

V.—IN REPLY TO MR. SCROPE'S OBSERVATIONS ON MR. MALLET'S
THEORY OF VOLCANIC ENERGY.

By ROBERT MALLET, C.E., F.R.S.

OBSERVATIONS by Mr. Scrope on the subject to which he has so long devoted his attention demand respectful attention, and that I should endeavour to assign my reasons for not agreeing with the strictures which he has made upon my paper, read to the Royal Society in June, 1872.

I must disclaim the justice of Mr. Scrope's charge that I have thrown aside "superficially" the labours of all preceding geologists on the subject of Volcanic Energy. I have never in my life consciously treated with other than respect the well-directed labours, in whatever department, of any man of science. In the opening sketch of my paper, which reviews past speculations as to the nature and origin of volcanic heat, truth, above all things, demanded that I should point out the baselessness of many of them. In doing this, I had the unpleasant task of pointing out that the speculations of geologists on this subject too often show an ignorance or disregard of any sound appeal to the physical and mechanical sciences while those of some mathematicians; equally unfounded, because the conditions actually existing in nature were ignored or set aside for others admitting of more convenient treatment, were yet passed current amongst geologists, because the latter either could not or would not decipher the symbols in which these unfounded speculations are wrapt up.

If I have appeared to underrate the views of geologists as to the nature and origin of volcanic heat, it is because I believe them wholly untenable when tested by the light of existing science. The older notion of the chemical origin of volcanic heat and energy so long persevered in by Daubeny and others, in the face of most obvious difficulties, at last died a natural death as a result of the examinations to which volcanic ejecta have been submitted. That which succeeded it—the so-called mechanical theory, or that of a nucleus in liquid fusion beneath an extremely thin solid crust—has already or is soon destined to give way under the searching examination to which it can be now subjected by the present state of thermotic and more especially of thermo-dynamic science. 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. At that time nobody, not even the late Sir John Herschel, was enabled to test the validity of the notions current amongst geologists as to the origin of volcanic heat. It is always an unpleasant shock to admit the untenability of the views we have held and have even promulgated with more or less authority for many years, yet the progress of science, so far as these opinions may be unsound, compels us to do so, whether we will or no; it has already done so, I believe, as respects the notions still espoused by Mr. Scrope,* of an immense liquid nucleus and excessively thin solid crust, as well as the notion of subterranean fiery lakes, or a continuous liquid shell between the crust and nucleus. The

[* Mr. Mallet, in this and other passages, certainly entirely misapprehends Mr. Scrope's views, since in several papers contributed by him within the last few years to this MAGAZINE he has expressly called in question the theory which Mr. Mallet ascribes to him. For example, this is the chief purport of two papers having for their title "On the Supposed Internal Fluidity of the Earth" [see GEOL. MAG. 1868, Vol. V. p. 537, and 1869, Vol. VI. p. 145], and again in an article "On the Cause of Volcanic Action" [see GEOL. MAG. 1869, Vol. VI. p. 196] he concludes his paper with these words:—"Since it has become the fashion of late among the writers of popular geological treatises to assume as a matter of fact, beyond dispute, that the substance of the globe, immediately beneath its thin superficial crust (and probably to its centre), is in a state of fluid fusion, and that the access of water from the sea above to this molten interior is the exciting cause of earthquakes and volcanos, I have thought it well to express my reasons for entertaining doubts, to say the least, as to the correctness of either hypothesis." (p. 199.) Probably Mr. Mallet has never considered these papers.—EDIT. GEOL. MAG.]

