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Robert Mallet F.R.S.

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III. Mallet's *Volcanic Theory* "tested" by the Rev. O. Fisher. By ROBERT MALLET, *F.R.S.\**

IN July and August last the Rev. O. Fisher, in asking me for some explanations in reference to my paper "Rock-crushing and its Consequences" (Phil. Mag. for July 1875), with which I supplied him, informed me that he was preparing a further paper, in which he proposed to test my theory of the origin of volcanic heat and energy by some application of mathematical reasoning. I venture to subjoin an extract from my letter in reply addressed to him on the 30th of August last:—"I have no wish to dissuade you from any criticism of my volcanic views which may occur to you as important to make; but at the same time allow me to remark that those views of mine are greatly more dependent upon a large number of physical considerations than upon any mathematical ones, and I do not think that symbols or arithmetic are likely to throw any additional light upon the subject, however they may tend to confuse it. I replied to Mr. Hilgard's and your own objections because it seemed necessary that I should fill up a lacuna purposely left in my original paper, and not in the spirit of controversy; nor do I wish or intend to engage in any further controversy as to any objections that may be made to any of my views.

"Bacon has, I think, somewhere said that, with respect to large and complex questions, it is best to lay aside instant discussion and allow time and rumination to wisen us upon the subject. Time and the advance of science in the future will no doubt afford surer tests of the truth or falsehood of my views than we now possess; but in the existing state of terrestrial physics, partial objections, even if well founded, seem to me of little value or use."

Mathematical reasoning is an admirable and potent instrument for the discovery of truth when the data upon which it is founded are exact, sufficient, and such as we are sure exist in nature; but all its validity depends upon these data.

We know almost nothing as to the nature of the interior of our globe; and it is only in a very imperfect way that we can even imagine the conditions, highly complex as these undoubtedly must be, under which mechanical strains act upon it even within a few miles of the surface, where we may reasonably infer its materials and their arrangement to be highly complicated, and differing from point to point in their chemical and

\* Communicated by the Author.

physical natures ; of the material and its arrangement at much greater depths we know absolutely nothing.

The theories propounded of the descent of glaciers present examples, now familiar to many, of mathematics misused, because as yet our knowledge of the physical nature of ice is so imperfect. This is still more true where mathematical calculation is attempted to be applied under conditions such as affect the interior of our globe (as compared with which the motions of glaciers are simplicity itself), as is done by the Rev. O. Fisher in his paper entitled "Mr. Mallet's Theory of Volcanic Energy tested," which appears in the *Philosophical Magazine* for October last.

I shall adhere to my resolution expressed in the above extract not to engage in any mathematical controversy in support of my views as to volcanic activity, but to leave it to the issue of advancing knowledge, when in time to come more extended and new forms of observation or experiment shall have afforded more certain physical data than we at present possess whereon to base our conclusions. In abstaining thus it must not be supposed that I admit the validity of the Rev. O. Fisher's conclusions, or that the pretentious title of his paper is in any wise justified by them. Passing by the earlier parts of his paper, as to which all that need be said may already be found in my paper in the *Philosophical Magazine* for July, the connected argument by which he professes to test my views commences at page 309. The physical data upon which it is founded have no real or probable existence in nature, and are in some instances in conflict with each other, while some of the numerous hypotheses involved in his calculation are not warranted by any thing set forth by me in my original paper (*Phil. Trans.* for 1873). For his own purpose he takes my small paper subsequently read (addition &c., read May 1874), in which upon certain hypotheses and suppositions I assigned limits of thickness to the earth's solid crust, as though it were part of my original paper and resting upon an equally assured base.

