AN

ABSTRACT OF A MEMOIR

ON

PHYSICAL GEOLOGY;

WITH

A FURTHER EXPOSITION OF CERTAIN POINTS
CONNECTED WITH THE SUBJECT.

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AN

ABSTRACT,

&c.

In a memoir entitled *Researches in Physical Geology,* lately printed for the Transactions of the Cambridge Philosophical Society, I have endeavoured to develope, 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 recognize. 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 phenomena, 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 phenomena would 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 phenomena; 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 phenomena 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 phenomena 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 phenomena characterized by general laws, or referrible to a definite and simple cause. Such however is not the case. The phenomena 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 phenomena are found to approximate.

The phenomena with which we are chiefly concerned in these investigations are those dislocations of the crust of the globe, which we recognize 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 phenomena 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 phenomena which these veins actually present to us.

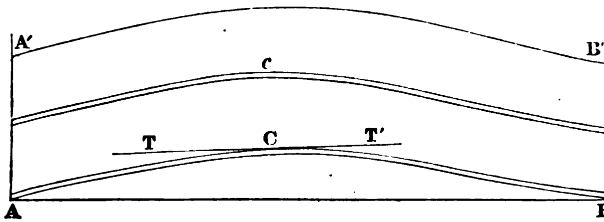
The planes of these dislocations approximate, in the first place, to verticality; and, secondly, their horizontal directions bear distinct relations to the general configuration of the elevated district in which they exist. If there be a central *axis* of elevation, the directions of dislocation are 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 groupes 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 phenomena 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 phenomena 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 recognize in the phenomena already alluded to. These fissures must, according to this theory, be regarded as the *primary* phenomena, with which all the other phenomena 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 co-extensive 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 asume 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 shewn 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 phenomena, 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 different depths in different places, but also along the same vertical line, so that one shall be placed under another. It might, I conceive, be shewn 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 *ABB′A′* be a section of



the mass by a vertical plane perpendicular to the axis of elevation, *ACB* originally coinciding with *AB*; 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, *ACB*. Then it is evident that the extension, and therefore the tension, at any point, as *C*, in the section, must be in the direction *TCT′* of a tangent to the curve line *ABC*. 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 pre-

* 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.

cisely the same as that of the first lamina, always supposing the original 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 shewn, 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 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 hypo-

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

thesis, 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 shewn 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 shewn,* 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

* See Memoir, p. 47.

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 difficult to calculate with any precision the resulting phenomena. 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

* 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.

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 phenomena, 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 approximations to general laws in the resulting phenomena. 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.*

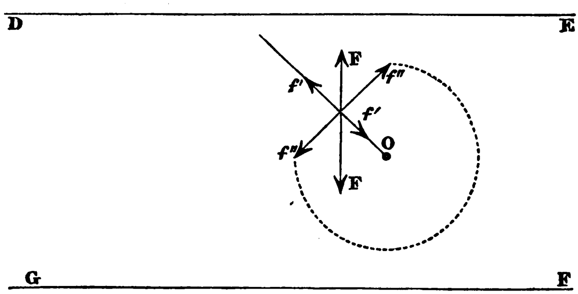
It will, perhaps, be somewhat more convenient for our further investigations, if we conceive the tensions at different points of one of our elevated, but still continuous and unbroken, component laminæ, transferred to corresponding points of a plane lamina.† For this purpose, imagine each

* 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.

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 *DEFG*. Suppose also a partial elevation 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 DE; secondly, a tension ($f_{,}$) in a direction passing through O (see p. 13); and thirdly, another tension $f_{,,}$ perpendicular to PO. 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, $f_{,}$, and $f_{,,}$, 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, $f_{,}$, $f_{,,}$ 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 $f_{,}$ and $f_{,,}$ (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.

II. Having now reduced the determination of the horizontal directions of the fissures produced in the elevated mass to that of the fissure which would be produced in a plane lamina, every point of which is subjected to known tensions, we may proceed with this latter problem. Our first object is *to determine the direction in which the tensions have the greatest tendency to cause a fissure to begin at any proposed point.* To give all requisite generality to the investigation, let us suppose there to be any number of these tensions, and let F, f_1, f_2, &c. denote their respective intensities at any proposed point, β_1, β_2, &c. the angles which their directions make with that of F;

$$\mu_1 = \frac{f_1}{F}, \quad \mu_2 = \frac{f_2}{F}, \quad \text{\&c.};$$

$$\Sigma\mu \cos 2\beta = \mu_1 \cos 2\beta_1 + \mu_2 \cos 2\beta_2 + \text{\&c.}$$

