1 inch thick. *Ceratiocaris Ludensis*, H. W. (GEOL. MAG. Vol. VIII. 1871, 104, Pl. 3, Fig. 3, $\frac{1}{3}$ rd of natural size), is the only British form at all approaching the North-American specimen. Of this there are seven body-segments and three long tapering spines (one nearly 9 inches long). The seven segments measure $8\frac{1}{2}$ inches in length, and nearly 2 inches in breadth (height) as lying flattened sideways in the slab; six of the segments measure individually from $\frac{1}{2}$ and $\frac{3}{4}$ inch to 1 inch in length, and the last (telson?) is $2\frac{3}{4}$ inches long. The published figure gives a very inadequate idea of this fine specimen (its carapace may have been 8 inches long): and on a visit to the Ludlow Museum lately one of us (T. R. Jones) thought that there are remnants of bases of prickles on the last two segments, roughly indicated on the figure referred to. The specimen will soon, we trust, receive all the attention of artist and palæontologist which it deserves.

Some large specimens have been met with in North America also; for example, *C. Deweyi*, Hall, "Palæont. New York," vol. ii. p. 320, pl. 71: the one segment drawn (fig. 1c) appears to have been $2\frac{1}{2}$ inches long; and one of the caudal spines on the same plate (fig. 1a) is $6\frac{1}{2}$ inches long.

A tail-spine from Bohemia, in the British Museum, is $5\frac{1}{4}$ inches long.

Tail-spines of *Ceratiocaris*, 3 inches long, have been not unfrequently found in the Lower Ludlow of Leintwardine, near Ludlow.

EXPLANATION OF PLATE XIII.

- FIG. 1. *Echinocaris Wrightiana* (Dawson sp.). Portions of abdomen (natural size).
 ,, 1a. Two body-segments seen from back or narrow aspect.
 ,, 1b. Another view of the lower (or posterior) of the two body-segments (seen in Fig. 1a) viewed from the left side.

From the Portage Group (Upper Erian) of Italy, New York. Collected by B. H. Wright, Esq.

- ,, 2. Three body-segments and telson, with one style and two stylets, of *Echinocaris armata* (Hall). Natural size. From Devonian strata of the Hamilton Group, State of New York. (After J. Hall.)
 ,, 3, 4, 5. *Echinocaris sublaevis*, Whitfield, from small calcareous concretions in the Erie Shales (Upper Devonian: Portage and Chemung Groups), at Leroy, Lake County, Ohio. (After Whitfield.)
 ,, 3. The half of a small nodule, showing some abdominal segments and the inside of a valve. Natural size.
 ,, 4. The four body-segments and telson with its spines seen in the nodule. Nearly twice the natural size.
 ,, 5. A left valve. Twice the natural size.
 ,, 6. *Echinocaris pustulosa*, Whitfield. A right valve; twice the natural size. From the Upper-Devonian nodules of Leroy, as above.

III.—ON CLEAVAGE AND DISTORTION.

By the Rev. O. FISHER, M.A., F.G.S.

(Continued from p. 276.)

PART V.

THE history of opinion upon the subject of cleavage seems to be this. Sedgwick had come to the conclusion that cleavage was a condition, "impressed on certain rocks and in certain regions by

the operation of some very extensive cause, after the stratified rocks had undergone great displacement,"¹ and he suggested that the cause might be some kind of crystallization. But Dr. Sorby discovered that the intimate structure to which cleavage is due is not crystallization, but a mechanical rearrangement of the particles of the rock: and he produced it artificially by squeezing clay containing flaky particles. Dr. Tyndall did the same thing with different substances, without introducing flaky particles. In either case it was by pressure that the result was obtained. Seeing therefore that pressure could succeed in laboratory experiments, and that cleavage in the field was ordinarily nearly at right angles to the direction in which the action of pressure might have folded the strata, it was natural to conclude that both folds and cleavage had been concomitant effects of such a pressure. And this appears to be the theory now generally adopted; and is of course in opposition to both of Sedgwick's conclusions. For my part I believe that Sedgwick was right in thinking that cleavage was produced by some very extensive cause operating *after* the rocks had undergone great displacement; while I agree with Sorby and Tyndall in attributing it to a mechanical action, accompanied with pressure. Nevertheless I do not think that this pressure was the same exhibition of force, that originally folded and elevated the rocks, as explained in Part IV.² Thus I am in accord with each of these authors in the domain in which his authority stands highest.

