IX. *On the Total Solar Eclipse of August* 29, 1886.

By Captain L. DARWIN, *R.E.*, ARTHUR SCHUSTER, *Ph.D.*, *F.R.S.*, *and* E. WALTER MAUNDER.

Received January 28,—Read February 14, 1889.

[PLATES 9, 10.]

CONTENTS.

I. ORIGIN OF THE EXPEDITION AND GENERAL PREPARATIONS. BY CAPTAIN DARWIN, ARTHUR SCHUSTER, AND E. W. MAUNDER.

AN expedition for observing the Eclipse of the Sun of August, 1886, was organised and sent out by the Royal Society, the necessary funds being obtained partly by a special vote from the Treasury, partly from the annual grant to the Royal Society, and partly from the Society's private funds. A Committee appointed by the Council of the Royal Society discussed the principal questions to which observers were to direct their attention, and distributed the available instruments amongst them. It was also decided that, as far as the scientific part of the work was concerned, the observers should be independent of each other, and report separately to the Society; but that they should elect one of their members as chief, to represent them in all dealings with the authorities in the West Indies. Mr. NORMAN LOCKYER was accordingly chosen to be this representative.

The present report only deals with the photographic results obtained by its authors. Mr. NORMAN LOCKYER was the only other observer who took out photographic instruments; most unfortunately, the weather proved so bad at the station he selected that he was unable to see anything of the eclipse.

Captain ABNEY was unfortunately not able to take part in the expedition, but he gave his invaluable help to the observers in their preparations, and in this way contributed most materially to whatever success the photographic part of the expedition may have obtained. The photographic plates used by Dr. SCHUSTER and Mr. MAUNDER were prepared by him, and we wish to offer him our best thanks for the assistance he has rendered us.

The expedition left England on the 29th of July, 1886, and arrived at St. George, Grenada, on the 12th of August. A letter had kindly been sent by the Colonial Office to the colony, stating what the requirements of the expedition would be. The members consequently found on their arrival that the Governor, Mr. W. J. SENDALL, had made every possible inquiry as to the best sites for the observatories, taking into consideration the weather probabilities as well as their personal comfort; and they have to thank him for the greatest courtesy and consideration during the whole of their stay in the island. The protection of the instruments having been mentioned in the Colonial Office letter, Captain I. C. MALING, the Colonial Secretary, very kindly prepared models of huts which could be cheaply and readily constructed on the spot. When the expedition arrived at Barbadoes on their outward journey, they found these models awaiting them. A telegram was despatched to Grenada approving generally of the design, and thus work was actually commenced before the arrival of the expedition in the island.

Before the observers left England, the President of the Royal Society had written to the Admiralty requesting the co-operation of any men-of-war that might be on the station. As a result of this communication, three of Her Majesty's ships—the "Fantôme," Commander R. H. ARCHER, R.N.; the "Bullfrog," Lieutenant J. MASTERMAN, R.N.; and the "Sparrowhawk," Lieutenant C. F. OLDHAM, R.N.—were found ready and prepared to render every assistance. Every member of the expedition felt grateful for the willing way in which the valuable assistance of both the officers and men of these ships was given; and the President of the Royal Society, in a letter to the Admiralty on the return of the expedition, expressed the value to science of such ready co-operation.

After the arrival of the expedition at Grenada, two or three days were occupied in selecting stations and making preparations for conveying the observers to their destinations. St. George itself was not favourably situated, and it was moreover considered advisable to scatter the observatories as much as possible, so as to avoid the chance of a single mass of cloud proving fatal to the whole expedition. The observers were therefore divided into four groups, and, with the aid of the above-mentioned men-of-war, they soon found themselves distributed amongst their various stations. Mr. MAUNDER, who accompanied the Rev. S. J. PERRY, was conveyed in the "Bullfrog" to Carriacou Island, where a station was selected near the southern extremity of the island, close to a small house called "the Hermitage," belonging to Mr. PETER DRUMMOND, a gentleman of Jersey, who happened most fortunately to be visiting Carriacou at the time, and who, in the most generous manner, gave up the use of his premises to the observers. Mr. SCHUSTER and Captain DARWIN were dropped at Prickly Point by the "Fantôme," and there they found excellent quarters in a house which Mr. F. M. CHADWICK, the Colonial Treasurer, kindly placed at their disposal.

II. PREPARATIONS FOR THE ECLIPSE AT PRICKLY POINT. BY CAPTAIN DARWIN AND ARTHUR SCHUSTER.

The instruments used during totality at Prickly Point were mounted on two equatorial stands, which were placed in separate huts at a distance of about 20 yards from each other. On the evening of Thursday, August 19, the polar axes of both stands were adjusted in the usual way. Finders had been attached for this purpose to the photographic cameras, but more attention should be given in future to have these finders in convenient positions for observation, and of not too small an aperture.

One instrument, which was under Captain DARWIN'S charge, was placed on a solid rock foundation, and the first adjustment was therefore considered sufficient, especially as extreme accuracy was not required for the purpose for which it was chiefly employed.

The foundation for the equatorial stand which carried Dr. SCHUSTER'S instrument did not, unfortunately, prove sufficiently firm. As there was reason to fear that the heavy rains during the week preceding the eclipse might have altered the position of the polar axis sufficiently to interfere with the sharpness of the image, and as the camera could not be reversed for adjustment as long as the spectroscopes were attached to it, these were once more removed on Friday, August 27, two days before the eclipse, and the routine of adjustment once more gone through. From observations taken the day after the eclipse it appeared that the altitude of the polar axis was about 3′ too low; the error was therefore sufficiently small not to produce a detrimental effect during the time of exposure.

The clocks of the instruments were frequently adjusted, by comparing the rotation of the hour circle when the instrument was going with the time marked on a chronometer. In neither case were the instruments and their stands designed for each other, and it was found impossible to balance the instruments properly without a too great increase of weight. Thus the work which the clocks had to perform was very different in different positions of the instrument. To remedy this evil as much as possible, the clock adjustment was carried on in the position which the equatorials were to have during totality.

Owing to the unsettled state of the weather, the preparations for the eclipse were carried on under great difficulties, and the time at our disposal was found barely sufficient. We arrived at the observatory on a Tuesday, and the remainder of that week was taken up with the erection of the equatorials and the preliminary adjustments of the various instruments. During the week preceding the eclipse much time was lost owing to the frequent interruption of the work by heavy tropical showers. Tuesday and Wednesday were wet and stormy and no direct Sun light was available, although much required, to get the instruments into working order. Thursday was fine, and in the morning good progress was made; but Friday was again wet, and was followed by a rainy night. Saturday, the day before the eclipse, was cloudy in the morning, and the Sun only appeared at intervals. Our experience has thus taught us that a fortnight's time for preparation is hardly sufficient when two observers have to look after five different instruments, all requiring careful treatment. After the days had been spent in continuous work, the evenings were taken up with the preparation of photographic chemicals and occasional star observations.

We were without intelligent help except during the two days when Mr. LAWRANCE came to Prickly Point. We had taken him out, jointly with Professor THORPE, as private assistant, anticipating the great difficulties we should have to encounter. His time, however, was chiefly taken up at Hog Island, where Professor THORPE was observing, but our thanks are due to him for the assistance he gave us during these two days. As for unskilled help, we engaged for the whole time one negro servant, but now consider we should have done better to have had two.

The damp climate, with its steady temperature, varying day and night only between 81° and 85°, proved very exhausting, and the work could not be carried on as well and as quickly as it might have been under more favourable circumstances.

The richness of animal life proved a source of great annoyance. Mosquitos abounded in our residence. Wasps built their nests and spiders spun their webs with remarkable rapidity. The photographic room and even one of the equatorial stands had to be cleared of wasps' nests, and a dense spider's web was found an hour before totality stretched across the slit of one of the spectroscopes; the slit having been perfectly clear the night before.

It is difficult to realise at home the special difficulties of temporary observatories, but we venture to suggest that more skilled assistance should be provided at future eclipses. Although at Prickly Point we were enabled to carry out our programme with hardly any mishaps, we feel obliged to point out the difficulties under which we worked and which might easily have led to serious accidents. For example, five minutes before totality, Captain DARWIN's clock stopped altogether, although it is believed every possible pains had been taken in its regulation. It was only by a lucky alteration in the balance at the last moment that it was induced to go on at all.

Captain ARCHER had kindly sent us two sailors to render help during totality, viz., SAMUEL BROWETT, signalman, and HENRY STEELE, petty officer. One of these two was placed between the two huts to call out the time at intervals of half a minute while the other was to assist Captain DARWIN in the manipulation of his instrument. They both did what was required of them admirably.

During totality Captain DARWIN required no other assistance, as his programme was not heavy; but with Dr. SCHUSTER, whose whole observations were included in that time, the case was different. He gratefully accepted therefore the help offered to him by Dr. P. F. McLEOD, the officer of health in Grenada, and by Mr. MURRAY, who accompanied the expedition as naturalist.

Captain MALING had undertaken to make a drawing of the outer parts of the corona. According to a suggestion of Mr. LOCKYER'S, a disc had been prepared $6\frac{3}{4}$ inches in diameter. When placed at the proper distance the Moon and the inner parts of the corona could be screened off by means of this disc. It was placed on a wooden support at the top of an incline which ran down from the observatory to the sea shore. A post was driven into the ground some distance away from the disc. The observer was to look through a small hole in the post. This arrangement, however, wants very careful adjustment, and we had not much time to spare, as the principal objects of the expedition monopolised nearly all our attention. Whether from want of adjustment or from other causes, we cannot now decide, Captain MALING'S drawing includes the whole of the corona down to the prominences. Captain MALING'S statement is included in this report.

The following Table gives the position of the observatory and various data connected with the eclipse:—

Latitude	12°	0′ N.
Longitude	61°	45′ W.

Commencement of totality—

	h.	m.	s.
G.M.T.	23	17	12
L.M.T.	19	10	12
Duration of totality	—	3	50

Altitude of Sun	18°	45′
Parallactic angle	80°	25′
Angle between circle of declination and Sun's axis towards East	20°	37′

III. Totality at Prickly Point. By Captain Darwin and Arthur Schuster.

It has been mentioned that during the week preceding the eclipse, which took place on the Sunday, the weather had been very unfavourable; but the clouds cleared away on Saturday afternoon, the sunset was fine, and experience had taught us that a fine evening was generally followed by a fine morning. Our prospects, therefore, were very good on Saturday night, but early on Sunday morning the wind rose, which was a bad sign. At five o'clock, however, when we got up, the sky was still perfectly clear; the clouds came from the East at half-past five, at first in the form of detached cumuli, but before long the whole sky towards the East was overcast. The Sun rose behind the clouds and was still hidden when the time for first contact arrived. It was only twenty minutes before totality that the crescent of the Sun appeared, and then the wind soon made a clearance all round him.

Ten minutes before totality the sky was clear. The last instructions were given, the instruments put in position, and everybody took his appointed place. Five minutes before totality, as the darkness increased visibly, the weather seemed safe. Another minute passed and danger once more threatened; a small cloud rising from the South-East was driven by the wind right towards the Sun. The Moon from above descended over the Solar disc, and it seemed a race between the Moon and the cloud which should cover it first. Totality began, and the corona became distinctly visible, but was obscured again almost instantaneously by the cloud. For about three-quarters of a minute the corona was only seen through a film of cloud as a narrow hazy ring. The corona finally appeared, and remained clear as long as totality lasted.

Lieutenant R. J. Kidd, then Private Secretary to the Governor of Grenada, took temperature observations every five minutes for one quarter of an hour before and after totality. His numbers are as follows:—

h.	m.	° Fahr.
6	55	82
7	0	82
7	5	82
7	10	81·75
7	15	81·75
7	20	81·5
7	25	82
7	30	82·5

As the commencement of totality took place at $7^h\ 10^m$ it will be seen that during totality the thermometer only fell one quarter of a degree Fahrenheit, and reached its lowest point about ten minuntes after totality. The whole change in temperature was exceptionally small.

IV. On the Accuracy required in Adjusting an Equatorial for Photographic Purposes during Total Solar Eclipses. By Arthur Schuster.

Observers preparing for a total Solar eclipse have in general only a very moderate time at their disposal, and it is of great importance to them to settle beforehand to what degree of accuracy the adjustment of their instruments is to be carried. The time which is spent over adjustment in any one direction must necessarily be taken away from other more important matters, as there is never any lack of work on these occasions.

If, for instance, a photographic picture of the Solar corona is to be obtained, it would be clearly waste of time to refine on the adjustments of the equatorial or the rate of the clock beyond the point at which the Sun's change in declination would produce a visible effect. We shall see that this consideration limits the time of exposure for which the full advantage of the aperture of the lens can be realised, and this again will give us a limit beyond which it would be unnecessary to adjust the equatorial. We might, indeed, take the Sun's apparent motion into account, and point the instrumental axis, not to the pole, but to some point near it, which might easily be determined by calculation. We shall see, however, that the available time of exposure of a 4-inch lens is quite sufficient for our present requirements, and that it is therefore unnecessary to make allowance for the Sun's change of declination.

When the instrument is nearly adjusted, the relation between the true declination of a star δ and the apparent declination δ' is given by—

$$\delta = \delta' - \gamma \cos(\tau - \vartheta),$$

where γ is the angle between the true pole and the instrumental pole, τ is the hour angle of the star, and ϑ the hour angle of the instrumental pole. If ϑ is constant the change of apparent declination from bad adjustment in a short time t is found from the above equation to be $-\gamma t \sin(\tau - \vartheta)$, which will, numerically, always be smaller than γt. If the time t, measured in minutes of time, is p, we can write for this maximum change—

$$44 \times 10^{-4} p\gamma.$$

We find in this way that a change of apparent declination of one second of arc per minute could be produced if

$$\gamma = 3' \; 49''.$$

Now, the change of the Sun's declination may be, and during the last eclipse was, nearly one second of arc per minute.* It will be unnecessary, therefore, to spend any

* During the total phase of the late eclipse, owing to the low altitude of the Sun (18° 45′), the apparent change of altitude due to change of refraction was about $2\frac{1}{2}$ seconds of arc; but the change in declination due to refraction was small, and generally the effects of refraction may be neglected.

time in making the angle γ smaller than that given by the above value, as a closer adjustment may in some cases make matters worse instead of better.

Special considerations may of course induce observers to adjust their instruments more accurately. If, for instance, the equatorial adjustment is to be done once for all about a fortnight or a week before the eclipse, and if the conditions of the foundation on which the pillar rests give grounds for fear that the position may slightly change, it would be wise to aim at a greater accuracy in the first instance, so as to give some play for change.

So far the adjustments do not depend on the size of the object-glass, but, if we want to make the best use we can of the aperture, an apparent shifting of the Sun's image during the time of exposure must be confined within small limits, which we have now to fix.

The central disc of a star, or of a small object at a great distance, has an angular radius Θ in the focal plane of the telescope, given by

$$\Theta = 1{\cdot}22\,\lambda/R,$$

when R is the diameter of the objective. Experience shows that small objects can, under favourable circumstances and with strong illumination, be resolved when the centre of one diffraction disc coincides with the first dark ring; but for objects having no well-defined boundaries we must give wider limits, and I therefore take the point of fair resolution to be reached when the first two dark rings touch, so that the star discs stand perfectly clear of each other. In order to find out experimentally to what extent a small shifting of the images, such as might be produced by defective adjustment of the equatorial, might influence the possibility of resolution, I drew on a piece of paper two circles touching each other, corresponding to the two star discs, and another similar set above them slightly displaced in the line joining the centres. It was found that a displacement of one-tenth of the distance between the centres of the disc could only affect the resolution to a slight degree. We may, therefore, consider a displacement through one-fifth of the radius of each disc allowable without impairing definition. There is always something arbitrary in the fixing of these limits, but it must be done in every investigation in which we want to determine the accuracy to be aimed at. As object-glasses of different makers would probably differ 10 per cent. in their observed resolving powers, the limit chosen seems a fair and reasonable one.

We may allow then an angular shifting of

$$\phi = \Theta/5 = 1{\cdot}22\ \lambda/5R = 1{\cdot}22 \times 10^{-5}/R$$

if we substitute for λ its mean value of about 5×10^{-5} cm. This gives a value for ϕ which varies inversely with the aperture, and is equal to 2·5 seconds of arc for one centimeter aperture. If the exposure takes place during p minutes, and if the change of the Sun's declination in one minute is τ seconds of arc, the displacement will be $p\tau$, and we get for the longest time of exposure compatible with full definition—

$$Rp\tau = 2\cdot5.$$

In the West Indian Eclipse R was 10 and τ was ·9, which gives about 17 seconds as time of exposure. This is sufficient for obtaining a good image; and we conclude, therefore, that an adjustment of the polar axis to about $3'\cdot5$ is sufficient for full definition. Photographs taken with longer exposures would chiefly serve to fix the fainter parts of the outer corona.

By the ordinary means of adjusting an equatorial it is not difficult to obtain the necessary accuracy; nevertheless, the calculation shows that we can by no means neglect to go through the full routine of adjustment, and no equatorial should be sent out for eclipse purposes without complete appliances in the way of good finders, circles which are easily read, &c.

The adjustment of the rate of the clock to the necessary accuracy is, as will appear, much more difficult.

We have seen that the angular displacement may reach, but should not exceed, $1\cdot22 \times 10^{-5}/R$ during the time of exposure. Reduced to seconds of time, this is equal to $\cdot17/R$, or, if the exposure is to be q seconds of time, the error of the clock should not exceed $\cdot17/qR$, which for an exposure of 17 seconds and an aperture of 10 cm. is one part in a thousand.

The instrument we had at our disposal did not allow an adjustment as accurate as this. At Prickly Point it was found difficult to go beyond an accuracy of one per cent. No record of the rate was taken at Carriacou, but it was probably no better than at Prickly Point. This would show that full definition would not be possible with an exposure of more than two seconds.*

We may finally calculate by how much the efficient aperture is reduced if the errors due to adjustment are greater than those we have hitherto allowed. If the centres of two star discs are separated by a distance $2\Theta + K$, a displacement equal to K would just bring them into contact. A telescope which to the eye would resolve two stationary stars at a distance 2Θ will, if during the time of photographic exposure the displacement is K, have its effective aperture reduced in the ratio of $(2\Theta + K) : 2\Theta$. Hence, if R′ be the effective aperture

$$\frac{R - R'}{R'} = \frac{K}{2\Theta}.$$

* Some of the statements made here do not agree with those made in the Preliminary Account ('Roy. Soc. Proc.' vol. 42, p. 180). The discrepancy is accounted for by the fact that I previously took wider limits for the allowable shifting due to the motion of the telescope, and that I have since then subjected the photographs to a closer investigation as regards fineness of detail actually shown.

Hitherto we have put $K = \frac{1}{5}\Theta$; but supposing the rate of the clock is such that during the exposure $K = 2\Theta$, which was very nearly the case during the late eclipse, the above equation shows that the effective aperture was reduced to half, or that the photographs could not show anything which an eye-observer with a telescope of 2-inch aperture might not have seen.

Let us now turn our attention to the resolving powers which have hitherto been actually obtained in photographs taken of the corona.

During the Eclipse of 1871 two prominences were separated in a photograph, which were at a distance of 15 seconds, and the corona itself gives no evidence of a finer structure. The aperture used was 4 inches.*

In the corona photograph of 1882 the diameter of one prominence subtends an apparent angle of about 15 seconds. As far as the photograph is concerned, we have no reason to suppose that an aperture resolving two stars at a distance of 15 seconds could not have shown everything that is seen on the photograph, for no detail of the corona gives evidence of smaller structure. This gives an aperture of 2 inches as sufficient to resolve all the detail shown in this photograph.

An examination of the drawing made by Mr. WESLEY of the present eclipse gives substantially the same result.

The 4-inch glasses used in the eclipses which I have mentioned all give, therefore, a resolution equal to that obtainable with a 2-inch aperture on a stationary object.

During the last eclipse the regularity of the clock motion was, as we have seen, not sufficient to give more perfect images; the same cause has very likely stood in the way of more perfect images during previous eclipses. We have at present no reason to suppose that the corona possesses any structural detail that could not be seen with an aperture of two inches, and it would be very desirable if eye observations could be taken at some future eclipse which would give us some idea of the detail of the structure visible with larger instruments.

But it is very remarkable that the definition obtained is just what we should have to expect, if we take the error of clock motion into account. If the motion of the clock is regular, we should by shorter exposures get in great part over the difficulty of adjustment, but in the instruments which have been at our disposal the mechanism of transmission from the clock to the telescope is not as perfect as it might be made, and the irregular vibrating motion produced by this cause would be sufficient to damage the sharpness of the image.

