T H E

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ART. I.—*Current Theories of Slaty Cleavage*; by GEORGE
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THE theory that slaty cleavage is due to pressure normal to the cleavage is old and very generally esteemed satisfactory. Sedgwick and others held cleavage to be mainly a phenomenon due to the crystallization or recrystallization of minerals in an appropriate orientation, and this idea with modifications has been advocated of late years by Messrs. Van Hise and Leith. Mr. Leith's recent paper* is the most authoritative exposition of it.

This geologist acknowledges that my theory of slaty cleavage† applies in certain cases to which he gives the name of fracture cleavage. According to him, such cleavage is mainly characterized by the presence of actual partings within the mass, but sometimes shows flow structure as well. He distinguishes flow cleavage from fracture cleavage, however, attributing to the former a greatly preponderating importance in nature and ascribing it to causes distinct from those which produce fracture cleavage. To this latter he attributes the fissility of those rocks in which a parallel arrangement of mineral constituents is ab-

*Rock Cleavage, U. S. Geol. Surv., Bull. No. 239, 1905.

†Like Tyndall and Daubrée, I consider a parallel arrangement of flattened grains unessential to cleavage. Rupture takes place (as Messrs. Van Hise and Leith concede) on planes of maximum slide or maximum tangential strain. Rupture is a gradual process and cohesion is impaired through flow before it is destroyed. Impaired cohesion in my theory is cleavage. Cleavage develops most perfectly when the stress tending to produce it is persistent in direction, because viscous resistance is then small. In a rotational strain there are two sets of mathematical planes on which maximum slide takes

sent throughout,* as well as cleavage which is not parallel to the mica plates of phyllites. Fracture cleavage, he says, is present abundantly in the rocks of the lithosphere.† It is perhaps desirable for me to comment on his paper as succinctly as possible.

Mr. Leith does not make the theory he supports entirely clear to me. He states, and repeats in his summary,‡ that the parallel arrangement of component minerals in slates showing flow cleavage is developed by recrystallization “mainly in planes normal to the greatest pressure.” By greatest pressure, I understand him to mean resultant stress. Other passages, however, seem to contradict these. In one of them§ he says that the final position of cleavage “may or may not be inclined to the greater stress depending upon the nature of the strain.” Again on another page|| he admits that pure or irrotational strains are of rare occurrence in rock masses and concludes from the nature of rotational strain that “the final position of cleavage is usually inclined to the axis of greatest stress.”¶ He maintains, however, that even in such cases cleavage is always tending to develop normal to the greatest principal stress.

In dealing with the strains accompanying the development of cleavage, Mr. Leith is more definite and states§ that “wherever the directions of shortening of a rock mass can be determined with certainty, any flow cleavage which may be present is normal to the greatest total shortening which the rock has undergone.”

place and both sets are parallel to the axis of rotation. They make with the greatest axis of the strain ellipsoid angles given by

$$\tan \omega = \frac{\pm B}{(ABC)^{\frac{1}{3}}}$$

A being the greatest axis, B the least and C the axis of rotation. The planes of maximum slide contain the circular sections of the ellipsoid only in a limiting case. During the progress of strain these mathematical planes sweep through wedges of the mass, but the two sets of planes sweep at different rates, one set having a relative angular velocity from, say, 20 to an infinite number of times as great as the other. On the planes which sweep rapidly, viscosity reinforces rigidity, there is no time for considerable flow to take place, and, unless actual rupture occurs, so that joints form, the effect will be small. On the other set of planes viscosity is small, the mass has time to yield by flow, cohesion is weakened, and cleavage results. In a word, the theory is that slaty cleavage is due to solid flow attendant upon rotational strains. So much of the energy of the system as is not potentialized is dissipated on the planes of maximum slide, and this may or may not lead to the alteration of mineral constituents, e. g., the transformation of feldspar into biotite. It is discussed in *Finite Homogeneous Strain, Flow and Rupture of Rocks*, Bull. Geol. Soc. Amer., vol. iv, 1893, p. 13, and in *Experiments on Schistosity and Slaty Cleavage*, U. S. Geol. Surv., Bull. 241, 1904.

* Idem, p. 127.

† Op. cit., p. 118.

|| Idem, p. 113.

‡ Idem, p. 134.

§ Idem, p. 138.

¶ Idem, p. 106.

