IMPROVING THE SCIENTIFIC FOUNDATION FOR Atmosphere-Land-Ocean Simulations

REPORT OF A WORKSHOP

Committee on Challenges in Representing Physical Processes
in Coupled Atmosphere-Land-Ocean Models

Board on Atmospheric Sciences and Climate

Division on Earth and Life Studies

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Support for this project was provided by the National Science Foundation and the National Aeronautics and Space Administration under Grant No. ATM-0135923 and the National Oceanic and Atmospheric Administration under Contract No. 52-DGNA-1-90024. Any opinions, findings, and conclusions or recommendations expressed in this publication are those of the author(s) and do not necessarily reflect the views of the agencies that provided support for this project.

International Standard Book Number 0-309-09609-X (Book)

Additional copies of this report are available from the National Academies Press, 500 Fifth Street, N.W., Lockbox 285, Washington, D.C. 20055; (800) 624-6242 or (202) 334-3313 (in the Washington metropolitan area); Internet, http://www.nap.edu.

Printed in the United States of America

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Preface

The majority of studies conducted by the Board on Atmospheric Sciences and Climate (BASC) are requested by government organizations or Congress. However, each year the members of BASC select a topic for special study (often called our summer study). These summer workshops provide an informal atmosphere where scientists and agency staff can talk frankly about current issues. BASC picks topics that serve agency needs but that might not be done otherwise. Sometimes case study approaches are used to highlight lessons learned about some practical problem, like communicating weather information accurately (NRC, 2003). Other times, as in this report, an issue is selected that is highly technical, philosophical, or forward looking and thus beyond what agencies often address given their immediate priorities.

For the 2004 summer workshop, BASC decided to explore the challenges in representing physical processes in coupled atmosphere-land-ocean (A-L-O) models. Modeling is a fundamental part of the infrastructure of the atmospheric and climate sciences, and progress in developing and testing physical parameterizations to accurately represent Earth's system is critical. The goals of this workshop were to identify physical processes that are poorly parameterized; explore impediments to modeling efforts, including issues related to how research is done and how the next generation of modelers is being developed; and discuss ways to invigorate model development and evolution.

To explore these issues, a five-person steering committee organized a workshop held July 12-13, 2004, at the J. Erik Jonsson Woods Hole Center in Woods Hole, Massachusetts. Nearly 30 people attended from various academic and research institutions, federal agencies, and the private sector, including 12 invited presenters and discussion leaders (see Appendix D for the workshop agenda and Appendix E for the participant list). All the invitees brought a specific area of modeling expertise to the table, but their perspectives and experience as members of the atmospheric and climate science communities were as important as their scientific proficiency. Other members of the community attended as well, each lending his or her own additional expertise to the discussions.

Although all the BASC summer workshops are organized to encourage interaction and discussion, this workshop was designed to be even more discussion-oriented than usual to allow participants to identify and explore a wide variety of issues. The workshop included two overarching presentations that challenged the participants to think creatively, and each session began with comments from a discussion leader. But most of the time was devoted to discussion and interaction, during which committee members served as rapporteurs to capture all the key points. This workshop report recapitulates those discussions, presenting a broad look at the science of geophysical modeling, some impediments to progress, and ideas for invigorating this field. Following regular National Academies rules for this type of workshop, this report does not contain consensus findings or recommendations. Rather it is a representation of the discussions that occurred during the workshop; the report presents the opinions of the participants but not necessarily the views of the committee.

The National Academies and BASC wish to thank the committee members—Kerry Emanuel, James McWilliams, David Randall, John Wyngaard, and Yuk Yung—for organizing this workshop and synthesizing the two-day discussion into this report. Thanks also are given to all the speakers and participants who contributed their time and energy to this workshop, to BASC program officer Julie Demuth for her leadership, and to BASC administrative coordinator Diane Gustafson for her tireless efforts. This BASC workshop sparked particularly animated discussion, and BASC hopes this report adequately conveys the discussions and encourages others to think about the challenges of ensuring that the atmospheric and climate communities are developing robust, accurate models.

Chris Elfring, Director, and Robert Serafin, Chair
Board on Atmospheric Sciences and Climate

Acknowledgments

This report has been reviewed in draft form by individuals chosen for their diverse perspectives and technical expertise, in accordance with procedures approved by the National Research Council's Report Review Committee. The purpose of this independent review is to provide candid and critical comments that will assist the institution in making its published report as sound as possible and to ensure that the report meets institutional standards for objectivity, evidence, and responsiveness to the study charge. The review comments and draft manuscript remain confidential to protect the integrity of the deliberative process. We wish to thank the following individuals for their review of this report:

Christopher Bretherton, University of Washington, Seattle
Raffaele Ferrari, Massachusetts Institute of Technology, Cambridge
Julian Hunt, University College London, England
Bjorn Stevens, University of California, Los Angeles

Although the reviewers listed above provided many constructive comments and suggestions, they were not asked to endorse the conclusions or recommendations offered by the speakers, nor did they see the final draft of this report before its release. The review of this report was overseen by Eric Barron, Pennsylvania State University. Appointed by the National Research Council, he was responsible for making certain that an independent examination of the report was carried out in accordance with institutional procedures and that all review comments were carefully considered. Responsibility for the final content of this report rests entirely with the authoring committee and the institution.

Contents

Summary

The 2004 Board on Atmospheric Sciences and Climate summer workshop was designed to explore challenges in representing physical processes in coupled atmosphere-land-ocean models. Participants discussed both the science of model parameterizations, including several key processes for which they believe improvement is necessary, and broader issues termed “cultural” because they are thought to be entrenched in the customs and structure of the atmospheric, climate, and oceanographic communities.

Many workshop participants agreed that progress in model development is being impeded, and they identified several likely contributors to this situation, many of them cultural:

- Widely available, easily run models and the current funding and academic environments may be turning both graduate students and their faculty advisors toward fast-turnaround research in numerical simulation and away from the traditional but much slower path of theory and observation.
- Bright young scholars best suited to tackling scientific problems may incorrectly perceive that the atmospheric and oceanic sciences are an applied field whose goal is merely to improve weather and climate forecasts.

- Progress in parameterization, which often requires interactions across traditional disciplinary boundaries, could now be inhibited by the compartmentalization of educational, research, and funding institutions.
- The rigidity of long-existing models and the lack of efforts to remove inferior or flawed physical representations hinder progress by preventing opportunities for new, fresh thinking.

Because the development of parameterizations for use in numerical models has become a fundamental part of the atmospheric sciences, climate, and oceanography, these trends could cloud the future of the fields. As such, many workshop participants believe there is a need to recognize, accommodate, and foster model parameterization science to ensure continued progress in model development.

1

Workshop Context

At the dawning of the age of numerical simulation in the 1960s, few imagined the influence it would have on the atmospheric and oceanic sciences. Today, two generations later, numerical models of the atmosphere and the ocean are central to weather prediction, research, and education. The size and speed of computers and their computational and data-handling techniques have improved enormously; today we can do fast, inexpensive computations of a breathtaking range of fluid-mechanical phenomena.

Great strides have been made over the past few decades in our understanding of the atmosphere and ocean, in our modeling capabilities, and in numerical simulations of the atmosphere, ocean, and land surface. Yet current representations of unresolved processes in the models tend not to adequately represent our knowledge of the underlying physics. Moreover, there is evidence that further progress in numerical simulations is being impeded by the slow pace of improvement in the representation of certain key physical processes in the models. In the arena of weather forecasting, this is manifest in the discovery that there is somewhat greater divergence among the various different operational numerical prediction models than there is among ensemble members of the same model created with different initial conditions (Mylne et al., 2002). At the same time, climate modelers must deal with substantial disagreement among models in their prediction of such fundamental effects as cloud feedbacks.

There is an emerging perception that the physics of the growing family of geophysical flow models is not receiving the continuing attention needed to make these tools more useful and accurate. In some cases the models have stagnated, changing little since their inception. Some of the reasons for this stagnation lie in the underlying science. For example, despite decades of intensive efforts by several science communities, adequate models of geophysical turbulence still do not exist. But some of the problem seems cultural; today's modelers and users of model output seem less engaged with improving model physics than with the increasingly sophisticated numerics, graphics, and architecture of the model system and with using models rather than observations to study geophysical flows. As education and training in the atmospheric and oceanic sciences turn away from model physics, students increasingly see model outputs as truth without the healthy skepticism that should be inherent with these tools.

No model is or ever will be perfect. The atmosphere and ocean are inherently nonlinear, and their chaotic physical processes occur over a vast range of scales, making it impossible to simulate every physical process at every scale. But because these models are often used to predict future events, which can have immediate to long-range policy implications, it is imperative that their underlying physical processes be represented as robustly as possible. To ensure success in this regard, it is vital that the science of geophysical modeling garners the attention and support necessary to continue its improvement.

The National Academies' Board on Atmospheric Sciences and Climate organized its 2004 summer workshop to explore and evaluate current efforts to model physical processes of coupled atmosphere-land-ocean (A-L-O) models (see Appendix A for the complete Statement of Task). Specifically, the parameterization of physical processes in A-L-O models was addressed, including associated errors, testing, and efforts to improve the use of parameterizations. During the workshop discussions, participants examined some intellectual and scientific challenges in modeling and highlighted the proposition that some of the key impediments to progress in representing physical processes are primarily cultural in nature. For reasons that may broadly have to do with the incentives and disincentives that exist in certain parts of the atmospheric and oceanic sciences, scientists in the field may be slipping into a mode of conduct in which the arduous and often unrewarding task of developing and rigorously testing new parameterizations is avoided in favor of tuning existing schemes—which can result in compensating errors—or developing new schemes without subjecting them to evaluations of their effects on the weather or climate system or without rigorous tests against observations. The advent of multi-model ensembles and stochastic parameterizations, although no doubt contributing to improved forecasts and better quantification of uncertainty, may have the unintended effect of reducing incentives to improve model physics. In the field of education, the increasing ease with which large models may be obtained and run tempts students away from basic understanding and toward mere simulation; likewise, the long gestation period for instrument development and field project design and execution can discourage students and young researchers from these important endeavors.

The dichotomy of simulation versus understanding was a theme that connected many areas discussed at the workshop, beginning with the opening talk on this subject by Isaac Held of the National Oceanic and Atmospheric Administration's (NOAA) Geophysical Fluid Dynamics Laboratory and Princeton University (see Appendix C for a paper by Held on this topic). As discussed during the workshop, meteorology, in particular, has benefited financially from the widespread perception that it is an applied science geared mostly toward weather forecasting and, more recently, climate prediction. Much research in the field is supported by funding agencies on the premise that it will improve forecasts, and the media often project an image of researchers beavering away at better forecasts. But the same perception that is successful in helping policy makers see the value of research in atmospheres and oceans may turn away talented young scientists who, steeped in the culture of physics and mathematics and driven primarily by curiosity, are best suited to tackling the difficult, often fundamental, scientific problems that our field faces.

