THURSDAY, APRIL 20, 1882

ECLIPSE NOTES

In the following notes I propose to discuss certain points which in my opinion it is desirable to investigate as fully and as carefully as may be during the coming eclipse.

The magnificent volume which astronomers have received from America during the last year, in which are garnered all the rich results, or most of them at all events, collected during the eclipse of 1878, may really be said to have brought to a focus the chief points of study which are open to us during eclipses. I shall, therefore, use this volume freely in connection with the various branches of research. But still there are points of interest which lie outside this book, for, since the year 1878, I for one, at all events, have been driven to the conclusion that our then views of the chemical and physical constitution of the solar atmosphere require considerable modification to make them accord with the facts.

I have taken many opportunities of showing that the various phenomena observed on the uneclipsed sun are more easily explained if we assume our chemical elements to be dissociated by the transcendental temperature of the sun, than if we hold that their molecular construction is the same there as here.

This question is one, the settlement of which is so important if it can be settled, that if an eclipse of the sun furnishes us with tests, it is our clear duty not to neglect them. I believe that an eclipse does furnish us with two or three such tests, and with reference to one of them, as I wish in these notes to bring together the various statements on the subject which have been made, I will begin by quoting from a discourse delivered by myself to the Astronomical Society last May. (Revised from a report in the *Observatory*.)

"The chemical constitution of the heavenly bodies is a question which necessitates some amount of attention from astronomers. Twenty years ago the observations of Kirchhoff and Stokes enabled us to get glimpses into the chemical constituents of the Sun. Nine years since, though we were in full presence of elements with which we are acquainted, other facts had been registered which exercised the minds of some observers. Kirchhoff's view was that the substances with which we are acquainted were demonstrated in the atmosphere of the Sun by an exact matching, both as regards wave-lengths and intensity, with the lines of certain chemical elements which he employed. Fraunhofer had earlier noted the coincidence of the bright yellow line of sodium with the line D. But Kirchhoff showed that not only in the case of sodium, but in iron, magnesium, and cobalt, and several other substances, there were coincidences which went to show that what was good for sodium was good for other bodies. But nine years ago we had not merely the opportunity of comparing these bright lines with the spectrum of the Sun's atmosphere as revealed by Fraunhofer, but we had the opportunity of studying the spectra obtained by observing very small portions of the solar atmosphere in regions where we should expect an exceedingly high temperature, namely, the inner regions of the solar atmosphere—the regions of spots and the regions of prominences. When we began to tabulate the lines thickened, the thing began to be very much less clear; of the 460 iron lines recorded by Kirchhoff only three were observed in the prominences. Then, when we got indication of a change of refrangibility of the lines due to the motion of the solar gases, we found about the year 1869 that the thickened lines which indicated iron vapour in the spots were not brightened in the prominences, so that a great many questions were raised; and when those questions were raised the idea of decomposition at a high temperature seemed to arise also. I bring before you to-night the results of some purely astronomical inquiries lately undertaken by the Solar Physics Committee. Of course a great many physical inquiries have necessarily entered into the researches. But the astronomical inquiries have had this object in view, namely—given the fact that a high temperature can decompose an elementary body, what happens to the spectra of those bodies when we examine the Fraunhofer spectrum, the spectrum of spots, and the spectrum of prominences? We have had before us the admirable work of Professor Young in 1872, but the work only lasted a month We felt we wanted more facts; so what we have been doing at Kensington during the last two-and-a-half years has been to obtain the spectra of 100 sun-spots—not a perfect record of all lines thickened, but results we could compare with Tacchini's; because, for prominences, we had to depend on Tacchini's observations, observations confined to the brightest lines of the prominences. The Committee therefore attempted something which was quite modest, and contented themselves with observing only the twelve lines most affected in Sun-spots. The question was, where to take the lines; and it was obviously the wisest course to take them in the most visible part of the spectrum; so that for two-and-a-hal. years we have been taking the twelve most widened lines between F and D. I will only trouble the Society with one set of these observations. At the top of this diagram[1] we have carefully chosen among the Fraunhofer lines, the lines stated by Angström to be coincident with the bright lines of iron; and we have given these lines of different lengths, the length representing the darkness of the Fraunhofer lines. In the next horizon we have the actual observations of the iron lines given by Angström, who used an electric arc with thirty or more of Bunsen's cells. We compared the intensities, also represented by length, as given by Angström and as given in the sun. You will see a considerable disparity. Below, we have the lines of Thalén, who used a powerful induction-coil, and the lengths of these also represent intensities. Comparing the Fraunhofer lines and Thalén's lines, you will see a still further disparity between the two spectra. Below, in these 100 horizontal strata are all the observations of the spots taken during the last two years. The first point which strikes one is the enormous number of iron lines, both in the solar spectrum and in the iron spectrum, which are not affected in spots or storms. It is as if on a piano only a few notes are played over and over again, always producing a different tune. The next point is the inversion of the phenomenon. If you examine the lines, you see that every line has been seen without the others. That hard fact is one which really is very difficult to understand, and what strikes one is the marvellous individuality, so to speak, of each of these lines. They do not go in battalions, or companies, or corporal's guards, but in single unities. The great importance of getting these observations was not so much for the observations themselves as for the comparison it enabled us to make with other observations; and naturally the next thing to do was to get a long series of observations of the prominences, because the prominences are hotter than the spots. The spots are caused by down-currents when the Solar atmosphere brings vapour from the cooler regions. They are opposed to prominences, which are ejections from the heated interior of the Sun. We have arranged here the observations of the prominences by Tacchini, since 1872. Here we are dealing with one substance—iron—over a very limited portion of the Solar spectrum; and what is the result? First of all you will see a very much greater

[1] See vol. xxiv. p. 322, Fig. 35.

simplification. The hottest part of the Sun has given us the fewest lines. Next, there is not a single line in common. Passing then from the iron lines in the spots to the iron lines in the storms, we pass from one spectrum

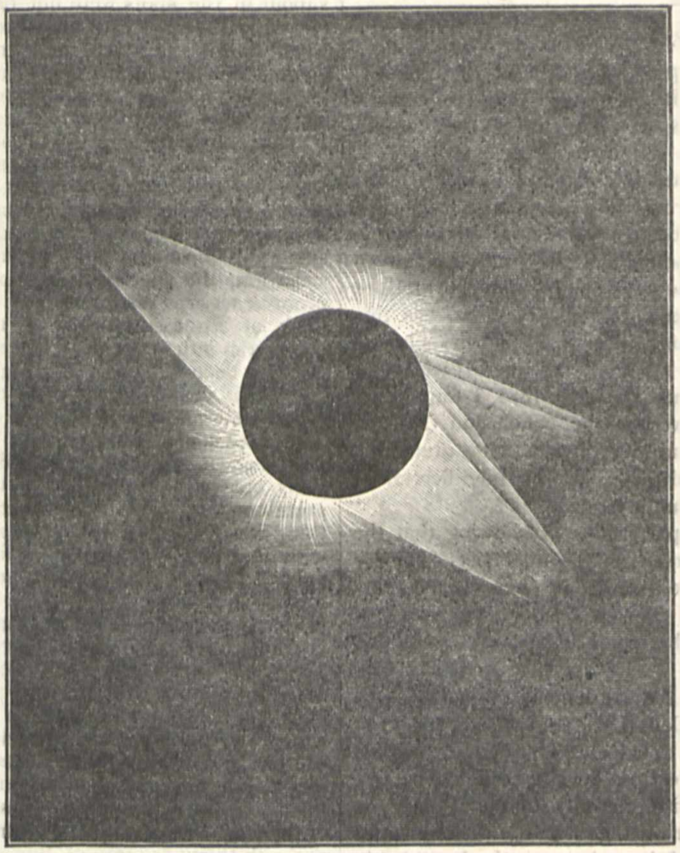


FIG. 1.—The Eclipsed Sun in 1878, from the photographs.

to another, and the two spectra are as distinct from one another as the spectrum of magnesium from that of chlorine or any other substance you please. I have ventured to put in red ink two other lines, because Tacchini

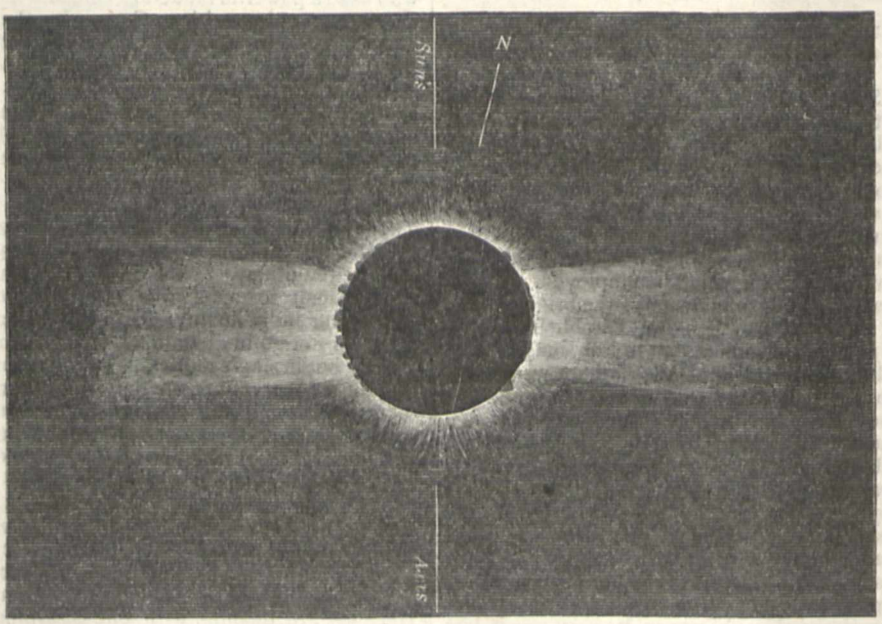


FIG. 2.—The Eclipsed Sun, August 29, 1867, as observed by Grosch at Colchagua, near Santiago.

found that about January 1873 the spectrum suddenly changed when the Sun was absolutely quiescent. There was no Solar rain, and we got the minimum of interference with local temperature. The iron lines vanished, and we got two new lines continued through a very long series of observations without any iron line at all; and these two lines have no Fraunhofer lines corresponding with them, nor do they appear in the spectrum of any chemical substance.[1] These phenomena are the last which one would expect. We can understand that differences in the quantity of the iron vapour present would make a certain difference in the spectrum; but we are driven to something quite independent of any change in the quantity of the iron vapour present. What, then, are we driven to? We see with every increase of temperature, passing from the general absorption of the sun to the absorption of the spots and to the radiation in the flames, increased simplicity, just as if a chemist were to talk to us about the action of temperature on substances which he has under control, and say the function of temperature was to simplify. Why, then, if this is the result of the working of temperature, why should not this simplification be due to the breaking up of the iron, if such iron exists at the exterior of the sun's atmosphere, into its finer constituents, as by the solar currents this iron is carried down into more highly heated solar regions? It has been stated there is no necessity for any view of this kind, but that the molecules of iron give out these vibrations, just as a series of bells vibrate differently according as they are struck in different ways. Fortunately, however, we need not have remained so long in doubt on a matter of this kind, because, as early as 1869 observations were made which showed that when the sun is in an excited condition iron vapours are among those vapours which show their motion by a change of refrangibility. So that we had the opportunity of learning whether these really were identical bells, so to speak, being struck in different ways. I think you will acknowledge

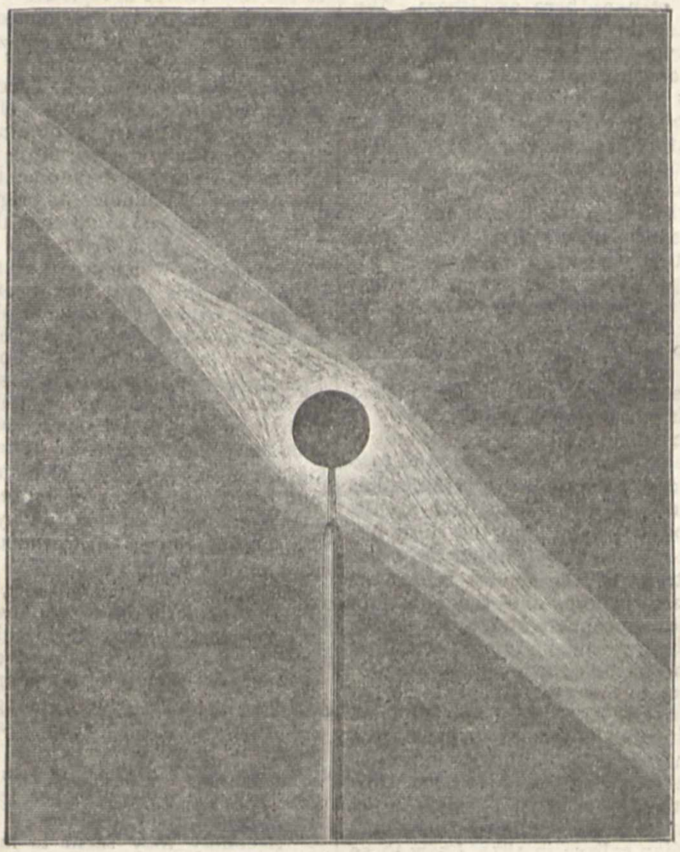


FIG. 3.—Prof Newcomb's observation (pp. 103, 104).

that if we are dealing with bells struck in different ways, however much the spectrum may vary, the molecules should be going with the same velocity. We found, however, when we came to make these observations, that the bells were going with different velocities; so that it cannot, by any possibility, be the same bells which on being struck give us those various notes. In another part of the spectrum these motions have been observed with very much greater success, for the reason that in that other part there are more lines which are observed to indicate considerable motion in Sun-spots. Limiting our observations to lines visible in the same field of view and at the same moment of time, it is a mere toss-up which line of iron shows a descending motion of thirty miles a second, and which line of iron does not move at all, either up or down; so that I think we are justified, so far as these observations go, in considering that there is great probability in favour of the view that we have in these lines, seen in spots and storms, the lines due to the constituents of iron, and not to iron itself, which are competent to resist the transcendental dissociating energies of these hotter parts of the Solar atmosphere. If so, we can bring it to the test; for if we accept any theory of evolution at all, we must imagine that, as our own Earth has cooled down, the Sun is cooling down; and if chemical forms are produced by that cooling, the complexity must be increased by reduction of temperature. If that be so, every reduction of temperature will be accompanied by increasing complexity of chemical forms, and then the highest temperature will be that condition in which we shall have the smallest number of elementary groupings of early forms. Dr. Huggins's work on the stars entirely justifies that view; and I want to point out the kind of test to

[1] See vol. xxiv. p. 368, Fig. 39.

which I allude. If these early forms really exist at the present moment in the hottest portions of the Sun, the spectrum of which is marvellously like that of Sirius, we ought not to expect these early forms to be confined to one of our earthly constituents. But what are the facts? The facts are very precise indeed. On this map we have the result of all the individual observations of the spots and the flames to which I refer. What we find is, that to every prominent line in the spots and in the storms, although these two have no line in common, there is a line common with our present instrumental appliances to iron, vanadium, and chromium, another common to iron and titanium and so forth; and the lines shown by Angström and Thalén, as common to two or more elements, are precisely those lines which are thickened in spots or are brightened in storms, so that the view we have here of early forms of matter is absolutely justified by this massing of lines here and there. We have been able to increase the number of 'basic' lines over this region by observing the lines constantly thickened in the spots. This does away at once with the idea that all these basic lines arise from the fact of the lines being double. For if they are to be doubled there is no reason why the 60 per cent. of lines neglected by the spots and the storms should not have been double lines. But neither Angström, nor Thalén, nor myself have picked up one of these basic lines when we passed from the atmosphere of the spots or the special atmosphere of the flames. Now, there is a moral to this, if you will allow me to enforce it. There is an eclipse of the Sun next year, lasting only, I am sorry to say, a minute and a very few seconds; but there is to be another the year after, lasting nearly six minutes, but it happens to be in a part of the world where it is always afternoon. In the observations of the future we must pay attention to these lines which have been picked out by nature herself in these spots and prominences. If I observed either of these eclipses, I should be content to fix my instrument on three iron lines between 4900 and 5000 ten millionths of a millimetre, because, of these three lines which are in the Fraunhofer spectrum, two have always been seen in spots without the third, and the third has always been seen in the prominences without the other two. If, then, the spectrum of the flames represents the lowest part of the atmosphere, and the spectrum of the spots represents the atmosphere above the flames and below the corona, than we ought to see these lines different in the corona, and in the corona we ought to see the lines which are dropped in these two regions. Of the twelve lines between 4,900 and 4,957, only one is picked out by Thalén for intensification, and that particular line is the line seen alone in the region of the prominences. There are eleven lines which are absolutely untouched by Thalén, showing, that absorption must be proceeding somewhere; and it is most interesting to determine where it is going on. In the Indian eclipse, in 1871, I saw these lines reversed before totality. I saw as it were hundreds of lines; but if I had confined my attention to the three lines I should have got a better idea of what the magnificent flashing out of those lines meant. It has been called the reversing layer; but I do not now believe it is the reversing layer for a moment; for, when it comes to be examined, we shall probably find that scarcely any of the Fraunhofer lines owe their origin to it, and we shall have a spectrum which is not a counterpart of the Solar spectrum."

As further thought led me to believe that this method of observation was one of the most important that could be employed next May, I laid great stress upon it in a memorandum which I subsequently submitted to the Government Committee on Solar Physics, and I pointed out to them at the same time that from what Captain Maclear and myself observed in India in 1871, there was a great probability, that on this question facts might be collected, not only at the exact moments of disappearance and reappearance of the sun, but perhaps even for two or three minutes both before and after totality, by keeping the slit of the spectroscope very carefully on a point where the cusps were narrowest.

The memorandum to which I have referred runs as follows:—

"The total eclipse of the sun which takes place in May next year will be visible in such an accessible region, that it is to be hoped that the precedents of 1860, 1870, 1871, and 1875 will be followed, and steps taken to secure observations, the more especially as the eclipse will happen somewhat near to the period of maximum sun-spots, and will allow of a comparison being made with the results obtained in India in 1871.