There does not seem to be any advantage at present in using larger apertures in future eclipses, at least if these larger apertures are accompanied by increased focal length. Most of the adjustments have to be more accurate for them, in order to get the advantage of the increased aperture, and the difficulties of mounting and adjusting are

* Owing to an oversight, the aperture was in the Preliminary Account stated to be 2 inches. The focal length of the lens was only half that used during the later eclipses, but the aperture was the same.

greater. The photographs would be larger and more convenient to copy, which would be an advantage, but it seems more rational to improve the definition obtained with a 4-inch lens, before larger apertures are used.

If we could increase the aperture without increasing the focal length, we could reduce the time of exposure and gain an advantage, provided that the clock motion is sufficiently uniform. All our investigations, then, seem to point to the desirability of an improvement not only in the average rate of the clock motion, for that could easily be effected, but in the steadiness and regularity of the motion.

I give, in conclusion, collected together, some formulæ which may be useful to future eclipse observers.

In the equations ϕ is the displacement in seconds of arc which is allowable, consistent with full definition as above defined.

R is the true aperture of the lens in centimeters.

R′ is the effective aperture of the lens.

q is the time of exposure in seconds.

Q is the longest time of exposure compatible with full definition.

γ is the greatest allowable angle between the true pole and the pole of the instrument in seconds of arc.

τ is the change of Solar declination measured in seconds of arc per second of time.

e is the error of the clock rate, measured in per cent.

In equations (2), (4), and (6) it is assumed that in adjusting the pole of the equatorial no account is taken of the change of the Solar declination during the eclipse.

$$\phi = 2 \cdot 5/\mathrm{R} \quad \ldots \ldots \quad (1),$$

$$\mathrm{Q} = 2 \cdot 5/\mathrm{R}\tau \quad \ldots \ldots \quad (2),$$

$$\gamma = 34600/\mathrm{R}q \quad \ldots \ldots \quad (3);$$

or, if the longest exposure Q is to be used,

$$\gamma = 13840\tau \quad \ldots \ldots \quad (4).$$

Finally, to determine R′, we easily find

$$\frac{1}{\mathrm{R}'} - \frac{1}{\mathrm{R}} = 60 \times 10^{-4} \times eq. \quad \ldots \ldots \quad (5).$$

This equation will allow us to determine the time of exposure allowable for a given effective aperture R′ if the clock rate is known. If the longest exposure Q is to be used, we get

$$\frac{\mathrm{R} - \mathrm{R}'}{\mathrm{R}'} = \frac{1 \cdot 5 \times 10^{-3} \times e}{\tau} \quad \ldots \ldots \quad (6).$$

V. The Photographic Camera. By Arthur Schuster.

1. *Adjustments.*

The lens used for obtaining photographs of the corona had a clear aperture of 4 inches (10·1 cm.), and a focal length of about 5 ft. 3 in. (160 cm.). It therefore gave images of the Sun of about ·3 in. in diameter.

The accurate adjustment of the camera is a matter of some difficulty, as distant objects are not sufficiently sharp to admit of delicate focusing. It was found better to focus on a sharp object at a moderate distance, and to find by calculation the correction for parallel rays. The correction to infinity δf for a distance u of the object focussed is given by

$$\delta f = f^2/u.$$

If, for instance, u were 1 kilometer, and f, as mentioned above, 160 cm., the correction would be 2·6 mm., and, as it proved impossible to adjust the focus more nearly than to about half a millimeter, an error of 20 per cent. in the estimate of the distance is allowable.

It is a matter of importance to be able to determine accurately the position of the corona in reference to the coordinates fixed in space. In Egypt and at Caroline Island this was done by means of a platinum wire stretched across the camera directly in front of the plate; the shadow of this wire appears on all photographs, and its position was determined by a succession of instantaneous photographs of the Sun's crescent taken directly before and after totality with a stationary telescope and at short intervals of time. This plan has some disadvantages. The shadow of the wire hides certain parts of the corona, and, for instance, in the photograph taken at Caroline Island the shadow is very awkwardly placed. Then, again, it is difficult to stretch the wire sufficiently tight. The camera has to be turned directly on the Sun while the crescent is being photographed, and the heat thus concentrated on the wire lengthens it and may change its position.

A different plan was therefore adopted. Two needles were placed one on each side of the camera, the line joining the points passing approximately through the centre of the plate; one of the needles was intentionally inserted somewhat obliquely, so that its shadow might always be recognised on the photographs. The image of the needles would thus completely determine the position of the plate in the camera. The direction of the line joining the needle-points can be very accurately determined with reference to the declination circle. We need only bring the image of some object, either terrestrial or celestial, just in coincidence with one needle-point, then with the other, and note the change of reading of the hour and declination circles. For safety, and as a check, photographs of the Sun's crescent might be taken after totality. The needles, in the present instance, did not reach sufficiently far into the centre of the camera. In previous eclipses the general illumination of the sky in the neighbourhood of the corona was sufficient to affect the whole plate; but it did not on this occasion, and the images of the needle-points only appear on one plate, owing to a cause to be mentioned presently. This one plate is, however, quite sufficient to fix satisfactorily the orientation of the corona. I believe that the method could be rendered perfectly safe in future eclipses, and it is certainly more accurate than that hitherto used.

2. *Arrangements during Totality.*

Dr. P. F. MACLEOD, the resident officer of health in Grenada, and Mr. MURRAY, who accompanied the expedition as a naturalist, had offered their assistance during totality. This was gratefully accepted, and my best thanks are due to them. Mr. MURRAY undertook to screen the camera while the slides were being drawn or pushed in, so as to prevent a confusion of image due to shaking when the slides are touched. Dr. MACLEOD took charge of the slides before and after exposure, handing them as they were wanted, and placing them in a bag after they had been taken out of the camera. I had originally intended to take eight photographs, but had to change all arrangements suddenly when I saw the corona obscured by a cloud at the beginning of totality. As the brighter parts of the corona were faintly visible, and as I did not know whether the cloud would clear away in time, two photographs were taken, referred to in Mr. WESLEY'S report as Plates 1 and 2. The exposure of these must have been about 15 seconds each.

It is only in a very indirect way that I could, after the eclipse, obtain an idea of the various exposures given, and not much value is to be placed, therefore, on the numbers. I knew the time I intended to expose, but I generally closed the camera sooner, as I was always afraid of a fresh cloud coming on.

The corona came out clearly while the third exposure took place. Instead of a plate, a piece of sensitized paper had then been put into the slide, but the result was not good.

Three further plates were taken with the corona clear, the last about 15 seconds before the end of totality. After the spectroscopic cameras had also been shut, I intended to take out all slides. Unfortunately I loosened the wrong hook, and drew the shutter instead of the slide, but, noticing my mistake at once, pushed it back. That moment the Sun came out, and the last plate was partly spoilt, but parts of the corona show well, and it has been used in making out the details of the corona. The accident was fortunate, in so far as the image of the needles necessary for orientation came out very clearly owing to the illumination by the reappearing Sun.

3. *Results.*

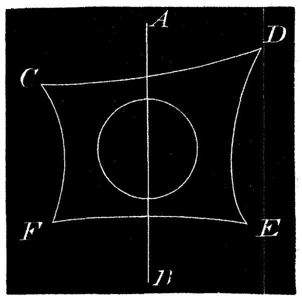
Mr. WESLEY, who has made a careful drawing of most of the recent eclipses, has described the corona as it is shown on the photographs on the present occasion, and I need not add anything to his remarks. But I should like to draw attention to one or two points which have a bearing on the speculations respecting the origin of the corona.

It is generally accepted now, I believe, that the Sun cannot have an atmosphere of anything like the extent of this luminous appendage. The reasoning generally given, and which seems conclusive, is that the pressure on the Sun's surface must, to judge from spectroscopic evidence, not exceed that on the surface of the Earth, and that an atmosphere comparable in size with the volume of the Sun would necessarily produce an enormous pressure. I do not know, however, whether attention has been drawn to the fact that, even if such a gaseous atmosphere existed, it could not be luminous to any great height unless it contained a great number of solid or liquid particles. The same laws of convective thermal equilibrium which regulate the decrease of temperature in the atmosphere act still more strongly on the surface of the Sun, and even taking the highest estimate which can reasonably be made of the Sun's temperature, the gases rising in the Solar atmosphere would quickly fall in temperature below the point at which they can be luminous. The only way in which they can be kept luminous is by containing sufficient matter to absorb the radiation from the body of the Sun, or by some independent cause, such as friction between solid and gaseous matter, or electric discharges.

If electric discharges are the cause of the luminosity, the matter through which the discharge takes place must either be supplied by the Sun or by something outside, such as streams of meteoric matter. The latter hypothesis has the great advantage that it may possibly account for the periodicity of Sun-spot phenomena, and it deserves, therefore, the attention of scientific men. Now, it occurred to me several years ago that if meteoric streams fixed in space were in any way connected with the corona we should find some evidence of them in the general shape as seen from the Earth. During eclipses which take place about the same periods of the year the Earth and the orbits of these hypothetical meteor streams would occupy the same relative positions. We might then expect some periodicity in the shape of the corona depending on the time of the year. To my surprise, I found that, as far as the evidence went, it seemed indeed to point in that direction. In the report of the Eclipse of 1875 a paragraph was inserted, which, without laying any stress on the point, called attention to the similarity between that eclipse and the one which had taken place the previous year during the same month. The eclipses which have taken place since have added to the evidence, and it seems worth while, therefore, to draw attention to it, without however in the least wishing to imply that I consider the fact as proved or even as probable.

The Solar corona often shows a rough symmetry about its axis of rotation, but deviates from complete symmetry owing to one of the halves being broader than the other. Hence, it often appears in the form of a trapezium. Fig. 1 may serve to illustrate this. AB is the axis; C, D, E, F the points of the longest rays of the corona, the distance DE being longer than the distance CF. Now, during the eclipses which have taken place early in April the eastern half of the corona is the one which is broadest, while in the eclipses observed in July and August the opposite held good and the western half was the broader. The eclipses of December and May have hitherto shown no difference between the two halves. It will no doubt be considered that the number of well ascertained coincidences is too slight to prove anything, and with this opinion I quite agree, especially as the changes in the corona which seem to depend on the Sun-spot cycle have to be taken into account. Nevertheless, it seems worth while to give the evidence here.

Fig. 1.



To begin with the eclipses of which we possess photographs, the only ones, perhaps, we can safely take into account, the following Table will illustrate my meaning :—

Date.	Year.	Corona.	Sun-spot numbers.
April 6	1875	Eastern half broader	20·5
May 17	1882	Absence of symmetry	60·0
May 6	1883	" "	63·7
July 29	1878	Western half broader	3·3
August 29	1886	" "	25·7
December 22 . . .	1870	No symmetry	135·4
December 11 . . .	1871	Symmetry, but both sides equally wide	98·0

The Sun-spot numbers represent frequency of spots as determined by Mr. R. Wolf. As far as 1878 these numbers are given in Mr. Ranyard's Eclipse volume ('Roy. Astron. Soc. Memoirs,' vol. 41) for the actual date of the eclipse. After that date the numbers are those given from time to time for the average of the whole year by Mr. R. Wolf in the 'Comptes Rendus.'

As regards the drawings, we have the following :—

Date.	Year.		Sun-spot numbers.
April 16 . .	1874	The drawings of Mr. BRIGHT and Mr. DEGERMANN show a distinctly greater width on the east side. In Miss ALICE HALL's drawing the eastern half is less broken up than the western, but not broader. In Mr. WRIGHT's drawing the points of the streamers on the eastern side are further apart than on the west	119·1
August 7 . .	1869	Observers speak of the trapezian form of the corona, which is important, as the Sun-spots, though not at their maximum, appear in greater number at that time than in either 1882 or 1883. The drawing of Mr. MEEK, given by Mr. SCHOTT, seems the only one that is oriented relative to the Sun, and here the western side of the Sun is the broad one	77·6
August 18 . .	1868	The drawings are too uncertain to come to any conclusion, but if lines are drawn giving the ends of the extreme rays, the rays on the west are in nearly all cases further apart	42·9

We have, therefore, not one case in which the eastern side was the broadest in the autumn or the western in the spring.

If we take the time at which the greater eastern width changes to the greater western width to be about the middle of June, and middle of December, we should expect that in the eclipse which has just taken place the two sides would be about equally developed, but if there is a slight asymmetry the eastern side should be the broadest.*

VI.—THE CORONAGRAPH. BY CAPTAIN DARWIN.

1. *Description of the Instrument and its Adjustments.*

The coronagraph was designed by Dr. HUGGINS as the instrument which would give the best chance of rendering it possible to obtain photographs of the corona in Sun light. For this purpose a reflector is to be preferred to a refractor, and special precautions are taken to avoid internally reflected light. The telescope is of the Newtonian form.

The instrument which I took out is by GRUBB, of Dublin, and was mounted on an ordinary equatorial stand. The light enters a tube 4 feet long, fitted with numerous diaphragms. It then passes into a tube 5 feet 6 inches long (a little less than the focal length of the mirror), of about double the diameter of the first tube, the mirror

* [*Note added July* 14, 1889.—This prediction has not been verified. In the Eclipse of January 1, 1889, although there is not much difference between the two sides, the western half is the broader. Nevertheless, the considerations in the text are of some use, as they show the importance of taking account of the relative position of Sun and Earth in discussing the shape of the corona in different eclipses.]

being at the further end. The beam enters this tube at one side of the centre, and is reflected back at a small angle from the mirror at the further end to the photographic plate, which is alongside of the place where the light enters. A partition in this tube separates the incoming and reflected rays for as long a distance as possible. Below this partition there are oval diaphragms, through which both beams pass. I may here remark that this last precaution is, I rather think, a mistake; for the backs of most of these diaphragms are necessarily visible from the photographic plate, and, to whatever extent they are useful in preventing stray light from getting to the bottom of the tube, they are proportionately harmful in reflecting this light directly back on to the photographic plate, which is very near to them. I was so much afraid of the direct Sun light striking the back of these oval diaphragms that I placed a temporary diaphragm of only 2 inches in diameter at the bottom of the outer tube, thus ensuring that the beam of Sun light should pass clear through the diaphragms in the inner tube. I should gladly have dispensed with this if I had dared.

The two tubes are held in position in a long wooden frame. There is a half-open space between the tubes, which has to be covered up by a cloth or other temporary contrivance. This opening should be provided with a permanent cover.

I arranged an instantaneous shutter close in front of the plate, that is to say, in the position which Dr. HUGGINS considered most advantageous. It consisted of a long wooden slide, with a rectangular opening 6 inches wide, which was drawn across an opening of $2\frac{3}{8}$ inches diameter by two pieces of elastic. It was released by cutting a thread. Both the elastics and the thread were fastened to the body of the telescope, and not to the camera or slide which takes the plate. This latter part is not too firmly fixed to the body, and I was anxious to avoid vibrations.

As Dr. SCHUSTER has fully described the adjustment of his instrument for equatorial movement, focus, &c., I will only remark that the same methods were used in the case of the coronagraph in so far as they were applicable.

2. *Observations in Sun Light and during the Partial Eclipse.*

The most important observations to be made with the coronagraph were with the view of testing the practicability of obtaining photographs of the corona during Sun light by this instrument. This could be done in two ways :—

(1.) By obtaining photographs shortly before or after the eclipse, and comparing any irregularity that might appear in the halo round the Sun with photographs of the corona taken during totality—a similarity of form indicating that the corona had been photographed.

(2.) By taking photographs during the partial eclipse. Then, if the light of the corona produced any effect on the plate, the dark limb of the Moon would be seen against it.

It was advisable in trying the first of the above experiments to take a series of photographs in each of the reversed positions of the instrument. Then any irregularity in the illumination round the Sun which was reversed on turning the instrument over would clearly be proved to be due to instrumental causes; whereas any irregularities which were visible in photographs taken in both positions, and which were not thus reversed, must be due to some outside causes, either atmospheric or coronal. In my instrument reversal was only possible near noon; and on this account, as well as on account of the Sun being at its greatest elevation, the best results would be obtained by taking one series of photographs shortly before noon in one position, and another series in the other position soon afterwards.

With regard to the length of exposure, I thought that there was no advantage to be gained for either test by giving a longer exposure than was necessary for the air glare to produce a result on the plate. I estimate the exposure that I gave with the automatic shutter at between one-fifth and one-tenth of a second.

The plates which I used for these tests consisted of gelatino-chloride dry plates on paper and on glass. These were prepared for me by two makers: by Mr. A. COWAN (for Messrs. MARION and Co.), to whom I am indebted for making for me a specially prepared batch of plates, and by Mr. WARNERKE.

Chloride plates were chosen in preference to bromide plates, because it was considered that their relatively greater sensitiveness to ultra-violet rays would be advantageous for distinguishing between the corona and the air glare. The plate to be chosen is no doubt the one which is most sensitive in that part of the spectrum where there is the maximum difference of intensity of light in the spectra of the corona and of the sky. I may here remark that Dr. SCHUSTER informs me that, according to his photograph, the maximum difference would appear to be in the less refrangible part of the spectrum, rather than in the violet; and, if that is the case, bromo-iodide plates would, I think, have been preferable.* Paper was considered better than glass, as tending to reduce the halation to a minimum; as a fact, I relied chiefly on the paper negatives.

I used various developers for these chloride plates, without observing any marked difference in the results:—

(1.) Hydrokinone, 1 grain to the ounce. To 2 oz. of the above add $\frac{1}{2}$ dr. of a saturated solution of potassic carbonate, with common salt as a restrainer.

(2.) Potassic citrate, 136 grs.; potassic oxalate, 44 grs.; water, 1 oz. To 3 parts of the above add 1 part of ferrous sulphate, 140 grs.; sulphuric acid, 1 drop; water, 1 oz. Potassic bromide was used as a restrainer.

(3.) A weak ferrous sulphate developer.

Some of the plates were developed in the West Indies, and some on my return home. The results appeared to be the same, but they were, I think, much easier to develop in England. Ice was used freely in Grenada, but I do not think the gelatine became quite hardened during the time of development, and the plates appeared to be more liable to fog.

* See p. 341.

3. *Results of First Test, or Photographs taken before the Eclipse.*

For the first test, that is, the one by means of photographs taken before or after the eclipse, I had intended to have taken several series, but fortune did not favour me. We reached Prickly Point on a Tuesday. By the following Sunday, although a great deal remained to be done in the way of final preparations and adjustments, I was able to take my first photograph. This left me six clear days before the eclipse in which to make the final arrangements, to obtain that experience so necessary when working in a new climate, and, in my case, with an instrument with which I was not very familiar, and also to obtain, if possible, one or more series of photographs for the test in question. I may here remark that, before starting, Dr. HUGGINS had given me every possible assistance and advice, without which I could hardly have undertaken the work; and that I had also spent several mornings in London in practising with the coronagraph, but from want of time I had to entrust a great deal of the troublesome preparation at home to Mr. LAWRANCE, to whose careful attention at this period I owe a great deal. As already remarked, the weather was very unfavourable, and on several days photographic work was an impossibility; on others I could only get casual photographs in the intervals between the frequent tropical showers, or in gaps in the cloudy sky. For two days after the eclipse the weather was also unfavourable. In fact it was only on the day before the eclipse that I succeeded in getting a series of photographs about noon in both positions; but, the rapidity of change in form of the corona being an unknown quantity, the shorter the interval the more valuable would the photographs be. The sky on that day was variable, but generally clear—as clear as it ever was during our stay in the West Indies, but not, I think, as clear as it often is in England.

Soon after coming home, Mr. WESLEY, who has had great experience with regard to photographs of a similar nature, very kindly undertook to examine this series. Several of the negatives exhibited corona-like markings, but Mr. WESLEY could not find any detail on any one of the photographs which was confirmed by the others. This was done before he had had an opportunity of seeing the photographs of the corona taken during totality. Had he succeeded in making a drawing of the supposed corona, its comparison with the true corona would have therefore afforded a valuable test as to its genuineness; under the circumstances this was, of course, impossible. Mr. WESLEY also informs me that he thinks that "the photographs taken on the same side are as little comparable with each other as with those taken on the opposite sides," which, as far as it goes, indicates that the irregularities are due to atmospheric rather than to instrumental causes. My own opinion on the above points coincides with that of Mr. WESLEY; but I was especially glad to obtain his assistance, because of his large experience, and also because he had not at that time seen any photograph of the corona.

4. *Results of Second Test, or Photographs taken during the Partial Eclipse.*

The second of the two tests could be applied by taking photographs either during the eclipse, before or after totality, or sufficiently near first or last contact for the Moon to be still eclipsing the corona. I succeeded in taking over twenty photographs during these periods. Many of these, when developed, showed what I may describe as a false corona, that is, an increase of density near the Sun, including the part between the cusps, and therefore in front of the Moon. In none of these can the limb of the Moon be seen against the corona as a background. Besides subsequent examination, I watched these photographs very carefully during development, without result. I mention this circumstance because they appear to have gone back considerably, so that, in several instances in which the air glare was clearly visible before fixing, it is now barely discernible.

