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THE STRUCTURAL FAILURE OF THE LITHOSPHERE[1]

As a foundation for ordinary human activities it is but natural that the lithosphere or solid earth should be a popular symbol of strength and permanence; but the geologist sees abundant evidences that it has fared badly in the contest with environmental forces, past and present. It has been weak and incompetent; it has bent, crumpled, broken and mashed; structurally it has failed; in considerable part it now consists of structural ruins.

The problem of the structural geologist includes the restoration of these ruins and a determination of the conditions and causes of failure. His problem is not rendered easier by the fact that it is seldom possible to see the structures in three dimensions, and that he must base his restoration on fragments of evidence seen at the surface or on the very limited outlook of underground openings or on inferences from environmental conditions. Furthermore, the geologist seldom sees rock failure in actual progress. If he does he may not recognize it because the movement is so slow. He arrives after the disturbance is over and must infer the nature of the forces and processes from the results. In attempting to picture conditions in the inaccessible deep zones, he must make long range inferences from the few available facts.

The study of structural geology naturally begins with the mapping and description of separate structures like folds, faults, joints, and cleavage. Too often this has been regarded as the end and not as a step toward the understanding of the structural conditions as a

[1] Address of the retiring vice-president and chairman of Section E, the American Association for Advancement of Science, Chicago, December 28, 1920.

whole. The necessity of integrating evidence and information from scant observations requires an understanding of the interrelations of structures and of great group characteristics of a given environment or of a given kind of rock. I would like to comment briefly on some of these broader considerations, not exhaustively, and certainly not with full understanding, but with a view to indicating some of the salient facts now known and the manner in which these facts have been built into certain generalizations and hypotheses as to movements of the lithosphere.

I. STRUCTURAL FAILURE IN THE ZONE OF OBSERVATION

We may direct our attention first to the structural failure of rocks extending downward only a few miles from the earth's surface. The characteristics of this region are disclosed to us by deformed rocks, some of which were once far below the surface, but now brought within our range of observation by the erosion of overlying rocks. This may be conveniently referred to as our zone of observation.

Heterogeneous Nature of Movement.—In this zone, some of the rocks have been deformed by rock flowage and some by rock fracture, both kinds of deformation often resulting in folding and tilting of beds. By rock flowage we mean "solid," "plastic," "massive," or "viscous" movement under great containing pressures during which the rock and its constituent minerals retain their properties of elasticity and rigidity. No one of these descriptive terms may be technically accurate and comprehensive, but the movement partakes of the characters expressed by all of them. The movement is not necessarily slow and continuous; there is geologic evidence that it is periodic. Rock flowage is essentially characterized by the parallel dimensional arrangement of minerals, like mica and hornblende, developed by recrystallization during the process. These minerals are present abundantly after the process, not before. Rock flowage is intimately associated with fracture, including the minute granulation and slicing of mineral particles, and including larger fractures, especially of the shearing type. While rock flowage and rock fracture constitute two distinct types of deformation, there is almost complete gradation between the two, and much deformation is not accurately described by either term. A displacement may take place along a clean fracture, or along a fracture on which there has been local rock flowage, or along a zone of closely spaced parallel fractures with rock flowage affecting all of the intervening masses, or along a zone of rock flowage in which evidences of fracture planes are indistinct or altogether lacking. A single shear plane may show all of these features. In a large way a considerable zone of flowage may often be interpreted, in its relations to displacement and stresses, in much the same manner as a fracture plane.

The difficulty of a precise definition of the two phenomena of fracture and flow is well illustrated in the so-called flow accomplished experimentally. Shearing, thrust, granulation and slicing are here strongly in evidence, while the parallelism of mineral particles brought about through recrystallization, so conspicuous in schists, slates and gneisses, which are the principal evidence of rock flowage in nature, is almost lacking in the experimental results. Deformation induced artificially is plastic flow, but the same kind of deformation observed in nature is often called fracture. With a longer time factor the experimental flowage would presumably more closely approximate that of nature.

Structural failure within our zone of observation, whether by fracture or flow, has not been confined to any particular plane or formation, but is so distributed as to indicate that adjustment of rock masses under deforming stresses has been accomplished by movement in many zones, in many formations, in all directions, and with all inclinations. Rocks in this zone as a whole have not yielded to stresses as homogeneous masses. In fact, even down to comparatively small units of volume the rule is heterogeneity. No matter how homogeneous the formation may seem, rock movement discloses zones of inherent weakness along which the movement is largely concentrated.

Causes of Movement.—Rock failure is evidence of overpowering stresses, but the causes and directions of these stresses are not so clear. Failure on a mountainous or continental scale points to great earth stresses of the kinds which have been variously ascribed to adjustments under gravity between earth masses of differing densities and topographic relief, to adjustments under gravity of a solid shell to a shrinking centrosphere, a conception based on the supposed transfer of heat and magmas from the centrosphere outward, to tidal strains, to changing centrifugal pressures caused by changes in rate of the earth's rotation, or to combinations of these causes.