"There is one new point (it is not necessary now to refer to the importance of registering the ordinary phenomena) to which I beg to invite the attention of the Committee.

"The discussion of the sun-spot spectra recently observed at Kensington, and of the prominence spectra observed at Palermo by Tacchini, since 1872, throws some doubt upon the validity of some of the conclusions based upon the results obtained by the English and American Government Eclipse Expeditions in 1870.

"In that year, at the moment of the disappearance of the sun, a large number of bright lines was seen to flash out, and it was supposed that these lines composed the spectrum of a thin layer near the sun, and were those the reversal of which produced the lines of Fraunhofer.

"Hence this layer has been termed, and generally accepted to be, the reversing layer. The conclusion seemed to be in harmony with the results obtained by Dr. Frankland and myself, who gave reasons for showing that the region in which the absorption of the elementary bodies of greater atomic weight than hydrogen, magnesium, and sodium must be below the chromosphere. This view was put forward at a time when the elementary nature of the so-called elements was never questioned, and before any of the recent results had been obtained.

"The observations made by the Government Eclipse Expedition which went to India in 1871, showed that this flashing out of lines was a real phenomenon; but as the observation was a general one, and as during the eclipse the Fraunhoferic lines were invisible, there was no absolute demonstration of the identity of the two spectra.

"The facts, now beyond question, that *quâ* the same element, the spectra of spots and flames differ, and that the spectra differ widely among themselves, throw great doubt upon the conclusion to which reference has been made.

"First they seem to indicate that some of the absorption takes place at a higher level than that occupied by the so-called reversing layer.

"Secondly they seem to indicate that many of the brightest lines seen during the flash to which reference has been made may be those seen thickened in spots or intensified in the prominences, although they do not occur except as excessively faint lines among the Fraunhoferic lines.

"In short, in 1870, the fact that the spot and prominence spectra are so widely different from the ordinary solar spectrum, had not received the attention it must receive in the light of the most recent inquiries, and it was taken for granted that because a large number of lines was seen, that therefore they occupied the same positions as the large number of lines which compose the ordinary solar spectrum.

"The recent work seems to show that the complete absorption spectrum of any one element is produced, not at one level, but at various levels, the absorption of all the levels being added together to give us the complete result.

"If this be so, the lines seen in the flash will not all be Fraunhoferic lines with the ordinary intensities.

"A crucial test, which can only be applied during an eclipse, and with difficulty then, will be to observe what happens during the flash to those lines which are specially picked out for intensification in spots and flames. We might expect to see the lines untouched in spots, the lines thickened in spots, the lines brightened in prominences, stretching to different heights.

"They would all appear to rest on the moon's limb, or on the sun's limb, if the cusps can be observed, because we are dealing with the section of a spherical mass, or rather, perhaps, of zones of concentric spherical strata.

"To apply this test under the best conditions, adjacent lines should be taken with cross wires, or some equivalent arrangement adjusted on the corresponding Fraunhoferic lines before totality.

"The iron lines at 4918·0, 4919·8, and 4923·1 will be the best to observe for this purpose, as they are close together, and two are always absent from prominences, and one is never thickened in spots."

When it was decided that an attempt should be made to secure observations of the coming eclipse, the next thing to do was to try to get over the tremendous difficulty that we have always experienced, namely, that during the eclipse itself, the sun's light, and therefore its spectrum, were absent, so that our familiar scale of reference is lost. This is at last got over in a manner so simple that the only wonder about it is that it has not been thought of before. I allude to the employment of a photograph of that part of the solar spectrum which we want, instead of micrometer wires or any other more elaborate means of determining positions, and this method I have already tested, and it works remarkably well.

What is requisite is that instead of a camera replacing the eyepiece it should really form part of it. The plate can be taken away and the eyepiece may be used in the ordinary manner, or a sensitive plate may be placed in it, and a photograph taken. It may then be taken out and developed, half of it wiped off before it is exactly replaced in its original position, and then we have a field of view, the eyepiece never having been separated from the camera during the whole of this time, half of which is occupied by the photograph, the other half with the spectrum of that part of the solar atmosphere which it is desired to study.

The instruments to be used during the eclipse—both telescope and spectroscope—will be identically those with which Capt. Maclear and myself observed the bright lines in 1871, so that instrumentally the chances are good.

I have already pointed out that it is necessary that the slit should lie on the narrowest point of the cusps. To secure this a 3⅜″ finder of exquisite definition has been solidly fitted to the telespectroscope with adjustments easy of application which shall insure this result, and in order that the observations may be continuous both in the presence and in the absence of the sun, a diagonal eye-piece with a prism twice the usual size, is employed. This slides easily in two grooves. Half of it is silvered, half of it not, and at the instant of totality the silvered portion is thrown into use.

It is hardly necessary to add that the slit of the spectroscope can be made to lie at any angle from the normal.

So much, then, for one possible test of the new views. There is another—not perhaps quite so direct, but one which it will be still of interest to make. Since 1871, when Janssen made the first observation of this nature, those observers who have studied the spectrum of the corona under good conditions with small dispersion have seen some dark lines as well as the ordinary bright ones, and it has been assumed for the most part that these dark lines are simply the dark lines of the ordinary sunlight reflected to us by particles in the solar atmosphere.

The possibility of putting this question at rest in the most absolute manner by comparing the spectrum of the corona with a photograph of the green part—that is to say, the most luminous part of the solar spectrum (for too much light must not be expected), renders this observation one of importance to make, and, thanks to Capt. Abney's recent researches in the science of photography, it is now as easy, however confusing it may be to those who believe in chemical rays, to obtain a photograph of the green as of the blue, and this will be done before the eclipse.

There is reason to think that if the new views have any truth in them the spectrum of the corona may—I do not say must—give us the ordinary solar lines changed considerably in intensity, but it is probable that this observation will be a delicate one at the best.

But more than our views have changed since 1878. The photographic attack now requires seconds only where formerly minutes were wanted. Nor is this all: the red end of the spectrum awaits a record which it is now easy to secure. Indeed, thanks to Capt. Abney's skill, plates have been prepared which it is hoped will grasp the red and green and blue light with equal vigour, so that one can now more than dream of a permanent record of the whole spectrum from the invisible violet at one end to the invisible red at the other.

We got the first photograph of the spectrum during an eclipse by means of instruments constructed in 1875 for the Siam eclipse in that year. In these instruments I employed a method first used by Fraunhofer, to save as much light as possible. The corona was its own slit and the prism was placed in front of the object-glass, and the dispersion of the prism used was small, because the method was new, the plates were slow, and we were anxious to secure something. We now know that we may safely go ahead, and a prism 3½ inches square in the side, of 60°, will be placed in front of a lens of 22 inches focus.

The length of the spectrum, if all goes well, will be four inches, including the infra-red, which Capt. Abney believes will be recorded in one minute's exposure, and this will be available in an eclipse of 72 seconds.

These extremely rapid plates enable other attempts to be made which formerly would have been considered hopeless. The ordinary photographs of the corona will be taken (by a lens of 5 feet focal length and 4½ inches in diameter) on plates sixty times more rapid than those prepared on the old process. This fact must be insisted upon, because it is evident that the shortness of the totality during the present eclipse is not such a drawback as it once would have been.

Another attack will be as follows:—An image of the sun will be thrown on the slit of a spectroscope by means of a heliostat and condensing lens. The size of the solar image thus obtained will be about ⅛ of an inch. The beam of light will be dispersed by a flint prism of 2 inches face, of 60°, and the spectrum will be brought to a focus on a sensitive plate by a lens of a mean focus of 9 inches. An attempt will be made to secure the whole of the spectrum, and for this purpose the plate requires to be inclined at an angle of 40° to the axis of the lens. The spectrum which it is hoped to obtain by this arrangement would have required an hour's exposure some years ago.

With this instrument attempts will be made to secure a photograph of the flash of bright lines at the beginning of totality, and the spectrum of the corona during totality, an arrangement being made for a comparison solar spectrum after totality by shutting off half of the slit.

So much, then, for the work suggested by applying views and methods which have been broached since the last eclipse.

The remarkable form of the corona, and its still more remarkable extension in 1878, and its great variation from that seen in prior eclipses with a single exception—that of 1867—will render observations of the form and extent of the corona in the present year of the highest importance, even if we had not Dr. Siemens' suggestive hypothesis to lend a more than usual interest to it.

I give an illustration (Fig. 1) copied from the American volume, which I owe to the courtesy of the Superintendent of the Naval Observatory at Washington, to show how my own observations of that eclipse, an account of which was sent to NATURE from America at the time, have been borne out by a discussion of the photographs. Side by side with it, in order that the equatorial extension and the almost identical tracery at the poles can be seen, I give a copy of a drawing made in 1867, both sets of observations having been made four years before the sun-pot maximum (Fig. 2).

Here, indeed, we have food for thought; for the currents in the solar atmosphere, revealed by these drawings, seem to be exactly those demanded by Dr. Siemens; and indeed, his hypothetical diagram which appeared in NATURE a few weeks ago, should be compared with them, in order that the points of resemblance may be grasped.

With reference to the other drawing (Fig. 3), which shows the remarkable observation made by Prof. Newcomb, I cannot do better than make the following quotation from the volume in question:—

"It had always seemed to me that the visual study of the faint outlying portions of the corona would necessarily be interfered with by the brilliant interior portions unless the view of the latter were intercepted. I therefore made preparations to repeat the experiment unsuccessfully attempted at Desmoines in 1869, of hiding the central corona by a screen about 1° in diameter, and examining such portions as might be visible outside of it. The screen now used was made of wood, about 12 inches in diameter, and was mounted on top of a telegraph pole which was set on the elevated ground to the west. The altitude and azimuth of the sun at the moment of central eclipse were carefully calculated, and the screen fixed in such a position that when viewed from the top of a stake driven in the ground alongside of my telescope it would cover the eclipsed sun. The angular diameter of the screen as measured with a sextant from the stake was 57′, its distance was about 60 feet. As this would cut off about 12′ of the corona all round the moon I considered it ample for the purpose, but the results showed that it might well have been somewhat larger.

"I remained in the dark room until about three minutes before the commencement of totality with the view of having my eyes as sensitive as possible. I then walked to the telescope, keeping my eyes partially protected from the light. The lurid colour of the landscape was very striking. The light seemed no longer to be that of the sun but rather to partake of the character of an artificial illumination. This appearance is very readily explicable by the fact that the light coming only from the limb of the sun belongs principally to the red end of the spectrum. As the last ray of sunlight was disappearing I stepped to a stake driven into the ground, the top of which marked the point from which the sun would be entirely hidden by the screen. A bright corona was plainly visible all round the screen, although a portion 12′ from the limb of the sun was entirely cut off. My attention was immediately attracted by a faint blush of light, extending out on each side at an angle of about 45° with the horizon, each end terminating in a long narrow ray. I made a very careful estimate of the length of these rays as 6° from the disc. They shaded off by insensible gradations, and struck me as having a great resemblance to a representation of the zodiacal light on a reduced scale. They were to all appearances continuous with the corona. With a view of judging whether their direction coincided with that of the ecliptic, I tried to judge whether the western one pointed towards the planet Venus, then plainly visible near the horizon. Its direction was apparently very slightly below that of the planet.

"The outlying portions of the corona other than those rays were extremely irregular; that is, there were several rays and other irregularities extending out in different directions. As these were common phenomena, I took no note of their details." J. NORMAN LOCKYER

April 18

(*To be continued.*)

PROF. WIESNER ON "THE POWER OF MOVEMENT IN PLANTS"

Das Bewegungsvermögen der Pflanzen: eine kritische Studie über das gleichnamige Werk, von Charles Darwin, nebst neuen Untersuchungen. Von Julius Wiesner. 8vo, pp. 212. Three Woodcuts. (Wien: Hölder, 1881.)

BEFORE attempting to reply to some of Prof. Wiesner's criticism, it is a pleasure to record my appreciation of the courteous spirit in which his book is written, and the uniformly respectful tone which he employs towards my father. His criticism is so extensive that there is hardly a single point of any importance in "The Power of Movement in Plants" with which Prof. Wiesner agrees. Yet in spite of this far-reaching difference of opinion, he is good enough to express himself warmly as to the value which the book possesses.

Wiesner devotes a good many pages to Circumnutation, and as this phenomenon and the theories connected with it form an important part of "The Power of Movement in Plants," I shall begin with this question. In the first place Wiesner finds fault with one of the methods employed by us in our observations on circumnutation, and gives a diagram (Fig. 3, p. 161) which shows that the method may lead to false conclusions. In the method of observation criticised by Wiesner, the position of the plant at any moment was determined by making a dot on a glass plate in such position that it was in a line with a mark on the organ whose movements were to be observed and with a stationary mark behind or below it. This method is obviously open to objections, and we never imagined it to be strictly accurate; but Wiesner shows that it is possible, by taking some pains, to make it very inaccurate.[1] But the arrangement given in Wiesner's diagram is one which no one would think of employing, if he wished to make the best of the method; in all our experiments we tried, as far as possible, to avoid the extremely oblique arrangement chosen by Wiesner. This may to a large extent be ensured by placing the fixed mark as near the base of the plant as possible, *i.e.* when tracing on a horizontal plane the movements of a vertical organ; if when the stem of the plant is vertical, the index attached to the plant is vertically above the fixed mark, then growth vertically upward will be represented by a single dot. By taking reasonable pains in making some such arrangement, I still believe that no very serious error will be introduced.

In the second method of observing circumnutation, a glass filament bearing two small sights was fixed to the plant, and its position was recorded by making a dot on a glass plate in line with the two sights. Against this method no such serious charges can be brought, and it was largely used, and was, as matter of fact, preferred by us. The methods given by Wiesner are in many ways, no doubt, preferable to those employed for "The Power of Movement," and are in principle the same as that described by me in the *Bot. Zeitung*, 1881, p. 473, which consisted in estimating the actual position of the moving point by means of a vertical microscope; Wiesner has also employed a vertical tube without lenses. In the latter case, the position of the tube is varied until the cross wires are vertically over the observed point, and the various positions of the cross wires at successive intervals of time can be recorded in a way we need not stop to describe. In the case of the microscope the movements are recorded by means of the eye-piece micrometer. This

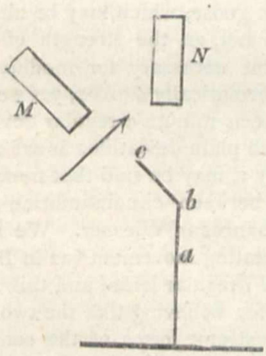


FIG I.

FIG. 1.—Diagram representing a plant which is supposed to increase in length by the portions *a b*, *b c*. M and N represent microscopes for observing the direction of growth.

method requires to be treated fairly and not to be burlesqued; it presupposes a knowledge of the general direction in which the growth of the organ under observation is proceeding, and the microscope should be parallel to this direction. If as in Fig. 1 a plant grows straight from *a* to *b*, it will be seen by the microscope M, moving in the direction of the arrow;[2] growth from *b* to *c* will not be perceived as lateral movement, but as growth towards the observer, and the same is true *mutatis mutandis* for the microscope N. Thus if we estimate the whole lateral movement which has taken place during the growth from *a* to *c* by the two microscopes M and N, we shall see that they give reverse results. It will therefore be seen that the same general knowledge of the direction of growth is required for Wiesner's and for our method, and that unless this knowledge is properly utilised, either method can be made to give wrong results.

In a notice like the present, it is impossible either to give or to attempt to answer all Wiesner's criticisms, and in what follows I cannot do more than notice what seems to me the more important points. Wiesner states that circumnutation is not nearly so general a phenomenon as we believe it to be. That growth in a perfectly straight line (with a qualification to be mentioned hereafter) is found to occur, and therefore that circumnutation is not an essential quality of growth, is not, in fact, an "Urbewegung" (Primordial-movement).

Let us first consider the circumnutation of roots. The observations given in "The Power of Movements" on this head were made by two methods. In some cases a glass fibre (P. of M., p. 10) was fastened by shellac-varnish to the tip of the root, and the movements of the end of the glass fibre were then recorded by making dots on a glass plate. In other cases the tip of the root was made to inscribe its course on the smoky surface of an inclined glass plane. By this means curious wavy and broken lines were drawn on the glass plates, which we believe to afford evidence of circumnutation. Wiesner confirms the results, but differs entirely in the conclusions which he draws. He believes that the coating of soot is the cause of the apparent circumnutation. He believes that the soot acts injuriously on the root, and causes it to curve away from the injured side, by means of the specialised sensitiveness which, as we have shown, enables a root to curve away when the tip is injured by caustic, &c. He supports this view by an experiment in which (Wiesner, p. 166) the inclined glass plate was coated with *semen lycopodii* instead of soot, and he always found that the course described was a straight unbroken line. This experiment is strongly in favour of Wiesner's view; but, on the other hand, I fail to see how Wiesner's explanation applies to the lateral movements of the root[1] which gives the waviness to the course traced, although it may legitimately be used to explain the movements away from the smoky surface which cause the line to be often a broken one.

The other observations on the circumnutation of roots recorded in "The Power of Movement" are set aside by Wiesner among other reasons, because they were made by fastening a glass filament to the tip of the root, a method which, as he states, disturbs the growth of the root and causes an apparent circumnutation. Moreover, Wiesner's own observations, made with a microscope, lead him to disbelieve in the existence of circumnutation in roots. Wiesner says (p. 174) that the circumnutation of roots is due to the antagonism between geotropism and the natural tendency to curvature existing in the root (Sachs' curva-

[1] It may, however, be observed that if the plant in Wiesner's diagram had grown straight on in the original direction, the tracing given would have been a straight line, and we should have drawn the correct conclusion that the plant was not circumnutating.

[2] Or rather in the reverse direction, owing to the reversal of the image by the microscope.

[1] The lateral movements are probably explained by Wiesner as the result of the antagonism of geotropism and that tendency to nutation which we call Sachs's curvature.

ture). He believes that first one and then another of these forces gets the upper hand, so that the tip of the root moves backwards and forwards.

