

THE  
GEOLOGICAL MAGAZINE.

NEW SERIES. DECADE IV. VOL. I.

No. III.—MARCH, 1894.

ORIGINAL ARTICLES.

I.—FOUR THEORIES OF THE AGE AND ORIGIN OF THE DARTMOOR GRANITES.

By A. R. HUNT, M.A.

FOUR Theories of the Age and Origin of the Dartmoor Granites have been lately current, viz. :—

- (1) The ordinary Plutonic of Sir H. de la Beche.
- (2) The Laccolitic-Plutonic of Mr. W. A. E. Ussher.<sup>1</sup>
- (3) The Volcanic of Mr. R. N. Worth.<sup>2</sup>

The above all assume the exclusively post-Carboniferous age of the granite, owing to its intrusion into the adjacent Carboniferous rocks; one of the best attested facts in geology.

(4) The combined pre-Devonian non-intrusive and post-Carboniferous intrusive (both plutonic), advanced by myself in 1889.<sup>3</sup>

Mr. Ussher has recently withdrawn his provisional<sup>4</sup> laccolitic hypothesis, on stratigraphical grounds, in favour of one which is practically equivalent to No. 4.<sup>5</sup>

As it is impossible to compress the work of some four years into a magazine article, the present paper must be confined to a sketch of my main argument.

My friend Mr. Worth has, with characteristic generosity, lent me the diagrammatic woodcut illustrating his ideal volcano; accompanied by the following remark: "Please do not limit your views of me; you owe it to yourself to make your case as strong as possible, and we shall not differ outside our theories." A too generous opponent indeed, and one hard to oppose.

The following woodcut was published to illustrate Mr. Worth's paper, "The Dartmoor Volcano," Trans. Plymouth Inst. 1888-89.

Mr. Worth brought his theory in the plainest language before the Geological Society in 1889, when it was discussed by our leading petrologists, apparently accepted by Prof. Bonney and Mr. Hudleston, and not objected to on principle by anyone.<sup>6</sup>

*Primâ facie*, however, there seems a serious objection to Mr. Worth's volcanic hypothesis, as it involves the "Elevation-crater theory" pure and simple.<sup>7</sup>

<sup>1</sup> Trans. Devon. Assoc. vol. xx. p. 154.

<sup>2</sup> "The Dartmoor Volcano," Trans. Plymouth Institution, etc., 1888-89.

<sup>3</sup> Trans. Devon. Assoc. vol. xxi. p. 238. <sup>4</sup> Trans. Devon. Assoc. vol. xx. p. 156.

<sup>5</sup> Proc. Som. Archæological Soc. vol. xxxviii. p. 204.

<sup>6</sup> Q.J.G.S. vol. xlvi. pp. 80-82.

<sup>7</sup> See Geikie, Text Book of Geology, pp. 241, 242.

[The publication of this article has been unavoidably delayed.—EDIT. GEOL. MAG.]

DECADE IV.—VOL. I.—NO. III.

I would venture to submit this preliminary point to geologists before making any attempt to discuss the volcanic theory of Dartmoor in detail.

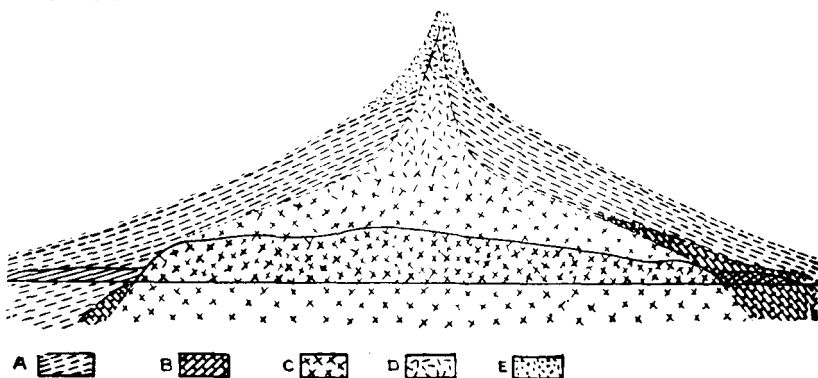


FIG. 1. "DIAGRAMMATIC RECONSTRUCTION OF DARTMOOR."

