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THE CONCEPT OF NATURAL LAW IN GEOLOGY[1]

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GEOLOGY operates largely without the concept of "natural law." The speaker became keenly aware of this circumstance when, a number of years ago, he began his efforts to derive from rapidly accumulating knowledge concerning the geology of different parts of the earth generalizations to form a reliable foundation for reasoning concerning the dynamics of the earth's crust.[2] When he spoke of them as "laws of crustal deformation," he had to meet the objections of others and his own doubts. He had to view the procedure of geologic investigations in the light of the fundamental methods of all science.

Some of the resulting reflections are presented here before men from many fields of science in the hope that they will lead to a clearer understanding of the nature of the geologist's work, and to that finer sympathy from which springs effective cooperation between men in different sciences upon which further progress depends in a large measure.

Geology is peculiarly dual in its aims: on the one hand, it is concerned with what happened *once* at a *certain place,* in *individual* mines, mountains, regions. Interest that centers on individuals is *history,* not science.[3] As a *science,* geology is concerned with the *typical* that finds expression in *generalizations,* whether they be called laws or something else. In his actual work, the geologist describes the individual and attempts to grasp its meaning in terms of the typical. If you catch him unawares, he will tell you that he tries to "explain" the "facts" of geology in terms of

[1] Presidential address read before the Ohio Academy of Science, at Toledo. A number of minor changes have been made in the final manuscript.

[2] Walter H. Bucher, "The Deformation of the Earth's Crust. An Inductive Approach to the Problems of Diastrophism." Princeton University Press, 1933.

[3] Heinrich Rickert, "Die Grenzen der naturwissenschaftlichen Begriffsbildung." Tübingen, 1929, esp. pp. 217–237.

the "laws of physics and chemistry." But the matter is actually much more complex.

For the purposes of the following discussion, we will do well to remind ourselves first of the essence of all scientific investigation and of the structure of science at large which results from it. Then we shall look at the scientific aspect of the geologist's work and the rôle which the concept of natural law plays in it.

Fairfield Osborn liked to tell the story of the little red-headed fellow he accosted in the elevator one day. "Well, my boy," said Professor Osborn, "what do you like best in the museum?" The little tot, in sepulchral voice, answered "fossils."[4] No doubt the circumstances justified the comic effect this answer produced. Yet the story sounds the funnier the less one knows about the mystery that attends the birth of a scientific career in a young mind. Every experienced worker knows of instances of children fascinated, without rhyme or reason, by a group of objects in a museum. They come back with longing eyes and at last overcome their shyness and ask questions. They want to know.

What is it they want to know? Generally, above all, what things are called—names. Few aspects of science amuse the layman more than what he considers the child-like delight scientists take in cryptic, if not bombastic, nomenclature. There are not a few men of science, in fields less encumbered by verbiage, who are inclined to share this sentiment.

Those who are closer to the realities of the descriptive sciences know better. The plain fact is that apart from troublesome taxonomic tangles every scientific name is a code word for a group of natural laws as significant and dignified as those of any science. When the boy-who-wants-to-know is told that the crystal he is holding in his hand is quartz, he is actually told that any substance of that crystal form, luster and fracture, always, by the inevitableness of experience which we call natural law, possesses a certain hardness, melting point, behavior towards acids, etc.[5]

If the shell he brought is *Polygyra,* he might be told: "Go back to the woods and collect a few shells just like this—they are common—but with the living animal still in it. You will find that animal possessing two pairs of tentacles, the larger ones each with an eye at the tip. You will find the mantle cavity lined with blood vessels, functioning as a lung. Dissolving the animal's head in potassium hydroxide, you will find a narrow ribbon beset with delicate teeth, many in a row, with specific shapes that can better be drawn than described, and so forth."

To the boy such predictions would seem like magic had he not been told early to accept the dry fact that all things have certain properties. The "properties" of objects are the "laws" of the descriptive sciences.

Conversely, it might be said that the "laws" of the so-called exact sciences are but expressions of "properties." When, over three hundred years ago, Kepler recognized, as a result of observations that extended over more than a decade, that the orbit of Mars is elliptical, he merely discovered a "property" of the planet's orbit. This is true, even though his discovery required rare insight, being the result of keen abstractions from a very large number of observations concerning the position of the planet, and involving, furthermore, the assumption that the limited number of determinations was representative of an invariable form of the path.

