DARTMOUTH COLLEGE,
Hanover, New Hampshire, May 26, 1871.

SIR: I have the honor to submit the following report of my observations of the eclipse of December 22, 1870, made at Jerez de la Frontera, in Spain, as a member of the party under your charge.

By the liberality of the trustees of Dartmouth College I was granted leave of absence for four months and authorized to take with me any astronomical or physical apparatus belonging to the institution, the mere expenses of transportation and insurance being paid from the Government appropriation.

LIST OF INSTRUMENTS LOANED BY DARTMOUTH COLLEGE.

The instruments furnished to the expedition by the college were as follows:

1. An equatorial telescope (by Merz & Sons) of $2^{m}.64$ focal length and $0^{m}.162$ aperture, provided with clock-work and the usual accessories.
2. A spectroscope, by Clark & Sons, specially fitted to the above-named telescope, and having the dispersive power of thirteen prisms of 55° each. Its telescope and collimator have each an aperture of 23^{mm} and a focal length of 177^{mm}. It is the same instrument described in the Journal of the Franklin Institute for November, 1870, where it is figured. This instrument was provided (at the expense of the Government appropriation) with Professor Winlock's beautiful arrangement for registering the position of spectral lines.
3. A "comet-seeker," by Merz & Sons, of 95^{mm} aperture and 760^{mm} focal length, equatorially mounted with slow motions. This was fitted with a solar eye-piece for observing contacts, and with an arrangement for tracing the image of the corona upon a ground-glass screen during totality.
4. The telescope, collimator, and five prisms of a large nine-prism spectroscope. The object-glasses have an aperture of 57^{mm} and a focal length of about 440^{mm}. The prisms are of corresponding dimensions, with a refracting angle of 45° each.
5. An ordinary single-prism spectroscope by Clark.
6. A so-called meteor-spectroscope (direct vision) by Browning.
7. A small induction-coil, with galvanic battery and set of Geissler tubes.
8. A collection of Nicols prisms, crystals, and colored glasses.
9. An excellent pocket-chronometer by Barwise.
10. Two spy-glasses and a binocular field-glass.
11. A combined compass and clinometer.

Of these instruments, Nos. 1, 2, 7, and 9 were used by myself. No. 3 was employed by Mr. Dean in observing the first contact, and afterward by Mr. Norman in sketching the corona. The telescope, collimator, and two prisms of No. 4 were combined into an instrument which was used by Mr. Abbay; the Nicol's prisms and crystals were loaned to Professor Pickering, and the other instruments to various amateurs who offered us their aid in sketching or other forms of observation.

Leaving Hanover on October 31, I sailed from New York for Liverpool on November 3, (in company with a large number of our party,) and finally arrived at Jerez, via Southampton, Gibraltar, and Cadiz, on December 9, one day previous to the time appointed, but still the last member of the party to come upon the ground.

My instruments had been carefully packed at Hanover and sent to Cambridge, whence they had been forwarded with the other instruments of the expedition. I found them at Jerez awaiting my arrival, and on opening the boxes everything came out in good order with a few trifling exceptions.

As the observatory for the meridian-instruments and the arrangements for the photographic corps had first to be attended to, it was not until December 16 that my instrument was mounted, and it was the 20th before I had it well adjusted, the weather having been unfavorable most of the time.

It was placed under a large tent, kindly loaned to the expedition by the Jerez Cricket Club, very near the instrument of Professor Winlock, but at a distance of 30 meters or more from the rest of the party.

INSTRUMENTS.

For the observation of the eclipse I used the large equatorial previously mentioned, armed with the Clark spectroscope. The telescope is a very good one. The spherical aberration is very nicely corrected, but the correction for color is somewhat overdone, (as is the case with most of the Munich glasses,) the focus for the C line being about 15^{mm} nearer to the object-glass than that for G. This is, however, of comparatively little importance in spectroscopic observation.

The driving-clock perhaps deserves something more than a passing notice, on account of certain peculiar features which I believe are new.

It is one of the small machines such as Merz & Son have been accustomed to furnish with their instruments, regulated by Fraunhofer's centrifugal friction governor, and although it went very fairly when everything was new and in perfect order, it has far too little power to drive so large an instrument under ordinary conditions. But I have succeeded in bringing it to satisfactory performance by the following simple modification: The axis of the driving-screw revolves (when the clock-work is running accurately) once in 7 seconds. Upon this axis is secured a wheel with seven teeth like those of a scape-wheel. By adjusting the governor so that it will, when left to itself, run a little too fast, and then slightly checking this wheel every second, it is evidently possible to secure a very close approximation to uniform motion, the principle being precisely the same as in Bond's spring-governor. This necessary periodical check is obtained thus: The armature of an electro-magnet is attached to a lever which carries at its extremity a piece of watch-spring about 25^{mm} long. While the current is flowing through the coils of the magnet, this spring is pushed down between the teeth of the scape-wheel, and would stop the machine entirely if allowed to remain there; but the magnet being connected with a clock which breaks the circuit every second, the spring is then drawn back for an instant and the tooth allowed to pass, having been merely checked a little, just enough to prevent the clock-work from running ahead. Fig. 2 is designed to illustrate the arrangement.

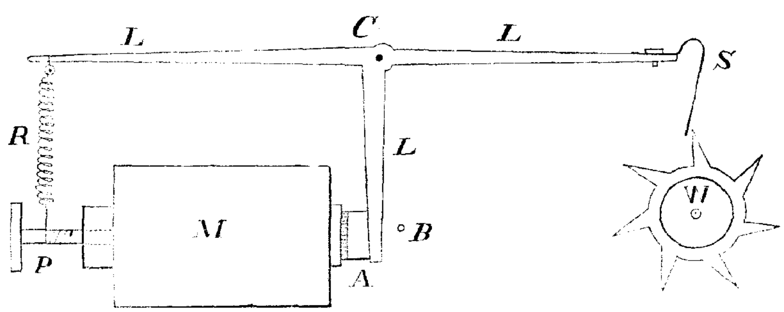


Fig. 2. Apparatus for controlling the driving-clock.

W, scape-wheel with 7 teeth; S, check-spring; C, center of motion of lever L L.; B, banking pin; A, armature faced with brass; M, electro-magnet; P, winding-pin, controlling the tension of the adjusting spring R.

In the observatory the magnet is connected with the solar clock if the telescope is to follow the motion of the sun; with the sidereal if a star is under observation. But having no clock at Jerez which could be conveniently used for this purpose, I had recourse to an expedient suggested by Professor Winlock, which answered admirably. A seconds pendulum, with a break-circuit attachment, but without other appendages, was mounted upon the telescope pier, and kept in motion by an occasional touch of the finger. It would swing without requiring any attention for about ten minutes at a time, and whenever the arc of vibration was too much reduced, the peculiar sound of the magnet at once attracted notice and called for the needed impulse.

I am confident that the plan of connecting the driving clock of an equatorial with the standard clock of the observatory, in some such way as I have described, will be found exceedingly satisfactory by any one who may try it—a vast saving of annoyance and vexation by rendering unnecessary the continual adjustments which are otherwise required by slight changes of balance in the instrument, variations of temperature, &c. If the performance of the driving-clock before being thus controlled be simply tolerable, then when connected with the standard clock it becomes as perfect as the clock itself, provided, of course, that the polar axis is accurately adjusted.

For the purpose of mounting the equatorial, a large post about 25 centimeters square was set into the ground to the depth of a meter, and the top was sawed off at an angle a little greater than the latitude of the place, the inclination being roughly measured by the clinometer.

Upon the top of the post was secured, by a single strong bolt near the upper extremity of the slope, a piece of plank about 20 centimeters wide by 760 in length; to this was firmly bolted the bed-plate of the polar axis. The plank being thus fastened to the post at one point only, it was possible to swing it around enough for the needed azimuthal adjustment, and by driving wedges between the top of the post and the plank, slight changes of the inclination of the polar axis could be readily effected; at the same time the weight of the telescope and its mountings was abundantly sufficient to keep everything in place unless purposely disturbed.

