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The Value of Outrageous Geological Hypotheses

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Source: *Science*, New Series, Vol. 63, No. 1636 (May 7, 1926), pp. 463-468

Published by: American Association for the Advancement of Science

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# SCIENCE

VOL. LXIII

MAY 7, 1926

NO. 1636

## THE VALUE OF OUTRAGEOUS GEOLOGICAL HYPOTHESES<sup>1</sup>

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SCIENCE: A Weekly Journal devoted to the Advancement of Science, edited by J. McKeen Cattell and published every Friday by

THE SCIENCE PRESS

Lancaster, Pa. Garrison, N. Y.

New York City: Grand Central Terminal.

Annual Subscription, \$6.00. Single Copies, 15 Cts.

SCIENCE is the official organ of the American Association for the Advancement of Science. Information regarding membership in the Association may be secured from the office of the permanent secretary, in the Smithsonian Institution Building, Washington, D. C.

Entered as second-class matter July 18, 1923, at the Post Office at Lancaster, Pa., under the Act of March 8, 1879.

MEETINGS of geological societies in these modern days are often somewhat prosaic as compared to those of an earlier time when the limits as well as the methods of geological speculation were less defined than now, and when contradictory differences of opinion were commonly expressed even with regard to fundamental ideas concerning the conditions and processes of earth history. That was a time when the scientific imagination, not so much hampered as it is now by standardized principles, was accustomed to roam with little restraint over the unexplored fields of geological investigation; a time when the facts regarding the earth's crust had been gathered from a relatively small part of its surface, when a theory was thought to be established if it explained nothing more than the facts which it had been invented to explain, and when lively discussion as to the merits of rival theories too often degenerated into polemical diatribes between rival theorists.

In those earlier days, attendance at the meetings of Section E of the American Association for the Advancement of Science—the only meetings in which geologists from different parts of the country were then brought together—was likely to be rewarded by a vigorous, not to say vituperative dispute between Marsh and Cope, not merely as to the completed structures and systematic relationships of the fossil vertebrates that they were finding in the fresh-water Tertiary deposits of the west, but also as to mere priority in finding and naming the fossils; and so eager was each of those eminent worthies to secure his prior claim for a new find before the other came upon it that, according to stories then current, one or both of them sometimes, while still in the western field, resorted to the telegraphic announcement of a name for a newly discovered fossil to be published in the eastern newspapers. In the years, half a century ago, when I first attended the meetings of the Boston Society of Natural History, they were occasionally the scene of emphatic contradictions between T. Sterry Hunt and M. E. Wadsworth on matters petrographic, for that recondite branch of geological science was then just taking form among us. Hunt knew exactly how rocks ought, in accordance with his theoretical views of terrestrial chemistry, to be constituted; while

<sup>1</sup> An address delivered before the Leconte Club of the University of California at its annual meeting at Berkeley, February 21, 1925.

Wadsworth, in view of his observational study of thin sections, knew exactly how rocks are constituted; and each of these convinced positivists maintained his view with earnest vehemence.

It is as a result of many verbal battles then fought without asking or giving quarter that geology has come in these modern days to be a relatively well-restrained and orderly science. How much more carefully are facts scrutinized, and how much larger and safer is the inductive base of our generalizations now than formerly. How narrowly limited is the special field, either in subject or locality, upon which a member of the Geological Society of America now ventures to address his colleagues; so narrow that he often has it pretty much all to himself, and so thoroughly does he cover it that when his statement is completed there is little or nothing left for any one else to say. How much more rigorously logical is the guidance of the train of thought by which advance is made from the facts of observation to the conclusions of theory; and if by good fortune a hearer differs from a speaker as to the track along which the train of thought should be directed, how seldom does he intimate his difference of judgment in any but the most courteous manner! How utterly extinct is rudely polemical dissension; so extinct indeed that the younger geologists of to-day must be surprised to learn that it ever flourished. I wonder sometimes if those younger men do not find our meetings rather demure, not to say a trifle dull; and whether they would not enjoy a return to the livelier manners of earlier times.