So clear is the evidence that great earth forces of this kind have been operative that other causes of movement have been perhaps underestimated or ignored in explaining local failure. Such are the pressures and changes of temperature attending the extrusion and intrusion of igneous rocks, in the vicinity of which there is often clear evidence of local failure, the recrystallization of rocks during long periods causing local changes of volume, the leaching of substances near the surface causing voids and weakness and consequent slump under gravity, and other volume changes under weathering. When rocks are in a soft and incoherent condition, they are especially susceptible to local stresses. Mud, marl, sand and salt deposits crumple and slip as the deposits are slowly built up, either under air or water. Local loading by water and ice or rock materials may cause them to fail. Unconsolidated glacial deposits show a variety of joints, faults, and folds. In the settling, consolidation, and desiccation of soft deposits, stresses are set up resulting in local failure. When the deposits are seen later as hard rocks it is difficult to determine the extent to which the failures are to be attributed to these early and local causes acting during the soft formative stages, and to what extent they are the result of regional deformation after the rocks are strong and hard.

The part played by the forces of crystallization in initiating earth stresses is yet but little understood. Growing crystals have been found experimentally to exert considerable linear forces. There seems to be evidence in rocks that these forces have been sufficient to widen openings or to expand the rock mass. Crystallization may also contract the rock mass. The impressive manner in which crystal habit asserts and maintains itself under most intense conditions of metamorphism seems to indicate the reaction of considerable forces of crystallization to external environment. It is the custom usually to explain such facts on the basis of adaptation to environment, and to put the emphasis largely on the environmental conditions as determining the outcome. It is clear, however, that these conditions have not been sufficiently intense to interfere with or overcome the tendency of crystals to take whatever form best suits their atomic structure—in other words, to develop their own habit. The philosophy of the precise relations between inherent crystallizing power and environmental forces is not understood, but enough is known to warrant the suspicion that the cumulative effects of the forces of crystallization may themselves initiate earth stresses of a high order of magnitude.

In my own structural field work, I have become impressed with the necessity for better criteria for the separation of rock structures due to local causes of the kind above indicated from the results of failure under the greater regional earth stresses. Of course there is no clean-cut separation between the two. An accumulation of minor and local causes may cause relatively large earth movements, and conversely major earth movements are resolved into a complex of minor related structural phenomena.

Angular Relations of Rock Structures to Causal Stresses.—Just as structures in themselves do not indicate all the causes of failure, neither do they indicate clearly the directions of application of stress. On the whole the geologist's attempt to relate specific structures with specific stress systems has not been highly successful. The various structures resulting from rock failure have usually been explained on the simple conception of the application of a non-rotational stress—either tension, causing elongation in the direction of pull, or simple compression, producing a shortening parallel to the principal stress and elongation at right angles to it. A fold, for instance, is assumed to indicate application of stress normal to its axial plane; a set of compressive joints is taken to indicate application of stress at 45° to the fractures; cleavage is taken to indicate application of pressure normal to its plane. Experimental work on rock deformation has been conducted mainly with the same limited assumptions, and the results have been widely quoted and applied to the interpretation of rock structures in the field. These conceptions may be correct as far as the immediate feature is concerned, but the forces are only minor constituents of the major causal movement and give no clue to its direction.

Much less attention has been paid to the conception that the compressional forces may be rotational, that is, that they may be applied in the form of a couple. Under this conception, the net result is a shearing between the heterogeneous rock units along planes ranging from parallel to 45° to the principal axis of stress, the shearing usually accompanied by local tension—in other words, no matter what the origin of compressional stresses and their angle of application, when applied to the heterogeneous rock masses constituting the earth they tend as a whole to act in couples and are resolved into components usually acting in directions inclined to the resulting planes of movement. A mountain making movement under this conception is a shear of certain rock masses over others, resulting in faults, joints, folds, and cleavage. Tensional stresses may be minor consequences of such shear. Field observations within the range of my own experience favor this view of the dominance of shear. It is the view also which geologists have commonly applied to an assumed shear of a thin brittle crust over a thin mobile zone below, though curiously enough not to the local structures that can be observed.

Illustration of Shear Structures.—To illustrate the prevalence of shear structures: Most folds are not symmetrical and indicate by the inclination of their axial planes a drag of one structural unit past another. When this relation is conspicuous they may be called "drag folds." A fold has usually been regarded as indicating direct shortening normal to its axial plane, and therefore application of stress normal to this plane. The Appalachian folds, for instance, have been ordinarily discussed as indicating pressure from the northwest and southeast. The same results, however, can equally well be produced by a differential or shearing movement acting in directions inclined to the trend of the fold axes or to the mountain range as a whole. Experimental reproduction of Appalachian folds under shearing stresses gives more satisfactory results than experiments with normal shortening.[2] The folds indicate the direction of the shortening or elongation, in other words of the nature of the strain, but not the angle of application of the stress.