This explanation differs from that proposed in the last paragraph more than might be supposed. It is analogous to Sharpe's theory, but is more general; for Sharpe supposed cleavage developed by an external pressure perpendicular to the induced cleavage, or to a pure strain, usually accompanied by lateral constraint; whereas Mr. Leith's second hypothesis is that, whether the strain is pure or not, the cleavage is normal to the least axis of the strain ellipsoid.

I entirely share Mr. Leith's opinion that pure strains are rare in nature. Some idea of their rarity may be gained by a little reflection. The direction of a force with reference to a resisting plane may be regarded as fortuitous. If so, the chance that the direction will be exactly 90° is infinitesimal, but if a variation of plus or minus half a degree is tolerated, the chance will rise to one in 20,626, which is the number of square degrees on a hemisphere of unit radius, or 360 radians. On the other hand, a zone one degree in width on a sphere at a polar distance of 45° has an area of 255 square degrees, so that the chance of a force having an inclination of $45^\circ \pm 30'$ to a fixed plane is 255 times as great as that it should be normal to the plane. The average value of all possible inclinations is an angle of one radian ($57^\circ 18'$) to the normal. Thus pressures at less than 45° to the plane are more probable than those at higher angles and normal pressure is least probable of all. Hence a pure strain is a highly improbable limiting case of rotational strain. Unmodified scission is also a limiting case, but is 360 times as probable as a pure strain.

It has been assumed in the preceding paragraph that rock masses undergoing deformation may be regarded as resting against a fixed support, and this is only partially true. Any supporting masses must yield by rotation to some extent, though the amount of such yielding must usually be exceedingly small as compared with the amount of deformation. When a dislocation occurs between the Andes and the basin of the Pacific, the trend of the range is not sensibly changed to accommodate the rocks adjacent to the fault system. However, so far as the supporting resistance does rotate, the probability of a pure strain is increased by the diminution of the rotational strain component. I shall assume that the probability rises to one in 10,000, though in my opinion this is a gross exaggeration.

Sharpe's theory is that cleavage is due to pure strain. The many geologists who are content with this theory ought to tell the rest of us what happens in 9999 cases in which the strain is not pure.

In the vast exposures of the Archean and early Paleozoic, millions upon millions of cases of dynamo-metamorphism are exposed to examination and when such numbers of instances

are to be dealt with the laws of probability become exact. Again, wherever there are evidences of dynamo-metamorphism, cleavage appears in the rocks, not always good cleavage, but still a fissile structure which should be accounted for. With every possible allowance for yielding of supports, it appears to me conclusively shown above that the average direction of deforming force to the resisting plane cannot have exceeded something like 45° , and hence also that schistosity is brought about as a concomitant of strains in which the rotational element is large. It follows to my mind that Sharpe's theory is inadequate, for if it were a sufficient explanation, not more than a tenthousandth part of the strained rocks ought to show cleavage or foliation.

The hypothesis that crystallization takes place in surfaces perpendicular to the resultant stress is attractive, but it must be tested first of all by determining for the simplest cases what the direction of resultant stress really is. Mr. Leith evidently

supposes that in the case of pure or irrotational strain in a homogeneous mass, the resultant stress coincides in direction with the least axis of the strain ellipsoid. Such a coincidence will truly exist between the external stress or surface traction and the axis in question when the strain is pure, but there is no such agreement between the resultant internal stress at an arbitrarily selected point and the local orientation of the strain ellipsoid. In pure strains the resultant stress acting on any material particle is in the direction of the motion due to this action. The paths traced out by the particles are called the lines of displacement or the "lines of flow" and the surfaces which are perpendicular to these lines are the elastic or plastic

equipotential surfaces. Hence if crystallization takes place on surfaces perpendicular to the resultant stress, these are the equipotential surfaces.

The simplest conceivable deformation is (irrotational) shear. In a shear the lines of flow are rectangular hyperbolas and the equipotential surfaces are rectangular hyperbolic cylinders. Little more complex is the case of two shears at right angles to one another. This corresponds to the axial homogeneous compression of a cube, slab or cylinder of constant volume. In discussing the mechanics of slate formation the cubical compressibility of the mass is of small importance because after the limit of elasticity is reached and flow begins, there is no further change of volume. A cut (fig. 1) showing the lines of flow and the plastic equipotentials is borrowed from W. J. Ibbetson's well-known work on elasticity.* The circular cylinder A, B, C, D is supposed compressed by uniformly distributed pressure to the shorter cylinder A' B' C' D', and during the process A moves to A' along the curve connecting the two, B moves to B', etc. The equipotential surfaces are hyperboloids of revolution represented by the equation given by Ibbetson,

$$2y^2 - x^2 - z^2 \pm c^2 = 0$$

where y lies in the vertical and c is a constant. They are represented by full lines in the figure.

