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

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XXXVII. *Reply to Dr. Boase's "Remarks on Mr. Hopkins's 'Researches in Physical Geology,'" in the Number for July.*
By W. HOPKINS, Esq., M.A., F.G.S., of St. Peter's College, Cambridge.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I BEG to reply through the medium of your Journal to some remarks by Dr. Boase, contained in your last Number [for July], p. 4, on my memoir on Physical Geology, and to add some additional facts which I have recently had an opportunity of observing.

Dr. Boase appears in the first place to object to the hypothesis on which the whole of my investigations are founded, that of the simultaneous action of an elevatory force on the exterior crust of the globe throughout regions of considerable extent, because he conceives that extensive dislocations produced by such a force would probably be attended with enormous convulsions proportioned to the extent of the rupture. This objection rests entirely on an assumption as to the intensity of the elevatory force. Dr. Boase has not given any reason for supposing that *explosion* must probably accompany dislocation, and without some such reason it is certain that we have no right to make the assumption. It is obvious, in fact, that we can have no means whatever of judging of the intensity of the elevatory force except by the effect produced by it. For anything we can know of its nature independently of inference from observed phænomena, it might be insufficient to produce an earthquake or adequate to produce an almost universal volcano. It may be observed, however, that the extent of simultaneous dislocation would do more than anything else to counteract the explosive tendency of an expansive fluid, because the more extensive the dislocations the more rapidly would the force of expansion be diminished, and the more equable would be the effect on the whole mass. If, on the contrary, a small portion only of the mass should give way, the expansive force would be but little diminished, and its continued action on the yielding part would unquestionably produce much more violent effects on that part than if the mass had yielded generally.

Dr. Boase has rested his objection partly also on the notion, that according to my views the fissures must necessarily begin at the under or *lowest* side of the elevated mass. He will find it carefully stated, however, in my memoir, that they "will

not commence at the surface but at *some lower part* of the mass. If the extensibility of the lower part of it be sufficiently increased by its higher temperature, the fissures will commence at some point between the upper and lower surfaces, and will be propagated both upwards and downwards, and may or may not, according to the degree of extension of the mass, reach either the upper or lower surface. This is a point of little consequence as far as regards Dr. Boase's objection above stated, but it may serve to account for the fact that eruptions of fluid matter in some cases have, and in others have not, accompanied dislocations and elevatory movements.

In my investigations I have spoken of *elevatory* forces, the idea of the higher portions of the earth's crust having been elevated being in general, perhaps, more familiar to us than that of the lower having been depressed. I would here observe, however, that so far as relates to the first production of fissures it is immaterial whether we suppose the mass to be bent upwards by a force beneath, or downwards by its own weight, provided the regions thus subsiding simultaneously be of the same extent as those which I have always spoken of as being simultaneously elevated. The secondary phænomena, however, resulting from the fissures produced by the upward, and by the downward movements respectively, would probably present certain characteristic differences; but I shall not now enter into any further investigation of them.

In the abstract (Art. V.) of my memoir which appeared in your Journal (vol. viii. p. 359), I have taken considerable pains to indicate the possible influence of a jointed structure existing in the elevated mass previously to its elevation, and how it might be ascertained *by observation*, whether or not this influence had been considerable. Dr. Boase, however, is disposed to arrive at the determination of this point by the shorter, but, in such matters, most unsatisfactory road of *a priori* reasoning. He observes: "If then solid rocks have necessarily a jointed structure, one of the data on which Mr. Hopkins's calculations are founded is invalidated, in as much as the elevating force can never have acted on a solid mass without the interference of this modifying circumstance." Now, in the first place I would observe that the process of solidification of all rocks must necessarily have been extremely slow, and that probably, therefore, all the modifications which they have undergone during that process must have been the gradual work of lengthened periods of time. It is impossible, then, to say what period might be necessary for any portion of the earth's crust to arrive at that state of its jointed structure which should produce any decided effect on the directions of its dis-

locations. In my investigations 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. Yet the only force which can possibly attach to the objection above quoted depends entirely on this assumption, which, in fact, involves the very point at issue, viz. whether the jointed structure of disturbed masses has been in great measure superinduced *previously* or *subsequently* to their elevation. It is not, however, by this kind of *à priori* reasoning, founded on what we are altogether ignorant of, that the merits of geological theories can be determined; and to attempt to do so is to depart from those principles of inductive philosophy which alone have enabled man to comprehend with clearness and precision so much that is beautiful and wonderful in the laws of nature. I have elsewhere stated that I have not entered into these discussions in the spirit of advocacy of preconceived opinions; and with respect to the two theories, of which one would assign the directions of dislocation principally to the manner in which the elevating force has acted, and the other to the previously jointed state of the mass, I have endeavoured to act with perfect impartiality. I have indicated how their relative claims may probably be decided *by observation*, by which alone, I assert, these claims can be determined, and not by the kind of reasoning on which both the objections above noticed are founded.

