Ecological Modeling

The attached set of articles about ecological modeling appeared in the Bulletin of the Ecological Society of America. The papers discuss and define several pertinent terms and concepts. The list of articles below is in chronological order:

Aber, J.D. 1997. Why don't we believe the models? Bulletin of the Ecological Society of America 78(3): 232-233.

Dale, V.H. and W. Van Winkle. 1998. Models provide understanding, not belief. Bulletin of the Ecological Society of America 79(2): 169-170.

Aber, J.D. 1998. Mostly a misunderstanding, I believe. Bulletin of the Ecological Society of America 78(3): 256-258.

Van Winkle, W. and V.H. Dale. 1998. Model interactions: a reply to Aber. Bulletin of the Ecological Society of America 79(4): 257-259.

Why Don't We Believe the Models?

For several years I have considered the effect of the term "modeling" on ecological discussions. Suppose you are deep into a discussion with a small group of colleagues on a current hot topic in ecology and someone says, "Well, we've looked at that from a modeling perspective and . . . ." Have you noticed the tendency for eye contact to be lost, and for the conversation to drift until returning to the safer grounds of experimental data or pure theoretical speculation? I call this phenomenon the "glazed-model-gaze."

Why have models failed to penetrate the heart of ecological sciences? The question may seem specious at first, given the existence of an entire journal devoted to ecological models, and the number of models that have been published and applied to ecological problems. But the evidence can be turned around, in that the existence of such a journal suggests a separation of ecological modelers from other types of ecologists. Modeling is not seen as a serious tool, like statistics, for example, that most ecologists use as a regular part of their work, despite the constant acknowledgment that we deal with complex and highly interactive systems, and that quantitative understanding and prediction are critical in the application of what we know.

We might take that a step further and say that a great many ecologists are very skeptical of the modeling process and tend to disbelieve or discount insights provided by modeling exercises. In this way, ecology differs from many other mature scientific fields in which quantitative model predictions, and verification of those predictions, are central. When the question is asked, as it usually is, "Are you a modeler or a field scientist?" there are few who would respond, "Both."

I suggest that there is a very good reason for this general distrust of models in ecology: modeling projects and modeling papers are not generally held to a consistent, rigorous set of standards of full disclosure during peer-review. We allow far more hand-waving in the presentation of modeling results than we do for experimental data. I would like to propose two achievable objectives that could help increase the value of the modeling process in ecological research: (1) establish a set of guidelines or standards by which papers presenting modeling results should be judged, and (2) increase clarity in the understanding of the difference between calibration and validation.

On the first point, I propose that all modeling papers should contain, at a minimum, the following sections, with the suggested content.

Model structure.—The diagram or schematic must be complete with all components and connections shown. More importantly, the equation(s) used for each connection should be stated explicitly or clearly referenced, and citations should be given justifying that equation form. If the equation was theoretical or invented, it should be stated that this is the case and justified on the grounds that no data were available on this process. This section of the paper should become a literature review of previous work on the processes modeled, thus ensuring that the modeler is aware of previous field and laboratory work. The modeling process and literature review may suggest an equation form not previously used in presenting empirical results, which can be a major contribution of the modeling process.

Parameterization.—ALL of the parameters used in the model should be listed (with units), and ALL values for those parameters given, along with references to the sources of those parameters. If the parameters are derived by calibration, this should be clearly stated, the calibration method described, and the calibrated values given. If the model is mostly a theoretical construct used for identifying questions, this should be stated explicitly. However, whenever possible, models should include realistic, empirically based parameterizations that tie the model as closely as possible to experimental data, and to the empirically based majority in the ecological community.

Validation.—No modeling paper should be accepted without at least some attempt to compare model predictions against independent data sets: data not used in any way in the derivation of the model's parameters. Ecology is data-rich and model-poor relative to other fields. There are very few aspects of ecology for which no validation data exist. Where this is the case, such as with predictions of large-scale phenomena for which experiments cannot be run, this should be explicitly stated by the authors. Even in such models there are often intermediate variables predicted by the model and for which independent experimental data can be found.

Sensitivity analysis.—Every modeling paper should present the effects of altering model parameters or input variables on model predictions to give the reviewers some idea of model responsiveness to such changes. This also provides information on the importance of specifying each variable correctly. A greater degree of uncertainty can be tolerated in parameters to which the model is relatively insensitive. A second type of sensitivity analysis might be called the "null model" approach, stated as, "How does the predictive ability of the model compare with that of a simple multiple linear regression model?" Stated another way, what is the increase in predictive accuracy achieved by moving from a statistical model to one that includes knowledge of the processes in the system?