Yes, our meetings are certainly prosaic to-day as compared to those of the earlier formative period when speculation was freer and when differences of opinion on major principles were almost the rule rather than the exception. Our younger members may perhaps experience a feeling of disappointment, or even of discouragement at the unanimity with which the conclusions of an elder are received by a geological audience; for it must dampen the enthusiasm of beginners if they gain the impression that all the larger generalizations of our science have been established, thus leaving for them to discover only items of localized fact. And a like feeling of discouragement must often be shared by the chairman of a meeting when, after his encouraging invitation, "This interesting paper is now open for discussion," only silence follows. Are we not in danger of reaching a stage of theoretical stagnation, similar to that of physics a generation ago, when its whole realm appeared to have been explored? We shall be indeed fortunate if geology is so marvelously enlarged in the next thirty years as physics has been in the last thirty. But to make such progress, violence must be done to many of our accepted principles; and it is here that the

value of outrageous hypotheses, of which I wish to speak, appears. For inasmuch as the great advances of physics in recent years and as the great advances of geology in the past have been made by outraging in one way or another a body of preconceived opinions, we may be pretty sure that the advances yet to be made in geology will be at first regarded as outrages upon the accumulated convictions of to-day, which we are too prone to regard as geologically sacred.

It was outrageous, two centuries ago, to interpret fossils as records of ancient life; for that interpretation did violence to the view then accepted as to the manner in which the earth had been formed and as to the date at which life had come to exist upon it. It was outrageous, little more than a century ago, to discover fossils of marine organisms in the disordered strata of lofty mountains high above sea level; for that discovery did violence to the ideas then obtaining as to the stability of the earth's crust. And it was equally outrageous, half a century ago, to be told that after mountains had been lifted up, they might in time be worn down to lowlands again, for that idea did violence to the views that had then come to be held regarding the instability of the earth's crust. It was an outrage upon the tacitly accepted principles of geological climatology, based on the postulate of a cooling earth, that there already should have been a glacial period in the past; and for that matter, the form in which the glacial theory was first promulgated was truly enough outrageous; nevertheless it now, in a much modified form, holds good as a standardized geological verity.

It was altogether outrageous to think that man had long been an inhabitant of the earth, instead of looking upon him as a new comer; and it was equally outrageous to discover that the sequence of fossils preserved in successive stratified formations indicated such a progression of life as would result from the evolution of later forms from earlier forms, instead of simply an arbitrary succession of independent creations. It is still rather outrageous to think that the earth has long been and possibly is still heating itself up by the slow compression of an originally uncompact interior under the weight of a heavy exterior, instead of thinking that it has long been and still is cooling by the slow loss of a great original store of heat. And in view of the many evidences of crowding in the outer crust, it may be thought wantonly outrageous to look upon the earth as possessing an expanding interior which, like the caged starling, "wants to get out." Yet I believe it the part of wisdom to view even that outrage, as well as the Wegener outrage of wandering continents and the Joly outrage of periodical subcrustal heating-up and breaking out, calmly, as if they were all possibilities; and it may

also come to be the part of wisdom to ask ourselves in what way and how far our present conception of the earth must be modified in order to transform such outrageous possibilities into reasonable actualities; for that is precisely the way in which the above-listed outrages and many others have gained an established place in our science. Of course, if we do not approve of the necessary modifications we may reject them, and with them the outrages that they countenance.

Let it be noted in passing that the omission of the original L from the leading word of the preceding paragraph unfortunately results in its being pronounced as if it were derived from "out" and "rage"; its true meaning would be better indicated if its form were outrageous, as it might well have been had not the L been lost on the way from Latin through French into English; for with the L preserved, the T and R would be joined in the second syllable and properly separated from the first. A word of opposite meaning would then be, not in-rageous, but in-trageous; and our language would be much more symmetrically developed if that and many similar opposites were added to it. However, if we are not allowed to say outrageous, we might—or at least those of us who pronounce the French-English word, "route," like the English word "root" might—say oo-trageous, and thus reasonably avoid the implication of an erroneous popular etymology. But this is an irrelevant digression.