To this explanation there are several objections. (1) If the root is pointing vertically upwards, as in some of Wiesner's experiments (p. 174), any tendency to spontaneous curvature in the root will assist geotropism, so that any circumnutation that may occur is inexplicable as the result of antagonism of the above-named forces. (2) Wiesner states (pp. 169, 174) that the movements did not take place in one plane: his explanation does not account for this fact. (3) He states (p. 172) that the "so-called" circumnutation takes place in the part of the root which grows most quick. But "Sachs' curvature," which he assumes to be one element in the "so-called" circumnutation, takes place at the base of the root. It is true that it may be said in favour of Wiesner's view that the root may be carried out of the vertical by Sachs' curvature; and if this were the case geotropism would bring it back to the vertical, and thus the direction of the circumnutation would correspond more or less with the plane of Sachs' curvature. But this would not account for movement in any other plane.

Circumnutation of Stems.—Wiesner concludes that there exist stems of plants which certainly do not circumnutate at all. This statement he founds (pp. 176, 177), however, on observations on plants whose line of growth is not a straight line, but is broken by lateral oscillations in various directions. The lateral movements being small and irregular, however, are held not to constitute circumnutation. In one case the growth of the grass seedlings under observation was at first accompanied by the above-mentioned very minute and irregular curvatures, but afterwards seems to have circumnutated in our sense of the term. I shall return again to these cases.

In the case of stems which show the S-shaped curve of "undulating nutation," Wiesner observed the tip (Faba, p. 178) move backwards and forwards in the plane of curvature; this he explains as the summation of the apogeotropic curvature of the lower part of the stem with the nutation (*i.e.* curvature) of the upper part.

Unless I misunderstand Wiesner in this point, it seems to me his explanation does not meet the facts; for I fail to see how *summation* of two curvatures can produce anything, except a variation in the rapidity of the curvature. I cannot see how it accounts for any movement in the opposite direction.

Nor again does Wiesner give any explanation of the movements which he observed in the plane at right angles to the nutation-plane. It should, however, be mentioned that in the epicotyl of the bean, Wiesner observed perfect straight growth after the undulatory nutation had ceased (p. 178).

Wiesner's observations on heliotropism (p. 182) in connection with circumnutation do not call for any special remark. He seems not to have taken the precaution to expose the plants experimented on to a *dull* light, and the plants consequently curved in nearly or quite straight lines towards the light, as occurred in our experiments.

The movements of the flower-head of the daisy (p. 183) Wiesner puts down to the effects of the weight of the flower-head. He also assumes that the circumnutation which he observed in a flowering spike of a Plantago is due to the irregular disposition of the florets on the inflorescence.

Circumnutation of Leaves.[1]—On this point Wiesner's views are briefly:—(1) Some leaves grow in absolutely straight lines without circumnutating. (2) He confirms the *facts* observed by us, namely, that the tip of the leaf does describe the complicated figures described by us as circumnutation, but he interprets the facts differently. He believes that the complicated forces acting on the leaves, viz. epinasty, apogeotropism, apheliotropism, influence of weight, &c., working in antagonism to one another, and alternately getting the upper hand, produce the movements in question.

This argument is one of the most important which Wiesner makes use of, and a careful consideration such as it deserves would require further experiment and observation. It is obviously difficult to distinguish between circumnutation modified by the contending forces, and the same contending forces acting on an organ without circumnutation. One feature in our observations is the almost constant presence of movements in a horizontal plane, movements therefore which cannot be produced by any of the contending forces above described. Against the numerous cases in which sideway movements occur, it may be mentioned that, according to Wiesner, cases occur in which no lateral movements can be observed (p. 192).

This will perhaps be a convenient place to discuss the minute irregular disturbances which Wiesner usually found to exist even where the organ did not properly circumnutate. Let us for a moment compare circumnutation with variability. Modifications of organs are brought about by the summation of small variations in certain directions, and thus we rightly consider variability as the necessary groundwork for modification. But we find some animals, *e.g.* the common goose, which may be almost said not to vary, yet we do not, on the strength of this fact, assert variability is not necessary for modification. No two organs are mathematically similar, yet we cannot draw a distinction between minute irregular deviations from the normal, and such plain deviations as are called variations. In the same way it may be said that no fast and firm line can be drawn between circumnutation and the minute irregular disturbances of Wiesner. We have shown that a true circumnutating movement (as in Brassica) is made up of very small irregular jerks, and this may be given as another reason for believing that the two kinds of movement are only extreme forms of the same phenomenon.

In summing up what he has to say on the subject of circumnutation, Wiesner says (p. 202) that the movements described by us as circumnutation are either disturbances of growth or they are produced by combinations of antagonising forces, *or they are identical with the revolving nutation of climbing plants.* From Wiesner's brief manner of dismissing the last mentioned class, it might be supposed that it has little or no bearing on the question. But this is far from being the case; it is precisely this class to which we attach importance. There can be no doubt that revolving nutation of climbing plants is a development or exaggeration of circumnutation. In the stolons of the strawberry we

[1] It is curious that Wiesner (p. 186) recommends the use of glass fibres affixed to a stem for observing circumnutation, while on p. 187 he suspects that circumnutation of leaves is disturbed by attached glass fibre.

have a mode of growth which may almost be said to be halfway between circumnutation and the revolving nutation of climbing plants. The scattered distribution of climbing plants throughout the vegetable kingdom proves the wide distribution of a form of growth from which revolving nutation is developed—and this form of growth is circumnutation. This widespread form of circumnutation cannot therefore be dismissed as Wiesner has done, since such a treatment of it is quite beside the question.

General Mechanism of Movement.—In "The Power of Movement" we have spoken of the movements of plants as being due to difference in turgescence and in ductility on opposite sides of the moving organ. In it we have pointed out that it is more correct to look at the difference in turgescence than to difference of growth as the primary cause. In this statement "growth" was meant to mean alteration in size due to intercalation of solid particles. If a turgescent stem is allowed to bend heliotropically and is then placed in a salt solution strong enough to destroy its turgescence the heliotropic curvature is in large part destroyed; showing that the curvature at any given moment is largely due to differences in turgescence. Wiesner (p. 33) made the following experiment: he took five seedlings and found that when placed in salt solution (10 per cent.) they became shorter on the average by 1·9 mm.; five other similar seedlings he allowed to grow for 4½ hours, during which time they increased in length by 6·2 mm. (average), Wiesner argues that if the whole increase in length during 4½ hours were due to turgescence, then the shortening caused by salt solution ought to = 6·2 mm. + the original 1·9 mm. which was shown to be in a turgescent state by the former experiment; this, however, was not the case. It would be almost as reasonable to measure the length of an epicotyl in a dormant seed, and to expect that after germinating and growing for a day or two the young plant should collapse in salt solution to the size of the rudiment which existed in the seed. I imagine that it is a generally received opinion, and one which does not require Wiesner's experimental demonstration, that increase of length by turgescence and the intercalation of solid matter proceed simultaneously.

The question, however, need not be considered in further detail, for on this point there is practically no difference between Wiesner's and our own view; he says (p. 35) that "growth is from the first a combination of several processes occurring simultaneously, of which however Turgor is at first the governing one (*vorherrscht*)." This view is the same as that of Sachs ("Lehrbuch," Eng. Tr., p. 712), who describes the interaction of turgescence and intercalation.

Heliotropism.—Wiesner's criticism on the new matter contained in our book with regard to Heliotropism is prefaced by a discussion of some length on the nature, &c., of the phenomena of Heliotropism. There is only one point in this discussion which I wish at present to call attention to. Wiesner holds to De Candolle's explanation of heliotropism, namely, the purely mechanical view that the convex side grows more quickly, simply because it is in shade. As this view does not account for apheliotropism since at least some apheliotropic organs also grow more quickly in darkness than in light, Wiesner assumes (p. 55) the existence, in the fibro-vascular bundle, of negatively heliotropic elements, whose growth is assumed to be favoured by light. It is a pity that this theory has not been at least partly tested by comparing the rate of growth of unicellular apheliotropic organs in light and darkness.

Again the theory seems to require that all apheliotropic organs should be positively heliotropic in light o low intensity; and this, as far as I know, is not the case with the roots of *Sinapis alba*, which I have shown ("Arbeiten des Bot. Inst. Würzburg," Bd. ii., Heft. 3) to be apheliotropic even with very dull light. But I do not lay great stress on this argument, as Wiesner's theory seems to me to rest too much on assumptions to be at present capable of being discussed.

Transmission of Heliotropic Stimulus.—In "The Power of Movement" (Chap. ix.) facts were given that seemed to us to show that one part of an organ may bend heliotropically owing to the illumination of another part, and that, therefore, a heliotropic stimulus is transmittable from one part of an organ to another. The experiment which led to this conclusion is as follows:—A number of small glass caps were prepared, some of which were left transparent, the others were painted black. These caps were then slipped over the tips of seedlings of Phalaris (canary grass), which were thus prevented from bending towards the light, although they did so when the unpainted glass caps were used. The same experiment was made with other seedlings—cabbage among the number. From these results it was inferred that the illumination of the upper part of the plant was necessary to make the lower part bend towards the light; or, in other words, that the heliotropic curvature of the lower part depends on some influence transmitted from the upper and illuminated part. In the case of cabbage seedlings we found that if the lower part is darkened, while the upper is illuminated, no heliotropic curvature of the lower part takes place. Thus we believed that the lower part is in some degree heliotropic, independently of the illumination of the upper part, although the latter is the most important factor in the illumination of the plant. This seems to have been misunderstood by Wiesner, whose criticism is largely directed against what he believes to be our view, namely, that a heliotropic stimulus can be transmitted to a part of an organ which is not itself heliotropic. This misunderstanding on the part of Wiesner is no doubt due to a want of clearness in what we have said on the subject.

Wiesner speaks with great positiveness on this subject, and asserts that he has shown to demonstration that our experiments do not justify our conclusions. He believes that the bending of the lower part of the plants observed in our experiments is due to what he calls "Zugwachsthum" (p. 72), that is to say, the effect of the weight of the upper part of the plant acting on the growth of the lower part. We have discussed the question whether the curvature of the lower part of the cotyledons of Phalaris can be due to weight, and have shown conclusively that it cannot be so ("Power of Movement," p. 469), but Wiesner makes no mention of this experiment. Pots of Phalaris seedlings were placed on their sides so that the cotyledons were horizontal and were at right angles to the direction of the incident light from a bright lamp. Under these circumstances they became bent close to their bases, nearly the whole cotyledon being thus directed towards the light.

This experiment I have repeated since reading Wiesner's book, and have found the results to be the same. The conclusion is inevitable and is in this case absolutely destructive of Wiesner's theory of "Zugwachsthum."

This theory he grounds on the following experiment (p. 69), in which he makes use of Sachs's method of observing heliotropism:—Seedlings growing in small vessels are fixed in the place of the minute-hand of a large clock, so that each seedling is at right angles to the axis of rotation, and rotates like the hand of the clock; they are then illuminated by light which is parallel to the axis of rotation, and therefore each seedling has one side constantly illuminated by light striking it at right angles. Owing to the constant rotation the effect of weight is eliminated, and thus any curvature which occurs cannot be due to "Zugwachsthum." Wiesner states that whereas the seedlings on the klinostat (Sachs' name for this instrument), were only curved in their upper parts; plants growing normally without being subjected to rotation were curved down to the ground. This seems at first a conclusive argument against our view, but I shall show that in the case of two plants, cabbage and Phalaris, it is not so.

We expressly stated (p. 479) that our experiments on cabbages were made on young seedlings "about half an inch or rather less in height," because when the plants have grown to an inch and upwards in height the lower part ceases to bend heliotropically. Now Wiesner's experiments were made confessedly on seedlings whose lower part was growing slowly, and which were therefore probably older than those which we employed for our experiments. When Wiesner made his rotating experiment with *young* cabbage seedlings they became curved down to the ground. This proves that the curvature which occurs near the ground in young cabbage seedlings is not due to weight; and this is the very curvature which we have shown not to occur unless the upper part is illuminated. I do not attempt to explain Wiesner's experiments on old cabbage seedlings, but those made with young ones are alone of importance for us, and they are conclusively on our side.

With regard to Phalaris I regret that I cannot confirm Wiesner's results, who states that these seedlings behaved like the dicotyledons experimented on; *i.e.* that when grown on the rotating apparatus they do not become bent down to the ground. I have experimented with young seedlings such as we should have used for the experiments on transmission of the light-stimulus, and found that many of them became well bent down to the ground. But it should be remarked that in some cases a certain amount of difference in this respect was observable between the plants on the klinostat and normal ones.

FRANCIS DARWIN

(*To be continued.*)

OUR BOOK SHELF

Through Siberia. By Henry Lansdell. Two volumes, with illustrations and maps. (London: Sampson Low and Co., 1882.)

IT is obvious that much scientific information cannot be expected from a traveller who was, to use his own expression, "flying across Europe and Asia," and who crossed Siberia from Ekaterinburg, in the Ural Mountains, to Tobolsk in the North, Barnaoul in the Altai, and Nikolaevsk on the Pacific, a distance of 6600 miles, in seventy-eight days, and whose aim was, during this very short time, to investigate the situation of Russian prisons. The author has, however, supplemented his own somewhat superficial observations by information obtained from good sources. The book is provided with many illustrations, partly taken from other works (without quoting the source from which they are taken), and partly from new photographs. These are sometimes very good, but sometimes they convey quite false ideas, as, for instance, the photograph of a "Buriat girl," who obviously is a metis, having very little in common with true Buriats.

P. K.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Limulus

IN a criticism published in the *American Naturalist* for April, 1882, on Prof. Ray Lankester's recent most able memoir, entitled "Limulus an Arachnid," Mr. A. S. Packard, whose most important researches on Limulus are familiar to all zoologists, and to whose courtesy I am indebted for a copy of his criticism, after stating other grounds which lead him to differ in opinion from Prof. Lankester as to the close relationship of the King Crab and the Scorpion, quotes in his final paragraphs extracts from published letters written by my late lamented friend and shipmate, R. von Willemoes-Suhm, from on board H.M.S. *Challenger*, at the Phillipine Islands and Japan in February and May, 1875, concerning certain Arthropod embryos which he had had under observation at Zamboangan, and which he then supposed to be the larvæ of *Limulus rotundicauda*. As Von Suhm and I worked together for more than two years daily with our microscopes within two feet of one another, we naturally discussed all that we did and observed in common, and we frequently talked about these supposed Limulus embryos, and looked at them together. It is as well, therefore, since the statements concerning them are being made use of to assist in disproving the position assumed by Prof. E. van Beneden, Prof. Lankester, and others as to the Arachnid nature of Limulus, a position of the strength of which I am myself persuaded, that I should state in print, that long before his death Von Willemoes-Suhm was completely convinced that he had been misled as to the larvæ, and told me that he felt sure they were not those of Limulus at all, but belonged to a Cirrhiped of some sort. I some time ago told my friend, Prof. E. van Beneden, who inquired on the matter, that such was Von Suhm's final conclusion. And I also long ago told Prof. Lankester, and this is no doubt the reason why no reference to Von Suhm's letters was made by the latter in his memoir.

It must be remembered that the only evidence in favour of Von Suhm's Nauplius larvæ being those of Limulus, lay in their general appearance, which simulated to some extent that of an adult Limulus, and in the fact that they were caught with the tow-net in Zamboangan harbour, a locality at which *Limulus rotundicauda* occurs.

H. N. MOSELEY

Oxford, April 15

Silurian Fossils in the North-West Highlands

THE publication of Dr. Heddle's geological and mineralogical map of Sutherland, which was noticed in NATURE, vol. xxv. p. 526, calls to mind some curious points with reference to that region—points on which we should like to have some further and more definite information.

Dr. Heddle quite acquiesces in the general accuracy of the stratigraphical conclusions arrived at by Murchison and his colleagues, and, as may be gathered both from his map and writings, has seen no cause whatever to induce him to believe either in the great fault of Prof. Nicol, or in the unconformity alleged by Dr. Hicks to exist in the adjacent county.

It would seem, therefore, that the chief bone of contention, viz. the age of the great mass of Upper Gneiss, which extends over the central and eastern parts of Sutherland, had been finally and irrevocably decided to be Silurian, notwithstanding the misgivings of the anti-metamorphic school.

There is just one more chance of avoiding the dreaded conclusion, and this "last phase of dissent" has appeared in the form of Dr. Heddle's map and accompanying papers, published in the *Mineralogical Magazine* for 1881. This last phase of dissent so far differs from the others, in that it is not based on foregone conclusions, and does no violence to stratigraphical facts, but is the natural and thoroughly unbiassed outcome of a long series of observations in the field and in the laboratory.

The question now to be solved, stated in the fewest possible words, amounts to this: What is the relation between the fossiliferous limestone of Durness, a limited patch on the north coast of Scotland, and the quartzo-dolomitic series, which, commencing at Loch Erribol, stretches southwards through the counties of Sutherland and Ross in varying phases of development for fully one hundred miles?

If this quartzo-dolomitic series is of the same age, or approximately of the same age, as the Durness limestone, which contains Lower Silurian fossils; then the last phase of dissent is knocked on the head, and henceforth orthodoxy reigns supreme. Aye, there's the rub; and this brings me to the point.

The palæontological facts bearing on this subject require to be re-stated with more confirmatory evidence. We can hardly be satisfied with such vague things as Serpulites, Fucoids, and the like; what is required in the present case is some clear and indisputable evidence that Lower Silurian fossils have been found in any part of the quartzo-dolomitic series away from the Durness basin.

Placing the most implicit reliance formerly in the statements of Murchison, that Orthoceratites had been detected by Mr. Peach and himself in Assynt, and further, that *Orthoceras* had been found in the upper quartz rock of Erribol, the fragment having been identified by Salter as *Orthoceras* (*Cameroceras*) *Brongniartii* (*Q. J. G. S.*, vol. xvi. p. 230), I have felt a little sceptical on the subject lately. Not that one would venture to doubt the perfect good faith of Murchison and his colleagues for a single instant. But it is possible to make mistakes in such matters, and we would wish to see something like a renewal of these alleged discoveries.

