BOOKS: GEOSCIENCES

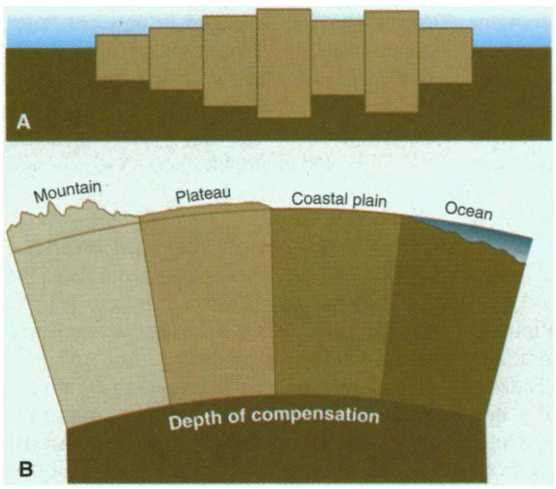
Held in Place By Practice

David Oldroyd

With all their resources, American geoscientists do much of the world's best geology. Thus some of them may be embarrassed that their predecessors were so slow to embrace continental drift or convection currents in the mantle and were initially so resistant to the doctrines of plate tectonics. Although there must be historical reasons for this reluctance to accept mobilist doctrines, hitherto they have not been examined in detail. Now Naomi Oreskes has accomplished the task in *The Rejection of Continental Drift*. Based on extensive archival research and Oreskes's studies over the past 20 years, her admirably clear and well-illustrated account is scientifically, philosophically, historically, and sociologically well-informed. All is achieved without recourse to esoteric detail or any mathematics: she is after concepts.

The Rejection of Continental Drift
Theory and Method in American Earth Science
by Naomi Oreskes
Oxford University Press, New York, 1999. 432 pp. $55, £49.50. ISBN 0-19-511732-8. Paper, $29.95, £21.50. ISBN 0-19-511733-6.

According to Oreskes, the reasons go back to the 19th century, when British scientists proposed two alternative models for isostasy (the tendency of Earth's crust to maintain a state of buoyant equilibrium vis-à-vis the underlying material). J. H. Pratt envisaged a uniform depth above which the total overlying mass was everywhere the same, with the density of crustal materials being least for the high mountain belts and greatest for ocean floors. In contrast, G. B. Airy postulated that topographically high regions were gravitationally compensated by invisible "roots" of low density crustal material, thus higher mountains were underlain by proportionally deeper roots. Under the direction of J. F. Hayford, the U.S. Coast and Geodetic Survey carried out the immense labor of making gravimetric determinations across the states. Hayford interpreted the gravity data as evidence that Earth's surface was essentially in a state of isostatic equilibrium, so that the contorted crust of the mountain regions was not under stress but apparently floated comfortably on high. In reaching this result, he utilized Pratt's hypothesis, which was simpler for computational purposes. It was also the "cheaper" model because the calculations required to determine the form of the geoid and to demonstrate that the crust was isostatically balanced were simpler than those needed under the Airy hypothesis. (The computations, incidentally, were carried out by a team of low-paid, mostly female, calculators.) When Hayford's baton was passed to William Bowie, the Pratt model went with it. The geodetic survey, an empirical accomplishment on a grand scale, gave an impress to the kind of geological work that came to be favored in America, or so Oreskes plausibly argues. The organization's work was a pragmatic success, in tune with the pragmatic tenor of American philosophy.



Alternative equilibria. The Airy, or "roots of mountains," model of isostasy (**A**) is based on Archimedes' principle of flotation. Crustal blocks are like icebergs, with higher regions underlain by deeper roots. The Pratt model (**B**) assumes a "uniform depth of compensation," so the topographic height of crustal blocks is inversely proportional to their density (darker color indicates higher density). Bowie concluded that Pratt isostasy describes the actual state of the crust, a belief incompatible with continental drift. [Adapted from W. Bowie, *Isostasy* (Dutton, New York, 1927).]

As Oreskes deftly explains, Bowie's "theory of the earth" held that crust and mantle were nicely balanced; isostatic equilibrium was only exceptionally, and temporarily, perturbed. The continents and oceans were relatively permanent features (another American doctrine of long standing, promulgated by J. D. Dana). Lateral movements of crust were relatively small-scale, simply occurring in response to the erosion of mountains and deposition of sediments in the oceans. Subcrustal flow occurred from regions of deposition to mountain areas, where compression led to uplift and maintenance of isostatic balance. So Bowie's influential opposition to drift was linked to his adherence to Pratt isostasy.