In the same work (1609) in which he "described" the orbit of Mars, he published two of his bolder generalizations, the first of which stated that the orbits of all planets are ellipses. This, the first of the famous three laws of Kepler, has come down to us as a prototype of "laws." Yet it is purely descriptive.

It is true, it can be expressed in mathematical form. In that case we say, for instance, the paths of all planets are of the form $r = \frac{p}{1 - e \cos \vartheta}$.[6]

But that is no reason for calling it a "law" instead of a "property." Many of the "properties" found in the descriptive sciences can be expressed no less successfully in mathematical form. The shells of nautiloid cephalopods, for instance, grow in the form of a logarithmic spiral, in other words, assume an outline such that the advancing edge follows a path of the form $r = a^{\vartheta}$.[7]

There is a better reason for calling some "properties" laws. It grows out of another aspect of scientific endeavor. The eager youngster in the museum asks not only "What?" but also "Why?" We are ready to tell him the properties of quartz and Polygyra. But then he asks, "Why is the quartz so hard?" and "Why does a radula with numerous, closely set, small teeth go with a shell such as Polygyra?" This is a more serious matter. He asked for "knowledge" before, now he calls for "understanding."

What, precisely, do we mean when we say we "un-

[4] Told by F. T. Davidson, president of the American Museum of Natural History. *Natural History,* Vol. 37, 1936.

[5] H. Poincaré, "The Value of Science," in "The Foundations of Science." The Science Press, 1929, p. 333.

[6] Where r and ϑ are the variables in polar coordinates, p is the "parameter"—the length of the normal erected in the focus measured to its intersection with the curve—and e the "eccentricity" ($e = \frac{r}{d}$, where d is the distance from a given line, normal to the major axis). This is the general equation for all conic sections.

[7] Where, again, r and ϑ are the variables in polar coordinates and a is a constant. See the chapter on logarithmic spirals (Ch. XI) in D'Arcy W. Thompson's fascinating book, "On Growth and Form." Cambridge, 1917, pp. 493–586.

derstand," in matters of science? In place of giving an abstract answer, let us picture to ourselves what happened when men learned to understand a simple fact of nature, for example, the *rainbow*.

First of all, this common meteorological phenomenon had to be observed carefully so that it meant more than a curved streak of colors in the sky. Its properties had to be observed and defined carefully, each of which has truly the dignity of a law, such as, "rainbows are segments of circles; the center of the rainbow lies on a straight line drawn through the sun and the observer's eye; a single rainbow is always violet on the inside and red on the outside," and so forth. But even had he had all this knowledge, Aristotle had no means of understanding even the simplest elements of the geometry of the rainbow. He ascribed it to the reflection of the sun's rays by the rain. He must have felt as unconvinced as we do about many so-called "explanations" of present-day geology.

Two hundred years later Ptolemy made a good start toward a study of refraction in what has been described as "the most remarkable experimental research of antiquity."[8] But no one seems to have noted a possible connection with the rainbow. When the scientific spirit awoke again near the end of the Middle Ages, it was recognized that rainbows owe their origin in some way to refraction, perhaps combined with reflection. Real understanding had to await knowledge of the true law of reflection. When W. Snell discovered (in 1621) the quantitative law of refraction, Descartes was quick to apply it to the problem of the rainbow. His calculated angle agreed with that observed. Men now "understood" the geometric laws concerning the rainbow as special cases of the broader law of refraction. The colors, however, remained a mystery. There were futile attempts at explanations, of course. We would call them working hypotheses to-day. Men might even have entertained "multiple hypotheses" in their search for a solution. The fact remains that no matter how large a number of multiple hypotheses was made, no solution could be found until the basic law was discovered, and cast into suitable form, upon which the solution depended. When Newton found the laws of dispersion, understanding came in a flash. "Understanding," then, as we use the word in science, consists in recognizing a specific law as the special case of a more general law.

