

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.

for it a Pre-Cambrian age (GEOL. MAG. p. 163)—thus admitting an error. Could Mr. Davies see my specimens, or the rock in the field, I am convinced he would have no doubt that at any rate the bulk of the mass is gneiss.

2. Dr. Hicks asserted that the “newer series” in Glen Laggan (left bank) was not metamorphic. I replied that though in a very different condition from the older series, these rocks were much more altered than is usual with Palæozoic deposits, and were rightly termed metamorphic. The real point at issue does not appear to me to have been made quite clear to Mr. Davies; so, with all respect for his opinion, I must adhere to my statement, supported as it is by good petrologists to whom I have exhibited the specimens and slides.

3. Dr. Hicks asserted that the older series re-appeared in Glen Docherty, and so passed up into Ben Fyn and the mountain group southwards. I stated that of this re-appearance (in itself so improbable) there was no stratigraphical or petrological evidence, and that the microscopic structure of these rocks in Glen Docherty came much nearer to that of the newer series in Glen Laggan than to any other. Here again I think the evidence has not been fully before Mr. Davies.

The last two points were the foundations of Dr. Hicks’s argument, so we need not occupy time by discussing the rocks of Ben Fyn.

I will venture upon two further remarks. One, that it is singular what importance the rocks of Gaerloch have assumed in the interval between Dr. Hicks’s first and second paper. In the one they are dismissed merely with a vague allusion, so that I did not think it needful to visit a locality which seemed to have no material bearing on the controversy. Now they are placed in the forefront of the battle. A comparison also of the sections in the two papers (Q.J.G.S. vol. xxxiv. pp. 812, 814, with GEOL. MAG. p. 159) will show that important changes, not of detail only, have been introduced (for one at least of which I should like to see the evidence). These changes ought to have been more carefully pointed out to the reader than they have been. The other remark is that Dr. Hicks alludes to my work as hurried. For that accusation I venture to say there is no other foundation than that I did not remain in the district so long as himself. This, indeed, is true; but to test a theory ought not to require so long a time as to invent it. At any rate I remained long enough to convince me that the above three assertions of Dr. Hicks could not be established. On that point no amount of delay would have altered my opinion. Further, to the work in the field, the study of about forty microscopic slides, from most carefully selected specimens, has been added. As it seems to me, the author who first makes a rock intrusive in Silurian beds,¹ and then (without again visiting the locality, be it noted) regards it as Pre-Cambrian (cf. Q.J.G.S. vol. xxxiv. p. 814 with GEOL. MAG. 1880, p. 159) is more open to the charge of hasty work.

T. G. BONNEY.

¹ In using the term admitted by Dr. Hicks, I do not wish to commit myself to any opinion as to the age of the “newer series.” It is possible that there may be very much Pre-Cambrian rock in the Scotch Highlands: my contention is that Dr. Hicks’s proof of this is erroneous.