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The Relevance of Theory for Contemporary Research in Atmospheres, Oceans, and Climate

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Key Points:
- Theory remains an essential cornerstone of geophysical sciences
- Researchers should resist the temptation to prioritize matching models to observations over getting the physics right
- Parameterizations firmly grounded in sound theory and tested against observations should remain a vital part of global modeling for some time

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Abstract Dealing with large data sets and complex computational codes demands increasing time and effort by researchers in atmospheres, oceans, and climate. The author argues for a more balanced approach to using models, observations, and theory to advance basic understanding.

Plain Language Summary As is becomes easier to undertake complex computer simulations of climate and weather, and as large volumes of satellite data become more available, it is tempting to use computers to simulate, rather than understand, nature. The author argues that simulation without understanding imperils scientific progress and, paradoxically, may impede the development of better models.

Advances in atmospheric science, physical oceanography, and climate science have relied on three fundamental approaches to natural science: observations (including those made during field experiments), theory, and modeling. Laboratory experimentation is also important in geophysical fluid dynamics, in atmospheric chemistry, and in biogeochemistry.

The last few decades have seen rapid expansion of satellite-based remote sensing, the speed and capacity of supercomputers, and the size and complexity of ocean, climate, and weather models. This has transformed both the nature of research in these fields and applications such as weather forecasting and climate prediction. Whereas just 40 years ago research was largely a matter of collecting and analyzing observations and interpreting them through hypotheses and theories, today's graduate students, postdoctoral fellows, and professional scientists spend much of their time dealing with often enormous data sets produced by remote sensing and numerical models as well as analyzing and debugging complex programs and setting up numerical experiments. While many graduate programs continue to offer instruction in basic theory of, for example, geophysical fluid dynamics, radiative transfer, convection, and biogeochemistry, today's students must also learn how to manipulate very large data sets and deal with computer codes written in several different languages and whose complexity often exceeds the ability of any single individual to comprehend. These developments have gone hand-in-hand with a tendency for research to be conducted by teams of scientists rather than one or two individuals. This evolution was accompanied by a rapid expansion of research output, as measured by the number of papers published and by the quality of numerical weather forecasts, atmosphere and ocean data assimilation, climate models, and other applications. By any measure, the last half century has been a sensational success story for our disciplines.

Yet there are signs of some weaknesses in this endeavor, and it is natural to focus on these to further accelerate progress. Some that come to mind include our poor understanding of the processes leading to the generation of tropical cyclones, the underlying physics of the Madden-Julian Oscillation and equatorially trapped disturbances, the nature and importance of vertical mixing in driving the circulation of the oceans, the possible influence of stratospheric processes on weather, and the role of clouds in climate and climate change. On longer time scales, we lack a comprehensive understanding of such essential processes as the behavior of large ice sheets and the carbon cycle in general. All of these problems are being addressed by many scientists, and on that basis one might be optimistic that progress will be rapid. But I believe that there are serious issues about how to approach many of these problems and have reservations about whether the current approach is optimal.

In a nutshell, the issues revolve around the temptation to prioritize matching models to observations over getting the physics right and to regard model improvement as largely a matter of increasing spatial resolution. Moreover, it is sometimes perceived as easier to run the model than to develop a comprehensive and satisfying understanding of the phenomena in question. In some subdisciplines, the traditional scientific goal of understanding nature is being subordinated to the desire and need to improve models. Paradoxically, this change in emphasis may also be impeding model improvement.

I offer a concrete example. Since the introduction of moist adiabatic adjustment by Syukuro Manabe and his colleague in the mid-1960s, great strides have been made in the development of representations of subgrid-scale cumulus convection for use in numerical models. But over time, many of the problems of climate and weather models have been blamed on the way convection is represented by these schemes. This is understandable as such schemes contain many parameters some of which are poorly constrained by observations, and the conditions for the well-posedness of the schemes are often violated.

Errors in the spatial and temporal distribution of convective rainfall are often blamed on the convection scheme. In one case in which I was involved, an otherwise good global model failed to produce an adequate Walker Circulation … a thermally direct circulation along the equator in the Pacific region. After spending months tinkering with the convection scheme and trying different schemes, it was finally determined that the problem arose from the failure to account for the effect of subgrid-scale turbulence on surface heat fluxes in the far western Pacific, where the model-resolved surface winds are often light. Once this was corrected, a reasonable Walker Circulation was obtained.

Months of time and effort could have been saved had the modelers been better versed in the quasi-equilibrium theory of moist convection. Convection in the tropics is largely driven by surface fluxes, and thus surface fluxes should be among the first suspects in the hunt for inadequate convective precipitation.

Another example is the failure of many models to simulate with any real fidelity the Madden-Julian Oscillation (MJO), a 30–50 day eastward-propagating disturbance near and along the equator accompanied by large variations of moist convection. Naturally, convective representations were on the top list of suspects, and much time and effort have been spent tampering with convective schemes. Some studies found that increasing the sensitivity of such schemes to mid-level atmospheric humidity produced a better MJO even though other measures of model fidelity were usually degraded. As I will argue presently, this may prove to be an example in which the desire to produce a good simulation led us away from correctly identifying the underlying physical defects of the convection schemes.

About 20 years ago, modelers frustrated with what they viewed as the failure of convective representations began to experiment with models that resolved or partially resolved convection itself. This began with the deployment of "superparameterizations," two-dimensional, convection-permitting models embedded within each grid cell of a larger-scale model that did not itself resolve convection. Global models running such schemes were found to simulate the MJO much better than did the same models running classical convection schemes. More recently, it has been possible to run convection-permitting models at a global scale, and these too show much improvement in the simulation of the MJO and other intraseasonal variability near the equator.