Newton's demonstration that all three of Kepler's laws, apparently so different in content, are merely the geometrical implications of an abstraction of by far more general character, is perhaps the most illustrious example of an intrinsically great feat of understanding.[9]

[8] G. Sarton, "History of Science," Vol. 1, 1927, p. 274. Quoted from W. C. D. Dampier-Whetham, "A History of Science," 1931, p. 54.

Quite instinctively, certainly without any particular method, we speak of "laws" when we recognize generalizations that are fraught with possibilities of understanding—in contrast to those generalizations which establish relationships to be understood—the properties. Thus we speak of Osborn's law of adaptive radiation in biology, of Ferrel's law in meteorology, of Rosenbusch's law in petrography. The more general a law, the greater its resolving power. In order to be general, it must be abstract. The more abstract it is, the more amenable it is to mathematical treatment —the more exact is the form in which it can be stated.

The sciences that face the objects of experience in their full complexity—such as the plants, animals, rocks, topography, weather—find the mathematical language inadequate to their task. The curves defining the shape of a trilobite or of a granite intrusive, like the flow lines of a weir in a river or of the air on a weather map, defy mathematical analysis. Accurate description of reality seen in nature and in experiment form the accumulated store of scientific knowledge in these sciences. From it there arise, through judicious generalizations, "specific laws" that give us the first sense of order in the concrete aspect of complex reality. Back of them, revealed by greater and greater abstraction from reality, we distinguish the more and more general laws of chemistry, physics, mechanics, electrodynamics, mathematics. This is the familiar picture of the hierarchy of the sciences. It is repeated in the structure of each branch of science.

In geology, we see geophysicists, students of geotectonics, regional geologists, stratigraphers, petrographers, mineralogists gather observations that are to lead ultimately to an understanding of the complicated spectacle of the earth. Like horizontal strata of decreasing complexity, lie the levels at which these men are working, each forming at least part of the foundation for the work of those above it.

At each level laws must be formulated without which understanding is impossible at the higher levels of complexity. This is the picture the geologist must keep before him if he is to do his best work and derive the deepest satisfaction from it.

Let us now see how this works out in practice. Take the case of the igneous rocks. Observations led early to the definition of numerous types and to attempts at classification. Mineral composition and texture were studied comparatively. Here, as always, comparative studies led to generalizations which were more than mere properties of types, generalizations that had the

[9] The proof with the mathematical tools that Newton had at his disposal was a very great achievement indeed. In modern vector notation the proof is quite simple and occupies not more than two pages for all three laws. See Louis Brand, "Vectorial Mechanics," New York, 1930, pp. 401–403.

character of empirical laws. "Nephelite-bearing rocks are free from quartz," is one. Rosenbusch's law concerning the typical sequence of (final) crystallization, from basic to acid minerals, is another.

Not until such laws were formulated could any serious desire for explanations, for "understanding," arise. Before, it would probably have seemed rather silly to ask, "Why is a basalt constituted the way it is?" One might as well ask, "Why do radulas of the Polygyra type go with the shells that characterize that genus?" That is the way it is—that is all.

It would certainly have been futile to go to the classical chemistry of the last century for an answer. No number of multiple hypotheses could have led to the trail of understanding; because understanding could not even be conceived until physical chemistry had reached the state of the phase rule.

But even after the basic concepts had been created, much work had to be done before an understanding of the characteristics of mineral associations in igneous rocks became possible. The level between the abstract theory and the complex reality of the rocks had to be filled by experiment on the properties of silicate melts. Investigations in that field are expensive. Industry was slow in entering that no-man's land. When the geophysical laboratory of the Carnegie Institution of Washington was established, it undertook to fill the gap.[10] The Kaiser Wilhelm Institute für Silibatforschung in Berlin-Dahlem was modeled after it. For over three decades the work has gone on. One by one the empirical laws of petrography are becoming intelligible, if not fully understood, in the light of the properties of silicate melts. Thus formulation of empirical laws, systematic exploration of artificially simplified systems: those are the steps from which springs the understanding of reality which is the ultimate aim of science.

Let us turn to another illustration. In a sense, mineralogic-petrographic studies on igneous rocks are not typical of geologic work. The units with which they deal can be taken into the laboratory, weighed, studied under the microscope, analyzed chemically.

