CORRESPONDENCE.

REPLY TO PROFESSOR PERRY'S COMMENTARY ON PROFESSOR MILNE ON VOLCANOS.

Sir,—Professor Perry has in effect charged me with committing what he calls the "common error of many geologists, who know a little mathematics, of imagining that they can create a mathematical theory for a phenomenon." It would be well if this sort of attempt were a little more common, and that geologists would apply a little more frequently the test of "how much" to their theories. Should errors be made they could be detected and exposed.

But in the present case this rebuke is scarcely merited; for, if your readers will look at Prof. Milne's paper, towards the foot of p. 169, they will see that the effect of pressure in retarding fusion, referred to by his apologist, is not even alluded to by the writer: but solely the effect of the coldness of the bottom water of the ocean in lowering the position of "any given isotherm."

For determining that point I believe that my figures give the simple consequence of the acknowledged law of mean increase of temperature; and I can scarcely be accused of having attempted to "create a theory" thereby. Perhaps however I lacked caution when I used without qualification the expression "melting temperature." So I will explain what I meant by it.

We must, I suppose, accept as a demonstrated fact, that the earth is "as a whole" extremely rigid. We believe that its interior is extremely hot. From these two propositions taken together it follows that the pressure to which the internal parts are subject induces solidity in matter which would otherwise be fluid through heat. This conclusion may be accepted without appealing to experiments, the results of which seem doubtful, respecting the floating or sinking of solid in melted rock. We know further that the superficial layer (which may be called the crust) is solid from cold. But we do not know whether there is a continuous and constantly liquid layer between the crust and that nucleus which is maintained solid by pressure, in spite of its high temperature. For my own part I suspect that there is.

We may feel sure however that at a certain depth the rocks are at such a temperature that, if not already fluid, they would become so if relieved of the superincumbent pressure. This temperature would be the same as that at which they melt at the surface, and is what I meant by "the melting temperature." Used thus, the term is merely a name for a particular isotherm. The temperature, at which the rocks might become fluid in spite of the pressure upon them, is more correctly termed "the melting temperature for the pressure." Prof. Milne's words on which I commented, do not make any reference to this condition.

Perhaps it may be asked—what has the melting temperature at the surface, where there is no superincumbent pressure, to do with the theory of volcanos?

If we suppose the crust to be crumpled by lateral pressure, the vertical pressure beneath the anticlinals must be thereby relieved.

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Consequently the heated rocks, if hitherto kept solid by pressure, would enter into fusion at somewhere about this melting temperature, when the pressure was thus removed. I was the first to point this out in 1868. It would seem then that the isotherm, corresponding to the melting temperature at the surface, will near about determine the thickness of the permanently solid crust.

Again, if a fracture were to be opened from below upwards, as might happen in any portion of a synclinal trough, or less advantageously from above downwards in an anticlinal; or if three or more faults radiating from a central vertical, combined with a slight horizontal shift, were to occur; then a funnel would be formed communicating with these hot rocks, and reducing the pressure at that spot nearly to the atmospheric pressure. Immediately the superheated rocks, which probably contain superheated water, if not already fluid, would enter into fusion. Steam would rush upwards, and lava would follow it; and although statical pressure could not perhaps carry this quite to the surface, yet the momentum, acquired by the molten rock in flowing towards and up the funnel, would for a while carry it still further, so that an overflow of lava would take place. But when this momentum was expended, it would sink back again into the funnel.

I have formerly offered some speculations upon these and kindred subjects, fairly open perhaps to the charge of "imagining that I have created a mathematical theory for phenomena." They are contained in three papers published in the Cambridge Philosophical Society's Transactions—viz. "On the elevation of mountains by lateral pressure, with a speculation on the origin of volcanic action": 1868.[1] "On the inequalities of the earth's surface, viewed in connexion with the secular cooling": 1873. And, "On the inequalities of the earth's surface as produced by lateral pressure, upon the hypothesis of a liquid substratum": 1875. These have been all of them placed in the library of the Geological Society.

HARLTON, *8th June.* O. FISHER.

THE "PRE-CAMBRIAN" ROCKS OF ROSS-SHIRE.

SIR,—Now that Dr. Hicks has completed his notice of the Ross-shire rocks, I must ask permission to make one or two comments, since the union of Mr. Davies's name with his own naturally strengthens his case. Mr. Davies's support, however, I venture to say, is more apparent than real; for in some respects no one disputes the conclusion; in others Mr. Davies speaks with reserve; while in others the evidence does not appear to me to have been fully placed before him.

I will therefore recapitulate the points in Dr. Hicks's original paper (Q. J. G. S. vol. xxxiv. p. 811) which I controverted in my notes upon the district (Q. J. G. S. vol. xxxvi. p. 93):—

1. He represented the so-called syenite in Glen Laggan as intrusive in the quartzite and limestone series. I asserted that this rock in the main was not igneous and was not intrusive, but brought up by faults. Dr. Hicks still maintains that it is igneous, but now claims

[1] Reviewed in *Nature*, vol. v. p. 381.