The success of convection-permitting models has led more than a few scientists to push for rapid migration away from models that reply on representations of convection to those that simulate that process explicitly, even if crudely. To do this effectively for climate problems will require exascale computing. (Bear in mind that with the current technology, an exaflop computer will require about 0.5 GW of power, the output of a small nuclear power plant.)

Is this really necessary? I think not. To begin with, moist convective clouds have dimensions on the order of 1–10 km and life spans of a few tens of minutes to a few hours. To accurately simulate them would require grids with cell spacing of around 100 m or less, far from what can be achieved today at the global level, even with exascale computing. So today's "cloud resolving models" are anything but, with typical grid spacings of a few kilometers. (These are more accurately described as "cloud-permitting" models.) More fundamentally, if the ensemble of convective clouds is close to being in a state of statistical equilibrium with the larger scales one is usually trying to predict, then one *should* be able to represent them by relating their statistics to their larger-scale environment. That is, they should be "parameterizable." But if the ensemble of convective clouds is far from statistical equilibrium, then almost by definition the particulars of individual convective events will have a noticeable influence on the larger scales. In that case, resolving the convection would not avoid the large resulting uncertainty in the prediction of the larger scales and one would be forced to run a large ensemble of convection-permitting simulations to quantify such uncertainty. This is not currently feasible at the global level and probably will not be for some time.

In short, why spend much effort and resources resolving that which cannot be predicted if one can predict the statistics of that which cannot be resolved? In our quest for accurate simulations, are we computing too much and thinking too little?

Assuming that the general behavior of the MJO does not depend on the chance spatial and temporal arrangement of individual convective cells, researchers should want to know why convective representations fail where those models that explicitly (albeit crudely) simulate convective entities succeed. Here is a case where theory could provide a satisfying explanation and also help us design better representations. There is no shortage of theories for the MJO, and in recent years they have converged on the idea that it is driven by modulation of infrared radiation by high clouds produced by deep convection. Yet the designers of convective representations have paid scant attention to how deep convection couples with high clouds. This provides an obvious path forward for those brave souls who are not yet willing to give up on representing stochastic, small-scale processes in terms of larger-scale variables.

Sometimes the quest for better simulations subordinates even simple physics. About 20 years ago I pointed out that most models of that era neglected to turn dissipated kinetic energy back into heat. For most atmospheric phenomena, this is indeed a small term in the thermodynamic energy budget (though technically required to close any net energy budget), but in strong windstorms like hurricanes, it becomes important. Moreover, no substantial computational benefit accrues from neglecting it. A few weeks later, a researcher came to me to report that he had added this term to his model and found that it made simulated hurricanes too intense, so he took it out again. (He had also ignored the interaction of his model storms with the upper ocean, an effect that would have weakened them.) For this researcher, getting the "right answer" was the goal, even if it is obtained for the wrong reasons. Still today, the conversion of dissipated kinetic energy back into heat remains an optional switch (whose default position is "off") in a state-of-the-art hurricane prediction model.

It strikes me that our fields advance most rapidly and in a most satisfying way when the development of observations, theory, and models march forward together. Historically, observations, and more recently models, have generally preceded theory. For example, cyclones were discovered hundreds of years before successful theories were developed. The spontaneous aggregation of deep moist convection was discovered in idealized model simulations and only afterward explained theoretically. In our field, theoretical predictions rarely precede observations.

Even though theory usually lags observations and models, it can help advance both as well as provide the conceptual understanding that is so central to science. For example, the theoretical understanding of spontaneous aggregation discovered in curiosity-driven, idealized models tells modelers what they have to get right to succeed in simulating aggregation, an example of which turns out to be the MJO. (The defect in convective schemes that prevents accurate simulation of the MJO may have more to do with coupling to high clouds than to mid-level moisture.) As another example, theory has been used to develop strategies for targeting observations in aid of weather forecasts.

The current desire to sidestep theory, codified in parameterizations, in favor of brute-force resolution of highly unpredictable small-scale processes almost certainly grew out of a frustration that theory has failed to catch up with progress in models and observations. But that does not mean we should abandon the attempt; indeed, to do so is to invite what may prove to be an unnecessary and wasteful expenditure of resources, and worse, the subordination of understanding to mere simulation.

So what is to be done? First and foremost, we must resist the wholesale migration of atmospheric, oceanic, and climate science away from a traditional curiosity-driven scientific endeavor to the more strictly applied venture of predicting weather and climate. This will become increasingly difficult as funding, at least in the United States, continues to shift away from open-ended basic research to work that yields short-term practical gains. Even the most pragmatic minded among us should recognize that long-term progress depends as much on the rare leaps forward made possible by basic research as it does on the far more common incremental gains from application-oriented work. We need to strike a balance between these two approaches.

Perhaps the greatest challenge is in the classroom. Whereas a generation ago students were made familiar with the observed behavior of the atmosphere, oceans, and climate and taught both the fundamental underlying equations and simplified theory that leads to conceptual understanding, today we must also teach them to deal with extraordinarily large data sets of remote sensing observations and model output and to design, run, and analyze computational models which have become an essential tool in advancing understanding. These new tasks are so demanding that they risk displacing some of the more foundational underpinnings of our science. But without those underpinnings, we are in danger of evolving into a data-driven world in which most intelligence is artificial and simulation has replaced curiosity as the driving force of our science.

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