ψ the angle which the required direction makes with that of F; we shall have for the determination of ψ,

$$\cot^2 \psi + \frac{1 + \Sigma\mu \cos 2\beta}{\Sigma\mu \sin 2\beta} \cot \psi - 1 = 0.*$$

This equation will determine the direction in which the tensions have the greatest tendency to cause a fissure to *begin* at any assigned point, but when its formation has begun, it is obvious that

* See Memoir, p. 18.

the state of tension in its immediate vicinity must be altered, and that the tensions thus modified may not have a tendency to *continue* the fissure in the same direction as that in which it was the tendency of the original tensions to make it begin. I have shewn*, however, that with our hypothesis as to the mode of action of the elevatory force (see p. 15) the above equation will be very approximately applicable to the action of these modified as well as to that of the original tensions.

The actual direction in which the fissure will be formed will not in all cases depend solely upon this tendency of the tensions, but partly also on the constitution of the elevated mass. If, however, its cohesive power be perfectly uniform, it is manifest that this direction will be determined by the tensions alone, or will coincide with that given by the above equation. It will appear also that this is equally true in certain other cases; when it is not so, the effect of any peculiar constitution of the elevated mass must be investigated. I shall now proceed with these points.

Let us still confine our attention to a simple lamina of uniform thickness. Its cohesive power at any proposed point may be estimated in exactly the same manner as the intensity of the tension at that point†. Let the point of the lamina be designated by *P*, and draw through it, in any direction in the plane of the lamina, a straight line

* Memoir, pp. 20, 21. † Memoir, p. 13.

whose length is unity. Then conceive two equal and opposite forces (f, f) acting uniformly along this line perpendicular to it, and in the plane of the lamina, on the contiguous particles situated respectively on opposite sides of the line, thus tending to form a fissure along it. The cohesive power opposes this tendency, and if it be uniform along the line just mentioned it will be measured by that value of f which is just sufficient to overcome it. If the cohesive power along this line be variable, f will manifestly not be a measure of it with reference to the single point P. In such case we must conceive the cohesive power to be equal (for the unit of length) at every point of the line to that at P, and then that value of f (which we may designate by Π) which would, under such circumstances, just overcome the cohesive power, may be taken as a measure of it *at the point* P, when estimated in the direction perpendicular to the above line through that point.

In the first place let us suppose the value of Π the same for every direction of this line; then is it manifest that the direction in which a fissure may be formed immediately at the point P cannot be determined in any degree by the cohesive power, since its value is the same for every direction through P. The same conclusion will clearly apply to every point where the value of Π is independent of angular direction, and equally so whether Π be the same or different for different points, *i. e.* whether the cohesive power be uniform or variable, so long as its variation depends solely on the *position* of the point P; or, in mathematical language, the above conclusion will hold whenever Π is a function only of the co-ordinates of P. In such case then, the fissure will be formed through P in that direction in which the tensions there have the greatest tendency to form it, and our equation will be as strictly applicable for the determination of this direction as if the lamina were perfectly homogeneous. We shall be able shortly to extend still further the conditions under which this equation will be similarly applicable.

It is easy to extend the above reasoning from a laminà to the general elevated mass.

If, however, the value of Π be different for different angular positions of our line of a unit of length through P, (as, for instance, when a laminated or jointed structure prevails in the mass, or any accidental line of less resistance passes through the proposed point) it is manifest that the direction of the fissure there will depend on the tensions and this variable value of Π conjointly, and the equation above given will no longer suffice generally for its determination. The case of laminated or jointed masses I professedly exclude from these investigations, since their lines of dislocation will necessarily be principally determined by their peculiar structure, and will therefore be in great measure independent of the causes whose effects I am investigating. The case however of the existence of partial and irregular lines of less resistance, regarded as modifying, and not as principal causes, comes within the sphere of our investigations. We may now proceed to this point.

Recurring again to the simple case of a lamina, it is easily shewn * that if a fissure in its continuous propagation through consecutive points meet a line of less resistance, it will be propagated across it without change of direction, or along it, according as a certain condition is or is not satisfied, this condition depending on the angle at which the fissure meets the line of less resistance, and the cohesive power along that line estimated in a direction perpendicular to it. If this angle be a right angle the condition is necessarily satisfied, as it must be also if the angle do not deviate much from a right angle, unless the cohesive power just mentioned be extremely small, so that in such cases the line of less resistance will have no effect on the direction of the fissure. If the angle just mentioned deviate too much from a right angle, the fissure will be propagated along the line of less resistance; but I have shewn † that when this ceases to be the case it will almost immediately resume the direction determined by our equation, so that if these lines of less resistance exist only partially and irregularly, and be of limited extent, they will only produce partial deviations in the direction of the fissure, without very materially affecting its

* Memoir, p. 24.

