

containing Bombay, Batavia ( $107^{\circ}$  E.), Zikawei ( $121^{\circ}$  E.), and Melbourne ( $145^{\circ}$  E.). Omitting hours, here are the times in minutes assigned for the commencements at the three last-mentioned stations:—

37.0, 39.7, 8.7, 48.7, 52.7, 42.7, 20.7, 44.0, 4.7, 48.7, 46.7,  
18.7, 54.7, 32.7, 24.7, 45.7, 52.7,  
31.3, 32.3, 0.3, 45.3, 46.0, 35.6, 21.3, 38.3, -1.2, 41.8, 41.0,  
15.3, 50.3, 27.3, 20.8, 41.3, 47.4,  
38.4, 33.4, 5.1, 50.0, 55.1, 45.1, 25.1, 45.1, 0.1, 55.1, 45.1,  
15.1, 50.1, 35.1, 25.1, 45.1, 45.1,

The number of 7's in the first and second lines, of 3's in the third and fourth, and 1's in the fifth and sixth, tell a tale to anyone who has eyes. It is the treatment of the data, however, to which I would direct attention. Zikawei, though intermediate in longitude, differs usually in the same direction, as regards time of commencement, from Batavia and Melbourne. Its time is earlier than that of Batavia in sixteen cases, and than that of Melbourne in fourteen cases, out of seventeen; and the mean algebraic difference is no less than 5.2 minutes in the one instance and 4.6 in the other. Instead of recognising that data such as these are useless for any purposes of high accuracy, Dr. Bauer gets out means from his two central stations and obtains an average of between three and four minutes for the time of going completely round the earth, a time markedly less than the data employed give between Batavia and Zikawei, stations differing by less than  $15^{\circ}$  in longitude or  $38^{\circ}$  in latitude.

A final criticism I might mention is this: If disturbances travel round the earth in the way Dr. Bauer supposes, why do not they quite complete the circuit, or even go round several times? In some cases, at least, the amplitudes of commencing movements at stations nearly  $180^{\circ}$  apart are very similar, so that going half-way round can have had but little effect, and movements with a much reduced amplitude would still have been conspicuous. Earthquake tremors, we know, do go round more than once.

As regards the theory vaguely outlined by Dr. Bauer (pp. 10-11), I am not surprised by his claim that "the careful reader will not fail to observe that . . . [it] is considerably different from that which Dr. Chree imputed to me." But unless I had had prophetic powers, how could it have been otherwise? The earlier theory reached or was based on a definite mathematical equation (*Terrestrial Magnetism*, June, 1910, p. 123),  $v = XneD/p$ , where  $v$  is "the velocity of the moving ions . . .  $n$  the number of molecules in a c.c. of gas at a pressure  $p$  dynes per sq. cm.,  $e$  the electric charge carried by an ion,  $D$  the coefficient of diffusion of an ion through the gas." Of  $X$  Dr. Bauer tells us:—"Regarding the variation of  $X$ , some preliminary calculations would appear to indicate that the potential gradient of 1 volt per cm. assumed may be of about the right order of magnitude for the heights concerned" (75 km. is the height finally suggested). My primary difficulties were two. Sir J. J. Thomson, the authority quoted by Dr. Bauer, applies the formula to ions moving in the direction of the field  $X$ . Dr. Bauer makes  $v$  a horizontal velocity; but "potential gradient" is a term usually applied to the earth's vertical field, and 100 volts per metre is the value usually ascribed to it, as an average (probably a low one), at ground-level. Potential gradient, in the usual sense of the term, is known from balloon observations to fall to one-tenth of its ground-level value at heights of a few thousand feet, and at 75 km. it is usually supposed to be infinitesimal. If by "potential gradient" Dr. Bauer means a horizontal field, he should say so explicitly, and explain how it is produced and how he reaches his numerical estimate. As regards his concluding remark on the question of theory, "If Dr. Chree has something better to offer I shall be glad to know it," I labour under the disadvantage of holding the view—antiquated though it may appear to some of my contemporaries—that before advancing a mathematical theory for any supposed phenomenon, it is desirable to make reasonably sure that the phenomenon actually exists.

If we take Dr. Bauer's latest conclusions respecting the

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fifteen storms he deals with in NATURE, we have nine going east and six west. The mean algebraic difference between the European and American times of commencement, + denoting earlier occurrence in America, is only +0.11 minute. In view of the fact mentioned by Dr. Bauer that the algebraic mean difference between the times deduced by two skilled observers, Drs. Venske and Krogness, for the commencements of six disturbances at Potsdam—one of the best equipped of stations—was no less than 0.4 minute, I think most physicists of experience will recognise the expediency of awaiting something much more decisive before passing a final judgment.

March 4.

C. CHREE.

TOGETHER with Mr. O. Krogness, I am just on the way to Khartoum, in Sudan, in order to carry out some researches on the zodiacal light.

In connection with these investigations, we propose to secure some magnetic records with very sensitive apparatus. Special care will be taken to obtain as accurate a time-determination as possible.

For one set of observations we shall use an hour-length of 20 mm.; for another set, with twelve times greater hour-length, records will be obtained between about 5h. p.m. and 3h. a.m. Greenwich mean time.

If at other observatories, especially near the equator, similar rapid records could be secured in the same time-interval, these would, I think, be of value in deciding the question of the simultaneity or non-simultaneity of abruptly-beginning storms.

Our records are intended to commence on March 20, and will continue for one month.

Similar records will also be obtained by Prof. S. Saeland in Trondhjem.

KR. BIRKELAND.

Berlin, March 3.

#### The Centenary of Bunsen's Birth.

ON March 31 it will be one hundred years since Robert William Bunsen was born, and it has been felt that that occasion should not be allowed to pass without his pupils and admirers in this country giving expression to their veneration of the memory of one of the greatest chemists of our times. A committee has been formed to make the necessary arrangements, and it was intended to celebrate the centenary by a dinner. Unfortunately, the two most distinguished pupils of Bunsen, Sir Henry Roscoe and Sir Edward Thorpe, are at present prevented by indisposition from attending such a celebration. In these circumstances the committee has decided not to proceed with the arrangements for a dinner; but, feeling that the occasion should not be allowed to pass unnoticed, they have resolved to send a signed address to the Heidelberg University from old Heidelberg students in this country, and to place a wreath on the Bunsen monument. May we request those old Heidelberg students who wish to participate in this movement to send their signatures to Prof. H. B. Dixon, of the Manchester University, the chairman of the committee, or to either of the undersigned honorary secretaries?

FRANCIS JONES (Manchester Grammar School).

J. GROSSMANN (Plymouth Grove, Manchester).

Chemical Laboratory, 157 Plymouth Grove,  
Manchester, March 11.

#### Life and Habit.

YOUR correspondent "W. H. M." (NATURE, March 2, p. 12), who believes that it is necessary for newly hatched chicks to learn to eat by imitation, should see a litter of pigs being born. Each little pig the moment that he is outside hurries over the sow's hind legs, and, in the second second of his outdoor life, has a teat in his mouth. If the navel-cord has not got clear of his late home, he tugs away at it with all his might. Seeing such a sight, one might suppose that before birth the creature had been eagerly looking forward to his first breakfast. Or did the splendid prospect flash into his mind only as he found his feet?

FRANCIS RAM.

54 St. John's Road, N., March 13.