III. *Volcanic Energy: an attempt to develop its true Origin and Cosmical Relations.*

By ROBERT MALLET, *A.M., C.E., M.R.I.A., F.R.S.*

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1. PLUTONIC action has long been loosely applied by geologists as a term for forces of some sort, of whose nature little was known, acting deep beneath the surface of our globe, and either not directly manifesting themselves at all at the surface, or, if so, chiefly in the form of earthquakes, thermal springs, &c.; while volcanic action, showing itself at the surface in the phenomena of extinct, dormant, or active volcanoes, has been very generally regarded as something different in nature as well as in degree of activity. Some relations have always, more or less vaguely, been admitted between these; but each has in turn been placed in the relation of cause and effect to the other. A third class of actions, those of "forces of elevation," though assumed to have some relations with the preceding, have very commonly been regarded by geologists as differing in nature from both, in degree as well as in kind. It is true that all these phenomena have been linked together by such wide and vague phrases as that of HUMBOLDT, who speaks of them as "the reaction of the interior of a planet upon its exterior;" but I am not aware of any attempt having previously been made to colligate them all as effects originating in one common cause, and that referable to the admitted cosmical facts and mechanism of our globe.

Sir WILLIAM THOMSON, regarding all these phenomena from the lofty point of thermodynamics (from which the writer also is about to view them in this paper), has distinctly colligated them as referable to dissipation of energy existing in our planet in the form of terrestrial heat, and has given to all its play of phenomena the title of "Plutonic action," which he defines as "any transformation of energy going on within the earth" (Trans. Geolog. Soc. of Glasgow, vol. iii. pt. ii.).

2. The writer accepts Sir WILLIAM THOMSON'S above views, so far as he is yet acquainted with them through publication, as the broad basis for future physical geology.

Sir WILLIAM THOMSON, however, has not attempted, so far as the writer knows, to bring his general view, that volcanoes, earthquakes, &c. result from transformations of terrestrial heat, so to bear upon the facts known respecting these as to explain in any way the immediate mechanism from the play of which within our globe these grand phenomena of nature are produced; nor to connect these with the "elevation theories" of geologists, so as to substitute a precise and true one for the current and erroneous notions as to the nature of those forces which have elevated mountain-ranges and generally produced the inequalities of our globe, apart from the subsequent moulding-actions of water and other surface agencies.

The attempt to do this, in outline at least, which is all, perhaps, that existing knowledge admits of, is the object of the present paper. The term "Plutonic action" is objectionable, as already and long used by geologists in a loose and different sense. *Vulcanicity* is preferable, as proposed by the writer (4th Report on Earthquakes, Brit. Assoc. Rep. 1858), as comprehending all energy resulting either in volcanoes (*Vulcanology*) or in earthquakes (*Seismology*). In the former of these, forces of elevation generally may be comprehended, and in the latter thermal springs, though, as will appear when the intimate relations of the mechanism of all of these are better understood, the boundaries of these divisions at some points grow indistinct.

3. The phenomena seen at volcanic vents, and those experienced in earthquakes, have been tolerably well observed; and the immediate mechanism of the latter may be considered as understood, though the cause or causes of commotion, and it may be very different at different times and places, are still somewhat obscure.

The deeper mechanism of volcanic vents, all nearly except what is visible and tangible, is so far quite obscure; and this mainly because no rational origin has ever yet been assigned for the production of the high temperature manifested at volcanic vents.

To assign a true origin for this is to possess the key of the whole; for given a veritable cosmical mechanism for the production of the *heat* that shall square with its local distribution, and with the capricious, non-periodic phenomena of eruptive vents, and all the other observed phenomena of volcanoes, from the largest to the most minute of these, become explicable and fall into place upon a cooling terraqueous globe. As respects a presumed connexion, if not identity, save as to degree of intensity, between volcanic and seismic mechanism, however vaguely referred to some common force of origination, it can scarcely be said hitherto to have advanced beyond this.

Volcanoes, notwithstanding Von Buch's unsound distinction into linear and central groups, have long been observed to follow the lines of surface-elevations, *i. e.* mountain-chains, the exceptions being only apparent. (4th Report on Earthquakes, Brit. Assoc. Rep. 1858).

So also the writer has shown (Earthquake Catalogue, Brit. Assoc. and Seismic Map of the world thereof) that earthquakes on the whole are found to occur within the area of great seismic bands which follow and extend at either side of the mountain-chains of the world. Again, though thermal springs occur everywhere (just as earthquakes may occur anywhere), yet on the whole they are chiefly manifested in regions which have been or are greatly disturbed by mountain elevation or by volcanic and seismic activity.

Thus we find this local relationship binding together the whole, viz. that volcanoes, earthquakes, and hot springs follow the lines of mountain elevation or dislocation.

And this is almost all that we can certainly affirm connects them as having some common origin beneath.

4. Any subterranean impulse may produce an earthquake; and for those impulses there must be more than one nature of origin, for the impulse producing a shock like that of Riobamba cannot be the same as those producing the tremors of Pignerol or of Comrie, lasting for years.

Close to the volcanic vent the eruptive throes may produce earthquake-impulse before or after these have broken through the earth's surface; but we cannot *phenomenally* connect most earthquakes with volcanic action at all, inasmuch as there is, even in the very greatest, no local change of terrestrial surface-temperature.

We can only refer them to a common origin, if we can discover and describe some play of cosmical mechanism set in motion by terrestrial heat, that shall be sufficient in energy to account for all the phenomena, that shall bear examination when brought into contact with each particular class of well-ascertained facts, either of volcanoes or of earthquakes or of mountain-chain elevation, that shall leave none of these facts inexplicable, and that shall so link itself on to cosmical physics generally as to be applicable to like phenomena in other planets than our own, or satellites, so far as we have facts ascertainable in relation to these.

5. Within the limits of this paper it is impossible to enter upon any large discussion of the theories that have been advanced as to the nature and origin of volcanic activity and elevatory action; a few remarks upon the notions commonly current on these subjects amongst geologists are necessary, however, to point out their more salient defects and to contrast the views about to be here advanced with those which are current.

6. Volcanic theories have been always of two classes, the chemical and the mechanical. Omitting all earlier, DAVY'S notion that volcanic heat was due to oxidation of the metals of the alkalies or earths by contact with water, supported by DAUBENY and in part by DE LA BECHE, is that which has mainly engaged attention. Had it not been for the splendour of DAVY'S genius and the announcement of this view at the moment of his great discovery of the bases of the alkalies, it would probably never have had even a momentary acceptance.

DAVY himself abandoned it at a later period. When we remember that the mineral constituents in the rocks known in the earth's crust do not contain on the average more than about 4 or 5 per cent. of the alkaline metals taken together, and compare the nature of the total *ejecta* of volcanic vents, more especially those of gas or vapour as ascertained by many labourers, amongst whom must be distinguished DAUBENY himself, ABICH, BUNSEN, ST.-CLAIRE DEVILLE, and FOUQUE, with such as *must* result from DAVY'S hypothesis, we can only wonder that one so absolutely gratuitous should ever have had a moment's acceptance.

There is no other "chemical theory" to put in its place. All great or violent chemical energies, powerful as these must have been in past time, when the materials of our planet were in vapour and dissociated by high temperature, have long since been as a whole satisfied.

7. As the thermal energy of the shrinking mass was dissipated, and it passed from vapour to liquid, and from that in chief part to solidity, the chemical energies of the sixty or more elements we know of became satisfied by combinations, the order and conditions of which chemistry may yet hope to trace, though it cannot do so at present*.

* STERRY HUNT and STONEY have, however, made the attempt.

8. That there now exist in the interior of the earth large masses of uncombined metals of great specific gravity and high fusing-point, and strongly electronegative in the chemical scale, as gold and platinum, to which we may add iron if its vapour existed in large excess in the original *nebula*, is highly probable, both on such speculations as we can so far form as to the order of the play of chemical forces during the globe's formation by condensation, and as perhaps the only way in which the earth's mean density can be reconciled with that of its known superficial crust.

But the chemical elements composing that crust are on the whole in a state of combination; and hence no chemical energy remains stored up for conversion either into heat or into work.

9. For we may obviously, in relation to vulcanicity, pass by those minute chemical changes at present going on within the crust, mainly through the action of air and water and matter dissolved therein on some of its various constituents. We may even omit as insufficient whatever of chemical action took place during the period (probably yet not *quite* ended) when the mineral lodes or veins were formed. In a word, the chemical elements of the crust and interior of our globe, so far as we know any thing about them, have long assumed a state of chemical equilibrium, and one generally of the most stable character. So that we are compelled to conclude that whatever evidences of chemical decomposition or combination may be presented to us by the ejecta of volcanoes, the chemical action has been brought about by elevation of temperature at the seat of action deep below.

10. That thermal energy has been in part transformed into chemical work and not chemical energy into heat. There is therefore no room longer for *any* "chemical theory" of volcanoes; and we are reduced to that commonly, though not quite accurately, called the mechanical one. This has passed current at different times in several forms, but all resting upon the assumption that our planet now and for long past consists of a liquid nucleus of fused matter at a very high temperature, covered by a solidified crust of matter chemically much the same, but the materials of the uppermost strata of which at least have been dislocated, broken up, redeposited, and variously arranged into surface formations by the long-continued action of those superficial agencies with which the geologist deals.

11. The hypothesis of the existence of a liquid *nucleus* intensely hot rests mainly upon two grounds:—1st, the nebular origin of our globe in common with all others, as suggested by LAPLACE, by which the interior of a cooling globe must be *hotter* than the exterior; 2nd, the observation of temperature, which proves to increase with the depth, though in a very discordant sort of way.

12. That our globe is *hotter* within than on or near its surface is *a fact*, but that it possesses a liquid nucleus in a state of fusion is only an hypothesis, though a very probable one. The rate of increment of temperature with depth, far too hastily assumed to follow everywhere one simple arithmetic law, and the facts of terrestrial conductivity, so far as these are known, induced the belief that the solid crust is comparatively thin.

An unlimited supply of liquid lava was thus provided; the question was, what brought it up through this thin crust, and ejected it and other matters at the surface. By some, amongst whom Signor BELLI may be distinguished for the ability of his writings, the crust was assumed so thin as to merely float upon the liquid globe beneath; and a mechanism being conceived for fracturing the crust into separate fragments, and their density being rather greater than that of the supporting fluid, these sunk more or less in the latter and forced up the liquid lava in the spaces between.

13. This, which may be called the hydrostatic theory, has had several modifications. Another school of geologists, and the more numerous one, assumes that infiltrated water reaches the liquid and incandescent nucleus at certain points, and that by the elastic force of the stream formed the lava and other *ejecta* are forced up; and this, too, has had several modifications.

These latter views are no doubt true so far as the general notion of ejection by steam pressure is concerned; but insuperable difficulties appear to arise against the origin of the incandescent matter coming from one great reservoir, common, therefore, to all the volcanoes on the earth.

14. There is no evidence of universal connexion with such a common source of liquid rock beneath.

On the contrary, volcanic vents even closely adjacent show no proofs of direct intercommunication.

Their efforts are not synchronous; their paroxysms are isolated and subject to no recognizable periodicity; their ejecta, solid, liquid, or gaseous, though showing a great general similarity in all parts of the world, are far from being identical in chemical constitution or in temperature; these vary at different times, and show signs of secular change in geologic time.

The liquid or solid ejecta show no such uniformity at *all* volcanic vents as should arise from their coming all ready fused from one universal reservoir, the contents of which, at the same depth, it has been supposed there is no ground for assuming to differ in composition; while they *do* show very distinct indications of having some relation to the rocks directly through which the vents pierce, or over which they are posited.

None of these difficulties, to which others of a not unimportant character might be added, have ever been explained away.

15. A further difficulty was placed in the way when the investigations taken from astronomical considerations were supposed to prove that the earth's crust, instead of being very *thin*, must be very *thick*. It was not inconceivable that the liquid rock of the nucleus might in some way reach the surface through ducts or fissures in a shell of 20 or 60 miles thick; but it was at least more difficult to conceive, if not quite inconceivable, by what force and how it should be propelled through ducts of 800 or 1000 miles or even more in depth. It is quite a separate question what degree of weight should be attached to the celebrated paper by the late Mr. W. HOPKINS, F.R.S., on Precession and Nutation, as proving a thickness of solid crust exceeding 800 miles or so.

The difficulties to his reasoning, arising from his neglect of viscosity and friction-

hold between the solid crust and the liquid nucleus at the surfaces of contact, have been forcibly stated by M. DELAUNAY (and have certainly been underrated generally, and, indeed, have only been met by the mathematicians who have been the partisans of Mr. HOPKINS's views), by showing that if the physical difficulty of such viscosity &c. be admitted, other mathematical difficulties must arise if we cling to the method of Mr. HOPKINS as capable at all of giving any answer to the question of thickness of the earth's crust, which the writer believes it is not. That that thickness is not small, however, the writer believes to be the fact upon considerations wholly different from those of Mr. HOPKINS; and that view receives support from the investigation of Sir WILLIAM THOMSON as to the rigidity of the earth, at least with those who admit the sufficiency of the physical data upon which his mathematical reasoning there is founded. The effect, however, of this conjunction of the reasonings of the physical astronomer and of the geologist has been to raise a new difficulty for both.

16. The geologist, chained down under 800 miles or more of solid rock, the real thickness of which he is in no condition to *disprove*, cannot get his liquid lava sea beneath to the surface; and he has no other source for volcanic heat and ejecta to suggest.

The mathematician has the *fact* before him; volcanoes exist. He admits the difficulty of the geologist, and meets it by the most lame and gratuitous hypothesis of lakes or isolated masses of liquid fused rock existing at different points and at different depths (which depths, however, must, on the whole, be shallow within the solid crust of the earth), and assumes that from these the volcanic vents are supplied.

17. Nothing can be more feeble and unconvincing than the attempt made by Mr. HOPKINS to give a rational explanation or support to this gratuitous and most improbable hypothesis, which, so far as the writer knows, that gentleman was the first to bring forward in his "Researches in Physical Geology," 2nd series, Phil. Trans. for 1842, Part II.:—"We are necessarily led," he says, "therefore to the conclusion that the fluid matter of actual volcanoes exists in subterraneous reservoirs of limited extent, forming subterraneous *lakes* and not a subterraneous *ocean*" (p. 51). He adds (same page), "If we find that the hypothesis of the existence of these subterranean lakes, at no great depth beneath the surface, does enable us to account distinctly and by accurate investigations founded on mechanical principles for the phænomena of elevation . . . . . then we have all the proof of the truth of our hypothesis which the nature of the case will admit of."

That is to say, if we admit Mr. HOPKINS's physical notion of elevatory force, viz. that it consists of the pressure vertically upwards of a fluid against the superincumbent solid crust, we may also admit his lakes. If, however, as we shall presently see, Mr. HOPKINS's fundamental conception of the nature of elevatory forces is erroneous and untenable, then the lake-hypothesis must stand alone and upon its inherent improbabilities. At p. 52 the only attempt made to produce a rational origin for these supposed fiery lakes is thus given:—"It would seem probable, I think, that their origin may be ascribed to the greater fusibility of the matter composing them; and their continuance in a state of fluidity may, I conceive, be accounted for partly by the same and partly by another, which I will proceed to explain."

This other cause, which occupies pages 52 and 53, is simply another hypothesis, that if the pressure of the superincumbent dome over the liquid lake be removed, partially or wholly, by its becoming a self-supporting dome or arch, then the relief from pressure thus arising must lower the fusing-point of the liquid material of the lake, and so keep it longer liquid.

18. There is nothing absolutely in any of the known facts as to the materials of our earth's crust to warrant our supposing isolated masses therein of far greater fusibility than the remainder, to say nothing of that material being *ex necessitate* penetrated by water from the surface, to which it is admittedly near; for without the water we can have no volcano. Other serious difficulties occur as we attempt to follow out this notion by a comparison with observed facts of volcanic action at the vents, for which space cannot here be afforded. We pass on to some remarks on the theories of elevation.

19. The words "elevation," "upheaval," "*Aushebung*," "*soulèvement*" have been continually employed by the geologists of all countries, but especially of our own, in the loosest way, so far as the forming of any definite conception of the play of forces or mechanism of the movements is concerned.

A wide survey of the writings of geologists proves, however, that the notion generally formed of a "movement of elevation" is that it is produced somehow by a force acting beneath a limited area and in a vertical line, or nearly so, and in a direction from below upwards. Commonly it is assumed or inferred that the pressure upwards, beneath the area "elevated," has been that of a fluid, gaseous, as by Von BUCH &c., or more or less perfectly liquid, as by most others.

20. It is true that some geologists (amongst whom was the late Mr. JUKES) had some not very clearly defined notions that such machinery of elevation would not account for the facts as to elevated masses observable in nature, especially the frequent smallness of area in relation to the abruptness and height of the masses elevated.

But that this notion, that elevation is produced somehow by nearly parallel forces acting radially to the spheroid, is even yet the one commonly maintained we need no proof beyond turning over the pages of the latest writers on geology. If we look at the figure on page 285 (2nd edit., 1862) of SCROPE'S 'Volcanoes,' we shall see evidently that such is the notion he formed; and, without citing further examples, it is that which HOPKINS distinctly enunciates as his fundamental conception of the matter in his paper on "Researches in Physical Geology" (Trans. Cambridge Phil. Soc. vol. vi. part i.), where he says (p. 10):—"The hypotheses from which I set out, with respect to the action of the elevatory force, are, I conceive, as simple as the nature of the subject can admit of. I assume this force to act under portions of the earth's crust of considerable extent at any assignable depth, either with uniform intensity at every point, or, in some cases, with a somewhat greater intensity at particular points—as, for instance, at points along the line of maximum elevation of an elevated range, or at other points where the actual phenomena seem to indicate a more than ordinary energy of this subterranean action. I suppose this elevatory force, whatever may be its origin, *to act upon the lower*

surface of the uplifted mass through the medium of some fluid, which may be conceived to be an elastic vapour, or, in other cases, a mass of matter in a state of fusion from heat. Every geologist, I conceive, who admits the action of elevatory forces at all will be disposed to admit the legitimacy of these assumptions." The first effort of our elevatory force will of course be to raise the mass *under which it acts*, and to place it in a state of *extension*, and consequently of *tension*.

21. If this fundamental notion be (as the writer believes it is) erroneous and opposed to all the facts observable in the great regions of elevation, *i. e.* in the mountain-chains of the world, then it must be admitted that the speculations which follow as to the formation of fissures &c. (in a word, the substance of the doctrines of this paper, however ingenious as a mathematical exercise) have no true reference to the facts as occurring in nature, and, promulgated with the authority of the author and with that sort of oracular sanction which mathematical symbols possess for those who are devoid of mathematical knowledge, have tended materially to retard the progress of a truer interpretation of elevatory forces.

22. If we were to assume (as has been done) that the appearance of the masses of the great continents above the sea-level was a work of elevation at all, it might be a case to which Mr. HOPKINS's notion could perhaps apply. But that the great continents have not been the work of such elevatory forces at all, but have resulted from the *deformation* of a cooling and contracting globe covered only by a thin and yet flexible solidified crust, sinking over great areas and relatively or absolutely rising over others, has been so convincingly urged by DANA and other American geologists that it is probably now admitted.

23. DE LA BECHE's notions as to elevation approached nearer to exactness than those of most of his contemporaries*; but to one man alone, CONSTANT PREVOST, belongs the honour of having clearly enunciated a true theory of elevatory forces, followed out by comparison with facts in nature, and by showing that these were inexplicable upon the notion of direct upheaval by a radial force from beneath.

* [To cite even by name all the authors whose works contain scattered passages from which notices may be gleaned (always more or less vague, disjointed, or even inconsecutive) as to the relation between terrestrial refrigeration and the formation of continents, mountains, &c., is here impossible. DE LA BECHE, in his 'Researches in Theoretic Geology,' 1834, pp. 121–162 *et passim*, writes far more clearly and connectedly than others of that period. His views, however, were anticipated by PREVOST. Those who desire to trace more fully the history of this branch of knowledge will find somewhat ample references to past authors in an able paper by Prof. DANA, "On the Geological Results of the Earth's Contraction," in Amer. Journ. of Science, 1847, vol. iii. ser. 2.

The leading idea of the present Paper (namely, the showing that the deformations producing continents and sea-beds, the elevation of mountains and depression of valleys, and the origination of volcanic heat and energy are all due to a unique cause at different stages of its long-continued action) could not have been anticipated by any of the many eminent men above referred to, by DANA or by the writer, because the imperfect state of the science of Thermo-Dynamics down to a later period rendered the leading idea itself impossible to them.]

N.B. Passages in the text or footnotes which, like the present footnote, are enclosed in brackets were inserted in March or April 1873.

24. The nature of the conception of this force, as more or less clearly shadowed forth by geologists, admits of no doubt, as regards French geologists at least, when we consider the force of their adopted term *soulèvement* (presumably *sublevare*, *sous-lèvement*, a lift by a force from beneath), and, indeed, is pretty evident in the German *Aushebung*; but the English "upheaval" and "elevation" admit of any cloudiness of conception or latitude of interpretation.

25. PREVOST'S ideas are scattered through numerous papers, extending over nearly twenty years, but are to be found in their most systematic and formal manner in vol. xxxi. part ii. of the 'Comptes Rendus' for September 1850, p. 461 *et seq.*, and, at an earlier period, in the 'Bulletin de la Société Géologique de France,' vol. xi. p. 183 *et seq.* His view is this, that, apart from the great deformations which hollowed out the ocean-beds, as to which he is not quite so clear, all elevations of the earth's surface of the nature of hills or mountains (in general the rugose contour of its surface) have been produced, not by vertical forces directly coming from some unknown deep-seated source, but by vertical forces, the *resultants of tangential pressures*, acting against each other in horizontal or nearly horizontal directions, and transversely to the ridges or lines of elevation, these tangential pressures originating in the contraction of the earth's crust by secular refrigeration. This view, which the writer believes to be true, has been followed out by DANA, the two ROGERS in America, by ELIE DE BEAUMONT and some other continental geologists, by showing how completely the observed facts fall into place and are accountable for by it, but oppose themselves to the notion of a vertical primary force.

26. The Rev. O. FISHER also, in an important paper "On the Elevation of Mountain-chains, and Speculation on the Cause of Volcanic Heat" (Cambridge Trans. vol. xi. p. iii, 1868), which the writer had no knowledge of until after the present paper had been written, has given support to PREVOST'S view, by proving the mechanical adequacy of the tangential pressure due to refrigeration to the elevation of the highest mountains of our globe.

Mr. FISHER'S views in the above paper as to the origin of volcanic heat are entirely different from those of the writer as here developed.

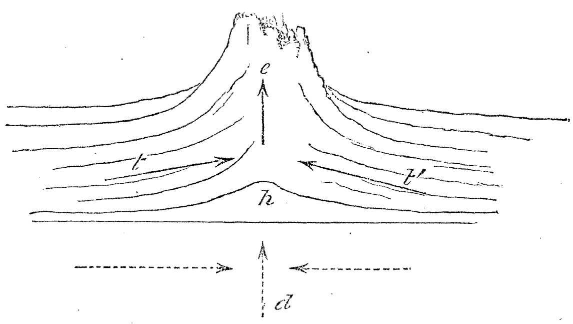
27. Upon HOPKINS'S view, all elevated parts must have *convex* surfaces, and, whether continents or ridges steep as the high Alps, must be *bombé* or dome-like in contour; they must all present the evidences of *tension* during elevation and in orthogonal directions. But the careful geological surveys and sections made since PREVOST'S day show that, on the whole, the sectional contour of large areas, much less those of continents, are not *bombé*, and that the transverse sections of all mountain-ranges prove their curvatures on opposite flanks to be not convex, but concave—a form which is generally the case in the fall of the land from all mountain-crests to the sea-level, as is evident by the concave form (approaching to something of a curve of the parabolic order) shown by the longitudinal section of all large river-courses.

28. The "fan-like" structure of the Alps (*structure en éventail*), so well elucidated by FAVRE and STUDER, VON LINTH, and other Alpine geologists, becomes a conclusive proof alone that the forces of elevation have been lateral compressions resolved into the

vertical. Thus the deficiency in density more recently found, or with good grounds suspected, as occurring beneath great mountain-masses receives its solution, whilst a greater density there would be the necessary result of direct vertical pressure.

29. Thus it is evident that the tangential, or nearly tangential, and opposing pressures t and t', resolved into the vertical e, by which the upraised mountain-mass is lifted, tend to reduce the density, if not to leave actual hollows, about h, *below* the level of the intersection of those three lines of force and *above* that at which the tangential forces are completely horizontal; but that a directly vertical lift, as by d continued on by e to the summit, must tend to produce increased density about h.

Fig. 1.



30. The intense plications and foldings over of strata and *absolute overturn* of whole mountain-masses, seen in all great regions of elevation, the tremendous bending, foldings over, and smashing up of beds, such as M. BURAT's sections of the Coal-formations of Central France or those of D'HALLOI and Von DECHEN of the formations of Belgium and of Westphalia, become thus accountable for, as are, indeed, all the salient phenomena presented by the surface and sections of our earth's crust, to follow out which in detail here is impossible.

31. One or two important results which follow from PREVOST's grand hypothesis, that all forces of elevation, in a word, the only known vertical lifting agent (volcanic vents excluded), are resolved or transformed tangential forces of compression due to contraction, and acting in antagonism at various but no very great depths beneath our globe's present surface, which as yet have attracted no attention, should be at least alluded to.

32. These tangential forces of compression, acting slowly, not at all points uniformly or alike at all times, and on the whole (as the globe cooled) with a diminishing intensity, act simultaneously, though unequally, over great areas of sectional surface of the earth's crust. Relatively to the tremendous crushing-power of the tangential forces themselves and to the long continuance of their action, the materials on which they act must be viewed as more or less plastic.

33. It follows that if by resolved tangential pressures acting against each other in one direction, or in parallel directions along lines or in planes, elevations are produced, the *compressions* in these directions must be accompanied by extensions in the orthogonal directions, by difference in resistance to pressure producing tensions.

34. Hence, by the final resolution of tangential compressions, surface-*fissures* may follow from these orthogonal tensions, so that generally the compressions shall give rise to crumplings up and crushing together of strata, to elevation, and to fissuring; in which last the phenomena may *simulate* the effects of direct elevatory forces, according with HOPKINS's hypothesis. And this is the reason why the actual directions of observed

fissuring in nature do, to a certain extent in one set of directions only, viz. in those orthogonal to the lines of horizontal pressure, coincide with those assigned by Mr. HOPKINS. But it further follows, admitting the relative plasticity of the less coherent formations of our earth's crust and the sufficient power of these tangential pressures for crushing up the others, that not only elevation may result from their vertical resolution, but that conditions must arise where these pressures act through considerable depths of materials differing in nature or in resistance, or in both, so that, by other analogous resolutions into the vertical or more or less toward it but in the opposite sign (*i.e.* downwards), *depressions* and hollows of any sort of form are producible.

35. Also that when these tangential and compressive forces, taken in a horizontal plane, make an angle in meeting greater or less (as may also happen in a vertical plane), then lateral displacements and all the phenomena of shifted strata or interrupted veins or faults can be produced.

36. We can also see that this view of the origination of the elevatory forces which have raised up our vast mountain-chains at once assigns a true and adequate cause for a limitation to their possible height; for as soon as the mass *already forced up* presses by gravity downwards upon the resolved vertical force e (fig. 1) to such an extent as to equal the resistance to crushing of the rocks beneath in the directions t and t', the further effect of the tangential pressures in those directions must be expended in crushing the rock between them to powder, where in some direction the solid can yield unequally, or in forcing off the material in some other direction than in the vertical. It is difficult to see how any limit is assignable to mountain altitude upon Mr. HOPKINS's hypothesis, unless by calling in other hypotheses for limiting the uplifting force itself, the intensity of which we cannot estimate in his case, inasmuch as its very nature is left unknown.

37. If, then, in PREVOST's notion we find an adequate and consistent theory of elevatory forces, it is the writer's belief that in it also (when followed to its legitimate consequences in one direction) we possess the clue that shall ultimately lead us to an equally simple, adequate, and consistent theory of Vulcanicity as here about being unfolded.

38. That the globe is hotter as we descend into it may be accepted as a fact, even from the very limited number of trustworthy experiments we yet possess; for artesian wells and small borings are alone to be relied upon, mines and coal-pits giving (for reasons not necessary here to enlarge upon) unreliable results.

39. And as we are certain that the geothermal *couche* of uniform annual temperature is everywhere above the existing temperature of the celestial spaces, so our planet must be a *cooling globe*; and if so, in accordance with all we know of the materials of which it consists, a *contracting globe*.

40. The rates of increment of temperature with depth, however, present great discordance even after we have excluded cases (such as mines or coal-pits at work) likely to contain accidental sources of error. Not only does the rate vary from 1° Fahr. in 15 feet of depth to 1° in more than 200 (MALLET, 'Neapolitan Earthquake Report,' vol. ii. p. 310), but in places within a mile or two of each other, and in the same formation, it varies as much as from 208 to 83 feet for 1° Fahr. (*op. cit.*). Even in the same vertical line of

boring the rate of increment has been found to vary at different parts of the descent. A large proportion of the observed temperatures certainly fall within the limits of 1° Fahr. in 30 feet and in 60 or 70 feet of descent.

The most complete and valuable collection and discussion of all the observations on record up to June 1836 which the writer has met with is to be found in a rare and scarcely known Inaugural Dissertation for the degree of Doctor of the Rheno-Trajectine Academy (4to, 101 pp., MÜLLER, Amsterdam, 1836), entitled "Disputatio Physica Inauguralis de Calore Telluris infra superficiem augescente," by A. VROLIK. The careful and laborious author places the following amongst his general conclusions:—"Variarum observationum autem eventus, adeo inter se discrepare ut certam incrementi legem pro unaquaque regione nondum statuere possimus." He believes it proved that in general the rate of increment is greater in plains and valleys than in mountains.

41. These conclusions have been sustained by the later observations made since 1836. DE LA BECHE, HERSCHEL, and BABBAGE enunciated more or less clearly a connexion between the increment of hypogeal heat and the conductivity of the superposed strata.

That was afterwards examined carefully by HOPKINS in one of his most valuable memoirs, that "On the Conductive Powers of various Substances, &c." (Phil. Trans. vol. cxlvii., 1857). Towards the conclusion he remarks, "On the whole, then, I cannot avoid the conclusion that the existence of a central heat is not sufficient in itself to account for all the phenomena which terrestrial temperatures present to us" (p. 835); that is to say, that a cooling globe, together with such effects as his experiments on conductivity of its materials warrant, are still insufficient to account for the observed discrepancies in hypogeal increment of heat.

42. One great source (if not the only one) for the unknown residual phenomena thus distinctly indicated by HOPKINS we hope presently to point out, and to prove its intimate connexion with existing volcanic phenomena.

43. Our present knowledge of hypogeal temperature, while thus and so far insufficient to sustain any very minute conclusions based upon annual absolute loss of heat by our earth, seems surely to support the fact that our earth is a cooling globe; while astronomical analogies and considerations of its figure seem equally to warrant the presumption that it has been a cooling globe at all times from a state of fusion; and if so, that from that period and up to the present it has been on the whole a contracting globe.

These are all the conditions we require to admit for the conclusions to be obtained by considering the sequence of the phenomena that have followed on refrigeration.

44. It may be remarked, in passing, that the general credence of geologists in the cooling at all of our globe has been more or less disturbed by the celebrated memoir of LAPLACE (Mécanique Céleste, tom. v. cap. iv. p. 72 &c.) "On the Cooling of the Earth as affecting the length of the Day," in which it has been taken for granted, by most geologists at least, that the great mathematician has irrefutably proved that our globe has not cooled sensibly for the last 2000 years.

45. With due reverence for the intellectual sovereignty of LAPLACE, the writer ventures

to suggest that his calculation really proves nothing as to what may be the actual facts in nature, because the physical data which he has employed are not such as actually occur in nature. The law of compression with depth adopted was objected to by Dr. Young; and the assumption that the coefficient of contraction is the same for the entire globe, whether liquid or solid, hotter or colder, seems to vitiate the results.

If a colder and consolidated crust possesses a greater density and a smaller coefficient of contraction than the liquid or solid but hotter matter beneath, by accretion and solidification from which the crust is gradually thickened, it is easy to see that such a relation may subsist between these quantities that the distance of the centre of gyration of the globe from its centre of figure may remain constant, either always or for very long periods, although the diameter of the earth as a whole may have diminished by contraction; and thus, since the moment of momentum must have remained unchanged, the angular velocity, upon which the length of the day depends, may have remained constant (or so nearly so that its variation may have been absolutely insensible) for 2000 years, or perhaps for a much longer period, notwithstanding that the world, as a whole, may have been losing heat all the time at a very sensible rate per annum, as seems to be the undeniable fact.

46. Let us take up the train of phenomena of refrigeration from the period when we may suppose the whole globe a liquid spheroid in fusion, rotating upon an axis inclined to its orbit as now, losing heat by radiation, and receiving that of the sun. Whatever the rate of refrigeration generally, it must have been *greatest* towards the poles; so that any solidified crust must have first formed about the poles and spread thence in two hemispherical sheets, getting thinner as they neared the equator, where they ultimately joined. From what we know by our mere furnace or laboratory experience of the effects of our highest temperatures on metals and on the materials that we must presume constitute the mass of our globe, it is certain that, at temperatures exceeding their fusing-points, they become more and more liquid up to some not yet known limit, and that at points a good deal above those of fusion they are all reduced to mobile liquids of extremely small viscosity.