I have also searched for any trace of a remarkably large prominence in the place where I knew it should be found, but without result. This prominence was hardly, if at all, covered by the Moon after totality, that is, during the period in which nearly all my photographs were obtained. Prominences are certainly more actinic than the corona, and we should therefore expect them to appear on the plate if the corona is obtainable by this method. However, if the air glare increases much more rapidly in intensity than the corona does as the Sun is approached, this argument is not sound, as the prominences might then be more overpowered by sky light than the outer parts of the corona would be.

5. *Photographs taken during Totality.*

It will be observed that the two experiments or tests just described were made by taking photographs before and after the eclipse, and during the partial phases. But during totality the instrument was not idle. The following was the programme which I had laid down for myself during the 3 minutes 50 seconds available :—

During the first minute a photograph was to be taken with the prismatic camera. After that, six plates were to be exposed with the coronagraph—four chloride plates with the same length of exposure as that given during Sun light, and two bromide plates with exposures of 5 and 10 seconds respectively.

This programme could not be followed exactly. Immediately after I had commenced exposing the prismatic camera, I looked up and found that a light cloud was drifting across the corona. The sky became clear again in about 50 seconds. I was anxious not to take any photographs with the coronagraph during the exposure of the prismatic camera, for fear of vibration; but, as nearly a minute had been lost, something had to be sacrificed, and I decided to take some of the photographs with the coronagraph before putting the cap on the prismatic camera. I do not think that the work has suffered in consequence, and at all events I obtained all the plates I had allowed for in my programme.

The instantaneous photographs of the corona were complete blanks, proving, I think, that the exposure had been far too short. I developed them with the same solutions, and for at least the same length of time, as when developing, immediately beforehand, some of the plates exposed during the partial eclipse; the instrument was in the same condition as before and after totality, when successful photographs were taken. These circumstances are worth mentioning, to show that I did not fall into some of the commoner traps with which the photographer is surrounded. Three of the photographs showed signs of fog, which was probably only due to the length of time which I allowed them to stay in the developer.

The long exposure photographs were not taken with any special object beyond that of obtaining a record of the corona. The plates used were bromo-gelatine dry plates prepared by Captain ABNEY, and I used the ordinary alkaline development. The extension of the corona shown on these plates is not very great, and they show signs of vibration; they have, however, I hope, been of use to Mr. WESLEY in his drawing of the inner parts of the corona. As my main object was to get instantaneous photographs, these plates had to be taken without removing the automatic shutter; the shutter had, therefore, to be worked by hand, and this probably caused the vibration. It may, however, have been caused by a puff of wind; and on another occasion I should take far greater precautions against this danger by surrounding myself with canvas screens in all exposed directions, and as high as possible.

6. *Comments and Conclusions.*

Returning again to the consideration of my observations with regard to the special uses of the coronagraph, it will be seen that my results are adverse to the possibility of obtaining photographs of the corona during Sun light with this instrument. It is, however, I consider, by no means proved that the method is impossible, for there are several reasons why this trial should not be considered conclusive.

(1.) The atmospheric conditions were very unfavourable. The air was fully charged with moisture, and on the morning of the eclipse the sky was certainly not of that dark blue which, no doubt, indicated atmospheric purity. It was slightly hazy, and not, I think, as clear as an average English blue sky. About a minute after totality I noticed a halo with prismatic colours round the Sun—an indication, I presume, of suspended matter.

(2.) The Sun was at a low elevation during the eclipse, and the station was only about a couple of hundred feet above the sea. Both these circumstances, no doubt, increased the air glare.

(3.) Professor THORPE'S observations at this eclipse show that the light from the corona was not so bright as on other occasions. This also appears to be the general impression amongst other observers who had seen previous eclipses. If this was due to the corona not being so luminous, the opportunity was, no doubt, an unfavourable one independently of the state of the atmosphere. But this effect is not to be distinguished from the light being diminished by an impure or dense atmosphere. Experiments comparing the light of the corona with Sun light immediately before and after the eclipse would be necessary to settle this point.

(4.) The exposure was too short. I feel confident that at Grenada an alteration in this respect would not have materially altered the result; but, even with the atmosphere in its unfavourable condition, I think a larger exposure should have been adopted. Before leaving England I had come to the conclusion that the right length of exposure would be that sufficient, and not much more than sufficient, for the air glare to produce an effect on the plate. I thought that the corona would be more readily visible if the background of the sky was faintly developed on the negative; and in this opinion I think Dr. HUGGINS would have agreed, as his photographs seem to have been treated in that way. I found that my instantaneous shutter, when set at its slowest rate, gave the required result, and I was at the time quite contented. I have, however, very carefully reconsidered this subject, and now come to the conclusion that this was a mistake. The question to be settled is—At what photographic density do we get the greatest ratio of small changes of shade in the negative, as seen by the eye, compared with the changes in luminosity of the objects photographed? Captain ABNEY gives some results in the form of curves in his Text-book of Science on Photography, which, if modified to suit this problem, would appear to indicate that the ratio is greatest when the photographic density is about one-third or half way between white and the deepest shade due to the full development of the image of a bright object.*

If this is the right interpretation—and I believe it to be so—then it is evident, in order to have the best chance of photographing the small difference of shade between the sky and the corona, I should have given an exposure which would have allowed me to bring up the sky to what I may call a third or a half full density. In order to have done this I should have lengthened my exposure.

Acting in accordance with the advice of Dr. HUGGINS, I adopted a slow development with weak solutions. I could not have followed a higher authority, but I must confess that I have some doubts whether a longer exposure with a quicker but well-restrained development might not have produced better results. The best time for watching for faint outlines is during development, and with a well-exposed plate we have an opportunity of observing the image in every stage.

All these considerations appear to me to point to longer and more varied exposures than I gave, together with a well-varied development.

My conclusions are, therefore :—

(1.) That my results do not prove the impossibility of photographing the corona in Sun light.

(2.) But they prove that under certain circumstances the light of the corona is not

* See p. 256. The ordinates of his curves represent the shade as measured by the proportion of black to white; the scale of shade as measured by the eye is somewhat different from this. The abscissæ represent the length of exposure, which I imagine to be equivalent to differences of luminosity.

sufficient to produce any effect on chloride dry plates with an exposure which is sufficient for the air glare or false corona.

Captain ABNEY pointed out to me—and I think correctly—that the sky or background to the Sun, as shown on the photographs, is due to two causes: first, the light of the sky, and, secondly, the reflected light from the interior of the instrument. Dispersion of light from the mirror or its surroundings would shed a uniform light over the whole surface of the exposed plate; whereas the sky is brightest near the Sun, and possibly not always quite uniform or regular in its appearance. Now, on several of the photographs that I took there is a perfectly uniform light over the whole sky, with no apparent increase of brightness near the limb of the Sun. This constitutes the only hopeful sign I have seen; because, if the above views are correct, it indicates that I have not given a sufficiently long exposure to photograph the sky round the Sun, and gives hopes that the false sky that I did obtain might be diminished by instrumental alterations.

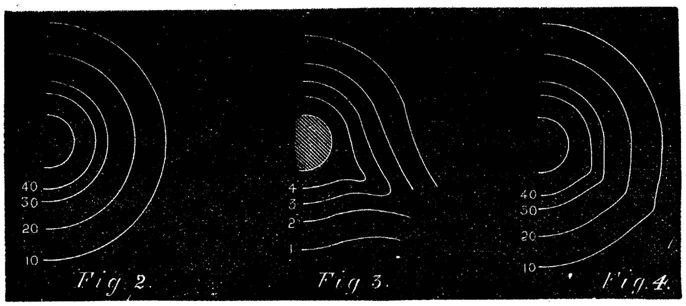
7. *Discussion on the possibility of obtaining Photographs of the Corona during Sun Light.*

But there are certain considerations which appear to me to indicate that a practical method of photographing the corona during Sun light is not likely to be obtained.

If I am right in considering that the increased density round the Sun in these photographs is a true picture of the sky, and not due to irradiation or internal reflection, then it will be seen that this sky illumination is not uniform round the Sun, and that the form of the false corona thus formed would always be difficult to distinguish from the form of the true corona, and would be liable to be mistaken for it. It is quite possible that changes in the appearance of the sky may be more visible when photographed than when seen direct, and that these changes may be as sudden as that from blue sky to cloud. At high altitudes this difficulty would no doubt be lessened, but it would, I think, always be a source of trouble. If, however, these irregularities in the false corona are not due to irregularities in the sky, I can only say that the instrumental defect which causes them is a very difficult one to cure.

In considering the utility of the photographs of the corona taken in Sun light, if obtainable, the effect of the image being superimposed on the much denser picture of the air glare should be carefully considered. In the first place, it is to be observed that the air glare or light of the sky increases rapidly as the Sun is approached, and that the light of the corona also increases in a similar manner. This will, I think, cause the corona to be distinguished with difficulty in the photographs taken in Sun light. This may not be so much the case when considering abrupt changes of shade in the corona, but, in so far as the form of the corona is distinguished by a gradual change of light, it certainly will be. In the following figures, which have no numerical significance whatever, and which are merely intended to illustrate my argument, I have assumed the corona to be only from 10 to 15 times less bright than the air glare. In fig. 2, let the lines round the Sun represent lines of equal intensity of sky light, the corona being supposed to be non-existent. In fig. 3, let the Sun be eclipsed, and the lines represent lines of equal intensity of corona light. Place one image on the other, and the lines of equal intensity of the combined light of corona and air glare will be as shown in fig. 4, in which it will be seen how materially the form of the corona appears to be altered. It is to be noted also that an alteration in the state of the atmosphere by altering the ratio of air glare to corona light would appear to alter the form of the corona as shown on the Sun light photographs.

For the purposes of the following discussion of the effect of combining the sky and corona lights on one picture, it is assumed that this particular difficulty does not exist; that is to say, that the air glare is uniform, or, what comes to nearly the same thing, that we are discussing a radial inequality of the corona. In dealing numerically with this problem, very little trust must be placed in figures used; but, however faulty they may be, they may, at all events, help to make the consideration of the subject more easy.



Captain ABNEY informs me his experiments with regard to sky light when compared with the results obtained by Professor THORPE at this eclipse show that at 30′ from the Sun's limb the corona, as seen at Grenada, was about 60 times less brilliant than the air glare at the same distance from the Sun's limb, as seen at South Kensington under the most favourable circumstances, that is, with the darkest sky measured. As the sky on this occasion at South Kensington was probably darker than the sky at Grenada, the ratio at this latter place was probably greater than 1 to 60; but, on the other hand, with a clearer sky the corona would have been brighter and the ratio therefore less. Hence the ratio of 1 to 60 is about half way between the ratio under favourable circumstances at South Kensington and the ratio at Grenada near the time of the eclipse. For the purposes of argument, let it be assumed that at Grenada the ratio of the light of the corona to the light of the sky at 30′ from the Sun's limb was as 1 to 50, a ratio, if the above figures are correct, no doubt considerably less than the true ratio.

Captain ABNEY also informs me that experiments he has made show that an abrupt change of $\frac{1}{2}$ per cent. in the density of a photograph is about the minimum change of shade that can be seen by the eye; or, in other words, that a photograph may be regarded as a drawing in which only 200 different shades can be used.

It has already been shown that to obtain the best results a photograph at a half or a third full density should be obtained. Let us assume the most favourable circumstances, and that the development has brought the part of the picture under consideration up to half full density. Then, if there are only 200 possible shades in any photograph, there are, therefore, only 100 possible shades below this shade, which for convenience may be represented by 100. Let the light which caused this shade be also represented by 100. Now, if the shade on the photograph varied proportionately with the light, each of the 100 parts of light would be represented by one shade on the photograph, and a change of light less than 1 per cent. would not produce a change of shade on the photograph which would be visible to the eye. But there is no doubt that under certain conditions the shade on these photographs varies proportionately more rapidly than the light which it represents, and, judging from Captain ABNEY'S curves, before mentioned, it is not quite a fanciful supposition to assume that under the most favourable circumstances it varies twice as rapidly in parts of the photograph; that is to say, that a change of $\frac{1}{2}$ per cent. in the luminosity of the object is the minimum change which would be visible on the photograph. Now, in a photograph taken in Sun light of the sky at 30′ from the Sun's limb, the 100 parts of light will consist of 98 of sky light and 2 of corona light. The sky light being assumed to be constant, the minimum visible change of $\frac{1}{2}$ per cent. in the total light must be due to a change of 25 per cent. in the total light of the corona; whereas, with a photograph taken during a total eclipse, on similar suppositions, a change of $\frac{1}{2}$ per cent. in the light of the corona will produce a visible change of shade on the photographs, or, in other words, the totality photographs will show 50 times as much detail as the Sun light photographs. It will be observed that, for the purposes of comparison of the photographs taken in Sun light and during totality, neither an error in the assumption of the shade to which the photographs were developed, nor in the assumption of the ratio of change of light to change of shade, nor in the number of possible shades in a photograph, would vitiate the result, as they would apply equally to both cases.

It therefore appears to me most probable that, under all circumstances, by whatever ratio the air glare is brighter than the corona, in very nearly the same ratio will the detail of the corona be obliterated in a photograph taken during Sun light, as compared with one taken during a total eclipse; that is to say, that, unless a change of shade in the corona were considerably more than 50 times as abrupt as the least change visible in the totality photographs at Grenada, it would be invisible in the photographs taken there in Sun light.

This naturally leads to the question—Are there abrupt changes of shade in the corona, or does the light diminish gradually as the distance from the Sun increases? As far as I have seen, an increase of exposure of the photographs taken during totality regularly brings with it an increased extension of the corona, as photographed; that is to say, that the light of the corona gradually diminishes in intensity from the Sun outwards. The detail of the inner parts of the corona soon gets obliterated as the exposure increases, which shows that this detail is due to delicate rather than to abrupt changes in intensity.

I conclude, therefore, that the effect of the light of the sky on photographs taken during Sun light, when the atmospheric conditions are at all comparable with those at Grenada, would be to entirely alter the form of the corona, in so far as that form is made visible by gradual changes of luminosity; and that, with regard to abrupt changes of luminosity, they would have to be exceedingly well marked before they would be visible in the Sun light photographs, whereas, as a rule, the form of the corona is indicated by somewhat delicate shading.

Thus, the possibility of photographing the corona in Sun light appears to depend on the extent to which at high altitudes, or at other localities, more favourable atmospheric conditions may be found. With clearer air, both the Sun and the corona will be brighter and the air glare less, but to what extent this change may be hoped for I do not know. It is to be observed, however, that besides air glare we have to deal with internal reflection, which increases with the increased brilliancy of the Sun. If my exposure, short as it was, was in reality sufficient for the internal reflection to produce an effect on the plate, with longer exposures and a brighter Sun, but under otherwise more favourable conditions, this might become a serious source of trouble.

The limb of the Moon has undoubtedly been seen for some time after totality (not at Grenada, where no doubt the atmospheric conditions were too unfavourable), and, if visible, it should, I think, be possible to photograph it. But we must be careful in using this as an argument in favour of the possibility of obtaining photographs of the corona in Sun light. In the first place, this phenomenon has generally been observed within a few minutes of totality. For example, Mr. S. P. LANGLEY, in 1878, at an elevation of 14,000 feet, was able to observe the limb of the Moon for 4 minutes 12 seconds after the reappearance of the Sun without taking any precautions to shield his eyes.* Mr. H. H. TURNER has kindly calculated for me that at this time only 0·068 of the Sun's surface was exposed; and, as the outer rim of the Sun has only about half the brilliancy of the central parts, it may be safely assumed that the total light emitted was not more than one-twentieth of full Sun light. If the air glare varies as the total light emitted, and if, as has been assumed for the purposes of argument, the ratio of air glare to corona were 50 to 1 in full Sun light, it would only be $2\frac{1}{2}$ to 1 under these circumstances. This would, of course, make the limb of the Moon much more readily visible against the corona. This argument does not apply to the cases where the limb of the Moon has been seen long before or after totality, but as long as the cusps are fairly sharp another circumstance must be considered. In all the photographs which I took during the partial eclipse in which there is a false corona, it will be seen that it is distributed round the mass of the Sun as a centre, and does not uniformly fringe its outline. The cusps often protrude out of it, and are quite free from it. Hence, the corona will be less overpowered by the air glare

* 'Nature,' November 18, 1886.

near the cusps than at other parts of the limb of the Sun. In the second place, in the case of the Moon being seen against the corona, we are dealing with a sudden change of shade, and not a gradual one, as in the case of the corona. This, as I have, I think, proved, would make it more easily photographed. It would have been encouraging to have obtained photographs of the limb of the Moon during the partial eclipse; but, unless they were obtained when a large part of the Sun was exposed, it would have done little towards proving the possibility of getting photographs of the corona in full Sun light.

It is to be observed that many of these remarks indicating the improbability of obtaining results by this method do not hold good in the case of exceptionally well marked features of the corona, where the changes of shade are really abrupt, and especially if they are radial in their direction. Thus, the first successes that we should expect to obtain would be in such cases as that of the "remarkably formed rift" in the corona of 1883, to which such a distinct likeness is reported to have been seen in photographs taken in Sun light by Dr. HUGGINS in England.

As for hoping to obtain any photographic records in Sun light of the outer limits of the corona as visible to the eye during totality, it is, I think, out of the question, as the following considerations will show, although the actual numbers quoted may be considerably in error. Judging from my own results, 10 seconds with a bromide plate would appear to be too small an exposure for the extreme limits. Assuming that the exposure I gave during Sun light was one-fifth of a second, and that the chloride plates are 30 times less sensitive than the bromide plates, then it would take 1500 times the exposure I gave during Sun light to photograph the outer parts of the corona during totality. On this point it is instructive to compare Mr. WESLEY's drawing from the photographs with Captain ARCHER's direct drawing, probably one of the most accurate ever taken.*

8. *Should the Experiment be Tried Again?*

In conclusion, the questions naturally arise: Is the experiment worth trying again, and, if so, under what conditions? As to the latter point, experience seems to show that:—

(1.) It should be tried at a station at great elevation above the sea.

(2.) It should be tried when the Sun is nearly vertical, during an eclipse of the corona by the Moon, Mercury, or Venus.

(3.) The exposure must be more varied and longer. At present I see no reason why increase of exposure should not simply increase the density of the air glare in the same proportion as that of the corona, thus gaining no advantage by the change.

Unless the air glare and internal reflection can be so diminished as to make an exposure of one second with an instrument like that used by me a possibility, I fear

* See Appendix to Captain ABNEY and Professor THORPE's paper on the Photometric Intensity of the Coronal Light during this eclipse (*infra*, p. 382).

no good results are to be expected. As to whether the experiment should be repeated, it is to be observed that the coronagraph is admirably adapted for taking photographs of the corona during totality, for ordinary records; for reflectors have several advantages over refractors for this purpose, and the special contrivances of this instrument would all be more or less useful under the circumstances. Hence, another trial can easily be combined with what may be described as the ordinary routine work of an eclipse. Atmospheric or other conditions are now certainly unfavourable, and time only can settle the question whether this is a permanent or a temporary difficulty.* I should therefore recommend the experiment being made again in connection with other work, but not before the next occasion when the path of a total eclipse passes over high ground (say, over 7000 feet) which can be conveniently reached, and where there appears to be a reasonable chance of having a clear atmosphere. But on all occasions it would, I think, be worth while arranging that some one, not necessarily a trained astronomer, should be detailed to observe for how long before and after totality the outer limb of the Moon can be observed. This was not done at Grenada. During partial eclipses a look-out should also be kept for this phenomenon.

Most of what I have said has been unfavourable to Dr. HUGGINS' method. But there is very material evidence in its favour, and, for the benefit of any one reading up this subject, I have given the necessary references in a footnote.†

VII. THE PRISMATIC CAMERA. BY CAPTAIN DARWIN.

1. *Description and Adjustments.*

The instrument was the same as that used by Dr. SCHUSTER, in Egypt, in 1882 (see 'Phil. Trans.' 1884, Part I.), and by Mr. LAWRANCE, at Caroline Island ('Phil. Trans.,' A, 1889).

The camera has a lens of 3 inches aperture, and of 20 inches focal length. There is no slit, and the prism, which is placed directly in front of the lens, has a refracting angle of 60°. A photograph taken with this instrument of any small object which has a bright line spectrum will show several images, each one corresponding to some

* It is a curious fact, that the limb of the Moon beyond the Sun has seldom been reported to have been observed long before totality during the last 15 years, although before that date it is said to have been seen on several occasions. See 'Astron. Soc. Mem.,' vol. 41, 1879, pp. 25–39.