The cultural issues come to a head in the field of climate research and prediction. As summarized in the concluding presentation by Dennis Hartmann of the University of Washington (see Appendix D for the workshop agenda), the culture of climate modeling has emphasized model intercomparison and the testing of model output against global observations. As Dr. Hartmann discussed, physical parameterizations are often viewed as blackbox subcomponents whose knobs, in the form of largely unobservable parameters, can be adjusted at will to obtain some desired result. Physical parameterizations, often with large numbers of unconstrained or loosely constrained parameters, are inserted into models and judged largely on the merits of their perceived sophistication and their effect on model performance. Ideally, the development and testing of parameterizations would include (1) evaluations of the effect of the physical process on the weather and climate system to assess its sensitivity and demonstrate the importance of the process, (2) comparisons of the parameterized process against observations (or high-resolution simulations of it), and (3) evaluations of the effects of the parameterization on the structure and evolution of the larger-scale flow. Rigorous offline tests of parameterizations against observations often are lacking, and some researchers believe that such tests are flawed because they omit the feedbacks that the process in question would interact with as part of a large model. The lack of rigor in the development and offline testing of some key physical parameterizations was viewed by some workshop participants as being due to a combination of two influences: (1) the isolation of climate modelers from observationalists and from those involved in numerical weather prediction and (2) the pressure on climate science to produce useful predictions of global warming, which is reducing to subcritical levels the skilled man-hours dedicated to developing and implementing improved physical parameterizations. A-L-O models are a mixture of scientific exploration and application, and it is important to protect the health of both activities without letting the latter eclipse the former.

The workshop during which these and a myriad of other issues were examined was a menagerie of discussions organized around six topic areas capped by an afternoon of synthesizing discussion (see Appendix D). Because of the interactive nature of the discussions, no attempts were made to attribute specific ideas to particular people. By the National Academies' policy for this type of workshop, there are no findings or recommendations in this report. Instead it summarizes the major discussion items that arose during the workshop; it is not an edited workshop transcript.

The workshop also sought to identify a limited number of key physical processes whose representation in models is regarded as problematic. Chapter 2 reviews the discussion of some parameterizations that are in need of improvement. Chapter 3 explores how parameterizations are developed and tested. Chapter 4 summarizes the workshop participants' thoughts on cultural impediments to improving model parameterizations and ideas for alleviating these problems and improving simulation of the oceans, atmosphere, and land surface. The committee's concluding thoughts are presented in the final chapter.

2

Development and Testing of Model Parameterizations: Some Examples

Workshop participants discussed the importance of having the physical basis of large-system models firmly established by process studies based on field and laboratory measurement programs, theory, and numerical modeling. Based on the workshop participants' expertise—including the workshop discussions and written input from the participants (see Appendix B for extended abstracts written by workshop invitees as requested by the organizing committee)—several key processes needing improved model parameterizations were discussed. In this chapter, a specific example of how parameterizations ought to be developed and tested is discussed, followed by a few examples of parameterizations that clearly need more work.

In many ways the research on fluxes between the atmosphere and oceans provides a nearly ideal example of how progress can be made in the representation of physical processes in large-scale models. A body of theory was developed, based mostly on dimensional reasoning, and field and laboratory experiments were designed to carefully and rigorously test the theoretical predictions. Field measurements led, in turn, to refinement of existing theory and, in some cases, to the development of new approaches. These were then once again tested in the field and in the lab, and further refinements were made. Even the very difficult problem of air-sea fluxes at the extreme wind speeds encountered in hurricanes has been addressed this way, with a number of laboratory and field projects under way to test theoretical predictions. This has led to the desirable outcome that there is very little variation in representations of surface fluxes over the oceans in large-scale models.

At the other end of the spectrum is the representation of clouds and convection in regional and global models. Parameterizations of clouds and convection purport to describe the higher-order statistics of ensembles of clouds (e.g., the variance of relative humidity or the vertical flux of energy due to cloud-scale motions) in terms of lower-order statistics (e.g., mean temperature, humidity, vertical velocity). Here all statistics are assumed to be based on averages over scales that are large relative to individual clouds.

Parameterizing clouds and moist convection in models presents several unique challenges to modelers. Layer clouds may span many grid cells horizontally, but may be thin compared to typical vertical layer thicknesses; conversely, convective clouds usually occupy only a small fraction of a grid cell, yet span many model levels. Convective clouds represent major local sources of enthalpy and water vapor, and layer clouds have large effects on both shortwave and longwave radiative transfer. Representing both is crucial to most atmospheric and climate models, yet doing so has proven notoriously difficult. Transfer of enthalpy and water substance by convective clouds is sensitive to very small-scale processes such as turbulent entrainment and cloud microphysics, but evaluating parameterizations of clouds and moist convection against direct observations of these processes is a formidable undertaking. As discussed in Chapter 3, the difficulty of directly testing cumulus and cloud parameterizations has led to some undesirable practices in developing and testing such schemes. However, a new U.S. Climate Variability and Predictability (CLIVAR)-based activity, Climate Process and Modeling Teams (CPTs[1]), was established in 2003 to provide a thorough, efficient forum for improving model parameterizations by bringing together theoreticians, field observationalists, process modelers, and scientists at the large modeling centers (Bretherton et al., 2004; *http://www.usclivar.org/CPT/index-newcpt.html*). Three pilot CPTs are

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[1] CPTs are sponsored by the National Science Foundation (NSF) and the National Oceanic and Atmospheric Administration (NOAA). The key objective of CPTs is to speed development of global coupled climate models and reduce uncertainties in climate models by building a new holistic community that exchanges knowledge, ideas, and needs. CPTs are driven directly by the scientific needs perceived within the modeling centers.

being funded currently, one of which is examining cloud-feedback parameterizations.

The development of parameterizations of land-surface processes presents another set of difficulties. Correct representation of fluxes of heat, moisture, and momentum between the land surface and the atmosphere is critical to successful simulation of climate and weather. Progress in developing high-quality representations is impeded by the difficulty of making representative measurements of key land-surface properties and processes, such as soil moisture and evapotranspiration. Point measurements of many of these are difficult, and even if high-quality measurements could be made, the inhomogeneity and temporal variability of the land surface can render such measurements unrepresentative of the area- and time-average fluxes needed for input into regional and global models.

In the case of unresolved internal gravity waves, different parameterization methods can give very different results, and observations do not constrain these different methods well enough to distinguish among them. There is an abundance of data in which gravity waves are detected, but only rare datasets provide the needed information—that is, momentum flux as a function of at least two wave characteristics and the propagation direction. It is a substantial observational challenge to characterize these intermittent and highly variable phenomena on a global scale using available measurements that were not designed to observe gravity waves.

Progress is being made using models to aid in the interpretation of global datasets and model studies of wave sources constrained by local observations. Early papers that described applications of gravity-wave parameterizations in global models used to commonly omit details on the momentum flux spectrum input into the parameterizations that were needed to understand the effects of the parameterized waves on the results, but this has been remedied in recent years (Manzini and McFarlane, 1998; Scaife et al., 2000, 2002; Giorgetta et al., 2002).

The representation of small-scale physical and biological processes in the oceans also presents several unique challenges. Oceanic general circulation models make severe compromises in their representation of many of the controlling dynamical processes. On scales smaller than oceanic basins, significant fluxes of momentum and dissolved materials are effected by mesoscale eddies; inertial, internal gravity, and surface gravity waves; double-diffusive mixing; and small-scale turbulent motions in the surface and bottom boundary layers. And, of course, the fluxes also are effected by the possible interactions among all of these phenomena and in their relationships with the large-scale circulation and stratification. In the general circulation models, these fluxes are represented by parameterizations of well-known equations, and they are designed and evaluated on the basis of theory, measurement, and fine-scale simulation. To further improve ocean parameterizations, there are two ocean mixing CPTs in addition to the aforementioned cloud-feedbacks CPT—one studies the interaction of eddies and the surface boundary layer, and the other studies the bottom boundary layer as a gravity current. A recent essay (Schopf et al., 2003) commissioned by U.S. CLIVAR in developing a plan for these CPTs surveys the relevant processes and the status of their understanding and parameterization.

The oceanic and terrestrial ecosystems are important functional elements of Earth's system. For instance, the ocean's biogeochemical cycling of nitrogen, carbon, oxygen, and so forth is carried out by the lower trophic levels from viruses through plankton. In large-scale models this is represented as transport, reaction, and population dynamics. For the most part, the model rules for these ecosystem dynamics are acts of imagination, usually involving abstraction of actual organisms to hypothesized generic forms but guided by overall conservation principles. Such constructs are exceedingly difficult to evaluate except at the gross level of chemical distributions and fluxes, both because of their organismic unreality and the technical measurement challenges of highly variable compositions with quite heterogeneous distributions.

3

The Framework for Developing Parameterizations

Throughout the workshop, participants discussed the purpose of model parameterizations as well as the methods and means of developing and testing them. These discussions broached subjects such as fundamental challenges of modeling component processes, problems with tuning model parameterizations as opposed to fully evaluating them, field programs to test and improve parameterizations, and the use of community models to develop and test complex dynamical systems.

UNDERSTANDING AND PARAMETERIZATION

The unattractiveness of the word "parameterization"[1] may betray a sense of imperfection that surrounds the concept. Perhaps it is regarded by some as an unsatisfactory fix awaiting the day when the process in question can be explicitly simulated. But parameterizations serve two

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[1] Parameterization is defined by the American Meteorological Society (2000) as "The representation, in a dynamic model, of physical effects in terms of admittedly oversimplified parameters, rather than realistically requiring such effects to be consequences of the dynamics of the system." An alternative definition appears in the summary section of this report (see Chapter 5).

purposes. From a practical point of view, they enable predictions and/or simulations; that is, they are a means to an end. In addition, however, parameterizations encapsulate our understanding of the physical interactions among disparate scales and processes. From this point of view, parameterizations are a route to understanding.

It is a truism of dynamical systems theory that simple component processes can combine in a coupled nonlinear system to yield unexpected emergent behaviors, through feedback among the components. The computational simulation models in wide current usage would seem rife with such possibilities, yet they usually are configured to maximize stability and minimize surprises. This is partly due to a prudent conservatism, but it does avoid the central issue of unexpected emergent behaviors. Because the large models can only have a small portion of their possible behaviors explored, an essential companion activity is discovering and fully understanding a suite of artfully conceived, elegant, minimally constructed models of canonical emergent behaviors germane to the atmosphere and ocean. Some examples of such elegant models are low-order geochemical box (i.e., well-mixed reservoir) models and oscillator models for El Niño-Southern Oscillation and paleoclimatic cycles, as well as more fluid-dynamical models for turbulent boundary layers, synoptic weather life cycles, and the turbulent equilibrium of a zonal baroclinic jet. Many more canonical models for relevant emergent behaviors are needed if our field is ever to come to trust and understand the abundant but complicated evidence from simulation models and measurements.

It is possible to conceive of a purely empirical route to parameterization in which understanding is only required at the level of asserting that the process can be represented in terms of large-scale variables and identifying which of those variables the process in question should depend on. The specific relationship between these variables and the process would be determined purely by experiment. Although such an empirical method may be possible, it would no doubt prove cumbersome. Moreover, not adequately understanding the physical processes would make it less robust, and it likely would be less satisfying intellectually. This highlights the intimate connection between understanding and parameterization, which can be regarded as an embodiment of a set of physical hypotheses. The most satisfying parameterizations start out with an understanding of the importance of the process to be parameterized, followed by an elegant hypothesis about the relationship between the process in question and the explicitly simulated variables, and concluding with the subjecting of that hypothesis to rigorous experimental evaluation. To the extent that the parameterization succeeds, it also serves to validate our understanding of the process. In this sense the process of finding an ideal parameterization is intimately connected with our quest to understand nature. At the heart of this endeavor lies the notion of elegance (see Appendix C for Isaac Held's paper describing this concept), which encompasses clarity and simplicity, coupled with explanatory and predictive power. Progress in developing better representations of physical, chemical, and biological processes will depend in part on conveying to prospective students and researchers the true relationship between such development and the quest for understanding nature.