The spectroscope was attached to the telescope by a stiff tube of hard brass about 32 millimeters in diameter. This is clamped to the tail-piece of the telescope by a pair of strong rings; and in a similar manner the frame of the spectroscope is secured to the rod by two clamp-screws, so that by simply loosening these the spectroscope may be brought nearer to or removed farther from the telescope in order to bring the slit to the exact focus of the particular rays of the spectrum under examination.

To facilitate this exceedingly important adjustment, lines were marked upon the rod corresponding to C, D_3, *b*, F, and G.

Sometimes, instead of using this arrangement, the spectroscope is slid to a little greater distance and the image is thrown up by a concave lens of long focus; this enlarges the image somewhat, and by sliding the lens slightly, in or out, a most accurate adjustment of focus is attainable. But a little loss of light and injury to the definition from the action of this supplementary lens makes the former method preferable as a general rule.

A small piece of card with an orifice in its center was fastened over the slit, and no other finder was necessary, as even during the totality the image thrown upon the card was abundantly bright to enable me to point to any desired portion of the corona with perfect certainty.

A little mirror was attached to the brass carrying-rod, and so arranged that with one eye I could see in it the card and the image upon it, while the other was at the eye-piece of the spectroscope.

As has been mentioned before, the instrument was provided with the beautiful arrangement of Professor Winlock, which, by a mere touch of the finger, records permanently upon a silver plate the exact location of any line that may be upon the cross-wires.

OBSERVATIONS.

The 21st of December, the day preceding the eclipse, was perfectly clear, and I took advantage of it to complete the adjustments of my instrument, and to examine the circumference of the sun for prominences. There were no very large ones, but several that were very brilliant and very active. One on the northwest limb was particularly so, and its spectrum contained not only all the bright lines I had ever seen before, but some new ones. I noticed especially an iron line below C, the reversal of 655 Kirchhoff, and the three chromium lines at 1601, 1605, and 1607 of the same scale. The red line has been seen before, but only rarely; on this occasion, however, it was so bright that I showed it without difficulty to several of the bystanders. The chromium lines are, I believe, entirely new.

It was my intention to examine the whole circumference of the sun on the morning of the eclipse, and to map down the different protuberances. This was prevented by the clouds, for during the night the sky had become overcast, so that the prospect was exceedingly gloomy for us. We made all our preparations, however, and a little while before the beginning of the eclipse the clouds cleared away somewhat, and I was in hopes that I might be able to use the spectroscope in observing the first contact.

But the chromosphere lines were only occasionally and faintly visible through the haze, (which even when thinnest was always sufficient to produce a well-defined halo of 22° radius,) and I was, therefore, unable to make a satisfactory observation. I had, however, a momentary glimpse of the moon's limb on the chromosphere, and announced her approach some ten seconds before Mr. Alvan G. Clark, who was standing very near, using the large finder of Professor Winlock's telescope, perceived the contact. His observation was made at $10^h\ 25^m\ 45^s \pm 2$ by my chronometer, which I had previously set by the standard chronometer in Mr. Dean's observatory, allowing for the error of the standard at the time of comparison, as given by Mr. Dean. I suppose that taking into account everything, the error of my chronometer could hardly have been more than from 3 to 5 tenths of a second at the time of Mr. Clark's observation; but as I looked at its face only *after* Mr. Clark spoke, taking my eye from the eye-piece of the spectroscope for the purpose, the *error* of *noting* may easily amount to the $\pm\ 2^s$ indicated.

Immediately after the contact, I took off three of the prisms from the spectroscope, reducing its dispersive power from 13 prisms to 7, the largest number which could be used with the registering apparatus. I was not able, however, to adjust the instrument and cut the comparison spectrum upon the register plate, until within a few minutes of totality, as we could only catch momentary and unsatisfactory glimpses of the sun between the thickening clouds. These seemed to grow continually denser and more numerous, so that we were all oppressed with apprehensions of total failure. But a few minutes before totality, a rift of blue appeared in the west, and we hailed it as affording a gleam of hope. Slowly it drifted along, directing its course straight toward the sun—now reduced to a narrow crescent. At last it reached it; in a moment more the moon had completed the event, and there, in clear air, between the dense masses of heavy clouds, hung the beautiful spectacle.

I had previously laid down for myself the following programme:

1st. Observation of 1474, and ascertainment of the distance to which it could be traced from the sun's limb.

2d. Examination of the corona spectrum for other bright lines, as well as for dark lines, and the registry of any that might be found.

3d. Examination of the extension of the chromosphere lines outward and inward upon the disk of the moon.

4th. Examination of the spectrum of a prominence, if time permitted, in order to find lines invisible except during an eclipse.

In accordance with the programme, as soon as the sun came out into the clear sky, I had adjusted the slit of the spectroscope accurately tangential to the limb of the sun at the point where the last ray would vanish, and brought the 1474 line to the cross-wires. It was already plainly bright, the atmospheric glare being so much reduced as to make it perfectly easy to see.

The lines of *b* were also distinctly reversed, as were several of the iron lines near E, and I even thought that I could see the three chromium lines which I had found the day before.

Very soon, as the crescent grew narrower, they shone out unmistakably, and all the other lines I have mentioned became continually more conspicuous, while the dark lines of the spectrum and the spectrum itself gradually faded away; until all at once, as suddenly as a bursting rocket shoots out its stars, the whole field of view was filled with bright lines more numerous than one could count.

The phenomenon was so sudden, so unexpected,* and so wonderfully beautiful as to force an

* It was unexpected simply because it had not been seen in 1868 and 1869. In 1869, having been led to expect something of the kind by Father Secchi's report of a layer close to the sun's surface, giving a continuous spectrum, I looked for it very carefully, but failed to see it, so that on this occasion I was wholly unprepared.

I now suppose that my previous failure was due to my having worked with a *radial* slit; in this case the lines would be so short (from 0″.5 to 1″.5) that they might easily escape observation.

It is of course possible that the phenomenon may have been caused by some unusual disturbance in the solar atmosphere, (such as Mr. Lockyer has already seen on one occasion,) by which the denser vapors were carried up into the chromosphere.

If, however, as seems much more probable, the layer always exists, and is the true birthplace of the Fraunhofer lines, it is quite possible that it may be detected even without an eclipse by observations made at some elevated station, such, for instance, as Sherman, at the summit of the Pacific Railroad.

involuntary exclamation. Gently and yet very rapidly they faded away, until to within about 2 seconds, as nearly as I can estimate, they had vanished, and there remained only the few lines I had observed at first.

Of course it would be very rash on the strength of such a glimpse to assert with positiveness that these innumerable lines corresponded exactly with the dark lines of the spectrum which they replaced; but I feel pretty fairly confident that such was the case.

The grouping of the lines seemed perfectly familiar and so did the general appearance of the spectrum, except that the lines which had been visible before the totality were relatively far too conspicuous.

Mr. Pye, as will appear from his report, also saw the same thing for an instant.*

As soon as this bright line spectrum had vanished, I gently pressed against the side of the telescope tube and forced the slit away from the image of the sun toward the east. All the lines in the field (the four lines of *b*, the three chromium lines, and two or three iron lines near E) immediately disappeared except 1474, while this continued bright, though of course growing fainter as the distance from the sun increased; but by opening the slit somewhat I could trace it to a distance from the limb of more than the sun's radius, or about 16′, as determined by a glance at the card attached to the spectroscope, upon which the image of the eclipse was distinctly and beautifully visible. Light flocculi of cloud were continually and swiftly drifting over it.

By touching the tangent screw the C and D_3 lines were then brought into the field of view and their behavior examined. So long as the slit was in the chromosphere they were dazzlingly bright; but as soon as the slit was removed from this they became *suddenly* fainter, although I could trace them to a distance of 4 or 5 minutes outside of the sun and even upon the disk of the moon. F was then tried, and behaved similarly, except that being less brilliant I could not follow it so far. I have no doubt, however, that their extension beyond the limits of the chromosphere was due to reflection from the haze and flocculi of cloud above mentioned, as I saw nothing of the sort in 1869, in a clear sky.