Add to the cylinder shown in this figure a second inverted cylinder at the bottom of the first, and suppose the two to represent only the central portion of a slab. Then the entire diagram would show the equipotential surfaces on which mica scales would form if they grew at right angles to the pressure in a mass subjected to pure strain.

In mere translation, or in rotation, no work is done against purely elastic or plastic resistances. Hence in a rotational strain at any given instant the elastic potential is the same as it would be for a pure strain of equal amplitude. There is an important difference between the two cases, however, for in pure strain the system of lines of flow and of equipotentials remains fixed relatively to the mass, so that the motion of the particles, however great, is confined to the lines of flow which pass through them at any instant. On the other hand, in a rotational strain the lines of flow and equipotentials are not fixed relatively to the mass, but only relatively to the axes of the strain ellipsoid, and, like these axes, shift continually with reference to the material particles of the body undergoing strain. At any instant, however, the equipotentials or surfaces normal to resultant stress can be definitely assigned.

* *Mathematical Theory of Perfectly Elastic Solids*, etc. London: Macmillan and Company, 1887, p. 172.

In figure 2, I have drawn out the equipotentials for a rotational strain which is identical with that illustrated in figures 3 and 4. The hyperbolas are the traces on the plane of the diagram of the hyperboloids of revolution whose equation is stated above, when the axes of co-ordinates are the principal axes of the strain ellipsoid. The two asymptotes are the traces of a two-sheeted cone of revolution, so that in the whole system of surfaces there is not a plane area. It is upon these surfaces of double curvature that mica should be deposited at the final moment of strain were it true that this mineral crystallizes perpendicularly to resultant stress. If such

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crystallization occurred during the whole progress of stress, the mica would be found not only on one system of hyperboloids, but upon innumerable intersecting systems of hyperboloids.

I am not aware of any lithological phenomena of a character corresponding to such equipotentials. Nothing more unlike the structure of a slate belt can be imagined, and I conclude that the hypothesis under discussion is wholly without foundation.* The fallacy, of course, consists in confusing the forces acting on the exterior of the mass with the resultant of these and the internal forces. It is this resultant which is actually exerted on any small group of molecules within the body.

The lines of flow may be considered as representing the absolute motion of particles of the mass. There is also a relative motion of the elements of mass which is rectilinear and takes place along the planes of maximum tangential strain. It is to impairment of cohesion caused by this relative motion that I suppose cleavage and jointing due.

Although the equipotentials are as far as possible from being plane surfaces, yet the mica scales in phyllites are arranged in

* Mr. F. E. Wright has published a preliminary note on some experiments which would seem to offer some support to the supposition that crystallization in glass is determined by external forces. In these experiments, however, insufficient care was taken to ensure uniformity of temperature, and when they were repeated with more precaution the results were practically negative. This Journal, vol. xxii, p. 224, 1906.

planes and must crystallize there in obedience to some law. Sharpe believed the micas secondary and that they crystallized most rapidly in the direction of least resistance.* I entirely agree with him.

It would appear from Mr. Leith's discussion that as a matter of fact the relation sought to be proved is not that mica scales form normally to local stress, but that they are arranged perpendicularly to the shortest axis of the strain ellipsoid. He offers a variety of evidence that this relation exists in those rocks which have undergone what he designates as flow cleavage, and this evidence is discussed in the fifth chapter of part one. He takes up first the distortion of pebbles in conglomerates, which he alleges are elongated in a direction parallel to the schistosity of the matrix. A schistose conglomerate is not a particularly favorable rock for a discussion of this description, because the schistose lamellæ wind in and out between the pebbles, and it is impossible to assign to them an average direction with any degree of accuracy.† My experience, however, does not coincide with his, so far as observation is concerned. Where conglomerates have been rendered schistose, and the pebbles are not so abundant as to touch one another, it is in some instances possible to break them out with adherent portions of the matrix. I have thus extracted many scores of pebbles from schistose conglomerates where the conditions appeared favorable, and I have found that each pebble came out with an appendage of schist, a sort of beard, which in almost all cases stood at a sensible angle to the major axis of the pebble. Nevertheless, I do not for a moment undertake to deny that there are conglomerates where there is sensible coincidence between these directions. The question is what such a coincidence would indicate. I do not think that Mr. Leith has put the correct interpretation upon it. In conglomerates, as everyone knows, there is a strong tendency for the pebbles to arrange themselves with their shortest axes perpendicular to the plane of bedding, though there is usually some imbrication or shingling. If the plane of bedding were parallel to the plane of fixed resistance, and if also a force were to act on the conglomerate at 90° to the plane of bedding, then the elongation of such pebbles as lay quite flat would coincide with the normal to the least axis of the strain ellipsoid. But each of these conditions must be of very rare occurrence, and that both should be fulfilled at once is in the highest degree improbable.