† (1.) "On a Method of Photographing the Solar Corona without an Eclipse." W. HUGGINS, 'Roy. Soc. Proc.,' No. 223, 1882.

(2.) "On some Results of Photographing the Solar Corona without an Eclipse." W. HUGGINS, 'Brit. Assoc. Rep.,' 1883.

(3.) "On the Corona of the Sun."—Bakerian Lecture for 1885. W. HUGGINS, 'Roy. Soc. Proc.,' No. 239, 1885.

(4.) "Photographing the Corona without an Eclipse." E. L. TRONVELDT, 'Observatory,' No. 117 1886.

line in the spectrum. Thus, in a photograph taken during totality, there will be several distinct images of each prominence, and, in measuring the distances between them, we are, in fact, measuring the distances between the corresponding lines in the spectrum of the prominence. A number of small prominences close together gives the appearance of a segment of a circle, while the continuous spectrum of the corona shows as a confused band across the plate. This will explain why it is difficult to obtain a clear interpretation of the various appearances shown in these photographs.

The accurate adjustment of the prismatic camera in its present form is difficult. In order that the photographic plate may be in focus for a large range of the spectrum, the camera is provided with a back which can be inclined to the optic axis of the instrument. In order to find the proper angle and position of the plate, Sun light which had passed through a collimator accurately adjusted for parallel rays was thrown into the instrument. The spectrum thus formed was focussed on the plate, and the instrument, therefore, adjusted for parallel rays entering in the direction of the optic axis of the collimator. The position of the blue and red end of the spectrum was then marked on the ground glass of the camera, and the collimator removed. If the instrument were then fixed on the equatorial, it would have been in focus, provided the camera were pointed relative to the Sun as it was before relative to the collimator. This was the awkward part of the adjustment; the camera was turned until the spectrum formed by the Sun occupied the position previously marked. The spectrum, of course, was blurred, owing to the finite size of the Sun, but the adjustment could be made approximately, and the result shows that fair definition was obtained. The slant of the plate essential for securing fair focus for rays of different refrangibility must be detrimental to the simultaneous focus of different parts of the Sun's circumference. The instrument can, therefore, never be a perfect one, but it affords valuable information in every eclipse in which a number of sharply defined prominences appear.

2. *Observations and Results.*

Only one photograph was taken during totality. The exposure was estimated at nearly two minutes, but for about fifty seconds the corona was nearly obscured by clouds. It has been explained how other photographs with another instrument, but on the same mounting, had to be taken at the same time as this one, and how it, therefore, came about that the plate shows signs of a shake.

The images of five prominences can be distinguished, but of these only two were measured, which will be called prominences Nos. I. and II. No. I. is evidently the large prominence on the north-west of the Sun which forms such a marked feature in some of the photographs. There are also two segments of rings in a different part of the plate.

The instrument used for measuring the distances apart of the images of the prominences was very kindly lent to me by the Royal Observatory, Greenwich. It was designed for measuring the Transit of Venus photographs, and consists of two parallel micrometers, rigidly fixed together and capable of sliding in one direction. With one micrometer the image is observed, and with the other the distance is read off on a glass scale.

Two of the images of all the prominences are especially dense, and from their position, and by reference to previous results, these can be recognised without much doubt as being due to the Solar H and K lines. Taking these as a basis, three other lines, *h*, H (γ), and F, can be recognised in the images of prominences I. and II. with some degree of certainty. The accuracy of this assumption can best be tested by seeing how nearly the wave-lengths of three of these five lines agree with the wave-lengths of three known lines, on the assumption that the two others are known. For this purpose F and *h* were taken as known with wave-lengths 4860·5 and 4101·2 respectively. Then the wave-lengths of the other lines were calculated on the assumption that the distances measured on the plate varied inversely as the square of the wave-length. This last assumption is, however, known to be inaccurate, and a correction had to be found and applied. This was done in the following manner:—A photograph of the Solar spectrum was obtained with the prismatic camera, but placing a slit and collimator in front of the prism. The distances apart of known lines of wave-length as near as possible the same as those believed to have been found in the spectrum of the prominences were measured; the wave-lengths of these lines were then calculated as before on the assumption that the same two lines were known. We, therefore, get in this Solar spectrum the known and the calculated wave-lengths of certain lines; the differences between the two give corrections which, if applied to the calculated wave-lengths of the lines of the prominences for the same part of the spectrum, should give their true wave-lengths. The results thus obtained are recorded in the following Table:—

Wave-length obtained as described above.		Order of intensity, both prominences.	Names and wave-lengths, known lines for comparison.
Prominence I.	Prominence I.		
4860·5	**4860·5**	3	**4860·5** F
4471·0	. .	5	4471·0 *f*
4339·3	4340·3	3	4340·3 H (γ)
4101·2	**4101·2**	4	**4101·2** *h*
3966·3	3965·0	2	3968·5 H
3932·8	3930·5	1	3932·8 K
3890·0	3890·0	3	? 3887·0 H (ζ)

The three other prominences showed only in the H and K lines, and were not, therefore, measured.

It has already been explained how it came about that the instrument was shaken during exposure. The result of the shake is that there are two distinct sets of images, corresponding to two positions of the instrument. The second set of images is far more dense than the other, and was alone used in the measurements. The images in this case are lengthened out in one direction, either by vibration or through imperfect movement of the clock, and prominence No. I. presents a curious appearance in consequence. The image is wide, and is also separated into two parts by a band of white, giving the appearance of another shake, but a comparison with other photographs shows that it is in reality due to the peculiar formation of this prominence. In this instance this white band was used in measuring the distances of the images. The first set of images is much more faint than the other, and, as far as the prominences are concerned, is only visible as the H and K lines of No. I. prominence. The existence of two segments of circles has been mentioned; these were extremely puzzling at first, until it was noticed that they coincided in position with this at first unobserved faint set of images; that is to say, that if the circles had been completed they would have run through the bases of these prominences. No other rings corresponding to other lines can be seen. The H and K lines are in all cases by far the strongest, and it is, therefore, not surprising that these should be visible when the exposure is not sufficient to produce any others. But it is remarkable that, in the second or stronger set of images, the H and K lines of the same prominence should not have rings corresponding to them, for they are far more dense; that is to say, it would appear at first sight that the same object produced an image during a short exposure, and yet failed to produce one during a considerably longer exposure. It is probable from its position that this bright inner circle was covered by the Moon during the period when the one set of images was being formed, but was visible during the other exposure, which is thus presumed to be the first. It is also to be observed that the part of the limb where these rings occur was that which was least obliterated by clouds at the commencement of totality.

VIII. The Spectroscopic Cameras. By Arthur Schuster.

1. *Resolving Power of the Instruments.*

In order to form a clear idea of the amount of detail we may expect in a photograph of a spectrum, it is necessary to enter further than is generally done into the details of the construction and adjustment of the instruments used.

Lord Rayleigh has given the theory of the spectroscope for the case of a sufficiently narrow slit. It is easy to extend his results so as to apply also to the case of slits of which the width cannot be neglected. If R is the horizontal width of the beam entering the prisms (the refracting edge being supposed vertical), r that of the beam leaving the prisms, the equation $\delta i' = \mathrm{R}\,\delta i/r$ defines the angle through which the emergent beam is turned, owing to a small rotation δi of the incidental beam. If α is the width of the slit, f the focal length of the collimator, the geometrical image of the slit will subtend an angle $\mathrm{R}\alpha/fr$ at the centre of the lens of the telescope or camera, and to this we must add the width (λ/r) of the diffraction band of an indefinitely small slit in order to obtain the actual angular width of the image. The angle between two rays having a refractive index differing by $\delta\mu$, is on leaving the prisms, as shown by Lord RAYLEIGH, $t\delta\mu/r$, where t is the aggregate effective thickness of the prisms. This angle must be equal to the angular width of the slit in order that a double line may just be resolved. We then obtain $t\delta\mu = \alpha\psi + \lambda$, when ψ stands for R/f and means the angle subtended by the useful horizontal aperture of the collimator at the slit. If $\delta\lambda$ is the difference in wave-length of the two lines, we find

$$\delta\lambda/\lambda = \frac{\partial\lambda}{\partial\mu}(\lambda + \alpha\psi)/\lambda t.$$

In order to define the actual and possible power of a spectroscope, it is convenient to distinguish between the resolving power of a spectroscope and the purity of a spectrum, and to introduce the following definitions (see 'Encycl. Britann.,' "Spectroscopy") :—

The unit of resolving power of a spectroscope in any part of the spectrum is that resolving power which allows the separation of two lines differing by the thousandth part of their own wave-length.

The unit of purity of a spectrum is that purity which allows the separation of two lines differing by the thousandth part of their own wave-length.

We speak, therefore, of the resolving power of a spectroscope, which is a constant for each instrument, and of the purity of a spectrum, which differs according to the width of the slit. Both resolving power and purity vary inversely as $\delta\lambda/\lambda$, and are equal to unity if that fraction is equal to 10^{-3}. Hence, as from Lord RAYLEIGH'S investigation for narrow slits,

$$\delta\mu = \lambda/t,$$

or

$$\frac{\lambda}{\delta\lambda} = t\frac{\partial\mu}{\partial\lambda},$$

$$1000\,\mathrm{R} = t\frac{\partial\mu}{\partial\lambda}.$$

Also, from the above equations,

$$1000\,\mathrm{P} = t\lambda\frac{\partial\mu}{\partial\lambda}\Big/(\lambda + \alpha\psi),$$

or

$$\mathrm{P} = \mathrm{R}\lambda/(\lambda + \alpha\psi).$$

Here P stands for the purity, and R for the resolving power. They are both numerically equal if the slit α is indefinitely reduced. If the light is weak, narrowing the slit will not diminish the intensity until the width of the slit, as calculated from geometrical optics, is equal to the width of a diffraction band. In that case $\alpha\psi = \lambda$ and the purity is half the resolving power. Hence we shall, when light is of consideration, only be able to make use of half the resolving power of a spectroscope. Whether slits which are wider than the limit here given show the fainter lines on a photograph more distinctly owing to their increased width is a matter for investigation; but it would, perhaps, be well in a future eclipse to adopt that width of slit which makes $\alpha\psi = \lambda$.

Two spectroscopes were to be used on the present occasion, and the following numerical data will allow us to calculate their resolving power and the purity of the spectrum which they gave :—

Spectroscope I.

	cm.
Focal lens of condenser to form image of the corona on the slit	34·0
Focal lens of collimator	106·0
Aperture of „	6·0
Width of slit (α)	·003
Greatest thickness of prism	13·2
Focal length of camera lens	83·1

The beam was limited by the aperture of the collimator lens. As the full aperture of the collimator was used, we must put $\psi = 6/106$ and $\alpha\psi = 17 \times 10^{-5}$; hence, for a wave-length of 4×10^{-5}

$$\lambda/(\lambda + \alpha\psi) = 4/21.$$

With the width of slit used, we have, therefore, made use of the fifth part of the resolving power. To calculate the latter, we have, in the first place, to consider that the effective thickness of the prism used was only 11·4 cm., as the aperture of the collimator lens was not sufficient to use the full width of the prism. The dispersive power of the glass was sufficiently near to that for which Lord RAYLEIGH has calculated the value 1·02 cm. as the necessary thickness to resolve the sodium lines. As, according to our definition, a resolving power of ·98 is required to separate the sodium lines, we may take the prism to give a resolving power of 1 for each centimeter thickness in the neighbourhood of the sodium lines; it will vary inversely as the third power of the wave-length, and will, therefore, be 3·2 for each centimeter in the violet. We have also to reduce the resolving power in the ratio of 10 : 12, as the beam is limited by a circular aperture. We have, therefore, finally in the violet

$$R = 11 \cdot 4 \times 3 \cdot 24/1 \cdot 2 = 31,$$

$$P = 4 \times 31/21 = 6.$$

The purity was such, therefore, that near the wave-length, 4×10^{-5} two lines differing by the six-thousandth part of their own wave-length should be resolved.

Spectroscope II.

	cm.
Focal lens of condenser to form image of the corona on the slit	26·5
Focal lens of collimator for G	32·5
" " telescope "	20·0
Aggregate thickness of two prisms	10·0
Width of slit (α)	·01

The beam was limited by the prisms, the aperture of the collimator and camera being more than sufficient to fill the prisms and transmit the beam.

The calculation is carried on as before, except that we must now take the useful instead of the full aperture of the collimator in order to find ψ. As the prisms were placed in minimum deviation for G, this can be calculated, and we find it to be 2·4 cm.; this determines

$$\alpha\psi = \frac{\cdot 01 \times 2\cdot 4}{32\cdot 5} = 74 \times 10^{-5},$$

$$\mathrm{P} = \mathrm{R}\lambda/(\lambda + \alpha\psi) = 2\mathrm{R}/39\,;$$

or, roughly speaking, the purity in this case was only the twentieth part of what it might have been if the slit had been narrower. The spectroscope was intended originally for the outer parts of the corona, and the slit was, therefore, intentionally kept rather wide. There was at the time no means of measuring the width of the slit, and I did not realise how much of the resolving power was lost by widening the slit. The full resolving power in the violet was 32, and therefore the purity realised 1·6.

2. *Adjustments of Instruments.*

In order to be able to differentiate between the spectra of different parts of the corona, its image must be thrown on the slit of the spectroscope; and for that purpose the plane of the slit must be placed in the focal plane of a condensing lens. This was done as follows: an auxiliary telescope was focussed for parallel rays, and the slit of the spectroscope looked at from behind through the condensing lens, which was moved backwards and forwards until a sharp image of the slit was seen in the telescope. The method is the same as that usually employed for adjusting the collimator of a spectroscope; only that, instead of observing the slit through the collimating lens, we observe it through the condensing lens.

The backs of the cameras employed could be tilted so as to have a larger portion of the spectrum in focus at once. It would be worth while to determine by calculation the best shape of the camera lenses in order that the foci of the different rays shall be as much as possible in a straight line. An achromatic lens is not as good for the purpose as an ordinary lens, but a combination might be found which would give better results than the ordinary lenses.

3. *Results.*

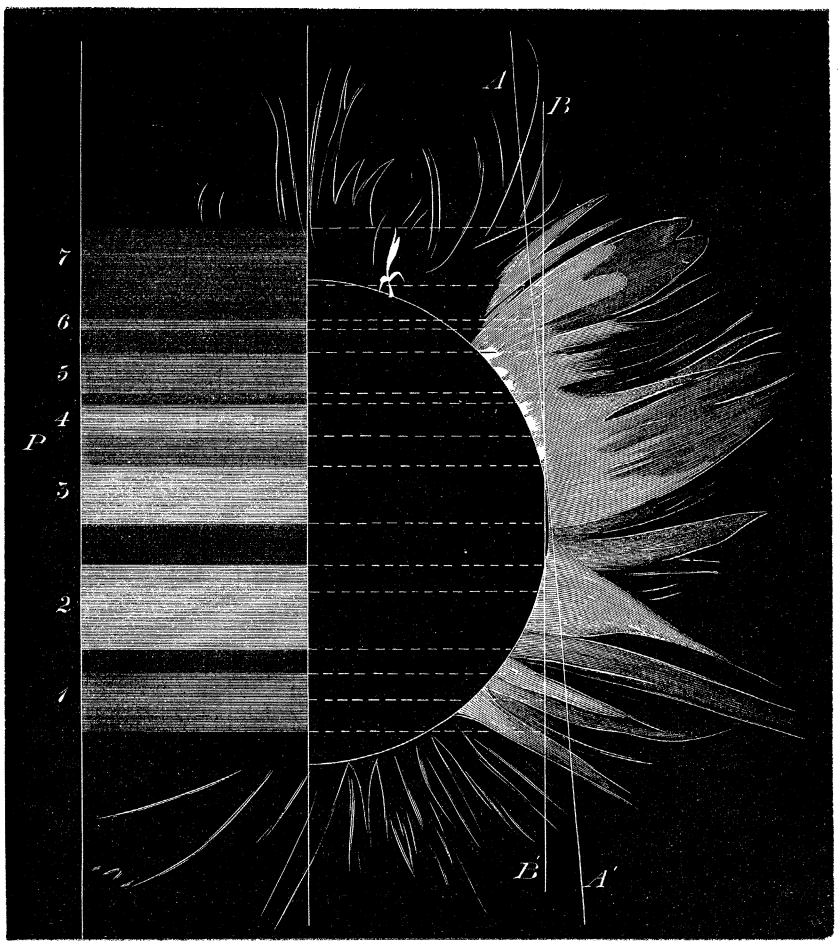
The general appearance of the spectrum of the corona, as shown on the plate exposed during the whole of totality in the spectroscope described above as No. II., is given in Plate 9, fig. 1. It has a streaky appearance, consisting of a series of bands stretching from the ultra-violet into the green, and is crossed by the bright lines of corona and prominences. The horizontal bands are parallel to the dust lines, but they must be chiefly due to the different brightness of the portions of the corona cut by the slit. This is shown by the fact that they are wider and more diffuse than the dust lines shown on the reference spectrum, and that we are able to recognise in the photograph of the corona itself the brighter portions cut by the slit. A few dust lines do actually appear, but can be easily distinguished. Fig. 5, p. 326, shows the section of the corona cut by the slit. The left-hand part of the drawing is a tracing of the streamers in the corona. The line AA′ is the line along which I had intended to place the slit. It is parallel to the declination circle, and tangential to the western limb of the Sun. It was, however, impossible to place the slit accurately in the position aimed at, and I think that, for reasons presently to be mentioned, the slit really ran along the line BB′. It must have crossed one of the prominences, for the prominence lines are very strongly marked, and this prominence will allow us to fix exactly the position of the image on the slit.

The right-hand part of the drawing gives the distribution of light and shade in the spectrum of the corona near the Solar line G reduced to the same scale as the outline of the corona. The images of the prominence appear along the line marked P. If we go from this line towards the southern side we find, in the first place, a strongly marked band, due no doubt to those parts of the corona which are closely adjacent to the Sun's limb. This band and the next are separated by an interval in which the photographic intensity is very small along the spectrum, and I believe that here the slit must to some extent have overlapped the image of the Moon.

Next comes a band, marked (2) in the drawings, in which all corona lines appear very strongly. Here the slit must have crossed the most intense streamer of the Solar corona, as will be seen by comparison with Mr. WESLEY's drawing. The most southerly band (1) coincides with the section of another streamer. It was the light interval between (2) and (3) and the portion of this band (1) which induced me to fix on the line BB′ as the most probable line of the slit.

The part immediately above the prominence towards the north, which must be close to the body of the Sun, is comparatively light; this is partly due to a speck of dust on the slit, but the continuous spectrum of the corona here must have been weaker. Generally speaking, the spectrum all over the northern side is fainter than that towards the south. It will be noticed that the bands are nearer together in the violet than in the blue. This is due to the fact that the back of the camera was tilted in order to have the whole range of the spectrum as nearly in focus as possible. The magnifying power was, therefore, smaller in the violet than in the blue, and hence the tapering of the spectrum towards the more refrangible side.

Fig. 5.



The spectrum can be traced from a wave-length 4950 in the bluish-green to about 3700 in the ultra-violet. The maximum of actinic intensity of the coronal light was decidedly more towards the red end of the spectrum than that of Sun light. This shows that the continuous spectrum is principally due to incandescent matter of a lower temperature than that of the Sun. Although the polarisation of the corona and the appearance of Fraunhofer lines show that part of the light is due to scattering of minute particles, only the smaller part can be owing to this cause, as otherwise the maximum of actinic intensity in the spectrum would be displaced towards the violet and not towards the red end of the spectrum. The faintness of the Fraunhofer lines is further evidence in the same direction, and also makes it probable that there is not much matter round the Sun sufficiently large to scatter Sun light without polarising it.

If we now turn to the lines shown on the photographs, our attention is at once arrested by the H and K lines. They form in this, as in previous eclipses, the most intense feature of the corona. In the Eclipse of 1882, these lines were so bright that the atmosphere scattered enough of their light to make them appear over the disc of the Moon and at a considerable distance outside the corona. In the Eclipse of 1883, the same lines appeared reversed over the lunar disc; on that occasion, therefore, the light scattered by the atmosphere in front of the Moon must have been derived from some source showing absorption lines. In our photographs the same lines end sharply with the corona, and we must conclude, therefore, that, in spite of the unfavourable atmospheric conditions, there was but little light scattered by our own atmosphere in the neighbourhood of the Sun. This is confirmed by the photograph of the corona itself; for, while on previous occasions the sky light formed a bright background, on which the shadow of the wire stretched across the camera showed distinctly throughout the whole plate, the two needles do not show at all on our plates, even on those which had the longest exposure. The analysis of the light seen in front of the Moon, or in the neighbourhood of the corona, seems to give us important information on the general light emitted by it, for in three successive eclipses bright lines, dark lines, and no lines at all appeared on the photograph.

