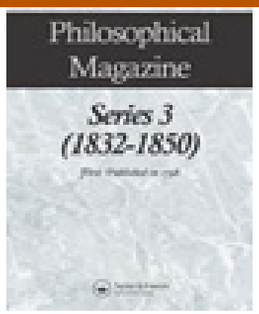

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IV. *Additional Remarks on* Mr. Hopkins's "*Researches in Physical Geology.*" *By* HENRY S. BOASE, *M.D., &c., Secretary to the Royal Geological Society of Cornwall.**

IT was with great pleasure that I read Mr. Hopkins's reply to my former remarks, because a discussion conducted in a proper spirit cannot fail to elicit truth, and may be the means of stimulating geologists to investigate more carefully the important question under consideration.

The point at issue between us is, as Mr. Hopkins has justly remarked, "whether the jointed structure of disturbed masses has been in great measure superinduced *previously* or *subsequently* to their elevation." Two other topics have been dwelt on in his reply,—the nature of the elevatory force, and the origin of veins,—both very interesting, but not, I conceive, so easily determined by *observation*, as the question more immediately under discussion.

Mr. Hopkins asserts, "that it is totally inadmissible to assume the earth's crust to have become jointed, before the action of the dislocating force upon it." I, on the other hand, contend that *solid* rocks not only existed *previously* to their having experienced elevatory movements, but also that such rocks must necessarily have had a *jointed structure*. This is, I think, a fair and plain statement of the case *sub judice*, divested of all its collateral intricacies: now then for the evidence.

As regards the structure of rocks, how does the matter stand at the present day? If we examine any formation from the oldest non-fossiliferous strata to the newest of the tertiary deposits, or indeed to the recent sandstones of the modern or diluvial epoch, the evidence is invariably the same: all demonstrate that *solid* rocks, whether they have or have not been subject to movements, possess a concretional structure, being intersected by *lines or joints* which divide them into determinate masses. And not only so, but granitic and trappean rocks, and even lavas, all, when solidified, are similarly circumstanced. Nor can this excite any wonder, since a contrary state of things would not be in accordance with the laws of nature,—it being a fundamental maxim in physics, that the particles of solids are united together by attraction of cohesion, which has a tendency to arrange bodies in definite forms. If we did not know the fact, would it not have been a legitimate inference that solid mineral masses might be found to possess a concretional structure? But in as much as all known *solid* rocks, whether igneous or aqueous, disturbed or

* Communicated by the Author.

in their original position, in mass or insulated between loose earthy beds,—in as much, I say, as all these exhibit joints or lines of structure, I think that I am justified in regarding this condition as a species of crystallization,—the inseparable consequence of the particles of an originally unconsolidated mass having been completely subjected to the operation of cohesive attraction.

If Mr. Hopkins's hypothesis requires it to be otherwise, it is incumbent on him to adduce facts in support of his opinion. Perhaps, he will not dispute this position, but content himself with maintaining that the rocks, when elevated, were not in a solid state. I shall, however, have no difficulty in establishing the contrary; for there is ample evidence of rocks, before dislocation and elevation, having been solidified.

But, says Mr. Hopkins, "in my investigation it is unnecessary to suppose any but the lowest degree of solidification in the elevated mass; and therefore it is manifestly quite inadmissible to assume that it could not be dislocated by an elevatory force before its jointed structure had become sufficiently developed to determine the directions of dislocation." I do not *assume* that an unconsolidated mass cannot have been elevated or depressed; because it is evident, that recent sedimentary deposits must be acted on, according to the movements of the older *solid* rocks on which they repose; but I do assert that rocks over extensive regions in every part of the globe, (and only one instance would suffice for the argument,) when thus acted on, were not in the lowest, nor in any intermediate degree of the process of induration, but were perfectly consolidated.

For instance, various series of rocks, including those of more than one epoch, have accumulated, during the lapse of ages, from the comminuted debris, angular and water-worn fragments of older rocks, in the hollows of which they have been deposited; these derivative rocks now exhibit faults, veins, and other indications which Mr. Hopkins ascribes to elevatory movements. Now it is of no consequence whether these upper derivative rocks were solid or not; but it is evident that the parent fundamental rocks must have been perfectly *solid* or they could not have furnished the pebbles, which very commonly consist of quartzose and other siliceous substances, not only belonging to older sedimentary formations, but also to *igneous* rocks which cannot be supposed to have been reduced to a state of detritus by aqueous action until they were actually *solid*. It may also be remarked, that it is generally admitted that movements, such as have taken place in former days, are now and will be hereafter in operation:

there can be no doubt then that all future dislocations must affect a basis of *solid* rocks possessing lines of structure, and therefore would be subject to the modifying circumstances which Mr. Hopkins is desirous of evading.

In opposition to this statement, can Mr. Hopkins adduce any evidence in support of his conjecture, that various degrees of solidification existed in all the rocks subjected to elevatory movements? If not, the convenience alone of his hypothesis ought not to be admitted as a sufficient argument; and my deduction from facts cannot be considered as satisfactorily answered by treating it as "*à priori* reasoning founded on what we are altogether ignorant of."

In order to illustrate the subject more fully, let us direct our attention to the principal movements which Cornwall is supposed to have undergone. These appear to be referrible to four periods, marked by—

1st. The protrusion of the granite through the stratified rocks, tilting them up at various angles, and injecting granite in the form of veins into the adjacent fissures.

2nd. The formation of porphyritic dykes or *elvan-courses* which traverse both the granite and the slate.

