

BOOK REVIEW

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PETERS, R. H. 1991. **A critique for ecology**. Cambridge Univ. Press, New York. 366 p. \$79.50 hardcover, ISBN 0-521-40017-1; \$29.95 softcover, ISBN 0-521-39588-7.

A critique for ecology is the culmination and recapitulation of a series of earlier publications by Peters. Unfortunately, the book is not as succinct and readable as his original works. His major theme is that ecology is a relatively young science and is weak in many areas because logical, nonoperational constructs and models rather than empiricism have permeated schools of thinking. He is concerned that society is not profiting from our current ecological expertise and that the lack of predictive ability in most ecological disciplines is impeding progress toward a stronger science of the environment. His message is clear: the only hope for ecology is through criticism ultimately leading to empiricism; however, he presents nothing new synthetically from either an ecological, philosophical, or epistemological point of view. The book attempts to stimulate a renaissance in the way that ecologists think about and conduct their science. Following his lead purportedly will result in actions that will provide feasible, attainable solutions to global environmental problems and will, concomitantly, strengthen political perceptions of ecologists and enhance their funding.

"Crisis in ecology" (chapter 1) extolls the value of "hard" disciplines that have "highly sophisticated sets of predictive relations" (p. 2) such as agronomy, disease control, and public health. Peters emphasizes the current crisis in ecology by highlighting a litany of insoluble questions from the pages of several widely used introductory ecology texts. Questions such as "What are the major attributes of a community?" and "Can guild structure evolve, even when resources are continuously distributed, as a means of reducing diffuse competition?" are unanswerable and are "perpetual conundra, as puzzling to one generation as the next. Because of this, they accumulate in the literature, crowding out resolvable problems, and becoming paradigmatic ecological questions" (p. 14).

Chapters 2 and 3 on "Criteria" and "Tautology" are most enlightening because they are specifically concerned with the power of predictiveness in ecology and how it leads to the construction of theory. Theories should be predictive tools about the phenomena of nature. Peters contends that, currently, much of the ecology "predicts neither the characteristics of organisms nor much of anything else" (p. 17). Although predictiveness is essential for any science to move forward, he offers other criteria as well for judging the quality of a particular theory including relevance, precision, practicability, simplicity, and generality. He also explores how ecologists have embraced tautologies to explain natural phenomena instead of seeking explanations based on falsifiable inferences. In his view, tautologies are "purely logical constructs that describe the implication of given premises and never reveal more than those premises contain" (p. 39). He argues that the theory

of evolution by natural selection ("one of the greatest unifying concepts in biology") is tautological because it can accommodate any evolutionary observation. That is, it is useful as a logical construct but unfalsifiable as a theory because it is not "hypothetical, risky, and dubious." He also quickly dispatches the logistic and Lotka-Volterra equations as tautologically meaningless and begins his continuing admonition of theoretical ecologists and of the substitution of jargon for ideas and theories. Unfortunately, he suffers from his own excessive jargon which makes the remaining chapters a lengthy, somewhat plodding account.

"Operationalization of terms and concepts" (chapter 4) explores nonoperational constructs and gives specific examples of how two conceptual relations (stability/diversity and competitive exclusion) that lack clearly defined terms can lead to predictive ambiguity. Peters rightly criticizes the overuse of ecological typologies, such as dichotomies, in situations where continua are more appropriate. This and remaining chapters begin to overwhelm the reader, however, by invoking a plethora of nomenclature (e.g. "omnibus term," "concept cluster," "panchestron," "normic and colligative explanations," "ex cathedra," etc.) to describe concepts and categories. In honoring every conceivable operational flaw with its own name, he commits the sin of excessive terminology which he so forcefully condemns throughout the book.