It must be understood that this sketch is diagrammatic and has no pretensions to scale, and that it is mainly suggestive and purposely made as simple in its conditions as possible. All below the horizontal line, which represents datum, is purely hypothetical; and all above the curved line, which indicates generally the present surface contour of the Moor and bordering rocks from north to south. This superstructure has been removed. A, represents Carboniferous rocks; B, Devonian; C, Granite; D, Felsite; E, Volcanic Material and Ejectamenta. The wedge-like intrusion of the granite has tilted and broken through the upper or Carboniferous rocks, and has thrust as well as heaved the lower or Devonian."

My own pre-Devonian hypothesis was first advanced at the Tavistock meeting of the Devonshire Association; on which occasion the following epitome of my paper, with the valuable comments of the President, Mr. W. H. Hudleston, F.R.S., appeared in the "Western Morning News." As reporters could have made nothing of so technical a subject, I conclude that the President's remarks were personally revised for the Press, as was my own abstract.

*"The Age of the Granites of Dartmoor and the English Channel.*

Mr. A. R. Hunt read a paper on the Relation of the Granites of Dartmoor to the Granites of the English Channel. The author drew attention to the following points of difference between the granites of Dartmoor and those trawled in the English Channel:—The Dartmoor granites were characterized by fracture, by the frequent presence of tourmaline, and were not gneissose. The Channel granites indicated compression, were sometimes hornblendic, and were associated with gneisses. The Dartmoor granites were intersected by injected and infiltrated veins. No veinstones (though such were very durable) had been forthcoming from the Channel. The quartzes in the Dartmoor granites and in both classes of veins contained fluid inclusions of brine, with cubic crystals of salt. No salt had been detected in the Channel granites. Chistolite occurred on the borders of Dartmoor, kyanite in the Bolt schists bordering the

Channel<sup>1</sup>—chemically identical, they differed only in specific gravity, the heavier mineral indicating the greatest compression. From these facts the author argued that the Channel and Dartmoor granites were both originally of pre-Devonian and probably of Archæan age. That after the deposition of the Devonian and Carboniferous rocks, the Dartmoor area was fissured by the same earth-movements which compressed the Channel area. The sea obtained no access to the Channel granite, but penetrated the Dartmoor area. The crushing of the lower rocks supplied the heat, whilst the sea supplied the water, which under high pressure dissolved the Archæan granite to be reconsolidated with salt here and there caught up in its quartzes. The reconstituted granites traversing the culm slates were truly post-Carboniferous; but the mass of Dartmoor is Archæan, occasionally altered by super-heated brine accompanied by boracic acid.

"The President said it was unquestionable that a very ingenious theory had been propounded by Mr. Hunt, although very different to that which had been hitherto held by most geologists. Without going very closely into the question, or dealing otherwise than superficially with the paper, he thought it was perfectly clear that the Dartmoor granite and the Channel granite were of quite a different age, and had an entirely different origin. To that extent he was in conformity with the views of the author. But the granites were quite different mineralogically. There was one very great difference, namely, that the Channel granites were not metalliferous in the way that the Dartmoor granites were. There were no granites in the British islands which were so metalliferous as the Devon and Cornwall granites, and it appeared to him that they had a very different origin to other granites. For that simple reason, as regarded the age of the Dartmoor granite, it appeared to him that the usual view that it was post-Carboniferous was the one most in accordance with the facts of the case, as far as he had been able to judge. But when they came to the differences of the mineral composition of these granites, he thought they had further proof that the alteration of Dartmoor granite had been produced subsequently by the numerous fissures formed. These fissures had been more or less injected by corroding aqueous vapours containing large quantities of chlorides and fluorides of heavy metals. These crystals of salt, which Mr. Hunt had found in such considerable quantity, he thought (it) excessively probable had been formed from hydrochloric acid gas acting on the soda in the rocks. That appeared a very much more reasonable explanation of the origin than that they came from the sea, or had anything to do with the sea whatever."

The question as to the marine or plutonic origin of the brine-inclusions referred to is of transcendent importance in connection with the question as to whether the water ejected by volcanoes is of meteoric or marine origin, or derived from the interior magma.

Hypotheses that might meet the case of pure water might well fail to explain the presence of the chlorides of sodium and potassium.

