsive energy, as between the ethereal particles and the ultimates of hydrogen, rendering these, in proportion to their specific gravity, conspicuous beyond all others in their power of driving the ethereal particles into closer mutual proximity. An alternative and better explanation, however, is furnished by the supposition that the ultimates of hydrogen are not simple homogeneous masses, but are really compounded of numerous and diverse more minute atoms, held together by a force greatly exceeding that of gravity, or even those of ordinary cohesion and chemical attraction. For to prevent such atoms from passing into absolute contact, and to admit of their being thrown into a state of vibration, the ether separating them must be compressed to such an extent as to render it capable of balancing the force by which the atoms are held in such close mutual proximity. Thus, making a small allowance for the force of gravity, we obtain as a measure of the attractive force by which the atoms constituting the ultimate of hydrogen are held together, about 1-7240th part of the total repulsive energy which the ethereal particles exert one upon another.

Now the observations on the spectrum tend to render the latter explanation of the extraordinary power of hydrogen in compressing the ether the more probable of the two. These show that hydrogen can either generate or interrupt ethereal waves of four different lengths and periods. They are as under—the lengths being stated in billionths of an inch, and the periods in parts of the billionth of a second:—

	Length.	Period.
Wave producing the line C,	25,852,720,	one 453rd.
Wave producing the line F,	19,149,930,	one 612th.
Wave producing a line near G,	17,098,700,	one 685th.
Wave producing a line near H,	16,157,500,	one 725th.

It being extremely improbable that there are four distinct sorts of hydrogenous ultimates, capable of either generating or absorbing those four sorts of vibrations, the most simple, if not the only, explanation of the phenomenon appears to be, that each ultimate consists of four diverse sorts of atoms, to which those four different vibrations are to be attributed.

It would be difficult to imagine any other mode of their generating such vibrations in the ethereal particles, than by their vibrating themselves either at precisely the same rates or at some octave of those rates. Nor could there be easily assigned any other reason for their absorbing or taking up the already existing vibrations of the ethereal particles of those four particular periods, than by their tendency to assume those rates, or some octave of them, and take up the motive energy from the ether to enable them to perform their movements. But in order to their vibrating at all, it is needful to suppose these atoms to be not in absolute mutual contact, but to be kept apart by a layer of compressed ether, the elasticity of which exactly balances their mutual attraction. Of the greatness of this attraction we have two proofs—the one the degree of compression which the ether undergoes under its influence; the second, the great amount of applied motive energy required to set the atoms a vibrating. In this particular these atoms differ from the hydrogenous ultimates themselves, of which they are the constituent parts. For the ultimates can be made to vibrate by a very small amount of applied force; whereas their constituent atoms require the application of an intense force to make them vibrate; while, under ordinary circumstances, they vibrate each kind only at one particular rate.

The foregoing views are strengthened by certain other phenomena which these four spectral lines of hydrogen present. Of the four, that producing the line F is by much the most conspicuous. Those near G and H are comparatively feeble, and require a higher temperature for their development. When strong pressure is applied, it is the line F that first manifests a decided increase in breadth; and it is by the gradual widening of the four lines, but especially of F, that the hydrogen spectrum ultimately becomes, under a very strong pressure, continuous like that from an incandescent body. This phenomenon, which would be nearly inexplicable on the supposition that the ultimates of hydrogen are simple homogeneous masses, receives an easy explanation on the assumption of their consisting of more minute atoms. The greater brightness of the line F would be accounted for by supposing the atoms, whose vibrations generate this wave, to be more numerous than any of the others. Those which generate the wave C would be next in number; then those which generate the wave near G; lastly, those which generate the wave near H. The rapidity with which the two last sets of atoms vibrate would explain their requiring a greater amount of applied energy to set them in motion. The effects of very high pressure would be explained by supposing the pressure to drive the atoms constituting the ultimate into greater mutual proximity than that in which they subsist under ordinary atmospheric pressure. The widening of the lines would result from an adventitious inertia of position developed in the atoms by their being thus forced together—those at the surface of the ultimate being more easily movable, and vibrating at slower rates, and with greater amplitudes, than those in its interior, exactly as in the case of a solid incandescent body, from which the ultimate of hydrogen would differ only in this respect, that, when freed to a certain extent from extraneous pressure, its constituent atoms are at liberty to vibrate in their own peculiar times, without being embarrassed in their movements by each other. The relief from pressure narrows the lines; so that, when the gas becomes much attenuated, the lines acquire an exceeding degree of fineness. The atoms then vibrate more precisely at their own definite rates, unaffected by the proximity of their neighbours. The pre-eminence of hydrogen over other bodies, in its power of compressing the ether and retarding the progress of the wave-motion, in proportion to its specific gravity, would, according to this hypothesis, be due to the higher proportion which the atomic force bears to the gravitating force in hydrogen than in other substances having heavier ultimates.

Thus the whole of the phenomena presented by hydrogen, in its relations to the luminiferous ether, receive an easy explanation from the supposition that its ultimates are not simple and homogeneous masses, but compound bodies consisting of numerous atoms of four diverse kinds—each sort having a different intrinsic inertia of its own, in virtue of which it has a tendency to vibrate at a particular rate. This hypothesis explains not only the four bright lines and the appearances they exhibit, but also the remarkable property of hydrogen in exerting on the ether a compressing power far exceeding in proportion what is due to its specific gravity.

Having thus established the probability of the compound nature of the ultimate of hydrogen—the lightest of all known ultimates, it appears unnecessary to enter minutely into the evidence tending to prove the same fact with respect to the other chemical ultimates. There is, however, one piece of evidence which seems to deserve special notice as bearing on the general question of the compound nature of the ultimates of the chemical elements. It is the fact that, in several instances where these originate waves of several different definite lengths, one or more of those definite waves are common to two or more diverse chemical ultimates. Thus, M. Ångström notes no less than eight waves in different parts of the spectrum as being common to iron and titanium, three common to iron and calcium, three to iron and nickel, 1 to iron and sodium, one to iron and magnesium, one to iron and manganese, one to iron and barium, one to cobalt and titanium, one to titanium and calcium, and one to calcium and manganese. Several other instances of similar coincidences are noticed in a paper by the Rev. T. R. Robinson, "On Spectra of Electric Lights" (Phil. Trans., 1862, p. 939). In particular he notes that the line C, which is one of those produced by hydrogen, is also common to six metals, whatever be the nature of the gas through which the spark between the electrodes passes. He specifies another line in the yellow region, which is, under similar circumstances, common to five metals, and a third in the blue also common to five metals. Mr. Huggins in his tables likewise notes a line near D as being common to arsenic and zinc, the longer D itself as being common to sodium and lead, the shorter D to sodium and barium. He has also found a line nearly midway between D and E common to tellurium and nitrogen, one a little beyond E common to oxygen and arsenic, and one a little beyond F common to nitrogen and chromium.

Of these phenomena, as well as of the multiplicity of lines shown by some of the metals, the supposition that the chemical elements are compounded of more minute atoms supplies a simple explanation. As the lines are supposed to be produced by the vibrations, not of the ultimates themselves, but of their constituent atoms, and to depend on the intrinsic inertia of these latter, when the same line is found to be common to two or more diverse metals or other elements, the most obvious inference is that the ultimates of those metals or other elements contain the same sort of atoms in combination with others of diverse kinds. Thus the same species of atom which, in the ultimate of hydrogen, produces the line C, occurs also in the ultimates of six metals, associated with a variety of other atoms having different periods of vibration. The circumstance also noted by the observers above named, that the same line will appear with diverse degrees of relative brightness in the spectra of the different elements in which it occurs, is also easily explained by supposing that the atom, whose vibrations originate that line, occurs in greater or smaller proportion relatively to the others with which it is associated in the ultimates of those different elements. There are thus none of the phenomena presented by the spectrum which cannot be explained by this hypothesis, while all the phenomena concur to give it support.

There is one peculiar case that demands separate notice. It is that of the rare metals erbium and didymium, the spectra from which, when they are rendered incandescent in the solid state, exhibit bright lines similar to those presented by the spectra from incandescent vapours and gases. The only feasible explanation of this phenomenon appears to be that a portion of the metals is thrown into the state of a very dense vapour, which clings to the solid, and that it is in this vapour that the bright lines are generated.* This phenomenon in no degree militates against the supposition of the compound nature of the chemical elements—rather the reverse.

Marvellous as are the facts and phenomena which have been here brought under review, our wonder is perhaps still more highly excited, when we contemplate the adaptation of the eye and the optic nerve to receive sensible impressions from the minute motions of the ethereal particles. For it must be borne in mind, that it is not the wave-motion (which merely serves to carry forward the transverse movement from point to point), but the inconceivably minute transverse motions performed by the ethereal particles, which are themselves really effective in producing vision, heat, actinism, and fluorescence. The extreme smallness of those movements altogether baffles our powers of conception; yet they manifest to us their existence in a very palpable manner. In the case of vision the particles of the optic nerve must vibrate synchronously with those of the ether, and must perform the same minute movements. This very minuteness, however, explains how it is that luminous waves, coming from a large field of view, can find simultaneous entrance into so small a circle as the pupil of the eye, and how the optic nerve can be simultaneously impressed by so great a multitude and diversity of vibrations.

There is a phenomenon, however, which is perhaps still more curious, and of which the author of this paper can speak from personal experience. It is that of the optic nerve being excited into action by purely internal causes,

* The probability of this explanation will be appreciated by those who remember the experiments of M. Waidelé on the adhesion of gases and vapours to smooth surfaces, by which he so successfully explained the curious images obtained in the dark by M. Moser.

in the absence of all external stimulus from the ethereal waves. This phenomenon presents two varieties. In the one, when the eyes are opened after sleep in a perfectly dark chamber, there is perceived a general brightness, and occasionally spectral images of various kinds are formed in this apparently phosphorescent haze. In the other variety, the eye is itself phosphorescent, and near objects are seen by the light thus strangely generated within the organ. The author has had numerous opportunities of observing the former phenomenon, and several of noticing the latter, which in like manner manifests itself when the eyes are first opened after sleep. White or light-coloured objects are then clearly perceived as if illuminated by Canton's phosphorus, only brighter. The illumination resembles that thrown upon an object by a lens or bull's-eye, exhibiting a bright central disc, from which it gradually fades away. On closing the eyes for a short time and re-opening them, the brightness is found to have vanished, and to be replaced by absolute darkness, both in the first and second varieties of the phenomenon.

It appears not improbable that the eyes of some nocturnal animals may have this same phosphorescent property in a higher degree, and that they may be thus enabled to find and follow their prey by means of light proceeding from their own eyes. No stronger proof than this could be adduced to confirm the conclusion already established by so many other phenomena, that light consists of the vibrations of an ethereal medium. For we have here similar vibrations excited in the eye itself from an internal cause, and these, reacting on the external ether, excite synchronous vibrations which are propagated in waves, first from the eye to the outward object, and thence back from the object to the eye. The phosphorescence of the eye in man is believed to be very rare; but the author has been assured that other individuals besides himself have observed the same phenomenon.

III. SOME FURTHER EXPERIMENTS ON PSYCHIC FORCE.

By William Crookes, F.R.S, &c.

"I am attacked by two very opposite sects,—the scientists and the know-nothings. Both laugh at me—calling me 'the frogs' dancing master.' Yet I know that I have discovered one of the greatest forces in nature."—Galvani.

IT was my intention to have allowed a longer time to elapse before again writing on the subject of "Psychic Force" in this journal. My reason for this resolve was not so much owing to want of new matter and fresh results,—on the contrary, I have much that is new in the way of experimental evidence in support of my previous conclusions,—but I felt some reluctance to impose on the readers of the "Quarterly Journal of Science" a subject which they might view with little favour. When the editor of a scientific journal is also an experimental investigator, or a student of any special branch of knowledge, there is a natural tendency on his part to unduly exalt the importance of that which is occupying his thoughts at the time; and thus the journal which he conducts is in danger of losing breadth of basis, of becoming the advocate of certain opinions, or of being coloured by special modes of thought.

The manner in which the experimental investigation described in the last "Quarterly Journal" has been received, removes any doubt I might entertain on this score. The very numerous communications which have been addressed to the office of this journal show that another paper on the same subject will not be distasteful to a large number of those who did me the honour to read my former article; whilst it appears to be generally assumed that I should take an early opportunity to reply to some of the criticisms provoked by the remarkable character of the experimental results which I described.

Many of the objections made to my former experiments are answered by the series about to be related. Most of the criticisms to which I have been subjected have been perfectly fair and courteous, and these I shall endeavour to meet in the fullest possible manner. Some critics, however, have fallen into the error of regarding me as an advocate for certain *opinions*, which they choose to ascribe to me, though in truth my single purpose has been to state fairly and to offer no opinion. Having evolved men of straw from their own imagination, they proceed vigorously to slay them, under the impression that they are annihilating me. Others, —and I am glad to say they are very few,—have gone so far as to question my veracity:—"Mr. Crookes must get better witnesses before he can be believed!" Accustomed as I am to have my word believed without witnesses, this is an argument which I cannot condescend to answer. All who know me and read my articles will, I hope, take it for granted that the *facts* I lay before them are correct, and that the experiments were honestly performed, with the single object of eliciting *the truth.*

It is edifying to compare some of the present criticisms with those that were written twelve months ago. When I first stated in this journal that I was about to investigate the phenomena of so-called spiritualism, the announcement called forth universal expressions of approval. One said that my "statements deserved respectful consideration;" another, expressed "profound satisfaction that the subject was about to be investigated by a man so thoroughly qualified as," &c.; a third was "gratified to learn that the matter is now receiving the attention of cool and clear-headed men of recognised position in science;" a fourth asserted that "no one could doubt Mr. Crookes's ability to conduct the investigation with rigid philosophical impartiality;" and a fifth was good enough to tell its readers that "if men like Mr. Crookes grapple with the subject, taking nothing for granted until it is proved, we shall soon know how much to believe."

These remarks, however, were written too hastily. It was taken for granted by the writers that the results of my experiments would be in accordance with their preconceptions. What they really desired was not *the truth,* but an additional witness in favour of their own foregone conclusion. When they found that the facts which that investigation established could not be made to fit those opinions, why,—"so much the worse for the facts." They try to creep out of their own confident recommendations of the enquiry by declaring that "Mr. Home is a clever conjuror, who has duped us all." "Mr. Crookes might, with equal propriety, examine the performances of an Indian juggler." "Mr. Crookes must get better witnesses before he can be believed." "The thing is too absurd to be treated seriously." "It is impossible, and therefore can't be."* "The observers have all been biologised (!) and fancy they saw things occur which really never took place," &c., &c.

* The quotation occurs to me—"I never said it was possible, I only said it was true."

These remarks imply a curious oblivion of the very functions which the scientific enquirer has to fulfil. I am scarcely surprised when the objectors say that I have been deceived merely because they are unconvinced without personal investigation, since the same unscientific course of *à priori* argument has been opposed to all great discoveries. When I am told that what I describe cannot be explained in accordance with preconceived ideas of the laws of nature, the objector really begs the very question at issue and resorts to a mode of reasoning which brings science to a standstill. The argument runs in a vicious circle: we must not assert a fact till we know that it is in accordance with the laws of nature, while our only knowledge of the laws of nature must be based on an extensive observation of facts. If a new fact seems to oppose what is called a law of nature, it does not prove the asserted fact to be false, but only that we have not yet ascertained all the laws of nature, or not learned them correctly.

In his opening address before the British Association at Edinburgh this year, Sir William Thomson said, "Science is bound by the everlasting law of honour to face fearlessly every problem which can fairly be presented to it." My object in thus placing on record the results of a very remarkable series of experiments is to present such a problem, which, according to Sir W. Thomson, "Science is bound by the everlasting law of honour to face fearlessly." It will not do merely to deny its existence, or try to sneer it down. Remember, I hazard no hypothesis or theory whatever; I merely vouch for certain facts, my only object being—the *truth*. Doubt, but do not deny; point out, by the severest criticism, what are considered fallacies in my experimental tests, and suggest more conclusive trials; but do not let us hastily call our senses lying witnesses merely because they testify against preconceptions. I say to my critics, Try the experiments; investigate with care and patience as I have done. If, having examined, you discover imposture or delusion, proclaim it and say how it was done. But, if you find it be a fact, avow it fearlessly, as "by the everlasting law of honour" you are bound to do.

I may at once answer one objection which has been made in several quarters, viz., that my results would carry more weight had they been tried a greater number of times, and with other persons besides Mr. Home. The fact is, I have been working at the subject for two years, and have found nine or ten different persons who possess psychic power in more or less degree; but its development in Mr. D. D. Home is so powerful, that, having satisfied myself by careful experiments that the phenomena observed were genuine, I have, merely as a matter of convenience, carried on my experiments with him, in preference to working with others in whom the power existed in a less striking degree. Most of the experiments I am about to describe, however, have been tried with another person other than Mr. Home, and in his absence.

Before proceeding to relate my new experiments, I desire to say a few words respecting those already described. The objection has been raised that announcements of such magnitude should not be made on the strength of one or two experiments hastily performed. I reply that the conclusions were not arrived at hastily, nor on the results of two or three experiments only. In my former paper (" Quarterly Journal of Science," page 340), I remarked:—" Not until I had witnessed these facts some half-dozen times, and scrutinised them with all the critical acumen I possess, did I become convinced of their objective reality." Before fitting up special apparatus for these experiments, I had seen, on five separate occasions, objects, varying in weight from 25 to 100 lbs., temporarily influenced in such a manner, that I, and others present, could with difficulty lift them from the floor. Wishing to ascertain whether this was a physical fact, or merely due to a variation in the power of our own strength under the influence of imagination, I tested with a weighing machine the phenomenon on two subsequent occasions when I had an opportunity of meeting Mr. Home at the house of a friend. On the first occasion, the increase of weight was from 8 lbs. normally, to 36 lbs., 48 lbs., and 46 lbs. in three successive experiments tried under strict scrutiny. On the second occasion, tried about a fortnight after, in the presence of other observers, I found the increase of weight to be from 8 lbs. to 23 lbs., 43 lbs., and 27 lbs., in three successive trials, varying the conditions. As I had the entire management of the above-mentioned experimental trials, employed an instrument of great accuracy, and took every care to exclude the possibility of the results being influenced by trickery, I was not unprepared for a satisfactory result when the fact was properly tested in my own laboratory. The meeting on the occasion formerly described was, therefore, for the purpose of confirming my previous observations by the application of crucial tests, with carefully arranged apparatus of a still more delicate nature.

That this is a legitimate subject for scientific inquiry scarcely needs assertion. Faraday himself did not consider it beneath his dignity to examine similar phenomena; and, in a letter to Sir Emerson Tennent, written in 1861 on the occasion of a proposed experimental inquiry into the phenomena occurring in Mr. Home's presence, he wrote:—"Is he (Mr. Home) willing to investigate as a philosopher, and, as such, to have no concealments, no darkness, to be open in communication, and to aid inquiry all that he can? . . . . Does he consider the effects natural or supernatural? If they be the glimpses of natural action not yet reduced to law, ought it not to be the duty of every-one who has the least influence in such actions personally to develope them, and to aid others in their development, by the utmost openness and assistance, and by the application of every critical method, either mental or experimental, which the mind of man can devise?"

If circumstances had not prevented Faraday from meeting Mr. Home, I have no doubt he would have witnessed phenomena similar to those I am about to describe, and he could not have failed to see that they offered "glimpses of natural action not yet reduced to law."

I have already alluded to the publication of the ill-success encountered by the members of the St. Petersburg Committee. Had the results been satisfactory, it must be fairly assumed that the members would have been equally ready to publish a report of their success.

I am informed by my friend Professor Boutlerow,* that during the last winter, he tried almost the same experiments as those here detailed, and with still more striking results. The normal tension on the dynamometer being 100 lbs., it was increased to about 150 lbs., Mr. Home's hands being placed in contact with the apparatus in such a manner that any exertion of power on his part would diminish, instead of increase, the tension.

In 1854, Count Agénor de Gasparin published a book,† giving full details of a large series of physical experiments which he had tried with some private friends in whom this force was found to be strongly developed. His experiments were very numerous and were carried on under the strictest test conditions. The fact of motion of heavy bodies without mechanical contact was demonstrated over and over

* Professor of Chemistry at the University of St. Petersburg; author of a work on Chemistry, entitled "Lehrbuch der Organischen Chemie;" Leipzig, 1868.

† Science *versus* Spiritualism. Paris, 1854. New York, 1857.

again. Careful experiments were made to measure the force both of gravitation and of levitation thus communicated to the substances under trial, and an ingenious plan was adopted by which Count de Gasparin was enabled to obtain a rough numerical estimate of the power of the psychic force in each individual. The author finally arrived at the conclusion that all these phenomena are to be accounted for by the action of natural causes, and do not require the supposition of miracles nor the intervention of spirits or diabolical influences. He considers it as a fact fully established by his experiments, that the will, in certain states of the organism, can act at a distance on inert matter, and most of his work is devoted to ascertaining the laws and conditions under which this action manifests itself.

In 1855, M. Thury, a Professor at the Academy of Geneva, published a work,* in which he passed in review Count de Gasparin's experiments, and entered into full details of researches he had been simultaneously carrying on. Here, also, the trials were made with private friends, and were conducted with all the care which a scientific man could bring to bear on the subject. Space will not allow me to quote the valuable numerical results obtained by M. Thury, but from the following headings of some of his chapters, it will be seen that the enquiry was not conducted superficially:—Facts which Establish the Reality of the New Phenomenon; Mechanical Action rendered Impossible; Movements effected without Contact; The Causes; Conditions requisite for the Production and Action of the Force; Conditions for the Action with Respect to the Operators; The Will; Is a Plurality of Operators Necessary? Preliminary Requisites; Mental Condition of the Operators; Meteorological Conditions; Conditions with Respect to the Instruments Operated upon; Conditions relative to the Mode of Action of the Operators on the Instruments; Action of Substances interposed; Production and Transmission of the Force; Examination of the Assigned Causes; Fraud; Unconscious Muscular Action produced in a particular Nervous State; Electricity; Nervo-magnetism; M. de Gasparin's Theory of a Special Fluid; General Question as to the Action of Mind on Matter. 1st Proposition; In the ordinary conditions of the body the will only acts directly within the sphere of the organism. 2nd Proposition: Within the organism itself there are a series of mediate acts. 3rd Proposition: The substance on which the mind

* Geneva; Librairie Allemande de J. Kessmann, 1855.

acts directly—the *psychode*—is only susceptible of very simple modification under the influence of the mind; Explanations which are based on the Intervention of Spirits. M. Thury refutes all these explanations, and considers the effects due to a peculiar substance, fluid, or agent, pervading, in a manner similar to the luminiferous ether of the scientist, all matter, nervous, organic, or inorganic—which he terms *psychode*. He enters into full discussion as to the properties of this state or form of matter, and proposes the term *ectenic force* (ἐκτένια, extension), for the power exerted when the mind acts at a distance through the influence of the psychode.*

There is likewise another case on record in which similar test experiments were tried, with like results, by a thoroughly competent observer. The late Dr. Robert Hare, in one of his works,† gives an engraving of an apparatus very similar to my own, by which the young man with whom he was experimenting was prevented from having any other communication with the apparatus except through water; yet, under these circumstances, the spring balance indicated the exertion of a force equal to 18 lbs. The details of this experiment were communicated by Dr. Hare to the American Association for the Advancement of Science, at the meeting in August, 1855.

The references I now give afford an answer to the statement that these results must be verified by others. They have been verified over and over again. Indeed, my own experiments may be regarded merely as verifications of results already obtained and published by eminent scientific men in this and other countries.‡

But I was not content with this. I felt that having the opportunity of showing these phenomena to others, I might

* Professor Thury's ectenic and my psychic force are evidently equivalent terms. Had I seen his work three months ago I should have adopted his term. The suggestion of a similar hypothetical nervous fluid has now reached us from another and totally different source, expounded with distinct views, and couched in the language of one of the most important professions—I allude to the theory of a nervous atmosphere advanced by Dr. Benjamin W. Richardson, M.D., F.R.S., in the "Medical Times," No. 1088, May 6, 1871.

† "Experimental Investigation;" By Robert Hare, M.D., Emeritus Professor of Chemistry in the University of Pennsylvania, &c. New York: Partridge and Britton, 1858.

‡ The Report of the Dialectical Society on Spiritualism will appear in a few days, and it will be seen that the Investigation Committee, though commencing their experiments with the entire conviction that they should expose an imposture, have ended by affirming that they are convinced of the existence of a force emanating from the human organisation, by which motion may be imparted to heavy substances, and audible sounds made on solid bodies without muscular contact; they also state that this force is often directed by some intelligence.

at a future time be blamed did I not, once for all, take the very best means of bringing them before the notice of the scientific world. Accordingly I forwarded an account of my experiments to the Royal Society on June 15, 1871, and addressed myself to the two secretaries of the Royal Society, Professor Sharpey and Professor Stokes, inviting them to my house to meet Mr. Home, at the same time asking them to be prepared for negative results, and to come a second, or, if necessary, a third time, before forming a judgment.

Dr. Sharpey politely declined the invitation.

Professor Stokes replied that he thought there was a fallacy in my apparatus, and concluded by saying—

"The facts you mentioned in the paper were certainly at first sight very strange, but still possible modes of explanation occurred to me which were not precluded by what I read in the paper. If I have time when I go to London I will endeavour to call at your house. I don't want to meet anyone; my object being to scrutinise the apparatus, not to witness the effects."

To this I replied on June 20th; the following extracts are taken from my answer:—

"I am now fitting up apparatus in which contact is made through water only, in such a way that transmission of mechanical movement to the board is impossible; and I am also arranging an experiment in which Mr. Home will not touch the apparatus at all. This will only work when the power is very strong; but last night I tried an experiment of this kind, and obtained a considerable increase of tension on the spring balance when Mr. Home's hands were three inches off. With him the power is so great that I can work with large and crude materials, and measure the force in pounds. But I propose to make a delicate apparatus, with a mirror and reflected ray of light, to show fractions of grains. Then I hope to find this force is not confined to a few, but is, like the magnetic state, universal. The subject shall have a 'most scrupulously searching physical scrutiny,' and whatever results I obtain shall be published. I consider it my duty to send first to the Royal Society, for by so doing I deliberately stake my reputation on the truth of what I send. But will the Society (or the Committee*) accept my facts as facts, or will they require vouchers for my integrity? If my statements of fact are taken as correct, and only my interpretation or arrangements of apparatus objected to, then it would seem to be right to give me an opportunity of answering these objections before finally deciding. The other supposition—that my facts are incorrect—I cannot admit the discussion of till I am definitely assured that such is entertained.

"Mr. Home is coming here on Wednesday and Friday evenings; if you can come on either or both occasions at 8 p.m., I shall be glad to see you, or if you only wish to scrutinise the apparatus, I will be here at any time you like to name."

On the 28th of June another paper was sent to the Royal Society. Two days after, Professor Stokes wrote a letter, from which I quote:—

"As I was otherwise engaged so as not to be able conveniently to go to your house, I may as well mention the possible sources of error which occurred to me with reference to your first apparatus. I don't suppose they all exist;

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* Alluding to a rumoured rejection of my paper by the Committee of the Royal Society.

but it is evidently, as you yourself would freely admit, for the *assertor* of a new force to remove all sources of reasonable objection.

"The breadth of the foot of the board was, I think, $1\frac{1}{2}$ or 2 inches, and the bell placed on it was, perhaps, 2 or 3 inches broad. (I can't carry the exact figures in my head.) Join the left edge* of the top of the bell, *a*, with the right hand edge, *b*, of the base of the bell, and let *e f* be the joining line. Then we may suppose the fingers to have pressed in any direction short of the limiting line *e f*. Also as the board was rigid, the fulcrum for aught we know may have been at *c*. From *c* let fall a perpendicular *c m* on the line, *e f*. Then the pressure of the finger may have acted at the distance, *c m*, from the fulcrum. Also, as the base lay flat on the table and both were rigid, for aught we know an infinitesimal, and therefore imperceptible, tilt communicated to the table at the time of trying the experiment may have shifted the fulcrum from the edge *d* to the edge *c*, so that the weight of the hand may have acted by an arm longer than before by *c d*, which would have contributed to the result.

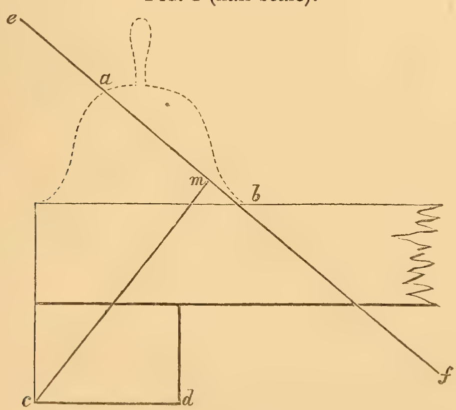
"In your second paper the uncertainty as to the broad bearing is removed. But when the hand was dipped into the water the pressure on the base of the glass vessel (after a little time if the connecting hole be narrow) is increased by the weight of the water displaced, and that would of course depress the balance.

"I don't think much of mere tremors, for it would require very elaborate appliances to prove that they were not due to a passing train or omnibus or to a tremor in the body of one of the company. . . . .
What do you wish to be done with the papers?"

