XLIV. An abstract of a memoir on physical geology; with a further exposition of certain points connected with the subject

W. Hopkins Esq. M.A. F.G.S.

To cite this article: W. Hopkins Esq. M.A. F.G.S. (1836) XLIV. An abstract of a memoir on physical geology; with a further exposition of certain points connected with the subject , Philosophical Magazine Series 3, 8:46, 227-236, DOI: [10.1080/14786443608648853](https://doi.org/10.1080/14786443608648853)

To link to this article: <http://dx.doi.org/10.1080/14786443608648853>

Published online: 01 Jun 2009.

Submit your article to this journal [↗](#)

Article views: 2

View related articles [↗](#)

Citing articles: 1 View citing articles [↗](#)

XLIV. *An Abstract of a Memoir on Physical Geology ; with a further Exposition of certain Points connected with the Subject.* By W. HOPKINS, Esq., M.A., F.G.S., of St. Peter's College, Cambridge.*

IN a memoir entitled "Researches in Physical Geology," lately printed for the Transactions of the Cambridge Philosophical Society, I have endeavoured to develop, by reasoning founded on mechanical principles, and by mathematical methods, the effects of an elevatory force acting simultaneously at every point beneath extensive portions of the crust of the earth, in producing in it dislocations and elevations such as we now recognise. I have not there, however, attempted to give any exposition of the mechanical principles on which the investigations are founded, beyond what was necessary to make the subject intelligible to persons familiar with investigations of a similar character ; but, with the hope that the interest which the subject of elevations must always possess in the estimation of the speculative geologist may appertain in some measure to any new theoretical views respecting it, I have now been induced to attempt a somewhat more detailed and popular exposition of the mechanical considerations which have entered into my own investigations, and which must in some measure, I conceive, enter into all others on similar points possessing any claim to a demonstrative character. I cannot expect to remove difficulties inherent in such investigations, and which must be felt to be considerable even by those best prepared to enter upon them ; but if I should succeed in so far diminishing them as to render the subject more accessible by the only way in which, in my opinion, it can be successfully approached, my object will be accomplished. What I have now written may be considered as an abstract of a considerable portion of my memoir, with a somewhat more detailed exposition of several points connected with the subject of it.

When natural phænomena, characterized by general laws, have suggested to us a general cause to which they may be referred, our first object must be to investigate the consequences of this cause acting under certain conditions, and to compare our results with those deduced from observation. Observation, however, unaided by theory, can rarely accomplish more than to detect approximations, more or less accurate, to those perfectly definite laws which the phænomena would

* Communicated by the Author.

accurately follow under the influence of the principal cause alone to which they are referrible. The coincidence between these perfectly definite laws and those deduced from our assumed general cause, independently of perturbing ones, must afford the strongest test of the truth of our assumption. The strength of the evidence thus derived, will of course depend in such cases upon the accuracy of the approximation to definite laws in the observed phænomena; but it is important to observe, that this first approximation must always be the most important one, and that it must be made the instant we begin to speculate on the causes of such phænomena as I have alluded to, if the slightest value is to attach to our speculations; and also that accurate (or what is synonymous in all, or at least in all but the simplest cases,) mathematical methods of investigating the effects which would result from our assumed general cause, are just as necessary in the case we are supposing, as if the observed phænomena presented accurate coincidences with the general laws to which they only approximate.

These remarks (sufficiently trite perhaps) are made with the view of meeting directly the vulgar objection of the uselessness of applying mathematical investigations to geological problems. To assert this is, in fact, equivalent to the assertion that that branch of the science with which we are immediately concerned presents no phænomena characterized by general laws, or referrible to a definite and simple cause. Such however is not the case. The phænomena do distinctly approximate to obvious geometrical laws, and there is a simple cause to which they may be referred, the effects of which it has been my object in the memoir in question to investigate on mechanical principles, in order that we may compare the laws obtained from these results with those to which the observed phænomena are found to approximate.