A faint, continuous spectrum, much brighter near the sun, always formed a background for the bright lines. I saw no traces of dark lines in it, though I looked for them carefully.

Besides 1474, no other lines were seen which behaved in a similar manner or could possibly be due to the corona.†

About a minute had been consumed thus far, and I determined now to take one deliberate look at the eclipse, and did so for about 10 seconds. What I saw certainly appeared to me very different from the impressions of the eclipse of 1869. Then, under an absolutely clear sky, the corona seemed to me somewhat smaller and more sharply defined than now; far more brilliant and more beautiful then, but striated only with fine lines without any heavy markings. (Some of the observers, however, who were in Kentucky in 1869, make a very different comparison of the two eclipses.) On this occasion the corona appeared very indefinite in its outline; roughly a square with its diagonals at an angle of about 45° with the vertical, (and with the ecliptic also, since the eclipse took place at noon, and the sun was near the solstice,) and having the sun somewhat out of

* Father Secchi, in a note published in No. 1834 of the Astronomische Nachrichten, reports something perhaps similar. He writes:

"Une minute ou deux après la totalité je fixai le spectroscope à la grande telescope de Cauchoix avec laquelle nous avions fait les photographies et je visai à l'extrémité des cornes de la phase; le spectre était très discontinu; je souçonnai d'abord quelque dérangement mais ce n'était rien; la discontinuité était très grande et visible, malgré que la fente fusse assez large, car elle était destinée à régarder la forme des protubérances et à en relever la différence avec celle que j'avais observée tout à l'heure.

"Quelques minutes après les cornes s'étaient élargies cette discontinuité disparut. Cette observation me parait très importante et elle nous ouvre un nouvel horizon sur la constitution de la bord du soleil. M. Mobile a terra nova a fait aussi une observation semblable."

† In the same letter of Father Secchi, from which a quotation has been already made, he writes:

"Mon collègue le P. P. Denza, directeur de l'observatoire de Moncalieri, observa avec un spectroscope que j'avais convenablement disposé, deux raies brillantes dans la couronne, une près de l'E de Fraunhofer, l'autre au milieu entre le vert et la jaune. Faute de temps on ne pût pas mieux fixer la position."

The one near E was evidently 1474, and it would seem pretty likely that the other "half way between the green and the yellow" might be one of the two faint lines which I saw in 1869, and *doubtfully* reported as corona lines.

the center. But I was most struck by several straight dark streaks apparently related to the protuberances, which extended out from the sun through the corona and into the sky beyond to a distance fully equal to the sun's diameter.

Mr. Gilman's picture of the eclipse of 1869, (published in the report of the Naval Observatory party,) gives a better idea of them than anything else I have ever seen, although I confess that hitherto I had thought it greatly exaggerated. I did not see so great a number of rays as he has shown in that figure, but those I did see, five or six in number, three of which were especially conspicuous, are accurately represented by his sketch. The darkness was not so great as in 1869. I had no difficulty whatever in reading the seconds dial of my pocket-chronometer. Undoubtedly the obscurity was rendered less effective by the gloom that had preceded it caused by the heavy clouds.

Resuming the spectroscope, I examined hastily the other portions of the sun's circumference to determine the extent of the corona as shown by the 1474 line, and with the following results: On the west limb I traced it to a distance of 13′, on the north, 12′, on the south, 10′. A few moments still remaining, I took a hurried glance at the spectrum of the chromosphere on the western edge in order to look for new lines, but found none. I saw the following: C, D_1, D_2, D_3, 1474, 1515, 1519, 1601, 1605, 1607, b_1, b_2, b_3, b_4, 1990, 2001, 2003, 2031, F, and 2185—twenty in all. The examination was not extended below C nor above 2185, for while I was looking at the last mentioned line, and before I could bring it to the center of the field, the sun emerged. I was somewhat surprised and disappointed at not finding any new lines; perhaps a closer scrutiny with a somewhat widened slit might have been more successful.

I ought to add that the positions of the lines above *b* were determined only by my general knowledge of that portion of the spectrum, and the fact that the lines named are often observed there. I did not have time to bring them accurately to the cross-hairs and cut them upon the register-plate. I hurried the observation in order to catch, if possible, at the close of the eclipse, the same reversal of the Fraunhofer lines which I had seen at the beginning, but I was not quick enough, and as the slit was not in the proper position, I did not see it.

The sun had hardly re-appeared from behind the moon when the clouds again covered him, and we saw no more sunshine until evening, when the sky cleared off beautifully from the west after a heavy storm of wind and rain, which seriously troubled us in dismounting and repacking our instruments.

OBSERVATIONS OF MR. ABBAY.

In 1869 Professor Pickering observed the eclipse with an ordinary chemical spectroscope of one prism, unattached to a telescope. In this way he obtained a spectrum due to the total light from the whole mass of luminosity surrounding the sun within a distance of 3 or 4 degrees. He reported a continuous spectrum, in which, however, there were several bright lines, one in the red near C, another near D, and a third near E, brighter than either of the others. I never doubted that this brightest line was 1474, but as Professor Pickering had only been able to make a rough estimate of its position, Mr. Lockyer and some others were disposed to dispute this conclusion and consider it to be F. To settle this question, and because this method of observation appeared to promise results of value when combined with those obtained by the telescopic spectroscopes, it was determined to repeat the observations on this occasion.

As, however, we had no spare observer, Mr. Abbay, of Wadham College, Oxford, member of the English party under the charge of Rev. S. J. Perry, which was located near us at Puerto Santa Maria, kindly volunteered, on the suggestion of Mr. Lockyer, to join us temporarily, and use an instrument compiled for the occasion out of the collimator, telescope, and two prisms of the large spectroscope belonging to Dartmouth College. The collimator and prisms were firmly attached in the proper position to a board, and the viewing-telescope was mounted upon an arm turning on a pivot under the center of the nearest prism.

This arm was moved by a tangent screw, and an arrangement was fitted to it with which the exact position of the telescope at any moment could be registered by the puncture of a needle-point upon a piece of card, a rough, but effective imitation of the more elegant registering-apparatus attached to the other instruments.

The board to which the prisms were fastened was itself secured by a screw at its center of gravity to the extremity of a horizontal bar so that it could move freely in a vertical plane. The bar, counterpoised at the other end, turned upon a vertical pivot on the top of a post firmly planted in the ground. Thus the collimator could be directed to any portion of the sky, and by means of a shadow pointed accurately upon the sun.

The slit was provided with a comparison prism, and Mr. Abbay brought with him a small induction coil and a Geissler tube, which gave the combined spectra of hydrogen, sodium, magnesium, mercury, and iron.

This tube, prepared for the purpose in Mr. Lockyer's laboratory, was first exhausted by a mercurial pump, which always leaves some of the vapor of mercury in the partial vacuum so formed, and then filled with rarefied hydrogen. One of its electrodes was of iron encrusted with a sodium salt, the other of magnesium. It was permanently attached to the instrument in such a position with reference to the comparison prism that its spectrum could at any moment be brought into view by simply starting the induction coil. No better reference scale could be desired for determining the position of any lines that might be observed.

I simply annex the report of Mr. Abbay, without attempting to account for the very curious circumstance that he saw the C and D_3 lines (I have no doubt that the line he called D was really D_3) only at the beginning of the totality, but afterward only 1474 and F; while Mr. Pye (and Professor Pickering, in 1869) observed no such change in the character of the spectrum.

Report of Mr. Abbay.

OXFORD, *January* 24, 1871.

MY DEAR PROFESSOR YOUNG: I am rather ashamed to be so late in sending you my report of the work done with the chemical spectroscope you so kindly lent me.

