ART. XXXVII.—*The Tarawera Eruption,* 10*th June,* 1886.—*A Criticism of Professor Hutton's (and others') Explanations of the Causes of the Eruption.*

By J. HARDCASTLE.

[*Read before the Hawke's Bay Philosophical Institute,* 8*th June,* 1887.]

A REPORT has been published by Professor Hutton, F.G.S., on the Tarawera Volcanic District in which he gives the conclusions he arrives at, after a visit to the locality and a study of a subdued phase of activity, as to the causes of the eruption in June, 1886. Those conclusions, it is to be inferred from a foot-note to page 12 of the report, are concurred in by Professors Thomas and Brown.

The explanation given of the cause of the eruption appears to me so much at variance with the probabilities of the case, as to invite criticism, especially as the general theory of volcanic action is involved. (See Report, pp. 14 to 18.)

Undoubtedly Professor Hutton is right in concluding that "the cause was local,"—"in the mountain," he says, italicising these words; "beneath the mountain" would surely be more correct, according to his own explanation. He is right, also, in saying that the heat "could not have been caused by upward conduction, through the solid crust, of the internal heat of the earth." Undoubtedly right also in concluding that "no chemical changes, at all competent to do the work, suggest themselves as the cause of the re-heating of the surface rocks." Having put aside these as incompetent causes, he mentions two others: the production of heat by (1) the crushing of rocks, and (2) the rise of a quantity of molten rock from somewhere in the depths of the earth. The first of these he dismisses after a very imperfect examination, as "very improbable;" the second he accepts as the true cause, though he shows that both the alternative modes by which it is suggested such uprise of molten rock might be brought about are open to objection, and makes no attempt to answer the objections to either, and expressly declines to choose between them.

I propose to show that the explanation put aside as "very improbable" is the most probable, and the true cause, and that an uprise of molten rock is very improbable.

In the first place, there is no evidence of an uprise of molten rock at all, in such quantity as would be necessary to re-heat all the rock that was ejected, and to vaporise all the water which, as steam, supplied the ejecting power. Professor Hutton finds such evidence in the earthquakes which for some months preceded the eruption. These, however, were in no way remarkable, and they afford no evidence as to their cause. Such an addition, as that supposed, to the mass of the crust beneath

Mount Tarawera, should have been indicated by other and distinctive signs than mere earthquakes, as by changes of levels, disturbance of springs, and so forth. No such signs are mentioned. It will scarcely be suggested that there existed an enormous cavity beneath the mountain, void even of water, of which the supposed molten rock took possession, and where it remained quietly stowed away while performing the heating work attributed to it. But if not, where was it bestowed, that no unmistakeable sign of its presence was manifested? And where is it now? Professor Hutton says all the matter ejected was re-heated surface-rocks. Then the supposed mass of molten rock must still be a source of danger. It must have retained at the close of the eruption at least as high a temperature as it had imparted to the rocks heated by it, and that was of volcanic intensity. Or has it retreated to the abyss whence it came? This explanation is prolific of puzzles, and more are provided by the modification made in it to account for the different nature of the eruption from the craters on the plains.

Not only is there no evidence that any uprise of molten rock took place, but both the modes by which it is suggested such an uprise could take place are, I believe, inefficient. One involves the admission of a thin crust over a fluid or plastic interior. The physico-astronomers disallow that. They threaten to stop tides, and twist and wrench the crust about every day unless that idea is given up, so the sooner it is given up the better. The alternative, that the molten rock is forced up by water expanding into steam, or by the expansive force of other gases imprisoned down below among the molten rock, must, I believe, be also given up. I hope to have opportunity to show on a future occasion, in some detail, that at no depth at which molten rock can reasonably be expected to be met with, according to this hypothesis, can steam or expansive gas of any effective force be formed at all. The familiarity we possess with the power of steam, and with the tremendous work it does in active volcanoes, has caused inquirers to neglect inquiry, and to attribute to high-pressure steam powers which it does not possess, and to credit it with work which it is really incapable of. Hot water, however hot, is not omnipotent. There are fixed relations between the temperature, density, and pressure of steam; and just as our boiler-makers can, and do, shut it down at a temperature of 400° or so, by means of a few quarter-inches of steel plate, so gravitation, working through a few miles of rock, can keep it in subjection at 2,000°. Steam plays an important part in most (not in all) volcanic eruptions; but it is not the prime cause.