The phænomena with which we are chiefly concerned in these investigations are those dislocations of the crust of the globe, which we recognise more particularly in *faults* and *mineral veins*, or rather in the narrow fissures in which what is properly termed the mineral vein is deposited. The latter phænomena might, in fact, be almost entirely comprehended in the former, since it is found very generally, where mineral veins occur in stratified masses, that the strata are somewhat higher on one side of the vein than the other. In general this difference of level (not exceeding, perhaps, a few feet) is not sufficient to be designated as a *fault*, though it sometimes increases so much as to be considered such. In these cases it would appear absurd to suppose that the fissure of the

mineral vein and the fault are not to be referred to the same mechanical origin, or that other veins in the same district should not be referred to the same cause as such an one as that just described, from which, except where the above-mentioned difference of level becomes great, they differ in no respect. It is also highly important to observe, that (as far as investigation has yet proceeded,) where faults and mineral veins co-exist in the same district, they follow, with reference to their positions, precisely the same laws.

I do not mean, however, to maintain that all mineral veins are necessarily to be referred to the same mechanical cause. I conceive that some of the Cornish veins—those, for instance, of St. Austle Moor—are clearly referrible to some cause quite distinct from that in which the veins of our limestone districts have originated. The latter possess, I believe, universally the characters which lead us to regard them as having originated, like faults, in dislocations produced by mechanical violence, while the former are almost totally destitute of these characters. It would, therefore, be absurd to conclude that these two classes of veins have necessarily had the same origin. It is not, however, from *à priori* considerations that these points are to be finally decided: but since the evidence of dislocation afforded by a fault is independent of its vertical magnitude, I cannot but regard the mineral veins of our limestone districts as indicative of dislocations in the masses in which they exist, equally with the faults with which they are so frequently associated. I therefore regard them in this point of view; the correctness of our doing so must, of course, be ultimately tested by the harmony which may exist between our theoretical deductions involving this hypothesis, and the phænomena which these veins actually present to us.

The planes of these dislocations approximate, in the first place, to verticality; and, secondly, their horizontal directions bear distinct relations to the general configuration of the elevated district in which they exist. If there be a central *axis* of elevation, the directions of dislocation are approximately parallel or perpendicular to it, as is the case in most of our mining districts; and if there be a central *point* of elevation, these directions diverge from it as a centre. Such appears to be the case in Mount Etna, and the groups of the Cantal and Mont Dor. The lake district in this country probably affords a similar instance.

These are the laws established by observation, so far as it has yet extended. Many anomalous cases may possibly exist, but they will not invalidate the conclusion, that, so far as the phænomena are characterized by these laws, they are

attributable to the action of some general cause, at least as extensive in its operation as the district throughout which the phænomena are observed to follow the same law without breach of continuity. This cause is assumed to be that which naturally suggests itself to the mind of every geologist, viz. an elevatory force acting simultaneously at every point of a portion of the earth's crust, of at least the extent just intimated, and of any assigned thickness. It is manifest, that the elevation of this mass must produce in it *extension*, and consequent *tension*, which, if of sufficient intensity, will cause those dislocations or *fissures* which we now recognise in the phænomena already alluded to. These fissures must, according to this theory, be regarded as the *primary* phænomena, with which all the other phænomena of elevation, as faults, mineral veins, anticlinal lines, &c., are connected as *secondary* ones.