The field of view was about 7°, so that the light passing through the prisms was composed of prominence, coronal, and general light extending to a distance of 7 or 8 diameters on each side of the sun. A short time before totality began, I arranged the slit so that the D lines just appeared as a single thick line, this being the narrowest slit which it seemed safe to attempt to use, and I had determined to narrow the slit considerably if the bright lines appeared as bands on a continuous spectrum. At $11^h\ 44^m$ Jerez time, I noticed the B line extremely black. As totality approached, the dark Fraunhofer lines slowly disappeared, leaving a dull spectrum, which also faded away immediately before three bright lines, (C, D, and F,) identified by means of the vacuum tube, made their appearance.

These three lines came into view within 2 or 3 seconds after the shout announced that totality had begun, and they remained about 8 or 10 seconds. When they disappeared, two very bright sharp lines were seen, one coincident with the bright F line given by the vacuum tube, the other less refrangible than *b*. After some trouble, I succeeded in placing the cross-wires on this bright line, and determined not to move the telescope during the rest of totality.

No other line appeared, although the C line of the vacuum tube was in the field on the one side and the F line on the other. I saw no continuous spectrum; the lines were bright on a dark ground. The F line was a little less bright than the other.

On the re-appearance of the dark lines after totality, I found that the cross-wires were on the vacant space between the lines 1464 and 1494 of Kirchhoff's scale. This measurement was as accurate as it was possible to make with the instrument, so I cannot say with certainty that the bright line seen was absolutely coincident with the 1474 line.

In order to get an idea of the dispersive power of the prisms, I tested the instrument by means of the light of the dull heavy clouds which obscured everything after totality, and found that I could not separate the D lines; but I was able to obtain four thick lines between E and *b*. I also saw 1464 and 1494 as single thick lines.

At the end of totality, I noticed no re-appearance of the bright lines C and D, nor do I remember at what moment the continuous spectrum again came into view.

At about the middle of totality, I looked up for a second or two at the corona. It appeared distinctly and unevenly radiated. The light was pearly white, apparently of about the intensity of the full moon, and the shadows cast by certain parts of the instrument seemed as deep as those of a bright moonlight night.

I think the dark Fraunhofer lines disappeared through want of light, and I do not believe it possible to prove their non-existence in the corona on account of that want of light.

I believe, but this is rather an opinion than the result of experiment, that the real corona is small compared with what we see, and that it is apparently magnified partly by irradiation and partly by reflection at an angle nearly 180° in the earth's atmosphere, so that under favorable circumstances bright lines may be obtained at an enormous distance from the sun's limb.

I am very glad that my results have confirmed your previous observation in 1869, and thanking you again for the use of your instrument,

I am, ever sincerely, yours,

R. ABBAY.

OBSERVATIONS BY MR. PYE.

Mr. Walter Pye, a young English gentleman, who was spending the winter in Jerez for the benefit of his health, had kindly offered his services as an assistant in any observations that might be desirable. As he had had some experience in spectrum analysis, and as there still remained another spectroscope unprovided with an observer, this was assigned to him.

The instrument was a star spectroscope belonging to the observatory of Harvard College, with a single prism of extra dense yellow glass having a refracting angle of 60°. The telescope and collimator had each an aperture of 23^{mm} and a focal length of about 180^{mm}. Its dispersive power was such that it showed without much difficulty the four lines of *b* distinct and separate. It was provided with the registering apparatus of Professor Winlock. This instrument was mounted on the same general plan as Mr. Abbay's; but to secure more light, at the same time allowing the slit to receive its illumination from the whole coronal region, I employed the following device: A small telescope, magnifying about 2½ times, with a field of about 7°, was carefully adjusted for distinct *distant* vision of a remote object, *i. e.* so that the rays from any portion of the object after emerging from the eye-piece should be exactly parallel to each other. This being placed in front of the spectroscope its effect is, not to form an image on the slit and thus restrict the observed spectrum to that of some particular portion of the coronal region, but simply to magnify the angular area from which the light proceeds to a diameter of about 4°, and thus to increase the light nearly six-fold. The contrivance succeeded perfectly, so that although the intrument was much smaller than Mr. Abbay's, I think it was fully its match in power and efficiency. A little more care in pointing was requisite on account of the condensing telescope, as we called it. To facilitate the operation, a thin piece of metal, with a round orifice in it of 2 or 3 millimeters diameter, was attached to the frame-work, and at a distance of about 30 centimeters a card was placed with a circle marked upon it. The instrument was directed by bringing the spot of light formed by the orifice into the center of this circle.

I append Mr. Pye's report, and it will be seen that his results are in perfect accordance with those obtained by Professor Pickering in 1869.

Report of Observation.

At the first instant of totality a great number of bright lines were seen, the effect being as if all the dark lines of the spectrum were converted into bright ones; these lasted only for an instant, and were seen with the slit nearly closed.

Then with a wide slit the following lines were observed: (1) C, very bright; (2) a bright line near D, probably D_3; (3) No. 1474—by far the brightest of all—peculiarly sharp and distinct; (4) F, the faintest, but sufficiently distinct.

A very small bright line also seemed to appear near 1474 for an instant, but as it could not be seen again its existence is doubtful.

The estimated relative brightness of the lines was C, 8.5; D_3, 5.5; 1474, 10.0; F, 3.0.

On the register plate—

Set No. 1 are the observed lines.

Set No. 2 are standard dark lines, commencing with C on the left, taken after the eclipse in a very imperfect light, and are not very reliable; on this set are two erased lines.

WALTER PYE.

JEREZ, *December* 22, 1870.

In order to bring out one or two points more clearly, I addressed to Mr. Pye a note making a few inquiries, and received the following as an appendix to his report:

Supplementary.

1. For about two minutes before totality the eyes were shaded according to your directions.
2. I should imagine that the duration of the number of bright lines was not longer than was sufficient to produce an impression on the retina, or less than ⅛ of a second.
3. I had in no way been prepared to expect this phenomenon, (*the reversal of the Fraunhofer lines.*)
4. No continuous spectrum was seen after totality until the slit was opened, when it could be easily seen; at neither time were any dark lines observed.
5. The slit was opened until the regulating screw did not act upon it, about 1½ turns of the screw.* The *b* lines would certainly have appeared as a single line and probably indistinct.
6. The small bright line near 1474 was nearer than it to the red, that is to the left of the register plate. It should be mentioned that it was just at the close of the totality that it was looked for again, when it could not be found.

JEREZ, *December* 23, 1870.

*Subsequent careful measurement showed that the screw ceased to act upon the slit when its width was very approximately 0.2 of a millimeter.—Y.

MR. NORMAN'S TRACING OF THE CORONA.

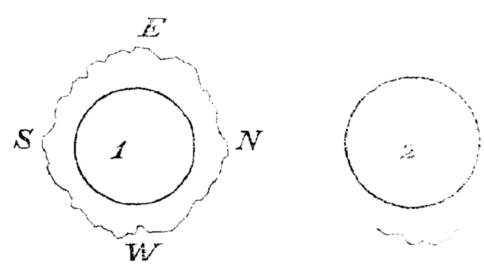
In accordance with an idea I had formed some time ago, I made arrangements to secure a tracing of the corona, hoping to ascertain whether there is any difference between the visible corona and the same thing as depicted by photography. For this purpose the comet-seeker previously mentioned, of 95 millimeters aperture, and 760 millimeters focal length, was employed. A diagonal eye-piece of low power was used, forming an image of the sun about 17 millimeters in diameter, upon a plate of glass very slightly roughened by grinding with the finest emery—just enough to give a hold to the point of a pencil. This image of the sun, seen by transmitted light of course, was very bright and sharp, and I was in hopes that that of the corona would be so likewise.

Mr. Norman, an English gentleman, resident in Jerez, and an artist of no inconsiderable skill, kindly undertook the instrument and during the totality made one tracing and commenced another, which are interesting when compared with the photographs.

He found, however, that the ground glass too much diminished the light, and he could not see on the plate many of the details (especially the dark rays) which were conspicuous to the naked eye. The drifting clouds greatly increased the difficulty.

If the experiment were to be repeated, I should propose that the tracing be made with a needle point upon a plate of gelatine or mica. I annex a copy of Mr. Norman's tracings.