* *Geol. Soc. Journ.*, vol. v, p. 129, 1849.

† On p. 116, Mr. Leith asserts that in a rock undergoing flow "the general effect of rigid particles is to transmit stresses locally in directions normal to themselves." I know of no such theorem in mechanics, and believe the statement incorrect.

If, on the other hand, either condition fails to be fulfilled, the position of the strain ellipsoid will not be indicated by the elongation of the pebbles and the divergence may amount to any angle. In a general way, this conclusion may be arrived at in a moment, for the ellipticity of the strain ellipsoid will be superposed on that of the pebble and the resultant figure will coincide with neither, either in amount or in attitude.

Specific examples are perhaps more convincing than general principles, and I have therefore computed some cases which are illustrated in figure 3.* In diagram *a* are shown a circle and three ellipses in a square which are to represent the sections of a sphere and three plastic ellipsoids within a cube. In *b* the mass is supposed to be strained by a force acting at 30° to the resistance, and this is assumed to be horizontal. The strain is the same as that represented in two other diagrams in this

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paper and some further details concerning it will be given presently. The sphere is of course distorted into the strain ellipsoid and the major axis of this figure will stand after strain at an angle of $22^\circ 37'$ to the horizontal. The greatest axes of two of the ellipsoids originally coincided with the direction of the resistance, but after strain they make with the horizontal angles $8^\circ 45'$ and $6^\circ 5'$, showing how the original ellipticity affects the final orientation. One of these elongated pebbles makes an angle of about 14° with the strain ellipsoid and the other about $16\frac{1}{2}^\circ$. No ellipsoid which is originally parallel to the resistance can have a negative inclination after strain, but the third ellipsoid illustrated dips at minus $11^\circ 42'$ in the unstrained state and after strain at minus $6^\circ 5'$, thus standing at an angle of nearly 29° to the strain ellipsoid. Had either its ellipticity or its attitude in the unstrained mass been different, its final inclination would be more or less than $6^\circ 5'$.

This last ellipsoid was computed with a secondary purpose, for, in its final position, it coincides exactly with the direction of the cleavage which, according to my theory, would be developed in the mass by strain. Either of the other pebbles if

* The angles given in the diagram are $\nu = 22^\circ 37'$, $\delta_1 = 8^\circ 45'$, $\delta_2 = 6^\circ 5'$, $\delta_3 = -6^\circ 5'$, $\phi = -11^\circ 42'$.

broken out of the metamorphosed mass would bring away a "beard" of schist standing at an angle to its axis.

These specific instances merely illustrate the general conclusion that the elongation of pebbles gives no information of any value as to the position of the strain ellipsoid. The same conclusion is immediately applicable to the flattening of angular mineral fragments.

The evidence afforded by fossils is as a rule no better than that derived from pebbles. It is easy to imagine an ellipsoid circumscribed about a trilobite for example, and then infer the apparent distortion in a given strain. The discordant results which have been deduced from the measurements of fossils by various observers are thus easily accounted for.* Nevertheless, with a sufficient amount of work I believe better results could be obtained. In a strained fossil there must be two central sections which are undistorted and possibly these might be found by accurate measurements in some favorable cases. If found, they would determine the position and the ellipticity of the section of the ellipsoid through the greatest and least axes. To determine with accuracy the undistorted sections of a fossil would be a delicate job and has not been attempted so far as I know.

The evidence from volcanic textures, such as the blow holes in pre-Cambrian lavas, is as poor as that from pebbles, since blow holes are substantially always ellipsoidal in unmetamorphosed lavas.

