Book Reviews

Biochemistry as a Political Institution

From Medical Chemistry to Biochemistry. The Making of a Biomedical Discipline. ROBERT E. KOHLER. Cambridge University Press, New York, 1982. x, 400 pp., illus. $34.50. Cambridge Monographs on the History of Medicine.

"Disciplines are political institutions that demarcate areas of academic territory, allocate the privileges and responsibilities of expertise, and structure claims on resources." So writes the author of this book, whose aim has been to focus upon the development of the symbiosis between biochemistry and medicine rather than upon biochemistry "as a system of ideas." Such an approach is rewarding because disciplines act as guides for both intellectual and political behavior. It is the discipline, above family, party, class, even educational experience, Kohler tells us, that shapes a scientific career. Since intellectual discourse among scientific communities is regulated by the disciplines, "they are indispensable for understanding innovations that may occur when academic boundaries and trade relations shift."

Having waved the flag for social history of science and fixed his gaze upon the political economy of science, upon the political map of scientific disciplines, where the language is of competition, entrepreneurship, and resource management, Kohler settles to the task of explaining why the building of the discipline of biochemistry has differed in the United States, the United Kingdom, and Germany. This occupies the first six chapters, which are followed by five chapters on the three distinct styles of biochemistry identified by Kohler as clinical, bioorganic and biophysical, and biological, each being defined in terms of "paradigms and constituencies" in other disciplines. Finally comes a chapter on the response of the biochemical community to the challenge presented by those calling themselves molecular biologists. Their success in unraveling the great unsolved problems of biology is contrasted with the biochemists' failure to become interested in them. Biochemists are pictured as burdened with their medical service roles and their commitment to utilitarian goals of nutrition and clinical medicine. These hindered them from making more radical changes in their programs, with the result that the institutional structures and relations of biochemistry have remained what they were when first created in the early 1900's.

In the case of Germany, physiological chemistry, modeled on the subjects treated in Liebig's *Animal Chemistry*, was institutionalized with the first chair at Tübingen in 1845, followed by eight other chairs. Meanwhile chemistry was transferred from the medical to the philosophical faculty, where it took its place with other natural science as *rein Wissenschaft*. This separated physiological chemistry from chemistry, and its institutional development went into decline, more chairs disappearing than were created until after World War I. Hoppe-Seyler, for all his industry, "could not remedy the basic institutional weakness of his discipline." Nor was the situation rescued by alternative affiliations with organic chemistry, pharmacy, pathology, and hygiene.

In the United Kingdom there was a more consistent pattern of association between biochemistry and physiology, which Kohler attributes to the strength of general physiology in the great tradition set by Huxley and Foster and to the opportunities for teaching medical students that British physiologists exploited. Biochemistry at Liverpool, as the important exception, only proves the rule, for its location soon shifted from hygiene to physiology under the influence of Sir Charles Sherrington. Yet the British physiologists were not prepared to support biochemistry against their own interests. Consequently there were but four independent chairs in the United Kingdom by 1925.

By contrast biochemistry in the United States was institutionally strong. This Kohler attributes to the movement to reform medical education. This movement culminated in the celebrated Flexner Report of 1910. Competition for survival under raised entry requirements to medical courses, which the reforms demanded, drastically cut the number of medical schools in the 1920's. With dwindling recruitments medical schools were forced into the arms of the universities, which brought about a further rise in academic standards. General chemistry being a requirement for entry, medical schools ceased to teach it. Instead they taught more specialized courses. Increasingly the old-style medical chemists were replaced by professional biochemists. "As the reform movement gained momentum," writes Kohler, "specialization became a good strategy for advancing a career. What had been a marginal role for medical chemists became the basis for a new biomedical discipline." Hence it was the backwardness of these medical schools in the biomedical sciences that gave the opportunity for the "sudden success of biological chemistry. . . . Biochemistry did not evolve gradually out of physiology, as in Britain; it was not split between organic chemistry and physiology, as in Germany. In the United States, biological chemistry emerged like a butterfly from the cocoon of medical chemistry."

In describing the styles of biochemistry Kohler identifies the clinical emphasis in the United States, the Oxbridge style of general biochemistry in the United Kingdom, and the chemical physiology and bioorganic chemistry in Germany. But these are broad characterizations, and Kohler is too good a scholar not to point to the important exceptions that crop up throughout these chapters.

Kohler's book is backed by a detailed study of the archives from departmental, personal, and grant agency sources. His knowledge and organization of all this material compel our admiration, and the text with its 52 pages of footnotes has been beautifully produced. His construction of the political geography of the discipline of biochemistry will serve as a constant reminder to us of the role of the educational marketplace in constraining and shaping the development of a discipline.

In this day and age we are surely receptive to arguments for such influences upon science. How far can they be taken? Is a discipline merely a political institution, and not also a body of knowledge built upon significant achievements like Buchner's extraction of zymase or Warburg's discovery of the *Atmungsferment*? Was F. G. Hopkins in 1913 simply describing the demarcation of territory when he urged young organic chemists who were attracted to biological problems to be prepared to do more than determine the constitution of animal products and their reactions in vitro? Was there no research experience behind his judgment that such achievements do not make a chemist into a biochemist?

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