47. On the other hand, it is equally certain that all metals, and such mixed materials as constitute rocks (acid and basic silicates), pass through a rapidly increasing phase of viscosity as they pass below the fusing-point on their way to ordinary solidity. It is this interval of rapidly increasing viscosity below the fusing-point and above that of complete solidification that enables platinum and iron &c. to be welded. This stage of viscosity in metals is very brief; but in earthy mixtures or compounds, such as the acid or basic silicates, it is much more prolonged, and the increase of viscosity towards the inferior limits of temperature much greater than in metals.

48. With the extreme fluidity of the molten spheroid at the exalted temperature that we must infer for it at the time of its first condensation from vapour, and after the first great chemical equilibrium of its elements (then entered into combinations analogous to those we now find in the globe so far as we know it), it is not conceivable but that

great currents of circulation must have arisen within the spheroid, by reason of the more rapid cooling at the poles than in equatorial regions, warmer currents proceeding superficially from the equator towards the poles, and colder currents returning about the axis of rotation, and dividing and ascending about the plane of the equator.

But the effect of these in equalizing the temperature of the superficial and deep parts of the spheroid would be small; it would, notwithstanding their influence, be always greatly hotter at the centre than at any part of the surface or near it. The exterior *couches* becoming more and more viscous as refrigeration proceeded, a *thin* solid crust would at length be formed over the whole spheroid, thickest at the poles.

This crust, while *thin*, though solid through a certain thickness, would be very nearly at the same high temperature as the viscous and the fluid matter beneath it. It would therefore still continue to part with heat by radiation at a rate not very far different from what the surface of the liquid spheroid did prior to formation of any crust.

49. But it is certain (as will be hereafter shown) that the coefficient of contraction per degree is less, for such materials as rocks are formed of, when in the solid than in the viscous or liquid state, and decreases as a function of the temperature down to the mean temperature of our present atmosphere.

It would depend upon the rate of surface-cooling, compared with that of the viscous matter below, and upon conditions of radiation &c., for which we have no sufficient data, whether the relations between the rate of cooling and of the contraction of the exterior and interior might not give rise to tensions within the *thin* crust; but upon the whole the effect would appear to be to produce tangential *pressures* also within the *thin* crust. To the compressions producible by these the thin crust would readily give way by wrinkling or folding over, or equally give way to tension by breaking to pieces. The density of the solidified matter in this thin crust was no doubt greater than that of the viscous layer beneath it, and the density of the last greater than that of the liquid matter still deeper; but the difference in density must have been small (as we shall hereafter see), the difference of temperature between such layers being so. Nor can we attribute any *great* difference in density to any abrupt molecular change in volume known to accompany the passage from the viscous to the solid state in the materials of rocks. [The differences in density observed in the passage of mineral compounds from the vitreous to the crystalline state, and *vice versâ*, are not great, and in those cases the cooling has been comparatively sudden. In the production of Réaumur's porcelain, the change by slow heating is preceded by abrupt cooling as glass. In our crust there was not abrupt cooling.]

50. The thin crust, even if more or less dislocated, would therefore probably still remain upon the surface, sustained by the viscous bed beneath and that by the liquid spheroid, and would not piecemeal sink down into the latter.

But should we assume it to do so, there appears the utmost improbability in the supposition, which originated with Poisson, and seems to be adopted by Thomson, that the solidified sheets broken from such thin crust and constantly renewed would

sink to the centre of the liquid spheroid, and *remain* there still solid, and so gradually, of such fiery débris, build up a honeycombed but solid nucleus, with liquid matter above it and a solid crust above all. When we take into account the excessive relative thinness of the original solid crust, a very few miles, perhaps only a few fathoms, and the small volume of its mass in comparison with the enormous volume of the liquid nucleus, through which the fragments of crust have to descend some 4000 miles, together with the enormous excess in temperature of the liquid approaching nearer the centre, it seems impossible to arrive at any rational conclusion, except that such sunken sheets of solid crust must be reduced to liquid fusion again by heat conducted into them from the heated liquid, the local temperature of which would be increased by heat generated by their fall through the liquid a distance nearly equal the earth's radius before they ever reached the centre; and assuming that the laws of gravitation *within* a spheroid, and the density of the crust and relative rate of compression with depth of the crust and of the liquid, would ever permit them to reach that centre, Poisson's hypothesis, which has been a stumbling-block in the way of more rational interpretation of these difficult questions of primæval geognosy, should, the writer submits, be set aside.

51. In cooling bodies, as the loss of heat in equal times is greater the greater the difference in temperature between the hotter and colder bodies, the rate of cooling of the globe, when its general temperature must have exceeded that of our hottest furnaces, and when, as we must presume, that of the celestial spaces was the same as now, must have been very rapid. The amount of contraction also (aided as the cooling rate was by the circulatory currents within the liquid before referred to) must have been proportionally rapid.

52. The viscous crust must be supposed greatly thicker than the solid sheet above it. The enormous amount of rapid contraction at this period was, in the writer's opinion, met by that deformation of the spheroid which hollowed out the ocean-bed to very much the general outlines that we now see, and so assigned the general forms to the continents.

This cannot be viewed at present as much more than a conjecture; still it is one not without support both from authority and from facts. It seems impossible to assign any other machinery or one adequate in force to a *soulèvement* or subsidence so vast, extending unbroken over such vast areas, and which should give to the sections of both land and ocean-bed their actual forms, which (without regard to mountain and valley) are but vast flat, raised-up plains, dipping more or less abruptly down to equally shallow, flat, saucer-like plains of ocean concavity. Lateral thrust or radial pressure at local centres seems here equally inapplicable. It is for the physical astronomer to investigate the causes of this very striking and obviously not chance-configuration of this deformation on the surface of our globe. [It should also be remarked that a certain order in the form of the land (coast-lines) and sea indicates that these are not the mere result of superficial actions, such as deposition, denudation, and local oscillations of level, but

must be ascribed to forces which acted (along with these or before them) upon a vast if not universal scale, and at far greater depths beneath the existing surface.]

53. It is a remarkable fact that the western coasts of nearly all the great masses of land are the steepest, and that even without reference to littoral ranges of mountain, such as the Andes or those of the Malabar coast*.

Now this is just what we should expect if the ocean-bed were the result of the depression of the surface of the spheroid by the deformation supposed; for the matter in the crust that descended, having a velocity of rotation due to its higher radius (that prior to depression), would, in descending to the level of the ocean-bed, tend to fall (and by so much of the energy of its whole mass of matter to push) to the eastward of the true vertical, just as a weight let drop from a height falls to the eastward of the vertical. This effect would be greater as the deformation was more rapid, but would never disappear while it occupied some finite time. [The rapidity of elevation has at all times depended upon the rate of refrigeration; and the latter, whatever its actual rate at any given epoch, must have been greater as the temperature of the earth's surface was higher as compared with space. Thus, when the temperature of the surface was, let us suppose, 1500° Fahr., the annual rate of elevation of great mountain-chains may have been considerable.] If, on the contrary, the continents were *raised* up by some inconceivable force of elevation, no such phenomenon would present itself; and it seems impossible to see how this could have occurred without the production of vast cavities beneath.

54. These sudden contrary flexures at the junctions of the continents and sea-basins initiated lines of fracture and of weakness in the early crust, along which we find ranges of mountains and volcanic action now.

55. The ocean-bed deformation took place most probably long before the surface-temperature was such as to admit of its being filled with a permanent ocean water, though probably for a long period depositions of boiling water, at an enormously high boiling-point, took place locally and in basins here and there, beginning towards the poles, alternately boiling away and being redeposited, and being thus attended with torrential rains and with great surface-currents and deluges of hot water.

56. To the solvent power of these and to their violent carrying and denuding action and prodigious powers of breaking up the rocky mass of the primordial crust, by comparatively sudden heating, by the continued conduction from below (in antagonism with the sudden cooling locally), by precipitation of comparatively cold water from above, must be ascribed powers of comminution and alternate denudation and deposition such as later geologic time, much less historic time, presents us with nothing but the faintest resemblance of.

And to this early machinery may be ascribed the production, as by a mighty mill,

* DANA (American Journ. Sci. 1847) has, however, shown a different possible cause for the greater prevalent steepness of mountain-ranges on one of their flanks, viz. the greater intensity of the tangential pressure at the less steep flank, or smaller resistance of the materials of the opposite one. This, however, gives no solution of the very general fact of western flanks being the steepest.—March 1873.

of the vast mass of comminuted material which has formed the assumed azoic and yet more or less stratified rocks, or of others of like character that may have preceded them.

57. At length the crust became further indurated, and with its viscous bed beneath greatly thickened and convection cut off by it, as well as its coefficient of contraction diminished, arrived at that state in which it was thick enough to *transmit* tangential stresses within its own sheet. When the balance of contraction in a given time between the crust and nucleus beneath was such (the latter still in great part liquid, contracting fastest because of its large coefficient of contraction) that the crust began to transmit compressive tangential strains within its thickness, these were of great energy and acted through great ranges. And now the crust, thick and stiff enough to transmit these strains through great distances, began to corrugate and double up upon itself, and thus to elevate the greater and lesser mountain-chains, many of these overthrown and again elevated, whose united volume, could we obtain it, would be in some degree a measure of the total contraction of the spheroid, from the epoch of the crust having become thick enough to transmit powerful tangential thrusts down to a comparatively late geological period; for whatever small changes in height may have occurred and are even now taking place (as in Sweden, &c.), no *great* elevations of mountain-ranges seem to have taken place in posttertiary or pleistocene periods.

58. In this state of thickness of the crust of the cooling globe, on principles already alluded to in treating of the nature of elevatory forces, the great elevation produced by transmitted tangential *thrusts* must have been acccompanied by large *induced* or *secondary* orthogonal *tensions*, or extensions due to *difference* of thrust, and thus by great fissurings and by most of the great phenomena of faults and dykes and crystalline-rock intrusions which we observe.

59. Lastly, as refrigeration still proceeded and the crust became far thicker and more rigid, approaching in both respects the state in which it most probably now exists, began that balance and play of forces the effects of which are now recognized as volcanic, with their attendant phenomena of earthquake, thermal springs, &c., and upon the precise mechanism of which we must now enter more largely.

This, which we may call the period of volcanic regimen of our globe, in comparison with the brief span of human history, appears at the first glance one of uniformity.

Yet it is one of decreasing energy, as has been the case with each of the epochs of refrigeration preceding it; they could not have been otherwise if resulting from the thermal energies of a cooling globe.

60. We thus recognize four great stages of the operation of refrigeration, each less in energy than the preceding, but all due to the one all-pervading motive cause, loss of heat.

1st. The formation and the deformation of a thin and flexible crust, and with it of the superficial parts of the viscous or liquid *couche* beneath, shaping out the land and sea boundaries.

2nd. The splitting and breaking up of that crust and the more rapid (but irregular) cooling &c. produced by the first partial deposit of water upon the

globe, while a large portion (of its surface even) may have been still red-hot, and communications partially open with the viscous interior; this accompanied by severe local tensions and compressions.

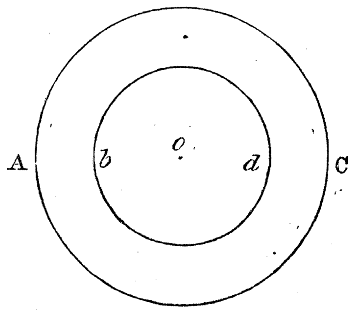
3rd. The increase of rigidity in the thickened crust, when it became able to transmit tangential thrusts due to contraction; and these (resolved as has been explained) elevated the mountain-ranges and originated the hypsometric configuration of the land, the establishment in regimen of the ocean and of the water-courses of the world, and with these the beginnings of climates fitted for successive forms of life.

4th. The epoch of a *greatly* thickened and stiffened crust with a comparatively slow rate of cooling of the globe, being the regimen which now exists, and of the play of the forces of contraction by cooling still going on, in and from which we now hope to show that volcanic action proper originated, and is preserved apparently uniform, though with a constantly decreasing energy.

These divisions in the progress of secular cooling are not merely arbitrary, for each is marked by a different way in which the effects of the contraction show themselves. The rate of contraction, enormous at first, because the rate of cooling was then most rapid and the coefficient of contraction the greatest, is met by deformation, afterwards by splitting up of the thin crust under tension (accompanied by welling up of the liquid matter from beneath), then, with a thicker crust, by folding over and elevation of ridges, primary tensions having given place to compressions, until at length the last and existing state of things is reached in which the crust has become of great thickness, and covers the still hot nucleus all over as a comparatively rigid spheroidal shell or dome; these stages more or less overlapped each other in their successive development.

61. How, then, is the contraction in volume of our globe which is now going on met? for if admitted to be cooling it must be still contracting.

Fig. 2.



We have a globe (A C) subject to the laws of gravitation, composed of a relatively thick crust, and enclosing a hotter nucleus, which is losing its heat by convection to the crust, through conduction and radiation from the surface.

The coefficient of contraction of the matter of the nucleus, which is at a temperature much higher than the crust, is greater than that of the material of the latter.

If the shell covering the nucleus were still thin and flexible it would give way, as formerly, by plication, and fall downwards by gravity as the diameter of the nucleus gradually diminishes by contraction. But the crust is now a thick rigid covering dome; its dimensions, owing to its existent small coefficient of contraction, are diminishing slowly as compared with those of the nucleus, whose coefficient, owing to its higher temperature, is much greater. Hence in fig. 2, taking the nucleus as $b\,d$, the thickness of the crust being $A\,o - d\,o$ (*i. e.* the integral thickness of all the crust, whether absolutely solid

or solid enough to resist as an equilibrated dome), the more rapid contraction of the nucleus causes it to tend to shrink away from the interior surface of the crust-shell, and to leave the latter partially unsupported; or, if we assume adhesion between the two at the surface of contact, then the contracting nucleus as it shrinks tends to pull all parts of the spherical crust-shell along with it, radially towards the centre.

In either case the direct result is to produce mutual pressures by tangential strain in all parts of the crust-shell, which, as being sufficiently thick and rigid, resists these forces as a dome.

62. If the dome be incapable of resisting the tangential pressures thus produced, it must crush by the mutual compression of its parts; and if not everywhere homogeneous, or if the pressure be greater in some places than in others, it will crush partially along the weakest places or those most strained.

But as the material of the rigid dome or shell is, like all other matter, compressible as well as amenable to shearing-strain, so these tangential pressures must be attended with compressions, and therefore with *motion* in the particles of the material.

63. The "work" produced by these mutual pressures and motion is transformed into *heat*, which either heats the whole spherical shell uniformly, if the compressions or work be uniformly diffused through its volume, or heats certain points or lines or planes within it more intensely if the work due to intervening volumes be more or less transferred to those places. Let us fix this in the mind by an illustration.

Suppose we have an egg with its hard covering shell and softer interior, and that by some means we could cause the soft interior to diminish uniformly in volume so as to shrink away (or tend to do so) from the interior of the shell, and thus to leave the latter exposed to the pressure of the atmosphere. As soon as this takes place tangential pressures are produced within the shell, so that (if we imagine it a form of equilibrium) its particles mutually approach each other. The temperature of the whole shell will therefore necessarily rise, by the internal work thus produced; and, for illustration, we may suppose the increase of temperature the same in all parts of the shell.

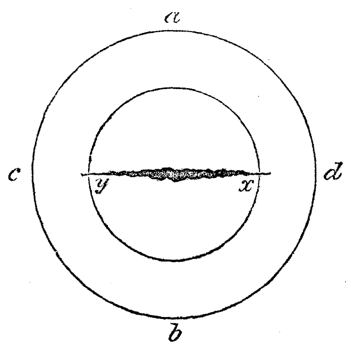
But let us now assume that the shell had been previously cracked through, along various irregular lines upon its surface (as we see eggs in boiling often are), or had like lines of weakness in it. As the compression will be greatest along those lines where the resistance to it is least, so the chief amount of the work of compression will be transferred to those lines of fracture or of weakness, and the increase of temperature produced by the greater part of the internal work will cause the parts of the shell about these places to become much hotter than the intervening parts of the unbroken shell. What the pressure of the atmosphere upon the unsupported egg-shell here does, is done by gravitation of the crust-shell itself, and attraction of the nucleus in the case of our globe.

64. Another and nearer illustration, indeed an almost parallel case, is presented by the phenomena that actually attend the cooling of large spherical or cylindrical masses of iron. A moderately sized sphere of *cast* iron which has been cast in an iron mould so

that the exterior becomes rapidly cooled to a rigid state, leaves a central cavity when cold, the last consolidating particles having parted company towards the centre under the constraint of the rigid shell, whose diameter does not diminish so as to accommodate the total contraction.

All very large cylindrical forgings of wrought iron, say of 2 or 3 feet diameter, become "flawed" as they cool, tearing asunder in greater or less irregular diametral and axial planes, as in section, fig. 3. The whole mass was at first, say, at a yellow heat and plastic; the outer coats cool first and most rapidly, and form a rigid arch (whose thickness we may suppose $\frac{cd-xy}{2}$) covering the still hot interior, and whose dimensions are determined by the volume of the latter which is grasped by the rigid arched shell. The cooling of the interior proceeds, the heat being lost through the rigid shell; and as the shell cannot yield to the radial pull of the contracting nucleus $y\,x$, the diminution in volume of the nucleus is met by its tearing asunder in some one diameter, $y\,x$, and by deformation of the cylinder itself in the orthogonal direction as soon as this rent has been formed.

Fig. 3.



The exact play and interdependence of these forces, here imperfectly stated, have been fully developed by the writer in a paper "On the Coefficients of Elasticity and of Rupture of Wrought Iron, &c.," printed in the 18th volume of the Minutes of Proceedings of the Institution of Civil Engineers, pp. 307–312, to which he would refer. The case of a cylinder is (as regards illustration) the same as a sphere, as the material of the nucleus is there too viscid to escape endways.

65. In the case of a *small* cylinder or sphere with a relatively very thick and rigid shell, the final contraction of the nucleus is met, as we see, by tearing asunder of the nucleus by radial tension, and not by crushing of the material of the covering dome by tangential compression.

But it is obvious that the one or the other must result, as the sum of the tensile resistances of the nucleus or that of the compressive resistance of the covering dome is the greater.

And in a very *large* globe such as ours, where the active force is gravitative in all its parts, and where transverse section in the plane of any great circle enormously exceeds that of the shell, even if we suppose that some hundreds of miles in thickness, it is obvious that it is the shell that must yield by crushing up, and not the still heated nucleus by pulling asunder, though it is quite possible that in some remote future stage of final refrigeration a diametral rent might occur, or some central cavity be left in the *then* cold and rigid nucleus itself of our globe.

66. The contraction *now* going on in our globe by its secular refrigeration is met therefore by the compression of the colder and more rigid covering shell and crust, and by the crushing of its material along lines or at places or planes of weakness by their mutual

pressures, produced by the gravitating of the shell itself towards the contracting and attracting nucleus beneath it.

The work thus developed is transformed into heat; that heat is greatest along those lines or planes or places where the movement and pressure together constituting the work are greatest. Along or about such places of concentrated compressive and crushing work the temperature may locally rise to a red heat, or even to that of fusion of the rocky materials crushed, and of the pressing together walls themselves adjacent to them. This, then, is the writer's view of the real nature and origin of volcanic heat as now produced in our globe; it comes not from a free communication with a primordially and still fluid interior ocean, nor from such communication with isolated lakes of liquid rock, which have no probable existence, but is *produced* below the places where it appears in volcanic vents, or beneath and adjacent to them, by the mechanical energy of the compressed-together shell, as that falls down by gravity upon the contracting nucleus; and the heat so *produced locally* is again *consumed locally* and disposed of by the origination of chemical work and by reconversion into mechanical work, chiefly of ejection.

And thus though volcanic energy as we see it on our globe is *not the direct* product of primordial heat of fusion, it is indirectly due to the loss of that heat, being simply one result of the cooling of our globe and of the acknowledged laws of gravitation.

Volcanic energy (or vulcanicity in general, as comprehending in it earthquakes and other of the so-called plutonic phenomena of geologists) may therefore be defined, according to the writer's view, as follows:—

Definition.

67. *The heat from which terrestrial volcanic energy is at present derived is produced locally within the solid shell of our globe by transformation of the mechanical work of compression or of crushing of portions of that shell, which compressions and crushings are themselves produced by the more rapid contraction, by cooling, of the hotter material of the nucleus beneath that shell, and the consequent more or less free descent of the shell by gravitation, the vertical work of which is resolved into tangential pressures and motion within the thickness of the shell*.*

68. It has been pointed out that in the earlier stage of secular cooling, when the immense contraction was met by *deformation* of the exterior portions at least of the spheroid, great lines of weakness through sharp curved bendings and fractures were produced. All subsequent action has tended to increase the number and extent of these; and it will pro-

* [The production of heat as a consequence of the condensation of gases or vapours in progress towards liquefaction or solidification has been noticed by Mr. Herbert Spencer in his "Essay on the Nebular Hypothesis" (Westminster Review, July 1858). It need scarcely be remarked that this is altogether different from the source of volcanic heat here pointed out, nor has the writer's view been in any way anticipated by Mr. Spencer.]

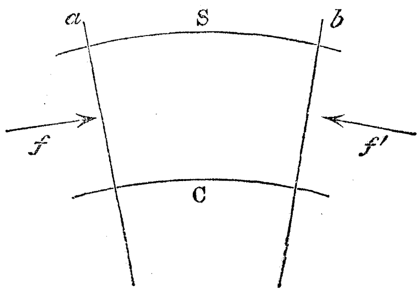
bably not be denied by geologists that every thing we know of points to the existence of great sweeping lines of weakness and of broken continuity in the crust of our globe, as, for instance, round the shores of the Pacific Ocean. Beneath and adjacent to such lines we must suppose the rocky materials fractured and broken up, and over large areas and to great depths reduced to discontinuous fragments closely pressed together and gripped in contact.

It is just along and over such places that we find volcanic vents. Along such planes of weakness, extending to great but unknown depths, the temperature should be highest by the concentration thereabouts of the movements of the compressed crust, as already shown.

69. But such heated areas are not confined to merely vertical planes, at least for so far in depth of the entire solid crust as consists of superposed and interposed formations differing in mineral character, or (what we are here alone concerned with) in compressibility and conductivity, one or both.

70. In fig. 4, if S C represent the depth of the earth's crust, a portion of which of unit thickness is comprised between the vertical planes *a* and *b*, and submitted to compression by the opposite tangential forces *f* and *f'*, then if the entire mass in depth between C and S be of homogeneous material of uniform compressibility, any rise of temperature produced by compression will be uniform throughout its mass.

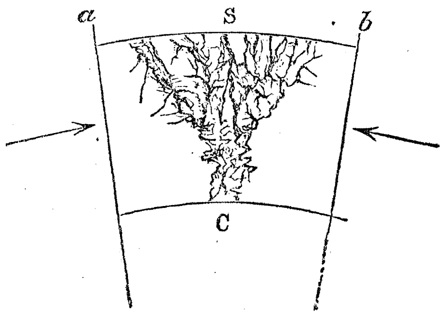
Fig. 4.



But if the compressibility be *less*, say towards C, than higher up, then, supposing an adequate force acting equally upon all points of the planes *f* and *f'*, and that the *more* compressible material is not reachable by the pressures until after the less compressible material shall have given way, the temperature will not be uniform, but will be greatest where the work of compression is so, and thus we should have variations of temperature with depth in any gradual order, whether ascending or descending.

71. This will also, though in a less regular way, be true if, as in fig. 5, the mass *a* C *b* include a great plane of weakness, due to fracture &c., extending in depth in a variable way. Thus if the material be more compacted towards C than higher up, and so less compressible, and if, as before, the tangential pressures *f* and *f'* act uniformly upon the vertical planes *a* and *b*, so that they can only approach each other at the rate the less compressible matter about C gives away, then must the highest temperature be developed about C, where there is the largest amount of work.

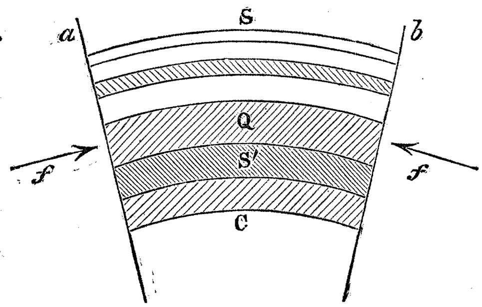
Fig. 5.



72. Thirdly, let us suppose the mass *a* C *b* to consist of several beds or layers of various

compressibilities and submitted to the forces f and f' as before; and, further, suppose that, for example, the bed Q (fig. 6) is less compressible than at S′ just below it, or than other beds in the series. Then, under the conditions, as the work of compression is greatest in the bed Q, that bed will, if its compressibility be everywhere alike, be uniformly hotter than the adjacent beds.

Fig. 6.



There will here be not a vertical or nearly vertical plane or plate of increased temperature, but a horizontal one.

73. The greatest amount of *work* may not be always in the least compressible rock; for as work is the product of pressure and *motion*, a soft easily yielding rock with a *large range* of compressibility may, by the application of a given amount of pressure, produce the greater amount of work.

74. The extent to which a bed of rock may thus be heated by compression *without any crushing*, i. e. by pressure far within its elastic limits, may be illustrated by an example. In the writer's own experiments on the compressibility of the rocks at Holyhead (Appendix to Account of the Earthquake Wave-Experiments made there, Phil. Trans. 1862, vol. clii. pp. 663–676), he found that certain quartz rocks and certain slate rocks both bore about 12,000 lbs. per square inch before their elastic limits were passed, the total compression at this pressure being 0·13248 for the quartz and only 0·04464 for the slate upon the unit length. The foot-pounds of work in compressing each to this amount (from which the rock would recover when released), divided by J (Joule's equivalent), shows that a prism of a foot square and 100 feet long of each rock would develop the following British units of heat:—

Quartz = 295·200.
Slate = 100·800.

If consisting of these rocks, therefore, and both compressed by precisely the same force, 12,000 lbs. per square inch, the bed Q (fig. 6) of quartz would be nearly three times as hot as that, S′, of slate beneath it, and the former would communicate its heat to the beds both above and below.

75. In this we see, then, one very sufficient cause for great inequalities in the rate of increments of hypogeal temperature, which, so far as the writer knows, has escaped notice, and which did not occur to Mr. Hopkins when (see his paper, Phil. Trans. vol. cxlvii., 1857, alluded to in a previous part of this paper) he discovered that central heat and difference of conductivity alone were not sufficient to account for the phenomena of increase of heat with depth. Indeed, as we shall see before concluding, the perturbations of the hypogeal temperature arising thus from intestine pressures and motions within the earth's solid shell *must* be far greater than has hitherto been suspected, and may amount to a very large fraction of the heat received from the nucleus.

The heat thus produced by such intestine work is, so to say, actually generated within

that shell, and in amount is dependent not so much on how much is received from below as on the amount of *contraction* of the material of the nucleus, which is, of course, a function of its total loss of heat in all ways. As the heat thus developed unequally by compression at different depths may vary, we at once obtain an explanation of what has been observed at Dukenfield shaft and elsewhere (namely, discontinuity in the series of heat increment), and perceive how a warmer or a colder *couche* may be interpolated. In fact we have a real source of perturbation distinct from difference of conductivity and presence of percolated water, which alone engaged Mr. HOPKINS's attention.

76. But the evolution of heat within the solid shell by the variable compressibility of the superposed formations thereof does not end with the compressions of the material of each or all the beds.

Any two superposed beds, such as Q and S′ in fig. 6, exposed to the same compressing force, as they have unequal coefficients of compressibility, *must slide upon each other*, and so produce frictional and disintegrative work between the sliding surfaces; this also becomes transformed into heat, and further tends to raise their own temperature and those of adjacent beds.

77. Lastly, we must take into account that the tangential forces cannot be always, as we have assumed in $f\,f'$ (figs. 4, 5, 6), uniform at all depths; for independent of any general law connecting the gravitation of the shell with those tangential forces (to which we shall presently refer), the mere inequality of resistance at various depths, which we have shown, must derange that equality of pressure, and even within certain limits change its direction locally from being strictly tangential into directions more or less oblique, both vertically and horizontally.

Although we *know* nothing of the constitution or order of the materials constituting even the solid shell of the globe deeper anywhere than perhaps 70 miles at most (inferentially), and to perhaps 25 miles by observation and inference, still the discussions of DUROCHER and others warrant our assuming it, to a far greater depth than above, as not differing greatly from the harder crystalline rocks of the surface, and indicate that below the stratified deposits, say below 25 miles or so in depth, the material may be presumed, with high probability, to be much more uniform, less shattered, and denser than near the surface.

78. If we have thus discovered a true and a sufficient cause for great local elevation of temperature within the solid crust of our globe, it is submitted that we have really discovered the origin and nature of volcanic action, and proved it to be only part of the acknowledged cosmical machinery of our globe, independently of any question as to how hot it was originally, or what length of time may have since elapsed, or what may be its internal temperature now (save that the interior of the globe is still hotter than the exterior, and that the whole is cooling), or whether the nucleus be liquid or solid, or the shell be thicker or thinner.

For, a sufficient source for the high local temperature at *some* depth below volcanic vents being discovered, the presence at their *foci* of water, fresh or salt, completes

the whole machinery by which these huge heat-engines work; and with the two together, all their ascertained phenomena as to ejecta and other products admit of easy explanation.

We shall recur to these and compare the chief of these phenomena and of plutonic action generally with our cause for the local production of the heat itself, though necessarily briefly. For volcanic phenomena such as have always been known to us in historic time or by traces left before that, and which are characterized by a general uniformity of products and mode of action all over the globe, as well as in all time, differing only or mainly in intensity merely, the necessary *coexistence* of some source of high local temperature and of water to form steam is required. It is obvious, therefore, that no such volcanic action as we are now acquainted with can have existed on our globe prior to the deposition upon it of the great masses of superficial waters penetrating its solid materials by capillarity and infiltration to vast depths—that is to say, not until after the external surface of the globe had permanently fallen to the temperature at which liquid water could deposit and penetrate the earth's crust; and this fixes an anterior limit in time earlier than which vulcanicity, as we now know it, cannot have existed upon our globe.

79. We have no very precise data for fixing this commencement of existing volcanic action in the geologic scale of succession, but it probably does not go back much beyond the end of the Secondary period, if so far. Prior to that vulcanicity seems chiefly to have been developed in the welling up of huge volumes of liquid rock between severed masses, or masses of heated dust or so-called ash, and probably in other ways, but without ejecta due to elastic steam, though possibly to such occasionally due to gases, but in any case to have been different in its action from the present system, and to have formed but a part of the machinery of folding over and ridge elevation of the earlier stages of cooling and contraction.

[It is not impossible that volcanic vents, or other sufficient evidences of true volcanic action of the explosive character now in play, may hereafter be discovered in the older sedimentary formations. The so-called deposits of "volcanic ash," the trap-dykes and porphyry-bosses of the Silurian rocks of the south of Ireland and of North Wales, &c., are evidences of igneous action indeed, but of that hydrostatic character which preceded the explosive volcanic action of the present epoch.

Some of the phenomena of explosive action are occasionally observable in igneous formations undoubtedly not volcanic, as the greenstone or trap-boulders and pebbles found by the writer imbedded in the great greenstone or trap-dyke of Galway (Trans. Roy. Irish Acad., 1834); but no sufficient evidence exists, so far as his knowledge goes, of any volcano, properly so called, existing in Silurian times, nor for long after. Nor, if the existence sporadically of such were proved, would that controvert the writer's view, that the great system of explosive volcanic vents as now established on our globe does not date back *in the main* further than above stated. No precise boundary in time can be assigned for the passage from the hydrostatic to the explosive

system. The change was gradual; and just as the epochs of land and sea forming by deformation overlapped that of mountain building by crumpling, and that again has overlapped in time our existing epoch of volcanic crushing and explosive action, so did the great epoch of hydrostatic igneous action overlap more or less the commencement of the existing volcanic era. The more ancient form of igneous action, by which the enormous trap protrusions &c. were poured forth upon a scale, as lately observed in California, wholly inconsistent with existing volcanic forces, continued in force down to comparatively recent periods, as of the Chalk, and may even yet be going on possibly under the sea. But two main characteristic facts remain—namely, that the most ancient igneous workings were hydrostatic and not explosive, while the existing or volcanic activity (properly so called) is explosive and is not hydrostatic; and, secondly, that upon the whole this last or existing form of igneous action (the explosive) does not, when viewed largely, date back further than to some part of the Secondary period, and that a preponderant amount of it is of still more recent date.]

80. It follows, therefore, that a like machinery of volcanic action to that now existing in our globe cannot have existed in any other planet anterior to its surface having assumed such thermometric conditions as enabled water or some equivalent fluid to become permanent upon its surface. That temperature might be very different from ours as now existing, and was once no doubt far above 212° upon our globe.

But it does *not* follow (as has, indeed, already been suggested) that in a planet or satellite constituted very differently from our globe volcanic action may not be maintained, for a longer or shorter time, by chemical actions, or by these and mechanical ones together, of a far different nature from the vulcanicity seen upon our globe—such as the evolution of gases from liquid or solid matter at one temperature and their absorption at others, as in the case of melted silver and copper absorbing oxygen, or of the numerous cases of such chemical actions in compounds discovered by TESSIE DU MOTAY, or in many other imaginable ways.

