

## A Reshaping in Biology

### **The American Development of Biology.**

RONALD RAINGER, KEITH R. BENSON, and JANE MAIENSCHN, Eds. University of Pennsylvania Press, Philadelphia, 1988. xii, 380 pp. \$37.95. From a conference, Friday Harbor, WA, Sept. 1986.

Through much of the 19th century, "biology" meant "natural history," the observation and analysis of flora and fauna in the wild. Natural history provided a Virgilian pastime for amateur collectors—people liked to believe that a good naturalist could not be a bad man—and it fueled popularizers intent upon instructing their sizable lay audiences that the works of nature revealed the beneficent hand of God. For professional scientists, natural history supplied, of course, the fundamental data of evolutionary debate. Natural history museums, their specimens arrayed in upright cases of glass and wood, were commonplace features in cities and on college campuses, where they were used for both teaching and research.

Morphological analyses of the specimens—for example, to ascertain the relation of form to function and the adaptive value of successive forms—figured prominently in the evolutionary discussions. However, the analyses were mainly speculative and inconclusive, much to the increasing dissatisfaction of many biologists late in the century. Calls were increasingly heard for what historians have called "a revolt against morphology"—a sharp turn in the direction of making biology concrete rather than speculative, experimental rather than purely observational, quantitative rather than qualitative. Between the 1880s and the 1920s, biology in America did change dramatically, differentiating partly into experimental fields such as embryology and genetics. But was the change broadly marked by a revolt against morphology?

*The American Development of Biology* began in consideration of that question but goes considerably beyond it. Intelligently organized and presented, the volume comprises ten well-researched, original essays by as many historians. The essays deal with fields that grew out of the concerns of natural history—including ethology and plant ecology—omitting consideration of biochemistry, physiology, and anatomy, which did not. They reach back into the mid-19th century and forward into the 1930s, exploring the character and work of natural his-

tory, the intellectual and institutional transformations of American biology, and the emergent boundaries and definitions of the discipline. A few, like Joel B. Hagen's rendering of Frederic Clements's theories of a physiological ecology, are rewarding though conventional intellectual histories. However, collectively, the essays bespeak the expansion in recent years of the study of the history of biology (and of other scientific disciplines) beyond the pure history of ideas to include social, economic, and institutional context and its shaping influence upon scientific research programs.

The volume's answer to its starting question is that there was no sharp or uniform revolt against morphology in late-19th-century American biology. Sally Gregory Kohlstedt demonstrates that, into the 20th century, natural history museums continued to be built on the campuses and to be regarded as essential for teaching and research. In a chapter on the organizational exfoliation of American biology into numerous societies, Toby A. Appel shows that experimentalists were gradually incorporated into the American Morphological Society itself, which in due course became the American Society of Zoologists (whose centenary next year occasioned the publication of this volume). The vertebrate paleontologist Henry Fairfield Osborn joined morphological matters to issues of heredity and evolution, institutionalizing the combination at the American Museum of Natural History in a research program that, as Ronald Rainger convincingly analyzes it here, was important not for Osborn's own theories, which were largely wrongheaded, but for the attention that he stimulated his younger associates to give to biogeography, the process and pattern of evolution, and the relationship between inheritance and development. Yet Osborn's program did not at the time revolutionize vertebrate paleontology; in the 1920s, many paleontological studies remained absorbed with descriptive, taxonomic questions. Biology was transformed gradually, Keith Benson argues in a solid essay on the shift from the museum to the laboratory: The transformation was not linear and its pace varied depending on the institution. Rather than a rejection of natural history, it involved subjecting natural historical materials to laboratory scrutiny and the microscope.