Geologic investigations in the strict sense of the term, on the other hand, deal with sizable parts of the earth's crust. While these are, of course, small compared with the objects studied by the astronomer, they have the disadvantage of being right up against us. They are vast compared to man's size. The amount of physical labor and time consumed in merely traversing the objects of study looms large. The methods of the surveyor and cartographer and graphic representations of diverse kinds must be used to record the knowledge gained. In typical cases the work of several men and of several field seasons is required to furnish a reasonably accurate description of one object of study, one individual, if you please.

Take the case of the oil-bearing strata of the Green River shale—sediments of a long extinct lake in western Colorado, Wyoming and Utah. In order merely to describe this stratigraphic individual adequately, it is necessary to traverse along as many lines as possible the 25,000 square miles covered by these sediments; to study the variations in the sediments, especially the near-shore phases; to measure the rhythmic lamination of the sediments, which is such a conspicuous feature of the formation, throughout the 2,000 feet of thickness and to compare the results for as many sections as possible; to look for and study carefully zones with significant minerals, such as a rather unique analcite bed and more numerous layers of other peculiar minerals; to study the nature of the organic material, chemically and above all with the microscope and microtome; to make collections, as exhaustive as possible, of the fossil leaves, insects, fishes, for which the beds are famous, and to establish the taxonomic identity of the microscopic and megascopic forms found. Vast labors in the field, in the laboratory, the herbarium and museum by a score or more of men were needed to complete, in the course of years, a reasonably accurate description of this formation.

The result of all this labor is the description of a *single individual,* one body of deposits. No laws can be derived from this individual. But laws are needed to understand its properties.

Take the most conspicuous property of these beds, the fine lamination that runs with mathematical accuracy through every microscopic section, in endless alternations of pairs of layers, measured in tenths of millimeters, one chiefly organic matter, the other largely inorganic. What does it mean?

Many possible explanations come to mind. The geologist, more than other men of science, must work with multiple hypotheses. For understanding, the geologist turns to limnology. One "law" of limnology suggests an explanation: the plankton of lakes reaches annually a relatively short peak, dropping to a minimum for the rest of the year. The microscope shows that the organic matter of the laminae consists of plankton. Here is a possible answer. Another "law" of limnology[11] points to the same answer: the deposits

[10] It is instructive to read the diverse suggestions which were received by the advisory committee on geophysics from outstanding geologists and geophysicists, at the founding of the Carnegie Institution (Year Book No. 1, 1902, pp. 44–70) and contrast with them the purposeful limitation of the work which was ultimately adopted and which has led to such uniquely valuable results.

[11] B. W. Perfiliew, "Das Gesetz der Periodizität der Schlammbildung und die Tiefgewässer-Bohrung." Verh. Intern. Verein. f. Limnologie, 1931, Vol. 5, pp. 298–306.

at the bottoms of modern lakes show similar alternations of organic (planktonic) and inorganic laminae.

The Green River beds represent part of the Eocene period. Radioactive minerals have furnished data which give a rough estimate of the length of the Eocene period. This can be compared with the length of time represented by the fraction of it comprised in the Green River shales, computed on the assumption that each pair of laminae records one year. The figure found, 6,000,000 years, compares favorably. One more test suggests itself. The thickness of the pairs of laminae is variable. If they represent deposits of one year each, their varying thickness must somehow be connected with fluctuations of climate. A "law" of climatology proves useful: climatic variations proceed in complex curves in which definite cycles are recognizable. When plotted, the laminae of the Green River shale reveal the same cycles.

Thus each new investigation points in the same direction: we are getting nearer and nearer to a satisfactory understanding.[12]

This case is typical of geological work. The empirical laws formulated in the course of studies on living environments and processes—although not labeled as such—form the means of understanding the properties of an individual of stratigraphy—the Green River lake beds.

But there are other famous fossil lake beds—like those of Florissant in Colorado or of Oeningen in Switzerland and many others, little studied. As one by one they are described and analyzed, common properties will be recognized and formulated. There are already in existence interesting discussions of the "Criteria" for lacustrine sediments.

