by a handful of women scholars from the 1940's until the 1970's, when, of course, the new feminist movement helped revive the doctrine of sexual equality.

By any reasonable standard this is a good book on an important topic. It deserves the widest possible readership. Yet this does not mean that every angle of vision taken or every interpretation made is unexceptionable. Essentially this is reform history. Certainly reform history is a popular, perhaps mainstream, enterprise within the historical profession. Certainly too an author has a right to pursue a particular line of inquiry. Yet reform history—in this instance, an ardent, open, feminist search for a usable past—yields at best a partial reconstruction of the past. A search for a usable past is by definition bound up with contemporary concerns that occlude the vision of the past. Much to her credit, Rosenberg has avoided the simplistic Manichean formulations that all too often contaminate mainstream reform history. Her analyses and interpretations are often sophisticated, especially on social phenomena. Yet problems remain. One gets the impression, for example, that the doctrine of sexual equality, which Rosenberg's *dramatis personae* worked so valiantly to create as an intellectual and scientific construct, is an enduring "truth of science." This seems to me an unduly whiggish or positivistic conception of science and its history. Nor does it seem a service to democratic civilization to imply that important public policy questions can be resolved by "scientific truth."

A consequence of Rosenberg's search for the scientific roots of the modern feminist sensibility is that the book does not quite coalesce intellectually about a coherent historical problem. Not all of the scholars so ably discussed were of comparable importance, either to a tradition of feminist scholarship or to their respective disciplines, or even to one another as thinkers and scholars. Certainly Woolley, Hollingworth, and Mead were major scholars in these and other respects. I was not persuaded in the other cases. And we learn only about these scientists' contributions to the doctrine of sexual equality, which was a fragment of their total work, consideration of which, I would suggest, might well strengthen, not weaken, Rosenberg's arguments.

Some specific interpretations are arguable. Rosenberg argues that, once Victorian (or, more precisely, Spencerian or Neo-Lamarckian) evolutionary natural*ism* was undercut by the post-1900 social sciences, its offspring, the separate spheres argument, went glimmering. The idea of the separate spheres has had a far more complex and enduring history. And Rosenberg insists that the work of Woolley and Hollingworth undercut psychology's assumption of inborn sex differences and led to a feminist scientific triumph. Hollingworth's work did build upon Woolley's. And in the 1920's sex differences as a subject of research virtually disappeared from psychology. Yet I wonder whose victory this really was. A comparison with the fate of the contemporaneous arguments in psychology concerning race differences is both instructive and depressing. Psychologists abandoned race differences as a scientific subject when the methodology underlying the more egregious claims in that regard became a matter of embarrassment and when a major public policy objective of scientific racism, immigration restriction, was accomplished. Perhaps sex differences hypotheses became less necessary too as the prewar women's movement collapsed and challenges within psychology to the idea of separate spheres emerged within a few years of one another.

There is another level of explanation that helps us understand what happened to this tradition of feminist scholarship, some elements of which Rosenberg sees very perceptively. The general model of evolution in both scientific and social thought changed drastically in the '20's. The prewar model defined species as types, arranged them in a hierarchial order of superiority and inferiority, and insisted that continuity and change were the consequence of natural (that is, innate) "forces." The heredity-environment controversy of the '20's brought the naturalistic cultural determinism of the new social sciences to the foreground of scientific discussion. The resulting new synthesis, which took shape in that decade, employed an essentially statistical definition of a species as a fluctuating population, juxtaposed species rather than arranged them in an hierarchy, and argued that man was the product of biological and cultural evolution. Only *man* as a single, interbreeding, culture-bearing species remained, with woman as such not an object of concern. Now it made no more sense to have a hierarchy of sex than of race. From the 1930's on the evolutionary model was entirely naturalistic and interdisciplinary, as in, for example, the many theories of culture and personality. This deeper intellectual change was impersonal, reflecting an even more profound and general shift in the culture and society concerning the relationship of the parts to the whole. I suspect many of its scientific architects did not perceive the implications suggested here, but such is the progress of science.

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Radiometrists and Plate Tectonics

The Road to Jaramillo. Critical Years of the Revolution in Earth Science. WILLIAM GLEN. Stanford University Press, Stanford, Calif., 1982. xx, 460 pp., illus. $37.50

The Road to Jaramillo is geologist-historian William Glen's account of some of the lines of research that figured in the much-heralded revolution in the earth sciences culminating in the theory of plate tectonics. It is the first such account to which criteria of adequacy developed by historians of science can fairly be applied, and it makes unusual claims about the key to the revolution, which the author locates far afield from its origins as represented in previous accounts of the subject.