I have carefully abstained in my memoir from any speculations on the causes which might produce this elevatory force—I merely assume its existence. It is easy, however, to conceive such a force to act as above supposed, if we assume the existence of a cavity beneath the elevated mass, either originally coextensive with it, or rendered so by the action of the elevatory force itself. Any vapour or matter in a state of fluidity from heat, forced into this cavity, or expanded there, will produce the elevatory force which I assume to have acted. This appears to be the simplest mode in which we can conceive such a force to be produced; and if we choose to set out from the more remote hypothesis of the earth's having been originally fluid, it might probably be shown that the formation of cavities such as above supposed, would, under simple conditions, be the necessary consequence of that process of cooling by which we must then suppose the crust of the globe to have assumed its present solidity. Instead, however, of assuming the existence of a cavity, we might suppose a portion of the solid matter of the earth, at a certain depth beneath its surface, to become by some means expanded, and by its expansion to elevate the superincumbent mass. This hypothesis, as far as my investigations are concerned, would equally suffice, as, in fact, would any other by which we could account for the simultaneous action of an elevatory force upon a portion of the earth's crust of sufficient extent. For many reasons, however, independent of my immediate object, I should not hesitate to reject this latter hypothesis as generally insufficient to account for observed phænomena, and as involving serious physical difficulties. If we adopt the hypothesis of internal cavities, we may observe that there is no reason why we should not suppose them to exist, not only at differ-

ent depths in different places, but also along the same vertical line, so that one shall be placed under another. It might, I conceive, be shown to be highly probable, if we should again recur to the hypothesis of the original fluidity of the globe, that the deeper cavities would in such case be the more extensive.

The immediate consequence of the elevatory force, as already remarked, will be to produce *extension*, and consequent *tensions*, in the elevated mass. Our first object must be to determine the directions of these tensions, for the purpose of ascertaining those of the resulting fissures. We shall afterwards consider the influence of the constitution of the elevated mass; at present it is only necessary to regard it as admitting of a certain small extension without rupturing.

I. For the greater simplicity let us first suppose the elevated mass to be of indefinite length, of uniform depth, and bounded laterally by two vertical parallel planes, beyond which the disturbance does not extend. Let A B B' A' be a section of the mass by a vertical plane perpendicular to the axis of ele-

vation, A C B originally coinciding with A B; and let us also suppose that every such section is precisely similar and equal. Then it is manifest that there can be no extension perpendicular to these sections*, and that, consequently, the whole extension must lie in directions perpendicular to the axis of elevation. Now let us conceive for a moment the elevated mass to consist merely of a very thin continuous lamina of it, A C B. Then it is evident that the extension, and therefore the tension, at any point, as C, in the section, must be in the direction T C T' of a tangent to the curve line A B C. Let us now conceive another lamina, similar to the first, but without any adhesion to it, superposed upon it. It is clear that its extension, and, therefore, its tension, must be precisely the same as that of the first lamina, always supposing the original

* The hypothesis of indefinite length in the elevation is equivalent to that of its being terminated by sections equal and similar to the one described in the text, so far as relates to the absence of longitudinal extension.

unextended dimensions of each to have been the same. Again, suppose a third lamina superimposed in the same manner, and then a fourth, and so on, till a mass of any assigned thickness shall have been thus composed. It will then follow, from what has been shown, that the tension at any point *c* of the mass in this state must lie in the plane of the section, and in the direction to the tangent of the curve-line *acb*, formed by the intersection of the vertical plane of the section with the lamina in which the point *c* may be situated.

The only difference between this hypothetical mass and any proposed actual mass of the same form and dimensions, will consist in this—that in the former there is no cohesion whatever between the successive laminæ of which we have supposed it to be formed. If, however, our laminæ should be superposed on each other in their unextended state, and made to cohere firmly together, (in which case the mass would differ in no wise from any actual mass,) and then elevated to the position represented in the diagram, it is easily seen that the position of each point of the mass would be exactly the same as in the hypothetical case above stated. Consequently, the *extension* of any portion of the mass (and therefore the tension) must be the same in the two cases. Hence then it follows that if *AB B' A'* represent any actual elevated mass, the direction of the tension at any point *c* will be that of the tangent line at that point as above described.