All that was necessary to make the outrageous occurrence of fossils reasonable and believable was to remodel our conception of the earth from that of a recently-and-ready-made planet into that of a very ancient and slowly changing planet, on which life had existed for ages and ages, always under the influence of environing conditions and in the presence of slow-acting processes very much like those of to-day; and when the ideal counterpart of the actual earth was once conceived in this fashion, the earth was still found to be just as comfortable a planet to live on as it had been in association with the earlier concept of a ready-made earth. All that was needed to explain the occurrence of marine fossils in the disordered strata of mountain tops was to replace the concept of an immovable earth's crust by that of a deformable crust; and although the rate of deformation was at first thought to have been violently rapid, the need of such hurry was later seen to be no need at all, but only a fancy; and thereafter the deformation was conceived to be a slow process. And so it has been with one of these outrages after another: their accommodation is easily accomplished by merely replacing one concept of the earth, under which they are unacceptable, by another under which they are acceptable; and the replacement once made, we are just as happy as we were before. To be sure, the

process of replacement may be mentally uncomfortable, even distressing, while it is going on; but the moral of that is that we must not allow our concepts of the earth, in so far as they transcend the reach of observation, to root themselves so deeply and so firmly in our minds that the process of uprooting them causes mental discomfort; and one of the best aids toward the realization of this moral may be found in frequently making explicit announcement of all the unproved postulates on which our favorite concepts are based; for then we shall not be so likely to forget that they are all preceded by a great big IF.

We shall be aided in following this counsel if we strive to recognize how far most of our concepts of the earth really do transcend the short reach of observation. It is usual for a field observer to record that he has seen, for example, a ridge of sandstone; yet all that he has actually seen is a series of small and disconnected sandstone outcrops, perhaps not occupying more than a twentieth or a hundredth of the ridge surface; and the composition of the rest of the surface and of all the interior of the ridge is only a matter of inference; truly, a good and justifiable inference, but not the less an inference for being good and justifiable. Similarly, it is customary for a field geologist to record the presence of a fault when he detects the repetition of a given sequence of strata, and indeed to believe in the displacement that the term, fault, implies, as if it as well as the recurrence of the sequence of strata were a fact of observation; yet not only are the underground extensions of the strata and their long-past displacement merely matters of inference, but even the fault-fracture itself is usually inferred instead of being seen; or if seen at all, it is seen only in small linear extent, thus leaving all the rest of its superficial trace as well as all of its surface, either lost in the air or buried underground, to the imagination. In thus making distinction between the few facts of actual observation and their large extension in a superstructure of inference, it is not intended to impugn for a moment the validity of well-reasoned superstructures, but only to emphasize the inevitable disproportion that must exist between them and their observed basis; and thus to make clearer the enormously speculative nature of geological science. For let it be noted that, in the case of a fault, we have to do with a double inference; first, the inference as to underground structures from surface outcrops; second, the inference of displacement because of the repetition of the inferred underground structures. Nevertheless, we believe that faults actually exist.

The very foundation of our science is only an inference; for the whole of it rests on the unprovable assumption that, all through the inferred lapse of time which the inferred performance of inferred geo-

logical processes involves, they have been going on in a manner consistent with the laws of nature as we know them now. We seldom realize the magnitude of that assumption. A philosopher of the would-be absolute school once said to me, in effect: "You geologists have an easy way of solving difficult questions: you account for the structures of the earth's crust by assuming that time and processes have been going on for millions and millions of years in the past as they go on to-day; but how do you know that time did not begin only a few hundred thousand years ago after the earth had been suddenly created in imitation of what it would have been if it had been slowly constructed in the manner that you assume?" The answer is as easy as the question: We do not *know*; we merely make a pragmatic choice between the concept of such an imitative creation which seems to us absurd, and a long and orderly evolution which seems to us reasonable. We might, to be sure, were we disposed to be disputatious, turn upon the would-be absolutist and ask him what he is going to do about it; but we have better use for our time than that.