To this I replied as follows, on July 1st:—

"In your letter of the 30th ult., just received, you are quite right in saying that I would freely admit that 'the *assertor* of a new force should remove all sources of reasonable objection.' In your previous letter of the 19th June, you write with equal fairness, that 'your opinion is that you (the R. S.) ought not to refuse to admit evidence of the existence of a hitherto unsuspected

FIG. 1 (half-scale).



force; but that before printing anything on such a subject, you ought to require a most scrupulously searching physical scrutiny of the evidence adduced in favour of the existence of such a force.'

* The diagram referred to here is shown, drawn to scale, in my answer further on. The experiment under discussion is the one figured and described in the last number of the "Quarterly Journal of Science," page 345.

"You have now been good enough to explain to me in detail what the fallacy is which you think exists in my first experiments, and what you consider to be the possible sources of error in my subsequent trials.

"On re-drawing the diagram you give in your letter, Fig. 1, to the full size, supplying the deficient data, viz., the position of the shoulder, *a*, and the point, *b*, your line *c m* appears to be about 2·9 inches long; and, as you assume that the fulcrum shall be at *c*, the lever becomes one of the third order, the two forces acting respectively at $p = 2{\cdot}9$ inches, and at $q = 36$ inches from *c*. What power, P, must be exerted at *p* to overcome a resistance or weight, Q, of 6 lbs. at the end of the lever, *q*?

$$Pp = Qq.$$

$$\text{Hence, } P \times 2{\cdot}9 = Q \times 36.$$

$$\therefore P = 74{\cdot}5 \text{ lbs.}$$

Therefore, it would have required a force of 74·5 lbs. to have been exerted by Mr. Home to have produced the results, even if all your suppositions are granted; and, considering that he was sitting in a low easy chair, and four pairs of sharp, suspicious eyes were watching to see that he exerted no force at all, but kept the tips of his fingers lightly on the instrument, it is sufficiently evident that an exertion of this pressure was impossible. A few pounds vertical pressure was all he could have effected.

"Again, you are not justified in assuming that the fulcrum was at *c*. Granting that 'an infinitesimal and therefore imperceptible tilt' might, at the very first movement, have thrown it from *d* to *c*, it is evident that the movement would at once throw it forward again from *c* to *d*. To have failed to have done so, the tilt must have been so obvious as to have been detected at once.

"But, as I said in my last paper, I prefer to appeal to new experiments rather than argue about old ones, and hence my employment of the water for transmitting the force. The depth of water in the copper hemisphere was only 1½ inches, whilst the glass vessel was 9 inches in diameter.* I have just tried the experiment of immersing my hand to the very utmost in the copper vessel (Mr. Home only dipped in the tips of his fingers) and the rise of the level of the water is not sufficient to produce any movement whatever on the index of the balance, the friction of the apparatus being enough to absorb the ounce or two thus added to the weight. In my more delicate apparatus, this increase of hydrostatic pressure produces a decided movement of the spot of light, but this difficulty I shall overcome by placing the water vessel over the fulcrum, or on the short side of it.

"You say 'you don't think much of mere tremors,' as if in the other experiments described in my second paper the movements of the apparatus were only of this kind. This is not the case; the quivering of the apparatus always took place before the index moved, and the upward and downward motion of the board and index was of a very slow and deliberate character, occupying several seconds for each rise and fall; a tremor produced by passing vehicles is a very different thing from a steady vertical pull of from 4 to 8 lbs., lasting for several seconds.

"You say the session is now over, and ask what I wish to be done with the papers.

"Three years ago (June 27th, 1868), I sent a paper to the Society, 'On the Measurement of the Luminous Intensity of Light,' just after the session closed. It was not read till December 17th. My *wish* would be for a similar course to be adopted in the present instance, although I am scarcely sanguine enough to expect that so much notice will be taken of these communications. So many scientific men are now examining into these strange phenomena (including many Fellows of the Society), that it cannot be many years before the subject will be brought before the scientific world in a way that will enforce attention. I confess that, in sending in these papers to the Society, I have been actuated more by the desire of being the first scientific experimenter

* For a description of this apparatus, see p. 484.

who has ventured to take such a course, than by any particular desire that they should meet with immediate attention. I owe to the Society the first intimation of important scientific results, and these I shall continue to send, '*pour prendre date*,' if for no other reason."

"The Spectator" of July 22nd contained an editorial note, in which it is asserted that my paper was declined by the committee:—

"The Royal Society, they say, was quite open to communications advocating the existence of a force in nature as yet unknown, if such communications contained scientific evidence adequate to establish its probability; but that, looking to the inherent improbability of the case as stated by Mr. Crookes, and the *entire want of scientific precision* in the evidence adduced by him, the paper was not regarded as one deserving the attention of the Royal Society."

This paragraph not only states that my papers were declined, but proceeds to state the grounds of their rejection. The fact is, that a quorum of the committee of papers not having been present, the question was deferred to the next session in November, and on inquiry at Burlington House, I am informed by the Assistant-Secretary of the Royal Society that my papers, with others, are still awaiting the decision of the committee. Consequently the statement of a rejection was not only premature, but purely imaginary.

It appears, however, that there were some grounds for this statement, for in "The Spectator" of July 29th, 1871, the editor replies as follows:—

"Our note was not founded on any mere rumour. The words we used contained an exact copy of the words conveyed to us as used, not, as we inadvertently stated by the committee, but by one of the secretaries, Professor Stokes, who in the absence of a quorum, exercised *pro tempore* the usual discretionary authority in regard to papers offered."

I am unable to explain how it is that Professor Stokes's statements to me and to the editor of "The Spectator" bear so different an interpretation, or why a weekly newspaper was chosen for first conveying to me a decision of the committee of papers of the Royal Society.

At the urgent request of gentlemen on the committee of section A, I communicated a paper consisting of about sixteen closely-written pages to the British Association, in which I recounted some of the experiments described in the present paper. Section A referred the paper to a committee to decide whether it should be read. Professor Stokes afterwards handed to me the following document:—

"*Report on Mr. Crookes's Paper.*

"August 7, 1871.

"The paper having been placed in my hands about ten o'clock, and a decision wanted in writing by a quarter to eleven, I have been obliged to be hasty.

"The subject *seems to be investigated in a philosophical spirit*, and I do not see the explanation of the result of the first class of experiments, while at the same time I am not prepared to give in my adhesion without a thorough sifting by more individuals than one. I don't see much use discussing the thing in the sections, crowded as we already are: but if a small number of persons in whom the public would feel confidence choose to volunteer to act as members of a committee for investigating the subject, I don't see any objection to appointing such committee. I have heard too much of the tricks of Spiritualists to make me willing to give my time to such a committee myself.

"G. G. STOKES."

Whilst I cannot but regret that a physicist of such eminence as Professor Stokes should "be hasty," in deciding on the merits of a paper which it is physically impossible he could have even once read through, I am glad to find that he no longer continues to speak of the "entire want of scientific precision in the evidence adduced" by me, but rather admits that "the subject seems to be investigated in a philosophical spirit."

In submitting these experiments, it will not seem strange that I should consider them final until rebutted by arguments also drawn from facts, and that I should seek to know on what grounds contra-statements are founded. Professor Allen Thomson, at the recent meeting of the British Association, remarked that no course of inquiry into the matter before us "can deserve the name of study or investigation." And why not? On the other hand, Professor Challis, of Cambridge, writes, "In short, the testimony has been so abundant and consentaneous, that either the facts must be admitted to be such as are reported, or the possibility of certifying facts by human testimony must be given up." It is certainly not too much to suppose that Dr. Thomson had some grounds for his statement; and, indeed, "I have," he owns, "been fully convinced of this (the fallacies of spiritualistic demonstration) by repeated examinations;" but where are the results of his investigations to be found? They must be very conclusive to warrant him in the use of such expressions as "a few men of acknowledged reputation in some departments of science have surrendered their judgments to these foolish dreams, *otherwise* appearing to be within the bounds of sanity." If Dr. Thomson's dogmatic denial arises from the mere strangeness of the facts I have set forth, what can he think of the address of the President for this year. Surely the conception of a nerve-force is no more difficult than that "of the inner mechanism of the atom;" and again, any investigation, be it worthy the name or not, bearing on a matter in which eminent men have avowed their belief, which takes a leading rank among the social questions of the day, and which numbers its adherents by millions, is surely as full of merit, and as instructive to all, as hypothetical inquiries into "interatomic atmospheres" and "gyrating interatomic atoms." Professor Huxley has observed, "If there is one thing clear about the progress of modern science, it is the tendency to reduce all scientific problems, except those that are purely mathematical, to problems in molecular physics—that is to say, to attractions, repulsions, motions, and co-ordination of the ultimate particles of matter! Yet these ultimate particles, molecules, or atoms, are creatures of the imagination, and as pure assumptions as the spirits of the spiritualist." But perhaps Dr. Allen Thomson's respect for mathematics is so great that he is blind to actuality. It does not speak well for modern scientific philosophy that, after the startling revelations of the spectroscope during the last decade, investigations should be scouted because they pertain to an ulterior state of things of which at present we have little idea. That I have furnished no dynamic equivalent of psychic force, or given no formulæ for the variable intensity of Mr. Home's power, is certainly no argument whatever against the existence of such a force. Men thought before the syllogism was invented, and, strange as it may seem to some minds, force existed before its demonstration in mathematical formulæ.

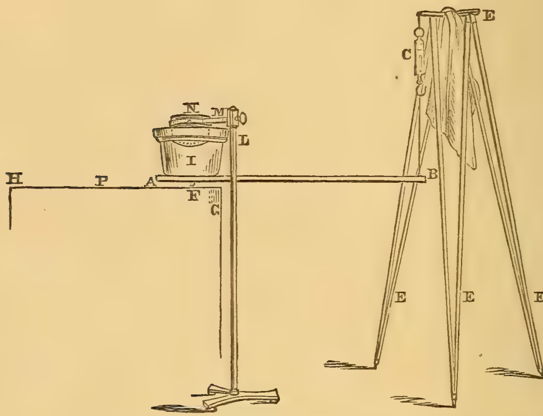
As an answer to Professor Balfour Stewart's rather bold conjecture, that Mr. Home possesses great electro-biological power (whatever that may mean), by which he influences those present, I point to the curves illustrating this paper; however susceptible the *persons* in the room might have been to that assumed influence, it will hardly be contended that Mr. Home biologised the recording instruments.

I will not occupy more time with personal matters, or with explanations forced from me in self-defence against uncourteous commentaries based on unjust misrepresentations; but I will proceed to describe the experiments, most of which, I may remark, might have been witnessed by Professor Stokes and Professor Sharpey, had they accepted the invitations I gave them.

On trying these experiments for the first time, I thought that actual contact between Mr. Home's hands and the suspended body whose weight was to be altered was essential to the exhibition of the force; but I found afterwards that this was not a necessary condition, and I therefore arranged my apparatus in the following manner:—

The accompanying cuts (Figs. 2, 3, 4) explain the arrangement. Fig. 2 is a general view, and Figs. 3 and 4 show the essential parts more in detail. The reference letters are the same in each illustration. A B is a mahogany board, 36 inches long by $9\frac{1}{2}$ inches wide, and 1 inch thick. It is suspended at the end, B, by a spring balance, C,

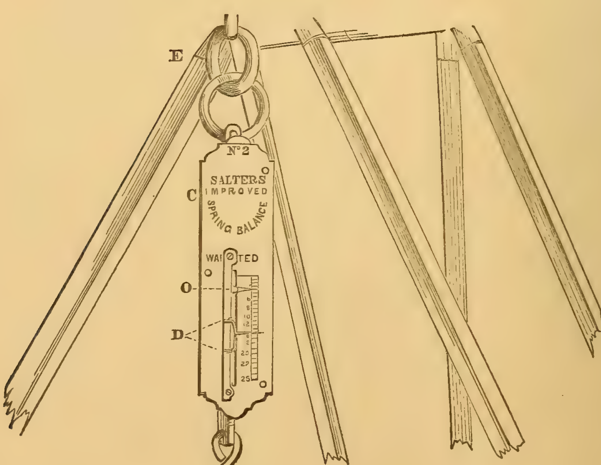
Fig. 2.



furnished with an automatic register, D. The balance is suspended from a very firm tripod support, E.

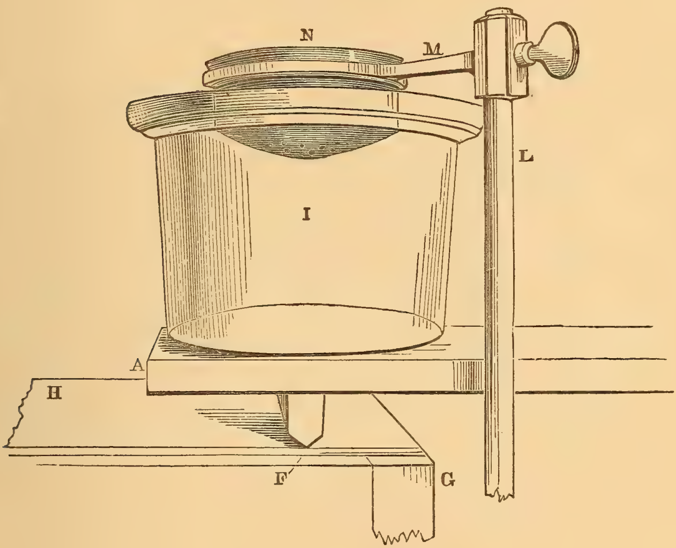
The following piece of apparatus is not shown in the figures. To the moving index, O, of the spring balance, a

Fig. 3.



fine steel point is soldered, projecting horizontally outwards. In front of the balance, and firmly fastened to it, is a grooved frame carrying a flat box similar to the dark box of a photographic camera. This box is made to travel by clock-work horizontally in front of the moving index, and it contains a sheet of plate-glass which has been smoked over a flame. The projecting steel point impresses a mark on this smoked surface. If the balance is at rest, and the clock set going, the result is a perfectly straight horizontal line. If the clock is stopped and weights are placed on the

FIG. 4.



end B of the board, the result is a vertical line, whose length depends on the weight applied. If, whilst the clock draws the plate along, the weight of the board (or the tension on the balance) varies, the result is a curved line, from which the tension in grains at any moment during the continuance of the experiments can be calculated.

The instrument was capable of registering a diminution of the force of gravitation as well as an increase; registrations of such a diminution were frequently obtained. To avoid complication, however, I will only here refer to results in which an increase of gravitation was experienced.

The end B of the board being supported by the spring balance, the end A is supported on a wooden strip, F, screwed across its lower side and cut to a knife edge (see Fig. 4). This fulcrum rests on a firm and heavy wooden stand, G H. On the board, exactly over* the fulcrum, is placed a large glass vessel filled with water, I. L is a massive iron stand, furnished with an arm and a ring, M N, in which rests a hemispherical copper vessel perforated with several holes at the bottom.

The iron stand is 2 inches from the board A B, and the arm and copper vessel, M N, are so adjusted that the latter dips into the water $1\frac{1}{2}$ inches, being $5\frac{1}{2}$ inches from the bottom of I, and 2 inches from its circumference. Shaking or striking the arm M, or the vessel N, produces no appreciable mechanical effect on the board, A B, capable of affecting the balance. Dipping the hand to the fullest extent into the water in N does not produce the least appreciable action on the balance.

As the mechanical transmission of power is by this means entirely cut off between the copper vessel and the board A B, the power of muscular control is thereby completely eliminated.

For convenience I will divide the experiments into groups 1, 2, 3, &c., and I have selected one special instance in each to describe in detail. Nothing, however, is mentioned which has not been repeated more than once, and in some cases verified, in Mr. Home's absence, with another person possessing similar powers.

There was always ample light in the room where the experiments were conducted (my own dining-room) to see all that took place.

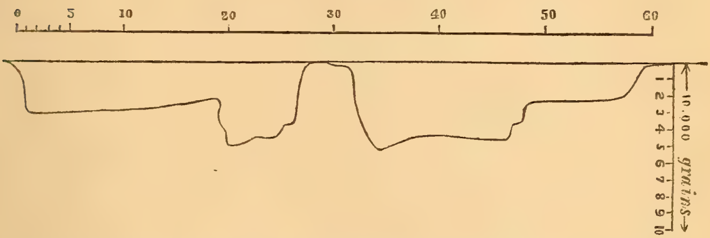
Experiment I.—The apparatus having been properly adjusted before Mr. Home entered the room, he was brought in, and asked to place his fingers in the water in the copper vessel, N. He stood up and dipped the tips of the fingers of his right hand in the water, his other hand and his feet being held. When he said he felt a power, force, or influence, proceeding from his hand, I set the clock going, and almost immediately the end B of the board was seen to descend slowly and remain down for about 10 seconds; it then descended a little further, and afterwards rose to its normal height. It then descended again, rose suddenly, gradually sunk for 17 seconds, and finally rose to its normal height, where it remained till the experiment was concluded. The lowest point marked on the glass was equivalent to a direct pull of about 5000 grains. The accompanying figure (5) is a copy of the curve traced on the glass.

* In my first experiments with this apparatus, referred to in Professor Stokes's letter and my answer (page 479), the glass vessel was not quite over the fulcrum, but was nearer B.

Experiment II.—Contact through water having proved to be as effectual as actual mechanical contact, I wished to see if the power or force could affect the weight, either through other portions of the apparatus or through the air. The glass vessel and iron stand, &c., were therefore removed,

FIG. 5.

Scale of Seconds.



The horizontal scale of seconds shows the time occupied in the movements, the experiment lasting one minute. The vertical scale shows the tension in grains exerted on the balance at any moment.

as an unnecessary complication, and Mr. Home's hands were placed on the stand of the apparatus at P (Fig. 2). A gentleman present put his hand on Mr. Home's hands, and his foot on both Mr. Home's feet, and I also watched him closely all the time. At the proper moment the clock was

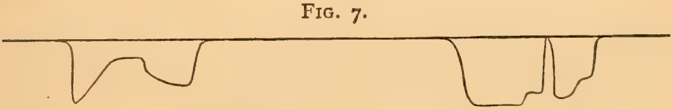
FIG. 6.

In this and the two following figures the scales, both vertical and horizontal, are the same as in Fig. 5.

again set going; the board descended and rose in an irregular manner, the result being a curved tracing on the glass, of which Fig. 6 is a copy.

Experiment III.—Mr. Home was now placed one foot from the board A B, on one side of it. His hands and feet

FIG. 7.

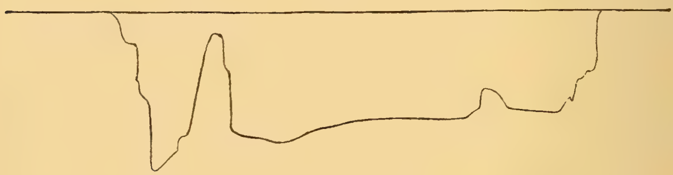


were firmly grasped by a bystander, and another tracing, of which Fig. 7 is a copy, was taken on the moving glass plate.

Experiment IV.—(Tried on an occasion when the power was stronger than on the previous occasions). Mr. Home was now placed 3 feet from the apparatus, his hands and feet being tightly held. The clock was set going when he gave the word, and the end B of the board soon descended, and again rose in an irregular manner, as shown in Fig. 8.

The following series of experiments were tried with more delicate apparatus, and with another person, a lady, Mr. Home being absent. As the lady is non-professional, I do

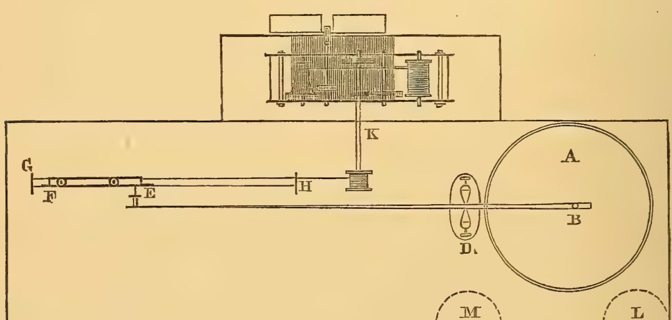
FIG. 8.



not mention her name. She has, however, consented to meet any scientific men whom I may introduce for purposes of investigation.

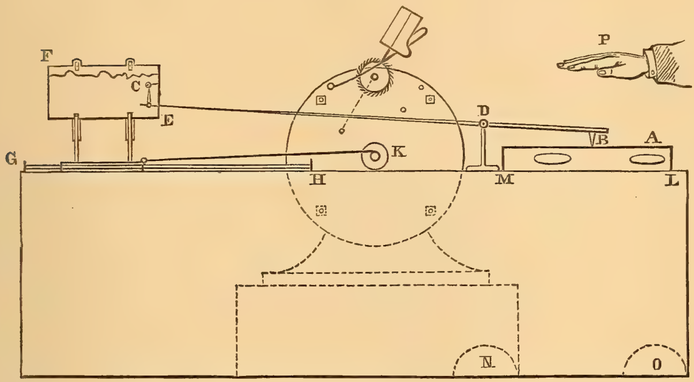
A piece of thin parchment, A, Figs. 9 and 10, is stretched tightly across a circular hoop of wood. B C is a light lever turning on D. At the end B is a vertical needle point touching the membrane A, and at C is another needle point, projecting

FIG. 9. (Plan.)



horizontally and touching a smoked glass plate, E F. This glass plate is drawn along in the direction H G by clock-work, K. The end B of the lever is weighted so that it shall quickly follow the movements of the centre of the disc, A. These movements are transmitted and recorded on the glass plate E F, by means of the lever and needle point C. Holes are cut in the side of the hoop to allow a free passage of air to the under side of the membrane. The apparatus was well tested beforehand by myself and others, to see that no shaking or jar on the table or support would interfere with the results: the line traced by the point C

FIG. 10. (Section.)



on the smoked glass was perfectly straight in spite of all our attempts to influence the lever by shaking the stand or stamping on the floor.

Experiment V.—Without having the object of the instrument explained to her, the lady was brought into the room and asked to place her fingers on the wooden stand at the points L M, Fig. 9. I then placed my hands over hers to enable me to detect any conscious or unconscious movement on her part. Presently percussive noises were heard on the parchment resembling the dropping of grains of sand on its surface. At each percussion a fragment of graphite which I had placed on the membrane was seen to be projected upwards about 1-50th of an inch, and the end C of the lever moved slightly up and down. Sometimes the sounds were as rapid as those from an induction-coil, whilst at others they were more than a second apart. Five or six tracings were taken, and in all cases a movement of the end C of the lever was seen to have occurred with each vibration of the membrane.

In some cases the lady's hands were not so near the membrane as L M, but were at N O, Fig. 10.

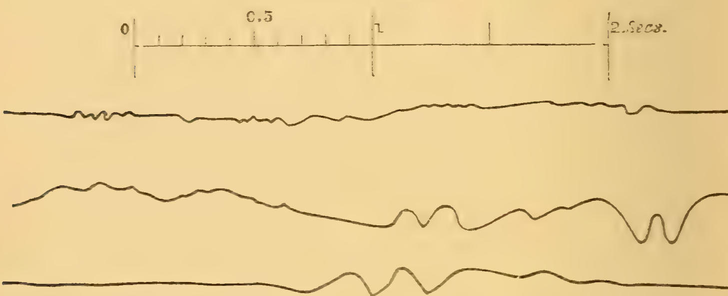
The accompanying Fig. 11 gives tracings taken from the plates used on these occasions.

Experiment VI.—Having met with these results in Mr. Home's absence, I was anxious to see what action would be produced on the instrument in his presence.

Accordingly I asked him to try, but without explaining the instrument to him.

FIG. 11.

Scale of Seconds.



I grasped Mr. Home's right arm above the wrist and held his hand over the membrane, about 10 inches from its surface, in the position shown at P, Fig. 10. His other hand was held by a friend. After remaining in this position for about half a minute, Mr. Home said he felt some influence passing. I then set the clock going, and we all saw the index, C, moving up and down. The movements were much slower than in the former case, and were almost entirely unaccompanied by the percussive vibrations then noticed.

Figs. 12 and 13 show the curves produced on the glass on two of these occasions.

Figs. 11, 12, 13 are magnified.

These experiments *confirm beyond doubt* the conclusions at which I arrived in my former paper, namely, the existence of a force associated, in some manner not yet explained, with the human organisation, by which force, increased weight is capable of being imparted to solid bodies without physical contact. In the case of Mr. Home, the development of this force varies enormously, not only from week to week, but from hour to hour; on some occasions the force is inappreciable by my tests for an hour or more, and then suddenly reappears in great strength. It is capable of acting at a distance from Mr. Home (not unfrequently as far as two or three feet), but is always strongest close to him.

Being firmly convinced that there could be no manifestation of one form of force without the corresponding expenditure of some other form of force, I for a long time searched in vain for evidence of any force or power being used up in the production of these results.

Now, however, having seen more of Mr. Home, I think I perceive what it is that this psychic force uses up for its development. In employing the terms *vital force*, or *nervous energy*, I am aware that I am employing words which convey very different significations to many investigators; but after witnessing the painful state of nervous and bodily prostration in which some of these experiments have left

FIG. 12.

Mr. Home—after seeing him lying in an almost fainting condition on the floor, pale and speechless—I could scarcely doubt that the evolution of psychic force is accompanied by a corresponding drain on vital force.

I have ventured to give this new force the name of *Psychic Force*, because of its manifest relationship to certain psychological conditions, and because I was most desirous to avoid the foregone conclusions implied in the title under

FIG. 13.

which it has hitherto been claimed as belonging to a province beyond the range of experiment and argument. But having found that it is within the province of purely scientific research, it is entitled to be known by a scientific name, and I do not think a more appropriate one could have been selected.

To witness exhibitions of this force it is not necessary to have access to known psychics. The force itself is probably possessed by all human beings, although the individuals endowed with an extraordinary amount of it are doubtless few. Within the last twelve months I have met in private families five or six persons possessing a sufficiently vigorous development to make me feel confident that similar results might be produced through their means to those here recorded, provided the experimentalist worked with more delicate apparatus, capable of indicating a fraction of a grain instead of recording pounds and ounces only.

As far as my other occupations will permit, I purpose to continue the experiments in various forms, and I will report from time to time their results. In the meanwhile I trust that others will be induced to pursue the investigation in its scientific form. It should, however, be understood that, equally with all other scientific experiments, these researches must be conducted in strict compliance with the conditions under which the force is developed. As it is an indispensable condition of experiments with frictional electricity that the atmosphere should be free from excess of moisture, and that no conducting medium should touch the instrument while the force is being generated, so certain conditions are found to be essential to the production and operation of the Psychic force, and unless these precautions are observed the experiments will fail. I am emphatic on this point, because unreasonable objections have sometimes been made to the Psychic Force that it is not developed under adverse conditions dictated by the experimentalist, who, nevertheless, would object to conditions being imposed upon himself in the exhibition of any of his own scientific results. But I may add, that the conditions required are very few, very reasonable, and in no way obstruct the most perfect observation and the application of the most rigid and accurate tests.

Just before going to press I have received from my friend Professor Morton an advance sheet of the "Journal of the Franklin Institute," containing some remarks on my last paper by Mr. Coleman Sellers, a leading scientific engineer of the United States. The essence of his cricitism is contained in the following quotation:—

"On page 341" (of the Quarterly Journal of Science) "we have given a mahogany board '36 inches long by 9½ inches wide, and 1 inch thick,' with 'at each end a strip of mahogany 1½ inches wide screwed on, forming feet.' This board was so placed as to rest with one end on the table, the other suspended by a spring balance, and, so suspended, it recorded a weight of 3 pounds; *i.e.*, a *mahogany* board of the above dimensions is shown to weigh 6 pounds—3 pounds on the balance and 3 pounds on the table. A mechanic used to handling wood wonders how this may be. He looks through his limited library and finds that scientific men tell him that such a board should weigh about $13\frac{1}{3}$ pounds. Did Mr. Crookes make this board himself? or did Mr. Home furnish it as one of his pieces of apparatus? . . . . It would have been more satisfactory if Mr. Crookes had stated, in regard to this board, who made it. . . . Let it be discovered that the 6 pound mahogany board was furnished by Mr. Home and the experiments will not be so convincing."

My experiments must indeed be convincing if so accomplished a mechanician as Mr. Coleman Sellers can find no worse fault with them than is expressed in the comments I have quoted. He writes in so matter-of-fact a manner, and deals so plausibly with dimensions and weights, that most persons would take it for granted that I really *had* committed the egregious blunder he points out.

Will it be believed, therefore, that my mahogany board does *weigh only* 6 *pounds?* Four separate balances in my own house tell me so, and my greengrocer confirms the fact.

It is easy to perceive into what errors a "mechanic" may fall when he relies for practical knowledge on his "limited library" instead of appealing to actual experiment.

I am sorry I cannot inform Mr. Sellers who made my mahogany board. It has been in my possession about sixteen years; it was originally cut off a length in a wood-yard; it became the stand of a spectrum camera, and as such is described with a cut in the "Journal of the Photographic Society" for January 21, 1856 (vol. ii., p. 293). It has since done temporary duty in the arrangement of various pieces of apparatus in my physical laboratory, and was selected for these particular experiments owing to its shape being more convenient than that of other available pieces of wood.

But is it seriously expected that I should answer such a question as "Did Mr. Home furnish the board?" Will not my critics give me credit for the possession of some amount of common sense? And can they not imagine that obvious precautions, which occur to them as soon as they sit down to pick holes in my experiments, are not unlikely to have also occurred to me in the course of prolonged and patient investigation?