† Memoir, p. 23.

general bearing. This reasoning again is easily extended to the general mass.

We shall now be able to arrive (as intimated above) at another and important condition respecting the constitution of the elevated mass, with which our equation will be strictly applicable to determine the direction of a fissure. If a single tension act at a point of a lamina, it is easily shewn *(and in fact is in itself sufficiently obvious,) that the resulting fissure will be perpendicular to the direction of the tension, the cohesive power being such as above shewn (p. 20) to be consistent with the strict application of our equation. In like manner it may be easily conceived, that since all the tensions act in the planes of their respective laminæ, whatever their horizontal directions may be, the resulting fissure, whatever may be its horizontal direction, must necessarily (independently of perturbing causes,) lie in a plane perpendicular to each lamina at the points where it intersects it. Hence, then, it follows that however small the cohesion may be between two successive laminæ or strata, this will produce no effect on the position of the fissure. In such case then its horizontal direction will still be accurately determined by our equation. This is important, because in a stratified mass the cohesion between different beds must probably be often much less than that between the constituent particles of each bed. The same conclusion will hold with respect

* Memoir, p. 14.

to any accidental planes of less resistance which do not deviate too much from horizontality; but if they be vertical, or nearly so, they will produce the accidental and partial deviations which have already been noticed.

In forming a judgment of the probable extent of these planes of less resistance, we must be careful not to be too much influenced by the impressions produced by the examination of a disturbed district, since we are now speaking of the existence of these planes in the *undisturbed* mass. I would also observe, that we are only concerned with this kind of discontinuity in the cohesive power, so far as it depends on local and irregular, and not on general causes, since, as already stated, I exclude those cases in which any regularly jointed or laminated structure may be supposed to have existed in the mass previously to its elevation. Now as far as the planes we are speaking of might be caused by accidental circumstances in the constitution or deposition of the mass, it would seem necessary to suppose them irregular in position and partial in extent; in which case, as we have seen, partial deviations only would be produced by them in the vertical or horizontal directions of the fissure.

It appears then from what has preceded, that the equation above given will accurately determine the direction of a fissure at any proposed point, produced by tensions such as we have supposed, not only in a homogeneous mass, but also in a mass in which there may be any number of planes of less resistance, provided they do not deviate too much from horizontality, and notwithstanding any variation in the cohesive power of the mass depending on the difference of position of one point and another. From the interpretation of the equation, it appears that the fissure (or rather its intersection with a horizontal plane) will in general be rectilinear only in the particular case in which the ratios $\frac{f_1}{F}$, $\frac{f_2}{F}$, &c. are the same for every point through which it passes, supposing the directions of the tensions at one point respectively parallel to those at another. There is, however, one important exception, viz. the case in which there are two tensions only, and these tensions at right angles to each other. The direction of the fissures will then be always perpendicular to that of the greater tension. If therefore the directions of this tension at different points be parallel to each other, the fissure will be rectilinear, whatever be the ratio of the two tensions. The case of a single tension is a particular case of the above, when the smaller tension vanishes. If there be two tensions making an acute angle with each other, the direction of the fissure will be within the exterior or obtuse angle between the directions of the tensions; and if one tension be considerably greater than the other, or if the angle between their directions do not deviate materially from a right angle, the fissure will lie much nearer to the direction of the smaller tension* than to that of the greater.

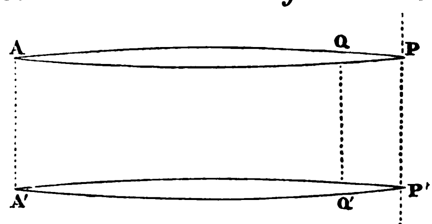
III. Having thus explained how a single fissure may be formed, and its direction determined, let us consider the formation of a number of similar fissures all following the same law, and not remote from each other, thus forming a *system of fissures*. In the greater number of cases in which such systems have been recognized the lines of dislocation have been approximately rectilinear and parallel. It will suffice, therefore, to take this case, which will be somewhat the most simple to explain.