(Mr. Norman's tracings.)



NATURE OF THE CORONAL LIGHT.

From the observations above reported, it is plain that the corona is to a certain extent self-luminous, and the self-luminous matter must of course be in the sun's immediate neighborhood. Its spectrum certainly contains *one* bright line, coinciding *exactly* with the dark line of the solar spectrum at 1474 of Kirchhoff's scale, this coincidence being just as well established as that of the red and blue lines of the protuberance spectrum with the hydrogen lines C and F. Whether the corona spectrum contains other bright lines is more doubtful, although the observations of Father Denza, quoted in a previous note, and my own observation in 1869, make it somewhat probable.

But while it is certain that the corona is in part self-luminous, it is hardly less so that it shines in part by reflected light, as indicated by the radial polarization, which seems to be pretty satisfactorily established by the polariscopic observations.

It might at first seem that if so the Fraunhofer lines must appear in the spectrum of this reflected light, but I think a little consideration will show that though they undoubtedly have a real existence, yet they must be so masked as to be very difficult of detection.

The total spectrum seen in an instrument like Mr. Abbay's or Mr. Pye's is, in all probability, composed of five or six different overlying spectra.

1. First we have the chromosphere spectrum, characterized by the hydrogen lines and D_3, derived from the prominences and those portions of the chromosphere not hidden by the moon.

2. In the second place we get a true gas-spectrum of the second order, in which 1474 is the most prominent if not the only line. The gaseous envelope from which this line is derived has been

called the *leucosphere*,* to distinguish it from the chromosphere, which it far exceeds in elevation and extent.

3. We have (hypothetically as yet, but very probably) a true continuous spectrum without any lines bright or dark, due to the incandescent solid or liquid particles near the sun—in other words, to meteoric dust or fog. For although it seems difficult to admit the theory of Mr. Proctor that the whole explanation of the corona is to be found in such meteoric matter, yet there is hardly room to doubt that it must contribute a very essential element.

4. We have as a fourth element the spectrum of the true sunlight reflected by the leucosphere and the meteoric dust. This light is characterized by its radial polarization, and could we examine it by itself I have no doubt we should easily find in its spectrum the Fraunhofer lines.

5. Overlying these is the light reflected by the particles of the earth's atmosphere, adding to the spectrum of course no new characteristics of its own, but only increasing the area and decreasing the intensity of the light and partially obliterating outlines and definition.

And if, as Oudemans supposes, cosmical dust between us and the moon is concerned in the coronal phenomena, then the light from this must also come into the account, a light much the same as that reflected from the particles of our own air, but with an added dash of true photospheric sunshine.

EXTENT OF THE CORONA AS INDICATED BY THE SPECTROSCOPE.

It is, of course, matter of great interest to determine the extent of the true solar corona. But the problem is rendered difficult by the action of our own atmosphere, which, especially when the air is somewhat hazy, expands the limits of a nebulosity in all directions, and in such a manner that it is not easy to distinguish between the atmospheric extension and the true corona either by the eye, the telescope, or the spectroscope.

When the air is thoroughly clear, however, as in the eclipse of 1869, this atmospheric effect is probably insignificant, as was indicated by the fact that the hydrogen lines were sharply terminated at the boundary of the chromosphere.

This year, however, the case was quite different, and they were observable, as has been stated, as much as 4 or 5 minutes outside of their proper limits. It will not do, therefore, to lay too much stress upon the fact that Professor Winlock was able to follow the 1474 line more than 20′ from the sun's limb; and yet the difference was so striking between the behavior of the hydrogen lines, which exhibited a most marked discontinuity of brightness at the edge of the chromosphere, and that of 1474, which simply grew uniformly fainter with the increasing elevation, that, personally, I have no doubt that the boundary of the true corona had not been overpassed by Professor Winlock.

By combining, however, the observations of Mr. Pye with those of Professor Winlock and myself, it is easy to show that the luminous area, from which we derive the spectrum characterized by the 1474 line, is far more extensive than the prominences and that portion of the chromosphere visible during the eclipse.

I have ventured to call instruments like those employed by Mr. Abbay and Mr. Pye, *integrating* spectroscopes, since they sum up in the spectrum which they show the total amount of light of each definite refrangibility derived from the whole luminous area in the field of the collimator. This field is, of course, a cone defined by lines drawn from the edge of the collimator object-glass through the center of the slit, (neglecting the length of the slit,) and indefinitely produced. All the luminous particles within this cone contribute equally to the spectrum, and the instrument gives no means of determining in what portion of the field any particular line of the spectrum originates.

* This term "leucosphere," first proposed, I believe, by Lieutenant Brown, at a meeting of the Royal Astronomical Society, last January, is in some respects objectionable, but as it is convenient, and I am not aware that any better one is in use, I employ it provisionally. If, as is quite possible, it finally turns out that the non-solar elements of the corona are only insignificant, the word will become unnecessary; but while the question is under discussion it is desirable to have a name for that portion of the coronal luminosity which all concede to be solar.

Mr. Lockyer would extend the term "chromosphere" for this purpose, but that word is so satisfactory as a designation of the red hydrogen stratum that such an extension hardly seems advisable—especially as in that case we must invent some new name for the hydrogen atmosphere.

But on the other hand, when a spectroscope is attached to a telescope in the ordinary manner, the object-glass forms a definite image upon the slit when the instrument is accurately adjusted, and the spectrum seen is simply the spectrum of that elementary portion of the luminous body whose image falls between the jaws of the slit.

Used in this way, I call the instrument an *analyzing* spectroscope in antithesis to the other, since in this case we virtually separate the luminous area into its elementary portions and examine the spectrum of each portion by itself.

Now in the analyzing instruments, as nearly as I can estimate, the C line is from 25 to 100 times as bright as 1474, the light-ratio being about the same as that between a star of the first magnitude and one of the fifth or sixth. If we write 50 for the ratio I think we cannot be far from right.

In the *integrating* instruments, on the other hand, according to the observations of Professor Pickering, in 1869, and of Mr. Pye, above reported, (with which also agree the observations of Mr. Abbay so far as they go,) the 1474 line is the brightest visible. Mr. Pye gives for the brightness of 1474 the number 10, and on the same scale calls C 8.5; this would indicate between them a light-ratio of about $\frac{100}{72}$ in favor of 1474.

If then we suppose the corona and chromosphere each to be of uniform brightness throughout, we must conclude that the angular area (square minutes) subtended by the leucosphere is the greater in the ratio of at least $50 \times \frac{100}{72}$ to 1—that is, the solar corona is about 70 times as extensive as the portion of the chromosphere visible during the eclipse.

And if we were to take into account the fact that the chromosphere is more nearly uniform in its brightness and more definitely bounded than the leucosphere, it is evident that this ratio would be somewhat increased, since the comparison between the C and 1474 lines is made with the analyzing spectroscope very near the sun's limb, where the brightness of 1474 is far above its average—where only, indeed, it can be seen at all except during an eclipse. From comparisons of D_3 and F with 1474 we also get results substantially accordant with the above.

It is difficult to estimate accurately the aggregate area of the prominences and chromosphere visible during the eclipse; it was perhaps equal to a ring 9″ or 10″ high around the sun. If so, the leucosphere would appear to have an average elevation of about 10′, a result which agrees pretty well with the photographs and drawings, nor is it inconsistent at all with the idea that the long rays and fainter outside radiance may also be solar appendages.

But with reference to these rays, both bright and dark, the indications at present do not seem to be at all decisive. On the one hand the remarkable agreement between the photographs taken by our party in Spain, and those of Mr. Brothers, in Sicily, is an exceedingly strong argument for their solar origin. On the other hand, however, we have indications that point almost as strongly to a cislunar source. For instance, the curious appearances presented by Lord Lindsay's photographs, and the fact that in comparing different stations along the track of an eclipse we find that at some (as for instance Sioux City and Shelbyville, Kentucky, in 1869) these rays are conspicuous, while at others (Burlington and Springfield, in 1869) they are not seen at all.

