whatever retarding effect its atmosphere may have exercised upon the cooling of the outer crust, that cooling was comparatively rapid, although the straining effects which I assume to have resulted from the causes referred to were still powerful enough, in Tertiary times, to result in the elevation of nearly if not all the great mountain chains now existing upon it. Whether the forces in question are still equal to bringing about changes in the surface similar to those which are revealed to us by the investigations of geologists as having occurred since the commencement of the Eocene period, can only be determined in the far distant future, although I am inclined to doubt it.

The straining referred to has, however, certainly not ceased, and will not cease until the thickness of the earth's rigid crust has become sufficiently great to prevent further solidification of the molten interior matter. The diminution which has apparently taken place in the intensity of volcanic action since the close of the Miocene period, seems to indicate the approach of such a condition of things, and that time, when it does arrive, will certainly be the commencement of the period in which the earth will attain its ultimate surface conditions.

Art. XLIII.—*On the Cause of Volcanic Action.*

By J. Hardcastle.

[*Read before the Hawke's Bay Philosophical Society, 13th September*, 1886.]

Abstract.

The first section of this paper reviews at length the arguments in favour of the dynamical theory for the origin of volcanic force, and the opinions accepted by the author may be summarized as follows:—

The conversion into heat of the work expended on the crushing and other internal rearrangement of rocks, (generally as subordinate phenomena in mountain elevation,) by horizontal pressures produced in the crust of the earth by its sinking upon a retreating nucleus, under the action of gravity, is the efficient source of volcanic heat of all degrees of intensity. The pressures, and the effect of their conversion into heat, may be roughly calculated. A specimen calculation shows the pressures required to elevate a mountain range 120 miles wide, $3\frac{1}{2}$ miles high above its supporting base, and from a crust 56 miles thick, must be 340 tons per square inch, the work of which, converted into heat, would raise the temperature of any mass of silica within which it acted by about 4,200° Fah., and other rocks in

proportion to their specific heat. The pressures needed to lift a mountain 20 miles wide and 1 mile in average height above its base, from a crust 20 miles thick, would be about 270 tons per inch, giving a temperature, if converted into heat, within silica, of 3,348°. In neither case is the initial temperature of the rock taken into account. The fusion of rock and extrusion of lava are the more important geologically, but it is not necessary that rock should be fused to give rise to volcanic phenomena. Temperatures of 550° and 1,000°, which would not affect a rock, give steam pressures of 1,000lbs. and 4 tons per inch, respectively, either of which, but especially the latter, would have great disruptive or explosive power, provided a vent was opened for them. The writer contends that volcanic steam, or fused rock, cannot open their own way to the surface; this must be provided for them by the movements which produce the heat fissuring the rocks above. He contends, also, that volcanic steam results from the heating of a wet rock; that violent eruptive phenomena cannot be caused by the access of water to heated rocks. It is suggested that in steam eruptions, (such as that at Tarawera,) the steam in escaping tears and crumbles up the free surface of the heated rock as frost acts on a clay bank: hence the fineness of the bulk of the ejecta. A rule is found to hold good in so many cases as to be worth further study—that volcanoes only appear where upheaving forces have acted about more than one axis, the volcanoes being found, not where the lines intersect, but in one or more of the angles formed by them.

The paper then proceeds to offer a history of the recent outbreak at Tarawera, on the lines thus laid down:—

