

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Precursors of Magnetic Storms.

IN kindly noticing (NATURE, December 30, 1909, p. 259) my short account of the magnetic storm of September 25, attention was directed to the fact that what I called the precursor was experienced all over the world.

As I have little or no opportunities to compare our curves with others, may I be allowed to ask for information through the columns of NATURE?

I call a "sudden start" of the magnet a movement which occurs after several hours of perfect calm, and causes the trace to make a "sharp angle," that is, so sharp that I can unhesitatingly tell the time of the occurrence to the nearest two minutes. Many disturbances, large or moderate, have a "sudden start"—at least here.

My impression has long been that disturbances with a "sudden start" very generally have a kind of "preliminary tremor" some hours before the start; the curve, which we suppose to be quite smooth, is interrupted by a short movement, which lasts but a few minutes, after which the curve resumes its smoothness for the remaining hours. The tremor may be very small indeed, but the two characteristics, to be found on a smooth curve, and to be of very short duration, make it quite easy to point it out and tell the time.

The start of the "preliminary tremor" is in the same direction as that of the disturbance itself, at least as a rule.

I do not venture to hold an opinion as to the connection, fortuitous or otherwise, between the two phenomena, but I should be very glad to know whether the "precursor" is also observed in other countries. The following is the list of all the "sudden starts" of H during the last fourteen months of Zi-ka-wei (January, 1907, to February, 1908, inclusive), with the time of the start and that of the preliminary. A comparison with the curves of some other observatory is invited. Probably the traces will not be found so smooth as here, and some of my "sudden starts" will correspond to progressive starts, but on the whole I hope that a comparison will be possible.

I use Greenwich time.

	Sudden starts		Preliminary		Remarks
	h.	m.	h.	m.	
1. Jan. 8	16	45	Jan. 8	1 45	
2. " 11	8	45	" 10	3 10	H not quite smooth. D very smooth.
3. " 14	19	35	" 14	10 30	
4. Feb. 7	8	10	Feb. 6	23 55	
5. " 9	14	12			Curves not quite smooth.
6. " 14	4	37	" 13	19 47	
7. Mar. 10	5	3	Mar. 9	16 (?)	Curve not quite smooth.
8. " 11	17	23	" 11	13 45	
9. " 21	13	33	" 21	9 50	
10. May 18	13	58	May 18	10 52	
11. June 3	22	55			Curves not smooth.
12. " 18	3	42	June 17	17 20	
13. July 10	14	23	July 10	11 0½	
14. " 25	4	18	" 24	14 40	There is another preliminary at 3½. Curves not smooth.
15. " 28	0	12			
16. Aug. 14	15	3	Aug. 14	11 0	
17. " 20	2	24	" 19	13 30	
18. " 30	7	15	" 30	5 8	And also 29 at 11h. 12m.
19. Sep. 10	1	50	Sept. 9	15 0½	
20. Oct. 13	7	45	Oct. 13	0 0½	
21. Nov. 21	10	45			The curve of H was lost, until 10h. But D does not show anything.
22. Dec. 4	5	45	Dec. 3	20 57	
23. Jan. 27	13	48	Jan. 27	0 25	
24. Feb. 22	12	8	Feb. 21	19 20	

NO. 2113, VOL. 83]

If we put aside No. 21, we have twenty-three occurrences. In eighteen of these there is no doubt; in the three cases in which no precursor was found, and in the two more or less doubtful cases, the curves were not smooth, that is, the "start" was not quite sudden.

In Zi-ka-wei, during the fourteen months considered, the eighteen disturbances which began with quite a sudden start all had one or two preliminaries. The interval between the forerunner and the disturbance ranges from two to seventeen hours of perfectly smooth trace.

Zi-ka-wei, China, March 11.

J. DE MOIDREY.

Centre of Gravity of Annual Rainfall.

THE mere *a priori* criticism which Mr. Watt, in his letter to NATURE of April 14, has bestowed on my letter to NATURE of March 31, dealing with a large class of concrete physical facts, is, to my mind, far from satisfactory or sufficient. In the last sentence of my letter I anticipated that it might meet with some such "simple" algebraic criticism.

I did not, as is implied, assert that the use of the C.G. of the year's rainfall would dispense with the ordinary graphic representation of the monthly distribution, but that it appeared to be a convenient method for comparing the rainfall distribution at a number of stations in the same country for the same year, as well as at a single station, or for the mean rainfall of the same country, for a succession of years. Obviously, therefore, Mr. Watt's simple types of rainfall for his imaginary stations A, B, C, which belong to very different regions on the earth's surface, have no application, even in theory, to my "suggestions."

As for the practical character of my proposals, the following may be given. They are quite as useful as the comparison of the variation of rainfall with that of population for a decennium in India, which was the subject of a paper given some years ago in the Journal of the Royal Meteorological Society, London. A comparison of the variations in the C.G. of the mean monthly rainfall for the past 50 or 100 years with the agricultural results of those years in the British Isles, &c., for which the data may be available; an examination of these variations in connection with the much-discussed question of weather cycles, so commonly based on rainfall statistics; the detection of serious clerical slips in the tabulation of rainfall; the interpolation of the probable rainfall figures for a month in the event of a rain-gauge or measure-glass being temporarily unserviceable; the detection of the ignorant or inadvertent use of a wrong measure-glass—a matter of frequent occurrence in India, and possibly not unknown in this country.

Rain falls with such seeming irregularity of quantity and date, even in India—and much more so in this "unspeakable" Scotland—that it would appear *prima facie* impossible that there could be any approach to constancy of the date around which the whole year's rainfall balances. If we look at the tables of monthly rainfall for a large number of stations (in the same country) and see that the figures are not even approximately the same for the months or for the whole year, if we consider the difficulties connected with the measurement of rainfall, which are discussed in many of the volumes of "British Rainfall," it is surely surprising that, in spite of all these things, the reported year's rainfall should balance round a date which does not differ by more than a few days for a great many, if not all, the stations in any one year, and that for another year the displacement of this central date is so nearly the same for all of them. By merely looking at the monthly figures or at the graphs of those figures, we cannot accurately estimate either the central date or the amount of its displacement.

The causes which determine the times and amounts of rainfall for any place or country are known only in a very general way indeed. They are so elusive that investigators in their despair have even had recourse to sun-spots or comets' tails as a possible cause of special excess or deficiency of rainfall. They are so elusive that even in India, with its comparatively regular rainfall-seasons and with its special equipment of experts, the problem of