"Explanatory science" and "Historical explanations and understanding" (chapters 5 and 6) detract from the overall message. They are long, rambling, and difficult to follow. Peters distinguishes between explanatory and predictive theories and defines the former as universal statements about processes (e.g. reductionism, mechanism, and causation). Surprisingly, he finds no real value in experimentation and contends that laboratory studies reveal little about nature. Field tests are said to offer nothing useful because potential edge effects, logistical constraints, lack of statistical power, and vandalism may influence the results. He reasons that natural experiments have the greatest chance of spurious correlation and believes that experimental approaches, in general, are flawed and therefore interpretation of any results is biased. Combining experimental techniques does allow one to suspect hypotheses that are not consistently confirmed; however, "even this confirmation would not guarantee that the manipulation will have the desired effect in novel conditions" (p. 141). In "Weak predictions" (chapter 7), he examines the appeal of popular theories, such as island biogeography, plant-herbivore coevolution, succession, and the *r-K* continuum that are supposedly predictive and are favored by ecologists because they "appear attractive." These theories have been repeatedly falsified by the scientific community and their continued use in introductory texts, courses, and scientific papers "continues to erode the standards of science."

"Checklist of problems" (chapter 8) begins with a misplaced, commonsense account of what should, and should

not, be contained in a well-written introduction, methods, and results section of any scientific paper. Included under the heading "methods" is a useful and concise excerpt from Green (1979) on statistical advice for biological research. Unfortunately, what appears as fodder for Peters' idea of a discussion section is a wordy diatribe filled with jargon sandwiched around a review of Flew's (1977) philosophical discourse in thinking. "Putting it together—competition" (chapter 9) focuses on the prevalence of competition theory in traditional ecological inquiry. Armed with the deductive tools developed from previous chapters, Peters smashes it soundly for its inability to predict anything useful and states that "the potential of competition theory has been explored by some of the brightest minds in ecology for decades without notable success. The rest of us might do well to look elsewhere" (p. 272). He argues that competition theory is vague at best and only through empirical studies could a solution possibly be extricated. As is characteristic of earlier chapters, he offers no constructive alternatives to what he envisions as flaws in current competition research. Instead, he seems content to criticize and then leave the reader wondering what approach, if any, would satisfy his predilection for predictiveness. The final, and somewhat anticlimactic, chapter ("Predictive ecology") gives eight classes of examples of predictive ecology (five of which are from his own earlier papers) and an account of the common properties that unite these classes.

We found the organization of the text disappointing. Throughout the book, Peters criticizes scientific accounts and rarely chooses to identify alternative hypotheses or methods of inquiry that would lead to other than phenomenological accounts. This is a major failing. Had he used the same organizational style so skillfully used by Underwood and Denley (1984)—which he cites at least four times—this book might have been more useful, especially at the graduate level. As is, it is a lengthy and redundant account of his past efforts that one must painstakingly wade through to find the meat. The primary purpose of this book was to highlight the fact that today's environmental problems are not being solved at a fast enough rate to respond to human pressures. Any reasonable ecologist would agree that we have a responsibility to provide feasible solutions to these problems. Peters believes, and we concur, that answers can be found through predictive studies (*sensu* Peters 1986) that will, concomitantly, enhance our scientific knowledge of the natural world.

The method Peters advocates for creating predictive models is a simple matter of sampling two variables together, plotting them with scatter diagrams, and estimating their degree of association through regression analysis. He argues that the "cause of such predictive success is irrelevant" (p. 133) and anyone who embarks on a pathway to determine the underlying mechanisms responsible for the relationship would be heading for "infinite regress." He believes that the most efficient and effective way to gain information about our environment and the organisms in it is to use data from the literature to construct theories. However, he fails to fully appreciate that experimental conditions, sampling schemes and efficiencies, and other less quantifiable, but objective differences exist between most studies (even, perhaps, temporally within the same researcher). Such differences may be minimal, but as Peters and Downing (1984) conceded, extracting data

from the literature may be biased depending on where the investigator was schooled.

