From Nature.

THE COMING TOTAL SOLAR ECLIPSE.

THERE is no doubt whatever that the eclipse which will sweep over the United States next July will be observed as no eclipse has ever been observed before. The wealth of men, the wealth of instruments, and the wealth of skill in all matters astronomical already accumulated there, makes us old-country people almost gasp when we try to picture to ourselves what the golden age will be like there, when already they are so far ahead of us in so many particulars.

Draper, Hall, Harkness, Holden, Langley, Newcomb, Peters, Peirce, Pickering, Rutherford, Trouvelot, and last, but not least, Young, are the names that at once run easily off the pen to form a skeleton list, capable of considerable expansion

with a little thought, when one thinks of the men who will be there. One knows too that all the enthusiasm of devoted students and all the appliances of modern science — appliances in the creation of which many of those named have borne so noble a part — will not be lacking. So that we may be sure that not only all old methods but all possible new ones will be tried to make this year one destined to be memorable in the annals of science side by side with 1706, 1851, 1860, and other later years.

Thank Heaven, too, there is no necessity that the thankless task of organizing an "Eclipse Expedition" from this country should fall on any unfortunate individual, among other reasons because — and this is a very hopeful sign of increasing general interest taken in scientific work — Messrs. Ismay, Imray and Co., the owners of the White Star Line, have expressed in the warmest manner their desire to aid English observers by a considerable reduction of fares, and the directors of the Pennsylvanian Railway Company, as the readers of *Nature* have already been made aware, have done the like in the case of observers coming from Europe in their individual capacity.*

The progress in that branch of knowledge which requires the aid of eclipse observations has been so rapid during the last few years that the eclipse of 1868, though it happened only ten years ago, seems to be as far removed from the present as the Middle Ages are in regard to many other branches of culture. The work done by the spectroscope since that year, when, in the hands of Janssen, Pogson, Herschel, and others, it added so enormously to our knowledge, has gradually covered larger and larger ground, and each successive eclipse in 1869, 1870, 1871 and 1875, has seen some variations in its use, so that its employment has proved the most novel, if not the most powerful, side of the attack.

Young's work of 1869 will no doubt form the key-note of much that will be done this year so far as the coronal atmosphere is concerned. It will be remembered that Young in 1869 observed a continuous spectrum, while Janssen in 1871 observed a non-continuous one, for he recorded the presence of the more prominent Fraunhofer lines, notably D. This positive observation from so distinguished an observer demands attention, not only on its own account, but because of the question which hangs upon it, which is this: does the corona reflect solar light to us or does it not, and if it does, *where* are those particles which thus act as reflectors? On this point the photographs taken in Siam in 1875 are silent, as the method employed was not intended to discriminate between a continuous and a discontinuous spectrum.

But although this point remains, how greatly has the ground been cleared since 1869! That wonderful line, "1474," is more familiar to us now; and yet there has been almost a chapter of accidents about it. In the first place, with regard to this line above all others, there appears to be a mistake in Angström's map; the solar line at 1474 is not due to iron at all; with the most powerful arc there is no iron line to be seen there. Then Secchi attributed it to hydrogen, though I am not aware on what evidence. But whatever be its origin, the fact remains that we now know by its means that the solar hydrogen is traversed and enwrapped by the substance which gives rise to the line to an enormous height, so that it forms the highest portion of the atmosphere which is hot enough to render its presence manifest to us by spectral lines. Here, so far as I know, only one point of difference remains. In 1871 I most distinctly saw the line trumpet-shaped, that is, with the base broadening as the spectrum of the photosphere was reached, while Janssen saw it stopping short of the spectrum of the photosphere. The importance of this point is that supposing one of us to be mistaken and one or other observation to represent a *constant* condition, then, if the line broadens downwards till the sun is reached we are dealing with a gas lighter than hydrogen, capable of existing at a high temperature, which thins out as the other gases and vapors do in consequence of its vapor-density being below that of hydrogen; or, on the other hand, if the line stops short as a constant condition, it represents a substance which is probably dissociated at the lower levels, and is therefore probably a compound gas; and then the question arises whether it has not hydrogen as one of its constituents.

