# The Myth of Darwinism

**The Non-Darwinian Revolution.** Reinterpreting a Historical Myth. PETER J. BOWLER. Johns Hopkins University Press, Baltimore, 1988. xii, 238 pp., illus. $27.50.

Cherished myths die slowly if at all. The history of Darwinism has more than its share of myths—that the Galápagos finches played a key role in the development of Darwin's theory, that in developing his theory of natural selection all Darwin did was read Victorian mores into nature, that Social Darwinists in turn read Darwin's theory back into society, and that Marx offered to dedicate a volume of *Das Kapital* to Darwin. But the biggest myth of all, according to Peter Bowler, is that something properly termed "the Darwinian Revolution" occurred soon after the publication of the *Origin of Species*. The main theme of Bowler's book is that, if Darwin inaugurated anything, it was a non-Darwinian revolution.

Bowler is critical of Whig histories of science in which the past is viewed totally from the perspective of the present. Scientists who turned out to be right are "keen observers," while those whose views did not lead directly to our current understanding are dismissed as "idle speculators." A major goal of the Darwin industry has been to neutralize such tendencies by evaluating the contributions of Darwin and his contemporaries in their own right. Richard Owen, for instance, was much more than an opponent of Darwinism. But Bowler contends that even the best of Darwin scholars continue to distort the history of evolutionary biology by concentrating so single-mindedly on Darwin. Just as 19th-century evolutionists pictured biological evolution as a tree with the main trunk culminating in the human species, Darwin scholars have tended to view the history of evolutionary biology as if the main trunk led from Darwin to the present. But both conceptions are strongly non-Darwinian. Because a truly Darwinian evolutionary process is so haphazard, biological evolution forms a bush or a coral, not a tree. There is no trunk, let alone a trunk leading to *Homo sapiens*. How haphazard *science* as a process happens to be is an open question. Scientific development may even have "trunks," but if so, Darwinism does not form one of them. At most, according to Bowler, it was a minor branch until this century. If there was a genuinely Darwinian Revolution, it occurred in the 1920s and '30s with the advent of the modern synthesis. To mix metaphors hopelessly, Darwinism was neither a blind alley nor the main stream. It was at most a rivulet in the non-Darwinian river.

Bowler portrays Darwin's theory as a "catalyst that helped to bring about the transition to an evolutionary viewpoint within an essentially non-Darwinian conceptual framework" (p. 5). If Darwinism was so much "in the air," why did no one else come up with a Darwinian view of evolution and why did only a very few accept Darwin's theory once he made it public? But how about all of Darwin's precursors, not to mention Wallace and his fellow Darwinians? Bowler argues quite persuasively that Darwin's putative precursors either did not advocate evolution at all or else were precursors to the *Non*-Darwinian Revolution. According to Darwin, biological evolution is gradual and has no clear direction, and natural selection is the primary directive force. The view of evolution that became popular after the *Origin* was saltative, progressive, and Lamarckian. Instead of adopting Darwin's theory, Darwin's contemporaries transformed it to fit their own pre-evolutionary preferences. The individualistic, competitive character of Victorian society may have led Darwin to forge the theory that he did, but it cannot explain the enthusiasm of his contemporaries for his theory because there was no such enthusiasm.