With a view to this determination I have lately made some careful observations in the limestone and gritstone district of Derbyshire. In a particular and thick mass of limestone which pervades the greater part of that mining district, the joints are remarkably well developed. They form two systems at right angles (or very nearly so) to each other, which preserve their directions with remarkable accuracy in every part of the district. The other beds also have their principal joints in the same directions wherever they can be distinctly recognised; and such also is the case with the immense mass of gritstone superincumbent on the shale and limestone. One of these directions is a little west of the magnetic north; the other being consequently a little north of magnetic east, while the directions of all the characteristic dislocations of the district are nearly east and west and north and south, thus deviating from those of the joints by an angle of from 20° to 30° , precisely of that magnitude which is too large to be possibly attributed to any error of observation, and too small to admit

of the formation of the existing dislocations subsequently to the existence of the joints in the perfection in which they are developed at present.

I have also observed another fact in several places both in this limestone district and the neighbouring coal district, of great importance with respect to these theories, viz. that deviation, in several instances, from the regular parallel directions of the longitudinal and transverse fissures which, as I have shown in my memoir, might be expected to accompany certain *partial elevations*, such as I have observed them to be associated with in the districts above mentioned. These facts are strikingly in accordance with the theory I have investigated, and as directly opposed to that which would assign these dislocations to the previous structure of the mass.

These observations agree also with those of Professor Phillips as recently given in his *Geology of Yorkshire*. The principal joints in that district have the same direction as in Derbyshire, while the absence in general of any perfect coincidence of lines of dislocation and of joints, affords conclusive proof that the former cannot have been *principally* determined by the previous structure of the mass*.

Among the *dislocations* of Derbyshire I include the great characteristic mineral veins of the district. They may, in fact, as I have elsewhere stated, be regarded as small faults, and uniformly in the vicinity of larger ones. There are also mineral veins, some of which are as manifestly formed in open joints. These differ from the former in no respect except in being of much smaller dimensions. The *vein-stuff* is perfectly similar, and we are led, I think, almost necessarily, on inspection, to the conclusion that it must have been deposited in the same manner in both classes of veins. Now where veins are formed along lines of faults, it surely seems almost impossible not to conclude that it must have been by some subsequent deposition or segregation along the line of dislocation, and consequently, from what I have above advanced, I consider it highly probable that the veins in open joints were formed there in a similar manner after the formation of the joints had commenced, and probably during their gradual enlargement by the contraction of the mass or other causes. The probability that open fissures once existed in the places where these veins are now formed

* The author of the work above referred to has attributed, in his theoretical speculations, more influence to the jointed structure in determining lines of dislocation, than I conceive the facts alleged by him will justify. At the time that part of his work was written, I am not aware that any other physical cause of the laws of these phenomena had been carefully considered.

seems much increased by the fact that partial cavities are frequently found in them, more particularly where the vein is very wide. Large *pipe-veins** are usually filled with sparry substances amongst which the ore is disseminated; but it frequently happens, I believe, that considerable spaces are still left empty, strongly indicating that such was once the case with the whole space occupied by the vein; and it seems highly probable that the usual vertical veins of the district have been filled in the same manner as the pipe-veins, (whatever that process may have been,) because veins of the former kind always communicate with these latter (of which the miner considers them the *feeders*); and I am not aware that any characteristic difference has been observed between the substances which occupy these two descriptions of veins.

Of the formation of the ore of a mineral vein I pretend to offer no conjecture. It seems to present an equal chemical difficulty whatever theory we adopt; but that some process of infiltration might be sufficient for the supply of the vein-stuff is indicated, I think, by the great masses of stalactitic formations met with in some cases, and their comparatively rapid formation in others. The toadstone of Derbyshire also contains in many places numerous veins and insulated small globular portions of calcareous matter to the depth of perhaps 200 feet, the formation of which it seems almost impossible to conceive except by infiltration. To a similar process too may be ascribed, I conceive, the existence in the interior hollows of fossil shells of crystalline substances differing as much from the mass in which the shells are imbedded, as vein-stuff frequently differs from the containing rock. These and similar facts have appeared to me to prove at least the adequacy of the cause assigned to produce the effects which I am at present disposed to attribute to it. I pretend not to offer an opinion as to whether at some antecedent geological period any solvent more powerful than water may have assisted in the process of infiltration. It may, perhaps, not be deemed very impossible†.

[To be continued.]

* Pipe-veins are spaces (frequently of considerable extent) usually existing between the beds of limestone, and filled as mentioned above. The ore in these, as in the vertical or *rake vein*, often bears an extremely small proportion to the other substances occupying the vein.

† I am glad to find, from the note in Dr. Boase's communication (p. 9) that Mr. Fox has suggested this same notion about the formation of veins, because he has probably derived it from observation of the Cornish veins, and his opinion is likely to have (and most deservedly) far greater weight with Cornish geologists, than any views emanating from myself.