I now turn to the explanation which Professor Hutton discards as "very improbable," the production of volcanic heat by rock-crushing.

This theory was originated by the late Mr. Robert Mallet. In working it out he, at some expense, had massive machinery constructed by means of which he crushed small cubes of various kinds of rock, and noted the force required to crush them. By means of other apparatus he measured the amount of heat generated in the fragments by the work of crushing. The amount of heat developed, as was to be expected from the law of conservation of energy, was always in proportion to the force employed. Having ascertained from the researches of others the annual loss of heat by the globe, he calculated the amount of contraction due to such loss of heat, and from the results of his crushing experiments calculated that the annual contraction, in terms of descent of the crush, must furnish power enough to crush an amount of rock that would be sufficient, and more than sufficient, to yield all the heat required for the average annual display of all kinds of volcanic activity. This calculation may have been perfectly correct; but it will not apply to particular cases. Professor Hutton points out the flaw in Mr. Mallet's theory: So many times the amount of rock to be fused must be crushed, and then all the heat developed must be focussed in a small portion of the fragments, which is an impossibility. (It is some years since I read Mr. Mallet's treatise, and I have only a few extracts and notes from it by me; but I think I have stated his theory and its defect fairly. Professor Hutton's allusions to it corroborate my memory so far as they go). Mr. Mallet made the mistake of reasoning directly from his machinery to the volcano, and unfortunately mislaid the germ of what I contend is the true theory, while attempting to bring the matter as a whole into subjection to arithmetic. His experiments were made with small cubes of stone, unsupported at the sides, and the average force required to crush them was—I gather from one of my notes—about 15 tons per square inch of the upper surface. Had he reproduced as far as possible the conditions under which rocks must be crushed in nature, when buried under a few miles of other rocks—had he enclosed his cubes in a strong steel box, or tried to crush the central portion of a slab strongly bound round the edges, he would certainly have had to apply much greater pressures to crush them, *and would have obtained proportionately higher temperatures in the fragments*, since the heat developed is in proportion to the force employed. Professor Hutton says ten volumes of rock must be crushed to furnish heat enough to fuse one volume. Possibly that is a conclusion from Mr. Mallet's experiments. Then, if the crushing of a piece of rock unsupported at the sides developes heat enough to fuse one-tenth of it, and if crushed under other circumstances ten times the force is required, in the latter case, ten times the heat being developed, *the whole will be fused.*

It *cannot* be doubted that beyond a mile or two beneath the surface the rocks are so well supported on all sides that enormous force must be required to crush them. Under such circumstances there can, of course, be no crushing into fragments. The conditions preclude any separation of the parts or particles to form interstices. But any change of shape, any forcible deformation, producing movement of the component particles amongst each other, would be equivalent to crushing to dust, so far as development of heat is concerned; possibly more than equivalent, since in crushing to dust power is absorbed in overcoming cohesion, and, I apprehend, does not reappear as heat; whereas it is conceivable that in deformation merely less of the force is so absorbed.

If a pressure of ten times 15 tons per inch is sufficient to fuse average rock, and, say, 300 or even 500 to fuse harder rocks, such pressures are available for the purpose. They are mere fractions of the stupendous pressures that the collapse of the crust would give rise to, if the rocks were rigid enough to call them forth. The possible crust pressures exceed 5,000 tons per inch, which gives a wide margin of crushing force over any possible resistance. The conversion of the work of the extreme pressure into heat would give solar temperatures to a considerable quantity of rock; so that the crushing theory easily accounts for terrestrial volcanoes, giving as wide a margin of temperature, almost, as of crushing power.

This development of Mr. Mallet's theory completely disposes of Professor Hutton's dust-heap objection; and the remainder of his "difficulties," those relating to the earthquake observations, instead of being difficulties, are proofs of the correctness of the crushing theory. There had been occasional earthquakes in the locality for some months before the eruption, indicating (for that is what an earthquake does indicate) that some portion of the crust was in motion, yielding to contractile pressures in some direction or other. Those crust movements may have occurred, but by no means necessarily so, in the exact region where the volcanic outburst afterwards originated, and they may have had much or little to do with bringing about the catastrophe. It is impossible for any massive movement in the crust to occur without heating in some degree the rocks which are crushingly affected by the movement, whether by friction of opposed surfaces, or deformation of larger or smaller masses. There may, however, be movements producing violent earthquakes which do not result in the heating of rocks up to fusing point, or to a temperature capable of vaporising water under the pressure due to the depth. On the other hand, a series of small movements, indicated by slight earthquakes, successively attacking and deforming the same mass of rock, will successively increase its temperature, even up to fusing point, if the attack be sufficiently sustained—if the pressures are equal to the production of the temperature required, and the series is not extended over too long a time. But fusion of a considerable quantity of rock might be effected by a single movement, under conditions which may exist in some spaces in the crust from time to time.

