esting twist to the punctuated-equilibrium-versus-phyletic-gradualism controversy. She contends that the large-scale separation of populations represents cases where allopatric isolation might not lead to "geologically instantaneous" speciation if development of a geographic barrier were prolonged. Of course, this argument requires the assumptions that gene flow between the populations undergoing isolation was sufficiently damped to allow gradual divergence and that this divergence was not augmented dramatically with complete closure of the barrier. Bretsky also asserts that critics of those who investigate ancestor-descendant relationships rarely consider the problem of intraspecific variation. Her opinions on this issue warrant lively discussion, but are largely unanswered in the volume.

The two remaining papers are discussions of "phylogenetic systematics" or "cladism." Gaffney clearly presents his views on phylogenetic reconstruction and the hypothetico-deductive method in science, the recognition of shared advanced characters as evidence for relationships, and the parsimony procedure in testing phylogenetic hypotheses. In the course of his discussion, Gaffney exposes many of the assumptions tacitly accepted in formulations of phylogenetic hypotheses. These themes are extensively treated in other publications, particularly in articles that have appeared during the last five years in *Systematic Zoology*. Gaffney's contribution, however, is a very useful summary of the basic working principles of cladism, complemented by a lengthy bibliography with references by topic. The paper is not meant to represent a consensus view of cladists. For example, Gaffney concludes (p. 101) that "morphoclines" and "character phylogenies" "can only be deduced by tracing a pattern of structures through a preexistent phylogenetic hypothesis, and are a corollary of that hypothesis." He also claims that morphoclines are misused when more than two states are hypothesized because "even before the polarity of the morphoclines is sought, the statement of it precludes large numbers of alternative sequences." Rather than assuming such morphoclines, Gaffney recommends their subdivision and testing with hypothetico-deductive methods. He provides some logical insight into the problem of morphocline analysis, but his opinions are likely to raise eyebrows among systematists who regard the development of character phylogenies as an integral part of cladistic analysis.

The problem of recognizing assumptions in phylogenetic analysis is further considered in a paper by Eldredge aptly titled "Cladism and common sense." Eldredge distinguishes cladograms (branching diagrams based on distributions of shared advanced characters) from more highly inferential phylogenetic trees, which place branching relationships in a framework of geologic time and allow for the possibility of ancestor-descendant relationships. A third level of reconstruction, and one requiring additional assumptions, is the adaptive scenario, which Eldredge defines (p. 192) as "a phylogenetic tree with an overlay of adaptive narrative." Eldredge presents his arguments on these matters in a refreshingly undogmatic and lucid fashion; even readers not particularly sympathetic to the objectives of cladism may find his discourse heuristic. Examples taken from the author's detailed studies of trilobite evolution nicely complement his arguments.

The invited commentaries, by Boucot and Wiley, reveal radically different attitudes and emphases. Boucot, in a very informal and sometimes amusing fashion, assails the cladists for introducing too much jargon for old concepts and burdening systematists and paleontologists with excessive terminology. He also maintains that cladists have misrepresented the consensus view of paleontological theory and practice. This point seems appropriately aimed in some instances, but one wishes Boucot had elaborated on it. His views on the debate at hand seem aligned with those of Gingerich and, especially, Bretsky insofar as he believes that the methods of phylogenetic reconstruction are highly dependent on the quality of sampling and that this matter has not received due emphasis by cladists. One might counter that cladism does not deny the possibility that better samples (which can also be interpreted as more information about taxonomic characters) might yield "more reliable" phylogenies; it is merely offered as a single method for samples of varying quality.

Wiley's contribution is less an explicit commentary on various opinions raised in the book than an exposition of his own views on species concepts and his conclusions that supraspecific taxa cannot be ancestors and supraspecific ancestors should not be invoked in phylogenetic reconstruction. His ideas are of theoretical interest, but the volume as a whole could have benefited from a more detailed critique of essentially "noncladistic" views. The impression of the book as a record of debate is weakened by the lack of such a critique.