TUNING VERSUS EVALUATION

A parameterization may be thought of as an embodiment of a set of physical hypotheses. As such, confidence in its performance must be based on rigorous tests against observations and/or direct or large-eddy simulations.

Parameterizations are typically based in part on simplified physical models (e.g., entraining plume models in convection schemes). The parameterizations also involve numerical parameters that must be specified as input. Some of these parameters can be measured, at least in principle, while others cannot. The introduction of parameters that cannot be measured even in principle needs to be avoided, but in our current state of ignorance it is sometimes unavoidable. The assigned values of unmeasurable parameters are always chosen to optimize the realism of model results, but this may introduce compensating errors.

Obviously, parameters that can be measured should be set to their measured values. But even here, experience shows that in some cases model results can be improved by departing from the measured values (i.e., by using incorrect values of the parameters). In such cases the errors introduced by the incorrect parameter values are presumably compensating for other, unknown errors in the model. The use of demonstrably incorrect parameter values was considered by some workshop participants as an egregious kind of tuning.

Observational tests of parameterizations can be divided into two types. First, there are direct tests of the parameterizations or the physical assumptions that underlie them. The second type of test consists of running a model that uses the parameterization and evaluating the results of the model against observations. Tests of the first type are more fundamental.

Perhaps a good example of this first type of test is the development, testing, and refinement of parameterizations of surface fluxes over the ocean. The early formulations, based strictly on dimensional reasoning, were subjected to rigorous tests in the field and in laboratory experiments. These experiments determined certain universal constants within reasonable bounds and pointed to systematic deficiencies in the early formulations. Based on these results, the formulations were refined and further experimental tests were made. Today, except perhaps at very high and very low wind speeds, these formulations are used in large-scale simulations with a high degree of confidence, and the constants that appear in the surface-flux formulations are not usually considered tunable parameters.

By contrast, the development and testing of convective parameterizations have proceeded along a different route, leading to the present circumstance in which many different schemes are in use and there is no general agreement on what constitutes a superior scheme. This different route is at least partially due to the fact that testing convective schemes in the field is very difficult compared to, say, testing surface-flux parameterizations. A methodology has been devised whereby rawinsonde arrays are used to measure horizontal and vertical advection, and surface-based and satellite-borne radiometers measure radiative fluxes. These fluxes, together with estimates of surface fluxes, are used to drive single-column models in which the only truly free process is the convection, which is parameterized. Integration of the single-column model over a reasonable period of time will result in large errors in quantities such as relative humidity, unless the convective fluxes are accurate. Field programs that are being carried out using this methodology include the GEWEX (Global Energy and Water Vapor Experiment) Cloud Systems Study (GCSS) (Randall et al., 2003) and the Atmospheric Radiation Measurements (ARM) program of the U.S. Department of Energy (Morcrette, 2002). Typically, field data are integrated to provide a comprehensive case study, which also is simulated by high-resolution numerical models. The observations are used to evaluate the high-resolution model results, and both are used to evaluate the parameterizations.

The first experiments designed specifically to enable parameterization tests of this type occurred during the 1990s. Recent and near-future examples include the Dynamics and Chemistry of Marine Stratocumulus (DYCOMS-II) experiment (Stevens et al., 2003), the Cirrus Regional Study of Tropical Anvils and Cirrus Layers-Florida Area Cirrus Experiment (CRYSTAL FACE) (Jensen et al., 2004), the Rain in Cumulus over the Ocean (RICO) experiment (*http://www.ofps.ucar.edu/rico/*), and ARM's Tropical Warm Pool International Cloud Experiment (TWP-ICE) (May et al., 2004). Such evaluations have revealed that cloud system resolving models, forced the same way, produce systematically better results than convective parameterizations. At the same time, few convective schemes in current use have been subjected to such rigorous tests. Often, a new scheme is simply inserted into a large-scale model and evaluated based on the overall performance of the model. This is usually an ill-posed enterprise, however, because model error generally arises from many components. Workshop participants stressed the need to test physical representations as far as feasible against observations or, especially where observations are lacking, against direct numerical or large-eddy simulations of the process in question.

Parameterizations can now also be tested against detailed numerical simulations of the process. Both large-eddy simulation (LES) and the exact but low Reynolds number form called direct numerical simulation (DNS) have been used in this way to complement and extend what can be done through direct observations. Because many processes must be parameterized in atmosphere-land-ocean models (A-L-O), and because they can strongly influence model fidelity, it is important that their parameterizations be developed and tested in this way to the extent possible.

COMMUNITY MODELS AS A FRAMEWORK FOR DEVELOPING AND TESTING PARAMETERIZATIONS

Complex dynamical-system computational models are essential tools for the science of weather analysis and forecasting, small-scale geophysical fluid dynamics, climate, oceanic circulation, and biogeochemical cycling in Earth's system. They have come to have such complexity in their functional breadth and computational technology that they must be developed, maintained, used, tested, evolved, and managed by ever larger and more diverse groups of scientists. National laboratories manage much of the communal modeling activity. Yet modeling practices could be more productive if the relationships of these laboratories with non-laboratory modeling scientists were strongly collaborative because a wealth of intelligence and labor exists outside laboratories (e.g., at universities, within the private sector). Although this principle is widely acknowledged, its practical implementations are still rudimentary.

In all cases, community models are applied by a community of users. Here "community" is understood to mean a multi-institutional group. Although the creation and support of a community model can be a huge and difficult task, it is relatively easy to induce a community of users to make applications of a well-constructed and well-supported community model.

In some cases, community models are also developed by a community. This is much more difficult to accomplish, for a variety of reasons. One is that, empirically, there are fewer people with the inclination or ability to do model development than there are model users. A second factor is that virtually all community models are identified with one primary or host institution, and the staff of the host institution inevitably feel some ownership of the model. As a result, outsiders who want to participate in model development may find that they need a collaborator on the inside of the host institution. Moreover, there are logistical and infrastructure issues that serve as a barrier to outsiders, including the technical difficulty of interfacing new code with a complex model that has complex data structures, testing the code, and then debugging it. Because it usually is faster and more effective to interact with the core model staff in person, an extended visit to the host institution may be required.

The continued development of an established community model needs to be carried out very carefully. It is important that the model—which can be thought of as the "crown jewels"—not be broken. As a result, established community models tend to evolve slowly and conservatively. For example, the recent new release of the Community Atmosphere Model (CAM), which is the atmospheric component of the Community Climate System Model (CCSM), is not very different from the preceding version. The legacy of the CAM can be traced back more than 20 years. Interestingly, some of the newer, less established components of the CCSM (e.g., sea ice, land-surface processes, biogeochemistry) have undergone much more rapid recent development.

4

Impediments to and Ideas for Progress in Model Development

Several impediments to developing better representations of physical processes in models were identified by workshop participants. Most of these are described as "cultural," meaning the impediments, to some degree, are entrenched in the customary way of doing research and education in the atmospheric and oceanic sciences and in the structures of universities, laboratories, and funding agencies as they have evolved through history. The impediments identified by the participants are summarized in this chapter. Subsequently, several ideas from the workshop participants for overcoming these impediments and for improving the progress of model development are presented.

First, and perhaps foremost, workshop participants expressed concern that the atmospheric and oceanic sciences do not attract the best and brightest graduate students. It was suggested that prospective graduate students tend to see meteorology as the strictly applied endeavor of making better weather and climate forecasts, which they often perceive as the subjective interpretation of satellite images and the application of advanced computer graphics. Participants hypothesized that this perception probably arises because the field is projected to the general public primarily through media weather forecasts. Likewise, climate science likely is perceived largely as an effort to assess and predict global warming. There is a vague impression that computers are used more for substantive reasons, but the idea that there are problems of large inherent intellectual interest, requiring backgrounds in advanced mathematics and physics—and that are equally as challenging as these fields in and of themselves—generally is lost. The field seems to be more successful in recruiting students attracted to the idea of weather forecasting or policy-oriented climate research than in recruiting students with a fundamental mathematical and physical curiosity about the atmosphere and ocean. This state of affairs may result in part from the way the atmospheric sciences and climate science promote research to funding agencies by promising improved forecasts.

Once enrolled at a university, the contemporary graduate student often finds himself or herself beholden to the goals of his or her professor's research grant, goals that are increasingly burdened by the necessity of producing short-term deliverables or of demonstrating broader impacts on society. This system blurs the line between student and employee. The student with a bright idea or who is singularly motivated to pursue a particular problem, however important that problem may be, may not be able to fit his or her interests into the available funding slots, even if the faculty are sympathetic to the idea.

Workshop participants noted that another challenge of engaging new people in the field is that the development and refinement of model parameterizations is not seen as an attractive career path by most young researchers. In the United States, most large modeling centers maintain groups dedicated to developing and improving parameterizations, but these groups tend to be small and, consequently, overtaxed. Moreover, these positions often are funded as support scientist or software engineer positions.

The current funding environment is subtly influencing the choices that researchers make, in ways that may inhibit progress in understanding and simulating climate and weather. For example, the development of instrumentation and the execution of field measurement programs, which are crucial for progress in a number of areas discussed at the workshop, require relatively long time horizons. Increasing emphasis on deliverables in some funding agencies together with tenure cycles at universities acts to discourage such labor-intensive and time-consuming endeavors in favor of activities such as simulation experiments with existing numerical models, where deliverable results often can be obtained in a short time.

In many areas the development of improved representations of physical processes requires knowledge of a broad variety of physics. For example, improved simulation of boundary layers and convection over land depends on accurate representation of land-surface processes coupled strongly with convection and precipitation and with clouds, which strongly affect radiation and therefore surface fluxes. But studies of these processes often are compartmentalized within laboratories, universities, and funding agencies. In the aforementioned example, soil properties are usually studied in departments of hydrology, whereas clouds and convection are studied in atmospheric science departments. Parameterization of the soil components cannot proceed in isolation from representation of boundary-layer and convective fluxes, or from parameterization of clouds, yet the structure of research organizations discourages the cross-fertilization that is required to make progress. Compartmentalization also compromises the efficiency of observational programs. Workshop participants cited several examples in which expensive field measurement programs were conducted at the instigation of one group of scientists, but certain key measurements that could have helped another group were not made, even though the added cost would have been marginal.

Another cultural impediment to scientific progress that was discussed by participants is the reluctance of different federal agencies, or even different divisions of the same laboratory, to cooperate for the greater good. Although endorsements of such cooperation are easy to elicit, experience shows that actual cooperation is rare. The problem likely stems from competition for resources, fear of loss of control, and/or diminished credit for the results.

IMPROVEMENT VIA EDUCATION

Workshop participants generated several ideas for addressing education-based impediments to progress in model development. Although addressed with enthusiasm and lively debate during the workshop, these are presented here as ideas rather than recommendations. As such, they are not thoroughly detailed, so they would need to be explored further should an institution wish to implement them. These ideas include agency-sponsored fellowships for students, instruction in model development, summer schools in which people with varying areas of expertise are brought to bear on a shared problem, and the need to better convey the intellectual excitement of the atmospheric sciences.