Perhaps I may conveniently refer to a paper of mine which was read at the Royal Society last Thursday in this connection, because it may be that the solar regions most worthy of the closest study at

* In fact Messrs. Ismay, Imray and Co. have just announced that they will take properly certified observers and bring them home again for the sum of 20*l.*, which is rather less than first-class single fare; so that English observers will be carried to Denver or the Rocky Mountains and back again for the sum of 34*l.*

the present time are precisely these higher reaches of the sun's atmosphere. There is little doubt, I think, that around the sun's visible atmosphere matter exists at a temperature low enough not to give us its autobiography in the bright-line manner, and there is evidence that matter existing under such conditions, absorbing as it must do some of the sun's light, will, if it remains elemental, give us an absorption of the fluted kind, or again will absorb only in the blue or ultra-violet region.

Now the more the chemistry of the reversing lower layer of the sun's atmosphere — that in which the upper level of the photosphere is bathed — is examined the more metallic it is found to be. For instance, my own work has enabled me to trace with more or less certainty eighteen metallic elements,* in addition to those recorded by previous observers; but of metalloids in this region I have traced none. The persistency with which metal after metal revealed itself to the exclusion of the metalloids led me to throw out the idea some time ago, that perhaps the metalloids lay as a whole above the metals, and shortly afterwards I obtained evidence which seemed to me of a very satisfactory nature as to the existence of carbon, its presence in the sun's atmosphere being rendered probable by fluted bands, and not by lines. There were two points, however, which remained to be settled before the matter could be considered to be placed beyond all doubt.

The first was to establish that the fluted bands generally present in the spectrum of the electric arc, as photographed, which bands vary very considerably in strength according to the volatility of the metal under experiment, were really bands of carbon — a point denied by Ångström and Thalèn.

This point I have settled by two photographs, in which the carbon bands remain the same, though one spectrum is that of carbon in air, the other of carbon in dry chlorine.

The next point was to insure accuracy by the most positive evidence that there was absolutely no shift in the carbon bands. Such a shift is produced when the part of the arc photographed is not perfectly in the prolongation of the axis of the collimator of the spectroscope. Its effect is to throw the lines of iron, for instance, a little to the right or a little to the left of the Fraunhofer lines with which they really correspond.

I have now obtained a photograph which supplies such evidence. There are metallic lines close to the carbon bands which are prolongations of Fraunhofer's lines, while the lines which I have already mapped at W. L., 39·27 and 39·295, in the spectrum of iron, are also absolute prolongations. Therefore there is no shift in the carbon flutings, and the individual members of the fluted spectra in the brightest portion are absolute prolongations of a fine series of Fraunhofer lines in the ultra-violet.

Now how does this connect itself with observations of the upper parts of the solar atmosphere?

Angström has already shown that the true carbon *lines* which we get when a coil and jar are employed are not reversed in the spectrum of the sun, and I have already shown that the calcium spectrum in the sun is similar to the spectrum obtained when the spark, and not the arc, is employed. Accompanying the change from a high to a higher temperature, there is a change in the intensity of the lines — some thicken, others become thinner. We can only match the relative thickness of the solar calcium lines by employing a very powerful coil and jar — so powerful, indeed, that the lines, and not the flutings, of carbon would be visible in the spark given by it. It is fair then to say that if carbon were present with the calcium *in the sun's reversing layer*, we should get the lines of carbon when we get the calcium lines appearing as they do.

As we do not get this evidence, we are driven to the conclusion that the carbon vapor exists not only in a more complicated molecular condition (as is evinced by the flutings) than the metallic vapors in the sun's atmosphere, but at a lower temperature. It must, therefore, exist *above the chromosphere*, that is, in a region of lower temperature. Lower pressure, again is indicated by the feeble reversal, so that everything points to a high level.

The question is, will this region be recognized during the coming eclipse?

Coming down lower we reach a level better known, and of which, perhaps, the interest during the eclipse will now be less, if we except the possibilities opened out to us by photography. One good photograph of the lines visible in the lower chromosphere will be of incalculable value. Attempts may be made on the cusps just before and after totality, and if only one

* These are strontium, lead, cadmium, potassium, cerium, uranium, vanadium, palladium, molybdenum, indium, lithium, rubidium, cæsium, bismuth, tin, lanthanum, glucinum, and yttrium or erbium.

of these succeeds we shall have the ordinary solar spectrum as a scale. If good pictures near H can be secured, enough information now exists for that region to enable us to determine the chemical origin of the bright lines photographed. These remarks apply to attempts made with spectroscopes furnished with slits in the ordinary way; there is little doubt, however, that the method utilized for the Siam eclipse in 1875, the method suggested by Prof. Young and myself for the Indian eclipse of 1871, will also be taken advantage of; here the chromosphere itself becomes the slit. A dispersed series of spectral images of the thing itself, instead of the spectrum of a part of the image of it focussed on a slit is obtained, the position of each image in the spectrum enabling its chemical origin to be ascertained if only a comparison spectrum can be secured at the same time.