The title refers to the discovery of a reversal of the earth's magnetic field at 900,000 years ago, the "Jaramillo event," and the heart of the book is a chronicle of the efforts of a number of scientists, particularly Cox, Doell, and Dalrymple of the U.S. Geological Survey at Menlo Park, California, to establish an absolute time scale for reversals of the earth's magnetic field, using potassium-argon isotopic dating. This technique, particularly the development of a mass spectrometer capable of accurately dating very young rocks, is the subject of the first part of the book; in the closing chapters the author discusses the application of the time scale to the now famous "*Eltanin* 19" profile of the ocean floor, with its symmetric magnetic lineations parallel to an ocean ridge. Glen argues that this confluence of data, when supported by geomagnetic polarity determinations from ocean sediment cores at Columbia's Lamont-Doherty Geological Observatory, "confirmed" the Vine-Matthews-Morley hypothesis of seafloor spreading and ushered in a revolution in the earth sciences, a revolution that Glen ranks with the achievements of Copernicus, Darwin, and Einstein.

Glen's account is based on 500 hours of interviews conducted with more than 100 informants (including most of the living principals in his story) and a wealth of published and unpublished papers, correspondence, and laboratory and administrative records. He has deposited his interview tapes and other materials as an archive in the Bancroft Library at the University of California at Berkeley, and selectively at the American Institute of Physics in New York. In an age of telephone conferences and preprints such materials will assume great importance for the history of science: in rapidly growing fields they may be all the historian has to go on. Even apart from the history he has written, the archive Glen has created stands as a significant achievement.

Glen's thesis is that radiometrists at Berkeley developed an early (late 1950's) interest and technological edge in dating young rocks by the potassium-argon method and that this program of research was exploited by other Berkeley-trained scientists at nearby Menlo Park, whose program was the development of a polarity-reversal time scale for the recent geologic past. Glen argues that their success in publishing such a time scale containing the Jaramillo event, ahead of competitors at the Australian National University and Columbia, gives them priority in precipitating the revolution in the earth sciences. He contends that the key to the revolution was the application of absolute geochronology to ocean floor magnetic data: "Had there been no potassium-argon polarity-reversal time scale, how and when might seafloor spreading have been confirmed?" (p. 353). The logic of the argument is that plate tectonics depends on seafloor spreading, that seafloor spreading depends on absolute time scales to confirm constant spreading rates, and that the first adequate time scale was produced by Cox, Doell, and Dalrymple. Glen insists on their priority even though they were not responsible for the concept of such a time scale, did not produce the first such scale, published their historic (sic) scale only weeks ahead of their competitors, and were unaware of the significance of their time scale for the hypothesis of seafloor spreading (p. 363).

This is a tenuous sort of priority, and a very slender peg on which to hang a scientific revolution. Had Neil Opdyke been in print a month earlier, had McDougall and Tarling been less conservative in their interpretation of the same data, there would be no priority for Berkeley. Indeed, the focus on Berkeley sometimes gives the book a wrong-end-of-the-telescope feel. The road to Jaramillo is not the main highway of the earth sciences revolution, for which an appropriate title would be "the Road to *Eltanin* 19." The road *from* Jaramillo is one of several similar roads; even if Cox, Doell, and Dalrymple had not embarked on their program at all, the revolution would have taken place at about the same time. This is because, as Glen points out on pp. 313 and 314 in a footnote, revolutions are not made by subspecialists but by people who know what the relevant results mean and can integrate them into a larger picture.

Yet one can see why the book came out as it did. Glen wrote it at Berkeley and conducted 164 of his 500 hours of interviews with scientists in the Bay Area, including 49 hours with Cox, Doell, and Dalrymple alone. When his narrative takes us overseas, it is most often to follow Berkeley scientists or to go home or afield with scientists who spent time at Berkeley. Glen knows very well (and says so) that geology is a parochial science: rocks collected in familiar terrain always seem more important than rocks collected elsewhere by someone else. The same holds true for history, and in this case Glen has been betrayed by his materials into asserting the greatest importance for the work of scientists with whom he has had the most contact. Moreover, he seems to have caught the spirit of the priority race from his informants and has produced a book that seems hurried to the point of breathlessness, with many flaws of narrative sequence, not well thought through, and destined to be superseded by a more detached consideration of the same data. With footnotes indicating new material as recent as April 1981, Glen has not digested the impressive and valuable archive he has created. The result is a *Road to Jaramillo* as rocky as an explorer's jeep track—a promising reconnaissance, no more.