81. The writer is now called upon to show that, assuming the origin for the heat thus produced, which is the moving energy of the volcano, the conditions are such as to prove it to follow from forces real and adequate to the result.

He proceeds to do so, and for this it is necessary to show:—

1st. That the gravitation of the unsupported or but partially supported solid shell is adequate to crush into powder all the materials of which it consists, and that no matter how thick the shell may be unless equal to the whole radius.

2nd. What is the total amount of contraction of materials analogous to the rocks of the solid shell, between their temperature of fusion or one above it and that of our atmosphere now.

3rd. What is the mean work per unit of weight and volume necessarily expended in crushing to powder the rocks of which the solid shell consists, and what is the amount of heat due to the transformation of such work.

Lastly, we have to compare these results and apply them to the actual phenomena of volcanic action on our globe.

The first of these involves a mathematical investigation only, the last two rest upon two extensive series of experiments which have been made by the writer and are here to be detailed.

82. First, then, the unsupported solid shell must crush by its own gravitation and that of the nucleus, if of any solid material known as part of our globe.

LAGRANGE, in his 'Traité de Mécanique Analytique,' cap. iii. sec. ii. (statique), "Sur l'équilibre d'une surface flexible," &c. (Bertrand's 4to edition, Paris, 1853), has given, though in an involved form and without proof, a theorem which is applicable to this question. This theorem has been reduced to simpler form by Professor S. HAUGHTON, F.T.C.D.*, who has applied it to a widely different subject from ours, and to whom the writer is indebted for having had his attention directed to its applicability to the present one.

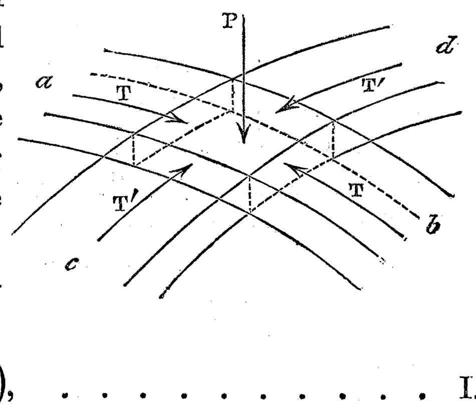
A proof of the theorem has been since produced by Professor R. S. BALL, of Dublin (Phil. Mag. vol. xxxix. pp. 107 & 108, Feb. 1870). The theorem may be thus stated:—

If a curved surface (of the nature of a hollow shell or membrane) be in equilibrium when exposed to forces acting normally to the surface everywhere, then the normal pressure at any point is equal to the force in the direction of the surface (or shell) at that point, multiplied into the sum of the reciprocals of the principal radii of curvature.

The pressure may be internal (as in a blown bubble), producing tensions, or may be external (as in the case before us), producing pressures or thrusts in the direction of the surface or tangential to it; and the surface may be extensible or inextensible, but it is one into the consideration of which cross or shearing strains do not enter.

83. Let P (fig. 7) be the normal pressure upon a unit of surface (square inch or mile) cut from a pair of intersecting ribbons of the curved surface, as $a\,b$ and $c\,d$, at right angles to each other and of unit breadth, T the tangential thrust on any of the faces of the unit square respectively opposite (which, as being small in relation to the radii of curvature, may be considered as plane).

Fig. 7.



Let the two radii of principal curvature (in ab and $c\,d$) be ϱ_1 and ϱ_2, then, as expressed in the theorem,

$$P=T\left(\frac{1}{\varrho_1}+\frac{1}{\varrho_2}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{I.}$$

T′ having the same value.

As regards the present application of the theorem, as the differences of ϱ_1 and ϱ_2 for our globe are very small (comparable with the difference between the polar and equatorial

* And by Professor MILLER in his 'Hydrostatics' (Cambridge, 1831).

radii) and scarcely sensibly affect the curvature of the surface within limited areas, we may consider our globe as spherical and $\varrho_1=\varrho_2$, whence equation I. becomes

$$P=\frac{2T}{\varrho}, \qquad \text{II.}$$

and

$$T=P\times\frac{\varrho}{2}. \qquad \text{III.}$$

If the curved surface be very largely elliptical, then the value of T given by this equation is not quite exact, as may happen in cases of local elevation or depression from local causes; but we are not here concerned with this further, as we shall view the globe as spherical without sensible error.

Now, to apply this to our case, let us suppose that we have a spherical shell of only one mile thick forming the surface of our globe, that it preserves its form by equilibrium, and that it is *wholly* unsupported by the nucleus below it, by the attraction of which, resulting in its own weight, it is urged towards the centre of the sphere.

84. Let the shell be of the most resistant rocky material which we know of, viz. granite or porphyry, crushing at 14 tons per square inch, or a little above 2000 tons per square foot, and of specific gravity=2·858, or weighing from 175 to 180 lbs. per cubic foot. Our results, which only require, as for illustration here, to be approximate, will not be materially affected by our assuming the diameter of the globe 8000 miles each of 6000 feet or 1000 fathoms, which will save a mass of figures. The result will be quite comparable with those we should obtain by taking the sphere at 7920 British statute miles. Now in equation III.

$$T=\frac{P\varrho}{2},$$

$\varrho=R$, the radius above taken for our globe, and therefore

$$T=2000\,P;$$

but in the case of the shell of 1 mile thickness, P=the weight in tons of 1 cubic mile of its material, *i. e.* of granite or porphyry, which, as a cubic fathom of water weighs very nearly 6 tons, will be $6\times2{\cdot}858$ tons per cubic fathom, or $1000^3\times6\times2{\cdot}858$ tons per cubic mile. Then $T=2000\times1000^3\times6\times2{\cdot}858$ tons, the horizontal thrust per square mile, which, divided by the number of square feet in a square mile, viz. $1000^2\times36$, gives as the value of T for the square foot

$$T=952666 \text{ tons},$$

which is more than 472 times greater than the crushing load given for granite or porphyry.

This simple investigation will sufficiently apply to any cubic mile in a like covering shell, even if 100 miles (or even more) in thickness, the variation in the force of gravity at that depth, $\frac{1}{40}$ of the assumed radius of the globe, being so small that we may here neglect it.

The subject may be viewed more intelligibly perhaps in the following way. As T is

proportionate to $\frac{P}{2}$, whatever value we take for R, a cubic mile of rock in any portion of the self-sustaining shell is exposed to a horizontal thrust upon each of its vertical faces $=\frac{R}{2}$ times its own weight.

That on our previous data will be 2000 times its own weight; but on the same data the crushing load for granite or porphyry being about 2000 tons per square foot, and the weight of the material 178 lbs. per cube foot, the modulus of cohesion, or the length of the column in feet of the material that shall crush by its own weight, is $\frac{2000\times 2240}{178}=25169$ feet$=4\cdot195$ miles of 6000 feet. But the height of the column of the same material representing the horizontal thrust is 2000 miles, or nearly 480 times the height of the crushing column.

In fact while the materials of the hardest and most coherent rock are crushed at 14 tons per square inch, the crushing force is here upwards of 6000 tons per square inch, if the equilibrated shell be, as assumed, wholly unsupported. It follows, therefore, that if $\frac{427}{428}$ of its total weight from attraction by the nucleus were supported by the latter, or that it were only free to descend by $\frac{1}{428}$ of the total gravitation, the materials of the shell must still crush.

85. If the thickness of such a terrestrial shell be considerable (as supposed by Mr. HOPKINS), the question arises at what depth from the outer surface will the *couche* of maximum tangential pressure be found.

[Let T and P be the horizontal thrust and the pressure, T as well as P being now referred to a unit of surface, and W the weight of a unit of volume, estimated at the depth at which it is situated, not as brought up to the surface of the globe, the equation for vertical forces in a sectorial element is

$$2\int_r^R \mathrm{T}r\,dr+\mathrm{P}r^2=\int_r^R \mathrm{W}r^2dr,$$

R being the radius of the globe, and r the radius vector from the centre drawn through the unit volume; whence

$$2\mathrm{T}r-\frac{d\,.\,r^2\mathrm{P}}{dr}=\mathrm{W}r^2,$$

the condition of equilibrium between T and P.

Two extreme suppositions as to the *physical* state of the globe (or its sectorial element) present themselves:—1st, that it consists of successive thin dome-like *couches* superposed, each rigid and self-supporting, so as to transmit no pressure to those below; or, 2nd, that no dome-like support exists, but that each *couche* transmits pressure to those beneath, as in the case of a liquid.

On the first supposition

$$\mathrm{P}=0 \text{ and } \mathrm{T}=\tfrac{1}{2}\mathrm{W}r;$$

on the second supposition P=T and

$$\frac{dP}{dr}=-W \text{ and } T=\int_r^R W dr.$$

If ϱ (a function of r) be the density, g being the force of gravity at the surface, then gravity at the depth r is

$$\frac{\frac{1}{r^2}\int_0^r \varrho r^2 dr}{\frac{1}{R^2}\int_0^R \varrho r^2 dr} g = \frac{R^2}{r^2}\frac{\int_0^r \varrho r^2 dr}{\int_0^R \varrho r^2 dr} g,$$

and

$$W=\frac{R^2}{r^2}\left(\frac{\int_0^r \varrho r^2 dr}{\int_0^R \varrho r^2 dr} g\right)\varrho.$$

Thus, for example, let ϱ be constant as to depth, then

$$W=g\varrho\frac{r}{R}.$$

Upon our first supposition

$$T=\tfrac{1}{2}gr\frac{r^2}{R},$$

and upon the second

$$T=\tfrac{1}{2}g\varrho\left(R-\frac{r^2}{R}\right).$$

In the first case $T=\frac{1}{2}g\varrho R$ at the surface of the globe, decreases with increasing depth, and becomes zero at the centre. In the second case $T=0$ at the surface and equals $\frac{1}{2}g\varrho R$ at the centre.

For the above expressions I am indebted to Professor STOKES, Sec. R.S.]

86. Mathematical considerations only would thus indicate that in a thick terrestrial crust, assumed rigid and composed of dome-shaped laminæ of superposed rock all of equal density and cohesion, the horizontal thrusts tending to produce crushing will be a maximum at the superior surface and a minimum at the inferior surface; but that if composed of loose discontinuous material, or of any material such, in relation to the vast volume of the crust and to the immense forces engaged, that it can transmit interior pressures in all directions, as a quasi liquid or plastic body, the maximum deforming thrusts will be greatest at the inferior surface and nothing at the superior surface.

87. So far mathematical investigation serves us; but it throws but little further light upon the question that most interests us here—namely, at what fraction of the entire depth of such a thick terrestrial crust will be found the *couche* of maximum vulcanicity, that is to say, of greatest work in crushing or deformation. This depends not only on the depth at which T is a maximum, but on that at which the *couche* of maximum resistance to T is to be found; and this latter depends upon the nature of the materials at all depths, as well as upon their state as forming the shell of a globe such as ours. Viewed upon the great scale, our earth's crust is neither quite rigid and dome-like nor

yet quite plastic, like a viscous liquid, to the movements of which, however, it may much approximate. The superior surface is the most rigid as to conditions due to temperature, but it is for miles in depth shattered, heterogeneous, and more or less discontinuous; the *resistance*, therefore, at the surface is smaller than at some considerable depth beneath.

Again, as we must suppose (to accord with almost any conception we can form of the change produced by hypogeal heat) that the lower surface of the shell is softer and more viscous than its upper surface, inasmuch as at the former it passes into a much hotter and probably liquid nucleus below it, so the thrust T (whatever its effect may be to *squeeze* and distort a more or less compressible viscous mass at the lower surface of the shell and just below it) must produce its greatest mechanical effect in dislocation and *crushing* at some point above. The mere *squeeze* of a viscous mass, producing compression or distortion or both, must produce transformation of work into heat, and so may reduce to a fused liquid state matter before viscous only; and in so far as this can act in the way of cause, volcanic activity must not be excluded from the local compressive actions, paramount amongst which is their effect in crushing, as being all manifestations of the tangential pressures within the crust to which volcanic action is here ascribed.

88. [It will be lower down from the earth's surface as the uppermost formations are less resistant, and higher above the lower surface where the materials pass from the solid to the liquid condition as the depth occupied by viscous matter is greater. It will also vary with the depth, in so far as the coefficients of density, ridigity, and ultimate cohesion are themselves affected by depth and pressure.

With a given thickness of solid shell the problem of the depth at which the volcanic *couche* should be found is at present, therefore, not determinable, though we may make many more or less probable hypotheses as to the constitution of the crust, from which probable depths for it may be inferred—as, for instance, if we assume a certain depth below the surface at which the material is homogeneous and unshattered, and that the coefficient of viscosity decreases according to some assigned law from a liquid inferior surface until we ascend to the above level.]

In these considerations (which the writer is quite conscious are very far from forming a mathematical investigation of an exact character, or embracing any thing like the whole of the complicated relations that are presented in nature) it is assumed that each spherical element of the entire thickness of the shell supports itself, and that the value of P is not largely affected by the diminution of the force of gravity in descending, no attempt being made to follow out the complex conditions that may arise when the crushing has taken place in various conceivable ways.

89. The writer's immediate object here is limited to proving that the resolved forces of gravitation are sufficient to produce crushing of the solid terrestrial shell, whether that be thick or thin, if left partially unsupported by the shrinking away of a cooling and contracting nucleus from beneath it; and this he believes he has now done.

90. He may be permitted, in corroboration of the general truth of what has been just advanced, to refer to an extremely able memoir (which scarcely, if at all, seems to be known to the physical geologists of Great Britain) by Signor Prof. GIUSEPPE BELLI, of the University of Pavia, entitled "Pensieri sulla consistenza ecc. della crosta solida terrestre," published in vol. ii. New Series, of the 4to Journals of the Institute of Lombardy, in 1850. In note 1 to p. 6 of the memoir, pp. 3–14, entitled "Sulla resistenza della crosta terrestre alla compressione," Prof. BELLI has given a long and able investigation of very much the same question as we have here considered, and arrived at results completely confirmatory, though by an entirely different path of investigation.

He assumes, on certain grounds, the solid shell to be rather more than 30 Italian geographical miles (60,000 metres) in thickness, that it is subject to its own weight, its density being taken at 2·5 to 3·0 as compared to water, arising from the attraction of the nucleus, whose density he takes at about 5·6, which he further supposes, though in contact with the interior of the shell, to offer no resistance to the descent of the latter through a very small distance; *i. e.* he supposes, as has been done here, the nucleus to shrink away by cooling from the crust above it.

Supposing the whole globe cut by a plane passing through the equator, he investigates the pressure by which gravitation will urge the annular surfaces of the two opposed hemispheres against each other, and arrives at the conclusion that it would be equal to the pressure of a tower (*torre*) or hollow cylinder of material equal in density to the crust and standing upon the annular surface of 30 miles' width, of 1716 Italian geographical miles in height—that is to say, in height equal to about half the radius of the earth.

Whence he shows that no known material could sustain such a strain; that were the crust of cast iron it could only support $\frac{1}{180}$ of the crushing force to which the gravitation of the whole system exposes it, if of porphyry $\frac{1}{350}$, if of wrought iron $\frac{1}{619}$, if of granite $\frac{1}{1077}$, and if of (*marmor*) primary limestone $\frac{1}{2590}$ of the whole.

He concludes, therefore, that the solid shell does not support itself as an equilibrated dome, but that, in fact, it is almost wholly supported by the fluid nucleus upon which it floats; and he then attempts to show that the rising of lava in volcanic vents is a consequence of the partial or unequal sinking of the discontinuous fragments of the shell into the liquid of the nucleus. It is to be regretted that BELLI, after having made so good a commencement, should have been misled by his supposition of a very thin crust and a liquid nucleus into an entirely wrong track, and so come to adopt a view of the mechanism of volcanic action often proposed before him, and which the writer believes to be untenable. It may be desirable to show, however, that by LAGRANGE'S method we can treat the question of the crushing of the crust from the same point of view as BELLI has done, and arrive at substantially a like result.

We have shown that, for the unit of length of a section of the shell,

$$T=\tfrac{1}{2}PR;$$

therefore, for the entire equatorial annulus or section of the shell,

$$T_0 = 2\pi R \times \frac{R}{2} \times P,$$

or

$$T_0 = \pi R^2 \times P;$$

but as P is the weight of the cubic unit of the shell, W, the weight of the hemispherical shell, is

$$W = 2\pi R^2 \times P;$$

hence

$$P = \frac{W}{2};$$

that is to say, the mutual pressure by gravitation (under the condition of shell and nucleus) of two hemispheric shells taken in an equatorial plane is equal to one half the weight of one hemisphere.

91. Signor BELLI enters into several subordinate discussions as to the probable modifications of resistance to crushing that may arise in rigid materials when exposed to great compression in all directions, and whether the tendency to crushing will be increased or diminished by the simultaneous action of two orthogonal pressures. These refinements are not required for our purpose, as we may conclude that no modification of condition of application can enable a rigid solid to remain coherent under pressures *several hundred* times greater than will crush between two opposite surfaces a cube of the same material.

92. The cube in this case is exposed to pressures on two faces, and is free to yield in four directions at right angles to the pressure and opposite to each other respectively; but a cube such as the unit-cube of our shell, exposed to pressures simultaneously on four of its faces, is only free to yield in two directions parallel to each other and at right angles to the pressures. Direct experiments on this point have not, to the writer's knowledge, ever been made; but certain facts well known to engineers appear to warrant the conclusion that rigid bodies, such as cast or wrought iron, steel, or bronze, are weakened (*i. e.* the tendency to become broken up is increased) by the simultaneous application of orthogonal pressures or tensions.

93. Thus, for example, the metal of the interior of a discharged cannon is exposed at any point at once to compression radially to the bore and to two orthogonal tensions, longitudinal and circumferential, and it is known that the resistance of the material thus is less per unit of section than of the same exposed to tension or compression in a single direction only.

94. Another question may arise. If the earth's solid shell be of very considerable thickness (as it almost certainly is), and if we conceive a cubic unit of it at a large depth exposed necessarily to pressures upon all its sides, how is the cube to suffer such deformation as shall admit of crushing at all?

The pressures on the two pairs of opposite and vertical faces are equal and opposite T and T_1.

The lower face of the cube *may* be free more or less to descend, by further compression of the material far below, whether solid, plastic, or liquid, but the uppermost face of the cube is free to ascend by merely compressing and lifting the column of material above it; and as we have shown that the value of T or T_1 must *always* and enormously exceed that of P, which is the vertical pressure, it follows that if P varies at different places in the shell, crushing will take place by vertical deformations and very readily.

95. Were the shell absolutely homogeneous and isotropic as to its material, still crushing must somewhere occur as soon as the pressure there had reached the limit of cohesion. But we must, as it appears to the writer, suppose the solid earth shell, however thick, to be heterogeneous, and discontinuous or fissured more or less at every depth, until the commencement of the plastic or viscous *couches* that intervene between its interior and the nucleus (whether that be liquid or only hot and soft relatively by heat) shall have been reached.

For the fissuring and shattering of the solidified crust, which began when it was very thin and was far more extensive and intense as it became thicker, and at a certain thickness (when the thermal equilibrium of the whole system had got more nearly to what it now is) began to decline, must have gone on, reaching continually further in depth as the thickness of the solid crust was increased by accretion to itself of more solidified matter at its inferior surface.

If, then, we admit the highly probable view that the solid crust of the earth at the present time is in *all* its parts more or less heterogeneous, if not in the nature of its materials, at least in the physical or molecular state of these at different points, there is no difficulty in admitting any amount of crushing and fissuring to be going on within it, and such crushing and fissuring *must* be local and irregular.

96. We have thus proved that localized crushing of the rocky material of our earth's crust *must* take place; and it will not be denied that heat must be produced by transformation of the work expended in crushing.

97. But two great questions now require to be answered—namely, *how much heat* is produced by the crushing of a given weight or volume of rock, and whether the total amount of crushed rock, or of heat due to it, that we can estimate on admissible data as occurring in a given time (a year or 1000 years) be adequate to account for the volcanic phenomena we witness on our earth's surface, or estimate as necessary thereto. The answers to these must be based on experiment.

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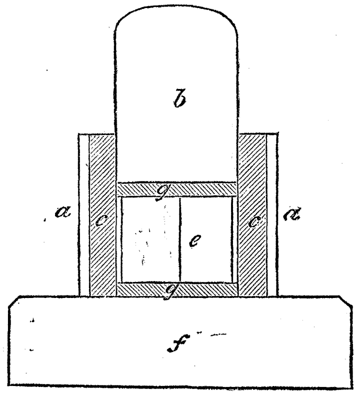
98. The mechanical work expended in the deformation or disintegration of a solid must all reappear either as heat or as external work of some sort.

In the case of very inelastic and easily deformed bodies, such as lead, Mr. HIRN has shown (Théorie Mécanique de la Chaleur, 2nd edit. part i. p. 58 &c.) that the heat

evolved is almost *precisely* the equivalent of the work expended in deformation. No experiments (except those now to be detailed) have been made, however, so far as the writer knows, upon rigid bodies, such as rocks &c., to determine whether this is likewise true of them, although *à priori* the fact could scarcely be doubted.

99. A few preliminary experiments were made to ascertain if this was the fact. A short cylinder of thin iron (*a a*, fig. 8) was lined with hard wood (*c c*), an iron plunger (*b*) was fitted to the interior, the diameter of which just admitted, without touching at the arrises (*e*), a cube of 1 inch on the side. The cylinder placed on a plate of iron (*f*) had a circular piece of moderately thick paper gun-wadding dropped into it; upon this the cube to be crushed was placed; upon the top of the cube another piece of gun-wadding, and then, the plunger (*b*) being inserted and made to bear firmly on the matters below it, a known weight (25 lbs.) was dropped from a known and constant height upon the plunger so as to crush the cube to powder. The height necessary was fixed by trial, so that very little more work should be employed than was just necessary to crush the cube.

Fig. 8.



The material operated on was statuary marble, whose specific gravity was 2·710 and its specific heat 0·205. The apparatus, it will be seen, admitted of rapidly shaking out the powder of the crushed marble into a small known weight of water at a known temperature, from the rise in temperature of which the heat developed in the crushed material was inferred. The wood lining to the cylinder and the paper above and below prevented any very sensible loss of heat by conduction.

Eight good experiments were obtained with this little apparatus, the details of which it is needless here to give, as they do not pretend to any great exactness, the paper gun-wadding having in every instance to be thrown into the water with the crushed rock, and there being some little loss by conduction to the cylinder, and, indeed, the apparatus being on too small a scale. The results, however, showed that, on a mean of the whole eight, the heat produced was the equivalent of the work expended within about $\frac{1}{92}$ of the whole.

100. Prior to making these few preliminary experiments, the writer consulted his friend the late Professor RANKINE, and found that the views of that competent authority agreed with his own—that in the crushing of rigid material, such as rock, *almost the entire* mechanical work reappears as *heat*, the extremely small residue of external work being employed in producing vibrations of sound (or analogous to those of sound, though perhaps not affecting the ear) in the apparatus used for crushing or in the air around. Thus, even in the most rigid bodies, crushing begins by compression and deformation, however small; the mass then cracks, and then the discontinuous and irregular prisms and wedges soon suddenly and finally crush to powder. We *hear* when it cracks, and work is consumed in the little impulse that originates the sound; but its amount, as compared to the totality

of the work consumed in the crushing of a solid mass of hard rock to powder, is almost infinitesimally small.

101. We may therefore admit that, subject to the inevitable errors of experiment, the whole of the work consumed in crushing matter like rocks reappears as heat, or that in an experimental cube of the rock crushed to powder by a weight W, in pounds, descending from a height h, in feet,

$$\frac{Wh}{J}=H$$

is the number of units of heat developed, J being JOULE'S equivalent.

[The volume and weight of the cube crushed being known, we can deduce the work required to crush a unit in weight (1 pound) and in volume (1 cubic foot) of each rock thus experimented on.

102. If, then,

(1) $\frac{Wh}{J}=H=$the units of heat produced by the work of crushing 1 cubic foot of rock

(which, assuming the specific heat of water constant with respect to temperature, represents the number of degrees Fahr. in a pound of water, or the number of pounds of water raised 1 degree by the work of crushing), the weight of 1 cubic foot of water at point of maximum density being 62·425 lbs., and that of ice 57·8 pounds,

(2) $\frac{H}{62{\cdot}425}=T=$the temperature to which 1 cubic foot of water is raised by H;

and, taking the heat of liquefaction of ice as 143° Fahr.,

(3) $\frac{H}{57{\cdot}8\times 143}=$the number of cubic feet of ice at 32° melted to water at 32° by H.

Also, the total heat of steam of 1 atmosphere being $1146°=966°+180°$,

(4) $\frac{H}{62{\cdot}425\times 1146}=$the number of cubic feet of water at 32° evaporated into steam at 212°; also

(5) $\frac{H}{62{\cdot}425\times 180}=$the number of cubic feet of water at 32° raised to the boiling-point.

Further, if w be the weight in pounds per cubic foot, and s be the specific heat,

(6) $\frac{H}{ws}=t=$the temperature or number of degrees of heat by which 1 cubic foot of such rock is raised by H;

so that, if f be the temperature of fusion of any rock,

(7) $\frac{H}{fws}=\frac{t}{f}=$the number of cubic feet of rock capable of being melted from temperature 0 by H,

assuming that the specific heat of the rock is constant as respects temperature, and neglecting (as small and unknown) its heat of liquefaction*.]

[* This passage and the last two columns of Table I. are modified from what was originally presented to the Society.]

103. Great numbers of experiments have been made by engineers and architects upon the resistance to crushing forces of various rocks employed in building &c., some of which, such as those of GAUTHEY and RONDELET, are of great exactness; but unfortunately these are nearly all inapplicable to our purpose:—(1) because those experiments being made with a view to structural purposes, the recorded crushing-pressure is very commonly taken as that at which the specimen of rock first begins to crack or give way; (2) because no reliable record is found of the height through which the surface producing the crushing-pressure has descended between its level of first application and that of the mass of powder produced, so that we have an unreliable and always too small value for W, and none at all for h; (3) the experiments recorded have been generally made on very small specimens (cubes of 1 centimetre or of 1 inch on the edge), and of rocks the lithological characters of which are imperfectly handed down.

104. It was therefore necessary to institute a completely new and independent series of experiments, and upon as large a scale as that for which competent apparatus could be procured.

The rocks to be experimented upon being reduced by the marble mason or lapidary's tools to exact cubes, or parallelopipeds as nearly cubes as possible, it is obvious that experiments for determining the work of crushing may be conducted either, as in the writer's preliminary ones, by the free descent of a weight just enough to crush, or by the steady increase of a load until the crushing in each case has occurred.

The latter is greatly to be preferred, not only as avoiding some conceivable sources of error in crushing by *impact*, but for the convenience afforded by the increase of a steady pressure up to crushing when operating upon a large variety of different rocks.

105. Through the obliging kindness and zeal for science of JOHN RAMSBOTTOM, Esq., Engineer, and by permission of the Directors of the London and North-Western Railway, the writer had fitted for his purpose, and placed with a staff of men at his disposal, a magnificent machine constructed from Mr. RAMSBOTTOM'S design for the locomotive works at Crewe; and not only to those gentlemen, but to W. M. MOORSOM, C.E., of the same works, are his best thanks due for the very efficient assistance in every way afforded him.

106. The testing-machine at Crewe thus employed consists of a large wrought iron balanced lever, as seen in Plate IX., so constructed that it can be applied to compression, tension, or torsion, the load being produced by the flow of water into an iron cylindric vessel suspended to the long arm of the lever, the weight of which at known temperatures is registered by an index at each instant. This simple form of testing-machine possesses great advantages in accuracy and certainty, as to the load actually visited upon the object, over any of the complex machines in which the production of the load or its registration is conducted through a *series* of connected levers.

By shifting the fulcrum the lever could be altered in power at pleasure from 10 : 1 to 20 : 1, and its own weight could be balanced so as, if desired, to form no part of the load visited on the specimen. The strength of the parts is sufficient to admit of a crushing-strain of 80 or 90 tons with safety. The largest size was chosen for the cubes of stones to be crushed that this limit would with safety allow. A cast-iron cage or frame,

shown in Plate IX. fig. 2, was prepared to receive each cube when exposed to pressure, which was applied by a cylindrical steel plunger moving freely in a deep cylindrical hole bored in the upper part. The lower part of the cage consisted of a flat steel plate upon which the cube of stone was placed, when the lower end or opposite face of the plunger applied itself to the top of the cube.

Thus the pressure upon the cube was preserved rigidly in the one vertical direction, whatever might be the motion (through a small arc) of the bearing edge of the lever itself upon the summit of the plunger. A small hydraulic press fixed between the fulcrum of the lever and this cage enabled the lever (and load if required) to be raised off from the head of the plunger at any time, and also enabled the cube about to be crushed to be conveniently placed between the faces of the crushing-cage without shock or any pressure acting upon it, except the small weight of the plunger itself; after which, by letting the water slowly out of the hydraulic-press cylinder, the bearing edge of the lever was brought gently and without jar or shock upon the head of the plunger, and the successive increments of load thereon then proceeded with.

The crushing-cage admitted of very precise measurements of the vertical distance between the crushing faces (*i. e.* that of the plunger and of the plate on which the cube was placed) at any instant.

Those distances from the commencement to the conclusion of each experiment were measured with great exactness by means of "distance callipers," with multiplying arms (on the same principle as "proportional compasses") increasing the indication tenfold, and provided with graduated arcs and verniers made by BECKER (now ELLIOTT Brothers), the indications being controlled by ordinary beam-calliper measurements read off from a steel diagonal scale. These measurements admitted of being made within less than $\frac{1}{1000}$ of an inch.

107. The following selection of rocks was made as tolerably well representing lithologically the whole series in depth of the earth's known crust from the least to the most rigid and coherent formations, viz.:—

Rocks experimented on.

1. Oolite.—Caen and Normandy.
2. Oolite.—Portland stone.
3. Dolomite.—Magnesian limestone, Yorkshire.
4. Sandstone.—Bradford, Yorkshire.
5. Sandstone.—Ayre Hill, Yorkshire (fine texture).
6. Sandstone.—Bramley Fall (hard millstone-grit).
7. Carboniferous limestone.—Devon marble.
8. Cambro-Silurian.—Conway slate (North Wales).
9. Cambro-Silurian.—Bangor slate (North Wales).
10. Basalt (greenstone).—Rowley Rag, Staffordshire.
11. Red granite.—Dartmoor, Devon.
12. Grey granite.—Guernsey.

13. Syenite.—Mount Sorrel, Leicestershire.
14. Blue granite.—Aberdeenshire.
15. Grey granite.—Aberdeenshire.
16. Porphyry.—Furnace Quarry, Inverary, Scotland.

108. Of each of these sixteen species of rock, half a dozen cubes, each as nearly as possible 1·5 inch on the edge, were prepared. Each set of cubes was roughed out from the same selected block of stone, and brought to the exact cubic form and size by grinding by Messrs. FIELD and Co., of Westminster, by whose care, with the help of their fine stone-working machinery, the work was done in a very excellent manner.

A larger-sized cube than 1·5 inch on the edge could scarcely have been employed, as respects the harder rocks, with safety to the testing-machine; but it is believed that no series of crushing experiments on rocks has previously been conducted on cubes so large.

109. The cubes could not be produced of *precisely* the same dimensions; but all were accurately measured before crushing by the instruments described, and the results reduced to a common standard.

110. The most scrupulous care, however, was taken to secure two opposite faces in each cube rigidly parallel to each other, so as to avoid unequal bearing when being crushed; and this result was fully attained by cementing the whole of the cubes down upon a flat metallic plate, and grinding off the uppermost faces simultaneously by another like plate, and then reversing the cubes and cementing them down again by the last ground face, repeating the process.

The experiments were conducted at Crewe works in August 1870, Mr. MOORSOM, C.E., assisting at all of them. Three good experiments were obtained for each description of rock, in some instances more.

111. Each experiment was conducted in the following manner:—

The testing-lever being raised by the hydraulic press, the crushing-plunger was raised, by a small hand lever of wood, from the base-plate of the cage sufficiently to admit of placing the cube.

The cube was then placed on the base-plate centrally, or with its centre of figure truly under the axis of the plunger.

A square piece of the *thinnest* and most *hard*-pressed letter-paper was placed upon the top and bottom faces of the cube, the size of the squares of paper being 0·20 inch less than the edge of the cube, so as to allow a free margin of 0·10 inch all round each of these faces, the object being by this very thin film of more compressible material to neutralize any residual departure from absolute parallelism between the upper and lower faces, and so secure perfect uniformity of bearing, a method which was found to succeed perfectly.

112. The cube being in place, the plunger was gently and without shock lowered upon it; and in like manner the lever itself, by aid of the hydraulic press, was lowered so as to bear upon the head of the plunger. The weight of the unloaded lever was then per-

mitted to bear as a load upon the plunger, and through that upon the cube. This gave an initial pressure (which was varied according as the species of stone was harder or softer) of a few hundred pounds, usually about at the rate of 500 lbs. per square inch, by which all parts of the testing-lever and the crushing faces with the cube were brought into perfect contact and bearing.

