Book Reviews

On Kinds of Models

Mathematical Models in Ecology. The Twelfth Symposium of the British Ecological Society, Grange-over-Sands, Lancs., March 1971. J. N. R. JEFFERS, Ed. Blackwell, Oxford, England, 1972 (U.S. distributor, Davis, Philadelphia). viii, 398 pp., illus. $21.

This book is a noteworthy contribution to the growing literature on mathematical models in ecology. The bulk of it consists of papers given at a symposium, and only 3 of the total of 14 are out of place in a work devoted to mathematical models. G. M. Van Dyne's contribution (which is twice as long as any other) is almost wholly concerned with the organization of a large-scale research undertaking, that of the U.S./IBP Grassland Biome Program. It is an interesting paper, however, and since it describes the deployment by a manager of a staff of mathematical modelers it is, perhaps, an example of metamodeling.

The two other irrelevant papers exemplify the way in which the term "mathematical modeling" has been unwisely stretched to embrace a variety of activities that are certainly not implied by it. The ordination and classification of vegetation (described by M. P. Austin) are not modeling. Nor is vegetation surveying (J. M. Lambert), and to write of sampling techniques as "sampling models" is to destroy, by dilution, the precision of a useful word.

What, then, is a mathematical ecological model? According to Van Dyne (p. 112) a model is formulated from hypothesized cause-and-effect relationships. It is therefore more elaborate than, though not different in kind from, a single hypothesis. According to D. W. Goodall (p. 174) a model is a homomorph, or map, of the system modeled; although a model can be static, "for predictive purposes . . . a dynamic model . . . is required—one in which . . . processes are represented." P. J. Radford stresses that the fundamental components of any dynamic system are rates (p. 281) and that therefore ecological systems should be modeled in terms of differential equations (p. 278).

Indeed, most of the authors seem to feel (rightly, I believe) that the word "model" necessarily connotes a dynamic model; even with this restriction, however, the word can be given several distinct definitions, all equally acceptable. There would therefore be a great gain in clarity if models were classified systematically and labeled accordingly. A simple dynamic model (one not divisible into submodels) can be classified dichotomously in at least four different (though not independent) ways: (i) it may treat time as continuous or discrete; (ii) it may be an analytic model or a simulation model; (iii) it may be deterministic or stochastic; (iv) it may be inductive (empirical) or deductive (theoretical).

Anyone about to construct a model thus has four decisions to make and a catalog of 16 styles to choose from. And anyone reading accounts of other peoples' models can consider them more judiciously with this classification in mind.

Distinction i is demonstrated by the contrast between Radford's paper and M. B. Usher's. Usher summarizes models of population growth based on projection matrices and the difference equations to which they lead. In his view (p. 36) "the matrix approach is more reminiscent of the biological process of birth and death," which are discrete events. Radford, however, in using differential equations to represent ecological processes, treats time as continuous.

Distinction ii is discussed in G. J. S. Ross's paper. His analytic models are static. Dynamic models that are amenable to analysis without being so simple as to be biologically naive are uncommon, and are now outnumbered by simulation models, those whose predictions can be cranked out at high speed on a computer. The importance of simulation models makes two of the symposium papers especially valuable: J. K. Denmead's, which is a brief introduction to analog computing, and Radford's, which acquaints the reader with DYNAMO as a simulation language.

Distinction iii is not given the attention it deserves. Most of the models described are deterministic. Admittedly one of the pest control models described by G. R. Conway and G. Murdie (p. 201) treats the movements of a predator as stochastic (they label their predator a "blundering idiot" because of its failure to learn from its mistakes). And Ross (p. 301) describes a model in which the sex ratio in a nematode population depends on each individual's probability of finding an unoccupied attack site on a plant root. But only Ross (p. 298) points out that with stochastic models one may obtain completely different realizations from repeated runs of a single model. This fact deserves greater emphasis. It explains the absurdity of trying to devise a separate model for every distinguishable kind of natural ecological event.

As to distinction iv, it is clearly expounded by Goodall (p. 181). As he remarks, to simulate a complex ecosystem may require construction of an empirical model built of numerous submodels. The researcher has to decide which (if any) of these submodels are to be deductive (and hence mimic underlying biological processes) and which are to retain their inductive "black box" status.

To construct appropriate mathematical models of ecological processes is, of course, only one half of an ecologist's labors. No less important is their testing, but the matter seems to get much less than half of most workers' attention. To accept as proof of the validity of a model the fact that its predictions come out right, even if this happens quite often, is naive. The most thoughtful discussion (in this volume) of the problem of model-testing is that of Ross. A good test that seems not to be mentioned explicitly, however, is to extract from the model (if one can) two or more different *kinds* of predictions, even though only one of them may be of practical interest. If a model correctly predicts two or more simultaneous and unrelated outcomes, it is highly unlikely that the correspondence between model and nature is due only to chance.

A few summary comments in conclusion. Besides its 14 papers the book contains a list of those who attended the symposium; an account of a modeling problem sprung on the participants for discussion without their having prior knowledge of its nature; four assessments of the proceedings by as many judges; and satisfyingly complete author and subject indexes.