Mr. Leith also attempts to use the distortion of beds and the attitude of folds to prove the position of the minor axes of the strain ellipsoid. In certain cases beds of a composition somewhat different from that of the slate are crenulated in a direction normal to the cleavage, and Mr. Leith considers this a proof that the "greatest shortening" of the mass is also normal to the cleavage. In figure 4, which illustrates my own theory of cleavage, I have drawn a bed before and after distortion, assuming that this bed retains its original length unchanged because of lack of plasticity. The axis of crenulation is exactly

* A part of the difficulty seems to be due to the fact that even flat organisms are by no means always deposited in strict conformity to the stratification. This is apparent in recent muds and in unaltered sedimentary rocks. Among schistose rocks a good example is afforded by the Ordovician slates of Arvonnia, Virginia. They contain *Cyclocystoides* which were originally circular flattened discs and have been converted into very regular ellipses with axes in the average proportion of 3 to 2 or thereabout. I have examined some specimens of this slate a few square inches in area on which the orientation of the little ellipses varied by at least 45° and in no regular manner, doubtless because of original variation in position. Again, where the plane of cleavage approaches the plane of sedimentation, but does not coincide with it, fossils may undergo a deceptive distortion. Thus in the Arvonnia slates there are cases where the ellipses representing *Cyclostoides* are very regularly oriented, but with their major axes at an angle of some 20° to the grain of the slate.

normal to the plane of cleavage assigned by my theory and marked by broken lines, but it is at an angle of 29° to the minor axis of the strain ellipsoid. The crenulation of a bed merely shows that the axis of folding lies between the minor axis of the strain ellipsoid and the direction of unaltered length; it does not even tend to prove the actual position of the strain ellipsoid. On any theory of slaty cleavage it is easy to construct crenulated beds at any of a wide range of inclinations to

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the cleavage, and therefore no one angle has any valuable significance.

Mr. Leith, furthermore, advances the view that intrusions of great masses of igneous rocks are known to compress adjacent rocks in directions normal to the periphery of the intrusive mass, and that cleavage is developed in the surrounding rocks parallel to the periphery of the intrusive masses. Now, when batholiths invade a region, they unquestionably produce an outward pressure which is commonly manifested by fractures and apophyses in the surrounding rock. The strains set up must be of an enormously complicated kind and the outlines of the batholith itself usually show great irregularity. That anyone should be able adequately to analyze these strains so as to determine the principal axes, or even to establish with any fair degree of approximation the parallelism of the schistosity to the outlines of the batholith, is to me quite inconceivable.

Mr. Leith's last argument on this subject is that crystals and pebbles included in schistose rocks are frequently fractured or sliced, and that this slicing does not take place parallel to the schistosity, but at a considerable angle to it. From this discrepancy, he argues that the cleavage has a different origin from the slicing, and that while the slicing occurs at an angle to the direction of greatest negative normal stress, the schistosity is perpendicular to it. Now, if an included pebble or crystal had precisely the same properties as the surrounding mass, it would of course yield like the surrounding mass, and would show the same schistosity and nothing more. On the other hand, if

the included mass were relatively very brittle, it would be cracked at the inception of strain, and would therefore exhibit a behavior of its own. On my theory of cleavage, this behavior can be fairly well followed up. Fig. 4 is a diagram supposed to represent a quadrangular surface of a plastic mass including a cube of a different character. I suppose this cube to be made of some substantially isomorphous material, such as glass or quartzite or some very fine-grained eruptive rock, and that this cube is also brittle. Then at the inception of strain, it will be cracked at angles of 45° to the axes of the initial strain-ellipsoid. It may crack in two directions, or only in one, and I shall suppose that the direction in which it yields is that which, according to my theory, is characteristic of the master joints in slates. Now, these lines of fracture will during continued strain change their inclination, precisely as if they were mere geometrical lines in the plastic mass. The several slices will slip over one another and be rearranged. Doubtless at the edges of these slabs there will be a certain amount of disturbance of the surrounding material, but there appears to be no reason to suppose that these disturbances will not so balance one another that the centers of inertia of the several slabs will behave with simple regularity. If so, these centers of inertia will also remain on a material line which will be deflected precisely as if the cube were absent altogether. It is then possible to compute for certain displacements the position which these centers of inertia will take, and therefore to exhibit the relative position of the slices after deformation is complete, and this is done in the second figure of the diagram.