Phylogenetic Analysis and Paleontology is a deceptively short book on some very thought-provoking issues. It presents few major concepts for the first time, but it succeeds in bringing together a diversity of viewpoints, sometimes antithetical, on the relationship between fossil evidence and phylogenetic reconstruction. It is a strength of the book that the disagreement that exists on these issues is not camouflaged. Such conflicts in a science can be disturbing to some, but in retrospect they usually mark times of healthy change. Paleontology and systematics are in a phase of exemplary self-examination that mandates the attention of their own practitioners and those from other sciences.

MICHAEL J. NOVACEK
Department of Zoology,
San Diego State University,
San Diego, California 92182

A Figure in Psychology

J. B. Watson. The Founder of Behaviorism. A Biography. DAVID COHEN. Routledge and Kegan Paul, Boston, 1979. vi, 298 pp. $20.

Right from the start, Darwinism tended to push psychology toward behaviorism, because it enlarged the scope of the subject to other creatures besides introspective *Homo sapiens*. Evolutionary doctrine said that monkeys and chickens ought to have some sort of protopsychology in them, but we could know about it only from how they acted. Well before 1913, when John B. Watson first declared himself a "behaviorist" in public, biological, psychological, and philosophical journals and monographs were increasingly full of behavior. Watson, however, made a virtue of necessity by arguing that the behavioral measures of comparative psychology were vastly preferable in a natural science, compared to the introspective methods of 19th-century psychology. His claim simply redefined psychology, which became, for him, the experimental science of behavior, rather than the reflective study of the mind.

Watson in 1913 was 35 years old, a handsome, successful, and influential professor of psychology at Johns Hopkins University. He had launched a career with ceaseless productivity as a comparative psychologist, but now he was confronting his contemporaries with something deeper than data. He had crystallized a school, "behaviorism," which psychologists had to accept or reject, for it became a fork in the road for

psychology. It need not be said that behaviorism polarizes, for the evidence is still abundant in the reactions to Watson's prime modern successor, B. F. Skinner.

This is a personal biography of Watson, not a scholarly analysis of his intellectual origins, his work, or his influence. It tells about the marital scandal and ensuing divorce in 1920 that cost him his chair at Hopkins and that kept him out of academic jobs for the remaining 38 years of his life. It also tells, at some length, about his second career, in advertising, which was no less successful than the first and a good deal more lucrative. Academic psychologists may know that Watson went to work for the J. Walter Thompson Company, but they do not usually know he became its highest-paying employee, its "chief show piece," according to a *New Yorker* profile of him in 1928. He directed inventive advertising campaigns for, among other items, Maxwell House Coffee, Ponds Cold Cream, and the Pennsylvania Railroad. He wrote and lectured to his advertising colleagues on the theory of selling more generally. In the popular press, though no longer in learned journals, he continued to be the spokesman for behaviorism, especially in relation to child-rearing. Few of today's psychologists in their 40's realize the extent to which American child-rearing in the 1930's, perhaps including their own, was shaped by popular articles Watson wrote on the subject in the 1920's, after he had shaken off the restraints of academic psychology. He had somehow derived from behaviorism an approach to parenthood that anyone today, even a fully committed behaviorist, would likely find forbiddingly unaffectionate.

This is a useful biography for filling in our knowledge about Watson after the departure from Hopkins. Unfortunately, Cohen's command of detail is occasionally shaky. For example, he underestimates the influence of Watson's teacher at the University of Chicago, the brilliant German biologist Jacques Loeb, and calls him a Frenchman besides. Watson is depicted working for the "Pentagon" in 1918, more than 20 years before it was built, or commenting on a psychologist called Raymond Cattell in about 1910, when *the* Raymond Cattell was about 5 years old. No doubt it was the unrelated James McKeen Cattell whom Cohen had in mind.

