Paleoecology

Environment and Archeology. An Ecological Approach to Prehistory. KARL W. BUTZER. Aldine-Atherton, Chicago, ed. 2, 1971. xxviii, 704 pp., illus. $15.

The first (1964) edition of this comprehensive work on the analysis and interpretation of the environmental and cultural record of the Pleistocene is familiar to all serious students of Quaternary research. The usefulness of this work has been considerably increased through the addition of several new chapters and the extensive revision of many sections in this new edition.

Greatest attention in revision has been given to interpretative sections which treat areas and time periods that have been subject to intensive study by Western European and American workers. In particular, new chapters are included on Late Pleistocene environments of North America and the early peopling of the New World and Australia. Two additional chapters on Africa have been completely rewritten, and all other interpretative sections have been either extensively or moderately revised. The topical and methodological sections of the book retain considerably more of the content of the original edition. Only the section on absolute chronology has been significantly revised. Chapters on paleontology and paleo-temperatures, cave sediments, and windborne and slope sediments show moderate revision. Throughout the volume there is evidence of editorial work designed to improve readability. In general, the new sections are better written and make easier reading than comparable material in the first edition.

One of the outstanding aspects of the revision is the expansion and updating of an originally impressive list of citations, which now includes over 1200 references. Over half of the almost 600 new entries are devoted to specific Old World problems and the remainder are approximately evenly divided between general topical and New World sources.

Mechanically, the new edition is set somewhat more closely and is not, I think, as well composed as the earlier one. The reproduction of most illustrations is of somewhat poorer quality, which in a few cases limits their interpretation. The absence of headbands in the binding suggests a less durable volume.

The incomplete nature of the Quaternary record frequently makes it possible to argue several interpretations of the same body of evidence, and thus it is not really very useful to dispute specific conclusions in Butzer's various syntheses. Following up the references cited will in most instances introduce the reader to the divergent views on any particular problem. Though other authorities may disagree with some of his conclusions, I do not think that such disagreement will detract from the wide usefulness of this work. The fact is that these syntheses are most welcome as the viewpoint of a scholar who, as a paleogeographer, is not narrowly specialized in archeology or geology, but is able effectively to combine the information of these fields with his extensive background in environmental interpretation.

Butzer has brought together far more material relating to the understanding of man and his Pleistocene and early post-Pleistocene surroundings than is available in any other single source and has treated it with a depth of understanding seldom encountered in a field as inclusive as this. I have little doubt that this volume will remain the standard point of departure for everyone interested in these problems for many years to come.

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Views of Faraday

Faraday as a Natural Philosopher. JOSEPH AGASSI. University of Chicago Press, Chicago, 1972. xvi, 360 pp., illus. $12.50.

The Selected Correspondence of Michael Faraday. Edited on behalf of the Royal Institution of Great Britain by L. PEARCE WILLIAMS with the assistance of Rosemary Fitzgerald and Oliver Stalleybrass. Cambridge University Press, New York, 1972. In two volumes. Vol. 1, 1812–1848. Vol. 2, 1849–1866. xx, 1080 pp., illus. $55.

In retrospect Michael Faraday is recognized as a towering figure in 19th-century physical science. We celebrate him as the discoverer of induction, of the electrochemical equivalent, of diamagnetism, of a means of producing continuous motion from an electric current, and of a host of other relationships any one of which would have been sufficient to give his name immortality. We admire his dogged pursuit of an anticipated experimental result: years spent looking for an effect on light by magnetic force (which he discovered) and for similar effects by electric and gravitational forces (which others found after his death). We marvel at his intuitive creation of the concept of lines of force which were subsequently written into physical law through Maxwell's equations.

But it is no longer acceptable historiography to remember a man simply because he was proven right or wrong. This is a point Agassi makes on several occasions, and properly so, though at the same time he is a bit unfair in assuming that present-day historians of science have not progressed beyond this simplistic view. It is generally recognized that a man should be studied against the background of his own times and judged in terms of the knowledge, theories, and prejudices that enveloped him; that we should be more interested in how he performed his science than in whether it came out right; and that we may well be more interested in the imagination and boldness (a favorite Agassi term) that went into one of his failures than we are in his successes. Both the works at hand help us in this sort of interpretation of Faraday, though for quite different reasons.