The more clearly the immensely speculative nature of geological science is recognized, the easier it becomes to remodel our concepts of any inferred terrestrial conditions and processes in order to make outrages upon them not outrageous. The more definitely it is understood that the concept of a shrinking earth is based upon certain anterior concepts as to the status of its unobservable interior, the more readily can we entertain the concept of an expanding earth, based upon certain other concepts as to the status of its interior; and it is that particular outrage upon our standardized beliefs that I propose we should contemplate, calmly if possible, and patiently at any rate. To encourage our patience, let me recall another outrageous idea of recent introduction, which in itself is only a sort of reaction from an outrage of somewhat earlier invention and a return toward a more primitive view; namely, the recent idea that those topographical features which we call mountains owe their leading feature, namely, their height, not as has been until lately supposed to a vertical movement of escape from the horizontal thrust by which their rocks have been crowded together, but to an uplifting force which acted long after the rocks were crowded together, and in which, as was thought when the view of a mobile earth crust was first promulgated, no component of horizontal thrusting is necessarily involved. A chief difference between that primitive view and its revival in the recent outrage is that the first view took little account of erosion and implied that each individual ridge and peak was the result of an individual or localized uplift; while the second view takes great account of erosion, not only

in ascribing the present intermont valleys to the long and slow action of that patient process during and after recent uplift, but still more in ascribing the destruction of the surface inequalities, that must have been earlier produced when horizontal thrusting forces crowded the mountain rocks together, to a vastly longer action of erosion before the recent uplift of the worn-down mass was begun; for where in the whole world can we find mountains that to-day owe their height to an upward escape from horizontal thrusting; in other words, where in the world can we find any existing mountains that are still in the cycle of erosion which was introduced by an upward escape from the horizontal thrusting that deformed their rocks, and not in a later cycle of erosion which was introduced by uplift alone after the inequality of surface form due to earlier thrusting had been greatly reduced, if not practically obliterated!

The conventional phrase, horizontal compression, has been avoided in the preceding paragraph and the alternative phrase, horizontal thrusting, has been used in its stead, in order to prepare the way for the rather mild idea that the same terrestrial forces which produce great overthrusts may also, if somewhat differently applied, produce rock folds, slaty cleavage, and various other phenomena ordinarily explained under the earlier phrase; and thus to prepare the further way for the altogether outrageous idea that overthrusts do not result from the effort of the outer crust to adjust itself to a cooling and shrinking interior, but from the effort of an in-any-way warming and expanding interior to rearrange the outer crust. Of course, this is "impossible"; that is, it is impossible in an earth of the kind that we ordinarily imagine the earth to be; but it is not at all impossible in an earth of the kind in which it would be possible. Our task therefore is to try to discover, as judicially and as complacently as we may, what sort of an earth that sort of an earth would be; and then to entertain the concept of that sort of an earth as hospitably as we can and to examine the behavior of such an earth at our leisure.

If an earth with an expanding interior had nothing more to do than to stretch its crust, there would be little trouble in our endeavor; but the concept before us compels the expanding earth to do various other things also; and especially to produce great crustal overthrusts, the cross-country advance of which is measured in tens or twenties of miles. Hence the outward radial push of the expanding interior must somehow be turned into an almost tangential thrust; and how that is to be done it is difficult to imagine. However, there is no reason for immediate discouragement on that account; it is very natural that our imagination to-day should fall short of conceiving all the possible behaviors of a warming and expanding

earth, because we are not practised in imagining—that is, in making an image of—that sort of an earth, although a good beginning in that direction has been made in such an essay as that by Boucher on “The pattern of the earth’s mobile belts.”<sup>2</sup> But we surely have yet much to learn as to what may be all the various reactions of an expanding earth-interior on the shell that encloses it, even though many possible reactions may be now conceived.