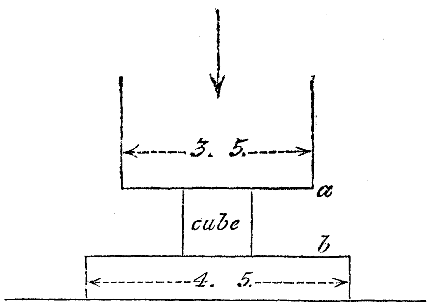
113. The dimensions of the cube having been accurately measured and noted before putting it into the crushing-cage, the altitude of the cube, or rather the exact distance between the crushing faces, was then taken. From the thinness of the papers on the top and bottom faces and the compression of these, this distance did not differ from the true vertical height of the cube by $\frac{1}{800}$ of an inch.

114. Water was now admitted in a small stream to the load bucket; and as each 1000 pounds of load was attained, the flow was stopped and the amount of compression of the cube taken by measurement between the crushing faces, as already described. The loading was thus continued until the first symptoms of disintegration were noticed in the cube, almost always showing itself by one or more extremely minute cracks in nearly vertical planes, or by a few fragments of powder detaching themselves from the sides.

At this stage the loading was stopped and the compression noted. The loading was then continued slowly until the cube finally gave way, crushing down before the descending plunger into absolute powder, which remained impacted together as a flat cake beneath the plunger, hindering its further descent.

115. The vertical distance between the face of the plunger and that of the base-plate (fig. 9) was then accurately taken, and this, deducted from the initial distance, gave the absolute distance through which the plunger had descended.

Fig. 9.



116. Had the entire volume of material in the cube remained always beneath the plunger in the form of a short cylindrical or round flat cake of powder impacted together and of equal density with the cube before it was crushed, inasmuch as the diameter of the plunger was 3·5 inches, the vertical distance *a b* (fig. 9) would have been for an exact cube of 1·5 inch=0·351 inch, which, deducted from 1·5 inch, would have given the descent of the plunger.

117. But the cubes were *not* all quite equal in volume, and the whole of the powder produced did not always remain impacted under the plunger, but some was thrown to a little distance. Hence it was necessary to measure in each case, to obtain the actual descent.

In a few cases, with the more rigid and elastic rocks, just before the final crush down, the cube split off one or more comparatively large undisintegrated fragments, which were thrown out laterally and escaped further action. Whenever any such fragments occurred means were adopted for their collection and preservation; and in such cases a correction has been applied to the result of the crushing based on this allowable assumption, that the work expended upon crushing that *portion* of the entire cube which was

1.	2.	3.	4.	5.		6.	7.
No. of Experiment.	CHARACTER OF CLASS OF ROCK.	Specific Gravity.	Weight per cubic inch and cubic foot.	Dimensions of Cubes crushed.		Area of surface of cubes.	Weight per squa inch at which fi signs of yieldin appeared.
		Water = 1000.	$\frac{\text{Col. } 3 \times 62{\cdot}4}{1000}$, for weight per cub. foot.	Height.	Surface.		$\frac{\text{Col. } 15}{\text{Col. } 6}$.
			lbs.	in.	in.	sq. in.	lbs.
1.	Caen Stone, Oolite	2337					
	Cube A		0·0867	1·45	1·45 × 1·40	2·030	2463·0
	„ B		145·8288	1·43	1·43 × 1·47	2·102	952·3
	„ C			1·44	1·44 × 1·44	2·073	1445·5
2.	Portland Stone	2462					
	Cube A		0·0888	1·47	1·50 × 1·50	2·250	2400·0
	„ B		153·6288	1·45	1·50 × 1·50	2·250	3555·0
	„ C			1·46	1·50 × 1·52	2·280	3459·0
3.	Magnesian Limestone ..	2571					
	Cube A		0·0928	1·45	1·52 × 1·47	2·234	3139·0
	„ B		160·4304	1·44	1·51 × 1·48	2·234	3811·0
	„ C			1·46	1·48 × 1·47	2·175	4147·0
4.	Sandstone from the neighbourhood of Bradford, Yorkshire	2478					
	Cube A		0·0894	1·46	1·45 × 1·48	2·146	9602·0
	„ B		154·6272	1·46	1·48 × 1·45	2·146	10420·0
	„ C			1·45	1·47 × 1·48	2·102	12890·0
5.	Ayre Hill Sandstone, Yorkshire	2408					
	Cube A		0·0869	1·47	1·47 × 1·49	2·190	......
	„ B		150·2592	1·45	1·50 × 1·47	2·205	......
	„ C			1·45	1·48 × 1·47	2·175	6192
6.	Bramley Fall Sandstone..	2506					
	Cube A		0·0904	1·45	1·50 × 1·53	2·295	crushed sudden
	„ B		156·3744	1·43	1·48 × 1·54	2·279	„
	„ C			1·45	1·49 × 1·53	2·279	„
7.	Devonshire Marble......	2717					
	Cube A		0·0981	1·47	1·52 × 1·49	2·264	11505·0
	„ B		169·5408	1·47	1·48 × 1·51	2·234	11748·0
	„ C			1·47	1·49 × 1·47	2·190	11872·0
8.	Conway Slate	2733					
	Cube A		0·0986	1·43	1·50 × 1·52	2·280	......
	„ B		170·5392	1·45	1·50 × 1·51	2·265	8407·0
	„ C			1·43	1·42 × 1·49	2·116	11129·0
9.	Bangor Slate	2859					
	Cube A		0·1032	1·44	1·49 × 1·53	2·279	18512·0
	„ B		178·4016	1·45	1·48 × 1·52	2·249	......
	„ C			1·47	1·51 × 1·48	2·230	12509·0
10.	Rowley Rag Stone	2827					
	(preliminary only) Cube A*		......	1·41	1·30 × 1·50	1·950	14358·0
	Cube A		0·1020	1·47	1·43 × 1·51	2·150	20930·0
	„ B		176·4048	1·47	1·50 × 1·50	2·250	24000·0
	„ C			1·47	1·49 × 1·46	2·170	27188·0
11.	Dartmoor Red Granite ..	2652		1·48	1·49 × 1·45	2·160	13888·0

7.	8.	9.	10.	11.	12.	13.	14.
ht per square at which first s of yielding ppeared.	Mean weight per square inch at which first signs of yielding appeared.	Crushing-weight per square inch.	Mean Crushing-weight per square inch.	Volume of Cubes.	Mean Volume of each sort of Rock.	Weights of Cubes crushed.	Mean Weigh of Cubes of ea sort.
Col. 15 / Col. 6.	Sum of col. 7 / 3.	Col. 17 / Col. 6.	Sum of col. 9 / 3.	Height × area of surface.	Sum of col. 11 / 3.	Col. 11 × col. 4 (weight per cubic inch).	Sum of col. 1 / 3
lbs.	lbs.	lbs.	lbs.	cub. in.	cub. in.	lb.	lb.
2463·0		2955·6		2·9435		0·2552	
952·3	1620·26	1766·0	2410·2	3·0060	2·9784	0·2606	0·2582
1445·5		2510·0		2·9859		0·2588	
2400·0		5711·1		3·3075		0·2920	
3555·0	3138·00	5711·1	5848·4	3·2625	3·2996	0·2880	0·2913
3459·0		6123·1		3·3288		0·2939	
3139·0		6502·0		3·2393		0·3006	
3811·0	3699·00	6255·0	7409·0	3·2292	3·2149	0·2996	0·2975
4147·0		9470·0		3·1763		0·2923	
9602·0		14486·0		3·1331		0·2800	
10420·0	10970·60	12476·0	14011·0	3·1331	3·1047	0·2800	0·2775
12890·0		15071·0		3·0479		0·2724	
......		6872·0		3·2197		0·2997	
......	......	7590·0	7301·0	3·1972	3·1905	0·2778	0·2772
6192		7442·0		3·1546		0·2741	
ed suddenly		5611·0		3·3277	(3·2975)	0·2998	
,,	......	3634·0	5049·0	3·2592	3·3166	0·2956	0·2977
,,		5903·0		3·3055		0·2978	
11505·0		17210·0		3·3292		0·3265	
11748·0	11708·30	13791·0	15684·0	3·2851	3·2780	0·3222	0·3215
11872·0		16052·0		3·2197		0·3158	
......		12234·0		3·2604	(3·1935)	0·3214	
8407·0	9768·30	10877·0	12543·0	3·2842	3·1601	0·3238	0·3148
11129·0		14519·0		3·0361		0·2993	
18512·0		22944·0		3·2827		0·3387	
......	15510·00	......	18439·0	3·2610	3·2726	0·3365	0·3380
12509·0		13934·0		3·2851		0·3390	
14358·0	......	29366·0	......	2·7495	......	0·2804	......
20930·0		30830·0		3·1741		0·3237	
24000·0	24039·30	27761·0	29125·0	3·3075	3·2264	0·3373	0·3290
27188·0		28784·0		3·1978		0·3261	
13888·0		18057·0		3·1975		0·3059	

ults of Experiments on the Work and Heat of Rocks crushed at Crewe Works, 1870.

14.	15.	16.	17.	18.	19.	20.	
Mean Weight Cubes of each sort.	Actual Pressure at which Disintegration commenced.	Mean actual Pressure at which Disintegration commenced.	Actual Pressure at which the Cubes were completely crushed.	Mean actual Pressure at which Cubes of each sort were completely crushed.	Vertical Range through which the Crushing Pressure acted.	Work expended in crushing the Cubes.	Mea expe crushin eac
um of col. 13 / 3.		Sum of col. 15 / 3.		Sum of col. 17 / 3.		Col. 17 × col. 19.	Sum
lb.	lbs.	lbs.	lbs.	lbs.	ft.	foot-pounds.	foot-p
	5000		6000		0·0883	529·80	
0·2582	2000	3333	3700	4966·0	0·0858	317·57	4
	3000		5200		0·0908	472·38	
	5500		12850		0·0950	1220·75	
0·2913	8000	7094	12850	13216·6	0·0958	1231·41	12
	7783		13950		0·0950	1325·25	
	7000		14500		0·0941	1365·32	
0·2975	8000	8000	13950	16333·3	0·0850	1185·75	14
	9000		20550		0·0925	1900·87	
	20000		31000		0·0916	2839·60	
0·2775	22000	23017	26700	29783·3	0·0975	2603·25	28
	27050		31650		0·0941	2980·16	
	....		15050		0·0975	1467·37	
0·2772	....	....	16700	15966·6	0·0908	1516·36	14
	15000		16150		0·0916	1479·34	
	....		12850		0·0991	1273·43	
0·2977	....	....	8500	11500·0	......	......	12
	....		13400		0·0983	1317·22	
	26000		38895		0·0991	3854·49	
0·3215	26000	26000	30755	34938·0	0·0975	2998·61	341
	26000		35155		0·0966	3395·97	
	....		27895		0·0633	1765·75	
0·3148	19000	21242	24584	27704·3	0·0800	1966·72	205
	23484		30634		0·0791	2423·14	
	42000		52095		0·0783	4079·03	
0·3380	....	34500	....	41590·0	......	......	343
	27000		31085		0·0900	2797·65	
......	28000	......	57265	......	0·0791	4529·66	...
	45000		66285		0·1050	6959·92	
0·3290	54000	52666	62463	63737·0	0·0941	5877·76	646
	59000		62463		0·1050	6558·61	
	[illegible]		[illegible]		0·1008	[illegible]	

...n ...es.	21.	22.	23.	24.	25.	26.	27.	Temp... foo... w...
	Mean Work expended in crushing Cubes of each sort.	Weight of large fragments thrown off on crushing.	Total Work expended in crushing the entire Cube.	Mean Total Work expended in crushing the entire Cubes of each sort.	Mean Total Work reduced to crushing 1 cubic inch and 1 cubic foot.	British Units of Heat corresponding to mean total work of crushing 1 cub. ft. and 1 lb. avoird. of each Rock.	Specific Heat of each class of Rock.	
	$\frac{\text{Sum of col. 20}}{3}$.		$\frac{\text{Col. 20} \times \text{col. (13+22)}}{\text{Col. 13}}$.	$\frac{\text{Sum of col. 23}}{3}$.	$\frac{\text{Col. 24}}{\text{Col. 12}}$.	$\frac{\text{Col. 25}}{\text{J}}$.	Water=1·00.	$\frac{\text{H}}{\text{W S}}$ =
	foot-pounds.	lb.	foot-pounds.	foot-pounds.	foot-pounds.	° Fahr.		
	439·92	none. " "	529·80 317·57 472·38	439·92	148·139 255,951·392	2·27 331·5	0·284	
	1259·13	" " "	1220·75 1231·41 1325·25	1259·13	381·603 659,409·984	5·5 854·1	0·265	
	1483·98	" " "	1365·32 1185·75 1900·87	1483·98	461·590 797,634·430	6·4 1033·2	0·245	
	2807·67	0·0868 0·0762 0·0829	4089·02 3566·45 4261·63	3972·37	1279·469 2,210,922·432	19·3 2863·4	0·215	
	1487·69	0·1003 0·1107 0·1036	2274·43 2517·15 2366·94	2386·17	748·214 1,292,913·792	11·1 1674·7	0·233	
	1295·32	0·1003 none. 0·0680	1913·36 . . . . . . 1706·99	1810·18	545·794 943,132·204	7·8 1221·6	0·238	
	3416·36	0·1135 0·1588 0·1223	5897·37 5905·66 5535·43	5779·49	1763·1157 3,046,663·9296	23·2 3946·4	0·203	
	2051·87	none. 0·2179 0·2083	. . . . . . 5998·49 7947·93	6973·21	2206·643 3,813,079·104	28·9 4939·2	0·218	
	3438·34	0·1571 . . . . none.	7586·99	. . . . . .	2311·205 3,993,762·240	28·9 5173·2	0·201	
	. . . . . . 6465·43	none. 0·1536 0·1310 0·1347	13244·70 9610·13 11168·32	11341·05	3515·08 6,074,058·24	44·6 7867·9	0·204	
		[illegible]	[illegible]				0·180	

Mean Heat=2448°·50 Fahr. per cubic foot.
14°·93 " lb. avoir.

	28.	29.	30.	31.
[eat lass .	Temperature in 1 cubic foot of Rock due to work of crushing.	Number of cubic feet and pounds of water at 32° evaporated into steam at 212°.	Volume of ice at 32° melted to water at 32° by one volume of Rock.	No. of Experiment.
·00.	$\frac{H}{WS} = \frac{\text{Col. 26}}{\text{Col. 4} \times \text{col. 27}}$.	$\frac{H}{62{\cdot}45 \times 1146}$.	$\frac{H}{57{\cdot}8 \times 143}$.	
	° Fahr.		cub. ft.	
	8·004	0·0046 cub. ft. 0·288 lb.	0·04008	1.
	20·98	0·0119 cub. ft. 0·774 lb.	0·1033	2.
	26·28	0·015 cub. ft. 0·9 lb.	0·125	3.
	86·13	0·04 cub. ft. 2·5 lbs.	0·346	4.
	47·79	0·0234 cub. ft. 1·46 lb.	0·2026	5.
	32·84	0·017 cub. ft. 1·066 lb.	0·147	6.
	114·679	0·055 cub. ft. 3·44 lbs.	0·477	7.
	132·85	0·07 cub. ft. 4·3 lbs.	0·596	8.
	144·29	0·071 cub. ft. 4·51 lbs.	0·613	9.
	213·23	0·109 cub. ft. 6·86 lbs.	0·925	10.
				11.

(Bracketed against Experiments 1–9, column 29:) Mean quantity of water evaporated by 1 cubic foot of crushed rock=0·03753.

(Bracketed against Experiments 10–11, column 29:) 0·078757.

	„ B			[illegible]	[illegible]	[illegible]	[illegible]
	„ C		176·4048	1·47	1·49 × 1·46	2·170	27188·0
11.	Dartmoor Red Granite ..	2652					
	Cube A		0·0957 165·4848	1·48	1·49 × 1·45	2·160	13888·0
	„ B			1·47	1·49 × 1·50	2·230	14798·0
	„ C			1·47	1·50 × 1·50	2·250	16888·0
12.	Guernsey Granite (grey)..	2858					
	Cube A		0·1032 178·3392	1·475	1·47 × 1·50	2·200	18181·0
	„ B			1·475	1·48 × 1·51	2·230	23317·0
	„ C			1·475	1·50 × 1·54	2·310	21212·0
13.	Mount Sorrel (red granite)	2653					
	Cube A		0·0958 165·5472	1·48	1·48 × 1·53	2·260	18181·0
	„ B			1·47	1·47 × 1·55	2·270	22467·0
	„ C			1·48	1·47 × 1·52	2·230	19282·0
14.	Aberdeen Granite (blue)	2707					
	Cube A		0·0977 168·9168	1·48	1·53 × 1·50	2·290	19150·0
	„ B			1·48	1·54 × 1·52	2·340	23299·0
	„ C			1·48	1·51 × 1·49	2·240	17928·0
15.	Aberdeen Granite (grey)..	2678					
	Cube A		0·0965 166·9072	1·48	1·49 × 1·52	2·265	17256·0
	„ B			1·48	1·50 × 1·55	2·325	16000·0
	„ C			1·48	1·53 × 1·50	2·295	17348·0
16.	Furnace Scotch Clay Porphyry	2594					
	Cube A		0·0936 161·8656	1·48	1·51 × 1·49	2·250	27111·0
	„ B			1·48	1·50 × 1·50	2·250	27555·0
	„ C			1·48	1·50 × 1·51	2·260	23781·0

[illegible] 27188·0	[illegible]	[illegible] 28784·0	[illegible]	[illegible] 3·1978	[illegible]	[illegible] 0·3261	[illegible]
13888·0 14798·0 16888·0	15191·33	18057·0 17984·0 22126·0	19389·0	3·1975 3·2854 3·3075	3·2634	0·3059 0·3144 0·3165	0·3123
18181·0 23317·0 21212·0	20903·33	27504·0 32338·0 30528·0	30123·0	3·2523 3·2963 3·4072	3·3186	0·3356 0·3401 0·3516	0·3424
18181·0 22467·0 19282·0	19976·33	24535·0 23458·0 30612·0	26201·0	3·3522 3·3486 3·3063	3·3357	0·3211 0·3207 0·3167	0·3195
19150·0 23299·0 17928·0	20125·66	25078·0 22780·0 20187·0	22681·0	3·3966 3·4643 3·3298	3·3969	0·3318 0·3384 0·3253	0·3318
17256·0 16000·0 17348·0	16868·00	20373·0 22122·0 24454·0	22316·0	3·3522 3·4410 3·3966	3·3966	0·3233 0·3320 0·3277	0·3276
27111·0 27555·0 23781·0	26149·00	32784·0 32540·0 27601·0	30975·0	3·3285 3·3300 3·3522	(3·3369) 3·3411	0·3115 0·3116 0·3137	0·3123

[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	59000		62463		0·1050	6558·61	
	30000		39005		0·1008	3931·70	
0·3123	33000	33666	40105	42965·0	0·0975	3910·23	436
	38000		49785		0·1058	5267·25	
	40000		60510		0·0983	5948·13	
0·3424	52000	47000	72115	67715·0	0·0858	6187·46	643
	49000		70520		0·1016	7164·83	
	40000		55450		0·0966	5356·47	
0·3195	51000	45000	53250	58988·0	0·1025	5458·12	597
	44000		68265		0·1041	7106·38	
	43800		57430		0·0875	5025·12	
0·3318	51000	44600	53305	51985·0	0·0908	4840·09	4631
	40000		45220		0·0891	4029·10	
	39000		46045		0·1025	4719·61	
0·3276	37000	38666	51325	51123·0	0·1025	5260·81	5176
	40000		56000		0·0991	5549·60	
	61000		73765		not given.	......	
0·3123	62000	58833	73215	69786·0	0·1083	7929·18	710
	53500		62380		0·1008	6287·90	

[illegible]	[illegible] 0·1347	[illegible] 11168·32	[illegible]	6,074,058·24	7867·9	Mean Heat=5650°·20 Fahr. per cubic foot. 33°·24 " lb. avoir.		
4369·73	0·0623 0·0329 0·0326	4934·28 4363·82 5867·71	5055·27	1549·083 2,676,810·240	20·9 3467·3		0·180	1
6433·47	0·1641 0·1189 0·1319	11640·61 9466·82 11466·33	10857·92	3271·838 5,653,722·24	41·0 7323·4		0·189	2
5973·66	0·1123 0·1002 0·0468	8195·39 7914·28 8314·47	8141·38	2440·68 4,217,495·04	33·0 5463·0		0·181	1
4631·44	0·0920 0·0972 0·1095	6934·67 6776·13 6043·65	6584·819	1938·48 3,349,692·168	25·7 4339·0		0·215	1
5176·67	0·1003 0·1136 0·1111	6843·43 7996·43 8379·89	7739·92	2278·726 3,937,638·528	30·5 5100·5		0·196	1
7108·54	0·5520 0·0690 0·0585	...... 10149·35 7734·12	8941·738	2676·280 4,624,611·840	37·0 5990·4		0·186	1

213·23	[illegible] cub. ft. 6·86 lbs.	0·925	
116·39	0·049 cub. ft. 3·02 lbs.	0·419	11.
217·24	0·1023 cub. ft. 6·39 lbs.	0·885	12.
182·27	0·076 cub. ft. 4·76 lbs.	0·6609	13.
119·2	0·06 cub. ft. 3·78 lbs.	0·524	14.
155·94	0·072 cub. ft. 4·44 lbs.	0·617	15.
198·97	0·083 cub. ft. 5·22 lbs.	0·724	16.

Mean quantity of water evaporated by 1 cubic foot of crushed rock=0·078757.

reduced to powder is to that which would have been necessary to reduce the entire cube to powder as the weight of the crushed part to the weight of the entire; so that the weight of those large uncrushed fragments having been ascertained subsequently, this correction was applied.

118. As the total descent of the plunger is in all cases small, less than 1·5 inch, the acceleration of its descent is not considerable, the rather as by the construction of the testing-machine a certain proportion of the crushing-pressure was relieved from the lever as soon as the plunger began to descend considerably, so that for the remainder of the descent the plunger was urged downwards principally by the weight of the heavy end of the large lever above it. Moreover, as each cube broke up at first into irregular prisms and wedge-shaped pieces closely adjacent to each other, which afterwards *crunched* down rapidly to powder, so the resistance to the plunger was not far from constant during its descent.

In this manner the entire series was gone through, only varied by very careful balancing off of the weight of the lever itself in the case of the very friable rocks, such as the Oolites.

119. The final pressure at the moment of crushing gives the value of W, and the descent of the plunger, subject to the correction described, that of h in the preceding equation.

120. In all the harder rocks, notwithstanding the close contact of the large mass of metal in the plunger and base-plate, the heat produced by the crushing down was easily perceptible by the hand, and was so great in the case of some of the granites and porphyries as to heat the plunger and base so much as to oblige a delay to let them cool to the same temperature as before, and as they were at in all the experiments, viz. about 57° Fahr. Had it been practicable to exclude the daylight, there can be little doubt but that a flash of light would have been found to attend each such crushing.

121. The results, both immediate or direct, and those deduced from this laborious train of experiments, are comprised in the accompanying large Table, No. I. The specific gravities, column 3 in this Table, were obtained by the writer by weighing one of the spare cubes of each rock, first in air and then in water contained in a thin cylindrical glass jar with a ground circular glass cover, the weight of which, as also of its contents of distilled water at 60° Fahr., was known. This method, which is very much more rapid than the usually prescribed methods, is to be commended for all specific gravities of solids heavier than water.

122. The specific heats of these several rocks (column 27) were obtained by the method of mixtures operating also upon these cubes, which were heated in boiling water, and then immersed in distilled water at temperatures of 50° to 54° Fahr., applying the usual corrections for the glass vessel containing the fluid, the thermometer, &c.

So few specific heats have previously been determined for rocks that these may be found to possess some general interest.

123. In column 8 and column 10 are given the mean loads per square inch at which the first signs of disintegration and at which the final crushing to powder occurred,

and the differences are thus obtainable. These vary much for different classes of rock.

If, however, we take a mean of the pressures at which first disintegration occurred, and of the differences between those and the pressures of complete crushing, we shall find that $\frac{1}{3\cdot778}$, or about $\frac{1}{4}$ added to the pressure of first disintegration, gives that of final crushing to powder—a result of some interest to the engineer and architect, and one which enables us in some degree to compare our results with the largest and most accurate series of experiments that the writer believes had previously been made on the resistance to crushing of rocks.

124. He refers to those of Mr. GEORGE WILKINSON, Architect, given in his work 'On the Practical Geology and Ancient Architecture of Ireland' (Murray, 1845), which were conducted by that gentleman upon almost the entire series of the rocks of Ireland from the highest to the lowest. The reliability of these experiments is known to the writer, as he himself devised and constructed the lever-apparatus by which they were performed; and the experiments were made in the engineering works of the writer's late firm by Mr. WILKINSON. As Mr. WILKINSON's object was to obtain *constructive* data only, the crushing-pressures recorded are seldom those at which the specimen (which in every case was 1 cubic inch in size) was reduced to powder, but that at which it was disintegrated only, and hence the results are all, as regards our point of view, here too low. If, however, we add to the mean of each class one-fourth part, we shall approximate to results more nearly comparable with those we have here obtained.

The following Table gives WILKINSON's highest, lowest, and average results, together with the number of different quarries and total number of experiments from which each average was ascertained. This summary has been made by the writer, and is not to be found in Mr. WILKINSON's work.

Class of rock.	Crushing-weights in lbs. per square inch.			Number of quarries.	Number of experiments.
	Maximum.	Minimum.	Average.		
Limestones	27510	1344	15053	125	210
Sandstones	26670	1239	8183	31	82
Sandstones across beds	18790	1680	8864	33	42
Sandstones in line of beds	20650	2940	9824	29	36
Slates across beds	27370	5040	13930	9	13
Slates in line of beds	21770	6160	11285	11	18
Granites	13440	2310	6657	8	20
Basalts, metamorphic	48020	7140	19025	12	25

The granites of Ireland generally are very friable.

125. It may be interesting to add the following volcanic rocks on the authority of PRUDHOMME ('Cours pratique de Construction'):—

Ancient lava (*Volvic stone*) of Auvergne .	28,446	lbs. per square inch.
Vesuvian lava (date not given)	8,392	,, ,,
Granitoid porphyry (Bazoche)	21,072	,, ,,

126. Cast steel in small blocks (according to FAIRBAIRN) may stand without crushing 120,000 lbs., or above 120 tons per square inch, or about four or five times the crushing-weight of the hardest known rocks.

127. In column 24 of our Table the mean total *work in foot-pounds* expended in crushing each sort of rock is given, and in column 25 the *foot-pounds of work* needed to crush 1 pound avoirdupois and 1 cubic foot of each sort of rock are given.

Then in column 26 this is, by our formula $\frac{Wh}{J}$, reduced to units of heat for the pound and for the cubic foot of rock; and dividing these by the specific heats given in column 27 and by the weights per cubic foot in column 4, we find the temperature in equal volume of rock; and in column 29 we obtain the number of cubic feet of water at 32° Fahr. that can be converted into steam of 1 atmosphere, or 212° Fahr., by the heat evolved by the crushing of 1 cubic foot of each class of rock.

Lastly, in column 30 we have the cubic feet of ice at 32° melted to water at 32° by the heat due to the crushing of 1 cubic foot of each of the sixteen typical classes of rock.

128. These coefficients of heat and crushing-work for each class of rock may be grouped together in various ways, so as to obtain *mean* coefficients for groups of rock or formations succeeding each other in depth.

Let us endeavour to obtain such a mean coefficient for the entire depth of the solid crust of the earth of 100 miles in thickness.

129. The following Table of the probable average depths of the *known* formations of our earth's crust has been given by Professor S. HAUGHTON (Geological Manual, p. 91):—

		Geogr. miles.
Neozoic	Tertiary to Triassic	4·512
Newer Palæozoic . .	Permian to Devonian	4·458
Older Palæozoic . .	Upper and Lower Silurian	5·082
Azoic	Quartz rock, Roofing-slates, Primary limestone	4·333
	Total depth =	18·385

geographical miles, or rather more than 20 British miles.

130. Assume the first 100 miles in depth of the earth's crust to consist of 20 miles of the above rocks (which are all of about equal respective depth) and of 80 miles beneath of crystalline rocks, and of unknown acid and basic magmas, crystalline or not, of a nature analogous to our most rigid crystalline rocks.

We may group the rocks of our Table I. into the newer, or Nos. 1 to 9 (viz. from the Oolites to the Silurian slates), and the older, from Nos. 10 to 16 (or from metamorphic to granites &c.).

The mean coefficient from the first is 2449°, and from the second 5650°; and the last may be taken to represent all below the azoic rocks; we therefore have 20 miles in depth at 2449° and 80 miles in depth at 5650°, which gives a mean coefficient for the entire depth of 5010°.

131. This is, however, certainly below the truth; for the Devonian limestone and North-Wales slates (Nos. 7, 8, and 9 have coefficients almost or quite as high as several of the granites and quartz rock of Holyhead, as deduced from my experiments on its crushing, Philosophical Transactions, 1862) have a coefficient exceeding 5316°, or equal to granite (see Appendix), and constitute much of the deepest 5 miles of our entire 20 miles of known rock; and there is to be found no doubt a large increase of metamorphic rocks of the Rowley Rag (No. 10) class at the lower part of the known series.

It will therefore be a better approximation to group together all the rocks in our Table I. *except* Nos. 10, 12, and 16, which have the largest coefficients, and take the mean of these for the coefficient of the known or stratified rocks, and the mean of Nos. 10, 12, and 16 for that of all the crystalline or other rocks beneath.

We shall then have the mean for all, *except* Nos. 10, 12, and 16, $=4110°$, and for Nos. 10, 12, and $16=7060°$, or for the whole depth

20 miles at 4110°,
80 miles at 7060°,

giving a mean for the entire depth of 6472° per cubic foot of rock.

132. A mean specific heat obtained in the same way from those given in column 27 gives 0·199 for the average of the whole 100 miles depth of shell; and from the specific gravities given in column 3 we deduce a mean for all the rocks experimented on of 2627. Taking that as the mean for 20 miles in depth, and assuming 2900 specific gravity for the 80 miles of denser rocks beneath, we obtain a mean specific gravity for the whole 100 miles of crust of 2842.

133. Whence we have the following numerical results:—

(1) $H=6472$ British units per cubic foot of crushed *mean* rock.

(2) $\frac{6472}{62\cdot425}=103°$ Fahr. = the temperature to which 1 cubic foot of water at zero is raised by H.

(3) $\frac{6472}{57\cdot8\times143}=0\cdot783$ = cubic feet of ice at 32° melted to water at 32°.

(4) $\frac{6472}{62 \cdot 425 \times 1146}=0 \cdot 0904=$cubic feet of water at 32° evaporated into steam at 212°;

or if the water be already at 212°,

$$\frac{6472}{64 \cdot 425 \times 966}=0 \cdot 107 \text{ cubic feet evaporated into steam of 1 atmosphere.}$$

(5) $\frac{6472}{177 \times 0 \cdot 199}=183^{\circ} \cdot 74=$degrees of temperature by which 1 cubic foot of *mean rock* is raised by H.

Taking 2000° Fahr. as the fusing temperature of such rock,

(6) $\frac{6472}{2000 \times 177 \times 0 \cdot 199}=0 \cdot 0918=$the number of cubic feet of mean rock at zero fused by H.

If the rock be previously at 300° Fahr., then

$$\frac{6742}{1700 \times 177 \times 0 \cdot 199}=0 \cdot 108=\text{the cubic feet of rock so fused;}$$

and if the rock at 300° be only heated to 1000° Fahr., or to a bright red heat (the melting-point of silver),

$$\frac{6472}{700 \times 177 \times 0 \cdot 199}=0 \cdot 262=\text{the cubic feet of rock so heated by H.}$$

134. From (3) it follows that the heat of liquefaction of 1 cubic mile of ice at 32° melted is equivalent to the crushing work of 1·277 cubic mile of mean rock when transformed into heat.

135. We have now to describe the second series of experiments, viz. those for the determination of the total amount of contraction of mineral masses analogous to those which we may suppose constitute the solid shell and probably the greater portion of our globe, upon cooling from their temperatures of fusion, or above that by known amounts, down to that of the mean of our atmosphere.

The experiments or observations that have heretofore been made on the subject are of the most unsatisfactory character, and contain such elements of error as to be wholly unreliable.

Bischoff's experiments on the total contraction of fused basalt, trachyte, and granite, &c. (originally recorded in Leonhard and Bronn's 'Neues Jahrbuch für Mineralogie &c.,' vol. for 1841, p. 565 &c., and vol. for 1843, pp. 1–54, the last containing the details of experiments by Bischoff himself) have been accepted merely on authority, and quoted by authors in book after book apparently without the details of the methods of experiment having been consulted in the original memoirs. Thus Professor W. Thomson (Thomson and Tate, Nat. Phil. p. 725) says, "Bischoff's experiments, upon the validity of which, so far as I am aware, no doubt has ever been thrown, show that melted granite, slate, and trachyte all contract by something like 20 per cent. by freezing," and proceeds to base on this erroneous coefficient a probable cause for volcanoes and earthquakes (pp. 725–727).

He can scarcely have looked at BISCHOFF's own statements, or their insecure foundations would not have escaped his acute mind.

BISCHOFF's experiments were conducted by two methods.