existence of a nucleus still in liquid fusion is a purely arbitrary assumption, and Mr. Scrope wholly misapprehends me when he affirms that the existence of such a nucleus forms any necessary part of my theory. All that I postulate is, that ours is a cooling globe, and that therefore the interior is hotter than the exterior. I know nothing as to the liquidity of the interior; and in so far as its existence can be examined by the as yet imperfect approaches of science, I am obliged to suspend my judgment, if not to disbelieve in it. Again, did such an enormous nucleus in liquid fusion exist, coated over only by a thin solid crust of some thirty or sixty miles in thickness, I believe it proveable that the surface-temperature of our globe must be greatly in excess of what it is—indeed, that the flood of heat poured forth from such an incandescent nucleus through such a thin skin, even were the conductivity of the latter as low as that of pipe-clay, would be such as to roast every organized being off the present face of our earth. 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. Even with such a thin skin, and the most rapid conceivable passage at its lower surface from the solid to the liquid condition, I believe it proveable that neither water could make its way through any channels that we are at liberty to suppose down to the liquid nucleus, nor liquid lava from the latter make its way up to the surface. If all this be so, then surely the time has come for substituting some theory that will better square with the facts: and such is that which I have produced. I do not at all expect that that theory will be accepted by many of the older school of Vulcanology, without the usual struggle with which new views widely differing from the old are always received by those holding preconceived and long-cherished opinions. To compare small things with great, I cannot forget that Newton's theory of Gravitation received but a partial acceptance even one hundred years after its promulgation. My paper has been but a first "attempt," as I have called it, to evolve a theory of volcanic heat and energy consistent with itself and with the facts in nature. In treating so vast and complex a subject, it can scarcely be but that future research may find numerical corrections necessary; but I believe the skeleton of the theory which I have sketched will be ultimately admitted as true: for there is no surer test of the soundness of any theory than that it not only explains the principal phenomena, but often throws the most unexpected light upon collateral ones before obscure, and with which it seemed to have no connexion; and this, as I have pointed out in my paper, is the case with my theory in several remarkable particulars.

Mr. Scrope's objections, if not very cogent or at all conclusive in my judgment, are at least so numerous that I fear want of space must compel me for the present to leave some of them unanswered. Several of his objections appear to me to rest on no better basis than that of the very imperfect grasp he has attained of the nature of my argument, and in some instances to misconceptions as to the experimental facts and their relations referred to in my paper. I do, indeed, adopt Prevost's view of mountain elevation by tangential thrusts, to which I have made allusion in tracing the successive stages of refrigeration of our planet; but it is an error to say with Mr. Scrope (p. 29) that my theory is "founded upon the assumed truth" of Prevost's views. My theory postulates nothing more, so far as volcanic action is concerned, than that our globe is still a cooling one, and subject to the known physical conditions of cooling bodies. It would remain equally true, whatever view might be taken as to the mode of elevation of mountain chains. It is a fact, however, that mountain elevation and existing volcanic action are but successive phases of the same play of forces.

It seems to have escaped Mr. Scrope's observation, that I have done more than to merely adopt Prevost's views. I have, I believe, been the first to point out the complete succession of connected phenomena produced by contraction during the secular refrigeration of our globe from its condition of liquid fusion.

It had not before been seen by any writer, so far as I know, that while the solidified crust is very thin, the tangential strains therein were tensile, and that, as the crust thickened, these were gradually reversed in direction, and became tangential thrusts. Admit this, which is rigidly demonstrable to be true, and it then follows inevitably that in the earlier epoch the wellings up of fused material from beneath were due to subsidences of the crust, and, as I have called it, hydrostatic, and of a nature entirely distinct from existing explosive volcanic action. The

change of signs in the directions of the play of forces was no doubt gradual, and whether it took place at one or at another geological period is immaterial to the truth of my theory of existing volcanic action, which might remain true, were we to suppose that such a change of action never took place at all.

Mr. Scrope founds an objection to my theory, "that it seeks and purports to find a second source of heat where one exists fully sufficient for the purpose." Were I to admit with him his hypothesis of the existence of a fused and liquid nucleus within a few miles of the surface, his remark would be in so far true; but as I neither postulate nor even admit the existence of such a source of volcanic heat, there is no superabundant hypothesis involved in my view. The transfer of heat into space from our cooling globe is the *primum mobile* of all existing volcanic action. The heat lost gives rise to mechanical work at a certain stage in the complex train of phenomena called into play by its loss, and part of that work is transformed into heat, which is that of volcanic action.