The prominence lines on the plate present a curious winged appearance towards the violet. At first sight it might seem that this is due to some defect in the spectroscope, such as a reflection on the inside of the slit; but, after careful consideration, I do not think that this is possible. One edge of the wing is perfectly sharp, and almost as black as the prominence line itself. The H and K lines in the corona seem perfectly sharp along their whole length, and do not show anywhere a trace of fuzziness such as would be produced by a defective slit. The reference spectrum shows the finest lines with perfect definition. The wing appears in all prominence lines, and, if it is not due to an instrumental defect, must be owing to a rapid motion of the matter forming the prominence. I find that an approaching motion of about 247 miles per second in part of the prominence would account for the displacement. According to YOUNG, such motions have been observed, though seldom. The limb of the Sun on which the prominence appears was receding.

Another fact of some importance is this, that, while the continuous spectrum on the northern hemisphere of the Sun is weaker than on the southern, the H and K lines are stronger. The hydrogen lines $H\gamma$ and *h* do not appear at all on the southern side, while I can, under favourable conditions, trace $H\gamma$ to some distance on the northern. It will be noticed from Mr. WESLEY's drawing that the side on which these lines appear is the side which was rich in prominences. A similar phenomenon was shown in the photograph taken in 1882, when the hydrogen and calcium lines appeared strong in the corona on that side on which they were strong on the edge of the Sun, presumably in a prominence.

The explanation which first suggests itself cannot be the true one. We might think that, if many prominences exist over one part of the Solar surface, the corona above, which undoubtedly does to some extent scatter the light falling on it, will show the prominence lines more strongly than other parts which have no protuberances near them. No bright line could, however, be produced in this way, for the light scattered by the solid and liquid particles of the corona must have the same composition as the direct Sun light which reaches the Earth. The particles are illuminated not only by the prominence light, but by the light from the whole Solar surface beneath them. If we imagine ourselves placed near the Sun, the spectrum there seen cannot differ on the average from that which we observe here, and therefore the scattered light must also be of the same composition, and must show dark lines on a bright background, and not bright lines.

Only one other explanation occurs to me. We must imagine that sufficient matter is actually thrown into the corona from the prominences to show the lines. This does not seem surprising if we look, for instance, at the large prominence on Mr. WESLEY's drawing. The life of such a prominence is short; had the eclipse taken place a few hours later, it might not have appeared as a prominence; but would it not have left behind it a sufficient quantity of calcium and hydrogen to show their characteristic lines? We need only consider the remarkable effects produced in our atmosphere by the Krakatoa eruption, and reflect that matter is projected into the Solar atmosphere on a vastly larger scale, to see that the coronal spectrum, as we observe it, may contain injected calcium, which only very gradually sinks back again into the Sun.

According to this view, we must separate the true coronal spectrum, which will be seen evenly all round the Sun, from the spectrum due to prominence matter, which will differ much in intensity during different eclipses.

The strongest of the true corona lines has, according to my measurements, a wave-length 4232·8. It is slightly less refrangible than the calcium line 4226·4 which forms a conspicuous feature in the Solar spectrum. That it is not identical with it is proved not only by the measurements, but by the fact that on the corona photograph the Fraunhofer line appears faintly by the side of the corona line. As the well known green corona line is always present in the spectrum of the chromosphere, it is very likely that this new line, which appears so strongly on our photographs, should frequently make its appearance there. YOUNG, in his excellent book on "The Sun," gives, indeed, a line 4233·0 as one of about 30 lines which appear there on "very slight provocation." It therefore seems highly probable that the two lines are identical, and that the comparatively weak Fraunhofer line 4233·0 is a corona line.

A faint Fraunhofer line has just been mentioned as appearing in the corona spectrum. The appearance of dark lines has been a source of considerable trouble in the reduction of the photographs. After all the measurements had been made, it struck me that some lines which I had put down as corona lines were really only the intervals between Fraunhofer lines, and I had to subject the photograph to a further careful examination. The effects produced by the overlapping of a spectrum of dark lines over one of bright lines is very complicated; especially, apparently, some of the weaker Fraunhofer lines can be traced, while some of the stronger ones do not make their appearance. I believe that this is due partly to the overlapping of bright and dark lines, but principally to an optical effect of contrast.

A bright line shows black on the negative and is bounded on both sides by an apparently lighter background. This is a well-known contrast effect. The H and K lines, for instance, seem to be surrounded by a lighter band, which follows the contour not only of the lines, but also of the wing by the side of the prominence. If, now, a Fraunhofer line happens to be by the side of a bright line, the contrast is strengthened, and both the bright and the dark lines appear more distinctly than they otherwise would. This is the only simple way in which I can explain some of the appearances of the photographs.

The triplet with a wave-length 4026·0, 4029·7, and 4036·8, which is represented in fig. 1, Plate 9, is a case in point; the group of Fraunhofer lines at 4031, about, is weaker than the strong Fraunhofer line 4045, and yet is much more easily visible on the eclipse photographs. This I believe to be due simply to the fact that the two lines 4029·7 and 4036·8 set off by contrast the intervening group of Fraunhofer lines.

Such contrast effects may, in some cases, have materially affected my measurements. I have made these as carefully as I could, and I believe that the great majority of the lines I have put down as corona lines are really such, but some of the weaker lines may be due to the optical effects I have just described.

Possibly the true spectrum of the corona may be still further complicated owing to the following cause:—The base of the corona gives us evidently a strong continuous spectrum, and it is possible that the lines of the outer corona may therefore appear as dark lines in a bright background. Captain ABNEY found some Fraunhofer lines reversed over the face of the Moon, while the G band was absent. The cause I have suggested may account for this. All these considerations show how very careful we must be in the interpretation of photographs of the coronal spectrum.

I had intended to have made a careful drawing of all I can see on the photographs. Figs. 2 and 3, Plate 9, are specimens of certain portions of the spectrum on a scale 40 times the original.* I had, however, to give up the work, as I found it too trying to the eyes. The length of spectrum represented in fig. 3 is in the original about 2 mm.

* The distances between the horizontal bands are magnified on a slightly different scale in the two drawings, so as to make them equal, while as before explained, the real distances decrease towards the violet.

By holding the photograph against a bright sky, and examining it with a lens of about half an inch focal length, I can see what I have tried to represent in the drawing. But I could only carry on the work about an hour at the time, and it was always followed by strong pain and neuralgia in the eye, lasting sometimes for several days. I am sorry, therefore, not to be able to accompany this report with a complete set of drawings. The two specimens which I can give will, however, give a fair idea of the appearance of the lines on the photograph. The band reaching from a wave-length 4318·4 to a wave-length 4323·7 has a curious shape. It is broad near the centre of the field, where it is widest, and there nearly covers the bright space on the least refrangible side of the G band, which comes out so strongly in photographs taken on a small scale. If it was of equal width throughout, I should have taken it for that band, coming out by contrast between the Fraunhofer line G and the group of lines at 4324. But the manner in which the line becomes thinner towards the outer parts of the corona shows that it must be a real band.

As we go from this band towards the less refrangible side, we come to very complicated markings until we reach the $H\gamma$ prominence. These markings I believe to be due to an overlapping of Fraunhofer lines and corona lines; whether the difference in the appearance on the northern or southern side is real or not I cannot be certain. The line at 4378·1 is the most conspicuous one in this part of the spectrum. By its side we can trace the Fraunhofer line 4383. The series of lines here seem to widen at the base of each bright band of the corona, and they are weak on the northern side.

I have not been able to trace with certainty a difference in the lines of the spectrum of different parts of the corona, except that already mentioned of the calcium and hydrogen lines. The group of lines at 4076 (fig. 3, Plate 9) at first sight looks as if there was such a difference; but we have here possibly only one broad band, and the lighter appearance in the centre may be a defect in the photograph, or it may be due to a Fraunhofer absorption line, which ought to be here, or, finally, to a reversal of a corona line against a hotter background.

The results obtained with Spectroscope I. are much less satisfactory. The plate had a very narrow escape during development. Owing to the hot damp weather the films were often found to detach themselves from the glass in the developing bath; but we had not found any difficulty when the bath was kept cool by ice, and when the film was soaked with alum. Captain DARWIN had kindly offered to do the whole of the photographic part of the work for me, and for this, as well as for continuous assistance in other ways, I have to offer my sincere thanks. The plate in question was developed during the afternoon of the eclipse. For some time nothing appeared, and when at last the image showed itself the plate at once began to frill at the edges. It was only by repeated treatment with ice and alum that Captain DARWIN saved the plate, but the image is very faint. What this is due to, I cannot say with certainty. The plate was one of Captain ABNEY's red end plates, and probably was less sensitive in the blue and violet than those used in the other spectroscope, which also were supplied by Captain ABNEY. The spectroscope was in good adjustment, and the reference spectrum taken with it on the day of the eclipse is perfectly sharp. If the development could have been carried further without danger, a better image might have been obtained. The plate shows in a very striking way the displacement of actinic intensity towards the red in the coronal spectrum. We can recognise on the plate the absorption of the G band and the strongest corona line, and thus fix the extension of the spectrum. The reference spectrum reaches from F into the ultra-violet, and is very strong between *h* and H. It shows, in addition, a band in the yellow or red, which, however, is very faint. The corona spectrum begins about F, is strongest between F and G, and falls off very rapidly in intensity about *h*. It can be barely traced between *h* and H. The lines shown on this photograph will be given in the annexed Table.

I must now describe the method adopted to determine the position of the lines which appear on the photographs. In 1882 a reference spectrum had been taken on the same plate previous to totality, but that reference spectrum was found to be of very little use in measuring the lines. It had to be near the edge of the plate in order not to interfere with the spectrum of the corona; and, owing to the curvature of the lines, we cannot directly compare the position of a line coming from the centre of the slit with one from near one of the edges. In order to compare accurately two spectra on the same plate, they must be in contact with each other, which would be impossible unless part of the coronal spectrum were sacrificed. A reference spectrum taken on a different plate is as useful as one taken on the same plate, if precautions are taken to fix well the position of the plate in the holder.

All good photographs of the corona which have been taken hitherto show the calcium lines coincident with H and K, and we can take these always as the starting point for our measurements. The distance of these lines from any unknown line will be the same in the centre of the reference spectrum as in the centre of the coronal spectrum, provided no shrinkage of the film has taken place during development. But, if a shrinkage has taken place, there is no reason to suppose that it is uniform all over the plate, and it may, therefore, be just as different in the reference spectrum and the coronal spectrum, if these are on the same plate, as if they are on different plates. Besides the H and K lines we can generally recognise some other feature in the coronal spectrum, consisting either of another prominence line or some well-marked Fraunhofer line; and in that case interpolation becomes easy with the help of the lines on the reference spectrum.

If we add to these considerations the danger of exposing to Sun light, or even day light, a plate which is to be used during the eclipse, merely for the sake of the reference spectrum, I think it will be conceded that I was justified in taking the reference spectrum on a separate plate. That this danger, which I had pointed out before the eclipse, is not imaginary, is shown by the failure of the Carriacou photographs.

In the present instance the hydrogen lines Hγ, *h*, and those in the ultra-violet, appear in the prominence spectrum as well as H and K, and they give us sufficient data to determine the wave-lengths of the lines which appear in the spectrum of the corona. The measurements were taken by means of a dividing machine reading to ·002 mm. The following Table gives in centimeters the distance of three of the hydrogen lines from H, as measured on the reference spectrum and on the spectrum taken during the eclipse.

	Reference spectrum.	Eclipse spectrum.
h	·3187	·3184
Hγ	·7943	·7930
F	1·5362	1·5382

h and Hγ appear as prominence lines, and their distances from H are practically the same on the two plates. About F there is more doubt. There is a faint mark on the eclipse spectrum which I had originally taken to be the image of the prominence which I expected to find there, but which may easily be an accidental spot on the photograph. The distance of this spot from H is 1·5450 or the tenth part of a millimetre further than F. The discrepancy seemed to me to be too great, and on examining the photograph again I found on the more refrangible side of the spot a distinct absorption line, which, when measured, gave 1·5382 as distance from H. Considering the faintness of the image near F, the measurement seemed now to agree, as well as could be expected, with the reference spectrum, on the supposition that F shows as an absorption line in the corona. I conclude, therefore, that the distance of a given line from H on the eclipse plate is the same as that of a corresponding line on the reference plate. In order to find the wave-lengths of the lines in the corona I proceeded as follows.

The distances of the following lines were carefully measured on the reference spectrum from H :—

	λ
Fe	4004·9
Fe	4045·1
h	4101·2
Ca	4226·4
Hγ	4340·1
Fe	4666·5
F	4860·7

Assuming the wave-lengths of F and of H to be given, we can, from the observed positions, calculate by some interpolation formula the wave-lengths of the intervening reference lines. The formula I used is the usual one

$$D\left(\frac{1}{\lambda_1^2} - \frac{1}{\lambda_x^2}\right) = \alpha\left(\frac{1}{\lambda_1^2} - \frac{1}{\lambda_2^2}\right),$$

where λ_1 and λ_2 are the two known wave-lengths, λ_x that to be determined. D is the distance measured on the plate between λ_1 and λ_2, α that between λ_1 and λ_x. The wave-lengths thus found are approximate only; but, by comparing the calculated values with those found in ÅNGSTRÖM's map and given above, we can construct a table of corrections for the reference lines. A curve was drawn having the calulated wave-lengths as abscissæ and the corrections as ordinates. This curve was found to be quite regular in shape. The same interpolation formula was now used to calculate approximate wave-lengths for the corona lines, and the corrections were read off from the curve. I have found this combination of interpolation by calculation and by a graphical method to be very convenient. In order to show the accuracy which may be obtained in this way I give the following example :—

I left out originally the reference line 4666·5 altogether. There was, therefore, a very large gap between the lines F and Hγ. The curve had to be drawn, more or less, as a straight line between the points corresponding to the two lines. Treating the reference line, then, first like an ordinary corona line, that is, finding its approximate wave-length by calculation and the correction from the curve, I found 4667·0 as its wave-length, being very near the correct value. The curve was improved by making it still further agree with this line, and, as there is no gap of equal magnitude in other parts of the spectrum, I concluded that now the errors due to interpolation are negligible. It is more difficult to give an idea of the possible error due to faulty measurement. Most of the important lines were measured several times, and, from the way in which the wave-lengths agree, I should say that an error of 1·5 near F and 1 near H in Xth metres will not often occur in the stronger lines. The position of the weaker lines is, of course, more uncertain.

As the strong corona line 4232·8 had to be used as the starting point for the measurements of the second plate, considerable trouble was taken to find its position. Perhaps the best idea of the accuracy which can be reached in such measurements can be got from a comparison of the individual measurements. The following then are in centimetres, the distances from H of the corona line and of the calcium line 4226, as measured on the corona plate and on the reference spectrum respectively. The measurements were taken at different times during the last two years. Those marked* were taken by my assistant, Mr. ARTHUR STANTON.

Distance from H.

Corona line.	Calcium line.
·5920	·5810
·5920	·5810
·5950	·5752
·5930	·5780
·5930	·5790
·5920	*·5804
·5930	*·5816
·5930	*·5804
·5902	·5810
·5910	·5824
·5930	·5822
*·5904	
Mean . ·5923	Mean . ·5802

Reduced to wave-lengths, the difference is 6·4, and, taking the wave-length of the calcium line as 4226·4, I arrive at the adopted wave-length 4232·8.

The plate taken with Spectroscope I. was more difficult to reduce. When the G absorption and the strong corona line had been recognised, the distances of the other lines were measured, and the corresponding wave-lengths found by help of the reference spectrum. All measurements of this plate were taken by Mr. STANTON, as my eyes could no longer stand the strain. The measurements are very difficult, as the band which has left a mark on the plate is exceedingly narrow, and it is sometimes impossible to distinguish accidental spots from real lines. Mr. STANTON has found a number of apparent absorption lines in the spectrum. Most of these agree with strong Fraunhofer lines, and this justifies the belief that his measurements are substantially correct as giving the positions of certain markings on the plates; but it is quite possible that specks of dust or other accidental marks have occasionally been taken for corona lines.

The following Table gives a list of the lines which have been measured on the two plates. For reference, the corona lines observed in 1882 and 1883 have been added. A query is attached to those wave-lengths taken in 1883 which have not been included in Captain ABNEY'S final list. The lines which are printed in thicker type are those I have measured repeatedly, and about which I can speak therefore with greater confidence; but all lines given have been measured at least twice. In the column headed intensity—(6) represents the highest intensity, and (1) the lowest. The intensities refer to the appearance of the lines in the centre of the spectrum, that is, on the prominence line. The numbers given are, of course, very approximate only, as the judgment about intensities much depends on the fatigue of the eye and other circumstances. Down to intensity (3) the lines are easily visible; (2) means more difficult to see, but not doubtful; the lines marked (1) are doubtful.

	Spectroscope II. Tangential slit.	Intensity.	Spectroscope I. Radial slit.	Eclipse 1882.	Eclipse 1883.	Remarks.
1	3923·9	2				
2	**3933·0**	6	..	3933	3934	K.
3	3942·3	2				
4	3950·1	2	..	3948		
5	3958·8	2				
6	**3968·1**	6	..	3968	3969	H.
7	3975·4	3				
8	3979·7	1				
9	3985·0	1	3936	..	3986 ?	
10	3990·0	2	..	3992		
11	3998·4	2	4003	..	..	An absorption line shown here in Spectroscope I., 3997, reversed across Moon's disc in 1883.
12	4004·0	1	..	..	..	
13	4009·7	3	4008			
				4015	4016	Strong in 1882.
14	**4026·0**	3	..	..	..	There is a group of Fraunhofer lines in the Solar spectrum about 4031, which is apparently visible between these lines; hence the actual positions of the lines are a little uncertain.
15	**4029·7**	4	..	..	4031 ?	
16	**4036·8**	4	..	..	4037 ?	
17	..	..	4047	4044	4045 ?	Apparently double in Spectroscope I.
18	4054·8	2	4050			
19	**4056·7**	3	4054	4057	4056	
20	**4063·5**	3	..	..	4064	The centre line shows only in the outer parts; possibly covered by Fraunhofer line 4071 in the centre. This line reversed across Moon's disc in 1863; possibly a broad line showing reversal in centre.
21	**4067·7**	1	4071	4067	..	
22	**4075·7**	3	..	..	4075	
23	**4084·2**	4	4087	4085	4085	4084 extends outwards in Spectroscope I. The two lines 4084 and 4089 are the most striking objects in this part, and look like band fading towards red (see fig. 3, Plate 9).
24	**4089·3**	4	4090			
25	**4099·7**	2				
h	4101·2	..	..	4101	..	*h* prominence line, presence in corona towards North uncertain; not visible in corona towards the South.
26	4105·2	2				
27	4112·5	2				
28	4122·7	2				
29	4128·0	1				
30	4138·0	2	..	..	..	There was a group of lines here in 1883, but the measurements do not seem to agree very well. Spectroscope I. appears to show an absorption line at 4143.
31	4144·2	2	..	..	..	
32	4148·9	4	..	..	..	
33	4156·2	3	..	..	..	
34	4165·0	3	4163			
35	4169·7	3	4166	4168	4169	
36	4173·6	2	..	4173 (short)	..	Absorption line shown in Spectroscope I. at 4180.
37	4177·3	3	..	4179		
38	**4183·5**	3	..	..	4185	

	Spectroscope II. Tangential slit.	Intensity.	Spectroscope I. Radial slit.	Eclipse 1882.	Eclipse 1883.	Remarks.
39	**4189·2**	3				
40	**4195·0**	3	4198	4195	4192	Extends outwards in Spectroscope I.
41	**4203·5**	3	..	..	..	Shows as absorption line in Spectroscope I.
42	**4211·8**	3	4212	4212	4213	
43	4216·5	3				
44	**4222·6**	3				
45	**4232·8**	5	4233	4224	4227	
46	4237·9 ?	1	..	..	..	Doubtful.
47	**4241·0**	4	..	4241	..	Shows as absorption line in Spectroscope I.
48	**4247·2**	4	..	..	4248 ?	
49	**4253·6**	3	4251	4252	4255	There was evidently a group of lines here in 1883, but the measurements given in the Report of that eclipse do not agree very well at this point.
50	4258·5	3	..	..	..	
51	4268·5	3	..	4267	..	
52	4275·5	2	..	..	..	
53	4280·6	4	..	..	4279	
54	4286·3	3	..	4289	4291	
55	4293·9	2	4293	..	..	
56	4301·0	2	..	..	..	These lines are probably only the intervals between the groups of lines which make up the G band.
57	4305·5	2	..	..	..	
58	4310·2	1	..	..	..	
59	**4318·4**	4	..	..	..	A band broad in the centre and narrowing in the outer parts of the corona.
	4323·7	4	..	..	..	
60	**4332·1**	..	..	..	..	The photograph here shows complicated markings (see fig. 2, Plate 9). The radial spectroscope shows an apparent absorption at 4336.
	4339·7	..	..	..	..	
61	**4346·9**	2	..	..	..	This line is strongly marked in the most southerly band, but this is possibly accidental.
62	**4354·7**	1	..	..	4353	
63	**4365·4**	3	..	..	..	Triplet very clearly marked in bands (2) and (3). The line 4378·1 is very conspicuous. 4370 is marked short and winged in 1882.
64	**4372·2**	3	..	4370	4370	
65	**4378·1**	4	..	..	4377 ?	
			4389	..	..	Extending outwards as an absorption. There is a strong Fraunhofer line at 4384.
66	**4387·6**	2				
67	**4395·8**	2	..	4395	..	This line shows strong in band (1).
68	**4402·2**	3	..	4401	4400 ?	These bands present a curious appearance in Spectroscope II., being very broad near the middle and weak. 4422 and 4427 appear strong in Spectroscope I. 4401 was short in 1882.
69	**4411·3**	2	..	4414	..	
70	**4418·3**	1	4422	..	..	
71	**4427·5**	3	4427	..	4427 ?	4427·5 seems rather stronger in the northern part of the corona.
72	**4434·8**	2				
73	4445·8	1	..	4442	4449 ?	
74	4452·9	3				
75	4460·7	2				
76	4468·5	3	..	..	4465 ?	

