hardly persuade myself that totality had set in.* A bright star in the southeast was noticed by bystanders. At $13^h 59^m 5^s$ it grew lighter, but the totality must have ended some seconds before this, as the sun was at the time thickly covered by clouds. Cleared again partially at $14^h 30^m$, clouded up at $15^h 5^m$, and remained so until after the end of the eclipse. During the progress of the eclipse no regularity in the timing of the photographs could be preserved, as they had to be taken during the temporary clear intervals. The correction of the chronometer, Kessel, 1287, is $+ 6^h 7^m 39^s.4$ to Catania sidereal time.

The first contact of the eclipse, therefore, was observed at the Catania station at $18^h 40^m 04^s.4$ Catania sidereal time, or $3^s.9$ later than the time predicted by the data of the American ephemeris.†

The computed times I obtained as follows:

	h. m. s.		h. m. s.	
Beginning of eclipse	0 36 42.8	Catania M. T., or	18 40 00.5	Catania S. T.
Beginning of totality	2 01 23.0	" "	20 04 54.6	" "
Ending of totality	2 03 01.8	" "	20 06 33.7	" "
Ending of eclipse	3 20 27.2	" "	21 24 11.8	" "
Duration of eclipse	2 43 44.4	M. T.	2 44 11.3	S. T.
Duration of totality	1 38.8	"	1 39.1	"

A few transits of stars for time were observed before darkness set in. The instruments were taken to Messina, and left in charge of our consul, Mr. Behn, to be shipped to New York. We reached Boston in the steamer Tripoli, February 2, 1871, and on the 4th I reported for duty at the office here. The instruments arrived in New York in the steamer Anglia, on the 24th of February.

The records, original and duplicate, and the computations connected with the eclipse, are deposited in the archives of the office.

I remain, sir, yours, very respectfully,

CHARLES A. SCHOTT,
Assistant United States Coast Survey.

Professor BENJAMIN PEIRCE,
Superintendent United States Coast Survey,
And in charge of the United States Eclipse Expedition to Europe.

SIR: Having been invited by you to join in the United States expedition for observing the late eclipse, I sailed from New York in October last, in company with yourself and some other members of the party.

During the passage to Liverpool, reflection upon the shortness of the period of totality led me to reconsider the views I first proposed as to the plan of observation, and with your approval I concluded to undertake spectroscopic observations of the corona. I arrived in London on the evening of the 26th of October, and soon after I was placed by you in communication with Mr. J.

* A pistol was fired off at $13^h 57^m 11^s.5$, the *estimated* time of commencement of totality. The phenomenon itself was hidden by clouds.

† The predicted times for Catania, by Agnello, (see his pamphlet,) are as follows:

First outer contact	0^h	38^m	$18^s.6$	Catania mean time.
First inner contact	2	01	01 .1	
Second inner contact	2	02	38 .5	
Last outer contact	3	20	19 .5	
Duration of eclipse	2	42	0 .9	
Duration of totality		1	37 .4	

$\phi = 37° 30' 2''.1$; $\lambda = 3^m 19^s.8$ east of Naples, for Piazza del Duomo, which is east and south of the Coast Survey station.

His assumed geographical position differs but little from mine, and does not account for the defect in the predicted time of beginning, which is over $1\frac{1}{2}$ minutes too late. Similar differences exist for *Augusto* $\phi = 37° 13' 48''$; $\lambda = - 1^h 00^m 52^s.1$ from Greenwich. Beginning by American ephemeris data $0^h 37^m 38^s$; first inner contact, $2^h 02^m 18^s$; second, $2^h 04^m 06^s$; end, $3^h 21^m 26^s$, Augusto mean time. Agnello gives $0^h 39^m 17^s$, $2^h 1^m 57^s$, $2^h 3^m 47^s.5$, and $3^h 21^m 21^s$, respectively. Using the data of the English Nautical Almanac, the predicted times for *Catania* become $0^h 36^m 22^s$ and $3^h 20^m 08^s$ Catania mean time, for first and last contacts respectively.

Norman Lockyer, so distinguished for his spectroscopic discoveries in the sun. I wish here to express my obligations to him for his suggestions and attentions. I was by him introduced to the eminent optician, Mr. John Browning, of London. After consultation with the latter upon what I wanted, he engaged to make for me one of his direct vision sun spectroscopes. The construction of this and of all the other instrumental appliances for the occasion was at my own cost.