In the first place I have considered in my memoir, how far it would be possible that the fissures of a system should be formed *consecutively*. For this purpose I have examined the modification which would be produced in the tension of the mass by the existence of a rectilinear fissure extending for any assigned distance, assuming, for the greater simplicity, the mass to be acted on by one system of tensions perpendicular to the fissure; and it appears that if we draw a line perpendicular to the fissure and meeting it at a point *P*, not too near its extremities, the tension at any proposed point of this line and in its direction (or perpendicular to the fissure) will be less than that which will be caused by the existence of the fis-

* Considerably nearer to it than the resultant of two forces respectively equal in intensity to the two tensions, and in the same directions.

sure, in a direction parallel to itself, provided the distance of the proposed point from the fissure be less than the radius of curvature at *P* of the curve formed by the intersection of the vertical side of the fissure with a horizontal plane*. Now it has been before stated that when there are two tensions at any point in directions perpendicular to each other, if they produce a fissure it must be perpendicular to the greater of these tensions, and therefore, in the present case, perpendicular to the former fissure. Consequently, since the radius of curvature above mentioned will, if the fissure be of considerable length, be very large at every point, it will be impossible for a second fissure to be formed parallel to the first and not very remote from it, by the general tensions to which the mass is supposed to be subjected†.

We may conceive, however, any number of parallel fissures in the case we are considering to be formed *simultaneously*. Thus suppose



* Memoir, p. 33.

† If there were another system of tensions perpendicular to the first, this conclusion would be true for still greater distances from the existing fissure. We may remark that these two cases of tensions would seem to be the only ones in which systems of rectilinear parallel fissures near each other could in any way be produced. See Memoir, p. 36.

two parallel fissures, *AP*, *A'P'*, produced by tensions acting perpendicularly to their directions, to begin simultaneously at *A* and *A'*, and also to arrive at *P*, *P'* at the same instant, *PP'* being perpendicular to *AP* and *A'P'*. There will manifestly be no reason why they should not in such case be continued simultaneously from *P*, *P'*, just as they began at the same instant at *A* and *A'*. If, however, the relaxation produced by the opening of *AP* be communicated through the distance *PP'* *instantaneously**, it is clear that as soon as *AP* should have advanced in its progressive formation by the smallest quantity further than the other fissure, the formation of this latter would be instantly arrested. Under such circumstances, then, the possibility of the simultaneous formation of two or more fissures would be rather a mathematical than a physical possibility. The fact is, however, that the relaxation produced by the one fissure is not communicated instantaneously to the distance of the other. *Time* will be necessary for this purpose, and this removes the difficulty of conceiving this mode of formation, since it is no longer necessary that the velocities of propagation of the two fissures should be mathematically equal. For, suppose one fissure to have reached *P* when the other has reached *Q'*, then it is easily seen that if the velocity of propagation of the first fissure should be such as to

* This would be the case if the mass were absolutely *inextensible.*

continue it from Q to P, in less time than the relaxation of the tension would be communicated from Q to Q', (QQ' being parallel to PP',) the continued formation of $A'Q'$ would not be arrested. Now I have shewn* that the velocity of propagation will be extremely great, so that the distance $Q'P'$ may be large, and all physical impossibility is therefore entirely removed. Let us suppose, for instance, a system of parallel fissures to begin simultaneously along the lower surface of the elevated mass, and to be propagated upwards†. If the mass be nearly homogeneous, the velocity of propagation will be nearly infinite; and if the fissures be not too near together, it is very possible that the time requisite for the relaxation of the tension to be communicated from one fissure to the distance of the next, may be greater than that which is necessary to propagate the fissures to the upper surface of the mass. In this case it is manifest that every fissure will necessarily be continued to that surface. It seems most probable, however, that in actual cases, similar to that just stated, a part only of the fissures commencing below would reach the higher portion of the mass. If its thickness should be very great, the fissures

* Memoir, p. 22.

† I have shewn (Memoir, p. 43) that fissures must generally commence in the lower portion of the mass; and we may remark, that according to our hypothesis respecting the rapid increase of intensity of the elevating force, from the instant the elevation commences, the formation of the fissures must begin almost accurately at the same instant.

reaching the surface would probably be at a proportionally greater distance from each other.

In this manner, then, the formation of systems of parallel fissures presents no difficulty. Adopting this view of the subject, we are immediately led to the conclusion, *that the whole of any disturbed district, characterised by a continuous system of parallel dislocations, must have been elevated simultaneously.* It is not, however, here meant to be asserted that the whole elevation must have taken place at once, but that that movement which determined the positions of the principal and characteristic dislocations by causing the commencement of their formation, must have been a great movement, and must have extended at least as far as such dislocations may be observed to follow continuously the same law. Subsequent efforts of the elevatory forces might take place in any number, but it is evident that they would have but little effect in producing new fissures parallel to the former, (since the mass would generally yield along the old ones) though they may be very instrumental in extending those existing previously. Partial elevations, or subsidences, may be easily conceived to be thus produced; but whatever alteration may take place in this manner, in the general conformation of the district, must be under the guidance, as it were, of the fissures previously existing.