The interpretation of rock cleavage or schistosity, a common though not the only evidence of rock flowage, affords an especially good illustration of the danger of using narrow assumptions as to its relations to causal stresses. Cleavage is a capacity to part along parallel surfaces determined by the parallel dimensional arrangement of mineral particles. There is abundant proof that the schistose rock has been elongated parallel to the cleavage surface, and cleavage thus becomes evidence of elongation. It does not follow, however, that the stress producing elongation was applied normal to it.

The elongation may well have occurred under a shearing stress of the sort which exists when a mass of dough is rolled out on the table by the application of stress inclined to the table surface. Field studies of cleavage seem to indicate that in the majority of cases

[2] Mead, W. J., "Notes on the Mechanics of Geologic Structures," *Jour. Geol.*, Vol. 28, 1920, pp. 521–523.

the cleavage is merely the expression of the yielding of a weaker formation or weaker part of a formation by a slipping or differential movement between harder members. Even in areas with regional cleavage, the same interpretation may be applied on a large scale when the harder units in adjacent terranes are taken into account. My own observations in old pre-Cambrian terranes tend to the conclusion that cleavage, indicating rock flowage, has been confined to comparatively narrow mesh-like zones between large massifs. The evidence leading to this conclusion that cleavage is the result of slipping between rock masses may usually be checked by drag folds which develop simultaneously in the softer rocks, and by fissures and faults which develop simultaneously in the harder rocks.

The zones of movement marked by cleavage may have almost any inclination or direction, but the plane of the cleavage itself has a strong tendency toward steep inclination or verticality. Both in strike and dip the cleavage is more uniform than the movement zone of which it is a part. The relation is not unlike that between folds and cleavage, to be presently discussed. This steep inclination of cleavage does not necessarily indicate prevailing horizontality of stresses on the assumption that cleavage must develop normal to stress. In part it may have this relationship, but when considered in relation to folds and relative movement of adjacent massifs it more often indicates shearing stresses inclined to the cleavage. So far as any general inference is possible, the tendency of cleavage to show uniform strike and steep inclination over great areas suggests differential movement in vertical or steeply inclined planes, the movements *in* these planes ranging from vertical to horizontal. It can not be explained by movement along planes tangential to the earth, which would require prevalence of flat or gently inclined cleavage. In short the attitude of cleavage, so far as it may be generalized, does not correspond to the conception of the tangential shearing of a competent surface zone over a mobile zone below.

Cleavage has a definite relationship to folds which is of great usefulness in interpretation of rock structures, and which affords valuable suggestions as to the general relations of cleavage to the great zones of flowage of which it is often an expression. Cleavage is approximately parallel to the axial planes of the folds. It therefore usually stands more steeply than bedding and is more uniform in dip and strike than bedding. Where cleavage is noted in a rock outcrop, the direction and inclination of the axial planes of the folds are thereby indicated—not only for the folds within the rock observed, but also, usually, for the folds in the adjacent rocks as well.

As a consequence of the fact that cleavage is roughly parallel to the axial planes of folds, it follows that the trace of any bedding plane on the cleavage surface indicates approximately the direction and degree of pitch of the fold, that is, the inclination of the axial line of the fold to the horizontal. A single fragment of cleavable rock appearing in an outcrop may be sufficient to establish the pitch for a considerable area.

The inclination of bedding to cleavage—always remembering that the latter indicates the attitude of the axial plane of the fold—indicates faithfully the position of the observed bedding on the fold, whether the fold be upright inclined or overturned. This principle is useful in determining whether a bed is right side up or overturned. Inferences of the same sort may be drawn from strike observations on bedding and cleavage in deformed areas.

Still further, cleavage is a phenomenon of rock flowage. The very existence of cleavage, therefore, means the rock has been deformed under the conditions of rock flowage, where the folds are likely to be of a rather intricate type, with much interior thinning and thickening of the beds. Even though evidences of this folding are not immediately at hand, the very existence of cleavage on a considerable scale indicates with reasonable certainty the existence not only of folds, but folds of the rock flowage type.

All of these inferences may be made inductively from a surprisingly narrow range of observation.

These remarks on cleavage apply to the structure ordinarily associated with the deformation of rocks which is almost without exception inclined to bedding or other primary structures. They do not apply to cleavage developed solely by load or gravity, which might reasonably be expected to be horizontal. The latter type of cleavage has been described for certain terranes and districts, as for instance in the Belt series of the Canadian boundary; but within my own observation of deformed areas it is a phenomenon of such local and special character as not to invalidate the generalizations above made. So far as load cleavage is assumed to develop under static conditions of load, without movement, I doubt its existence. Cleavage usually indicates movement, not static pressures.