Fellowships

As noted, the current funding of graduate students through research grants produces a set of incentives and disincentives that do not always work in the best interests of scientific advance. An alternative approach would be to fund students with fellowships granted directly by the agencies. This already is done in a few cases, but it could be implemented more broadly. A wider use of fellowships in place of assistantships would empower students; that is, it would increase the students' freedom to choose their schools and advisors and to base their thesis work on research projects of their own conception. It also might serve to ease pressure on proposal writing, management, and review as well as mitigate the disincentives that discourage conducting research with long time horizons.

Education in the Art of Model Development

Education naturally proceeds from the simple, basic, and well known to the elaborate, subtle, and uncertain, especially in the sciences. At present, university curricula in atmospheres and oceans do a good job of teaching about computational mathematics, geophysical fluid dynamical theory, and the observational record, but they do not do well enough in preparing students for postgraduate research involving the practices of complex computational simulation modeling and modern measurement techniques. Although in most universities the time constraints on expanding the curriculum are severe, this deficiency potentially could be ameliorated by such things as shifts in course contents (e.g., perhaps doing more in the style of case studies), expansion of apprentice research opportunities with professional modelers and instrumentalists, and the creation of inter-institution, special-topic programs for motivated students. Regarding the latter, interaction with multiple institutions (e.g., universities, research laboratories, private companies) may be prudent if a student wishes to conduct research that, for example, requires a class not taught at their university or requires collaboration with an instrumentalist. In other words, rather than adhere to the usual departmental requirements, such motivated students may design a special, unique program with his or her advisor combined with scientists from other institutions. Such special programs are best suited to the student with a firm, fundamental knowledge base; thus, they likely are best restricted to someone at the Ph.D. or postdoctoral level.

Summer Schools

In a multidisciplinary field such as climate, it is rare to find any one university that can provide students with the necessary training in all the important subjects, which range from fluid mechanics to radiation to remote sensing to chemistry. Summer schools run by universities or national centers are an effective way to supplement the education of graduate students, postdocs, and young scientists. They bring the workers on the forefront together, promote interactions, and give students hands-on experience in cutting-edge research. Notable examples of successful summer schools include those frequently conducted by the Woods Hole Oceanographic Institution and Cambridge University. More of these kinds of opportunities could potentially better educate and inspire the younger generation.

Conveying the Intellectual Excitement of the Atmospheric and Oceanic Sciences

As mentioned above, there was a concern among many of the workshop participants that the sciences of the atmosphere and ocean suffer from misperceptions about their nature, and this affects the kinds of students attracted to the work. Several ideas for combating this were discussed at the workshop. At the heart of all these ideas is the notion that it is important to convey to prospective students the sense of intellectual excitement that pervades the field, in contrast to the image of mundane, subjective work portrayed by the media and others. One idea for accomplishing this might be to hold a series of summer schools aimed at undergraduate students in fields other than atmospheric science and oceanography. These might complement the graduate summer schools proposed above. Another idea is to encourage experienced professionals in our field to communicate to a broader audience by publishing in broadly-based scientific journals (e.g., *Scientific American*, *Discover*) and by publishing books aimed at the general, scientifically literate reader. The field also could benefit from an eloquent and prolific spokesperson comparable to Richard Dawkins (Dawkins, 1990), who speaks for evolutionary biology.

IMPROVEMENT VIA RECHARGING LARGE MODELING GROUPS

As models age, they become refined, but they also lose their flexibility. Thus, workshop participants discussed the notion that continual renewal of large modeling efforts improves their effectiveness. Although designing and building a new model from scratch can be an incredibly large and daunting task, it can also serve as an invigorating renewal. Fresh-start modeling projects represent opportunities to implement the most current and novel ideas and simultaneously to involve young, fresh scientists in the model-building enterprise. An excellent example is the European Centre for Medium-Range Weather Forecasts (ECMWF) operational weather prediction model, the first version of which was created in the late 1970s by a group of young, talented, hard-working scientists. ECMWF brought forth many important modeling advances, and their success is due, in part, to the fact that the first ECMWF model was developed from scratch, making use of the strongest ideas available at the time. Today, 25 years later, the ECMWF model is evolving more slowly, but it continues to be one of the world's best. ECMWF's continuing success is favored by their frequent and institutionalized interactions with other scientists from around the world, through seminars, workshops, and extended visits. Moreover, with its practice of hiring young scientists for five-year contracts, ECMWF serves as a good example of making the development and improvement of parameterizations an appealing career choice. Workshop participants thought that the modeling enterprise in the United States might be improved if its modeling centers similarly increased incentives and rewards and elevated the intellectual excitement for such work.

The concept of the aforementioned Climate Process and Modeling Teams (CPTs)—a collaboration with responsibility for investigating a particular physical process (e.g., cumulus convection) in a comprehensive way from theory, measurements, and fine-scale computational simulations to the design, implementation, and evaluation of its parameterization in a complex system model—seems a very attractive approach to closing the gap between fundamental science and Earth-system simulation. During the experimental phase, the three pilot CPTs have been constrained not to propose new measurements. If these experiments work well, then expanding the scope of the CPTs could be beneficial in addressing additional processes and carrying out new field measurement programs in which this comprehensive responsibility is inherent in their conception and design.

Workshop participants also discussed that, as part of the process of continually improving models, efforts need to be made to remove physical representations that have been shown to be inferior or defective. It is important that the justified desire for multi-model ensembles not result in the retention of parameterizations that are known to be deficient.

IMPROVEMENT VIA CROSS-SCALE INTERACTIONS

A premise of Earth-system simulation models is that dynamical control inheres in the planetary and synoptic scales with necessary, but essentially simple, parameterized representation of the effects of processes on finer scales. Although this premise has worked well in practice for modern simulations of weather and climate, it must ultimately fail to be correct. As long as simulation models span a limited range of scales, the importance of cross-scale dynamical coupling cannot be investigated. Even the occasional modern use of an embedded finer-scale subdomain within a coarser large-domain grid (e.g., a nested-grid hurricane forecast) only partly encompasses the coupling possibilities as long as the information flow is only down-scale, from large to small. The more ambitious approach to cross-scale coupling is to routinely embed either more fundamentally based submodels in place of existing parameterization schemes. This new approach, called multi-scale modeling, involves embedding models that explicitly resolve the smaller scales as components of global circulation models. The embedded high-resolution models take the place of conventional macro-parameterizations. Prospective benefits include improved simulations of the global circulation, and mathematical convergence of the multi-scale model to a global high-resolution model, in the appropriate limit. Drawbacks include very large (but possibly feasible) computational requirements.

IMPROVEMENT VIA MODELING TECHNIQUES

Given both the variety of possible feedbacks among component processes and subsystems and the spatial and temporal non-smoothness of atmospheric and oceanic fields near the grid cutoff scale of Earth-system simulation models, it is unlikely from the perspective of computational mathematics that the model solutions are very accurate with respect to the underlying partial differential equations (or even worse types, regarding smoothness) on which the simulation model is based. To the extent that potential solution non-smoothness is dealt with by artificial computational damping, there is further divergence from the true solution of the underlying equations. As a practical matter, present modeling methods are largely an empirically determined compromise between desired accuracy and smoothness of its solutions. Studies of simulation convergence with increasing grid resolution are rare and often ambiguous in their results. Evidence for non-modularity of complex simulation models—in which swapping one component algorithm for another that in theory is comparably reliable and accurate leads to quite different model answers—suggests that they have a rough fitness landscape (see Appendix C) that is rarely considered in evaluating their answers (apart from the common practice of parameter tuning a given model's answer to a more appealing outcome). It therefore seems likely that the choice of computational algorithms is often influential, if not determinative, in model-simulated behaviors. If so, then much more exploration of simulation sensitivity to computational methods would be useful to better understand the model solution's validity.

Physical parameterizations and discretization methods are often regarded as very distinct aspects of a model's formulation. This is a misconception. The implementation of a parameterization inevitably raises discretization issues, which can be quite challenging and are typically linked to the discretization methods used to represent the resolved-scale motions. Therefore, several workshop participants suggested it would be best if the physical and numerical aspects of a model's design be considered as closely coupled problems. Furthermore, it may be that new approaches to the challenges of numerical integration could be valuable, such as using exact discrete representation to formulate parameterizations.

IMPROVEMENT VIA TESTS IN NUMERICAL WEATHER PREDICTION MODE

There are no fundamental differences in formulation between the global models used for climate simulation and those used for numerical weather prediction (NWP). Models used in operational NWP are evaluated daily for their ability to simulate the evolution of particular weather systems. The statistics of such forecasts have been analyzed to identify systematic errors that emerge in the first few days of simulation. In many cases these systematic forecast errors are closely related to the errors that the same models produce in simulations of the present climate. These considerations led the ECMWF (1999) and the World Climate Research Program (2001) to advocate the use of NWP as a means of evaluating climate models. The idea is to use climate models to produce weather forecasts for real cases, identify the forecast errors, and trace those errors back to weaknesses in the models' formulations—in other words, to do what NWP centers do as a matter of course.

There are precedents for this. The United Kingdom's Met Office has been using a single modeling system for both climate simulation and NWP for years now. In addition, the Max Planck Institute for Meteorology in Hamburg, Germany, developed its climate model by starting from a version of ECMWF's NWP model. Until recently, however, the United States lagged behind in this area. This has now been corrected through a program called CAPT, the CCPP-ARM (Atmospheric Radiation Measurements) Parameterization Testbed, where CCPP stands for the Climate Change Prediction Program funded by the U.S. Department of Energy. CAPT is using analyses of global weather from NWP centers, in conjunction with field observations such as those provided by ARM, to evaluate parameterizations of subgrid-scale processes in global climate models (Williamson et al., 2005; Boyle et al., 2005). CAPT's methods were first applied to the community atmosphere model (CAM) and are gradually being used with a wider variety of models, including the GFDL's Atmospheric Model 2 (AM2). Workshop participants thought that much could be gained if these methods were widely adopted by the U.S. climate modeling community.

5

Concluding Thoughts

Many workshop participants agreed that progress in model development is being impeded by the slow pace of improvement of the parameterizations of physical processes. Workshop participants noted a disturbing tendency away from a rigorous course of development and testing of new parameterizations and toward tuning existing parameterizations or using new but inadequately tested ones. The participants identified several likely contributors to this situation:

- Widely available, easily run models and the current funding and academic environments may be turning both graduate students and their faculty advisors toward fast-turnaround research in numerical simulation and away from the traditional but much slower path of theory and observation.
- Bright young scholars best suited to tackling scientific problems may incorrectly perceive that the atmospheric and oceanic sciences are an applied field whose goal is merely to improve weather and climate forecasts.
- Progress in parameterization, which often requires interactions across traditional disciplinary boundaries, could now be inhibited by the compartmentalization of educational, research, and funding institutions.

- The rigidity of long-existing models and the lack of efforts to remove inferior or flawed physical representations hinder progress by preventing opportunities for new, fresh thinking.