Consider the claim that the revolution in the earth sciences ranks with the achievements of Copernicus, Darwin, and Einstein (p. 3). However important it has been to geology, would anyone assert that plate tectonics recasts mankind's place in nature and relationship to the rest of the universe in the same way as heliocentrism, biological evolution, and relativity did? This is certainly an exaggeration.

An example of the narrative problems is presented by the statement on p. 114 that Sigurgeirsson used a spinner magnetometer to measure rock samples from the six most recent magnetic epochs, taking into account secondary magnetization and studying the "next to oldest transition (R_3-N_3) in detail." The reader's comprehension here would be aided by the explanation of the notation R_3-N_3, which appears later on p. 135, the description of the spinner magnetometer, which comes on p. 181, some discussion of the concept of magnetic epochs (pp. 245–250), and an explanation of secondary magnetization. Most (not all) of this can be dug out of the index, but to do so will tax the ingenuity and patience of the nonspecialist. Similarly, in the summary of the history of field-reversal studies, we hear successively of developments in 1964, '73, '62, '54, '34, and so on; wouldn't it have been better to put them in chronological order?

Another problem in the narrative is the richness of detail, which sometimes reaches the point of clutter. On p. 41: "Reynolds was first introduced to Curtis in 1952 on the back steps of the geology building, Bacon Hall, near the LeConte physics annex, by John Halsey, a graduate student in geology under Charles Gilbert who had been a naval officer colleague of Reynolds's." Do we really need to know this before we know whether Curtis is a student or a faculty member and what he has to do with the story? I think not. Moreover, there is a lack of balance in the detail presented: that Doell's 1952 seminar included a proposal for tests of the theory of continental drift using magnetic directionality data from the Grand Canyon is passed over with the same attention as Doell's failure to get into the air force in World War II because of asthma.

Glen's partiality for the Berkeley group leads him into serious problems of historical method and interpretation. As a case in point, he dismisses the polarity-reversal time scale produced by Martin Rutten and co-workers in the Netherlands as "premature" and calls the scale produced four years later by Cox, Doell, and Dalrymple "Scale One" (see p. 224). Glen argues that Rutten got his rock ages from others and obtained his magnetic data in the field with a hand-held compass rather than a sophisticated laboratory magnetometer. Yet Rutten was in print in October 1959 with a correlation of potassium-argon dates and polarity reversals, with the periods of normal and reversed polarity in the right places. His results were cited by Cox, Doell, and Dalrymple in their first scale in 1963: this definitively establishes his priority. That others considered him an interloper (whatever that means) and that he did not use only his own data is no reason for dismissing his work. Claims that Cox and Doell had a more sophisticated and self-consistent program are beside the point; they do not change the order of discovery.

The matter of Rutten raises another issue. On p. 130 it is asserted that Rutten lacked the training to follow up his early time scale, and this is presented as evidence that Rutten's scale was premature and not a serious part of the development of such scales. It is disturbing to find that the remark concerning Rutten's alleged lack of competence is "unattributed on request." In fact, remarks "unattributed on request" are scattered throughout the book, and in almost every case they are highly critical of the capacities and judgments of scientists involved in the story. Whether accurate or not, such statements are unacceptable in a work of historical scholarship. Reporting them reduces historical narrative to gossip, hearsay, and innuendo. In this case, we hear that the informants are paleomagnetists who worked with Rutten, and since there are six candidate names in the bibliography, suspicion falls on all of them, a disservice to those not culpable for disparaging remarks they do not wish to acknowledge.

