II.—THE SOURCE OF VOLCANIC HEAT.

By G. POULETT-SCROPE, F.R.S., F.G.S., etc.

IN his letter to the Editor, inserted in the last Number of this MAGAZINE (p. 332), Mr. Mallet says he is "compelled to repeat" the statements he had persistently made, and I had more than once denied, as to my having, "in almost all my writings," upheld "the moribund thin crust and liquid nucleus theory," and "Hopkins's fiery lakes."¹ He defends himself from the charge of having "misapprehended," as was suggested in an Editorial foot-note to his paper, or, as I more truly phrased it, "misrepresented," my views on these subjects, refusing to be bound by what I may have "written in the scattered magazine articles" to which he was referred, on the plea that "an author's notions are usually gathered from his acknowledged systematic works." Now when one public writer imputes to another opinions which the latter denies to have ever been his, candour and courtesy—not to speak of higher considerations—surely require of the former some examination into the fact before he repeats the imputation. What then is the fact? The only "systematic work" on volcanic geology to which I can plead authorship is the *Treatise on Volcanos*, etc., pp. 462, Longmans, 1862, re-issued without alteration, 1872, with which Mr. Mallet must be acquainted, since he did me the honour to accept a copy from me in, I think, the year 1865. Had Mr. Mallet, upon my disavowal of the views he imputed to me, before repeating the imputation twice over, only taken the trouble to open the volume and consult the Index, or but cursorily turn it over, he must have seen in its proper place in the work two pages, headed in capitals, "THEORY OF A FLUID NUCLEUS TO THE GLOBE, DOUBTFUL." In these two pages and the subsequent four (264–269), I give my reasons in detail for believing that *the globe is at present solid throughout*, with the exception of such locally "separate parts or pockets, as it were, of more or less intensely heated and liquefied mineral matter, as may exist at greater or less depths or distances," within or beneath the "foei" of active volcanos (p. 267). To this last extent, and no further, I expressed my concurrence with Mr. Hopkins's theory, while dissenting from his view of "a widely extended belt or continuous shell of molten mineral matter between the nucleus of the globe, *solidified by compression*, and its outer crust" (p. 268). In fact, I there, as well as in my "scattered writings," expressed

¹ In order that there may be no room for evasion as to the nature of the theory attributed to me from first to last by Mr. Mallet, I quote here one or two passages from his "Reply" to my Observations on his Theory, etc., *Geol. Mag.* for March, 1874, p. 127:—"The so-called mechanical theory of a liquid nucleus, that is, of a nucleus in liquid fusion beneath an extremely thin solid crust, has already given way, etc." "*Mr. Scrope's own notions, which involve that very thin crust and liquid nucleus, as most recently formulated by him*, do not, I believe, materially differ from those formed and enunciated by him some thirty years ago." . . . "The progress of science has, however, shown the untenability of the views still espoused by Mr. Scrope, of an immense liquid nucleus, and an excessively thin solid crust, as well as the notion of subterranean fiery lakes, or a continuous liquid shell between the crust and nucleus." . . . "Yet this gigantic incandescent nucleus and parenchymatous surface skin Mr. Scrope, and the school to which he belongs, must have, or their theories are impossible."

opinions the very reverse of those so pertinaciously fastened upon me by Mr. Mallet. And were I to assert that he, "in his writings" upon earthquakes, had attributed them to the oscillations of an extremely thin crust over a liquid nucleus, I should not misrepresent his views more completely than he has misrepresented mine upon the source of volcanic energy. I venture, then, to put the question to the scientific world, whether any reputation can be secure, if it be allowable for a writer, publishing his work from the vantage-ground of the Royal Society's Philosophical Transactions, and with the object of maintaining a theory of his own, to impute to another writer on the subject opinions which he has not only not enunciated, but which he has in his "systematic works," as well as in others, disavowed in the emphatic and conspicuous manner employed by me in the passages above referred to concerning the views "saddled upon me" by Mr. Mallet? And I ask, further, when the imputation has been denied, is it consistent with the courtesy and honourable conduct usual among gentlemen that it should be repeated again and again, when it would have been so easy to refer to the work in which its incorrectness, to use no stronger word, lies patent to the most cursory glance?