It is mere verbiage to talk of this transformation of work into heat as involving any second source of heat, it being derived primarily from the only source conceivable, namely, the hotter interior of our planet. When a "billet" of iron is taken from the furnace and passed between the rollers, its temperature rapidly and visibly augments by reason of the violent distortion to which the heated mass is subjected. Would it be any philosophical objection to the true explanation of the phenomena to say that the increased heat of the bar was derived from a second source, and not from the furnace? If the interior be not in liquid fusion, of which we have no proof whatever, or being assumed in fusion if it be covered by a solid crust of even two or three hundred miles in thickness, then I affirm that there is no other conceivable way in which we can find a source of volcanic heat and energy, except that which I have pointed out.

An objection is made by Mr. Scrope, that of the three ways in which heat is lost by our globe,—viz. by conduction, by which I presume he means radiation into space, by hot springs, and by volcanic vents,—I ascribe the two former to direct cooling, and the last to an entirely distinct and independent origin. There is here surely a strange confusion of ideas. The heat—which is the transformed work of contraction and source of volcanic action—has no distinct and independent origin; it is merely one form of the actions brought into play by the loss of heat due to radiation into space. Again, as a matter of fact, Mr. Scrope is quite in error in saying that I ascribe the heat of thermal waters exclusively to hypogæal heat directly brought up by them. The great mass of thermal waters issue at far below 212° Fahr., and their feeble heat in the vast majority of instances is no doubt due to surface-waters penetrating the earth to a small depth, and returning more or less heated by it; but I have nowhere denied, even suggestively, that waters are also evolved at the surface, heated by direct connexion with or proximity to volcanic vents. However heated, they are but one form of the complex phenomena of our cooling globe, of which the volcano is another. Mr. Scrope seems to have failed to remark that my estimate of the annual heat lost by all the thermal waters in the world proves that their total refrigerative action is insignificant; we may dismiss them from consideration as affecting my theory.

Mr. Scrope says (p. 29), "There is no difficulty in understanding how the great fissures in the solid crust of the globe, which are marked outwardly by active, or once active, volcanoes, may penetrate so far into the interior of the heated nucleus as to give vent to an amount of heat sufficient to fuse the rocks through which they pass, and to some of the already fused or viscid underlying matter." To me there is every difficulty. Assuming a liquid nucleus, of which there is no proof whatever, covered by a solid crust of some hundreds of miles in thickness, it is, I believe, demonstrable that no open fissures could penetrate through such a crust; and if they did, the liquid matter could not reach the surface through them. Can I rightly understand Mr. Scrope to say that his open fissures would pass up heat from the nucleus better than the solid rock forming their walls? If so, this seems to repeat the error as to the heat-wave of some of the earlier writers on earthquakes, who imagined that the wave of shock passed more readily through cavernous fissures in the earth than through its solid mass. If this be the insecure basis upon which Mr. Scrope has founded the solution—to which he refers—of the great inequalities in the increments of hypogæal heat nearly everywhere observable,—viz. its lateral transfer when stopped by badly conducting strata to such open fissures;—or even if

we assume his fissures full of water, with a copious surface supply,—then would his solution signally fail to account even for such differences of increments as can be traced to the differences in conductivity alone. But Hopkins, in one of the most valuable of his papers, has shown that there are differences of increment not traceable to conductivity alone, and for which he was unable to offer any explanation. Facts also have been observed since his time which cannot be accounted for upon Mr. Scrope's notions: such as a continued increment, and then a decrement of hypogeal temperature, again becoming an increment, in the very same shaft; two shafts, alike as to wetness, not far apart, in the very same formation, extending to a vast depth below, and extending for miles all around, and yet showing great disparity of increment; two shafts not far apart, in different rocks, extending far around in every direction, but in the line joining them, and yet with differences of increment the very reverse of those due to conductivity alone. For these and other like cases Mr. Scrope's views, even if physically well founded, would offer no solution whatever. It is, however, as I have already briefly remarked, one of the striking confirmations of the truth in nature of my theory that it does, as a collateral consequence, offer a complete and consistent solution of this previously unsolved puzzle—viz. the immense inequalities in hypogeal increments.

