essentially developed an official line" that is patently untrue. Medvedev has been able to draw upon the revelations in 1989 by some of the officials and scientists involved that this was in fact the case. Marples, writing earlier and drawing his inferences largely from the events and reportage of 1987, clearly demonstrates considerable skill at what might be called "sovietology"—the technique of plumbing and interpreting Soviet reports, newspapers, and interviews and reconstructing situations not only from what is actually said but also from what is not said and from the overall context of the message.

Both books highlight many of the key mistakes or shortcomings that figured prominently in the accident and that the Soviets have been reluctant to publicize. These include such problems as the initial construction of the unit in 1984, when it was licensed for commercial operation without certain crucial safety systems installed; the lack of instruments in the first hours and days to adequately measure the level of radiation in the immediate vicinity; the deliberate initial under-reaction to the accident in an effort to maintain a "business-as-usual" facade; the evacuation fiasco; the mistakes made in taming the reactor fire; and the botched cleanup of the special zone. They invariably trace these problems back to certain longstanding features of the Soviet system.

This is, in fact, an important theme in both books. The authors see certain aspects of what might be termed "the Soviet system," not just the incompetence of the plant operators on the tragic night as the Soviets have charged, as root causes of the accident. They persuasively argue that it was the "system" that placed unqualified operators on the staff of a major nuclear power plant, produced the defective reactor design, was responsible for the poor quality of the initial construction of the station, and established the operating procedures for the plant. And the litany goes on. Medvedev makes this point somewhat more explicitly by stating his belief that open societies deal far more effectively with such high-risk technologies and industrial accidents.

Both authors also quietly excoriate the International Atomic Energy Agency for its role in the cover-up by the Soviets. They both rate the August 1986 IAEA meeting in Vienna as a public relations success for the U.S.S.R. The international forum was used to allay much of the anger of the West about the initial lack of openness and was used to "sell" the Soviets' official line that operator incompetence caused the accident. Both authors conclude that although the Soviets were forced to make considerable concessions and had to present a lot of damning information at the meeting, the official report submitted to the IAEA was for the most part a cover-up. The authors submit that the IAEA really did not press the Soviets because it was not expedient to do so, as Chernobyl threatened the entire global nuclear industry of which the IAEA is a part.

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Prescriptions for Ecology

Ecological Experiments. Purpose, Design, and Execution. NELSON G. HAIRSTON, SR. Cambridge University Press, New York, 1989. xiv, 370 pp., illus. $52.50; paper, $24.95. Cambridge Studies in Ecology.

Ecology as a science has had a successional sequence starting with the early explorer-naturalists who described habitats and organisms. The first half of this century brought rigorous experiments on the mechanics of competition and predation, but the work was confined to laboratory systems. In the '50s and '60s natural history became the raw material for an expanding theory of ecology of the structure of natural communities, but this work was done by the elaborate statistical and mathematical manipulation of quantitative observations. It is always difficult to determine priority, but the first widely cited ecological experiments done in the field to address theoretical issues appear to be Connell's 1961 studies on the distribution of barnacles, Paine's 1966 study on predation by starfish in the rocky intertidal zone, and Eisenberg's 1966 study on population regulation in freshwater snails.

Nelson G. Hairston, Sr., now reviews the status of ecology as an experimental science. The subtitle of his book, *Ecological Experiments: Purpose, Design, and Execution,* suggests a handbook. This book is not a handbook, except in the sense of being a guide to the mind of its author, one of the primary figures in the field for the last 40 years.

Hairston begins with an essay on the big questions and how they have been addressed. The emphasis is on the traditional question of community ecology, What determines the abundance of species in nature?

Field observations are recognized for their value in posing the questions, but their interpretation by cleverness alone is rejected and the use of natural events, such as a landslide or a hurricane, as the basis for a "natural experiment" is abhorred because it "evades the issue of why manipulative experiments are conducted, which is first to test the validity of a specific idea, and second to avoid the charge of a posteriori reasoning" (p. 10). Or, as Hairston more characteristically states, "Nature has no stake in being understood by us" and "Ecologists who are not thoroughly familiar with the organisms involved risk wasting a great deal of time" (p. 31).

