

SPECIAL SECTION ON AMERICAN MORPHOLOGY AT THE TURN OF THE CENTURY

Introduction: Were American Morphologists in Revolt?

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The following papers on American morphology are dedicated to the memory of our friend Dov Ospovat.

The history of late nineteenth-century and early twentieth-century American biology offers an exciting research area, replete with questions. Not only did an extraordinary profusion of new techniques and theories emerge at the turn of the century, but at roughly the same time biology began to undergo professionalization and to become institutionally accepted. Underlying these changes was a fundamental shift in emphasis toward "modern," "analytical," "experimental" biological science, a shift in both the content and the methods of American biology. American biology also came into its own as an autonomous pursuit, and a number of eager young Americans enthusiastically endorsed various aspects of the "new biology." Because of these complex changes, an increasing number of historians of science have recently become concerned with turn-of-the-century biology in America.

In particular, Garland Allen's views have been read so widely that it seems necessary to consider his work carefully. In fact, this consideration constitutes the shared task of the following papers. The three of us have, through our respective studies, come to question both some of the facts Allen presents and some of his interpretations of what happened in turn-of-the-century American biology. We all agree that important shifts in emphasis did occur, but the three of us have reached different conclusions than did Allen about the nature of the science and of scientific change at the time.

As a result of our parallel interests but our "revisionist" interpretations, we organized for the History of Science Society meeting in New York in December 1979 a session entitled "Morphology and the Emergence of Modern American Biology." That session, which included Garland Allen's response and comments by Stephen Gould, and subsequent conversations with Garland Allen and others working on similar problems, have led to the following revised papers, all based on but expanded beyond the thoughts presented in New York. To our delight, historians of modern biology have proved remarkably open and enthusiastic in their willingness, indeed eagerness, to discuss even their

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most cherished ideas with colleagues. Conversation has proved stimulating and productive, and we hope this special section on American morphology, including our three papers, Garland Allen's response, and an overview assessment by Fred Churchill, will encourage further discussion and exchange of ideas.

Because Allen's views have provided the take-off point for the papers that follow, it is essential that we explain what we think his work suggests and the issues on which we disagree. To avoid redundancy in our papers, we have outlined in this introduction his views and the major problems they pose.

Basically, Allen has described, especially in *Life Science in the Twentieth Century*, the shift in American biology as a move from morphology to experimentation.¹ Indeed, he went even further in that book, suggesting that the new experimentation actually involved a revolt from morphology. This is one of the primary points on which we initially disagreed with Allen and which has continued to prove troublesome. To us, it makes no sense to discuss a move from morphology, which is defined by subject matter (namely, organic structure), to experimentation, which is characterized by a methodological approach. In fact, one can operate quite well as an experimental morphologist; so a biologist who endorses first morphology and later experimentation need not be considered as shifting his approach from one to the other. The concept of a revolt seems to make no sense in this context.² Furthermore, no revolt from morphology took place historically. Each of us has come to emphasize this point in his or her respective study: Rainger by showing the continuation of tradition in morphological paleontology, Benson by documenting the concern of the transition figure William Keith Brooks with both morphology and experimentation, and Maienschein by showing that even full-scale acceptance of experimentation did not involve rejection of morphological interests in many of the critical cases cited by Allen. Thus we all insist that no general revolt from morphology occurred.

Garland Allen has, since our New York exchange, open-mindedly admitted his error on this point, and he has shifted the ground of our disagreement. Yet he still sees other dichotomies and revolts as he studies the period. Most recently, he has pursued the significance of the

1. Garland Allen, *Life Science in the Twentieth Century* (Cambridge: Cambridge University Press, 1978).

2. John Beatty, in his review of Allen's *Life Science* in *Ann. Sci.* (in press), also emphasizes this point.

Introduction: Were American Morphologists in Revolt?

naturalist-experimentalist dichotomy, arguing that the naturalist tradition, which had predominated at the end of the nineteenth century, gave way to the rapid ascendancy of the new experimental tradition. He has accounted for the shift in emphasis that we all agree occurred on some level in American biology in the early twentieth century as a change (revolutionary according to some sense of the term) in which of those two supposedly opposed traditions predominated.