The answer to this as to all other like objections is, Prove it to be an error by showing where the error lies, or, if a trick, by showing *how* the trick is performed. Try the experiment fully and fairly. If then fraud be found, expose it; if it be a truth, proclaim it. This is the only scientific procedure, and this it is that I purpose steadily to pursue.

IV. THE RECENT GUN-COTTON EXPLOSION.

IT is now some eight years ago that the Gun-cotton Factory at Stowmarket was first established. The great improvements which had just then been completed in Austria in the way of manufacturing the explosive seemed to promise a hopeful future, and inspired chemists and manufacturers with fresh confidence in its qualities and utility. General von Lenk's modifications in the preparation of pyroxylin had been so far successful as to furnish a material at once serviceable and under control, and if the Austrian gun-cotton was not perhaps all that a military or industrial explosive should be, still its complete and perfect elaboration appeared imminent. Indeed, until Lenk took the matter in hand, the employment of pyroxylin as an explosive agent had never been attended with even the remotest success, and our readers no doubt well remember the terrible and frequent accidents that characterised the first attempts to deal with the material in large quantities. Explosions in England, France, and Germany, marked the era of its first short and disastrous reign, and unanimously almost, both abroad and at home, it was resolved to abandon a compound so dangerous and little understood as Schönbein's newly discovered nitro-cellulose proved to be.

Von Lenk paused for a time before bringing the subject again forward, so as to allow prejudice to subside as much as possible, and then with matured plans and fresh experiences sought to obtain further consideration for his *protégé*. Great credit is in truth due to the Austrian Engineer Officer in prosecuting so earnestly his researches with an agent which had excited such universal suspicion and distrust; but fortunately the improvements effected by him were so simple and obvious, that his Government deigned once more to make a trial of the invention. And here, at the risk of being tiresome, it behoves us just briefly to recapitulate in what manner General von Lenk's system of manufacture differed from the ordinary method of procedure. The first form of gun-cotton was, as we know very well, that of cotton-wool, the cellulose being immersed into a mixture of strong sulphuric and nitric acids for a certain period, to effect its conversion into a compound highly charged with nitrogen. Little care was taken to secure a material of the highest explosive force, nor was the elimination from the finished gun-cotton of free acid or organic impurities very strictly studied, so that the result was frequently a product of an unequal and unstable character. The violence with which this material exploded was quite ungovernable, and its lack of uniformity formed an element of risk and danger not easily exaggerated. Lenk's cotton, on the other hand, was a far less treacherous and dangerous article to deal with; the cotton was converted into pyroxylin in the form of skeins or woven twist, and these were treated with strong acids for so long a period, that a more highly explosive, and at the same time more stable and perfectly converted, gun-cotton was the result. Elimination of the acids was secured by immersion of the cotton in running water for the space of three or four weeks, and finally a treatment with silicate of soda or potash followed, which although possibly without effect in retarding the combustion of the cotton—the avowed purpose of its introduction—was doubtless of some importance in checking any incipient generation of acid that might be set up. This yarn gun-cotton burnt with much less violence, and came to be employed with some prospect of success in ordnance and small arms, when twisted and woven into cartridges of different forms; but much more experience of the subject was nevertheless required before it could be safely adopted in this way as a substitute for gunpowder. The military authorities in Austria were in the meantime indisposed to carry on a costly and lengthened investigation, such as a thorough study of gun-cotton promised to be; and as it was evident, even in its improved form, that the cotton was unsuitable for general army purposes, an abandonment of the material necessarily followed.

But the progress made was too valuable to be lost sight of. Scientific men in this country were fully alive to the possible value of this improved gun-cotton as a military and industrial agent, and attention was ere long called to the subject at a meeting of the British Association in 1862, and further investigation of the matter definitely decided upon. Seeing that scientific opinion in England was friendly disposed towards the new explosive, the holders of the patents in Austria at once cast about for manufacturers over here willing to undertake the preparation of the Lenk cotton, and thus it came to pass that presently the Gun-cotton Works at Stowmarket were established for the purpose of preparing the material on the Austrian system. M. Révy, who held the English patent at the time, was, we believe, the projector of the works, which were constructed under his immediate direction, and in a short time the manufacture and application of the material was commenced. For several years affairs went on exceedingly prosperously, if we except one fatal accident, and confidence increased so much among the miners and and quarrymen by whom the pyroxylin was employed, that an enlargement of the Works soon became necessary to meet the increasing demands.

The mode of manufacture at Stowmarket underwent a change in 1868. It will be remembered that soon after attention had been called to the subject at the British Association, a committee was appointed by Government to inquire systematically into the subject. General Sir Edward Sabine, K.C.B., the President of the Royal Society, was appointed chairman of this body, which consisted of several members of the British Association, and officers from the scientific corps of the army. The decision of this Committee was decidedly in favour of pursuing an investigation of the properties of gun-cotton, with a view to its probable employment in military and industrial affairs; and another Committee—that on Floating Obstructions—which included among its members officers of high rank and attainments in the Engineers and Royal Navy, not only confirmed this dictum, but before separating in 1868, expressed a decided belief in the importance of pyroxylin as an agent for mining and torpedo purposes. The favourable opinions thus expressed were, it is but fair to state, based upon results obtained with the compressed or pulped form of the material devised by Professor Abel; and it is not to be wondered at, therefore, that after such favourable testimonies from scientific officers representing every branch of the Service, that the Stowmarket Gun-cotton Company should set aside the Austrian mode of manufacture for that of Mr. Abel, which had been so universally commended.

Of the safety of manufacturing this compressed gun-cotton, and of its perfect stability when properly prepared, there can hardly be a doubt; but that our readers may judge for themselves of the manufacture, we will briefly detail the points in which the process differed from the earlier method of Von Lenk. Instead of the costly yarn previously employed, a good quality of cotton-waste was used for conversion into pyroxylin, and this after being steeped for some forty-eight hours in the acids, was well cleansed and subsequently beaten into pulp. In this form it was put through the poaching process, where by means of paddles the disintegrated mass was perpetually kept suspended in water until all trace of acid or other impurity had disappeared, and then a treatment with carbonate of lime followed to impart an alkaline tone to the material. The pulp was afterwards pressed into discs or cakes of any desirable form, and dried upon wire nets at a somewhat high temperature. It is not until gun-cotton of this kind is heated to something above 300° F. that any trace of nitrous fumes are perceptible, and its actual igniting point is seldom below 350°; for, as will readily be understood, the exceedingly perfect washing process to which the cotton is subjected must infallibly remove all trace of free acid.

We are compelled to make these few preliminary remarks before approaching the subject of the recent explosion, in order that our readers may fully understand the actual *modus operandi* adopted at Stowmarket, and at the same time be cognisant of the circumstances that seemed to justify the Gun-cotton Company in undertaking the manufacture of this powerful explosive. The confidence expressed by the many eminent authorities on the subject who composed the committees of which we have made mention, certainly constituted a very substantial guarantee of the comparative safety of the material; and, moreover, the practical experiments made with considerable quantities of the material by the Royal Engineers, and mine and quarry managers, for several years past, confirmed the favourable opinion formed by scientific men.

Under these circumstances, then, it is not surprising that the Stowmarket Company prosecuted the manufacture of gun-cotton, and that, alive to recent improvements, it adopted in later years the simple and ingenious modifications elaborated by Professor Abel, whose persevering and exhaustive investigations are well known to all of us. Year by year the fame of gun-cotton as an invaluable mining agent spread abroad, and as a new and particularly effective method of removing submarine rocks and sunken wrecks, as likewise for employment in torpedoes, it has stood for some time without a rival. It should be borne in mind that a more powerful explosive than gunpowder is in many warlike matters simply indispensable, and military men were indeed delighted to find in gun-cotton an agent which, while free from those risky attributes that characterise most of the violently detonating compounds known to chemists, was at the same time possessed of such extraordinary force. Official committees had lauded its qualities; military men without exception were satisfied with its practical results; for industrial purposes it was extensively employed; the scientific reports were unanimously in its favour; and in face of these circumstances, Government very naturally decided upon its adoption into the service.

In the midst of this comes the appalling explosion at Stowmarket. An establishment covering some three acres of ground, and consisting of factories, outhouses, and magazines, so built that the group of buildings presented the appearance of a little model village, has been literally scattered to the winds by the ignition of some ten tons or more of our new military explosive, and this just as the consummation of its success was at hand. Where the principal magazine once stood, an extensive pond 15 feet in depth is now to be found, located, it would appear, in the middle of a ploughed field, for so complete is the annihilation of the building that scarcely a shattered fragment remains to mark its previous whereabouts. The workshops and outhouses around present one mass of ruin and desolation, and the strong iron shafting and metal piping have been dashed into fragments as numerous and small as those of the most fragile portions of the structure. Some of the roofs and walls are literally shivered into atoms, and the only whole pile remaining is singularly enough a tall brick chimney. When we state that out of 130 men, women, and children that were employed upon the premises as many actually as 100 were killed or wounded, the destructive nature of the catastrophe will indeed be fully understood.

To what cause is the explosion to be ascribed? Shall we straightway determine once for all to give up gun-cotton, which as long as it has been known has always borne the character of a faithless and unreliable servant, and thus have done with it for ever? Shall we discard it as a material whose nature is too little understood, and whose behaviour we find it impossible to control? But if we decide on such a course, then to what substitute shall we turn our attention? —to nitroglycerine, dynamite, picric powder, or some of the fulminates,—for compounds of this sort we certainly must have for warlike purposes. Truly, we cannot but hesitate before adopting any of these latter explosives, for in many cases their stability is, we know, more than doubtful, and thus we shall do wisely to make at any rate some inquiry into the cause of the terrible event, and not to throw away ruthlessly all the information and experience that has been acquired during the past fifteen years.

In the first place, however, irrespective of the actual cause of explosion, we cannot admit for one moment that there was any reason on earth why its effects should have been so disastrous. We have here a store of a dozen tons of dry and highly explosive gun-cotton within a few yards of a populous factory, where, indeed, not one ounce of the material need have been stored. Nothing but wet, and therefore uninflammable, cotton should have been in the factory, for the drying, as also the storing, might have been carried on just as well many miles away from the buildings. It is nothing but this silly overweening confidence in the safety of gun-cotton that has caused such widespread destruction; for when it is remembered that the simple infraction of a rule by a careless workman would have been sufficient at any time to have caused ignition of the mass, the callousness of the authorities at Stowmarket is simply astounding.

The accident, has, however, happened, and it remains but to find the cause. In this task we have fortunately some slight clue, and one which we doubt not will afford a satisfactory explanation of the whole affair. The Gun-cotton Company had recently entered into a contract to supply Government with the material to the extent of 200 tons. Of this quantity two deliveries, each consisting of 10 tons had been made, the last supply singularly enough having been delivered to the Government authorities on the very day of the explosion at Stowmarket. The cotton had, in fact, only quitted the Company's magazines but a short time previously, and consisted of identically the same description of material that exploded with such disastrous results. Under these circumstances, supposing the cause of explosion to have been due to the gun-cotton itself, it follows that an examination of the store now lying on Government premises at Upnor Castle would furnish important evidence in the matter.

By order of the Secretary of State for War an inspection was accordingly instituted at Upnor, and to prevent in the meantime any repetition of the disaster, the disks or cakes were at once saturated with water. The boxes were opened one by one, and the contents carefully examined: for some time nothing of a suspicious nature was to be observed, but presently a few of the packages were found (containing material only just received from Stowmarket) which enclosed disks here and there very strongly tainted with acid. Not only did litmus paper readily proclaim an acid reaction, but both eyes and nose at once bore evidence to the fact, so marked was the presence of the free acid.

Here, then, was no doubt the true solution of the difficulty, for it is well known that impure and acidified gun-cotton packed in close localities and heated continuously for some time is prone to rapid decomposition, followed after a time by spontaneous ignition. A small quantity of bad cotton would not only decompose of itself, but likewise contaminate and perhaps set fire to good pyroxylin in its immediate neighbourhood, so that even minute charges of the inferior material are necessarily very dangerous in a confined magazine. The evidence we possess of the state of the magazines, roofed in with slate, and exposed for days to the full glare of an August sun, seems quite sufficient to us to account for the decomposition of any impure gun-cotton, and of this there was evidently a goodly portion at Stowmarket, seeing that the Government supply was admitted to be of a picked and specially good nature.

The manager of the works, in reference to the disaster, speaks, it is true, of two separate events, the first of which he specifies as an explosion, and the second as a detonation of fearful force. Another witness testifies to seeing a flare-up previously to the report, and both these statements are doubtless quite correct. It may have been that a detonation of some portion of the cotton followed the first milder form of explosion, and wrought a great part of the damage; but in any case the primary phenomenon was the spontaneous ignition of a large mass of the cotton, and we may confine ourselves, therefore, to seeking the reason of this first catastrophe.

It is, we believe, conceded by most of the witnesses, both scientific and unscientific, that the cause of the calamity was certainly due to the presence of free acid in the compressed cotton, which should instead have presented an alkaline, or at any rate a neutral, reaction. How did this acid, then, come into the cotton? The explanation received by the jury, and which can scarcely be deemed satisfactory even by its advocates, is that the acid was wilfully and deliberately put into the cotton by some disaffected person, although, as the coroner could not help admitting, there was singularly enough no motive or suspicion pointing to such an infamous act. To us, we must say, there seems as much direct evidence to warrant this belief as there was to support the hypotheses that the explosion resulted from the falling of a meteoric stone, or from the act of a foreign emissary, both of which plausible solutions have been suggested. True, a certain amount of sulphuric acid was found in portions of the bad gun-cotton, which could not of course have existed therein if the material had been but tolerably washed in the first instance; but it is surely somewhat rash on this frail circumstantial evidence alone to deduce proof of foul play. If the catastrophe can be explained in any other way, why is it at all necessary to bring a charge of so grave a nature even against "persons unknown;" and that other explanations are possible, and probable, any one who is cognisant with the manufacture of gun-cotton must admit.

All agree, we repeat, in saying that there was free acid at any rate in some samples of the cotton, and, moreover, we have it on record as a positive fact that gun-cotton in a state of decomposition will explode spontaneously when exposed for some time to a high summer temperature, a circumstance which has been scientifically demonstrated beyond cavil. Can anything, then, be more conclusive? We think not; and now there remains to be explained the difficult matter—how the acid got into the cotton and thus set up incipient decomposition in the first instance.

Putting malice altogether on one side, the presence of acid in the cotton may be due to two causes; either the acid was never perfectly removed in the poaching process, or in drying the discs became overheated and nitrous acid liberated in this manner. Although we do not by any means incline favourably to the latter explanation, still the account given of the manner in which the cotton was dried shows that the process was carried out in a very loose and careless manner, and that instances were not rare of the ignition of the cotton on the frame by overheating. From the drying-house the gun-cotton was at once sent into the magazines without further testing—a grave oversight we cannot but think—the material being subjected to examination only when in the wet stage of manufacture.

It was during the actual manufacture of the material, the evidence seems more conclusively to point, that the mischief originated. The testimony of the workpeople proves beyond doubt that the manipulations were of late conducted in an unusual hasty manner, and that vigilant care and attention, so essential in a manufacture of this kind, was by no means exercised. Instead of dipping only 75 lbs. of cotton per day, which would seem for some time past to have been the regulation quantity, double that amount was expected to be treated by the labourers in the dipping-house, and as a matter of consequence acceleration in the conversion process signified also the shortening of the washing operation. If properly conducted, it is difficult to see how any trace of acid could possibly remain in the cotton, for the precautions provided for its elimination were as perfect as could well be devised. After centrifugating to remove the greater portion of the acid, the dipped cotton was placed for some time under a formidable cataract, which literally beat out the acid from the fibres; breaking up and tearing into pulp in a plentiful supply of water followed, and in this finely divided state it was churned or poached in another bath for many hours or even days. During this process also a small quantity of chalk and lime-water was added to impart an alkaline rather than an acid reaction, and it was at this stage of the manufacture that samples were always taken for testing. Indeed, until the chemist reported the batch to be free from all trace of acid,—for it appears acid was often found in the poachers,—the poaching was constantly proceeded with, and not till it had passed the various tests satisfactorily was the pulp pressed into shapes and dried.

The increase of work at Stowmarket was due to the execution of the large Government contract to which we have alluded, and it is to be feared that press of business created a lack of vigilance upon the part of the foremen and others. We have it in evidence that on one occasion a batch of very inferior cotton was in some way or another actually finished and dried, and although the circumstance created at the time much concern among the managers, still no satisfactory solution of the affair could be suggested. The cotton was supposed to have passed rigidly through all the operations, and stored in the ordinary manner, and yet on subsequent examination it was found to be fully impregnated with nitrous fumes and in a condition similar in all probability to that presented by a few of the discs delivered at Upnor. Inasmuch as the chemist whose duty it was to examine the samples protested that his tests had always been carefully and critically applied, we must suppose that either some mistake occurred in sampling or that no specimens of this particular batch had been sent to his laboratory, but that it left the factory without being properly washed and poached. What happened once may happen of course again, and in the hurry and bustle that unfortunately but too evidently existed, there is no material doubt in our mind but that an impure batch of gun-cotton—the charge from one of the poachers probably, weighing about half a ton—was passed into the magazines.

It is much to be regretted that better scientific supervision was not exercised in a matter so purely technical as that of manufacturing gun-cotton. The particularly satisfactory nature of recent experiments carried on at Woolwich in regard to the combustion of gun-cotton under ordinary circumstances seems to have led, if not to a callous, at any rate to a too confiding, belief in the harmless nature of pyroxylin. Such experiments were, of course, not without value, but a store of some 15 tons of the explosive, heated for many days in a tropical sun, was hardly likely, if accidentally ignited, to burn away harmlessly in the same way as half-a-dozen boxes of the material put into a bonfire. An explosive of any kind, especially when stored in large quantities, must always be regarded as a most eminent source of danger, and it is not only foolish and wrong, but positively criminal, under any circumstances to treat it otherwise. During the different stages of manufacture constant attention is indispensable in watching the gradual and important changes that the cotton undergoes, and this attention should be given not only by a skilful chemist, but by one specially learned in this particular branch of chemical knowledge.

In conclusion a few words are perhaps necessary touching the stability of pure gun-cotton, for a sweeping explosion of this kind cannot but shake in some degree the confidence that previously existed in the permanence of the material. The best proof that can be afforded is, we think, the fact that quantities of gun-cotton, not pounds, but hundredweights, have been stored up for the last seven or eight years in ordinary Government magazines exposed to all weathers, both summer and winter, and this material without exception is proved to be as sound and uniform in appearance as on the day of its manufacture. Moreover, gun-cotton from the Stowmarket factory has been stored, transported, and used since the years 1863 and 1864 both by Government and private individuals in very considerable quantities, and, so far as we are aware, no authenticated accident from spontaneous decomposition and ignition has arisen. If we add to these practical results the experiences of those chemists who for the last ten years have made pyroxylin their especial study, it must be acknowledged that there is as yet no real evidence to contradict our present views, and we shall deeply deplore the circumstance if the panic now existing has the effect of hastily throwing on one side the valuable information which has only been acquired after so many years of laborious investigation.

V. THOUGHTS SUGGESTED BY PATENT RIGHTS.

THE Royal Commission on Patents still continues and must keep many persons brooding over the subject. We cannot entirely forget it. It reminds us, as indeed every question in social life now does, that new foundations are sought for our institutions, and every new part, if only a chamber or pinnacle, must stand independent of the old, lest it should be disgraced by a too close connection with an antique model. Patents confer rights defined by words and not by natural borders of mountain or shore. Their extent cannot be measured by chains, and we can only see it by understanding clearly the spirit of the men who made the laws and of the country which upholds them. But in these days we ask, what right has any man to exclusive privileges? We certainly will not discuss the rights of man. It is our belief that nature has not yet defined them to us in their fulness, and we wait for the revelation. We know, too, that man has not been able to define them to his own satisfaction, except when self has been too much the master. But we certainly think every man wrong who imagines that these rights ought to be the same in every state of society, and who denies that laws must change their form according to our progress. It would be sad, however, if this change extended to the very spirit of the laws themselves, and if the principles of justice were to alter in the very minds of men. It would be still harder, however, if men were to imagine that perfect justice was consistent with perfect truth or with the nature of man. All great forces are tempered by other great forces, or the universe would be really monotonous. A certain hard reasoning in our times seeks for pure sharp justice everywhere; and mercy, which never will cease to protest with perhaps equal power, is called by some narrow-minded men sentimental.

The utilitarian looks only to that work which produces money or goods, and forgets that man's life may be better destroyed than deprived of that which may be called sentiment. We cannot call the feeling of a desire to be useful to society mere sentiment, but we may call it sentiment of a dignified kind. When a man labouring for society is neglected and despised, his disappointment is not mere sentiment, but it is a sentiment arising in the noblest minds which history has seen. To a great extent our happiness arises from our sentiments even in the lower ranks of civilisation; but in the higher ranks, where the external is cared for, the whole burden of man's life must lean on his sentiments only. We know that some men will give us various meanings of the word sentiment, but taking the meaning so abundantly given to it, we are justified in the assertion.

It is not the belief of all persons, but we think most will agree, that man is entitled to push forward his rights into the external domain of nature to the utmost of his power: when nature objects he is easily repulsed.

We know nothing to limit him but his capacity. Regarding his rights over other men, we should probably have said the same thing had we lived in the time when to conquer nations and to keep slaves was to be thought great; but we now learn from history that the rights of a man are limited by the rights of his fellow men, whether it be in a hand-to-hand struggle, or in the region of thought and feeling only. After leaping over several syllogisms we may from these data decide that patents, like every other desire of man, must be bounded by the desires of other men, and that justice in granting them must be tempered with mercy for the weakness of the individual struggling for self against a nation. To save time let our readers connect for themselves more fully these fragments of thought.

There are men who prefer the purely utilitarian view combined with the national, calculating the results in wealth obtained for the nation as a whole. They look at the statistics of trade and the revenue derived from income tax, and draw conclusions which seem hard to contradict. This view overrules at present, but we have surely learnt that great wealth in a nation may co-exist with great misery, and great appearance of power with great weakness. The true prosperity of a nation must consist in the progress and happiness of the individuals. It is very difficult for us of late to avoid centralisation. We think over all the country on the same subject every day; and when great undertakings are demanded we rush to co-operate, and the work becomes to a certain extent national, however nominally private. By this means we have been able to do great works, and the railway and steamboat systems are wonderful examples, whilst co-operative societies are forming to protect the individual in the smaller details of life. We must rejoice to see the work done. So thoroughly do we seem satisfied with it, that we seek to train the minds of men in the same wholesale way;

and we have a central University of London seeking as much as possible to make all men of the same standard, expecting thereby to carry out the utilitarian national method and obtain the most out of every man.

We object to all the modes of treating men if carried out singly. We object to justice dealt out with mathematical accuracy—for in that case who would stand. We object to an intrusive mercy which would weaken the minds of men, and which elicits that abundant false sentiment too often used as an argument against sentiment the most ennobling. We object to measure the happiness of nations by their wealth and by their power; and we object to any mode which turns many individual minds in one direction unless for some special purpose, instead of leaving them to develope naturally; we are not creators, and do not know what is best for the whole race as well as each individual.

Every one must see the wonderful advantage gained by the union of many individuals for one purpose. We admire it even when it is connected with a debased machinery which makes railways for the purpose of benefitting a few individuals, because nature is higher than they, and the community gains in the end. All men have agreed that Æsop's bundle of sticks teaches a lesson not to be gainsayed. But there are many of us who see in great combinations too much one-sidedness of thought, and prefer to see combination confined to such departments as cannot be managed by a few.

Applying these ideas to the Patent question, it would appear as if there existed an opportunity of encouraging many centres of thought and action, and of counteracting the too great unity of thought and feeling, introducing itself by our latest forms of teaching and examining,—a system apparently borrowed from France, where it has given an unmanageable amount of activity to the capital, and done much to annihilate the independence of the provinces and of individuals.

It is our duty to encourage individuals in the course of thought which is peculiar to them if it offers any advantage to mankind. We are in danger of directing the national mind. Direction is good for the young to a certain extent, for a nation it is dangerous; if it is against the natural impulses it causes curious twists and misshapen growths. Who knows except by looking at the past the probable course of humanity? And who can draw the line of the past straight so as to know its direction? And who knows whether nature intends that the path shall be straight, or whether the simile is wrong, and we are in the trunk of some tree of civilisation or in one of its small branches? There are men and societies who seek to direct the line of invention, and offer prizes; and within certain limits the system answers, but it is for details. The want of great results from this plan is marvellous. We may well say the wind bloweth where it listeth: how little can we even control our own minds. The result is not exactly that which we wish when we begin a study, it is sometimes far beneath our hopes and sometimes far above them. A poet expressed thus the influence of the unknown upon him, "Perhaps it may turn out a song, perhaps turn out a sermon." And we may join to this words from a very different source, "So it is with every one who is born of the spirit." If the glow of genius be in him—if he be beyond the range of thought given him at examinations—he will go fearlessly where his sails carry him, and he will discover new lands; but if we follow him and direct him he may lose the way—the inner guidance under which he is may be lost, as a tune may be in a crowd of discordant sounds, and the real distinctive part of the individual may never be recovered.

It seems better that in such a case a man should have his own way, and a patent frequently suits well, allowing him time to work and to keep himself by his work, when he has a useful thought promising good to some extent at once, and encouraging him to produce more. There are men who imagine that honour is sufficient, and that an equal number of inventions would be made if no patents were allowed. They make a confusion of thought. A man gives out a scientific discovery for the honour, but so far from a man taking a patent for the honour, most persons are ashamed of them and take them simply because of the money, suffering rather a certain amount of scientific obloquy. If the patents were not granted these inventions would not be made. However, this is not a matter of reasoning. We who have lived long among scientific men and also inventors in the arts more or less scientific know it to be true. Numerous inventions have ceased to be carried to completion because of the uncertainty of patents, and because of the frequent robberies committed by the numerous highwaymen who especially prey on such wealth. We must not forget the stimulus of honour, it would be better to see it more closely allied to patents; but the material weal is so dear to man, that we err still more by forgetting it. Let us forget it in the case of the scientific man himself who works apparently for pure honour. Still how many even of these would work if no universities gave them a living? We must not look too near; the number of men that work at science and make no living by it is not great. Even literary men whose work is pure thought, do they not make their thought a business? And are not the poets of the highest class ready also to receive their reward from every new poem and new edition, adding to their fields and flocks and herds? Have the materialists and utilitarians suddenly found that there is a class of men called inventors to whom money is nothing,—a class that will be satisfied with honour, and so be made to work for the benefit of the nation at a very cheap rate. It is the old principle, " So long as the poor slaves do our work for their living only, let them live like the lower animals. We shall take their work until their friends raise up a great emancipation-explosion. If they must be free we shall try what can be done with coolies. Or if we must give them land they shall be 'bound to the soil,' and work for us free a part of the year." But freedom finds them out. The masters, however, are on the alert. "Let us give them wages enough to keep them well." This is done, and the days of good wages arise because good wages bring good workmen in some places. " But let us keep them to their own work; let us give them no education lest they rebel; let us not teach them the trade lest they be independent; let us teach one to tie a thread and one how to wind a reel; but let us not teach any one all the art." And so these uneducated fragments of machinery are tossed about our great towns, until they are caught in the central whirlpool, diminished in body and stunted in mind, sickly, dirty, and short lived; they keep whirling nearer and nearer to the great gulf, until the outside world raises up friends and demands universal education.

We scarcely expected that the spirit of the task-master would find a resurrection in a new and parliamentary form, and seek to make slaves of the inventive genius of the country, saying, " Work for me and find your own living." This is the lowest stage of slavery—it is done by the despot who has conquered; he refuses even bread. He counts the cost thus, " What shall I gain if I do not feed these men, and how much shall I gain if I feed them?" This is the worst utilitarian-national view. This is what some people call taking a wide view of a subject. It is known to be a narrow view; it is known that good wages and good work are most advantageous, and necessarily must come together in time. Yet these men who are studying politics see only the narrowest application of the principle. The age is famous for an immense number of very long, narrow views, and individuals imagine their own to be wide because of this. The age may be, but all views from one point only are narrow; a man may even study astronomy all his life, and be as narrow-minded as he who studies only the making of telescope-tubes.

We have tried very hard to understand why a man who cultivates mechanics for years, and learns how to apply its powers in a new way, should not have the value of his labours preserved from robbery as well as he who has cultivated an acre of ground in an old-fashioned and imperfect way and sown it with potatoes. But one of the great arguments used by the opponents of patents is still more wonderful. It is believed that the course of science and discovery is so straight that we have only to move on and we shall come to everything. If one man does not gain the new idea another will. If this is true of anything it is true of land. Somebody was sure to find out the unknown acre soon, and now we reward him by the firmest of all possessions. Now we are sure of the acre being discovered, but not sure of the mechanical invention. The number of actual inventions is small. We have seen for a life-time the discomfort of men at coach-offices and railway-stations who are obliged to use many small tickets, and to tear them from a printed sheet containing large numbers; they generally took an opportunity at a moment of leisure to cut a dozen or two nearly off, leaving only a corner attached. It was long before the idea of having them nearly cut by puncturing came into any one's mind. And yet the opportunity was before the eyes of millions of nineteenth century men. Some other man than the actual patentee would have found it unquestionably, we believe, but it stood undiscovered for many periods of fourteen years. Is it a great reward to give one the benefit of it to himself for one such period? The case is quite obvious, and we might rather say why should it be given for one such period only. We may ask in connection with it, for how long shall a man be allowed to cultivate his field? For how long shall a landlord refuse to improve his land, diminishing the food of the country? We should allow him to continue till public opinion acts on him or his successors, but we should do it consistently, and not as the opponents of patents.