Having carefully examined the rocks in the light of Mr. Hudleston's

<sup>1</sup> Note. Withdrawn, *GEOL. MAG.* 1892, p. 291.

suggestion, I submitted the question to the British Association at Leeds. The following extract is from the published abstract of my paper:—

"The theory of the marine origin of the saline inclusions in the Dartmoor rocks seems to harmonize well with the view commonly entertained that the chlorine and chloride of sodium emitted by volcanoes are derived from the sea.<sup>1</sup>

"In the case of volcanoes the presence of hydrogen and chlorine may be accounted for by the dissociation of the water and of the chloride of sodium by the intense heat, and the combination of the two gases thus formed would result in the production of hydrochloric acid.

"In the case of the cooler granites there is no question of dissociation and of gases, but of the entanglement of brine and steam at more moderate temperatures.

"Thus the access of salt water to highly heated rocks seems to account for some of the more important gases emitted by lavas and of the more characteristic fluid inclusions caught up by granites.

"An alternative theory, that the crystals of salt in the Dartmoor rocks 'had been formed from hydrochloric acid gas acting on the soda in the rocks,' does not seem to the author to account for the crystals in the quartz-veins of the culm slates, or to explain the complete permeation of the granite by the chloride of sodium. Moreover, the one theory accounts for the presence and origin of the hydrochloric acid as well as of the soda, whereas the other has to assume the previous existence of soda and the advent of hydrochloric acid from unknown quarters."<sup>2</sup>

The very title of this paper was omitted from the newspaper reports, it being dismissed as being merely of local interest!

Mr. Ussher's last paper referring to Dartmoor appeared in 1892, and General Mc'Mahon's in 1893. In the discussion which ensued on the reading of the latter, Professor Bonney is reported to have said that "In his opinion Mr. Ussher's theory was quite untenable. If the fusion of a peripheral portion of the Dartmoor mass was due to crushing . . . Was there any evidence that a rock could be fused by pressure alone, any more than by a gentle stewing in sea-water which also had been suggested? . . . No good was done for science by proposing hypotheses which, in avoiding one difficulty, raised a number of others far more formidable."<sup>3</sup> On the same occasion General Mc'Mahon said that "the word used by Mr. Ussher was 'fusion,' and it was applied to the results of the N. and S. squeeze on the rigid and obstructing pre-Devonian rocks."<sup>4</sup>

There seems great misapprehension here. So far from Mr. Ussher attributing fusion of the Dartmoor granite to "pressure" or "crushing," or to a "N. and S. squeeze," he seems never even to have used the terms "pressure" or "crushing" in reference to the granite, and his only suggestion of a possible source of heat appears to be "a rise of the isogeotherms or plutonic action" (a quotation from

<sup>1</sup> See *Characteristics of Volcanoes*, J. D. Dana, p. 8.

<sup>2</sup> Report Brit. Assoc. 1890, p. 815. <sup>3</sup> Q.J.G.S. vol. xlix. p. 397. <sup>4</sup> *Loc. cit.* p. 397.

my own paper). He also states that "the metamorphism (in the granites) was produced *after*, and perhaps as a new phase in, the great dynamic movements to which the contortion and cleavage of the Palæozoic rocks are due."<sup>1</sup> And in another place we learn that the fusion of the obstructive masses took place *after* the mechanical effects produced by their obstruction had attained their maxima.<sup>2</sup>

So far as I can gather from a careful study of Mr. Ussher's and General McMahon's papers, Mr. Ussher's almost incidental references to the behaviour of the granite itself (for his main point is the sedimentary rocks) have been misapprehended.

