Lyell did not permit any direction or progression in the history of life. He argued that all evidences of progression, such as the "late" appearance of mammals, were illusory and would be rendered inoperative as soon as older deposits were searched more thoroughly. Change yes, but progression no.

By the 1840's, the vindication of a progressionist viewpoint was assured, and with it the second victory of empiricism over a narrowly held type of steady-state view. This was due to many workers, but particularly to Roderick Murchison (1792–1871) and Adam Sedgwick (1785–1873), who were describing the oldest fossils then known. By 1841, their Cambrian-Silurian faunas were recognized from many places in the world; they were always the same, quite distinct from younger faunas, and clearly indicated a directional and progressive change in the history of life. Lyell, however, clung to his steady-state vision at least until the early 1850's. A more flexible view of what a steady state might involve was apparently not possible for him because to grant a directional change might have the effect of endorsing the chief mechanism of adaptational change then considered, that is, the hand of the Creator. Lyell, according to Rudwick, was motivated above all else by his desire to remove geology from theology.

But why was some form of evolutionary theory not acceptable within a steady state? Rudwick notes that some, most prominently H.-G. Bronn (1800–1862), were able to set aside the theological point, since no one doubted that God could act through secondary laws, such as gravity. Thus it would have been possible to insist upon "natural laws" without directly implicating a higher being. The question for Lyell, however, was how to explain the "designfulness" of adaptation. And in the 1840's, there was the additional all-important issue of the origin of man, for if man evolved by chance "he could not be held morally responsible for his actions, and the whole fabric of society was thereby threatened" (p. 207).

A particularly influential "natural" law was widely promulgated by the comparative anatomist Richard Owen (1804–1892). Owen combined functional insight with an explanation of the origins of structure in looking for homologous development as variations on archetypal themes. Since the various archetypes were recognized in the oldest rocks then known, their origin was essentially unknowable. Hence the diversification of functional themes could be studied without concern with the origin of the archetypes themselves. By 1850 this view of a historical progression of particular forms was widely accepted.

Within the framework provided by Owen, scientists (epitomized by Bronn) returned to the question whether regularities in the pattern of species appearance could "explain" the observations from nature. To derive various empirical "laws," not unlike the later Cope's law or various ecological laws, was held to be the goal of the paleontologist.

It was by shifting attention away from such empirical summaries to the cause of change in individuals that Charles Darwin (1809–1882) recast the issue before paleontologists. Rudwick supports the view that Darwin's barnacle monograph was indeed a test of the power of the evolutionary hypothesis to explain both morphologically "retrograde" and "advanced" lineages and not the simple empirical study it has often been claimed to be. Darwin also found, as had been emphasized by paleontologists for many years previously, and indeed is often emphasized today, that typical faunas of fossil barnacles (or brachiopods or trilobites or snails) give "no positive evidence for slow trans-specific evolution" (p. 234). The fossil record was a liability to Darwin, and he made the most of its admitted gaps in defending his theory. Through the 1860's and 1870's the paleontological input to the evolutionary argument shifted from a search for the origin of species to illustrating the succession of genera, as for example in the evolution of horses, and to drawing attention to those significant intermediate forms which did exist, such as the reptile-like bird *Archaeopteryx*. Natural selection as the sole or even the most important causative factor in species evolution seems to have been increasingly deemphasized by the 1870's. The term "mutation" was coined by the paleontologist W. Waagen (1841–1900) in describing rapid morphologic changes observed in a vertical sequence of Jurassic ammonites, and other, equally "natural" forces, also not clearly understood in mechanism, were offered to explain the paleontological fact of successive morphological change.

On this note, Rudwick closes his narrative in 1870, when the major modern theoretical lines in paleontology had been cast. The view in ascendancy then is probably also dominant today, namely that the empirical summation of the "facts" of the fossil record is the way to derive paleontological laws. Now, a century later, this conception of the fossil record is being questioned for yet a third time, again by presentation of equilibrium models (D. M. Raup, *Science* **177**, 1065 [1972]; D. M. Raup, S. J. Gould, T. J. M. Schopf, D. S. Simberloff, *J. Geol.* **81**, 525 [1973]).

Rudwick has interspersed through the book many comments about the changing degree of professionalism in science in general, about the way in which nationalism and internationalism have influenced changes, about the role of translations and review articles in giving publicity to points of view, and about the importance of personality and academic standing in determining the flow of events. This book is rich in ideas and has abundant anecdotes to illustrate particular points.

But why be concerned with our history? As Rudwick so correctly emphasizes, "The loss of historical perspective would lead to conceptual impoverishment" (p. 266). In moving ahead, one forgets why one is there. Rudwick has now provided us with an excellent book which will indeed bring historical perspective to the recurring debate about the use of equilibrium vs. historical models in paleontology.

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Origins of the Newer Sciences

Foundations of Scientific Method. The Nineteenth Century. Papers from a conference, Bloomington, Ind., Nov. 1970. RONALD N. GIERE and RICHARD S. WESTFALL, Eds. Indiana University Press, Bloomington, 1973. x, 306 pp. $10.