Nothing perhaps will tend more to corroborate the views I have been explaining on this important point of the formation of systems of fissures, than the attempts we may make to account for it otherwise, assuming always that the phenomena are due to the action of mechanical causes extraneous to the mass itself, and independent of that kind of internal molecular action to which the existence of joints, or of a laminated structure, may possibly be owing. In the first place, I have shewn that two parallel fissures not remote from each other could not be formed consecutively by a repetition of the elevatory action extending to the whole elevated mass; this consecutive formation, if it should take place at all, must therefore be owing to consecutive partial efforts of the elevatory force at different points of the mass. But I have shewn* that, if the elevatory force be confined to a portion of the mass of comparatively small superficial extent, fissures must either be formed diverging from it in all directions, such as have been recognized in Mount Etna, and in the groupes of the Cantal and Mont Dor, or concentric about the vertex, so that it is mechanically impossible that systems of parallel fissures could be thus produced. In fact, I can in no way conceive this successive formation of parallel fissures, without hypotheses respecting the mode of action of the elevatory force which are infinitely too arbitrary to be admitted for an instant.

After one system of fissures is formed, there is no difficulty whatever in conceiving the forma-

* Memoir, p. 47.

tion of a second system perpendicular to the former. The existence of two rectilinear parallel fissures must evidently destroy all tension in the portion of the mass between them, but will have no effect on the extension, or therefore on the tension which may exist in a direction parallel to the fissures, the only one in fact in which any tension can be impressed on a part of the mass so situated. Consequently whatever tendency there may be to form a second system of fissures, it must necessarily be in a direction perpendicular to that already existing. This second system might be formed by any forces, however partial or irregular their action, (always assuming it not absolutely *impulsive*) since the only direction in which the mass could admit of any tension being impressed upon it would be, as just stated, that parallel to the first system. It seems to be mechanically impossible that any second system of parallel fissures could be thus formed except in the direction here stated.

IV. The two systems of fissures which I have described are those which must be regarded according to this theory as *primary phenomena*, from which, as before stated, the *secondary phenomena* of mineral veins, faults, anticlinal lines, &c., must be derived. For this second part of the subject, I must refer to the second Section of my Memoir, where I have entered in detail into the manner in which these latter phenomena may be conceived to be derived from the former. The number of phenomena which we are thus enabled to account for, as the consequences of a simple cause from which they are deducible by strict mechanical reasoning, appears to me, in the present state of our speculations and practical knowledge, to give to the theory I have been attempting to develope the strongest claim to the attention of geologists.

It will be observed, that a most essential part of this theory consists in the relation which it assigns between the directions of dislocation and the general configuration of the elevated mass *at the instant previous to its rupture*. It may, at first sight, appear impossible to ascertain what this form may have been, now that we can only examine the mass in its dislocated state; but this difficulty, though it must always exist in a greater or less degree, will not appear so serious a practical one, when we consider that since necessary relations must exist between the form of the mass at the instant above mentioned, and the lines of dislocation, and again between these lines and the actual disturbed form of the mass, some such relations must also exist between this latter and the previous form. Thus if the actual form be approximately conical, we may conclude it to have been also conical at the instant of dislocation; and if the disturbed district be of great length as compared with its width, and presents a well-defined axis of elevation, we may conclude the unbroken elevation to have been approximately cylindrical. Partial elevations which may have been superimposed upon a general one, as already described, at the instant previous to dislocation, must generally be more difficult to detect, since it may frequently be impossible to distinguish present indications of them from similar elevations which may have been produced by the elevatory force entirely *subsequently* to the formation of the fissures. On such points the observer must of course exercise his discrimination and judgment. Deviations from rectilinearity or parallelism in the lines of dislocation are not to be regarded necessarily as anomalies. We frequently speak, it is true, of the law of parallelism in such phenomena, as if that were their essential characteristic, but it is manifest that, according to our theory, this is only a secondary property in them, depending on the rectilinearity of the general axis of elevation. In conical elevations such as those before alluded to (p. 12) there is no approximation to parallelism in the observed fissures; and if the general axis of elevation be curvilinear, the longitudinal fissures preserving their parallelism with it (according to theory) will be also curvilinear, while the transverse fissures perpendicular to the former at their points of intersection will no longer be parallel. These deviations from rectilinearity and parallelism are due to the action of the general elevatory force, and to the nature of the general elevation. Others more limited may be due to partial elevations. Where they are observed, the local configuration of the mass must be examined, and thence the directions of the consequent tensions superimposed on that of the general mass must be inferred. The equation of page 18 will then determine the resulting directions of the fissures, and therefore the nature of the deviations. In simple cases, the remarks in page 25 will enable us to do this with sufficient accuracy to compare the theoretical deduction with the observed phenomena.