In placing greater emphasis on the older literature in developing predictive models, Peters denigrates the use of experimental field manipulations to explain anything important about the real world. Problems in interpreting the results of, for example, field experiments in studies of marine benthic invertebrates have been discussed by Dayton and Oliver (1980) and Underwood and Denley (1984) who advocated use of competing or multiple hypotheses (*sensu* Chamberlain 1897) to address complex ecological questions. We disagree that field tests elicit ambiguous, nonpredictive results. One has only to look critically at the field research in marine benthic ecology conducted by Peterson (e.g. 1982), Underwood (e.g. 1981), and Vadas et al. (e.g. 1986) to witness the kind of strong criteria for scientific knowledge that Peters encourages.

There is a crisis in ecology. Others, notably Platt (1964), Dayton (1979), Strong (1983), Underwood and Denley (1984), and Elner and Vadas (1990) offer viable solutions which favor testing multiple hypotheses and strict adherence to strong, inductive inference. These papers and others (including earlier works by Peters) are important contributions to creating a vision for future ecological investigations. Joseph Pulitzer once wrote an admonition to writers: "Put it before them briefly so they will read it, clearly so they will appreciate it, picturesquely so they will remember it, and, above all, accurately so they will be guided by its light" (p. 4, Price et al. 1984). *A critique for ecology* is a catchy title that is attractive by itself and will lead many to purchase it. It is neither brief nor particularly clear, and most who attempt to read it will probably not glean enough guidance from it to justify their effort. We are torn between advocating the purchase of this text and suggesting that ecologists interested in critiques of their discipline create a compendium of journal articles. After much debate, we recommend the latter choice.

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References

- CHAMBERLAIN, T. C. 1897. The method of multiple working hypotheses. *J. Geol.* **5**: 837-848.
- DAYTON, P. K. 1979. Ecology: A science and a religion, p. 3-16. *In* R. J. Livingston [ed.], *Ecological processes in coastal and marine systems*. Plenum.
- , AND J. S. OLIVER. 1980. An evaluation of experimental analyses of population and community patterns in benthic marine environments, p. 93-120. *In* K. Tenore and B. C. Coull [eds.], *Marine benthic dynamics*. Univ. South Carolina.
- ELNER, R. W., AND R. L. VADAS. 1990. Inference in ecology: The sea urchin phenomenon in the Northwest Atlantic. *Am. Nat.* **136**: 108-125.
- FLEW, A. 1977. *Thinking straight*. Prometheus.
- GREEN, R. F. 1979. *Sampling design and statistical methods for environmental biologists*. Wiley.
- PETERS, R. H. 1986. The role of prediction of limnology. *Limnol. Oceanogr.* **31**: 1143-1159.
- , AND J. A. DOWNING. 1984. Empirical analysis of zooplankton filtering and feeding rates. *Limnol. Oceanogr.* **29**: 763-784.
- PETERSON, C. H. 1982. The importance of predation and intra- and interspecific competition in the population biology of two infaunal suspension-feeding bivalves, *Protothaca staminea* and *Chione undatella*. *Ecol. Monogr.* **52**: 437-475.
- PLATT, J. R. 1964. Strong inference. *Science* **46**: 347-353.
- PRICE, P. W., C. N. SLOBODCHIKOFF, AND W. S. GAUD. 1984. *A new ecology: Novel approaches to interactive systems*. Wiley.
- STRONG, D. R. 1983. Natural variability and the manifold mechanism of ecological communities. *Am. Nat.* **122**: 636-660.
- UNDERWOOD, A. J. 1981. Structure of a rocky intertidal community in New South Wales: Patterns of vertical distribution and seasonal changes. *J. Exp. Mar. Biol. Ecol.* **51**: 57-85.
- , AND E. J. DENLEY. 1984. Paradigms, explanations and generalizations in models for the structure of intertidal communities on rocky shores, p. 151-180. *In* D. R. Strong et al. [eds.], *Ecological communities: Conceptual issues and the evidence*. Princeton.
- VADAS, R. L., R. W. ELNER, P. E. GARWOOD, AND I. G. BABB. 1986. Experimental evaluation of aggregation behavior of the sea urchin *Strongylocentrotus droebachiensis*: A reinterpretation. *Mar. Biol.* **90**: 443-448.