It has been said that if a literary man gives an idea in an essay or a poem every one may use it, and the patent right (or copyright) extends only to the form. We do not call this an argument at all. A literary work frequently has its chief, sometimes its only beauty, in the form. What is a poem or a drama without the artistic form and form of expression, or what is any work of fiction or historical work apart from it? If a writer uses expressions from Dickens or from Carlyle—both men whose style is peculiar—it does not diminish the value of their works; on the contrary, it excites the desire in many to see them, and renders the copyright more valuable. Every author delights in being copied and imitated, and if it should be done secretly others have a delight in showing it clearly. In the copyright law we have given literary men a right as definable as the right of land; are we to refuse a similar right to those who work in the applications of science?

To our mind the only defensible objection to patents would lie in the impossibility of defining them if this existed. We throw aside as pure rubbish the objections made by manufacturers in the necessity of paying for patent rights, as we throw aside the arguments of the Americans, who object to pay for our copyrights, and so pirate our books. It may be difficult to define, but law itself is a difficulty daily increasing, and the rights of workmen of every class, even the rights of the House of Lords itself, are continually under discussion. Yet the work of law must be done, unless we fail in our interests and make outlaw classes from sheer inability to tell how to protect them. To examine this supposed difficulty connected with patents, we must recur to the supposition that there are many persons at one moment rushing on an invention, and that by the laws of progress it comes without special merit of one individual. We can easily imagine this to be the opinion of men not thoroughly conversant with the whole subject. There are a class of secondary inventions of which this is strictly true. Let us take for example a very remarkable discovery, marking distinctly an era not long past—that of the first aniline colour by Perkin. If he did not deserve to have that preserved to himself, it is hard to see how any man ought ever to have any possession whatever. Let us imagine for a moment that part of the work was accidental, that does not matter; it is such an accident as does not happen to fools. Besides, if it were wholly accidental, we may say it is perhaps by an accident that a man becomes a king, or a wise man, or an idiot; but he is no less such, and we leave such in pretty firm possession of their characteristics, although their rights are variously disputed. This first discovery by Perkin may be called a primary one; it begins a new class of bodies. After its mode of operation was pretty well known, it was not very difficult to obtain a large number of other bodies more or less allied. These are evidently secondary discoveries, and in them the chief difficulty of the law rests. As dependent processes increase around a primary they become easy; they may come into the hands of men who have little science, and the latest may be the best in a mercantile point of view. What is to be done with this last; is it to be ignored? Here comes in with great propriety the utilitarian national view. These later often come into the hands of wealthy and energetic men, who see that facility and cheapness of manufacture have been attained, and now is the time to launch out money. The people then benefit, and the national advantage of patents is gained to the utmost. Surely it is justice to the inventor that these subsidiary processes shall not overpower his, and it is justice to the nation that the inventor should not have the power of limiting the expansion of his invention. This point was really the greatest difficulty in patent laws, but surely the acumen of legal and scientific men will overcome it, so as to do justice both to the real inventor and the improver of the invention. We are greatly ashamed to see the many little shifts whereby a patent is evaded. Sometimes the real value of the patent is in the result and not in the method. There may be many ways of attaining the same end. We see no reason in such cases for refusing the patent to the result; let the grant be as a reward of ingenuity or wisdom, and not a narrow-minded attempt to obtain the most and to give the least. A scientific judge would probably throw aside most of the attempts at robbery by men who use the law to break it; but we do not see how one who has not studied science can see all the bearings of the subject. The writer of a previous article in this journal was severe both on legal and scientific men; but he ought to have shown more clearly that their faults lie where they interfere with each other, because in the imperfect education of both they are each unintelligible to each other. In a scientific application some might prefer the judge to be a scientific man with a wide education if such can be got in sufficient number; they are very rare indeed who can think otherwise than in their own grooves, and the best would require a legal assessor of course.

It must not be supposed that we neglect the utilitarian national point of view; we object only to its exclusive use. Sometimes it is convenient to look at the sun through a pin-hole, and the wisest astronomers look at it through little tubes, or examine its spots projected on disks; narrow views are for certain occasions the best, but the highest glory of the sunlight or of a man is not best seen by dissection. We cannot forget that when a nation is in danger private interests are counted as nothing: so if any man will prove that the labour of individuals in the direction of invention in the arts must be treated on different principles from other labour because of national necessities, we must yield; but in yielding say that the hope of the state must be temporary only if it depends for progress on men who are to be robbed of all their wealth of invention as soon as made, and irritated by their belief in the injustice of their treatment. By some strange delusion some men believe that inventors will make quite as much money without patents as with. According to a quotation in a recent number of this Journal, it would seem as if Count Bismarck believed that by excessive sharpness it might be managed, but the inventor must be sharp indeed if he is sharper than a thief of inventions. If the profits of an invention are divided over many instead of given to one, each must receive only a small share. This is not a matter to reason, it is certain. The inventor cannot by any amount of sharpness keep before all his opponents for fourteen years. They may begin where he leaves off, and without any trouble at all they are up to him. Man may advance in knowledge, but he cannot advance in time, he must live to-day; no amount of sharpness will ever lead him a moment nearer to-morrow than it will his dullest neighbour. And why should a man require to be sharp in order to obtain justice or in order to make a living? It will be a sad world when only such persons can live. Must we give up punishing thieves on the plea that if we had been as sharp as they we should have lost nothing. It is acknowledged that an individual cannot as a rule defend himself in a great community, and therefore we have laws and law officers. These are for the protection of men not sharp enough to protect themseves; the very sharp people would rather be without them.

Let us not forget the assertion under examination, that inventors would make as much without as with patents. As a rule inventors have not money; this might have been used as a powerful argument to show that inventions were made for the sake of the money. It is, in fact, a rare thing for a rich man to enter into the society of inventors; he may continue to invent after he has entered. The idea among them is "Let him take castles who has ne'er a groat."

Now, if all men were honest, patents would not be required, truth would be equal to a patent for the whole life. But if all men were honest and good, what would be the use of policemen or criminal courts, judges or barristers? and how small would the work be for arranging matters in a friendly way in our civil courts? He that has money will carry out the invention to most profit; and we know from daily observation that unless he is obliged to pay the inventor he will not do it. Now there is no use in denying these things, they are well known. If inventors were to be so foolish as to go on inventing, and to be robbed without any hope whatever, they would be much duller than we imagine them to be. We leave with little attention the question whether inventors work for simple honour. The very fact that, whenever an invention, scientific or otherwise, can be used for making money, it is patented, is a sufficient answer. But supposing it to be true that honour only were wanted, it would be still an argument for patents. Men would then receive their proper place as inventors and be registered as such. Although we know that this would not satisfy them, it may be called supplementary reasoning. A good guide to the inventions made would be invaluable, and if it were authoritative still more so. A good beginning is made in the abridgments by the Patent Office, but we are sorry to say it is far from being a record of purely honourable achievements; we should be very glad to see it weeded so that reading might be less wasteful of time.

We may now look at the method by which the patenting stage of an invention is arrived at. Some people, especially such as have read about inventions and discoveries in past times, and who have impressed on their minds stories from their school books about glass being discovered by accident when some people made a fire to cook by on the shores of Syria, and about hens with clay on their feet going over some sugar, have some idea that inventions are generally accidents. If we examine the history of the two inventions alluded to above, that of making glass as it is now required to be made, and that of purifying sugar as it is now purified, we shall find that each subject requires a large portion of a lifetime for full understanding; nay, more, if we look at the history of the men who have made certain of the inventions included in the history, we shall find that they have devoted the greater part of active, intelligent, and valuable lives to one small portion. Such we assure the men who believe in these typical inventions-by-accident, is the truth concerning the progress in the arts. For glass we must have something more than a wooden fire under a sand pot in a desert; we must have the fine adjustment of the silica to the soda and the potash, and the lead or the lime; the fine toning of shade at times by manganese; the colouring by oxides of copper, of zinc, of uranium, and other metals; the Siemens's furnace for the large pots, and the heavy machinery for lifting the melted mass pouring on the thick iron tables, and rolling it; the fine arrangements for annealing at a proper temperature, and the machinery as well as the choice of sand, emery, and oxides of iron for the polishing. Then the blowing, colouring, cutting, moulding, designing. We shall not enumerate all the complicated labours of sugar refiners. The hen and the clay have long been forgotten, and the sugar refiner must use the latest inventions of science, namely, the newest forms of polariscope, and the finest chemical tests to examine the samples in order to see whether the sugar is worth buying and worthy of his labour. No accidents help men as a rule in inventions, except such as happen to all men who are ready to see the phenomena of nature as they occur. There are a few accidental appearances, slight openings into unknown chambers of nature and art, which are shown but for a moment as if some stone had fallen down and the slaves of nature had not had time to cover it; but as a rule inventors dig and fight these slaves on their own ground until by conquest the two become friends. We could tell of many patents that required years of labour before they were fit for the public, and even of some which were the result of the experience of half a life. We could tell also of others taken by two persons in the same month; of these many came to nothing. One, as an instance, was an idea caused by the general current of thought regarding atmospheric railways, and the people preferred to have the patent first lest it should be lost to them, so much do inventors live in terror of pirates. We are led again to this point, Do individuals think, or does the community? We agree with the writer in the January number of this Journal, "It is to the individual that nature and providence give the rich rare gifts that advance humanity. The history of the few has been the history of human progress; and a lost thought may roll for ages through creation without finding a mind to comprehend it or a brain to make it useful to society." Had Horace not lived, who would have given us his charming odes? would they ever have been written by Virgil instead, in order to make up for the want of a Horace? Who would have given us the rich gossips of the east of the Mediterranean if

Herodotus had not done it? No man has a right to say that it ever would have been given; we believe it would have been lost as the history of most other times has been lost to us, leaving only the scanty pickings of the scholar. Had David not written the Psalms the deepest insight given to us of the human heart would have been lost. Providence does not send one man immediately to do the work of another who has been neglected. If the world neglects its benefactors it must suffer, and it does suffer. This is most assuredly true of the great men, the prophets, the sages, the wise rulers, the great poets and historians; their wisdom lies for ages unnoticed. The question arises, does the same argument apply to science and to the arts? It does, although not with equal power. The reason of the inequality lies in the line which science has drawn for enquiry, a good deal of the road is already roughly made a-head of the part where the side-lands are inhabited. But only roughly made, and we frequently err by fancying that it is made at all, and he who goes quite off the common track is often the most successful. Had Faraday not lived we have no reason to believe that our knowledge of magnetism would have been where it is—we are almost certain it would not. Had Joule not been born it is equally certain that our knowledge of heat and force would have been in a very dim state. We see no men that were inclined or able to take his exact place. In the applications of science Davy knew the action of light upon silver salts, and tried to make pictures, but failed. It was not done till about thirty years afterwards, and not well done on paper for fifty years.

Even in those things demanded in daily life how slow is thought, and how confined to a few! Phosphorus is known for two hundred years before we can make matches by its help. Ether is known, we do not remember how long, but very long before it can be used for preventing pain in some of our greatest days of trial. Davy tried the inhalation of gases, and he, a man of genius, missed anæsthetics. We have here the same great fact—it is individuals who are remarkable, and few individuals. It is so in size, it is only occasionally that one man is larger than his fellows. It is so with the lower animals also, and one horse, or one cow, bull, sheep, or swine, when thus advanced by nature, becomes so much admired, that its race is soon carefully spread over all the country, at least as much as time will permit,—an artificial added to a natural selection. It is the same with plants, and one rapidly spreads its progeny everywhere, advertised in all corners and sold by all florists.

Perhaps some will say that rarity is true of great inventions and not of small. The greater the invention, the greater certainly is the truth of that for which we argue. The smaller the invention is, the greater is the chance of many persons making it. That is admitted. And surely there ought to be some limit to the admission of an invention into the patent list. There is a certain class of trivial ideas that ought never to be admitted in our opinion, and there is a class of dreamy patents which may be called nonsense. A man ought to make his idea clear, otherwise it is a speculation at best and unfit for the arts. Then there is the whole class of subsidiary patents, such as are mere branches of others. These seem to us the only class difficult to deal with, but we trust in the wisdom of our lawyers and scientific men to overcome these difficulties.

Another point has been lately raised, shall we give patents to those who bring inventions from abroad? At first, one is inclined to say, let the patents come from any part so long as the community is benefitted. On the other hand, it may be said, that a person who sees an invention in a finished state abroad has really little trouble in introducing it here, and cannot stand in the place of the real inventor. The rule we believe is, that if it exists in a foreign country unpatented for a few months it cannot be patented here. We should make quite the contrary arrangement. If it existed for a long time in a foreign country patented or not patented, if it were only useful, we should thank the man who brought it. If it were a thousand years old and no man during all that time had sense to bring it to us, we should reward him who at last saw its advantage. His vision is his genius.

Yet we confess the two classes stand on different bases, but we prefer to treat them equally, and imitate, as a nation, the abundant liberality of some of the Saracenic and Eastern princes, who are said to have had beside them the wisest and best men that could be found; each receiving abundant reward for the importation of his wisdom and the example of his goodness. The age haggles, splits hairs, reasons small, and imagines itself to be sharp, but broad, fine, human feeling, will bear down all this little appearance of exactness. Who cares if every blade of grass on the lawn has the same shape if only the whole lie smooth and equal. A few great principles laid down would get rid of much of the small reasoning and law-making. To obtain a patent a man must declare that he is the first inventor. This test act ought to be abolished. It seems as if few men knew who first invented anything. It is quite sufficient for a man to say that he believes he has a right legal and moral to apply for a patent, and that no man has a right to oppose him. This would also allow a man to have his patent taken out by another man chosen by him. What is it to the community whether the real inventor's name is given or not? We say again, the inventor would prefer, as a rule, to hide his name, because money is the object of inventors in the useful arts. A patent is a matter of business. If an inventor wants fame, he seeks it by publishing his inventions in journals or scientific societies. This of itself is a sufficient answer to those men who insist on saying that inventions would be as numerous without patents as with.

It has been proposed that no questions shall be asked in taking a patent—that is, that every man shall patent what he pleases. This looks quite a patent agent's idea to increase business. We think it a mistake. It will help many men to impose on the public. They are too much helped already, and men with impunity even use the word patent when there is no patent. It is a confusion of right and wrong, and this is an abundant reason for refusing such a system. It is in reality practised now but not avowed. We object to the practice and the avowal. Let a patent be a real invention, with truth on the side of both the contracting parties—the patentee and the government.

A proposal has been made to extend the duration of a patent to twenty-one years. With our opinions we cannot object to this. We know by abundant observation that it is difficult in the space of fourteen years to bring a patent, even an excellent one, into use, and we know, also, that expenses must be incurred during the whole time. This is very contrary indeed to the supposition of some men who imagine that an invention is an idea caught by accident and given to the public without expense. Men labour for the whole fourteen years, not only endeavouring to perfect the manufacture, but to teach the public its advantages. If fashion should suddenly desire a mauve or magenta colour, the lucky manufacturer gains money rapidly, because fashion has a method of teaching the whole Christian world its ideas, principles, and practice in a few months, and thoroughly outstrips all societies by whatever name for the improvement of mankind in everything whatever. But let it be shown that certain kinds of water must not be drunk at all, and that others require at least to be filtered, ages seem insufficient to teach this, and even a plague can do it but partially. The inventor who was assisted by fashion would make his money at once; the man who would make a perfect filter, supposing such a thing to be found, would probably struggle for years. It would seem very unfair to leave the latter to be robbed as soon as he should have by great expense taught the public, and it does not seem fair to treat the two differently. Besides, if fashion may treat one well for a while, it may leave him with equal suddenness. To our mind, the very least additional benefit that could be granted to patentees would be a readier mode of obtaining an extension of the time. The enquiries regarding extension of patent right do not satisfy. It is extremely difficult to get the truth fully told, and science has but little to say in courts of law, or says it with a small subdued voice easily silenced, because it has no recognised legal position —a mere tool tossed between good and bad, instead of a great and benevolent power, more than national, more than world-wide. Perhaps its cultivators are chiefly to blame; who can tell? An objection to patents is often raised by those whom we may call cosmopolitans. Their ideas are alluring; they lead us to large and unselfish thoughts, and when we think of "the great world spinning for ever down the ringing grooves of change," our small every-day duties become invisible,—we almost imagine that they may as well be left undone, and then we come to a sudden stop. If we all thought so, of what character would be the changes in the world; they might be backward for man,—it is only by minute attention to details that great establishments are kept in order. This is enough; the man who works for his fellow-creatures over all the world with equal ardour will show a small result of his labours; experience shows that he will do more by cultivating more carefully a smaller portion around him to make it an example and a power. No men have done more good to the world than patriots whose aims have been pure and their views confined entirely to their own country. The reason is simple enough. Man is small and his life short, and he does best who does his work most perfectly; if perfectly, it is necessarily in a confined sphere. There are a few men whose office seems more brilliant—men who bind together the various attempts of individuals, communities, and nations; but this is only possible when the work at home is well done.

We have little more to add, and we have said nothing that had not in spirit been said elsewhere; but as desired, we thought on the subject for a little, knowing that it occupies the minds of many persons, although it has come before the public very rarely as yet, and the public do not seem to know how much it concerns them. On one point only we shall add a proposal, namely, expense. We know from observation that the payments are felt to be very heavy by many deserving and industrious inventors, and if Government will not grant a diminution, it may be, perhaps, persuaded to allow these payments, or at least one of them, to be made somewhat later than now. Probably the best way would be to take the payments, with the exception of a very small preliminary one, out of the profits of a patent, to be collected as the income-tax is. A good patent might very well pay a considerable percentage. We calculate, or at least we conjecture, that 1 per cent on gains by patents cannot be less at this moment than £20,000; and it would be no hardship to pay 2 per cent, besides the ordinary income-tax of course. This would be enough for all that is required for registration, examination, and publication, and there would be a pleasure in seeing payments made by men who had prospered, instead of by poor men who had nothing but hope before them.

We may sum up the principal points:—

1. A man's property in his own invention ought to be protected as carefully as his property in land, or in houses, or in any movable goods.

2. As an invention when once explained is more readily taken from a man than houses or lands, special protection ought to be given, just as exposed property receives more of the attention of the police.

3. As inventions in the arts are made for the purpose in almost every case of making money, it is not good to burden them by exactions before the money is made. The true principle would be to make the gains of the patent pay, not all equally as now, but in proportion to the gains. This would be paid with pleasure, and the Government would receive the payment with pleasure, because it had given an equivalent by affording the patentee special protection.

4. There ought to be some discrimination in giving patents. Even a little advice to persons might be useful. When they come with their little fancies they might be shown that it would be better to go home without a patent.

5. A better mode of judging patent cases is required. The scientific element has not its due position. It would not be good to give it all the power. Scientific men have faults like other men, and one is that they are apt to be afraid of or to avoid ideas new to science, although these

new things may be the most rare and desirable. This preference for the general supposed course of true science sometimes blinds them.

6. Although we think this the age of inventions, we are in every corner in want of more. It is for the good of the nation and the world that they should be produced more rapidly. They would be if encouragement were given; the better kind would probably soon supersede the lower. It is proposed that every obstruction now depressing the minds of inventors, and keeping back some of the best, shall be removed, and an attempt be made to put in its place an encouragement or facility.

VI. ON MODERN BRITISH ORDNANCE AND AMMUNITION.

By Lieut. S. P. Oliver, Royal Artillery.

IN the last number of the "Quarterly Journal of Science," the new 11·6-inch gun* of 35 tons was alluded to as the latest addition to the British Service ordnance. The following details of its dimensions and construction are given beneath, viz.†:—

		Inches.	
Nominal length		191·5	
Total length		195	= 16 ft. 3 in.

		Inches.
Trunnions	Width between	56
	Diameter	13
	Width	7·75
	Axis from breech end	73·15
Length of	Bore	162·5
	Rifling	135
Twist increasing from 0 to $\frac{1}{35}$ calibres		406
Grooves 9	Width	1·5
	Depth	0·2

Vent: hardened copper placed at an an angle of 45° with the vertical, and enters the bore at 12 inches from bottom of bore, so as to strike a battering charge of 120 lbs. pebble powder at 4-10ths of its length from the end of cartridge.‡

* This gun has since been bored up to 12 inches, with at present doubtful advantage.

† *Ante*, Minute 28,806.

‡ See Proceedings of Department of the Director of Artillery, Minute 28,879.

CONSTRUCTION.

			Inches.
Steel tube	Length		168·5
	Thickness at	Muzzle	2·45
		Breech	3·45
Breech piece (coiled)	Length		104·5
	Diameter (exterior)		34·5
1 B. Coil, length			57·5
B. Tube, length			56·5
C. Coil	Length		90
	Diameter (exterior)		56

Palliser shell { Length 33 inches. Weight 700 lbs.

Minimum Service charge, 75 lbs. pebble powder.
Maximum battering charge, 95 lbs. ,,
Intended for Turrets of "Devastation" class.

With a proof charge of 130 lbs. of powder, *i.e.*, nearly half as much again as the Service battering charge, the 700-pounder projectile left the muzzle with a velocity of 1370 feet per second; the copper piston in the rear of the shot, by which the pressure of the gas in the interior of the bore is estimated (according to the depth to which it is driven in upon itself), indicating a strain of sixty-four tons to the square inch.*

This is certainly the finest gun ever turned out by the Royal Gun Factory, to which it does so much credit, and is a signal victory of the F. or cheap construction. It is greatly

* The Special Committee on Gunpowder and other Explosives, in investigating the action of gunpowder, &c., in the gun, make use of three methods:— 1. The determination of the time a projectile takes to traverse various intervals within the bore of a gun, which is accomplished by means of Captain Noble's chronoscope, which registers by means of electric currents upon a recording surface (revolving discs), travelling at a uniform and very high speed, the precise instant at which a shot passes certain defined points in the bore. 2. The velocity outside the bore at the distance of fifty yards from the muzzle, by the Navez-Lenes' chronoscope. 3. The "*crusher*" gauge, which is alluded to above, which determines the pressure exerted by the gas at various points of the bore (usually five in number). If the "*crusher*" be inserted in the *breech* the cylinder is of copper; if in the *chase* of the gun where the pressure exerted is comparatively small, it is of an alloy of *lead*.

With a piston area of 1-6th of square inch, a pressure of 20 tons on the square inch reduces the length of a *copper* cylinder from 0·500 inch to 0·229 inch; similarly a pressure of three tons on the square inch reduces a *lead* cylinder from 0·500 inch to 0·240 inch. This "*crusher*" has been made so as to fit the bush in the base of all Palliser shells. Much better results have been furnished by the "crusher" gauge than by the Rodman system.

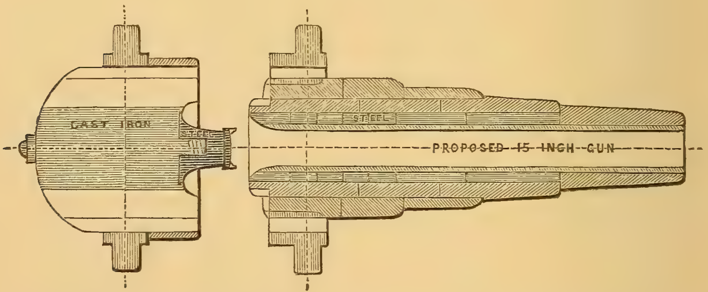
The observations for pressure and velocity independently taken by the three above-named methods, have corroborated and confirmed one another in a remarkable degree. For details see Proceedings of Department of Director of Artillery; Minutes 27,929, 28,072, 28,738.

to be doubted if the famous 1000-pounder of 50 tons, constructed (of cast-steel subsequently densified by forging), by Herr Krupp, at his Titanic Essen establishment, would stand such a crucial test as that to which the 35-ton Fraser has been so successfully submitted.

The last startling sensation in propositions of new guns has been made by Captain Morgan, Royal Artillery, in his "Proposal for a *very heavy* breech-loading gun of novel construction."

This novel arrangement is certainly original, and consists in the gun being formed of a barrel with a movable and extraordinarily heavy breech, which is to be blown to the rear by the explosion; in other words, the gun is to

FIG. 14.



Captain Morgan's Proposed Monster 15-inch Breech-Loading Gun. Charge 200 lbs. of Powder. Weight of Projectile, 1200 lbs. (From Minutes of Proceedings of the Royal Artillery Institution, vol. vii., p. 145.

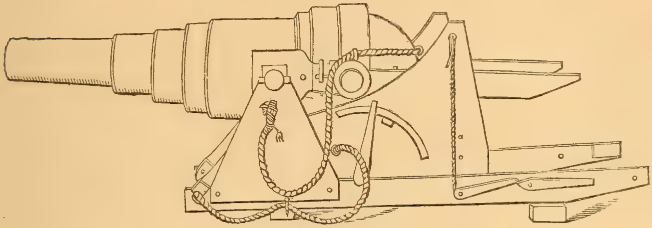
fire at both ends, the force of the discharge partly expending itself upon the heavy breech-piece, which is to recede up an inclined plane. A model was exhibited lately at the Royal Artillery Institution, on a scale calculated for a 15-inch gun, firing 200 lbs. of powder with a projectile of 1200 lbs. weight.

Captain Morgan is perhaps too sanguine when he states that he is "confident that on his system a 20-inch gun might be obtained firing a 1-ton shot, with a breech-piece and barrel of 50 tons each." If foreign naval constructors commence putting 20-inch iron plating on their armour-clads, we shall need 20-inch guns to pierce them.

Outsiders appear to have very vague ideas of the time in which such heavy guns as we now have in the Service can be loaded, run up, laid with accuracy, and fired; and perhaps the best way of affording information on this point will be to quote the results of actual experiments for rate of firing made at Shoeburyness.

By this means it has been ascertained by practice, that a 9-inch M. L. rifled gun of 12-tons, mounted on a wrought-iron carriage, with casemate slide, and traversed by means of tackle, can be worked by a detachment of thirteen men under a N. C. officer, loaded by a 30-pound cartridge and 250-pound plugged shell, laid on a target (5 feet square

FIG. 15.

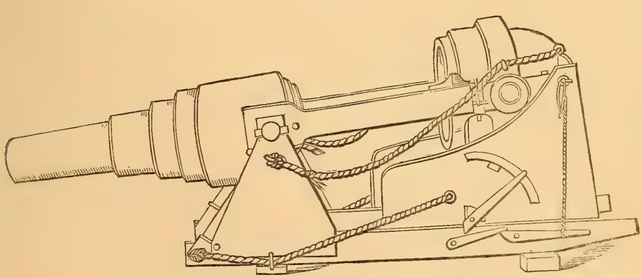


Loaded ready for Firing

and moving at the rate of 8 miles an hour), at a thousand yards range, and five rounds fired in three minutes and twenty-two seconds. The moving target being hit at least once in the five rounds; whilst every shell will be thrown so near the target that it would hit a mark as large as a man-of-war launch.

So also at a standing target, the 23-ton gun, 12-inch, or 600-pounder, has been fired a series of rounds through

FIG. 16.



After Discharge, showing recoil of Breech-piece.

a port with an average rate of one minute and thirty seconds for each round. In the instance quoted the above-named gun was manned by a detachment of seventeen men; it was mounted on a turntable, which had to be traversed round each time to bring the muzzle under the cheeks of a 16-foot gyn, rigged as shears, in order to enable the 600-pound shell to be lifted up and placed in the bore. In another instance the same gun was fired three times in four minutes and nineteen seconds. Compare this with the ancient rate of firing heavy cannon two hundred years ago, as given by William Eldred, sometime master gunner of Dover Castle, during the reigns of Queen Elizabeth, James I., and Charles I. "One may well make ten shots an houre if the peeces be well fortifyed and strong; but if they be but ordinary peeces, then eight is enough, always provided that after forty shots you refresh and cool the peece, and let her reste an houre, for fear lest eighty shots shall break the peece, being not able to endure the force and heat."* The largest piece of British ordnance at that period (1646), appears to have been the Canon Royall, weighing 71·5 cwts., and throwing a 63-pound shot.

From what I have mentioned above, therefore, it would appear that our modern ordnance, although apparently cumbersome, are very manageable, and much more handy than the unprofessional observer would at first suppose.

Captain Sharpe's ingenious models of "*revolving guns,*" now exhibited at the International Exhibition, Kensington, cannot be passed by without the notice which they deserve; their economy of space is great, and the amount of dead weight reduced to a minimum, whilst their extraordinary facility for obtaining extreme angles of elevation is a prominent feature of the system. Mr. Watts thinks the system admirably adapted for gunboats, and two great naval authorities, Sir Thomas Hastings and Sir Robert Smart, both agree in praising it. At the same time I must demur to the following paragraph in Commander Sharpe's pamphlet: †—

"Our present 25-ton guns are but carronades on a large scale, with a smashing rather than penetrating power, and have not even the proportionate length of the old 32-pounders of 56 cwts., which is about 19 diameters of bore in length. Following the same proportion, the 25-ton gun, with its 12-inch diameter bore, ought to be 19 feet in length, whereas it is little more than 14 feet" (p. 17). And again (p. 18),

"The tendency of modern gunnery is to give wide diameters of bore with low velocities to the shot—a practice the very reverse of what the penetration of iron-plating plainly requires. A velocity of 1600 feet in a second, on which the old Woolwich ranges were estimated, is now rarely if ever

* Proceedings of the Royal Artillery Institution, vol. vi., p. 283. On the Field Artillery of the Great Rebellion. By Lieut. Hime, R.A.