The interpretation of jointing and faulting has likewise suffered from far too narrow and simple assumptions of the mechanical conditions. Quoting from a recent paper by Mead,[3] such a simple structure as an open fissure or joint "obviously due to tensional stresses (so far as the fissure itself is concerned) may be an incident in simple elongation, shear, cross-bending, compression or shortening, or torsional warping. A reverse fault implies conditions of shortening or compression but may in addition to this possibly be an incident in a general shearing movement, or a phenomenon of simple cross bending, or may be due to torsional warping." In my own field of experience I have been impressed with the frequency of joints and faults developed as incidents in differential or shearing movements. There is rapidly accumulating evidence of the existence of great thrust faults with low dips as prominent features of diastrophism.

When the shearing movements have been determined by the study of a single type of structure like folds, important corroborative evidence may be obtained from other structures. Instead of regarding structures as independent units, each with its own set of mechanical conditions, they may be viewed as a group expression of some major movement. When so viewed the shearing nature of the movement often becomes obvious.

Distribution of Movements.—Within our zone of observation, it is difficult to say inductively whether or not there has been more movement or less movement with depth. Neither is it possible with any satisfactory degree of definiteness to discern controlling attitude or pattern in the complex of movement zones. The zones range from vertical to horizontal, are parallel or intersect. The original horizontal position of stratified rocks naturally suggests dominance of the horizontal element in movements affecting them, because of resolution of forces along bedding planes of weakness, but the beds soon become inclined or vertical when deformed and disturbed zones may be anything but horizontal. The less deformed masses between may have almost any shape. Locally they may be discoidal, or sheet-like, or oval or rod-shaped, or rhomboidal. Interesting attempts have been made to discern some controlling pattern, both in large and in small structural features, but subjective hypotheses enter to so large an extent that the reality of the pattern presented is often not convincing to others.

Possible Increase of Rock Flowage with Depth.—Within a few hundred or at most a few thousand feet of the surface, fracturing, much of it open, is clearly the dominant process, though even here soft rocks may yield by flowage. In the lower part of the zone of observation combined fracture and flowage is the rule. Fractures are more commonly of the closed shearing type. It has been easy to assume that this combination is merely transitional to a zone of flowage below. The fact that rocks which have been deeply buried are often highly schistose as a result of rock flowage has been cited as indicating increased rock flowage with depth. I have shared in this view. From some familiarity with ancient and formerly deeply buried terranes, I am not sure, however, but that a careful inductive study of field sections requires considerable qualifications of this generalization. Many instances may be cited of rock flowage occurring high

3 *Loc. cit.*, pp. 505–506.

in the geologic section and rock fracture below. On the whole, the oldest rocks undoubtedly show greater evidences of rock flowage, though even here such evidences are localized in relatively narrow and numerous zones. These rocks have suffered more periods of deformation, some near the surface and some deep below, than the younger rocks. The present evidences of flow do not necessarily indicate that all the flowage occurred at great depths. Plutonic intrusions of great mass often, not always, cause rock flowage in the adjacent beds, and so far as such intrusions are more numerous with depth, rock flowage may increase. On the other hand, some plutonic intrusions in younger series which have not been very deeply buried likewise cause rock flowage. Certain it is that shearing movements, resulting in displacements which we call faults, have extended down to the bottom of our zone of observation. These partake of the nature of rock fracture in their confinement to planes and in their relations to stresses, but whether the processes be called flow or fracture is partly a matter of definition to which we shall presently make further allusion.

II. THE UNSEEN ZONE BELOW

Below the zone where the evidences of structural failure can be observed, conceptions of the structural behavior of rocks are based on such a variety of assumptions that the layman, and for that matter the geologist, has much difficulty in understanding and reconciling the various views. It is certain that rocks fail in this zone; there is evidence which permits of no other conclusion; but the manner, distribution, and causes of this failure are by no means clear. There are certain fundamental facts upon which any hypothesis must be built.

Known Facts.—Tidal experiments have shown that the earth as a whole is stronger than steel and acts almost as an ideally rigid substance.

The behavior of earthquake waves indicates that the earth behaves as a solid throughout; and for the outer quarter of the earth, at least, the waves increase in velocity of transmission with depth, showing that elasticity and rigidity increase faster than density.

Under surface conditions a dome of the strongest rock, corresponding to the sphericity of the earth, has a calculated supporting strength equal only to a very small fraction of the dome's own weight; but experimental work on deformation of rocks has shown that, with increase of containing pressures or cubical compression, the rock takes on a rigidity capable of resisting enormous stress differences. The range of experimental evidence is not yet sufficient to show the magnitude of these differential stresses necessary to produce deformation under the conditions of pressure which might be reasonably inferred below our zone of observation; but quoting from Adams[4] "the experiments seem to indicate that with a containing pressure of about 10,000 atmospheres, which would be equivalent to a depth of about twenty-two miles below the surface, it would be impossible to make the marble flow, except under a pressure which would be simply colossal." Geologic evidence seems to indicate a supporting strength in the deep zone far greater than that of surface rocks.