In one he fused the rock in clay crucibles holding only a few pounds, determined the capacity of the empty crucible by the weight of its contents in mercury, and the volume of the unfused rock by its specific gravity and weighing. He then fused the rock, and to obtain its volume when liquid he *dipped* the depth of the surface below the brim of the crucible by means of a graduated iron wire, and then permitted the whole to cool. He then filled up over the surface of the solidified rock with mercury until the iron dipping-wire touched the liquid; from the weight of this mercury he infers the volume of the contraction; and filling now the whole vacuity of the crucible with mercury, he obtains, by weighing and deduction of these latter volumes of mercury from that which the empty crucible originally contained, the volumes of the rock when fused and when cold.

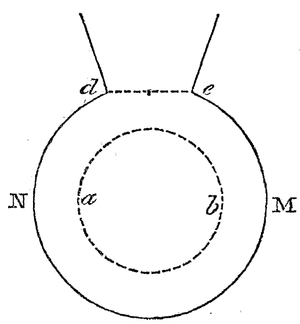
Substantially this is BISCHOFF's method; though for all his steps and apparent precautions his original lengthy memoir must be consulted.

Now it scarcely needs to be shown at any length that reliable results by this complex and indirect method are perfectly impossible, even were not one condition which really vitiates the whole found to be entirely disregarded.

He was obliged to use mercury because his crucibles absorbed water; but the strong capillarity of mercury and its high specific gravity could not fail to introduce great errors in the deduction from its weight in such very limited volumes.

The supposed *expansion* of the crucible and enlargement of *its* capacity when heated was taken by endeavouring to measure the internal diameter of its brim when cold and when containing the liquid rock, and it is *assumed* the expansion affected all parts of the crucible alike, so that its capacity, in fact, was proportionate to the cube of this one imperfectly measured dimension. But this cannot be true, from what we know of the changes of form of pottery in baking, even were all parts of the crucible heated alike. But what of the *contraction* of the crucible, in common with all other earthenware, by heating, which depends not only on the temperature to which it is raised, but on the time of its exposure to the heat?

Fig. 10.



Of this which is certain to have largely affected BISCHOFF's results, we do not find a word of remark.

BISCHOFF admits the difficulty of this class of experiment, and he seems in the end to have but little faith in his results, for he adopts another method suggested to him by ALTHAUS, an *Oberbauinspector* at Saynerhütte. This consisted in filling a cast-iron globe (fig. 10) or shell, M N, provided with a funnel-shaped neck, with the fused rock. During the setting by cooling of the fused mass a solidified crust formed across the neck *d e*, and as soon as this had reached quite across it was assumed (upon the most manifestly insufficient

grounds) that a solidified shell equal in thickness to half the diameter of the neck, or to $\frac{de}{2}$, had formed all round the interior of the globe, so that at that moment $a\,b$ was the diameter of the still liquid ball of fused rock within this crust. Cavities were formed within this by the final cooling and consolidation of the whole, which BISCHOFF supposes represent its total contraction. If, he says, the temperature of the liquid rock running into the sphere were exactly known, with the weight of the whole ball and that of $a\,b$ and the specific gravity of the whole, we should have enough to determine the total contraction. As it is he applies to his assumed ball $a\,b$ the unreliable coefficient of contraction deduced from his crucible experiments, and so arrives at a result.

In one instance, at least, even this result is further vitiated by an extraordinary oversight.

His calculation is thus:—

Diameter of the sphere N M 21 inches.

Thickness of the crust $\frac{de}{2}=\mathrm{N}a$ or $b\mathrm{M}-1\frac{1}{2}$ or 2 inches.

Diameter of the liquid ball $a\,b=21-1\frac{1}{2}$ or 2 inches$=19$, or $19\frac{1}{2}$ inches; average $19\frac{1}{4}$ inches.

Cubic content of this ball$=3733$ cubic inches.

Contraction$=3733\times0\cdot06$ (the coefficient of the crucible experiment)$=224$ cubic inches.

Now on BISCHOFF's own data the calculation is as follows:—

Diameter, as before, 21 inches.

Diameter of liquid ball $a\,b=21-2$ ($1\frac{1}{2}$ or 2 inches)

$=21-3$ or 4 inches,

$=$average 17 or 18 inches, or mean $17\frac{1}{2}$ inches.

Cubic content of ball of that diameter$=2786\cdot87$ cubic inches.

Contraction $2787\times0\cdot06=167$ cubic inches.

136. Mr. DAVID FORBES ('Chemical News' for October 1868) has noticed, though not in any detail, the general unreliability of BISCHOFF's results. He, however, endeavours to substitute for these deductions of his own from experiments still more fallacious, in support of his notion that the acid or basic silicates of which our earth's rocks consist *scarcely contract at all* in passing from the liquid to the solid state.

137. His facts are derived from the supposed dimensions of fused Rowley Rag stone (basalt) in the liquid state, and when again solidified after having been cast into moulds at Messrs. CHANCE's glass-works to form thin patent artificial stone, and from melted slags cast by himself into cast-iron ingot moulds of moderate capacity.

As regards the first, the moulds into which the fused rock was cast were formed of what founders call "dry sand," *i. e.* loamy sand, in which the impression of a wood "pattern" or model is made, the moulds being then dried and heated to something like a red heat, at which temperature the liquid rock is poured into them to fill the cavity.

Measurements of the wood "pattern," "often of some feet in length," and of the solidified counterpart in fused rock cast from it, according to Messrs. CHANCE's statement, proved them to be *precisely* the same size, from which it is inferred that there was no contraction at all in the rock by cooling.

Now this is exactly the same mistake that has been made by Dr. PERCY in his 'Metallurgy of Iron,' where he infers the coefficient of total contraction of cast iron from the difference between the size of the wood pattern from which the casting is made and the casting itself.

In both these cases the pattern, in order to be extracted from the damp "rammed up" sand, has to be "rapped" by the workman—that is to say, he strikes the pattern in all directions so as to make the cavity in the sand bigger than the size of the pattern, the difference being for clearance to enable the withdrawal of the pattern. If the sand mould be then dried and heated to something like redness, it will by expansion (on the whole, for there may be some little contraction at first on drying) cause the cavity of the mould to become still larger, and it is at this temperature that it receives the liquid rock.

Hence the equality in size between the wood pattern and the again solidified rock, even if *exactly* the fact, does *not* prove that there was *no* contraction, but that the amount of contraction was equal to the enlargement of the cavity of the mould, equal to the bulk of the pattern by the "clearance" given to the latter by "rapping" and by the further enlargement of the mould itself by heating; and as neither the volume of the clearance (which is never the same in any two cases) nor the enlargement by expansion (which may vary with the quality of the materials of the mould at different times, as well as with the temperature) are known, so no conclusion whatever can be drawn from those data as quoted from Messrs. CHANCE.

138. Then as to Mr. FORBES's own results, we surely cannot rely upon measurements made upon blocks of slag cast into iron ingot moulds of only 10 inches long, 6 inches wide, and 6 inches deep=360 cubic inches.

The contractions stated to be thus deduced at Eidfors Iron Works, Norway, of the slag (blast-furnace no doubt), which were from $1\frac{1}{2}$ to 3 per cent. (wide limits, the largest of which must yet be within the errors of these experiments), cannot be relied upon when we are left in the dark as to the temperature of the moulds, and therefore their actual size at the moment of consolidation, or whether any and what final cavities formed inside the solidified mass, and take into account the impossibility of measuring with any great exactness the true dimensions of such a small and even not rectangular block of slag with necessarily always a more or less rough and uneven surface.

139. Mr. FORBES's further experiments on basic slags from Staffordshire blast-furnaces, "cast into sand moulds," are vitiated through just the same reasons as those on the Rowley Rag.

140. Lastly, Mr. FORBES adduces some measurements made at Birmingham on blocks of cast glass cast in iron moulds, which either showed almost no contraction or (according

to Dr. LLOYD, of the Park Glass-Works) one of only $\frac{1}{272}$ lineal, on blocks the largest of which was but 40 lbs. weight, and not rectangular in form, being lenses in fact. We shall see further on what reliability there is in these last.

141. A few experiments on the expansion of rocks at high temperatures, but far below the fusing-points, by R. W. FOX, are recorded in the Philosophical Magazine for 1832 (3rd ser. vol. i. p. 338). Granite increased in bulk at a dull red heat by from $\frac{1}{52}$ to $\frac{1}{56}$, porphyritic felspar (from an "elvan course") the same; clay-slate augmented in size, in the direction of the cleavage, $\frac{1}{65}$ to $\frac{1}{77}$, by redness scarcely visible in the dark, and greenstone $\frac{1}{80}$. The whole of these statements look unreliable, and it is not clear whether he means cubic or lineal dilatation.

142. BISCHOFF'S final results have been summarized by Mr. FORBES as follows:—

	Volume when in fusion.	Volume when cooled as glass.	Volume when cooled in the crystallized state.
Basalt	......	963	896
Trachyte	1000	888	818
Granite	......	888	748

so that the last is taken to suffer a contraction of about one fourth of its liquid volume!

The distinction made between the glassy and the crystalline states is, in this case at least, to a great extent arbitrary; for all mixed silicates which crystallize *segregate* in cooling into crystallized bodies which float in a surrounding glassy magma that never crystallizes (unless perhaps when *devitrified* by long heating), and it depends upon the relative proportions of the two, and upon the rate and other conditions of cooling, whether the intermingled crystals shall ever coalesce or the entire mass ever assume a crystalline state.

Beyond the experiments now noticed, the writer is not aware of any others involving high temperatures, although we possess some good and reliable data as to the expansion of some stony bodies at temperatures not much exceeding the boiling-point of water.

143. The writer therefore found it necessary to institute experiments himself as to the total contraction between liquid fusion or above it and solidification of such bodies as might represent tolerably closely in chemical and physical qualities the basic and the acid silicates of the natural rocks we are acquainted with.

The blast-furnace slags of the Barrow Iron-Works seemed to approach pretty nearly the former, and British plate-glass the latter.

144. The writer has to thank Mr. T. F. SMITH, the Manager, for enabling him to conduct his experiments at the Barrow Works (near Furness Abbey, Cumberland), and Mr. MURDOCK, of the same Works, for aid in carrying them out.

145. At those Works red hæmatite only is smelted by coke fuel, with a very pure limestone as the sole flux, in blast-furnaces of the very largest class, urged by blast at a temperature of 700° to 900° Fahr. The slag formed when the furnaces are making fine grey pig iron (for the production of Bessemer steel) was that chosen for experiment.

It is usually of a light fawn colour, or of various shades of yellowish or bluish light grey.

The following are typical analyses of those slags:—

	A.	B.	C.
Silica	38·00	40·93	44·80
Alumina	10·00	9·49	11·21
Lime	42·00	42·01	36·05
Magnesia	1·65	0·68	1·24
Manganese protoxide	trace	1·83	2·12
Iron protoxide	2·08	0·64	0·84
Potash	1·60	0·59	1·56 (Potash and Soda)
Soda	2·03	0·57	
Sulphide calcium	2·45	2·72	2·35
Phosphoric acid	,,	0·01	,,

All these analyses show an excess from oxidation of the iron and manganese in the process.

The limestone used as flux contains about 97 per cent. carbonate of lime, with about 2 per cent. of silica, alumina, magnesia, and protoxide of iron, so that the greater proportion of these last found in the slag were contained in the hæmatite ores.

146. If we compare these analyses with the following of basalts &c., it will be seen that these slags in composition approach them closely:—

Basalts.

	A.	B.	C.
Silica	36·68	48·47	55·16
Alumina	14·34	30·16 (Alumina and Iron oxide)	7·42
Iron oxide	22·30		10·12
Magnesia	9·18	6·89	12·68
Lime	15·59	11·87	13·60
Soda	3·93	1·96	0·66
Potash	0·77	0·65	0·36

A and C are the extremes of 11 analyses of various basalts, and B is the normal basalt of Bunsen, from a mean of many analyses (Blum's Handbuch der Lithologie &c. 1860, pp. 180–192).

147. The difficulties in the way of obtaining any reliable measure of total contraction of fused rocks are undoubtedly great. Consideration as to their nature, and some preliminary experiments, caused the writer to conclude that the only method for approach to exactness was to operate upon very large volumes so as to extinguish many errors of experiment, and in such a manner that the volume of the fused rock could be directly *measured*, as well as its volume after solidification.

148. Three very large and thick hollow cast-iron cones, open at both ends (used at

Barrow for Bessemer steel ingot-moulds), were employed as receptacles and measures for the liquid slag.

The form of these and their dimensions are shown in Plate IX. fig. 4. Flat cast-iron base-plates were provided for these, so that when standing thereon with the wider end of the cone downwards it formed a close vessel, with its mouth or top free.

In the same vertical line at one side of one of these cones were bored partly through its thickness three holes, at top, mid height, and bottom, sloping downwards, to contain mercury or water, in which to insert a thermometer to ascertain the temperature of the mass of iron in the cone itself, the thickness of which, as seen by Plate IX. fig. 4 (No. 2), is great. The content of any one of these cones exceeds 8000 cubic inches, or above 4·6 cubic feet.

149. At successive periods each of these cones was fixed perfectly upright upon its base-plate, the latter being upon a secure and level foundation, and placed close to one of the blast-furnaces, and conveniently posited so that the slag could be run directly from the furnace into the cone, with arrangements whereby the run of slag could be instantly stopped upon the cone being filled.

The cone being at the temperature of the atmosphere at the place, about 51° Fahr., was then run up full of liquid slag, the run being stopped without difficulty precisely as the slag reached the level of the top or brim of the cone.

The slag in every case ran as liquid as very hot cast iron, and formed a pure unbroken and flat surface on the top of the cone, with a slight rounded edge due to capillarity all round the circle.

Means were taken that the stream of slag should come pure and unmixed with scoriæ &c. from the centre of the volume at the time contained in the furnace, which was for a short time previously "dammed up" to admit of its accumulation.

150. The slag so filled in continued for some time liquid; its top surface rapidly commenced to descend below the brim of the cone as the liquid slag began to lose heat by conduction to the mass of iron in the cone, and so to contract and the cone itself to expand.

Very soon, however, a solidified crust began to form over the top, the thickness of which was judged of by its resistance to penetration with a light pointed steel rod, and very soon after the before flat and level crust commenced to get hollow or concave towards the centre. This announced that a self-supporting crust had then formed also all round the cone, whose interior, however, was still liquid. This therefore marked the moment at which the true dimensions of the cast-iron cone itself must be known, as giving the true volume of the slag when consolidating at its surfaces. The dimensions of the cast-iron cones having been taken with accuracy when cold, *i. e.* at 49°, 51°, and 52° Fahr. respectively, we only need the temperature of the cone of cast iron to fix its capacity at the instant of incipient consolidation of the slag within it.

151. The cones were run up full of slag as rapidly as possible, the time of complete filling being from 4 to 8 minutes from commencement.

By separate experiments it was ascertained that water boiled in all three of the holes in the cone in 7 minutes after the commencement of filling, which may be taken as the time in which the mass of iron in the cone had reached 212° Fahr.

It was also found that the firm surrounding solid crust had formed in 22 minutes (average), and that consolidation commenced in about 20 minutes from commencement of filling. As the *difference* between the temperature of the iron cone and of the slag was very great during the whole of this time, we may admit that the increments of heat imparted to the cone were equal for equal intervals of time, and the cone gained 137° (or from 75° to 212° in these experiments) in 7 minutes.

152. Hence, as

$$7 : 137^\circ :: 22 : x,$$

the temperature of the cone at the period of consolidation of the firm crust may be taken at 430°·6 Fahr. But as the interior of the cone was hotter than the outside, and so exercised an expanding effect upon the outer portions, we must increase that number somewhat, and may estimate it at 450° Fahr.

153. The coefficient of expansion of cast iron, as given by LAVOISIER and RAY (mean of both) is

$$0{\cdot}00000618 \text{ for } 1^\circ \text{ Fahr., or } 0{\cdot}00278 \text{ for } 450^\circ;$$

and this is the coefficient of expansion which, applied to the dimensions of the cold cones, give those at 450°, as in col. 3, Table II., which are those that fix the volume of the slag at the time of its first consolidation.

154. The temperature of the liquid slag on entering the cone (sensibly the same as in the blast-furnace) was considerably above its fusing-point. It has been stated that the level of the top of the slag in the cone sunk rapidly, *parallel to itself*, and then, directly after a thin crust had formed, began to become *concave*. At this moment the mean depth of the top of the slag below the brim of the cone was taken by measurement. As the altitude of these cones is between four and five times the diameter of the brim, these measurements in *depth* afford very accurate determinations of volume, and it was found to average for the three experiments 130 cubic inches; and as 8160 is the mean capacity of the cones as taken cold, corrected for temperature, it follows that, taking the volume of the liquid slag as filled into the cones at *some* temperature above its fusing-point at 1000, the volume at its inceptive setting-point or fusing temperature is 983.

155. To approximate to what these temperatures really were, it was necessary to obtain that of the blast-furnace itself, which may be admitted as that of the slag issuing from it.

The difficulties attending the use here of any form of pyrometer, to say nothing of the uncertainties of the indications of those instruments, finally determined the writer to employ POUILLET's method, or that of mixtures, by running out directly from the furnace a certain quantity of liquid iron into water of known weight contained in a large wooden vessel, and weighing afterwards to obtain the weight of the iron which had entered. The weight of water and of iron being known, with their specific heats, and

the temperatures of the water before and after mixture with the liquid iron, the temperature of the latter is obtained. The weight of water employed was always 400 lbs.

Three good experiments were obtained as follows:—

	Original temperature of water.	Temperature of water after mixture.	Weight of liquid iron.
1. . .	$88^{\circ}\!\cdot7$	$188^{\circ}\!\cdot6$	77·5 lbs.
2. . .	100·4	152·6	41·0 „
3. . .	111·2	170·6	43·5 „

No steam was given off by running the iron into the water, and the loss of heat to the wooden vessel was very small on so large a volume of water.

The specific heat of cast iron at the highest temperature given by SCHINTZ in his work on the blast-furnace (pp. 47–51) is 0·145.

If t° be the temperature lost by the liquid iron, t the increase of temperature of the water after mixture, w the weight of water = 400 lbs., w' that of the liquid iron, the specific heat of water being taken without sensible error as 1·000 within the above range of temperatures, and s being the specific heat of the liquid iron, we have

$$t^{\circ}=\frac{wt}{w's};$$

to which we must add the initial temperature of the water before mixture to obtain the temperature of the liquid iron.

Applying this, we have

t No. 1=100°, t No. 2=52°·2, t No. 3=59°·4.

No. 1 . . . . t°=3470° Fahr.

No. 2 . . . . t°=3525° „

No. 3 . . . . t°=3735° „

And adding to these the initial temperature of the water, we have for the actual temperature of the liquid iron—

No. 1 . . . . 3470 + 89° = 3559° Fahr.

No. 2 . . . . 3525 + 100 = 3625 „

No. 3 . . . . 3735 + 111 = 3846 „

the mean of which is 3677° Fahr., or 2011° Cent.

156. This determination coincides nearly enough with SCHURER'S results, who fixes the temperature of the zone of fusion in blast-furnaces at between 2650° and 2000° Centigrade.

It is, however, *something* below the truth, because at its intensely high temperature some heat must have been lost by the liquid iron on its way to the vessel of water, and some to the *bottom* of the wood vessel, with which it first dropped into *near* contact.

We may therefore estimate the actual temperature of the furnace at about 4000° Fahr., and the above 3677° or, in round numbers, 3680° as the temperature of the liquid slag as it entered the cones.

157. The solid conical blocks of slag cooling within the iron cones reached the atmospheric temperature in 12 hours from filling, parting with 3680° in that time; but consolidation commenced in 20 minutes (or $\frac{1}{36}$ of 12 hours) from running into the cones. If, therefore, we admit that the rate of cooling in the first 20 minutes was five or six times as great as the mean rate (which the laws of cooling would seem to justify), then the heat at the stage of incipient consolidation was

$$3680^\circ-(6\times103^\circ)=3062^\circ, \text{ or say } 3000^\circ.$$

158. After each cone of slag was sufficiently solidified, the cone of iron (with the slag within) and the base-plate were lifted together, and in the vertical position, by a crane on to a waggon and carried off on the furnace tramway and deposited vertically upon a level iron platform within range of another crane, and so left to cool. After 24 hours the iron cone was gently lifted off the cone of slag it contained; and owing to the contraction of the slag and to the taper form, widest at bottom, the parting took place very readily and without materially fracturing the cones of slag, which were found sufficiently regular in form and smooth in surface to admit of accurate measurement.

159. The admeasurements were then made by means of steel callipers and scales for diameters and heights, the former being controlled by circumferential measurements with a well graduated steel flexible strip or tape*.

160. On breaking up these cones no large segregated central core of matter different from the general mass was found, nor any large cavities. The exterior, where it had come into direct contact with the iron base or cone, was vitreous, and of a more or less bluish tint; but all the remainder showed itself, on examination with the naked eye or lens, as a tolerably uniform mixture of ash-grey crystals, more or less distinct, enveloped in a light greenish-yellow or pale-brown glass, the proportion of the crystalline to the glassy matter being very great, and greatest as we approached the centre, where in some places the grey crystals (resembling Wollastonite) were very clearly developed. The material operated on thus shows itself as a true crystallized rock, and not a mere vitreous mass.

* [Since the reading of this paper it has been asked why the writer did not determine the volume of these cones of slag by weighing in air and in water, or, more directly, by making water-tight the base of the iron cone with the cone of slag within it, and ascertaining the volume of water requisite to fill up the vacancy between them. Both these methods occurred to me, and, indeed, were those employed (with mercury) by Bischoff, but were abandoned by the writer, on the grounds of many practical difficulties, in favour of the method of direct measurement.

The cones of slag being intersected by large though closed cracks in various directions could not be disturbed or weighed in air, still less in water, without much hazard, and the apparatus needed must have been powerful and expensive to make. The making the cast-iron cones water-tight at the *inner* edge of their bases would have been extremely difficult, as the iron cone, as soon as it was raised a few inches, could not be lowered again by reason of numerous minute fragments of slag detached having fallen down between; some water under so large a head would have penetrated these cracks; and for either method so many sources of error would have arisen, and so many corrections would have had to be applied, that simple measurement appeared to promise the better results.]

Table II.—Results of Experiments on Slag Contraction, Barrow Iron-Works.

1.	2.			3*.			4.	5.	6.	7.
No. of experiment.	Dimensions of cones at 51° Fahrenheit.			Dimensions of cones at 450° Fahrenheit.			Cubic contents of moulds when cold. Temp. =51° F.	Cubic contents of moulds at a temperature of 450° F.	Volumes of cones of slag as measured at 53° F.	Total contraction = difference of volumes of columns 5 and 6.
	Diameter at top.	Diameter at bottom.	Height.	Diameter at top.	Diameter at bottom.	Height.				
	inches.	inches.	inches.	inches.	inches.	inches.	cubic inches.	cubic inches.	cubic inches.	cubic inches.
Cone No. 1..	14·25	16·00	46·00	14·28	16·04	46·12	8274·1550	8334·2310	7646·2269	688·0041
Cone No. 2..	14·40	15·75	45·44	14·44	15·79	45·56	8115·5705	8175·0005	7796·3884	378·6121
Cone No. 3..	14·30	15·80	45·45	14·34	15·84	45·57	8091·9931	8156·5373	7658·0758	498·4615
Mean values..	....	....	....	....	....	....	8160·5722	8255·2896	7700·2303	521·6926

Mean coefficient of total contraction, original volume=1000, from 3680° to 53°, as 1000 : 932·76.

Mean coefficient of contraction from fluidity to solidification, or (by estimation) from 3680° to 3000°, as 1000 : 983.

Or if volume at 53°=1000, at or near above the fusing-point it will be 1072 nearly.

Or if volume at 53°=1000, at the temperature of solidification it will be 1017·3 nearly.

161. On inspecting Table II. we arrive at the following results:—

The coefficient of cubic contraction for the slag between the temperature of its issue from the furnace and that of its incipient consolidation, or between 3680° and $3000°=\frac{17}{1000}$, or the original is to the contracted volume as

$$1000 : 983;$$

and the coefficient of total cubic contraction, or that between 3680° and 53°, is nearly $\frac{67}{1000}$, or the original is to the contracted volume as

$$1000 : 933,$$

which is scarcely 6 per cent. in place of 20 to 25 per cent., as given by Bischoff.

162. We thus at once see that the difference in specific gravity, less than that between ice and water, between red-hot but solidified or even cold slag (or analogous fused rocks) and the same in liquid fusion is so slight that, coupled with the viscous or pasty condition which intervenes between the two states, it would readily admit of a thin or a thick terrestrial solidified crust being supported by and upon the surface of the liquid globe beneath, and lends no support to the view of terrestrial consolidation at the centre first, by continual subsidence of such crusts, as imagined by Poisson, nor to the notions as to the nature of volcanic action which Sir W. Thomson has based on that assumption (Thomson and Tate, Nat. Phil. p. 726 &c.).

163. A few experiments were made by the writer on the rate of cubic expansion of fragments (of a few pounds weight) at ranges between 55° and 600° Fahr., by heating them immersed in mercury in a glass vessel with a graduated stem, eliminating by calculation the expansion of the glass and the mercury, whose coefficients are well known.

These seemed to indicate that within these low ranges the expansion of the slag does

* The columns 3 are deduced from the coefficient of dilatation of cast iron, taken=0·00000618 for 1° Fahr.

not much differ from that of glass itself; but we cannot attach much value to experiments thus made.

164. Admitting them for what they are worth, however, the curve (Plate X.) would seem to represent something like the increments of the coefficients of expansion of these slags at various temperatures, through the range from 0° to 3680°.

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165. We now pass from the basic to the acid slags or silicates, of which British plate-glass may be taken as a type, as in chemical constitution not widely differing from the acid silicates in natural rocks, as a comparison of the following analyses shows:—

Plate-glass (DUMAS, 'Chim. appl. aux Arts').

Silica	73·85	68·6
Alumina	3·50	1·2
Magnesia	,,	2·1
Lime	5·60	11·0
Oxide iron	,,	0·2
Oxide manganese	,,	0·1
Potash	5·50	6·9
Soda	12·05	8·1

Gneiss (extremes of 4 analyses).

Silica	75·91	66·46
Alumina	14·11	16·20
Oxide iron	2·03	5·81
Lime	1·14	2·82
Magnesia	0·40	2·17
Potash	4·16	3·98
Soda	1·77	3·20
Water	1·16	1·59

Granites and Syenites.

	A.	B.	C.	D.
Silica	74·25	68·56	61·72	56·78
Alumina	11·58	14·44	13·57	16·64
Oxide iron	2·41	5·04	7·16	9·58
Lime	1·08	3·85	5·88	5·12
Magnesia } Soda }	10·01	{2·78	3·33	2·63
		{ ,,	3·12	5·30
Potash	,,	3·36	3·37	2·58

A and B are the extremes of five analyses of granites, C and D the extremes of four analyses of syenites.

Trachytes.

	A.	B.	C.
Silica	77·92	76·67	61·03
Alumina	12·01	14·23	17·21
Oxide iron	1·32		4·84
Lime	0·76	1·44	1·43
Magnesia	0·13	0·28	2·07
Potash	3·27	3·20	7·16
Soda	4·59	4·18	4·64

A and C are the extremes of five analyses, B Bunsen's normal trachyte of Iceland.

Porphyry, Pitchstones, Obsidian, &c. give closely allied results.

The preceding analyses of rocks are all taken from Blum's fine work, 'Lithologie oder Gesteinlehre' of 1860.

166. The writer has been enabled to obtain, from measurements made daily and habitually in the progress of British plate-glass manufacture, certain returns, from which the coefficient of contraction in that material, between a temperature not far below perfect fusion and that of the atmosphere, may be obtained, though not with perfect accuracy, yet with an approximation to truth as great as, or possibly greater than, by means of any small number of direct experiments.

167. In the manufacture of plate-glass the molten glass is suddenly poured out from the melting-pot upon the surface of a large cast-iron horizontal table. In breadth the mass is restrained from spreading laterally beyond two parallel strips of iron, fixed upon the table, the thickness of which determines that of the glass plate itself, which is produced by rapidly rolling over the heap of viscous glass a very heavy iron roller, by the passage of which the heap is evenly spread out, and a nearly rectangular plate produced, two of the sides being quite rectilinear and parallel, while the two other sides or ends are a little irregular.

The moment after the roller has thus acted, a superintendent applies to the sheet a pair of graduated beam-callipers (specially prepared), and measures from a mean point chosen in the upper and lower ends of the plate (which he marks at once *on the glass*) the mean length of the plate (the breadth being always the same), and the glass plate is as soon as possible slid off the table and removed to the "*leer*" to be slowly cooled. After it has become quite cold and is removed from the "leer" for storage, it is again measured by the same callipers applied to the same marked places; the width of the plate (known while hot by the space between the parallel rulers) is now also measured.

The surface of the plate is then calculated from those two dimensions, and the dimension and surface registered in a book, and so of every plate and of every day's work in the year.

168. Whether this be the universal practice of the trade or not, it has been so at the

Works of the Thames Plate-Glass Company at Blackwall; and the writer is indebted to the intelligent Manager, Mr. F. M. WALLER, for extracts from his books for the manufacture of the year 1861, giving those measurements taken from about 40,000 superficial feet of plate-glass.

169. From a reduction of these it appears that the total surface of the hot glass being 36172 square feet, that of the same at atmospheric temperature was 35692. Therefore the linear dimensions were as the square roots of those numbers, or as 190 : 189, or

$$100 : 99{\cdot}47,$$

whence the lineal contraction of British plate-glass, between its *very* soft viscous condition, or very near to but below its temperature of perfect fusion, down to that of the atmosphere, say 50° as the mean of the year, is $=0{\cdot}53$ per cent.

170. But three times this is *quam proxime* its cubic contraction, or 1·59 per cent.

Hence 1000 in volume of glass very near its fusing-point became 984·10 at 50°, or the contraction in volume is thus $\frac{16}{1000}$, which is somewhat (though probably not much) below the truth, were we to take the glass at the higher temperature of actual fusion.

171. The *total* contraction of the Barrow slag to that of the plate-glass is thus as

$$1000 : 933 : 984,$$

or the total contraction of the glass in passing through its whole range of temperature from somewhat below fusion is about equal to that of the slag in passing through about 680°. The writer places much reliance on this result, based as it is upon so large an area of observation, and obtained by the repetition of the same sort of measurements by the same person for long periods, and being altogether of a character to cause minute errors to disappear from the final result*.

172. We therefore may be permitted to conclude that rocks consisting of acid silicates contract still less than those of basic silicates, and that a terrestrial crust of the former is still more capable of floating upon the same in fusion beneath.

173. As applied to our globe, it is highly probable that any inferences that may be drawn from either of these coefficients must be subject to the changes in volume that may arise in the mass cooled from changes in its molecular arrangement, such as that from the vitreous to the crystallized condition, data for which are unknown. Nor, in our ignorance of the proportions in which basic and acid silicates or other bodies constitute our globe, are we able to fix any mean coefficient for the whole.

We have now, however, to attempt such applications of the numerical results arrived at as in the present state of our knowledge may enable us to illustrate the theory of volcanic action here propounded, and in some degree to test its feasibility, if not its truth.

174. Sir W. THOMSON has shown that in the agglomeration from the nebulous state of its particles from infinite distance there has been expended nearly 14 millions of foot-tons

* It may be mentioned that the Manager of the Thames Plate-Glass Works has never been given by the writer any information as to the object of his inquiry as to glass contraction, nor has he to the present day any knowledge of the coefficients for slag obtained by the writer.

of work upon every ton weight of our existing globe; and as that agglomeration was gradual, the greater part of this enormous energy was dissipated in space as heat, and is the first and most enormous dissipation of energy in the formation of our planet. The liquid globe became gradually a partially or (for any thing we can affirm to the contrary) now a wholly solid one, with a still heated nucleus.

175. The earth's diameter being at present 7916 British miles (adopting the coefficient of expansion as 933 : 1000), it was when liquid a globe 8105 miles in diameter, or at the temperature of incipient consolidation (supposing the whole globe ever was in that condition) 7957 miles in diameter; and if when liquid its mean temperature exceeded 4000° Fahr., the diameter would have been still greater.

The earth, therefore, between its period of liquidity and its present state has shrunk in diameter by 189 miles at the least. If we take as a rough measure of the energy involved in this, that it is equal to more than the work of the entire mass of the spherical shell of $94\frac{1}{2}$ miles thick dropping through 47 miles, we derive some idea of the prodigious energy dissipated or transformed in this second stage of our forming world.

If we assume $\frac{1}{4}$ the heat of the liquid spheroid to have been dissipated before the solid crust acquired such thickness as to transmit powerfully and to great distances tangential compressive strains due to contraction from that to something approaching its present state, that contraction represents work equivalent to 186,120 foot-tons for every ton of matter in the spherical shell comprehended between the radius of liquid fusion and the existing radius of our earth.

176. That portion of this immense energy not dissipated as radiant heat was, as we have seen, consumed as work in the deformation of the spheroid, in the plication of the thinner crust, and in the elevations by tangential pressure of the mountain-chains.

177. At length we arrive at the present condition of our globe, with its primitive energy almost exhausted, yet with enough remaining (in what comparatively is but the dregs or ashes) to carry on what seems to us the prodigious work of existing vulcanicity.

178. The earth is still a cooling globe; and whether we adopt ÉLIE DE BEAUMONT'S figures (0·0065), or THOMSON'S (0·0085), or J. D. FORBES'S (0·007 millimetres) for the thickness of the plate of ice which, covering the whole earth's surface, if melted to water at 32° Fahr. would equal the heat lost annually by our globe, the result will be that from 575 to 777 cubic miles of ice liquefied to water at 32° represents the annual loss of heat at present from our globe.