Bowler has no trouble in showing that such putative Darwinians as T. H. Huxley and Ernst Haeckel held pseudo-Darwinian views of evolution. They disagreed with Darwin on too many issues central to the evolutionary process to count as Darwinians. In order to show that Wallace was no Darwinian, Bowler is forced to narrow the gauge of his analysis. According to Bowler, Darwin himself helped establish the myth of Wallace as a codiscoverer of natural selection because his fear of being forestalled led him to read too much into Wallace's manuscript. On calmer reflection, two fundamental differences between Darwin and Wallace emerge—one concerning their understanding of selection, the other their conception of varieties. Darwin conceived of selection as consisting primarily of competition among individual organisms such that evolution continued even during periods of environmental stability. His theory of sexual selection was one result of his competitive outlook. Wallace, to the contrary, viewed selection in terms of individual organisms coping or failing to cope with their environments. For Wallace, extinction occurred only at times of particular environmental stress. As a result, Wallace was not all that enthusiastic about Darwin's theory of sexual selection. Although neither man was consistent in his use of the term "variety," Darwin tended to mean intrapopulational variants, whereas Wallace used it to refer to peculiar groups belonging to the same species, something like subspecies. In times of environmental stress, entire varieties are extinguished, their places taken by better-adapted groups of organisms. Hence, the practice of plant and animal breeders of selecting single individuals to breed did not strike Wallace as being all that analogous to selection in the wild.

I find myself in total agreement with Bowler's evaluations of various Darwinians, pseudo-Darwinians, non-Darwinians, and anti-Darwinians. I also agree that Darwin and Wallace differed with each other in several important respects, but I do not see why Bowler has to make Darwin conceptually unique in order to substantiate his main thesis that non-Darwinian versions of biological evolution became much more popular than Darwin's version. Regardless of how one classifies Wallace, his version of evolutionary theory did not become popular either. According to Bowler, the developmental view of biological evolution came to prevail after Darwin, and Wallace was no more a developmentalist than was Darwin.

The history of evolutionary theory as such was repeated in anthropology. Darwin had even less effect on the study of human evolution than he had on our understanding of biological evolution at large. Theories of human evolution were strongly developmental and progressive, converging on Europeans as the pinnacle of the evolutionary process. Inevitably anthropologists viewed human evolution as occurring in stages. Social Darwinism was many things. One thing that it was not is Darwinian. Herbert Spencer after all was a Lamarckian. In fact, present-day sociobiology is the first attempt to apply a genuinely Darwinian form of evolutionary theory to the human species. As in the case of Darwinism, social Darwinism was not tried and found wanting, it was not tried at all. Once again, cultural determinists find themselves in a corner. The amount of tinkering necessary to draw the needed causal connections between ideology and the content of particular scientific theories is so extensive that their position "degenerates into absurdity." With enough tinkering "virtually *any* theory can be used to justify *any* social policy" (p. 155). But Bowler also finds himself in a corner because Darwin was not always able to resist the attractions of the progressionism so popular in his day. Although Bowler admits that it may seem "rather silly" to think of Darwin as betraying one of his most important insights, that is precisely what he did on occasion with respect to progressionism.

Although Bowler directs his book at Darwin scholars, it can be read with profit by anyone interested in Darwinism. Just as most of us can recall the secret enjoyment we felt in school when one of our classmates was being punished and not us, we are liable to get vicarious pleasure out of Bowler's chastising the Darwin industry. I have complaints on only two matters, one that could be remedied, the other not. Although the notions of Darwinian, pseudo-Darwinian, non-Darwinian, and anti-Darwinian are central to Bowler's analysis, I could not always follow his usage. He defines the terms but does not manage to stick to his definitions, in part because such categories have two dimensions—conceptual and social—and the two do not always go together. Scientists who disagree with each other over fundamentals can nevertheless cooperate. Although Huxley supported Darwin in his attempt to reorient biology, he disagreed profoundly with Darwin on evolution and contributed little conceptually to the development of Darwin's research program. Huxley was not socially anti-Darwinian, but he differed conceptually hardly at all from several of Darwin's opponents who were.

More important, Bowler is frustrated by the continuing emphasis on Darwin in histories of evolutionary biology, but in his own attempt to counter this bias he himself is forced to pay too much attention to Darwin. I see no way out for Bowler in this book. In order to show Darwin's actual role in evolutionary biology, he is forced to talk a lot about Darwin. If other Darwin scholars are convinced by his radical conclusions, then future historical works may rectify this pervasive imbalance, but I doubt this will happen. The myth of Darwinism has become too much a part of our worldview. Bowler may convince Darwin scholars that Darwin was really a minor figure in the history of evolutionary biology from the middle of the 19th century until the modern synthesis, but future works will still revolve around Darwin. Non-Darwinian theories will be classified first and foremost in relation to Darwinism and only then evaluated in their own right. After all, Bowler did not title his book "The Developmental Revolution." I doubt that very many authors in the future will be able to resist including the name "Darwin" in their titles any more than Bowler was.