My aim was primarily to have the spectroscope so mounted upon, or in connection with, an equatorial moved by clock-work, that it could be revolved with ready freedom around the center of the sun's image, as a center of revolution, the slit of the spectroscope always spanning the coronal ring radially from the sun's limb outward, and of course sweeping the entire ring. This aim was, in great measure, already anticipated in Mr. Browning's arrangement. The only modification by myself in this respect was designed merely for greater security, both of the precision and of the perfect freedom, of the motion of revolution of the spectroscope system.

In case there should appear one or more condensed masses of white light, similar to the two noticed by myself in the eclipse of 1869, it was not improbable that such a condensation might be marked by a local comparative brilliancy in the bright line in the spectrum of the corona. And with sufficient power in the spectroscope, it was also a possible contingency, especially if such bright line coincide with a dark line in the skylight solar spectrum, that such local brilliancy of the line might be visible some minutes before and after the total phase, in the same way that Messrs. Lockyer and Janssen brought out the red hydrogen line in the absence of all eclipse. This might altogether give a considerable period of time, and it was thought desirable to provide a means for recording with approximate exactness the position of any such condensation of light at three several recorded times, in case it should appear in the spectrum some minutes before totality. And if this should be otherwise, I still very much desired to locate with some precision what should be seen in the total phase. These views I state now, partly by way of apology for having run the risk of failure by undertaking more than could be accomplished with ease and certainty in the time at my disposal. The consequence was that several untoward circumstances and unexpected accidents prevented the completion of the arrangements, so that when the eclipse came they were in large part not ready for use.

The parts brought actually into use for service on the eclipse consisted of the sun spectroscope, made for me by Mr. Browning, as above mentioned, of the Coast Survey 4-inch Dollond telescope, of 6 feet focal distance, and of a temporary equatorial mounting, on which both these were supported. This equatorial mounting was specially arranged for the circumstances of the eclipse in Sicily, and gave a direct rigid support in declination to the object-glass of the telescope. The stellar focus of the object-glass fell within two or three inches of the northern cylindrical bronze pivot of the equatorial axis, and upon this pivot was directly supported the chief one of the pair of bronze bearings, fixed in the telescope-tube, on which the spectroscope system could be freely revolved. This special and temporary arrangement was resorted to with the view of insuring the telescope against shake in manipulating the spectroscope, and this purpose it served effectually.

The angle of aperture of the collimator of the spectroscope was much larger than that of our 4-inch Dollond of 6 feet focus, having been first intended for a different telescope. In order, therefore, to enlarge this small angle of aperture of the telescope and make it fill that of the collimator of the spectroscope, there was mounted in the spectroscope system, so as to revolve with it, a combination of a small plano-convex lens of 1½ inches focal distance, with a small plano-concave lens of one-half inch focal distance, placed at a distance from the plano-convex equal to the difference of their focal distances. The pencils from the object-glass, traversing first the plano-convex and then the plano-concave, came to their foci at the slit of the spectroscope at something over one eighth of an inch beyond the plano-concave. In this way such of the telescopic pencils as traversed this combination, embracing a field of over 13 minutes in diameter, arrived at the slit of the spectroscope as if they had come from a 4-inch object-glass of 32 inches focal distance. Of course the introduction of these lenses was in itself objectionable, but for the occasion there was no other choice, since the gain in intensity of light by enlargement of the angle of pencil, and reduction to four-ninths in the linear extent of the telescopic image, was much greater than the loss by reflection of the four additional surfaces.

In view of the shortness of the time, I left the spectroscope entirely to Mr. Browning, the maker, confining myself to the arrangement of some of its accessories. The instrument proves to be one of the first order of excellence, but I will remark that it is not as large and powerful an instrument as I had in mind in designing my plans of observation. It contains two bundles of prisms, each bundle consisting of three prisms of Chance's heavy flint-glass, and four reverse prisms of crown-glass, all cemented together. The angle of dispersion of a like bundle tried at Mr. Browning's was found to be not far from midway between the angle of dispersion of one and that of two prisms of common flint-glass of 60°. The angle of dispersion of the two bundles is, therefore, supposed to be about equal to that of three prisms of common flint-glass of 60°. The breadth of the transmitted pencil of rays of any one refrangibility in nearly the whole of the visible spectrum, is, in the air, about four-tenths of an inch along the plane of dispersion, so that in analyzing power the two bundles together represent a common flint-glass prism of 60°, measuring 2 inches on its sides. As the Huygenian eye-piece furnished with the little telescope of the spectroscope was of somewhat low power, I had an extra Ramsden eye-piece of greater power provided, and have used it exclusively. With this the little telescope gives a power of about 9, being a little over 20 for one inch in effective width of aperture. The little telescope is mounted on pivots, so as to sweep the length of the spectrum.