179. On the assumption that our globe has cooled from a much higher temperature and is still cooling, it is not deniable that at the remote geological epoch when the existing form of volcanic action began, no matter whether that was anterior to the Secondary epoch or when, the annual loss of heat must have been greater the further we go back in time, and must have exceeded 777 cubic miles per annum.

We shall, however, adopt THOMSON'S, as probably the nearest the truth. As the latent heat of fusion of ice is $=143°$ Fahr., and taking the cubic foot of ice to weigh 57·6 lbs., we have $143° \times 57{\cdot}6 = 8237°$ of heat in the cube foot of liquefied ice.

But we have already found that the heat developed by the crushing to powder of 1 cubic foot of mean rock is $=6472$ British units of heat.

Hence a cubic foot (or cubic mile) of ice requires 1·27 cubic foot or mile of crushed rock to liquefy it.

Therefore, if the total amount of all the heat annually lost by the earth were produced from crushed rock in the contracting crust (which it certainly is not), it would only require $777 \times 1{\cdot}27 = 987$ cubic miles of such crushed rock to produce it.

180. This seems a large amount; but it is as nothing compared with the mass of the globe.

If, for example, we suppose it all crushed within a shell of solid crust whose volume is but one fourth that of the entire globe, it is less than the one 65 millionth part of the volume of that spherical shell, and if spread over the earth's surface would form a mere film. There would therefore be nothing incredible were we to suppose that nearly the whole of the heat annually lost by the entire globe was equalled by that produced by the crushing of its crust by the contraction of the entire globe. Nor, as it appears to the writer, would the annual amount of contraction necessary to account for that amount of heat be inadmissible. It is certain, however, that the whole of the heat lost annually by the globe cannot come from such a source, but only a very small proportion of it, because the lost heat is the source of the contraction.

181. We appear obliged by the phenomena of hypogeal increase of temperature (perplexing as they are) to conclude that by far the largest proportion of the heat annually lost reaches the surface from a cooling nucleus, which surface, though in various degrees, it everywhere reaches; whereas volcanic activity is confined to narrow lines spread widely apart over the surface and parting with very little heat by lateral conduction.

The spheroidal wave of heat constantly passing upwards from the nucleus to the surface everywhere must be dissipated from the dry land by radiation, consumed in heating the water of the ocean and helping in the production of its currents, and is partly brought up by thermal waters which have infiltrated cold from the surface (see Appendix A).

182. Contraction due to this constant cooling (and that greatest in the nucleus, which is hotter and contracts most for given amount of cooling) is the necessary result, and with it crushing together of the crust as it follows down after the shrinking nucleus, and through the work expended in that crushing the production of a distinct source of heat, the amount of which must be demonstrably sufficient if it be the true source of vulcanicity.

Let us then attempt to estimate, so far as our knowledge of the amount of volcanic action going on upon our globe may enable us, whether it be sufficient or not.

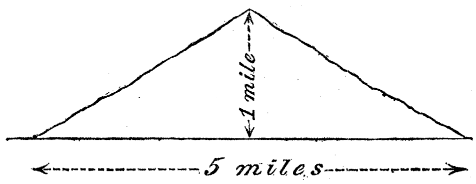
183. Volcanic energy as witnessed on our globe is mainly expended in three ways:—

1. Heat converted into work of elevation and ejection.
2. Heat employed in fusing or heating solid ejecta.
3. Heat wasted and dissipated in steam &c. at volcanic vents.

184. Had the writer been fortunate enough to have completed his projected pyrometric determination of the actual temperature deep in the Vesuvian crater (which the Royal Society aided some years back, but which the sudden alteration of the state of that volcano prevented), we should have been better prepared to make the following estimation.

185. All the volcanic cones upon our globe may be averaged as each equal in volume to a solid cone of one mile in height above the base of five miles in diameter; for while many are far below this height, and the majority of those measured do not reach this elevation, all the loftiest cones (above sea-level), such as Cotopaxi, &c., stand upon elevated tablelands or plateaux not volcanic or but covered skin deep with volcanic matter, and so have only a real elevation as volcanic cones of about 5000 feet. Thus Cotopaxi stands on the plain of Quito, 9000 feet above the sea. Antisana and two or three others only upon the whole globe appear to be exceptions; but it is very doubtful whether any of these cones really rise from sea-level; and the number is so small of these cones reaching 10,000 feet and upwards as not materially to affect the result.

Fig. 11.



186. The volume of one such cone (5 miles base $\times$ 1 mile high) is 6·54 cubic miles. As all volcanic cones are mere "cinder tips," masses of dust, lapilli, and scoriæ, to which the volume of solid lava beds bears a very small proportion, so we cannot take the average specific gravity of their materials at probably more than 2·0, or 0·05 of a ton per cubic foot; so that in one such cone we have 48,133,730,304 tons, to elevate which to the height of the centre of gravity of the cone 0·25 mile above its base requires 63,536,524,001 foot-tons; and as there are 2°·9 or nearly 3 British heat units in a foot-ton, dividing the above we obtain 21,178,841,333,760 British units of heat equivalent to the work. But we have found that one cubic foot of crushed (mean) rock evolves 6472 such units, dividing by which we obtain 3,272,370,686 cubic feet of such crushed rock to perform the work, which is $\frac{100}{4498}$, or less than $\frac{1}{45}$ of a cubic mile of crushed rock to perform the lifting work from base of the cone; or if we suppose it lifted from 10 miles deep below the base of the cone, then $\frac{40}{45}$, or less than 1 cubic mile of crushed rock.

187. As respects the heating and fusing work, the observations made everywhere on volcanic cones indicate that but a very small proportion of their total mass has been fused, the rest having been merely heated. It is probably below the truth to assume that there is twenty volumes of such heated matter (dust, lapilli, scoriæ, &c.) to one of fused lava.

The whole of this material, before being exposed to volcanic heating, exists at the hypogeal temperature due to its depth.

We may assume this temperature as above 300° Fahr., or that due to from fifteen to twenty thousand feet depth, that the *heated* material is raised to 1000° Fahr. (the

melting-point of silver) and the *fused* material to 2000° Fahr. (that of liquid cast iron) from the initial temperature.

188. It follows from equation (6) that 1 cubic mile of crushed *mean* rock will thus heat 0·262 cubic mile of the heated material, or will fuse 0·108 cubic mile of the fused material, the specific heat of all being taken as the same.

And from the relative proportions of the dust and scoriæ to lava above assigned, we obtain as the result that 1 cubic mile of crushed *mean* rock will supply the quantity of heat necessary for 0·255 cubic mile of the mixed heated and fused material constituting a volcanic cone, or that 3·92 cubic miles of crushed *mean* rock is required for each cubic mile of volcanic cone.

Hence each such cone of the volume previously assigned requires $6·54 \times 3·92 = 15·636$ cubic miles of crushed *mean* rock for its heating work.

We have to add to this the lifting work as previously found $=0·888$ cubic mile, in total $=16·524$ cubic miles of crushed rock.

189. We have further to estimate the waste. From the low average conductivity of rock materials (perhaps not $\frac{1}{30}$ that of silver) the loss of heat by conduction to the walls of the volcanic focus and tubes to surface may be taken as insensible. The main source of waste is in heat units expended in producing steam, that yields no effective action in lifting or is spent in aërial ejection above the crater.

190. We have no data for more than a probable conjecture as to what may be the average amount of this. It may be remarked, however, that the sources of loss of effective action of the steam here do not resemble those in the steam-engine, in which, as HIRN has shown, about nine units of heat are wasted to one utilized, but rather resemble those of the gunpowder-gases in a cannon through windage and powder blown out unconsumed, &c., the losses from which ballistic experiments prove to be much smaller.

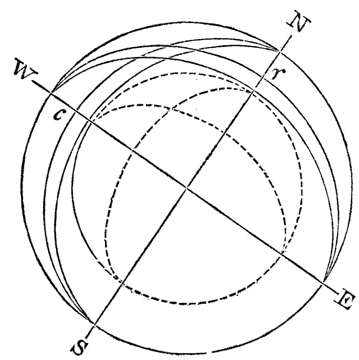
We shall allow, therefore, here that double the units of heat utilized in lifting are wasted, or that three times the lifting work is that actually expended, or $3 \times 0·888 = 2·664$ cubic miles of crushed *mean* rock.

191. Thus the lifting, heating, or fusing, and wasted work together, for one volcanic cone of the mean volume above assigned, demand a total of 18·3 cubic miles of crushed mean rock, or 18 cubic miles in round numbers.

192. Now there are about 400 volcanic cones known or stated to exist on the globe (a number sanctioned by HUMBOLDT and other good authorities), and averaging them all at the dimensions above, we find that $400 \times 18 = 7200$ cubic miles of crushed rock would have sufficed for their production.

193. [It thus appears that were the entire of the 987 cubic miles of crushed rock representing the *annual* loss of heat of our globe all consumed in volcanic energy, it would suffice to form all the volcanic cones upon our earth in less than *eight years.* Tens of thousands of years have been actually spent on the operation, from which we may see how excessively small must be the amount annually expended in vulcanicity.]

Fig. 12.



194. Taking now the earth's mean diameter at 7912 miles, two spherical lunes at right angles to each other, N*c*S and W*r*E, fig. 12, each of 180° chord, having a radial depth of 10 miles, and having no thickness at the extremities N S and W E, would at a width of only 255 feet at the equator and pole respectively be equal in volume to the 7200 miles; so that a circumferential contraction on the surface of the earth, if all in two orthogonal great circles not exceeding 255 *feet* on about 25,000 *miles*, is all we need to supply the volume of crushed rock within the first 10 miles of the surface, or vastly less if the contraction be extended down to a crust of 100 miles or 800 miles deep; and this crushed rock is not subtracted from the earth's volume, but simply transposed from beneath to its surface.

These 400 volcanic cones probably do not very inadequately represent the totality of volcanic action that has taken place since the Tertiary or even a more remote epoch; we know not how long that is; but if we spread the volcanic action over even a few thousands of years, we see what an almost infinitesimally small amount of annual crushing by contraction is sufficient to account for the Phenomena.

It is not likely that future exploration will greatly add to the number of known volcanic cones, extinct, dormant, or active. Africa, the only great continent not yet tolerably known, seems to contain very few and probably none in the interior; Borneo, New Guinea, and the Antarctic continent may have a few; but it is improbable that 10 per cent. remain altogether to add to those already known.

Again, it is highly improbable (on grounds which it is impossible here to enlarge upon) that there are *many* submarine volcanoes, most probably none at all over the vast area of the bed of the deep ocean. One area alone has been discovered beneath the Atlantic Ocean, traversed as that has been for centuries now by ships continually.

Some no doubt there are in shallow water, here and there, and more especially off the Pacific coasts of the American continent.

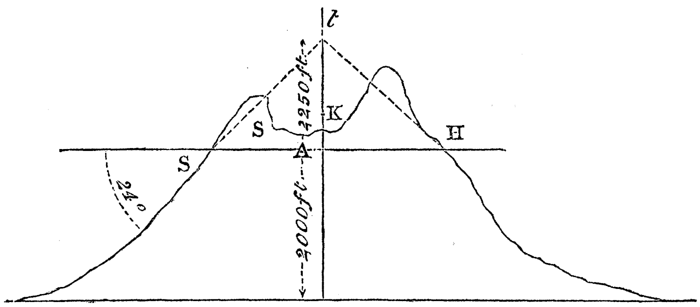
However, let us grant " scope and verge enough," and assume that one half as many more volcanoes as those we know of remain to be added, or that their total is 600 in place of 400; the result will be that in place of 7200 cubic miles of mean crushed rock we shall require 10,800 cubic miles—a volume still perfectly insignificant when spread over the vast volume of the earth's crust at 800 miles or even 100 miles thickness, and diffused in time over the unknown ages that have elapsed since the commencement of the existing forms of volcanic action.

195. We may illustrate the matter in another way, taking for basis the best observed volcano in the world, Vesuvius, and estimating the annual vulcanicity of the whole globe by the scale afforded by its very numerous eruptions.

Let it be assumed that the whole cone of Vesuvius above the level of the Hermitage,

or below the Atrio del Cavallo, A, taken at 2000 feet above the sea-level, has been

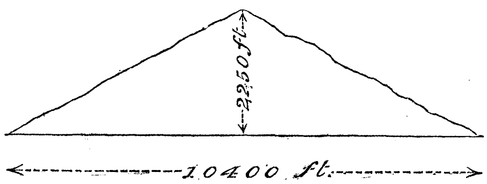
Fig. 13.



removed and replaced since the year 79 A.D. This is certainly above the truth, and exceeds Professor PHILLIPS's estimate (Vesuvius, pp. 248–9 &c.); for it takes no account of the mass of Somma, S, and the remains of the old mountain at the sea side, H (Pedamentina), still remaining, and lets these go against all the lava-streams since A.D. 79 and the wind-diffused dust, the united volume of which cannot but be much less than that of Somma alone from the Punto del Naso down to the level of the Hermitage.

The base-angle of the imagined cone S t H we take from SCROPE, and the writer's own observation of the angle of Somma at the level of the Atrio, at 24°, the total height of the Vesuvian summit t at 4250 feet, which gives for the altitude of the cone (removed more than once) 2250 feet and the diameter of base 10,400 feet, or for simplicity let us call it 10,560 feet or 2 miles. The altitude being 0·426 of a mile, we have 0·446 cubic mile for the volume of the cone. Let it be assumed that this volume has been *thrice* blown away and evolved again during the last 1800 years; then 1·338 cubic mile of melted or heated and elevated material represents the *useful* work expended on the volcano for the above period.

Fig. 14.



196. Supposing that work not to be paroxysmal (as it really is in every volcano) but spread uniformly over the 18 centuries, we have $\frac{1\cdot338}{1800}=0\cdot000743$ cubic mile per annum, or 109,368,114 cubic feet per annum, raised 2000 feet from sea-level to the base of the cone and $\frac{1}{4}$ of 2250 feet further to its centre of gravity.

Taking, as before, the weight of the material at $\frac{1}{20}$ of a ton per cubic foot, we have for the lifting work 14,010,053,610 foot-tons, equivalent to 4,831,052,974 British units of heat. Dividing this by the units of heat in a cubic foot of mean crushed rock as before, we obtain 746,481 cubic feet, or less than $\frac{1}{200,000}$ of a cubic mile of crushed rock as necessary to do the annual lifting work of the cone itself. Allowing, as before, that double the number of units of heat are wasted of those usefully employed in lifting work, we have for the lifting work and waste together ·000015 cubic mile of *mean*

crushed rock; but, as before, we assume the lift to be 10 miles (and not from sea-level to centre of gravity of the cone), or 21 times nearly the above.

Hence we have for the total work of the "cone building" in elevation, heating, and fusing (taken, as in the previous case, at 3·92 cubic miles of mean crushed rock to each cubic mile of volcanic cone material) as follows:—

Total lifting work and waste	·000315
Heating and fusing work on ·000743 cubic miles of cone material .	·00291
Total in mean crushed rock in cubic miles per annum	·000606

197. Now the total number of *active* volcanic foci known on our globe, as given by HUMBOLDT and others, is 270; let us take them at 300, and that all, small and great, when active are as active as Vesuvius, and produce the same average annual amount of equally heated or fused ejecta while in a state of activity. Both assumptions are greatly above the truth, probably, when we consider how many *very* small volcanoes there are (such as Stromboli) always active, but whose ejecta in a year (at present and for all history) have been insignificant in proportion to the few very great ones and the immense periods of dormancy of the majority. How many of the active volcanoes are on the average in eruption every year we know not; it will be admitted as probably above the truth if we assume that one in every three of them is so; that amounts to 100 cones always at work, at the rate of activity which we have taken for Vesuvius, which is notoriously one of the most frequently active on the globe*.

The final result, then, is that $\frac{300 \times \cdot 000606}{3}$, $= \cdot 0606$ cubic mile, represents in crushed rock per annum a considerable excess above the total existing annual vulcanicity expended on our earth.

198. [Another form of estimate may be employed. The following are amongst the greatest single flows of lava of which any attempt has been made to approximate to their volume:—

Graveneire	57 million cubic metres.
Pariou	33 million cubic metres.
Mont Sinuire	172 million cubic metres.
Come	344 million cubic metres.

These are all in Auvergne, as given by M. LE COQ (Epoques Géolog. d'Auvergne, t. iv.), who states the great uncertainty attending the cubation of every lava-stream, and deems that of Come as very doubtful.

Skaptar Jokul, 1783: 164 millions=1640 millions of cubic metres (Voy. en Islande, &c.).

I omit another from the same work, affirmed, on utterly fallacious data, to have exceeded the volume of the entire mass of Mont Blanc.

* The catalogues that exist of the periods of activity and of rest of Iceland, Etna, and Vesuvius appear to contain the fact that in those cases at least the years of activity to those of dormancy do not reach one in three (see DAUBENY's 'Volcanoes' and PHILLIPS's 'Vesuvius').

Etna, 1669: 600 millions cubic metres, according to BORELLI, but on very unsatisfactory data.

M. CORDIER (Essai sur la Température de l'Intérieure de la Terre, &c.), after discussing several of the greatest lava-streams on record, including the above, and admitting their uncertainty, comes to the conclusion that 1000 millions of cubic metres exceeds the extreme volume of any lava-stream of which we have any evidence whatever.

This applies to lava-streams from volcanic action *proper*, and excludes those enormous wellings forth (*épanchments*) of basalt, trachytes, or other ancient lavas antecedent to the existing epoch of true or explosive volcanic action, such as have been observed in California, the magnitude of which often wholly dwarfs that of the very greatest volcanic outpourings.

The writer believes all the greater figures preceding, including M. CORDIER's estimate, to be greatly in excess of the truth; but let us adopt M. CORDIER's volume. Let us further assume that, in addition to this 1000 million cubic metres of material *all* fused, we have the same proportion as present supposed of *heated* but not fused incoherent material, viz. twenty times the volume of lava; and, lastly, that the entire is *lifted* from ten miles depth, and, in addition, to the summit of a cone higher than Hecla, or 5280 feet (=1 mile) more, and that, as before, we have double as much lifting work wasted as is usefully employed, *i. e.* three units of heat to do the work of one.

We then obtain the following results:—As there is 1·307 cubic yard in 1 cubic metre, 1000 millions of cubic metres =1307 millions of cubic yards fused from 300° Fahr., requiring 9·9 cubic yards, or nearly 10 cubic yards, of crushed rock for each cubic yard fused, $=10\times1307=13070$ millions of cubic yards of crushed rock.

Then for *heated* material raised from 300° to 1000°, we have $20\times1307=26140$ millions of cubic yards, requiring 3·8 cubic yards of crushed rock for each cubic yard heated, or $3{\cdot}8\times26140=993320$ millions of cubic yards of crushed rock.

Then for the lifting work, taking the density at 1·2 ton per cubic yard, or 1·2 ton $\times$(993320 millions+13070 millions) =1006390 millions of tons lifted; and as $3^\circ=1$ foot-ton, $\frac{1006390 \text{ millions}}{3^\circ}=335463$ million units of heat to lift it 1 foot high; and dividing by 6472 units of heat in 1 unit of volume of crushed rock, we require 50·3 cubic yards of crushed rock to lift 1 foot high, or $50{\cdot}3\times5280=265584$ million cubic yards to lift 1 mile high, or 11 times this for 11 miles, in all =2921424 million cubic yards of rock crushed.

Then for wasted heat we have double this, or 5842848 millions of cubic yards of crushed rock.

Summing up our results, we have:—

Fusing work	= 13070	million cubic yards.
Heating work	= 993320	,,
Lifting work	=2921424	,,
Wasted work	=5842848	,,
Total in cubic yards of crushed rock . .	9770662	,,

which, divided by the cubic yards in one cubic mile, 1760^3, or

$$\frac{9770662000000}{5451776000}, =1792 \text{ cubic miles}$$

of crushed rock to produce the whole.

The lost heat (in crushed rock) of our globe could produce this amount in less than two years. But such an eruption as the above does not occur probably in a hundred years, if such an eruption ever occurred at all.

But that is only the $\frac{1}{16285}$ part of the equivalent in mean crushed rock of the heat at present lost annually by our earth, which, as we have seen, is equal to 777 cubic miles of ice melted, or to 987 cubic miles of mean rock if crushed (992 cubic miles if we take the weight of a cubic foot of ice at 57·8 pounds).]

The volume of rock necessary to be crushed annually as thus found is (as in the former estimate) perfectly insignificant, when compared with that of a solid crust of 800 miles thick or even of 100 miles in thickness.

199. Finally, it is apparent that even were our estimates of past or of existing volcanic energy of our globe below the truth to such an extent that *ten times* the estimated amount of crushed rock would be needed to supply it, we should still have an ample storehouse of energy for it in the heat *now* annually lost by our globe, leaving still the greater part of that to be wasted by radiation into space.

200. The rock thus crushed *transfers* a portion of its own mass from a greater or less depth to the surface, and frees the cavities from which it is ejected of so much bulk, permitting them thus to close in and accommodate the dimensions of the shell to those of the shrinking nucleus beneath; but the mass is *only* transferred, it is not lost; and the transfer might have no effect whatever on the length of the day, even were its mass far greater.

Nor is the volume of rock crushed (to perform the volcanic work) necessarily all ejected; from any one point, on the contrary, much of it may cool again *in situ* with extreme slowness, and get recompacted into solid rock.

201. The writer believes, however, that a considerable proportion *is* ejected, and that this is, in fact, the function or final cause in the cosmos of vulcanicity. It is the means whereby a contracting solid crust gradually, and, though paroxysmally, on the whole harmlessly, adjusts itself to the dimensions of the nucleus shrinking away from beneath it; and were it not for this provision in the grand machine, or were the solid crust so rigid and constituted that its parts could not locally crush up, and the crushed matter be cleared out and thrown up to the surface, prodigious paroxysmal convulsions must result, with perhaps ages intervening between them, which would probably overturn the whole economy of the surface upon which the existence of organized life is now dependent.

Admitting fully within what wide limits of error estimates such as these alone admit of being made, the writer yet submits that he has proved to a high degree of probability that

1st. The crushing of the earth's solid crust affords a supply of energy *sufficient* to account for terrestrial vulcanicity*;

2nd. That the necessary amount of crushing falls within the limits that may be admitted as due to terrestrial contraction by secular refrigeration;

and if so, that the cause thus assigned is probably the true cause of existing volcanic action, will further appear on comparing the conditions, or some of them, that we can predicate must follow from such crushing action going on locally within the earth's solid crust, with some of the best known facts of observation of volcanoes themselves, to which we now proceed.

202. A primary characteristic of the view of volcanic action here proposed is, that it is only one phase of a *unique* force which has always been in action, though always decreasing in energy, since our planet was nebulous.

It introduces no hazy hypotheses of "reaction of the interior against the exterior," of internal distension by unknown gases, of chemical actions in the interior unsupported by proof of their existence.

It simply postulates an always cooling globe subjected to gravitation, and through these two undeniable premises it links together as the successive products of two forces only, refrigeration and gravitation, the formation of the land and ocean-beds, the elevation of mountain-chains, and volcanic action as now existing. Simplicity is the characteristic of every hypothesis upon which any true theory of the operations of nature has ever been produced.

203. The long prevalent view of geologists, that volcanic heat and steam explosive power arose from water making its way from the surface through an exceedingly thin solid crust to a universally liquid and fiery nucleus, is only tenable on the admission of such thinness of crust (probably 30 to 50 miles at most) as is quite incompatible with observed thermal conditions both on the surface and beneath it.

But if we admit a very much thicker solid crust (from 300 to 800 miles), it is incredible that surface-water should ever find its way through such a depth of dense material to the liquid nucleus; yet without water we can have no volcano, steam being admitted on all sides to be the ejective agent. The wholly gratuitous hypotheses of SCHALER, of Boston (Proc. Bost. Nat. Hist. Soc. 1866), of a liquid spherical shell between a solid nucleus and a solid crust, and of HOPKINS, of isolated liquid subterraneous lakes of lava within an otherwise solid globe, do not remove the difficulties as to their connexion with the surface-waters, and are exposed to insuperable objections to their existing at all.

204. SCHALER'S nucleus must be in unstable equilibrium as to position. Objections

* As this paper cannot be extended so as to include any special considerations of the effects as to fusion, chemical combination, or decomposition producible by a given amount of thermal energy acting on the substances within our earth's crust, it is here only possible to remark that in such considerations the effects of heating substances under pressure, as pointed to in the recent experiments of DEVILLE and GERNEZ and of FRANKLAND, all tending to show that the same amount of heat is more effective chemically as the pressure under which it acts is greater, must not be neglected; nor, on the other hand, those of M. CAILLELET (Les Mondes, 1869) on the limits to chemical action set by its taking place under pressure.

have been already urged to Hopkins's lakes. Either the lakes or the liquid zone lie far too deep to be in communication with the surface-waters; and if once such a communication be supposed established, no reason can be assigned why the volcanic eruption thus brought about should ever end before either the limitless supply of water were ended or the reservoir of liquid lava completely pumped out.

205. Whether coming from liquid spherical shell or lake of lava, it is hard to see why the lava of the same volcanic vent drawn from the same reservoir should not always be the same.

Every thing indicates that the actual focus, where fire and water contend and produce volcanic action, is at no great depth below the volcanic vent.

The directions of the shocks felt during eruptions by observers not far removed from the axes of volcanic vents conclusively establish this. The centre of impulse of these shocks is coincident (on the whole) with the volcanic focus. Now, were this at a great depth, the emergent wave-paths around the base of the volcanic cone, and for considerable distances from it, must be almost vertical; houses &c. shaken down must show that they were so by forces suddenly throwing them upwards and letting them fall again in lines not very far from vertical. But such are not the facts even in the close neighbourhood of the great South-American and Oriental volcanoes; the shocks near the base are felt to approach nearer to horizontality than verticality, as is also the case with the best observed European eruptions.

206. But the writer is enabled to produce direct proof of this in the case of the greatest of European cones. In 1864, while exploring Etna, he noted and measured the directions of the cracks produced in a large number of more or less ancient church-towers and other buildings by the shocks of successive eruptions at various periods.

These observations were made at different towns or places, extending round a very large arc of the total circumference of the mountain. In every instance the masonry fractures pointed to a wave-path coming from near about the axis of the cone, and from a centre of impulse situated not very many miles below the level of the sea. This is conclusive as to the focus being not very deep; were it 800 miles deep, or half that, the injury done to towers and buildings must present a wholly different character, and the apparent verticality of the shocks could not escape universal notice.

207. But if the volcanic foci lie in lakes or perennial spheric sheets of liquid lava, then they must be, *ex hypothesi*, below the thick crust, and at an immense depth, and that nearly the same everywhere.

But water *must* reach that depth, great as it may be; and assuming the possibility of its access, it is difficult to see how, unless (notwithstanding the high temperature) it is compressed to a greater density than the liquid lake of solid lava, it can so get beneath or mixed up with the latter as to be able to blow it up in a *boursouflé* condition, and enable it to reach the surface through ducts or fissures of 500 or 800 miles in length, the walls of which are comparatively cold.

208. Recognized phenomena of different volcanoes indicate that they do not all come

from even nearly the same depth; but the depth must be nearly the same for all if the foci be in a spherical *couche* of liquid lakes left by refrigeration of the rest of the globe, as imagined by HOPKINS.

209. These difficulties disappear on our theory. The focus of heat may be at any depth, because crushing of the solid crust may occur at any depth, dependent on thickness of crust, *couche* of maximum tangential pressure or of greatest crushing resistance, &c.; and in general the tendency must be for the crushing simply to occur at no great depths from the surface.

210. The crushing is local, both as to surface and depth; where it occurs, being in the weakest parts of the crust, the fissures for admission of water are the most likely to be present.

211. The result of the crushing is to produce irregular masses, on the whole tending to verticality, of pulverized rock, heated more or less highly, that may extend to any depth within the solid crust; but it is only to such depth as water can percolate or infiltrate by capillarity that the deepest focus of volcanic activity can be found.

Below that the crushed and heated rock may exist, but it remains quiescent unless water reaches it or gases be evolved by chemical action increased by the heat. When water does reach such a heated mass of crumbled rock, it readily finds its way through the whole mass, which absorbs it as red-hot sand does water poured into it. Steam is produced if the temperature due to crushing be sufficient to raise both the crushed rock and the water (under the pressure of its superincumbent column and the resistances of the water-ducts) to the fusing-point, *boursouflé* lava results, and at sufficient elastic steam pressure is ejected, perhaps enlarging its own vent of issue by some preexistent fissure, by fusion as it rises, and subsequently by abrasion.

212. The researches of later years, and especially of JAMIN and DAUBRÉE, have shown that *infiltrating* water through the capillary pores of permeable rock may continue to pass through such against a heavy steam pressure; so that whilst the water continues to enter thus a heated cavity full of hot crushed rock, urged by an insistent head and by capillarity in the rock, no steam can escape back through the porous rock preoccupied by the water, just as a porous filtering stone under a head of water would continue to pass water into a fire, though the latter contained steam or gases under great pressure which could not pass out through the stone. We have thus all the conditions in our focus needed for the production of such variations of lava as we actually witness. Analyses have shown that while there is a great general similarity of constitution in lavas all over the globe, they yet do differ enough in constitution considerably to affect their degree of fusibility. We are also able to observe that some volcanic vents produce more fusible lavas than others, that some volcanoes produce little lava and much heated and pulverized material, and some nothing but the latter, no lava at all.

213. We can also see (in a good many cases at least) that the fusibility of the lava, and the proportion of its supply to that of unfused heated matter, dust, and lapilli, is referable to two (coexistent or not) elements of cause—the more or less fusible chemical

constitution of the lava, and the higher or lower temperature of the focus; and we can further see that the constitution of the lava has some relation to the successive lithologic formations in which the focus and vent are situated or through which they pass.

Siliceous crystalline rock and aluminous rocks alone, pulverized and fused, produce, for example, highly infusible lavas; siliceous and calcareous, still more those with certain portions of aluminous or ferruginous rock, much more fusible ones. The old but valuable experiments of KIRWAN are worthy of being still consulted on these points (KIRWAN'S 'Mineralogy'), the fusibility in all cases also being largely influenced by the alkaline contents of the water, sea or fresh, that finds its way to the focus.

214. Now all these conditions are accountable for on the supposition of local and often more or less distinct foci of heated and pulverized rocks, differing in composition at different depths of the heated column.

We have also an adequate cause for the great differences of temperature at different and even closely adjacent vents, in that the heat at the focus is not derived from any invariable source at a nearly constant and uniform temperature, as in HOPKINS'S notion, but is directly proportionate to the local tangential pressure which produces the crushing and the resistance thereto, and may vary to any extent at different points or at the same point at different times.

215. In this, too, we find an adequate and easy explanation of the absolutely non-periodic activity of volcanoes and their occasional sudden and violently paroxysmal action, as well as for their long periods of repose, and for the absolute extinction of some and the breaking out of new ones at points where none previously existed.

216. The secular cooling of the globe is always going on, though in a very slowly descending ratio. Contraction is therefore constantly providing a store of energy to be expended in crushing parts of the crust, and through that providing for the volcanic heat. But the crushing itself does not take place with uniformity, it necessarily acts *per saltum* after accumulated pressure has reached the necessary amount at a given point, where some of the pressed mass, *unequally pressed* as we must assume it, gives way, and is succeeded perhaps by a time of repose or by the transfer of the crushing action elsewhere to some weaker point.

217. Hence, though the magazine of volcanic energy is being constantly and steadily replenished by secular cooling, the effects are intermittent, and just provide from year to year the amount of force which is consumed in vulcanicity.

It is one of the many cases in nature in which the uniform development of a force results in variable and intermittent action as the effect of the force; it is steadily produced and accumulated, but unequally or paroxysmally expended. That such slowly accumulating pressure on local points of rigid solids does produce their giving way by crushing paroxysmally may be illustrated to the senses by pressing slowly and steadily a lump of sugar held by the fingers against a table; some of the points or surfaces in contact crush to powder, there is a momentary repose; we continue the pressure, or slowly or slightly increase it, more crushing, and another repose follows.

218. One cause for the extinction or long repose of volcanoes has been more or less perhaps recognized, they may get "drowned out:" the activity is dependent upon a certain balance between the supply of heat and the supply of water; if the latter become excessive, action dwindles to the solfatara or ceases altogether or for a time. But no prior theory assigns any cause for variability in the supply of *heat*: if that came from an immense liquid nucleus, it must be always the same and inexhaustible practically; if from the imaginary lakes, it might be slowly exhausted, but could not rapidly decline nor suddenly vanish, unless by the draining dry of its contents or of the heat of the whole lake.

On our hypothesis, however, we find an adequate explanation for the sudden production at a given spot and for the rapid exhaustion of the source of heat, and for its possible non-production again at the same spot or its production at another, adjacent or remote; in other words, for the long observed shifting of the position of volcanic vents in the course of time, as well as for the secular enlargement of the superficial areas within which their action occurs, as remarked by HUMBOLDT (Cosmos).

