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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