In the case of the Tarawera eruption, the evidence seems to show that there had been slight crust movements going on, intermittently, for some time (indicated by slight earthquakes); and as the earthquakes which immediately preceded the eruption were not violent, (not indicative of enormous crushing effect,) we may conclude that a preliminary series of attacks had been made upon a region of rock beneath the mountain, possibly raising its temperature to a high point, yet short of that necessary to form steam under the pressure due to the depth of the field of action; that on the night of the eruption, further movements, slight in themselves (as indicated by slight earthquakes) but critical in direction and the amount and nature of the deformation produced, raised the temperature of some portion of the deformed region so much that it was fused, and the water in this and adjacent portions was enabled to expand into steam, and that the steam, finding an old or a new fissure by which to commence its escape, quickly enlarged this into a wide rift, up which it bore millions of tons of crumbled, rocky matter. Heavier earthquakes followed the outbreak, caused, not by explosions of steam, but by crust movements becoming more extensive, conceivably facilitated by the removal of portions of the previously resistent rocks by ejection, or by the fusion and squeezing out of the way of a portion, or by both means together.

The eruption from the plain commenced later, and a difference is noted by Professor Hutton between the ejections from the craters on the mountain and those on the plain—fused rock being discharged from the former, none from the latter. I would suggest that the explanation of this difference is this: that more powerful pressures were required to carry on the work of contraction beneath the load of the mountain than beneath the plain, and the effective exercise of the higher pressures developed a higher temperature. The difference in the time of the outbreaks may be accounted for in this way: Previous crust movements in the same direction may have proceeded in some degree beneath the plain, while being retarded beneath the mountain by its weight. The pressures directed under the mountain accumulated until they overcame the extra resistance, and that so effectually that renewed and sudden strain was thrown upon the related rocks under the plain (related as to liability to compression), which strain they were unable to resist. Just as if one were pulling a carpet on which stood a heavy box, the carpet would be pulled forward on each side of the box; if a special pull were given to move the carpet beneath the box, that on each side would yield again. The difference of temperature may, however, be otherwise accounted for, and possibly other explanations might be suggested. The pressures, and the distance through which they acted, may have been equally great beneath the plain and the mountain, but the quantity of rock deformed beneath the plain may have been larger; in which case the heat developed, being spread throughout a larger mass, would not be so intense. Seeing that the larger portion of matter ejected issued from the lower range of craters, this appears to be the most reasonable suggestion of the two.

In the case of both eruptions, the relief afforded by the removal of matter at some points would cause greater strain to be thrown on other points, and these yielding in their turn, (the yielding being indicated by the earthquakes accompanying the eruption,) heat, and consequently steam and weakened rock, were provided for the continuance of the volcanic display.

Other facts, recorded by Professor Hutton and others, might be mentioned to show that the crushing theory fits them perfectly; but this paper has run to great length already, and I hope I have sufficiently shown that Mr. Mallet's theory, properly understood, is not so "very improbable."

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## Art. XXXVIII.—*On the Artesian Well System of Hawke's Bay.*

By H. Hill, B.A., F.G.S.

[*Read before the Hawke's Bay Philosophical Institute, 13th June, 1887.*]

Plates XVI., XVII.

Few things add more to the conveniences and general health of a town or a district than a good water supply. Happily for the people of Napier, and for those dwelling on the plains known as the Ahuriri, the Karamu, and the Heretaunga, they have a supply of good well-filtered water which is practically unlimited.

The discovery in this district of what are known as artesian wells, dates back a good many years. The first well sunk in Napier was the one in Hastings Street, near Mr. Swan's brewery, Mr. Garry, so long and so well-known in connection with Garry's foundry, being the gentleman who successfully carried out this important and beneficial work. It ought also to be recorded to Mr. Garry's credit, that he was the first to discover artesian water