Citing William Bateson at some length to establish that the dichotomy did exist, Allen depicts naturalist biology as based on field study, descriptive methods, and concern with the organism as a whole, and with its evolutionary context. Experimental biology was supposedly dependent on analytical methods and experimental manipulations in laboratory studies, and dealt with an organism as the sum of its parts. Allen insists that the naturalist-experimentalist dichotomy was fundamental, that it had characterized biology throughout the nineteenth century, and that it had assumed a new importance by 1900.³

Prior to Allen's work, Ernst Mayr had suggested a distinction in biology between functional studies and evolutionary studies.⁴ The former analyze inward and address questions about how something works, while the latter ask "why?" within the historical, evolutionary context. Allen refers to Mayr's distinction as if it were the same as his own.⁵ Yet Mayr does not claim that functional and evolutionary studies are in opposition to each other. Both are compatible, both are valuable, and at times the two areas overlap, according to Mayr. They would seem to come into a dichotomous or opposition relationship only when individual scientists advocate studies solidly within one or the other range of possibilities and, for sociological or institutional rather than internal scientific reasons, come into conflict over who should receive the greater part of limited resources, greater status, and so on.⁶

Nonetheless, at first Allen appears to be on more solid ground with his naturalist-experimentalist distinction than with his idea of a revolt

3. Garland Allen, "Naturalists and Experimentalists: The Genotype and the Phenotype," *Stud. His. Bio.*, 3 (1979), 179-209; Allen "The Transformation of a Science: Thomas Hunt Morgan and the Emergence of a New American Biology," in *Organization of Knowledge in Modern America, 1860-1920*, ed. Alexandra Oleson and John Voss (Baltimore: Johns Hopkins University Press, 1979), pp. 123-210.

4. Ernst Mayr, "Cause and Effect in Biology," *Science*, 134 (1961), 1501-1506.

5. Allen, "Naturalists and Experimentalists," p. 179.

6. Mayr does not address the question of whether there is opposition, but it seems quite clear that he does *not* suggest that there is a dichotomy or opposition.

from morphology. At least this dichotomy – or distinction, or self-conscious antagonism – did exist. It is logically acceptable because Allen has defined both naturalist and experimentalist in terms of both subject matter and methodology. We can understand what he means in this case. Yet again there are problems, largely because the key terms in the dichotomy are difficult, elusive, and, we think, ill and imperfectly defined. In fact, it seems to us that throughout the period in question, the sense of what a naturalist and what an experimentalist were and the way in which such a distinction was even important shifted in gradual, subtle ways.

Even though there was a naturalist-experimentalist conflict in some sense around 1900, Rainger's study of paleontology shows that the naturalist study of paleontology underwent changes in the early years of the twentieth century that Allen did not take into account, and therefore Allen's characterization of naturalist biology is no longer adequate. Benson demonstrates that Brooks represents a joining of the naturalist and experimentalist elements; his interpretation of this important figure calls into question the general usefulness and accuracy of the naturalist-experimentalist dichotomy. And Maienschein establishes that the concept of experimentalism changed and was not strictly opposed to elements of the naturalist tradition, as Allen suggested.

In short, we have a major historiographic difference. Allen sees dichotomies and seeks to understand discontinuities and revolts in the history of biology. In contrast, we see more complex changes that cannot be stated in oversimplified terms as dichotomies; we seek to understand continuities and gradual change. True, as the pace of change increases – as it did around 1900 in American biology – the essentially evolutionary process might appear to produce revolutions. There might, therefore, seem to be little real difference between Allen's and our views of scientific change. It might be argued that the real difference is simply one of emphasis and orientation. Yet we think this difference is basic. An emphasis on dichotomies and explanation in terms of disputes and disagreements, suggesting revolt and revolution, offers a very different vision than our evolutionary approach supports. It is this overall suggested picture of revolutionary change, not just the specific points we have cited, that we seek to question and to revise.

The result of our dialogue should not be a mere compromise between Allen's revolutionary view and our alternative position. Neither should it be some sort of Hegelian synthesis, for we really are not suggesting an antithesis or radically different alternative to Allen's thesis. Rather, we seek to combine what is strong and instructive from each of our

Introduction: Were American Morphologists in Revolt?

approaches in order to provide a revisionist picture of the shifts in early twentieth-century American biology and to understand better the processes by which science changes.

We are responding to Allen's view that "history is not static, but the events and our reading of them are constantly changing. Thus the best history tries to phrase issues so precisely that others can go beyond and investigate them more thoroughly."⁷ Our attempt in the following papers is to phrase the questions still more precisely and to begin to refine the published account of what actually occurred, and why – in short, we strive to understand why turn-of-the-century American biology is especially significant for both practitioners and historians of science.

7. Allen, *Life Science*, p. xii.