For example, let the enclosing shell be defined as that part of the whole sphere which is exterior to the depth at which the next inner shell is warming more rapidly than any other. If that depth be great, the chief thrust of the expanding interior will be exerted on a thick shell; if small, on a thin shell; and the effects of interior expansion visible on the surface will surely be different in the two cases. It seems conceivable that the total thrust of expansion may, in so far as it produces batholithic movement, be slowly concentrated at the weakest part of the shell, and there permit the interior movement to be locally increased by the conversion of cubic expansion into linear expansion or intrusion; this being the converse of the process by which an unduly heavy and therefore isostatically subsiding part of the crust may slowly distribute its local movement through the whole of the interior and there produce a diminished spherical extension, as Lawson has suggested. It seems also conceivable that the movement of a localized batholithic introduction may find advantage in making an outward escape from compelling interior pressures, by changing the direction of its ascent from vertical to oblique, and thus diminishing the rate at which it has to raise the overhead crust. Whether an obliquely ascending mass of this kind could eventually, as it approaches the surface, drive along a slice of crust ahead of it and thus produce what we call on overthrust, is evidently problematical; but if an overthrust could be produced in that way it would be gratifying in certain respects.

If an obliquely ascending batholithic intrusion works its way through a heavy shell toward the surface and there drives ahead of it a crustal slice which we recognize as an overthrust, the oblique emergence of such an overthrust slice will cause an underdrag of the covering rocks in the rear of thrusting advance, and thus displace them with more or less extensional jostling so that they will cover a greater breadth of surface than that which they occupied before being underdragged. Surely the need of some such underdragging ought to have been recognized long ago when the prevalence of so-called normal faults which indicate superficial extension, over other faults which indicate compression, was inductively established; and the need is still greater to-day when great faults, such

as those of the Basin Ranges, have been found to dip at moderate angles, such as 50° or 40° to the downthrow; for such was the conclusion reached by Gilbert in his latest season of field work in the Great Basin a little over ten years ago.<sup>3</sup> It is true that some geologists maintain the possibility of producing so-called normal faults as an indirect effect of horizontal compressional forces; but even if so contradictory an effect may thus be possibly produced, it by no means follows that such faults can not also be produced much more directly by extensional forces; and the possible cause and working of extensional forces should therefore be investigated; for there is no generally accepted mechanism, like an underdrag, adequate for the strong extensional dislocation of crustal blocks with diverse displacements, to be found in the usual schemes of dynamical geology; and in the lack of such a mechanism, any process, even a fantastic process, that will cause a strong underdrag seems worthy of at least an hour’s consideration.

But let no one imagine that I here put forth the idea of an expanding earth interior, with its imagined consequences of an obliquely out-and-overthrust mass exerting an underdrag on the superficial crust in its rear, as an idea to be believed. I do not believe it myself, and am therefore doubly far from asking any one else to believe it. The idea is set forth simply as an outrage, to do violence to certain generally established views about the earth’s behavior that perhaps do not deserve to be regarded as established; and it is set forth chiefly as a means of encouraging the contemplation of other possible behaviors; not, however, merely a brief contemplation followed by an off-hand verdict of “impossible” or “absurd,” but a contemplation deliberate enough to

<sup>3</sup> Since giving the address on which this article is based I have had opportunity of seeing several Basin Ranges, some in southeastern California, in company with that most competent of guides, Dr. L. F. Noble, of the U. S. Geological Survey, and some in Utah in the helpful company of Professors Schneider and Mathew, of the State University at Salt Lake City, and of Professor M. O. Hayes, of Brigham Young University at Provo; and the evidence then found for the occurrence of slanting fault surfaces seems to me indisputable, not only in the Basin Ranges themselves but also in the much longer bounding ranges of the Wasatch mountains on the east and the Sierra Nevada on the west. Far from the Range blocks being vertically uplifted without compression, as Gilbert first proposed in his report on the Wheeler Survey fifty years ago, still farther from their being the crowded blocks of a collapsed arch, as others have supposed, the Basin Range blocks seem to be the irregularly uplifted and diversely tilted blocks of a former lowland of erosion which has suffered a pronounced extension of its former east-west breadth, as I have briefly stated in the Proceedings of the National Academy of Sciences for July, 1925.