The field of structural geology represents a higher level of complexity in geologic studies. In certain belts the strata of the earth's crust lie deformed: broken by fractures, or crushed along shear zones, bent into folds and thrust one on top of the other along thrust planes. Here the difficulties in the way of study are multiplied. Even if the earth's crust were transparent and the actual features of the deformation could actually be seen to the depth of a mile or two, the mere task of recording, in intelligible manner, the three-dimensional pattern of a folded mountain chain would present great difficulties. With only the surface accessible to the eye and most of it concealed by vegetation and mantle rock, the unraveling of the structure of such regions as the Alps or the Scottish Highlands represents a herculean task, for the body as well as the mind.

(Known to the writer at present only through a review in *Bot. Zentralbl.*, Vol. 164, p. 98, 1931, to which Dr. Th. Just called attention.)

[12] I have quoted from a series of classical investigations by W. H. Bradley, of the U. S. Geological Survey. See especially U. S. G. S. Prof. Paper 158, 1929, pp. 87–110; *ibid.*, Prof. Paper 168, 1931, 58 pp. and 28 plates.

While the anatomist can produce hundreds of sections with his microtome in a fraction of an afternoon, it takes years of topographic surveying and decades of geologic mapping, each man refining and correcting the observations of his predecessors, before a series of reasonably accurate cross-sections can be produced through such a region as the Swiss Alps or the folded Appalachians. Here again, then, the descriptions of individuals is still, by force of circumstances, the dominant occupation of the geologist.

Nevertheless, it is just in this field that the search for typical patterns, for laws of deformation began early. Types of folds and patterns of fractures were early recognized and given names. The practical importance of many structural features in mining and prospecting led to a high development of the formal aspects of rock deformation. As early as 1878, Albert Heim's "Untersuchungen über den Mechanismus der Gebirgsbildung" brought exemplary precise formulations of valid generalizations.

But understanding of the structural facts in terms of more basic laws has barely begun. For classical physics, proceeding along strictly inductive lines, had limited itself at first almost entirely to the simplest aspects of the deformation of bodies, which are below the elastic limit, where relatively simple equations can be tested by crucial experiment. Valiant attempts were made by geophysicists to attack the problems of rock deformation with the inadequate tools of the classical theory of elasticity. Papers were written that were learned but not convincing. Understanding could not be expected until physicists ventured forth from the abstract field of simplest cases into the realities of the world of bodies at large. This time industry has filled the gap with its practical demands, and a new empirical science, the physics of materials, has come into existence. Such basic properties as plasticity, strength, rigidity, brittleness are being studied exhaustively. Empirical laws are being cast into mathematical form. Fracture patterns and other forms of deformation are being studied and analyzed. In that way laws of behavior of materials are being formulated out of which ultimately will come a true insight into the nature of geological structures.

One phase of modern studies of granite massifs may serve as a concrete illustration. At the time when the merciless inflation of the after-war period crippled the activities of many German geologists by making distant traveling impossible, Dr. Hans Cloos at Breslau took a devoted body of students into the numerous granite quarries of the near-by mountains of Silesia and carried on systematic studies of the innumerable fractures that cut every granite body in nature.[13]

[13] He carried out near his home investigations inspired in pre-war days by his careful studies of beautifully exposed granite regions in the deserts of Southwest Africa.

Thousands of readings giving the attitude in space of all important fracture surfaces observed were plotted graphically. Soon definite systems of fractures were recognized that run uniformly through a granite mass: fractures aligned at right angles to the strike; longitudinal fractures; conjugate shearing planes, and pinnate fractures along the contacts. This assemblage of fractures has since been found to be practically universally present in intrusive bodies of granite.[14] Their presence constitutes empirical laws of great value.

When these empirical laws had been established, Dr. Cloos undertook to supply the knowledge that was needed for understanding. In a series of ingenious laboratory experiments, he has attempted to reproduce the essential aspects of orogenic deformation. Reasoning in terms of factors of similitude, he used a mixture of water and clay for his experiments, a substance yielding to gravity and yet of such coherence as to fracture after the fashion of such solids as granite.[15] When he showed his extraordinary results at the next International Congress, more than one geologist shrugged his shoulders: "What can one learn from wet mud about granite?"

