

BOOK REVIEWS

The ontogeny of phylogeny

David L. Hull

PUBLICATION is an unnatural event in the life of a scientific theory. Prior to publishing, a scientist can work in relative privacy, changing his views as he sees fit. After publication, however, the process is made more difficult by the critical stare of the scientific community. Darwin worked on his theory of biological evolution from 1837 until his death 45 years later. Historians have charted his intellectual voyage month by month, sometimes day by day — the *Beagle*, his decision to develop a theory of transmutation, his reading Malthus, the Sketch of 1842, the Essay of 1844, the receipt of Wallace's paper, the hurried publication of the *Origin*, and the trials and tribulations that followed. Ospovat retraces familiar ground in Darwin's story from 1838 to 1844 and then fills in the last gap — the period between 1844 and 1859.

Historians have ignored this period primarily because nothing much of any interest seems to have happened during these years. By 1844 Darwin had already decided that species evolve by chance variation and natural selection. The essentials of his theory were complete. Ospovat argues to the contrary that Darwin made several fundamental changes in his theory some time in the second half of 1856. Prior to that date, Darwin thought that every species is perfectly adapted to its environment, a belief that he carried over from natural theology. He considered that significant amounts of variation occur only in response to intermittent changes in the environment. As a result, evolution for Darwin at this stage in his development was "punctuational". However, in 1856 Darwin formulated his principle of divergence, a principle that he thought was as important as that of natural selection. According to this principle, variation exists within species at all times, allowing them to adapt to variations within their range, including the presence of other organisms. By a sort of "division of labour", species become subdivided into varieties, and these varieties are incipient species. From the beginning Darwin believed that evolution is in some sense progressive, but only after 1856 could he explain why the biosphere tended to become increasingly complex.

In 1855 A.R. Wallace published a paper on the law which regulates the introduction of new species, a paper that both Charles Lyell and Edward Blyth brought to Darwin's attention. Lyell urged Darwin

The Development of Darwin's Theory: Natural History, Natural Theology, and Natural Selection, 1838-1859. By Dov Ospovat. Pp.301. ISBN 0-521-23818-8. (Cambridge University Press: 1981.) £20, \$39.50.

once again to publish lest he be forestalled, and Darwin began his projected multi-volume *Natural Selection*. In 1857 Darwin and Wallace exchanged letters on Wallace's 1855 paper. Then Wallace's paper "On the Tendency of Varieties to Depart Indefinitely from the Original Type" (1858) arrived, not exactly like a "bolt out of the blue". What if Wallace's first paper had prompted Darwin to publish an "abstract" of his views in 1855? What if Wallace's 1858 paper had not caused Darwin to publish the *Origin*? What if Darwin had instead doggedly persisted in his original plan for his Big Book? Answers to such contrary-to-fact questions in history are extremely risky. Even so, Ospovat suggests that the *Origin* would have been a very different book had Darwin published it before formulating his principle of divergence. Instead of making the principle an integral part of his theory as first presented, Darwin would have been forced to introduce it later as an emendation. Similarly, he might have quietly passed on Wallace's paper for publication instead of agreeing to publish a short piece of his own along with it. If he had, I think that very little would have changed except later commentators would have been precluded from going on at such interminable length about Darwin's churlish behaviour. Darwin would still be viewed as the founder of the modern theory of evolution. The Darwin-Wallace papers of 1858 caused hardly a stir; Wallace's paper published alone is unlikely to have engendered more interest. But what of intellectual justice? From this perspective, Wallace still loses out, but this time to Patrick Matthew who had anticipated both Darwin and Wallace as early as 1831.

Although Ospovat makes the requisite passing remarks about the social, political and religious sources of Darwin's scientific views and concludes with a chapter on the development of Darwin's theory as a social process, his book is almost entirely an internalist history of the very best sort. Ospovat not only chronicles Darwin's conceptual development but also presents plausible reconstructions of the factors that led him to adopt the views he did. Not

all of these factors are narrowly "scientific". For example, the tenacity with which Darwin held on to his belief in perfect adaptation stems at least in part from considerations that anyone, both then and now, would count as theological. Darwin was also influenced by prudential considerations of the sort that play a central role in the politics of science. For example, Ospovat argues that Darwin understates the progressive nature of evolution in the *Origin* for fear of offending Lyell and Huxley. But he does not repeat the usual facile claims about the direct translation of sociopolitical interests into the content of science.

Under normal circumstances, I would conclude this review by remarking that the appearance of *The Development of Darwin's Theory* portends a brilliant career for its author. Sadly, however, shortly after the completion of this manuscript, Dov Ospovat died at the age of thirty three. □

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Red for green

H. McAllister

The Biological Aspects of Rare Plant Conservation. Edited by Hugh Synge. Pp.558. ISBN 0-471-28004-6. (Wiley: 1981.) £30, \$84.

DESPITE its title, most of this multi-authored book is very usefully devoted to the ways in which botanists in different nations work out which species and communities are in need of conservation in their own countries. As a result, the differences in the scale of the problem between the impoverished but well-known floras of north-western Europe, the rich, less well-known floras of such countries as South Africa, Australia and India, and the very rich, little-known tropical rain forest floras, are clearly brought out.

In north-west Europe the distribution and frequency of occurrence of most species is fairly well documented, so it is