These trends could cloud the future of the fields of atmospheric science, climate, and oceanography. Models are widely used, perhaps much more so than could have been anticipated at the dawning of the computer age some 40 years ago. The growing user community includes many nonspecialists who are not prepared to assess or question the reliability of model predictions. As computers become larger and allow even finer model resolution, some local model forecasts will become increasingly stochastic. It was not clear to the workshop participants that the atmosphere, climate, and oceanography communities are prepared or even constituted to make the required adjustments, refinements, and improvements in model parameterizations that this will require. It is particularly troubling that if our unresponsiveness to the parameterization challenge is indeed culturally driven, its implications carry an aspect of inevitability.

Atmospheric scientists and oceanographers understand both the practical and scientific importance of the parameterization problem. But we also need to bestow it academic dignity by acknowledging it as a difficult and important problem in mathematical physics. Indeed, it involves, among other things, turbulence, which is commonly referred to as the outstanding unsolved problem in classical physics. The parameterization problem—which could be defined as the identification and understanding of the physics of unresolved processes and the compact, optimal representation of this physics in numerical models—deserves serious attention from the best and brightest.

This report summarizes the discussions of the workshop participants on parameterization approaches and frameworks and on specific targets for parameterization improvement. Among the key issues identified by the participants were the following:

- An important field of parameterization science has emerged over the past 40 years as a result of the computer revolution. Workshop participants believe that our educational, research, and funding institutions need to recognize, accommodate, and foster this new field.

- The modeling community needs to encourage, support, and pursue alternative computational approaches (e.g., multi-scale computing) and better computational methods.
- More extensive and rigorous comparisons of models with observations and field experiments designed to support such comparisons are needed.
- The cultural issues thought by the workshop participants to be limiting progress in model development might not be self-correcting; they could require the institutional adjustments that are occasionally but necessarily made as society and the atmospheric and oceanic sciences respond to changing conditions.

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A

Statement of Task

This workshop, which will be open to the public, will use key presentations, panelist discussion, and open discussion to explore and evaluate current efforts to model physical processes of coupled atmosphere-ocean-land models. Specifically, the following questions will be addressed at the workshop:

- What is the status of and what are the major errors associated with the parameterization of physical processes in A-L-O models ranging from local-daily scales to regional-decadal scales? What effects do these errors have on model output compared to other sources of error?
- How can model parameterizations be improved to represent the essential physics in A-L-O models? How can these parameterizations be tested rigorously, and what supporting infrastructure is needed to do so?
- What is the appropriate balance between the efforts being directed toward improving physical parameterizations and the efforts being directed toward other model development and application activities?

B

Input from Workshop Invitees

Each workshop invitee[1] was asked to give a quick overview from their disciplinary perspective to spark discussion in their session and to provide an extended abstract that would capture their thoughts for the workshop record. Most drafted their pieces prior to the workshop, but a few modified or wrote their pieces near the end of the workshop. The content and format of these pieces vary. Some give a brief summary of the state of the model parameterizations in which they have expertise (i.e., as defined by the six discussion sessions of the agenda as shown in Appendix D). Some discuss their opinions for means to improve parameterizations or aspects that require focused efforts for improvement. And some address fundamental problems in the atmospheric and climate communities that hinder advancements in modeling.

ATMOSPHERIC GRAVITY WAVE EFFECTS IN GLOBAL MODELS

Joan Alexander, Colorado Research Associates

Gravity waves are a major mode of geophysical variability on scales of $O \sim$ 10–1000 km in stably stratified fluids. They span a wide range of frequencies and vertical scales as well. We have a simple linear theory that accurately describes wave propagation, which has been a valuable aid in interpretation of

[1] Because Isaac Held and Dennis Hartmann each gave an overarching presentation, the committee did not ask them for a brief summary. Therefore, this appendix includes only the summaries from the other 10 invitees.

observations and which forms the basis for all parameterizations of gravity wave effects in global models.

Gravity wave effects on the global scale include (1) mean-flow forcing effects, which are currently parameterized, and these are considered the most important effect below the stratopause; (2) mixing effects, which are weak in the stratosphere compared to existing model numerical diffusion but are important at higher altitudes; (3) energy dissipation and direct heating, which are only important in the upper atmosphere; and (4) cloud and heterogeneous chemistry interactions, which are important in conditions that are otherwise marginal for ice cloud formation, having impacts on ozone chemistry, stratospheric dehydration, potential radiative feedbacks, and convective cloud initiation, but these effects are not currently parameterized.

There are a variety of global-scale mean-flow forcing effects that are currently treated via gravity wave parameterization. These include (1) providing a drag force on the jets in the upper troposphere and lower stratosphere; (2) alleviation of the winter cold-pole problem common in GCMs; (3) enabling an earlier, more realistic stratospheric springtime transition to summer easterly winds; (4) providing roughly half of the total wave-driven forcing for the quasi-biennial oscillation in the equatorial lower stratosphere zonal winds; (5) providing a drag force on the middle atmosphere jets, with accompanied reversal of wind direction and the radiative equilibrium temperature gradient at the mesopause; (6) forcing in the semiannual oscillation in winds near the stratopause and mesopause; and (7) modifying planetary waves and tides in the middle atmosphere with a variety of effects, including both amplification and reduction of amplitudes and vertical wavelengths of the planetary-scale waves.

Arguably the primary way that gravity waves influence tropospheric climate is via their effects on planetary wave propagation. Mechanistic model studies show that planetary wave refraction and vacillation cycles are very sensitive to winter stratosphere wave drag via the mean flow. Monthly-mean latitude-height wind distributions are generally used to assess the fitness of a particular gravity wave parameterization in model tuning, yet mechanistic model studies show that slight variations in gravity wave drag can give very similar mean-wind distributions while giving very different stratosphere warming frequencies because of this wind sensitivity. Without any wave drag, the winter polar jet can get stuck in a perpetual cold phase that excludes planetary waves entirely. The basic components of a gravity wave parameterization are (1) the source definition, (2) the momentum flux spectrum emanating from the source, and (3) the flux dissipation with height. Sources and dissipation are the nonlinear parts of the problem that parameterizations struggle to describe. We expect the dissipation to be controlled by basic instability mechanisms. There is currently some disagreement about how to describe this portion of the problem, but there is hope that small differences in these descriptions will be relatively negligible compared to the high-sensitivity parameterizations that have (and should have) the characteristics of the waves emanating from the sources. The key is thus defining the momentum flux spectrum as a function of at least two propagation properties of the waves and their propagation directions. This is an enormous observational challenge, particularly since we are dealing with highly local and intermittent phenomena, and the information is needed on a global scale.

Ideally, we need to understand what controls the properties of the momentum flux spectrum from different sources. This is an active area of research involving both observational and theoretical and modeling studies. The observational studies include both local observations from radar, lidar, and radiosonde, as well as airborne and satellite measurements. This problem has not risen to a sufficient level of importance in the minds of the funding agencies to warrant a focused observational campaign or satellite instrument, but data sources designed for other purposes are used to attack the observational challenge with some continuing success.

The important sources include (1) flow over topography, (2) convection and fronts, and (3) jet stream instability and adjustment processes. Note that mountain waves are parameterized with zero phase speed, so they can only slow the winds. A full spectrum of waves is needed to model a circulation like the quasi-biennial oscillation (QBO). Mountain waves are limited geographically, mainly in the northern hemisphere. Convection is a very important source in the tropics and summer seasons. Different gravity wave parameterization methods all use linear theory to describe the wave propagation with height, but they differ in the descriptions of the wave sources and the wave dissipation with height. Note that the GISS model uses a very unique wave source description that allows feedbacks on the sources from changes in the climate like no other model. This could be a factor that leads to its greater AO sensitivity to greenhouse gas changes.

Can we find sufficient observational constraints for the parameterization of gravity wave effects? Many of the questions from the committee focused on this issue. As described above, there is a wealth of data that detect gravity waves, but it is a rare set of data that can provide sufficient information simultaneously on the wave momentum flux as well as the propagation properties of the waves, and no such data exist on the global scale. As with other physical processes, we must use models to aid in the interpretation of datasets, recognizing the observational limitations, to infer the wave properties needed to constrain parameterizations. I and others in the field are engaged in these kinds of research studies.

It is likely that such global observations will help constrain parameterizations but that the real advances in parameterization will come in the development of realistic source parameterizations, based on models tightly constrained by observations. The research community is actively working toward this end. Forecast and climate modeling centers are interested in raising their model tops into the middle atmosphere, and this will require the addition of realistic gravity wave parameterizations. These are too poorly constrained at this time. Parameterization developments are behind the model needs but will eventually be sufficient, probably before the needs are considered top priority.

LAND-ATMOSPHERE INTERACTIONS: HYDROLOGIC CYCLES

Alan Betts, Atmospheric Research

I believe major progress has been made in the past few years in understanding and evaluating the coupling of physical processes over land in global models (Betts, 2004; Betts and Viterbo, 2005; Koster et al., 2004; Lawrence and Slingo, 2004). Consider the basic NASA question about the functioning of the global water and energy cycle: What are the effects of clouds and surface hydrologic processes on Earth's climate? We have struggled for more than a decade to address this, yet understanding the fully coupled system, especially the complexity of the cloud interactions, has remained elusive.

It now appears that these climate interactions over land are within reach, as they can be diagnosed in models and in data, and consequently with this understanding the development of an Earth modeling system to represent them is now possible.

Let me comment on questions raised prior to and during the workshop:

(1) Are we losing the ability to make essential improvements in model physics because we are more concerned with fine-tuning existing representations? These are different exercises. New physics means the ability to step back and understand what is wrong. This needs a long-range plan and excellent scientific oversight and actually hiring people tasked to do it! It is easier for understaffed modeling centers to fine-tune.

(2) Are we losing the ability (and perhaps the will) to make critical but often arduous tests of model physics against observations? The real-world link is critical, and too many student projects get lost in virtual reality.

(3) Is the emphasis on quantifying model uncertainty diluting efforts to improve model physics? Yes, quantifying uncertainty, when model interactions between processes are poorly understood, is an illusion.

(4) What is the status of and what are the major errors associated with the parameterization of physical processes in atmosphere-land-ocean (A-L-O) models ranging from local-daily scales to regional-decadal scales? What effects do these errors have on model output compared to other sources of error? The primary source of error in the global water and energy cycle, which is at the core of Earth's climate system, is in the tropics, where the dynamics, clouds, and physics are all tightly coupled. Over land it involves the coupling of many processes both at the land surface and in the atmosphere. Energy is transferred by the phase changes of water, both at the surface and in the atmo-

sphere, and clouds interact tightly with both the short- and long-wave radiation fields. Precipitation, atmospheric dynamics, and the radiation fields are tightly coupled. At the surface over land, the precipitation, surface hydrometeorology and vegetation, surface and boundary layer fluxes are also tightly coupled. Only with global models can we simulate all the interactions involved, and many processes are parameterized because the range of time and space scales involved is so broad that explicit simulation of them all is not possible. The consequence of this is that the model system must be evaluated carefully to see if the coupling between, say, cloud albedo, boundary layer depth, surface fluxes, and evaporative fraction is properly represented (Betts, 2004; Betts and Viterbo, 2005).