<sup>2</sup> *Journ. Geol.*, xxxii, 1924, 265–290.

seek out just what conditions would make the outrage seem permissible and reasonable.

Let me close this address by explaining to this hospitable and sympathetic conclave why it seems peculiarly appropriate for me, an easterner, to set before the westerners here gathered the particular outrage with which I have detained them. It is because my contacts with the geology of the Pacific slope during the winter of 1924-25—very unbecoming contacts, because of my preconceptions—have been outraging the views that I have more or less unconsciously gained on the Atlantic slope as to the demure quietude of the later geological periods. In the east, the Miocene, Pliocene and Pleistocene have witnessed only leisurely processes of degradation, deposition and deformation, all of small relatively measure; but here on the Pacific slope those periods have been characterized by an extraordinary activity; deposits of enormous thickness have been laid down, and those deposits have been deformed and eroded on a scale that is really rather disconcerting. Is it not fair, therefore, that in return for the incredible stories that have been told me here as to what has happened lately in Californian geology, I should take a turn at telling some outrageously impossible stories myself? In any case, there stand the Basin Range fault blocks, just beyond the eastern skyline of California, displaced in such a manner as to extend over a greater breadth of country than that which they previously occupied; and if it is not possible to explain their extension by underdrag, as an indirect reaction of a passive exterior crust on an expanding earth interior, then we must ask by what other outrageous process it is proposed to explain them.

W. M. DAVIS

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### OPPORTUNITIES FOR RESEARCH AT THE OCEANOGRAPHICAL INSTITUTE OF MONACO

THE original plan of Prince Albert I was to establish at Monaco a museum especially devoted to the collections made by him in the course of his scientific cruises, pursued each year from 1885 until the outbreak of the war. Later this plan was enlarged and the museum as it now exists is devoted in a general manner to all phases of oceanography. As a point of interest to its tourists who throng its Riviera each year it is second only to the Casino of Monte Carlo. The number of visitors has increased considerably in recent years and is now approaching one hundred thousand annually.

In addition to its popular interest as a museum and aquarium, attention should be called to its importance as an institution for original research and for its pub-

lication of both biological and physical oceanographical investigations.

Unfortunately, since the death of Prince Albert in 1922 the resources and activities of the institution have been somewhat curtailed. The successor of Prince Albert, not having the same interest in science as his father and not caring to assume the expense of maintenance of his father's steam yacht, the *Hirondelle II*, promptly sold it to an American moving picture corporation. Consequently, further data and material for research obtained by annual cruises are no longer supplied to the institute. The amount already on hand is, however, very great and is sufficient for many years' work. In accordance with an arrangement made by the prince before his death the publication of the results of his cruises is to be completed without cost to the institute. The seventieth volume is now in press and it is estimated that a total of about one hundred will be required for the complete publication. The rapidity with which the remaining volumes are issued will depend of course upon the rate at which work can be pursued, but that they will eventually appear seems without question.

In a letter addressed by Prince Albert I to the minister of public instruction of France, dated April 25, 1906, the foundation of the Oceanographic Institute is described in the following words:

Having consecrated my life to the study of the oceanographical sciences I have recognized their importance to many facts of human activity, and I am prompted to secure for them the place they deserve in the solicitude of the government as well as in the consideration of scientists.

Many countries have already sent scientific expeditions to all the seas of the world and these furnish to oceanography a solid basis for its development, but France, in spite of the special interest which the science of the sea holds for her, has not shown it the same interest, as it has other branches of science. However, I have given at Paris during several years lectures attended each time by a more numerous and attentive audience, for which public powers, in the person of President Loubet and members of the government, have by their presence exhibited a certain interest.

Accordingly, I have desired to fill a void by myself creating and establishing at Paris a center of oceanographical studies, closely connected with the laboratories and collections of the Oceanographical Museum of Monaco, where I have assembled for twenty years the results of my personal investigations and those of eminent collectors who have come to me from all countries of Europe.

In addition to the original four millions the prince left to the institute another million at the time of his death. The income derived from this foundation, together with that obtained from the admission fee of