

## Darwin's "Big Book"

**Charles Darwin's *Natural Selection*.** Being the Second Part of His Big Species Book Written from 1856 to 1858. R. C. STAUFFER, Ed. Cambridge University Press, New York, 1975. xii, 694 pp. \$49.50.

*I have long discovered that geologists never read each other's works, and that the only object in writing a book is a proof of earnestness, and that you do not form your opinions without undergoing labor of some kind.*—CHARLES DARWIN (1)

Many of our greatest ideas are known best in abridgement. In an honest mood, almost any philosopher will tell you that he learned Kant's metaphysics from the *Prolegomena*, not from the *Critique of Pure Reason*. Steno decided to serve God rather than write his magnum opus; yet his *Prodromus* revolutionized geology nonetheless. And Charles Darwin wanted to call his book "An Abstract of an Essay on the Origin of Species"—but his publisher wisely demurred.

After an arduous eight years devoted to barnacles, Darwin decided that the time had come to display a heresy he had nurtured for 16 years. In September 1854, he recorded in his diary: "began sorting notes for species theory." By June of 1858, when Wallace's letter arrived with its sketch of an identical theory, he had completed more than half of his "big book." Lyell and Hooker persuaded Darwin to present an earlier unpublished account along with Wallace's sketch in a joint paper before the Linnean Society. Nothing prompts a scientist more than the urgings of priority. Darwin abandoned the painstaking documentation of his big book and, nine months later, completed the "abstract" heard round the world. The *Origin of Species* contains some 155,000 words and nary a footnote. *Natural Selection* would have been almost three times as long and copiously documented; the extant portions cite almost 750 books and articles. Since Darwin's success lay in documenting the *fact* of evolution (he had, in his time, very few takers for his theory of natural selection), the publication of his longer version is no mere antiquarian indulgence: it is probably the publishing event of the decade in history of science.

Stauffer's version contains nine of the 11 chapters that Darwin had completed (the

first two were expanded and published in 1868 as the two-volume *Variation of Animals and Plants under Domestication*). I cannot praise highly enough the meticulous work of Stauffer and a staff of assistants in rendering the text right down to the details of Darwin's misspellings. And it was no easy task. Darwin's handwriting is abominable in itself, and he also quarried the manuscript for later works by cutting out sections of text (these have been restored, when possible, from published versions). Darwinian scholarship has assumed the aura of Biblical exegesis. A student must know his inks and watermarks, his excisions and interpolations. Stauffer has done all this and has even attained the ideal of scholarly selflessness by adding not a word of commentary on ideas and concepts. Whether this is a good thing or not I do not know; I certainly could not have done it.

What can I say except that *Natural Selection* is a joy to read? It is full of insights and subtle observations that never found their way into the *Origin* (p. 197 on ecological succession, p. 206 on the self-regulation of parasites lest they destroy their hosts and themselves as well, p. 207 on the operation of density-dependent mortality at different stages of the life cycle, p. 247 on niche theory, p. 271 on coadaptation, p. 354 on preadaptation, and so on). *Natural Selection* is rich where the *Origin* is often condensed beyond recognition. The metaphor of the wedge is famous enough as an expression of equilibrium theory and the control of diversity and adaptation by competition, but the *Origin's* few lines are confusing and telegraphic compared with the original:

Nature may be compared to a surface covered with ten-thousand sharp wedges, many of the same shape and many of different shapes representing different species, all packed closely together and all driven in by incessant blows: the blows being far severer at one time than at another; sometimes a wedge of one form and sometimes another being struck; the one driven deeply in forcing out others; with the jar and shock often transmitted very far to other wedges in many lines of direction [p. 208].

We also resolve many minor puzzles of the *Origin*. My colleague R. D. K. Thomas pointed out to me that Darwin's account of the natural increase of elephants is miscalculated in the *Origin*. We had formulated a

hypothesis about Darwin's mathematical skills, but *Natural Selection* makes it clear that, in rapidly condensing, Darwin simply miscopied. (*Natural Selection* cites rates for three and four pairs of offspring in a mother's life; the *Origin* settles on three pairs, but gives the figures only for four pairs.) Finally, *Natural Selection* is graced with philosophical comments rigidly excluded from the *Origin*. Consider, for example, this on historical contingency and the nature of determination in natural history:

The chemist may throw a dozen salts into solution and may hope to predict the result; the naturalist cannot do this with the living beings dispersed by ten thousand ingenious contrivances all round him; but when we see the virgin forest reassuming its beautiful variety apparently in the same exact proportions, over the ancient Indian ruins, we must see how little of what we call chance has to do with the final result [p. 198].