It would almost appear, however, that Prof. Bonney's questions, whilst missing their mark in Mr. Ussher, force me to take the defensive, owing to my having written—"The crushing of the lower rocks supplied the heat."<sup>3</sup> This hypothesis, whether tenable or not, is fairly orthodox, for, assuming that "pressure" is used here for friction or crushing (the results of pressure), we may turn to Mallet for experiment, and to Callaway for observation. Mr. R. Mallet, F.R.S., has the following passage in the Quarterly Journal of the Geological Society:—"The writer has thus shown that crushing alone of rocky masses beneath our earth's crust may be sufficient to produce fusion."<sup>4</sup> Describing a particular rock, Dr. Callaway writes in the same publication:—"During the metamorphism partial fusion, resulting in plasticity, took place. This effect is found so often to occur where the shearing is at its maximum, while the adjacent rock is merely a crushed solid, that the generation of heat by the shearing-process becomes a probable inference."<sup>5</sup>

It may be as well to point out that I never suggested a crushing of any portion of the Dartmoor granites that has ever been seen by man. As already stated, the Dartmoor granites as exposed to view are "characterized by fracture,"<sup>6</sup> and instead of indications of pressure there is every sign of relief of pressure. Felspars and quartzes are occasionally split by divisional planes and re-cemented; but I know of no instance even of dislocation, much less of crushing or shearing, in the main mass of the granite. However, there is nothing to prevent the upper crust having been in a state of strain, while lower horizons were under stress, so, in these latter, dynamic action may well have supplemented the earth's heat in producing the comparative moderate temperatures indicated by the granite. With respect to the "fusion . . . by a gentle stewing in sea-water" referred to by Professor Bonney, it is possible my critic may have misunderstood the following sentence, viz. "altered . . . by moderate heat in the presence of salt water."<sup>7</sup> By moderate heat I merely meant a temperature not much, if at all, exceeding the critical temperature of water, and the term was used relatively to volcanic temperatures, which latter seem inadmissible if only because they would dissociate the elements in the chloride inclusions.

<sup>1</sup> Proc. Somerset Archæological Soc. vol. xxxviii. p. 217.

<sup>2</sup> *Loc. cit.* p. 208.

<sup>3</sup> See *ante*, p. 99.

<sup>4</sup> Q.J.G.S. vol. xxxi, p. 517.

<sup>5</sup> Q.J.G.S. vol. xlix, p. 422.

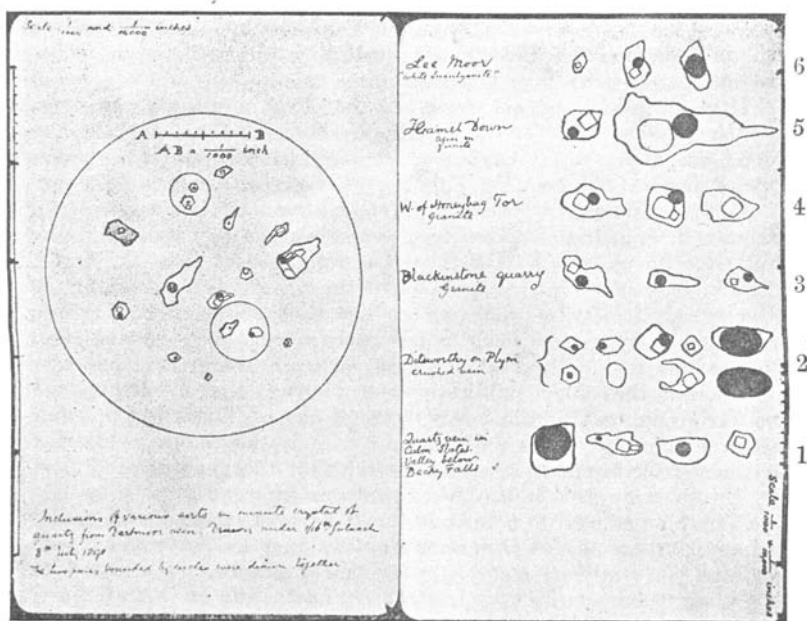
<sup>6</sup> See *ante*, p. 98.

<sup>7</sup> Report Brit. Assoc. 1889, p. 570.

The particular point I desire to emphasize on the present occasion is the intimate association in the Dartmoor area of fluid inclusions, containing cubic and other crystals, with fluid inclusions consisting apparently of plain water.

FIG. II.

FIG. III.



In a slice of a veinstone from near Manaton there is an hexagonal crystal of quartz about  $\frac{3}{10}$  inch in diameter. The gradual growth of this crystal is defined by tourmaline and tourmaline microliths.<sup>1</sup> It is crowded with inclusions, both irregular and with crystalline outlines (*i.e.* negative crystals). We can see that it has never been viscid, plastic, or colloidal, or anything else than a mineral gradually crystallized out from solution.