I have shown in my paper (Phil. Trans. part 1. vol. for 1873, p. 168, par. 69 to 75) that these inequalities are due to the different amounts of work expended in the horizontal compression of strata, varying in compressibility or resistance when transformed partially into heat; work which is now going on more or less within every part of the superficial crust of our globe. The heat thus produced may in places be so slight as scarcely to affect the thermometer; while at others, as my theory declares, it may rise to the highest temperature of volcanic action. The source to which I attribute volcanic heat and energy is not a mere local phenomenon existing alone along the lines of volcanic vents, but is a great cosmical condition pervading every part of the thick and solid crust of our globe, and varied only in degree at one place or at another, dependent upon the amount of work expended at any point, and transformed into heat in the unit of time.

Mr. Scrope suspects that I have but an imperfect acquaintance with the phenomena of volcanoes in eruption, and then proceeds to say, "or he would not speak as he does of the expenditure of heat in the explosions of steam from a volcano in eruption as 'not resembling that which takes place in a steam engine, but rather that of powder exploded in a cannon, the loss from which is shown to be much smaller.' The contrary is really the case; the explosions from a volcano in activity resembling precisely in character (and apparently in cause) those of a Perkins steam-cannon fed by a continuous escape of steam from a boiler." Mr. Scrope has here made a giant of his own, founded upon an almost ludicrous mistake as to my meaning. I have not said one word in any part of my paper, or in paragraphs 189–190, about the resemblances to either steam engine or cannon of the explosive efforts seen at volcanic vents in eruption. I am, in the passage quoted, referring simply to the amount of *waste of heat* that should be allowed for as most probable in estimating the total amount of heat annually expended to produce the existing volcanic phenomena of our globe. The waste of heat due to lifting action in eruptions by steam blown off uselessly is, no doubt, as Mr. Scrope says, immense; and I have made an immense allowance for it by assuming that every three units of heat thus expended only do the lifting work of one. I have, as I conceive, made a most ample allowance in my estimate of the annual heat required for existing volcanic action, for that which is usefully consumed or wasted in the three great operations of melting the solid ejecta, vaporising the gaseous ones, and lifting the whole to the height ejected. But whether my figures be accepted as exact or not, I have also shown that the heat annually lost by radiation from our globe (which, as Mr. Scrope calls it, is the *primum mobile*) is so vastly in excess of that which is demanded to account for all existing volcanic action, that Mr. Scrope may, if he pleases, increase by some hundreds of times the expenditure I have assigned, without the result affecting the validity of my argument.

As to Mr. Scrope's suspicion that I have but an imperfect acquaintance with the natural phenomena of active volcanoes, my own field of personal observation has been, I believe, not very far from co-extensive with his own; but besides my own personal observations, which have neither been few nor unsystematic, I have made diligent use of the eyes and observations of others, and readily acknowledge the debt I owe

in this respect to Mr. Scrope himself, over whom I have possessed this advantage, whatever it may be worth, that my volcanic travels have been made not before, but since, the state of thermotic science and of other branches of physics has enabled measure and quantity to be applied to such phenomena. It is not upon the vastness of field of observation, but by the combined "eye-sight and insight" with which chosen portions of it may be regarded that the interpretation of natural phenomena depends. Von Buch and Humboldt afford us a remarkable illustration of this. Both possessed almost unrivalled opportunities of volcanic observation, yet to neither, so far as I am aware, do we owe a single important advance in volcanic theory.

I have nowhere denied, as Mr. Scrope assumes, that the preponderant portion of the ejecta constituting volcanic cones has been *at some time or other* in a state of fusion. What I have affirmed, and taken as part of the basis of my estimate, is, that *in any one eruption*, and upon the average of all known volcanoes, not more than one-twentieth of the matter ejected is *at that time* reduced to the condition of liquid fusion, the rest being merely more or less highly heated, but not to the fusing point. I believe a majority of those who have best examined the subject will agree with me in this.