Besides it is well known that several eager and experienced searchers have paid visits to the North-west of late years, and, although they found very curious and enigmatical markings in the quartzite series, neither Prof. Blake nor Dr. Callaway, for instance, have succeeded in obtaining a form which could be unmistakably regarded as a Silurian fossil. Moreover, Prof. Blake, who was engaged about the year 1878 in making investigations for his great work on the British fossil Cephalopoda, endeavoured to trace the history of these alleged discoveries, but without success.

Those who may be regarded as Murchison's heirs and successors, must see how vital this point is, and we look to them, not to be content with hunting up old statements as to the discovery of recognisable Silurian fossils, but to afford us the means of satisfying ourselves, beyond the possibility of a doubt, that Silurian fossils do occur in the quartzo-dolomitic series. When this is done, all controversy on the "North-west Succession" should, in the absence of any startling and unexpected discovery, cease; but, until it is done, "the last phase of dissent" will continue to be regarded as a possible explanation by those who are not wedded to any theory, but who require that no link in the chain of evidence shall be wanting. W. H. HUDLESTON

23, Cheyne Walk, S.W.

Magnetic Storm

IT may interest some of your readers to know that a magnetic storm of unusual intensity raged from about midnight of Sunday the 16th to midnight of the 17th. The photographic records are only now being developed, so time will not permit of a detailed account being furnished for this week's number.

We observe a tremendous spot which appeared on the sun's disc first on the 13th, is now rapidly approaching the central meridian, and a group observed on Saturday a little in advance of it, appears to have undergone considerable change in the interval. Possibly those observers furnished with better appliances than we have at our disposal will be able to give fuller information respecting what has taken place on the solar disc during the last few days. G. M. WHIPPLE

Kew Observatory, Richmond, Surrey, April 18

Sea-shore Alluvion—Dungeness or Denge-nesse

As Lambarde points out, lying in Walland and Denge marshes, the "neshe" or Saxon "nesse," a "nebbe" or "nose" of land extending into the sea derived its name from the last marsh—Somner terms it "Stone End"—"Lapis appositus in ultimo terræ." Grunville Collins, in 1693, says, "You may keep within nine or ten fathom of it close to the shoar." Westward of Folkestone great changes have taken place in the condition of the old havens, due to the early accretion and continuous extension up to the present time of this remarkable spit of shingle formed to windward of a tidal estuary. The whole area at the present time between the Royal Military Canal which runs from Sandgate west of Folkestone to Rye, and which forms the base of the Ness, twenty miles in length, and southward to the sea exhibits parallel series of curves running in undulating waves, displaying the periodical accessions to the coast very similar to the annular rings in timber; the surface of which, landward, is gradually brought into cultivation. Lydd, at a comparatively recent period a port, is now three or four miles from the sea. Two natural roadsteads are formed by this spit, in which, dependent on the quarter from which the wind prevails, seven to eight hundred vessels may be seen riding at anchor, lying within two or three miles of Lighthouse Point, the extremity of the Ness.

Numerous projects have from time to time been brought forward for the formation of a harbour of refuge, by running out a pier from the extremity of Dungeness; but having reference to the large amount of speculation as to its origin and progress, the Legislature have wisely hitherto turned a deaf ear to any tampering with a breakwater of nature's forming, affording, as it does, two excellent havens of refuge under certain conditions of weather, for all these shingle nesses possess the remarkable property of creeping across, and having deep water at their extremities.

It has been assumed with some plausibility that the meeting of the tides (which, however, is much further eastward) has influenced its origin. A formation of this description is, however, very little influenced by the tides, and similar shingle spits are found tailing round and across the outfalls of tidal rivers of great velocity, and a similar spit—Langley Point, has formed to the westward under Beachy Head, east of Eastbourne, where there is no such assumed meeting of the tides, and the origin of which may also be traced to a now extinct tidal harbour (Pevensey) to leeward of it.

On the east coast, masses of shingle form similar nesses, such as Landguard Point, inclosing Harwich Harbour, Orfordness, inclosing Orford Haven, and others.

The average progress of Dungeness, in a south-eastern direction has amounted to six yards per annum, and reaching over certain periods an average of eight yards per annum has been attained; this, however, is local, and accompanied by periodical wasting away along the curved bays east and west of Lighthouse Point. This action may be seen in Rye bay, where there is less shingle and more sand by patches of diluvial peat cropping up through the foreshore.

A determinate south-east movement of the extremity of the Ness results from these variations in outline as may be seen on reference to the Ordnance Sheet of H.M. Geological Survey, the position of the old "fulls" to the westward being laid down thereon, indicating plainly the eastern leeward movement.

The following extracts from various hydrographic authorities show the high estimation held for this natural breakwater and its attendant harbours of refuge by naval men.

Norie in his "British Channel Pilot," says:

. . . "You may round this point in 10, 11, or 12 fathoms. The strongest tide runs in 15 fathoms. Ships bound down channel, and meeting here with westerly winds, may anchor to the eastward of the '*Ness*' in 10 or 12 fathoms. . . . You may also anchor to the westward of the 'Ness,' with north-east winds, in 7 or 8 fathoms."

Capt. Martin White, R.N., in his Sailing Directions for the English Channel, says:—

"The West Bay of Dungeness affords good anchorage against north-easterly winds, *and is certainly preferable to Dover Road*."

"When the wind is between north and by east and west and by south, the Eastern Bay affords good shelter to vessels of all classes in from 4 to 12 fathoms, and upon pretty good holding ground."

The last edition of the "Admiralty Channel Pilot" says :—

"Off the pitch of the Ness, near the lighthouse, it is steep-to, there being 4 fathoms at 100 yards, and 15 fathoms at 330 yards from the beach. . . . The roads on either side of Dungeness afford excellent and extensive anchorage, according to the state of the wind, with good holding ground, consisting of fine sand over clay and mud."

The Chesil bank, to the West of Portland, in a deep embayment, away from the current of tide, is a remarkable example of the heaping-up power of the wind-waves from the south-west, as it is piled up at the leeward or eastern end, next Portland, 30 feet above high water, or treble the normal height above the sea of such formations.

The Harbour Commission of 1840 reported against Dungeness as a site for a refuge harbour of artificial formation, on account of the continued increase of the spit, as indicated by the necessity for altering the position of the lighthouse, which at its re-erection in 1792 was 100 yards from the sea, and at the period of a then recent survey was 218 yards distant, showing an increase of 118 yards in 47 years. The original lighthouse was, at the erection of its successor in 1792, 640 yards from the shore. The Harbours of Refuge Commission of 1844 also reported on it, referring to its excellent anchorage, and the danger of interfering with nature in such a spot. The changes in this remarkable formation have been so extraordinary, as fully to endorse these official conclusions, and the various vicissitudes it has undergone demand more careful consideration than is usually afforded by those who advocate artificial works, the effect of which on these nicely-balanced movements would possibly be as problematical as the turning of these shingle "fulls" into quarries for building purposes would be suicidal, and either course might result in a baneful interference with one of the finest natural breakwaters on our coasts.

J. B. REDMAN

Dispersal of Bivalves

REFERRING to the fact mentioned in Mr. Darwin's letter, that mussels are sometimes brought up on the point of a hook, it is common, in shell collections, to find "Heart Cockles" (*Isocardia cor*), which look exactly as if they had been drilled each with a small hole, centred at some point in the opposed edges of the two valves.

These specimens have been taken by the long-line fishermen on the Irish coast, and the apparent puncture is caused by the animal having closed upon the shank of an accidentally intrusive fish-hook with such force as to crush the edges of the shell against the steel wire.

Numbers of this comparatively rare species have been thus procured, for Isocardia will allow itself to be drawn in with the line rather than open its doors to new possibilities of danger while the hook is within.

D. PIDGEON

Holmwood, Putney Hill, April 12

The Yellow River and the Pei-ho

AS bearing on the subject of my paper on the hydrology of the Chinese rivers, which was published in NATURE (vol. xxii. p. 486), I take it upon myself to forward the substance of some observations made by Mr. T. W. Kingsmill—president of the North-China branch of the Royal Asiatic Society—at a meeting of the Society in September, 1880.

Having made measurements of the cross-section of the Yellow River, and having obtained the most reliable information he could gather regarding the depth of water and the speed of the current at different seasons, Mr. Kingsmill roughly estimates the discharge as follows :—

Extreme low water	18,000 cubic feet per second.
Ordinary ,, ...	36,000 ,, ,,
Flood discharge	112,000 ,, ,,

The average discharge he is inclined to estimate at about three-fourths of the single estimation supplied in Sir George Staunton's narrative; and he places it at, or rather surmises that future careful observations will estimate it to be, 300,000,000 cubic feet per hour, or about 83,000 cubic feet per second.

With reference to the Pei ho, Mr. Kingsmill, from an observation made in the summer of 1879, estimated its water-discharge at 9000 cubic feet per second. My own estimate was confined to the winter months; but for reasons given in my paper I considered it fairly typical of the whole year, viz. 7700 cubic feet per second. That I was justified in so doing, this independent observation of Mr. Kingsmill sufficiently proves.

H.M.S. *Lark*, Auckland, February 28

H. B. GUPPY

Table of the Appearance of Rare Lepidoptera in this Country in Connection with the Sun-Spots

THE following table is a numerical abstract of the records relating to the capture of certain rare lepidopterous species in the United Kingdom, condensed from a larger table presenting an abstract of the pages of the *Magazine of Natural History*, *Zoologist*, *Entomological Magazine*, *Entomologist*, *The Entomologist's Weekly Intelligencer*, *Naturalist*, *The Entomologist's Annual*, *The Yorkshire Naturalist*, *Newman's British Butterflies*, and other works. It will show the relation existing between the sun-spot cycles and the appearance of the species, yet not quite so distinctly as my larger compilation, since, in order to adapt it to the pages of NATURE, it has been necessary to equalise the sun-spot cycles, which has caused, I fear, a certain overlapping of the cycles of capture, which really are well defined. I was not aware until quite recently that any one had been before me in this branch of entomology, but I now find my remarks in the *Journal of Science* for August, 1881, corroborated in a previous publication (Dr. F. G. Hahn, "Ueber die Beziehungen der Sonnenfleckenperiode zu meteorologischen Erscheinungen," pp. 155-157, Leipzig, 1877). This pamphlet is noticed (E. D. Archibald, NATURE, vol. xix. p. 145, article, "Locusts and Sun-Spots").

Years.	Sphinx Convolvuli.	Deilephila Galii.	Deilephila Livornica.	Chærocampa Celerio.	Chærocampa Nerii.	Colias Edusa.	Colias Hyale.	Argynnis Lathonia.	Vanessa Antiopa.	Pieris Daplidice.		
1832, 43, 54, 65	1	1	4	25	2	309	62	16	13	—	433	
1833, 44, 55, 66	1	2	1	1	2	382	71	3	1	—	464	*m*
1834, 45, 56, 67	23	14	11	16	6	117	18	10	5	1	221	
1835, 46, 57, 68	735	14	40	14	4	440	1109	136	79	12	2583	+
1836, 47, 58, 69	121	7	5	1	1	116	6	48	14	6	325	
1837, 48, 59, 70	162	233	30	3	2	95	13	10	3	9	560	M
1838, 49, 60, 71	20	3	20	11	1	345	7	3	1	8	419	
1839, 50, 61, 72	5	—	2	5	—	80	47	308	7	3	457	
1840, 51, 62, 73	9	3	8	—	2	5	5	20	—	—	52	–
1841, 52, 63, 74	—	—	—	2	--	9	—	27	5	4	48	
1842, 53, 64, 75	162	—	1	11	1	67	213	9	—	1	465	

The numbers give the amount of captures in the years specified; but in the case of *C. Edusa* and *C. Hyale* a 10 has been placed for every notice of "abundant," a 5 for every notice of common, the number of captures not being often stated.

Abbreviations employed + maximum appearance, – minimum of appearance, *m* minimum of sun-spots Wolf, M maximum of sun-spots Wolf.

A. H. SWINTON

Binfield House, Guildford, March 23

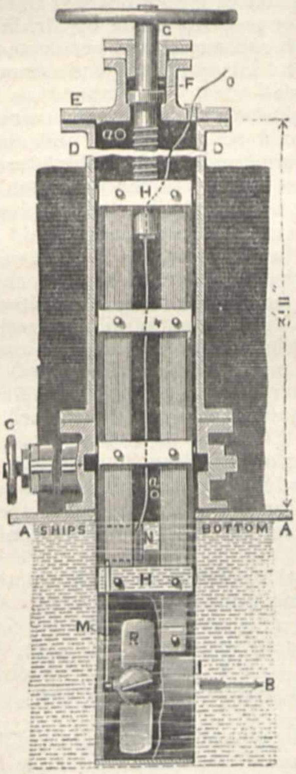
THE APPLICATION OF ELECTRICITY TO SHIPS' LOGS

THESE are days of rapid scientific progress, and the great interest so recently excited by the application of electricity, in a new and startling way, to transmit information, has been almost eclipsed by the attention which its use for lighting and the transmission of power has attracted. Though no longer confined to signalling, yet this is still its most important use, and one for which its employment is being extended in many directions, always with the most satisfactory results. It is with the application of electricity this fleet messenger for giving a constant record of the rate of a ship, that this article is concerned. Before, however, dealing with this matter, it will be well to say a few words about logs generally.

Ordinary ships' logs are of two kinds, called respectively harpoon and taffrail logs. Harpoon logs, which are the more extensively used, consist of a cylinder, on one end of which works a fan or screw, registering the number of revolutions by means of clockwork within the cylinder, the dial being visible through a glass face. To the other end, which is conical, a rope is fastened, so as to enable the log to be towed by the ship. A modification of this is called the "detached" log, the revolving fan and case for the clockwork being two separate pieces. To both these logs there are several objections, such as the almost unavoidable entrance of salt water to the wheelwork, the drag on the ship, which is often as much as 40 lbs. or more, the inconvenience of hauling in the log each time it has to be read, and lastly, the loss of the whole instrument, should the towing-line break.

To obviate these objections, the taffrail log was invented, and which goes a step further than the detached log, by taking away the recording portion to the taffrail of the ship, and causing the towing rope to transmit the revolutions of the fan to it. These logs are, in many respects, a great improvement on the first; the registering dial is less liable to damage, and is always visible; the tension of the towing line is less; it is therefore less liable to rupture, and even when this does take place, the fan is

FIG. 1.



easily replaced. On the other hand, the unsuitable nature of the towing line for transmitting torsional force, is obvious. Further, the slip of the fan must by seriously affected by the constant variation, in length, of the submerged portion of the line. In Walker's log these difficulties are partly met by the use of a governor, consisting of a pair of weights fixed to the towing line, and revolving with it; but this device can only modify the evil.

The late Mr. W. Froude, who pointed out other objections to existing logs, endeavoured for some time to devise an electrical log, in which the revolutions of an accurately formed screw should be communicated to the deck, *not* by the revolution of the towing rope, but by an electric current in wires carried by it. Eventually he succeeded, in conjunction with Mr. Brunel, in constructing an instrument of this kind, which was applied to Sir W. Thomson's yacht, *Lalla Rookh*, and worked very well, till by some mischance, it carried away and was lost. Mr. Kelway had, meanwhile, been working independently with the same end in view, and had constructed an electric log, which he brought before the notice of the Admiralty. A trial of this last instrument was undertaken by Messrs. Froude and Brunel, on board H.M.S. *Shah*. In this trial, the registering portion was placed on the poop, and self-recording apparatus was used, by which at every revolution of the fan a pen was lifted from a strip of paper moved by clockwork, thus causing breaks in the otherwise continuous line. On a line parallel to this, time in half seconds was simultaneously recorded. The result of this trial was to clearly demonstrate the satisfactory action of Mr. Kelway's log, and it was afterwards applied, together with accurately formed fans, to H.M.S. *Iris*, and the yacht *Alberta*. Its further extension has not been proceeded with, apparently in consequence of Mr. Froude's decease. Quite recently the same inventor has put into practice the happy idea of placing the electrical log to work altogether under the ship's bottom. The way in which this is done is shown (Fig. 1). A water-tight case is securely fixed to the bottom plates (AA), in this case a frame (HH) is moved up and down, and in the lower part of the frame the fan (R) works. The fan communicates its motion by a vertical spindle (M) to a box (N), in which electric contact is made and broken eight times in a mile. The wire (OO) can thus transmit a record of the distance passed over to a dial or dials fixed in any part of the ship.

This invention has the advantage of allowing the screw of the instrument to work in water of uniform pressure, and to a great extent free from the disturbing action of the waves. There is undoubtedly a body of water carried along by the surface friction of the ship. The depth to which this extends is unknown, but there is strong reason to think it is very small, and would not therefore affect the fan. The log itself, however, offers an excellent opportunity of investigating this obscure point, since it can easily be raised or lowered to different positions.

The complete instrument is at present being exhibited at the Crystal Palace Electrical Exhibition, and an account of its various applications has been recently given in a paper by Mr. Kelway. These applications are many and important, and the invention, besides being very suitable for its original purpose, promises to afford valuable information, not to be obtained by the use of ordinary logs.

H. S. H. S.

THE TONNAGE QUESTION

ONE of the most interesting papers discussed at the recent meeting of the Institution of Naval Architects was on the Revision of the Tonnage Laws. The author, Mr. W. H. White, is, from his position as Chief Constructor at the Admiralty, as well as from his well-known attainments, a singularly impartial and able judge of this most difficult question. The occasion which called forth the paper was the report on the tonnage question lately issued by the Royal Commission, which last year took evidence on this subject. The report was, as is well known, not signed by all the commissioners. Two of them, viz. Mr. B. Waymouth, Secretary to Lloyd's Register, and Mr. Rothery, Q.C., the Wreck Commissioner, wrote independent reports, which differed widely from that of the majority, and from each other.

The state of the Tonnage Laws has for a long time past given rise to serious complaints, on various grounds. As matters stand at present it is possible for a steamer not only to have no tonnage at all, but even, as is the case of a vessel well known on the Clyde to have a negative tonnage. It is also alleged that the indiscriminate measurement of all inclosed spaces on deck for tonnage has a direct tendency to produce unsafe vessels, by taxing the covering in of the large open spaces above the engine, &c., and also by unduly taxing vessels provided with hurricane decks, which, from their nature, can never be entirely filled with cargo.