I believe that the first occasion when a doubt occurred to me regarding the usually received view upon cleavage was when I recorded the following entry in my note-book in 1867, being in company at the time with Mr. Pengelly.³

Although no author has, so far as I know, used the comparatively

¹ Phillips' Report on Cleavage, 1856, Brit. Assoc. Reports, 1857, p. 373.

² GEOLOGICAL MAGAZINE, 1884, p. 271.

³ Since sending my MS. to the Editor, I have enquired of Mr. Pengelly, "What is the relation of the cleavage to the unconformity?" His reply is as follows:—

"The cleavage in the section at Hope's Nose affects both sets of rocks, and it is not diverted by the unconformity. The following is briefly the reading of the section:—

1st. The formation of the lower series by the deposition of triturated organic exuviae; the process being frequently intermitted, as is proved by the interstratification of volcanic ash.

2nd. By the operation of some approximately lateral force, the beds were contorted, and were fractured at the point of greatest flexure.

3rd. The uppermost beds of this lower series were exposed to the action of the waves, and planed down to an approximately horizontal surface.

4th. After this denudation, the beds of the upper series were deposited unconformably on the lower, and are now nearly horizontal.

5th. Though these processes must have absorbed a large amount of time, they were all completed within one and the same division of the Devonian period, as is proved by the specific identity of the numerous fossils in the two series.

6th. After the deposition of the upper series, cleavage was set up in the entire mass.

7th. Assuming that cleavage is due to pressure, there are conditions under which pressure can produce contortion without cleavage, and conditions under which it can produce cleavage without contortion."

modern term "shear" in connection with cleavage, still that mode of action is necessarily implied in any mechanical theory. Thus Phillips wrote of "a creeping movement of the particles of the rock along the planes of cleavage, the effect of which was to roll them

FIG. 1.

DIAGRAMMATIC SECTION AT HOPE'S NOSE, NEAR TORQUAY.

Thirty feet thickness seen. The same cleavage runs through the whole series in spite of the unconformity, showing that it was not produced by the pressure which caused the folds.

forward in a direction always uniform over the same tract of country."¹ And Sir John Herschell says that, "Cleavage *does not absolutely require violent force*, but only intermolecular movement in a given fixed direction." This is a shear, and he remarks upon "the tendency of the particles to arrange themselves when in motion all in one direction, according to the laws, *not of pressure BUT of friction*, a distinction which is quite necessary to be borne in mind."² But it is not quite clear from this passage, whether he supposed the planes of cleavage and of shear to be the same or not. I assumed in my former paper that this is the case. But a suggestion from Mr. Harker has led me to re-consider this point, and to inquire whether the greater diametral plane of the ellipsoid of distortion may not be the cleavage plane.

The excellent illustrations to Sharpe's paper have enabled me to test this question, and I have little doubt that the above conclusion is correct.

Fig. 2.

¹ On certain Movements in the Parts of Stratified Rocks, Brit. Assoc. 1843. Quoted by Sharpe, *loc. cit.* p. 76.

² Phil. Mag. vol. xii. p. 198.

One mode of representing the distortion by shearing in a plane perpendicular to the strike (the plane of dip) is as follows. Draw a number of lines parallel to the shear, dividing the circumference of the circle of Fig. 1 into equal small arcs, and produce these lines to cut the ellipse. The points where they cut the ellipse will be the places, in which the corresponding points of the circle will be found after being sheared. (It is easy to conceive an analogous construction if the circle has been compressed as well as sheared.) And points along the radii of the circle will take up their positions along these radii of the ellipse, as defined by their intersections with the parallel lines. We may then, if we please, draw our fossil, or other object, within the circle; and draw a distorted figure within the ellipse, guiding the pencil by corresponding intersections of the parallels with the radii in the circle and in the ellipse respectively. But for practical purposes it is more convenient to use the parallelogram into which a square is distorted, as in the figure below. By dividing up the square into a number of small squares, and the parallelogram into the corresponding small parallelograms, it is then very easy to draw the outline of any object within the square, and to represent its distorted form within the tessellated parallelogram.

FIG. 3.