Should it turn out that they are only visible at stations where the air is more or less hazy and turbid, they must naturally be considered as atmospheric phenomena, produced, of course, not by true photospheric sunlight, (which, as has been abundantly shown by many writers, cannot illuminate the air near the moon's place,) but by the light from the prominences and the lower regions of the leucosphere. If, on the contrary, as seems to be the case, this radiance appears under unexceptionable atmospheric conditions, we are almost shut up to one of two theories: either on the one hand that of Professor Norton and Mr. Proctor, whose views regarding these rays are nearly identical, and represent them to be streams of matter, similar to cometary substance or auroral beams,*

* Since my name has sometimes been referred to in connection with the so-called "auroral theory of the corona," it is perhaps proper for me to state that I make no claims to its origination. So far as I know, Professor Norton, of Yale College, was the first to publish a connected theory of the subject, basing his conclusions largely upon his discussions of Donati's comet, published some years ago in Silliman's Journal.

Professor Winlock also informs me that he has held and published a very similar opinion, and so, I believe, have driven by solar repulsions; or, on the other hand, that of Oudemans, who considers them to be purely optical effects produced in cosmical dust between us and the moon by the sunlight streaming across the uneven and ragged edge of our satellite. Evidently the subject requires careful and patient study for its elucidation.

NATURE OF THE CORONAL ENVELOPE AND ITS RELATION TO THE SUN.

Another interesting series of problems relates to the substance composing the leucosphere and the relation of this envelope to the sun—whether it is a true solar atmosphere or a mere cloud of transient particles—a flock of meteors, as Mr. Proctor supposes.

Waiving the difficulty of supposing such a multitudinous and continual supply of meteoric matter as the theory would require, it apparently fails in accounting for the peculiar form assumed by this envelope, which seems to be deepest over just those regions where the spots are most numerous, and to be governed even in the minutiæ of its outline by the arrangement and magnitude of the prominences.

I find, also, another great objection to it in the powerful winds and cyclones which prevail in the regions above the chromosphere at elevations as great as fifty, and even one hundred thousand miles. These winds, by which the tops of the solar flames are whirled and driven, present, so far as observations now go, every characteristic of true aerial currents in a continuous medium; and the whole appearance and behavior of the protuberances, except at the moment of eruption, is that of clouds floating in an air. It is impossible to conceive any more exact resemblance than that which exists between these objects and the lighter clouds of our own upper atmosphere.

But if we then consider the leucosphere as a true solar atmosphere, how can we reconcile its enormous extent with the smallness of the pressure at its base, which seems to be established by the experiments of Lockyer and Frankland, as well as Wüllner?

There seem to me to be two possible explanations of this: first, and perhaps on the whole most probable, this atmosphere may consist of some new kind of matter whose density is far below that of even hydrogen; or, on the other hand, it may be composed of matter whose specific *gravity* (*not density*) is diminished, annihilated, or even rendered negative by some solar repulsion, such as appears to be operative in the formation of a comet's tail.

It appears to be certain, from the observations of Angstrom and Kirchhoff, that the 1474 line which characterizes its spectrum coincides with a line in the *iron* spectrum within the limits of any present means of observation; and so close a coincidence can hardly be accidental. Yet in the spectrum of iron this line is only a faint and unimportant one, one of the last to make its appearance under the stimulus of the electric spark, and so little conspicuous that Mr. Huggins has failed to map it.

It is, to say the least, very difficult to understand how, if this line be really of the same origin as its fellows, it should remain the sole survivor of changes which have exterminated all its stronger associates; accordingly it becomes natural to suppose, as I suggested in 1869, that in the spectrum of iron this line may be due, not to the iron itself, but to some associated substance, (possibly related to the peculiar magnetic properties of this remarkable metal,) to some occluded gas which can also exist free in a state of inconceivable tenuity, as we have it in the leucosphere, and probably also in the streamers of the aurora and the tails of comets—a near relative, so far as gravity is concerned, to the luminiferous ether and to the Urstoff of German speculators.

With this view I believe Mr. Lockyer agrees, so far, at least, as to think the leucosphere composed of a new form of matter.

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several European astronomers. My own father, more than twenty years ago, was accustomed to teach essentially the same thing; so that when in 1869 I discovered (as I supposed) the coincidence of the bright line in the corona spectrum with a line in that of the aurora and declared my belief in the essential identity of the phenomena, I considered myself as simply subscribing to a view already current, and bringing a strong argument to its support.

I may add further that a careful observation of the auroral spectrum with the best means I have yet been able to command, and an examination of the observations of others, have convinced me that while thus far nothing appears which is inconsistent with the absolute coincidence of the two lines, still it cannot be considered to be established.

I think the probable error of the position of this aurora line, which is an exceedingly difficult object, and has yet been observed only with single-prism spectroscopes, must amount to five or six divisions of Kirchhoff's scale.

But on the other hand it is certain that alterations of temperature and pressure produce great changes in spectra; and although I think no case is at present known where, in the course of such changes, one of the least conspicuous lines is the most persistent and the last to disappear, it is perhaps not impossible that this may be the case with iron, and that in the upper solar atmosphere the complicated spectrum of the metal may reduce to this single line.

It is certainly a curious circumstance, perhaps favoring this idea, that the other iron lines which from time to time appear in the chromosphere spectrum, are nearly all of about the same order of intensity as 1474; the more conspicuous iron lines like E and G are never reversed, while in the case of other substances their strongest lines are the first to become bright.

If, then, we admit a sufficiently repulsive force, it seems still possible to suppose that the leucosphere may consist of *iron* in the state of vapor and fog; and the well-known wide diffusion of this substance in meteoric matter makes it comprehensible how its lines should occur in the spectrum of our own aurora and in any other places where they may be found. It would seem that we must look to the physical laboratory for light upon this subject.

CONSTITUTION OF THE SOLAR ATMOSPHERE.

From what has been said it is perhaps evident that I am disposed to accept a very simple view of the constitution of the solar atmosphere.

Without discussing the nature of the body of the sun, I may perhaps venture to assent to the views of Zöllner, who considers that the phenomena of the protuberances almost demonstrate the existence of a "*Trennungs-Schicht*," or *crust*, either solid or liquid, through which from time to time burst out these masses of incandescent hydrogen.

Over this, as it seems to me, lies the atmosphere, composed of vapors and gases, *each arranging itself or tending to arrange itself (according to the views of Dalton,) as if it were the only one in existence.*

That is to say the *magnesium* atmosphere is approximately of such elevation, and of such density at each elevation, as it would be if the only atmosphere of the sun were so much magnesium vapor as now exists there; it being supposed, of course, that by some extraneous means the surface of the sun be kept under the same pressure and at the same temperature as at present.

According to this view there is near the surface of the sun a certain layer, probably less than five hundred miles in thickness, which contains all the gases in intimate mixture; this is the birthplace of the Fraunhofer lines, and I suppose I obtained a glimpse of it at the moment when totality began.

Ascending, we successively pass the limits above which the different gases do not rise, these limits being lowest for the vapors of greatest density; the hydrogen and the unknown D_3 element, on account of their lightness, reach a much higher level than the others, while far above even these towers the coronal matter.

Of course I do not intend to ignore the enormous vertical and horizontal movements which agitate this atmosphere, originating, mainly, it would seem, in forces acting from beneath, giving to the upper surface of the chromosphere a form as irregular and fantastic as a sheet of flame, elevating its general level to some extent, and often carrying up magnesium, sodium, and iron to the very summit of the prominences.*

Considering also the close and immediate connection between unusual disturbances on the solar surface, and magnetic storms on the earth, it is altogether probable that this wild commotion is accompanied by a development of electric force abundantly sufficient to account for all the observed resemblances between the corona and the electrical phenomena of our upper atmosphere.