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## Max Delbrück

**Thinking About Science**. Max Delbrück and the Origins of Molecular Biology. ERNST PETER FISCHER and CAROL LIPSON. Norton, New York, 1988. 334 pp., illus. $19.95.

Max Delbrück was one of the most influential biologists of our era, a leader in the conjunction of microbial genetics and macromolecular chemistry that led to the field we now call molecular biology.

Delbrück was originally trained as a theoretical physicist by and among those scientists who were at the center of the European physics community just as the first wave of excitement from the development of quantum theory was subsiding. The authors of this new biography document, from his own publications and letters, that Delbrück felt frustrated in physics since the great paradoxes had already been resolved. He wanted desperately to make an important discovery and expected that biology might be fertile ground. More specifically, he felt that by finding the ideal simple system for a particular problem and by mounting an all-out assault, a situation might be found in which the known understanding of the natural world would be insufficient to explain the results; new laws of physics would be necessary.

In 1937 Delbrück arrived in the United States from Germany, looking for just such a system. He held a Rockefeller Foundation fellowship to visit several of the most important centers of genetics research. At the Biology Division of the California Institute of Technology he found out about bacterial viruses, also called phage (short for bacteriophage), which seemed perfect for the study of replication. Because of the small size and rapid replication of both the virus and the host this was an ideal system for the use of quantitative methods that came naturally to a physicist.

Delbrück remained in the United States during the war and continued his work with phage, moving to Vanderbilt University as a physics instructor at the end of two years of support from the Rockefeller Foundation. In addition to his research he used all the opportunities available to spread his enthusiasm for phage work and to invite others, particularly physicists, to join him. Perhaps the most important event in this "advertising" campaign was the establishment in 1945 of the phage course at Cold Spring Harbor Laboratory, a small private institution on the north shore of Long Island that had long specialized in genetics. This intense three-week course was designed to give its students, who were highly trained in some other field, enough knowledge, both theoretical and hands-on experimental, that they could begin doing phage research in their own laboratories.

In 1969 Delbrück, who had been invited back to be a professor at Caltech in 1946, was awarded the Nobel Prize together with Salvador Luria and Alfred Hershey. Delbrück was honored for his career as a leader in the development of this experimental system that was crucial to our understanding of gene action, rather than for any particular experiment.

In 1966 the Cold Spring Harbor Laboratory published a book of great importance in recording the history of the young field of molecular biology. *Phage and the Origins of Molecular Biology* (traditionally abbreviated *PATOOMB*) was published as a festschrift for the occasion of Delbrück's 60th birthday. *PATOOMB* was different from most such volumes. Most of the contributions were reviews of the influences, personal and intellectual, that enabled the authors to make the important discoveries that constituted the new field, and the book became a classic because of the depth of feeling shown by the authors for Delbrück and the crucial influence he had on their work.

Clearly *PATOOMB* was a marvelous resource for the authors of this biography; it also presented a problem. Although many of the best stories of Delbrück's legendary intellectual dominance, love of practical jokes, and unique methods of motivating his coworkers are told again and may influence today's students, they do not have the same impact as when told by those who were actually there.

In contrast, a strong point of this new biography is that it takes us beyond a description of the famous phage years and presents the wide range of Delbrück's experimental and theoretical contributions. As early as 1950, he had begun to search out new problems that could be approached by the kind of concerted effort on a single simple experimental system that had worked so well with phage. Although the other experimental systems he chose never approached the popularity of the phage system, he continued to have a strong influence