	Spectroscope II. Tangential slit.	Intensity.	Spectroscope I. Radial slit.	Eclipse 1882.	Eclipse 1883.	Remarks.
77	4474·4	2	..	4473	4473	
78	4482·7	2	4481	..	..	Extending outwards in Spectroscope I.
79	**4485·6**	3				
80	4493·4	1	4491	..	..	Extends outwards in Spectroscope I.
81	4498·5 }	3				
82	4505·4 }	2	..	4501	4501	Marked double in 1882.
83	4508·9	2				
84	4515·6	2	..	..	4518 ?	
85	4520·7	1	4520	4526		
86	4530·0	1	4527	..	..	Shows as an absorption line in Spectroscope I.
87	4536·1	2				
88	4541·3	1				
89	4547·8	2	4550	..	4546 ?	This and the succeeding two lines are very broad at the base of each horizontal band.
90	4557·2	2	4563	..	4555 ± 3 ?	Marked in Spectroscope I. as the beginning of a band.
91	4570·2	2	..	..	4571 ?	The continuous spectrum here is very strong, and the line difficult to measure.
92	4579·7	1	..	..	4577 ?	
93	4588·8	1	4586			
94	4596·2	2	4593	..	..	Extends outwards in Spectroscope I.
95	4605·9	2	..	..	4604	
96	4616·9	2				
97	4621·7	1	4620	..	4620	
98	4627·9	4	4631	..	4636	
99	4644·0	3	..	..	4642	
100	4652·1	3	4652			
101	4657·9	3				
102	4667·6	1				
103	4678·9	2	..	..	4673	
104	4686·0	1	..	..	..	Broad.
105	4696·0	..	..	..	4695	
106	4708·0	..	..	..	4706	
107	4725·0	..	..	4721	4717	
108	4733·0	..	..	..	4730	
109	4746·0	..	..	..	4738	
110	4796·0	..	..	..	..	Very uncertain.
111	4881·0	..	..	..	..	
112	4834·0	..	..	..	..	
113	4877·0	..	..	..	..	
114	4891·0	..	..	..	..	
						End of visible spectrum at 4948.

If we compare together the measurements taken during different eclipses, we find that the agreements are good, especially those taken with Spectroscope II. in 1886 compared with those taken in 1883. The resolving powers in the two cases were about equal, and double that used in 1882. Nearly all lines found in 1883 have a representation of intensity (3), or above in 1886. Our data as yet are insufficient to judge whether there is any great difference in the spectra during different eclipses. The slit in 1883 was radial; that in 1886 was placed tangential to the Sun's limb, and this would account for some difference in the relative intensities of lines, for some of the lines in the radial spectroscope fade undoubtedly more rapidly as we go outwards in the corona than others. A number of lines are wide in those parts of the corona which show the continuous spectrum, strongly thinning out where that spectrum is weak. Nevertheless, I cannot help thinking that, if the line 4232·8 had been as strongly marked in previous eclipses as it has been in this, it would have attracted special notice. On the other hand, a line 4015, which was marked strong in 1882, and which was present in 1883, does not appear in our photographs. There is, therefore, some ground to believe that the spectrum of the corona differs much on different occasions, although we want further evidence to settle the point definitely.

I have collected in the following Table the most persistent lines in the spectrum of the corona; that is to say, those which appear in all three eclipses.

1886.	1883.	1882.
4056·7	4057	4056
4084·2	4085	4085
4169·7	4169	4168
4195·0	4195	4192
4211·8	4213	4212
4232·8	4227	4224
4253·6	4255	4252
4372·2	4370	4370
4402·2	4401	4400
4474·4	4473	4473
4498·5 } 4505·4 }	4501	4501 (double)
4725	4718	4721

It seems curious that the discrepancy is greatest for our strongest corona line. The line 4227 in 1883 may have been the calcium line. That our corona line is less refrangible is proved not only by the measurements which are given above in detail, but can be shown by simply placing the corona spectrum film to film against the reference spectrum. If the H lines are made to coincide, the corona line is shown distinctly to be by the side of and not above the Fraunhofer line belonging to calcium.

For the greater part of these lines the agreement is as good as can be expected.

Amongst the stronger lines the following have been observed in 1886 and 1883, but not in 1882 :—

1886.	1883.
4029·7	4031
4036·8	4037
4063·5	4064
4075·7	4075
4183·5	4185
4247·2	4248
4280·6	4279
4378·1	4377
4427·5	4427
4468·5	4465
4627·9	4636
4547·8	4546
4557·2	4555 $\pm$ 3
4570·2	4571

Here again the agreement is good for the greater number of the lines.

If we endeavour to trace coincidences between the corona lines and the lines of known elements, we meet with serious difficulties. Owing to the multitude of lines, accidental agreements will be frequent, and no certain conclusions can be drawn unless we can trace a number of coincidences, or, at any rate, discover some group repeated with all its characteristic features. Nor must we forget that the green corona line, which, before the Egyptian Eclipse, was the only known corona line, has no representative in the spectrum of terrestrial substances, and the same holds for a large number of the lines occurring most frequently in the Solar chromosphere.

It should, therefore, not surprise us if we cannot recognise any of our elements in the corona.

I have been much puzzled, however, by a series of coincidences, which, although I have finally come to the conclusion that they are accidental, yet seem to deserve being mentioned in this report. If we look at photographs of the line spectrum of nitrogen, the most striking features in the violet are as follows :—

Nitrogen.		Corona.	Manganese.
1. A very strong line at	3994·5		
2. A triplet, the least refrangible being the strongest	4025·3	4026·0	4029·5
	4034·4	4029·7	4031·8
	4041·7	4036·8	4040·6
3. A triplet, in which the two least refrangible are the strongest	4228·9	4232·8	4227·0
	4236·4	4237·9 ?	4234·6
	4240·6	4241·0	
4. A group of lines, having its two strongest lines at	4628·9	4627·9	
	4641·2	4644·0	

Now almost the three strongest corona groups occur exactly at the same places as the groups 2, 3, and 4. When we compare the individual lines within each group, however, the agreement is not so satisfactory, and the defective coincidence in group 3 argues strongly against its reality. The strongest corona line is at 4232·8, falling in the middle between the two nitrogen lines; but the line 4228 is really a weak nitrogen line, and while the other two are about equally strong and so broad, at any rate at atmospheric pressure, that they would not be separated on our photographs, but show as a broad band with 4239 as centre, I conclude that the strongest corona line can have nothing to do with nitrogen; and this makes the other coincidences very doubtful, especially as the strongest of all violet nitrogen lines at 3994·5 has no representative in the corona spectrum. Nevertheless, I think it would be worth while to look for the strong green nitrogen line in the spectrum of the corona on the next occasion. I have also examined the spectrum of oxygen, finding a number of curious coincidences which are also very likely accidental only.

Finally, there are some of the corona lines which seem to lie very near lines of manganese. As Mr. LOCKYER has shown that the spectrum of manganese plays an important part in cosmical spectroscopy, the coincidences deserve careful consideration. I have, therefore, added a column for the manganese lines in the above Table. The strongest manganese lines in the violet are 4235·0 and 4230·7; the mean of these two numbers is 4232·85, or exactly at the place at which the strongest corona line is placed. I think that our opinion as to the presence of manganese in the spectrum of the corona must depend on the question whether the two manganese lines could possibly, with the instruments used, look as a single line. My own opinion is against such a view. Our photograph resolves lines which are quite as close or even closer together than the two lines in question, which are quite sharp. The corona line on our plate has a certain width filling a space between 4231 and 4235·0, that is to say, it fills exactly the two spaces between the two manganese lines, but does not overlap them. It is strongest near its centre. From the width of the slit used, I calculate that an indefinitely thin line would, in this part of the spectrum, cover a space of about 2·6 units, so that the manganese lines should be separate, and reach respectively from 4228·4 to 4232·0, and from 4233·7 to 4236·3. The corona line should, therefore, show very decided signs of duplicity, and I cannot reconcile its actual appearance with the supposition that it is the representative of the two manganese lines. For the present, then, our attempt to identify corona lines has only led to negative results.

4. *Summary of Results and Suggestions.*

In conclusion, I give a summary of the principal results of the spectroscopic camera, together with a few suggestions which may prove useful to future eclipse observers.

Summary of Results.

1. The continuous spectrum of the corona has the maximum of actinic intensity displaced considerably towards the red, when compared with the spectrum of Sun light. This proves that it can only in small part be due to light scattered by small particles.

2. While on the two previous occasions on which photographs of the spectrum were obtained lines showed themselves outside the limits of the corona, this was not the case on this occasion. Hence there must have been less light, due to the scattering in our atmosphere.

3. Calcium and hydrogen do *not* form part of the normal spectrum of the corona. The hydrogen lines are visible only in the parts overlying strong prominences; the H and K lines of calcium, though visible everywhere, are stronger on that side of the corona which has many prominences at its base.

4. The strongest corona line on the present occasion was at $\lambda = 4232 \cdot 8$; this is probably the same line as 4233·0 often observed by YOUNG in the chromosphere.

5. Of the other strong lines, the positions of the following seem pretty well established:—

4056·7,	**4084·2,**	4089·3,	**4169·7,**	**4195·0,**	**4211·8,**
4280·6,	4365·4,	**4372·2,**	4378·1,	4485·6,	4627·9.

The lines printed in thicker type have been observed also at the Caroline Island and Egyptian Eclipses.

6. A comparison between the lines of the corona and the lines of terrestrial elements has led to negative results.

Suggestions concerning the Spectroscopic Arrangements in future Eclipses.

1. In order to distinguish better any difference in the spectra between different parts of the corona, a larger image should be thrown on the slit. A lens of 4 or even 5 feet focal length might be employed with advantage. The aperture of the lens need not be larger than that required to fill the collimator with light.

2. The width of slit should be equal to $f\lambda/R$, where R is the useful aperture of the collimator lens, λ the wave-length, and f the focal length of the collimator. In order to prevent difficulties, due to dark lines, &c., f should be about 4 or 5 feet.

3. A resolving power of about 12 in the yellow, if full use is made of it, seems sufficient. This can be obtained either by one large prism or two small ones.

IX. Photographic Results obtained at Carriacou Island. By E. W. Maunder.

The work allotted me in the observation of the eclipse was purely photographic, and was intended to be, in its general character, a duplication of that undertaken by Dr. Schuster. The photographs which I was to take were to be both of the corona itself and of its spectrum. For the former I was provided with a lens of about $4\frac{1}{4}$ inches aperture, corrected for the photographic rays, and having a focal length of about 5 feet. The diameter of the image of the Moon on the photographs, which were taken in the primary focus, was therefore about six-tenths of an inch. For the photographs of the spectrum I had two spectroscopes, the second of which was only provided immediately before the instruments were packed up for shipment. The first spectroscope had two prisms, each 1·75 inch in height and 2·5 inches in base, and with refracting angle of 62°; and it was used in conjunction with a condensing lens of 3·5 inches aperture, and focal length of 17·5 inches. The second spectroscope had one prism, 2·6 inches both in height and base, and with refracting angle of 60°; the condensing lens used to throw an image of the Sun on the slit of this spectroscope was 3 inches in aperture, and had a focal length of 14·5 inches. These three instruments were all attached to the same frame, which was mounted equatorially and supplied with clock-work. The polar and declination axes and the R. A. and declination circles were those of the Corbett Equatorial of the Royal Observatory, Greenwich; the driving-clock also belonged to the same instrument, but the stand to which these were attached was made specially for the expedition. It was a tripod stand, composed of pieces of angle iron bolted together, and was found to be light and portable, and at the same time strong and steady. In addition to the camera and spectroscopes, a telescope of 3·6 inches aperture and 5 feet focal length, together with its finder, was mounted on the same stand; and a lens of 1 inch aperture and 4 feet focal length, with a little screen in its primary focus, was attached to the side of the coronal camera as a finder.

Owing to a delay in the selection of the equatorial mounting to be assigned to my use, and to the fact that the second spectroscope was added to the equipment as an afterthought, the preparation of the entire instrument was thrown so late that it was completed only just in time to be packed for shipment, and I had no opportunity, even for a moment, to test its performance as a whole, or of the different parts separately, except the coronal camera, until my arrival at Carriacou. On setting up the instrument there, it was at once seen that the driving-clock drove in the wrong direction, the Corbett equatorial having been last used in the Southern hemisphere. By the help of one of the artificers of the "Bullfrog" this was altered, and the driving of the clock rendered fairly good. Its actual rate was not determined, as the necessary alterations were not completed until the day before the eclipse. But the gearing of the hour circle into the driving-screw of the clock remained loose and unsatisfactory, and it was not found possible to remedy it. The spectroscopes, too, when put together, proved to require many alterations in small details, which cost the most unceasing labour, but which were all effected very satisfactorily before the day of the eclipse.

The programme which I was to carry out with the above instruments comprised the taking of one photograph of the spectrum of the corona during totality with each of the two spectroscopes, and of seven photographs of the corona itself with the five-foot coronal camera. The slit of the two-prism spectroscope was to be adjusted so as to point east and west, and to lie across the centre of the Sun; the slit of the single-prism spectroscope was to lie north and south, and to form a tangent to the east limb of the Sun.

The expedition reached Grenada on the afternoon of Thursday, August 12, and on the afternoon of the following day the huts which had been made for the Rev. S. J. PERRY and myself were taken on board H.M.S. "Bullfrog," which left St. George's at daybreak on August 14, in order to convey us to Carriacou, an island some twenty miles north of Grenada, and the largest member of the chain of islets known as the Grenadines. We anchored in Grand Ance Bay, off Hillsborough, the principal village of the island, and made the necessary examination of the country near, and inquiries respecting other portions of the island, in order to be able to select the most suitable site for our observing station. As there were no safe anchorages for the gunboat on the east of the island, and as Grand Ance Bay was on the west of the island, in its broadest part, and was, moreover, surrounded by steep and lofty hills, we were compelled to fix upon Tyrrell Bay, a little further to the south, where the island was much narrower, and where the hills were only about 200 feet in height. A position was finally selected on the top of the ridge, from whence a good eastern horizon was obtained. The site chosen was about 300 yards to the north of a small house known as "The Hermitage," the residence of Mr. DRUMMOND, and its approximate position as given by the Admiralty chart was W. long. 61° 29′, and N. lat. 12° 27′.

A heavy rain storm on August 16—the fringe of the tornado which wrecked the neighbouring Island of St. Vincent—rendered the steep slopes of the hill so muddy and sodden that the work of conveying the huts and instruments to the summit proved a lengthy and laborious task, and the huts were not finally completed and the instruments erected until Friday, August 20. The eight days still remaining before the eclipse were then devoted to the necessary adjustments of the various instruments and the alteration of the driving clock. The work was continually interrupted by short showers and passing clouds, but all the cameras were brought into good focus, the several telescopes and condensing lenses all rendered truly parallel, and the elevation and azimuth of the mounting ascertained to be correct within a minute of arc—the circles with which the instrument was provided being divided to half degrees, but reading to minutes by means of a vernier—before the day of the eclipse.

The entire programme of the eclipse had also been rehearsed on two occasions, viz., the mornings of Thursday and Saturday, August 26 and 28. Friday, August 27, was wet.

On the day of the eclipse the Sun rose behind cloud, and the sky was generally overcast, though with breaks here and there. A smart shower fell shortly before the eclipse, which necessitated a hasty closing of the observing huts, but it passed off before totality began, and the total phase was observed in what appeared to be an entirely clear space of sky. An alarm clock, ringing at every tenth second, was set up in the observing hut, and the timekeeper started the clock at the moment totality commenced, and called out at every tenth second the number of seconds yet remaining before the first re-appearance of Sun light. The clock face had been numbered for 205 seconds, and the total phase was over about one second and a half after the last had been counted, so that, estimating half a second for the delay in starting the clock, the duration of totality must have been 3 minutes and 27 seconds.

As totality approached I watched the Sun in the finder of my telescope, and gave the word "Start the clock" to the timekeeper as the last ray of Sun light disappeared. He started the striking-clock very promptly, and called the seconds very sharply and clearly throughout the eclipse.

I drew back the slide of the camera of the two-prism spectroscope first, then that of the single-prism spectroscope, and then proceeded to expose seven plates upon the corona itself with the five-foot camera, the several plates being exposed for the following times:—

First plate	0·2 second.
Second plate	2·0 ,,
Third plate	10·0 ,,
Fourth plate	40·0 ,,
Fifth plate	7·0 ,,
Sixth plate	4·0 ,,
Seventh plate	0·2 ,,

The fourth plate was exposed at the word "120 seconds," *i.e.*, 85 seconds after the commencement of totality, and closed at the word "80 seconds." The seventh plate was exposed before the word "20 seconds." It would have been quite possible to have obtained at least two more short exposure plates had I had them ready, but I had not judged it wise to attempt more than the seven of the original programme, as I had not been able to manage more than that number during the rehearsals, but I found that I was able to work more rapidly and collectedly during the eclipse itself than during the preliminary drills.

At "10 seconds" the timekeeper gave the word "close cameras," and I closed the single-prism spectroscope first, and the two-prism spectroscope afterwards. Both were closed some seconds before the end of totality. I was able to look up at the corona during the exposure of my plates, and I watched it through the finder during the 40 seconds exposure of the fourth plate. I saw no trace of a red or rosy tint in either chromosphere or prominences, but I remarked two exceedingly bright and beautiful prominences of the intensest silver whiteness. The taller of these is very well shown on some of my photographs.

The light during totality was feeble, but was just barely bright enough to enable me to read the programme which I had written out in a bold round hand, and had pasted on the top of the coronal camera.

After totality the Sun was covered by light cloud almost immediately, but a photograph was secured to give the direction of the two needles which were fixed in the east and west sides of the coronal camera, close to the sensitive plate; and later on the Sun was brought on the bottom of the slit of each of the spectroscopes and the plates were re-exposed for a second in order to secure a reference spectrum.

The photographs were not developed until after the return of the expedition to England, when Captain ABNEY kindly consented to undertake the operation. No ice could be obtained at Carriacou, and many of the best trial plates, taken for the purpose of ascertaining the focus of the different cameras, had been spoiled or completely lost by the heat. It was, therefore, thought unwise to run the risk of developing the eclipse photographs at Carriacou, and the plates were accordingly securely sealed up, and brought home undeveloped.

Of the seven photographs of the corona taken with the five-foot coronal camera, five proved to be good, one showed some deformation of the image, and the seventh was spoiled. The spoiled plate was the fourth in order of exposure, and was exposed for 40 seconds; the accident which rendered it useless was brought about in the following manner:—Mr. DRUMMOND, the owner of the estate on which we had fixed our observing station, and who had been our most self-sacrificing host, had looked through the little telescope attached to the coronal camera during the first 80 seconds of totality, but immediately on the exposure of the plate in question he stepped down and I took his place. Unfortunately, in making the transfer in the semi-darkness, the instrument received a severe jar, a jar rendered the more serious by the unsatisfactory character of the gearing of the R. A. circle alluded to above. The clock, however, drove the telescope very satisfactorily, both before and after this occurrence. The other plates were placed, with the photographs of the other observers, in the hands of Mr. W. H. WESLEY, Assistant-Secretary of the Royal Astronomical Society, who has prepared a drawing from the collation of the entire series. The plates exposed upon the corona were supplied by Captain ABNEY, and were $3\frac{1}{4}$ inches by $4\frac{1}{4}$ inches.