The rocks in the deep zone are under higher temperature and greater pressure than in the zone of observation. Some notion of the quantitative values of these factors is afforded by downward extrapolation of observed gradients nearer the surface.

The density of rocks within the zone of observation averages about 2.7; the density of the earth as a whole as determined astronomically is in round number 5. It follows, therefore, that the density of part of the earth must be higher than 5, and that the density of the deep zone must be higher than at the surface; but beyond this the distribution of density in the deep zone, both vertically and horizontally, are unknown.

By means of the plumb line and pendulum,

[4] Adams, Frank D. and Bancroft, J. Austen, "On the Amount of Internal Friction Developed in Rocks during Deformation and on the Relative Plasticity of Different Types of Rocks," *Jour. Geol.*, Vol. 25, 1917, p. 635.

it is known that the horizontal distribution of densities is heterogeneous. The density is low in the earth protuberances and high in earth depressions, as if the earth masses were in flotational equilibrium. The subcrustal densities are balanced against topographic relief. This is called isostatic equilibrium. Certain parts of the earth, called negative elements by Willis,[5] seem to have been subjected during geologic history to long-continued deposition. Other parts, called positive elements, have been more commonly subjected to erosion than deposition. Negative elements are heavy and positive elements are light. Loading and unloading is not necessarily the primary cause of movement, but may serve to accentuate an inherent and prevailing tendency to isostatic adjustment between masses of differing density.

Isostatic balance is not complete. Some parts of the earth vary from this condition, suggesting that they have sufficient strength to sustain themselves in opposition to isostatic tendencies.

The observed relations between density and relief may be explained on the assumption that the differences in density extend uniformly to a depth of about 75 miles, called the depth of isostatic compensation. This figure is favored by geodesists. No one knows, however, the density gradients deep below the surface, or the extent to which there is heterogeneous vertical distribution of density. If instead of assuming the uniform downward extension of densities observed at the surface, assumptions are made of other vertical distributions of density, various other depths of compensation may be calculated, ranging up to several hundred miles. So far as geologic evidence goes, it seems to favor the view that depth of compensation is not uniform.

A comparison of the up-lift of mountain masses with their horizontal shortening indicates how deep the mountain making movements have extended.[6] In general sharp close folding indicates a comparatively shallow depth, whereas broad open folds, approaching the plateau type of deformation, can be explained only by movements of material extending to great depths. Major features of continental and oceanic relief also seem to require the latter inference. If the amount of shortening observed in some mountain ranges were to extend downward indefinitely, mountains much higher than those actually formed would have resulted; hence the conception of considerable movements of a shallow shell without equivalent movement below, and thus perhaps the conception of mobility of an intervening layer, though at widely different depths in different localities.

Geologic evidence points to periodicity in earth movements, indicating that the adjustment to stress is not uniform and continuous.

Finally, magmas originate well below our zone of observation and presumably take part in the mechanical easements. From the known conditions of rigidity already indicated, it seems certain that liquid condition is local and intermittent. Quoting from Gilbert:[7]

> The continuous or secular relations of pressure, temperature, and density in the subterranean region from which liquid rock rises at intervals may be assumed to be such that moderate change of condition either induces liquefaction or else so lowers the density of rock already liquid as to render it eruptible; and such a balancing of conditions implies some sort of mobility.

From these facts it is clear that earth movements extend to considerable depths below our zone of observation, that the movements are periodic, that the earth as a whole is more rigid than steel when subjected to sudden stresses like earthquake shocks or tidal pulls, that it yields slowly and periodically to long continued stress; that as a whole it is sufficiently weak to allow a large measure of isostatic adjustment, but still strong enough to

5 Willis, Bailey, "Discoidal Structure of the Lithosphere," *Bull. Geol. Soc. of Am.*, Vol. 31, 1920, p. 277.

6 Chamberlin, R. T., "The Appalachian Folds of Central Pennsylvania," *Jour. Geol.*, Vol. 18, 1910, pp. 228–251; "The Building of the Colorado Rockies," *Jour. Geol.*, Vol. 27, 1919, pp. 145–164, 225–251.

7 Gilbert, G. K., "Interpretation of Anomalies of Gravity," Prof. Paper 85, U. S. Geol. Survey, 1914, p. 34.

support considerable structures against isostatic tendencies; that it is not essentially molten or fluidal in the ordinary sense; that molten magmas are probably local and incidental.

As to depth and distribution of the movements, and as to the manner of movement, whether by fracture or plastic flow or by some unknown process, there is wide divergence of opinion. Likewise, there is doubt as to the laws or control under which stresses may be transmitted. We may refer briefly to these questions.

Does a Zone of Weakness or Mobility Exist in the Unseen Depth?—A common conception of the distribution of movement deep below our zone of observation confines it to a single spherical zone of weakness or mobility surrounding the centrosphere and surrounded in turn by a rigid shell. This zone is supposed to be marked by a capacity to yield readily to long enduring strains. It may be in part the generating zone of magmas, which may be a factor in its supposed easy yielding. The conception of the existence of a weak and mobile zone has found expression in several ways.