Within this crystal, and not more than  $\frac{1}{1000}$  inch apart, there are two inclusions, both of them negative hexagons, with small bubbles; but one contains a cube, and the other contains none. There is also a negative hexagon with a relatively large bubble,  $\frac{1}{1000}$  inch distant from an irregular inclusion with a very minute bubble.

Among the liquid inclusions, as figured above (Fig. II.), there is a tourmaline microlith which seems to have caught up a cube of some chloride during crystallization. The inclusions drawn are selected, and not in their relative positions, excepting the two couples within small circles.

<sup>1</sup> See Trans. Devon. Assoc. vol. xxi. p. 261, plate, fig. 3.



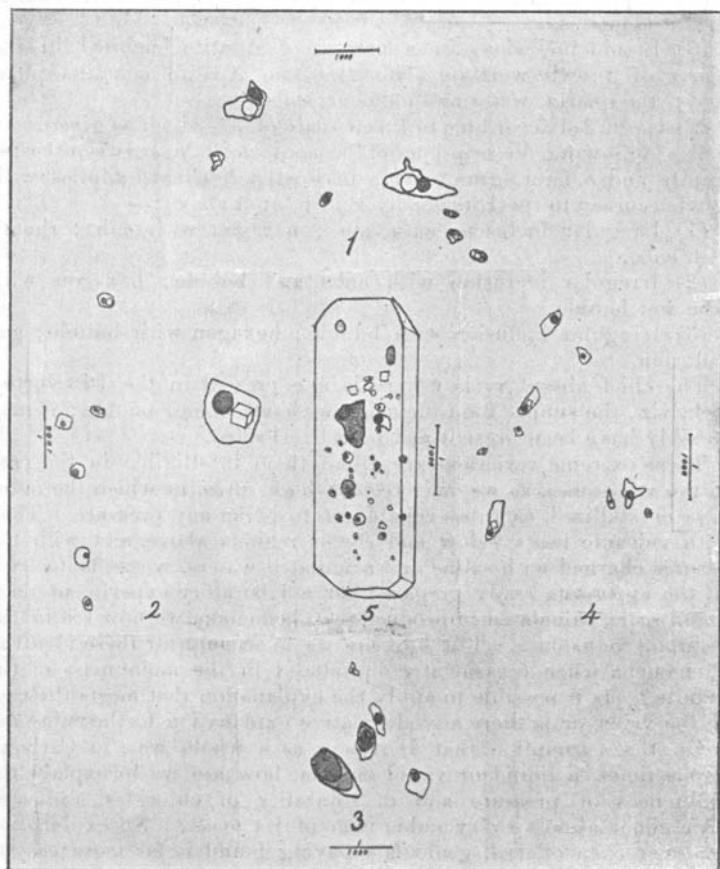
On the second cut (Fig. III.) are drawings selected from six specimens, of which the five lower ones deserve special notice, viz., commencing with the lowest:—

(1) Quartz vein in Culm slates, valley below Becky Falls.

(2) "Ditsworthy—vein is intrusive in a tongue of slate which extends into the granite across the Plym valley from Ringmoor—a very narrow strip." (Worth.)

Quartz, decomposed felspar, white mica, brown tourmaline decomposing into green. A very remarkable rock, with inclusions (as shown above) varying to the utmost extreme.

FIG. IV.



(3) Blackinstone Quarry, and (4) Honeybag Tor, ordinary granites.

(5) Hamel Down, quartz-felspar-tourmaline vein; negative crystal apparently based on the rhomboidal prism.

It will be observed that the same type of liquid inclusions, in similar variety, can be traced from the simple quartz vein, through various injected or infiltrated compound veins, to the ordinary granite of Dartmoor.

Description of Fig. IV.:—

- (1) Liquid inclusions in a quartz-vein in Culm slate in the valley below Houndtor Wood, south of the Becka brook.
- (2) Liquid inclusions in the quartz of Trowlesworthite.
- (3) Inclusions with one and two liquids in a granite (No. 20) trawled in English Channel (for comparison).
- (4) Liquid inclusions from normal porphyritic Dartmoor granite, Heytor Quarries.
- (5) Liquid inclusions in a crystal of Apatite enclosed in the quartz of Trowlesworthite (No. 2). The Apatite contains plain fluid; the quartz, water and cubic crystals.