Mr. White, in his paper, reviews the whole question historically. He begins by propounding the question, should *internal capacity* be still retained as the basis of measurement, regard being had to the present conditions of trade and shipping? He shows that some of the earliest tonnage laws on record had this basis, such for instance as the French Ordonnance de la Marine of 1681, and the English law of 1720. The vessels to which these laws applied were mostly engaged in the wine and spirit trade, and the measure of 42 cubic feet to the ton, which was the basis of the law of 1681, very approximately expressed the dead-weight capability of the ship as well; for 42 cubic feet were allowed for stowing four bariques of wine, which weighed approximately one ton. Thus the vessels measured by this rule were enabled very approximately to carry as many 20 cwt. tons as they contained tons of cubic capacity. The principle of measurement by internal capacity, as now accepted, was not adopted till the year 1833. In 1836 the New Measurement Law, proceeding on these lines, was passed, and in 1854 came the more perfect Merchant Shipping Act of Moorsom. In framing this law, two fundamental conditions were accepted:—

First, that the taxable tonnage of a ship should be represented by her freight-earning power.

Second, that the space available for the conveyance of passengers and cargo should be taken as the measure of freight-earning power.

The great question of the moment is, whether the changes in the construction and propulsion of ships made since 1854 have not necessitated some modification of the doctrine that internal capacity is the fairest measure of the possible earnings of most ships. Mr. White thinks that this is scarcely a matter for argument, it being generally admitted that in the great majority of ships of the present time, the limit of freight-earning is the dead-weight capability. He points to the awning deck class as a case in point. "It undoubtedly has much to recommend it as regards safety and good behaviour; yet it appears that the internal capacity is so great in proportion to the carrying-power, that the whole available space can never be utilised, even when the lightest cargoes are carried."

In support of this view Mr. Waymouth stated that the "aim of the ordinary ship-owner is to have a vessel which will carry as many 20 cwt. tons upon as few 100 cubic feet tons (on which he pays his tonnage dues) as he possibly can." It is, as Mr. White points out, perfectly obvious that the number of 20 cwt. tons in a given ship depends upon the load-line, and that consequently there is a close connection between tonnage legislation and load-line rules, so much so, that contrary to the opinion of the majority of the Commission, the two questions should be considered together.

The majority of the members of the Royal Commission were averse to any change in the present principle of measurement by internal capacity, for reasons which are stated at length in their report, and which are nearly all founded upon the inconvenience which would result from any change to foreign countries which have copied our Tonnage Laws, and to the various port and dock authorities throughout the world. Though averse to any change in principle, they yet recommended certain amendments to the existing law, which related chiefly to the deductions which should be allowed from the gross tonnage, and also as to the mode of measuring the tonnage of iron ships, particularly of those having cellular double bottoms. Regarding the deductions from gross tonnage, it is to be noted that no provision has been made for the case of awning-decked vessels. Also the Committee was of opinion "that the exemption of any closed-in space from measurement into tonnage, as an inducement to owners to increase the safety of ships is unsound in principle, and if adopted would have to be followed by new restrictions, upon which fresh complaints would be founded." This is an alternative which in our opinion is preferable to continuing regulations which admittedly discourage the building of safe types of ships.

The proposals of the Commission as to the measurement of the tonnage space of cellular double-bottomed ships appears to us to err in the same direction. If their recommendation were carried out, part of the space between the double bottoms would actually be included in the space available for tonnage measurement. Now it is absolutely impossible to carry freight between the double bottoms, and on the other hand, vessels built on this system are the strongest afloat; consequently the recommendation is not only a violation of the principle of the Act of 1854, but also unfairly handicaps this excellent type of ship.

It will thus be seen that the majority of the Royal Commission recommend that things should be left as they are, subject to certain amendments in detail, some of which latter appear to be wrong in principle, and are moreover unpopular with both builders and owners.

The alternative proposal made by Mr. Waymouth was that dead-weight capacity should be adopted as the basis of measurement. His proposals are summarised in the following words:—

"I propose that the total dead-weight carrying capability of a vessel should be ascertained, and also the line to which she is immersed when equipped ready for sea, without cargo on board. In the case of a sailing vessel there should be no consumable stores on board, and similarly in a steam vessel, the engines should be complete, and the boilers full of water, but there should be no coals on board. Under these conditions it is considered that steamers would be in a relatively fair position, one against another, and also in relation to sailing vessels.

"The dead-weight required to immerse a vessel from the light line to a maximum load line fixed by authority, would denote her utmost carrying capability (in tons of 20 cwt.), compatible with safety in ordinary circumstances.

"There is a growing disposition, on the part of shipowners, to regard with favour the fixing of such load line, provided that the authority on whom the duty would devolve be so constituted as to inspire confidence in its decisions."

Mr. Waymouth's proposal would, if adopted at once get rid of an enormous mass of difficulties. On the other hand, the fixing of a load-line would be certain to give rise to numerous disputes, and moreover, though doubtless applicable to the majority of merchant steamers, the dead-weight system would not apply to passenger steamers.

Mr. Rothery's proposal was of quite a different nature. It is at least open to doubt if the principle of the Act of 1854, viz. that the freight-earning capability of the vessel should be the basis on which to assess her taxable tonnage, is correct. It is not by any means clear that there should be any connection whatever between the two. There is another principle which has much to commend it from the common-sense point of view, viz. that the service rendered to the vessel by the institution to which she has to pay, should be the basis on which to calculate the payment. This is the view adopted by Mr. Rothery in his report. Now, in the case of a dock or port, the service rendered to a vessel for accommodation is proportional to the space which she occupies in the water, and to the length of time which she occupies it. The exact space occupied by a vessel in the water is proportional to her displacement, and hence Mr. Rothery proposes to adopt the system of displacement tonnage.

Mr. White does not enunciate any views of his own as to the best basis on which to assess the dues, in the event of a revision of the present law. It is not, however, difficult to perceive that he favours the principle of *service rendered* to the vessel, and not the freight-earning power as the basis of assessment. Mr. White differs, however, from Mr. Rothery in the mode in which the space occupied by the vessel should be measured. He considers that for all practical purposes this space is equal to the parallelopipedon formed by the extreme length, extreme breadth, and the mean draught, and consequently thinks that "parallelopipedon tonnage," as it is called, has much to recommend it. The possibility of berthing other vessels at the same dock or wharf is not sensibly altered by the under-water shape, consequently the above seems a fair measure of service rendered.

Mr. White does not consider that the above proposal would lead to the adoption of a box-shaped type of vessel. He thinks that the cost of propulsion of a steamer would effectually check any such tendency.

Mr. White concludes his most able paper by the following piece of advice, which we trust may be taken to heart by whatever government finally undertakes to revise the tonnage laws.

"In conclusion, I would venture one remark respecting the course of procedure which promises to give the best results, if a revision of the tonnage law is decided upon. Valuable as the labours of committees and commissions may be in testing the feeling of those interested in shipping, and putting on record the opinions of competent authorities who view the subject from different standpoints, it does not appear that a satisfactory revision can be looked for in this direction. The precedent to be found in the preparation of the law of 1854 seems to be a good one. Following after the work of the commissions came the careful, extensive, and laborious inquiry of Moorsom, a scientific expert, having a thorough acquaintance with the subject, and placed in direct communication with the shipping community. If the long-talked-of Central Council or Advisory Board should be constituted to deal with matters relating to the mercantile marine, and if it should be assisted by a competent scientific staff of naval architects, we may hope that, among other much-needed action, will be included the revision of the tonnage laws in a sense that will give more general satisfaction than could otherwise be obtained."

THE NAVAL AND MARINE ENGINEERING EXHIBITION

THE Exhibition which Mr. Samson Barnett, jun., has opened at the Agricultural Hall, and which closes to-day, contains a very large number of objects connected directly and indirectly, and sometimes even totally disconnected with naval purposes. The collection is by no means totally devoid of novelties and of objects of considerable scientific interest. The Exhibition contains numerous models of recently-built war and merchant ships, a few small marine engines and boilers, and portions of large-size marine boilers, together with fittings of engines and boilers in great variety. There are also several specimens of steam steering gear, ships' telegraphs, steam capstans, cranes, and machinery generally for loading and unloading vessels, boat-lowering apparatus, life-saving appliances, dredging gear, and refrigerating appliances. Naval artillery was not well represented, but Messrs. Hotchkins and Co. exhibited some fine specimens of their beautiful revolving cannon, which have been adopted in the navies of several foreign governments, notably in those of France, Germany, Russia, and Italy.

The ships' models are as a rule very deficient in interest, in spite of the fact that they represent many of the most famous of modern vessels, such as the *Devastation* and *Polyphemus*, among men-of-war, and the *Servia*, the *City of Rome*, and the *Ravenna* amongst passenger steamers; for they were mostly half models of the outsides of the vessels, which, though they give a very good idea of the exterior form, afford no information as to the construction, the interior arrangements, or the engines and boilers. This is somewhat disappointing when we remember what strides have been made in recent years in the construction of iron ships.

In the Department of Marine Engines and Boilers there was a remarkable absence of models, or even of drawings of the very fine engines with which our first-class war and merchant steamers are now fitted. By far the most important objects exhibited in this section were the magnificent flanged front plates of boilers, one of these being fifteen feet in diameter, and made in a single piece, with three flanged openings for furnaces, from a single 3-ton ingot of Siemens' steel. The same firm also exhibited several specimens of Fox's corrugated furnaces, an invention which has conferred the greatest benefits on the cause of steam navigation, by rendering possible the use of the very high boiler-pressures which are so essential to economy of fuel. Mr. David Joy also showed a model of his own celebrated valve-gear, which has given such excellent results with locomotives at Crewe, and which will doubtless soon become favourably known to marine engineers. This valve-gear is probably the most serious competitor to the old link-motion driven by eccentrics, first adopted by Stephenson for locomotives, and which has remained in pretty general use up to the present time. Mr. Joy's motion, besides being simpler, effects a better distribution of the steam, in many respects, than the link-motion.

Amongst the most interesting features of the Exhibition were the refrigerating machines. Of these there were four, exhibited by Messrs. Bell-Coleman, Messrs. T. Pigott and Co., the Haslam Foundry and Engineering Company, and Messrs. J. and E. Hall. As we have so recently described the principle of action of these machines, it will not now be necessary to go into details. It may, however, be mentioned that they are at the present moment being used by the Peninsular and Oriental, the Cunard and the Orient Steam-ship Companies, and also by the London and St. Katharine Dock Company, and the Orange Slaughtering Company. The successful application of mechanical refrigeration to the preservation of fresh meat and other provisions, is a subject of such immense importance, that we are not surprised at the great interest excited by these machines.

Amongst the miscellaneous exhibits we can specially mention the numerous collapsible and other life-boats, and the boat-lowering apparatus, some of which are really admirable. Also the wire-rope rigging, and the stout wire torpedo nets, exhibited by Messrs. Bullivant and others.

It seems a pity, considering the great amount of interest which has been excited by this Exhibition, that it should only remain open for ten days.

TOTAL ECLIPSE OF MAY 17

WE have given from time to time, in the Astronomical Column, particulars of the approaching total eclipse, pointing out that it is visible at a point on the Nile, in lat. 26° 32′ N. We are glad to be able to state, that an expedition left this country yesterday with the view of obtaining photographic and spectroscopic observations. The expedition has been organised by the Science and Art Department and the Royal Society combined, on the recommendation of the Solar Physics Committee.

The expedition sails to Suez in the Peninsular and Oriental steamship *Kaisar-i-Hind*, and a good idea of the local arrangements made will be gathered from the accompanying article, which we reprint from the *Daily News* of yesterday :—

May 17, 7 a.m., sun eclipsed, visible at Greenwich. Thus runs the records in our pocket-books. So short, sharp, and decisive, that to talk about an eclipse expedition in connection with it seems at first sight an absurdity, unless indeed we dignify by that name a jaunt in a penny steamer to Flamsteed's famous hill. Thanks, however, to School Boards, boys' clubs, and the like, the explanation will be apparent to everybody, and it is this. For, although on the 17th of next month the moon will come between us and the sun in such a way that part of the sun will be covered at Greenwich, which may be taken as a short title for the British Islands, the covering-up will not be total, and we shall have, therefore, only what is called a partial eclipse, hardly worth looking at, from the physical astronomers' point of view. But a thin line can be drawn on the globe from the West Coast of Africa, through Egypt, Persia, Central Asia, and China, along which the moon will entirely cover the sun; and here, instead of a partial eclipse, we shall have a total one.

This is one of the most important phenomena we can observe in the whole domain of physical astronomy, for a reason with which the readers of the *Daily News* are already familiar, namely, that when the light of the bright interior nucleus of the sun which we usually see is prevented from illuminating our upper air, by the interposition of the dark moon, the sun's atmosphere, which we never see except at such times, is revealed in all its majesty, and invites study on the part of those who care for the mechanism of the universe in which their lot is cast. Now England is going to be represented at a point on this thin line, and therefore we must talk about an eclipse expedition in connection with this event; for while we write, the directors of the Peninsular and Oriental Company, who have ever shown the keenest anxiety to further the interests of science, are allowing the sacred bullion-room of the *Kaisar-i-Hind*, now getting up steam in the Royal Albert Docks, to be desecrated—as some will think, and they are welcome—by packing-cases of ungainly shape containing such instruments and combinations of brass and iron as have never been built before. The expedition in question, which has been equipped and manned by the Science and Art Department and the Royal Society combined, at the suggestion of the Solar Physics Committee appointed by the Government, of course is intended to occupy a position along that thin line to which reference has already been made, and the most easily accessible point is one on the Nile, about 100 miles north of Thebes, in lat. 26 deg. 32 N.; the most easily accessible, but not perhaps quite the best, for the reason that on the Nile the totality—that is, the period during which the moon entirely covers the sun—will only last some 72 seconds, whereas at Teheran there will be 104 seconds of darkness, which it is to be hoped the Russian astronomers will utilise. Seventy-two seconds! The time is not long, and when it is stated that the preparation of the new instrumental combinations and the investigation of the new methods to be employed have required three months' solid work and thought, many will ask whether the game is worth the candle. The following considerations will show that it was distinctly the duty of the men of science interested in these problems to endeavour to secure observations:—In the first place, it is a little discouraging to travel thousands of miles, and to go through all the preliminary work and anxiety connected with such an expedition, even if the eclipse is two or three minutes long, should the weather chances be 2 or 3 to 1 against success. On the Nile the weather chances—that is, the ordinary weather chances—are perfect, and there will be neither rain nor cloud. Secondly, the eclipse happens at the most critical time of the solar activity, thereby offering a most marked contrast to the last one observed in 1878. Then the sun was quite quiet. It was in a condition of almost unparalleled repose. Now, judging by what has happened, the sun should be in a condition of intense action, and from the recent rapid increase both of spots and prominences we know the prediction is being fulfilled. Thirdly, the remaining ten eclipses observable during the present century—for, thanks to the diligence of Mr. Hind and others, we know almost to a minute when and where total eclipses will be visible until the year 1900—are not conveniently situated for observation, as the following list will show:—Next year one occurs with a maximum totality of 6 min., but the course is almost entirely over the Pacific Ocean. In the Marquesas, however, a duration of totality of 2 min. 53 sec. may possibly be available. The next (1885) is accessible only in New Zealand, where the greatest totality will be almost 2 min. The longest eclipse, as before, falls on the Pacific. The eclipse of 1886 has the longest totality in the century, but this falls over the Atlantic. In Grenada the totality will last $3\frac{1}{4}$ min. On the African coast, south of Angola, the duration will be more than $4\frac{1}{2}$ min. The eclipse of 1887 will be best observed in Russia, and 50 miles north of Moscow the totality will have a duration $2\frac{1}{2}$ min. On Lake Baikal the totality will last 3 min. 38 sec. The eclipse of 1889 will have at Angola a duration of about $3\frac{1}{2}$ min., and at Barbadoes of about $1\frac{3}{4}$ min., its greatest duration being upon the East Atlantic. The next eclipse (1892) falls entirely on the South Pacific, and Antarctic Oceans, and must be lost, although the duration of totality extends to more than 4 min. On the whole, the most favourable eclipse for observation in the present century will be that of 1893, which enters the American continent near Coquimbo, where totality lasts nearly 3 min.; then, crossing Brazil, it leaves the land near Ciara, where the duration will last $3\frac{3}{4}$ min. Crossing the Atlantic, it will again reach land near Bathurst, with a totality lasting about 4 min., and, crossing Central Africa, will leave the land finally near Khartoum. The eclipse of 1894 will occur almost, if not entirely, over the sea or the inaccessible regions of Central Africa, but it may be total in the Seychelles. High latitudes are singled out for the path of the next eclipse (that of 1896), which, entering the Old World in Norway, passes across through Siberia to Japan. At Tana, in Finmark, a totality of about $1\frac{3}{4}$ min. may be observed. Hindostan will be the most favourable locality for observations of the eclipse of 1898, which will have a totality of a little more than 2 mins.