In 1875, in the expedition to Siam, the photographs of this nature were obtained by means of a prism, and the results obtained by that expedition led me to think that, possibly, this method of using the coronal atmosphere as a circular slit might be applied under very favorable conditions if the prism, or train of prisms, hitherto employed, were replaced by a reflection grating, with which the generosity of Mr. Rutherford has made many of us familiar, for the simple reason that while a prism only gives us one spectrum, a brilliant grating placed at right angles to an incident beam gives us spectra of different orders, so-called, on each side of the line, perpendicular to its surface. Of these two or three are bright enough to be utilized on each side, so that we can get six in all.

To test this notion I made the following experiment with a grating given to me by Mr. Rutherford. This magnificent instrument contains seventeen thousand two hundred and eighty lines to the inch, ruled on glass and silvered; its brilliancy is remarkable.

In front of the condenser of an electric lamp adjusted to throw a parallel beam, I placed a circular aperture, cut in cardboard, forming a ring some two inches in interior diameter, the breadth of the ring being about one-eighth of an inch. This was intended to represent the chromosphere, and formed my artificial eclipse.

At some distance from the lamp I mounted a three and three-fourths inch Cooke telescope. Some distance short of the focus I placed the grating; the spectrum of the circular slit illuminated by sodium vapor and carbon vapor was photographed for the first, second, and third orders on one side. The third-order spectrum, showing the exquisite rings due to the carbon-vapor flutings, was produced in forty-two seconds. The first-order spectrum, obtained in the same period of time, was very much over-exposed. It is, therefore, I think, not expecting too much that we should be able to take a photograph of the eclipse, in the third order, in two minutes. Similarly, we may hope for a photograph of the second order in two minutes, and it is, I think, highly probable also that a photograph of the first order may be obtained in one minute. To make assurance doubly sure, the whole of the totality may be used during the coming eclipse, but if there be several such attempts made it will certainly be worth while to try what a shorter exposure will do.

Now, by mounting photographic plates on both sides of the axis, one solidly mounted equatorial of short focal length may enable us to obtain several such photographs, with varying lengths of exposure. I insist upon the solidity of the mounting because, if any one plate is to be exposed during the whole of totality, the instrument must not be violently disturbed or shaken while the eclipse is going on. I think, however, it is quite possible to obtain more than one photograph of the lower-order spectra without any such disturbance in this way. The same plate may be made to record three, or even four, exposures in the case of the first order in an eclipse of four minutes' duration, by merely raising or lowering it after a given time, by means of a rapid screw or other equivalent contrivance, so that a fresh portion of the same plate may be exposed. Similarly, the plates on which the spectra of the second order are to be recorded may be made to perform double duty.

If one equatorial thus mounted were to be devoted to each quadrant of the coronal atmosphere, it is certain, I think, that most important results would be obtained.

It will be convenient here to give the results arrived at by the Siam expedition with an instrument of this description, which, for shortness, was called a prismatic camera.

The plates secured present at first sight a very puzzling appearance; they are unlike anything ever obtained before, and a good deal of thought had to be spent upon them before all the knowledge they were afterwards found capable of furnishing to

us was properly appreciated. One of the plates was exposed for one minute at the commencement of totality, the other for two minutes at the end. The differences between them are those due to the phases of the eclipse. In the first, two strong protuberances close together are photographed; these are partially covered up in the second, while another series is revealed on the following limb in consequence of the motion of the moon over the sun.

Now in both the photographs — that exposed for one minute and that exposed for two — the strongest of the prominences are repeated three times, that is to say, three spectral images of them are visible, each of these images being produced by light of different wave-lengths which the prominences emitted.

The question is what are these particular wave-lengths thus rendered visible? Unfortunately no photograph was taken of the cusps either before or after totality; a scale therefore was out of the question; and when the task of assigning wave-lengths to these spectral images fell upon Dr. Schuster and myself, while we were preparing the report which was sent in to the Royal Society last year, the difficulties we encountered were very considerable.