Mr. Scrope also objects that I have not taken into account "the dust carried away by the winds or waves, and scattered over thousands of square miles of the surrounding areas." But this again is not so. I have included all dust and fragmentary matter in the nineteen-twentieths of ejecta heated to below fusing point, and I believe most amply allowed for its mass. The mass of dust carried to any considerable distance by the wind is relatively very small, and of that carried away at present by sea-currents we know simply nothing. Were I to admit Mr. Scrope's objections here as valid, and add as largely to the heat expended in each eruption as he could show any reasonable ground for, the increased numerical result would not in the least degree invalidate the argument of my paper, in which 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. I may extend this remark to nearly the whole of Mr. Scrope's objections in other directions, which merely cavil with my numerical data, without supplying any better or more exact ones, and which in any event do not affect my argument, or the theory deduced from it. I must pass almost without notice Mr. Scrope's energetic denials of my view, that on the whole the most ancient volcanic activity observable on our globe's surface was hydrostatic, and not explosive in its character, as at present. Some of his objections rest on mere misconceptions of my views, and all are asserted rather than proved; and whatever view be taken as respects this, does not affect the validity of my theory. The "unauthorized notion of the existence of vast masses of 'dust' beneath the earth's crust," which Mr. Scrope attributes to me, and which, he says, "pervades much of my theoretical view of the cause of 'hypogeal disturbances,'" has no existence but in his own imagination. I have nowhere even suggested the existence of any such masses save in proximity to volcanic foci and vents, and from which it is the province of the volcano to dislodge them.

The most sweeping objections to my views urged by Mr. Scrope are to be found at pp. 31, 32, commencing with the words "But the data for forming any opinion," etc. The remarks here made by Mr. Scrope arise from a radical misconception of the nature of my argument, the very basis of which he does not seem to discern. Observations, long continued at Paris and Edinburgh, prove that the annual loss of heat from our globe at present is equal to that necessary to melt 777 cubic miles of ice at zero to water at the same temperature. It is a result as well assured as most physical data dependent on continued observations, and sanctioned by the authority of such men as W. Thomson, the late J. D. Forbes, Elie de Beaumont, etc.; it is certainly not above the truth, but may probably be considerably *below* it. Having ascertained experimentally the units of heat evolved from the unit of volume of mean rock crushed,—that is, the mean of the various rocks of the whole series of formations,—I am enabled to determine how many cubic miles of such mean rock, if crushed, would evolve as much heat as that annually lost by our globe. So far, the numerical data by which I have tested my theory do not admit of dispute. The theory itself—viz. that volcanic heat and

energy are due to the mechanical work and its transformation into heat incident to the cooling of our globe—stands firm, and is wholly independent of these numerical data. But to test the credibility of my theory, I bring it into contact with these numerical data; and it is only necessary to show that, making an ample estimate of the amount of heat demanded by the volcanic action annually expended upon our globe, its total amount *does not exceed* that due to the total heat annually lost by radiation, or in any other way, measured in terms of melted ice or its equivalent in crushed mean rock. If the annual volcanic heat were equal to or exceeded the total heat annually lost by our globe, then the theory could not be true; but if, as I have shown, the total annual heat of volcanic action be but a minute fraction ($\frac{1}{1569}$) of the total annually lost by radiation from our globe, then a strong corroboration is afforded to the credibility of the theory itself, which really rests upon the indisputable fact that crushing and its physical consequences *must* take place in the outer portions of a cooling globe such as ours. It is therefore mere waste of time to cavil with the numerical data upon which I have based my estimates of the heat annually expended in volcanic energy, unless it can be shown that these are numerically deficient and require correction, not by adding to them to the extent of two or three fold, but to that of fifteen hundred fold or more. No one will affirm that my estimates, which are in several respects ridiculously ample and beyond all probable truth, are in error of deficiency to the above enormous extent; and if not, my argument remains untouched.

