How Biology was Unified

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of mammalian populations in relation to geography and ecology would have helped to give the volume more lasting value. Practitioners of vertebrate population genetics ought to have access to this book, but few will need it for their personal libraries. Rather, it is almost of greater interest as an historical documentation of changing views and standards. During the early 1970's workers in many electrophoresis labs seemed to be able to find selection acting everywhere. But then Lewontin's lab found almost too much variation, and, with perserverance, a few theoreticians drove home the point that the selective/neutral question was a statistical one. Moreover, the "neutral" and "slightly deleterious" camps could predict overall frequency distributions and patterns; thus the views of these schools were reflected in bold, testable conjectures that started to capture the imagination of empirical workers.

Times have continued to change and statistical standards have risen higher; we now expect to see electrophoretic variation treated as a neutral descriptor of population structure. More sophisticated studies and analyses than appear here are required in papers investigating adaptive or behavioral links. Consider, for example, the recent paper by Gaines and Whittam (Genetics 96:767–778) re-examining some of the same data discussed in this volume. There, in relation to genetic changes in vole cycles, the Lewontin-Krakauer test is employed and careful attention is given to the robustness of the results to any possible deviations of the data from the assumptions of the test. Many workers now remain skeptical about the adaptive significance of genic variation and links to behavior in the absence of such strong tests and consistent replicates. This volume reflects the literature during the period when that skepticism was starting to spread.

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HOW BIOLOGY WAS UNIFIED[1]

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There are biologists who believe that the evolutionary synthesis is crumbling, but none can doubt that for essentially a scientific generation a remarkable consensus prevailed concerning the nature of evolutionary processes. We live and work in the framework of the synthesis established between 1925 and 1950. Ernst Mayr was a participant in the synthesis, and in 1974 he chaired two conferences "to elicit as much information as possible about any factor, scientific or otherwise, that had had a positive or negative influence on the occurrence of the synthesis." Participants included important contributors to the synthesis, such as Th. Dobzhansky, E. B. Ford, G. L. Stebbins, and C. D. Darlington. Others, such as G. G. Simpson, B. Rensch, and J. Huxley, were important correspondents of the editors. Other participants ranged from modern evolutionary biologists (including R. Lewontin, S. J. Gould, H. L. Carlson, R. L. Trivers, M. Ghiselin) and scientists who were observers of the synthesis (V. Hamburger, A. Weinstein) to philosophers and historians (D. H. Hull, D. Shapere, F. B. Churchill, R. W. Burghardt, Jr.).

The editors have attempted to produce a coherent package from a great diversity of materials including papers written for the conferences, tapes of the conferences, comments elicited by the conferences, and questionnaires submitted by individuals unable to participate. The book which results is a somewhat chaotic assemblage of fascinating material, written by the editors and a number of the participants. Editorial comments help bridge some of the gaps, but others remain. Yet, this is an enormously entertaining, stimulating, and exciting book. It doubtless will serve as a point of departure for future historians of science, who will seek and probably find evidence of revisionism.

A masterful prologue by Ernst Mayr (48 p.) served as a point of departure for the conferences and it opens the book. Mayr dated the synthesis as occurring between 1936 and 1947, and focused on ten or 12 people, each occupying a special niche. He posed questions for the conferences to discuss, and laid out a program for analysis.

Part one of the book focuses on the contributions of different biological disciplines (Genetics, Cytology, Embryology, Systematics, Botany, Paleontology, Morphology). Several of these disciplines are introduced in a useful way by Provine, and then followed by one to several papers dealing with the topic area. Many of the individual contributions are unusually thoughtful and analytical, including those by Le-

[1] *The Evolutionary Synthesis; Perspectives on the Unification of Biology*. Ernst Mayr and William B. Provine (Editors). Harvard University Press, Cambridge, Massachusetts, 1980. xi + 487 pp. $25.00.

wontin (theoretical population genetics), Hamburger (embryology), Churchill (biogenetic law), Gould (paleontology) and, especially, Adams (on Severtsov, Schmalhausen and Russian morphology).

Part two deals with the synthesis in different countries. This section is less successful than Part one because it is essentially a disconnected series of vignettes. However, the two chapters on the Soviet Union by Dobzhansky and Adams are particularly well done and illuminating. I learned most from the contributions by Adams in Parts one and two concerning Russian scientists. The synthesis was about to be made in the Soviet Union when politics intervened in the Lysenko era. Even now the contributions of Chetverikov, Schmalhausen, and others are generally overlooked by western biologists. Those who feel that morphology and embryology stood outside the synthesis should read Schmalhausen.

Part three is labeled "Final Considerations." Shapere opens this part with a philosophical overview. Provine contributes a very interesting Epilogue that attempts a summary of the conferences. Provine asserts that the evolutionary synthesis will be seen as the "primary development in twentieth century biology." With less than two decades yet to go in this DNA century, I would guess that most biologists would dispute this view. Provine also comments on some differences in viewpoint that arose during the conference, and mentions areas not settled by the conference (for example, what were the relative roles played by genetics and systematics?). Part three closes with a series of biographical essays of uneven coverage and quality.

It is unforgiveable that Sewall Wright, an active scientist half a century after his truly synthetic contributions, was not invited to participate. Mayr too quickly (as usual) dismisses Wright's contributions, or gives them faint praise. However, to Lewontin, Hamburger, Gould, and others, Wright is a giant. We live in an era in which the influence of Wright is seen to grow daily, and his absence is a gigantic hole in the fabric of this book.

If there is a consensus it is that Dobzhansky's *Genetics and the Origin of the Species* (1937) characterized the evolutionary synthesis. Some would see it as the tying together of threads from Chetverikov, Fisher, Haldane and Wright at the level of whole organisms in populations, thus culminating a twenty-year trend. In contrast, others (notably Mayr) apparently consider Dobzhansky's book to be the major element in the initiation of the synthesis and the stimulus for the many books which followed during the next ten years.

I recommend this book to all evolutionary biologists. Everyone will learn something, and everyone will be dissatisfied with parts (for me there is too much on Morgan and far too little on Wright). What we do today is still, to a surprisingly great extent, the result of questions answered and asked during the period of the evolutionary synthesis. This book is an important contribution to an understanding of motivation and orientation in evolutionary biology.