

The Gap between Simulation and Understanding in Climate Modeling

BY ISAAC M. HELD

Should we strive to construct climate models of lasting value? Or should we accept as inevitable the obsolescence of our models as computer power increases?

THE NEED FOR MODEL HIERARCHIES.

The complexity of the climate system presents a challenge to climate theory, and to the manner in which theory and observations interact, eliciting a range of responses. On the one hand, we try to *simulate* by capturing as much of the dynamics as we can in comprehensive numerical models. On the other hand, we try to *understand* by simplifying and capturing the essence of a phenomenon in idealized models, or even with qualitative pictures. As our comprehensive models improve, they more and more become the primary tools by which theory confronts observations. The study of global warming is an especially good example of this trend. A handful of major modeling centers around the world compete in creating the most convincing climate simulations and the most reliable forecasts of climate change, while large observational efforts are mounted with

the stated goal of improving these comprehensive models.

Due to the great practical value of simulations, and the opportunities provided by the continuing increases in computational power, the importance of understanding is occasionally questioned. What does it mean, after all, to understand a system as complex as the climate, when we cannot fully understand idealized nonlinear systems with only a few degrees of freedom?

It is fair to say that we typically gain some understanding of a complex system by relating its behavior to that of other, especially simpler, systems. For sufficiently complex systems, we need a model hierarchy on which to base our understanding, describing how the dynamics change as key sources of complexity are added or subtracted. Our understanding benefits from appreciation of the interrelationships among all elements of the hierarchy.

The importance of such a hierarchy for climate modeling and studies of atmospheric and oceanic dynamics has often been emphasized. See, for example, Schneider and Dickinson (1974), and, especially, Hoskins (1983). But, despite notable exceptions in a few subfields, climate theory has not, in my opinion, been very successful at hierarchy construction. I do not mean to imply that important work has not been performed, of course, but only that the gap between

AFFILIATIONS: HELD—NOAA/Geophysical Fluid Dynamics Laboratory, Princeton, New Jersey

CORRESPONDING AUTHOR: Isaac M. Held, NOAA/GFDL, P.O. Box 308, Princeton, NJ 08534

E-mail: isaac.held@noaa.gov

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comprehensive climate models and more idealized models has not been successfully closed.

Consider, by analogy, another field that must deal with exceedingly complex systems—molecular biology. How is it that biologists have made such dramatic and steady progress in sorting out the human genome and the interactions of the thousands of proteins of which we are constructed? Without doubt, one key has been that nature has provided us with a hierarchy of biological systems of increasing complexity that are amenable to experimental manipulation, ranging from bacteria to fruit fly to mouse to man. Furthermore, the nature of evolution assures us that much of what we learn from simpler organisms is directly relevant to deciphering the workings of their more complex relatives. What good fortune for biologists to be presented with precisely the kind of hierarchy needed to understand a complex system! Imagine how much progress would have been made if they were limited to studying man alone.

Unfortunately, Nature has not provided us with simpler climate systems that form such a beautiful hierarchy. Planetary atmospheres provide insights into the range of behaviors that are possible, but the known planetary atmospheres are few, and each has its own idiosyncrasies. Their study has connected to terrestrial climate theory on occasion, but the influence has not been systematic. Laboratory simulations of rotating and/or convecting fluids remain valuable and underutilized, but they cannot address our most complex problems. We are left with the necessity of constructing our own hierarchies of climate models.

Because nature has provided the biological hierarchy, it is much easier to focus the attention of biologists on a few representatives of the key evolutionary steps toward greater complexity. And, such a focus is central to success. If every molecular biologist had simply studied his or her own favorite bacterium or insect, rather than focusing so intensively on *E. coli* or *Drosophila melanogaster*, it is safe to assume that progress would have been far less rapid.

It is emblematic of our problem that studying the biological hierarchy is experimental science, while constructing and studying climate hierarchies is theoretical science. A biologist need not convince her colleagues that the model organism she is advocating for intensive study is well designed or well posed, but only that it fills an important niche in the hierarchy of complexity and that it is convenient for study. Climate theorists are faced with the difficult task of both constructing a hierarchy of models and somehow focusing the attention of the community on

a few of these models so that our efforts accumulate efficiently. Even if one believes that one has defined the *E. coli* of climate models, it is difficult to energize (and fund) a significant number of researchers to take this model seriously and devote years to its study.