All magnified according to linear scale of  $\frac{1}{1000}$  inch as given.

The following description of a contact slide between coarse granite and a micro-granite vein in Lustleigh Cleave applies with equal accuracy to the Ditsworthy vein figured above:—

- (1) Irregular inclusion with cube; hexagon with cube; rhomb with cube.
- (2) Irregular inclusion with cube and bubble; hexagon with cube and bubble.
- (3) Irregular inclusion with bubble; hexagon with bubble; gas inclusion.

The chief absent variety here is one present in the Ditsworthy rock, viz., the simple fluid inclusion without cube or bubble; it may possibly have been present and been overlooked.

These extreme variations are all of them intelligible in the case of the veinstones, as we may regard the fissures, in which the latter were crystallized, as tubes capable of resisting any pressure. Then with volcanic heats below and cooler regions above, and with the fissures charged with saline and acidulated waters, we seem to have all the apparatus ready prepared for a laboratory experiment on a grand scale, calculated to produce the phenomena we now see in the resultant veinstones. But how are we to account for these identical phenomena when occasionally reproduced in the main mass of the granite? Is it possible to apply the explanation that meets the case of the veins, or is there any alternative explanation forthcoming?

On the assumption that Dartmoor as a whole was, in Carboniferous times, a liquid or viscid magma, how are we to explain the differences of pressure and of solubility of chlorides, indicated throughout almost every cubic inch of its mass? No explanation has ever been offered, geologists having found it far more easy to minimise the importance of the facts than to account for them.

It is usually admitted that the tourmaline of Dartmoor is commonly a secondary mineral; or, as its presence implies the introduction of two new elements into the granite (fluorine and boron), it is perhaps more accurately described as an imported mineral. Assuming the



existence of an ancient granite, a sufficient explanation suggests itself, both of the presence of the imported tourmaline and of the imported chlorides. If a piece of granite be heated in an ordinary fire, even to a bright red heat, and cooled, its general appearance is not much affected; it is still rigid enough to be sliced for the microscope, but differential expansions in its dissimilar minerals have rendered it permeable by fluids, as can be readily tested by a dye, such as aniline. Now, if a deep-seated granite were exposed to alterations in temperature sufficient to set up such differential expansions, the most compact rock would be minutely cracked throughout, and be rendered permeable by liquids and gases, especially under pressure. The doctrine of the rise and fall of the earth's internal heat through the crust, by the deposition and denudation of sediments, has long been accepted by geologists. All we require is an application of this doctrine to Dartmoor, so that our granite, before or during Carboniferous times, should have suffered sufficient alterations of temperature to render it porous, and so lay it open to the invasion of solvent liquids or gases. Indeed, many granites are so rotten and decomposed, from whatever cause, that they suck up fluids like a sponge.

Now a porous granite being granted, there is little further difficulty; for although some modern geologists decline to entertain the hypothesis that sea-water could gain access to heated granite and set up chemical changes therein, the weight of the evidence is against them. Years before a section was cut, or the chlorides in liquid inclusions discovered by Dr. Sorby, the almost omniscient De la Beche, with well nigh incredible foresight, pointed out the probability of saturated solutions of salt being accumulated in granite, by sea-water obtaining access to the rock when highly heated.<sup>1</sup> De la Beche also pointed out the important chemical effects which the various salts dissolved in sea-water might be expected to have on the granite itself.<sup>2</sup>

Given a porous granite, covered by sedimentary rocks thick enough to bring the internal heats within reach of a superincumbent ocean, and we have all the machinery requisite for the solution of quartz<sup>3</sup> and silicates; and, with the addition of fluorine and boron, for their redeposition in a variety of forms such as quartz, tourmaline, topaz, fluor spar, and triclinic feldspars; all of which occur as introduced minerals in the west country granites.

The late Mr. J. A. Phillips has recorded the occurrence of saline water in Huel Clifford mine, 1320 feet below the sea-level.<sup>4</sup> Prof. Le Conte has calculated that at a depth of 10,000 feet below the surface the temperature would be 230 F.,<sup>5</sup> a very moderate depth in terms of ocean soundings, being 1666 fathoms. But even 230 F. is a temperature not to be neglected, seeing that the glass tubes of the water gauges of ordinary locomotives become corroded.

