# Book Reviews

## Darwin's Crucial Years

**Darwin on Man. A Psychological Study of Scientific Creativity. HOWARD E. GRUBER. Together with Darwin's Early and Unpublished Notebooks, transcribed and annotated by Paul H. Barrett. Dutton, New York, 1974. xxviii, 496 pp., illus. $20.**

This work is two books within one cover, as its double subtitle indicates. The title and subtitle seem to promise in some respects less and in others more than the two books actually contain. In the first book no chapter is exclusively or even precisely devoted to the subject of Darwin on man and only one is principally about scientific creativity, but in all 12 chapters both those interesting topics are extensively interwoven with related aspects of Darwin's life, especially in his most crucial formative years.

Darwin's life and his work on evolution (he did even more on other subjects) involve several special problems for the historian and the psychologist. Gruber, who is both, emphasizes four of these problems and treats them all in some depth although at unequal length. When and how did this creationist theology student become an evolutionary naturalist? When and how did he envision and formulate the theory of natural selection? Why did some 20 years elapse after that event before he published his views on evolution in general? Why then did more than 12 years elapse before he published on the evolution of man in particular?

Gruber holds the view, disputed as all these points are and yet supported by strong evidence, that Darwin remained a biblical fundamentalist throughout the voyage of the *Beagle* (1831–36) but that within approximately six months after his return he became an evolutionist. The possibility of such a change seems to require something even more basic than the voyage itself. Gruber sees part of this background in Darwin's teachers, who included only one evolutionist, the Lamarckian Grant, but on an informal basis also naturalists who were creationists but otherwise excellent. Gruber emphasizes, however, what he calls a family Weltanschauung indicated by parallels between the attitudes of Charles and those of his grandfather Erasmus Darwin. The supposed parallels are rather farfetched. Erasmus died seven years before Charles was born. In his youth Charles read some of his grandfather's didactic poetry with admiration but, as he later made clear, without conviction or instruction. There is little evidence of the supposed Anschauung in the members of the family with whom Charles was in fact associated.

It is well known that Charles himself said that his conviction about evolution —one could call it a discovery or a conversion—arose from his observations on the *Beagle* and especially from the fossil mammals of Argentina and the living birds and tortoises of the Galápagos Islands. That has generally been accepted, as it is by Gruber, and there is no serious reason to doubt it if one takes it that the change occurred in retrospect after the voyage and that the fossils, birds, and tortoises were merely outstanding examples of a vastly greater body of evidence that began to accumulate on Darwin's excursions as a budding naturalist in years before the voyage.

Gruber's account, although also ranging much further afield, concentrates especially on the short span 1837–39, from Darwin's becoming an evolutionist to his first formulation of natural selection. Gruber emphasizes that Darwin's first theory of evolution, before he thought of selection, differed radically from his later views and was dropped in three months or less. He scolds other Darwinists for evading discussion of this misstep, although two versions of the manuscript record for it were in fact published in 1960. Gruber then goes on to indicate that after discarding that theory (1837) Darwin continued to grope rather futilely until September 1838, when he read Malthus and arrived at the theory of natural selection. Gruber subscribes to the view that natural selection was already a widely current concept and that Darwin's rather slow creative act was to see that this could explain progressive change and need not be only what we now call stabilizing selection.

I believe that this view understates or indeed misstates the case and the degree and kind of creativity involved. Before Darwin the concept of selection (rarely called that) was supposed to explain why evolution does *not* occur. Darwin's explanation of why evolution *does* occur under the direction of natural selection was a great deal more than and different from mere application of an identical concept in a different context. I further believe that the whole sequence of events up to this point can be clarified if put in a different way. This is not entirely a criticism of Gruber, because much of this different interpretation is implicit and some is explicit in his detailed, enlightening text.

Darwin became an evolutionist in 1837 as he contemplated the many facts that he then knew about species in nature. He did this, I believe, by taking evolution as a testable premise, hence as a hypothesis, making deductions from that premise, and comparing them with deductions from the alternative hypothesis of special creation. The alternative deductions were tested by the facts of nature, especially as regards the distribution and the variation of species. It then became obvious to Darwin, as it still is to any informed, open-minded, and rational person, that the facts agree only with the hypothesis of evolution. The next step was to consider possible causal, natural explanations of evolution. The first one that occurred to Darwin (it was again a hypothesis) met few deductive tests and was almost immediately discarded. It had no further influence on Darwin's thought and never had historical importance because nothing about it was published until long after later knowledge had made it merely absurd. Darwin then proceeded further, continuing the consideration of multiple hypotheses until finally he found one that fitted all the data relative to an important aspect of evolution: natural selection as an explanatory factor in progressional (not necessarily progressive) and usually adaptive evolution. Gruber opposes the idea that multiple hypotheses lead to scientific innovation, but

his discussion of Darwin shows the method at work. Such hypotheses can be successive as well as simultaneous.

