

## LETTERS TO EDITOR

---

### THE SOLAR ECLIPSE OF AUGUST 30, 1905, AND MAGNETIC PHENOMENA.

I have read with interest Mr. Charles Nordmann's contribution<sup>1</sup> to the study of the effects produced on the magnetic declination by the total solar eclipse of August 30, 1905. Whilst appreciating, as it deserves, Mr. Nordmann's ingenuity, I must confess that the facts which he advances, p. 23, seem to me to be inconclusive, and to bear most probably a different interpretation from that which he supposes. Before explaining my reasons for this view, I should like to make some preliminary remarks.

Mr. Nordmann's curves, p. 22, have no claim to acceptance except as the embodiment of the figures quoted on p. 20. The unit of force employed is said to be only  $0.14 \gamma$  ( $\gamma = 1 \times 10^{-5}$  C. G. S.). Of the five stations dealt with, Stonyhurst has the lowest horizontal force, and there  $1 \gamma$  answers approximately to  $0.2$  of declination. Thus when Mr. Nordmann on p. 20 gives values to  $0.1$  of his unit, he is practically recording declination changes to  $0.003$  or less. It is unusual to measure declination curves to nearer than  $0.1$  (answering usually to  $0.1^{\text{mm}}$  approximately), and two observers will often differ by this amount in their estimate. Mr. Nordmann, it is true, has used not his actual curve measurements, but smoothed means of the type  $(a + 2b + c)/4$ ; but even taking this into account one cannot but regard the figures on pp. 20 and 21 as suggesting an accuracy at least ten times that ordinarily attempted.

The next point relates to the method employed on p. 19 for eliminating the disturbance element and the regular diurnal variation. As to the latter, only a general statement is made, but apparently a diurnal inequality was calculated from a series of days preceding and following the eclipse. It must, however, be borne in mind that, as well known, there is no such thing as a fixed regular diurnal inequality characteristic of a month or a portion of a month. If one calculates an inequality from the quieter days of a month, and then from all but the highly disturbed days, one does not get the same results. As the days immediately before and after the eclipse were more disturbed than their neighbors, the assumption that part of the change during the day of the eclipse may be ascribed to a regular diurnal inequality derived from certain other days of the month involves a greater element of uncertainty than if no disturbance had occurred.

<sup>1</sup> *Terr. Mag.*, V. 12, No. 1, 1907, pp. 15-26.

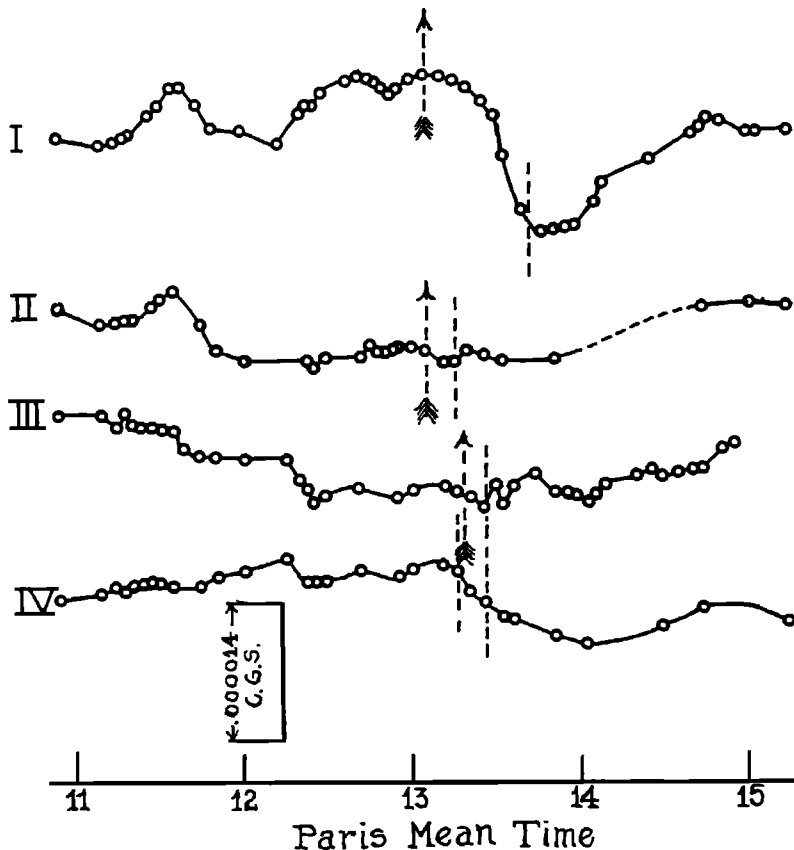
As to the disturbance element, the method of elimination, if I rightly understand it, would be satisfactory only if the disturbing forces perpendicular to the local magnetic meridian were at any given instant identical at all the stations. But the stations, it must be remembered, were in different latitudes and longitudes, possessing considerably different declinations, whilst Stonyhurst and Philippeville are over 2000 kilo meters apart. Taking these points into account, it would seem to me a very remarkable thing indeed if the diurnal variation and the disturbance effects were wholly eliminated from the figures on pp. 20 and 21.