And yet, despite the extra burden of trying to create a consensus as to what the appropriate climate model hierarchies are, the construction of such hierarchies must, I believe, be a central goal of climate theory in the twenty-first century. There are no alternatives if we want to understand the climate system and our comprehensive climate models. Our understanding will be embedded within these hierarchies.

THE PRACTICAL IMPORTANCE OF UNDERSTANDING. Why should we care that we do not understand our comprehensive climate models as dynamical systems in their own right? Does this matter if our primary goal happens to be to improve our simulations, rather than to create a subjective feeling of satisfaction in the mind of a few climate theorists?

Suppose that one can divide a climate model into many small, distinct components and that one can test and develop each of these modules in isolation. If the components have been adequately tested, is there any need for an understanding of what happens when they are coupled? To the extent that one can break down the testing process into manageable pieces, this bottom-up, reductive model development strategy is without doubt appropriate and efficient. Understanding is needed at the level of the module in question, so as to ensure its fidelity to nature, but is the understanding gained at a higher, more holistic level valuable to the climate modeling enterprise? Are we better off limiting ourselves to trying to understand particular physical processes of climatic relevance?

The radiation code in atmospheric models (the clear-sky component, at least) is a good example. The broadband computations used in climate models are systematically tested against line-by-line computations based on the latest laboratory studies and field programs. When atmospheric observations and/or laboratory absorption studies require a modification to the underlying database, this new information makes its way eventually into the broadband climate model codes.

However, we are very far today from being able to construct our comprehensive climate models in this systematic fashion. Despite several major observational campaigns designed to guide us toward appropriate closures for deep, moist convection, little consensus exists as to the best formulations for

climate models. Systematic use of cloud-resolving simulations, as a middle ground between closure schemes and observations, promises to improve this situation, but there is still a long way to go.

When a fully satisfactory systematic bottom-up approach is unavailable, the development process can be described, without any pejorative connotations intended whatsoever, as engineering, or even tinkering. (Our most famous inventors are often described as tinkerers!) Model builders put forward various ideas based on their wisdom and experience, as well as their idiosyncratic interests and prejudices. Model improvements are often the result of serendipity rather than systematic analysis. Generated by these informed random walks, and being evaluated with different criteria, the comprehensive climate models developed by various groups around the world evolve along distinct paths.

The value of a holistic understanding for model development is in making this process more informed and less random, and, thereby, more efficient. To the extent that we understand which aspects of a moist convection scheme are most important for exaggerating the double ITCZ in the east Pacific, our attempts to ameliorate the double ITCZ in our comprehensive model will be that much less random and more informed.

A holistic understanding of climate dynamics also helps us relate one comprehensive model to another. To the extent that we have some understanding of which aspects of convection schemes, boundary layer models, or gravity wave drag formulations matter for various aspects of our climate simulations, we can appreciate why one simulation is better than another without laboriously morphing one model into the other. We should then be able to take advantage more efficiently of the successes of other models.

The climate simulation community does organize itself to perform a large variety of Climate Modeling Intercomparison Projects (CMIPs), with those underlying the Intergovernmental Panel on Climate Change (IPCC) being the best known. By avoiding unnecessary differences between calculations, and getting different groups to compare results carefully, these projects teach us which aspects of simulations are robust and which are not. CMIPs that involve integrating the models with idealized boundary conditions (e.g., the Aqua-Planet Experiment Project, www-pcmdi.llnl.gov/projects/amip/ape) should help in relating high-end simulations and more idealized models. But, comparisons of complex models that are not easily morphed into each other, even if forced with idealized boundary conditions, invariably leave us

without a satisfactory understanding of why models differ. We must simplify the models as well as the boundary conditions.

FILLING THE GAP. Even the simplest levels of the hierarchies that I have in mind are turbulent and chaotic models that one cannot hope to understand in all detail. This is not meant to imply that even simpler models do not have important roles to play. But, complexity in the climate system prevents us from generating convincing simple quantitative theories for many of the questions that interest us. My concern here is with models that attack some of the core sources of complexity in the climate system, allowing us to address questions of climate maintenance and sensitivity, and that cannot be fully solved by an individual researcher but, rather, require the concerted efforts of a variety of investigators.