SECTION OF A *Spirifer* DISTORTED BY SHEARING. PART OF SHARPE'S DIAGRAM (FIG. 15) SHOWING THE DISTORTION OF A *Spirifer* BY CLEAVAGE AT SOUTH PETHERWIN.¹

The outline of a *Spirifer* given in the diagram has been taken along a section slightly inclined to the line of the hinge, so as to give a distorted form analogous to that in Sharpe's figure. On comparing the two figures, it can be easily seen that, on a chance assumption of the position of the fossil and of the amount of the shear, the distortion agrees very well with that observed by Sharpe. Some allowance may fairly be claimed for want of closer agreement

¹ Quart. Journ. Geol. Soc. vol. iii. 1846.

with the dotted restoration in his section, which is evidently imaginary, and shows the mesial fold too much to the left. It comes out clearly, on comparing the two diagrams, that the diametral plane of the ellipsoid corresponds with *xy*, the plane of cleavage in Sharpe's figure. The plane of cleavage, therefore, does not lie in the direction of movement among the ultimate particles of the rock, but is inclined to it. It will be shown later on that the inclination may be as great as 45° , and diminishing from that as a maximum, the two directions may become nearly, though never quite, identical as the shear is increased.

An inspection of the ellipse shows that it will have two diameters which are equal to the diameter of the circle. One of these will be in the direction of the shear, and the other similarly situated on the other side of the minor axis. Objects found along these diameters will not be distorted, while those lying nearer to the major axis will be lengthened, and those nearer to the minor shortened.

Our ellipse evidently corresponds with that given by Phillips¹ in illustration of Dr. Sorby's views regarding unequiauxed particles. And if such particles were originally lying at all angles in the circle, they would become packed together most closely about the diametral plane of the ellipsoid, and thus, according to his theory, that ought to be a cleavage plane, as we have seen that it is.

It is important to notice that in our diagram there is not any compression whatever represented, so that it appears that lateral pressure is not required for the geometrical phenomena of distortion by cleavage. Nevertheless, if the reasoning of Part IV. is sound, which I believe to be the case² (excepting the assumption that the cleavage and shear coincide), a pressure across the tract is mechanically requisite to avoid faulting. The effect of this will be to condense the rock while it is being converted into slate. We must therefore use the smaller ellipse in Fig. 2, if we wish accurately to delineate the distortion. This, however, will in no way alter the general character of the phenomena.

The bedding of the strata will of course be in general distorted, as well as the fossils. And the very remarkable phenomena of close folds on a small scale accompanied by cleavage planes, such as is represented in Dr. Sorby's diagram,³ may according to the present theory be explained. We observe that the axes of the folds are nearly parallel to the cleavage. We may therefore regard each semi-fold as approximately a semi-ellipse, which has arisen from the distortion of a semi-fold much less compressed. If, then, this rock was more rigid than the matrix in which it was inclosed, it would not, like it, yield entirely by viscous shearing, but partly by fracture,

¹ Phil. Mag. Jan. 1856.

² The argument (p. 273) regarding the circumstances which would lead to a viscous shearing as distinguished from faulting supposes that a horizontal stress causes a shearing of the parallelepiped. We now regard the shearing as causing a stress. The relations of the forces will remain unaltered, although the directions of the motions will be reversed.

³ Edin. New Phil. Journ. 1853, vol. iv. p. 138, copied by Professors Tyndall and Phillips.

and it can be seen in Sorby's figure that the directions of the cracks cut up the bent layers into *voussoirs*, just of the forms and in positions which would be requisite to extend a fold already existing, into a narrower one of sharper curvature. This applies to the folds which point upwards as well as those which point downwards. The bending and crowding of the cleavage surfaces, to accommodate themselves between the folds of the less yielding rock, is analogous to what may be seen under the microscope, where the *laminæ* of the matrix of a slate bend round a distorted harder granule.

The principle just enunciated is capable of an application far wider than can be now entered upon. It shows that all folding is distorted by the action which has produced cleavage: and that, when we see a fold in a cleaved region, we can no more assert that it is of its original form, than that a fossil distorted by cleavage is so. This will explain the sharp folds dipping inwards on the flanks of a mountain chain. It will also explain some cases, at least, of crumpling, which may be but distorted ripple marks. And it is interesting to see how closely the distorted section of ripple-marked bedding, if across the cleavage as given by the tessellated parallelogram, agrees with that of a crumpled rock.

Moreover, since this action affects the minute structure of the rock, and leaves its impress upon microscopical sections, the molecular movement, which in a suitable material produces cleavage, may in such rocks as are not liable to take on that peculiar condition, give rise to some analogous structure, which petrologists might identify.

