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To cite this article: Mr. O. Fisher Clk. F.G.S. (1876) XVII. Remarks upon Mr. Mallet's strictures on the mathematical test applied to his theory of volcanic energy by Mr. O. Fisher, Philosophical Magazine Series 5, 1:2, 138-142, DOI: [10.1080/14786447608639014](https://doi.org/10.1080/14786447608639014)

To link to this article: <http://dx.doi.org/10.1080/14786447608639014>

Published online: 13 May 2009.

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Specific Gravity of some of the Trap Rocks.—In the paper by Professor Dana before cited, the specific gravity of one variety of trap from this province, taken by myself for his purpose of comparison, is given. I may conclude with some additional examples, taken at about 60° Fahr.

	Spec. grav.
1. Columnar trap, one weathered surface. West side of Digby Gut	2.994
2. Crystalline trap, massive, above amygdaloid. Gulliver's Hole, Digby Neck	2.962
3. Crystalline trap, massive, above amygdaloid, not very broken. Point Prim, Digby Gut	2.918
4. Crystalline trap, massive, above amygdaloid, newly broken. East of Sandy Cove, Digby Neck	2.915
5. Amygdaloid. Hall's Harbour	2.747
6. " Peter's Point, Margaretville	2.697
7. " Partridge Island	2.659

XVII. *Remarks upon Mr. Mallet's Strictures on the Mathematical Test applied to his Theory of Volcanic Energy by Mr. O. Fisher.* By MR. O. FISHER, *Clk., F.G.S.**

MR. MALLET'S theory of volcanic energy is essentially a question of "how much?" which can only be decided by mathematical calculation. He has himself applied the method to the problem in his great paper; so that any argument against that mode of treatment of the subject lies against himself as well as against me. His general remark respecting the applicability of mathematical reasoning seems intended to reassure that class whom he describes towards the end of his paper as persons upon whom mathematical symbols exercise a sort of fascination like that of the serpent's glance upon small birds. He would bid them shake off their fear and be free; for he tells them "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 are sure to exist in nature. But all its validity depends upon these data," which we all know are usually unattainable. As a contrast to this description, I give Thomson and Tait's account of the matter.

"Until we know thoroughly the nature of matter, and the forces which produce its motions, it will be utterly impossible to submit to mathematical reasoning the exact conditions

* Communicated by the Author.

any physical question. It has been long understood, however, that an approximate solution of almost any problem in the ordinary branches of natural philosophy may be easily obtained by a species of *abstraction*, or rather of *limitation of the data*, such as enables us easily to solve the modified form of the question, while we are well assured that the circumstances so modified affect the result only in a superficial manner" *.

This is the method I have taken; and if Mr. Mallet desires seriously to impugn my conclusions, he must either show that my formulæ are insufficient to give the full amount of heat developable under his theory, or else he must substitute in them such admissible values of the quantities involved as will support his views better than those that I have taken out of his own writings. His "lofty point of thermodynamics," from which he so complacently assails the conclusions of previous geologists, will not serve him now; for I occupy a station on the same range of heights, and one which commands his own. If he will do what I have suggested, we shall know whose house is, what he calls mine, "a house of cards."

Although Mr. Mallet disclaims attempting a refutation of my arguments, he has attacked them in those places where he evidently thinks them most assailable. Naturally, "for my own purposes" (that is, for the purpose of my argument—neither an unfair nor unreasonable act) I took his "small" (quarto) paper, which I did *not* represent to be part of his original memoir, and used it. It was from this paper, published in the 'Transactions,' that I took those "peascods" and put them into my "mathematical mill." They were Mr. Mallet's own "peascods;" and if he has any "wheat" in store which he holds back from the world, I shall be happy to put it also into the mill, and to see what sort of flour it will yield.

The quantity which I have represented in my formulæ by μ , μ' is not, as Mr. Mallet supposes, a coefficient of friction. True, the letter is commonly used for that purpose; and probably that led Mr. Mallet to suppose that it was so used here. But he might have observed that in such a case the dimensions of the equation would have been incorrect, and we should have had a length equal to an area. Nevertheless it is true that μ and μ' will depend respectively upon friction and adhesion, and increase and decrease with them. Now, if any one will take the trouble to look at Mr. Mallet's paper in this Magazine "On Rock-crushing &c.," p. 8, he will see it is stated that the coefficient of friction has been put as high as $\frac{1}{4}$ of the pressure; and "should this coefficient increase pro-

* Natural Philosophy, § 438.

portionately under the enormous pressures to which a discontinuous mass at several miles depth may be subjected, we can readily see that the transformed heat of friction produced by internal movements taking place in such materials after crushing has occurred, must be the source of a large amount of heat over and above that originally due to the crushing itself." It is evident from this passage that Mr. Mallet thought a large value for friction was favourable to his theory (as developed by Prof. Hilgard); and he afterwards generously assumes the smaller value 0.5 for the coefficient. I purposely gave him the advantage of the larger estimate. But I am quite content that Mr. Mallet should make use of any coefficient of friction which he thinks admissible.