Williams surely must have felt uniquely fortunate as an editor in being able to start off his chronologically arranged selections of correspondence with a delightful letter from young Faraday describing the values of letter writing. Thus in July of 1812 he wrote to Benjamin Abbott:

> I dear A-----, naturally love a letter, and take as much pleasure in reading one, (when addressed to myself:) and in answering one as in almost anything else. . . . I also like it for what I fancy to be good reasons, drawn up in my own mind upon the subject; and from those reasons, I have concluded, that letter writing improves; first, the hand writing secondly, ... the expression, the delivery, the composition a manner of connecting words.

thirdly it improves the mind, by the reciprocal exchange of knowledge. fourthly, the ideas; it tends I conceive to make the ideas clear and distinct. . . . fifthly it improves the morals: I speak not of the abuse, but the use of Epistolations, (if you will allow me to coin a new word to express myself) and that use I have no doubt, produces other good effects. . . . [p. 3].

Faraday took pains with his own writing, so that most of his letters are well constructed and indeed a pleasure to read, though often punctuated rather bizarrely. The chief value of the correspondence, of course, is the opportunity it gives us to see him on a truly contemporary basis. The importance, however, is somewhat mitigated by the following factors. Williams himself drew heavily on this material in writing his comprehensive biography of Faraday, which was published in 1965. And most of the more interesting selections (about 25 percent of the total) have been reproduced either there or in one of the earlier biographies (though often in abbreviated form). Furthermore, the existence in published form of Faraday's laboratory diary with all of its detail makes the letters not unique as source material.

The title indicates that this is selected correspondence. Unfortunately Williams gives us no very good indication how the selection was made, except to say that the letters between Faraday and C. F. Schoenbein have been omitted because of their availability in separate published form. We don't even know the total number from which the 814 letters reproduced here were chosen. Williams states that he wanted "to illustrate all the facets of Faraday's life" (p. vii), and the fact that 202 separate correspondents are represented gives substance to his aim. But notably absent are any letters to or from Faraday's wife.

In spite of these caveats, historians of science will obviously find that this work contains much useful source material. Others may enjoy browsing through it for a glimpse of Faraday's mind—and of scientific activity in the 19th century. I commend in particular the exchange with Benjamin Abbott, which covers the years of Faraday's youth and shows him struggling in his early development as a scientist. There is the correspondence with William Whewell, through which familiar words like "anode," "cathode," "ion," and "electrolyte" and unfamiliar ones like "electrotonic" and "sphondyloid" are coined. And there is a letter to the *Times* in which Faraday describes some observations he made while on a boat trip on the Thames:

The appearance and smell of the water forced themselves at once upon my attention. The whole of the river was an opaque brown fluid. In order to test the degree of opacity, I tore up some white cards into pieces, and then moistened them, so as to make them sink easily below the surface, and then dropped some of these pieces into the water at every pier the boat came to. Before they had sunk an inch below the surface they were indistinguishable, though the sun shone brightly at the time, and when the pieces fell edgeways the lower part was hidden from sight before the upper part was under water [p. 801].

Annotations to the letters are slight but adequate. The existence of Williams's biography obviates long and detailed disquisitions.

Agassi also states that he would like to present a broader view of Faraday than he feels has been done in the past. Specifically, he writes that his purpose is "to consider and compare two portraits of Faraday . . . the private, personal, or psychological, and . . . the public or scientific" and "to integrate the two as much as possible" (p. xi). But the contents do not bear this out; instead the basic theme follows from the title, that Faraday wanted desperately to be recognized as a philosopher and was frustrated because his theoretical constructions (of space full of lines of force) were either misunderstood or ignored by his contemporaries, and his imagination and boldness were likewise lost on them.

Agassi's mode of attack is to assume that everyone thinks of Faraday as strictly an experimentalist, show that he was strongly driven theoretically, and then argue that there is a dialectical interplay between the two à la Karl Popper. The question arises, especially after Williams's biography, whether all this needs to be done. The answer is probably no. The argument could be

"Professor Faraday F.R.S. F.G.S." [From the Smithsonian Institution] condensed into a shorter monograph with, I think, much more satisfying results.

This would still leave room for a number of nice insights which Agassi injects along the way. For instance, he disputes the view that Faraday gained from his isolation and singular devotion to work, analyzing specific examples where isolation led to inefficient operation (p. 230). He also has an explanation for Faraday's method of performing so many experiments and so many variations of experiments in pursuit of a particular relationship. Agassi believes this was a natural outgrowth of Faraday's theoretical "program"—a program which consisted of explaining existing theories in terms of lines of force. Instead of filling in the detailed rules and laws theoretically, he would suggest to himself various possible empirical corollaries which he would then probe experimentally (p. 230).