Prediction.—Only after the above standards have been addressed, should the model be used to predict something. Perhaps the greatest disservice ecologists can provide comes from allowing poorly described and unvalidated models to be used to predict the results of policy actions. It is equivalent to basing policy decisions on data we know to be seriously flawed. It also fosters the false impression that we know more than we do about the systems we study, which is then often in contradiction to what the experimental data suggest.

On the second point, all model papers, and all reviewers of models should be clear on the distinction between "calibration" and "validation." They are two distinct sides of the modeling coin, as opposed as night and day, and cannot be substituted one for the other. Calibration is the use of information on system behavior or outputs to derive parameters within the model. If all measurements are used in the calibration process, then no independent data sets are left for validation, which is the process of comparing model predictions with independent data sets not used in deriving the parameters. Calibration is thus a method for deriving parameters for the model, while validation is a comparison of model predictions with additional, independent data sets.

Unfortunately, the calibration process can be abused in ways that remove the chance to gain insights into ecological processes through modeling. In the worst applications of the calibration process, a model with n parameters is calibrated to $n/10$ or $n/20$ measured output variables by manipulating parameter values until the model's predictions match those few measured values. While parameter manipulation might be done in a subjective way through adjustments by the model user, or in an objective way by various Monte Carlo or other randomized search methods, the result still contains what in statistics would be called a serious negative-degrees-of-freedom problem. There are many, many sets of derived parameters that would give the same result. There is a very strong realization of this among those who do not do modeling, and the charge is always raised that modelers can produce any outcome they desire. Using the calibration procedures described here, that is the case. When the parameters derived by this procedure are not fully reported, this sense of lack of rigor is reinforced.

Perhaps one of the worst characteristics of a calibrated model is that it cannot fail. With negative degrees of freedom, accurate prediction of the few output variables is assured. When models cannot fail, we cannot learn from them. We cannot, then, use models to frame questions and to help derive future research programs. The modeling process becomes sterile and unenlightening. We can at least provide this degree of rigor to calibrated models; require that the model predictions be compared against totally independent data. The validation step can be applied to a calibrated model, as the two steps, calibration and validation, are totally distinct. One is a method for deriving parameter values and the other is a method for assessing the accuracy of the model, at least within the bounds of the validation data set.

In a pure case of a validated model, the parameters are derived directly from published data and the model is then run, without parameter modification, and predictions are compared with additional published data to see how well model predictions match those data. If the agreement between predicted and observed is not "good," that is an interesting and useful result, suggesting that our knowledge is imperfect. Analyses of why the model "failed" can suggest where future research should be focused to reduce uncertainties in our understanding of the integrated response of an ecological system. In addition, by knowing that a model can fail, we can then have more confidence in it when it does succeed.

What is meant by a "good" validation cannot be addressed here, but must be addressed by the ecological community. There is also complexity in the gray areas between the extremes of calibrated and empirically parameterized models, dealing particularly with the error limits within which empirical parameters can be specified due to measurement uncertainty. These and other important questions need to be addressed openly and directly by all ecologists, not to a subgroup of ecologists called modelers.

I hope that the case has been made. Modeling can be a much more valuable tool in ecological research than is currently the case, and I think that major advances can be made by making modeling both more rigorous and more accessible. By fully disclosing model structure, by making models available, at no cost, to any colleague who requests them, by providing support (to the extent possible) and using models as a basis for discussion of important ecological unknowns, we can advance our quantitative understanding of ecology and increase the precision and value of our knowledge in the solution of problems. Perhaps in the next generation, when the question is posed, "Are you a modeler or a field scientist?" a larger number of ecologists will respond, "Both." With luck, the question may become irrelevant.

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Models Provide Understanding, Not Belief[1]

Aber (1997, "Why Don't We Believe the Models?") expresses concern that ecologists do not believe models and thus do not generally use them in their research or analysis. While we agree with Aber's concerns and proposals, we think "belief" in a model is an inappropriate goal. Models shouldn't be believed more than any other scientific hypothesis. "Belief" suggests a faith or trust based on incomplete information. Instead, models should be used to improve understanding or insight. The goal should be not to believe models, but rather to use them to understand the system.

Our comments on Aber's article focus on simulation models and build upon our experience in applied ecology, where industries or agencies are looking for models to help resource managers, regulators, and lawyers make decisions and resolve environmental conflicts. Thus the world we deal with must accept incomplete information, and typically makes decisions using the best available information. In cases where the scientific evidence is incomplete or contradictory, decisions are often made without scientific input (e.g., as Wiens [1996] found in the aftermath of the Exxon Valdez oil spill).