Crust pressures, acting (as shown by the great fissure-lines) upon an axis lying north-east and south-west, accumulated in the elastic compression of certain beds until they were able to bring about movements of some kind in the rocks within which they acted, and which were at no great depth beneath the surface, but whose extent and thickness I make no attempt to estimate. During a fortnight or more before the outbreak these movements were going on, as was shown by the earthquakes experienced in the locality. (That the focus of action was situated at no great depth is indicated by the fact that the shocks were merely local.) The movements affected a considerable mass of wet rock, and were only effected by the exertion of considerable force. Judging from the resulting great amount of the ejections, it is probable that the action involved such a deformation of some part of the area of rock compressed as would have amounted to crushing at the surface, and the heat developed in such a case would be proportional to the force employed in the crushing. While this was going on below, the upper rocks were being cracked and fissured by the movements. The line of crushing appears to have passed under the Tarawera, or very near it, and the first fissure *made available* for the escape of steam from the heated beds passed through one side of the mountain. (It is not unlikely that the mountain was plentifully fissured and creviced beforehand.) On a way being opened for the escape of the steam, it was promptly taken advantage of. For some time the force of the steam would be largely employed in tearing away the sides of and enlarging the vent, the product of this action being the larger stones described as underlying the sand and dust on and near the mountain. All this time, as afterwards during the continuance of the eruption, the steam in escaping from the heated rock (which was possibly crushed, certainly weakened in its cohesion,) would tear off and crumble off its "face," and carry the fragments out through the fissure, to scatter them to the winds.

There is no evidence, I understand, that any portion of the ejecta had been fused, but the fineness of the great mass indicates that the rock from which it was derived was very thoroughly crushed by the movement which heated it, by the escaping steam tearing it to pieces, or by both actions together.

The subterranean rock movements continued, as indicated by continued earthquakes; the fissure through the upper beds was extended, and a second set of eruptions set up further south, the subterranean action being similar to the first. In connection with this second eruption, I should like to offer a suggestion as to the cause of certain noises that have been described as "horrible roarings," that ceased after a time, by those who were unfortunate enough to be in Wairoa on that memorable night. These may have been common volcanic sounds, but they may not. One of the chief centres of the second eruption was Lake Rotomahana, from the bed of which very copious ejections took place. Now one of the most horrible noises I ever heard is that caused by the condensation of steam within a body of water, as when a locomotive-driver turns a steam jet into his water-tank—a measure of economy when his steam is blowing off. Exchange the locomotive-tank for a lake, or quarter-inch pipe for an aperture possibly some yards in area, and 150lbs. pressure for, say, 1,000lbs., and one can imagine a *cause* for the "horrible roarings" heard at Wairoa. This noise would cease as soon as the escaping steam had carried up material enough to construct a cone, or cones, to the surface of the lake. A great deal of the water which went to make the mud that overwhelmed Wairoa may have been carried into the air as spray by the powerful steam jets that played through the lake. At any rate, a considerable quantity of water must have been carried up in this way.

An interesting question is: What is the nett result of the eruption in the nether regions? Has a cavernous space been formed by the removal of so much solid material? I think not.

I think that by the expansion of unremoved rock by heat, and still more by its expansion by escape from the elastic compression it was previously subjected to, the place of the rock removed has been occupied. Were it not so, the extensive fissurings that have occurred in the bed of Rotomahana must have allowed the whole of its waters to sink into the cavity on the subsidence of the grand eruption. Possibly there has been some slight sinking of the ground immediately above the locality whence the rock was removed, (I have read something about the southern end of Lake Tarawera having subsided 18 inches,) but the other means of filling the gap may have been sufficient for the purpose at present. As the heated rock cools and contracts further sinkage must occur, of which the deepening the existing lakes would be one indication. The second set of eruptions has been spoken of as hydrothermal, as distinguished from volcanic. I confess I do not understand the distinction—that is, if by the second eruption so much solid matter was ejected as I understand there was. It would seem to be a proper distinction to call that action hydrothermal which seemed to arise from access of water to heated beds; but, (as contended above,) no considerable eruption could be originated in this way. There could be no solid ejections worth speaking of. For true volcanic action the water must be in the rock when heated, or, must have time to permeate a heated rock before a fissure of escape is provided, when the same results would follow. Yet it must be more difficult for steam to break up a solid rock, than one that from the effects of recent mechanical action upon it has lost much of its cohesion.

It has been remarked that there were no "warnings" of the eruption. There never are other warnings of a new outbreak than such as were given to those living in the neighbourhood. There were numerous earthquakes which indicated that movements were going on below. The springs were affected, being more copious, without meteorological cause, indicating that the movements were compressive—the water being squeezed out of the fissures in the strata. But it was impossible to gauge the extent of those movements, or foresee their actual effect.