This error of judgment aside, there is an issue of more general significance that bears on the allegation of priority for the Berkeley groups. At fault here is Glen's equivocal use of the term "program" in referring to the relevant research efforts. Philosopy of science journals are full of "solutions" to "problems" attacked by "teams" with "programs"—a style of analysis known as rational reconstructionism—in which one pieces together the logic of a sequence of discoveries and determines which research efforts (from a logical point of view) were crucial in attaining a result, whether or not the principals understood their position in the sequence at the time. But there is another and quite different sense of the term "research program," referring to the series of studies one carries out and records in laboratory records; and, finally, there is a third sense: the program one outlines in the "program proposal" to a funding agency, stating the larger context of the work and its justification.

Glen uses the term in all three ways, and it is often not clear from the context which sort of program he means. For instance, he speaks of the "young-rock dating program" at Berkeley, and the "geomagnetic time scale reversal program" at Menlo Park. But, as Glen's materials show, there was no "young-rock dating program," but a "rock dating program" in which young rocks were sometimes dated, not always willingly. Similarly, at Menlo Park there was not a "geomagnetic time scale reversal program" but a rock magnetics program with a reversal component, and a controversy, at times acrimonious, over the ownership of data, the use of facilities, and proper direction of research. That of all the rock dates at Berkeley the youngest ones were significant and that in the magnetics program the directionality data were overshadowed finally by the reversal data, were learned in retrospect by everyone, the thrust of Glen's narrative notwithstanding. In fact, Cox, Doell, and others had a "program," within a "program," which fits the analytic definition of a "program"—*caveat lector*. The reader must decide at each juncture which sense is meant.

Although the book is announced as intellectual history, it is not: the organizing intelligences are offstage, and although certainly not minor figures in the narrative are well in the background for the first 275 pages. Moreover, except in the beautifully detailed account of the study of magnetic self-reversal, we hear more of proposed research and final results than of the actual doing of the research. This is institutional rather than intellectual history. As such the book has real strengths. Glen shows how important it is that scientists in charge of academic and professional departments introduce their students to a broad range of fundamental problems. Verhoogen and others at Berkeley, John Jaeger at the Australian National University, and James Balsley at the U.S. Geological Survey emerge as men who had an eye for bright young scientists willing to take some chances, who steered them to fundamental, front-line topics and gave them support, money, time, encouragement, and protection—with stunning results. Glen's focus on the academic and professional setting of time scale research shows again and again a sustained interest in tests of the theory of continental drift at major university centers, from the 1950's on—a conclusion certainly at variance with the idea that continental drift died with Wegener in 1930.

In spite of its serious flaws, *The Road to Jaramillo* opens up an approach to earth science history that I hope others will follow—making careful note of Glen's failures as well as his successes. There are tantalizing leads here, and a mine of information for Glen himself, and many others, to exploit.

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A Framework for Archeology

Archaeology as Human Ecology. Method and Theory for a Contextual Approach. KARL W. BUTZER. Cambridge University Press, New York, 1982. xiv, 364 pp., illus. Cloth, $29.50; paper, $12.95.

Since the 1960's many archeologists have adopted an ecological approach to their discipline, but—particularly in Europe—the majority continue to think more as historians than as ecologists. By adopting an explicitly ecological framework and recasting the discipline within it, Karl Butzer presents non-ecologically minded archeologists with an alternative mental construct for the subject as a whole. And to those colleagues who already work within an ecological paradigm he offers a conspectus of the field that is valuable chiefly for its comprehensiveness.

Integral to Butzer's conception of archeology as human ecology is the assumption that systems theory provides an appropriate model for interpreting past relationships between culture and environment. However, he acknowledges that, although systems-theory concepts allow coherent hypotheses to be formulated, they tend to be too complex to be applied directly to the data base of archeology. They contribute to the ultimate goal of understanding the dynamics of past human ecosystems, but at the level of operational research some more practical approach to the data is needed. This is provided by the concept of archeological context, which Butzer defines (p. 4) as "a four-dimensional spatial-temporal matrix that comprises both a cultural environment and a noncultural environment and that can be applied to a single artifact or to a constellation of sites." Thus broadly defined, "contextual archeology" embraces such established subfields as geoarcheology, archeometry, archeobotany, zooarcheology, and spatial archeology.

Part 1 of the book consists of two introductory chapters, the second of which examines spatial and temporal variability in environmental systems and includes a useful six-order classification of scales of climatic variation ranging from a few to several million years' duration. A method of summarizing concepts and data in tabular form, with frequent supplementary diagrams, is introduced in this chapter and used effectively throughout the text. Both the tables and the diagrams add materially to the value of the book, especially for teaching purposes.