Coming now to the data, at first sight there seems a truly remarkable coincidence between the time of occurrence of the minimum in the curves and that of totality at Philippeville, and again of the times of maximum and that of totality or central phase at the other stations compared. But it will, I presume, be allowed that if the phenomenon is an eclipse one, it should be repeated when we compare *any* two of the stations. I had the curiosity to make this comparison in some cases, and the results are exhibited in the accompanying curves. The first curve, I, differs from that of Mr. Nordmann for Stonyhurst and Philippeville only in that it is drawn through all the observation points. The others are obtained by taking the differences between Mr. Nordmann's columns on pp. 20 and 21. For example, Stonyhurst less Burgos  $\equiv$  (Stonyhurst less Philippeville) — (Burgos less Philippeville).

The eclipse was total at Burgos and Tortosa, and partial at Poissy. The portions of the three last curves near the time of totality are, it will be seen, wholly unlike the first of mine or any of Mr. Nordmann's curves. There is, it is true, a resemblance between curves I and II similar in kind to the resemblance between Mr. Nordmann's curves, but it occurs between the hours of 11 and 12. This same resemblance would, I may add, appear in any comparison curves in which Stonyhurst plays a part. The explanation which I should give is simply that the method of eliminating the disturbance element left largely uncompensated a movement on the Stonyhurst curve sufficiently pronounced to dominate between the hours of 11 and 12 any disturbance element left in the figures from the other stations. But if the methods employed leave an uncompensated disturbance element at Stonyhurst between these hours, may they not have equally left a similar element at Philippeville between the hours of 13 and 14 or 1 and 2 P. M.? Having regard to the Stonyhurst-Burgos, Poissy-Tortosa, and Burgos-Tortosa curves, I think the probabilities are that at least a large part—possibly the whole—of the apparent eclipse effect which the curves of Mr. Nordmann's fig. 3 show is simply the result of disturbance at Philippeville.

It will, I hope, be clearly recognized that the above criticisms relate only to Mr. Nordmann's methods and deductions. These methods may be unsound, and yet an eclipse effect may exist. But supposing an effect of some kind to exist, there is no obvious reason why it should attain a

maximum at the precise instant of totality or central phase, and alter very rapidly near that instant. If we accept the usual view that the diurnal inequality is due, at least mainly, to electric currents in the atmosphere, and that either the potential differences to which these currents are due, or the electric conductivity of the medium or both, depend



I (*Stonyhurst-Philippeville*); II (*Stonyhurst-Burgos*); III *Poissy-Tortosa*; IV (*Burgos-Tortosa*).

on solar radiation, it is natural to conclude that anything obstructing that radiation will produce some effect on the diurnal inequality. It must, however, be remembered that the regular diurnal changes, though most rapid during the day, do not show any symmetry about 12 noon, and are in progress the whole night through. Even the 24-hour term in the diurnal inequality does not generally have its maximum phase at noon. The increased activity of the diurnal changes at sunspot maxi-

70  
mum, to which Mr. Nordmann alludes, is not peculiar to the day hours, but in England at least, as I have shown, seems *relatively* most pronounced at night and in winter.

An increased conductivity of the upper atmosphere at the time of sunspot maximum is a view which I suggested myself some years ago, but the phenomena I have met with indicated that, so far at least as the diurnal inequality is concerned, the effect, however caused, persists for some time. Again it seems reasonable to suppose that the portion of the atmosphere where the hypothetical electric currents occur is at a height of the same order as, if not identical with, that where auroral phenomena exist. In temperate latitudes it is generally supposed that the seat of aurora is seldom less and usually considerably more than 50 kilometers. One would thus expect the diurnal inequality at any spot on the Earth's surface to be very appreciably influenced by currents whose horizontal distance is several hundreds of kilometers. Taking all these facts and the narrow extent of the band of totality of a solar eclipse into account, one would expect an eclipse effect, if one exists, to actually commence at a station before the eclipse begins there, and to endure some time after it ends, and not to show any specially sudden development at the precise time of totality.<sup>1</sup> The phenomenon will naturally of course rise to a maximum, and the time during which its existence can be detected must depend upon the sensitiveness of the instruments. Thanks to the initiative of the Department of Research in Terrestrial Magnetism of the Carnegie Institution of Washington, a very large number of magnetic observations must have been taken during the eclipse, so that further discussion of data is probable. If any one should follow Mr. Nordmann's procedure, I hope he will test its conclusions in the way indicated above before finally accepting them.

Richmond, Surrey, Nov. 27, 1907.

C. CHREE.

---

#### REGARDING THE MAGNETIC EFFECTS OF THE TOTAL SOLAR ECLIPSE OF AUGUST 30, 1905.

I am pleased that Mr. Chree's remarks on the subject of my article in this Journal (March, 1907) afford me the opportunity of returning to this interesting question, and I beg to make the following observations:

1. In the table on p. 20 (*loc. cit.*) I give for various stations the differences at each instant observed between the force which at each station tends to draw the needle to the east and the value of this same force at Philippeville. I give the values of the forces in terms of 0.1 of my unit which is equal to 0.147. Mr. Chree assumes that I guarantee the exactness of these values to the nearest 0.1 of this unit; one will find nowhere in my article any such contention. I have measured, as is

<sup>1</sup> This was shown by me to be the nature of the eclipse magnetic effect (Cf *T. M.*, Vol. V, p. 165 and Vol. VII, pp. 189-192.)—L. A. B.