Other lapses are more serious. In sympathy with the subject of his biography, Cohen tries to make something out of Watson's work that cannot be found in it. There is no doubt that Watson was a potent catalyst in the formation of modern objective psychology, but he was truly just a catalyst. Next to nothing of scientific interest was left of Watson's own behavioral theories by 1924, when the *New York Times* reviewer of his book *Behaviorism* said it marked a "new epoch in the intellectual history of man." By then, the behavioral stream had moved on and branched in the work of K. S. Lashley, W. S. Hunter, E. R. Guthrie, Stevenson Smith, A. P. Weiss, and others. By 1945, behaviorism had consolidated around the fundamental issue of the nature of the learning process, to which Watson had contributed little beyond drawing attention to the work of the Russians, Bekhterev and Pavlov, on simple conditioned reflexes. The conditioned reflex has a place in modern behavior theory, but not nearly as central or as large a place as Watson, or for that matter Pavlov or Bekhterev, gave it. Like many journalists who write about the subject today, Cohen fails to realize that the subordination of behaviorism to the conditioned reflex was a brief diversion and has been out-of-date news for more than five decades.

R. J. HERRNSTEIN
Department of Psychology and Social Relations, Harvard University, Cambridge, Massachusetts 02138

Aplysiology

Behavioral Biology of *Aplysia*. A Contribution to the Comparative Study of Opisthobranch Molluscs. ERIC R. KANDEL. Freeman, San Francisco, 1979. xvi, 464 pp., illus. Cloth, $40; paper, $20.

The gastropod mollusks, in particular the land snail *Helix* and the sea hare, *Aplysia*, were introduced into cellular neurophysiology primarily by the work of Angelique Arvanitaki in the 1940's and Ladislav Tauc starting in the '50's. The nerve cells of these animals lie at the surface of central ganglia, where they are readily visible and recognizable from individual to individual. The somata of these cells are commonly large and easy to impale with microelectrodes, and their electrical distance from their synapses is short enough that synaptic activities are readily recorded. It was thus inevitable that these animals would become popular targets for neurophysiological analysis. Their popularization has unquestionably been hastened and its form strongly influenced by the work of Eric Kandel and his associates on *Aplysia*. Work that has been done on *Aplysia* ranges from neurochemistry of parts of isolated nerve cells, to biophysics of nerve cell potential generation, to pharmacology of synaptic action, to the neuronal bases of behavior. Although Kandel and his collaborators have contributed importantly in all of these areas, their most distinctive contribution has been in establishing *Aplysia* as a model animal for investigating the relationship between nerve cells and behavior.

Behavioral Biology of Aplysia and Kandel's earlier book, *The Cellular Basis of Behavior* (1976), together constitute a major effort to bring both knowledge of the nervous system and behavior of *Aplysia* and an approach to the study thereof to the widest possible audience.

The purpose of *The Cellular Basis of Behavior* was to provide budding neurobiologists with the rather considerable neurophysiological background needed for understanding work on cellular bases of behavior in invertebrates and to summarize the work on *Aplysia* itself. Both from my own classroom experience and from comments of students at a variety of levels, I believe the book to have been a masterly success. *Behavioral Biology of Aplysia* will reach a smaller but I believe no less enthusiastic audience. Its goal is to reintroduce *Aplysia* from a broader point of view; that is, to draw attention to questions of adaptation, evolution, and comparison between closely related forms. It is intended as a handbook of the biology of *Aplysia* for serious students of the animal's behavior and nervous system.

To this end the book brings together a wealth of facts on classification, distribution, system physiology, nutrition, natural history, experimental psychology, experimental biology, and central nervous system physiology of the opisthobranch mollusks, the subclass of the gastropods to which the genus *Aplysia* belongs. There are a variety of ways in which students of *Aplysia*'s behavior and nervous system can benefit from the book.

First, the knowledge the book provides will aid in such practical matters as maintenance of laboratory cultures and physiological preparations. Beyond this, the book is a compendium of useful information on natural history and ecology. Comparative psychologists wanting to design experiments to test the ability of *Aplysia* to learn for food reward and interested in minimizing delays of reinforcement will learn here that even when food is quite close it takes *Aplysia californica* about half an hour to find it; those interested in learned food aversions and specific predispositions to learn should find it useful to be told that