In constructing this diagram, advantage has been taken of a little problem solved in my former paper on this subject. I have there shown that, provided Hooke's Law holds and Poisson's ratio is assumed at one-fourth, a force inclined to the surface of the mass at an angle of 30° will bring about just this distortion. Now, there are substances for which Poisson's ratio is equal to one-fourth, especially the glasses. Hooke's Law is applicable to small strains with perfect accuracy; for large strains it affords only a first approximation. The diagram may therefore be erroneous to some extent, but the only error which it can contain is in the direction of the applied force, and this error probably does not exceed one or two degrees at most. It is impossible to draw such a diagram without assuming some law between stress and strain.*

It thus appears that my theory of cleavage completely explains the slicing of a pebble and the inclination in the position of the

*If x, y is the position of a point in the unstrained mass, and x', y' the point to which it is brought by strain, then $x' = 1.0577 x + y$; $y' = 0.7691 y$. It follows that $\nu_0 = 36^\circ 58'$, $\nu = 22^\circ 37'$.

fractures to the resultant cleavage shown in the cut by broken lines. In fact, if my theory is correct, some further information might possibly be obtained from such pebbles as to the operations to which the mass has been subjected. It may be observed in the diagram that the line of the centers of inertia of the several fragments nearly coincides with the direction of cleavage, but does not do so absolutely. The difference is so slight that it might be attributed to bad drawing, but this is not the case. The difference is only two-thirds of one degree, and the significance of this difference is this: The lines of the center of inertia coincide with the direction of the first fibers to undergo maximum tangential strain, whereas the cleavage represents the final direction of maximum slide. Now, as has been mentioned above, there is another set of planes of maximum tangential strain which, in this particular case, has wandered through a wedge of the mass bounded by planes at 28° from one another; thus the particles lying between the direction of the lines of the center of inertia and the cleavage have been subjected to maximum tangential strain more than forty times as long as the particles in the other direction, and it is to this difference that I attribute the development of the cleavage. If instead of being absolutely brittle the enclosed cube yielded plastically to a minute extent before rupture, the fractured mass would show a trace of cleavability in the direction of the centers of inertia of the slices. If rupture were to take place simultaneously on both sets of planes of maximum slide, double displacements resembling those shown in my paper on *Simultaneous Joints*,* figure 9, would probably occur. Fig. 5 is

borrowed from Mr. Diller's paper on the Taylorsville region of California,† figure 4, and shows a sliced quartzite pebble from a schist. A sliced rock pebble is more instructive than a cleaved feldspar because the cleavage of the mineral will modify the angle of slicing.

Mr. Leith presents no further argument from observation for his contention that "wherever the directions of shortening and elongation of a rock mass can be determined with certainty, any flow cleavage which may be present is normal to the great-

* Washington Academy Sci., vol. vii, p. 274, 1905.

† U. S. Geol. Surv. Bull. on Taylorsville Region, not yet in print.

est shortening which the rock has undergone." Certainty seems to me a strange word to apply to conclusions from such evidence. Not one of the methods can be depended upon to determine the position of the strain ellipsoid within many degrees, excepting the untried expedient of seeking the undistorted sections of a fossil.

Mr. Leith discusses at some length the superposition of fracture cleavage on flow cleavage. He interprets many observed cases as indicating that after flow cleavage has been developed, a fracture cleavage has been superimposed upon it, of course under radically different conditions of stress. He borrows from a report by Mr. Dale* an illustration of such a case taken from a thin section, and I shall follow his example

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in figure 6. When a double structure is developed in rocks the hypothesis that each is due to a separate cause always seems to me very dubious. If parting, or weakness (cleavage), is developed in one direction, a subsequent fortuitously oriented force intense enough to produce rupture at all is almost certain to lead to movements on the old surfaces, and, if this will not suffice to relieve strain, something like granulation usually ensues. Hence I am led to believe that the divisions shown in Mr. Dale's plate have a common origin and were simultaneously developed. Mr. Leith concludes that the diagonal fractures in the cut are developed on my theory. But if I apply my theory *in extenso* to this case, the exact orientation of the strain ellipsoid is at once determined. The major axis should bisect the acute angle between cleavage and the cracks, and therefore stand at an angle of 19° to each. The position of the minor axis follows. Supposing the volume of the rock constant, the minor axis will be proportional to the tangent of 19° or $0.344 = B$. Hence at once the linear compression in the direction of

* U. S. Geol. Surv., 19th Am. Rep., pt. iii, pl. 28. p. 208. From a slide of a Cambrian roofing slate near West Paulet, Vermont. The black spots in the lower part of the cut are pyrite.