No individual or small committee can decide what the appropriate model hierarchies are; rather, models must prove themselves over time, and as they do so hierarchies, ideally, emerge naturally. I give one atmospheric example of the kind of model that I have in mind, designed to help close the gap between idealized modeling and high-end simulations.

Frierson et al. (2005, manuscript submitted to *J. Atmos. Sci.*) describe an idealized model configured to study the interactions between moist convection and the large-scale circulation. It solves for the flow of an ideal gas on a rotating sphere, and contains gray radiative transfer that is a function only of temperature, a highly simplified boundary layer mixing scheme with a prognostic boundary layer depth, a series of different convective closures, but no condensate, and a homogeneous surface. Moist processes complicate climate models through the effects of latent heat release on atmospheric circulations, and through the effects that the vapor and condensate have on radiative heating. Here, the latter are ignored so as to isolate the direct effects of latent heat release. The different panels in Fig. 1 show snapshots of the rate of precipitation using three distinctly different convection schemes. We need to understand which aspects of convective closures control the disparate structures of the intertropical convergence zone, seen in these three panels. We also need to ensure that we can generate results such as these in a robust and reproducible manner. There may very well be details of implementation that we tend to ignore in documenting our models that affect these solutions. Can we obtain reproducible results on how simulated tropical storm climatologies depend on model parameters and on resolution?