† A Description of Revolving Guns, Expanding Carriages, and Winged Shot. By Commander Sharpe, R.N. 1871.

gained; the 25-ton and other similar guns are *therefore relatively far less powerful than guns of twenty years back, possessing neither the relative range nor relative penetrating power of the old 32-pounder of* 56 *cwts.*"

Now, according to Captain Noble and Captain Stoney, gun-power is* properly estimated by calculating the dynamical force (*vis-viva,* or energy) of the projectile at various ranges, in foot-tons; whilst the comparative penetrative effect by dividing the *vis-viva* by the diameter of the shot; from this it is found that the 12-inch gun can deliver a blow at 1000 yards range twelve times as severe as the 68-pounder smooth-bore (95 cwts.), whilst as regards their relative power of perforation, the 12-inch gun is eight times as powerful as the 68-pounder.

By recent experiments it was found that our 10-inch guns of 18 tons penetrated 15 inches of iron (in three 5-inch plates), upon which the 15-inch Rodman smooth-bore American gun of 20 tons only made a shallow indent.

Captain Sharpe's expanding carriage is ingenious, but not to be compared with Captain Moncrieff's, whilst it seems doubtful if his proposed winged-shot are theoretically or practically possessed of the advantages claimed for them; indeed, the inventor would appear to be desirous of giving up the rifling principle and returning to the old smooth-bores, when he says, p. 23, "Striking the wings obliquely gives a rotary motion to the shot (*should such motion be required*)."

Our present service Palliser-shells at present meet nearly all practical requirements, and when fired with full battery charges at 200 yards range, are calculated to perforate iron plates of a thickness of 1 inch greater than their diameter.† A few facts about these shell may not be uninteresting; first, as to their shape, which is cylindro-conoidal. It has been found that for accurate shooting a length of at least two calibres is necessary, but of course this varies immensely; thus the double shell of a 7-inch gun is 27·2 inches, *i.e.*, all but four calibres; after many experiments as to the best form for the head of the projectile, it has been decided nearly conclusively that an ogival head struck with a radius of 1½ diameters is the best adapted both for flight and penetration. It has also been found that shot fly better when hollow than when solid, the weight when distributed further from the axis giving a longer radius of gyration.

* Royal Artillery Institution Paper, vol. vi., p. 116 *et seq.*

† See Captain Orde Browne's Treatise on Ammunition for Rifled Ordnance, p. 215.

These principles govern their form. Next as to their construction. The so-called chilled Palliser-shot or shell are cast of a peculiar iron in sand-moulds, with metal ends or chills for the heads ; the head of the projectile is thus chilled white and rendered intensely hard, fit for punching a hole in an iron plate, although at the same time it is so brittle that the tip or point is occasionally broken off by the impact of a shell rolled or struck obliquely against it ; for, strange as it may appear, the point which may penetrate directly through several inches of armour without injury may be fractured by a very slight transverse blow (Captain Orde Browne). On the other hand, the body of the shell (or rather sides and base, for it is cast with a core) being cast in sand, is partially annealed, and composed of an evenly mottled iron possessing more tenacity and toughness, which enables it to better withstand the shock of discharge. All these shells are cast with undercut holes, into which the rifling studs of a soft alloy (10 parts copper to 1 tin) are swedged.

Our iron-clad Navy and our plated Forts (when they are plated) need have little to fear as to the result of an artillery duel, whether they have to meet 1000-pounder Krupp's, or the 20-inch Beelzebubs and Puritans of the American turret-ships. The English public may assure itself what all foreign powers will be ready to admit, that we possess the most powerful ordnance and most unrivalled ammunition in the world, and that we owe our possession of them to the ability of our departmental officers. We owe our present construction of guns to Colonel Campbell and Sir W. Armstrong, with Messrs. Fraser and Anderson ; the carriages on which they are mounted and accessory machinery to Colonel Clerk and Captain Moncrieff ; our powder to Colonel Younghusband ; our projectiles and exquisite fuzes to Colonel Boxer and Mr. Abel ; our chilled shell to Major Palliser ; with many others, hard-workers and able coadjutors in every department. Woolwich is well up to, if not in advance of, the age, and her factories can produce, if necessary, 6000 tons' weight of wrought-iron cannon per annum. Already there is a talk of turning out larger guns, and the drawings of 800-pounders, and even 1000-pounders, are actually ready, and can be manufactured whenever the Secretary of State for War thinks fit to order them, so Captain Stoney informs us,* and, moreover, adds that the time necessary for their manufacture is at the rate of one week per inch of calibre ; for example, twelve weeks for a 12-inch gun.

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* Proceedings of Royal Artillery Institution, vol. vi., p. 432.

NOTICES OF BOOKS.

The Beginning: its When and its How. By MUNGO PONTON, F.R.S.E. London: Longmans, Green, and Co. 1871. Pp. 572.

THIS may fairly be described as an extraordinary work. Touching upon upwards of a thousand topics, laying under contribution almost every member of the circle of the sciences, obtaining aid from philological research, and entering, sometimes deeply, into the mazes of metaphysics, it induces upon the reader a feeling of almost perplexity, which is only overcome by the extreme interest which it inspires. The author divides his work into two parts. In the first part, the teachings of the Sciences are laboriously examined with the object of obtaining the most probable notions concerning the antiquity and the conditions of matter, the origin of suns and planetary systems, the luminiferous ether, the evolution of vitality, and the origination of genera and species. In the second part, these teachings of science are placed parallel with the biblical records; and the conclusion is drawn, that faith and science can go hand in hand; that true philosophy and religion, far from offering violence the one to the other are, when viewed in the true spirit of scientific enquiry, mutually corroborative.

After an ingenious argument, reducing to philosophical certainty the view that the luminiferous ether must be infinite in extent, the author, in regard to the question of the antiquity of matter, considers that the probabilities that matter existed from all eternity, and that it was created by the volition of an eternal mind, are nearly equally balanced. Obviously as the human mind can, from the very nature of things, form no conception of matter apart from force, every dictum concerning it abstractedly must be pure speculation; but the author considers that matter which existed in space was at first an "assemblage of substantial ultimates, each having definite size, definite form and impenetrability, but having no relative properties whatever, each ultimate being absolutely indifferent to every other ultimate in the universe." The first species of physical force was probably the endowment of these ultimates with mutual repulsion; hence the first conception of ether. The next step was the application of the divine energy to produce a vibratory motion upon the ultimates, those constituting the luminiferous ether and those which afterwards became ponderable being at first probably indistinguishable. Thus the same vibration which in the case of the imponderable ultimates constituted light, in the case of those subsequently endowed with gravity constituted heat. The laws of gravity could of necessity be alone the work of an infinitely intelligent and calculating mind. From these commencements followed the assemblage of the substantial ultimates into separate centres, and the evolution and co-ordination of the various forms of force. The earth, which was probably at its earliest stage gaseous, condensed somewhat suddenly into liquidity or viscidity, the temperature of space being probably very high. The cooling and the formation of the crust of the globe was probably very protracted, the atmosphere was constituted by the escape of vapours through fissures in the crust, volcanic action producing the irregularity of the earth's surface. After a rather short chapter on the moon, the author enters at length upon the question of the probable constitution of the sun, excluding the view that it is an incandescent mass or a body in a state of combustion. Its bright "faculæ" are not flames, but may be produced by the heaping up of (hypothetical) light-generators into masses of greater thickness than the general photosphere. The solar surface is acknowledged to be highly electric, but objections are raised to the view that the sun's light is due to electric discharges passing through highly rarefied and very dry gaseous media. The "meteoric theory" is met also by many objections. The force generated in each cubic foot of the solar photosphere would probably be equivalent to that sufficient to raise $5\frac{1}{2}$ lbs. a foot high in a second. This energy is probably not greater than that which many living beings are capable of displaying as mechanical force or as luminous or thermal vibrations. It is obvious that the author inclines to a belief in special solar light-generators, the nature of which is undiscovered. The author considers the primeval vegetation of the earth to have existed before the sun; it was probably destroyed to afford the pabulum which was necessary for subsequently appearing forms of animal life, centralisation of light in the sun intervening between the two epochs. The author illustrates his description of the low forms of organisation by some beautiful drawings of *Foraminifera*, *Polycystina*, *Diatoms*, the spicules of sponges, &c., his object being to show the marvellous evidences of superintending design afforded by the beauty of the productions of these organisms. After showing the appearances of living things to be not simultaneous but successive, the author enters upon the question of vital origination. The hypothesis of "spontaneous generation," or, as he terms it, "apparent organic origination," finds in him a strong opponent. The champions of this theory can scarcely be gratified with the manner in which their views have been received by the cultivators of science. M. Pouchet found that a majority of the *savans* of the Académie des Sciences condemned his conclusions. Dr. Bastian, who fights the battle in this country, found in Professors Huxley, Tyndall, and Frankland, prophets who blessed the enemy. And Mr. Ponton considers that all experiments fall short of proving the production of living organisms from dead organic materials in conjunction with the ordinary forms of physical force.

He commences with a wide and somewhat unsatisfactory definition of life as "an energy capable of influencing both the material ultimates and the physical forces." There is in man a conscious revelation that life consists in a somewhat distinct from the elements of his organism; this somewhat Mr. Ponton terms an *organiser*, which receives its organising faculty direct from the Creator. The organiser is characterised by four properties—indefinite extensibility, indefinite divisibility, penetrability, and the capability of binary combination; it cannot be material, but itself exercises a peculiar influence over matter; it is not itself a force, but it has a special power over the physical forces, in virtue of which it can compel the ultimates to perform motions altogether unlike those which they perform under the influence of the physical forces alone. The organisers, according to our experience, do not originate forces, but merely avail themselves of the forces already existing; but they are endowed, besides their faculty of organisation, with the psychical attributes of volition, instinct, consciousness, &c. The author devotes a chapter to "Protoplasm," and analyses the writings of Huxley, Stirling, and Beale. He strongly condemns the physical theory of vitality, which he interprets Professor Huxley to adopt, but is in very considerable accord with Professor Beale, who has devoted to the subject such laborious investigation.

Reverting to primordial creation, Mr. Ponton considers that the balance of evidence is vastly in favour of the view, that the earliest organisms were formed by a *gradual* process. Concerning their mode of evolution there are two hypotheses: the first enunciating that evolution of the various forms took place from a single organiser; the second, that during the creative epochs, the Deity established in succession a vast number of organisers. In considering the first hypothesis—that of Organic Derivation—the author adversely criticises Mr. Darwin's views. Whilst acknowledging the existence of certain difficulties, he leans to the second hypothesis—that of Specific Creation.

The second part of the work is of great interest. The author carefully analyses the expressions in the Scriptural account of the Creation in the original Hebrew, and places them side by side with the hypotheses deduced from scientific observation; the lessons derived from these mutual interpretations are in some instances startling, and, though in some they are in the nature of vague surmise, the author succeeds in impressing his reader with the conviction that the two records, observed and revealed, have very close mutual co-aptations. In every part of his work the author has placed himself *en rapport* with the actual state of science, and his observations will be read with interest by a large number of scientific and literary men.

The Manufacture of Russian Sheet-Iron. By JOHN PERCY, M.D., F.R.S., Lecturer on Metallurgy at the Royal School of Mines, and to the Advanced Class of Artillery Officers at the Royal Artillery Institution, Woolwich. London: John Murray. 1871.

THE method of manufacturing Russian sheet-iron has long been thought a process the details of which were supposed to be kept secret by the various firms interested in the production. But Dr. Percy has succeeded in presenting to the public a full account of the process afforded him by several eminent Russian engineers employed in the manufacture. This particular variety of sheet-iron differs from that produced elsewhere in being remarkable for its dark grey, polished surface, and its extreme ductility. It is extensively used in Russia for roofing, and in the United States, where it is known as stove-pipe iron, in the construction of locomotive engines and for stoves. The pig-iron employed in the manufacture results from the smelting of magnetite and the red and brown hæmatite ores with charcoal in a cold-blast furnace, the conversion of the pig-iron into malleable iron being effected either in the charcoal finery or by puddling. The malleable iron is rolled and beaten into bars and sheets, and is then subjected to a re-heating process to which the ductile nature and peculiar colour are due. Packages of three sheets are heated to redness, and immediately before rolling there is strewn between the sheets a quantity of powdered charcoal. The rolled sheets are sheared to size, and bound up in packets of about one hundred sheets each. These packets are re-heated in a peculiar furnace, powdered charcoal having again been placed between each sheet; the packets are then thoroughly hammered. When the sheets are cool, they can be delivered to the purchaser. Dr. Percy suggests that, if an attempt should be made to extend the manufacture to this country, it would not be necessary to imitate the Russian process in every particular; and that instead of the peculiar annealing furnace used in Russia, the method of annealing in covered cast-iron vessels pursued in tin-plate works might be advantageously adopted. Dr. Percy is to be highly commended for this careful compilation on so important a subject, hitherto a matter of much doubt and controversy; and his work should be consulted for the chemical investigations as to the nature of the iron, and for the description of the peculiar machinery employed in its manufacture.

Power in Motion: Horse Power, Wheel Gearing, Driving Bands, and Angular Forces. By JAMES ARMOUR, C.E. London: Lockwood and Co. 1871.

To meet the wants of practical men engaged in engineering works requiring the use of tackle, driving-bands, wheel gearing,

&c., these manuals by Mr. Armour are admirably adapted. This member of the series is fully worthy of its predecessors, both in the fulness of detail, and in the simplicity of the formulæ employed. Most of the questions are solved by simple arithmetic and by logarithms; in cases where trigonometrical relations have to be considered, as in the determinations of angular force, everything is so clearly explained that no difficulty can be said to exist. There can be no doubt that Mr. Armour's works will be much appreciated.

The Elements of Plane and Solid Geometry. By H. W. WATSON, M.A., Sometime Fellow of Trinity College, Cambridge; late Assistant Master of Harrow School. London: Longmans, Green, and Co. 1871. Small 8vo., 285 pp.

THIS little work is one of a series of text-books of science published by Messrs. Longmans, and intended for the use of artisans and of students in public and other schools. Mr. Watson has with liberal views exposed himself to criticism in endeavouring to sift from the books of Euclid only the propositions and so much of each demonstration as will be likely to benefit those for whom he writes. It needs, however, only a glance at his work to show that a new era has dawned upon elementary geometrical science, when men of Mr. Watson's mathematical standing acknowledge our elementary teaching to be at fault. Euclid's prolixity—so puzzling to the young geometrician—has in this work given way to an extended application of the principle of superposition. It is true that Euclid himself recognises the importance of this principle, but he employs it timidly, and, as in Prop. 5, Book 1, often puts himself out of the way to avoid its use where it would greatly assist the student, and would, indeed, be the more correct method. Another innovation on which Mr. Watson is to be complimented, is the arithmetical treatment of ratio and proportion, thus rendering the 5th Book of immense value in instruction. There is, under the present system, hardly a boy of a lower form who, although having passed through fractions, has an adequate idea of the properties of ratio and proportion, while even in published scientific works of an elementary character, the terms are often confounded. The work certainly deserves the earnest attention of all who wish to see geometry studied elementally on a truly logical method.

A Treatise upon Terrestrial Magnetism. Edinburgh and London: William Blackwood and Sons. 1871.

THE writer of this work, which is unsigned, first considers the present theories accounting for the phenomena of the earth's magnetism, and then expounds a new hypothesis. Assuming the earth an immense magnet, and inferring from his reasoning that the sun does not influence the earth by acting upon it simply as a magnet, he then considers the only other known way in which the sun can produce terrestrial magnetic effects—namely, by bringing into existence a current of electricity. It is then argued that, assuming there is a mutual attraction between electricity and matter, and that therefore the matter of the sun attracts the electricity of the earth and *vice versa*, there would be a greater constant electrical density existing on the surface of the earth immediately under the sun. This is, of course, allowing the assumption that electricity is more or less free to move among the particles of matter of which the earth is composed, and that in a body moving through strata of electricity of varying density, the same effects are produced as when a current is caused to flow round the body. Now, as the earth moves from west to east, there will, on these assumptions, be a current circling from east to west, inducing a magnetic polarity north and south. The magnetic poles would therefore be coincident with the poles of the ecliptic, and would revolve around the terrestrial poles in the same time. But this is not the case, and the difficulty is surmounted by supposing that the earth is analogous to a magnetised bar of hard iron, and that, consequently, the changes in the magnetism are correspondingly slower in proportion to the hardness of the ferruginous matter and the strength of the current. The secular, annual, and diurnal variations now become easy of illustration, because they may be referred to so many combined causes.

Of the relation of sun-spots to magnetic disturbances, the writer remarks :—" Of the nature of this connection we have at the present time no knowledge. It may be that this connection is not direct, but results from an intermediate agency—viz., that of the aurora; for there appears to be a like variation in the auroral displays, these being more frequent in proportion as the number of sun-spots are greater, and *vice versa*. The aurora, when present, always disturbs the magnetic needle; the greater or less amount of disturbance may, therefore, be dependent upon the greater or less frequency of auroral exhibitions."

The reasoning throughout is ingenious, but founded on so hypothetical a basis, that it is clearly impossible to draw conclusions. The arguments in favour of the theory, adduced in the shape of calculated charts as compared with recorded observations, are very voluminous and certainly striking. The book is well worth perusal as a very fair and clear statement of what has been observed and advanced with regard to the magnetism of the earth. The collection of charts, copies from those of Sir Edward Sabine, would alone render the work of value.

A Handbook of Practical Telegraphy. By R. S. Culley, Member of the Institute of Civil Engineers, Engineer-in-Chief to the Post Office Telegraphs. Fifth Edition. London: Longmans and Co.

That another edition of Mr. Culley's work should be so soon on hand shows that it is in increasing demand as a professional text-book. This edition far surpasses the preceding in completeness. Much that before was left unexplained to the student is now considerably elaborated, and there has been a valuable addition of plates and descriptive matter relating to the connections of Sir C. Wheatstone's automatic system. Professor Hughes's type-printing telegraph is also more fully detailed. There is introduced an explanation of Sir W. Thompson's new syphon-recorder; and much necessary detail has been added on the subject of relay and translating systems. A relay is, perhaps, the instrument presenting the greatest difficulty to the learner, because the most complicated on an ordinary telegraph line. In submarine work especially, much has been done for the reader; the formulæ have been re-arranged, engravings introduced showing the connections of the Atlantic and other long cables, and secondary and cable currents very tersely explained. There are also many data and general remarks as to the management of circuits in this edition that will be valuable to the working engineer. It must be remembered that Mr. Culley has to write with a double purpose—to convey information to learners, and to provide a handbook for the gentlemen of his staff; and certainly he is to be congratulated on the happy result attending his endeavours. There is no doubt that the edition now submitted to the public is the most practical handbook in the profession; it is devoid of the unnecessary mathematical theorising that has unluckily beset telegraphy generally. The book will be very acceptable to those now studying for contingent appointments in the Indian Telegraphs, especially as it is the recognised text-book of the department.

A Complete Course of Problems in Practical Plane Geometry; adapted for the use of Students preparing for the Examinations conducted by the Science and Art Department. By John William Palliser, Second Master, and Lecturer on Geometrical Drawing at the Leeds School of Art and Science, Mech. Inst., &c. London: Simpkin, Marshall, and Co. 1871.

This is a capital little work, giving the construction of the geometrical figures plainly and systematically. But there is one great fault—it has been too hastily edited, and numerous typographical errors have been allowed to pass; thus, the *radical sign of square root* in the definition is said to signify that the expression to which it relates should be *squared*. These minor errors greatly deteriorate a work that would otherwise be of the highest utility, not only to the student, but in every office where drawing has to be done.

Switches and Crossings. Formulæ for ascertaining the Angles of Crossings, the Lengths of Switches, and the Distances of the Points of the Crossings and Heels of the Switches from the Springing of the Curve. By WILLIAM DONALDSON, M.A., A.I.C.E., Author of a Treatise on Oblique Arches. London: E. and F. N. Spon. 1871.

IF plate layers, or even the foremen, for whom Mr. Donaldson professes to write, have to study works of this character, one can hardly wonder that something should go wrong somewhere. From beginning to end the book is a mass of inexplicable formulæ —inexplicable because ordinary mathematical expressions are made subservient to other renderings. Having his symbols to construct, surely Mr. Donaldson could have devised something better than the employment of the ordinary signs of the powers and the series. Formulæ for practical application should be so written that a mathematician of tolerable acquirements can read them at sight; but here one is perplexed by the subversion of conventional symbols, and has continually to turn to the first chapter for explanation. But, this notwithstanding, every credit must be given for the patient labour entailed by so much calculation. The idea is good, and all cases of junctions are discussed; but is all this labyrinth of signs and letters necessary? If so, there can never be a dearth of mathematicians as long as a platelayer is to be found.

Elementary Treatise on Natural Philosophy. By A. PRIVAT DESCHANEL, formerly Professor of Physics in the Lycée Louis-le-Grand; Translated and Edited, with extensive additions, by J. D. EVERETT, M.A., D.C.L., Professor of Natural Philosophy in the Queen's College, Belfast. In four parts: Part II. London: Blackie and Son. 1871. Medium 8vo.

THIS is the second volume of Professor Everett's translation of M. Deschanel's "Traité Elementaire de Physique," and is exclusively devoted to the consideration of Heat. Of the admirable character of the work we have already had occasion to speak when noticing Part I.; and this character is fully maintained. The subject is considered under the heads of Thermometry, Expansion of Solids, Liquids, and Gases, Fusion and Solidification, Evaporation and Condensation, Ebullition, &c. A very clearly written chapter on Thermo-dynamics, embodying the most recent experiments, the editor claims for himself. Professor Foster's nomenclature of units of heat, the gramme-degree, pound-degree, &c., has been adopted. Comparing the work with the French original, it is easy to see that great care has been bestowed upon its production, and that much fresh matter has been introduced.

The Technical Educator; An Encyclopœdia of Technical Education. Volume I. Cassell, Petter, and Galpin: London and New York.

THIS work is a practical sequel to the theoretical lessons contained in the "Popular Educator," published by the same firm. The main subject is Drawing, with its applications to Design and Ornamentation; but Civil and Military Engineering have a great deal of space devoted to them; while under a third head may be considered Practical Chemistry as applied to Manufactures and Agriculture. The several papers are written in a clear, concise manner, admirably adapted to convey the amount of information required by the artizan and mechanic for whom the pages are intended. Too much praise cannot be afforded to Messrs. Cassell for their many and successful attempts to place sound elementary knowledge within the reach of the working classes.

Annual of Scientific Discovery; or, Year-Book of Facts in Science and Art, for 1871. Edited by John TROWBRIDGE, B. Sc., Asst. Prof. of Physics in Harvard College; aided by W. R. NICHOLS, Asst. Prof. of Chemistry in Mass. Inst. of Technology; and C. R. CROSS, Graduate of the Institute. Boston: Gould and Lincoln. London: Trübner and Co. 349 pp.

THIS is a *resumé*, chiefly from the various periodicals, of the most important discoveries and improvements in science during the year 1870, including a list of recent scientific publications, and obituaries of eminent scientific men. As a reference for dates and main principles the work will doubtless be useful.

Astronomy Simplified for General Reading. By J. A. S. ROLLWYN. London: William Tegg.

THE general reader who is unlucky enough to take up this book with any amount of faith in its contents would be led to imagine that the most exact science was a myth. Mr. Rollwyn, who lives in a glass-house hypothesis of his own, is certainly to be pitied for throwing stones. When an author publishes a book intended to overthrow received scientific opinions, it is certainly, to say the least, unfair to our great scientific teachers, to dedicate his work to their implicit pupils, the vast mass of general readers. *Palmam qui meruit ferat.* First, then, why does not the author of this very mediæval work bring some of the discoveries in Spectrum Analysis, promised on the title-page, to bear upon his subject before he undertakes to render null the labour of those intellects who have annihilated space in the investigation of the planets. Bitter experience will teach Mr. Rollwyn that fact is to be met only by fact. But the hardest knock is left to the final chapter, where in a few lines we are treated to an easy method of squaring the circle. Again, does Mr. Rollwyn know that there is a form of argument commonly called begging the question. It is almost incalculable the amount of harm this work may do in the hands of those to whom it is addressed. With the exception that it is well got up, there is nothing to be said in its favour.

Transactions of the Woolhope Naturalists' Field Club for 1870. Hereford: The "Times" Office. 1871.

A MORE pleasant series of papers it would be difficult to find. Carefully edited, they are interesting, and in some instances valuable, descriptions of the natural history of places at which the members have met, the contributions on Fungi being well worthy of notice. The illustrations are good, especially the series of photographs of the remarkable trees of Herefordshire.

The Technical History of Commerce; or, Skilled Labour Applied to Production. By JOHN YEATS, LL.D., F.G.S., F.R.G.S., &c., assisted by several scientific gentlemen. London: Cassell, Petter, and Galpin. 1871. 431 pp. 8vo.

DR. YEATS, in bringing the second of his three volumes on Commerce before the public, expresses the hope that, as a sketch of Technical History, it will be found useful in assisting to direct the subject-matter of education more to the occupations of the people. And in this particular the work decidedly supplies the want of a manual clearly stating the progress of the various branches of industry, and to whom and to what we owe their origin. A quotation will show the neatness of style and arrangement. When treating of food-stuffs, Dr. Yeats says:—"Notwithstanding its great apparent variety, food, in its ultimate analysis, falls into four classes, viz.—proteids, fats, amyloids, and minerals. The proteids are compounds of carbon, hydrogen, oxygen, and nitrogen, and include such food-stuffs as the albumen of white of egg, the syntonin of muscle and flesh, the casein of milk, &c. The proteids subserve the process of the formation of tissue as their special function, though they share with the next two classes in maintaining the heat of the body. Fats contain but three of the four elementary principles mentioned above, nitrogen being absent. Their amount of hydrogen is more than sufficient to form water if combined with the oxygen of the compound. In this class are included all oils, fat of meat, &c. Amyloids resemble fats in composition, except that the amount of hydrogen is but just sufficient to form water with the contained oxygen. This class includes sugar, starch, dextrine, &c. Fats and amyloids perform the function of maintaining animal heat. The fourth class includes such substances as sulphur, phosphorus, lime, and the salts of various alkalies and earths. One of the chief functions of this class of food-principles is to afford strength to the more solid parts of the animal frame." More technical subjects are treated in a similar manner: the style is neither too difficult to be readily understood, nor so simple as to pall upon the reader. Dr. Yeats may be considered to have opened up a fresh path for our high-class school-book writers.

Handbook of British Fungi. By M. C. Cooke, M.A. 980 pp., 6 plates, 408 figures. Macmillan. 1871.

This long-expected work by one of our most industrious mycologists has at last made its appearance. It contains descriptions of all known species of British Fungi, amounting to the enormous number of 2809. Each genus is illustrated by a carefully executed woodcut. The student is aided in the discrimination of the genera of the *Agaricini* by a series of coloured tables. It is much to be regretted that the author has been unable to supply an introductory chapter on the structure, affinities, &c. of the Fungi, and in particular on the terminology employed; the work has, however, so far exceeded the limits originally intended that it has been found impossible to give such explanations however desirable. The author hopes to publish an introduction at some future time, and also occasional supplements. The work supplies a want felt by every mycological student, there having been no collected description of British Fungi since that by the Rev. M. J. Berkeley, M.A., in 1836; all information since that time had to be searched for in the pages of numerous British and foreign periodicals. The thanks of every lover of botanical science are due to the author for his valuable work, which though of necessity a compilation is not wanting in original matter.

PROGRESS IN SCIENCE.

MINING.

THE Royal Commission appointed some five years ago to inquire into the probable duration of our supply of coal has recently issued its long-expected Report. An enormous mass of information has been collected by the several committees into which the Commission was resolved, and after much pains-taking investigation, some important general conclusions have been attained.

In attempting to fix a limit to the depth at which coal may be profitably worked, it has been found that the chief difficulty arises from the increased temperature consequent upon increased depth. With a view to determine the maximum temperature compatible with the healthful prosecution of mining work, some experiments were made by Dr. J. Burdon Sanderson in a Cornish mine, where the air was heated by a hot spring of 114½° F. It appears that the temperature at which work can conveniently be prosecuted depends greatly on the hygrometric state of the surrounding air. At the working face of the coal, air is always more or less humid, but the deepest collieries appear to be the driest. On the whole, the Commissioners conclude that a depth of 4000 feet may be fairly attained.

Another branch of inquiry related to waste in working coal. It appears that under a favourable system of working the loss is only about 10 per cent, but that in a large number of cases it amounts to as much as 40 per cent.