The widely held belief in the existence of a zone of rock flowage below a surficial zone of fracture has commonly carried with it an assumption of the relative weakness and mobility of this zone. In fact "zone of rock flowage" and "zone of weakness" have come to be almost synonymous in discussion of this problem. Doubt as to this correlation is expressed later. Even if the existence of a single zone of rock flowage were proved, it does not necessarily follow that this is a zone of weakness.

Van Hise assigned a depth of only six miles to the top of this zone, though with the important reservation that increased rigidity under containing pressures would greatly increase this figure.

Adams and Bancroft,[8] on the basis of experiments with rock failure under great containing pressures, conclude that the amount of tangential thrust required to produce movements increases so rapidly below the surface "that the great movements of adjustment by rock flow or transference of material in the earth's crust from one point to another—other than the transference of rock in a molten condition—must take place comparatively near the surface," and that the ease of movement "increases rapidly in proportion to their nearness to the surface." The mobile zone thus implied is inferred from experimental results to be limited to depths within 35 miles, below which a condition of no mobility seems to be assumed.

Gilbert conceived "a relatively mobile layer separating a less mobile layer above from a nearly immobile nucleus," the mobile layer agreeing in depth with the depth of isostatic compensation.

Barrell called this weak zone the asthenosphere and assigned its provisional boundaries at depths of 75 and 800 miles from the surface. This he conceived to underlie the zone of isostatic compensation, which was calculated by Hayford to be 75 miles below the surface.

Hayford assumed concentration of movement within the lower part of the zone of isostatic compensation, that is within 75 miles of the surface.

Willis concludes that there is a zone of adjustment below 40 miles and extending to the base of the asthenosphere, and that the adjustments necessary to isostatic undertow take place mainly between 45 and 100 miles from the surface.

In contrast to these conceptions of a deep mobile zone, are the views of T. C. Chamberlin and R. T. Chamberlin, who postulate multiplicity and irregularity of movement zones.

R. T. Chamberlin[9] concludes that mountain making diastrophism affects wedge shaped masses and implies steeply inclined zones of movement.

T. C. Chamberlin emphasizes the superficial nature of diastrophic movements of the mountain making kind, whether these are tangentially compressive or the result of creep of continental masses under gravity. In regard to deeper, so-called massive, movements of the

[8] *Loc. cit.*, p. 635.

[9] *Loc. cit.*

kind reflected in major features of continental and oceanic relief, he does not assume any mobile substratum, but rather steeply inclined zones of movement. As he states it:[10] "Inherited inequalities of specific gravity are, perhaps more than any other agency, the governing power in shaping if not actuating diastrophic movements"—but that "the normal mode of isostatic adjustment in such an earth is thought to be wedging action in the form of movements on the part of its constituent tapering prisms, conical, pyramidal, or otherwise, in response to the varying stresses imposed on them. . . . They should reach to whatever depths may be seriously affected by differential stresses of an order requiring readjustment. No undertow in a hypothetical mobile substratum is necessarily involved and none is postulated."

These are only a few of the views that might be cited to indicate the wide range of hypotheses possible as to depth, number, and attitude of deep mobile zones. The very diversity of these views emphasizes the restricted range of known facts. The requirement of proof naturally rests most heavily on hypotheses which most precisely restrict the locus of movement. So many assumptions must enter into this proof that in our present state of knowledge it can not be rigorous. The safest scientific attitude for the time being would seem to be one of rigid adherence to the known facts, and the recognition of the possibility of more than one hypothesis to explain them. This is not incompatible with a sympathetic attitude toward the efforts of those attempting proof of a single hypothesis.

Until the time comes when it is possible to furnish definite proof of any specific localization of movement, my own inclination is to keep clearly in mind the distribution of movements within the zone of observation, already summarized, as perhaps the best guide to the condition that may be assumed at least for some distance below our lowest observations. This measuring stick is short, but there are some reasons for believing that it is as good as any yet available to measure our course through the complex of hypotheses possible in the deep zone. Especially is it desirable to keep in mind the fact that cleavage, indicating rock flowage, as observed in the deepest part of our zone of observation, does not in general have an attitude required by the conception of tangential shearing in a mobile zone. This does not disprove a different attitude below, but it does eliminate an affirmative bearing on the question which has been sometimes implied.