The Rev. O. Fisher assumes a rigid crust and rigid nucleus, that these are in contact at a definable spherical surface, that at this surface they adhere or stick together, and that when this adhesion is broken by tangential forces originating in contraction of the nucleus, the surfaces of contact drag over each other with an enormous resistance, which he supposes at the moment of rupture equal to the whole weight of the crust. This fanciful coefficient of adhesion and friction ( $\mu$  and  $\mu'$  in the author's formulæ) he professes to take from me ; but what I have assumed for sake of illustration only at pages 8-9

(Phil. Mag. July last), in reference to disintegrated material, affords no warrant for the Rev. O. Fisher's application of it under conditions essentially different, while for his coefficient of adherence, or for the existence of adherence at all, he offers no warrant whatever. The coefficient of friction, which I have assumed in illustration only, is not 0.75, but 0.5; and whether either of these be true or not, for the moderate pressures of a few pounds per square foot as in Morin's experiment, they cannot be true, and in fact involve a physical impossibility, where the pressure per unit of surface enormously exceeds the crushing-resistance of the material as in his case, where the pressure is that of a column 400 miles in height. Yet it is upon these data that his argument rests, and by which he manages to get rid of the largest portion of the work due to the descent of the crust, and so to prove the residue insufficient for the production of volcanic heat. There is nothing to warrant the supposition that a crust 400 miles thick, which is the value our author assumes for  $k$ , would be compressed equally throughout its depth or crush simultaneously throughout its thickness; nor can it be assumed that volcanic activity is found uniformly diffused throughout the depth of such a crust, but must be supposed, as I have shown in my original paper (§ 87), to be confined principally to the upper strata of the crust, where, as may easily be seen, in an elastic and flexible crust local lateral displacements may take place sufficient to produce crushing and volcanic action without any dragging of the crust as a whole over the nucleus.

If these data and others which I have not specified, as well as several assumptions which the paper involves, be false, as they undoubtedly are, then must the conclusions be false likewise, and this testing of my theory be but weighing it in a false balance.

But somewhat further on we find the author overthrowing, in the following sentence, the entire mathematical house of cards which he has with so much parade erected:—"If, however, as is more likely, the crust rests upon a fluid or viscous layer, the resistance to lateral motion will be much smaller; but we are not able to guess what it will be, so that we cannot *a priori* assign a value to  $\mu$ " (page 316). Now, as the only conceivable assumption that we can make is that adopted by all physical geologists, namely that a solid crust passes by an intervening viscous layer into a hotter nucleus below it, so this statement on the part of the Rev. O. Fisher is to admit that his whole mathematical argument is baseless and worthless. It seems to me a notable example of the misuse of mathematics which Professor Huxley, in one of his addresses as President of the Geological Society, not less wittily than truly

illustrated, by saying that if we put peascods into the mathematical mill we cannot expect it to yield wholesome wheat-flour. Such ill-founded calculations do not advance, but retard truth; they do so especially when applied to such questions as are related to physical geology, upon which opinions are adopted by large numbers who know nothing of mathematics, and by whom mathematical symbols are too commonly taken as tests of truth, and upon whom their parade exercises a sort of fascination like that said to affect birds under the glance of the rattlesnake. Were this intended as a refutation of the Rev. O. Fisher's paper, and not merely to point out the invalidity of his conclusions resting on such infirm data, I might point to the physical impossibility which appears to me to be involved in the first part of the answer he has given to his own question, page 317, "If the work of descent of the crust is not transformed into the heat of volcanic energy, it may be asked what becomes of it?" He says part of it "is transformed into heat within the nucleus," his own assumption being that the nucleus itself is *hotter* than the heat of vulcanicity. But this, as well as the string of improbable suppositions not containing any thing new, with which the author endeavours to prop up old volcanic theories at the conclusion of his paper, I pass without remark.

IV. *The Second Proposition of the Mechanical Theory of Heat deduced from the First.* By C. SZILY, Budapest\*.

THERE is not, and never will be, any theory which could dispense with fundamental hypotheses incapable of demonstration and explanation; still a theory must be regarded as more perfect the less it stands in need of such undemonstrable assumptions. The mechanical theory of heat, according to the present view of it, rests upon two propositions of this sort. The first (named after Mayer and Joule) is no other than the universal principle of the Conservation of Energy, in its application to heat.

The second proposition (that of Carnot and Clausius) cannot be expressed so simply, or be so readily fitted into the framework of a general physical principle, as the first-mentioned. This second proposition is formulated by Clausius as follows†:—

"Whenever a quantity of heat is converted into work, and the body through which the conversion is effected is finally

\* Communicated by the Author. Translated from the *Mathematikai Ertekezések*, vol. iv. 1875. The original memoir was presented to the Hungarian Academy of Sciences, May 10, 1875.

† Pogg. *Ann.* 1854, vol. xciii.