Mostly a Misunderstanding, I Believe
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(Kenfield 1966) and is a classic in the basic vegetation science dynamics of old-field landscapes and the introduction and management of stable vegetation types. Those of us who were in on the secret of Egler's pseudonym (an anagram) found it amusing that there was even a biography of Kenfield on page 231. He simply did not want to be interrupted by the general public, and was always "out of the country" when contacted by the publisher (Hafner). Fortunately, this fine book is now available from the Connecticut College Arboretum in the second revised edition.*

Throughout his life, he lived on and expanded his family home lands (Aton Forest) in northwestern Connecticut into a 450-ha (1,100-acre) natural area preserve and field research area.

With the establishment of Aton Forest Inc. and the Aton Forest Fellowship Trust, it is anticipated that a sizable endowment will continue to promote the holistic type of ecology (including humans) which he fostered over his long and productive career. He has left a remarkable legacy in the old fields and woodlands at Aton Forest, where long-term ecological processes can continue to be documented and studied. Frank Egler left the world better than he found it, by acquiring and protecting a legacy of "natural" and managed ecosystems where future scientists can attempt to understand the systems he felt were "not more complex than we think, but more complex than we can think."

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*Available for $25.95, plus $3.00 shipping and handling. Connecticut College Arboretum, Connecticut College, New London, Connecticut 06320.

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I read Dale and Van Winkle's (1998) reaction to my "editorial" (Aber 1997) on a lack of rigor in ecological modeling with much satisfaction. The points of agreement greatly outnumber the points on which we disagree. It seems that the crux of the disagreement derives from a misuse of language on my part that can be easily corrected.

Dale and Van Winkle open by stating that "belief" in models is an inappropriate goal, in that belief implies acceptance on faith or trust, rather than on compelling information. That was a surprising definition of the term to me, but, as it turns out, one supported by *Webster's*. I agree here that accepting models (or choosing not to) without critical evaluation is at the heart of the problem presented by modeling in ecological research.

The list of statements to which Dale, Van Winkle, and I would all ascribe seems to include: (1) the value of increasing rigor in the process of publishing models, (2) the advantages of taking a minimalist approach by using the simplest model that proves "adequate" (as well as agreement on the difficulty of defining "adequate" in a general way), (3) the fact that a model represents a set of working hypotheses and assumptions about the important interactions within a system, (4) the value of models that "fail," and (5) the value of documenting the modeling process.

I would also agree with two additional points made by Dale and Van Winkle, which they expressed as possible areas of disagreement. These include: (1) that models are never complete and never represent perfect knowledge of the system, and (2) that sources of uncertainty need to be understood and presented in papers. Indeed, it is the frequency with which models are presented that match observed data exactly (which can only occur with negative degrees of freedom and a lack of rigorous validation, as discussed in my original letter) that causes the largest rift with field scientists, who know that the unknowns are substantial and important.

I can detect only one area in which there might be an important difference in the approach to modeling expressed in my letter and that of Dale and Van Winkle (1998). That is in the value of the modeling process in the absence of substantial quantitative information. Dale and Van Winkle suggest that "The empirical information for rigorous calibration or validation commonly is not available," but then go on to describe the value of the modeling process in assisting scientists in "sharing their expertise to develop a simulation model." Two things trouble me about this line of reasoning. First is the apparent interchangeability of calibration and validation in the first part of the statement. I would suggest that calibration cannot be rigorous without validation. If all the data relative to a system are used to derive the parameters in the model (calibration), then there are no independent data left to test the accuracy of that calibration (validation). I would maintain that any model with more variables than observations from which those variables can be derived (the negative degrees of freedom problem) cannot be calibrated rigorously, and there is no basis for determining the accuracy of such a model. Deriving 40 parameters from four observations (a worst case, perhaps, but such examples can be found in the literature) just won't work.

The second is the value ascribed to a more qualitative type of activity in which "expertise" is shared and a model constructed, mentally, if not in code. As scientists, we are compelled to express our understanding quantitatively. When a particular problem or system cannot be expressed quantitatively, then we need to admit that we do not understand that problem or system, and begin the process of research that will lead to quantitative understanding. I would agree that those participating in the "expert" method of deriving a conceptual model will enhance their own understanding of how a dynamic system with feedbacks can produce counterintuitive results. This is part of the educational process that can increase awareness of the importance of system analysis in the study of ecological or social systems, and that can be appropriate in the classroom or in informal discussions. We should not, however, expect such a process to lead to models representing real systems or, more important still, that can be used in policy making.

The danger here is that existing dogma about how a particular system works can be reinforced by including unchallenged or unmeasured interactions. I have seen such modeling efforts contorted until the "looks good" criterion is met—until the model gives the results that the experts knew "should occur" before the process began. For example, if the "looks good" goal is one set a priori by a corporation or organization with a particular point to prove, then modeling is quickly subverted to these other goals and becomes meaningless or even dangerous. Systems analysis and modeling are great debunkers of dogma if pursued openly and rigorously—and great reinforcers of dogma when pursued inappropriately.

To conclude, I agree wholeheartedly with Dale and Van Winkle's assertion that "belief," at least as defined by *Webster's*, has no place in the modeling process. I also agree with many other points they make. I hope to see more dialogue on the modeling process in order to increase its value in Ecology. In a field that demonstrably deals with some of the most complex systems in nature, it seems only natural that systems analysis through modeling should be on the top tray in the toolbox.

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Model Interactions: a reply to Aber

Aber's perspective on our comments regarding his 1997 article on modeling has led to a fruitful discussion on the role of ecological models (Aber 1997, 1998, Dale and Van Winkle 1998). The sequence of titles in this interchange portrays our increasing agreement about the use and abuse of ecological models. Aber first published an article in the ESA *Bulletin* (Aber 1997) entitled "Why don't we believe the models?" in which he called for more integration of models into other areas of ecology. We responded with an article (Dale and Van Winkle 1998), "Models provide understanding, not belief," in which we urged that models not be accepted on faith but be used to forward the hypothesis-testing aspects of ecological science. To this, Aber replies (Aber 1998) with "Mostly a misunderstanding, I believe," that agrees with many of the points we raised. Our extended discussion with Aber may serve as a "model interaction" of the way in which interchange can clarify a field of study.

Nevertheless, Aber's most recent comments prompt a response on our part. We endorse his proposal for better guidelines and standards for model application and publication. However, we emphasize modeling as a process that enhances understanding of a system, and note that publication is only one of its products. The process of modeling requires formulating hypotheses about how components of a system are related, and allows exploration of the implications of those hypotheses. It identifies sensitivities and uncertainties in a system, and forces us to specify which components we envision as deterministic or stochastic.

The modeling process has a valuable role to play in the overall iterative scientific process of hypothesis formulation (Overton 1977). It contributes to the design of experimental and monitoring studies (and the successful securing of funding for these studies), the development and application of mechanistic or simulation models, and the interpretation of results. The use of models in the scientific process is appropriate even when initial information about a system is sparse. The model can then be used to organize existing information, indicate the sensitivities of the system, and point out gaps in knowledge. For example, Aber summarizes one of his modeling papers by pointing out that "models are often more interesting when they fail than when they succeed" (Aber and Driscoll 1997). Even so, the interim conclusions of the