Are Deep Movements Accomplished by Rock Flowage Rather than by Rock Fracture?—It remains to consider the manner or processes through which deep movements are accomplished, whether by plastic flow, by fracture or by some combination of these kinds of deformation. The widely current hypothesis is that deformation in the deep zone is mainly by rock flowage. The deformed rocks have not been seen, nor have the environmental conditions been accurately measured; yet there are weighty considerations favoring this view:

Experimental work has shown that rock flowage requires containing pressures equal at least to the crushing strength of rocks, and these pressures surely exist in the deep zone. Within the zone of observation even the strongest rocks have locally suffered rock flowage and hence have locally, even at that shallow depth, been under containing pressures sufficiently in excess of their crushing strength to permit flowage. With greatly increased pressures at greater depths it is logical to argue that conditions for flowage would be improved. Under these conditions the resistance to deformation is a function of the internal friction or viscosity of the rock. This property does not of necessity bear any relation to the compressive strength or competency of the rock—qualities which determine its behavior in the absence of great containing pressures. Quartzite or granite, so far as we know, may have no greater viscosity than marble or slate. Adams' experiments show diabase and marble in a composite

[10] Chamberlin, T. C., "Diastrophism and the Formative Processes," *Jour. Geol.*, Vol. 21, 1913, p. 520; Vol. 26, 1918, p. 197.

specimen behaving similarly. In fact marble actually penetrated the harder diabase. Likewise, gypsum penetrated steel. While there are probably differences in the internal friction or viscosity of different rocks under these conditions, the results are nevertheless homogeneous in approximating rock flowage—in contrast to the heterogeneous results under less containing pressures where competency and strength of rocks play a part.

Earth temperatures increase with depth. Increase in temperature aids and accelerates rock flowage. This is evidenced by flowage of hard rocks at moderate depths at batholithic contacts. Also facts of physical chemistry show that increase of temperature increases molecular activity, hastens endothermic reactions (anamorphic reactions are largely endothermic), increases solution, both liquid and solid, and hence recrystallization, and decreases viscosity or internal friction.

Notwithstanding these and other considerations, any conclusions as to the existence of a deep zone in which all rocks flow when deformed is hypothesis, not proved fact, and perhaps will always remain so. The environmental conditions are not accurately known; and even if each of the factors were measured, their conjoint effect is still speculative. Variations in the time factor alone may determine whether a rock flows or fractures. Rock flowage which has occurred in rocks now accessible to our observation fails to indicate increase with depth with sufficient clearness and definiteness to warrant confident downward projection.

Experimental evidence has been construed to indicate that under great containing pressures, of the kind probably existing at depth, the movement under thrust or shear is of the nature of rock flowage, but this is partly a matter of definition. The rock breaks and granulates, often along definite planes, but the parts are still held together; it really flows. Displacements along these planes may partake of the nature of faults, and there is no development of true flow cleavage determined by a parallel arrangement of minerals under recrystallization, the common geologic evidence of rock flow. Presumably with longer time and proper conditions of temperature and mineralizers, parallelism of newly developed minerals, characteristic of rock flow, would result. So far as the experimental results go, however, they fail to exhibit structures which in ordinary geologic field interpretation would be classed as *typical* rock flowage. They would be called fracture or combined fracture and flowage. They would be described as shear planes and faults. They might suggest rupture of the kind that originates earthquake shocks.

Rock flowage has been widely assumed to indicate weakness and mobility. The correlation of rock flowage with weakness may arise from the fact that certain soft rocks such as shales, which are inherently weak, may often be observed to have undergone rock flowage, while adjacent strong rocks have been unaffected. Or, a zone of flowage passing through a homogeneous formation unquestionably indicates movement along the flowage zone, and, therefore, indicates the weakness of this zone relative to adjacent undeformed parts of the mass. But it would be equally valid to argue that where fracturing has been concentrated along a zone between undeformed rocks it too indicates movement, and therefore relative weakness. It is a long step from this to the conclusion that flowage indicates greater weakness than fracture. It is entirely conceivable that it might require more energy to make rock flow than to make it fracture. Indeed there is some reason for believing, both from experimental work and from observations in areas of combined fracture and flowage, that relief actually takes place first and most easily by fracture and that flowage occurs only when it is possible to concentrate much more energy into the rock. Both structures show weakness relative to adjacent undeformed masses, but in relation to each other degree of weakness is a much more complicated problem.

Our question, then, as to the extent to which deep movements are accomplished by rock flowage can not be simply and definitely answered in the present state of knowledge.

The preponderance of environmental evidence seems to indicate that rock flowage is the distinctive kind of movement, but so many qualifications, definitions and assumptions enter into this conclusion that my present inclination is to keep firmly in mind the complex facts of deformation in our zone of observation as a possible key to the interpretation of unseen movements. This attitude will require us to pay more attention than heretofore to the possibilities of heterogeneous structural behavior at great depths. Particularly should we keep in mind the fact that the kind of rock flowage accomplished experimentally produces structures which in the earth have sometimes been called fracture or combined fracture and flowage. We may assume a downward extension of combined fracture and flowage, as observed in the field, and still meet the conditions of flow implied by experiment.