Everybody I think will consider that we were justified in expecting the lines of hydrogen to be represented in such a photograph. Now the photographic hydrogen lines are those at F, near G, and at *h*, and the silver salts usually employed are such that the action is most intense near G, less intense near *h*, and least at F; the running down from G to F being rapid, and that from G to *h* much more gradual, so that while at one end F may be said to be the limit of photographic activity, at the other it is continued long past *h*. We were therefore justified in assuming as the preliminary hypothesis, that the image of least refrangibility was produced by the F light of hydrogen, the more so as the continuous spectrum also photographed — which continuous spectrum, as we had independent means of determining, came from the base of the corona — gave us also an idea of the part of the spectrum in which each image was located.

Taking then F as a starting-point and assuming the next line to be the one near G, we had a quite satisfactory method of checking the assumption, by comparing the real distance between the images with the calculated one.

A goniometer was therefore brought into requisition, and the angular distance between F and the line near G carefully measured in order to determine the dispersion of the prism actually employed. This dispersion was one which should bring the images about as far apart as they were actually found to be; this therefore was so far in favor of our assumption, that is to say, it did look as if we had got hold, on the photographs, of images of the prominences built up by the F and G light of hydrogen.

It was next the turn of the third line, the one at *h*. On the assumption already made, it was easy to determine the distance from the G image, at which the one representing *h* should lie. In this place, however, we found no image whatever of any of the prominences.

Now this was a very extraordinary result, and there was only one way, so far as we could then see, of accounting for it. Dr. Frankland and myself, nearly ten years ago now, produced evidence which seemed to indicate that this line of hydrogen was only produced by a very high temperature. This being so, then, we should have to conclude that the prominences were of a relatively low temperature; this, however, I am far from saying, and here there is undoubted work of the greatest value to be done at the next eclipse, and I for one feel certain that our American cousins will do it.

I have not, however, yet referred to the strongest image of all shown in the photographs. This lies a little further from the central one than does the first on the other side of it. On the assumption before stated its wave-length lies somewhere near 3957. This number, of course, is only an approximate one, but the region occupied by the line was obviously so near the boundary of the visible spectrum, that a long series of experiments, in which we called in the aid of photography and fluorescence, was made in order to determine whether an unrecorded hydrogen line existed in that region. All I can say is that the point may be said to be yet undetermined. It is quite true that in several vacuum tubes which Dr. Schuster and myself employed, a strong line more refrangible than H was seen, but then these same tubes unfortunately showed us lines in the visible spectrum, which beyond all doubt did not belong to hydrogen. The elimination of impurities is such a delicate matter, and one requiring such a large expenditure of time, that our report was sent in leaving this point *sub judice*. We tried hydrogen at atmospheric pres-

sure in order to get such a predominance of the hydrogen vibrations as to mask the impurities, but this did not serve us, for the continuous spectrum was so bright in the violet and ultra-violet as to render observations of lines next to impossible. Owing to many reasons, Dr. Schuster's absence from London being one of them, we have not been able to renew the search.

The near coincidence of this spectral image with the H line leads us to ask the question whether Young's beautiful work in his mountain observatory might not help us on this point. Young found the calcium lines always reversed in the penumbra, and near every large spot. This important statement shows us that calcium is one of the metallic vapors which is most frequently ejected from below into the prominences; it is possible, therefore, that the prominences, the spectral images of which were photographed, may have been due to an eruption of calcium. This, of course, is only a suggestion, but the fact that it is a suggestion merely shows how important it is that this point should engage attention next July. If the prominences are then constituted as they were in '75, this violet line will doubtless turn up again, and that is why I have been most anxious to point out not only the conclusions to which we have been led, but the extreme difficulty of arriving at any conclusion whatever, unless by one method or another we have an absolute comparison of the spectrum of the prominences with that of the sun itself.

I have before referred to the fact of the registration on the plates of a continuous spectrum. If we were to suppose the whole light of the corona to be due to 1474 light, for instance, we should expect to get just as definite an image of the corona in the prismatic camera as in an ordinary one. And if everything outside the moon gave us nothing but a line spectrum, the moon's limb would have a perfectly defined edge. Now as a matter of fact, only one such edge is seen in the photographs. We have only one complete ring with a thoroughly defined hard outline, such as that to which reference has been made. This hard ring corresponds to the second spectral image of the prominences, and is a continuation of it. Supposing we were right about the prominences, the ring would be due to the high-temperature *h* line of hydrogen (supposing us wrong it might be a companion line to 1474); as the observations of Respighi, Janssen, and others, in the Indian eclipse of '71 endorsed the American observations of '69 that the hydrogen lines are the strongest in the photographic parts of the corona, we may very possibly be really dealing with hydrogen.