Our use of simulation models is based upon the concept that models can synthesize the best understanding of the situation given current information. Aber's comments seem to be directed toward the idea that perfect understanding of a system can be developed and expressed as a model. It is only in this latter case that belief in a model would be warranted. Yet our perspective includes a minimalist approach to modeling. That is, modelers should strive to include the least amount of information that adequately explains the phenomena of interest. The term "adequate," of course, is case specific, for what may be appropriate in one case will not necessarily work in another situation. A model can thus be considered a set of hypotheses about the way a system works given certain assumptions and context.

These assumptions are a key part of the modeling process. Aber makes some good points about the value of documenting a model's structure, specifying its parameter values and sources, performing validation, and presenting a sensitivity analysis. Yet the assumptions and sources of uncertainty of a model also need to be set forth. The assumptions determine the level of detail needed in a model and situations to which it can be applied. Uncertainty analysis indicates the influence of a parameter, given the actual variation it represents, on the output variable. Thus, uncertainty analysis complements the sensitivity analysis that Aber calls for. Identifying the sources of uncertainty in a model helps a user know when the limits of the model's applicability have been reached.

The challenge of generating broader use of models is more fundamental than proposing that all modeling papers should contain the components proposed by Aber, in particular, convincing calibration and validation results. In fact, Aber's proposal is not possible in many cases. The empirical information for rigorous calibration or validation commonly is not available.

This challenge does not mean that there is no scientific value in developing simulation models in ecology. The process of a group of scientists collaborating and sharing their expertise to develop a simulation model can be a worthwhile scientific accomplishment, even if a working computer code is not completed (as occasionally occurs). Development of a simulation model is an integrative, interactive, and iterative process (the three "i's"). Simulation modeling is a powerful process for the synthesis of data, theories, and opinions over scales of space, time, and biological organization. It also is a process for creating new insights and questions for new experimental studies. Thus, we agree with Aber's point that new insights and questions emerge even when models in some sense "fail" to meet the expectations of their developers. Nevertheless, the ultimate purpose for many models is to use them in decision making.

Using model projections for decision making

Simulation models are particularly useful in cases where the field, laboratory, and environmental data are not available, not appropriate, or not directly applicable to the decision being made. In these cases, results from simulation models can provide a valuable perspective on alternative decisions. These model results may be needed to complement existing information or to relate extant data to the conditions at hand. However, even when extensive data are available, the complexity of the situation may require a model for interpreting interactions or expanding to larger spatial scales, longer time scales, or higher levels of biological organization.

Effectively used, the model results do not so much mimic data from the real world as reveal our current understanding of the environment. They can provide information regarding what the real world might and could do but not necessarily what it *will* do. In addition, the model results *always* contain uncertainties because they are based on current understanding of interactions and field and laboratory studies. That is why we call model results *projections* (estimates of future possibilities) rather than *predictions* (something that is declared in advance). Therefore, great caution is required in basing decisions solely on model results. Models produce approximations to real situations and

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are only as good as the assumptions they are based on. Because these assumptions are typically case specific, caution must be used in applying a model developed for one circumstance to another situation. This appropriate application of a model has time implications as well. Thus a corollary to a dictum often adopted by modelers that "Reality Is a Special Case" is that Reality (t) ≠ Reality (t + 1). Until information is available to validate the model for the situation at hand, model results should be viewed with caution. Nevertheless, the model results are the logical implications of existing data, produced via a process that assimilates and applies what we do understand.

The caution required in interpreting model calculations is perhaps best illustrated by an example documented by Christensen et al. (1981) and Barnthouse et al. (1984). Under the scrutiny of legal proceedings, two computer simulation models were developed to determine the potential impact of several power plants on fish populations. One model, emphasizing a particular body of understanding, concluded that there would be little impact and that changes in the fish population could be explained by natural factors. The second model, relying on a different understanding of how fish populations interact with their environment, concluded that significant impacts would occur. Both models were subjected to intense scrutiny, but the difference in conclusions remained. The simple fact is that our current level of understanding of complex environmental systems, as reflected in the model, will rarely be adequate alone to provide simple answers to environmental questions. In spite of the limitations to our understanding of complex environmental systems, model projections remain our best source of information for extrapolating limited theory and field and laboratory data to the real-world decision arena.