The estimate hitherto made of distortion has been of that in a plane at right angles to the strike. To estimate that in one parallel to it (say on a bedding plane), we observe that every generating circle of the sphere, perpendicular to the plane of the paper, has been changed into its own corresponding ellipse. This ellipse will be of the same breadth as the circle in the direction of the strike, but will be elongated or shortened according to its position in dip. Its area will not be equal to that of the circle, as was the case with the vertical section of the ellipsoid. The result will be that the impressions of fossils on the plane of bedding may be either elongated or shortened in the dip, but will not be altered in the strike. They will therefore in general cover a different area, greater or less, from what they did before distortion; while the apparent alteration of their proportions in the direction of the strike will be only comparative. If we employ a divided square and a correspondingly divided *rectangular* parallelogram, their lengths being taken proportional to the radii of the circle and of the ellipse, parallel respectively to the original and to the sheared bedding, it is easy to delineate the distortion of any fossil; and it will be found to agree with that seen in nature. The comparison is best made in the case of *Trilobites*. The same thing may also be readily done with proportional compasses. When the bedding nearly coincides with a cleavage plane, the fossils become much elongated, and so enlarged, and thus the great size of the Tintagel fossils is accounted

for. If the plane of bedding lies so that the fossils become shortened, they may be crumpled, and exhibit striae parallel to the strike.

While speaking of distortion, the greenish oval spots, which occur on some purple slates, must not be passed over. It is perfectly obvious that they are discoloured regions, and that the chemical action has radiated from a foreign body, usually to be seen at or near the centre. The first question is, whether they were formed before or since the cleavage. Either is possible. If before, then they have been distorted into their present form, and, if originally spherical, they will give accurately the form of the ellipsoid of distortion. If formed subsequently to cleavage, they may be accounted for by the chemical influence spreading most readily along the grain of the slate, and with greatest difficulty across its laminae. It is common to see exactly similar oval spots where the colour has been discharged around nail-heads upon deal boards, which have been stained to imitate oak and exposed to damp. The nail is in this case the foreign body, and the spot is elongated in the direction of the grain of the wood. It is difficult to see how the rock can have been affected by local decomposition of the colouring matter, unless subjected to the percolation of aerial water; and this leads me to incline to the idea that these spots are comparatively recent. Discoloured bands of bedding are in the same category.

On studying Dr. Haughton's ingenious paper in the *Phil. Mag.* for January, 1856, I was much perplexed at some of the results which he had obtained. For instance, it seemed a very unaccountable thing that the ellipsoid of distortion should commonly come out a very flattened spheroid. This could only be explained by an inconceivably great condensation of the material, with scarcely any lateral movement. I think that the probable cause of this anomaly is that Dr. Haughton has not taken into consideration the alteration, in the angle between the original bedding and the cleavage, produced by the shear. He calculates the form of the ellipsoid by means of the distortion of a fossil out of its known proportions as measured on the cleavage plane, and the angles (ϕ , ϕ') observed between the constant dip of the cleavage and two different dips of the bedding, assuming that the bedding has not been disturbed by the cleavage; whereas it seems that observation cannot determine the inclination of the original bedding to the cleavage, because the bedding, as well as the fossils, has been displaced. The formula which shows the amount of displacement for a given shear will be given further on.¹

To assist me in forming an estimate of the amount of condensation which may have been produced by pressure during the formation of a slate, Professor Liveing kindly examined for me a piece of the Devonian argillaceous purple rock, taken at the depth of about 1000 feet, in the boring at Turnford, near Cheshunt; and also a fragment from a slab of purple slate. He found the Devonian rock more like slate, than are ordinary clays, in its chemical composition. It therefore appears to me to be a fair subject for comparison. Its

¹ Equation 3.

specific gravity at 15°·5 C. is 2·644; and the loss on ignition is 5·08 per cent. The slate examined had the same specific gravity as the Devonian, viz. 2·64; and the loss by ignition was 2·44 per cent. If we divide the water in the Devonian, so that one portion combined with the rocky matter will give the composition of slate rock, we shall have,

$$\begin{array}{r} \text{Devonian rock} = 2\cdot77 \text{ water} \\ \quad \quad \quad 2\cdot31 \text{ water} \\ \quad \quad \quad 94\cdot92 \text{ rocky matter} \end{array} \} = \text{slate rock.}$$

$$100\cdot00$$

$$\text{That is, Devonian rock} = 2\cdot77 \text{ water} \\ \quad \quad \quad 97\cdot23 \text{ slate rock}$$

$$100\cdot00$$

If then 2·77 per cent of water was to be removed from the Devonian rock, the remainder might be taken to have the composition of a slate. Probably the loss of this amount of water would cause the material to shrink somewhat; but we will neglect that cause of shrinking, and credit the whole of it to compression.