V. It has already been stated, that all consideration of those portions of the earth's crust, in which a regularly jointed or laminated structure may have prevailed previous to their elevation, has been excluded from the investigations contained in my memoir, because I wished to keep them distinct from any speculations respecting the operation of other causes than the one whose effects I proposed to develope. In our general speculations, however, on theories of elevation, it is necessary to consider how far these dislocations which, according to the theory I have been discussing, must be regarded as primary phenomena, are any thing more than secondary ones, depending on lines of less resistance, produced by some such particular structure as that above mentioned. Much valuable information respecting joints may be expected from the forthcoming work of Professor Philips on the Geology of Yorkshire; but at present we know but little accurately about this important feature in the structure of rocks, and of its cause, I conceive, absolutely nothing. We have, therefore, no positive reason to conclude that this peculiarity of structure has generally been superinduced after the elevation of the mass in which it exists, though, in some cases, there appears little doubt of such having been the fact. It is highly important, however, to determine whether any perfect coincidence does exist in the directions of dislocation, and of joints in the same district; and it is to be hoped that geologists will direct their earnest attention to this subject. Should this coincidence be established generally in districts where the fractures are parallel and at right angles to each other, it will still be important to ascertain how far it exists in elevations approximating to the conical form; and in all cases whether the directions of joints bear any relations to the configuration of the mass, as modified by partial elevations. All these are points of interest which we may hope by accurate and careful observation to determine. Should the coincidence, however, between joints and lines of fracture be perfectly established, we shall still have to consider whether the joints, by their prior existence in the undisturbed mass, have determined the lines of fracture, or these latter phenomena have exercised an influence in determining the positions of the joints, supposed to be subsequently formed in the elevated mass. Now, assuming the coincidence just mentioned, there must of course be the same relations between the general conformation of the elevated mass and the directions of joints, as those which have been already stated to exist between that conformation and lines of dislocation; and therefore, if we assume the prior existence of joints, and also that the lines or axes of elevation have been principally determined by the points of application of the elevatory force in the lower portion of the elevated mass, we must necessarily conclude, that some relation must exist between the causes which have produced the jointed structure, and the action of the elevatory force; *i. e.* between the action of a force extraneous to the mass, and that internal molecular action, to which it would seem absolutely necessary to refer the formation of joints in the undisturbed mass. To assert such relation to be physically impossible, would, in the present state of our knowledge, perhaps be absurd; but it does appear to me that the difficulty of conceiving it is so great as to form a most serious, if not a fatal objection, to any theory in which it should be involved as a necessary consequence. To avoid this objection, we might proceed on the hypothesis, that the lines or axes of elevation have been principally determined by the lines of less resistance along the joints, rather than as above supposed; and such might be the case, if the principal line of action of the elevatory force* should not deviate materi-

* It must be recollected that we can only judge of the superficial form and dimensions of the mass to which the elevatory force has been applied, by those of the actual elevations. These appear unquestionably, I conceive, to justify

ally from either of the two directions at right angles to each other in which we are assuming joints to exist. If, however, that principal line should approximate to an angle of 45° with these rectangular directions, and should be of considerable length, the hypothesis would be, I conceive, altogether inadmissible.

Supposing then the accurate coincidence of the directions of joints and those of fracture to be hereafter established, it would appear that the hypothesis of the laws discoverable in lines of fracture being generally due to the prior existence of some regular structure in the undisturbed mass, would still involve serious difficulties, on account of the relations existing so generally between the lines of fracture and the configuration of the elevated mass, for which, with the above hypothesis, it seems almost impossible to conceive any efficient physical cause. On the other hand (still supposing the coincidence of the directions of joints and of lines of fracture), if we assume the formation of joints to have been posterior to the elevation of the mass, this coincidence will still remain to be accounted for. Our ignorance, however, of the process by which this structure may have been superinduced, will not at present allow us to do this. The fact must continue to offer a theoretical difficulty, but one, I conceive, very

justify the notion of sufficiently determinate *lines of action* of the elevatory force, at least in a sufficient number of instances to give due weight to the argument in the text.

different in its nature to that above stated, since it would appear, I think, probable, that, taking a portion of the elevated mass bounded by adjoining fissures, the position of the joints subsequently formed in it should have some relations to the boundaries of that portion. The fact, therefore, of the coincidence (or rather parallelism) of direction above mentioned, while it presents a difficulty, does not seem to offer any *à priori* objection to the theory which involves it. It is proper, however, to observe that though it should appear, for the reasons now stated, that a preference may generally be due to the theory which would assign the production of fissures to the elevatory force alone, we should by no means be justified in the rejection in every instance, of that which attributes the directions of those fissures to the previous structure of the mass, and especially in those cases in which we fail to recognize distinct lines of elevation, or the usual relations between them and the lines of dislocation. And here it may be remarked as a striking fact, that the only mining district in this country in which there is any difficulty (as far as I have yet ascertained) in tracing these relations, is that in which, for independent reasons, it appears most necessary to recognize the influence of a previously veined or jointed structure on the directions of its dislocations. I allude to the mining district of Cornwall.