Conclusion

Aber (1997) proposes "two achievable objectives that could help increase the value of the modeling process in ecological research." His first proposal is to "establish a set of guidelines or standards by which papers presenting modeling results should be judged." His second proposal is to "increase clarity in the understanding of the difference between calibration and validation." These proposals are sound and, to whatever extent implemented, will increase the value of the modeling process in ecological research. However, as discussed above, we feel his perspective on the process of modeling in ecological research inappropriately emphasizes belief rather than understanding. The failings he identifies with models and modelers reflect, in part, unrealistic expectations more than a situation that can or should be changed.

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Mostly A Misunderstanding, I Believe

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 modeling process typically modify the statement of the original hypothesis and possibly the model, thus setting the stage for the next spiral in the cycle.

Aber comments that scientists are "compelled to express our understanding quantitatively," but does not see a role for qualitative information in modeling. In fact, he says, "We should not ... expect [a more qualitative type of activity] to lead to models representing real systems or, more important still, that can be used in policy making" (Aber 1998). However, the very need for decisions to be made and policy actions to be taken in the face of uncertainty forces us to use models that are not perfect.

It is in those instances in which information is deficient that the modeling process may be most useful. Many cases arise in which qualitative information is valuable. In fact, models typically use both qualitative and quantitative information and do not always result in quantitative projections. Modeling the effects of climate change is an example. No one knows how much change in precipitation will alter biota in a given region, but it is still valuable to use models to explore the possible implications of various scenarios of climate change. Such exploration of scenarios informs policy makers about which aspects of the ecological systems they should be most concerned.

Furthermore, a clearcut distinction between qualitative and quantitative information is neither realistic nor appropriate. There is a continuum. Frequently we characterize our confidence about information by using inequalities, such as "greater than" and "less than," or upper and lower bounds. At other times, we may use a rough mean tendency (sometimes called a "guesstimate") to represent a general understanding about some unmeasured quantity (such as the assumption that past windstorms removed 20% of the biomass of impacted forests in New England [Aber and Driscoll 1997]). This type of semiquantitative, or categorical, knowledge is frequently the basis of both parameter values and equations that we use in models.

We agree that calibration and validation, as defined by Aber (1997), are two separate steps, and we call for clarification of their definition and use (Dale and Van Winkle 1998). Often, however, independent data for validation are not available at the time the model is developed. In that case, we use whatever data are already available to calibrate the model, and validation must wait until new information is available. We note, however, that the number of observations for validation is rarely as great as the number of parameters in the typical multiprocess simulation model. Validation is a test of the calibrated model; almost always and not surprisingly, there is some lack of fit between the model projections and the field data. This lack of fit should stimulate a reevaluation of the model, and also an evaluation of the data being used and the questions being asked. Any data set is but one slice of reality, and in ecology there are always legitimate issues about the reliability of the data because of sampling bias, size-selective gear, spatial and temporal aspects of the sampling or testing design, and so forth. Our point is that the modeling process can be an important component of the entire scientific process and not merely a tool to be used after both calibration data and independent validation data are available.

Aber's comments point out the need to clearly define modeling terms such as variables, parameters, observations, calibration, validation, and their relationships. If modelers use these terms in different ways, how can we expect managers or other ecologists to understand discussions of models and their use? The misunderstanding of terms revealed in these interactions with Aber calls for better clarification of the components of simulation models.

In our experience much of the frustration related to ecological models results from unrealistic expectations by all parties involved. Discrepancies often exist between (1) reality and the expectations of those funding or reviewing a model application, concerning how the results should contribute to decision making or advancing ecological understanding, and (2) claims made by modelers at the beginning of a project and those made at the end. These discrepancies arise in part from a lack of understanding of the modeling process on the part of decision makers, marketing on the part of modelers, uncertainty in the model projections, variability in the natural system, immaturity of ecological theory, and factors not included in the model, yet which influence the outcome of decisions. One way to address these frustrating discrepancies is to increase interchange between the modelers and the decision makers. Such *model interactions* can serve only to improve communication and thus create more realistic expectations of the contributions of models.

So where has this discussion on models left us? It has clearly suggested a need for:

- understanding models as part of a process that includes exploration and refinement and not only as final publication,
- greater use of models to help improve ecological understanding,
- standardization of terminology (e.g., calibration, verification, and validation),
- minimizing the gaps between claims, expectations, and the scientifically legitimate roles of models and the modeling process in policy and management.

The challenge continues to be to develop credible models that range the gamut from improving ecological understanding to being useful for decision making.

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