Mathematically derived theory is somewhat unfairly relegated to "giving the appearance of scientific rigor to what in principle is a more sophisticated version of the same process of explaining what has been observed" (p. 11).

Laboratory experiments are discounted because of the simplicity of the laboratory environment and the restricted array of species that can be included. Planned experiments in nature are declared to be the way to go. This said, Hairston reviews the elements of a valid experiment: knowledge of initial conditions, inclusion of controls, replication, and dealing with systematic variability by using statistical blocks or stratified samples. A third chapter discusses trade-offs in ecological experimentation: generality versus confidence, realism versus sophistication of experimental design, and sophistication of experimental design versus adequate replication in the field. The discussion is not a lesson in statistics, but rather an exercise in careful thinking. For example, "The duration of an experiment should be determined in advance, because of twin temptations: to stop when the results are pleasing, or to continue until they become so" (p. 31). These first three chapters should be carefully read by anyone planning ecological research or serving as a consulting statistician to ecologists. Even those of us in the choir should listen to the sermon. Others may be outraged.

The next four chapters, about three-fourths of the book, are habitat-by-habitat reviews of the results of experimental field studies. The criterion for inclusion is a planned manipulation to test an a priori hypothesis. These chapters are not a complete review of ecological experiments that have been done in the last three decades, but they cover a remarkably broad selection and are a testament to how much experimentation has contributed to our knowledge of how communities function. Each study is briefly abstracted and then the judgment is handed down. I found myself waiting with joyful anticipation for the word. "To recapitulate, the flaws in this study were as follows" (p. 49); "I have criticized these experiments elsewhere, but the nature of this book requires that I repeat the criticisms" (p. 115); "The experiments represent the sacrifice of realism to rigid adherence to a preconceived design" (p. 117).

One long chapter reviews experiments in forests. Most of this chapter is presented as a

vindication of a paper many ecologists love to hate, a 1960 essay by Hairston, Smith, and Slobodkin (HSS) in which the authors proposed a series of deductions about how terrestrial communities are organized. Historians and sociologists of science will treasure this review of the impact of a 30-year-old classic by its senior author. The evidence is strongly in favor of the HSS hypothesis, although at times Hairston drops his rigor to accept as confirming results that do not conform to the standards of design introduced in the first three chapters. We are all guilty of wishful thinking now and again.

Other chapters review work on successional communities, deserts, and freshwater and marine environments. In the discussion of ponds, there is also a rather detailed review of the excellent work of Nelson G. Hairston, Jr., on aquatic communities. The chapter on marine communities does not include work on pelagic habitats.

The final chapter reviews conclusions that can be drawn from field experiments. These closing arguments are persuasive and may serve to close an era in ecology in which a few theoreticians pursued a grand unified field theory for ecological systems. Hairston quite firmly pronounces that the most important conclusion of the book, and therefore of a quarter century of ecological experimentation, is that at best generalizations can be made at the level of specific environments (forests, successional communities, deserts, freshwater, or marine systems). Most ecology textbooks review the evidence for the relative importance of competition, predation, and mutualism across all systems. The next generation of textbooks may follow Hairston's advice and adopt an organization that contrasts the relative importance of these mechanisms of interaction in each of the major biomes. This is anticipated earlier in the book (p. 53): "I am not ready to abandon the hope of synthesis, at least for some environments. The field might yet produce another Darwin."

The book has an excellent index including ecological subjects, habitats, and Latin binomials with a separate listing of authors. I expect many students will use this index to discover who wears the white hats and who wears the black hats in the eyes of a person who has had a tremendous impact both directly and indirectly on the training and direction of a generation of ecologists. I hope this book motivates the current generation of students to seek the training in statistics that their advisers often lack.

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