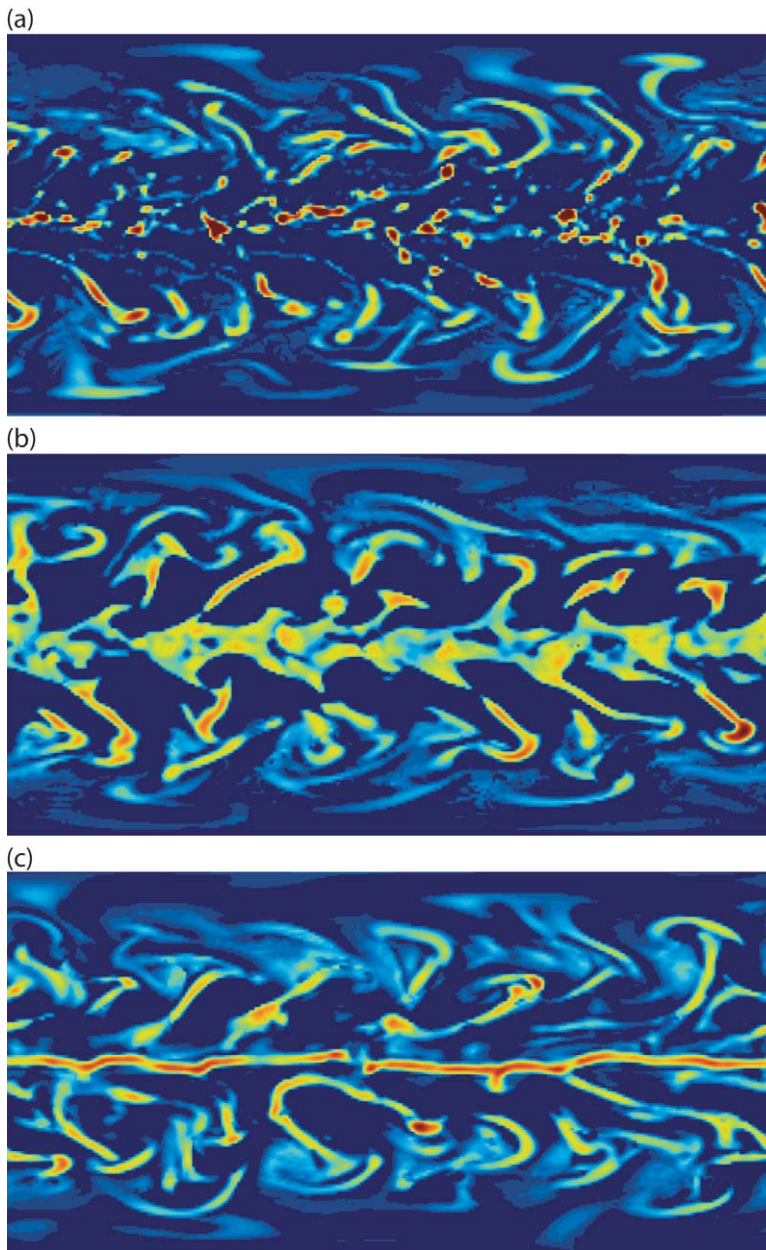


FIG. 1. Instantaneous precipitation over the globe from three versions of an idealized moist general circulation model, using three convective closures. [Courtesy of D. Frierson.]

This particular dynamical framework is surely not optimal, but I am convinced that clearly defined idealized models of this sort are urgently needed. We especially need extensions of this kind of model focusing on cloud simulations and feedbacks. The literature on the role of clouds in determining climate sensitivity in comprehensive models is extensive, but also confusing. Idealized models with reproducible cloud feedbacks would be an important addition to the tools we use to study this problem, helping us sort out the results from comprehensive models

that are dependent on details of implementation from those that are more robust.

On a personal note, I have devoted a large part of my own career to the study of the general circulation of the atmosphere using models that ignore water vapor and the release of latent heat. For example, I have returned repeatedly to Phillips' (1956) original general circulation model, or, as we would refer to it today, the two-layer quasi-geostrophic model of a statistically steady baroclinically unstable jet on a β plane, forced by linear radiative damping and linear surface friction. In fact, this model is still my choice for the *E. Coli* of climate models. I have also explored the dynamics of simple dry primitive equation models on the sphere. By encouraging work with idealized models of the *moist* general circulation, my intention is not to deemphasize research on dry general circulation models, but rather to reenergize it. It is only by creating intermediate stepping stones that we can take the insights from these dry models and confidently relate them to the behavior of our comprehensive models. In order to fully understand the very important poleward movement of the midlatitude low-level westerlies as climate warms, I am confident that we will need to map out much more thoroughly how the position of the westerlies is determined in both moist and dry idealized climate models.

The models described produce climates that are independent of longitude, but each can be modified to create zonal inhomogeneities. There have been bursts of activity in the past involving idealized models of the zonally asymmetric climatic response to orography and land-ocean geometry. We need another sustained burst, taking advantage of increases in computational power, building on past progress and careful studies of models with zonally symmetric climates, focusing, in part, on how the storm tracks and stationary waves mutually shape each other, as well as on the interactions between land surfaces and

the hydrological cycle, and hopefully contributing to a firmer foundation for the analysis of regional climate change.

The possibilities for interesting hierarchies multiply as we consider coupled atmosphere–ocean–cryosphere models, and as we take advantage of the computational breakthroughs, allowing for the explicit resolution of deep convection in the atmosphere and mesoscale eddies in the ocean. This multiplicity of hierarchies emphasizes the difficulty of our task.

THE FUTURE OF CLIMATE THEORY.

Accepting that the construction and analysis of climate model hierarchies yields important understanding, and given the many efforts underway devoted to models of various levels of complexity, can this effort be made more productive? I highlight two related tendencies that have slowed the systematic development of climate model hierarchies. (My own work illustrates these tendencies nicely, and this discussion is as much a self-critique as it is one of the field more generally.)

Elegance versus elaboration. Our goal must be to reduce the number of idealized models that we analyze. Otherwise, we are left with a string of more or less interesting results, few of which have been intensively examined by more than 2 or 3 people, and that we never quite manage to relate to each other. Furthermore, when models are underanalyzed and not reproduced by others, we are never certain that the computations have been performed properly. But how can this inefficient deployment of our theoretical resources be avoided? The key, I feel, is *elegance*.

An elegant model is only as elaborate as it needs to be to capture the essence of a particular source of complexity, but is no more elaborate. Many of our models are more elaborate than they need be, and this is, I believe, the prime reason why it is difficult for the field as a whole to focus efficiently on a small number of models. If a particular scheme seems unnecessarily baroque, why should I use it as a basis for my own research? What lasting value will my study have?