The highest degree of complexity is reached when the earth as a whole becomes the object of study—in what is now widely called geotectonics. Here it is not the details of structure of individual mountain ranges, but the pattern of folded mountains on the face of the earth as a whole that demands attention; not the relations of one plateau to another, but that of the continental platforms to each other and to the oceanic floors; not the events of volcanic outbursts in one region, but the connection that exists between belts of volcanism and belts of deformation; and so forth.

To the complexities of three-dimensional structure, the time element is added as the fourth dimension. The geologist's intricate and still very crude methods of relative timing must be used to determine whether epochs of folding are short, spasmodic events, or long-drawn-out processes; whether mountain folding takes place simultaneously in many parts of the earth followed by periods of quiet or whether some folding has been going on somewhere on earth throughout geologic time; whether epochs of mountain folding coincide with times when continents became submerged by a rising sea level, or with times of emergence—and so forth.

In the four volumes of "The Face of the Earth," a work of rare compass, depth and beauty,[16] the product of thirty years,[17] Eduard Suess has laid the foundation for all such inductive studies concerning the deformation of the earth's crust. In this work the masterful picture of the then available knowledge of the structure of the whole face of the earth, covering over 2,700 pages, culminates in a few chapters of "Analyses" and "Hypotheses" of only 138 pages[18] which grip by their moderation, the tentative and cautious tone of their interpretations. Fertilized by the fervent enthusiasm and the almost inexhaustible riches of this work, structural analysis has since been carried on systematically into most parts of the world. The detailed literature on regional tectonics has grown beyond the grasp even of a genius. But the growing number of authentic summary accounts and of tectonic analyses of limited areas, on the other hand, is making accessible more and more the generalized results from many parts of the earth. From them gradually broader generalizations are being derived that hold good everywhere to-day and will stand the test of future observations, generalizations that are true "laws." A few such laws were established long ago, as, for instance, Elie de Beaumont's law of the episodic character of orogeneses (1829); Godwin-Austen's law of the tendencies of posthumous folds to trend parallel with the lines of earlier folding (1856); Hall's law of the coincidence of belts of folding with belts of abnormally thick sediments (1859).

Ten years after Suess' death, Stille published his "Grundfragen der vergleichenden Tektonik."[19] The title of this remarkable work makes effective use of the expression "Comparative tectonics" and the text culminates in the formulation of a "law," Stille's "orogenes Zeitgesetz."[20] It is a broad generalization based on two properties of orogenic movements concerning their incidence in time. It is a general law of broad resolving power. The writer believes it to be valid. But geologists are by no means agreed that the two properties on which the law is based are really general and essential. Such a generalization can hardly be called more than an "opinion," until it can be shown to rest on a valid basis. On the whole we are not ready to formulate such broad laws. The first

14 For a bibliography of papers by Cloos and his collaborators, see Robert Balk, *Bull. Geol. Soc. America*, 36: 695–696, 1925. (A larger paper by the same author, entitled "Structural Behavior of Igneous Rocks," will soon appear as one of the Memoirs of the Geological Society of America.)

15 The following papers deal with tektonic experiments of this type in general: H. Cloos, *Centralbl. f. Min.*, Abt. B, 1928, pp. 609–621; *Natur und Museum*, 1929, pp. 225–243; *ibid.*, 1930, pp. 258–269; *Naturwissenschaften*, 18: 741–747, 1930; *Geol. Rundschau*, pp. 353–367, 1930; *Naturwissenschaften*, 19: 242, 1931.

16 In his epilogue to the French edition, Termier has called this work "bien un poème, dans le sens complet de ce mot splendid qui veut dire création." "La Face de la Terre," Vol. 3, pt. 4, p. 1713, 1918.