(5) How can model parameterizations be improved to represent the essential physics in A-L-O models? How can these parameterizations be tested rigorously, and what supporting infrastructure is needed to do so? A supporting diagnostic infrastructure is essential, but good frameworks exist; one was part of the ERA-40 system (Kållberg et al., 2004). Models can be evaluated

- locally at "points" such as flux towers where we now have as many as 10 years of data;
- on river basin scales, where precipitation and streamflow constrain the water budget by evaluating the way in which observables and processes are coupled in models and data (three time scales are easily verifiable: diurnal, diurnally averaged, and seasonal); and
- in data assimilation, where the fit of the model to the data is an indicator of the accuracy with which the modeling system fits reality.

(6) What is the appropriate balance between the efforts being directed toward improving physical parameterizations and efforts directed toward other model development and application activities? A good modeling center needs both. The paradigm is straightforward:

- Quantify model errors on a range of time and space scales.
- Identify links/causes in either data assimilation or representation of physical processes, using data as a guide.
- Basic research on new representations of physics, in parallel with pragmatic improvements, again tied to data.
- Complete this development cycle in three to four years.

This appears simple, but very few centers actually complete this cycle (e.g., ECMWF) because it requires science-driven (as opposed to institutional) management and adequate resources or efficient utilization of resources.

LAND-ATMOSPHERE INTERACTIONS: CARBON CYCLES

Scott Denning, Colorado State University

Land-atmosphere interactions associated with the carbon cycle can be decomposed into two classes according to the time scales over which they act. Fast (ecophysiological) processes dominate carbon exchanges on time scales of seconds to years and are strongly coupled to exchanges of water and energy; slow (ecological) processes are responsible for time-mean sources and sinks of atmospheric CO_2 over scales of decades to centuries. Fast processes are important to model because they help us get exchanges of energy and water right and are relatively well understood. Slow processes dominate uncertainty in future atmospheric CO_2 and are poorly represented in current models. The slow processes are the "climate" of the carbon cycle and are inextricably linked to the rest of the climate system.

An important strategy in carbon cycle science is to observe variations in carbon compounds in the atmosphere and ocean and use them to understand underlying processes that govern sources and sinks. The trouble is that the fast processes dominate the observations, but the slow processes dominate the future behavior of the Earth system. We use models of the fast processes to "see through" high-frequency variability in the observations and thereby test mechanistic hypotheses about the slow processes. How do we model both? How do we know in what ways we're wrong and how to do better?

Fast carbon cycle processes are the "weather" of the carbon cycle and are driven by radiation, temperature, and precipitation. They include photosynthesis (conversion of atmospheric CO_2 to organic matter), autotrophic respiration, and decomposition of organic matter back into CO_2. They are observed hourly by micrometeorological methods at a network of well over 200 sites around the world. Stomatal physiology enables vegetated plant canopies to modulate the Bowen ratio of surface energy exchange and strongly couples exchanges of carbon at the land surface to exchanges of energy and water. Diurnal and seasonal cycles of atmospheric CO_2 are largely explained by these processes and provide strong constraints on their parameterization in climate models. Interannual variability in the fluxes is less well understood and parameterized, especially at regional and larger scales.

Important unresolved issues in the parameterization of photosynthesis include (1) representation of physiological stress due to dry soil and dry air; (2) canopy radiative transfer and the effects of direct versus diffuse light on photosynthesis and transpiration; (3) heterogeneity and nonlinear response to drying within grid cell; and (4) management effects such as urban and suburban development, crop fertilization, development, irrigation, and harvest. Respiration and decomposition are fast processes that provide first-order links to the slow processes. Decomposition is typically parameterized as being directly proportional to the sizes of a set of pools of organic matter (e.g., dead wood, leaf litter, soil carbon), which must be initialized everywhere in a coupled model. The initialization problem is compounded by the parameterization of the sensitivity of decomposition to temperature and moisture, which appear not to be universal and are relatively less well constrained by observations relative to influences on photosynthesis. Incorrect parameterization of initial carbon pools and environmental dependence of respiration rates can then hide errors in models of the slower carbon flux processes that control future atmospheric CO_2.

Eddy covariance measurements of surface fluxes of heat, moisture, and CO_2 are tremendously valuable for elucidating the dependence of land-atmosphere exchanges on radiation, temperature, and precipitation (soil moisture). The network of tower sites has unfortunately been oversold as a way to directly observe the time-mean source or sink of carbon, which is probably the weakest aspect of their measurements. The very small footprints observed by this method severely limit their utility for quantifying slow processes that control the carbon balance.

Slow ecosystem processes that must be included in fully coupled climate models include (1) competition for resources and space among plant functional types and the related disturbance/succession/recovery dynamics in ecosystems; (2) biogeochemical cycling in soils and other organic matter; (3) intentional and inadvertent fertilization; and (4) the responses of ecosystems to changes in atmospheric composition and climate. These processes are more difficult to observe, or rather to extrapolate to large spatial scales based on limited observations than their fast counterparts.

To test mechanistic models of changes in slow carbon cycle processes, parameterizations in climate models must be evaluated against observations at spatially-aggregated scales. It is necessary but not sufficient that these parameterizations be evaluated against local data. For example, forward modeling of carbon storage and biomass over time scales of decades following forest fire or harvest must be compared to biometric inventory measurements (such as are available for over 100,000 plots within the United States). It is quite likely, however, that a parameterization of ecosystem biogeochemistry, plant competition, and succession could reproduce the broad statistics of these observations and still fail to capture variations at larger spatial scales due to poorly constrained extrapolation. There is also a need, therefore, for predictions made by forward models of these slow processes to be compared quantitatively to integral properties of the coupled system such as changes in atmospheric carbon gases. This is analogous to comparison of predictions made by cloud models to observable quantities at larger than climate model grid scales, or the comparison of spatially-integrated predictions of runoff to discharge from large river basins. This is a generic requirement for subgrid-scale physical parameterizations in climate models, not a particular requirement for carbon cycle science.

There is an emerging consensus in the carbon science community that diagnostic modeling and data assimilation provide a framework for leveraging large-scale observations to better constrain parameterization of both fast and slow processes that modulate the interaction between carbon and the physical climate system. One example of such an approach is the use of transport inversions to estimate average carbon fluxes at regional/monthly scales and to interpret the results to constrain parameterizations of slow ecosystem controls on the budget. This requires treatment of unresolved time and space variations in the fluxes, especially to the extent they are coupled to or covary with transport processes (e.g., through rectifier effects). Excellent parameterization of fast processes and specification of highly-resolved spatial variability must be included in such calculations, or errors in the space-time patterns are unavoidably aliased into errors in the time-mean, regionally-integrated fluxes through aggregation error. Other aspects of the physical problem that are highly relevant for this exercise include the parameterization of clouds and planetary boundary layer processes in transport models used for inversions of atmospheric CO_2 (as they also show up in other parameterization problems).

Cultural issues that hamper progress in carbon cycle parameterization include the traditional divides between observationalists and modelers, as well as perhaps more unique divides between modelers of fast versus slow processes. The design of field experiments, modeling activities, and data assimilation/ inverse modeling efforts to address these issues remains a high priority and a difficult problem to tackle.

CONVECTION IN COUPLED ATMOSPHERE-LAND-OCEAN-MODELS

Leo Donner, Princeton University

Thirty years after the initial publication of a cumulus parameterization including a cloud submodel (Arakawa and Schubert, 1974), the representation of convection in atmospheric general circulation models (AGCMs) at major climate research institutions is problematic. In general, these centers are at best employing the method of Arakawa and Schubert without further advances, and in many cases the methods used are not even at the level of Arakawa and Schubert. This is despite significant new observational knowledge of convection during that period and substantial success in modeling convective systems using high-resolution models, which can resolve the largest individual deep convective elements and many aspects of organized convective systems.

Cumulus parameterizations in major-center AGCMs generally continue to use as cumulus submodels only convective mass fluxes. Momentum transport is often treated crudely, if at all. There has been over the past several decades only limited research on closure for cumulus parameterizations, despite its central importance and evidence from observations of problems with current approaches, especially at subdiurnal time scales (Donner and Phillips, 2003).

Treatments are absent or extremely limited of interactions between deep convective towers and mesoscale circulations, interactions between convection and boundary layers, and interactions between deep and shallow convection. In many cases this is despite compelling observational evidence of the importance of these interactions; for example, observational evidence of the importance of mesoscale circulations associated with deep convection has existed for at least 20 years.

The sociological reasons for this situation probably can be found by examining the role of convection in AGCM development at major modeling centers. This development is strongly driven by goals of reducing biases in climate simulations and reducing uncertainty in climate sensitivity (to anthropogenic changes in atmospheric composition). Convection (and most other physical processes) tend to be viewed as tools toward reducing these biases and uncertainties. Since it is often unclear which physical processes are responsible for particular biases and sensitivity uncertainty, there has been a serious lack of focus on improving the fundamental physical soundness of convective parameterizations and an emphasis on tuning physical parameterizations to produce realistic climate simulations.

Recent experience at major modeling centers suggests that the tuning approach with current convective parameterizations may be reaching limits as to its usefulness in attacking model biases. The unphysical nature of the tuning process is also increasingly apparent and unsatisfactory to its practitioners. It is also clear that tuning to past or present climate conditions may not capture future climate change and is thus of limited use as a means of reducing uncertainty in climate sensitivity.

There are possibilities for advancing current parameterization capabilities using observations and high-resolution model results, but this will require enhanced efforts. A particularly promising avenue is emerging with current computational advances. This approach is multi-scale embedding of convection-resolving models in AGCMs (Randall et al., 2003). This is a major conceptual advance on past methods, which have required that central controls on the problem be based on closure assumptions, whose very existence has never been established. The embedded convection-resolving models draw on well-established dynamics of convection and rationalize many of the choices on how to treat issues related to cloud submodels. They have an enormous potential to indicate outstanding research issues that must be addressed, for example, the roles of microphysics and smaller-scale circulations.

PHYSICS OF AIR-SURFACE INTERACTIONS AND COUPLING TO OCEAN-ATMOSPHERE BOUNDARY LAYER PROCESSES

Chris Fairall, National Oceanic and Atmospheric Administration

My remarks are limited to consideration of parameterizations of fluxes at the air-sea interface. The fluxes of interest include the traditional meteorological forcing (momentum, sensible heat, and latent heat), precipitation, and net solar and infrared radiative fluxes. For climate purposes we must expand our considerations to include fluxes of trace gases (e.g., CO_2, DMS, ozone) and particles. Oceanographic and meteorological energy and buoyancy forcing are made up with different weightings of the basic fluxes.

Precipitation is a critical variable, but in the global climate modeling context is not parameterized in terms of surface variables. Radiative fluxes at the surface do involve surface variables, but the principal source of variability (and uncertainty) is clouds. Although surface radiative flux parameterizations do exist, again from a climate model point of view the surface radiative flux is viewed as part of the entire atmospheric column problem.

For turbulent fluxes (meteorological, gas, and particle), the bulk flux model, where fluxes are computed as the product of wind speed, sea-air contrast, and a semi-empirical transfer coefficient, is essentially universally used in GCMs (and in higher-resolution models). In the last decade advances in ship-based measurement technologies and in physically-based formulations of bulk models have resulted in major progress. Meteorological transfer coefficients are now known, on average, to about 5 percent for wind speeds from 0 to 20 m s^{-1}. In the last five years, direct covariance measurements of CO_2 flux from ships have reduced the uncertainty in CO_2 transfer significantly but have also illuminated the importance of (presumably) wave-breaking processes. Computations of global mean oceanic CO_2 flux show large sensitivity (factor of 2) to the choice of simple wind-speed-based transfer formulations.