Although this, too, has been questioned, Gruber reasonably concludes, with Darwin himself and with other evidence, that Darwin's new insight and the beginnings of his theory of natural selection dated from his reading Malthus in September 1838. Yet the Darwinian theory was not publicly announced in abstract until 1 July 1858 and in reasonably full form, by publication of *The Origin of Species*, until 24 November 1859. Gruber gives as a main reason for this long delay fear of unorthodoxy and of consequent persecution, perhaps not in as severe a form as that suffered by Bruno or even by Galileo (both are mentioned in this connection), but still in a form genuinely dreaded by Darwin as evidenced by a dream, to which Gruber repeatedly refers, which barely permits but hardly seems to require so extreme an interpretation. As another cause of delay Gruber gives Darwin's feeling that his theory was incomplete unless it could explain the origin and transmission of adaptive variations. In Gruber's view this matter was dealt with by changing it from a problem requiring solution for completion of the theory to a premise for that theory. But to the extent that it was ever adopted, that attitude was reached by 1839 at the latest and can hardly have been involved directly in some 20 further years of delay.

There is no doubt that Darwin was concerned about opposition, but I believe that this concern is overstated as fear of persecution. What he was most intent upon was to convince a majority of his scientific peers. He did convince them with his publication in 1859, and it is almost certain that he would have failed, as he realized, if he had published in 1839. His theory in 1838–39, then summarized in terms of heredity, variation, and superfecundity and left at that level in Gruber's discussion, was indeed simplistic and naive in comparison with the elaboration and sophistication of *The Origin*. The progress was due mainly to the intense and productive labor of the intervening years, even though these had no fully overt relationship to evolutionary theory. Furthermore, in 1838–39 Darwin had not really evaded the problem of origin and inheritance of adaptive variation. He still relied heavily on the inheritance of acquired characters, a false hypothesis which he never wholly rejected but on which he relied less in later years.

Darwin was an indefatigable writer, producing not only a large number of published papers, monographs, and books but also thousands of notes and thousands of letters unpublished in his lifetime. Many have since been published, scattered widely in different journals and books, but many are still unpublished. The second book of the present work contains less than is suggested by "Darwin's early and unpublished notebooks" but does include two previously unpublished notebooks from the years 1838–39. They are here called "The notebooks on man, mind and materialism," but by Darwin the first was titled "Metaphysics on morals and speculations on expression" and the second "Metaphysics and expression." Although they are brief, in Darwin's usual way they supplied some materials for later books, scattered through especially *The Origin of Species* (1859), *The Descent of Man* (1871), and *The Expression of the Emotions in Man and Animals* (1872). They are here well annotated by Barrett, with commentaries by Gruber, and they are a useful and scholarly addition to materials for the study of Darwin's early thought. There is also a collection of loose notes, mostly isolated comments on articles and books read in or about 1837–39, also well annotated by Barrett. Other contents of Book Two of the present work are miscellaneous and relatively unimportant, and do not need listing or special comment here. Some involve questionable editorial judgment.

It has not been possible to agree entirely with Gruber's interpretations. It should be added that the application of Piaget's studies of "the formation of intellectual structures in children" to Darwin's mature intellectual creativity, anticipated by Piaget in a foreword to this work, has not been carried far in the text, wisely, as it does not seem really relevant. There are innumerable other points that could be profitably discussed, but it is hoped that this review has now sufficiently indicated the contents of the work and also its stimulating effect for anyone interested in its subject, which must include almost everyone.

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## Biological Continuities

**On Development.** The Biology of Form. JOHN TYLER BONNER. Harvard University Press, Cambridge, Mass., 1974. xii, 282 pp., illus. $10.

This book is essentially a review of reviews. In 259 pages of text, with a minimum of illustrations, Bonner covers the whole vast literature of organismal development, from viruses and microorganisms to mammals and higher plants. He does this "so that the nature of development becomes illuminated by its relation to other themes of biology." Of necessity, therefore, the book is both a synthesis and an analysis. Bonner relates development to biochemistry through comparisons between cycles of synthesis and degradation of macromolecules, similar cycles undergone by certain organelles, and cycles of cell multiplication, including the replication of DNA. He establishes its relationship to reproduction by emphasizing the point that the essence of reproduction is the reproduction of entire life cycles, not just adult phenotypes, and that the molecular information that initiates a new cycle during cleavage and early embryonic development is provided in part by the action of genes at the end of the previous, parental cycle. In drawing connections with inheritance he emphasizes the fact that nuclear DNA provides the pattern of order only with respect to the synthesis of informational macromolecules, particularly the various kinds of proteins. Order at the supramolecular level, although chiefly a consequence of macromolecular order, nevertheless requires the action of processes which operate in the cytoplasm, far from the nucleus, and which are as yet poorly understood.

One of the most illuminating of Bonner's analyses is his tracing of different levels of complexity, from bacteria through unicellular and colonial eukaryotes, and from simple multicellular forms to societies, particularly those of insects. To many biologists, the analogies Bonner draws between simple multicellular organisms and the beginnings of animal societies will be novel and will provide food for thought and further research.

The second half of the book, entitled The Molecular View of Development, emphasizes control mechanisms, rates and timing of metabolic processes, and the localization of substances in different parts of a structurally complex organism. Discussion of the last