* There is another view of the solar atmosphere which may perhaps be tenable. Since it is unsafe to take the non-detectibility of a substance by the spectroscope as a proof of its absence, it is perhaps not impossible to assume that the solar atmosphere is roughly homogeneous throughout; only in this case we must also assume the very doubtful fact that the denser gases lose their luminosity at higher temperatures, and consequently at lower levels. Accordingly when we find the sodium lines reversed in a prominence, it would indicate, not the bringing up of sodium from a lower level, but the raising of this portion of the solar atmosphere to a higher than usual temperature.

But I do not see how we can reconcile a homogeneous atmosphere of such elevation with the undisputed smallness of the pressure at the sun's surface, to say nothing of other difficulties hardly less serious.

I am aware that there are objections to the theory I have indicated, arising from the phenomena of the diffusibility of gases, but so far as I can see they are not conclusive against it. One thing is certain, that too much caution can hardly be used in applying conclusions derived from laboratory experiments to the solar atmosphere, where the circumstances are so widely different.

SUGGESTIONS WITH REFERENCE TO THE OBSERVATION OF FUTURE ECLIPSES.

At the request of Professor Peirce I annex the following notes with reference to methods of observation, and points to which special attention should be directed in future eclipses.

A.—*Photography.*

1. Photographs of the partial phases by Professor Winlock's method, with a lens of long focus, for the purpose of determining the relative positions and motions of the centers of the sun and moon.

2. Photographs of the corona made with a telescope of large aperture (30^{cm}, if attainable,) fitted with a lens near the eye-end, like the Barlow lens, only *convex* instead of concave. This lens to be of such focus and so placed as to reduce the diameter of the solar image to about 7.5^{mm}, or 1^{cm}.

This lens can be made to improve the corrections of the object-glass for the actinic rays. Great care should be taken in adjusting the diameter of this lens, and the size and position of all stops and diaphragms, not to interfere with light coming from any point within $1\frac{1}{2}°$ of the sun.

With an object-glass of the size mentioned we might expect, I think, to get a strong picture of the whole coronal region, with an exposure not exceeding 5 seconds, and a series of such pictures would infallibly settle the question as to how much of the phenomenon is cis-lunar.

3. As a substitute for the above, photographs taken with a camera of wide angular aperture carried by an equatorial mounting and clock-work; or simply strapped to the tube of an equatorial, which could be in use for viewing or sketching, without interfering with the photography.

4. Photographs of a prominence highly magnified, the image being thrown up by an eye-piece to a scale of from 5″ to 15″ per millimeter, so as to give the details. This would, of course, require a large telescope, with accurate driving-clock, and perhaps is hardly of sufficient importance to justify the expenditure.

B.—*Spectroscopic observations.*

1. Determination of the instants of first and last contact.

2. Careful examination of the cusps to see if the moon's limb in any way modifies the spectrum. Brushes of red light have been reported by some observers as appearing at the cusps; if so, what is their spectrum?

3. Close to the sun's limb look for a layer in which the Fraunhofer lines originate. Just before totality it should give a nearly continuous spectrum, and just at the moment of totality should show all the dark lines of the spectrum (excepting of course those of terrestrial origin) reversed. The spectroscope employed cannot have too high dispersive power. The image of the sun should be not less than 1 inch in diameter, and since the thickness of this layer does not exceed $\frac{1}{1000}$ of the sun's diameter, evidently the most critical care must be exercised in reference to all the adjustments for focus, &c., and the slit should be very narrow.

4. Determine the thickness of this layer by noting how long the lines continue bright. This will require a chronograph capable of being read to $\frac{1}{100}$ of a second.

NOTE.—It is possible, as has been stated before, that no such layer exists, and that the phenomenon seen by Mr. Pye and myself was due to some unusual disturbance of the sun's surface.

5. Examine the prominence spectrum carefully for new lines just before and after totality, when the atmospheric glare is greatly reduced. Examine specially the upper part of the spectrum above F.

If there are observers enough to attend to other points, continue the observation through totality, particularly noting whether any diffuse band appears over *b* (not higher up in the spectrum, but replacing b at greater distance from the sun,) and whether the F line runs higher than the other hydrogen lines.

The upper part of the prominence spectrum deserves, and will probably repay, careful study, and is very difficult to deal with except during an eclipse.

Use a spectroscope of high dispersive power.

6. With a similar spectroscope having the slit *slightly* widened, examine the spectrum of the corona for new lines, especially between 1474 and D_3.

The telescope employed should be of large angular aperture, giving an image of the sun about 1^{cm} in diameter.

7. With the same instruments interpose a Nicol's prism in front of the slit, and note the effect, if any, upon the spectrum of the corona when the Nicol is rotated; but any results thus obtained must be carefully considered and checked, being complicated by the polarization produced in the refraction through the prisms. Place the slit at several different points and different angles with the sun's limb.

8. With the same instruments (except the Nicol) but with *widely* opened slit, study the "dark rays;" if they are really channels in the monochromatic leucosphere their outline will be visible through the slit in the same manner as the forms of the prominences. It is hardly necessary to add that all the spectroscopes used in the above-mentioned observations ought to be *automatic*, and provided with some accurate registering apparatus.

9. Examine the appearance of the eclipsed sun with a "meteor-spectroscope"—having no collimator or slit. So far as the corona is monochromatic it will be *distinctly* seen notwithstanding the prism, while those portions of it which shine only by reflected sunlight will be indistinct, their light being dispersed. Even without a telescope the same object may be attained, to some extent, by merely looking at the corona through an ordinary prism or a direct vision combination.

10. Repeat the observations with the integrating spectroscope. There are some curious discrepancies between the observations of Mr. Abbay and Mr. Pye which need to be cleared up.

C.—*General telescopic observations.*

1. During the partial phase look for the projection of the moon beyond the sun's limb and for brushes of light at the cusps.

2. Notice whether the coronal radiance shifts from one side of the moon to the other during the totality, using considerable aperture but very low magnifying power.

3. In order to secure accuracy in sketching, insert in the focus of the object-glass a transparent plate divided into squares, or marked in some other systematic manner, and use a paper marked off to correspond.

4. Look for spots of light on the moon, and examine carefully the gradation of light from the limb toward the center during totality.

5. Look for intra-Mercurial planets.

6. With all the telescopic power available examine the structure of the corona to ascertain whether it is made up of filaments; and, if so, whether they are straight or curved; and, if curved, whether they are *concave* or *convex* toward the sun.

7. Observe carefully whether there are any *nuclei* in the corona—anything like "resolvability" of a star-cluster.

D.—*Naked eye or field-glass observations.*

1. Changes in the corona, such as shifting of the light from one side to the other of the moon, alterations in the position of the dark and bright rays, &c.

2. Observe whether the external radiance, into which the rays appear to reach out from the leucosphere, presents any distinguishable outline; and, if it does, of what character.

3. Look for narrow dark and light bands moving over the surface of the earth just at the moment of total obscuration.

4. Notice what portion of the sky is darkest, and how the light varies from this to the brighter portions.

E.—*Physical observations.*

1. With a thermopile and galvanometer measure the total radiant heat from the corona, and test it for "quality" by interposing various transparent screens of known thickness. Compare it with the heat received from the uneclipsed sun by means of a vessel of boiling water or some other constant source of heat.

2. Forming the image of the corona in the focus of a telescope, test the different portions of the image with a minute thermopile of a single pair, in order to ascertain the relative intensity of the heat from its different parts. Especially ascertain how much more heat comes from the prominences than from neighboring portions of the corona.

With respect to polariscopic and photometric observations I have nothing to offer.

With the greatest respect, I am, dear sir, yours very sincerely,

C. A. YOUNG.

Professor J. WINLOCK.

POINTS TO BE SPECIALLY ATTENDED TO IN THE OBSERVATION OF FUTURE ECLIPSES.

A.—*Photography.*

1. Photographs of the sun during the partial phases of the eclipse, with a lens of from 8 to 16 meters focal length, according to Professor Winlock's plan, for the purpose of determining the relative position of the centers of sun and moon.