Mr. Scrope also objects to the validity of my experimental results as to the heat evolved by crushing rock specimens, as applied to our subject, in a way which I cannot avoid saying betrays much want of clearness as to the physical conditions involved. He says, "These crushing experiments being made upon small cubical blocks in a dry state, at the temperature of 57°, and subject on four of their sides to no other resistance than that of the atmosphere, are wholly inconclusive as to the effect of pressure on similar rocks miles under ground, permeated with water, at temperatures probably far exceeding 1000°, and in contact on all sides with resisting media at least as unyielding as themselves. How is it Mr. Scrope does not see that if a cube of rock, crushed by pressure on two opposite faces, the other four sides being in free air, requires a certain amount of work, it will require more work in proportion when these four sides are supported by other material, and that if there be more work thus expended, there must be more heat produced by its transformation? Now the store of crushing power in our globe is practically limitless, exceeding, as I have proved, the resistance to crushing of the most resistant rocks known to us by nearly five hundred fold. This objection therefore, rightly interpreted, is an *à fortiori* argument in favour of my results. But then my crushed cubes were dry. They were not dryer than rock not water-soaked usually is. What does Mr. Scrope *know* of the wetness or dryness as to imbibed moisture, or even as to the very nature of any rock at even thirty miles, not to say a hundred miles or more in depth? They are certainly not likely to be generally water-soaked, or even capable of imbibing water; and if they were so, can Mr. Scrope prove that they would necessarily require less work to crush them? But my experiments were conducted at the temperature of the atmosphere only. Is Mr. Scrope prepared to prove that rocks heated to 1000° necessarily require less work to crush them than at 57°? It is certain that fire-brick, which is an artificial clay porphyry or a sandstone, millstone grits, and many granitic rocks, and generally most neutral and basic silicates, offer about as much resistance to crushing at temperatures of 1000° or even more, in fact up to within a few degrees of their fusing points, as at ordinary temperatures. This is a fact illustrated every day in the construction and use of our blast and other furnaces, and in the well-known results of conflagration upon the materials of our architectural structures. As to the further objection urged, that I have paid no regard in these experiments to the effects of great pressure in raising or lowering the fusing point of rocks exposed to it, I have not left that quite disregarded in my paper, and I feel that it would be superfluous to seriously discuss the objection. The limits within which the fusing temperature of rocks can be raised or lowered by differences of pressure only are unquestionably extremely small, so small that I believe the results cannot possibly play any important or leading part in geologic phenomena. The fact of such differences existing at all is far from certain; it has merely been analogically inferred from a few experiments on spermacetti and wax, etc., and on ice, all,

except the last, of little certainty. The limits of difference have never been even analogically assigned by any physicist, and they are certainly smaller as the rigidity is greater and the fusing temperature of the body is higher. The notion was seized upon by Hopkins, with but little examination, as offering some feeble support to his wild hypothesis of subterranean lava lakes.

Mr. Scrope thinks that my theory "fails to account for the fact that volcanic eruptions are almost wholly confined to certain lines or bands traversing the earth's surface," which, he goes on to say, "indicate the existence through long geologic ages of great rents in the solid crust, the direction of which is generally parallel to the coast outlines of the continents or the axes of their mountain ranges." (p. 32.) My theory, as pointed out in my paper, does adequately account for the arrangement of volcanoes such as we find them, because it is along these bands that we can see must have existed the lines of least resistance to crushing after the mountain elevatory work had been done. Let me ask, on the other hand, what rational solution of the observed arrangement of the volcanic and accompanying seismic bands on our globe, which are not always coast-lines, but often cross the seas and oceans, is afforded by either the old, and as I regard it exploded, notion of a universal ocean of molten lava beneath an excessively thin crust, or by Hopkins's hypothesis of fiery lakes scattered within a solid and very thick crust? If we had the universal ocean of molten lava within thirty or sixty miles of the surface, why should it confine its visits to the latter to any linear arrangement? Why should not the greater number of volcanoes be found about the tropics, where, upon any theory of cooling of our globe, the crust, no matter how thick, must be the thinnest? And why should we have any volcanoes at all about the neighbourhood of the Poles? Or, again, why should Mr. Hopkins's fiery lakes be arranged in lines, or in any other way, unless scattered *par semé* over land and sea bottom? It is absurd to discuss the possibility of existence of the deep open fissures imagined by Mr. Scrope along coast-lines or anywhere else in our globe, after what the Rev. O. Fisher and I have proved as to the enormous tangential pressures existing in the earth's crust, which must crush into contact the walls of all such fissures with a force nearly 500 times greater than the resistance of solid granite or porphyry.