The estimation of the quantity of available coal in the known coal-fields of the British Isles was one of the main objects of the investigation. Taking 4000 feet as the maximum depth to which workings may be extended, and excluding all seams less than one foot in thickness, it is estimated that there exists in the several coal-fields of these islands, upwards of 90,207 millions of statute tons of coal. In addition, however, to this amount, it is well known to geologists that in certain districts vast tracts of coal lie hidden beneath the Permian, New Red, and more recent strata. Taking into account only such coal as will be found within 4000 feet of the surface, and deducting 40 per cent for loss and other contingencies, the Commission estimates that not less than 56,273 millions of tons of coal exist below these post-carboniferous rocks. Adding this quantity to that known to exist in our coal-fields, we obtain a grand aggregate of 146,480 millions of tons as the amount of available coal in the British Isles. It remains to consider how long this supply is likely to last. Assuming that our present rate of consumption—115 millions of tons per annum—remains constant, this amount of coal will not be exhausted for 1273 years. But such an assumption is obviously erroneous. Let the rate of production increase at the rate predicted by Professor Jevons, and this quantity will be exhausted 110 years hence. It is shown, however, by Mr. Price Williams, that although the population of the United Kingdom rapidly increases, yet the rate of increase is diminishing. Introducing this correction into Jevon's estimate, it is shown that the supply will hold out for 360 years. If instead of taking a geometrical increase in the rate of consumption, an arithmetical increase be calculated by adding 3 millions of tons each year, it is found that the supply will be exhausted in 276 years.

Another interesting question discussed in this report is the probable extension of the coal measures beneath the newer rocks of the South of England. Mr. Prestwich, following Mr. Godwin-Austen, argues in favour of this extension, but his views are strongly opposed by Sir Roderick Murchison.

The prosperity of some of our colonies is so closely connected with the development of their mineral wealth that colonial statistics of mining are always of interest. From Mr. Brough Smyth's "Mineral Statistics of Victoria for 1870" we learn that the quantity of gold exported from the colony in that year amounted to 1,222,798 ozs., being a decrease of 118,040 ozs. on the amount exported during the previous year. This diminution is certainly in part referable to the damage done by the heavy floods of 1870. The quantity of gold purchased by bank managers and gold buyers on the fields last year amounted to 718,727 ozs. from alluvial workings, and 585,576 ozs. from quartz veins. The total number of miners employed in working gold on the 31st December, 1870, was 60,365, and the average earnings per man for the year 1870 was £81 os. 6d. From the first date of the discovery of the gold fields of Victoria up to the end of last year, the quantity of gold exported amounted to not less than 39,399,328 ozs. 6 dwts. Reckoning this quantity at £4 per oz., its value reaches £157,597,313. In addition to this, the quantity used in the colony should be added, but it is of course impossible to estimate this amount with anything approaching to accuracy.

Nearly thirty years' residence in Cumberland has given Mr. T. Ainsworth an opportunity of collecting "Facts developed by the Working of Hæmatite Ores in the Ulverstone and Whitehaven Districts from 1844 to 1871." These observations were presented to the British Association at the recent meeting. The author asserted that the hæmatite was not confined to the limestone, but might be found in other kinds of rock, and even between two different strata. He maintained that the distribution of the hæmatite bore some relation to the coal fields, and that the direction of the ore-deposits was tolerably constant.

At the same meeting a paper was read by Mr. J. Sinclair Holden on "The Aluminous Iron Ores of County Antrim." These ores have of late years acquired considerable economic importance, and are now extensively used for admixture with the siliceous hæmatites in order to flux the silica and produce an easy-flowing slag. Mr. Holden described the beds as intercalated among the basaltic rocks, dipping generally to the south-west, and being traceable along the coast for about seventy miles. A notice of these aluminous ores was presented some little time back to the Geological Society of London, by Mr. Ralph Tate and Dr. Holden.

At the annual meeting of the Miners' Association of Cornwall and Devon, Mr. Robert Blee read a paper on "The Comparative Health and Longevity of Cornish Miners." It appears that the rate of mortality among children of miners does not differ from that of other classes. Indeed, in the parishes of Camborne and Gwennap, the proportion of deaths among the children of miners was less than among other children, whilst in Redruth and Illogan it was somewhat greater. It was found that between the ages of 10 and 30 years, 28 per cent of miners died, as against 18 per cent of men who were not miners. Between 40 and 60 years of age, 36 per cent of miners died, and only 20 per cent of non-miners. 9·07 per cent of miners lived to the age of 70, whilst 31·06 per cent of men following other occupations reached the same age. Pulmonary disease was the common cause of death, and the means recommended for preventing its ravages were efficient ventilation of the mine, improved means of ascent, and protection from sudden changes of temperature.

The health of the Cornish miner also formed the subject of an address by Dr. Barham to the British Medical Association at their recent meeting at Plymouth. He contrasted the health of the miner in the West of England with that of the miners in Northumberland, Durham, and Staffordshire. In Cornwall deaths from consumption are immensely in excess of those in other districts, whilst the proportion of accidents is much less. The prevalence of consumption was referred to insufficiency of light and air, to exposure, and to liability to inflammatory affections.

A safety-lamp having for its especial object an increased power of illumination has been devised by Mr. R. Brown, of the Shotts Iron Company, Glasgow. A simple plano-convex lens is set in the lower part of the wire-gauze cage, and surrounded by a conical shell of tin plate, which serves at once to reflect the light and prevent breakage of the lens. The light is also increased by a reflector behind the flame. Further protection is afforded by the use of segmental shields, one of which is fixed behind the reflector, whilst two others are movable, and can be made to slide to a greater or less extent round the lamp.

METALLURGY.

Whilst it is well known that the Russians have long manufactured a thin form of sheet-iron having a beautifully smooth and black surface, the details of the manufacture have not generally been made public. Dr. Percy has recently collected several descriptions of the process, and has published a pamphlet upon the subject.*

The iron works at which this form of iron is produced are situated on the eastern slope of the Ural mountains. The furnaces are fed with magnetite, red and brown hæmatite, and carbonate of iron; these ores being smelted with charcoal. The puddled bars having been cut into certain lengths are heated to redness, and rolled into square sheets. These plates are cleaned with a wet broom of green fir leaves, powdered charcoal is spread between the sheets, and the plates, having been made up into packets of threes, are several times re-heated to redness and passed through the rolls. The sheets are then cut to an uniform size, and brushed over with a mixture of birch-charcoal powder and water. They are then arranged in packets of from 70 to 100 sheets each, placed in a re-heating chamber of peculiar construction, and the temperature slowly raised for several hours, whilst oxidation is prevented. The packets having been sufficiently heated are removed and placed under a tilt-hammer. Finished sheets are inserted alternately between the hammered plates, and the packets are then subjected to a second hammering, which removes the wavy appearance resulting from the previous hammering, and produces a smooth surface. The packets being then opened, the sheets are cleaned with a wet broom, and when cool are cut to the standard size, when they are ready for the market. The only secret of the operation by which the beautiful surface is produced, appears to consist in the use of charcoal powder.

At a recent meeting of the Institute of Mechanical Engineers at Middlesbro'-on-Tees, Mr. I. Lowthian Bell read an able paper, "On the Preliminary Treatment of the Materials used in the Manufacture of Pig-iron in the Cleveland District." The ore used contains 31 per cent of iron, chiefly as carbonate of protoxide, with 28 per cent of earthy substances. When the ore is roasted in the mine-kiln the water is driven off, a part of the sulphur is expelled, and, if properly performed, the carbonic anhydride is also expelled, whilst peroxide of iron is produced. Two modifications of this process have been suggested: one is to get rid not only of carbonic anhydride and water, but also of the oxygen of the ore before its introduction into the blast-furnace; and the other is to charge the ironstone just as it comes from the mine. Mr. Bell admitted that the application of any method which would remove from the blast-furnace every trace of carbonic anhydride would cause the process to be less dependent upon the nature of the sources of heat than it is at present; but he considered that the additional cost of deoxidising the ironstone would be too great to render its adoption profitable. The author also noticed some improvements recently attempted in the manufacture of coke, the object being to increase the percentage of fixed carbon obtained from a given quantity of coal. The experiments had not, however, given successful results.

At the same meeting Mr. W. Crossley read a paper "On the Manufacture of Iron in the Hæmatite District." In describing the blast-furnaces used in Furness, he stated that the bell-and-hopper arrangement used for taking off waste gases was not extensively employed in that district, as it was commonly supposed to act prejudicially on the quality of the iron, and to throw a heavier back pressure on the furnace—objections which the author does not hold to be valid. Two kinds of hæmatite are used, one being hard and dry, and employed for producing pig-iron; whilst the other is soft and damp, and is used for fettling. As the ore is rich in silica and destitute of alumina, it is thought desirable to mix with it some of the Irish aluminous ores in order to produce a good flowing slag. In smelting the ore, it must not be charged

* The Manufacture of Russian Sheet-Iron. By John Percy, M.D., F.R.S.; London: Murray. 1871.

in too large pieces, and must be properly distributed through the furnace, whilst from time to time the contents of the furnace require to be gauged.

We learn from the "Mining Journal," that some little stir has been excited by the recent discovery in Low Furness of an enormous deposit of hæmatite, estimated by competent authorities at not less than one million tons.

At the Round Oak Ironworks, belonging to the Earl of Dudley, Howatson's puddling and heating furnaces have lately been tested with excellent results. The peculiarity of these furnaces consists in supplying hot instead of cold air for the combustion of the fuel, whereby a great saving of coal and iron is said to be effected, whilst the working of the furnace is improved and quickened. In the heating-furnaces the cold air is caused to enter a flue or heating chamber surrounding the base of the stack. Having become heated by contact with the sides of this flue, the air traverses a series of horizontal flues, parallel to each other under the bed of the furnace, whence it passes to the ash-hole beneath the fire-grate, and thence through the fire-bars to the fuel. To ensure perfect combustion of the gases from the fuel and to prevent smoke, a vertical flue is made in the side walls of the fire-grate, and is connected with a horizontal flue furnished with perforations above the bars, which admit and distribute the heated air over the fire.

In the puddling-furnace the cold air is first admitted under the bed of the furnace, which it helps to cool, and then proceeds to a flue surrounding the base of the slack, whence it passes along the sides of the furnace to the end, and finally descends to the ash-pit.

At the time we write, the Iron and Steel Institute is holding a meeting at Dudley, under the presidency of Mr. H. Bessemer. Some interesting papers on metallurgical subjects are before the Institute, and will be duly noticed in this journal.

MINERALOGY.

So rarely has the diamond been found in a veritable matrix that considerable interest attaches to any fresh instance of its occurrence *in sitû.* According to Professor P. von Jeremejew,* of the Mining Institute of St. Petersburg, microscopic crystals of diamond occur as enclosures in the mineral called xanthophyllite. Specimens of this mineral from the Schischimskian mountains, in the Slatoust mining district in the Urals, have been found to enclose crystals which present the form of the hexakistetrahedron, combined with a slightly developed tetrahedron—the faces of the first form being distinctly curved, whilst those of the latter are perfectly plane. The greater number of these crystals are colourless and transparent, but some few are tinted brown. Those enclosures are most abundant in the greenish plates of xanthophyllite, which occur in the neighbourhood of nodular aggregates of talcose schist and serpentine; moreover, these two rocks also enclose similar microscopic crystals.

From the recent researches of Professor Zepharovich, it appears that two distinct minerals have hitherto been confounded under the common name of *Freieslebenite.** It is well known that much difference of opinion has prevailed respecting the crystalline system to which this species should be referred—some authorities placing it in the rhombic, others in the monoclinic, and others again in the triclinic system. Zepharovich now shows that the native compound, $Ag_4Pb_3Sb_4S_{11}$, is dimorphic, and forms two distinct species, which differ not only in crystalline form, but also in specific gravity. One of these minerals assumes monoclinic shapes, and has a density of 6·35; whilst the other crystallises in the rhombic system, and has a density of only 5·9. For the former species the name Freieslebenite is to be retained, whilst for the latter Zepharovich proposes the name *Diaphorite*, from *διαφορα*, "a difference." The mineral which has formed so large a part of the ore raised at the silver-mine of Hiendelaencina, in Spain, belongs to the species Freieslebenite, which is found also at Freiberg, in Saxony. Diaphorite occurs at Przibram, in Bohemia, and at Bräunsdorf, near Freiberg.

* Jahrbuch für Mineralogie, &c., 1871, Heft 3, p. 275.
† *Ibid.*, p. 277.

Much light appears to be thrown upon the probable origin of certain native metals by some researches by Mr. W. Skey, of the Geological Survey of New Zealand.* These experiments show the powerful reducing action which ordinary metallic sulphides exert upon most salts of gold, silver, and platinum. For example, a single grain of iron pyrites was found competent to reduce $8\frac{1}{6}$ grains of gold. The author concludes that although organic matter, as commonly supposed, has probably played some part in effecting the reduction of certain native metals, yet most gold and silver deposits—especially those deposits which occur in the deeper-seated rocks—owe their origin entirely to the deoxidising effects of metallic sulphides upon solutions of salts of the precious metals.

The mineralogist not less than the physicist is interested in the elaborate researches on the pyro-electric properties of the topaz which have been undertaken by Herr W. Hankel. No fewer than sixty-four crystals from Saxony, Siberia, Brazils, and Asia Minor have been subjected to examination. The frequent association of pyro-electric properties with hemimorphic forms of crystal led Haüy to suppose that the topaz, like tourmaline, must be hemimorphic. Such is not, however, the case, and Hankel's studies point to the general conclusion that pyro-electricity is by no means necessarily correlated with hemimorphism, but appears to be a general property of all crystals. At the same time it is found that the distribution of electricity in a crystal varies according as it is hemimorphic or not. In hemimorphic crystals the opposite extremities of the principal axis are crystallographically dissimilar, and hence assume opposite polar conditions; whilst in crystals which are not hemimorphic the two ends are physically similar, and consequently exhibit the same polar condition. In the latter case the distribution of electricity depends in great measure on the external form of the crystal, and may be modified by altering its shape; but in hemimorphic crystals the distribution appears to depend essentially on the asymmetry of the molecules, and therefore suffers no alteration by any change in the external form of the crystal.

Another physico-mineralogical paper claims a brief notice here. Professor Dove has examined the behaviour of different kinds of native silica when freely suspended in the magnetic field. Sections were cut from pure and colourless rock-crystal, from smoky quartz, and from agates composed of alternating layers of chalcedony, jasper, amethyst, and other varieties of quartz. The sections were successively suspended between the poles of a powerful electro-magnet, and their deportment completely established the *diamagnetic* character of silica in all its varieties.

Professor How, of Windsor, Nova Scotia, continues his researches on the mineralogy of this part of the Canadian Dominion, and has published the description of another new borate found in the gypsum quarries of Winkworth, in Hants County. *Winkworthite*, as he terms the new species, is found in nodular masses, more or less crystalline on the exterior, and breaking with a flat fracture which presents irregular glistening facets. Scrapings, viewed under the microscope, appear as transparent oblique-angled plates. Two specimens yielded slightly different results; the one containing $11CaO.SiO_2.9SO_3.3BO_3.20HO$, and the other $11CaO.SiO_2.8SO_3.4BO_3.20HO$. Winkworthite is consequently intermediate in composition between selenite and Howlite, or silico-borocalcite, and may have originated from the reaction of the elements of these two bodies, either during deposition or subsequently.

Under the name of *Cæruleolactine*, Petersen describes a new species of hydric-aluminium phosphate found in the mine of Rindsberg, near Katzenellnbogen, in Nassau. The mineral occurs in a deposit of brown iron ore, which, however, is not worked in the neighbourhood of the cæruleolactine in consequence of the presence of phosphorus. The new species presents a bluish milk-white colour, whence the name, and appears to stand between Wavellite and Kalaite—approaching to the former in its chemical, and to the latter in its physical characters. It is instructive to compare the formulæ of these three species:—

* Chemical News, vol. xxiii., p. 232.

Kalaite .. .. .. $2Al_2O_3.P_2O_5+5H_2O$.
Cæruleolactine .. $3Al_2O_3.2P_2O_5+10H_2O$.
Wavellite .. .. $3Al_2O_3.2P_2O_5+12H_2O$.

Dr. Petersen has also analysed the mineral described by Breithaupt as *Variscite*, and finds it to contain $Al_2O_3.P_2O_5+4H_2O$. It appears to be closely related to Damour's *Callais*—a material which is found, worked into ornamental objects, in the old Celtic graves of Brittany.

The New Zealand *nephrite* or *jade*—a substance so largely used by the Maories for purposes of ornament—has frequently been subjected to scientific examination, but often with discordant results. Dr. Kenngott has availed himself of some specimens recently obtained from a large block sent to Germany, and his microscopic and chemical examination leads him to conclude that the different forms of New Zealand jade are micro-crystalline or imperfectly-slaty varieties of grammatite.

A new fluoride from Arksutfiord, in Greenland—the celebrated locality for cryolite—has been described by Professor G. J. Brush under the name of *Ralstonite*. It is essentially a hydric aluminium fluoride, crystallising in colourless or white octahedra, and somewhat resembling in its general characters the rare Cornish mineral—fluellite.

Monzonite is the name which Von Kobell has applied to a new mineral from Mount Monzoni, in the Fassa Valley, Tyrol. It occurs in compact masses, of a pale green colour, somewhat resembling a green hornstone, but is readily fusible. Analysis shows it to be a new silicate of alumina, protoxide of iron, lime, magnesia, soda, &c., referable to the formula $2(3RO.2SiO_3)+2Al_2O_3.3SiO_3$.

At the celebrated salt-mines near Stassfurt, in Prussian Saxony, crystals of *boracite* have recently been found in tetrahedral forms.

An analysis of *allophane* from the oolites of Northampton has been published by Mr. Herman in the "Quarterly Journal of the Geological Society." Mr. D. Forbes in examining this mineral has detected phosphoric acid.

Professor Rammelsberg publishes an analysis of the meteoric stone of Chantonny; together with short memoirs on the sulphide of iron occurring in meteorites, and on the composition of Lievrite.

Papers by Professor Maskelyne on Dufrenite and a new mineral from Cornwall, and on the localities of Dioptase, were read before the Chemical Section of the British Association. In compliment to Professor Andrews, the President of the Section, the name of *Andrewsite* has been bestowed upon the new Cornish species. It occurs in globular forms, or in discs with a radiate structure, and presents a bluish-green colour. Its sp. gr. is 3·475, and its composition may be thus formulated:—$3\{Fe_2P_2O_8+Fe_2H_6O_6\}+CuP_2O_8$. As the only locality hitherto recorded for *Dioptase* has been the copper mine of Altyn-Tubeh, in the Kirghese Steppes, it is interesting to learn that Professor Maskelyne has found this rare species in old specimens from several localities in Chili.*

ENGINEERING—CIVIL AND MECHANICAL.

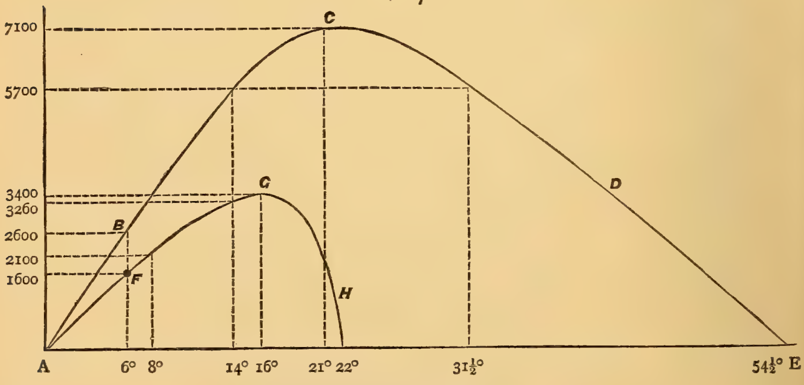
Guns and Armour.—Since the first introduction of heavy iron plating for the protection of our ships and forts, a continuous rivalry has been going on between guns and armour-plating. The experiments undertaken when a new gun was introduced, or a new kind of armour-plating invented, resulting at one time in favour of the former, and at another time in favour of the latter. At present guns have achieved the last victory over a new target designed by the War Office authorities. It is uncertain, however, at present, whether this success is to be attributed solely to the superiority of the guns and projectiles, or to the inferiority of the target; and referring to the great success achieved by the Milwall shield, in July, 1868—in which the armour-plates were backed with Mr. Hughes's 7-inch hollow stringers, placed horizontally, whilst the rear skin-plates were strengthened by hollow stringers placed vertically—it is just possible that the present victory of the guns may be due more to the inferiority of the target than to their own merits. This question may,

* Chemical News, vol. xxiv., p. 99.

however, shortly receive a practical solution, as we understand that Mr. Hughes intends to send another and a still better shield to Shoeburyness for trial. The experiments with the new War Office target took place at Shoeburyness last July. The target measured 48 feet long by 9 feet high, and although built up in one, it really represents two systems of targets. In one the armour-plating is 8 inches thick, with a backing of teak 18 inches thick, and a ¾-inch iron skin with iron ribs in the rear. In the other the front plate is 8 inches thick, backed by 5½ inches of teak, behind which is a 5-inch armour-plate backed with 6 inches of teak and a 1½-inch iron skin. The guns brought to bear upon this compound target were the 9-inch Woolwich muzzle-loading rifled 250-pounder, and the 11-inch Woolwich muzzle-loader rifled 500-pounder gun. The 9-inch gun was directed against the 8-inch armoured portion, and the 11-inch was laid against the target carrying the 13 inches of divided armour, the ranges being in both cases 200 yards. Both guns penetrated the targets, the 11-inch weapon doing exceptionally good work in sending its projectiles through the two armour-plates and backing of the second target. Both plates were cleanly penetrated, the puncture disclosing the fact that the metal was of splendid quality. The second part of the programme for the day consisted of comparative trials with the nominal 4-pounder breech-loading Prussian rifled field-gun, fired with 9 lb. cylindrical projectiles, against the English muzzle-loading 9-pounder and 16-pounder field-guns. The general results of this practice were that the English 16-pounder fired 25 rounds in 13 minutes 30 seconds, making 14 hits upon the target; the English 9-pounder fired the same number of rounds in 8 minutes 37 seconds, scoring 13 hits, whilst the Prussian field-gun fired a like number of rounds in 10 minutes 15 seconds, making 13 hits. Competitive practice was then carried out with the three guns against targets representing troops with good results.

Stability of Ships.—This subject has engaged a considerable amount of consideration since the unfortunate loss of the *Captain*. Amongst other contributions towards a full investigation of this branch of scientific research, we notice a "Lecture on Sea Waves," by W. J. Macquorn Rankine, C.E., LL.D., F.R.S., delivered before the Royal Institution, which we referred to in the July

FIG. 17.



A B C D, official curve of stability.
F, point fixed by inclining experiment.
F G H, probable continuation of curve.

number of this Journal, and a paper on "The Stability of Ships," by W. H. White and W. John, Fellows and late Students of the Royal School of Naval Architecture, and Members of the Institution of Naval Architects, recently read before that Institution. Mr. M. W. Ruthven, C.E., in a letter recently addressed to some weekly contemporaries, remarks that the *Captain* was evidently lost for want of stability, while the official curve of stability was such that, if the ship had really possessed it, no pressure of wind upon any extent of sail she could possibly have carried could have forced her over. Upon further reflection, Mr. Ruthven found the official curve of stability to be grossly incorrect, and not even agreeing with the inclining experiment which had been made at Portsmouth in order to ascertain the position of the centre of gravity. The inclining experiment, as stated in evidence, proved that 80 tons placed on the deck at 20 feet from the centre line, heeled the ship 6 degrees, and this, of course, was the measure of her stability at 6 degrees. This 80 tons multiplied by the 20 feet give 1600 foot-tons as her stability at 6 degrees; while the curve produced in court showed a stability, at 6 degrees, of no less than 2600 foot-tons. The official curve gave 7100 foot-tons as the maximum stability at 21½ degrees, with a stability of 5700 foot-tons at 14 degrees, which was retained up to an angle of 31 degrees; while, in point of fact, the highest real stability of the *Captain* was only about 3400 foot-tons at 16 degrees, and 3260 at 14 degrees, which she carried only up to 18 degrees. The accompanying diagram shows the difference between the assumed and the real curve of stability of the *Captain*, as explained by Mr. Ruthven.

Stone Caissons.—A new application of Ransome's patent concrete stone has recently been introduced by a Mr. Butler, which seems likely to come into extensive use, and to effect a great revolution in the construction of hydraulic works. The object of this invention is to employ cast-stone caissons in the construction of foundations for piers, bridges, river-walls, and all kinds of hydraulic works. The rapidity and ease with which blocks of any form and size can be produced on the spot where they are to be employed, are no small considerations in favour of this new application of Ransome's stone, whilst the materials which form its base are generally found in abundance where hydraulic works are carried on. This application of Mr. Ransome's process has been suggested for two reasons—the first, to provide a cheap and thoroughly efficient substitute for stone for hydraulic works; and, secondly, to render unnecessary the construction of false works, coffer-dams, &c., and to avoid the employment of iron-cylinders and caissons, now of necessity so extensively used. Experimental tests have shown the Ransome stone to be second only to granite in its powers to resist a crushing force. The strength of granite to resist crushing varies from 8000 to 12,000 lbs. per square inch; the Ransome stone, 8960 lbs.; Bramley Fall, 5120 lbs.; and Portland stone, 2630 lbs. per square inch. In practice, the materials forming the stone will be moulded *in situ* into blocks, either solid or cellular, of the required shape and dimensions. The cellular blocks form, however, the special peculiarity of this system. For bridge-piers and abutments the blocks may be rectangular or circular, for dock- and river-walls they may be square or hexagonal—in fact, any required shape may be given to them. For convenience in sinking the blocks (an operation which is precisely similar to that employed in sinking iron cylinders) the lower edge of the bottom length would be chamfered, and, when necessary, shod with iron. The horizontal joints would be made preferably with alternate projections and depressions in the sides of the blocks, and the vertical joints are made good with timbers halved into each block.

Selenitic Mortar.—A most valuable addition to the building arts has recently been invented by Colonel Scott, R.E., of South Kensington, and called by him Selenitic Mortar. The process of production consists in mixing with the water used in the preparation of the mortar, a small quantity of sulphate of lime in the form of either plaster-of-paris or gypsum, or by adding green vitriol. The mixture is prepared in the pan of an ordinary mortar-mill, in which the water and the sulphate are first introduced, and subsequently the lime. After the lime has been ground for three or four minutes, the sand, burnt clay, or other ingredients are added, and the whole are ground for ten minutes more. By this invention, ordinary lime can be at once converted into a species of cement-mortar which sets rapidly and well, and can be used for concrete, bricklayers'-work, or stuff for plastering, at a cheaper rate than that made from lime in the ordinary way. The use of sulphuric acid has been found to give the best results, although sufficient acid is contained in plaster-of-paris to prevent the lime from slaking, which in effect is the secret of the whole process. The lime, by this means, is enabled to take twice as much sand as when slaked. Experiments made against Portland cement, showed that while a joint made with the latter, after standing fourteen days, separated with a weight of 56 lbs., the cement in most cases coming clean away from the tiles which were joined together by it; the selenitic mortar, composed of 1 part lime to 5 of sand, under precisely similar conditions, required a weight of 158 lbs. to overcome adhesion, and then the fracture took place completely through the cement, half remaining on each side.

Narrow Gauge Railways.—The great revolution in the present day as regards the means of locomotion, consists in the adoption of a narrower gauge, and cheaper class of railway than that employed on the first introduction of railways. The rare occurrence of first-class railways proving remunerative has rendered it more and more difficult to raise the necessary funds for their construction, at the same time that the demands for an extension of railway communications have rapidly increased, and so, notwithstanding the opposition to any such change by many leading members of the profession, narrow-gauge lines having steeper gradients and sharper curves than were formerly considered admissible may now be considered as an accepted necessity. In India, a gauge of 3 feet 3⅜ inches has been adopted after very deliberate consideration by a committee appointed to report on the subject, and this will speedily become the standard gauge of the country. Australia, Tasmania, and New Zealand are all following in the same course. In Russia, the 3 feet 6 inch gauge has been definitively accepted, and an extensive system of lines on that gauge is now under construction. In Egypt the same width is to be adopted; whilst in the United States more than 2000 miles of narrow-gauge lines are in actual progress, or about to be commenced. California is organising railways on a reduced gauge in all directions, Canada is following in the same direction, and even for Prince Edward's Island contractors have been invited to tender for the construction of a 3 ft. 6 in. line from Casumpec to Georgetown, a distance of 120 miles. Light railways, and narrow gauge railways, are too often confounded together as being one and the same thing, whereas they are in reality totally distinct. The former subject has been recently brought before the Civil and Mechanical Engineer's Society, in a paper read by Mr. William Lawford, M. Inst. C.E. Light railways may often be of the same gauge as our first-class lines, only of a different construction. As feeders to main lines they will doubtless prove valuable adjuncts to the existing system of the country, especially in those districts where the natural features of the land would otherwise require expensive works. One important feature in connection with light railways must not be lost sight of, viz., that it would be impossible to carry on them a large and rapid passenger traffic, such as is now the case with the main lines of the country. A large and rapid traffic means heavy engines, heavy rails, &c.; but for an omnibus or light goods traffic, a light railway, with light permanent way materials, light engines, &c., might be made with great advantage, not only to the travelling community, but also to shareholders. The author then proceeded to give a description of a short line of "light railway" which had lately been constructed for the Duke of Buckingham and Chandos, by means of which the Duke's estates at Wotton are brought into connection with the Aylesbury and Buckingham Railway at Quainton. This line (4 ft. 8½ in. gauge) is six miles in length, with a branch of one mile and a half. It is throughout eminently a line of light works, and, with few exceptions, a surface line, the highest embankment being 12 feet, and the deepest cutting 10 feet. There are no road-bridges, the turnpike and other roads being crossed on a level. The rails are bridge-rails, weighing 30 lbs. to the yard, and are secured into longitudinal creosoted timbers, 6 feet 6 inches long, by means of fang-bolts; there are transoms 4 inches by 4 inches at every 12 feet, kept in their places by a wrought-iron tie-rod, ¾ inch in diameter. The ballast is 10 feet wide, and is 6 inches to 9 inches thick under the bottom of the timbers. The line is only partially fenced, the existing hedges being utilised for that purpose. The estimate for the works is only £1400 per mile, exclusive of the cost of land. A paper on the subject of "Railway Gauges" was read by Mr. R. F. Fairlie, before Section G of the late

British Association. After tracing the history of the Festiniog Railway, and the labours of the Commission which came over from Russia last year to investigate the subject of communications in this country, Mr. Fairlie proceeded to consider the false economy of the present system of railway traffic management. "I think," he says, "no more striking illustration of the error of our present system can be conceived than is afforded by the daily practice of a magnificent company like the London and North Western Railway, who, at the present moment, be it remembered, have commenced to double the width of their road through press of business, yet who are sending out daily, and daily receiving, at Euston Square, some 4400 passengers in carriages which contain sitting accommodation for 13,500, and who carry their enormous freight in increments, averaging less than one ton, in waggons having six times that capacity." Taking into consideration the real requirements of traffic, Mr. Fairlie comes to the conclusion that "the conditions under which a railway should be laid out to meet these requirements are clearly not those which rule the present system; ample experience proves the contrary, showing that no line, however full of business, can be worked to its full capacity. We are led, then, unmistakably to a narrow gauge, to the adoption of passenger-carriages which shall be filled, of waggons which shall be almost fully loaded, and of weight which shall bear a reasonable proportion to their capacity, and we are led to the adoption of very long trains and powerful engines."