Now the edge of the corona, or the upper part of it considering it as the sun's atmosphere, as seen on our photographs, is precisely such as would be given by homogeneous light; that is, there is a distinct image, and there is one image and not three or any other number. Have we any means of determining the wave-length of the light by which this image has been produced? Let me give an idea of one method which we employed. A circle of the same size as the image of the moon on a photographic enlargement of the original negative was cut in paper and placed over the enlargement until the corona was symmetrical round it, as we know it to have been symmetrical round the moon's body, or nearly so, at that phase of the eclipse.

We found as a considerable endorsement of the assumption which we made regarding the hydrogenic origin of the chromospheric images, that the paper circle in this position had its circumference coincident with the hard ring to which I have referred as being a continuation of the middle spectral image of the prominences. Next, one of the ordinary photographs of the corona was enlarged to the same size as that of the one produced in the prismatic camera. When these were superposed so that the outlines of both coincided as much as possible, it was again found that the edge of the moon lay along the ring.

Now then for the continuous spectrum. The general woolliness of the photographs which at first sight gives rise to the idea that they were out of focus, and that there is nothing to be got out of them, is of course only in one direction, that at right angles to the edge of the prism employed. There is a well-defined structure running parallel to this direction, which of course is the line of dispersion; this structure is doubtless due to irregularities in the corona, drawn out by the prism into bands; it is easy to determine the limits of this continuous spectrum.

Examining the centre of the photographs we find that on one side the structure stops short at F, on the other it extends to a considerable distance beyond the prominence image in the ultra-violet, spaces of light being visible beyond 3530.

From these data we concluded that the continuous-spectrum-giving region extends at least to a distance of 3′ of arc from the sun's limb. This continuous spectrum is

well shown on photographs taken at the beginning and end of the eclipse. One of the plates of the prismatic camera was exposed, until the signal for the end of totality was given. Dr. Schuster states that all the observers agreed that the signal was given rather too late, and the fog on the plate indicates an intense illumination; nevertheless, the edge of the sun is not drawn out into a continuous band but rather into three distinct bands. It is probable, therefore, that when the plate was exposed, only the lower part of the chromosphere had appeared, and that it gave out light of such intensity that everybody imagined that the sun itself had come out of eclipse. I observed this myself in 1871, and a very striking fact it is.

So much then for the results obtained by the prismatic camera in '75. When the report is issued — and its issue cannot be much longer delayed — it will be seen that the hasty sketch I have now given can be followed in greater detail.

One of the most remarkable points about the expedition to Siam was the failure to obtain even spectra of the sun with the ordinary telespectroscopic cameras employed. No doubt the unforeseen delays which left very little time for the adjustment of instruments, have a great deal to answer for. I have little doubt that if the attempt is made next July, when any quantity of skilled help will be at hand, and any amount of rehearsal will be possible, that a full measure of success will be obtained, at all events for the most photographic part of the spectrum. An ordinary photograph of the corona was obtained by Dr. Schuster in two seconds; and my experience with photographic spectra enables me to say that this photograph was taken by means of an almost monochromatic light — that near G. Now as the coming eclipse will enable an exposure of almost one hundred times longer than this to be employed, I do not think that the undoubted feebleness of the object need be feared. Besides, this method would enable us to pick up the light of these lower reaches of the chromosphere which, as has been already stated, are of such extreme brilliancy as to have been mistaken, on many occasions, for the sun itself.

Up to the present time no attempt has been made to obtain a photographic record of the polarization of the corona. The difference of colors indicating radial polarization observed by me when I used the biquartz in 1871, certainly have left the impression on my mind that it would be quite easy to obtain a permanent record of them. This would be a very valuable result, and one which would set at rest a question which, though I consider it settled in my own mind, is yet, I believe, held to be still doubtful by many interested in these matters.

In what I have written I have touched only upon obvious work suggested by the previous observations. I have little doubt that the preparations of the skilled astronomers of the United States include many surprises and daring attempts among the solid work which we are quite certain of.

All here wish them the extremest measure of success, which I am sure their efforts will do more than command.

J. NORMAN LOCKYER.