The transformation was energized in part by the vision of some that biology in Ameri-

ca, unlike that in Europe, ought to be made to rest on a fundamental core of concepts, problems, and techniques. The vision proved to be unrealizable in any grand sense, not least because, as turn-of-the-century observers pointed out, there was no unity to biology similar to that in physics or chemistry. The diversity of biology blocked attempts to form anything resembling an omnibus American Biological Society. (In the late 1930s, Scott F. Gilbert points out in a chapter on Edward Everett Just and Richard Goldschmidt, attempts to integrate merely genetics and embryology would still be doomed to failure.) All the same, advocates of a unified core pursued their vision, emphasizing the study of marine organisms, especially invertebrates, and the employment of experimental methods in a frame of studies that encompassed embryology, heredity, and evolution. And they exercised a decidedly shaping influence within a widening—and significant—realm.

Many advocates of the core were located at key universities, leading new centers of research activity, graduate education, and laboratory investigation that provided opportunities to pursue the core program at the workbench and disseminate it through publications and the training of students. Among them, at the University of Chicago, was Charles O. Whitman, who, as Jane Maienschn argues in a persuasive and discerning chapter, fostered a school of cell-lineage studies stressing the importance of both the cytoplasm and the nucleus in cellular development. And Whitman led the move to turn the Marine Biological Laboratory in Woods Hole, Massachusetts, into a major enterprise for the core, a summertime magnet for its advocates and their families, the nucleus of a biologically like-minded national community. As Philip J. Pauly writes in an imaginative chapter on the summer enclave, Woods Hole "formed the center for a cohesive academic elite" whose members rapidly established their program as the heart of the life sciences in America. Each summer between 1892 and 1910, an average of 63 scientists worked there, joined by up to 30 more investigators at the nearby Fish Commission laboratory plus 100 students, the total research population marked by strong representations from the University of Pennsylvania and Harvard as well as from Chicago, Johns Hopkins, and Columbia.

Perhaps the advocates of the core helped establish a particularly American biology. However, although many of the essays touch upon the special character of biology in the United States, none deals with the subject systematically, and neither does the volume as a whole. Moreover, the enlarge-

ment of historiographic approach that marks this volume leads to some questionable claims—for example, in the essays of Gilbert and Pauly. Gilbert interprets the cellular theories of Just and Goldschmidt in terms of their likely sociopolitical commitments—an intriguing exercise but an unconvincing one, especially in Just's case, not least because it rests on a reading of Just's cellular texts as a political metaphor that Gilbert does not support with appropriate corroborative evidence. Pauly, applying concepts of gender analysis to Woods Hole, contends that its vitality derived partly from the insulation it provided the research-oriented summer residents from an increasing contemporary perception of biology as effeminate, a branch of sciences that required "feminine" qualities of patience or delicacy and whose routine taxonomic work was being taken over by women. The evidence here advanced for the claim (and for a variant on it in the same chapter concerning the distress of biologists at their lack of martial utility during the First World War) is flimsy. Given that, the claim itself seems uneconomical, a gratuitous add-on to the plausible and, one would think, sufficient explanation otherwise argued by Pauly himself that the appeal of Woods Hole lay in its pleasant combination of family resort and fine laboratory, its advantageous marine-biological facilities, and the stimulation of capable colleagues.

However, socio-institutional treatment clearly pays off handsomely in Pauly's analysis of the Woods Hole influence in American biology, as it also does in Richard W. Burkhardt's splendid assessment of Whitman and Wallace Craig as pioneers of ethology in the United States. Despite the major significance of Whitman's and Craig's ideas on animal behavior, neither was able to establish the field, lacking suitable institutional opportunity (and, in Craig's case, a secure professional position). In a crisply incisive essay, one of the gems in the book, Diane Paul and Barbara Kimmelman deploy intellectual, economic, and institutional considerations to account for the rapid assimilation of Mendelism to plant breeding in the United States: Agricultural research installations, hoping to combat the economic consequences of overproduction by developing new varieties, were already well embarked at the turn of the century on programs of hybridization and cross-breeding; Mendelism explained the observations of experience and provided a guide for future work. Paul and Kimmelman explain in a similar fashion the ascendancy in American agriculture of hybrid corn over potentially superior pure-dominant varieties: The farmer would have been able to replant the pure-dominants;

since hybrids would decline in quality after the first generation, he would have to purchase fresh hybrid seed each year. Their study indicates, as does *The American Development of Biology* on the whole, that historiographic adventurousness can yield not only adventure but angles of understanding and perspective that may be otherwise unattainable.