All of this I find quite stimulating. But at the same time it seems to me that the book has some major difficulties. Most important, perhaps, is that it doesn't take proper account of Williams's 1965 biography. Agassi states that most of his research and even a first draft of the book were completed in 1956; indications are that no major modifications were made after that. References to Williams appear mainly as addenda at various appropriate points; a detailed discussion of the work is avoided. This avoidance is critical because the major thread through Williams's book is Faraday's continual concern—originating in the 1820's when he was experimenting with steel and glass (Agassi neglects this phase of Faraday's work)—with activity occurring in the space between atomic centers. Williams casts this all in terms of a stronger relationship to the atoms of R. J. Boscovich than I would choose to make, but that's not the point here. Clearly he emphasized the same sort of theoretical commitment by Faraday as Agassi, and this should be recognized.

Agassi's style, which is florid and often entertaining, can also be obscure. The book would have gained considerably from an additional revision to tighten up the language.

Matching the passages where Agassi has added new scope to the interpretation of Faraday, there are a number of places where he seems to wander off on the wrong track. I shall mention three instances. In the first he discusses the origins of Faraday's self-imposed semi-isolation and calls it in large part a reaction to the controversy with Wollaston in 1821 over the discovery of the rotation around a magnet of a wire carrying an electric current (p. 34). No mention is made of Faraday's affiliation with the small and self-contained Sandamanian church. The exact impact of this relationship on his scientific work cannot be assessed, but its influence on the rest of his life was obviously considerable, and in the direction of an isolated and asocial existence. I don't think it should be ignored in treating these same characteristics in his scientific activities. Second, in his discussion of the concept of conservation (p. 203ff) I think Agassi is wrong not to differentiate more strongly between energy and force. Admittedly these are words that lacked clear differentiation in the early 19th century, and when Faraday talks of conservation of force he sometimes means something very close to the conservation of energy. But this very confusion made it even more important that measurements and calculation be done so that very specific comparisons could be made and units could be defined. This Faraday, unlike Mayer and Joule, did not do. Third, in a discussion of Ohm's law Agassi describes it as being false for circuits with appreciable inductive effects (p. 242); but such is not the case as long as the current is unidirectional and constant.

Both books are well indexed by name and subject and are easy to use for reference purposes. I welcome them as additions to the Faraday literature.

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A New Kind of Man

Coulomb and the Evolution of Physics and Engineering in Eighteenth-Century France. C. STEWART GILLMOR. Princeton University Press, Princeton, N.J., 1971. xx, 328 pp., illus. $13.50.

"One of the broader theses of this book," says Stewart Gillmor, "is that the great development of 'empirical' physics of the late eighteenth century came not only from improved and sophisticated experimental techniques and the wide use of mathematical analysis but from a fusion of these methods of investigation as seen in the work of a man like Coulomb" (p. 82). One might take exception. Wasn't Newton himself the same sort? Actually, Gillmor would agree; Coulomb followed Newton's style closely and deliberately. Yet the fact remains that until the late 18th century physics remained largely divided between the gadgeteers and the mathematicians, each claiming, perhaps accurately, to be Newtonian. Studies in heat, physical optics, magnetism, and electricity seemed beyond mathematical treatment except in trivial ways. Celestial mechanics, on the other hand, could not be bought into the laboratory. Gillmor's thesis seems fair and significant.

Why Coulomb into the breach? Gillmor does not tell us—he simply describes. Coulomb was a new kind of man, a scientific engineer, and as such found himself at the one point of intersection of the empirical and the mathematical, in fluid (and in this case, soil) mechanics as well as in the practical mechanics of rigid structures. Later, it was not a big step to the study of electricity and magnetism. Furthermore, Coulomb came up through the military service, through the one branch of the civil service able to accommodate scientific engineering. The Corps du Génie had existed since the days of Louvois and Vauban, providing the necessary matrix of need and tradition. And it was far more likely that the engineer would move into the Academy's sphere than to have an academician move the other way. Once in the Academy, Coulomb brought with him a new sense for measurement, for the problems of working with real physical objects, that the abstract mathematician often lacked. Contemporary science was born from the fusion.

To say all this is to go beyond the limits of Gillmor's essay, no doubt. He sticks to the business at hand. After a two-chapter biographical sketch, there are four chapters dealing with Coulomb's many memoirs in detail, followed by a brief epilogue. The whole is presented with grace and clarity, and it is clear that Gillmor is fully competent to understand the magnitude of Coulomb's achievement. The bibliography and the attention to detail show that the research was not merely adequate but exhaustive. Not only is Coulomb's work explained, the work of his predecessors is always summarized. The only ingredient lacking in Gillmor's story is information on Coulomb's personality, information that is, apparently, simply not to be found. Coulomb remains a man who seemed to desire his privacy and did not intrude his per-