It has become customary, in this age of inflation, to lament that so few readers will be able to own a fine book. But I think we should reverse our perspective. Monarchies should be eliminated because they are unfair to monarchs. I feel most sorry for Stauffer; he has labored for years to communicate with a few libraries and even fewer lucky reviewers.

Despite the dislike of professionals for "iffy" history, no one will be able to avoid the fascination of an obvious question: What if Wallace had died of his fever at Ternate and Darwin had retained the leisure to complete and publish *Natural Selection*? I think the answer is clear. It would not have made a particle of difference (except that more people would have been converted to evolution without reading Darwin).

Darwin rests in Westminster Abbey, near (if at the feet of) the immortal Newton. But he lies there because he convinced the world of the *fact* of evolution, not because his theory of natural selection triumphed in his day. I cannot think of a single unambiguous supporter of natural selection among Darwin's contemporaries. Wallace, his compatriot, was a rigid selectionist, but he drew the line at man's brain (for how could selection have established an organ with so much unused potential if the brain of "savages" is equal in innate endowment to the brain of civilized Europeans?). Lyell, his convert, was persuaded late in life about evolution, not about selection as its mechanism: "My only objection is . . . to your assigning to [natural selection] more work than it can do and not guarding against confounding it with the Creative power to which . . . the capacity of ascending in the scale of being must belong" (2). And Haeckel and Huxley, his two great bulldogs, convinced millions of

laymen about evolution, but did not accept its control by selection. Haeckel dedicated his greatest work jointly to Darwin, Lamarck, and Goethe—and his theory is a hodgepodge of their influences. A Lamarckian inheritance of acquired characters is, to Haeckel, “an indispensable foundation of the theory of evolution” (3); “the origin of thousands of special arrangements remains perfectly unintelligible without this supposition” (4). Huxley doggedly maintained his belief in the saltational origin of species—anathema to Darwin, who saw gradual transition as the crucial test of natural selection. Late in his life, he wrote to Bateson: “I see you are inclined to advocate the possibility of considerable ‘saltus’ on the part of Dame Nature in her variations. I always took the same view, much to Mr. Darwin’s disgust” (5). And, although the famous letter he wrote to Darwin after reading the *Origin* does offer to “go to the stake” in support of the book, that support is explicitly reserved for the chapters on geology, paleontology, and geographic distribution—and just as explicitly denied to the chapter on natural selection (6). When Huxley spoke of “Darwinism,” he merely referred to the notion that life evolves.

The theory of natural selection did not triumph until the 1920’s and 1930’s when a rising science of population genetics equated small mutations with Darwinian variability and demonstrated that small selection pressures could account for evolutionary change. And the “modern synthesis” between traditional subdisciplines of natural history and genetic theory did not begin much before Dobzhansky’s 1937 work on *Genetics and the Origin of Species*.

Darwin had little luck with his theory in his own day, but he triumphed with his facts. For all the recent talk about Darwin’s creativity as a theorist, the fact remains that he set his task as an essay in documentation. Earlier 19th-century evolutionists had been long on speculation and short on information. Darwin would not repeat a procedure that had given evolution such a bad name. “How awfully flat I shall feel,” he wrote to Hooker in 1854, “if, when I get my notes together on species . . . the whole thing explodes like an empty puff-ball.” He would eschew speculation and provide information: “Lamarck . . . has done the subject harm, as has Mr. Vestiges [Robert Chambers, author of the anonymous *Vestiges of the Natural History of Creation*]” (7). The striking thing about *Natural Selection* and the *Origin* is not so much the new theory of natural selection as the procedure of careful and copious documentation. Chambers had derived a rat from a goose through the

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intermediary of a duckbilled platypus. Lamarck, for all the genius of his thought, presented no documentation for his ideas and confined his examples to speculation about giraffes' necks and drunkards' intestines. Darwin floated seeds, spoke to pigeon fanciers, and watched earthworms.

We arrive then at the key point: Darwin triumphed by his documentation and convinced the world that evolution had occurred. Yet he did it with his abridgement, the *Origin*—without footnotes and without citation of sources. Since he could not have been more successful in the impact of his documentation, the longer version was clearly not necessary to achieve his result.

But could the longer version, with its more copious documentation, have carried the day for his theory of natural selection? The answer again is clearly no; for the difficulties of natural selection in 1859 placed its vindication far beyond the power of any data then available. First of all, the genetic key was missing and not to be supplied for another 40 years. Natural selection requires a particulate theory of inheritance to ensure the preservation of favorable variants in populations. Second, and perhaps more important, natural selection was philosophically far too radical for Victorian minds; for it explodes any concept of inherent progress, denies to life an ontological status separate from inanimate matter, and attributes the properties of mind to the highly complex workings of a material brain. The 19th century was not ready for this brand of materialism. Today, all scientists accept materialism (at least in their workplace), and the philosophically astute realize that it poses no threat to our love for music, subjective insight, and love itself. Yet, when I read the tracts of the Creation Research Society and watch Arthur Koestler groping for inherent meaning, I wonder if we are ready for Darwin yet.