This book is a collection of 11 essays prepared for a conference marking the tenth anniversary of the founding of the department of history and philosophy of science at Indiana University. The conference had two prime aims: to re-emphasize the mutual dependence of philosophical and historical approaches to science, and to contribute to the understanding of the growth of scientific methodology in the 19th century. These aims have been reasonably fulfilled. The book is most distinctive by virtue of its range, from

logic (R. E. Butts, L. Laudan) through physics (L. P. Williams, M. Hesse), geology (D. B. Kitts), biology (G. Buchdahl, D. L. Hull, F. B. Churchill), and physiology (J. Schiller), to sociology (V. L. Hilts) and economics (H. S. Gordon).

Much excitement in the history and philosophy of science has been generated recently by attempts to provide a unified theory of scientific change. This collection of essays bears on this interest because of its attempt to utilize historical case studies to support philosophical points and because the 19th century was the period when scientific methods were applied to the biological and human sciences. Specifically, by demonstrating the complexity of 19th-century methodologies, these essays clearly expose the difficulty, not to say danger, of moving from individual historical examples to generalized theories of scientific change. The detailed studies show the many variables —philosophical, social, psychological, empirical, experimental—which introduce and support putative scientific theories. For example, Gerd Buchdahl's study of Matthias Schleiden's inductive method grounds it in philosophical maxims derived from Schleiden's post-Kantian education. Historians have not always been so appreciative of the fertility of post-Kantian philosophy for experimental biology. Frederick B. Churchill contrasts the experimental embryology of L. M. Chabry and W. Roux in the light of different French and German traditions (in teratology and mechanistic physiology respectively). Again, Victor L. Hilts argues that Francis Galton advanced to the notion of statistical correlation because of his interest in men who were different from, and not representative of, the average, whereas Adolphe Quetelet, lacking this interest, did not. Schleiden, Roux, and Galton are all characteristically seen as protagonists of scientific method, but whether there are unified grounds for this remains to be seen. The whole issue is implicitly raised in H. Scott Gordon's review of Alfred Marshall's foundation of scientific method in economics through concentration on the mechanics of the market rather than on the historical process of the development of wealth. Twentieth-century economists work within concrete frameworks, but whether their methods are scientific in the sense in which the physical sciences are is problematical.

It appears to me that the immediate achievement of these essays is historical. Only secondarily will the implications for an understanding of scientific method become clear. The editors have refrained from drawing out the implications because they "were convinced that the present understanding of the question is such that any attempt to single out a central theme was unlikely to be fruitful" (p. ix). The complexity of the historical variables discussed supports their view, but the relevance of the studies in the book to current interest in scientific change needs to be emphasized. The studies achieve important historical insights because they concentrate on the reasons for the application of particular methods. Thus Mary Hesse's interest in analogical argument helps clarify the historical development of Maxwell's electromagnetic theory. Again, Joseph Schiller, in discussing Claude Bernard's experimental method, sets up a framework for asking useful questions about the physiological contributions of earlier workers such as Bichat and Magendie.

The strength of the majority of the essays lies in their detailed historical discussion of methodologies that are of general philosophical concern. Points of weakness appear where the historical treatment is insufficient. Thus Robert E. Butts's study of Whewell's logic of induction does not sufficiently consider that Whewell's natural philosophy changed in structure over time and that it exemplified early Victorian metaphysics. What Whewell's view of induction means for 19th-century scientific methodology will become clearer when his metaphysics (for instance, the theological understanding of causal relations) has been identified. David Hull's study of Darwin and 19th-century philosophies of science is perhaps the widest-ranging of the essays, but it does not do justice to the metaphysics of Herschel, Whewell, and Mill. Hull is too willing to see issues as polarized rather than complex; he ignores a long historical tradition (going back through Newton) when he remarks, "The facility with which Herschel, Whewell and Mill could demand the exact verification of scientific hypotheses and the exclusion of occult qualities from science on the one hand while on the other asserting God's direct intervention in natural phenomena is nothing less than schizophrenic" (p. 122).

Fortunately, the philosophically informed willingness to study historical detail is predominant. As a result, we have a useful contribution toward solving the problem, clearly presented by J. T. Merz at the beginning of the present century, of explaining why the application of methods, claimed to be "scientific," suggested the ideal of the unification of diverse physical, biological, and human disciplines during the 19th century. And the existence and nature of this unity must be shown if there is to be a single version of the process of scientific change.

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Old World Prehistory

South Asian Archaeology. Papers from a conference, Cambridge, England, July 1971. NORMAN HAMMOND, Ed. Noyes, Park Ridge, N.J., 1973. xii, 308 pp., illus. $20.

The papers in this volume are introduced by an essay listing nine major problem foci for archeology in southern Asia (F. R. Allchin). These problem foci are essentially historical in nature, relating either to the description of major epochs or to their "origins." This restatement of problems shows both how little the perception of archeological research objectives has changed in this area over the past 25 years and how a few directions of change are now beginning to appear. The papers that follow in many instances reflect some of these new directions, but in a very limited way.

In the realm of the natural sciences Goudie's report on the geology, geomorphology, and prehistory of the Gujerat plain suggests the gains to be made by developing integrated studies of the environmental and ecological conditions surrounding cultural developments in the subcontinent. A surface survey for Afghan Seistan (K. Fischer) underlines the importance of an understanding of the ecological forces at work in this part of the world.

One of the greatest needs in archeology in this area has been the systematic study of settlement organization through extensive horizontal excavation. This has been attempted for the Paleolithic at the site of Chirki on the Pravara River, a tributary of the Godavari (G. K. Corvinus), and for the major Bronze Age Harappan city of Kalibangan (B. K. Thapar). The effort for the Paleolithic promises to break that