Another factor was T. C. Chamberlin's doctrine of multiple working hypotheses, according to which several tenable explanations should be entertained without undue attachment to any one in particular. Although Chamberlin's "method" was supposed to guard researchers against the evils of uncritical adherence to grand systems (Francis Bacon's "idols of the theater"), it led to an overzealous emphasis on the collection of facts, which were supposedly separable from theory. But thinking about the matter was not always straight. For Bowie, isostasy was a "fact" whereas continental drift was a "very interesting hypothesis." That Pratt's assumption was lurking within Bowie's "fact" was largely lost sight of.

Also relevant was the doctrine of uniformitarianism: Lyell's claim that events of the geologic past must be explained by processes observable today. This, by Oreskes's account, made it impossible for Charles Schuchert to come to terms with drifting or sliding continents, envisaged by such overseas theorists as Wegener, Joly, Holmes, and du Toit. For Schuchert, the problems of paleobiogeography could not be accounted for by continents drifting from one latitude to another, because then there would be no satisfactory "uniformitarian" correlation between faunal types, supposed latitudinal positions of continents, and climatic conditions. Knowledge about present faunal distributions could not be applied to the past, and thus the present would no longer be the key to the past. But Schuchert was willing to countenance minor crustal dislocations. Like Bailey Willis, in the 1930s he developed the ill-conceived doctrine of isthmian land bridges to explain the paleontological evidence that so impressed drifters, such as Wegener and du Toit, and even mobilist skeptics, such as the Cambridge geophysicist Sir Harold Jeffreys. Schuchert and Willis argued—as American geology students would be taught well into the 1960s—that biotic similarities across the oceans were not evidence for continental

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drift; instead flora and fauna had spread among ancient continents via narrow, intermittently emergent land bridges.

Probably matters would have proceeded differently if the course of events had not been interrupted by World War II. Although military research generated vast amounts of fresh data about the ocean deeps, much of this information was classified and the normal scientific analysis and debate was stifled. So it was not until the 1960s that, with the help of new geophysical data, the log-jam was broken and the plate tectonics revolution occurred. This part of the story is well known, but Oreskes recounts it with her characteristic clarity.

According to sociologist R. K. Merton, one of the chief norms of the scientific community is "organized skepticism," an idea that might seem to mesh well with Chamberlin's views on the virtues of multiple working hypotheses. In the case before us, however, the skepticism in the American geological community from the 1930s to the 1950s was anything but "normal." Intense "organized skepticism" was directed at the ideas of the drifters, but not at those of "fixists" such as Bowie or Willis. The jerry-rigged doctrine of isthmian links held on until well after the war—not being viewed with the appropriate skeptical, jaundiced, Mertonian, Chamberlinian, and perhaps Popperian, eye.

Oreskes asks why this was so. In brief, she thinks—and I am persuaded that her argument is essentially correct—that the style of American geology favored the large-scale collection and mathematical analysis of data, all treated in a grand "Texan" fashion. The work of the Coast and Geodetic Survey fit this mold. It was not the way of the (relatively) lone creative thinkers such as Wegener or Holmes.

Should we hold this against the American geological or scientific community? I don't think so. Such large-scale efforts can indeed be successful, as in the geomagnetic research that eventually helped break the conceptual log-jam of continental drift. It was, however, perhaps the case that Hayford's work was too successful; in the case of the development of American tectonic theory, it came to have a deleterious effect. Was the Coast and Geodetic Survey's work a chicken or an egg? Did the U.S. scientific community (the chicken) naturally generate that kind of work? Or did the work produce the geological community (the egg) that it did? On the whole, I suspect that there may have been a bit more chicken than egg, given the pioneering circumstances of 19th-century American geology. I think Oreskes might agree, but perhaps it is not a question that can be answered decisively.

Nonetheless, Oreskes provides an admirable analysis of the "great failure" of American geological theorizing in the 20th century. *The Rejection of Continental Drift* also demonstrates the hegemony of geophysical research vis-à-vis geohistorical research in the American geological community. It is surprising, therefore, that Oreskes gives so little attention to seismology. But this is the only major reservation I have about an otherwise excellent volume, which many geologists (and non-geologists) will want to read, and which all reputable libraries will be sure to purchase.