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Air and Sea

Atmosphere-Ocean Interaction. E. B. KRAUS. Oxford University Press, New York, 1972. viii, 276 pp., illus. $18.75. Oxford Monographs on Meteorology.

The sea surface is a restless creature. It both responds to the atmosphere and modifies its condition. Exchanges of matter, momentum, and energy are basic to the global climate balances at one end of the scale and to the physical and chemical environment enjoyed by phytoplankton at the other. A tantalizing variety of physical processes are involved, and their interplay continues to challenge both oceanographers and meteorologists. For example, as Kraus points out, the chemical composition of droplets in spindrift, swept from the crest of a wave in a high wind, is variable and different from the bulk composition of seawater because of the existence of a surface film of largely biological origin. The evaporation of these droplets provides salt nuclei for subsequent precipitation in moderate and low latitudes.

This book is concerned with the physics of the processes involved in the interactions between the ocean and the atmosphere. Its scope is a good deal wider than Kitaigorodsky's "Physics of Air-Sea Interaction" (at present available only in Russian), which concentrates on the local exchange processes for momentum, heat, and matter. There is rather less emphasis here on the overall parametrization needed by numerical atmospheric modelers, perhaps for good reason—where the physics is lacking, attempts at parametrization are likely to be misleading. The states of matter near the interface, ranging from mushy ice to bubbles and spray, illustrate the complexities involved. Surface waves, turbulent transfers near the interface, both above and below, the structure of the planetary boundary layer and large-scale, low-frequency planetary waves all receive some attention. More detailed accounts of each of these are available elsewhere but not placed so firmly in this context.

Not always easy to read, the book suffers at times from an unevenness in level. On the one hand, ideas about surface tension are described in detail, even though these should be common knowledge to juniors in a good physics course. On the other hand, the mathematical approaches given for the specification of atmospheric and oceanic turbulence are likely to be tough going for all but well-trained physical oceanographers and meteorologists. The latter problem is, I suppose, intrinsic to the subject matter, but the professional or the advanced graduate student, to whom the book is addressed, may find some parts of it either prolix or, when results are given without the supporting arguments, frustratingly brief. Nevertheless, it complements well the existing books on this subject and with its breadth of coverage and fine bibliography will surely find its place on the bookshelves of both meteorologists and physical oceanographers.

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Climate and Human History

Times of Feast, Times of Famine. A History of Climate since the Year 1000. EMMANUEL LE ROY LADURIE. Translated from the French by Barbara Bray. Doubleday, Garden City, N.Y., 1971. xxiv, 426 pp. + plates. $10.

Ladurie has given us a wealth of carefully considered data on the geologically recent history of climate in *Times of Feast, Times of Famine*. This synthesis is derived from a mass of contemporary documentary evidence of crop yields, dates of grape harvests, and reports on wine quality and on advances of glaciers, mostly from the long series of records from France and the Alpine region.

As a student of past climate as deduced from palynology I find it exciting but almost daunting to encounter the precision with which the professional economic historian can pinpoint the years of abnormal climate, when the radiocarbon-dated pollen diagrams cannot yet indicate which decades were affected by climatic alterations. Ladurie has proceeded cautiously in evaluating the phenological data, avoiding the trap of trying to derive paleoclimatic inferences from historical events which were not direct climatic indices; he set himself the task of compiling data to which he could apply "a qualitative method comparable in rigor if not in accuracy and variety to the methods of modern meteorology." A potential pitfall is the change in areas of crop growth due to economic and cultivation changes; for example, the northward movement of olive growing from 1550 to 1600 occurred not because of climatic warming but because the growers were trying to exploit an expanding market, actually at a time of colder climate. Ladurie also maintains that late medieval reductions in French grape cultivation resulted not from climatic change but from high labor costs due to the plague and disruptions of war. He is opposed to the climatic-determinist approach to history, exemplified by Huntington, and criticizes C. E. P. Brooks, for using circular reasoning in his studies of paleoclimate, for using secondary rather than available primary sources, and for introducing a fictional glacial retreat in the middle of the "Little Ice Age." Historians draw criticism for lack of interest in climatic history, and Ladurie castigates those students of climate he calls "cycle mad."

The long series of French and German wine harvest dates and quality reports have been examined for their phenological interest: early harvests resulted from warm springs and summers characterized by anticyclonic conditions, and late ones from cool springs and summers with cool and cloudy cyclonic conditions. The correspondence of German records with those from France is reassuring, and indicates regional climatic control. Reliable German records assembled by K. Müller cover the years 1453 to 1950, with some data back to A.D. 1000. The French data (many of them gathered by Angot) are ample back to the 17th century, with fewer series covering the 16th century, and with some data back to the 14th century. The validity of the phenological data is confirmed by comparisons of vintage dates and wine quality reports with meteorological records in the last two centuries. Germany experienced generally warm summers from 1453 to 1552, but from then until about 1600 the summers were over-