In the above reasoning I have assumed the *accurate* coincidence of the directions of joints and of lines of fracture, and it is important to observe, that this accuracy of coincidence is essential to the theory which would assign the latter phenomena to the prior existence of the former. A difference of a few degrees in the angular positions of the above lines would, if clearly established, be fatal to this theory, because, as I have already explained, although a fissure produced by an elevatory force would cross a line of less resistance under a certain condition, without change of direction, that condition cannot be generally satisfied when the angle between the fissure and line of less resistance is small, and in such case the fissure will be propagated exactly along the latter line. Observations on this point would therefore demand great care and accuracy. It is also important to remark, that the accurate coincidence above spoken of will require two co-existing systems of joints to be at right angles to each other, since such is the law recognized in lines of dislocation. Observation, however, so far as it has proceeded, appears in many instances, I believe, to contradict this law in the directions of joints. Should any other laws be established hereafter, or very frequent deviations from the one just mentioned, it will probably be found necessary to abandon altogether the notion that lines of dislocation have been principally determined by the directions of joints, rather than by the mode of action of the elevatory force.

The theory which it has been the object of my memoir to develope, enables us to account for nearly all the more important phenomena of elevation; but before we finally decide on its relative claims to our adoption, we are manifestly called upon to remove as far as possible, by accurate observation, the uncertainty which still remains respecting the possible influence of a jointed structure in producing what I have termed the primary phenomena of elevation. These speculations are thrown out with the hope of indicating some of the more critical points of enquiry on which the ultimate determination of this question must turn, and which are generally best indicated in such cases by theoretical discussion. The necessary relations which I have shewn must exist, according to one of these theories, between the directions of dislocation and the general, and in some cases local, conformation of the elevated mass, will probably do much towards enabling us ultimately to decide between them; and it is therefore of the first importance that the observer who may hereafter wish to elucidate this subject, should remark these relations as carefully as those which may exist between the dislocations themselves, or the joints with which they may be associated*.

* It would be important, as before intimated, to observe the directions of joints in a conical elevation with lines of dislocation diverging from its vertex. I am not aware that the existence of a similarly diverging system of joints has ever been suspected. It would also be highly desirable to observe whether there be any continuity in the joints of two contiguous but distinct formations, and particularly when

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We may here observe, that the only difference between the two theories we have considered, consists in the cause which they assign for what I have termed, with reference to the theory with which I have been more particularly occupied, *primary* phenomena. The *secondary* phenomena of faults, mineral veins, anticlinal lines, valleys, &c. will be deducible from the primary ones just in the same manner in both theories, so that nearly the whole of the investigations contained in the second section of my memoir will be equally applicable to both these theories.

In that section I have entered, as before intimated, with considerable detail into an examination of the secondary phenomena of elevation, such as anticlinal lines, longitudinal and transverse valleys, ejected and injected horizontal beds of trap, veins of trap and granite; and also the different phenomena of mineral veins, such as the *throw* and depth of a vein, the comparative widths of the best bearing veins and cross courses, and the *shifts* or *heaves* so frequently recognized at the intersections of veins. I have also stated reasons for concluding

one formation is primary and the other sedimentary. The perfect continuity of the veins in Cornwall, in passing from the killas to the granite, forms a curious feature in the geology of that district, if we are to regard the former as a sedimentary deposit. In such case, it would clearly demonstrate that the regular structure to which, I conceive, many of those veins must be referred, was superinduced in that district after the great dislocations which must have accompanied the injection of the granite.

that the fissures of mineral veins must have been filled by some process of infiltration or segregation (which I profess not further to define) from the surrounding mass; and here, viewing this point with reference to the subject of joints, I would further observe, that the formation of a vein (by which is here meant the matter contained in the fissure) might take place along an open joint, exactly in the same manner as along a fissure produced by any other means. If, therefore, we find veins (such as those before alluded to in Cornwall) which cannot be supposed to originate in the dislocating effects of an elevatory force, we should carefully examine how far the directions of these veins appear to coincide with those of the leading joints. From a hasty inspection of the Cornish veins, I have a strong impression that this coincidence will be found to exist in that district. It would be important to ascertain this fact by careful and detailed observation; for, should it be established, it will immediately destroy the hypothesis of the *contemporaneous formation* of such veins as a necessary alternative, and at least remove one inconceivable process from the speculations of geologists, more especially with respect to those who may at once be disposed to allow this mode of formation of the Cornish veins, while they contend for the fact of the mass in which they are found being a sedimentary deposit. The difficulty of the theory of all similar veins will be reduced by my hypothesis to that of the formation of joints, a process hard enough to conceive, but which has its analogy in that of crystallization, and must of necessity be recognized. The process of contemporaneous formation of veins, without the previous formation of fissures as receptacles for the segregated or infiltrated matter, appears to me inconceivable in itself, and unsupported by any analogies drawn from the known operations of nature.