Mr. Scrope remarks that I "follow those geologists, Lyell, etc., who consider eruptions to be occasioned by the influx of water from seas or lakes above through fissures into foci of heated lava below. And that I reject as wholly untenable the notion that water could have originally existed in molecular combination with the crystalline matter of the rocks before they were melted into lava." Surface water must in some way reach volcanic foci to account for the phenomena observed at volcanic vents, and the theory held by Mr. Scrope, whether originating with him or not, and by a few other geologists, that the water from which the steam issuing from volcanic vents is formed has been derived from water either chemically combined or vesicularly contained in the rocks from which the lava has been formed, is wholly untenable. It follows, therefore, that Mr. Scrope's favourite notion that the expulsion of volcanic ejecta is due to the expansion by heat of such combined or vesicular water, and that the lava rises in and is expelled from the vent by what he calls its "intumescence," by a process which he has himself likened to the frothing forth of a bottle of champagne, is utterly untenable, being inconsistent alike with the phenomena and with the physical laws upon which it is supposed to be founded. If Mr. Scrope will recur to paragraphs 210 to 218 inclusive of my paper, I think he will see that he has overlooked much that I have there said, and in part (no doubt unintentionally) represented the very opposite of my meaning. So far from admitting that there is no limitation to the depth to which water may percolate, I distinctly state that "it is only to such depth as water can percolate or infiltrate by capillarity that the deepest focus of volcanic activity can be found." (par. 211.) Nor, of course, do I deny that rocky masses may be fused in contact with water at a sufficient temperature and pressure. But I do deny that such water can be the source from which the steam of volcanic eruptions is derived.

I can but glance at the objections to Mr. Scrope's view. To treat it thoroughly would require a whole part of this JOURNAL or more. What does any geologist *know* of the rock thirty or sixty miles deep? That it is hydrated or hydroferrous at these or much greater depths, not to say that the still deeper molten rock of the nucleus is so, is a mere assumption. But let us admit that it all,

at whatever depth, holds as much water as the most highly hydrous igneous rocks known at our surface,—that is, at a maximum about two, and on the average less than one per cent. by weight. Has Mr. Scrope ever calculated what supply of steam for elevation and waste this would afford him from a column of his liquefied hydrous rock of, say, a mile square, and sixty miles in depth? The pressure at the base of such a column of the density of granite would be more than 20,000 atmospheres. Has Mr. Scrope ever considered what amount of expansion his vesicular water could undergo under such a pressure and at a temperature equal to that of melted lava, or, if he likes, much higher? Has he considered the bearing on this of Dr. Andrew's late researches upon the expansion of liquids at great pressures and temperatures, and of the temperature at which it is probable from these "the critical point" for water is reached, or the effect upon the temperature of the column of the steam bubbles expanding in proportion to their distance from the base? Suppose these and other formidable physical difficulties removed, and that such a column could be lifted by the machinery assigned, the expansion of the evolved steam is at every point in the height of the column, whose temperature we may even grant to be constant throughout, proportionate to its depth in the column; it is, as we have supposed, 20,000 atmospheres at the base, and one atmosphere at the top of the column. Once this equilibrium is obtained, even slow frothing over or overflow of the column at the surface, much less reiterated spirting and ejection, is impossible. Any analogy to the champagne bottle is merely delusive. But again, how can we account on such a mechanism for that pulsatory ejective action which characterizes all volcanic eruption?—for those roaring blasts of steam, recurrent at uncertain intervals, which Mr. Scrope himself has so well referred to, in the preceding part of his remarks, as resembling the volley discharged from a Perkins steam-gun?

If Mr. Scrope will recur to the par. 210 to 218 of my paper, he will find that I do not reject any of the causes usually assigned for the irregularity, intermittence, recuperation, change of position, etc., observed at volcanic vents; but I say that to account for these phenomena something more is wanted. We must have some adequate cause for variation of the volcanic energy itself at its focus. Those requisite conditions my theory supplies; but it is wanting to the older notion; for whether the source of energy be derived from a universal subterranean ocean of molten rock, or from Hopkins's fiery lakes, that energy must be constant, subject only to an insensible secular decay. I do, indeed, say that it is but a partial view to affirm that volcanic eruptions can act as safety-valves to prevent earthquakes; though it is needless I should occupy space here by showing my grounds for that opinion. Mr. Scrope, however, wholly mistakes my meaning when he goes on to say (page 34): "Yet he himself argues that his crushing mechanism for producing the heat at intervals, which gives rise to volcanic eruptions, obviates the occurrence of paroxysmal 'Cataclysms' which would probably destroy all living things upon the globe's surface. And what can be meant in this connexion by 'Cataclysms' but earthquakes of tremendous violence?" What I do mean is made perfectly clear by the paragraph of my paper, 221, which I commend to Mr. Scrope's re-perusal. Were there not the self-adjusting mechanism by which the volcanic energy produced is just sufficient at very short intervals to remove a proportionate amount of material to enable our earth's crust to subside, but that such subsidence should be delayed for long intervals, then at last the crust would become to such an extent unsupported as to produce not only earthquakes, but such tremendous crushing together and rending as must suddenly destroy the terrestrial regimen upon which the existence of all organized life depends.