219. Lastly, it presents a complete solution to the question, Why should volcanoes present the linear arrangement they do on our globe, and why should they on the whole follow the lines of great mountain-chains? As manifestations of a common cause, contraction by secular cooling, but different in degree, the mountain-ranges heaped up by tangential pressure (as has been already stated) have been formed along lines of contrary flexure and of great fissuring and weakness in the earth's crust. But it is along such lines of weakness that the crushing by tangential pressure of the cooler and more rigid crust must principally occur; so that we may admit that at present the entire, or nearly so, of the tangential pressures produced by secular cooling are balanced by crushing *limited* to those great lines.

Hence the line of volcanic vents follows the mountain-chain:—1st, because there the fissures and vents of a shattered crust are chiefly found; 2nd, because it is in such lines that the chief crushing goes on as being the weakest places, so that there the heat for the volcano is provided. Here the "lake theory" signally fails. On what conceivable grounds shall we imagine these imaginary lakes arranged beneath great curved lines on the surface, as beneath the immense line of volcanoes that girdles the Pacific?

Why, if produced as residues of a frozen liquid nucleus, should they not rather be scattered pretty evenly in a *couche* beneath the whole surface of the earth? But if they be so, then the greater part of them must be hermetically sealed up from water, or, by hypothesis, we should have volcanoes dotted all over the earth; and this last is equally true as applicable to SCHALER's liquid zone or a *universal* liquid nucleus.

220. Along such lines of volcanoes one vent or another may start into activity, according as the crushing energy beneath supplies more heat and pulverized material than another; and we find an explanation for the observed (or supposed to be observed) fact without calling in the very crude notion that one volcanic vent relieves another, all drawing from a common fiery ocean or from lakes beneath.

221. We also see that it is but a partial view to say the volcano is a safety-valve to the earthquake; for the volcano is really the safety-valve for the relief from time to time of the effects of contraction by cooling of our globe; and there is perhaps no more convincing consideration, indicating that the motive cause here assigned for vulcanicity is the true one, than is found in the fact that the assigned mechanism is one of nature's balancing adjustments, that the volcano's work is exactly proportionate to the crushing energy of the contraction that brings it forth, and which is thus drained off, gradually on the whole, though intermittently, in place of accumulating, until by the crushing together of vast masses at once of the earth's thick crust, cataclysm must arise destructive to the living creation. The action resembles that of the escapement of a clock, which lets the weight drop not uniformly, but slowly and gently, which if permitted to descend suddenly through a large space, must destroy the machine.

222. If, then, this be the true nature of volcanic activity upon our globe, it must be so for other planets, so far as their construction is analogous to that of our own.

And should future improvements of the telescope ever enable us to examine the surfaces of the other bodies of our solar system with sufficient exactitude, we shall be able in some degree to test this. At present, however, we can only apply it to our own satellite, and ask does it give us any information as to the peculiarities that its surface reveals to us in respect to bygone vulcanicity therein.

223. Without ocean or atmosphere, volcanic action such as we have upon our globe is impossible; it is possible, however, that the former vulcanicity of the moon was maintained until her whole ocean (which, in that case, must have been a very limited one) and her atmosphere had been wholly absorbed.

If that be so, as a precise balance is not probable, then it is likely there is still more or less unoxidized or otherwise chemically uncombined material in the moon.

But whether the vulcanicity of the moon be a completely spent energy or not, we find upon its surface elevations, and what appear to be craters, vastly exceeding in altitude and in width any thing upon our globe, and which seem perfectly abnormal as compared with the relative small size of the moon to our earth.

224. If vulcanicity be dependent merely upon communication, through the crust, with the liquid nucleus, partial or universal, and water-access to the same, there is no imaginable connexion between the *intensity* of volcanic action and the size of the planet on which it occurs; or if any, it is this, that the smaller planet cooling fastest may have a thicker crust, and so volcanic intensity should show itself less the smaller the planet is. But if it be, as here contended, a consequence of secular cooling, then the *intensity* will be greater as the progress of cooling is more rapid.

Now the cooling of any globe of like constitution and at the same distance from the sun must be directly as its surface and inversely as its mass—that is to say, as $\frac{D^2}{D^3}$, D being the mean diameter. Hence the rate of refrigeration of the moon from this cause alone must have been greatly more rapid than that of our globe, and hence the *intensity* of vul-

canicity in a given time far greater also, because contraction of the crust is proportionate to the rate of cooling, and the heat produced by crushing of the crust proportionate to that rate.

We therefore see here a sufficient cause for the greater height of the mountain-ranges, as well as for that of the volcanic craters, in the moon than on our earth.

This greater height was no doubt further exalted by the diminished action of gravity in the smaller globe opposing elevation, as well as by the want of density though probable *great hardness* of the material constituting the moon.

225. The density of the moon is stated by HERSCHEL ('Astronomy') to be 0·536, our earth's density being taken as unity; and the mean density of our earth being about 5·5, it follows that that of the moon is about 3·0, which is about that of corundum, sapphire, quartz, and hydrous metallic silicates; so that, with the exception probably of a small and possibly metallic nucleus of greater density, the whole may consist of aluminates and silicates of great rigidity, and therefore producing very great resistances to contractile crushing, and hence great elevation of temperature at local points during cooling.

And this appears supported by the *Rillen* of MÄDLER, which to the writer's eye, with Mr. NASMYTH's best telescope, and also to that gentleman, appear as *cracks* or deep sharp-edged fissures in a rigid surface. The prevailing direction of these *Rillen* is about at right angles to the lines of elevation, which is just what we should expect if the elevating force were such as here indicated, viz. a tangential compression, for in that the lines of tension must be orthogonal to the lines of elevating pressure, and therefore the resulting cracks orthogonal to the lines of elevation.

226. It would be foreign to our direct purpose, and lead too far, to go into the probable causes that have led to the great relative diameters of the lunar craters.

Thus far we discern that volcanic activity generally is dependent upon the solid and liquid materials (elements) constituting any planet, upon their conductivity (rate at which they can part with heat), upon the mass of the planet, upon which both its original temperature (when all in fusion) and its rate of cooling are dependent, and upon its distance from the sun; possibly also upon whether or not it traverses in space regions of variable temperature.

227. In the sun itself we but behold vulcanicity in its earliest and most potent stage, that of the condensation and chemical exhaustion of a primordial world of vast dimensions, with its vulcanicity exhibited in (to the imagination) terrific grandeur. In our own globe we see it developed by quite the like train of causation acting differently at successive epochs, extending over time that we cannot measure, and now dwindled down to its present point, when it is but part of the beneficent machinery of our earth, to make it the safe abode of plants, animals, and man himself. In the moon we see it, after having passed through all its stages, died out and gone.

228. If, then, in what has been here advanced, and, so far as the writer's knowledge extends, for the first time, the cause assigned for volcanic heat, viz. the crushing of the earth's solid crust, accounts for *all* the phenomena, leaves none unexplained or

inexplicable, and introduces no consequences in themselves either inexplicable or contrary to observed facts, but all parts of the theory fit together with the facts so far as we observe them in nature, like the parts of a "dissected map" from which no one piece can be left out nor to it any new one added, then the writer submits that, on true principles of philosophy, the theory may be regarded as a true interpretation of Nature.

[*Note.*—The views of BABBAGE and of Sir J. HERSCHEL as to the nature of volcanic heat have been alluded to at p. 158. It would have been difficult, however, before the development of the principles contained in the preceding paper to have shown the insufficiency of those views.

As they still linger in the minds of some geologists it seems desirable to advert to them here. HERSCHEL'S views are found, as stated by himself in letters to Sir CHARLES LYELL and Sir RODERICK MURCHISON, in vol. ii. of the Proceedings of the Geological Society of London.

Briefly they may be said to embrace two distinct heads: 1st, subversions of mechanical equilibrium by sedimentary deposits unequally laid upon an extremely thin solid crust floating on a liquid nucleus; 2ndly, the consequences of such sedimentary deposits in causing a local rise in the geothermal *couches.*

The former we may pass, the existence of any such thin crust being inadmissible. As regards the latter it is enough to say that it fails to afford any adequate supply of heat sufficient to account for terrestrial vulcanicity.

The gradual rise of an isothermal *couche* in the way suggested by HERSCHEL may possibly afford an adequate supply of heat to account for the phenomena of metamorphism, or more properly pyromorphism, but not for the play of any ejective volcano. Space forbids any proof of this here; but the nature of the objection may be illustrated by an example.

If we suppose, what certainly does not exist anywhere on our globe, that sedimentary deposits are going on continuously over some large area at the rate of 50 feet in depth per annum, it will require above 105 years to deposit a mile in depth; if we further suppose the conductivity of the superficial sediment the same as of that already deposited and deeply buried, the annual rise of any given geothermal *couche* beneath a given square mile of surface will be 50 feet, and the increase of temperature of the material just above the former level of the *couche* will be about 1 degree. The heat added, therefore, will be but one degree or thereabouts in a mass of sediment 50 feet in thickness and a square mile in area. This amount converted into units of heat is therefore the entire magazine of heat to supply the annual volcanic work per square mile, and, even on this extreme supposition as to rate of deposit, would prove wholly inadequate for the fusing and lifting work and waste required for the phenomena of active vulcanicity as now existing.

On principles derived from the above it may further be shown that making the very largest admissible estimate as to the total amount of sedimentary matter deposited annually over the entire globe, that amount would be altogether inadequate, upon HERSCHEL'S views, to supply the heat required for the annual vulcanicity of our globe as estimated in the preceding pages.

HERSCHEL'S views were apparently hastily struck off, and so far as they have been accepted by American and other geologists, have been so apparently on the credit due to the reputation of their author.

Had thermodynamics, however, been sufficiently advanced in HERSCHEL'S lifetime to have enabled him to test his views by their aid, there can be no doubt but that that illustrious man would have himself discerned the untenability of his theory.

In conclusion it may be added that the notion of some geologists that no supply of surface-water is necessary to existent volcanic action, but that the supply of water is derived from that liquid preexisting in vesicular cavities of the deep-seated rocky material, may be shown to present no adequate supply for the volume of steam in relation to the solid and liquid ejecta as seen at volcanic vents.—July 1873.]

Appendix.

A. *Thermal Springs.*

Authors on Vulcanology generally view thermal springs as one of the manifestations of volcanic action. That they are, in many instances, connected with volcanic vents, whose energies are nearly or for a time exhausted, is evident from such instances as the great hot springs of Auvergne or of Iceland; but that they are *not* related directly to volcanic agency in the vast majority of instances is equally obvious. In no case can we consider the surface-waters of supply descending many miles under ground; so that thermal waters, such as those of the British Islands and of a large portion of Europe, cannot be viewed as manifestations of volcanic activity, but as due to the surface-waters descending a certain moderate depth (which cannot be great, as no absolutely boiling spring is known except close to volcanoes), getting heated by the warmer beds below and reascending. We must therefore view the vast majority of the thermal springs of the globe in the light, not of volcanic phenomena at all, but as simply one of the means by which hypogeal heat is carried to the surface to be dissipated by radiation. It becomes an interesting inquiry in relation to our subject to estimate whether in this aspect the influence of thermal springs is great, or what proportion of the total heat lost annually by our globe is thus brought to the surface.

The writer has attempted this estimate in the following way:—

The thermal springs of Europe are those best known; next to these, perhaps, may be viewed those of India as catalogued by the Messrs. Schlagenthweit. Adopting Dr. Daubeny's catalogue ('Volcanoes,' &c.) for the former, there are in Europe 154 hot springs scattered over an area of $3\frac{1}{2}$ millions of square miles, evolving 6577353752 cubic feet of water per annum at a mean temperature of 57° Fahr. above the mean annual temperature of the localities. In India (as comprehended in the above catalogue) there are about 100 hot springs scattered over an area of $1\frac{1}{2}$ million of square miles, evolving 4345000000 cubic feet of water per annum at a mean temperature of 51° Fahr. above the mean annual temperature (taken for all India as 75° Fahr.).

These together, therefore, distributed over an area of 5 millions of square miles (of tropical and temperate latitudes) evolve about 10862353752 cubic feet of water per annum at a general mean (quantity and temperature both taken into account) of 54°·6 Fahr.

If, now, we assume that all the remainder of the dry land of our globe present like thermal springs in like abundance, then the total evolution annually from the 52 millions of square miles of dry land is 112968337520 cubic feet at 54°·6 Fahr. above the mean temperature of the localities. That is equivalent to 34336880705 cubic feet of water raised from 32° to 212° Fahr., or to 0·2332 cubic mile of water boiling under 1 atmosphere brought to the surface per annum.

If, now, from the insufficient character of our data, and especially as respects South America and Africa, we suppose that the above estimate does not comprise more than

one half the volume of hot water really brought annually to the surface, or that the mean temperature of the hot water is below the truth, and if we double the result so as to include these, we find that the totality of the thermal waters of our globe does not probably exceed half a cubic mile of boiling water per annum, the equivalent of which in melted ice at 32° shows that the entire amount of hypogeal heat thus carried off is not the one thousandth part of the total annual heat lost by our globe, as taken at 777 cubic miles of ice melted to water at 32°.

Whether, therefore, thermal springs be viewed as mere adjuvants to the dissipation at the surface of hypogeal heat, or as products of volcanic heat directly and in *all* instances, their influence is insignificant, and does not affect the views contained in the text.

B. *Heat and work from Quartz-crushing.*

Derived from the work done in crushing quartz in Victoria Colony Goldfields. Taken from R. B. SMITH'S 'Goldfields of Victoria,' royal 8vo, 1869, p. 543.

Weight of each stamp-head.

		lbs.		lbs.	
(1)	Ballarat district . .	336	to	896	General average = 642 lbs.
(2)	Beechworth	448	,,	868	
(3)	Sandhurst	560	,,	896	
(4)	Marybois	448	,,	840	
(5)	Castlemaine	280	,,	840	
(6)	Ararat	560	,,	784	
(7)	Gippsland	336	,,	896	
	Average . . .	424	to	860	

Each stamp-head falls.

	ft.		ft.					
(1)	0·583	to	0·833		50	to	80	strokes per minute.
(2)	0·417	,,	1·333		45	,,	80	,, ,,
(3)	0·708	,,	1·500		45	,,	72	,, ,,
(4)	0·666	,,	1·000		60	,,	80	,, ,,
(5)	0·500	,,	1·250		35	,,	80	,, ,,
(6)	0·500	,,	0·833		60	,,	80	,, ,,
(7)	0·583	,,	0·917		70	,,	80	,, ,,
Average	0·5653	to	1·0951	Average	52·143	to	78·857	,, ,,

General average 0·8302 *foot* and 65·5 *strokes* per minute.

Weight of Quartz crushed per 24 hours by one stamp-head.

	lbs.		lbs.					
(1)	2016	to	7840	Size of powder	40	to	224	per square inch.
(2)	2240	,,	8960		65	,,	144	,,
(3)	1680	,,	6720		64	,,	130	,,
(4)	3360	,,	6720		60	,,	140	,,
(5)	2240	,,	8960		64	,,	160	,,
(6)	2800	,,	3360		100	,,	121	,,
(7)	3360	,,	4592		70	,,	250	,,
Average	2528	to	6736	Average	66·143	to	167·000	,,

General average 4632 lbs. General average 116·571 per square inch.

Horse-power given to work each head.

	H.P.		H.P.	
(1)	1·00	to	1·50	
(2)	0·75	,,	2·00	
(3)	0·75	,,	1·00	
(4)	0·83	,,	1·75	General average=1·2057 H.P.
(5)	0·50	,,	2·00	
(6)	0·60	,,	1·50	
(7)	0·83	,,	1·87	
Average	0·7514	to	1·66	

Actual specific gravity of Victorian Gold Quartz from Royal School of Mines:—

By their determination . . . . =2·6307
By MALLET's determination . . =2·6224

Actual of Wicklow Quartz (BRETT):
MALLET's=2·6214.

This is but a *mere contrast* as regards the conclusions here sought for.

Specific gravity of quartz on various authorities:—

SENFT	(specific gravity)	=2·5 to 2·8	
ZIRKEL	,,	2·6	
KIRWAN	,,	2·64 to 2·67	Average of all 2·6469
BEUDANT	,,	2·641 to 2·654	
HUAY	,,	2·670	

1 cubic foot of water at 60° weighing 62·5 lbs.:

$$1\cdot0000 : 62\cdot5 :: 2\cdot6469 : x=\frac{62\cdot5\times2\cdot6469}{1\cdot0000}=165{,}431 \text{ lbs. av.}$$

=weight of cubic foot of quartz.

The *specific heat* of silicic anhydride, SiO_2, is given by WATTS (Chem. Dict. "Heat," vol. iii. p. 32) as =0·19132, water being unity; 0·179 by my determination of quartz from Wicklow, Ireland.

Specific heat of quartz 0·1719 (HERMANN) } GMELIN'S Handbook,
0·1913 (REGNAULT) } vol. i. p. 245.

Now the average of the whole data results in these:—

642 lbs. falling 0·8302 foot 65·5 times per minute for 24 hours,

Produces 4632 lbs. of quartz in powder,

116·571 meshes per square inch;

or 642 lbs. falling $0·8302 \times 65·5 = 54·3781$ feet per minute for 24 hours,

or $24 \times 60 = 1440$ minutes $= 78304·464$ feet.

That is, 642 lbs. ×78304·464 feet =50,271,465,888 foot-pounds, being the *work done* to crush to powder 4632 lbs. of quartz,

or $\frac{50,271,465,888}{4632} = 10853·08$ foot-pounds per 1 lb. of quartz,

or 10853·08 foot-pounds ×165·431 lbs.=1,795,435,877 foot-pounds per cubic foot of quartz of specific gravity 2·6469.

Then, dividing by JOULE'S equivalent, we have

$\frac{1795435·877}{772} = 2325·69$ degrees Fahrenheit in 1 lb. of *water* as the equivalent of the *crushing work* per cubic foot of quartz; or, as steam from water at 32° of 1 atmosphere and therefore at 212° contains 1146·7 units of heat, the work of crushing 1 cubic foot of quartz would vaporize at 212 degrees 2·028 lbs. of water, for

$$2325·69 : 1146·7 :: x : 1 \text{ gives } x = \frac{2325·69}{1146·7} = 2·028.$$

Now let us take the results, not from general averages, but from the *highest* and *lowest* data given, having regard to the fineness of the pulverized quartz produced, *i. e.* assuming that the average finest powder is due to the largest expenditure of power.

Then, first, we have from what precedes, that (*highest*) 860 lbs. falling 1·0951 foot 78·857 times per minute produces in 24 hours 2528 lbs. of quartz-powder of fineness to pass through 167·0 parts of a square inch,

or 860 lbs. falling $1·0951 \times 78·857 = 86·356$ feet per minute,

or $86·356 \times 1440 = 124352·64$ feet per day of 24 hours,

which is 860 lbs. ×124352·64 feet =106943270·4 foot-pounds, being the work done to 2528 lbs. of quartz,

or $\frac{106943270·4}{2528} = 42303·5$ foot-pounds expended per 1 lb. of quartz,

or $42303\cdot5 \times 165\cdot431 = 6998310\cdot31$ foot-pounds per cubic foot of quartz; and dividing by $J=772$, $\frac{6998310\cdot31}{772}=9065\cdot2$ degrees Fahrenheit in 1 lb. water as the equivalent of the crushing *work* of 1 cubic foot of quartz-powder of $\frac{1}{167}$ of a square inch of fineness.

Secondly, we have the *lowest* data, viz. that 424 lbs. falling 0·5653 foot 52·143 times per minute produces 6736 lbs. of quartz-powder of a fineness $=\frac{1}{66\cdot143}$ of a square inch in 24 hours.

Assuming in both calculations that the greatest power is consumed in producing the smallest quantity in total of the finest powder, then

$$424 \text{ lbs. falling } 0\cdot5653 \text{ feet } \times 52\cdot143 = 29\cdot479 \text{ feet per minute,}$$

$$\text{or } 29\cdot479 \times 1440 = 42449\cdot76 \text{ feet per 24 hours,}$$

which is 424 lbs. $\times 42449\cdot76 = 17998698\cdot24$ foot-pounds, being the work to crush 6736 lbs. of quartz,

$$\text{or } \frac{17998698\cdot24}{6736} = 2672\cdot01 \text{ foot-pounds expended per pound of quartz,}$$

$$\text{or } 2672 \times 165\cdot431 = 442031\cdot632 \text{ foot-pounds per cubic foot of quartz;}$$

and dividing by $J=772$, $\frac{442031\cdot632}{772}=572^\circ\cdot58$ Fahr. in 1 lb. of water as the equivalent of the crushing work of one cubic foot of quartz-powder of $\frac{1}{66\cdot143}$ of a square inch fineness.

We thus come to this: $\frac{1}{167}$ of a square inch, allowing say $\frac{1}{50}$ inch for the diameter of the wires of the wire gauze or screen, was about $\frac{1}{18}$ inch average diameter of the largest fragments; $\frac{1}{116\cdot571}$ of a square inch, same allowance, was about $\frac{1}{14}$ inch average diameter of fragments; $\frac{1}{66\cdot143}$ of a square inch, same allowance, was about $\frac{1}{10}$ inch average diameter of fragments.

Then the proportionate foot-pounds to sizes of fragments and equivalent of heat are (omitting decimals):—

Size	$\frac{1}{18}$	$\frac{1}{14}$	$\frac{1}{10}$
Foot-pounds	6998310	1795435	442031.
Heat, degrees Fahr.	9065°	2325°	572°.

These results are valuable, from the general corroboration they afford of the correctness of the results as to crushing experimentally given in the body of this paper. The friction of the stamping-machinery is, as may be seen, excluded from our calculation; and except what small amount of power due to the fall of the "stamp-head" may be consumed in splashing about the water constantly flowing over the crushing quartz, there is almost no power consumed except directly in the pulverization of the quartz.

C. *Work of crushing, derived from experiments* (*Phil. Trans.* 1862) *on Holyhead Rocks.*

The *hardest* quartz rock (sp. gr. 2·656).

Across lamination crushed with 37,000 lbs. on the square inch.

Parallel with lamination . . 20,000

2)57,000

Mean of quartz both ways . 28,500 lbs.

Then, on a cubic foot,

$$144 \times 28{,}500 = 4104000 \text{ lbs. to crush};$$

and supposing the cube all crushed to powder and dissipated laterally, then this pressure would have descended 1 foot; we therefore have the work of crushing the 1 foot cube

$$= 4104000 \times 1 = 4104000 \text{ lbs.},$$

and

$$\frac{4104000}{772} = 5316^\circ{\cdot}06 \text{ Fahr. in 1 lb. of water.}$$

This result, however, is below the truth, as the quartz rock was not crushed absolutely to powder.

Mallet

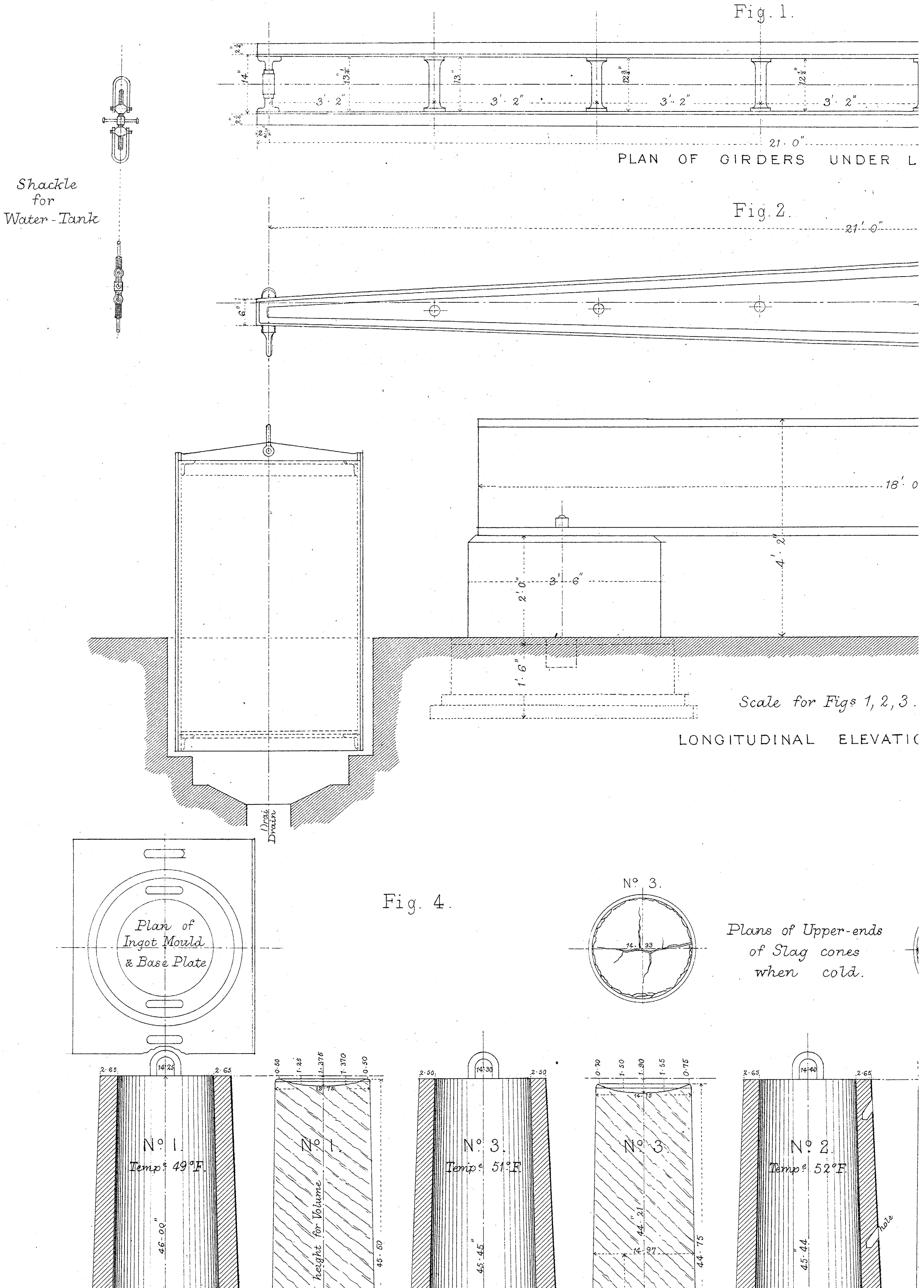
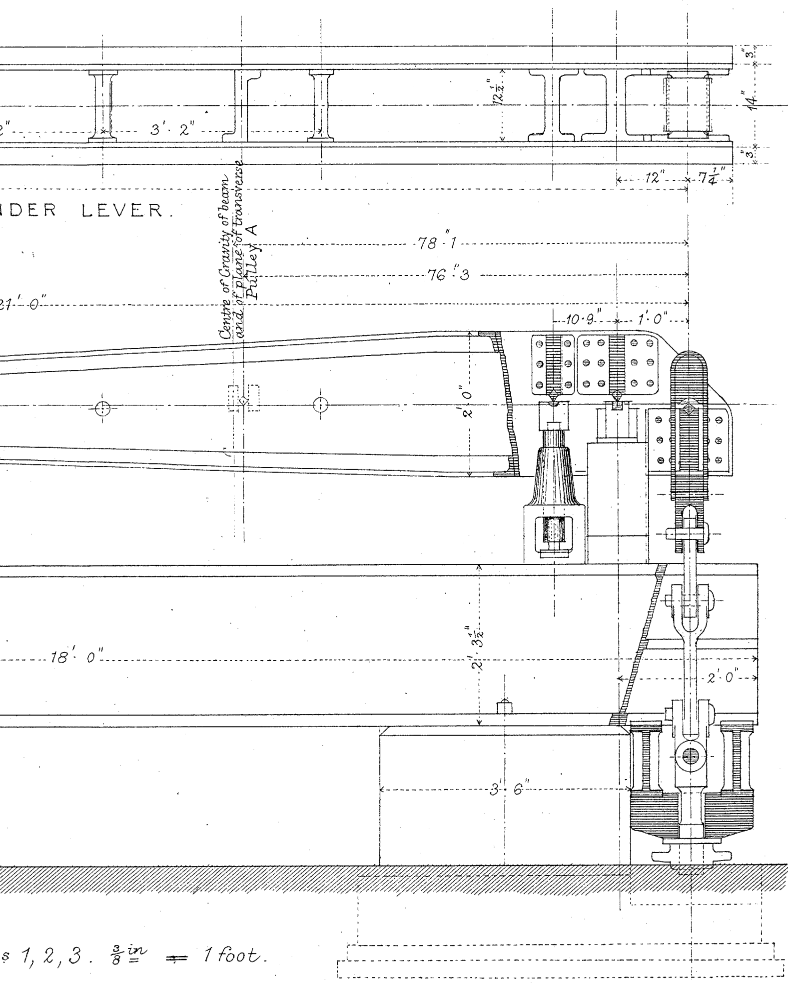
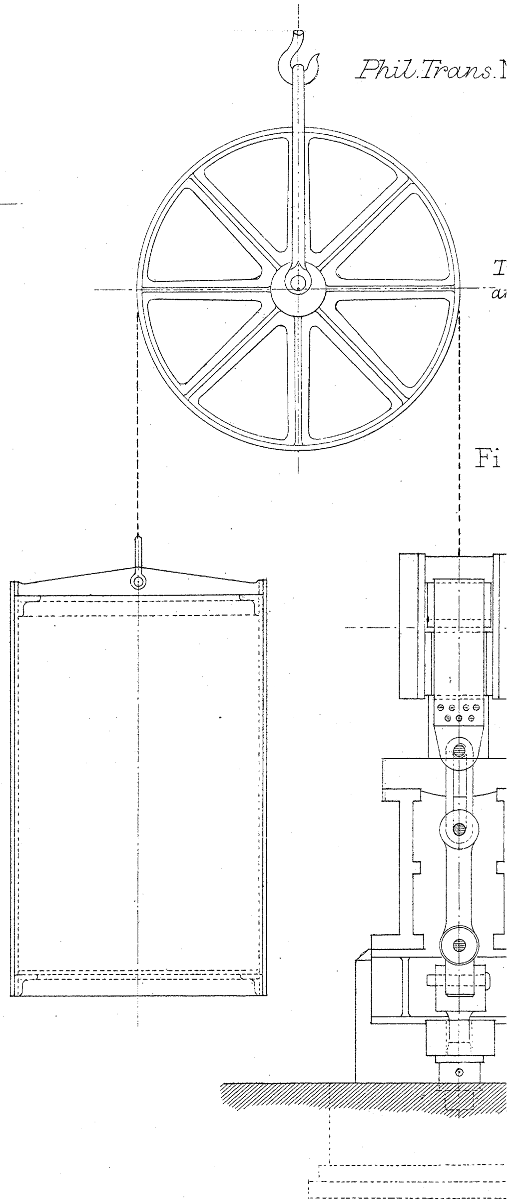


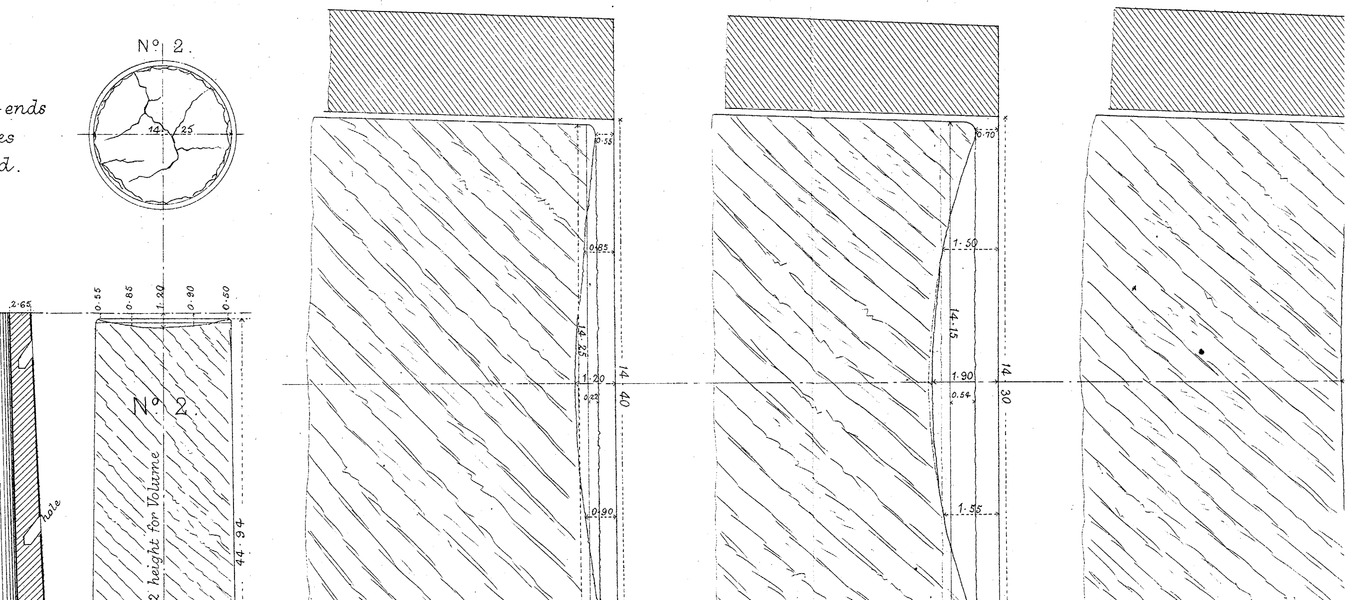
Fig. 1.
PLAN OF GIRDERS UNDER L
21. 0"
3'. 2"
Shackle for Water-Tank
Fig. 2.
21'. 0"
18'. 0
4'. 2"
3'. 6"
2'. 0"
1'. 6"
Drain
Scale for Figs 1, 2, 3
LONGITUDINAL ELEVATIO
Fig. 4.
Plan of Ingot Mould & Base Plate
No. 3.
Plans of Upper-ends of Slag cones when cold.
No. 1.
Tempe 49°F.
46.00"
No. 1.
height for Volume
45.50
No. 3.
Tempe 51°F.
45.45"
No. 3.
44.21"
14.97
44.75
No. 2.
Tempe 52°F.
45.44"
hole





s 1, 2, 3. ⅜ in = 1 foot.