It will be seen from the above list that although doubtless attempts will be made to secure observations of some of these eclipses, yet that in no case are we likely to get such a grand harvest of facts as have been secured during the last twenty years, notably during the eclipses of 1868 in India, 1869 United States, 1870 Mediterranean, 1871 India, 1875 Siam, and 1878 United States again. This condition of things of course makes one hope that the coming seventy-two seconds will be utilised to their utmost, and in consequence of the warm co-operation of the Egyptian Government, which has been appealed to by our Foreign Office, a serious attack is contemplated; that is to say, in consequence of the local transport facilities afforded, large instruments, combining great solidity and fineness of adjustment, will replace the mountain artillery, so to speak, which alone can sometimes be employed. The something like thirty cases into which each separate part of the two equatorial stands and telescopes to be used, with their spectroscopes and cameras, have been separately packed for convenience of carriage, will, on disembarkation at Suez, be placed in a special van which, on arrival at Cairo, will be ferried over the Nile, and so on to Siut, the most southerly railway station in Egypt. Another instance may be given of the keen interest which the Egyptian Government is taking in the matter. The shorter the totality, of course the nearer the exact central line of the eclipse must the observing party be in order to secure the maximum of the calculated period of obscuration. Stone Pacha, chief of the staff, who is personally sparing no pains to make all preliminary arrangements, and to secure the success of the observations, has detailed one of his officers, at present stationed in Upper Egypt, to check the latitudes of the French map where the eclipse track crosses the Nile. Indeed, he has done more than this, and here comes the dark side of the picture so far as the observers are concerned. The eclipse happens in the very midst of the Khamseen season—that is the period of fifty days during which Lower Egypt is apt to be swept by a hot, exhausting south-east wind, so dust-laden at times, that the sun is totally obscured. To escape the chances of this double eclipse, or at all events to minimise them, it will be necessary for the party to occupy high ground. Stone Pacha is, therefore, after consultation with our Consul-General in Egypt, prospecting for a camp and providing the necessary camp equipage, and although he himself will not, we believe, accompany the expedition, it is hoped that an officer of the Khedive's household, himself an adept in astronomy, which he has studied at both Paris and Washington, will accompany the expedition as guide, counsellor, and friend, to assist in making the necessary arrangements with the various local authorities. So much, then, by way of preliminary. Now, a word on the instruments to be employed on this occasion. The resources of modern science place many means of attack in the hands of the astronomer. To get an idea of the physics of the solar atmosphere—what it looks like—to study, so to speak, its circulatory system, to which such special attention has been recently directed by the bold hypothesis of Dr. Siemens—to investigate its extent, and to determine the luminosity of its various regions, we have the astronomical telescope, and, better even than this for some purposes, the photoheliograph, that is an instrument which enables us to obtain a photograph of all the sun's surroundings visible during the eclipse. To determine the chemical nature of the various regions, a question to which the keenest interest attaches at the present time, we have the spectroscope and the spectroscopic camera. By means of these instruments we can see what we cannot photograph, and photograph what we cannot see. In former eclipses, when the duration of totality has been longer, it has been possible to have different instruments mounted on different stands—there has been time to go from one to the other. But on this occasion such a course would be impossible. On one stand, therefore, we have four telescopes and two spectroscopes for eye observation. On another stand we have a photoheliograph and spectroscopic camera for photographic registration. In the observing telescope two spectroscopes are so arranged that a movement of the eye through two inches is all that is required to pass from the greatest spectroscopic dispersion (7 prisms of 60°) to the lowest (1 prism of 60°) which can be conveniently employed during an eclipse. In this way it is hoped that the spectrum of the brightest and the spectrum of almost the dimmest part of the sun's atmosphere can be observed, and for the first time in the history of eclipse observation, comparisons will be made with the solar spectrum itself, as a solar photograph taken before totality will be used as a scale. Much is hoped in the way of the photographic record, for since the last eclipse, the science of photography, following step by step the new views of molecular grouping suggested by the spectroscope, has provided us with silver salts, identical in chemical composition, but so different physically that the red part of the solar spectrum can now be recorded as satisfactorily as the blue part of the spectrum has ever been. Nor is this all. The rapidity with which an image can be impressed upon a sensitised plate has been enormously increased, so that if all goes well, seconds now take the place of minutes, and more can be recorded in five seconds now than was possible in five minutes twenty years ago.

We have no space in the present article to refer more precisely to the exact work which it is proposed to undertake, but this much may be said, that in eclipse, as in all other kinds of scientific observation, each attempt made to secure facts, instead of exhausting, increases the number of points of interest to be investigated, and nowa-days we not only get this principle at work from eclipse to eclipse, but daily work in our laboratories and physical observatories suggests questions which can only be solved at such times. Hence eclipse observations are getting more and more connected with terrestrial chemistry and terrestrial physics by this intermingling of laboratory and eclipse work, and hence also the area of general interest will be increased as time goes on.

In conclusion, we may state that the Government expedition will consist of Mr. Norman Lockyer, F.R.S., and Dr. Schuster, F.R.S., with their assistants, Messrs. Lawrance and Woods. Capt. Abney, who was at first detailed for the duty, is prevented by ill-health from joining the expedition, but the photographic preparations have been made under his supervision. It may be added that Mr. William Black and Mr. J. Y. Buchanan, late of H.M.S. *Challenger*, will accompany the expedition, and it is hoped that Prof. Tacchini may join it at Cairo. The *Kaisar-i-Hind* leaves Gravesend at 12.30 this day.

THE EDINBURGH FISHERIES EXHIBITION

THE International Fisheries Exhibition, which opened at Edinburgh on the 12th ult., and to which reference has been already made in the columns of NATURE, is likely to prove a complete success—not only commercially, but also as an exhibition of much that is interesting in the natural history of our most valuable marine animals.

The apparatus of capture takes up a large space inthe Waverley Market Hall, but no particular novelty in the way of fish-traps is shown; no artificial bait that would supersede the whelk or the mussel in the taking of cod has apparently been yet invented, although much required. Several models of improved rigs for fishing-boats are shown, but no improvement has apparently been effected on the ponderous beam trawl, which is much complained of as a cumbrous instrument of fish capture. The exhibition is rich in specimens of stuffed fish contributed by various bodies of London anglers—but why not label them? It is not given to every visitor to a fishery exposition to know a jack from a perch. Some of the late Mr. Buckland's finely modelled and correctly coloured fishes have been sent from Kensington—notably a model of a salmon captured in the Tay, which weighed, when taken, 72 lbs. Considering the importance of the salmon to Scotland as a rent-yielding fish, we had expected to find in the exhibition a methodical display of the progress of that mysterious disease which has of late overtaken that fish; but, beyond a drawing of an afflicted salmon, we saw no other indication of the calamity. The display of oysters in progress of growth from the *spat* to the stage of reproduction, although not large, is exceedingly interesting. We pass over, in the meantime, the merely commercial exhibits, of which of course there are many, and shall only say of the piscicultural exhibition, that it merits at our hands a much fuller notice than we have room to bestow on it at present. A large number of maps, charts, books, and engravings, bearing on the natural history of our food-fishes and the economy of the fisheries, may be seen and consulted in the exhibition hall, whilst the illustrations of river pollution and purification attract a large degree of attention. Despite the deficiencies at which we have hinted, the exhibition is a valuable one, and, although not all at once, may lead to some new departures in the art of fishing, which may tend to augment in a striking way the national commissariat.

Among the exhibits which have attracted special attention are those sent by Mr. Oscar Dickson, of Gothenburg, and the Swedish collection generally, and of these we give some notice below. They are exceedingly varied and, in addition to being of great scientific value, are also interesting to persons who have a monetary interest in our fisheries. Mr. Dickson's name is already familiar to the readers of NATURE, in connection with his well-timed support of various Arctic expeditions fitted out from Sweden, and along with his exhibit, or as we may say forming part of it, are two cabinets of preparations of marine animals brought home in the *Vega*, embracing spoils of the sea, obtained by Baron Nordenskjöld in his famous voyage of 1878-80. It is much to be regretted that a catalogue of this exhibit has not been prepared, or at any rate cannot yet be obtained, but a glance at the jars is almost sufficient to show us that there are numerous "finds" of great interest to zoologists. The collections made of worms and crustaceans, of crinoids, sponges, and holothuridæ are exceeding varied, and have been preserved with much care; they evince the activity of all concerned, particularly Dr. Julius Stuxberg, who was "lord of the dredge" in both of Nordenskjöld's Yenisei voyages. The work done with the dredge has already been chronicled in the account given of the voyage of the *Vega*, and it is to be regretted that, from the want of a catalogue, we are unable to do more at present than make this general reference to the collection, which shows us how rich in varied life are the depths of the sea in the high latitudes visited by the professor. A portion of the skeleton of the famous Steller's sea-cow (*Rhytina stelleri*) excites much attention. This animal, there can be little doubt, is now extinct; but when living, it must have been of large proportions, and not unlike a gigantic seal; it would probably be from 25 to 38 feet in length, and weigh 75 cwt., more or less. These remains are of signal importance, proving, as they do, what has been over and over again denied—the existence of a marine mammal of great size and power, herds of which used to browse on the fields of sea-weed.

As may be supposed, "the Swedish Collection" is rich in preparations of the herring: there are over seventy specimens of that fish (*Clupea harengus*), exhibiting its growth from the ova to its most complete stage—that of reproduction. The growth of the sprat (*C. sprattus*) is likewise illustrated in a series of twenty-four preparations, which are of singular interest, so many persons believing that the latter fish is simply a young herring. It is certainly a curious circumstance that the two fish are frequently caught in the same nets, but upon being handled a difference is at once felt, although when taken, both fish are of the same size. The sprat can be at once distinguished from the young herring by means of its strongly serrated abdomen, and when closely examined, it presents several other differences alike of colour and form. A few preparations to show the growth of the pilchard (*C. pilchardus*) are also contained in the Swedish collection. Although the fisheries of Sweden are not of so much value as those of some other countries, great pains have been taken by those in charge of them to teach their fishermen how to make the most of what they have access to—hence the careful preparations of the herring. On the Cattegat an important fishery has been organised, but taken as a whole the total value of the Swedish fisheries is not more than 400,000*l*. per annum. But the Swedish fishermen, not content with the produce of their own waters, venture to the west coast of Norway and the west coast of Jutland, and find it to their interest to do so. Their chief fishing industry when at home, excepting their labours on the coast, consists in capturing the small Baltic herring, which they accomplish by means of seine nets, of good material, and very well made. The industry of herring catching, according to Mr. Oscar Dickson, is of great antiquity in Sweden, and the product of the shoals at particular times, has been of far greater value than the figures we have quoted above serve to indicate. From time to time the fishermen have been cheered by the advent of great bodies of herrings, and at one period, the take in some years, amounted to between two and three million barrels of eight cubic feet each per annum. The catch of herrings last year, the statistics of which have not yet been prepared, is said to have been "positively enormous." It is somewhat remarkable, however, that the figures of the Swedish herring fishery, which were circulated at the Berlin Exhibition of 1880, on the authority of Dr. Lundberg, do not correspond with those for which we are indebted to Mr. Oscar Dickson; according to Lundberg the herring-fisheries of Sweden are of the value of 5,000,000 *marks* annually, but the "millions" of barrels indicated by Mr. Oscar Dickson mean "millions" of *pounds* (not *shillings*) sterling. It is to be hoped this discrepancy of figures will be explained.

Besides these herring exhibits, there is much that is worthy of notice in the Swedish collection. There is, for instance, a display of the eggs of birds which prey on fish. This selection is from the prolific store of Mr. Ramberg, whose collection at Gothenburg is of world-wide celebrity. Some of the eggs which are shown are those of very rare birds. The development and growth of one of our flat fishes (*Rhombus lævis*) is exemplified in all stages of growth, some of which are singular, as, for instance, the changing of the eye in the flounder from one side of its head to the other. We hope Mr. Oscar Dickson's contributions and those from the Gothenburg Museum will be shown in London next year, and that Dr. A. W. Malm will again be in charge of the whole of the Swedish collection, which would require a much larger amount of space for its description than we have at present to bestow.

One of the most interesting of the exhibits shown in the Fisheries Exhibition is Sir John Graham Dalzell's smooth sea-anemone (*Actinia mesembryanthemum*), which is familiarly known as "Granny." It was taken from a rock pool at North Berwick, on the Firth of Forth, so long ago as August, 1828, and was then placed in the glass jar in which it is now exhibited. At that time it was thought it might be seven or eight years old, and its age at present may be over sixty years. During a period of twenty years "Granny" produced 334 young ones. In 1851, after the death of Sir John, who, according to the article "Aquarium," in the "Encyclopædia Britannica," was a keen student of marine animals, several of which could always be seen at his house in a humble kind of aquarium, this anemone was placed in the possession of the late Prof. John Fleming, and was carefully tended by him so long as he lived. Shortly before the death of the Professor, which took place in November, 1857, "Granny" unexpectedly gave birth, during a single night, to 240 living young actiniæ. Dr. James McBain, R.N., who took a warm interest in zoological affairs, was the next custodian of the smooth sea-anemone, which was presented to him by the widow of Prof. Fleming; it remained in his custody until a few days before his death in March, 1879, when he presented it for safe keeping to Mr. John Sadler, curator of the Royal Botanic Gardens in Edinburgh. In the glass jar, along with "Granny" three out of seven young ones are shown, born on February 18 last. As may be supposed, with such an interesting biographical record, Sir John Graham Dalzell's smooth sea-anemone is a decided feature among the "exhibits." Sir John was the author of "Rare and Remarkable Animals of Scotland," as well as numerous other works now forgotten.

NOTES

SIR H. COLE, K.C.B., late director of the South Kensington Museum and Inspector-General of the Science and Art Department, died on Tuesday night at his residence, Philbeach Gardens, Earl's Court. Sir Henry was in his usual health until Monday last, when he visited a photographer's and sat for his portrait. On returning home, he complained of feeling unwell, and gradually became worse. The cause of death is stated to be disease of the heart.

WE have received a most remarkable communication in the form of a pamphlet from the late Professor of Physics and Chemistry at the Royal Military Academy, which, were it not for his high scientific position and extended experience, we should hesitate to allude to. The grievance mostly complained of is the want of discipline existing amongst the cadets, so much so that the instruction in the above-mentioned very important subjects could only be carried on in an imperfect manner detrimental to the service. That such a state of affairs has or does exist in an important public department like the Royal Military Academy is a very serious matter. The pamphlet, for which Prof. Bloxam is answerable, exposes, if correct, a state of things that certainly should not exist in any educational establishment, and more especially not in one of the importance of this school for the future officers of the army. The following, from one of the letters contained in the pamphlet, certainly exhibits, if correct, a very undesirable state of affairs. "During the last ten years, I have pointed out to successive Governors, that the character of the classes was deteriorating, and I have now to state, with profound regret, that the conduct of the majority, when they first come under my instruction, and of a considerable proportion up to the end of their curriculum, is that of boys in an ordinary boarding-school with a very low standard of morals and discipline; so that the position which was formerly held by Professors Faraday and Abel has now become that of an usher, and such services as I am competent to render as a teacher of science are in a great measure lost to the Academy, although a knowledge of chemistry and physics has become more than ever necessary to the Scientific Corps."

THE honorary degree of LL.D. has been conferred upon Mr. G. J. Romanes, F.R.S., by the University of Aberdeen.

M. HENRY GIFFARD, the eminent French engineer, the inventor of the injector known by his name, of captive balloons, and of the experiments with directing steam balloons, has died after a protracted illness, at his residence at Paris, at the age of fifty-seven. He was unmarried, and has left his large fortune, amounting to several millions of francs, to the French Government to be devoted to the promotion of scientific research.

AT the Royal Institution the probable arrangements for the Friday evening meetings after Easter are as follows:—April 21: Prof. Dewar, Experimental Researches of Henri Ste. Claire Deville, Hon. M.R.I.; April 28: Prof. Abel, Some Dangerous Properties of Dusts; May 5: Prof. R. Grant, The Proper Motions of the Stars; May 12: A. G. Vernon Harcourt, The Relative Value of Different Modes of Lighting; May 19: Sir Frederick Bramwell; May 26: Sir Henry S. Maine, Sacred Laws of the Hindus; June 2: H. H. Statham, The Intellectual Basis of Music; June 9: Prof. Burdon Sanderson, The Excitability of Plants.

WITH the exception of Mr. Samuelson, M.P., and Mr. Slagg, M.P., the Royal Commissioners on Technical Education, viz., Mr. Woodall, M.P., Professor Roscoe, Mr. P. Magnus, Mr. Swire Smith, and the secretary, will leave England on Friday for a tour of several weeks in Switzerland and Germany, beginning at Alsace. Mr. Woodall, who will return earlier than the other members of the party, proposes also to visit the famous potteries of Saarguemines. In the course of their tour the commissioners will visit a number of works, technical schools, &c., in order to be able to include in their next report the result of their observations as to the influence of scientific and technical education upon industrial pursuits and foreign manufactures.

MR. E. W. WHITE, F.Z.S., of Buenos Ayres, who was present at the inauguration, on March 15, of the Continental Exhibition, held in the outskirts of that city, sends us a short notice of it. The building of wood and zinc, painted of a sombre colour, and a pretty close copy in miniature of that of the Paris Exhibition, covers about eight acres of ground, occupying the site of the Plaza Once. The present Exhibition is the result of the unceasing efforts of the Industrial Society of Buenos Ayres. To foster South American, and especially native, production, and stimulate their industries, were the special objects of the Committee, so that Europe was debarred as an exhibitor, in all but the single item of machinery. This restriction put upon exhibits without the limits of the continent of South America seems to Mr. White a suicidal policy, as it is only by comparison with the superior that the inferior has a chance of improvement. "What immediately strikes the observer on passing through the galleries is the wonderful profusion of native products, especially from the provinces, but the paucity of purely native industries; for although the city and province of Buenos Ayres make a good show in leather work, furniture, and carved woodwork, glass, inks, jewellery, millinery, carriages, typography, lithography, photography, liquors, beers, biscuits, metal castings, mosaic and tilework, &c., such is chiefly due to the intelligent foreign workmen resident in her midst, making use to a very great extent of foreign material; whilst, on the other hand, ores, marbles, wines, cereals, wool, silk, hides, leather, tans, woods, lignite, medicinal herbs, rice, tobacco, sugar, cotton, fruits, manures, dried and potted meats, fossils, &c., of the interior parts of the country astonish and indicate a future wealth which is boundless. Agricultural machinery is well represented, and forms a very interesting feature of the Exhibition; here England maintains her supremacy, owing to the intelligence and activity of her agents. Far otherwise, however, is it in other classes of machinery, such as that adapted to sugar manufacture, in which France, with inferior resources, rules as sole monarch; the fact is the moral of the bundle of faggots is lost upon our countrymen, who are always disunited, always late, and never learn, never forget. The inauguration took place in the midst of great splendour. Dr. Avellaneda, the ex-President of the Republic, and the honorary president of the Exhibition Committee, delivered the inaugural address, which was a brilliant piece of oratory, to which General Roca, the President of the Republic, surrounded by the whole diplomatic corps, foreign and native, replied in a very effective speech, declaring the Exhibition open."