17 Eduard Suess, "Das Antlitz der Erde," Vol. I, 1883; Vol. II, 1888; Vol. III, pt. 1, 1901; Vol. III, pt. 2, 1909.

18 In the English edition of Sollas and Sollas.

19 Berlin, 1924 (Gebr. Borntraeger).

20 *Loc. cit.*, pp. 44–45.

task is to crystallize knowledge concerning the observable properties of the structure of the earth's crust. Stille's book is primarily devoted to this task as far as the time element in orogenesis (and epeirogenesis) is concerned. The speaker has attempted to extend this work to other properties. On the strength of the convictions set forth in this address he spoke of them as "laws," *i.e.*, "specific laws" of crustal deformation, from which ultimately such "general laws" as Stille's will be derived. He might have listed his "specific laws" as "properties." He preferred the more challenging "law." The word he could not use, suggested to him more than once, was "theses." "Too much of the geotectonic literature of the past was concerned with the theses of men rather than with laws of nature."[21]

This, then, is the essence of all geologic work:

(1) Not to be satisfied with the accumulation of facts alone, but to draw from them, at every opportunity, all possible generalizations. In other words, to search for the general properties, the "specific laws" in the reality with which we deal, and ultimately for such "general laws" as can be safely formulated.

(2) To seek understanding of the empirical laws thus found by studying the findings of workers in the nearest level of complexity. For the geologist, that means stepping from the records of the past to the experiences of the present; from the properties of vast bodies to those of small units of the laboratory; from the complex systems of nature, to simpler systems capable of analysis. It means oceanography, limnology, hydrology, the physics of materials, the physical chemistry of silicate melts, and so forth, not simply elementary text-books of chemistry or physics.

Neither of these two points is followed as widely as one should expect. As to the first, a multitude of facts is still published with little or no evidence of thought concerning their broader bearings. As to the second, uncontrolled speculation still too often takes the place of that systematic searching for an adequate foundation for understanding which is needed. When we try to understand the Permian glaciation, we still invoke movements of the continents or of the poles (or both), instead of instigating that prolonged systematic research by competent meteorologists which alone can make sure that wide areal glaciation down to sea-level is impossible in equatorial latitudes; we use similar speculations when we speak of the existence during the geologic past of higher vegetation near the poles,[22] without having brought about investigations needed to decide if trees can grow where the night and day last half a year each; we demand vertical uplift of the continental shelves of nearly two miles to "explain" the submarine valleys before having placed the problem into the hands of experienced hydrologists familiar with the complex currents set up by the tides, and so forth.

Speculation that does not lead directly to further search for facts and laws is idle. The joy of understanding arises from reasoning, not from guessing; from the purposeful grouping of general facts, not from the mere play of the imagination.

Two things remain to be said. One refers to the human side of this task. The other points to the future outlook.

For the human side let us turn back once more to the boy-who-wanted-to-know in the museum. Let us assume that his enthusiasm held out through his high-school years—we are optimists, you see. He has pushed through as fast as possible, impatient of any delay, to that field of science which at the time satisfied his fancy. He has become a geologist. The more he learns about the strata and structures, the less he is satisfied with the mere description of "facts." He turns to basic works on limnology, hydrology, physics of materials. And now he finds much that he can not read. Not only a strange terminology, but the unfamiliar concepts and lines of reasoning of the basic sciences: physical chemistry, vector mathematics, statistics, etc. He wishes he had become a mathematician first, then a physicist and then a chemist. He looks with envy upon those who work in the abstract fields of artificial simplicity, where knowledge is exact and reasoning rigorous. His own problems seem muddled, his own mind inadequate.

But when he seeks cooperation from the fortunate ones who are mathematicians and physicists, he makes a discovery. Just as he, in the typical case, at his stage of life can not spend the energy and time necessary to acquire a working knowledge of the mental tools of the basic sciences, so the workers in the abstract sciences find the task too great to acquire the perspective over the intricacies of unabstracted reality without which creative work is impossible. Moreover, he who is accustomed to the "linear order of thought which is necessarily cultivated in . . . mathematics" finds it distasteful, if not difficult, to acquire "the habit of parallel . . ." or "complex thought," as T. C. Chamberlin[23] once called it, in which a multitude of interrelated properties must be visualized and weighed as parts of an integral whole.

The recognition of this difference is wholesome.

[21] Walter H. Bucher, "The Deformation of the Earth's Crust," p. vii (Preface). Princeton, 1933.