Traction Engines.—The adoption of traction engines for farm purposes, and of road steamers for short traffic purposes on our high roads, is gaining considerably in favour, and each year adds to the purposes for which these engines prove their capacity. At the recent show of the Royal Agricultural Society, at Wolverhampton, competitive trials took place between engines constructed by different makers, and the various forms of wheel-tyres—elastic and non-elastic—were amongst the most interesting features connected with them. A paper on the subject of "Road Steamers" was read by Mr. R. W. Thomson, before Section G of the British Association, in which the trials at Wolverhampton were referred to, besides other evidences of the uses to which these engines are now applied with advantage. The question of traction engines or steam locomotives to work on common roads is of much more ancient date than railways; the difficulties that had to be overcome, however, proved at that date too great, and they were for the time set on one side in favour of railways. These latter, however, failed to supply certain local requirements, and engineers again set to work to produce a suitable road engine. At one time it was found that the rough road broke the machinery, and the engine was then made so heavy that it destroyed the road; and when it was found that the surface of the ground would offer no hold to the wheels, the wheels in revenge, as it were, were provided with claws which gripped the ground, but tore it woefully. Mr. Boydell then followed with his endless rails, which, though a scientific solution of the difficulty, failed in practice. Mr. Thomson then adopted elastic tyres, and by surrounding the wheels with solid india-rubber, 4 inches or 5 inches in depth, succeeded in overcoming in some degree the evils attending inequalities of road-surface, and making it a matter almost of indifference whether the road be hard or soft; the only kinds of soil on which the india-rubber tyres cannot work being ground so soft as to flow away from under the wheel, or wet clay, which has a tendency to ball upon the wheels, and so impede the action of the india-rubber. When used for farm purposes the width of the tyre is an important consideration; it has been found that an engine weighing nearly 7 tons, mounted on tyres 9 inches wide, could not move itself over a damp, heavy clay field; whilst an engine weighing 6½ tons, but mounted on tyres 12 inches wide, was not only able to travel itself with ease over this field, but was also able to haul the engine with narrow tyres and a 3-furrow plough, which was attached to the narrow-tyred engine in such a way that it could not be easily removed.

TECHNOLOGY.

The difficulty of uniting iron to brass is caused by the unequal rate of expansion in the two metals, which destroys the unity when the temperature is changed. A new alloy of copper is announced, and the inventor claims that its expansion by heat is so similar to that of iron and steel, that the surfaces may be regarded, when joined, as permanently united, for all practical purposes. The formula is as follows:—Tin, 3 parts; copper, 39½ parts; zinc, 7½ parts.

A new plan for meat preserving has been been introduced by an engineer, whose experience in sugar refineries and other extensive works in hot latitudes has ensured a practical and economical solution of one of the most important problems of the day. Mr. T. F. Henley does away with steeping meat in water, and with boiling and otherwise treating it in the most costly way. He simply squeezes a definite amount of juice out of the fibre, and by mechanical desiccation preserves the latter intact. The pressed meat thus obtained contains 10 per cent of alcoholic extract and salt, and over 50 per cent of fibrin and other albumenoid constituents. It is exceedingly rich, and so is the meat-juice, which Mr. Henley evaporates in vacuum pans. The juice contains about 15 per cent of alcoholic extract, and over 50 per cent of albumen. The ancient method of abstracting water only from the animal matter is relied on as the preservative, and the low temperature at which the evaporation is carried on prevents any loss of flavour or other deterioration. It is perhaps strange that so cheap and simple a process should not have been suggested before. Mr. Henley has worked at it for some time, and perfected it so as to ensure its immediate adoption. The first works, on an extensive scale, are to be opened in the River Plate, on the Estancia Nueva Alemania, where cattle have been reared and fattened for the European markets.

Acetate of alumina has been found very useful for the purpose of rendering woven fabrics waterproof without thereby impeding the passage of perspiration. Professor Balard prepares the acetate of alumina by dissolving 30 grms. of acetate of lead in ½ a litre of water, and also 24 grms. of sulphate of alumina in ½ a litre of water. These solutions, having been mixed, are next filtered, after which the fabric is immersed therein for a quarter of an hour, and, after having been well drained, is dried in the air.

Dr. E. Kopp has published a lengthy memoir containing, in a condensed form, all the information on the subject of distinguishing silk, wool, and vegetable fibres from each other, and also the various methods employed on the large scale for separating animal and vegetable fibres when they occur in mixed fabrics. Among some of the particular reactions we find the following for detecting wool in silk, and *vice versa*, based upon the fact that wool contains sulphur, while silk does not. The tissue to be tested—it should be, however, white, not dyed—is put into a solution of caustic potassa or soda, wherein oxide of lead has been previously dissolved; woollen fibres become black when immersed in this liquor, whereas silk remains unchanged. Another test for the same purpose is ordinary nitric acid, which dissolves silk in the cold, but hardly affects wool.

The following mixtures have been found, after a series of experiments, to be the best for red, green, and blue Bengal lights:—For red, 9 parts of nitrate of strontia, 3 parts of shellac, 1½ parts of chlorate of potassa; for green, 9 parts of nitrate of baryta, 3 parts of shellac, 1½ parts of chlorate of potassa; for blue, 8 parts of ammoniacal sulphate of copper, 6 parts of chlorate of potassa, 1 part of shellac. This latter ingredient need only be coarsely pulverised. The mixtures here alluded to are suitable for use in theatres and rooms, as by the combustion no injurious vapours are given off.

Dr. F. Springmühl has described a series of experiments made with the view to ascertain how far, and under what conditions, sodium may be employed as a substance suited to cause, by contact either with water or other materials, the explosion of vessels wherein these substances are contained. The force exerted is by no means small, as may be inferred from the following:—46 grms. of sodium and 18 grms. of water yield 2 grms. of hydrogen, a bulk of 22471·9 cubic centimetres. The space required for the sodium only amounts to 44·7 c.c.; the water is contained, previous to its coming into contact with the sodium, in a small bulb, which is solidly fixed by means of a neck to the bulb wherein the sodium is contained, which latter may be made of 50 c.c. capacity. Taking 50 c.c. for the capacity of the explosion bulb, the pressure of the gas generated inside will be 450 atmospheres, equal to 6800 lbs. to the square inch.

From a lengthy memoir by Mr. P. Champion on the properties and industrial manufacture of nitroglycerine, we quote the following particulars. Nitroglycerine is absolutely insoluble in water, but soluble, in all proportions, in ether, methylic alcohol, and ordinary alcohol (but, as regards the latter, only at a temperature above 50° C.) Nitroglycerine is somewhat volatile, without decomposition, above 100°, and is not liable to spontaneous decomposition when pure. When exposed for several hours to a temperature of −15°, nitroglycerine becomes thick, but not solid; while a prolonged continuation of cold of only −2° freezes this body, converting it into a crystalline mass. Fuming nitric acid dissolves, but also decomposes, this fluid; and the same effect is produced by concentrated sulphuric acid, and also by the mixture of sulphuric and nitric acids employed for the preparation of nitroglycerine. This latter fact explains the deficiency of the theoretical quantity (246) which is obtained in the preparation of nitroglycerine from 100 parts of glycerine. The author's experiments prove that pure nitroglycerine boils, but does not explode violently, at 185° C.; volatilises slowly at 194°, rapidly at 200°; deflagrates violently at 217°; detonates difficultly at 241°, but violently and completely at 257°; at a higher temperature the detonation is less violent, and at 287° is accompanied by flame. At a low red heat, nitroglycerine assumes the spheroidal state, and is volatilised without detonation. While nitroglycerine detonates with great violence by a smart blow it is not affected by electrical shocks.

The substance known as dynamite (consisting essentially of nitroglycerine absorbed by any suitable inert powder) is met with in the trade made up in cartridges weighing, on an average, 71 grms., and packed in boxes weighing with contents, about from 25 to 30 kilos. M. P. Guyot happening to have in his possession a number of cartridges containing dynamite (the body of the cartridge is made of stout grey paper), found that these objects, after some lapse of time, became moist and oily looking, and a cardboard box in which the cartridges were kept was also found impregnated with a liquid, which, on investigation, turned out to be nitroglycerine. A small piece of the paper so impregnated exploded violently when brought into contact with glowing coals, and the like effect was observed when a piece of the paper was laid upon an anvil and smartly struck with a hammer. The author also found that the wood of the boxes in which dynamite cartridges are kept becomes, by slow degrees, impregnated with nitroglycerine, and thereby a most dangerously explosive material, which may give rise to serious accidents in warehouses where dynamite is kept.

In a subsequent publication the same author proposes that instead of paper, parchment should be used for making the cartridges filled with dynamite, so that no nitroglycerine can escape by soaking the paper, parchment being impervious to the liquid just alluded to.

In a memoir on the accidental and spontaneous explosion of explosive substances, and on a preventative of such occurrences, Dr. Zaliwski says that the explosive property of the inflammable substance depends upon the hygrometric condition of the atmosphere—that is to say, that gunpowder and other explosive materials may become, even without elevation of temperature, spontaneously explosive, but that the smallest trace of oxalic acid is, if mixed with the explodents, sufficient to prevent spontaneous explosion due to the cause alluded to, owing to a catalytic effect which precedes the loss of basic water of the acid alluded to. The author further says that this fact can be readily experimentally proved by adding, to a mixture of sulphur and chlorate of potassa, for instance, or to any other explosive mixture, a certain quantity of oxalic acid, after which these materials may be heated even up to their point of fusion without exploding.

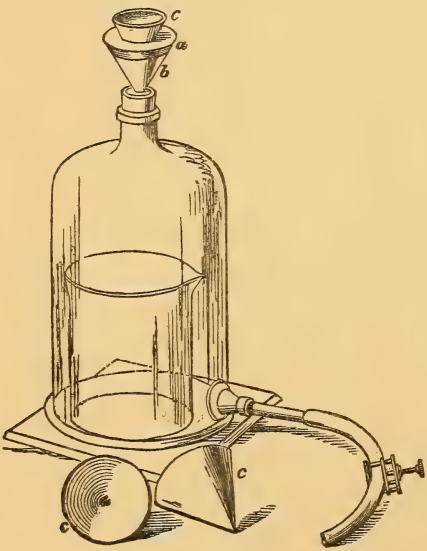
At a comparatively remote period, saltpetre was regularly manufactured in most European countries by a process too long to be further alluded to here, and well known, undoubtedly, to most of our readers. MM. Thiercelin and Willm state that the richness of the saltpetre-yielding materials recently tested by them varies very much. They found that the efflorescence now and then met with, especially on the walls of old stables, contained as much as 67 per cent of saltpetre; but this material is rare. Old wood-ash yielded ½ per cent; the mortar of a wall covered with ivy yielded 2·6 per cent; and the mortar of the same wall at a spot not covered with that plant, 4·6 per cent of saltpetre.

CHEMICAL SCIENCE.

The following test for the detection of small quantities of sulphur present in coal-gas has been proposed by Dr. V. Wartha. Form first before the blowpipe, in the loop of a platinum wire, a bead of pure soda, and next pass this bead over the edge of the gas flame, after which the bead is held in the interior of the flame in order to deoxidise the sulphates and sulphites of soda into sulphuret of sodium; the bead is then transferred to a porcelain basin, crushed, and some nitro-prusside of sodium added, whereby the smallest trace of sulphur will be detected. This reaction is fifty times more sensitive than that upon silver-foil; and the test can be performed in about three minutes, whereas Dr. Vogel's sulphur-copper reaction for this purpose takes four hours.

The idea of using a cone made of very porous eathenware, and as a substitute for the paper filter, has been carried out by Mr. C. E. Munroe in the following manner:—The cones are made of very light, porous earthenware, and have an angle of about 60°. They are used in the following way:—A section of a seamless rubber tube, *a*, is stretched around the mouth of a funnel, *b*, preferably a Bunsen funnel, allowing a portion of the tube to project above the top. This part will immediately arrange itself at right angles to the top of the funnel; into the circle thus formed the cone, *c*, is put. It is then connected with the Bunsen pump. When the cone is moistened and the pressure applied, the rubber band forms an air-tight joint, and the liquid runs through with great rapidity. Before the cones are applied to quantitative work they must be carefully washed, first with concentrated hydrochloric acid, then with distilled water, dried, and weighed. A small porcelain crucible is kept at the balance in which to weigh them. The cones can be used repeatedly, and can replace paper filters in every case. They will, undoubtedly, be found to be of great value in commercial work, for drying crystals and filtering corrosive liquids. As they will stand sudden changes of temperature without breaking, they can be substituted to advantage in many cases for crucibles.

FIG. 18.



When solid nitrate of silver, either in crystals or sticks, is placed upon glowing charcoal, deflagration takes place, the silver being left in the metallic state, while binoxide of nitrogen and carbonic acid are evolved. The nitrate is fused by the heat of the reaction and sinks into the pores of the charcoal, and as each particle of charcoal is replaced by metallic silver, the structure of the original wood is preserved. With proper management, pieces of silver of any desired size can be prepared, showing the exact structure of the wood. Dr. Chandler recommends that a crystal of nitrate be placed on the end of a piece of charcoal, and the blowpipe flame directed upon the coal near the crystal to start the reaction. When deflagration begins, crystal after crystal may be added. The nitrate fuses, passes down through the porous metal already reduced until it reaches the glowing coal, where it is reduced. Lumps of silver weighing an ounce or more, which exhibit most beautifully the rings of the wood, have been prepared in this manner.

The behaviour of arsenic acid with hydrochloric acid, with a view to obtaining an acid free from arsenic, has been examined by J. Mayrhofer. His paper contains the record of the results of some experiments made with the view to ascertain under what conditions and in what state of combination arsenic is carried over by the distillation of hydrochloric acid which is contaminated with arsenic. It appears that the degree of concentration of the last-named acid, and the relative proportion of arsenic acid present, and also the care taken in cautiously distilling, are of influence in obtaining an arsenic-free distillate; but, moreover, the treatment of the hydrochloric acid with chlorine, or, better still, with sulphuretted hydrogen, previous to distillation, to be next carefully conducted, will ensure the distillate being free from arsenic.

In some recent experiments in chemical dynamics, Dr. J. H. Gladstone, F.R.S., and Alfred Tribe, F.C.S. had occasion to study the action of nitrate of silver on copper plates in various positions. They observed that, when the plate was vertical, there was rather more corrosion at the bottom than at the top. This is easily accounted for by the upward current, which flows along the surface of the deposited crystals, and which necessitates a movement of the nitrate of silver solution towards the copper plate, especially impinging on the lower part. It was also found that, when the copper plate was varnished on one side, it produced rather more than half the previous decomposition, and was most corroded at the edges of the varnish. By making patterns with varnish, this edge action became very evident. This was explained by the fact that the long crystals of silver growing out from the copper at the borders can spread their branches into the open space at the side, and so draw their supply from a larger mass of solution than the crystals in the middle can do; and increasd crystallisation of silver means increased solution of copper. This was proved by making the varnish a perpendicular wall instead of a thin layer, when the greater corrosion was not obtained. In a plate completely surrounded with liquid, the greatest growth of crystals is also evidently from the angles. It was likewise observed that, if a vertical plate be immersed, the lower part in nitrate of copper and the upper part in nitrate of silver, there is greater corrosion about the point of junction; this was attributed to the greater conduction of the stronger liquid.

A new precipitating reagent for copper has been proposed by Mr. Hugo Tamm, which promises to be of great service in analysis. Such a reagent, to be perfect, must fulfil certain conditions. (1). When employed for the determination of a solid substance it should be volatile, or, if it is fixed, it should not form, with the precipitate, compounds of indefinite composition. (2). It should form, with the element to be determined, a compound as insoluble as possible. (3). It should not introduce in the liquid separated from the precipitate any substance likely to alter the behaviour of its constituents, with general or respective reagents, and, least of all, it should not introduce any substance difficult of separation from any of the constituents. (4). It should separate the element to be determined, in the shape of a compact precipitate, in neutral or acid liquors, and allow the other elements of the combination to remain in solution. This is, perhaps, the most important principle in analytical chemistry. The reagent which Mr. Tamm proposes for copper fulfils these conditions, and is, consequently, very perfect; in practice it will be found very useful. It is obtained by dissolving in distilled water equal weights of sulphocyanide of ammonium and of bisulphite of ammonia. This mixture keeps well, and can be used several months after its preparation, providing it is not left exposed to the air for any length of time; but, should a slight alteration take place in its composition, this would be of no consequence. When added to a solution containing copper, it immediately precipitates white sub-sulphocyanide of copper as an insoluble powder readily washed, whilst it precipitates scarcely any other metal which may be present.

The difficulty in burning off organic matter has often been experienced by analytical chemists, especially when the substance under incineration yields a readily fusible ash. M. A. Béchamp has proposed the use of nitrate of bismuth in the state of an aqueous solution of known strength to be mixed with the material to be incinerated, provided the water contained in the substance has been previously ascertained by drying at 100°. The nitrate of bismuth solution (the bulk thereof to ignite and calcine readily 100 to 150 grms. of substance should contain from 3 to 4 grms. of oxide of bismuth) having been mixed with a fresh and weighed portion of the substance (yeast is taken in illustration, as it is well known that this is very difficult to burn off completely), the mixture is first dried gently on a water-bath, next heated on a sand-bath hot enough to cause the mass to blacken, after which it burns away as tinder, and, if required, the ignition is completed over the lamp. Should any fear exist that some metallic bismuth has been formed, nitric acid is added to the ash, and the heating repeated, so as to destroy the nitrate of bismuth thus produced. From the solution the bismuth may be removed by sulphuretted hydrogen.

Ch. A. Bœhme relates an occurrence which appears to lead to the conclusion that chloral hydrate may be spontaneously decomposed when kept in sealed bottles. Two of these, each containing 1lb. of the substance alluded to, were obtained from a leading drug-house at New York; one of the bottles was opened at once, and nothing special noted in its contents; the other bottle was placed in a store-room, and on being after some time opened, a dense cloud of fumes was observed to issue from its mouth. These fumes had the characteristic odour of chloral hydrate, but were more stifling, reddened blue litmus-paper, and, on further testing, were found to consist partly of hydrochloric acid. The lumps of the hydrate near the top of the bottle had crumbled to a crystalline powder freely soluble in water, somewhat in chloroform, but insoluble in sulphide of carbon and oil of turpentine. The author's opinion is that the sample alluded to was pure hydrate of chloral at first, but had been decomposed by standing.

A subject of some importance in analysis has been pointed out by Mr. J. Myers, who finds that sulphuretted hydrogen is often contaminated with arseniuretted hydrogen. At the ordinary temperature the two gases alluded to can co-exist without decomposition, which only takes place at the boiling-point of mercury. The author, while experimenting, thought that the arseniuretted hydrogen might be due to the presence of some arsenic in the sulphuret of iron employed, but, on investigation, it turned out that the source of the arsenic was in the commercial sulphuric acid used for the evolution of the sulphuretted hydrogen.

LIGHT.

The fine purple colour of the vapour of iodine has been found by Dr. Andrews, F.R.S., to arise from its transmitting freely the red and blue rays of the spectrum, while it absorbs nearly the whole of the green rays. The transmitted light passes freely through a copper-red or a cobalt-blue glass. But if the iodine vapour be sufficiently dense, the whole of the red rays are absorbed, and the transmitted rays are of a pure blue colour. They are now freely transmitted as before by the cobalt glass, but will not pass through the red glass. A solution of iodine in sulphide of carbon exhibits a similar dichroism, and according to its density appears either purple or blue when white light is transmitted through it. The alcoholic solution, on the contrary, is of a red colour, and does not exhibit any dichroism.

The spectrum of lightning has been examined by Mr. J. Gibbons during a severe thunderstorm, but having only a pocket spectroscope, he was unable to determine the position of the bright bands with any degree of accuracy. Two narrow blue-green bands most frequently made their appearance; they were somewhat narrower than those of the chloride of copper spectrum, and separated by a wider interval. At times when the flashes were particularly vivid, bands of various degrees of luminous intensity and of breadth were distributed over the whole spectrum; and, during the occurrence of a flash *en zigzag*, a group of narrow ones—two being very brilliant—appeared in the more refrangible part of the red, besides those bands in the blue and green, which were always most conspicuous. The complexity of these spectra gave the author the impression that this department of meteorological chemistry would prove a most promising field of investigation.

Microscopy.—The cells of pure tin now extensively employed in mounting microscopical objects have been the cause of some mischief in the case of tissues injected with chromate of lead and mounted in fluid (probably slightly acid). The lead in the coloured injection has been reduced, and can be seen in numerous crystalline patches, while the yellow tint has almost totally disappeared; the whole combination of cell, fluid, and easily reducible metallic salt, probably formed a miniature battery, and hence the spoiling of the preparation. The use of these cells should always be avoided in mounting tissues injected with vermillion, chromate or carbonate of lead, Prussian blue, or other metallic pigments, glass cells being preferable and incapable of doing injury. However, for the majority of mountings in fluid and all dry preparations the metal cells may be used with perfect safety, no preservative fluid at present used by microscopists acting upon them. The above-mentioned case must be regarded as purely exceptional, and by no means as prohibiting the employment of these economical and useful cells.

A fortunate accident has enabled Mr. Wenham to establish the reality of the appearances known as the ! markings on the Podura scale. In a slide of *Lepidocyrtus curvicollis* (the "test Podura") prepared by Mr. S. J. McIntire, a fracture caused by the slipping of the cover has left some of the ends of the much disputed markings projecting; owing to the toughness of Podura scales such fragments are difficult to obtain, and opportunities for such observations seldom occur. The Podura difficulty has been solved in precisely the same manner as that of *nodular* or *beaded* structure of the *Diatomaceæ*, in which the examination of fragments revealed the truth; the varied appearances presented being caused by alterations in the illumination and focus and adjustment of the object-glass. The late R. Beck, Mr. Hennah, Dr. Pigott, and others have shown how easily false appearances are to be produced, especially in transparent bodies, such as glass rods, bosses, &c.

Mr. N. E. Green has employed the lime-light for the purpose of illuminating microscopical objects viewed with high powers by reflected light. The objectives used have been $\frac{1}{8}$th and $\frac{1}{12}$th by Ross, and $\frac{1}{12}$th by Gundlach, of Berlin. The long working distance of the latter objective has been found especially serviceable. The lime-light was placed about six inches from the microscope, and concentrated upon the uncovered object by means of a small condensing lens. The surfaces of *Biddulphia*, *Triceratium*, and *Isthmia* exhibited an appearance compared by Mr. Green to that of a nutmeg grater, but in his drawings closely resembling lunar craters. The "*beads*" or "*hemispheres*" of *Pleurosigma Hippocampus* and *P. formosum* were distinctly seen, and also those of *P. angulatum*, but in this instance it required the most careful adjustment of the light to develope their elevations. Mr. Green explains the conflicting appearances exhibited by these *Diatomaceæ* when examined by transmitted light by considering that the hemispheres really surmount cylinders of a perceptible length, as seems to be proved by examining the edge of a broken or abraded specimen, or by focussing downwards, and these cylinders by compression take naturally the hexagonal form, so that according to the part focussed, and the nature of the illumination employed, dots, lines, hexagons, or beads may be seen.

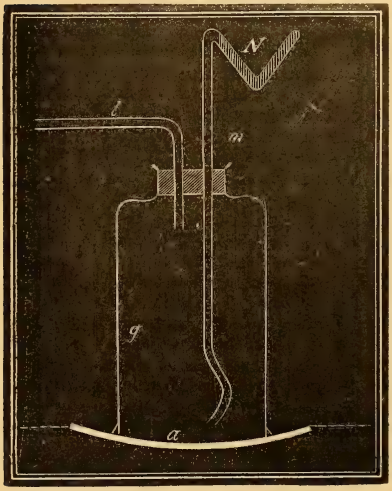
Mr. Sorby makes a valuable communication to the "Monthly Microscopical Journal" for July on the "Spectrum Method of Detecting Blood." After describing the best combination of prisms to be employed in the spectrum-microscope, which are recommended to be of moderate dispersive power, and the cells, and other apparatus and reagents useful in these examinations, he proceeds to demonstrate the characteristics of the blood spectrum, and the changes caused in it by the action of reagents. The changes caused by exposure to damp are next noticed, and valuable information is given respecting the detection of old blood-stains. The action of mordants and dyes in fabrics, and the variations in manipulation caused by their presence, are duly treated upon, and the recognition of blood in very diluted solutions is shown to be possible. The whole paper is of great interest to all workers with the micro-spectroscope and those engaged in medico-legal enquiries.

Mr. Ladd has contrived a polariscope to be used with the microscope in the examination of the rings about the optic axes of crystals, which has the advantage of being less expensive than ordinary appliances for the purpose, and also gives so large a field that both the axes of sugar are easily brought into view. Beneath the stage is placed a pair of lenses somewhat resembling an ordinary Huyghenian eye-piece, but with a nearly hemispherical front glass; this acts as a condenser. A similar combination of lenses is attached to the body of the microscope in the place of the objective; a double convex lens is placed in the bottom of the draw-tube, which is here used for the purpose of focussing; beneath the condenser is placed the ordinary Nicol prism, and a low power eye-piece with a Nicol prism above it as an analyser completes the arrangemet.

HEAT.

In the phenomenon of the spheroidal state, the globule will float when the vapour beneath it is able to support the pressure of the atmosphere plus the weight of the globule. If we remove the former factor, a much smaller vapour tension will be required to produce the phenomenon, as may be proved by the following experiment, described by E. Budde, in which, with the aid of the air-pump, a Liedenfrost globule is supported upon a metal plate whose temperature is below 100° C. A bell-shaped glass vessel, *g*, is firmly cemented to a copper plate, *a*. Through the stopper which closed the upper opening pass two glass tubes, *l* and *m*. The first attaches by caoutchouc tubing to the air-pump. The second reaches within the vessel nearly to the plate *a*, while above it is closed and bent into an N form. The bent portion is filled with water. The plate is now placed upon the water-bath, which soon imparts to it a temperature of from 80° to 100° C. The air-pump is now put in operation; the water in N evolves air-bubble and vapours (gentle heating will facilitate the operation), which mainly accumulate in the upper end of the tube—and force a portion of the water through *m*. The water falls boiling, or very

FIG. 19.



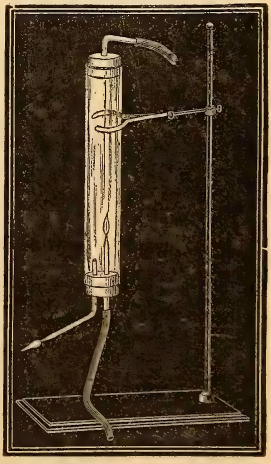
nearly so, upon the plate beneath, the temperature of which is, under the abnormal conditions, considerably above the boiling-point of the water—and all the conditions necessary for the production of the spheroidal state are present. If the rarefaction is carried until the barometer indicates 10 c.m. (about 4 inches) of mercury, and the water-bath is heated to about 90° C., the experiment will succeed without the slightest difficulty, and the spheroids obtained will evince an energetic movement. The experiment is not a mere physical curiosity, but possesses an importance which our educated readers will doubtless have already appreciated, inasmuch as it is decisive in confirming the theory of the spheroidal state. It proves that the force which sustains the globule obeys the laws which govern the tension of vapours.

Dr. Andrews has examined the action of heat on bromine. If a fine tube is filled one-half with liquid bromine and one-half with the vapour of bromine, and after being hermetically sealed is gradually heated till the temperature is above the critical point, the whole of the bromine becomes quite opaque, and the tube has the aspect of being filled with a dark red and opaque resin. A measure of the change of power of transmitting light in this case may be obtained by varying the proportion of liquid and vapour in the tube. Even liquid bromine transmits much less light when heated strongly in an hermetically-sealed tube than its ordinary state.