LEVATION OF LEVER. &c



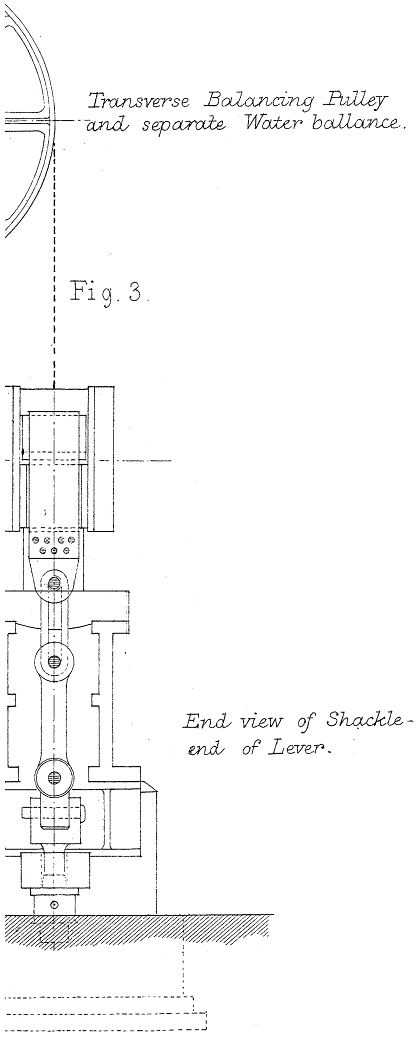

Transverse Balancing Pulley and separate Water ballance.
End view of Shackle-end of Lever.

Fig. 3.

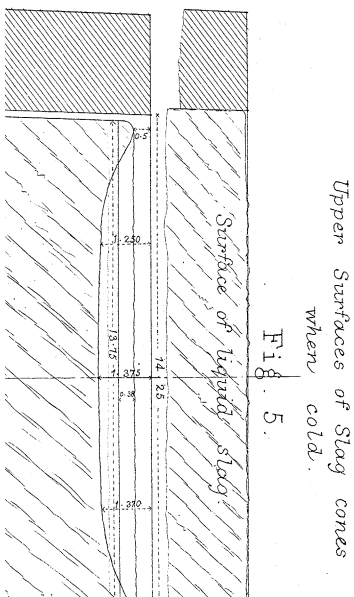
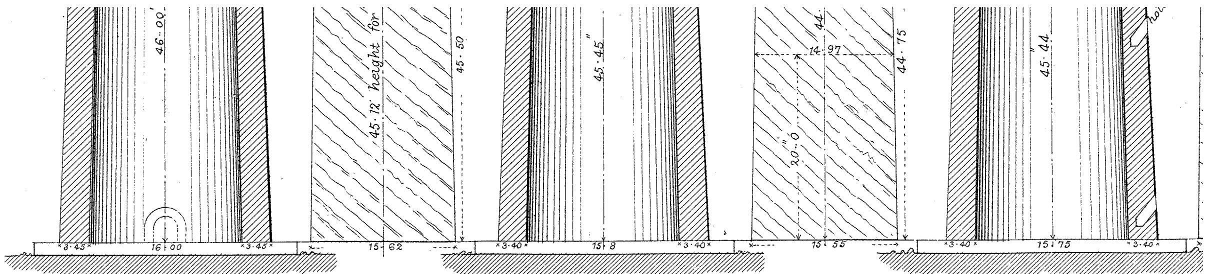

Upper Surfaces of Slag cones when cold.
Surface of liquid Slag.
0.5
1.250
13.15
14
25
1.375
0.38
1.370

Fig. 5.

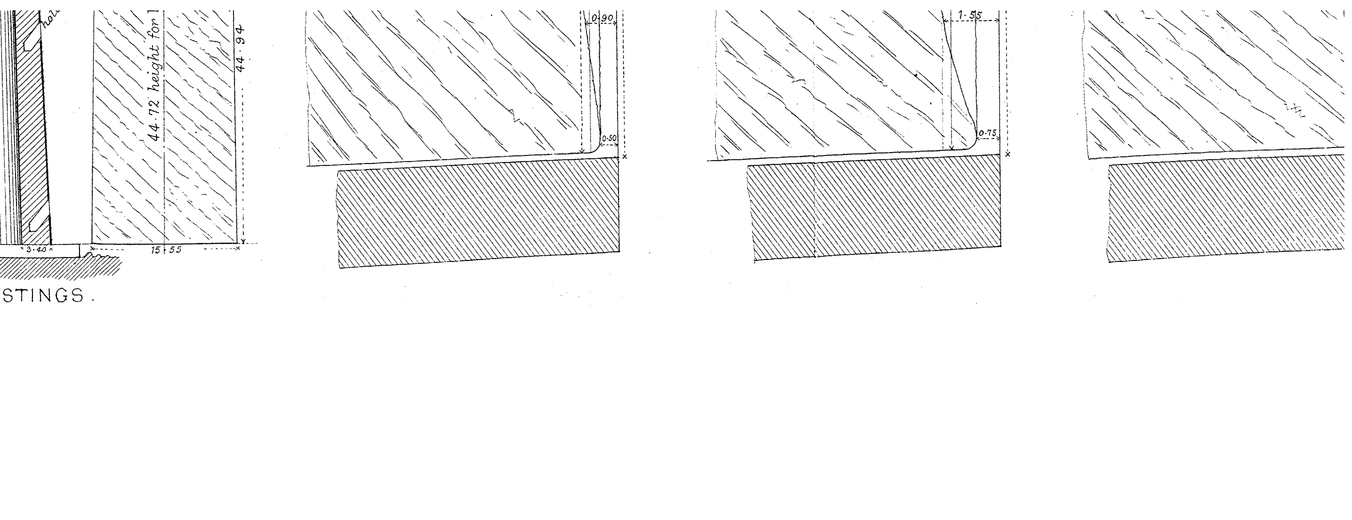


VERTICAL SECTIONS OF CONES, BASEPLATES & RESPECTIVE SLAG CASTINGS.

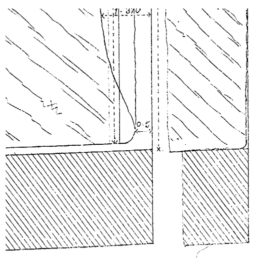
W. H. Wesley sc.

ins: 12 9 6 3 0 1 2 3 feet.

SCALE.

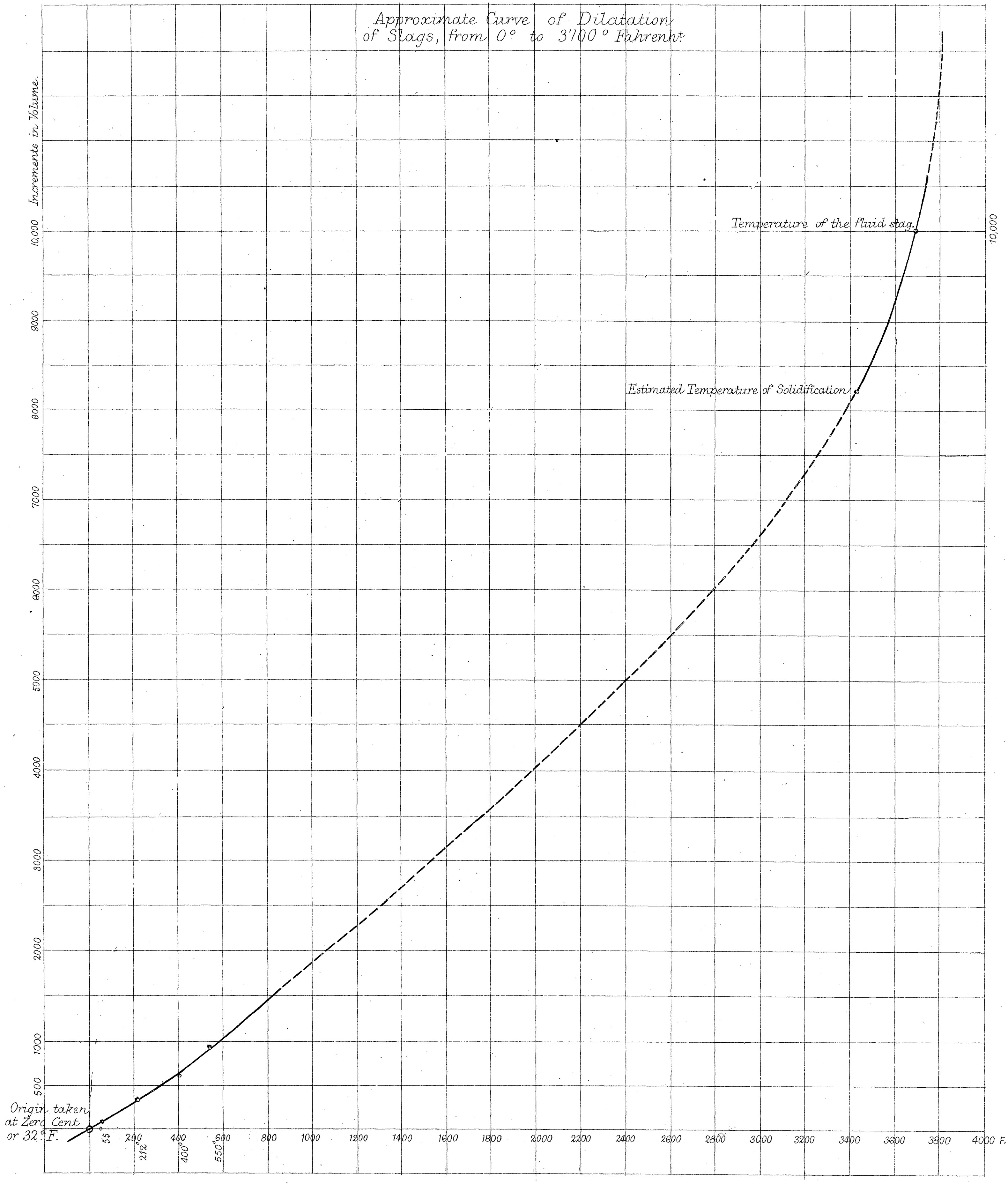


STINGS.



cones

W. West & Cº imp.



W.H.Wesley lith. W.West & Co. imp.

Errata in Mr. Mallet's *Paper on Volcanic Energy.*

Paragraph.	Line.		
186	7	*for*	63,536,524,001 *read* 63,536,524,001,280.
,,	8	,,	dividing *read* multiplying.
,,	,,	,,	21,178,841,333,760 *read* 190,609,572,003,840.
,,	10	,,	3,272,370,686 *read* 29,358,710,136.
,,	11	,,	$\frac{100}{4498}$, or less than $\frac{1}{45}$ *read* $\frac{197}{1000}$ or less than $\frac{1}{5}$ of &c.
,,	13	,,	$\frac{40}{45}$, or less than 1 cubic mile *read* $\frac{40}{5}$, or 8 cubic miles.
188	11	,,	0·888 cubic mile *read* 8 cubic miles.
,,	12	,,	16·524 *read* 23,636.
190	9	,,	$3\times0{\cdot}888=2{\cdot}664$ *read* $3\times8=24$.
191	2	,,	18·3 *read* 39·636.
,,	3	,,	18 *read* 40.
192	3	,,	$400\times18=7200$ *read* $400\times40=16{,}000$.
193	3	,,	*eight years* read *sixteen and a half years.*
194	5	,,	255 *read* 535.
,,	7	,,	7200 *read* 16,000.
,,	9	,,	255 *read* 535.
,,	33	,,	7200 *read* 16,000.
,,	34	,,	10,800 *read* 24,000.
196	6	,,	4,831,052,974 *read* 42,030,163,804.
,,	8	,,	746,481 *read* 6,234,079.
,,	,,	,,	$\frac{1}{200{,}000}$ *read* $\frac{1}{23{,}612}$.
,,	11	,,	·000015 *read* ·000127.
,,	17	,,	·000315 *read* ·002667.
,,	19	,,	·000606 *read* 005577.
197	13	,,	$\frac{300\times\cdot000606}{3}$, $=\cdot0606$ *read* $\frac{300\times\cdot00577}{3}=\cdot577$.
198	42	,,	$\frac{1006390}{3^\circ}=335463$ *read* $1006390\times3^\circ=3019170$.
,,	43	,,	50·3 cubic yards *read* 466·5 cubic feet.
,,	44	,,	$50{\cdot}3\times5280=265584$ million cubic yards *read* $466{\cdot}5\times5280=2463120$ million cubic feet.
,,	45	,,	2921424 million cubic yards *read* 27,093,226 cubic feet.
,,	46	,,	5842848 millions of cubic yards *read* 54,186,440 cubic feet.
,,	51	,,	2921424 *read* 1003452.
,,	52	,,	5842848 *read* 2006905.
,,	53	,,	9770662 *read* 4016747.
,,	55	,,	$\frac{9770662000000}{5451776000}$, $=1792$ *read* $\frac{4016747000000}{545177600}=737$.

Table I.—General Results of Experiments on the Work and Heat of Rocks crushed at Crewe Works, 1870.

1. No. of Experiment.	2. Character or Class of Rock.	3. Specific Gravity. Water=1000.	4. Weight per cubic inch and cubic foot. (Col. 3×62·4)/1000, for weight per cub. foot. lbs.	5. Dimensions of Cubes crushed. Height. in.	5. Surface. in.	6. Area of surface of cubes. sq. in.	7. Weight per square inch at which first signs of yielding appeared. Col. 15/Col. 6. lbs.	8. Mean weight per square inch at which first signs of yielding appeared. Sum of col. 7/3. lbs.	9. Crushing-weight per square inch. Col. 17/Col. 6. lbs.	10. Mean Crushing-weight per square inch. Sum of col. 9/3. lbs.
1.	Caen Stone, Oolite ...... Cube A .... „ B .... „ C	2337	0·0867 145·8288	1·45 1·43 1·44	1·45×1·40 1·43×1·47 1·44×1·44	2·030 2·102 2·073	2463·0 952·3 1445·5	1620·26	2955·6 1766·0 2510·0	2410·2
2.	Portland Stone .... Cube A .... „ B .... „ C	2462	0·0888 153·6288	1·47 1·45 1·46	1·50×1·50 1·50×1·50 1·50×1·52	2·250 2·250 2·280	2400·0 3555·0 3459·0	3138·00	5711·1 5711·1 6123·1	5848·4
3.	Magnesian Limestone .. Cube A .... „ B .... „ C	2571	0·0928 160·4304	1·45 1·44 1·46	1·52×1·47 1·51×1·48 1·48×1·47	2·234 2·234 2·175	3139·0 3811·0 4147·0	3699·00	6502·0 6255·0 9470·0	7409·0
4.	Sandstone from the neighbourhood of Bradford, Yorkshire .... Cube A .... „ B .... „ C	2478	0·0894 154·6272	1·46 1·46 1·45	1·45×1·48 1·48×1·45 1·47×1·48	2·146 2·146 2·102	9602·0 10420·0 12890·0	10970·60	14486·0 12476·0 15071·0	14011·0
5.	Ayre Hill Sandstone, Yorkshire .... Cube A .... „ B .... „ C	2408	0·0869 150·2592	1·47 1·45 1·45	1·47×1·49 1·50×1·47 1·48×1·47	2·190 2·205 2·175	 6192	......	6872·0 7590·0 7442·0	7301·0
6.	Bramley Fall Sandstone.. Cube A .... „ B .... „ C	2506	0·0904 156·3744	1·45 1·43 1·45	1·50×1·53 1·48×1·54 1·49×1·53	2·295 2·279 2·279	crushed suddenly „ „	......	5611·0 3634·0 5903·0	5049·0
7.	Devonshire Marble...... Cube A .... „ B .... „ C	2717	0·0981 169·5408	1·47 1·47 1·47	1·52×1·49 1·48×1·51 1·49×1·47	2·264 2·234 2·190	11505·0 11748·0 11872·0	11708·30	17210·0 13791·0 16052·0	15684·0
8.	Conway Slate .... Cube A .... „ B .... „ C	2733	0·0986 170·5392	1·43 1·45 1·43	1·50×1·52 1·50×1·51 1·42×1·49	2·280 2·265 2·116	...... 8407·0 11129·0	9768·30	12234·0 10877·0 14519·0	12543·0
9.	Bangor Slate .... Cube A .... „ B .... „ C	2859	0·1032 178·4016	1·44 1·45 1·47	1·49×1·53 1·48×1·52 1·51×1·48	2·279 2·249 2·230	18512·0 ...... 12509·0	15510·00	22944·0 ...... 13934·0	18439·0
10.	Rowley Rag Stone ...... (preliminary only) Cube A* Cube A .... „ B .... „ C	2827	...... 0·1020 176·4048	1·41 1·47 1·47 1·47	1·30×1·50 1·43×1·51 1·50×1·50 1·49×1·46	1·950 2·150 2·250 2·170	14358·0 20930·0 24000·0 27188·0	...... 24039·30	29366·0 30830·0 27761·0 28784·0	...... 29125·0
11.	Dartmoor Red Granite .. Cube A .... „ B .... „ C	2652	0·0957 165·4848	1·48 1·47 1·47	1·49×1·45 1·49×1·50 1·50×1·50	2·160 2·230 2·250	13888·0 14798·0 16888·0	15191·33	18057·0 17984·0 22126·0	19389·0
12.	Guernsey Granite (grey).. Cube A .... „ B .... „ C	2858	0·1032 178·3392	1·475 1·475 1·475	1·47×1·50 1·48×1·51 1·50×1·54	2·200 2·230 2·310	18181·0 23317·0 21212·0	20903·33	27504·0 32338·0 30528·0	30123·0
13.	Mount Sorrel (red granite) Cube A .... „ B .... „ C	2653	0·0958 165·5472	1·48 1·47 1·48	1·48×1·53 1·47×1·55 1·47×1·52	2·260 2·270 2·230	18181·0 22467·0 192[illegible]2·0	19976·33	24535·0 23458·0 30612·0	26201·0
14.	Aberdeen Granite (blue) Cube A .... „ B .... „ C	2707	0·0977 168·9168	1·48 1·48 1·48	1·53×1·50 1·54×1·52 1·51×1·49	2·290 2·340 2·240	19150·0 23299·0 17928·0	20125·66	25078·0 22780·0 20187·0	22681·0
15.	Aberdeen Granite (grey).. Cube A .... „ B .... „ C	2678	0·0965 166·9072	1·48 1·48 1·48	1·49×1·52 1·50×1·55 1·53×1·50	2·265 2·325 2·295	17256·0 16000·0 17348·0	16868·00	20373·0 22122·0 24454·0	22316·0
16.	Furnace Scotch Clay Porphyry .... Cube A .... „ B .... „ C	2594	0·0936 161·8656	1·48 1·48 1·48	1·51×1·49 1·50×1·50 1·50×1·51	2·250 2·250 2·260	27111·0 27555·0 23781·0	26149·00	32784·0 32540·0 27601·0	30975·0

1. No. of Experiment.	11. Volume of Cubes. Height × area of surface. cub. in.	12. Mean Volume of each sort of Rock. Sum of col. 11/3. cub. in.	13. Weights of Cubes crushed. Col. 11 × col. 4 (weight per cubic inch). lb.	14. Mean Weight of Cubes of each sort. Sum of col. 13/3. lb.	15. Actual Pressure at which Disintegration commenced. lbs.	16. Mean actual Pressure at which Disintegration commenced. Sum of col. 15/3. lbs.	17. Actual Pressure at which the Cubes were completely crushed. lbs.	18. Mean actual Pressure at which Cubes of each sort were completely crushed. Sum of col. 17/3. lbs.	19. Vertical Range through which the Crushing Pressure acted. ft.	20. Work expended in crushing the Cubes. Col. 17 × col. 19. foot-pounds.
1.	2·9435 3·0060 2·9859	2·9784	0·2552 0·2606 0·2588	0·2582	5000 2000 3000	3333	6000 3700 5200	4966·0	0·0883 0·0858 0·0908	529·80 317·57 472·38
2.	3·3075 3·2625 3·3288	3·2996	0·2920 0·2880 0·2939	0·2913	5500 8000 7783	7094	12850 12850 13950	13216·6	0·0950 0·0958 0·0950	1220·75 1231·41 1325·25
3.	3·2393 3·2292 3·1763	3·2149	0·3006 0·2996 0·2923	0·2975	7000 8000 9000	8000	14500 13950 20550	16333·3	0·0941 0·0850 0·0925	1365·32 1185·75 1900·87
4.	3·1331 3·1331 3·0479	3·1047	0·2800 0·2800 0·2724	0·2775	20000 22000 27050	23017	31000 26700 31650	29783·3	0·0916 0·0975 0·0941	2839·60 2603·25 2980·16
5.	3·2197 3·1972 3·1546	3·1905	0·2997 0·2778 0·2741	0·2772	 15000	....	15050 16700 16150	15966·6	0·0975 0·0908 0·0916	1467·37 1516·36 1479·34
6.	3·3277 3·2592 3·3055	(3·2975) 3·3166	0·2998 0·2956 0·2978	0·2977	....	....	12850 8500 13400	11500·0	0·0991 ...... 0·0983	1273·43 ...... 1317·22
7.	3·3292 3·2851 3·2197	3·2780	0·3265 0·3222 0·3158	0·3215	26000 26000 26000	26000	38895 30755 35155	34938·0	0·0991 0·0975 0·0966	3854·49 2998·61 3395·97
8.	3·2604 3·2842 3·0361	(3·1935) 3·1601	0·3214 0·3238 0·2993	0·3148	.... 19000 23484	21242	27895 24584 30634	27704·3	0·0633 0·0800 0·0791	1765·75 1966·72 2423·14
9.	3·2827 3·2610 3·2851	3·2726	0·3387 0·3365 0·3390	0·3380	42000 27000	34500	52095 31085	41590·0	0·0783 ...... 0·0900	4079·03 ...... 2797·65
10.	2·7495 3·1741 3·3075 3·1978	...... 3·2264	0·2804 0·3237 0·3373 0·3261	...... 0·3290	28000 45000 54000 59000	 52666	57265 66285 62463 62463	...... 63737·0	0·0791 0·1050 0·0941 0·1050	4529·66 6959·92 5877·76 6558·61
11.	3·1975 3·2854 3·3075	3·2634	0·3059 0·3144 0·3165	0·3123	30000 33000 38000	33666	39005 40105 49785	42965·0	0·1008 0·0975 0·1058	3931·70 3910·23 5267·25
12.	3·2523 3·2963 3·4072	3·3186	0·3356 0·3401 0·3516	0·3424	40000 52000 49000	47000	60510 72115 70520	67715·0	0·0983 0·0858 0·1016	5948·13 6187·46 7164·83
13.	3·3522 3·3486 3·3063	3·3357	0·3211 0·3207 0·3167	0·3195	40000 51000 44000	45000	55450 53250 68265	58988·0	0·0966 0·1025 0·1041	5356·47 5458·12 7106·38
14.	3·3966 3·4643 3·3298	3·3969	0·3318 0·3384 0·3253	0·3318	43800 51000 40000	44600	57430 53305 45220	51985·0	0·0875 0·0908 0·0891	5025·12 4840·09 4029·10
15.	3·3522 3·4410 3·3966	3·3966	0·3233 0·3320 0·3277	0·3276	39000 37000 40000	38666	46045 51325 56000	51123·0	0·1025 0·1025 0·0991	4719·61 5260·81 5549·60
16.	3·3285 3·3300 3·3522	(3·3369) 3·3411	0·3115 0·3116 0·3137	0·3123	61000 62000 53500	58833	73765 73215 62380	69786·0	not given. 0·1083 0·1008	...... 7929·18 6287·90

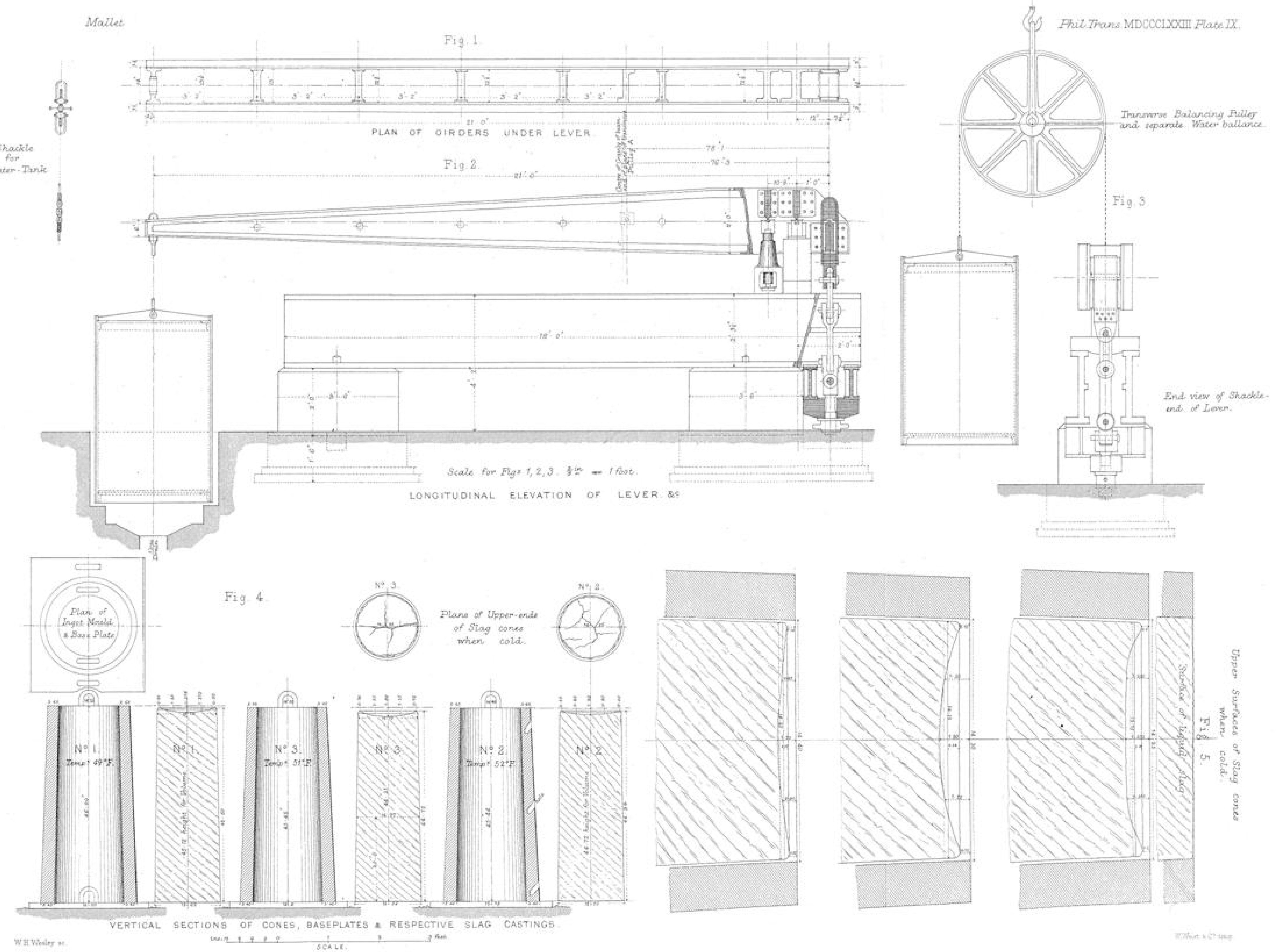
1. No. of Experiment.	21. Mean Work expended in crushing Cubes of each sort. Sum of col. 20/3. foot-pounds.	22. Weight of large fragments thrown off on crushing. lb.	23. Total Work expended in crushing the entire Cube. Col. 20 × col. (13+22)/Col. 13. foot-pounds.	24. Mean Total Work expended in crushing the entire Cubes of each sort. Sum of col. 23/3. foot-pounds.	25. Mean Total Work reduced to crushing 1 cubic inch and 1 cubic foot. Col. 24/Col. 12. foot-pounds.	26. British Units of Heat corresponding to mean total work of crushing 1 cub. ft. and 1 lb. avoird. of each Rock. Col. 25/J. ° Fahr.	27. Specific Heat of each class of Rock. Water=1·00.	28. Temperature in 1 cubic foot of Rock due to work of crushing. H/(W S) = Col. 26/(Col. 4 × col. 27). ° Fahr.	29. Number of cubic feet and pounds of water at 32° evaporated into steam at 212°. H/(62·45×1146).	30. Volume of ice at 32° melted to water at 32° by one volume of Rock. H/(57·8×143). cub. ft.	31. No. of Experiment.
1.	439·92	none. „ „	529·80 317·57 472·38	439·92	148·139 255,951·392	2·27 331·5	0·284	8·004	0·0046 cub. ft. 0·288 lb.	0·04008	1.
2.	1259·13	„ „ „	1220·75 1231·41 1325·25	1259·13	381·603 659,409·984	5·5 854·1	0·265	20·98	0·0119 cub. ft. 0·774 lb.	0·1033	2.
3.	1483·98	„ „ „	1365·32 1185·75 1900·87	1483·98	461·590 797,634·430	6·4 1033·2	0·245	26·28	0·015 cub. ft. 0·9 lb.	0·125	3.
4.	2807·67	0·0868 0·0762 0·0829	4089·02 3566·45 4261·63	3972·37	1279·469 2,210,922·432	19·3 2863·4	0·215	86·13	0·04 cub. ft. 2·5 lbs.	0·346	4.
5.	1487·69	0·1003 0·1107 0·1036	2274·43 2517·15 2366·94	2386·17	748·214 1,292,913·792	11·1 1674·7	0·233	47·79	0·0234 cub. ft. 1·46 lb.	0·2026	5.
6.	1295·32	0·1003 none. 0·0680	1913·36 ...... 1706·99	1810·18	545·794 943,132·204	7·8 1221·6	0·238	32·84	0·017 cub. ft. 1·066 lb.	0·147	6.
7.	3416·36	0·1135 0·1588 0·1223	5897·37 5905·66 5535·43	5779·49	1763·1157 3,046,663·9296	23·2 3946·4	0·203	114·679	0·055 cub. ft. 3·44 lbs.	0·477	7.
8.	2051·87	none. 0·2179 0·2083	...... 5998·49 7947·93	6973·21	2206·643 3,813,079·104	28·9 4939·2	0·218	132·85	0·07 cub. ft. 4·3 lbs.	0·596	8.
9.	3438·34	0·1571 .... none.	7586·99	......	2311·205 3,993,762·240	28·9 5173·2	0·201	144·29	0·071 cub. ft. 4·51 lbs.	0·613	9.
10.	...... 6465·43	none. 0·1536 0·1310 0·1347	13244·70 9610·13 11168·32	11341·05	3515·08 6,074,058·24	44·6 7867·9	0·204	213·23	0·109 cub. ft. 6·86 lbs.	0·925	10.
11.	4369·73	0·0623 0·0329 0·0326	4934·28 4363·82 5867·71	5055·27	1549·083 2,676,810·240	20·9 3467·3	0·180	116·39	0·049 cub. ft. 3·02 lbs.	0·419	11.
12.	6433·47	0·1641 0·1189 0·1319	11640·61 9466·82 11466·33	10857·92	3271·838 5,653,722·24	41·0 7323·4	0·189	217·24	0·1023 cub. ft. 6·39 lbs.	0·885	12.
13.	5973·66	0·1123 0·1002 0·0468	8195·39 7914·28 8314·47	8141·38	2440·68 4,217,495·04	33·0 5463·0	0·181	182·27	0·076 cub. ft. 4·76 lbs.	0·6609	13.
14.	4631·44	0·0920 0·0972 0·1095	6934·67 6776·13 6043·65	6584·819	1938·48 3,349,692·168	25·7 4330·0	0·215	119·2	0·06 cub. ft. 3·78 lbs.	0·524	14.
15.	5176·67	0·1003 0·1136 0·1111	6843·43 7996·43 8379·89	7739·92	2278·726 3,937,638·528	30·5 5100·5	0·196	155·94	0·072 cub. ft. 4·44 lbs.	0·617	15.
16.	7108·54	0·5520 0·0690 0·0585	...... 10149·35 7734·12	8941·738	2676·280 4,624,611·840	37·0 5990·4	0·186	198·97	0·083 cub. ft. 5·22 lbs.	0·724	16.

Col. 26, Experiments 1–9: Mean Heat=2448°·50 Fahr. per cubic foot. 14°·93 „ lb. avoir.

Col. 26, Experiments 10–16: Mean Heat=5650°·20 Fahr. per cubic foot. 33°·24 „ lb. avoir.

Col. 29, Experiments 1–9: Mean quantity of water evaporated by 1 cubic foot of crushed rock=0·03753.

Col. 29, Experiments 10–16: Mean quantity of water evaporated by 1 cubic foot of crushed rock=0·078757.



W. H. Wesley sc.
W. West & Co. imp.