Mr. Scrope concludes by saying, "On the whole I admit the plausibility of Mr. Mallet's suggestion that some local development of heat must attend the crushing and squeezing of rocky matter during the internal movements to which their fractures and contortions, as well as the slaty cleavage of many, prove them to have been subjected." This is cloudily expressed; but if Mr. Scrope means, as he must be understood to do, that these crushings are still going on, then I am content to accept his admission, though expressed with a minimum amount of approbation, because it in effect admits my entire theory. All the objections in his remarks, numerous as they are, are mere questions of "how much"; they are, in fact, mere cavillings with the numerical data upon

which I have sought to test the validity of my theory. The theory is nowhere impugned by him. My "estimates" of the annual expenditure of volcanic energy do not profess to greater exactness than the imperfect state of our topographical knowledge and the state of science will admit. They will be subject, no doubt, to future correction, though I am sure without endangering the theory itself which they illustrate. I cannot but remark that while Mr. Scrope has objected to my numerical data, he has not in a single instance supplied in numbers any better ones, which, as it seems to me, every scientific objector is bound to do. Mr. Scrope ends by declaring that he "prefers" his own old notions to my new ones. That is natural; but we must all yield in the end to the progress of truth, and to this I am sure Mr. Scrope, whose truth love and candour I know from personal intercourse, will, I hope, live to prove no exception.

NOTICES OF MEMOIRS.

MINERALOGY.

- I.—MINERALOGICAL OBSERVATIONS ON BROCHANTITE. *Mineralogische Beobachtungen: V. Von Dr. Albrecht SCHRAUF. Sitzungs- b. d. k. Akad. d. Wissensch.: Math.-Naturwiss. Classe. lxxvii., 1873. pp. 275-360.*

THIS Paper, which forms the fifth of a series of communications presented by Dr. Schrauf to the Vienna Academy, is devoted, for the most part, to a discussion of the crystallography of Brochantite. Several minerals, differing from one another both chemically and morphologically, have hitherto been grouped together under this specific name; and, although our knowledge of many of these varieties is still imperfect, yet the author feels justified in referring them to four distinct types, namely:—(1). The Brochantite of Rézbánya in Hungary, of which two varieties (*a* and *b*) are recognized; and some of the Cornish and Russian Brochantites. (2). The Waringtonite of Cornwall, and a third variety (*c*) from Rézbánya. (3). The Brochantite of Nischne-Tagilsk, in Siberia. (4). The Königin of Russia, and a fourth variety (*d*) from Rézbánya.

Dr. Schrauf points out the relation between the crystalline forms of Brochantite and those of Malachite. Just as Malachite was originally described as prismatic and subsequently determined to be monoclinic, so it appears that careful measurements of Brochantite tend to remove it from the prismatic system. The author believes that some varieties of Brochantite are monoclinic and others triclinic.

In addition to the crystallographic details, the paper includes a comparative review of the paragenetic and chemical relations of the Brochantite group of minerals.

Two folding-plates of crystalline forms and projections accompany the memoir. F. W. R.

- II.—THE FIBROUS QUARTZ OF THE CAPE, A PSEUDOMORPH AFTER KROKYDOLITE. *Der Faserquarz vom Cap, eine Pseudomorphose nach Krokydolith. Von Herrn Dr. F. WIBEL. Leonhard u. Geinitz's N. Jahrb. f. Mineralogie u.s.w. 1873, Heft iv. pp. 367-380.*

IT is a curious fact that whilst quartz so commonly occurs crystallized, it has rarely been observed in distinctly fibrous forms.