The following passage strikes me as certainly unfair:—"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 *à priori* assign a value to μ .' " Why did not Mr. Mallet quote the sentence which follows and qualifies this?—"But a probable estimate [for μ] may be arrived at from the consideration that a great circle of the sphere cuts on an average at least three lines of weakness, as appears upon the inspection of a map showing the lines of volcanic action." In fact my formula is general, and can be applied whether there be a solid nucleus or a viscous substratum.

It is well known that some physicists insist upon a solid nucleus; and no doubt Mr. Mallet would have been quick to notice the omission had I not adverted to that case; but by altering the value of μ , the formula equally well applies to the case of a viscous layer beneath the crust; and I have so applied it, though Mr. Mallet has left it to be inferred that this most important case has been overlooked by me. The truth is that I have disproved the theory in this case as well as in the other, taking the coefficient of friction as low as $\frac{1}{20}$; and seeing that Mr. Mallet suggests $\frac{1}{2}$, this value must surely be small enough to satisfy him.

Again, referring to my statement that, "if the work of descent of the crust be not transformed into the heat of volcanic energy, it may be asked, What becomes of it?" Mr. Mallet objects to my reply, that part of it is transformed into heat within the nucleus,—and argues that this cannot be, because I assume that the nucleus itself is hotter than the heat of vulcanicity. I suppose Mr. Mallet will admit that part of the

work of descent of a hammer upon a piece of red-hot iron is transformed into heat within the iron, although the iron be hotter than the hammer becomes through the work upon it ; and in principle the cases do not differ.

There is in Mr. Mallet's reply one argument which, if based upon true assumptions, would be of real importance. It is as follows :—"There is nothing to warrant the supposition that a crust 400 miles thick, which is the value our author [taking it from Mr. Mallet] 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."

Taking the latter supposition first ; if the lateral displacements be confined to the upper strata, this merely makes the nucleus (for this purpose) larger. And my formula may be applied as for a solid earth, introducing such a value of k as will correspond to the depth to which, at any place, the movements are supposed to extend. But it is manifest that no greater temperature can be obtained under these circumstances. The case in which a high temperature would really be induced would be that wherein the whole pressure was sustained upon a small area only of a vertical section, so that, when the rocks gave way, the crushing should be confined to that area. But in a body like the earth, which has gone on cooling for so long, and whose crust has been so long subject to the compressions required by the theory, it is inconceivable that there should be vast, open, vertical chasms, with faces in contact only here and there, or even alternations of hard and soft strata, under such conditions as to have the supposed effect in localizing the work. It is, however, only on the supposition of some such arrangements of the rocks as these that there can be any analogy between the localization of heat in the crust and the instances adduced by Mr. Mallet in a former paper, such as those of the ordinary flint and steel, or the cutting of a file by means of a rotating disk of soft iron.

As a parting compliment to my paper, Mr. Mallet tells his readers that my concluding sentences contain a "string of improbable suppositions not containing any thing new." Mr. Scrope, who, ripe in years and honours, has so lately been removed from us, took a somewhat different view. To set up

his authority upon the main subject in opposition to Mr. Mallet's would be a begging of the whole question in dispute. But it will be admitted that what was novel to him in his favourite subject could hardly be otherwise than *new*. I received a letter from him in October last, in which, after expressing his gratification that the results of my calculations confirmed his long-held opinions, he added, "There is, however, one of the points you put forward which never struck me before, but which now appears to me most valuable, namely that the enormous amount of steam that has escaped from the interior in early times as well as down to the present, has been, and is, the cause of those *subsidences* of the crust, to which the basins of seas and oceans, and the crumplings of the terrestrial rocks are owing, far more than to any general contraction of the nucleus by cooling."

I would request any person who may happen to read this to correct the following error in my former paper (*Phil. Mag.* October 1875):—At p. 316, line 6, insert *k*; for " $1\frac{1}{2}$ mile" read "533 miles," and *dele* the following line.

XVIII. *On the Polarization of Electrodes in Water free from Air.* By J. A. FLEMING, B.Sc. (Lond.), F.C.S., *Science Master in Cheltenham College**.

IN the 'Electrical News' for September 2, 1875, I published a short paper, previously read at the Meeting of the British Association at Bristol, entitled "The Decomposition of an Electrolyte by Magneto-electric Induction." In it experiments were described showing that when an electrolyte (*viz.* dilute sulphuric acid) flows in a very strong magnetic field, the electromotive force generated by its motion effects the electrolysis of the liquid, such electrolysis being held to be proved by the subsequent polarization of the electrodes. No actual liberation of gases was observed, since the electromotive force brought into play was insufficient to effect free decomposition. At the time it was read Professor H. A. Rowland raised certain doubts as to the correctness of the conclusions drawn; and his remarks were published, appended to my paper, in the 'Electrical News.' As these objections would, if correct, have deprived the experiments entirely of any little interest they might otherwise have had attaching to them, it was obviously necessary to examine them carefully.

Professor Rowland's remarks amounted practically to saying that he thought the electrolyte was not really decomposed, but

* Communicated by the Author.