Of course it is often the case that the scientist works within a departmental and teaching structure that is hidebound and insular. But the discipline surely also includes non-teaching, non-university staff, not so affected by the way the educational cake has been cut. Hence any discussion of the *achievements* of biochemists must often refer to those less affected by the medical and clinical constraints felt by their teaching contemporaries, for instance because they worked in the Rockefeller Institute, the Kaiser Wilhelm Institute for Leather Research, the Carlsberg Laboratory, the Lister Institute, or the Institut Pasteur. Equally, an evaluation of the growth of a discipline cannot be made only on the basis of the statistics of university teaching positions as long as there exists a flourishing research tradition outside the teaching arena.

Now Kohler admits that "some minimal level of intellectual achievement is, of course, a necessary condition for institution building," but he confesses, "I do not believe, as I once did [1973], that particular theories have, in general, a causal role in the creation of disciplinary institutions." What, we may well ask, is the difference between "necessary condition" and "causal role"? Surely there are good grounds for claiming that the central place of intermediary metabolism in biochemistry was due to the theory of the metabolic pathway *and* to the appropriateness of such a conception for the investigation of nutrition and clinical disorders therein. The results of such a focus were successes in some areas, for example the tricarboxylic acid cycle as illustrated on the dust cover of Kohler's book, and failures in others—the multienzyme system for protein synthesis. This model, as Bartels has well shown, was widely favored among biochemists but was eventually displaced by the template concept. Kohler states that the association of biochemistry with medicine resulted in its development in the narrow context of human physiology and pathology rather than in the broader context of general biology with its "major problems," which he considers were wrongly ignored by most biochemists and were picked up by other disciplines, principally by those swashbuckling molecular biologists. This looks suspiciously like a Whiggish retrospect upon what the discipline *ought* to have done, and upon what the biochemists *should have* identified as the "great problems." The fact that biochemists saw protein synthesis as a great problem long before its successful solution and that biochemists at Massachusetts General Hospital and the New York University School of Medicine helped to solve it does suggest that medical and clinical shackles cannot have been so restricting as this book suggests. To identify what were the major problems in need of solution retrospectively makes for poor history, but the corollary that, say, respiration and fermentation were not major problems is simply absurd. Surely it was precisely the concentration of biochemists upon enzyme-controlled reactions in metabolic pathways that allowed them to contribute so much to the solution of these problems. Moreover, the fact that biochemistry has retained its allegiance to medicine and yet has both contributed to and absorbed the fruits of molecular biology suggests that the discipline not only has shaped the careers of biochemists but has itself been shaped by them.

In short, the study of the political ecology of a discipline on its own may yield a false sense of the all-sufficiency of such a mode of analysis, just as the old-style study of the history of ideas tended to give a distorted image of the power and independence of ideas. It would indeed be a sad day if history of science were to become dominated by the history of the ecology of disciplines. It would be dull, too—the details of academic appointments, of the growth and decline of departments, of battles within societies, can tax the reader's patience and cause one to yearn for zymase, coenzyme I, and the *Atmungsferment*.

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Vitalism Reexamined

The Strategy of Life. Teleology and Mechanics in Nineteenth Century German Biology. TIMOTHY LENOIR. Reidel, Boston, 1982 (distributor, Kluwer Boston, Hingham, Mass.). xii, 316 pp. $59.

Few aspects of the history of biology are more commonly misunderstood than the issue of vitalism and mechanism. Superficial treatments of the subject regularly lump a wide spectrum of views, sharing the feature of opposition to the proposition that biology can be reduced completely to physics and chemistry, together as expressions of the single creed that a vital force directs the phenomena of life. The advent of a modern, progressive biology is often identified with the overthrow of this putatively empty creed. Historians of biology who have studied carefully the early 19th century, the period most crucial to this supposed transition, are well aware of the inadequacy of such conventional accounts; but little has been done to confront the complexity of the positions that those on the "losing" side of this debate actually maintained. In *The Strategy of Life* Timothy Lenoir has taken a major step in that direction by analyzing sympathetically and skillfully a tradition of thought represented by many of the leading German biologists of the period. Their viewpoint cannot be fitted within either of the simple categories of mechanist or vitalist; for it combines elements assumed to characterize both sides of this dichotomy.

These German biologists, Lenoir stresses, were not advocates of that romantic philosophy of nature of the period known as *Naturphilosophie*; nor did they argue that design in nature implied a divine Creator, as did their British counterparts. Rather they believed that organization was a fundamental feature of biological events. Some kind of organization must be present in the first place for an organism to develop, and the development itself follows an ordered pattern that cannot be derived from physical and chemical principles. These features cannot be understood without invoking purposes. Teleological thinking is therefore both justifiable and inevitable. Organisms *function*, however, in strict accordance with physical and chemical laws; and physical and chemical methods ought to be pursued as far as possible in the examination of biological processes. The principal philosophical foundations for this position the German biologists acquired from Immanuel Kant. To these principles they gradually added concrete programs for implementing them in systematic research.

Lenoir describes the thought and investigations of the several generations of German biologists who he shows developed this tradition. The early group included especially J. F. Blumenbach, C. F. Kielmeyer, G. P. Treviranus, and J. R. Meckel. The two most prominent members of the next generation were the dominant physiologist of the era, Johannes Müller, and the most important embryologist, Karl Ernst von Baer. Von Baer is, in fact, the central figure in this book, in part because he was one of the most sensitive thinkers about its basic principles, and because he lived to defend them into the 1870's.