BROWSINGS

The Babylonian Theory of the Planets. *N. M. Swerdlow.* Princeton University Press, Princeton, NJ, 1998. 652 pp. \$39.50, £27.50. ISBN 0-691-01196-6.

Swerdlow offers an explanation of how Babylonian scribes calculated the numerical parameters of their planetary theory from observations of ominous celestial phenomena (which they compiled for over 600 years). He argues that in reducing irregular but periodic events to mathematical description, which allowed divination despite bad weather, the scribes developed the first empirical science.

Cell Lineage and Fate Determination. *Sally A. Moody, Ed.* Academic Press, San Diego, 1998. 652 pp. \$149.95, £99.95. ISBN 0-12-505255-3.

The contributors provide a broad overview of how findings from eight model organisms form our current understanding of the mechanisms by which embryonic cells attain their final differentiated state. For each animal, they present the important developmental processes, the experimental usefulness, and several chapters describing interdisciplinary research on key questions.

The History and Practice of Ancient Astronomy. *James Evans.* Oxford University Press, New York, 1998. 494 pp. \$65, £49.50. ISBN 0-19-509539-1.

While tracing ideas from ancient Babylon to Renaissance Europe, Evans emphasizes the details of astronomical practice. He discusses the evidence used to reconstruct ancient astronomy, and shows readers how they can do astronomy using ancient methods.

Information Ages. Literacy, Numeracy, and the Computer Revolution. *Michael E. Hobart and Zachary S. Schiffman.* Johns Hopkins University Press, Baltimore, MD, 1998. 317 pp. \$29.95, £25. ISBN 0-8018-5881-X.

Hobart and Schiffman provide an account of the storage and processing of information from prehistoric to contemporary times. They emphasize how new technologies (writing, the printing press, mathematical models, and computers) have profoundly influenced human thought and culture.

Limited by Design. R&D Laboratories in the U.S. National Innovation System. *Michael Crow and Barry Bozeman.* Columbia University Press, New York, 1998. 349 pp. \$40, £32. ISBN 0-231-10982-2.

Drawing on interviews, surveys, and quantitative data collected from a wide range of research and development laboratories (government, industry, and university), Crow and Bozeman examine how the labs function as a system. They offer policy ideas for more effective development and use of the labs' potential for innovation.

Polar Journeys. The Role of Food and Nutrition in Early Exploration. *Robert E. Feeney.* American Chemical Society, Washington, DC (distributor, Oxford University Press, New York), 1998. 309 pp. \$41.95. ISBN 0-8412-3349-7. Paper, University of Alaska Press, Fairbanks, AK. \$27.95. ISBN 0-912006-97-8.

Feeney discusses how polar exploration was influenced by advances in food preservation, shortages or overabundances of vitamins, and lessons learned from Arctic natives. His extensive use of the explorers' accounts helps portray their experiences, and supports his conclusions on the importance of nutrition to their success.

States of Mind. New Discoveries About How Our Brains Make Us Who We Are. *Roberta Conlan, Ed.* Wiley, New York, 1999. 224 pp. \$24.95. ISBN 0-471-29963-4.

The authors offer eight essays adopted from a public lecture series on how our memories, emotions, personalities, and health are influenced by the biological activity of the brain.

The Zebrafish. *H. William Detrich, III, Monte Westerfield, Leonard I. Zou, Eds.* Academic Press, San Diego, CA, 1998. Biology. 432 pp. \$99.95. ISBN 0-12-544161-4. Paper, \$64.95, £44.95. ISBN 0-12-212170-8. Genetics and Genomics. 432 pp. \$99.95. ISBN 0-12-544162-2. Paper, \$64.95, £44.95. ISBN 0-12-212172-4. Methods in Cell Biology, vols. 59, 60.

The tropical freshwater fish *Danio rerio* has become a model system for the genetic analysis of vertebrate development. These two volumes offer researchers comprehensive coverage of the laboratory protocols now available for studies of developmental or cell biology. The first volume focuses on gene expression and function, early embryonic development, and organogenesis. The second covers methods for identifying and mapping mutations, cloning the causative genes, and using zebrafish databases.