B has reduced the unit length to 0.344 unless the strain was attended by diminution of volume, in which case the shrinkage in this direction was somewhat greater. The product of the other two axes is $AC=1/B$ for the case of incompressibility. Some further relation must be exactly known to determine the ratio of A to C. This the cut does not afford, but an approximation can be obtained without difficulty. The fixed plane in the case of pure strain is in the direction of the major axis, while in the case of scission on my theory, it coincides in direction with the cleavage. When pure and rotational strains are combined the resistance lies between the two extremes noted, which in this case differ by 19° . Now, as I shall point out presently, the strain exhibited in the cut bears evidences of a large amount of rotation and the fixed plane was probably nearer the cleavage than the major axis. The assumption that it made an angle of 6 degrees with the cleavage cannot be more than a degree or two out of the way, and if this were the correct value, it easily follows that $A = 2.36$ and $C = 1.23$.

I have drawn in to the cut, fig 6, the ellipse with these axes and also shown the two lines representing the traces of the planes of maximum tangential strain. One of them coincides accurately with the cleavage while the other has exactly the same direction as the joints. If this cut is compared with fig. 10 of my former paper, which was issued six years before Mr. Dale's plate, it will be seen that the similarity is very great. According to my theory, cleavage should develop in the acute angle between the direction of the applied force and that of the fixed resistance on one set of planes of maximum slide, while the other set of such planes will be marked, if at all, by joints. That appears to be exactly what has happened in the case under discussion, and it is the great difference between the two structures which lead me to infer that the element of scission in the strain is large.

Having an approximation to the values of the axes, it is a mere matter of detail to compute the component pure strains and scission. The former are due to the vertical force component, the latter to the horizontal component. To combine them to a resultant applied force it would be needful to know or to assume the relations between strain and stress. Hence I merely indicate a pressure which must lie between the minor axis and the cleavage, but an uncertain angle.

It would appear that my theory throws far more light on this occurrence than does Mr. Leith's, and affords a very satisfactory explanation of an interesting occurrence which on a natural scale covers only one square millimeter.

While my theory of cleavage does not necessarily involve the formation of new minerals, such as mica, it affords a means

of accounting for the frequent occurrence of mica on at least one set of plane surfaces wherever the rocks affected by cleavage have presented great resistance to deformation. During the progress of a strain in a body which is not ideally brittle, there is at first a cubical compression affecting the whole mass alike and becoming constant at the elastic limit. Thereafter an expenditure of energy takes place and this is confined to the planes of maximum slide, where it is dissipated through the viscosity of the material. It is not needful to prove this statement, which is involved in the very definition of viscosity. The temperature along these surfaces must rise, and, by increasing the mobility of the molecules, the heat liberated will promote chemical recombination in so far as compounds of lower potential are stable at the prevailing temperature and pressure. In rotational strains the energy expended per unit mass along those planes of maximum slide which are nearly fixed relatively to the material particles will be many times as great as along the set of planes which wanders rapidly through the mass, and the chemical effects which may take place will be in some direct proportion to this expenditure. Thus in slates due to rotational strains it is comprehensible that one and only one set of planes should be marked by abundant mica scales. In irrotational strains there may be two or more mica-coated surfaces. On the other hand, there is no liberation of heat during distortion which characterizes planes perpendicular to the least axis of the strain-ellipsoid and none distributed along the equipotential surfaces.

A few pages of the memoir under review are devoted to a discussion of strain and stress, and these call for some comment lest they should mislead. The author states that when the form of a body alone changes, the strain is called distortion, which is in entire accord with usage. A few lines further on he adopts from Van Hise a term "pure shortening" for "any irrotational strain in which all three principal axes are changed in length in such a ratio that the volume remains constant." As is well known, a "pure" distortion is synonymous with an irrotational distortion, so that shortening as here employed is absolutely synonymous with distortion. Why so well-chosen a term as distortion should be replaced by one which does not suggest its definition is not clear. Shortening is indeed absolutely misleading, for its opposite would be elongation, a term frequently used in the discussion of strains, but in a very special sense. It means extension in a single direction *without* change of dimensions at right angles to this direction; so that if the ratio of elongation is 2, the volume of the mass is doubled. Elongation in this technical sense is merely an analytical device. Only such cellular structures as pith,

cork and sponge have properties which enable them to be used in approximate illustration of (negative) elongation. Mr. Leith not only defines pure shortening, but illustrates it. The illustration, however, shows a strain in which only two of the axes have undergone change, and which is neither more nor less than a (pure) shear. Perhaps, then, shortening is only a new term for shear.