Anxious to have the means of trying the effect of multiplying the dispersion of the instrument, I devised, and had made, an artifice by which the light could be passed three times through the two bundles of prisms. This would involve a reduction of one-half in the quantity of light by division of the pencil, and a further reduction from loss by reflections and absorption. In these losses the light of the continuous spectrum would share in common with any monochromatic light; besides this, the continuous spectrum would suffer from the trebled dispersion a threefold reduction of intensity, in which the bright line of the monochromatic light would not share. Whether the result would favor the eye in analyzing the latter from the former, would obviously depend on the sufficiency or insufficiency of the original intensity of the latter. There was not time before the eclipse to perfect the adjustment of this contrivance and remedy a defect which existed in it. It was, therefore, thrown out entirely, but it may be as well to describe it. It will recall the methods by which the English philosophers and our own Professor Young have made the light pass twice through the prisms. But in the element by which the light is passed through the third time, it is, I think, new, and though objectionable in placing glass surfaces very near a focal image, may yet receive other applications which I purpose to communicate on another occasion. For ease of verbal description, imagine the spectroscope to be placed with the line of collimation of its collimator, horizontal, and its plane of dispersion, or plane of sweep of its telescope, vertical, and, of course, the slit of the collimator horizontal. The whole half of the slit on the one side of the axis or line of collimation of the collimator is to be closed. The slit, for the remaining half of its length on the other side of the axis, is left open for the admission of light as usual. A pencil of rays of any one refrangibility, issuing from any one point in this line of light, will pass through the prisms as parallel rays and continue on in the usual manner until they arrive at the object-glass of the little telescope. But before the pencil enters the object-glass one-half of it meets a semicircular plane-reflector, attached to the end of the telescope, so as to cover one of the halves into which the object-glass is divided by a vertical diameter. And the plane of this reflector is normal to the axis or line of collimation of the little telescope. The other half of the pencil, going through the uncovered half of the object-glass, reaches the eye of the observer, who sees, deprived of half its light, the usual spectrum vertically spanning, say the right-hand half of his field of view. As the vertical width of the half pencil is undiminished, the increase of diffraction, it is supposed, will take place solely along the lines of the spectrum, and not at all across them. The same remark applies to the reflected half of the pencil, whose course it remains to trace. The little telescope can be moved in its vertical sweep until its axis and the normal of the plane-reflector have the same inclination to the horizon as the pencil of the one refrangibility, and this last has to them only its small inclination in azimuth. The consequence is that it is returned through the prisms and collimator in such a manner that all the pencils of the same refrangibility would form upon the closed half of the slit a reflected image of its open half, and extending above and below would be a doublymagnified image of the neighboring parts of the spectrum. But just before the reflected half pencils come to their several foci in the reflected double spectrum, they encounter, first the convex surface, and next the plane surface, of a plano-convex lens, whose stellar focal length is roughly equal to that of the collimator itself. The part of the lens before the open half of the slit should be cut away, to prevent the reflection and scattering there of a portion of the intense light of the slit. At the plane surface of the lens the half pencil enters, cemented to that surface, a right-angled reflecting glass prism. The right-angled edge of the prism, opposite to the side by which it is cemented to the lens, is set back between the jaws of the slit until it lies nearly coincident with the closed half of the slit itself. The lens causes any ray which comes from the center of the collimator object-glass to return to that center after its two reflections from the prism, and the half pencils which come from one side the vertical diameter of the object-glass are reversed horizontally and thrown to the other side on their return, and so, making a third transit through the prism, they pass through the open half of the object-glass of the little telescope, and the observer sees a trebly-magnified spectrum vertically spanning the left-hand side of his field of view as the primary one does the right.

It is to be remarked that the vertical spectrum which would be formed by the half pencils arriving at the right-angled prism is inverted, in consequence of the two reflections from that prism, and this is essential, since otherwise the third dispersion would be subtracted from the first two instead of being added.

