have been made. Further on the criticism reads, "I fail to see any adequate consideration of the variability of depth, of the absence of synchronism in the disturbing force in the direction of the canal." This "absence of synchronism" is precisely what the criticised equation 308 (or 311) enables us to take account of.

It seems to me that enough has been given in §§ 38, 42, 45, 55, and 63 to show that the variability in depth has not been permanently lost sight of, and also enough to convince one that "areas" as nearly uniform in depth as are many portions of the ocean can, as a first approximation, be treated as bodies having strictly uniform depths.

(2) Of course there are instances where the deflecting force due to the earth's rotation becomes important; for example, most moderately narrow arms of the sea in which the current is swift—such as the English Channel, Irish Channel, and Gulf of Georgia. But if in any of these a large stationary wave actually exists, it is hard to see how the times of its high and low waters near the loops can be seriously affected by this force, and these are the only times which chapters vi. and vii. undertake to determine. Near the nodes, when the current is swift, the deflecting force may, in a canal the width of which is but a moderately small fraction of a half wave-length, cause high water at one end of the nodal line, and at the same time low water at the other. This is true because the narrowness of the body permits its transverse slope to respond at once to the transverse forces. A progressive wave can be so superposed as to diminish or even destroy the range at one end of the nodal line while increasing the range at the other end.

Considering now a broader "area," with one or both of its lateral boundaries wanting, it is hard to see how the transverse motion occasioned by the earth's rotation can seriously interfere with the character of the stationary wave, and especially the time of elongation of the particles; for its effect cannot accumulate and so tend to produce a transverse stationary oscillation. If, on the other hand, a square or rectangular "area" about half a wave-length wide have solid lateral boundaries, it would seem that the deflecting force might, except in the equatorial regions, so alter the mode of oscillation that it could not be ignored even in the first approximation. So far as I know, there is no near approach to this case in any of the "areas" which probably exist (see Fig. 23 of my paper).

Hence, while it is true that the free oscillations in a rotating rectangular sheet of water is an unsolved problem, we see that the critic's remark, "It seems to follow that either Lord Kelvin or Mr. Harris is wrong," if in any sense true, really has very little to do with the case. In a word, taking an oscillating body as a whole, it seems to me that the oscillation, in accordance with a simple mode, can generally be regarded as the fundamental and important thing, and the effect of the earth's rotation a modifying or induced phenomenon.

(3) Now in regard to the improbability "that any large portion of our curiously shaped oceans should possess even approximately the critical free period," several things can be said. In the first place, we are not restricted to *single* half wave-lengths; the rectangular "areas" may run in any direction; the "areas" may be approximately trapezoidal, triangular, or of other forms, their free period may differ perhaps 10 per cent. or more from the period of the forces, and still have their tides greatly augmented by their approach to critical lengths. There are, indeed, portions of the ocean which cannot be covered by any areas the periods of which would be satisfactory, and in which it would be possible for the tidal forces to incite a considerable tide. Upon referring to the map, Fig. 23, it will be seen that one such region exists west of Australia, another south of New Zealand, another east of southern South America, the Arctic Ocean constitutes another. Upon referring to the map of the diurnal tides, Fig. 24, it will be seen that the South Atlantic, the South Pacific, and all of the Arctic Ocean are not regions where we can reasonably expect to find large diurnal tides.

Referring again to Fig. 23, and noting that the ocean is for the most part actually parceled out into areas of considerable width the free periods of which can hardly differ greatly from twelve lunar hours, and are, moreover, so situated that the forces do not approximately destroy one another, as can be seen by applying the rule quoted in the criticism, it may, perhaps, be justifiable to ask how it happens that the times of high and low water at the loops, as determined by this rule, do approximately agree with observed times, unless there is some considerable truth in this "partial explanation of the tides."

Recently I have been working out in considerable detail the tides in the equatorial belt of the Indian Ocean, where it is fair to assume that the effect of the deflecting force must be small. The work goes to show that the theory set forth in the criticised paper is substantially correct. I therefore venture to refer Prof. Darwin to this discussion, which will appear in the March number of the *Monthly Weather Review*.

To avoid needless misunderstanding, it may be added here that I am well aware of the incompleteness of the treatment given in my paper. For instance, mathematicians have not up to this time been able to treat the simple problem of a rectangular "area" the rigid boundary of which consists of only two opposing end walls, although much has been done upon analogous problems relating to the open organ pipe. Even an approximate absolute value of the range of tide (excepting in small deep bodies) has not been attempted in this paper, because its determination would involve the numerical value of frictional resistance, which can be kept in abeyance when we seek only the times of tides in systems which have as free periods very nearly the tidal period. Many deductions and refinements were purposely omitted from my paper—the chief aim being simplicity. I hope eventually to be able to consider more fully matters like these in connection with detailed studies of the tides in various seas. R. A. HARRIS.

Washington, D.C., March 28.

March Dust from the Soufriere.

SIR W. THISELTON-DYER has kindly forwarded to me a packet of volcanic dust sent to him by Dr. D. Morris, which fell in Barbados last month after an eruption of the Soufriere of St. Vincent, a brief description of which may be of interest. The sample, Dr. Morris states, was collected at Chelston, Bridgetown, on sheets laid out upon the lawn, the material being brought in and weighed every hour, and the fall continuing from 11 a.m. to 5 p.m. on the day of the eruption. It is free from all extraneous matter, and may be regarded as typical of the ash which fell on Barbados. The weight of this is estimated at about 6000 pounds (avoir.) per acre. At an average rate of three tons per acre, this would be equivalent to about 300,000 tons for the whole island.

The dust is of a dull dark brown colour, showing on close examination a minute speckling with a lighter tint. If poured on a piece of white paper and removed in the same way, a distinct warm-brown tint remains, produced by the very finest part of the powder, which is not easily removed. In Dr. Flett's excellent account of the dust which fell in Barbados after the eruption of May 7 (*Quart. Jour. Geol. Soc.*, lviii., 1902, p. 368), it is stated that this was at first brown, then slightly redder, and at last a whitish-grey impalpable powder. A bulk sample of that fall is distinctly greyer than the recent one, and a small one of the fall of 1812, in my possession, is a rather pale grey with a slight brown tinge. The new sample under the microscope differs only in detail from that described by Dr. Flett. The fragments, as a rule, do not exceed 0·01 inch, and are thus very slightly smaller than some in the May eruption; from 0·06 to 0·08 is a rather common size, and there is a fair amount of exceedingly minute dust. The principal minerals are the same, plagioclastic felspar, hypersthene, and a green augite, but in the first steam cavities are now more abundant than glass enclosures, and I think brown glass is more often adherent, but to make certain of this point requires a fuller examination than I can give for the next few days. T. G. BONNEY.

The Lyrid Meteors.

THE Lyrid meteors excite an interest that might be regarded as quite disproportionate to their numerical importance. They are a very rare shower, and even when considered by experienced observers as unusually abundant, they seldom appear at a higher rate than about twenty per hour.