<sup>1</sup> Report on the Geology of Cornwall, Devon, etc., p. 378.      <sup>2</sup> *Loc. cit.* p. 387.

<sup>3</sup> Mr. J. B. Hannay has kindly informed me that he has dissolved quartz in superheated steam and produced transparent quartz crystals thereby.

<sup>4</sup> Phil. Mag. 1873 (2), p. 32.

<sup>5</sup> *Loc. cit.* p. 45.

However, in the case of Dartmoor, there is no necessity to limit the temperature to that of the isogeotherm of the granite itself in post-Carboniferous times; as, on our hypothesis of the rock being permeable by liquids, the heated gases and liquids may have been derived from much lower levels.

But to return to the evidence of the associated saline and fresh-water inclusions: these seem to indicate a consolidated granite, subsequently heated, cracked, and permeated by liquids. The question at once arises whether this granite, the original granite of Dartmoor, first consolidated before or after the deposition of the adjacent Devonian and Carboniferous rocks? If after their deposition, it is inexplicable how a mass of heated, deep-seated granite, supposed by many to be even volcanic in character, twenty miles in diameter, and which as such must have taken centuries to cool, could have had so little effect on the adjacent sedimentaries as to fail in every known instance to obliterate the exact point of contact. If before their deposition, then the granite is pre-Devonian, which is the point of my argument.

It seems likely enough that a partial aqueo-igneous solution of the ancient Dartmoor granite, accompanied by earth movements in post-Carboniferous times, would have resulted in the injection of the dissolved material into fissures of all kinds, and especially into the main lines of weakness between the granite and the adjacent sedimentaries. Thus all the local contact-alterations, so insufficient if attributed to the action of the main mass of the granite on its primary consolidation, would find an adequate cause in the contact of the newer intrusive or re-constituted granites, which, so far as I am aware, never occur in any considerable mass relatively to the whole crystalline area.

It would greatly facilitate further research in the Dartmoor area, if the following three points could be definitely decided:—

- (1) Whether the volcanic hypothesis is tenable?
- (2) Whether the chlorides in the quartzes are of marine or plutonic derivation?
- (3) An explanation of the immediate juxtaposition of brine and fresh-water inclusions.

It is to be regretted that the problems connected with fluid inclusions excite at the present time so little interest, even when not treated with absolute contempt. Some of them are perplexing enough. Take for example Trowlesworthite.<sup>1</sup> In my slide of this rock a certain crystal of Apatite (identified by Mr. Harker) contains liquid inclusions with small and active bubbles and with no indication of chlorides; whereas the quartz contains some inclusions with salts and others apparently without. This rock thus presents even greater complications than the granite mentioned by Dr. Sorby in which the felspars contained fresh water and the quartzes brine.<sup>2</sup>

At the time that Dr. Sorby delivered his Presidential Address to Section C. at Swansea, it was generally supposed that while super-

<sup>1</sup> See Worth, *Trans. Dev. Assoc.* vol. xix, p. 494.

<sup>2</sup> *Rep. Brit. Assoc.* 1880, p. 670.

heated water dissolved solids, superheated steam above the critical temperature did not do so. The brine and freshwater inclusions were therefore explicable on the hypothesis that the feldspars caught up compressed non-solvent steam above the critical temperature, and that the quartzes caught up solvent water under that temperature. Of late years, however, experimentalists seem agreed that compressed gas above the critical temperature has even greater solvent properties than the superheated liquid under that temperature. Thus the hypothesis that the purity of the water in different minerals may be owing to the inclusion of highly compressed non-solvent steam above the critical point seems to break down. It seems possible that certain experiments made by Mr. J. B. Hannay may clear up this particular difficulty. According to Mr. Hannay,

(1) "Gas must have a certain density before it will act as a solvent, and when its volume is increased more than twice its liquid volume its solvent action is almost destroyed" (Proc. Roy. Soc. vol. xxx. p. 486).

(2) "Retaining the volume the same, the higher the temperature the greater the solvent power" (Proc. Roy. Soc. vol. xxx. p. 486).

(3) "The vaporous state can be clearly defined as a distinct state of matter."