WE are glad to welcome the *New Zealand Journal of Science*, the first number of which for February, has come to hand (Dunedin, Wilkie, and Co.). It seems to have been started in a proper spirit, and we trust it will continue as it has begun, to devote its pages to science in New Zealand, and to form a means of intercommunication between workers in science there. Among the articles in this number are the following:—"What is an Earthquake?" by Prof. F. W. Hutton; "A Visit to the Weka Pass Rock Paintings," by Mr. W. M. Maskell; "On the Preservation of Invertebrata," by Prof. T. J. Parker; "On a Common New Zealand Pycnogonid;" "The New Zealand Micro-Lepidoptera;" with general notes, meetings of societies, and correspondence.

WE have received from Messrs. Trübner and Co. a handsome and richly illustrated quarto, "The Horse in Motion, as shown by Instantaneous Photography, with a Study in Animal Mechanics, founded on Anatomy and the Revelations of the Camera, in which is demonstrated the Theory of Quadrupedal Motion," by J. D. B. Stillman, A.M., M.D. The investigations are executed and published under the auspices of Mr. Leland Stanford, of Palo Alto Farm, California. We hope shortly to notice this work at some length, and meanwhile make the following extract from Mr. Leland Stanford's preface, which shows the exact part taken by each of those concerned in the investigation:—"I have for a long time entertained the opinion that the accepted theory of the relative positions of the feet of horses in rapid motion was erroneous. I also believed that the camera could be utilised to demonstrate that fact, and, by instantaneous pictures, show the actual position of the limbs at each instant of the stride. Under this conviction I employed Mr. Muybridge, a very skilful photographer, to institute a series of experiments to that end. . . . When these experiments were made, it was not contemplated to publish the results; but the facts revealed seemed so important, that I determined to have a careful analysis made of them. For this purpose it was necessary to review the whole subject of the locomotive machinery of the horse. I employed Dr. J. D. B. Stillman, whom I believed to be capable of the undertaking. The result has been, that much instructive information on the mechanism of the horse has been revealed, which is believed to be new, and of sufficient importance to be preserved and published."

We notice an interesting innovation introduced into the secondary schools at Wilna, Russia, namely, a popular medical course for the scholars of the higher class. Nearly all scholars have expressed the wish to follow these lectures, which contain general notions of hygiene and those necessary for giving medical assistance in simpler diseases, and the results obtained during the first year are said to be very satisfactory.

According to the *Mémorial Diplomatique*, the Congress of Electricians for determining the length of the column of mercury equivalent to the theoretical ohm, the advisability of establishing a system of tele-meteorology, and a system of observations for the electricity of the air, will meet on May 1 at Paris. Almost all the foreign governments, including the British, have appointed their delegates. The names of two only are wanting.

The Ethnological Museum of the Trocadero, Paris, has been inaugurated by a visit of the delegates of learned societies and a lecture by Dr. Hamy, one of the directors of the new establishment.

The meeting of the delegates of the learned societies of France took place at the Sorbonne on April 11, under the presidency of M. Leopold Delisle, member of the Institute belonging to the Commission of History, Archæology, and Philology. The Commission of Sciences was presided over by M. Milne-Edwards; M. Catalan was nominated president of the section of mathematics, M. Filhol of physico-chemical sciences, and M. Coteau of natural history. The sittings were not so numerous as in former years, and the communications were almost devoid of any real interest. The concluding sitting took place on April 15, M. Ferry, Minister of Public Instruction, being in the chair. He delivered a speech purporting to justify some changes in the general organisation of the Congress. They do not appear to have been beneficial to the institution, if it is possible to judge by the results of the present session, which has been the most barren of the whole series from 1863, when it was created by the government of Napoleon III., owing to the exertions of Leverrier.

The fourth general meeting of the delegates of the French departmental meteorological commissions met on April 13, under the presidency of M. J. Ferry, Minister of Public Instruction. The report on the working of the Central Bureau was presented by M. Hervé-Mangon, president of the Council of that institution. The Central Bureau receives daily ninety-seven telegrams from foreign parts, and fifty-two from France. These documents are utilised for drawing five maps in the morning and three in the evening, representing isobaric and isothermic curves, and variations of these elements from the preceding day, the force and direction of winds, rain, and storms. The warnings are sent by telegraph to eight agricultural regions and four maritime districts. The report states that these previsions were justified eighty-two times in one hundred. M. Hervé-Mangon proposes to extend the system of elegraphic connection to the Azores and Cape de Verde Islands in the south, United States in the west, and Ireland in the north. M. Hervé-Mangon reviewed the progress of high-level observatories. The observatory on the top of Pic du-Midi has just been fitted up at an altitude of 2877 metres, about twice the height of the Puy-de-Dome. General Nansouty's observations are wired every day, but this hardy observer and his followers having been blocked up by snow he was unable to attend the meeting. Before the end of the year a new mountain observatory will be completely established on Mount Aignal. The number of pluviometrical stations in the whole of France is 1561; the system is complete only in eighteen departments, in sixty-nine deficient, in ten *nil*. For the observation of thunderstorm phenomena forty-six departments send regular reports. Some details were also given on the expedition that France will send to the Antarctic regions. The station will be located somewhere on the islands situated to the south of Magellan Straits, very likely Orange Bay in Terra-del-Fuego, or Saint Martin Creek in Hermite Island.

We learn from the *Izvestia* of the Russian Geographical Society that N. W. Kaulbars, well known by his travels in Turkestan, has just published a very interesting work, "Notes on Montenegro," which contains under this modest title a very good description of that country. He knows the country from his own experience, having visited it three times as Russian commissioner, and having travelled throughout the Principality. The geographical and physical description of Montenegro is very complete.

Mr. C. Wolcott Brooks sends us an abstract of a paper which he read on March 21 at the Californian Academy of Sciences, giving the temperatures of the ground in the Forman shaft of the Comstock lode, at Virginia City, Nevada, taken by Charles Forman, superintendent, and forwarded by him for presentation to the Academy. They are taken from the surface to the depth of 2300 feet, as ascertained by drilling holes not less than 3 feet deep into the rock, and inserting into the hole a Negretti and Zambra slow acting thermometer, of the pattern adopted by the Underground Temperature Committee of the British Association, and standardised at Kew. These holes were closed with clay, and the thermometers were left in for twelve hours, not less than three holes being tried at each point. The following are the depths in feet, and temperatures in degrees Fahrenheit:—

Feet.	Deg.	Feet.	Deg.
100	50½	1300	91½
200	55	1400	96½
300	62	1500	101
400	60	1600	103
500	68	1700	104½
600	71½	1800	105½
700	74¾	1900	106
800	76¼	2000	111
900	78	2100	119½
1000	81½	2200	116
1100	84	2300	121
1200	89¼		

Messrs. Blackie and Sons have issued the second volume of the new edition of their "Imperial Dictionary," the first volume of which we noticed some time ago. It extends from *Dep* to *Kyt*, and is in all respects up to the standard of the previous volume.

MESSRS. JARROLD AND SONS have published a "Handbook to the Rivers and Broads of Norfolk and Suffolk," by Mr. G. Christopher Davies, who seems to be thoroughly acquainted with every winding and nook of these curious features of East Anglian scenery. It seems a really charming and easily accessible place for a quiet and refreshing holiday, gives ample scope for the collecting naturalist and the fisher who loves a well-filled basket.

FROM the *Report* of the Rugby School Natural History Society for 1881, we see that the work has been fairly sustained. The *Report* contains few papers by the members of the Society themselves, considerable space being given to an abstract of four instructive lectures on the Natural History of Islands, by Mr. A. R. Wallace. Appended to Mr. Seabroke's usual observatory report, is a Syllabus of work with the instruments in the Temple Observatory, which shows that very thorough instruction in practical astronomical work is available for the Rugby boys.

THE scarcity of water is excessive in France and Germany; the level of the Seine has never been so low since 1734. The quantity of rain which fell this winter has not reached half the usual quantity. The engineers of the City of Paris and the Government are trying to find protection against such a scarcity, which will turn to a calamity if rainy weather does not set in shortly.

IN February of last year an account was given in this journal of Baeyer's method for preparing artificial indigo (NATURE, vol. xxiii. p. 390). The fifth step in the process, as there described, consisted in the preparation of *orthonitrophenylpropiolic acid:* in a patent recently obtained by the "Badische Anilin und Sodafabrik," bye-products obtained from this acid are employed as sources of indigo. By the action of alkaline-reducing agents, *e.g.* ammonium sulphide, on the ethyl salt of this acid, *ethylic indogenate* is obtained; thus, $C_8H_4 . NO_2(CO_2 . C_2H_5) + 2H_2 = C_8H_6NO(CO_2 . C_2H_5) + H_2O$. *Indogenic acid* (melting at 122° to 123°) is obtained by saponifying this ethylic salt; the acid easily gives off carbonic anhydride, either by boiling in aqueous solutions or by heating to its melting-point, with the production of *indogen*, C_8H_7NO, an oily liquid, showing yellow-green fluorescence. Any of these substances—ethylic indogenate, indogenic acid, or indogen—readily yields indigo blue by the action of dilute acids or alkalies, when freely exposed to the air, without heating.

THE recent study of the Rhone glacier by M. Gosset is probably the most detailed and exact that has ever been made of a glacier. According to Prof. Rutimeyer (who has recently written on the results of these researches) a precise topographical knowledge of the glacier is supplied; and the scale of representation (1 : 5000) allows of following all the details of form. There are also exact data as to the glacier's movements. Four rows of stones of different colours were placed, in 1874, on its surface, and their position has been precisely noted from time to time. These observations prove that the glacier advances much more rapidly in the upper part (600 to 680 m. since 1874) than near the extremity, where the progress has only been 150 m. below the cascade of ice; also that the ablation, *nil* in the higher parts, is very great in the lower; and that the difference in the progress of the central and the lateral parts of the glacier is much greater in the first part.

M. MONTIGNY published, a short time ago, some interesting observations on the effects of lightning on trees placed near a telegraph wire. A more extended examination of the road from Rochefort to Dinant has enabled him to mature his conclusions, and he now affirms (*Bull. Belg. Akad.*, 1) that "in the section of road beyond Rochefort, nine kilometres in extent, where one notices poplars that have been struck by lightning near a telegraph wire, the fulminant fluid has scarcely produced its effects, except in places where the provocative action of the wire is favoured by the influence exerted on it by a considerable group of lofty trees; this action is especially favoured in places where the road traverses woods on an elevation, but the differences of height seem to have less powerful influence than the surrounding and neighbourhood of wood." This conclusion agrees with what Arago observed as to the objects and places which lightning strikes by preference.

THE additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus*) from East Africa, presented by Mr. Robert Mills; a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, presented by Mr. T. W. Gourlay; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mr. E. W. Hills; a Black-backed Jackal (*Canis mesomelas*) from South Africa, presented by Capt. E. Jones; a Canada Goose (*Bernicla canadensis*) from North America, presented by Mr. George Edson; a Saker Falcon (*Falco sacer* ♂), captured in the Red Sea, presented by Mr. Battersby; a Chimpanzee (*Anthropopithecus troglodytes* ♀), an Angolan Vulture (*Gypohierax angolensis*), a Mueller's Parrakeet (*Tanygnathus muelleri*), a Ludio Monkey (*Cercopithecus ludio* ♂), a Blackish Sternothere (*Sternothærus subniger*) from West Africa, an Opossum (*Didelphys*, sp. inc.), a Kinkajou (*Cercoleptes caudivolvulus*), a Great-billed Rhea (*Rhea macrorhyncha*) from South America, four Meyer's Parrots (*Pæocephalus meyeri*) from East Africa, a Magellanic Goose (*Bernicla magellanica*) from Patagonia, a Brazilian Tree Porcupine (*Synetheres prehensilis*) from Brazil, a Western Black Cockatoo (*Calyptorhynchus naso* ♂) from Western Australia, two Indian River Snakes (*Tropidonotus quincunciatus*), an Indian Cobra (*Naia tripudians*) from India, purchased; a Crested Screamer (*Chauna chavaria*) from South America, received on approval; two Golden-headed Parrakeets (*Brotogerys tui*) from South America, received in exchange; three Chilian Pintails (*Dafila spinicauda*), hatched in the Gardens.

OUR ASTRONOMICAL COLUMN

COMET 1882 *a*.—Observations by Prof. Tacchini at the Observatory of the Collegio Romano, Rome, on April 6, gave the following place of this comet:—

M.T. at Rome. h. m. s.	R.A. h. m. s.	Decl. ° ′ ″
April 6 at 14 38 7	18 29 33·62	+44 54 34·9

From this position, which was kindly communicated by Prof. Tacchini, and those obtained at Harvard College, U.S., on March 19, and at Vienna on March 28, the following elements result:—

Perihelion passage 1882, June 9·96974 G.M.T.

	° ′ ″	
Longitude of perihelion	54 25 17·2	From Ap. Eq.
,, ascending node ...	204 37 42·1	April 0.
Inclination	73 35 39·4	
Log. perihelion distance	8·748238	

Motion—direct.

The co-ordinate constants to this parabola are (App. Eq. May 0):—

$$x = r\,[9{\cdot}96219] \sin (v + 127° \; 10'{\cdot}3)$$
$$y = r\,[9{\cdot}86101] \sin (v + 61 \; 33{\cdot}8)$$
$$z = r\,[9{\cdot}90055] \sin (v + 197 \; 45{\cdot}4).$$

Hence with the X, Y, Z of the *Nautical Almanac* the following positions are found:—

At Greenwich midnight

	R.A. h. m. s.	Decl. ° ′	Log. distance from Earth.	Sun.
April 20	19 15 41	+57 27·7	0·0528	0·1610
22	19 25 49	59 31·0	0·0414	0·1488
24	19 37 33	61 36·7	0·0303	0·1362
26	19 51 22	63 43·7	0·0196	0·1230
28	20 7 51	65 50·4	0·0093	0·1091
30	20 27 49	+67 54·8	9·9995	0·0946

Mr. J. T. Barber of Spondon, Derby, informs us that on April 6, the night of the above observation at the Collegio Romano, he considered that the total impression given by the comet's light was about equal to that of a star of the seventh magnitude. If we take the theoretical intensity of light (represented by the reciprocal of the product of the squares of the distances of the comet from the earth and sun) as *unity*, we find the intensity on the following dates:—

May 12 ...	7·1	May 20 ...	11·8	May 28	24·2
16 ...	9·1	24 ...	16·0	June 10 (perihelion)	1590·0

THE SOLAR ECLIPSE OF MAY 16.—The *Nautical Almanac* gives the following particulars of this phenomenon, which is seen as a small partial eclipse in these islands:—

	Begins. h. m.	Greatest phase. h. m.	Ends. h. m.	Magnitude (sun's diameter=1).	Angle from N. point of first contact. *Direct.* °
Greenwich	18 10·5	18 46·0	19 23·0	0·186	158
Cambridge	18 13·2	18 47·7	19 23·7	0·175	159
Oxford	18 7·2	18 41·2	19 16·7	0·173	160
Liverpool	18 6·2	18 36·7	19 8·4	0·139	163
Edinburgh	18 13·2	18 40·2	19 8·1	0·105	167
Dublin	17 55·2	18 22·9	18 51·5	0·116	166

If we apply the Littröu-Woolhouse method of distributing the times approximately over this country we have the following equations:—

G.M.T. of	h. m.
Beginning	18 6·12 + [0·4696] L − [9·2403] M.
Greatest phase	18 43·58 + [0·2142] L + [8·5528] M.
Ending	19 22·58 + [9·4197] L + [9·4134] M.

The magnitude is given by 0·205 − [8·115] L + [7·250] M.

Here the latitude of the place for which the Greenwich times are required is put $= 50^{\circ} + L$ (and expressed in degrees and decimals), and M is the longitude from Greenwich, taken positively towards the east, and expressed in minutes and decimals of time.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—Prof. Odling will conclude this term his course on the Atomic Theory; Mr. Fisher will lecture on Inorganic Chemistry; and Mr. F. J. Brown will form a class for practical instruction in organic chemistry.

Prof. Lawson will lecture at the Botanic Gardens on the General Morphology of Plants, and will continue his course on the Elements of Systematic Botany.

Mr. Yule will give a course of demonstrations at the Magdalen College Laboratory, on the Physiology of the Nervous System.

A Postmastership in Physical Science is offered by Merton College in June. The examination will be held in common with Magdalen and Jesus Colleges. The Postmastership is of the annual value of 80*l*., and is tenable for five years from election, provided that the holder does not accept or retain any appointment incompatible with the pursuance of the full course of University studies. After two years' residence the College may raise, by a sum not exceeding 20*l*. per annum, the Postmastership of such Postmasters as shall be recommended by the Tutors for their character, industry, and ability.

Candidates for the Postmastership, if members of the University, must not have exceeded six terms of University standing, but there is no limit of age.

MR. J. PERRY, M.E., has been elected to the Chair of Mechanical Engineering at the City and Guilds Technical College, Finsbury, at the open election this week.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 30.—"On the Movement of Gas in 'Vacuum Discharges.'" By William Spottiswoode, F.R.S., and J. Fletcher Moulton, F.R.S.

In the preparation of tubes for our experiments, it was often noticed, that after the exhaustion had been carried to a certain degree, the passage of a strong current had the effect of increasing the pressure. This appeared to be due to an expulsion of gas from the terminals themselves by the passage of the discharge. And accordingly the use of such currents from time to time during the process of exhaustion was adopted for making the vacuum more perfect and more permanent than otherwise would have been the case. On the other hand, it was also noticed, that after the tube had been taken off the pump and sealed in the usual way, the passage of a strong current had in some instances the effect of decreasing the pressure. We thus met with two effects, apparently due to the same cause, but diametrically opposite in character.

Matters remained in this rather confused state, until we observed, with more care than before, a tube of which the exhaustion was near the phosphorescent state, and of which both terminals were metallic cones, and consequently presented large surfaces for any action which might take place upon them.