The specific gravity of the remaining mass of the Devonian (its volume being unaltered) will bear to its former specific gravity the same ratio, that its present mass bears to its former mass, so that it will now be $97\cdot23 \div 100 \times 2\cdot644 = 2\cdot57$. We must next suppose this substance compressed, until its specific gravity becomes that of slate. The volume after compression will be to that before compression inversely as the specific gravities: and 2·64 being the specific gravity of slate, the volume of the compressed rock will be, $2\cdot56 \div 2\cdot644$, or 0·97 of its original volume. Thus we see that a compression of less than three per cent. would suffice to reduce the Devonian rock to slate. But being confined on all sides (as would be the case deep in the earth), the rock could not expand laterally, and the change of dimension could take place only in the direction of the pressure; so that we may say that 100 feet thickness of the Devonian rock of Turnford would not quite be compressed into 97 feet on being converted into slate. This is a compression of only three per cent.

It is probable that, if a substance was homogeneous, a shearing action, accompanied by compression, would not induce cleavage. But the rocks which have been cleaved were not homogeneous. Every particle, therefore, being liable to distortion, would be thinned in the direction of the shortest axis, and elongated in that of the longest axis of the ellipsoid. The breadth only of the particle, measured in the direction of the strike, would remain unaltered. These changes would convert the rock into an assemblage of flattened ovoid particles of unequal strength, and render it liable to cleave, the fracture running among the particles parallel, on the average, to the major diametral plane of the ellipsoid. This agrees with the structure

of a slate as revealed by the microscope. The unequiaxed particles referred to by Dr. Sorby would also be rearranged, as already explained, mostly in the said plane, and would assist in promoting cleavage parallel to that plane. Thus we should conclude that Dr. Sorby and Prof. Tyndall have both asserted *veræ causæ* concerned in the production of this structure.

Dr. Sorby has remarked that, "Many of the finer-grained slates used for roofing contain minute rounded grains of mica, seldom so much as $\frac{1}{16}$ th of an inch in diameter, and usually much less, which are of nearly the same thickness as width, and not merely flakes. When these are cut through in the thin sections used for microscopical examination, they are seen to be composed of many laminae. When the line [plane] of lamination—that is, of the crystalline cleavage of the mica—coincides with the cleavage of the slate, then these rounded grains retain their form unaltered." In this position, according to our view, the grains would be elongated, but not broken up in any way, because every lamina would be similarly affected in the direction of lamination. Perhaps this may satisfy the above observation, because the original form cannot be known. "If the lamination is perpendicular to the cleavage, the rounded form still remains, but the laminae are generally not straight, being irregularly bent in just such a manner as if they had been compressed in the direction perpendicular to the cleavage of the slate." In fact they are in the case of the minor axis of our ellipse, which had originally the length of the radius of the circle. "Those, however, which lie with their lamination at intermediate angles, as, for instance, 30° or 40° to the cleavage of the slate, do not retain their original form, but are broken up and extended out in the plane of their lamination."¹ These particles I should suppose to have had their planes of lamination nearly in the direction of the shear. Being planes of little friction in the crystal, it has become deformed by minute faulting along them, instead of by viscous shearing. In the instance referred to by Dr. Sorby, it would therefore appear that the plane of shear was inclined to that of cleavage at between 30° and 40° . Dr. Geikie gives an illustration² showing one of these faulted grains, and it accords perfectly well with the above explanation.

The diagram, representing the traces of the several planes in ordinary positions upon a vertical plane perpendicular to the strike, will render the relation of the various angles more intelligible. If a be the angle which the side of either rhombus in Fig. 2 makes with its base, then $\cot a$ will be the measure of the shear. θ is the inclination of the shear to the horizon, and is the same angle as θ in the former Parts of this treatise. ϕ is the inclination of the cleavage plane to the shear. Then the dip of the cleavage will be $\theta - \phi$, which is an angle that can be directly observed. ψ is the inclination of the sheared bedding to the shear. Hence $\psi - \phi$ is

¹ "In just such a manner as would occur if the dimensions of the slate had been changed as previously mentioned."—"On the Origin of Slaty Cleavage," Edin. New Phil. Journ. vol. IV. p. 140, 1853.