The bearers of this biological tradition did not identify themselves by any single label. To circumvent that lack, Lenoir has named the framework of ideas that they held in common "teleomechanism." He views this tradition, however, as separable into three stages, for which he has invented subtitles. "Vital mechanism" was the earliest version of the program. The more powerful versions of von Baer and Müller he names "developmental morphology." The third stage, resulting from the critiques of Hermann Lotze and Justus Liebig and from the work of Carl Bergmann and Rudolph Leuckart in the 1840's, eliminated the concept of a special vital force from the teleological framework; Lenoir calls this phase "functional morphology." The final two chapters of the book focus on the development of the new research programs that collided with and ultimately overshadowed teleomechanism, especially the reductionist approach of Hermann Helmholtz and the Darwinian account of evolution.

The central themes that the teleomechanist biologists maintained remained very similar from the time of Kant to their last echoes in von Baer's criticism of Darwinism in 1874. Adopting the scheme of Imre Lakatos, Lenoir defines these persistent ideas as the "hard core" of their research program. Many variations on these themes appeared, however, as they were applied to particular areas of biological investigation—to embryology, to the advent of the cell theory, to the major developments in physiology and physiological chemistry of the 1840's. In describing this interplay between general principles and specific investigative efforts, Lenoir is especially illuminating. In his summaries of the ways in which observational or experimental approaches to particular biological problems grew out of, sustained, or were interpreted by means of the teleomechanist principles, he provides probing insights into the ways in which general ideas derive new meanings from changing contexts.

This book represents an impressive overall achievement and should stimulate a major change in the way future historians of science treat this formative period in the emergence of modern biology. There are, however, some prominent difficulties. The most serious, I believe, lies in the retrospective labels Lenoir attaches to his group of biologists. Although he makes clear in the introduction that he has himself imposed the terms "teleomechanism" and its subcategories on the thought of his subjects, the regular repetition of these terms soon builds the impression that "vital mechanism," "developmental morphology," and "functional morphology" were clearly delineated schools in their own time. The risk in these designations is that they impose more definite boundaries between ideas of individuals than actually existed and suppress basic differences between those grouped together.

A lesser flaw, but nevertheless a problem for readers, is that Lenoir's strategy requires him to repeat the same basic ideas over and over in order to demonstrate that all the important German biologists he describes held to them. The result is persuasive but somewhat oppressive. Most superficial, but most distracting, are deficiencies in the style and editing of the book. In his best passages Lenoir expresses his ideas forcefully, vividly, and with originality. In other places, however, he lapses into cumbersome, wooden sentences that more than belabor his arguments. Moreover, typographical errors are blatantly frequent. Most of them are minor, but a few are serious enough that the sense of a whole sentence or paragraph nearly disappears. In the printing of the book the type has been so clumsily aligned at the margins that some adjacent lines appear to be set in different type. A book of the significance of this one deserves more careful final preparation.

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Molecular Evolution

Macromolecular Sequences in Systematics and Evolutionary Biology. Papers from a symposium, Vancouver, Canada, 1980. MORRIS GOODMAN, Ed. Plenum, New York, 1982. xiv, 418 pp., illus. $45.

The study of molecular evolution has recently achieved a new level of sophistication as emphasis has shifted from the study and comparison of amino acid sequences of proteins to the direct analysis of the underlying nucleotide sequences. As technological advances in molecular biology make possible the rapid ascertainment of nucleotide sequences of a great variety of genomic regions (the coding regions of which can be inferred and translated into amino acid sequences through knowledge of the genetic code), it is appropriate to review what we have learned so far about mechanisms and phylogenetic patterns of molecular evolution. This appears to have been a goal of Goodman in assembling this group of papers.

Six of the papers originated from a 1980 symposium. Goodman has added three more, providing a volume that selectively spans an admirable breadth of topics in molecular evolutionary biology. The papers have been revised to include references through 1981 and are thus reasonably up-to-date, a difficult task in this fast-paced field. Though the quality, scope, and intended audiences of the contributions vary considerably, the book is a useful summary of some of the important features of macromolecular evolution as revealed by the analysis of protein sequences. It also provides a gentle yet reasonably thorough introduction to new directions in molecular biology and evolution, particularly for evolutionary biologists relatively uninitiated in molecular biology. Molecular biologists might also glean some direction concerning interesting organisms, sequences, or gene families to investigate at the nucleotide sequence level.

The book begins with a chapter by Novacek that reviews anatomical and paleontological interpretations of the phylogeny of eutherian mammals. The phylogenies and dates of divergence proposed for eutherians form the basis of many of the later arguments concerning rates and patterns of molecular evolution. The next four papers focus on several families of proteins. Their structure-function relationships and patterns of phylogeny and evolution are revealed by comparisons of their amino acid sequences from a variety of organisms. Some authors focus on the phylogenetic relationships of the proteins themselves (as in Hunt and Dayhoff's analysis of chromosomal protein families). The implications of the patterns of protein phylogeny for the systematics of the organisms are also discussed (for example by Beintema and Lenstra with respect to ribonucleases, by De Jong with respect to eye lens proteins, and by Goodman *et al.* with respect to globins). Ironically, the instances where sequence and organism phylogenies disagree are the most enlightening. It is from these examples that we often discover significant yet previously overlooked evolutionary mechanisms (for example concerted evolution) and evidence of the frequency with which events such as gene duplication occur. Put in the context of the dates of divergence postulated by paleontologists, molecular phylogenies also provide a rich though potentially unreliable source of information on the frequencies and rates of various modes of sequence evolution. Individual proteins clearly cannot be counted on to provide accurate molecular clocks. In addition, what may be a reasonably accurate clock in