We all want our work to be relevant to the big issues in climate dynamics. This relevance requires a certain level of realism in one's simulations, and pressure to reach the required level of realism pushes models toward ever-increasing elaboration. Yet, in the process, one's model often loses much of its attraction to other researchers, who may not be in agreement with all of the choices made in the process of elaboration.

We justify our research, to ourselves and others, by claiming some mixture of short-term practical consequences and lasting value. High-end simulations are primarily driven by the need to meet practical applications, requiring them to be as realistic as possible. These simulations need not be of lasting value, because they will be supplanted by more comprehensive models as computer resources increase. When global nonhydrostatic atmospheric models resolving deep moist convection become common in future decades, today's global warming simulations will be of historical interest only. But the importance of the problem is such that we cannot wait for this to occur; we need to do our best now, knowing full well that these efforts will be obsolete within most of our lifetimes. While there is no value in elaborating these comprehensive simulations in ways that have no practical consequences or no hope of confronting data, an emphasis on elegance can be counterproductive; a large number of details may very well be needed to get a useful simulation.

As we back off from this high end, the balance between elegance and realism becomes more of an issue. My reading of the literature is that elegance is often sacrificed unnecessarily, primarily for the sake of competition with comprehensive models. The latter seem, after all, to be extraordinarily inefficient at attacking many key climate problems. Yet, in an era of exponentially increasing computation power, this competition is often less valuable than we might like to admit, given the time scale at which studies become feasible at a more comprehensive level.

It may very well be that in addition to needing fewer idealized climate models, we need a larger number of comprehensive models! Given the large number of choices that must be made in the construction of a comprehensive climate model, and the complexity of the metrics that one is trying to optimize, there is clearly value in trying to sample more widely in the space of possible models. Given the difficulty of creating a single model, both in human and computational resources, this seems paradoxical, but the efforts at the Hadley Centre at creating an ensemble of climate models (Murphy et al. 2004) are encouraging in this regard. Favoring a large number of such models is consistent with the claim that a given comprehensive model is not constructed with lasting value as the primary goal.

Elegance and lasting value are correlated. An elegant hierarchy of models upon which the field as a whole bases its understanding of the climate system can be of benefit to future generations for whom our comprehensive simulations, valuable as they are at present, will have become obsolete.

Conceptual research versus hierarchy development. A theoretically inclined researcher might design and build a model for a particular purpose and then discard the model. The model is not intended, in many cases, to have life of its own, but is, rather, a temporary expedient. In the limiting case, the model is not fully described and the result not fully reproducible. Or, an existing model might be used in the same way, but with the focus on the concept, not on the model itself. I refer to this as *conceptual* research. Much of the best work with comprehensive models can be classified as conceptual, as can, for example, much of the paleoclimatic research with computationally efficient climate models of intermediate complexity. In this context the model is a useful tool that helps one think about the system and search for ways in which to interpret observations.

Some might argue that all modeling is conceptual in this sense, that all models are just expedient tools and not themselves the final goal, and that individual models never deserve to be thought of as having lasting value. Given the level of complexity that we face in the climate problem, I do not think that this

is a viable perspective. Without the solid foundation provided by the careful study of appropriate model hierarchies, there is a danger that we will be faced with a babel of modeling results that we cannot, in any satisfying way, relate to one another. We must try to create models of lasting value, in addition to facilitating conceptual research. Ideally, we need some models of intermediate complexity that we take just as seriously as do the biologists who map out every single connection in the nervous system of the snail.

CONCLUDING REMARKS. The health of climate theory/modeling in the coming decades is threatened by a growing gap between high-end simulations and idealized theoretical work. In order to fill this gap, research with a hierarchy of models is needed. But, to be successful, this work must progress toward two goals simultaneously. It must, on the one hand, make contact with the high-end simulations and improve the comprehensive model development process; otherwise, it is irrelevant to that process, and, therefore, to all of the important applications that are built on our ability to simulate. On the other hand, it must proceed more systematically toward the creation of a hierarchy of lasting value, providing a solid framework within which our understanding of the climate system, and that of future generations, is embedded. Funding for climate dynamics should reflect this need to balance conceptual research, simulation, and hierarchy development.

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