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## A Medical Scientist

**Charles Richard Drew.** *The Man and the Myth.* CHARLES E. WYNES. University of Illinois Press, Champaign, 1988. xvi, 132 pp. + plates. \$19.95. Blacks in the New World.

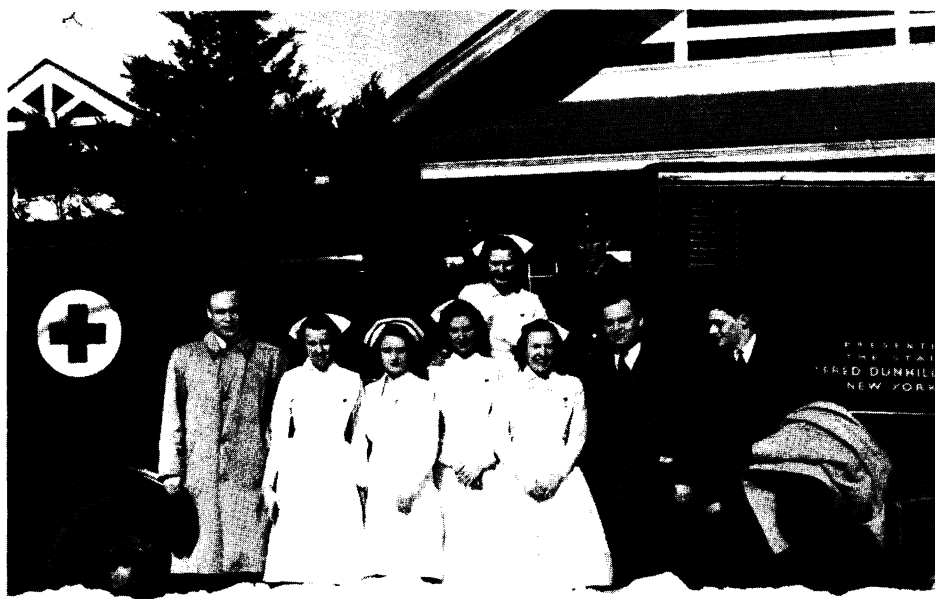
As a surgeon, teacher, moving spirit in the World War II blood supply program—and the central figure in some durable myths of American racism—Charles Drew has long deserved a biographer. Concisely, Charles E. Wynes puts the record straight.

Born in 1904, Drew was heir to the contradictions that beset the remarkable black community of Washington, D.C. Though segregated and discriminated against, its members enjoyed some patronage from the federal government's erratic

support of Howard University and Freedmen's Hospital. Drew himself received an excellent education at Dunbar High School, that "fiefdom of advantaged blacks" (p. 11), a seedbed of doctors, lawyers, cabinet members, and at least one senator.

After a college education won by his athletic prowess, he entered medical school at McGill University in Montreal, making a late but intense discovery of the joys of the intellect. He returned to Washington with his M.D., drawn by his family and by a post at Howard where he hoped "to help his people" (p. 21). He soon won a chance for advanced training at Columbia University and ultimately received the degree of Doctor of Science in Medicine.

His subject was transfusion and the banking of blood and plasma, and his sense of timing was excellent: while he wrote his thesis, the opening of World War II guaranteed bloodletting on a scale never before imagined. Drew's presence in New York was also fortunate; in 1940, he was selected as medical supervisor of the "Blood for Britain" program, and he stayed on to help establish the American program that grew out of it. Here is a crucial point in the Drew legend, and Wynes is at some trouble to define Drew's contribution—essentially, technical leadership, the bloodmobile, and a central system of quality control. His popular reputation as the developer of blood plasma was undeserved. Nor did he resign in



"Charles Drew with the first mobile blood collection unit." [Moorland-Spingarn Research Center, Howard University; from *Charles Richard Drew: The Man and the Myth*]