Direct measurement of particle fluxes is still exploratory, and interpretation of such measurements is uncertain and particle size dependent. Small-particle (radius $r < 1$ micron) fluxes can be determined with reasonable accuracy using direct covariance measurement, while large-particle ($r > 10$ micron) fluxes can be effectively determined from mean concentrations. A number of parameterization issues for surface fluxes are listed below:

- Representation in GCM
  - Except for P, most observations are point time averages
  - Concept of gustiness sufficient?
  - Mesoscale variable? Precipitation, convective mass flux, …
- Strong winds
  - General question of turbulent fluxes, flow separation, wave momentum input

  - Sea spray influence
- Waves
  - Stress vector versus wind vector (two-dimensional wave spectrum)
  - z_o versus wave age and wave height
- Breaking waves
  - Gas and particle fluxes
  - Distribution of stress and TKE in ocean mixed layer
- Gas fluxes
  - Bubbles
  - Surfactants (physical versus chemical effects)
  - Extend models to chemical reactions
- Particle fluxes
  - Interpretation of measurements
  - Source versus deposition

The central theme is that the parameterizations are in good shape in regimes where we have good observations. Note that wave processes (on the oceanic and atmospheric sides of the interface) dominate this list.

Existing research programs in the United States provide a good venue for attacking many of these issues, but there are major gaps. The international Surface Ocean Lower Atmosphere Study (SOLAS) is the showcase program for fundamental research (measurement and modeling) on many of these topics. Research on oceanic gas transfer suffers from fragmented sources of support, but the large, highly organized U.S. Carbon Cycle program has (in my opinion) too much emphasis on observations to constrain oceanic PCO_2. Unfortunately, the U.S. SOLAS program has lost momentum, and there is at present no U.S. agency funding lead. The withdrawal of the Office of Naval Research as a major player in funding ocean wave and particle flux research has also hurt. Because the gas transfer, particle, and wave problems are all tied together, it makes sense to address these problems in a holistic fashion, which requires an initiative such as SOLAS.

Overall, the U.S. research effort in this area has major strengths in top scientists and some unique infrastructure. There are clear concerns about the aging workforce and uncertainties about the training of the next generation of flux scientists (this is part of a general pattern of a declining population of students interested in getting their hands dirty). There is another issue of concern to me—that is, the recent trend of changing the emphasis of the national government laboratories toward performance measures, deliverables, and products and away from strategic technology development. This is partly based on the failed concept that operational segments of NOAA, the Department of Energy, etc., can simply order up from private industry the technology they think they need. The problem is that funds tend to move down agency stovepipes, but fundamental advances in technology and science cannot be confined to the same stovepipe going back up.

OCEAN MIXING

Raffaele Ferrari, Massachusetts Institute of Technology

Much of the influence of the ocean in climate involves the oceanic uptake and transport of scalar properties such as heat, fresh water, carbon, fluorocarbons (CFCs), and the like. On daily to decadal time scales, this uptake and transport is controlled by upper ocean processes occurring in the surface mixed-layer and the wind-driven gyres. The turbulent mixing in the surface mixed-layer sets the rate at which properties are exchanged between the ocean and the atmosphere. The wind-driven gyres transport meridionally these properties, once the surface waters are subducted in the interior. In this session we will discuss what the key subgrid-scale processes are, which need to be parameterized to properly simulate the uptake and transport of properties in ocean models used for climate studies.

In present climate models, the ocean horizontal grid resolution is $O(100)$ km or larger, and the vertical grid resolution is tens to a hundred meters. At this resolution the subgrid ocean processes that need to be parameterized can be divided into two categories:

- mesoscale eddy fluxes due to balanced motions generated through instabilities of the mean circulation and
- microscale turbulent processes due to unbalanced turbulent motions such as breaking internal waves, shear instabilities, double diffusion, or boundary layer mixing near the surface and bottom.

More powerful computers may decrease these scales to a marginal mesoscale eddy resolution of $O(25)$ km in the next 10 years, but horizontal grids of better than $O(10)$ km are needed to adequately resolve the fluxes produced by mesoscale motions. Even marginal mesoscale eddy resolution, sometimes called "eddy-permitting" resolution, requires some parameterization of the missing eddy transports. This has elicited a large literature in the last 10 years on parameterization schemes for mesoscale eddies in the oceanic interior and microscale turbulence in the surface mixed layer. This has not always been the case. One of the most significant problems with early ocean models was the high level of microscale turbulent mixing inherent in the numerics. These high levels of mixing affected the heat transport, stability, and variability of simulated climate in coupled models. Thus the emphasis in model development was on hydrodynamic codes. New numerical methods now allow scientists to construct models, which can operate with far smaller levels of spurious mixing.

Although research continues on improving the hydrodynamic codes, it is clear that modern ocean models suffer more from errors in the parameterization of subgrid-scale motions than from errors in the simulation of resolved hydrodynamic processes.

Parameterizations of mesoscale processes in oceanic general circulation models represent the adiabatic release of potential energy by baroclinic instability as well as the stirring and mixing of material tracers along isopycnal surfaces (Gent and McWilliams, 1990). These quasi-adiabatic conservation properties have led to a series of dramatic improvements in oceanic models. However, close to the boundaries, eddy fluxes develop a diabatic component, both because of the vigorous microscale turbulence in boundary layers and because eddy motions are constrained to follow the topography or the upper surface, while density surfaces can and often do intersect the boundaries. The dynamics of these diabatic near-boundary fluxes are not well understood, and there is as yet no standard parameterization. Recently a CPT has been funded to develop new approaches to mesoscale eddy parameterizations at the ocean boundaries, based on better dynamical understanding and analysis of available observations.

The physics of microscale turbulence in the oceanic boundary layers is the subject of a vast literature and parameterizations exist. Less is known about microscale turbulence in the ocean interior, and parameterizations are very rudimentary. The difference in development between the two fields has historical and practical reasons. The boundary layer problem benefited from the similarities with the well-developed corresponding atmospheric problem. Furthermore, surface boundary layers are fairly accessible and observations are available to test the proposed parameterization schemes. The situation is opposite for microscale turbulence in the ocean interior. The physics is very different from the atmospheric case, mostly because of the lack of radiative processes. Observations are very sparse and do not allow careful testing of numerical schemes. Ray Schmitt gives a comprehensive review of the progress being made on these issues. However, the conclusion is that more research is needed if we are to quantify the effects of interior microscale turbulence on the ocean's climate and develop appropriate closure schemes to reproduce those effects.

REPRESENTATIONS OF DIAPYCNAL MIXING IN OCEAN MODELS

Raymond Schmitt, Woods Hole Oceanographic Institution

Although it is recognized that the diapycnal mixing coefficient for heat, salt, and tracers is much less than the isopycnal mixing rate, fluxes may actually be larger because vertical gradients are so much larger than horizontal gradients. In addition, it is well established that diapycnal mixing is essential for maintenance of the meridional heat flux in the ocean; only the water mass transformations connected with diapycnal mixing can allow new dense water to enter the ocean depths. It has been conventional to assign a uniform diapycnal mixing coefficient to all of the ocean interior, tuning its value to yield a reasonable thermohaline circulation. However, recent observations have revealed tremendous dynamic range in the rates of turbulent vertical mixing, with mixing coefficients varying by four orders of magnitude within a small region of an ocean basin. Fortunately, there are strong indications that tides, topography, and internal wave dynamics control the rate of interior ocean mixing, so significant progress toward its parameterization appears quite feasible. Model runs show dramatic differences in circulation and meridional heat flux when a spatially variable mixing rate is introduced.

Tracer release experiments have provided convincing evidence of the spatial variations in turbulent mixing rate and have also shown that in some regions the rates of mixing of heat and salt are different due to double-diffusive convection. In the main thermocline of the tropical Atlantic, salt and tracers mix at a high rate of 1 $cm^2 s^{-1}$, which appears to be twice the rate for heat. This region is dominated by strong thermohaline staircases, the dynamics of which are poorly understood. The inverse case of diffusive convection, with heat mixing faster than salt, appears to dominate fluxes in large portions of the Arctic, which must influence the rate at which oceanic heat is made available for melting sea ice. Both forms of double diffusion occur in fine-scale intrusions and serve to provide a diapycnal flux from isopycnal stirring processes. Much work remains to properly parameterize double-diffusive mixing, with theoretical, experimental, observational, and numerical approaches needed.

These and other ocean mixing processes are discussed in a white paper titled "Coupling Process and Model Studies of Ocean Mixing to Improve Climate Models—A Pilot Climate Process Modeling and Science Team" (Schopf et al., 2003). Issues treated include equatorial upper ocean mixing, surface boundary layer processes, entrainment in gravity currents, mixing in the Southern Ocean, geography of internal wave mixing, and deep convection and restratification. The purpose was to promote Climate Process and Modeling Teams (CPTs) for U.S. CLIVAR to focus on these mixing topics. However, only two topics were initiated at very modest levels, and no consideration was given to field work. There is a serious shortfall in funding for field work, as the Office of Naval Research has ceased to be a significant funder of turbulence studies, NOAA never was, and NSF is under tremendous budgetary pressures. The situation is reaching a critical stage, as capabilities to perform crucial turbulence measurements and tracer release experiments may soon be lost.

CLOUD PROCESSES

Steve Sherwood, Yale University

Clouds have been recognized as the key source of divergence in model climate sensitivity. Particularly important are the height of middle- and upper-level cloud tops (affecting planetary emission to space), the amount of thin cirrus (ditto), the water content of all clouds (affecting planetary albedo), and the areal coverage of low clouds (ditto). Convincing theories do not exist for predicting most of these characteristics from first principles, since they ultimately depend on details of convective transport and microphysical behavior (although the altitude of the highest cloud tops in the tropics may be constrained by radiative cooling, as recently suggested by Hartmann and Larson, 2002).

A literature search reveals that attention to the treatment and role of clouds in GCMs really took off at the beginning of the 1990s, at around the time their importance to climate change uncertainty became widely recognized. Hot topics at that time were cloud feedbacks and attempts to relate observed cloud variations to the local thermodynamic state. This work did not lead to resolution of the main problems, since observed cloud variations are overwhelmingly controlled by dynamics, to a degree that a very precise understanding of dynamics would be needed to tease out the small but persistent impacts of thermodynamic or other subtle factors that might come into play in climate change. More recent work has shifted emphasis somewhat to microphysical forcing (e.g., aerosols) of clouds, chemical roles of clouds (e.g., processing of atmospheric sulfate), and underobserved but potentially important cloud types (thin cirrus and contrails).

In GCMs the cloud and convective physics are customarily separated. The cloud scheme must predict six variables: cloud fraction, liquid and ice concentration, liquid and ice effective radius, and single-scatter asymmetry parameter (for the ice, depends on shape). These variables are needed primarily for radiation. Prediction has proceeded through three phases: initially (1960s to 1970s), all were specified according to observations. Later (1980s to 1990s), models started diagnosing variable cloud cover and water content based on (primarily) the local relative humidity and (typically) temperature and/or height, respectively. This diagnosis was often unconnected to what the cumulus parameterization was doing, leading to the possibility of vigorous convection, but no clouds, in a grid cell (some parameterizations allowed for convection-dependent diagnosis rules making a very crude connection). Now, most models carry total cloud water (and sometimes either cloud cover or higher moments of the cloud water distribution) as prognostic variables, with better consistency between the convective and cloud schemes. Many (reasonable) ad hoc assumptions are required. One next step is super-parameterization, in which a Community Regional Model (CRM) is run inside each global grid location.