There is no difficulty in extending reasoning precisely similar to the above to any more complicated form of the elevated mass, of which the upper and lower surfaces were originally parallel, and horizontal, and we shall arrive at this conclusion.—*If we conceive the mass, previous to its elevation, to be composed of horizontal laminæ (or thin strata) the directions of the tensions at any proposed point of the mass when elevated but still unbroken, will lie in the tangent plane to the curved surface formed by that originally horizontal lamina in which the proposed point may be situated; and the intensity of the tensions will be the same*, in different laminæ at points similarly situated in each.*

If the mass in its undisturbed position be not of uniform depth, (*i. e.* if the upper and lower surfaces be not parallel,) the above reasoning would not be accurately applicable. The case, however, we have considered may be taken as the standard one to which others will approximate with more or less accuracy, particularly as physical reasons might be assigned

* There are causes why this should be only very approximately true. (See Memoir, p. 42.)

why an extensive cavity within the earth should be nearly horizontal. Adhering then to this case, it is manifest that the extension of each component lamina of the mass will depend on the *form* assumed by it when the mass is elevated, since its boundaries, by hypothesis, remain immoveable. Consequently the direction of the tension in the *tangent plane* before mentioned must also depend upon the form of the lamina. This direction is not generally horizontal, but since it will usually be nearly so, and will always determine the horizontal direction, or azimuth, of a vertical plane drawn through it, we shall be understood when it may be convenient to speak of the *horizontal* tensions.

It is manifest then that the determinations of the *directions* of the tangential tensions in the elevated mass, must in cases such as the above be a purely geometrical problem, as may be easily elucidated by a few instances. In the elevation already described (of which the segment of a cylinder, by a plane parallel to its axis, may be regarded as the approximate type, and which may therefore be termed *cylindrical*) it has been shown that this tension lies entirely in a vertical plane perpendicular to the axis. If the elevation approximate to the form of a cone (which may be conceived to be formed by the superposition of similar conical shells), it may be shown*, that if each lamina remain unbroken, the direction of the only tension will be parallel to the slant side of the cone, and will pass through its axis; but that if a dislocation exist along the vertical axis, the principal tension at any proposed point (particularly near the vertex) will be perpendicular to the vertical plane passing through that point and the axis, there being also another tension in that plane. If again the form of the elevation should approximate to the segment of a sphere, there will be two tensions at each point of the mass, one of which will lie in the plane through the proposed point and the vertical axis of the elevation, the other being perpendicular to that plane.

The above are some of the most simple forms which the elevated mass can be conceived to assume; they may, however, be taken as the approximate types of many of the general elevations which present themselves to our observation, considered independently of their local irregularities. When the superficial boundary of the elevated mass is very irregular, (particularly if the superficial extent be not very great,) the directions of greatest extension, or of greatest tension, will be very different in different points; and it may become very dif-

* See Memoir, p. 47.

difficult to calculate with any precision the resulting phænomena. Cases however may easily be conceived without such difficulty, though more complicated than the simple ones above alluded to. Suppose, for instance, recurring to our hypothesis of internal cavities, one cavity of great extent to exist at a certain depth, and another smaller one within the mass above the former, and communicating with it, so that any fluid pressure acting in the lower should be communicated immediately to the upper one. That portion of the elevated mass which lies directly above the upper and smaller cavity, may manifestly be subjected simultaneously to the tension impressed upon the whole mass from the action of the elevatory force in the larger cavity, and to that produced by the partial elevation above the smaller one. These two sets of tensions may be conceived to be superimposed the one on the other, in the same manner as any two sets of forces in equilibrium may be so superimposed*. Their intensities and directions will depend on the forms of the general and partial elevations respectively. Thus we may have a partial elevation of which a cone or segment of a sphere should be the approximate type, superimposed upon a general one of which the type should be the segment of a cylinder. Other combinations might be formed in a similar manner.

Should it appear preferable to consider the subject independently of the hypothesis of internal cavities, we have only to conceive our partial elevations to be produced by a more intense action of the elevatory force at those points. As regards the resulting state of tension, it is perfectly immaterial which hypothesis we adopt.