"A vapour over a liquid holding a coloured solid in solution is colourless, but on passing the critical temperature the whole becomes coloured" (Proc. Roy. Soc. vol. xxxiii. p. 321).

Thus, according to Mr. Hannay, superheated water under the critical point is a solvent liquid. Superheated steam under the critical point is a non-solvent vapour. Superheated steam over the critical point is a gas, and, if compressed to two volumes of the liquid and less, is a solvent.

In the case of the feldspar and apatite above referred to, if these minerals crystallized below the critical temperature, but under pressure not sufficient to compress the vapour into liquid, they would enclose compressed non-solvent vapour, which would ultimately condense into fresh water. Another explanation of these minerals enclosing plain water, even above the critical point, would be the absence of chlorides, or of soluble minerals alien to the growing crystals, during the process of crystallization.

Liquid inclusions of the normal type in granitoid rocks contain plain water with mobile, and sometimes very active, bubbles. Inclusion of salts are exceptional. The presence of fresh water during crystallization of the quartz would account for the one sort; and the presence of sea water would account for the other. The difficulty lies in the rapid alterations from fresh water to salt, and *vice versa*.

The analogy of the marine boiler and engine, with condensation of fresh water in the condenser, and possibly in the cylinders, and with accumulation of brine in the boiler, suggests a possible explanation of these complicated inclusions, viz. heat acting on salt water under pressure producing rapid alternations of brine, steam, and fresh water, during the crystallization of the minerals concerned. It may be noticed that the variations are often so great, and the

alternations apparently so rapid, within very minute areas, say  $\frac{1}{1000}$  of an inch, that the hypothesis of any general fluidity or viscosity of the rock-masses in which such inclusions occur is very difficult to reconcile with the phenomena observed.

As exception has been taken in the columns of the *GEOLOGICAL MAGAZINE* to my attempting problems beyond my powers, I may mention that I have repeatedly thrown this Dartmoor question aside as too overwhelmingly difficult; on the other hand it has seemed a pity that observations which may be of use to other workers should be entirely lost.

It may also be borne in mind that the last words written hitherto by Dr. Sorby on the subject of inclusions and crystalline rocks, are the significant ones, as true now as when they were addressed to the Geological Section of the British Association in 1880—"There is still much to be learned respecting the exact conditions under which some of our commonest rocks were formed."

POSTSCRIPT.—Fig. 4 represents five distinct groups of inclusions, drawn separately with the camera lucida, the inclusions of each group being in their relative positions. The five drawings for the purpose of the process block were marshalled on a sheet of white paper and reduced by photography.

## II.—WOODWARDIAN MUSEUM NOTES.

### CERTAIN FOSSILS FROM THE LOWER PALÆOZOIC ROCKS OF YORKSHIRE.

By SIDNEY H. REYNOLDS, M.A., F.G.S.

(PLATE IV.)

I UNDERTOOK this description of certain fossils from the Lower Palæozoic rocks of Yorkshire at the request of Mr. Marr. The late Mr. Thomas Roberts had begun the piece of work, but left it unfinished. Nearly all the fossils referred to were collected by a party of Cambridge geologists, under the guidance of Prof. Hughes, in the Summer of 1889. The fossils come from two horizons, viz. :—

1. The *Phacops elegans* zone=Stockdale Shale series (Valentian) of Wharfe.
2. The Bala beds=Coniston Limestone series of Norber Brow and Wharfe.

Mr. Marr (*GEOL. MAG.*, Dec. III. Vol. IV. No. 1, 1887) records the following species from the Stockdale Shale series of Wharfe :—

*Petraia*, sp.  
*Phacops elegans*, Boech and Sars.  
*Cheirurus bimucronatus*, Murch.  
*Encrinurus punctatus*, Emmr. ?  
*Leptæna quinqucostata*, McCoy.

To these may be added :—

*Illænus*, sp.  
*Encrinurus punctatus*. var. *arenaceus*, Salt.  
*Harpes judex*, Marr and Nich.  
*Staurocephalus*, cf. *Murchisoni*, Barr.  
*Cyphaspis*, cf. *rastritum*, Törnq.  
*Cyphaspis*, sp., cf. *Burmeisteri*, Barr.