A number of devices, some of them simple, others complex, have from time to time been described for showing the reciprocal combustion of the elements of water, and experiments of a similar nature. Most, if not all of these, however, as will be found upon testing, either do not entirely remove the danger of an explosion from the operator, or they require the exercise of an unusual amount of care and dexterity to be used with success.

The accompanying arrangement, which is of the most simple character, and which we saw for the first time on the lecture table of Professor Himes, we have since repeatedly used to show the burning of oxygen, air, chlorine, &c., in hydrogen, burning gas, or hydrocarbon vapours. The experiment can be performed with such ease that it is worthy of notice.

Fig. 20.



The arrangement consists of a cylinder of glass, about a foot or a foot and a half in length (the kind used commonly as chimneys for the argand-burner can be had of proper length). This is furnished above and below with a cork; the one at the upper end has one, that below has two, glass tubes of the form shown in figure. The whole affair is supported from the retort stand. The hydrogen (in the H and O experiment) is admitted through the upper tube; when it has completely displaced the air it is ignited below—the cork having been removed—and the supply is regulated until only a weak hydrogen flame remains. The oxygen supplied through the straight tube in the lower cork is now turned on slightly, and the cork fitted into its place. The flame of hydrogen at the opening is extinguished, but the oxygen, in passing up through it, is ignited, and burns now in the centre of the cylinder. The surplus of the hydrogen escapes now from the second tube below, and can be there ignited. This last flame serves the purpose of a good indicator, by which the supply of gases in the cylinder can be regulated, and which, of course, leaves the size of the flame at the will of the operator. Once in operation, the experiment may be left to take care of itself for the remainder of the hour.

After briefly referring to the experiments of the Florentine Academicians, and of Huyghens (1667), on the force exerted by the freezing of water in closed metallic vessels, M. Boussingault relates a series of experiments made by him last winter in order to ascertain whether water, when put into a strong vessel (a steel cylinder of great strength, and so arranged that the dilatation, or expansion, of the water when cooled below $+4{\cdot}1^\circ$, could be prevented), would or would not remain liquid, even when exposed to a cold very considerably below its point of congelation, in consequence of the expansion due to the cooling down from below $+4{\cdot}1^\circ$ being prevented by the strength of the vessel containing the water and stopper (steel plug) fitted thereto. The result of this investigation was found to be that water remains liquid under the conditions alluded to, even at a temperature of -18°, but freezes instantaneously as soon as the impediment caused by the resistance of the plug which hermetically closes the steel vessel, was removed, and free play was given to the expansion of the liquid. It should be noted that the sides and bottom of the steel vessel alluded to were of such great strength as to be practically unyielding.

ELECTRICITY.

E. Bourgoin describes a series of experiments, from which the following main results may be deduced:—When the galvanic current passes through acidulated water, it will be seen that, though the total effective work done by each pole is the same, the quantity of acid found in each compartment at the end of the experiment will be found to vary considerably. The three following cases may be distinguished:—(1) The acid is accumulated regularly at the positive pole; this occurs with sulphuric, nitric, phosphoric, benzoic, succinic, camphoric, &c., acids. (2) There is no loss at the positive pole; the loss is only experienced at the negative pole, since half of the electrolysed acid is regenerated in the other compartment. (3) The two compartments become simultaneously poorer; this occurs with lactic, tartaric, citric, and in general, all the very readily oxidisable acids.

M. Gramme, of Paris, has recently completed a magneto-electric machine from which he obtains a continuous current. The principle is that of passing a permanent magnet through a coil of wire constantly in one direction; or, what is virtually the same thing, keeping the magnet fixed, and causing the coil to move. To effect this M. Gramme employs an annular coil of 200 metres of copper wire of 2 m.m. in diameter, wrapped in silk. By a multiplying wheel and pinion this coil is caused to revolve between two concave armatures attached to a series of permanent magnets. One of these armatures developes in the circuit an induced current in one direction, whilst the other armature induces a current in the opposite direction but of the same intensity. To collect these currents two rubbing-pieces are placed in metallic contact with the coil at the neutral points of the two armatures: one of these contact pieces receives the positive electricity, the other the negative, becoming respectively the positive and negative poles of the electro-motor. By this means and by a rather complicated method of winding the wire on the coils, M. Gramme obtains a perfectly continuous current, capable of decomposing water in a voltameter, and of producing all the results obtained with the battery. Instead of permanent magnets, the inventor in practice employs electro-magnets. In the latter case there is always sufficient residual magnetism to induce a current in the annular coil when motion is imparted, and as in Siemens's and in Wheatstone's instruments this weak current speedily induces a stronger, a maximum of 700 or 800 revolutions per minute can be attained. With a half-horse power, nearly 20 inches of platinum wire one millimetre diameter can be heated to incandescence. The machine will be very useful where a constant current of high intensity is required.

Electricity certainly seems to be entering into nearly all sciences. Sir Charles Wheatstone has at length called in its aid to Hygrometry. A thermopile of five couples has the faces exposed in an ebonite dish in which water or spirit is placed. The reverse faces of the pile are exposed to the air, insulated in solid paraffin; two connecting wires are carried to a delicate galvanometer of a few turns. The variations in deflection of the needle of the galvanometer thus become a relative measure of the evaporation of the liquid contained in the ebonite cup, and by this means very great accuracy can be attained, as the degree of deflection may be read off by a microscope. As yet, we believe, no definite observations have been made with the instrument, which is intended for Kew Observatory.

MM. de la Rive and E Sarasin have communicated to the Société de Physique et d'Histoire Naturelle de Genève a most interesting paper, embodying the results of their experiments on the action of magnetism upon electrified gases. The experiments are detailed at great length, and have already appeared in English in the "Philosophical Magazine." The following are the deductions:—

"1. That the action of magnetism when it is exerted only on a portion of an electrical discharge transmitted through a rarefied gas, determines in that portion an increase of density.

"2. That the same action, when it is exerted upon an electrical discharge placed *equatorially* between the poles of an electro-magnet, produces in the rarefied gas through which it is propagated an *increased* resistance, which is as much greater as the gas itself is a good conductor.

"3. That this action, on the contrary, determines a *diminution* in resistance when the discharge is directed *axially* between the two magnetic poles, this diminution increasing with the conductivity of the gas.

"4. That when the action of the magnetism consists in impressing a continuous movement of rotation on the electrical discharge, it has no influence on the resistance to conduction, if the rotation is effected in a plane perpendicular to the axis of the magnetised soft iron core which determines the rotation; while it considerably diminishes it if the rotation takes place so that the electrical discharge describes a cylinder around the axis of the rod.

"5. That these different effects apparently cannot be attributed to variations of density produced in the gaseous medium by the magnetic action, but very probably their explanation will be found in the perturbation induced by that action in the arrangement or disposition of the particles of rarefied gas necessary for the propagation of electricity."

It would appear the decrease of conductivity corresponds to the constrained position into which the electrical discharge is forced under the influence of the magnetism. The gases were inclosed in tubes to which was attached the necessary apparatus for measuring the variation in pressure.

It is well known that in working the tangent galvanometer with currents of high intensity, the tangents are not proportioned to the strength of the currents, owing to the needle being removed from the direct magnetic field. Professor Trowbridge, of Harvard College, has designed a galvanometer with which the results are more nearly proportioned. It consists of the ring and needle of the ordinary tangent galvanometer, the ring, however, instead of remaining perpendicular, being made to take any angle between the perpendicular and the horizontal planes. The deflection of the needle when the coil is horizontal being *nil*, the different angles of inclination give increased deflections. The deflection remaining constant, the intensities of the currents vary as the cosines of the angles of inclination of the coil. The instrument is therefore termed the cosine galvanometer. Many determinations of the intensity of the same current can be made by forming a table of the values of the cosines of the different angles of inclination in terms of the deflection of the needle which currents with a known interposed resistance produce. In the tangent galvanometer but one determination can be made. The delicacy varies inversely as the cosine of the angle of inclination. With large deflections, therefore, this instrument appears to give closer results than the ordinary tangent galvanometer; and used with a Gaugain's multiplier, very accurate estimations can be made.

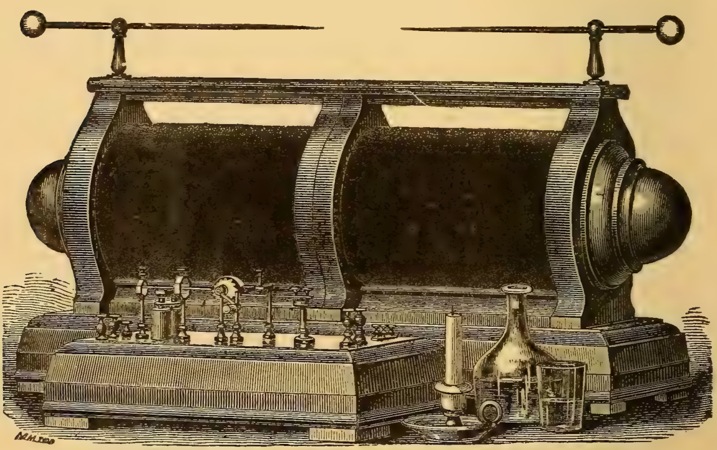
AMERICAN CORRESPONDENCE.

By Professor Leeds, Stevens Institute of Technology.

Large Induction-Coil.—I have obtained from Dr. Wahl, the Secretary of the Franklin Institute, the following details of a very large induction-coil, probably the most powerful now existing, which has lately been constructed for Professor Morton by Mr. E. S. Ritchie, of Boston. Mr. Ritchie, as is well known, was the first, by several fundamental improvements, to make the induction-coil an efficient and reliable source of electricity; and it would seem that by constant attention to the subject, he has succeeded in making the most perfect instrument yet constructed. The coil now described, containing but 44⅓ miles of wire, 40 inches in length, and weighing about 206 pounds, gives, with but three cells, sparks 21 inches in length, and, after several months of constant use and severe tests, is in perfect condition.

The accompanying woodcut, which is a faithful copy from a photograph of the coil, with some familiar objects as standards of comparison, will give a general idea of its structure and arrangement. It is made in three parts, one

Fig. 21.



consisting of the condenser, enclosed in a mahogany case, as shown in the foreground carrying on its upper surface the automatic and hand-break piece, commutator, &c., and two others, forming the coil itself. These last are so arranged that they may be separated from each other, and used apart or united for quantity. The pole cups, by which the halves of the coil and condenser are united, have been omitted by the engraver on account of their confusing effect, as they were superposed by the perspective of the picture.

The particulars of construction are briefly as follows:—The iron core consists of iron wires, about $\frac{1}{16}$th inch thick, and weighs 14 pounds. The thickness of the wires is immaterial, except as it affects their annealing. These wires are not insulated from each other, and are simply bound together with a covering of oil-silk and cloth for strength.

The primary wire is 200 feet in length, and 0·1655 of an inch or about $\frac{1}{6}$th of an inch in diameter, and weighs 17 pounds. The secondary wire is 234,100 feet, or about 44⅓ miles, and 0·07 inch in diameter, and weighs 44½ pounds. It was made of Lake Superior copper, of the best electrical conductivity, and is covered with white silk. It is wound according to the plan devised by Professor Ritchie, in a series of spirals representing the thickness of the wire and its insulation, and has additional insulation of paraffin paper interposed at regular intervals.

The insulation between the primary and secondary consists of glass bells and vulcanite spools so proportioned as to offer the greatest resistance at points of highest tension, and proved by actual experiment to be 50 per cent greater than a spark of 21 inches would penetrate under the existing conditions. The condenser contains 325 square feet of tin foil insulated with oil silk, 100 square feet being in permanent connection with the primary circuit, and three buttons throwing on 100, 75, and 50 feet respectively at will. The break-piece is of the combined automatic and hand movement, attached by Mr. Ritchie to all his large instruments, the automatic break being operated by a single cell battery, connected or thrown out at pleasure by a button on the surface of the condenser-case. The total height to upper surface of horizontal strip is $18\frac{1}{2}$ inches; total length of base from end to end of round caps over primary, 40 inches; height of base, 5 inches; width of base, 13 inches; length of each section of secondary bobbin, 13 inches; external diameter of secondary bobbins, 9 inches.

The battery for exciting this coil was made according to Professor Morton's direction, by Messrs. Chester Brothers, of New York, and consists of three glass jars, 10 inches in diameter and 12 inches high, into which are lowered by means of a windlass plates of carbon and zinc, 8×10 inches, five of each occupying each jar. The liquid employed is the mixture of potassic bichromate solution and sulphuric acid, now used in several forms of battery. When the solution is fresh, an immersion of 3 inches develops the full power of the coil.

In connection with a Leyden jar of $1\frac{1}{3}$ square feet surface it produces sparks of $2\frac{3}{4}$ inches in length, and with one of Professor Morton's secondary condensers

FIG. 22.

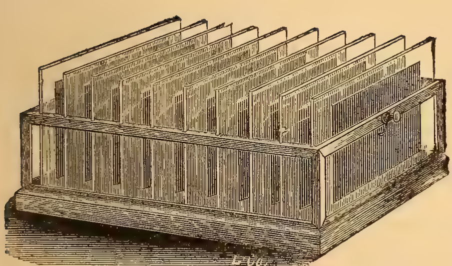
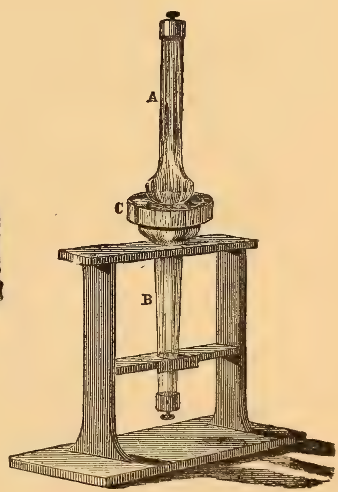


FIG. 23.



(see "Journal Franklin Institute," vol. liii., p. 256) shown in the accompanying cut (Fig. 22), and containing 20 coated panes, it gives sparks 14 inches in length, and of the intense whiteness and loud report of the Leyden jar discharge.

Blocks of glass 3 inches thick are penetrated, and seem to represent pretty accurately the same resistance as the 21 inches of air, for when the points are separated 21 inches, and other wires connected with the columns for piercing the glass, several sparks will pass in air before one with a red flash goes through the glass, then several sparks in air will occur before another spark will traverse the glass block again. Curiously enough, the spark in glass, as in air, seems to render its path a worse conductor than before, for it rarely happens that two sparks in a thick block go even partially by the same route, though the conducting points remain in exactly the same position.

The apparatus employed to pierce glass blocks is constructed as follows:—Two glass hollow columns, A and B (Fig. 23), are provided with five wires along their wires, the space about the wires being filled in with a mixture of wax and resin. These pillars are made very broad at one end, which is ground flat, and are provided with brass caps and binding screws at the other. They are cemented with wax and resin by the broad ends to the block of glass, C, to be pierced, the frame shown in the cut greatly facilitating the process.

Employed to illuminate a Gassiot's cascade when a secondary condenser of eight coated panes is in circuit, and an interruption or spark in air likewise interposed, the amount of luminosity is truly surprising, and greatly exceeds anything that we have seen with any other coil, even those made by Mr. Ritchie to give 15-inch sparks.

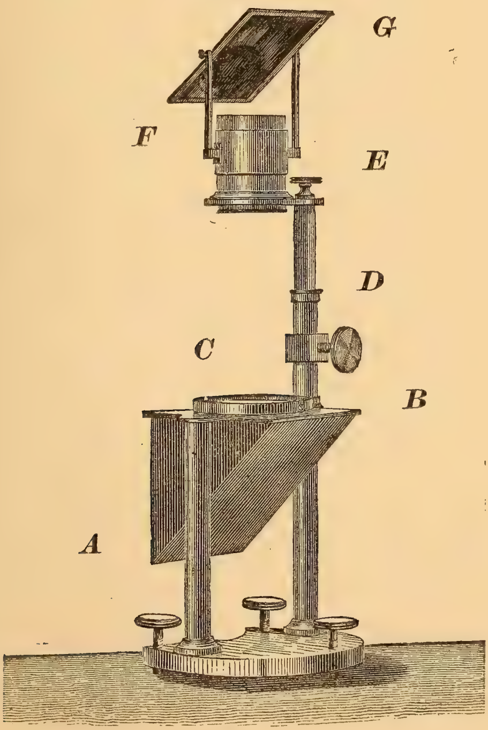
When coupled for quantity, the spark length is reduced to 12 inches, and the quantity is conspicuously increased, as is indicated by the sound and the aureola. When the poles are about 4 inches apart, this aureola may be blown into a flame-like surface, extending 3 inches from the line of discharge. Connected with a battery of four Leyden jars, the sparks are deafening, and afford light enough to illuminate a zeotrope disk 4 feet in diameter, so as to make the movement of its figures perfectly distinct at a distance of 60 feet. It is of interest to note in conclusion that a determination of the resistances made by Professor Morton gave for the resistance of the primary wire 0·13 of a unit (Brit. Ass.), and of the secondary wire 40,400 units.

Remarkable Electro-Magnet.—With the intention of attempting in conjunction with Professor Mayer to solve various outstanding problems in diamagnetism, in which unsatisfactory or negative results only have been obtained hitherto, Professor Morton has had made by Messrs. Wallace and Sons, of Ansonia, Connecticut, an electro-magnet of unusual form and of very great size. The poles of the core, which is made of the best wrought iron, are 3 feet 3 inches in height and 6 inches in diameter. They are hollow, the diameter of the bore being 3½ inches, and the thickness of the cylinder is therefore but 1¼ inches, which is a little greater than the thickness required for total electro-magnetic effect, as determined by Professor Mayer in accordance with the principles made known by him in a recent number of the "American Journal of Science." The diameter of the wire is one-fifth of an inch, and its total length is about 2000 feet. It is wound upon eight brass spools, which can be taken off or set on the core at pleasure. The weight of the magnet when complete is 1540 lbs.—that of each spool 112 lbs. It is proposed to employ three such galvanic batteries as were described in connection with the induction coil to supply an adequate galvanic current, and to connect them for quantity. This will give a total area of zinc surface amounting to 27 square feet. As yet no experiments have been made with a view of testing the performances of this mammoth magnet, but I hope shortly to present some account of them to the readers of this journal.

On the Projection of Magnetic Spectra, Cohesion Figures, &c., upon the Screen.—A very ingenious apparatus was recently exhibited before the American Institute by Professor Morton, for the projection upon a vertical screen of the images of objects, such for example, as waves in a tank of water, cohesion figures of various liquids, magnetic spectra, and the like, which can only be produced satisfactorily when the objects are maintained in a perfectly horizontal position. The original idea and general plan of the instrument shown was, as the speaker stated, due to Professor J. P. Cooke, of Cambridge, his own work in connection with it being confined to the devising of a convenient mechanical arrangement of parts, the improvement of the combination of condensing lenses with the reflecting lenses so as to secure a white and evenly illuminated field on the screen, and the discovery that an ordinary silvered mirror would serve for the final reflection as efficiently as a metal speculum or glass silvered by Foucault's plan, which are so difficult to obtain and keep in order. Faraday and Tyndall, the speaker further remarked, had employed an electric lantern turned on its back to throw images on the ceiling, and he himself had tried the same thing with a lime-light, and with a square prism had endeavoured to direct the rays on the screen, but with results unsatisfactory for reasons presently to be stated.

Mr. C. J. Woodward had also described a similar arrangement in the "Chemical News," vol. xix., p. 21. But last summer during a visit to Cambridge, Professor Cooke had kindly shown him in operation a lantern, in which the light was first thrown in a vertical direction by a mirror placed in front of the condensers, then passed through the horizontal object and object-glass, and lastly, was projected towards the screen, by a mirror silvered on its face. The only drawback to this instrument was, that the object being practically removed to some distance from the condensers, the field of light on the screen was shaded and discoloured.

FIG. 24.



The arrangement adopted by Professor Morton, and operating with such success that with it experiments were lately shown with striking effect in the Academy of Music, in Philadelphia (a building seating more than 3000 persons), was as follows:—

The lantern condenser in the first place is made of three lenses, the first two of such curves as to give with the light placed at about two inches from the nearer one a practically parallel beam. This beam is received upon a mirror, A B, placed at an angle of 45°, and after reflection from it falls upon the third lens placed horizontally at C. This concentrates it upon the objective at E, from which it passes to the mirror F G, and is so reflected to the screen. This mirror, moreover, is not silvered on the exterior surface, but in the usual way, though with pure silver on the back. Yet no want of definition is to be perceived in the image, owing no doubt to the fact that the faint reflection from the first surface is inappreciable in comparison with that from the metallic silver. In several articles published in the "Chemical News" and elsewhere, a square prism has been described as being used for the same purpose. But this arrangement leaves a third of the field dark, because about one-third of the cone of rays entering the prism is at an angle too great for total reflection. To exhibit magnetic spectra, a plate of glass is placed upon the third lens of the condenser, iron filings scattered evenly upon it, a small steel magnet placed beneath, and the glass lightly tapped with a pencil point. The various phenomena of wave motion, interferences, reflexions, &c., are demonstrated by Professor Morton, with an apparatus contrived by Messrs. Hawkins and Wale, the instrument makers, who satisfactorily carried out his instructions with regard to the making of the lantern itself. It consists of a metallic box with a sheet-rubber cover, provided with a long metal tube; this is so placed that the tube is about one quarter of an inch above the point in the tank which it is desired to make the centre of the wave motion. On tapping the rubber diaphragm a momentary puff of air is driven from the tube, producing exactly the disturbance needed. By placing an elliptical ring inside the tank, the reflexions, interferences, &c., are correspondingly modified.

By letting fall drops of ether, alcohol, carbolic acid, oil of cinnamon, coriander, cloves, &c., on the surface of water contained in the tank, cohesion figures are produced, which may be admirably exhibited in a lantern of this description, and so likewise the electric decomposition of metallic solutions.

Another experiment of peculiar beauty consists in attaching a ring of thin rubber, 5 inches in diameter, to a corner segment of a glass Chladni plate 12" square, and filling the ring about $\frac{1}{8}$ inch deep with water. On vibrating the plate with a bow (the corner segment being of course in the lantern field), the area is filled with the most beautiful crispations, changing with the tone as the harmonics are sounded. This experiment, as we witnessed it in the Academy of Music, was of most unusual beauty.

Stevens Institute of Technology, U.S.,
July 15, 1871.

SCIENTIFIC INVENTIONS AT THE INTERNATIONAL EXHIBITION OF 1871.

SCIENCE is well represented at the present Exhibition, although the inventions and models to be viewed are not numerous. Each, however, either embodies the application of some highly scientific and known principle, or is the result of much thought and labour in a new field. There is an improvement in the arrangement this year much to be commended—the actual working of every model—rendering the scientific department not only more attractive to the uninitiated, but greatly assisting the comprehension of the invention. The general aspect of the Exhibition and Royal Albert Hall is well known from descriptions in the daily press; and we can at once proceed to chronicle that which is new in a technical sense. Messrs. Siemens, as usual, have contributed largely. The Pyrometer, by C. W. Siemens, F.R.S., has already been noticed in the pages of this journal. Dr. Werner Siemens exhibits two of his electrical inventions—the Electrical Distance Meter, an apparatus for obtaining a correct estimate of the position of distant objects, applicable more especially to the marking of the course of a hostile vessel approaching a battery of torpedoes, and to similar purposes. The other invention is the Dynamo-Electric Mine Exploder, the principle of which was described in a paper to the Royal Society in February, 1867. Mr. C. W. Siemens also contributes a model of furnaces for the production of cast-steel from iron ore, and a new Gyrometric Governor for steam-engines. Instead of the controlling pendulum hitherto employed, Mr. Siemens substitutes a parabolic cup dipping into water, and rotating about a vertical axis. Above the cup, the top and bottom of which are both open, are several vanes: the water, as the cup revolves, rises in the interior with the increase of rate, and, if the friction thus produced is not sufficient, overflows against the vanes, enforcing uniform velocity by a differential motion acting on the throttle valve of the engine.

Sir Joseph Whitworth contributes a measuring machine, a modification of that which obtained the Council Medal in 1851, capable of being read to the one-millionth of an inch. The principle is that of employing the sense of touch to aid that of sight, the gauge being made to pass between two perfectly true parallel planes, until the movement of the micrometer brings the surfaces so close that it is possible to feel the contact. This mode of delicate measurement follows from Sir Joseph Whitworth's invention of the method of obtaining a true plane surface.

Mr. J. Warsop exhibits an Aëro-Steam Engine, showing the application of the well-known fact that all water holds air in solution, and which acting as an elastic spring between the molecules of water, promotes ebullition. On this principle Mr. Warsop introduces, from an air-pump worked by the engine, a current of heated air into the water contained in the boiler. The air is heated by being forced through pipes placed in the flue or the smoke-box, thus utilising what has hitherto been waste heat. The air is passed into the water from a long perforated tube extending the whole length of the boiler, so that by the force of impact the whole body of water is constantly stirred and aërated. This agitation prevents the deposit of saline matter. Thus there is both a saving of fuel and a prevention of incrustation.

Messrs. Cooke and Sons exhibit a fine achromatic equatorial telescope (7435), of 10 inches aperture and 12½ feet focal length, with a clock in which there are arranged two separate trains of wheels driven by the same weight. One train is in gearing with a revolving fly, and also moves the telescope. The other drives an ordinary clock pendulum; this train having one wheel attached to an arm swung from a pivot on the frame, and shifting to the right or left with the pendulum. By this means the exact measurement due to a pendulum clock is obtained without any unevenness of movement.

Messrs. Elliott Brothers exhibit several new instruments. The Omnimeter, for surveying, consists of a theodolite telescope with a powerful vernier microscope. The telescope is pointed successively to two lines upon a staff of known length, and the arc described by the telescope gives the distance of the staff. The Planimeter, for estimating as accurately as possible the area of plans, resembles a large pair of compasses with a revolving disc at the joint of the legs, one of which is fixed, the other traced over the plan to be measured. The area is read off the disc, according to any scale to which the instrument has been set. Messrs. Elliott also exhibit Mr. Richards's steam indicator, which has been successfully employed in recording the variation in pressure of engines running at the highest speed.

Captain Le Boulengé contributes a chronograph for measuring very minute intervals of times. Two cylindrical bars, held up by electro-magnets, are allowed to fall freely in parallel vertical lines close together. One bar is of some length, and is sheathed with a covering of zinc; the other is short and drops upon a table almost as soon as released. In doing so, the blow struck upon the table liberates a knife, which indents a mark on the soft metal of the long falling bar. If the chronograph be applied to the measurement of the velocity of projectiles, the latter bar is set free when the bullet pierces the first screen, and the shorter bar when the bullet cuts the target. The distance through which the bar has fallen of course readily gives the time of the passage of the bullet.

Mr. T. E. Rowe exhibits a model of a Relume Signal Lamp, or a lamp which shall re-light itself automatically. It is well known that a compound straight bar will become bent by the action of heat, and will straighten itself again on cooling. Such a compound bar is fixed above the flame of the lamp, and a catch fixed to the end of the bar is held in place while the lamp is alight. So soon as the lamp is blown out, the bar begins to cool; when quite straight the catch is removed, liberating a spring which causes some matches to be ignited and light a second wick. This lamp is especially applicable to distant signal stations. Connected with the subject of light is the improved Jet Photometer of the late Mr. G. Lowe for the estimation of the illuminating power of coal-gas. It appears that when gas issues at a given pressure from an orifice of constant size, the height of the flame varies directly as the illuminating power of the gas. It amounts to the same thing if the height of the flame be kept constant, while the pressure is varied, ensuring a more delicate observation than could be made of the height of the flame. A table has been prepared showing the value of the light for the variable pressure as compared with the ordinary measure of sperm candles.

Messrs. Bergius and Whyte contribute the solution of a very difficult problem—the ascertaining of the depth of a marine sounding without any reference to the length of line attached to the sinking weight. As the pressure below the surface of the sea increases in proportion to the depth, means have been taken to record this pressure. The sounding weight is hollow, and a small tube communicating freely with the sea is carried nearly to the top of the apparatus. The compression of the air begins in this tube; the water soon rises to the top, and overflows into the chamber; there is no escape for the water so entering, and the measure of its amount, by means of a scale, shows the depth in fathoms to which the instrument has been sunk.

Mr. F. S. Duckham exhibits a hydrostatic weighing machine, constructed on the principle of the hydrostatic press, which will weigh up to 40 tons, though itself no larger than an ordinary spring balance. The object to be weighed is hung upon a piston, fitting in a small cylinder, and pressing upon the upper surface of some water; the amount of pressure, measured by a gauge, readily gives the weight.

Before leaving the building everyone passes to the enclosure in which Mr. Hodgson's wire-tramway is at work; this seems to be taking up and delivering its loads as indefatigably as when on the Brighton Downs, where the public first became acquainted with the results that can be achieved by this invention.

In this short notice it is impossible to enumerate all the improvements exhibited; the endeavour has been to show that although many complaints have arisen as to the paucity of scientific invention, they are certainly combated by the consideration of the value of the exhibits. The admirable reports of Professor T. M. Goodeve, M.A., Professor Abel, F.R.S., Lieutenant T. English, R.E., and Henry Sandham, Esq., on the several branches of science, will fully bear out this view.

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