[22] Since this was written, coniferous wood of araucarian type has been reported, found together with leaf impressions of cycadophytes in an Upper Triassic or Jurassic florule associated with coal, which was collected in south latitude 86° 58′, *i.e.*, within 3 degrees of the South Pole. Wm. C. Darrah, SCIENCE, 83: 390–391, 1936.

[23] T. C. Chamberlin, *Jour. Geol.*, 39: 164, 1931. Reprinted from the original in *Jour. Geol.*, 5: 837–848, 1897.

Every geologist needs such an experience to find the right pride in his work. The worker in the complex levels of science is the explorer, the man from the basic sciences the guide. One derives his significance from the other, and both derive the sense of dignity in their calling from the joint goal which is an ordered insight into complex reality.

As to the outlook for the future: I have spoken so far of two steps that enter into the mental process of the geologist. But there are really three:

First comes *analysis, "re-solving"* the complexity of geological reality into its constituent, essential elements of orderly recurrence, formulated as generalizations which we call "properties" or "specific laws."

Second, *interpretation, "translating"* these "properties" or "specific laws" into the terms of the more "general laws" of the basic sciences. This constitutes "understanding."

The third is *synthesis, "putting together"* again, collecting in our consciousness the numerous threads of understanding into one adequate perspective of nature as we see it in its entirety. "Analysis" is in full swing. "Interpretation," and with it "understanding," is growing, as laws are being formulated in the fields of investigation that lie between geology and the basic sciences.

But in the larger aspects of geology the final synthesis which is to give us a satisfying, logically consistent and coherent picture of the earth's crust and the forces that have formed and are forming it, is a hope for the future. It will be brought about by the growth of a tendency which is taking form now. Analysis produces specialists; the growing need for synthesis will produce encyclopedists. H. G. Wells put it this way: "The world, if all goes well with it, will consist very largely of specialists, who know every detail about and every relationship of something, and of people . . . who will know everything in outline and in correlation. Specialism and encyclopedism are necessary correlatives."

Analysis and interpretation require the field and the laboratory. Synthesis is done in the philosopher's study. Most geologists, and for that matter most scientists at large, mistrust the man who studies not nature, but natural laws. His work looks like the boring and hoarding of the bookworm. Most scientists are still unaware of the difference between unimaginative compiling, the cataloguing of facts, and the creative power that is required for all true synthesis. Yet it is such synthesis alone which can make us fully aware of nature. For, to see the work of many in one perspective, to realize the common bond of natural law that permeates all our experience of nature, means to realize the "internal harmony of the world" which, as Poincaré pointed out repeatedly,[24] is "the only true objective reality," the ultimate goal of all science.

[24] H. Poincaré in "The Foundations of Science." The Science Press, pp. 207, 209, 1929.

THE STRUCTURE OF NATURAL AND SYNTHETIC ANTIGENS[1]

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WERE I to confine my remarks, in opening this discussion, to a literal interpretation of the subject or to definitely established facts, our committee's generous time allowance would be much too long. I must therefore risk your disapprobation, to fill the time allotted, by taking a broad view of the topic and by entering occasionally upon the realm of the speculative. As I have taken the bit in my teeth, let me also give credit here to all my colleagues in this field, and since you will have no difficulty in recognizing the contributions discussed, I may be permitted to save time by mentioning few names.

With regard to the structure of the natural antigens of the type of the animal proteins there is little that can be said. Denaturation effects a change in specificity, as does also reduction of disulfide linkages, but the latter effect may be due to a splitting of the molecule at the –S–S– linkage as well as to the conversion of these groups into –SH. Of greatest promise, however, has been the work of Bergmann on gelatin and Waldschmidt-Leitz on clupein, substances which are not antigens at all. Extension of these fundamental researches on the number and arrangement of the amino acids to more complete proteins will afford a direct approach to an understanding of the chemical basis of the specificity of proteins, the class of substances to which most antigens belong. Work along these lines should eventually clear up the riddle of species specificity and tell us why, and how, for example, horse serum albumin differs from human serum albumin. While it is probably the occurrence of definite groupings of amino acids which confers the property of serological specificity on these natural

[1] Opener's address, Joint Discussion on the Structure of Natural and Synthetic Antigens, Second International Congress for Microbiology, London, July 27, 1936.