2. Photographs of the corona made with an equatorial of large aperture (25 to 30^{cm} if possible) fitted with a lens near the eye-end like the Barlow lens so-called, except in being convex instead of concave. This lens should be of such focal length and so placed as to reduce the diameter of the solar image to less than 1^{cm}, and could be made to improve the corrections of the object-glass for the actinic rays. Special care must be taken to avoid interference with the impression of anything within three or four degrees of the sun by any stops or diaphragms. The object, of course, should be to secure such a series of pictures as will show whether any changes take place in the coronal streamers; and, if so, whether they stand in evident relation to the moon's motion and the inequalities of her limb.

3. Photographs with an ordinary camera strapped upon the tube of an equatorial driven by clock-work, or else arranged with its optic axis parallel to the axis of the earth, and having the light thrown in by the flat silvered mirror of a Meyerstein heliostat, I think not so good as the preceding.

4. Photographs of the most remarkable prominences, on a large scale for the purpose of studying their details. The image of the sun should be thrown up by an eye-piece to a diameter of from 15 to 20^{cm}, *i. e.* the scale of the photograph should be about $10''$ to the millimeter. This would of course require a large telescope.

B.—*Spectroscopic observations.*

1. Observations of the instant of first and last contact by means of the occultation and re-appearance of the chromosphere.

2. Careful examination of the cusps during the partial phase to ascertain if the moon's limb appears to modify in any way the spectrum of the chromosphere. Brushes of red light have been reported by some observers as appearing at the cusps; if so, what is their spectrum.

3. Look for a stratum close to the limb of the sun giving a nearly continuous spectrum just before the eclipse becomes total; and at the moment of totality giving a spectrum in which the dark Fraunhofer lines are all reversed.

Pretty high dispersive power, and a very accurate adjustment of the slit in the exact focus of the collimator, are essential; also care in placing the slit exactly tangential to the solar image, and precisely in its plane.

The observation is important because, though unlikely, it is certainly not impossible that some unusual chromospheric storm, such as Mr. Lockyer has once seen, may have produced the phenomenon observed by Mr. Pye and myself.

4. If the stratum is found, determine the precise duration of the reversal of the lines at the commencement of the totality by means of a chronograph, and repeat the observation at the re-appearance of the sun, in order to ascertain the thickness of the layer.

5. During the partial phase, especially near the time of totality, examine with the highest dispersive power available the more refrangible portion of the spectrum for new prominence lines. If an observer can be spared, this ought to be done also during the totality. This upper portion of the spectrum needs to be much more thoroughly studied than has yet been done.

6. With a spectroscope of high dispersive power attached to a telescope of large angular aperture, giving an image of the sun not more than 1^{cm} in diameter, examine the spectrum of the corona for new lines, especially determine whether there are any between D_3 and 1474. For this purpose the slit may be slightly widened. Also note the extension if any of the hydrogen lines above the chromosphere and upon the moon's disk.

7. With the same instrument and widely opened slit search for monochromatic radial beams; if any such exist they can be seen through the 1474 line in the same manner as the prominences are studied through C and F. In this way the structure of the corona will also probably come out more distinctly, being cleared of the diffuse light from other sources.

8. Place a Nicol's prism in front of the slit of the instrument, and note the effect, if any, of its rotation upon the spectrum of the corona. But on account of the partial polarization of the light in its refraction through the prisms, any results thus obtained must be received with reserve, and carefully checked.

9. Examine the appearance of the sun through a so-called meteor-spectroscope, (having no slit or collimator.) So much of the corona as gives the monochromatic light will be *distinctly* seen, while the rest will be made indistinct. The same object may be obtained by looking at the sun with the eye naked, and armed with a small telescope, through a prism, or, better, a train of 5 or 6 prisms.

10. Repeat the observations of Professor Pickering in 1869, and of Messrs. Abbay and Pye in 1870, with an integrating spectroscope, *i. e.*, a simple chemical spectroscope unattached to a telescope. There remain discrepancies which need to be cleared up. It is *exceedingly* important that in all cases when possible the observer of the spectroscopic phenomena of totality should have had his eyes carefully prepared by previous seclusion in darkness for some 4 or 5 minutes.

C.—*Polarization.*

I leave this subject wholly to Professor Pickering and others.

D.—*Photometry.*

I have nothing to offer.

E.—*General telescopic observations.*

1. Look for the projection of the moon beyond the disk of the sun, and for brushes of light at the cusps.

2. With considerable aperture and very low power notice whether the coronal radiance shifts from one side of the sun to the other during the totality.

3. For the purpose of securing accuracy and rapidity in sketching, use paper previously divided into compartments on some convenient plan, and in the focus of the telescope (using, of course, a positive eye-piece) insert a piece of plane glass or mica marked in the same manner.

4. With considerable power study the base of the corona to ascertain, if possible, whether the curvature of the filaments is *convex* to the sun, indicating a *repulsive* force.

5. With all the telescopic power at command look for *nuclei* in the corona, or for any signs of meteors or comets in the sun's immediate neighborhood.

6. Possibly it is worth while to continue the search for intra-Mercurial planets.

7. Observe differences of color between the different parts of the moon's disk, or bright spots upon it.

F.—*Naked-eye observations.*

1. Note any changes that occur in the appearance of the corona during the totality, and differences of color in its different parts.

2. See if the outer boundary of the corona is like the boundary of a *cloud*.

3. Look for the dark bands reported to move over the surface of the earth at the moment of totality by Secchi and others.

4. Note what portion of the sky is darkest during the totality—it should be a *ring*.

G.—*Physical observations.*

1. Measure the radiant heat from the corona with a thermopile and galvanometer without a telescope and ascertain the effect of interposing different transparent screens of known thickness, (*e. g.*, a screen of glass, a screen of quartz, a screen of alum, &c.,) in order to ascertain the *quality* of the heat.

2. With a linear thermopile (like that used in Rosse's experiments upon the moon) explore the image of the corona formed in the focus of a telescope in order to ascertain the relative temperatures of its different portions.

3. Having suspended a small magnet by a wire in such a manner that it shall be maintained at an angle of about 30° with the magnetic meridian, observe (by means of a mirror attached to the magnet, and a telescope with a scale) whether the magnet *twitches* at all as the moon in its progress covers or uncovers spots and prominences, and especially whether it experiences any unusual disturbance at the beginning or end of totality. (I do not expect any.)

ALLEGHENY OBSERVATORY,
Allegheny, Pennsylvania, April 15, 1871.

SIR: I have the honor to submit the following report of the observations made by me, with the party under your charge, at Jerez, Spain, on the solar eclipse of December 22, 1870.

Together with Captain Ernst, Professors Young and Pickering, Mr. Ross, and some members of the Sicilian party I left Southampton, England, on the 26th of November, in the Peninsular and Oriental Company's steamer Poonah, and reached Gibraltar on the evening of December 1. Here we staid some days awaiting a boat for Cadiz, and as it seemed uncertain when the regular steamer would leave, Captain Ernst and I started in advance of the rest of the party in a small Spanish vessel, reaching Cadiz on the 8th and Jerez on the 9th of December.

I had limited the instruments taken with me, by your advice, to a small portable telescope, a Savart's polariscope, and to a polarizing solar eye-piece, and to these was added at Jerez an equatorial telescope of 4 inches aperture, without clock-work or circles, the property of the Harvard College observatory. This instrument had been fitted with a small spectroscope, which was removed some days before the eclipse. The instruments actually employed by me were the following:

1. The equatorial just mentioned. It has a good objective, which I have frequently used on close double stars with high powers, the images being sharp and free from diffuse light. Tried by all usual tests it is a more than ordinarily good glass, and if no positive results were obtained by the direct study of the coronal structure with it, it was due to no defect of the instrument. The mounting is by Troughton & Simms, but the tube was for the occasion bolted to an equatorial stand of iron, made by Messrs. A. Clarke & Co. for another telescope. This iron stand rested on a pier which I had made of the only accessible material: two pieces of joist sunk to the depth of three feet below the soil, surrounded by well-rammed earth, and united at the bottom and at the