How Are Stresses Transmitted in the Deep Zone?—In our zone of observation stresses are clearly transmitted by the competent members of the lithosphere. In any area of deformation evidence may usually be found of the control of the structure by one or more competent members. When the notion was widely held that the interior of the earth was molten or fluidal, hydrostatic stress conditions were naturally assumed. With the later knowledge that the earth acts essentially as a solid throughout, this view was largely abandoned in favor of the view that rocks in the deep zone act as rigid competent members capable of transmitting stresses in definite directions. The vector properties of cleavage and other structures supposed to develop in this zone were cited to indicate the definite orientation of stresses. It does not follow from this, however, that pressure conditions were or are not hydrostatic, especially under slow movements. Rocks under compression from all sides greater than their crushing strength seem to transmit stresses in a manner suggesting approach to hydrostatic conditions of pressure. When the stress differences are such as to require it, there is movement in the direction of easiest relief. The stress as reflected by the movement would seem to have been transmitted in a definite direction, and yet the pressures may have remained hydrostatic. If we were to imagine a volume of liquid deep below the surface subjected to differential stress sufficient to deform its containing walls, it is clear that the movement would be in the direction of easiest relief, notwithstanding the hydrostatic conditions within the liquid. Periodicity of movement is possible under this conception. Rock structures indicate movement only, not necessarily the inherent stresses. Movement of rocks under the conditions supposed to obtain deep below the surface seems likely to be at least in part a matter of relief of materials so contained between rigid members that the direction of escape is definitely oriented. Of course this supposition assumes that on some scale, small or large, there are units of mass competent to act as retaining walls for materials acting under hydrostatic pressure. If all the mass in the deep zone were under hydrostatic pressure, the retaining walls might be regarded as the solid shell above, inequalities in the competence of which would control the movements in the direction of easiest relief. However, rock structures, such as cleavage and folds, with vector arrangement of the sort observed near the surface and of the sort supposed to exist below, tell us only of the direction of movement and fail to indicate whether the stresses are hydrostatic or otherwise.

CONCLUSION

Within the zone accessible to observation movements of rock masses are accomplished by fracture and flowage. These processes may be distinct and separate, or so interrelated as to make definition difficult. The zones of movement are many, their positions and attitudes diverse. In general they indicate shearing or grinding movements between rock masses, accomplished both by fracture and flowage, and caused by stresses inclined to the zones of movement. This conception is taken to afford the best initial basis for the interpretation and correlation of observed rock structures. There is no certain evidence of increase or decrease of movement toward the bottom of this zone. Beyond a shallow surface zone, there is no certain evidence of increase of rock flowage and decrease of rock fracture with depth. There is no certain evidence that rock flowage means greater weakness than rock fracture. There is no certain evidence in rock flowage that pressures are dominantly hydrostatic or dominantly those of competent solid bodies.

Movements are known to occur in the zone below our range of observation, but their nature and distribution are the subjects of varied hypotheses based on a few known conditions. Much of the sharper diastrophism seems to be confined to a thin surficial zone. Deeper movements, of a more massive type, periodic, and possibly slower, seem to be implied by the relative movement of great earth segments as represented by continents and ocean basins. Their depth is unknown. Most of the current hypotheses agree in assuming a single mobile zone in which rocks move dominantly by rock flowage. The basic requirements of reasonable hypothesis, however, may be equally well met by a conception of movement much like that of the zone of observation. This does not require or postulate the conception of the existence of any single mobile zone, or zone of slipping, or zone of flowage, or of an asthenosphere. It supposes movement irregularly distributed in many zones, with any inclination, and accomplished by both fracture and flowage as far below the surface as movement extends—always remembering that some of the structures geologically described as fractures, may be expressions of mass movement of the kind defined as flow in experimental results.

Conditions of temperature and pressure and vulcanism become more intense with depth, but it remains to be shown that their conjoint action results in a uniform environment, and even if it does, that this condition is not upset by what might be called a heterogeneity of the time factor as represented by differing rates of deformation. If homogeneous environmental and time conditions are assumed, it is yet to be shown that these are sufficient to overcome the heterogeneity of the physical properties of the rocks and to cause homogeneous behavior through any considerable zone. It is not even certain that they may not fix and accentuate the heterogeneous properties of rocks. Certainly in the zone of observation there is comparatively slight evidence of their efficacy in causing more uniform deformation with depth.

In short, as between alternative conceptions as to the conditions in the deep zone, the burden of producing affirmative evidence would seem to rest heavily on any conception involving radical departure from the known irregular distribution and manner of movement within our zone of observation. We come, therefore, to the Chamberlin conception of a heterogeneous structural behavior of the earth.

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SCIENTIFIC EVENTS

DINNER IN HONOR OF THE RETIRING SECRETARY OF AGRICULTURE

THE success of Secretary E. T. Meredith in interesting the public in the investigational work of the U. S. Department of Agriculture has been unique. His prompt recognition of the needs of the department and his activity in behalf of the investigators there, have attracted the attention of scientific men throughout the country. Coming to the secretaryship at a time when the morale of the scientists in many government departments was being seriously impaired through discouragement as to the possibility of securing adequate support for investigation, his campaign of education had the effect both of awakening the public to the extent and importance of the work, and of heartening the workers.