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## Newton at a Major Juncture

**The Mathematical Papers of Isaac Newton.** DEREK T. WHITESIDE, Ed. With the assistance in publication of M. A. Hoskin and A. Prag. Cambridge University Press, New York. Vol. 5, 1683–1684. xxiv, 628 pp., illus. \$65. 1972. Vol. 6, 1684–1691. xxxvi, 614 pp. \$72.50. 1975.

In the spring of 1684, for reasons that are not entirely clear, Isaac Newton, Lucasian Professor of Mathematics at Cambridge University, tardily complied with university statute by depositing in the library fair copies of his lectures on algebra delivered during the previous decade. From all evidence, the text of the 97 lectures that make up the deposited manuscript was composed as a whole within a few months, and the absence of any draft versions makes it difficult to gauge how much of the content had actually passed over the lectern. The manuscript represents to all intents and purposes Newton's final word on matters algebraic. By early 1684 mechanics, especially the problem of planetary orbits determined by central forces, was taking increasing hold of his attention; with the visit of Edmond Halley in late July and early August of that year, Newton's career took its fateful turn leading to the *Principia*.

In the latest two volumes of his already classic edition of Newton's mathematical papers, Derek T. Whiteside provides material enabling us to catch Newton at this major juncture of his career. Volume 5, consisting primarily of the deposited lectures on algebra (pp. 54–517), is in essence a companion piece to volume 4 and completes the record of Newton's activities at Cambridge in the quiet and fruitful decade between the optics controversy of 1672/73 and Halley's visit. Volume 6 makes it possible to follow Newton through several reworkings of the treatise on motion begun in 1684 on the basis of his insight into the generality of Kepler's area law for a body moving under any centrally directed force. That treatise ultimately became the core of books I and II of the *Principia*, and so volume 6 is also a companion piece, not to previous volumes in this edition but rather to John Herivel's *Background to Newton's 'Principia'* (Oxford University Press, 1966) and especially to Alexandre Koyré and I. Bernard Cohen's variorum edition of the *Principia* itself (Harvard University Press, 1971).

A comparison of the material in the two volumes reveals contrast and even irony. Newton seems to have been ambivalent in his attitude toward algebra right from the start. For all its heuristic powers, it seemed to him a dodge that lacked the elegance

and force of geometrical demonstration. Except for his method of approximating roots, he made no essentially original contributions to the subject. Drawing his concepts and methods from others, most notably Gerard Kinckhuysen and René Descartes, he failed to work on them that special transformation he effected on predecessors' results in other fields. The lectures he deposited in 1684 had mathematical and stylistic faults which he left unrevised (though we have from roughly the same period a "First Book of Universal Arithmetic" [volume 5, pp. 538–621] which begins the process of polishing).

Newton might never have turned back to the work had not his successor in the Lucasian chair, William Whiston, come across the manuscript in 1705/6 and decided to publish it. Newton could do little to stop Whiston, managing only to get the title changed from *Arithmetica Universalis sive Algebrae Elementa* to *Arithmetica Universalis sive De Compositione et Resolutione Arithmetica Liber* and to keep his name out of the book. Whiston printed the text (London, 1707) as he had found it, errors and all. Only after a popular English translation by Joseph Raphson appeared in 1720 did Newton undertake minor revisions for a second Latin edition in 1722, again hiding his authorship. Neglected when given originally, ignored when deposited in the library, never publicly acknowledged by their author, the published lectures nonetheless became after Newton's death perhaps his most popular and widely read work.

By contrast, as both volume 6 of the *Papers* and the variorum edition of the *Principia* show, on the subject of mechanics Newton wrote and rewrote, derived and rederived, calculated and recalculated in a never-ending effort to be more precise, more exact, more elegant (for the story of this effort after 1687, see I. B. Cohen's *Introduction to Newton's 'Principia'*, Harvard University Press, 1971). This was the subject he created, where every previous result took on new form and meaning at his hands. He wrote for publication, he meant to be read, and he had the reader in mind. Yet it appears that few people actually read the *Principia* with the care it deserved, and Newton earned the reputation of having written a deliberately obscure treatise. In fact, he did not earn it; as Whiteside remarks (volume 6, p. 25),

Why the *Principia* so quickly gained its ill-deserved popular reputation of being impossibly difficult is not easy to understand: certainly, though his natural terseness of style and crabbed mode of presentation was no help to its comprehension and assimilation, there is no evidence that Newton sought deliberately to be any more esoteric therein than he needed be. While the undiluted richness of their intricate mix no doubt