Mr. Leith says that Thomson and Tait call an irrotational or pure shear a "simple" shear. This is incorrect. They designate by "simple" shear the strain I ventured to rename scission, while they call a pure shear simply a "shear."* It was the confusing similarity between the terms for essentially different strains which led to my innovation. In my nomenclature, *pure* is a qualification of shear which is superfluous excepting when the reader might possibly forget the definition of scission.

Farther on, he states that "scission is equivalent to a pure shortening combined with a rotation of the body as a whole." If the verbal definition is accepted, this is wholly untrue. If the diagram is correct so that shortening = shear, it is true in a sense, because the two processes may each give an equal ellipsoid in the same orientation; but the boundary condition of the two ellipsoids would not be the same, so that if each were elastic their behavior on being set free suddenly would differ fundamentally. The ellipsoid due to scission would not only vibrate about the mean spherical figure, but (because of inertia) spin on its mean axis; while that due to pure deformation followed by rotation would not spin. If the ellipsoid were plastic, that produced by scission would show a fiber or cleavage under proper conditions of rupture or etching. Rolled steel is a case in point; and when blocks of steel are used to test explosives, the substitution of rolled metal (which has undergone scission) for forged metal (in which, so far as possible, scission is avoided) can be detected at a glance.

Mr. Leith states that "stress is the action and reaction between two adjacent parts of a body." This definition defines nothing at all. If stress is action, it cannot be reaction. In fact, the phrase sounds like a confused echo of the theorem as to the equilibrium of different stresses embodied in Newton's third law. Stress is simply the total force measured per unit area which is exerted between contiguous bodies or contiguous parts of a body. It is distinguished from "bodily" forces such as gravity, which are measured per unit volume.

The author further states that "any possible stress may be regarded as equivalent to three normal stresses whose directions

* See Nat. Phil., paragraphs 172 and 682. Love employs the same terms, which are in accord with the commonly accepted definitions.

are mutually perpendicular." This is true only for a case of static equilibrium. In any authoritative text-book on dynamics will be found Poinso't's theorem, that the forces acting on any point are reducible to a resultant *and a couple*.^{*} If Mr. Leith's statement were correct, it would be possible to reduce a couple to a single force. It is, of course, the couple which when resisted gives rise to the rotational strain, just as it is an unresisted or partially resisted couple which gives the rotation of a planet or a rifle ball.

All the arguments which Mr. Leith offers in favor of his views of slaty cleavage and against my theory have now been passed in review; while to his denial of my accuracy in reporting the orientation of bubbles in cakes of ceresin and in stating the directions of cleavage,[†] I have no reply to make. Messrs. Van Hise and Leith do not seem to me to have improved upon Sharpe's theory, and, so far as I can see, geologists must choose between the pure strain theory of the able Englishman and my rotational strain theory. Sharpe's theory is consistent but leaves much to be explained, both from a molecular and a molar standpoint. When the prevalence of rotational strains is admitted, so that a slate belt becomes tectonically equivalent to a distributed fault, this theory does not apply and cannot be adapted.

It appears to me that Mr. Leith is in duty bound to make public exact reasons for his assertions, to give precise methods for determining the position of the strain ellipsoid or the equipotentials in a slate, to show why there is no cleavage on planes of maximum slide, and to explain thermodynamically how it happens that the planes on which the entire energy of deformation is expended are not those on which feldspar is converted into mica. We are past the stage in which mere opinions or general impressions should be allowed decisive weight.

The distinction which Mr. Leith draws between flow cleavage and fracture cleavage is substantially a real one, which I pointed out 14 years ago. "Flow will tend to take place," I stated, "along one set of planes of maximum slide "because of the inferior viscous resistance." The other set of planes of maximum slide, it was inferred, would be marked by ruptures such as joints. I did not perhaps sufficiently consider that the scale of the whole structure might be microscopic, yet the magnified reproduction of Mr. Dale's slide precisely resembles a reduced photograph of the structure which I had in mind.

Washington, D. C., April, 1907.

^{*} Cf. e. g., Thomson and Tait, Nat. Phil., section 559 g.

[†] *Op. cit.*, p. 129.