important of these conditions I take to be the equatorial position of the Aruwhimi basin; the second that it is situated in the heart of the continent. Both of these, but more particularly the former, determine it as the seat of ascending air-currents, and therefore of their dynamic cooling on a gigantic scale, and it is to this dynamic cooling that the high rainfall of the region is to be ascribed.

Very probably a considerable portion of the precipitated moisture is locally re-evaporated, so that, as suggested long ago by Sir John Herschel in the case of the Brazilian forest rainfall, the same water is precipitated again and again. There are not, I believe, in the lower atmosphere, any steady winds blowing outwards to carry away the evaporation of the damp forest tract, and the main loss of water to be supplied by easterly or other winds is that carried off by the river drainage, probably less than half of the rainfall. The air which has ascended to the higher regions of the atmosphere as a part of the main circulation of the globe, parts with nearly the whole of its vapour in the act of ascending.

We have a case in some respects analogous to that of the Upper Aruwhimi in the very damp and equally forest-clad province of Upper Assam. This too is characterized by a very calm atmosphere, being girt with lofty mountains on the north and east, and also shut off on the south and south-west from the Bay of Bengal by hills of considerable elevation. Such gentle winds as blow in the valley are chiefly from the east or down valley. Yet the rainfall is over 100 inches in the year, and the whole tract is one of marsh and dense forest. It is indeed not situated under the equator, and herein it is less favourably conditioned as a region of excessive rainfall than the basin of the Aruwhimi.

As the result of a long study of the rainfall of India, and perhaps no country affords greater advantages for the purpose, I have become convinced that dynamic cooling, if not the sole cause of rain, is at all events the only cause of any importance, and that all the other causes so frequently appealed to in popular literature on the subject, such as the intermingling of warm and cold air, contact with cold mountain slopes, &c., are either inoperative or relatively insignificant.

Folkestone, April 11. HENRY F. BLANFORD.

"Les Tremblements de Terre."

M. FOUQUÉ'S letter (NATURE, March 28, p. 510) does not meet the main points of my criticism of his book. He thinks that a pendulum swinging in synchronism with the ground's motion is the right thing to use as an absolute seismometer. M. Poincaré's mathematical note, to which he refers as supporting his view, does not support it, but shows why such a pendulum is unsuitable. It is necessary to emphasize this, for it relates to a fundamental matter in the dynamics of earthquake measurement—a matter on which the work done of late years in Japan seems to me to be so intimately based that a misunderstanding about it must be fatal to a proper appreciation of that work. And, in point of fact, I did not find that M. Fouqué gave an appreciative account of what any of the Japanese observers had done. As to his mention of Prof. Ewing's seismograph, in particular, I criticized it not so much because it was meagre as because it was incorrect,—so incorrect as to justify the inference that the author was not acquainted with that instrument.

THE REVIEWER.

Hertz's Equations.

MR. WATSON'S criticism, that Hertz's equations are only true for places at some distance from the oscillator, is no doubt perfectly valid. [There is, by the way, an insignificant and obvious misprint of λ for ρ about the middle of his letter.] But this was entirely recognized by Hertz himself; he treated the oscillator as infinitesimal, knowing that it was nothing of the kind when you got near it, and refrained from drawing his diagram-curves into its neighbourhood, for this very reason.

The fact is surely that, to work out completely the case of electric oscillators in a compound body formed of a couple of spheres joined by a cylinder, would tax the resources of a strong mathematician; and it is impossible that the vibration can be, in any sense, a pure one; all manner of sub-vibrations must be superposed upon the main.

From the physical point of view, some general notion of what was happening at a distance of a wave-length or more from the oscillator was desirable, and this Hertz satisfactorily obtained.

But, to work out what is happening in the immediate neighbourhood of a dumb-bell oscillator must be left, I imagine, to the time when some pure mathematician may devote his attention to this particular shape of conductor, if the case appears to him of sufficient interest. At present I see no special reason why it should be so regarded, but of that Mr. Watson is a better judge. I hope he may see fit to attack the problem.

Grasmere, April 13. OLIVER J. LODGE.

THE COMPRESSIBILITY OF HYDROGEN.

AS stated in the obituary notice that appeared in NATURE (vol. xxxviii. p. 598) at the time of the melancholy accident which caused his death, Wroblewski was engaged in an investigation of the behaviour of hydrogen on compression. The results of this investigation, as far as it had then advanced, have now been made public (*Monatsh. für Chem.*, 1888, p. 1067 *et seq.*). They are of a most important and interesting nature, and form a fitting memorial of the patience and skill of the observer, who most unhappily was not spared to bring this, the last and most complete of a long series of similar investigations, to a close.

Hydrogen has long occupied an exceptional and isolated position among gases. This is due to the fact that, as Regnault first pointed out, hydrogen forms the sole exception to the law that the product of the pressure into the volume, pv, of any gas decreases with increasing pressure, the exact converse being true in the case of hydrogen, this product showing a regular increase. It is true that, as since shown by Amagat and others, this behaviour of hydrogen becomes general for all gases when the pressure is increased beyond a certain limit, but before reaching this limit the product pv invariably decreases until a minimum is reached for all gases with the exception of hydrogen. For hydrogen neither the decrease nor the minimum have yet been observed, the gas as hitherto examined showing an invariable increase of pv with increasing pressure. The natural inference was, however, that the exception was only apparent, and that the minimum above noted would be found to occur also with hydrogen if the gas were examined at lower pressures than those hitherto investigated—that is to say, at pressures below one atmosphere. But a difficulty in the way of this hypothesis arises from the fact that the critical pressures of all gases are found to be *below* the pressure at which the minimum value for the product of pressure into volume occurs, and therefore on the above reasoning the critical pressure of hydrogen would have to be phenomenally low and considerably beneath one atmosphere.

To gain a further insight into the relation of volume to pressure in the case of hydrogen, Wroblewski decided to investigate this relation through a wide range of temperature. For this purpose he selected as temperatures sufficiently apart, the boiling-point of water, 100° C., the melting-point of ice, 0° C., the boiling-point of liquid ethylene, $-103°.5$ C., and the boiling-point of liquid oxygen, $-183°$ C. The pressures employed varied from one to seventy atmospheres.

The method of experimenting was exceedingly simple. The gas at a known pressure was forced into a bulb of known capacity having a capillary neck, and kept at one of the above four temperatures. A sufficient length of time was allowed for the gas to attain the fixed temperature; it was then transferred to a eudiometer, and its volume measured. It is needless to add that every precaution was taken both in purifying the gas and in applying the necessary corrections.

The results with the three first of the above temperatures agree with the behaviour of hydrogen already observed, the product of volume into pressure constantly increasing with the pressure. It was found that for the range of pressures under investigation (one to seventy