For the equatorial motion an extemporized clock-work was devised, on a plan admitting of ready and easy construction, with due regard to precision, and a mechanic in London was employed to make it, under an engagement to have it ready by the 20th of November. When I came to leave for Sicily, in the beginning of December, part of this work was still not done, and part of the remainder required doing over. In consequence I had to take it with me incomplete, in addition to a supply of tools, with the hope that I might find time to finish it myself, at my station. But to my great vexation, on the day of the eclipse the clock-work failed to come into use, for the want of only a few hours more of time, and my only resource was an assistant to shift the instrument little by little in right ascension. For this service I had to rely on my interpreter, who, though unpracticed, soon learned to perform the duty in a manner more satisfactory than might have been expected.

The regulator for the clock-work was a pendulum suspended by a small steel wire several feet in length, so as to admit of motion in a circular orbit, maintained by a radius-bar at the end of the clock-train, in the manner now well known. Airy was, I think, the first to apply the pendulum in this way for this purpose. At the beginning of the clock-train was placed a brass windlass, some $1\frac{3}{4}$ inches in diameter, to be driven by a very small iron wire, acted on in its turn by a circular arc of wood, attached to the equatorial in such manner as to be almost connected with the object-glass of the large telescope. Into the clock-train entered also a brass wheel of some 7 inches in diameter, acting, by a second fine iron wire, upon a second brass windlass, one purpose of this being to mitigate the effect of inaccuracy in gear-teeth.

Had there been time, it was in contemplation to connect with this last-mentioned windlass by an extra iron wire a fillet of paper some 6 or 7 inches in width, which would thus be moved, at the rate of about an inch in one minute, over a small table at the hand of the observer. The width of the fillet was intended as a scale of heights above the sun's limb, at about one inch to one minute of arc. In case of the occurrence of any strongly localized mass of light in the corona, such as I myself saw in the elipse of 1869, and in case of its being traced by a powerful spectroscope in advance of the totality, the height from the limb, and the time, could both be instantly recorded together by a pencil-mark, against which, immediately after, could be entered, upon the fillet, the line or lines of the spectrum. I need hardly add here that the aim of this mode of observing would be, by repeated observation of the same object, to trace, if possible, the presence or absence of indications of planetary motion.

I had made at Mr. Browning's a micrometer arrangement of a glass prism of very small angle; but as this was only resorted to in the hurry of the occasion, on account of the facility with which it furnished a micrometer scale, mechanically transferable to the paper fillet, and is objectionable, on the score of its reflecting surfaces, I will not go into the details. As it failed to come into use, I need not say it was excluded on the occasion of the eclipse.

On the day of the eclipse I spent much of the forenoon in an effort still to get the clock-motion ready, the length of the pendulum having been calculated as nearly as practicable. But it became manifest that it was impossible, and I prepared to observe with such means as were ready. The large telescope having been set approximately to the declination of the sun's center, my Italian assistant in a short time learned to follow the sun's diurnal motion, at signal given, with very tolerable success. An extemporized brace was applied in such a manner as to hold the telescope free from shake. In the matter of adjusting the width of the slit of the spectroscope, I deferred to the judgment and great experience of Mr. J. Norman Lockyer, the distinguished head of the English expedition, who was at the same station, and who did me the favor to make the adjustment upon the skylight spectrum. This adjustment remained unaltered till after the totality. As already intimated, the slit of the spectroscope was placed radially to the sun. As the eclipse advanced, the spectroscope system, while clouds did not interfere, was revolved continually to and fro, so as to sweep with the radial slit the entire convex limb of the solar crescent, enabling me to watch the diurnal motion, and to make an occasional rectification of the telescope in declination. As the time of totality approached I began to carry the slit a little beyond either cusp, where nothing remained but the mixed spectra of the sky and corona, but I noticed nothing which I could not see in the skylight spectrum at any time. It may be remarkable that the red chromospheric line did not catch my eye. But in view of the fact that repetition of these trials was speedily cut off by clouds, and in view also of the possibility and probability that the very brilliant full sunlight spectrum left the eye out of condition by the persistence of its impression, I attach very little weight to this negative result. Had my arrangements been completed, the sun's image was to have been screened out without excluding the light external to its limb.

As it now became evident that the total phase was to be lost in an extensive mass of cloud, I turned from the instrument and gave myself to a general survey of the interesting scene around me. The sky in the west was clear to a considerable height above the horizon, of which I had an unobstructed view in that quarter. I watched for the rising of the dark curtain, an appearance mentioned as having been seen by some on the approach of the shadow in the eclipse of 1869. The phenomenon in question was not, however, perceived on this occasion, and subsequent reflection suggested that the smaller diameter of the shadow gave less reason to expect it. The change of the western skylight to the rich ruddy glow which it exhibited, with no distinctly noticeable change, through the whole of the totality, and with a degree of luminosity equal, for aught I can say, to that of evening twilight 30 or 40 minutes after sunset, was quite gradual. The illumination of the landscape around was, I should say, far greater than in the light of the full moon, though very different in the whiteness of the latter. Every object in the landscape continued to be seen with ease and distinctness, much more so than in the moonlight. It is not unlikely that some of this effect might be due to the absence of the deep shadows of moonlight.