But Mr. Mallet has a peculiar way of conducting a scientific controversy, as will be evident to all who have looked into his paper, where, to take one example, in the compass of half a dozen lines (p. 152), he expresses his contempt for Mr. Hopkins's "fiery lakes," as he styles them, in the following choice epithets: "a lame and gratuitous hypothesis," "feeble and unconvincing," "most improbable," "erroneous and untenable," "inherently improbable,"—epithets which, as in other instances that might be adduced,¹ Mr. Mallet seems to consider a sufficient and complete refutation of the opinions he has thus denounced. True it is that Mr. Hopkins's views are wholly at variance with Mr. Mallet's rock-crushing derivation of volcanic heat, and therefore to be "put down." But it is not unimportant to observe that the former has the support of Professors Dana and Sterry Hunt, dynamical geologists of far higher authority than Mr. Mallet.²

One word upon a passage in Mr. Mallet's last letter in the *GEOL. MAG.* for July, p. 332, which professes to be an argument against my view of the derivation of volcanic heat from "a rise in geothermal temperature produced by deposition of sediment," namely, that "no

¹ See for another example p. 133 of Reply, etc. (*GEOL. MAG.* for March, 1874), in which Mr. Mallet, after declaring that "the limits within which the fusing temperature of rocks can be raised or lowered by difference of pressure are unquestionably too small to play any important part in geologic phenomena," adds: "The notion was seized upon by Hopkins as offering some *feeble* support to his *wild hypothesis* of subterranean lava-lakes." And lower, in the same page, he characterizes my "notion" that water existed in the rock-matter from which lava is formed previously to its fusion, as "wholly untenable," and again, three lines further on, as "utterly untenable"—a favourite epithet with Mr. Mallet, and employed by him, here as elsewhere, in lieu of argument.

² See Silliman's *Journal* for July, 1873, p. 10, and the same work for April, 1873, pp. 264-7.

body can raise the temperature of another by conductive transference of part of its heat to a temperature as high as its own, and therefore, if the subcortical matter of our globe" (as I have supposed) "passes locally under varying conditions of heat and pressure . . . to a liquid or even vaporous state, there must be matter more deeply situated at a still higher temperature, i.e. *there must be a liquid nucleus*." Was there ever such a *non sequitur* as is contained in this last phrase? It lays down as a law in physics that "no body" can by conduction reduce another body to fusion, unless it be itself fused—a proposition which every plumber's apprentice and every boy who melts ice with a hot poker practically refute. But if we charitably give Mr. Mallet credit for not intending to broach this palpable absurdity (although his words imply it), but suppose him to have in view *not all* bodies, but only subterranean rocky matter, can he deny that some rocky strata may, owing to their peculiar composition or structure, or permeation with water, or other conceivable conditions, enter into fusion or igneo-aqueous plasticity at a somewhat lower temperature than that at which some other subjacent or adjacent rock retains its solidity? Or again, will Mr. Mallet deny that pressure may raise the fusing point of rocky matter composing a deeply buried stratum or zone, so as to maintain it in a solid state at a higher temperature than that at which a body of the same material above it, but exposed to a less amount of pressure, will enter into fusion? The whole passage I believe to be as false in physics as it is dogmatic in tone. And yet Mr. Mallet looks down with supreme contempt, from what he calls "the lofty point of Thermo-dynamics" (p. 147, *op. cit.*), upon "the erroneous notions on these subjects commonly current among geologists"!

Mr. Mallet dismisses me with a challenge to reduce my views as to the origin of volcanic heat to "a definition," such as that which figures conspicuously in p. 167 of his paper. It is of course easy to give an imposing air to an opinion or conjecture by printing it in italics, with the title of "Definition" above it. And it is perhaps natural, after doing so, to fancy that you have thereby demonstrated its truth—as Mr. Mallet repeatedly affirms that he has done. But it remains after all a mere guess—more or less plausible according to the arguments, observations, or experiments by which it can be supported. I have already said that I am not prepared to dogmatize on this obscure and difficult question; but in my recent letter in the *GEOL. MAG.* for May, 1874, p. 237, I briefly stated what I will here repeat, my preference over Mr. Mallet's theory as to the exclusive origin of volcanic heat in the crushing of superficial rocks as they follow downwards a shrinking nucleus, of the opinion that it (volcanic heat) is derived chiefly and directly by conduction or convection, or both, from that intensely heated interior mass of the globe upon which Mr. Mallet's own theory rests as a postulate.

Let me briefly dissect Mr. Mallet's "Definition."

That the globe is a condensed nebula still gradually cooling from a state of fusion, is an hypothesis only; though, I admit, a plausible one. That its nucleus is consequently shrinking, is another hypothesis,