The two spectrum plates were also supplied by Captain ABNEY, and were $1\frac{5}{8}$ inch by $4\frac{1}{4}$ inches in size. Both these unfortunately proved to be useless, for, on development, the coronal spectrum was found to be masked by an ordinary Solar spectrum.

It appears most probable that, whilst taking the reference spectrum, I inadvertently exposed the plates to full Sun shine for a moment or so; for it does not appear possible that the exposure at the actual time of the eclipse can have been prolonged beyond the duration of the total phase. It is to be much regretted that the attempt was made to secure anything beyond the coronal spectrum upon the same plate with it, and that any instrument not absolutely necessary should have been mounted on the same stand as the cameras and their accessories. But for this mistake I should probably have had to report the success of all the nine photographs instead of that of only six of them.

X. Description of the Eclipse and Drawing of the Corona. By Captain Irwin C. Maling, Colonial Secretary.

The Total Eclipse of the Sun on the 29th August, 1886, was observed by me from Prickly Point, Grenada, West Indies, the station selected by Captain L. Darwin, R.E., and Mr. A. Schuster, F.R.S., of the Eclipse Expedition; they kindly requested me to take charge of the disc, and the following are the results of my observations:—

Previous to the commencement of totality my eyes were covered for 10 minutes to enable the sight to be as strong as possible; I had, however, scarcely begun my drawing when a small drift of cloud passed over the eclipse, hiding it for about 40 seconds, after which time it was perfectly clear, and I was enabled to continue my observations. The Moon was surrounded by a bright halo resembling that painted round the heads of saints in old pictures, from which long streams of light extended, varying much in length, form, and apparent density. The longest ray was on the upper right side. It was of a bright pale yellow, fading into white at the extreme point; it appeared to be about two and a half times the diameter of my disc.

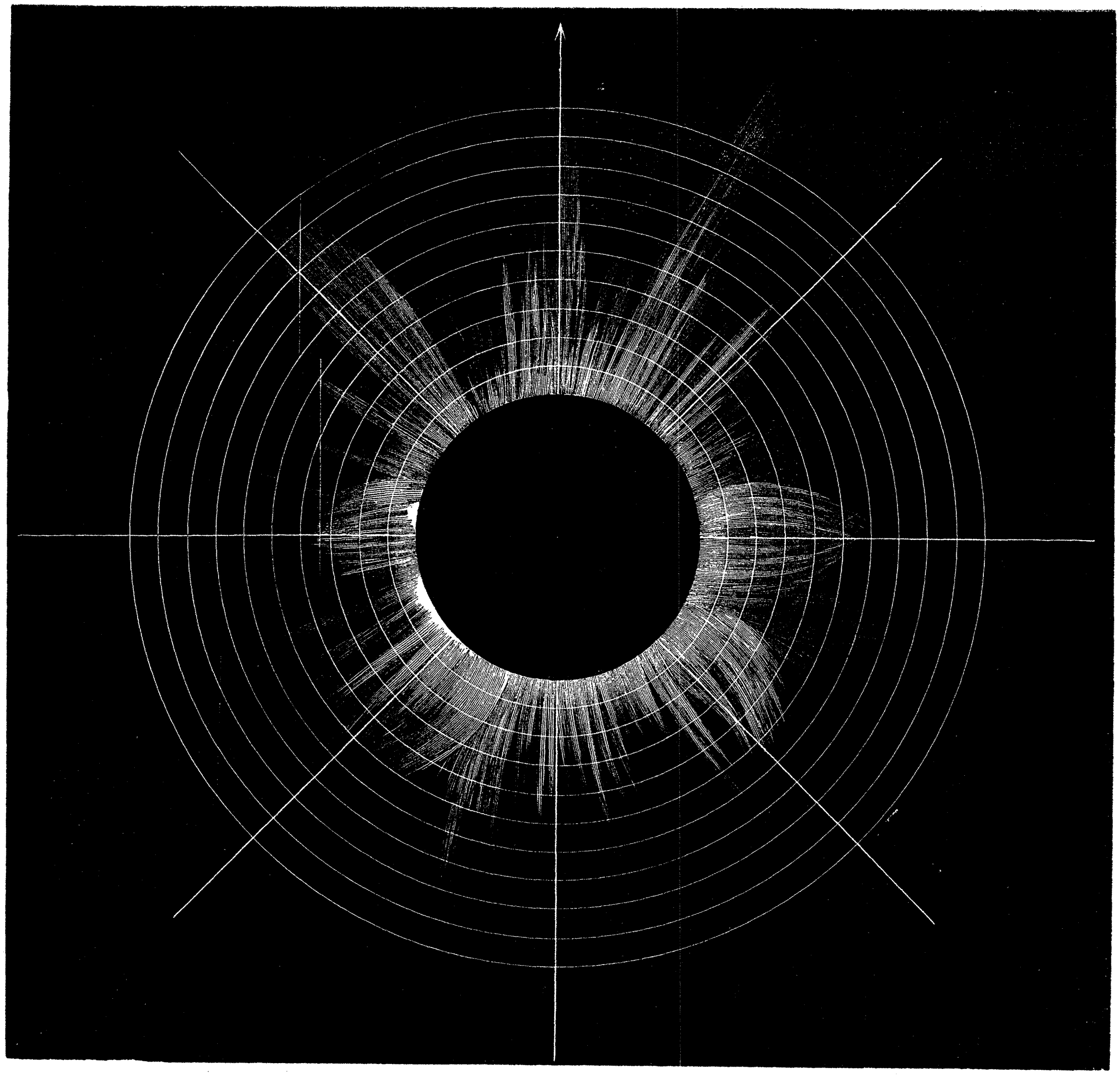
The next longest, and I admit most beautiful, streamer was on the left side, in about the 315°, counting from zenith to the right, and immediately above a small red prominence. This ray differed from its companion on the right, inasmuch that it was of a conical shape, dense along the edges and upper curve, and gradually thinning towards the corona. Its colour appeared to be of a whitish-yellow, and the centre seemed to be hollow, as if one could see through it. This description also applies to the smaller conical shaped rays on the right and the lower part of the corona.

I further observed two small prominences of a red salmon colour on the left of the Moon. The upper one was round, the lower irregular and angular, apparently in contact with the Moon, or immediately contiguous to it.

It will be observed that a small space between the long conical streamer and zenith is left bare. I was unable to complete my drawing, owing to totality being over.

I attach the original drawing in chalk done on the spot. It has not been touched in any way since.

Zenith.



XI. On the Photographs of the Corona obtained at Prickly Point and Carriacou Island. By W. H. Wesley.

The drawing from which the plate (Plate 10), has been engraved was made from a series of 7 negatives, taken by Mr. Maunder, and 5 by Professor Schuster. In the original negatives the diameter of the Moon's disc is $\frac{5}{8}$ inch, and the drawing has been made to a scale of $2\frac{1}{2}$ inches for the Moon's diameter. The following is a brief description of the individual plates.

Mr. MAUNDER'S *Negatives.*

Plate 1. Exposure 0·2 sec. Corona well defined, but not extending further than 7′ from limb. Three prominences on N.E. limb, and lower part of great prominence on N.W.

Plate 2. Exposure 2 secs. Corona well defined, but not extending further than 11′ from limb. Same prominences visible as on Plate 1.

Plate 3. Exposure 10 secs. Corona can be traced on N.W. to nearly a Lunar diameter from limb, but is extremely ill defined, showing scarcely any detail. Negative so dense that prominences can hardly be made out.

Plate 4. Exposure 40 secs. Corona of great extent, but ill defined. There has been much shake, and two separate images, about 13′ apart, are superposed upon the plate.

Plate 5. Exposure 7 secs. Corona can be traced on N.W. to about 27′, but detail very imperfect and indefinite. Not quite so dense a negative as Plate 3.

Plate 6. Exposure 4 secs. Greatest height of corona 22′. Dense negative, but detail very ill defined.

Plate 7. Exposure 0·2 sec. Scarcely a trace of corona. Plate fogged, but prominences on N.W. and W. limb perfectly defined and better seen than on any of the plates.

Professor SCHUSTER'S *Negatives.*

Plate 1. * Exposure 15 secs. (?) Lower portions of corona only are just visible. Two prominences on N.E. limb, and lower part of great prominence on N.W.

Plate 2. * Exposure 15 secs. (?) Corona slightly more shown than in Plate 1. One prominence visible on N.E. limb, and lower part of great prominence on N.W.

Plate 4. Exposure 20 secs. (?) Corona of greater extent than in any of the plates of this series, reaching on the N.W. to a height of nearly 26′. Details of coronal structure very well shown. Negative dense near limb; prominences not very distinct.

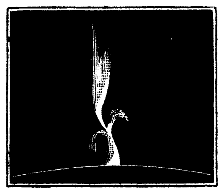
Plate 5. Exposure 15 secs. (?) Coronal detail well shown, but extension somewhat abruptly cut off at a height of about $\frac{1}{3}$ of a Lunar diameter. Dense near limb; prominences not well seen.

Plate 6. Exposure 5 secs. (?) Somewhat thin negative; details of corona in N. and S. polar regions very clearly defined, but a superposed image (a few minutes of arc from the principal image), with a trace of the re-appearing crescent, has blotted out the corona on the W. side.

* The corona was behind a film of clouds while these photographs were taken.

The Prominences.

These have been drawn from Plates 1, 2, and 7 of Mr. MAUNDER's, and Plates 1 and 2 of Professor SCHUSTER's. Twelve prominences are visible on the plates. Of these, four inconspicuous ones are near together in the N.E. quadrant. On the W. limb is a well-marked group of seven prominences extending from 20° N. to 10° S. of the equator, and at about 45° N. is a very remarkable branched one, rising to a height of 5′. The accompanying woodcut gives an enlarged view of this prominence, which is perfectly shown on Mr. MAUNDER's Plate 7.



The Corona.

This has been mainly drawn from Plates 4 and 6 of Professor SCHUSTER's. A somewhat greater extension is given in Mr. MAUNDER's Plate 3, but its definition is so imperfect that but little use has been made of this plate. To ensure greater accuracy, two perfectly independent outline drawings were made to scale; these were then superposed, and further measurements made in every case of difference.

The rifts at the N. and S. poles, which have generally characterised the corona, were well shown in 1886. They were very wide, the northern rift extending along the limb for a distance of more than 40°, and the southern for 50°. They are almost symmetrically placed about the Sun's axis. Both rifts are filled with rays, somewhat similar to the polar rays of 1878, but not nearly so fine, numerous, or regular. The southern rift is less obvious than the northern, the rays filling it being broader, longer, and less definite. These rifts are bounded on each side by groups of more or less synclinal structure, which are clearly separated from the masses of equatorial rays. The synclinal groups bounding the great rifts are very unsymmetrical, those on the east being comparatively low and depressed towards the equator, while the corresponding rifts to the west rise to a much greater height, and are nearly radial in direction. The general mass of coronal structure on the western side is therefore much greater than that on the east, although the rifts are symmetrically placed. This want of symmetry extends also to the masses of equatorial rays, that on the west extending along the limb nearly twice as far as the corresponding mass on the east; it also rises to a much greater height, and has a far more complicated structure than the comparatively low and structureless eastern equatorial group.

The specially *synclinal* structure is best seen in the mass bounding the northern rift to the west. In the centre of this mass is the tall prominence before referred to, and over this prominence the coronal rays bend towards each other on either side. The base of this group is encroached upon by the broad equatorial mass, which appears to overlap it. The group bounding the northern rift on the east shows but little structure. The synclinal group to the west of the great southern rift is nearly radial, narrow, and conical, and extends to a height of quite 24′ from the limb. The corresponding group to the east extends not much more than half this height, and is broad and depressed towards the equator.

On comparing the corona of 1886 with those of other years, it appears decidedly different from any previously photographed. The great northern and southern rifts extending for some distance along the limb, and the character of the rays filling the rifts, recall to some extent the more extreme form shown in 1878. The depression of the main coronal mass towards the equator, however, even on the eastern side, is not nearly so great as in 1878, and the western side is totally different in character. In fact, the western half of the corona of 1886 shows a striking resemblance to the corresponding side in 1871, but the compressed eastern half in 1886 and the wide polar rifts have nothing in common with the corona of 1871.

In 1875 there was the same decided want of symmetry between the two sides of the corona, but in this case it was the western half which was more depressed towards the equator. In 1875, also, there was a greater tendency towards the extreme polar depression of 1878.

The corona of 1886 has no resemblance to that of 1882, which had no conspicuous polar rifts. Almost equally dissimilar was the corona of 1883, with a single great rift at the north pole only, opened at an angle of about 90°, but hardly extending to the limb, and very unsymmetrically placed with regard to the Sun's axis. Of the Eclipse of 1885, I believe the only successful negatives were those taken by Mr. Radford, at Wellington, N.Z., now in the possession of the Royal Society, but which have not yet been published. I am not certain about the orientation of these, but, as far as I can judge, they present no resemblance to the corona of 1886.

In conclusion, it may be said that this corona occupies a middle place between the extreme forms of 1871 and 1878.

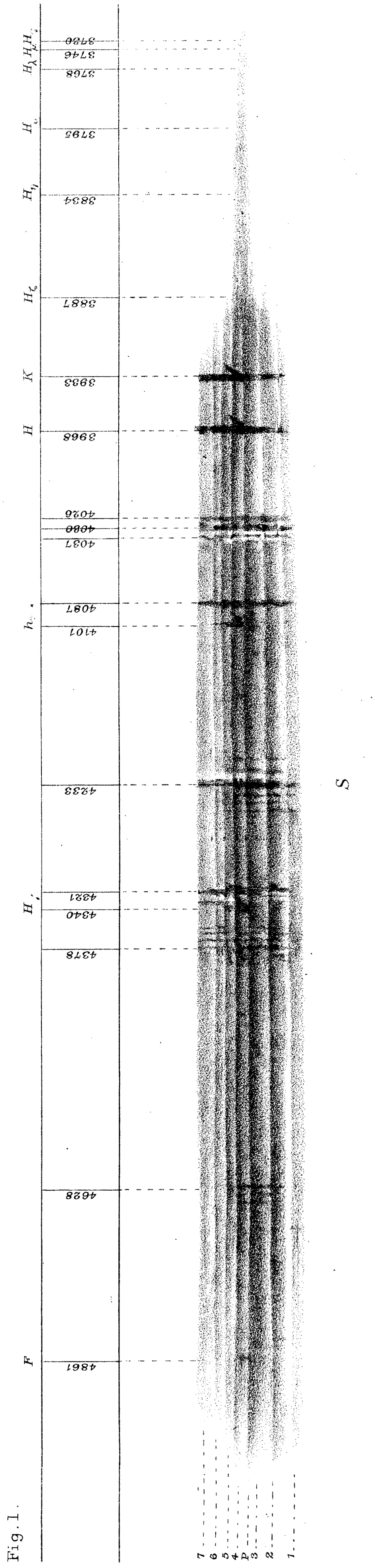


Fig. 1.

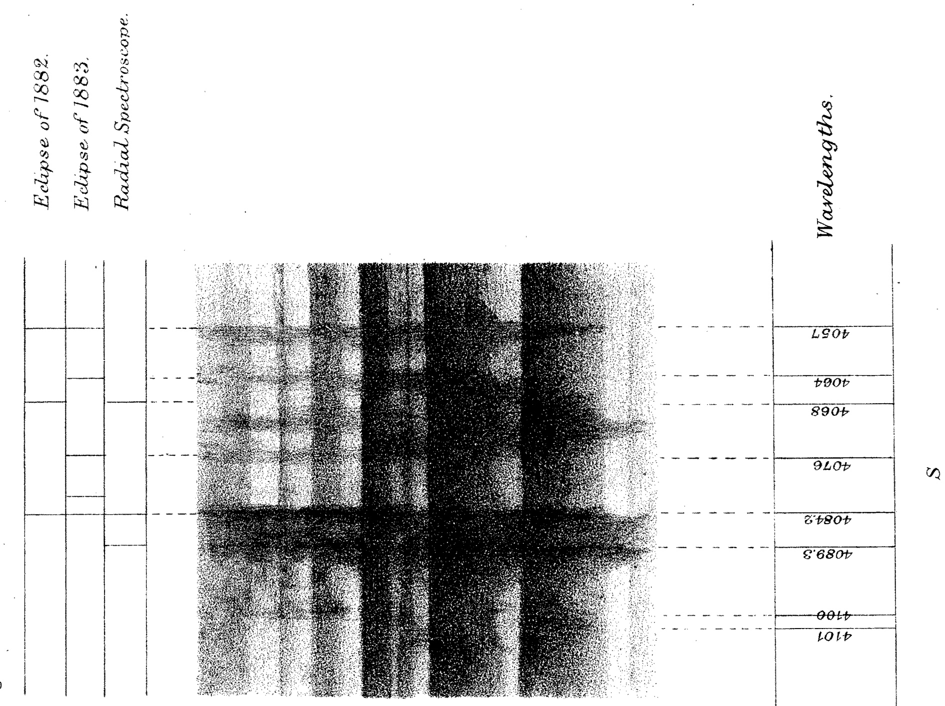


Fig. 3.