At one moment during totality a glimpse of the corona came out through a partially hazy opening in the clouds for a period which I thought did not exceed two seconds. I think it will be to no purpose to state the impression which I got of it under such circumstances.

After the sun again came out, I now took the first available opportunity I had had to test the spectroscope on the full sunlight. Closing up the slit on the solar image I had the satisfaction to see the lines of the solar image come out with a splendor of definition that fully evinced the skill of the maker.

My station was in the gardens of the Convent Benedetti, at Catania.

Among the various arrangements contrived on the voyage to Liverpool, but which could not be attempted, I will briefly mention one in which the observer, without taking his eye from the spectroscope, can by a single movement instantly throw out the prisms and the slit, a reflector taking the place of the former, and disclose to the eye a full view of the object, with the position of the slit marked by a pair of spider-lines. I see that Professor Young has a different arrangement for accomplishing, with similar facility, the same purpose.

Respectfully submitted.

J. HOMER LANE.

Professor BENJAMIN PEIRCE,
Superintendent of the United States Eclipse Expedition.

WASHINGTON, *June* 30, 1873.

DEAR SIR: The following is my report on the observations of the total eclipse of the sun on December 21, 1870. In accordance with your directions I went to Catania to observe the eclipse. I was to have observed it with the same spectroscope and telescope with which I observed the eclipse of 1869, but fitted with Professor Winlock's attachment for recording the position of the spectral lines. Unfortunately, owing to a mistake with which I was in no way concerned, this instrument was dispatched to Spain, and the circumstance was not discovered in time to get the instrument or to go to it. Mr. Adams, of the English expedition, was good enough to lend me an eyepiece with a Savart polariscope. I fitted this to a telescope which I found among the instruments of our own party, in order to observe the nature of the polarization of the corona. I was stationed at the villa of the Marquis di San Giuliano, some three miles behind Catania, on the road to Etna. The weather was remarkably clear in the morning, but a storm blew up at the time of the eclipse, and it was raining during the total phase, at least at the beginning of it. Fortunately, a very small opening occurred in the clouds, so that the observations could be made, although under disadvantageous circumstances. I had previously tested the telescope for polarization, and found none perceptible. The plan was to set first upon the dark face of the moon and turn the polariscope so that the bands disappeared, and then observe the position angle (from the center of the moon) of that part of the corona on which the bands attained their maximum. I observed two parts of the corona, differing 180° in position angle, and found the plane of polarization to be about 6° from the radial position, being more nearly vertical. The parts observed upon were 65° and 145° from the vertex towards the east in position angle. The measures were made upon a part about six or seven minutes from the limb. The measures both of the position of the plane of polarization and of position angle were recorded by scratches upon the lacquer of the eye-piece, the edge of the polariscope affording the means of measuring the latter.

Yours, very respectfully,

C. S. PEIRCE.

Professor BENJAMIN PEIRCE,
Superintendent Coast Survey.

Report of Mrs. Charles S. Peirce.

DEAR SIR: My duty as a member of the United States Coast Survey Expedition to the Mediterranean for observing the eclipse of 1870, was to sketch the corona, and I will premise that my knowledge of drawing extends merely to outlinings and shading single objects. That is, I cannot group a landscape rapidly and effectively, but any one object in a landscape I believe I can copy with great accuracy.

After we arrived at Catania, Mr. Lockyer, of the English expedition, kindly lent me a copy of the twelve observations, more or less, which he had drawn up, as being important for the sketchers to make, and these I conned diligently. I also heard, through Professor Peirce, that Mr. Lockyer was advising his sketchers to practice from pictures of former coronas, pinned up on the wall, and to see how many outlines they could dash off in a given time.

I immediately acted upon this hint, but the only copy of a corona of which, as it happened, I could have the use, was one of a former eclipse by Padre Secchi, with what, I must think, exaggerated rays and streamers radiating out from it in all directions. This I pinned high up on my bed-hangings, and copied endless times, by first drawing circles on my paper, and quartering them,