In what may be considered to have been its normal condition, this tube showed three or four large white striæ with a dark space of considerable size round the negative terminal. On passing the discharge through the tube for some minutes, the dark space increased, the striæ became fewer and feebler in illumination, the green phosphorescence began to show itself, and the discharge showed the usual signs of reduced pressure. On suddenly reversing the current, the striæ became again more numerous and more brightly illuminated, precisely as they would be by an increase of pressure, while the other features of the discharge in a great measure resumed their original character.

The most probable explanation of these phenomena appears to be this, that the effect of the discharge is actually to alter the pressure in the tube, not by any modification in the chemical composition of the gas, but simply by driving occluded gas out of one terminal, and by drawing it in, or occluding it, at the other. On reversing the discharge, the operation is reversed, and the occluded contents of one terminal are thrown along the tube to be occluded at the other. This view of the mechanism whereby the observed phenomena are produced is supported by the absence of these appearances when the terminals are comparatively small and the pressure is such that the occluded contents of the metallic mass forming one terminal would form only a small fraction of the total mass of gas in the tube; for in that case the pressure, and consequently the appearance of the discharge, would be affected only in an inappreciable degree by the injection of the contents of the terminal. It should also be added that, when the terminals are of unequal size, the effects are unequal, as might have been expected.

The phenomena in question appears to have so important a bearing on the mechanism of the discharge itself that it becomes a question of great interest to determine whether the missing gas is to be found in either of the terminals; and, if so, whether the ejection takes place at the positive, and the occlusion at the negative terminal, or *vice versâ*. For this purpose, I have devised a tube with three terminals, but have not yet had time to complete its construction or to make the experiment.

Zoological Society, April 4.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—Mr. Sclater exhibited and made remarks on an example of a rare Flycatcher (*Cyanomyias cœlestis*) from the Philippines, which had been sent to England for determination by Dr. Moesch of Zurich.—Mr. Sclater also exhibited and made remarks on two specimens of the Subcylindrical Hornbill (*Buceros subcylindricus*), which had been formerly living in the Society's Gardens.—Dr. A. Günther read the description of a new species of freshwater Turtle from Siam, a specimen of which had been recently acquired by the British Museum. The author proposed to name it *Geoemyda impressa*, from the peculiar shape of the principal upper plates, which are not merely flattened, but distinctly concave.—Mr. W. A. Forbes read a paper on the structure of the convoluted trachea of two species of Manucode (*Manucodia atra* and *Phonygama gouldi*), and added remarks on similar conformations in the tracheæ of other birds.—Mr. J. E. Harting read a paper on the eggs of three species of wading-birds which had been obtained by the Rev. W. Deans Cowan in the neighbourhood of Fianarantosa in the Betsileo country, Madagascar. The species to which these eggs belonged were *Glareola ocularis*, *Ægialitis geoffroyi*, and *Gallinago macrodactyla*. Much interest attached to these eggs, as not having been previously described.—A communication was read from Mr. E. P. Ramsay, C.M.Z.S., containing the description of a supposed new species of *Tephras*, an example of which had been obtained by the late Mr. S. White while collecting at the Aru Islands. The author proposed to name it *Tephras whitei*, after its discoverer.

Chemical Society, April 6.—Dr. Gilbert, president, in the chair.—The following papers were read:—On the action of acetyl chloride on fumaric acid, by W. H. Perkin. The author criticises the statements of Auschütz, and considers that the views of that chemist as to the above reaction are unsatisfactory. Probably the acetyl chloride removes a molecule of water from the fumaric acid yielding maleic anhydride.—Some arguments in favour of the prism formula of benzene, by U. K. Dutt.—On a convenient apparatus for the liquefaction of ammonia, by J. Emerson Reynolds. This essentially consists of a stout iron U-tube, into one leg of which is cemented a stout glass tube, containing dry ammonia gas; the other leg of the U, which is closed by an iron cap, contains some strong solution of ammonia. The intermediate space is filled with mercury. On heating the solution, pressure is produced, sufficient to liquefy the gas.—On the transformation of urea into cyanamide, by H. J. H. Fenton. On gently heating urea with metallic sodium, a violent reaction ensues; hydrogen is evolved, and a body, having the composition and all the properties of cyanamide, is formed.—On the action of haloid acids upon hydrocyanic acid, by L. Claisen and F. E. Matthews. A crystalline substance, having the formula $2HCN+3HCl$, is obtained. By the action of alcohol on this body, the hydrochloride of the base, $HCNHNH_2$, was prepared.

Royal Horticultural Society, April 11.—Dr. M. T. Masters in the chair.—*Rhododendrons*: Mr. Mangles and Hon. and Rev. Mr. Boscawen exhibited species, seedlings, and hybrids of which the following were specially worthy of note:—*R. Forsterianum*—between *R. Edgeworthii* (male) and *R. Veitchianum* (female), raised by M. Otto Forster, of Lehenhof, Austria. It is a beautiful combination of both parent forms, being very large and fragrant. It appears to be quite barren. Hybrid between *R. campylocarpum* (yellow, male) and a crimson hybrid (female). The flowers are pendulous. The yellow tint of the male has nearly gone, but the characters of the flowers are retained. It is pink when first opening, but almost white finally. This hybrid has good pollen, and bears seed.—Hybrid between *R. Thomsoni* and *R. Fortunei*. It is peculiar in having far more flowers in the truss than either parent. It is apparently fertile, and has an abundance of good pollen.—Hybrid between *R. argenteum* and *R. ponticum*. The flower is very inferior to that of the male, which bears large, well-shaped white flowers, being small, tubular, and pink, with aborted anthers. The colour of ponticum being prepotent.—*Hyacinth blossoming underground*: Mr. A. H. Smee, of Wellington, sent a specimen which, in consequence of being under a stone, had blossomed six inches below the surface. The leaves were white, but the flowers a deep purple.—*Plants exhibited*: *Telopea speciosissima*. Mr. Green, gardener to Sir G. Macleay, exhibited a splendid truss of scarlet flowers and bracts of this proteaceous genus from Sydney, where it is a common shrub, but produces much less fine blossoms there than the specimen exhibited. It was first figured in the *Bot. Mag.* in 1808. He also exhibited a fine specimen of the monkey orchis, from Italy.

Edinburgh

Royal Society, April 3.—Prof. Balfour, vice-president, in the chair.—Sir William Thomson read a paper on the conditions of stable equilibrium of a rotating mass of gravitating liquid. Laplace had proved that a given moment of momentum in a given mass of fluid of oblate spheroidal form, such as had been shown to be a form of equilibrium by Newton and Maclausin, required for equilibrium a unique value of the excentricity. Jacobi had extended the theorem to the case of an ellipsoid rotating round the shortest of its three unequal axes. By considering the Jacobian ellipsoid which differed infinitely little from a spheroid of revolution, Sir William found a certain value for the moment of momentum such that the equilibrium of the spheroid would be stable if, and only if, its moment of momentum were not greater than this critical value. The conditions under which a disc-shaped ellipsoid would split up into two distinct masses, and the limiting values of the excentricities in the Jacobian figure consistent with stability, were also discussed.—Prof. Tait communicated a note by Prof. C. Michie Smith on atmospheric electricity, which the author had found to be strongly negative on the Suez Canal and in Madras, on occasions when other evident atmospheric conditions would have led one to expect strong positive electrification.—Prof. Chrystal communicated an interesting paper by Mr. A. Jamieson, on recent tests of Swan's lamp for fall of resistance with increase of electromotive force, and ratio of candle-power to work expended. Curves and tables of numbers were shown, giving the relations between these quantities for all the forms of incandescent lamps now in use. The resistance of the lamps when heated was measured by shunting the current through a high resistance galvanometer—Sir W. Thomson's potential galvanometer. The energy expended in keeping the lamps incandescent was estimated, and the results obtained with the incandescent lamps compared with those obtained by other investigators with the arc light.—Dr. A. P. Aitken communicated the results of preliminary observations made by the Committee appointed by the Highland and Agricultural Society to investigate the nature and causes of the two sheep diseases, "Loupingill" and "Braxy." Regarding the former a great variety of opinion existed as to its cause—inclement weather, bad herding, the presence of ergot in the pasture, ticks, &c., being all assigned as possible causes. The committee had concluded, however, that these were merely aggravations, and that the disease was probably due to the presence of an organism located in the cerebro-spinal fluid. The investigations into the nature of Braxy were just begun; but in the coming autumn the committee hoped to continue the inquiry.—Prof. Tait, in the first of three short notes, pointed out the origin of the difficulty of measuring beknottedness electromagnetically. He traced it to the fact that every knot may be looked upon as consisting of separate loops, which may be linked or laced together. When there is linking there is electromagnetic work; when there is lacing, none. In the second he showed that the form of Saturn's shadow on a plane as seen from the earth could be calculated by the same process as that employed for the image of a circle produced by a thin prism; and also, how to determine from the *observed* appearance of the shadow the form of the meridional section of the rings supposed to be surfaces of revolution. Finally he pointed out the analogy between Action in particle-dynamics and Velocity-potential in hydro-kinetics.

Paris

Academy of Sciences, April 8.—M. Jamin in the chair.—The following papers were read:—On the elliptic integral of the third species, by M. Hermite.—Note on the principle of a new photographic revolver, by M. Janssen. In the old instrument the plate is stopped each time an image is taken; in the new, it and the screen with slit have each a continuous rotatory motion. The magnitude and relation of these motions determine the rapidity of succession of the images and the conditions of their formation. The method proved successful with solar granulations. Images may be had at intervals of less than $\frac{1}{180}$ second; thus an insect's flight might be photographed.—Haloid salts of silver and potassium, by M. Berthelot.—On the union of free hydrogen with ethylene, by the same.—On the specific heat of hyponitric acid, by MM. Berthelot and Ogier.—On a thesis of meteorology recently maintained before the Paris Faculty of Sciences, by M. Faye. In this thesis, on the fœhn and the sirocco, M. Hebert has recourse to the theory of descending vortical movements, and M. Faye regards this as a step in advance. Progress would be accelerated (he urges) by giving meteorology a place in the Faculties.—On some types of plants recently observed in the fossil state, by M. de Saporta. These plants are from the Permian of the Oural region, and the Cretaceous of the Fuveau valley (Bouches-du-Rhône).—M. Bert was elected Member in Medicine and Surgery, in room of the late M. Bouillaud.—Researches on the passage of electricity through rarefied air, by M. Edlund. He has proved experimentally that the principal obstacle met by the current at the surface of passage between the electrodes and the rarefied gas, is due to an electromotive force giving an opposite current; which force, beyond a certain limit of rarefaction, continuously increases.—On a class of unicursal curves, by M. Darboux.—On hypercycles, by M. Laguerre.—On uniform doubly periodic functions, with essential singular points, by M. Appell.—On the theory of uniform functions of a variable, by M. Mittag-Leffler.—General relation between any seven points of a conic section; conic of homology; properties common to three homographic figures, by M. Parry.—Study of solar apparatus, by M. Crova. This gives results of a year's experiments by a Government Commission at Montpellier with a solar mirror and boiler; (similar experiments have been made at Constantine). The maxima of yield generally correspond to the minima of intensity of radiations. The absolute quantity of heat utilised depends essentially on the temperature of the air. In our climates it is not possible to reach half the utilisation realisable in the most favourable circumstances; and the sun does not shine continuously enough to favour the practical use of the apparatus.—On the heat due to magnetisation, by M. Pilleux. He was able to heat, to more than 200°, the iron core of an electro-magnet, with the alternative currents of a Meritens' machine; a non-magnetic core was not heated. Using various iron and steel cores, the coercitive force is proved to increase their heating under frequent magnetisation and de-magnetisation; it acts like resistance to the passage of electricity.—On the absorption-spectrum of pernitric acid, by M. Chappuis. This spectrum is important, especially as a means of verifying that ozone has been prepared with oxygen exempt from nitrogen.—On the electrolysis of distilled water, by M. Tommasi. His experiments prove that water may be electrolysed, even by the current of a very weak battery, provided the calories liberated by this battery are at least equal to the calories absorbed by water in being decomposed, about sixty-nine calories.—On the determination of nitric and nitrous acid in the state of ammonia, by M. Guyard.—On the effects of compression on the hardness of steel, by M. Lan. The compression of fused steel has been practised both in France and England; increasing the hardness and the proportion of combined carbon.—On the composition of hydrated carbonic acid, by M. Wroblewski. At zero temperature and about 16 atm. it consists of 1 eq. of carbonic acid and 8 eq. of water.—On the bisulphydrate and cyanhydrate of ammonia, by M. Isambert.—Action of sulphuretted hydrogen on saline solutions of nickel and metals of the same group, by M. Baubigny.—On ammoniacal chlorides of zinc, by M. André.—On the hydrate of sulphuretted hydrogen, by M. de Foucrand.—Synthesis of quinine, by M. Maumené. The discovery of H_8N furnished him with the means of this synthesis—details of which are not yet given.—Action of fuming nitric acid, and action of hydrochloric acid on pilocarpine, by M. Chastaing. — Gastric microzymas and pepsine; remarks on M. Gautier's note of March 6, by M. Béchamp.—On the existence of products similar to ptomaines in gastric and pancreatic digestion of several albuminoid matters, by M. Béchamp.—Digestion of fatty and cellulosic matters, by M. Duclaux. The emulsion of fatty matters by the pancreas he regards as a phenomenon almost exclusively physical (not produced by diastase). The agents of digestion of cellulose seem to be small rod-like organisms, which one finds in seeds in the crop of birds or the paunch of ruminants. M. Faye recalled former experiments of his own on emulsion.—On the resistance of African asses to charbon fever, by M. Tayon.—Researches on the nervous system of larvæ of dipterous insects, by M. Brandt.—The Alcyonarians of the Bay of Marseilles, by M. Marion.—On the development of the ganglion and the ciliated sac in the bud of Pyrosoma, by M. Joliet.—Artificial reproduction of witherite, strontianite, and calcite, by M. Bourgeois.—On the artificial reproduction of a crystalline hydrated silicate, by M. de Schulten.—On the limit between the lias and the inferior oölite, according to documents left by Henri Hermite, by M. Vélain.

April 10.—M. Blanchard in the chair.—The following papers were read:—On the secular displacements of planes of the orbits of three planets, by M. Tisserand.—Movements acquired by different parts of a liquid within a vessel or reservoir, whence it escapes by an orifice, by M. de Saint Venant.—Philosophical essay on the method named by its author the "Science of order," by M. Villerceau.—Use of instantaneous photography for analysis of movements in animals, by M. Marey. He gives details of his photographic rifle, as applied to birds' flight. A central axis, rotated by clockwork twelve times in a second, when a detent is released, commands all the pieces. There is an opaque disc, with a small hole; behind this a disc with twelve holes, which rotates intermittently; and behind this a sensitised plate, circular or octagonal, on the margin of which the images appear. To get good effects in the phenakistiscope it was necessary to increase the number of images, and M. Marey found 1-1400th second (as against 1-720th before) sufficient exposure. M. Marey found the chief results of his study by the graphic method confirmed. He makes some remarks on bats' flight, which is difficult to photograph.—On some recent types of fossil plants (continued), by M. de Laporta.—Note on the quarantines imposed at Suez on maritime arrivals from the extreme East, by M. de Lesseps. He holds the measures taken are useless and vexatious. The evil (cholera) should be vigorously attacked at the centres of infection formed by the large concourses of pilgrims, and care taken to disinfect putrid dejections, and destroy objects that may have been contaminated.—On the necessity of destroying the winter egg of phylloxera, by M. Balbiani.—On the winter egg of phylloxera, by M. Mayet.—Observations of comet *a*, 1882, at Lyons Observatory (Brünner 6 inch equatorial), by M. Gonessiat.—Observations of the same at the Roman College, by M. Tacchini.—Observations of solar eruptions in 1881; spectrum of Well's comet, by the same. There are two maxima of the former between ± 10° and ± 30°, and more eruptions appeared in the north than in the south; 40 metallic eruptions were observed, against 10 in 1880. (Curves of the solar phenomena are given).—On hypercycles, by M. Laguerre.—On the integration, by Abelian functions, of certain equations with partial derivatives of the first order, by M. Picard.—On Fuchsian functions, by M. Poincaré.—On the theory of uniform functions of a variable, by M. Mittag-Leffler.—On general inversion, by M. Vanacek.—Resistance of a prismatic and homogeneous bar, of supposed infinite length, to transversal shock and longitudinal shock, by M. Boussinesq.—Experimental researches on the thermal conductivity of minerals and rocks, by M. Thoulet. The novelty of his method (applied to glass, forged iron, and anhydrite) consists in substituting a precise estimation of times for determination of temperatures.—Evaluation of thermal conductivity by measurement of times during a variable period, by M. Lagarde.—On electrolysis, by M. Tommasi. He proves the law that when a voltaic current traverses several electrolytes, it is necessary (for decomposition to occur) that the quantity of calories produced by the pile be equal to the sum of the calories absorbed by each electrolyte *plus* the calories required to overcome the total resistance of the electrolytes.—Researches on the solubility of aluminates of lime in water; influence of this on the final hardening of hydraulic materials, by M. Landrin.—On the relation between isomorphism, atomic weights, and comparative toxicity of metallic salts, by Mr. Blake. His experiments (in which the substances were introduced directly into the blood) prove, in opposition to M. Richet, an intimate connection between chemical function and physiological action.—On some physical properties of bichlorised camphor, by MM. Cazeneuve and Didelot.—Peptones and alkaloids; reply to M. Béchamp, by M. Tanret.—On the rapidity of propagation of the inoculated bacterium of charbon, by M. Rodet, with M. Davaine. He found great irregularity in the effects of destruction of the inoculated part.—The puceron of Latania, by M. Lichtenstein.—On the density and the chlorination of sea-water obtained in the *Travailleur* in 1881, by M. Bouquet de la Grye. The density and saltness are shown to increase on passing from the ocean to the Mediterranean. The surface-waters are less salt and dense than the lower, and generally the increase varies in the same sense as the depth. Measurements of density at 400 m. depth, opposite the Rhone mouth, and in the Gulf of Gascogne, indicated a difference of height of 0·72 m. (agreeing with M. Bourdalone's observations).—On a recent communication of M. Dieulafait, on ophitic rocks of the Pyrenees, by M. Virlet d'Auost.

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