VI. There is another point on which I have touched incidentally in the conclusion of my memoir—the application of the principles already explained to the theory of Elic de Beaumont, respecting the parallelism of mountain-chains of contemporaneous elevation. I have before stated, that in whatever manner we may conceive an elevatory force to be produced, there seems no reason why we should not suppose it, in some cases, to have acted at a much greater depth than in others. Now I have already explained (p. 27) the reason for concluding that the formation of a system of fissures, according to our theory, must be simultaneous; and also (p. 28) how the simultaneous formation is facilitated by the circumstance of *time* being necessary for the transmission of the relaxation of the mass produced by the opening of a fissure. From that explanation it will easily be seen, that if a number of fissures commence simultaneously in the lower portion* of an elevated mass of great thickness, and great superficial extent, it is most probable that those only will reach the upper surface which are remote from each other. These fissures will be large, and all the phenomena resulting from them may be expected to be on a proportionate scale. Anticlinal lines† will almost necessarily be formed along them; and thus it is as easy to account for two parallel mountain ranges, as for two neighbouring anticlinal lines on a scale of comparatively small magnitude; and our theory will thus assign a physical cause for the law of parallelism in mountain chains of contemporaneous elevation, as contended for by M. Elic de Beaumont, if, at least, the application of that geologist's theory be restricted within certain limits. I have no intention, however, of now insisting on this extensive action of the physical cause I have been considering; but I would observe, that the extent of this action can only be determined by that of those portions of the earth's surface throughout which the laws of observed phenomena may be continuous, and in accordance with our theoretical deductions.

To persons not habituated to the investigation of the accurate relations between mechanical causes

* I have shewn that these fissures must generally commence in some lower portion of the elevated mass. See Memoir, p. 43.

† See Memoir, p. 51.

and their effects, much of the previous reasoning may appear too refined to be applicable to our subject; but it must always be recollected, that this reasoning is immediately applied to hypothetical problems, to which it is strictly applicable, and which are chosen so as to bear the closest analogy to the corresponding ones which nature presents to us; and it is simply on the strictness of this analogy that we are called upon to decide, in judging of the admissibility of our mode of investsgation. It is important to have a clear conception of this principle, on which the application of strict analysis to the problems of nature must always be made. In fact, this is the principle on which every one must tacitly (sometimes perhaps unconsciously) proceed, in forming a distinct idea of the necessary relations between any physical cause acting under complicated conditions, and its remoter consequences. We must form our conclusions from the consideration of some comparatively simple but strictly analogous case, and apply them to the actual one, with such limitations as circumstances may require. The advantage which the mathematician possesses, consists in this—that the standard case to which he refers his more complex problem, is a definite one, from which he has means of deducing his results free from that uncertainty which necessarily attends other modes of investigation. It is a standard case of this kind, which I have endeavoured to supply for geological theories of elevation; nor am

I without hopes, that the attempt may at least so far succeed as to remove some of that indefiniteness on this subject, by which the earlier speculations in every science must almost necessarily be characterized. More particularly, perhaps, may this be asserted of geology, which, notwithstanding the rapidity of its growth, is yet hardly strong enough to emerge from the cloudiness in which its phraseology alone, with reference to the phenomena of elevation, by addressing itself more to the imagination than the judgment of the student, has sometimes been sufficient to involve it. An impression has thus been too frequently created, that little hope exists of elevating the science to any rank among the stricter physical sciences. Such a notion, however, appears to me most fatal to its healthy progress. The author of the *Principles of Geology*, whatever may be thought of some of his theoretical views, must be allowed by all to have set us an example well calculated to improve in this respect the tone of geological speculation, inasmuch as he has boldly grappled with the difficulties of his problems in detail, and not been content to meet them with indeterminate generalities. In these investigations I have endeavoured to act upon the same principle, as the only one on which, if we are to speculate at all, we can speculate with safety; and if, perchance, a somewhat vague and misty sublimity which has appertained to this branch of the science should thus be diminished, ample compensation will be made if we should in return confer upon it a portion, however small, of the more naked dignity of demonstrative truth.

St Peter's College,
Jan. 7, 1836.