The states of tension above described refer to the mass in its elevated but unbroken state, *i. e.* previously to the formation of those fissures which must of course be formed when the tension shall become greater than the cohesive power of the mass. The tension will begin to be produced at the instant the act of elevation commences, and will increase till it acquires the intensity just mentioned. Time will be necessary for this, but it may possibly be so short as to give to the action of the elevatory force the character of an *impulsive* action, which would probably produce the most irregular phænomena, and such as would be altogether without the sphere of calculation. I exclude therefore the hypothesis of this kind of action, not as involving in itself any manifest improbability, but as inconsistent with the existence of distinct approxima-

* One of these sets of tensions may possibly modify the other, but in a general explanation, or in a first approximate calculation, this modification may be neglected.

tions to general laws in the resulting phænomena. It would appear probable however that the time above mentioned will be short, and I therefore assume it to be so, and that consequently the tensions increase *rapidly* but *continuously* from zero to that degree of intensity which is necessary to overcome the cohesive power of the elevated mass. This assumption has also the advantage of facilitating some parts of the mathematical investigation*.

It will, perhaps, be somewhat more convenient for our further investigations, if we conceive the tensions at different points of one of our elevated, but still continuous and unbroken, component laminae, transferred to corresponding points of a plane lamina†. For this purpose, imagine each point of the curved lamina projected on a plane horizontal one, and that the same tension exists at each point of the latter, as at the point of the former, of which it is the projection; the direction of each tension in the horizontal lamina being the projection upon it of that of the corresponding tension in the curved one. Now one of our ultimate objects will be, to determine the horizontal directions of the fissures which must result in the elevated mass, when the tensions become of sufficient intensity to produce them, and these directions may be considered as coinciding with those which would be produced in our hypothetical horizontal lamina. Consequently our investigation will be reduced to the determination of these latter directions.

To elucidate this, suppose our general elevation to be such as first mentioned above, or what I have termed cylindrical. Its projection on a horizontal plane will be a parallelogram,

represented by D E F G. Suppose also a partial elevation

* See Memoir, p. 21.

† We may remark that the vertical elevation of the disturbed mass, in the state above described, is always extremely small compared with its horizontal extent.

approximately spherical, superimposed upon the general one, such that O shall be the projection of its vertical axis, and the dotted circle that of the circumference of its base. Then taking P as the projection of any proposed point in the partial elevation, we must suppose applied there, first, a tension (F) impressed on the mass generally perpendicular to D E; secondly, a tension (f_i) in a direction passing through O (see p. 233); and thirdly, another tension f_{ii} perpendicular to P O. From these data the directions of the fissure through P, when the tensions become sufficient to produce it, must be determined. And here we may remark, that since one lamina of our elevated mass will be similar to another, the tensions F, f_i , and f_{ii} , will be very approximately the same for each; and that consequently the direction of the fissure just mentioned will equally determine the horizontal direction of the fissure which shall pass through any point of which P is the projection. The extensibility of the mass being assumed to be small, the intensities of the tensions F, f_i , f_{ii} will be proportional to the extension each would produce in the mass at P, if it acted separately, or to the additional extension produced by each when acting simultaneously. The accurate determination of these intensities would in most cases present great difficulties. In general, however, it will be sufficient to consider such tensions as f_i and f_{ii} (belonging to the partial elevation) merely as forces producing modifications in the effects of F, the nature of which can be determined with sufficient accuracy for practical purposes.

[To be continued.]

XLV. On the Aurora of November 18th, 1835. By the Rev. T. R. ROBINSON, D.D.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

MR. STURGEON's notice of the aurora of ☿ November 18th (not 16th as misprinted,) induces me to send you the notes which I made of its appearance, as from his positive statement "that he saw no appearance of aurora to the south of the zenith, though frequently looked for," this seems to be one of the very rare cases where auroral phenomena can be proved to occur in a low region of the atmosphere. They are as follows, the time being reduced to Greenwich.

"Nov. 18. Sky strongly illuminated, but covered with clouds till 9^h 20^m, when two arches were visible, which broke