which the recent dispute among physicists, to which Mr. Mallet has been a party, whether mineral matter when consolidating (and especially when crystallizing), from a state of fusion, expands or contracts, shows to be yet doubtful. That such shrinking, if it really occurs, takes effect, not in the production of internal rents, but in "the more or less free descent of the shell by gravitation," is again but a conjecture. And that "the compression or crushing of portions of that shell" thereby occasioned produces an amount of heat, "locally within the solid shell," sufficient to effect its partial fusion, is a further conjecture which, from the probable slowness of the crushing process, if it does take place at all, and the many other modes in which the energy so set in motion may be supposed to be dissipated without the development of any extraordinary amount of heat (as I suggested in the *GEOLOGICAL MAGAZINE* for May, 1874, p. 237), can hardly be considered, I think, more than possible. Lastly, if all these hypotheses be granted, what proof or probability is there that the heat so generated by an action which must be supposed general and uniform throughout the entire subsiding shell,¹ does not merge in the general body of heat transmitted through it by conduction from the heated interior of the globe, but is exclusively directed towards the volcanic vents or fissures, "few and far between," which penetrate its surface? The extremely small fractional proportion of the total amount of heat that escapes into outer space through volcanic activity, which Mr. Mallet himself estimates at only 1 in 1500,²—instead of being, as Mr. Mallet seems to suppose it, an argument in favour of his theory, tells, in fact, strongly against it, since it is reasonable to presume that the smaller fractional portion of heat observed to escape from the surface of the globe proceeds chiefly, if not wholly, from the same source as the larger, namely, that enormous store of heat which the interior of the globe on Mr. Mallet's own view contains, and of which such a vast amount is continually transmitted by conduction to its surface. It is strange that Mr. Mallet should not have perceived the true inference from his figures. He argues, in fact, that because the amount of heat lost from the globe through volcanic vents is infinitesimally small as compared with that lost by superficial radiation into space, *therefore* the former *must* proceed from a different source from that which produces the latter. It might just as well be argued that because the heat received by our globe from the sun is but a minute fraction of the total amount of solar heat radiated through space, *therefore* the former must have a different origin in the composition of the sun from the latter. If any further argument were needed to show the fallacy of Mr. Mallet's exclusive reference of volcanic heat to the crushing of sub-

¹ In Mr. Mallet's own words, "not a mere local phenomenon, but a great cosmical condition, pervading every part of the thick and solid crust of the globe," p. 130 of Reply, *GEOLOGICAL MAG.* for March, 1874.

² "I have proved that the total amount of heat annually carried off from our globe by existing volcanic action cannot by any possibility exceed the $\frac{1}{1500}$ part of the total heat annually dissipated from our globe."—Mallet, *loc. cit.* p. 131.

siding rocky matter, it might be found in the fact that the traces of extreme crushing, such as corrugation, slaty cleavage, etc., do not occur in volcanic regions—which rarely show signs of much disturbance, but only dykes, *i.e.* narrow fissures filled with lava-rock, cutting through undisturbed, or but slightly disturbed strata, making it probable (as I said in my first notice of Mr. Mallet's theory) that if any heat is, or ever was, generated by the compression of rocks, its effects should be looked for rather in the metamorphosed and plicated rocks of mountain chains than in the lines of volcanic eruption.

I must apologize to the readers of the GEOLOGICAL MAGAZINE for this continued controversy with Mr. Mallet, on the double ground of, 1st, that gentleman's repeated and persevering misrepresentation of my views on volcanic action, and 2ndly, the inherent weakness of the theory he has advanced with so much pretension as "a precise and true theory, in substitution for the current and erroneous notions as to the nature of those forces, from the play of which, within our globe, those grand phenomena of nature (volcanos) are produced." —Phil. Trans. p. 147.

III.—ON THE PHYSICAL CAUSE OF THE SUBMERGENCE AND EMERGENCE OF THE LAND DURING THE GLACIAL EPOCH.

By JAMES CROLL, of the Geological Survey of Scotland.

(Continued from page 314.)

THE greatest extent of the displacement of the earth's centre of gravity, and consequently the greatest rise of the ocean resulting from that displacement, would of course occur at the time of maximum glaciation, when the ice would be all on one hemisphere. But owing to the following circumstance, a still greater rise than that resulting from the displacement of the earth's centre of gravity alone might take place at some considerable time, either before or after the period of maximum glaciation.

It is not at all probable that the ice would melt on the warm hemisphere at exactly the same rate as it would form on the cold hemisphere. It is probable that the ice would melt more rapidly on the warm hemisphere than it would form on the cold. Suppose that during the Glacial epoch, at a time when the cold was gradually increasing on the northern and the warmth on the southern hemisphere, the ice should melt more rapidly off the Antarctic Continent than it was being formed on the Arctic and Sub-arctic regions; suppose also that, by the time a quantity of ice, equal to one-half what exists at present on the Antarctic Continent, had accumulated on the northern hemisphere, the whole of the Antarctic ice had been melted away, the sea would then be fuller than at present by the amount of water resulting from the one mile of melted ice. The height to which this would raise the general level of the sea would be as follows:—

The Antarctic ice-cap is equal in area to $\frac{1}{23.48}$ of that covered by the ocean. The density of ice to that of water being taken at .92 to 1, it follows that 25 feet 6 inches of ice melted off the cap would