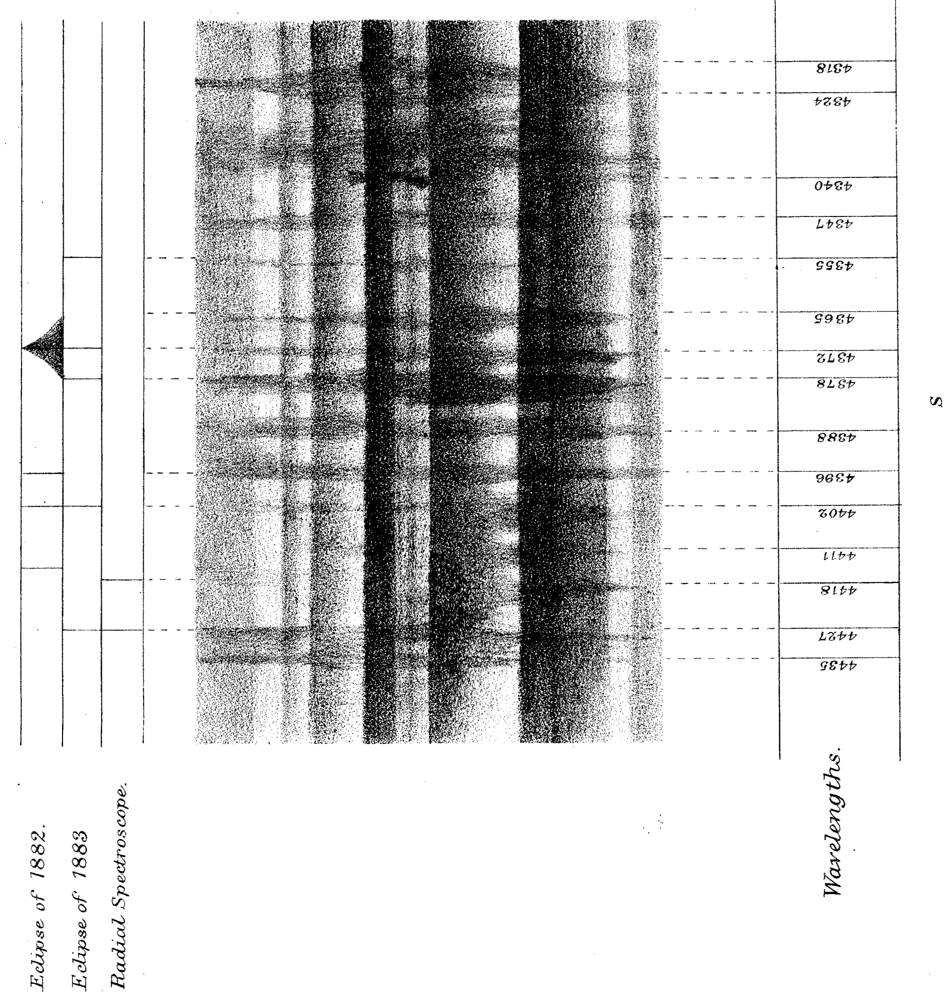
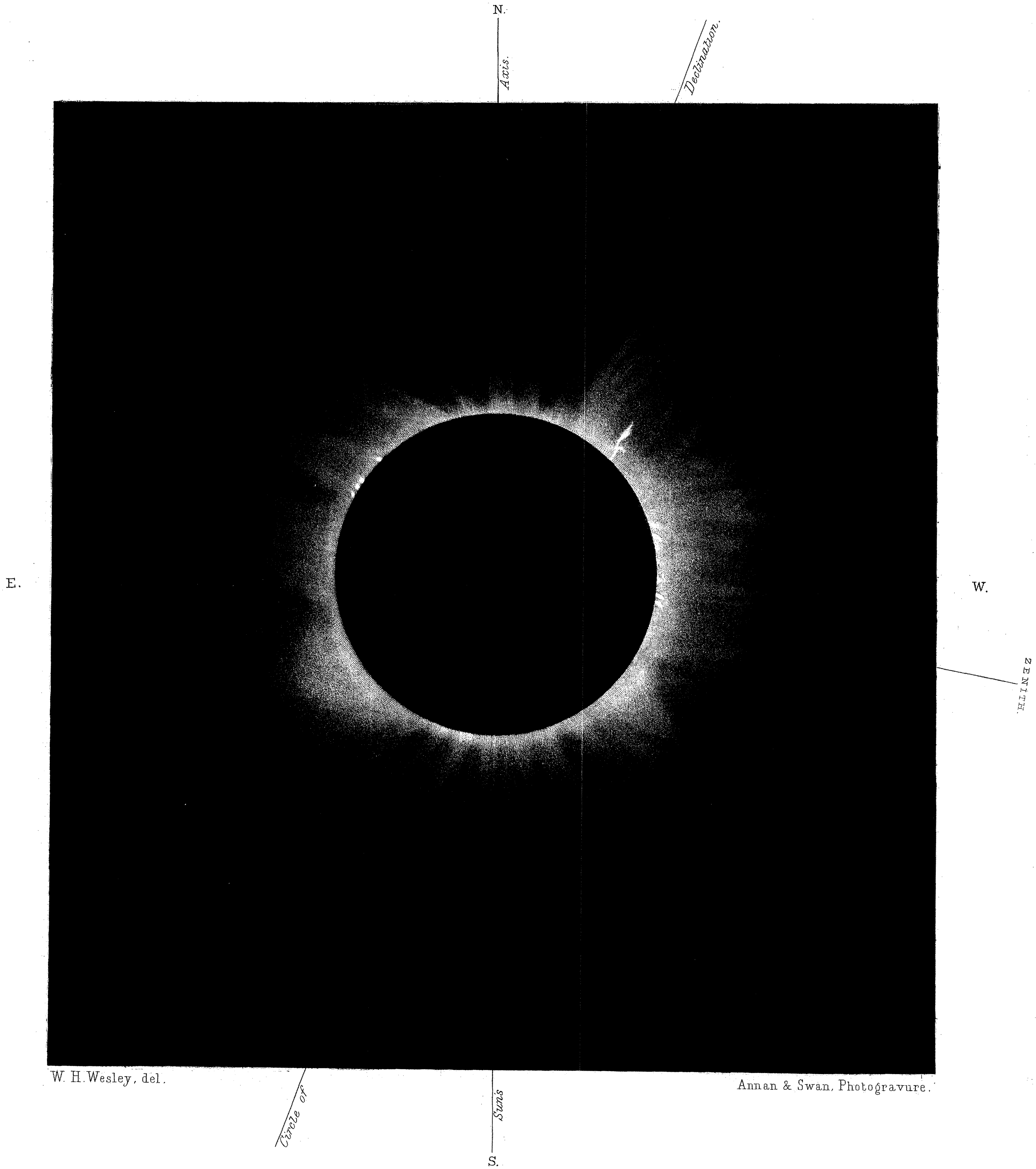


Fig. 2.

West, Newman lith.



W. H. Wesley, del.

Annan & Swan, Photogravure.

Fig. 5.

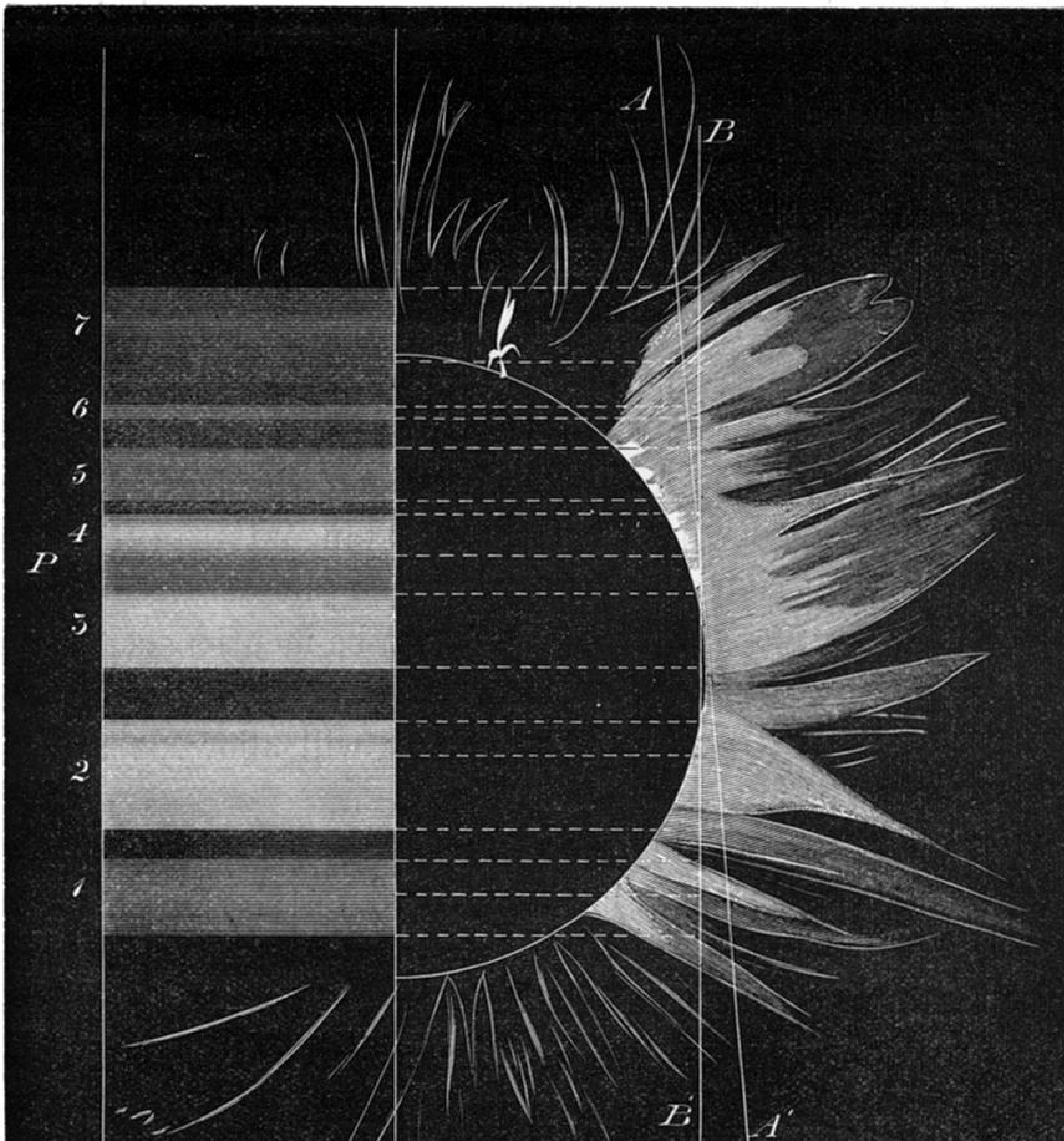


Fig. 1.

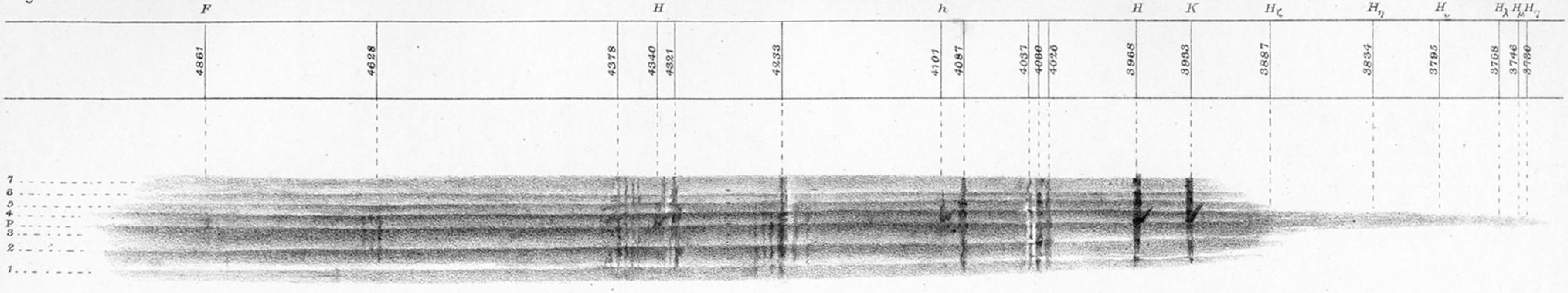


Fig. 2.

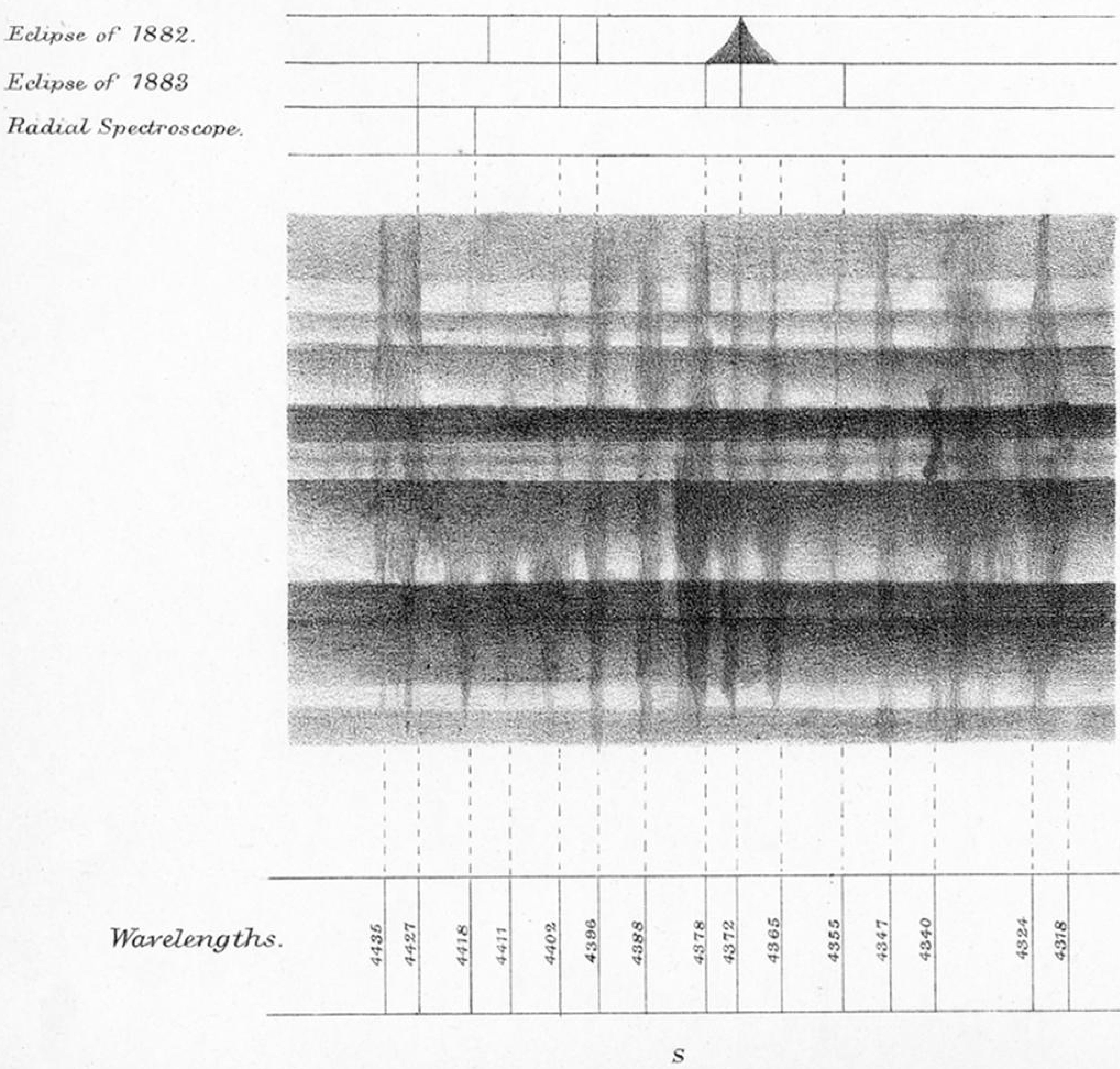
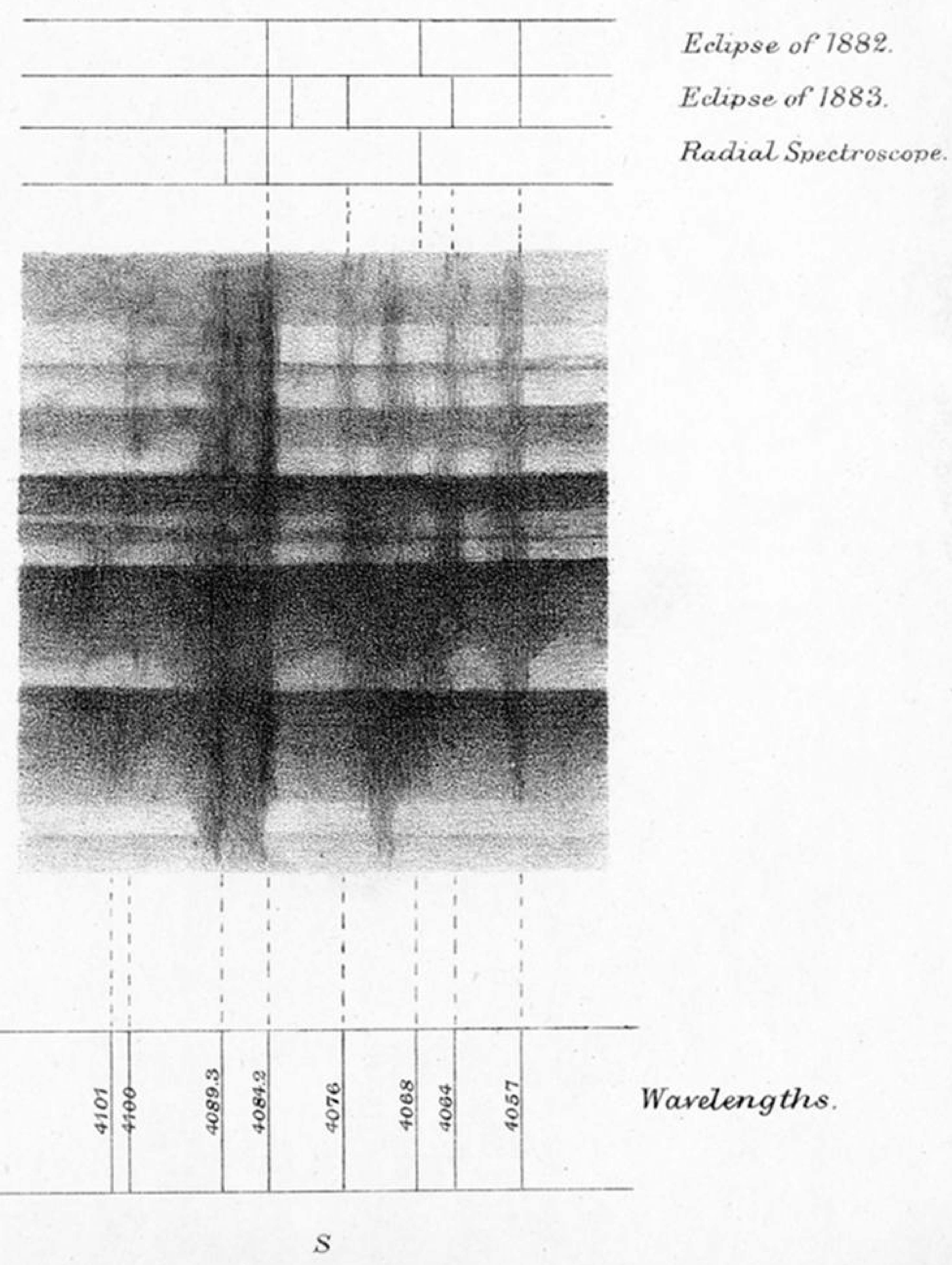
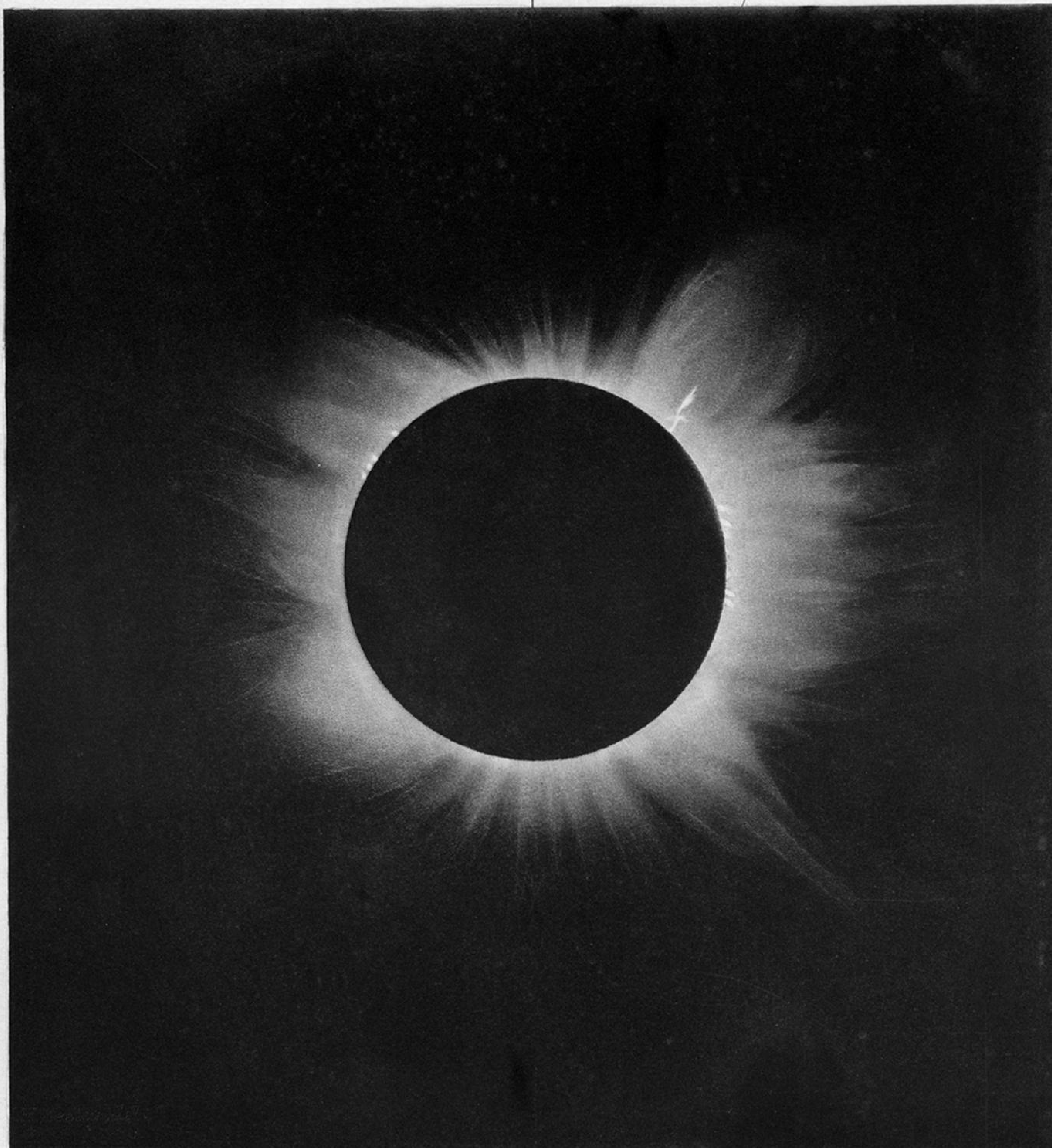


Fig. 3.





W. H. Wesley, del. Annan & Swan, Photogravure.