3rd. The production of metalliferous veins, intersecting the granite, slate, and elvans; and,

4th. The introduction of another system of veins, traversing all the preceding formations, locally termed *cross-courses*.

1. What was the condition of the stratified rocks, when the first and most remote movement occurred? Could it have been at the lowest degree of solidification, or indeed, at any degree short of absolute solidity? I think not. 1st, Because, admitting that process to have been "the gradual work of lengthened periods of time," we have a very sufficient limit in the countless ages which must have elapsed between the deposition of the non-fossiliferous strata of Cornwall and the formation of the carboniferous or the saliferous group, whichever may be determined to mark the period when the granite was protruded. 2ndly, Because the nature of the detritus derived from the older strata, and contained in the conglomerates formed before the elevation of the granite, indicate that the parent rocks must have been in a *solid* state. And lastly, because the sharp angular portions of slate, included within the granite veins, demonstrate that they could not have been forcibly detached from rocks only partially *solid*. Thus then we see that even *in limine* Mr. Hopkins has great difficulties to contend with, in refusing to admit the perfect solidity of the inferior disturbed strata; and these must necessarily be increased at each successive step. But before advancing

we must allow the melted granite to cool, and the supposed belt of altered slate to assume its new or superinduced lines of structure. This being accomplished, then follows the—

2nd. Movement, as denoted by the formation of fissures, in which the dykes of elvan now occur, and which run continuously through both granite and slate. We have seen that the latter rock was already solid, and there can be little doubt that the granite was also in a similar state; for very highly inclined fissures, twenty to fifty feet in width, could scarcely have been maintained in an ignited mass, possessed of any degree of viscidity; because, if in any state short of solidity, the incumbent pressure would cause the mobile mass to sink into and obliterate such fissures.

3. The movement which gave origin to the metalliferous veins seems also to have operated on *solid* rocks, since they cut through granite, slate, and elvan which must have been previously in a state to furnish considerable quantities of angular portions of all these rocks with which the veins abound: independently of the fact that this movement was subsequent to former dislocations which, it has been shown, occurred in rocks already solidified.

4. The movement, indicated by the *cross-courses* also containing detached portions of rocks, must likewise have been effected in a solid mass. It is superfluous to make any further remark on this head; but I will here observe, that Mr. Hopkins has fallen into an error in stating that *cross-courses* are universally recognised to be of irregular width, as compared with the "bearing" veins. The fact is, that both systems are exceedingly irregular in this respect; but if any rule obtains, it is the reverse of Mr. Hopkins's statement. Some importance appears to have been attached to this difference in width, but I cannot detect the nature of its bearing.

From these facts* it appears, that in Cornwall the rocks

* In discussing this subject I have endeavoured to keep the argument as simple as possible, and therefore have not dwelt on the phænomena of veins. But as a specimen how the difficulty of Mr. Hopkins's position increases when applied to some only of the details, I may observe that two parallel systems of veins frequently occur inclined towards, and intersecting each other, at great angles; whilst they are both traversed by a third system or cross-courses. All these veins are parallel to, and partially identical with three systems of joints or lines of structure, dividing the mass into concretions which are generally of a rhomboidal form. Here, then, (without complicating the matter still further with joints and veins, which in Cornwall, and probably in other countries, traverse the quadrilateral concretions diagonally,) we have systems of veins crossing each other at acute angles, a condition which Mr. Hopkins has stated cannot have been produced by the elevatory or other extraneous forces, as these, he says, "could only tend to produce systems of fissures crossing each other at right angles."

were *solid previously* to their elevatory movements; and that they also possessed lines of structure is more than probable, since the fragments contained in the granite veins, in the elvans, and in the two systems of mineral veins, exhibit the *same* concretional forms as those into which the corresponding rocks are now divided by weathering or mechanical action.

Thus I have endeavoured to substantiate my former statement, that the elevatory force could not have acted on a *solid* mass without the interference of *lines of structure*; a circumstance which would produce, according to Mr. Hopkins, such considerable modifications in the resulting phænomena, that "to a mass thus constituted these [his physical] investigations must not be considered as generally applicable."

When I commenced this reply, it was my intention to have offered a few remarks on Mr. Hopkins's lengthened comments on the hypothesis which I have advocated concerning the origin of mineral veins in *primary* districts; but as it is immaterial, in the present case, whether the veins, granite, and slate are or are not all contemporaneous, I think it best not to have our attention diverted from the point at issue, which must be determined by facts, and not by the gratuitous *postulata* of either hypothesis.

I cannot, however, conclude without again acknowledging the great obligation geologists are under to Mr. Hopkins for his interesting investigations; and, though differing from him on some points, I am not insensible to the great advantages which must accrue to geology in controlling wild speculations by the application of the rigid laws of physical science.

Penzance, Nov. 8, 1836.

V. *On the true and extended Interpretation of Formulæ in Spherical Trigonometry.* *By* JAMES THOMSON, *LL.D., Professor of Mathematics in the University of Glasgow.**

1. WHILE the rules and theorems given in the modern books on trigonometry, in reference to spherical triangles, are sufficient for all practical purposes, yet there are some peculiarities and some curious relations of such triangles, which have either been overlooked in all the works with which I am acquainted, or have been merely glanced at in casual or passing remarks; and hence, as may be expected, some parts of the theory are still imperfectly developed. I shall proceed†

* Communicated by the Author.

† The formulæ quoted in this paper will be found in my Elements of Plane and Spherical Trigonometry, and in most of the modern treatises on the subject.