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THE TYRANNY OF COMPLEX SPATIAL MODELS

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As a subject matter editor for various journals, I often arbitrate among reviewers, authors, and editors. Few topics are more reliably contentious than spatial models. Reviewers proximal to data collection and analysis often consider the models ill-constructed, the questions poorly posed, and the results uninformative. Frequent complaints are that 1) the model is too complex, 2) the model leaves out key processes (i.e., it isn't complex enough), 3) the model uses the wrong modeling approach, or the 4) parameters or data are poorly measured, so as the make the modeling at best a thought exercise, and at worst useless. Submitting authors, as modelers, often object that the empiricists don't appreciate the difficulty in modeling, nor the hierarchy of processes represented, nor recognize emergent properties at higher levels in the hierarchy. Models have been both abject failures and wildly successful, and are regularly used in a range of fields, from medicine through insurance to engineering, improving health, building wealth, and improving lives across the globe. Yet there are relatively few, universally-acknowledged examples of successful spatial models. As an empiricist who often models, I recognize the partial truth in various perspectives, and wish to share observations that may help us more effectively evaluate and use spatial models.

First, the best modeling papers clearly state why they modeled. We model for many reasons: to organize knowledge, identify key relationships, identify unknowns, illuminate interactions and feedbacks, expose key sensitivities and thresholds, and to guide future data collection. We sometimes, but only sometimes, model to obtain accurate, specific estimates of future condition. This mis-perception on the part of reviewers, and model users, often leads to a conclusion that a model is useless when it is unable to accurately predict the future for new conditions. Modelers may help ease acceptance of their models by explicitly identifying their goals, for example, using models as a way to build scenarios, bridge gaps in knowledge, and improve planning (e.g., Castella *et al*., 2007). While predicting specific futures is the long-term goal of much modeling, there is much value in modeling on the way there.

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Spatial science has witnessed a proliferation of spatially-explicit predictive models, but the best progress has been made on organized, decades-long efforts spanning many research labs. Modeling papers within this framework are most helpful when they describe how a general problem within the initiative may be solved, rather than just another application of their model to a specific set of conditions. Perhaps the most successful and best examples of this are weather models, with have shown a steady improvement in predictive accuracy, both in time and space (e.g., Simmons and Hollingsworth, 2002). Few members of the research community are interested in the application of a weather model to a new location or set of conditions, but rather, most of the papers are on how new, general approaches work relative to each other or older approaches, the stability of parameters across conditions, how improvements in data quality or frequency in time and space improve model performance, or in characterization of the primary drivers of model uncertainty across a comprehensive range of conditions. These studies take place in a model context, an "ecosystem" of existing code, data, previous studies, and current questions regarding the modeling system. Few problems will have the near-universal appeal or utility of weather prediction, and few organizations will have the resources to maintain and support a unified effort at global scales, but developments over the past decade make long-term, group efforts easier, less expensive, more feasible, and likely. As noted by Olaya (2010), free, open GIS software allows a broader number of participants, self-organizing communities are developing to provide data (Goodchild, 2007), and research laboratories have developed transparent, collaborative GIS and modeling systems, some spatially-explicit (e.g., Seapodym, Lehodey and Senina, 2008, or the Community Surface Hydrology Modeling System, http://csdms.colorado.edu/wiki/Main_Page). Smaller studies in this larger effort can help, but should be clear about where current knowledge frontiers lay, and what specific aspect of model capabilities or behavior the study addresses.

Many spatial modeling systems and studies commit the grave error of over-determination. This is a natural consequence of converting complex, non-spatial models to spatial forms, often at very high spatial resolutions. Unfortunately, this usually leads to intractable rigorous estimation, or nearly arbitrary parameter or initial condition assignment. All models have at least three general components: structure, parameters, and data, and spatial models are particularly difficult to develop because of spatial interactions increase the demands on all three components. Spatial interactions complicate model structure, often introduce new parameters, and require spatially-explicit data across a range of geography.

The Universal Soil Loss Equation (USLE) and successors is a good example of the difficulty of transferring point-based methods to spatial models. USLE and its successors, MUSLE and RUSLE, estimate soil erosion for fields with a characteristic dimensions of 10s of meters, and is based on soils characteristics, climate, slope, vegetation type, and management practices. More than 10,000 plot-years were measured in developing USLE (Nearing & Romkens, 2000), and over 16,000 research papers have been published on some aspect of USLE soil erosion models. Yet many papers have highlighted the difficulty with estimating aggregate soil erosion in spatial versions of the USLE (Tiwari *et al.*, 2000). More complex successors have generally fared worse in model-data comparisons, or the ability to reflect the response behavior of observed systems, at least without extensive calibration.

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In the face of overdetermination, many spatial modelers commit another grave error, a data matching parameter optimization, generally described as calibration. The ills of a reliance on calibration have been described elsewhere (e.g. Jakeman *et al*., 2006), but spatial models are particularly susceptible to spurious calibration. Many spatial representations of complex, point-based models carry all the model complexity to the spatial models, and have tens to hundreds of model parameters per modeling unit, and thousands to hundreds of thousand per study area. Free calibration in such instances is practically guaranteed to provide a model that reproduces observations in any one study. Many spatial models are over-determined, in that there is so much flexibility in the model that a large number of parameter sets can be found to match or nearly match study data. Parameters are optimum in the very narrow sense that they fit the study data quite well, but we should have little confidence in their generality, either for other conditions, or for a slightly different set of data that could plausibly have been collected for the study at hand. Editors, reviewers, and readers should be skeptical of modeling papers that collect, calibrate, run, and report.

There are several preferable alternatives. One is a modeling framework without calibration. Model parameters are estimated independent of the study at hand, or generally (e.g., Aber *et al*., 1996). Alternately, if some model fitting is conducted, we should require a more thorough testing of model fit, for example bootstrapping, leave-one-out model fits, independent data sets, or y-scrambling can all provide information on model response and the relative importance of model parameters, at least with the data set collected.

However, the best way forward is often the application of one of the comprehensive uncertainty analysis methods developed and more widely applied over the past decade. These methods include General Sensitivity Analyses (GSA, Ratto *et al*., 2001), Generalized Likelihood Uncertain Estimation (GLUE, Beven, 1992), and Monte-Carlo Markov-Chain analysis (MCMC, Richardson *et al*., 2007). These and similar approaches asses model parameters and data across a full-range of plausible parameter and data space. These methods provide an evaluation of predictive influence of data and parameter values, can identify important parameter ranges, relative uncertainties and interactions among parameters, and sensitivities of model outputs to improvements in parameter estimation and data quantity and quality. These methods can identify key relationships, and help specify the range of conditions under which scenarios should be run.

In summary, we should routinely apply developed methods to more comprehensively test our models, and detect and reduce overfitting. Where appropriate, we should consider models as broader collaboratives, and work to develop them across the broadest set of scientists and users, extending, improving, and testing the limits of their ability to distill our current knowledge about a system, and guide the development of new knowledge.

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