² Text-Book of Geology, p. 310.

Dr. Haughton's ϕ , and may be observed; but he has taken the bedding as retaining its original position.

FIG. 4.

The chief difficulty that will be felt in accepting the theory now proposed, that cleavage is due to an internal movement of the rocks rendered necessary by the disturbed region having been left after elevation in a position too lofty for equilibrium, will probably arise from the great amount of the vertical movement necessary to give a sufficient amount of shear. But it must be recollected that an *equally great amount of movement is implicitly assumed* in the theory which attributes the cleavage to the identical compression which crumpled the rocks. For, whatever be the cause, the observed distortion implies a corresponding shear. In mitigation of this difficulty it may be noticed that the shear is relative, and, if great at one place in one direction, may be compensated (on the average) by one equally great in the opposite direction no very long way off. Such changes would be betrayed by changes in the direction of dip of cleavage, which in fact takes place pretty frequently. Again, the great relative shifts, which are known to take place in certain cases by faulting, may be equally expected to occur by shearing in cases where faulting is not possible. Subsequent disturbances are to be expected, for Jukes says that, "when running sections in North Wales, he was occasionally struck by the fact of a sudden change in the strike and dip of the cleavage occurring immediately after crossing a fault. It seemed to indicate that the cleavage plains had been affected by the faults, and no longer lay in their original positions with respect to the horizon and compass bearings."¹

To express the ratios of the axes of the ellipsoid of distortion; let them be a, b, c . Then b will be the radius of the sphere. Suppose the sphere to be compressed into a spheroid, and that its polar axis becomes mb . Then m will not differ much from unity. We have already estimated it at 0.97 in a particular case. It may

¹ Manual of Geology, p. 271, ed. 1862.

be proved by the properties of conjugate diameters that the ratios of the axes of the ellipsoid of distortion will be,

$$\frac{a}{b} = \frac{1}{2} \left(\sqrt{1+m^2 \operatorname{cosec}^2 a + 2m} + \sqrt{1+m^2 \operatorname{cosec}^2 a - 2m} \right) \\ \frac{c}{b} = \frac{1}{2} \left(\sqrt{1+m^2 \operatorname{cosec}^2 a + 2m} - \sqrt{1+m^2 \operatorname{cosec}^2 a - 2m} \right) \quad (1)$$

And by means of the equation to the ellipse, taking for the co-ordinates $b \cos \phi$ and $b \sin \phi$,

$$\cos^2 \phi = \frac{1}{2} + \frac{1 + m^2 \operatorname{cosec}^2 a - 2m^2}{2 (\sqrt{1+m^2 \operatorname{cosec}^2 a})^2 - 4m^2} \quad (2)$$

Since m cannot be greater than unity, nor $\operatorname{cosec} a$ less than unity, the second term is always positive. Hence, $\cos^2 \phi$ is always greater than $\frac{1}{2}$, and ϕ less than 45° . The inclination of the cleavage to the shear will commence at this angle, and diminish continually as the shear increases.

To express the alteration in the dip of the bedding, or of any other plane, caused by the shear, supposing its inclination to the direction of the shear to have been Ψ and to become ψ , it can be proved that,

$$\cot \psi = \cot \Psi + \cot a.$$

It must be remembered that a is not independent of m . But practically m might be taken as unity, and then, if by means of the observed distortion of an object, we could find either of the ratios of the axes of distortion, we could find a the angle of shear, from the corresponding equation of (1); and the inclination ϕ of the plane of cleavage to it from (2); and, the dip of the cleavage $\theta - \phi$ being known from observation, we should then know θ , the dip of the shear; or the direction in which the movement took place: ignoring subsequent disturbances.

IV.—REMARKS ON SERPENTINE.

By Prof. T. G. BONNEY, D.Sc., F.R.S., Pres. G.S.

THE very full and able review of Dr. Sterry Hunt's paper on "The Geological History of Serpentine,"¹ gives me an opportunity, of which I have been for some time desirous, of commenting on certain parts of it relating to my investigations of the typical rock which bears that name. In commenting adversely upon these, I must not be supposed to express a difference of opinion in regard to every part of the paper, because there are many relating to the Archæan schists with which I am in general agreement.

In a matter of this kind, where so much depends on the meaning of a word, it is necessary to be clear and precise in our definition. No term has been more vaguely used than 'serpentine.' In the first place, it is applied both to a mineral and a rock. Again, in the

¹ See page 276 of the present volume.