The choice of how to diagnose cloud water content illustrates the climate problem in a nutshell. Observations show that clouds at higher altitudes (lower temperatures) contain less water, roughly in accord with the decrease in saturation water vapor concentration. One can, among other options, choose to diagnose water in a model either from local temperature or height. This choice was considered by Hack et al. (1998) in the NCAR CCM2, who altered the (height-dependent) default scheme by making it temperature dependent. The resulting model cloud climatology changed relatively little compared with model data discrepancies. However, as pointed out by Somerville and Remer (1984), the assumption of temperature-dependent water content leads to a strong negative feedback on climate change due to the increase in global cloud albedo that accompanies global warming. Thus, the climate sensitivity is sensitive to a parameterization choice that cannot be justified one way or the other on empirical grounds but must be defended on the basis of basic system understanding. Though this type of problem motivates increasing the complexity of schemes, I believe that a well-supported argument in favor of a particular diagnostic parameterization may ultimately be better than a more sophisticated parameterization that pushes the uncertainty back to lower-level constants and unsupported assumptions. On the other hand, it may prove essential to carry cloud water in the model and perhaps other things too (TKE, for example).

In general, the ability to predict behavior in a situation not previously experienced is commensurate with understanding. The above example demonstrates that basic understanding is essential to climate prediction. We do not have enough previous examples of climate change (in fact, any, in the case of anthropogenic climate change) on which to proceed otherwise. One can be more confident that one understands the system if one can find elegant models (those with a high a priori probability of being correct) that are nonetheless powerful in explaining previous observations. In Bayesian terminology, the posterior probability of a model being correct is proportional to the product of the prior probability (i.e., elegance as judged by an expert) and the likelihood function (i.e., ability to explain evidence). This leads us to the necessity of developing simple (elegant) models as part of a hierarchy, as advocated by Hoskins (1983).

In my view the scientific method has somewhat fallen by the wayside as a guide to climate researchers. The key to progress is the formulation of testable and understandable hypotheses (simpler models), which can then be tested by comparing the behavior that they predict should occur in a comprehensive model with the actual behavior of such a model. It is not necessary to understand the comprehensive model per se, only to document that its behavior is consistent (or not) with the simpler theory. This can also be called "modeling models," except that the goal is not to understand the model being modeled (as might be implied by that choice of words) but to test the validity of the simpler model in the face of complexities that were not considered in its formulation.

To the extent that the simple models can serve as parameterizations, this process also provides the basis for GCM development.

A perusal of the literature on clouds and GCMs reveals that exploring model sensitivity and making predictions accounts for nearly all GCM-related research, especially recently, with hypothesis testing or offering new explanations for observations occurring only rarely. The problem with the more popular types of GCM study is that they depend heavily on the fidelity of the GCM for their value, rendering their value questionable given the manifold weaknesses of present-day GCMs (parameter sensitivity in a model does not prove sensitivity in the real world to the corresponding process). The hypothesis-testing and observation-explaining paradigms are inherently more robust since they connect the model to something else.

Due to the complexity of the cloud problem, progress will depend not only on a complexity hierarchy of models but also on a scale hierarchy in which the behavior of microphysical parcel models, eddy- and cloud-resolving, regional, and global models are connected. A first principles microphysical calculation of hydrometeor growth in a single deep convective updraft is roughly as complicated as a climate calculation with a GCM. But this is not the only problem. Cloud microphysics suffers from significant gaps in our basic understanding, including our inability to agree on (or in some cases even invent) explanations of the following: anomalous ice particle concentrations observed in some clouds, precipitation from very shallow cumuli, cumulus electrification, and persistent supersaturations of water vapor with respect to ice near the tropopause even within cirrus clouds. Thus, we are not simply limited by computational power. Reinvigoration of cloud and ice microphysics is desperately needed; experimental and field observations are far too few (and satellite information too limited) to resolve the questions. On the positive side I believe we are poised, through CRMs with relatively detailed microphysics, to learn much in the not-too-distant future about how previously neglected microphysical degrees of freedom may be affecting macroscopic convective behavior. This may require a new look at convective parameterization.

The key question, assuming reasonable hypotheses can be advanced for the unresolved fundamentals, will be how to connect the scale hierarchy of models usefully. A promising new strategy is super-parameterization. This will be too expensive to do except in research mode, but fully coupling adjacent models in the scale hierarchy may be the secret to unlocking workable strategies for developing good parameterizations. The necessity of this approach becomes even greater if, as many fear, universally correct parameterizations for convective and cloud processes do not exist and specific ones must be tailored to each GCM.

Bringing the philosophical and physical issues together, we are faced with the following questions:

1. Do universal parameterizations exist for convective and cloud processes?
2. How much microphysical detail is enough? How do we tell?
3. What microphysical mechanisms and/or degrees of freedom are important to weather and climate? How do we discover them?
4. How can we develop sensible, testable hypotheses to aid us in making sense out of terabytes of model output and/or satellite data? How important is this?

CHALLENGES IN RATIONAL CLIMATE SCIENCE

Bjorn Stevens, University of California, Los Angeles

The challenge in representing physical processes in coupled atmosphere-land-ocean models is foremost a political, not a scientific, one. Our understanding of numerical methods and physical processes far outstrips our ability to systematically implement, test, and evaluate representations of this understanding in large-scale models. Moreover, work of the latter type has little reward for those evaluated using the norms of academia. Building large-scale models is a social enterprise that involves the cooperation and interaction of many communities. It also requires an infrastructure to which we appear unwilling to commit. Imagine if state-of-the-art numerical weather prediction (NWP) models, such as have been developed by the United Kingdom's Met Office, or the European Centre for Medium-Range Weather Forecasts, had been developed using our social model for the development of climate models. It is not a coincidence that the most successful NWP models have been produced by centers with well-defined goals, staff with long-term support who are evaluated based on their ability to meet these goals, and fertile interactions with the broader scientific community. The central challenge in representing physical processes in A-L-O models is to overcome this structural deficit and develop the institutions capable of harvesting the immense, but often unstructured, insights of the broader academic community.

Cultural challenges are also evident, in particular how to attract talented and innovative people to the field. There is a perception, which resonates with my experience at the University, that analytically gifted people are not attracted in sufficient numbers to the meteorological and oceanographic sciences. This is a profound problem; to address it we need to capture the public imagination by highlighting the mystery and majesty of our field and forge strategic alliances with those more recognizable areas toward which more analytically talented students tend to gravitate (i.e., math and physics). One way of recapturing our imagination is by collectively recognizing challenges, or outstanding problems, and devoting resources and prestige toward their resolution. I am fond of the model of the Clay prize in mathematics, for which the community partakes in a collective act of question stating. Overall, however, progress on this front is bound to be incremental.

From a physical perspective, the challenge is how to exploit technological advances in an attempt to improve the physical basis for the representation of physical processes in A-L-O models. An indispensable strategy is to use observations to identify regimes (i.e., recurrent patterns), simulations to understand, and observations to confirm our understanding of such regimes[2]. The use of numerical simulation invariably involves the solution of equations whose fidelity to the physical system is questionable, usually because limited numbers of degrees of freedom invariably require the simulation to represent the aggregate effect of some number of unresolved processes (e.g., small-scale turbulence, microphysical, radiative, chemical, or biological processes). Thus simulation is necessarily approximate, and its use requires judgments. Such judgments are typically rendered in one of two ways:

- a priori: wherein the simulations are evaluated based on the fidelity of the underlying approximations being used. Here one recalls the rich literature, primarily within the engineering community, dealing with representation of unresolved scales in LES. Many believe that A-L-O models ultimately must be rationalized in a similar way.
- a posteriori: here the simulations are evaluated based on their ability to represent benchmark flows or suggest new phenomena that are subsequently found in nature. An underappreciated branch of a posteriori testing is what one might call discovery.

Neither strategy can be used in isolation. To the extent that the underlying equations are approximate, a posteriori tests will always be the ultimate measure of the degree of fidelity of particular aggregation hypotheses. To the extent that one deviates from any given benchmark regime, a priori statements of accuracy become increasingly important. Both strategies provide new and fertile territory for observation and experiment, although for many flows only one or the other strategy avails itself. Because of its interplay with both strategies, LES of atmospheric boundary layer flows is unique among the many types of flow simulation. Typically its equation sets are the best justified, as they often retain the actual flow as a limit, and the flows it simulates often encompass scales that are likely to allow for benchmark solutions to be directly observed or perhaps reproduced in the laboratory (i.e., wind tunnel tests of flow over terrain, the development of dry convective boundary layers, relatively homogeneous cloud-

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[2] Although much is made of Moore's law and its implication for simulation, the impact of this exponential has been nearly as profound on observational technology, especially remote sensing. Remote sensing's ability to resolve flow features in multiple dimensions and over a variety of scales complements these recognized attributes of simulation.

topped boundary layer flows such as are evident in stratocumulus regimes or the trades). Because it naturally distills many of the questions and strategies that might be used to rationalize other forms of simulation, it provides a useful lens through which to address the broader challenge stated above.

In studies of cloud-topped boundary layers, LES has been used to great effect to refine our understanding of important cloud regimes. We are only beginning to use observations to test this understanding, with the recent DYCOMS-II field experiment being an example of an a posteriori test in the stratocumulus regime; the pending RICO field program is designed to do the same for the trade cumulus regime. DYCOMS-II has demonstrated that LES is profoundly sensitive to the representation of small-scale turbulence near phase boundaries and in the presence of intense stratification and in so doing raises the profile of such questions for the observational and experimental communities. Nonetheless, the interplay between observation and simulation continues to be extraordinarily fruitful for studies in the atmospheric boundary layer. Given a sufficiently nurturing political and cultural environment, A-L-O models should begin reaping these fruits in the coming decade.

As a supplement to this note I also offer two articles, one published and one in preparation. The first gives a more philosophical overview on the use of LES and its relation to other more traditional ways of doing science; the second shows the example of a posteriori testing referred to above: (1) B. Stevens and D. H. Lenschow, 2001, "Observations, experiment and large-eddy simulation," Bull. Amer. Meteorol. Soc. 82:283-294 and (2) B. Stevens et al., 2004, "Observations of nocturnal stratocumulus as represented by large-eddy simulation," Mon. Wea. Rev. (in press).

INTERROGATION AND PARAMETERIZATION OF ATMOSPHERIC AND OCEANIC BOUNDARY LAYER PROCESSES

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The atmospheric and oceanic boundary layers (ABL and OBL) are shallow but critical components of geophysical flows. In these thin layers turbulent mixing promotes the exchange of momentum and scalars between the atmosphere and land surfaces and couples the atmosphere and ocean at the air-sea interface. The ABL and OBL respond to large-scale forcing by generating three-dimensional, time-dependent turbulent motions. They are rich in structure with the important turbulent coherent structures varying with stratification: large-scale thermal plumes fill the ABL under unstable stratification; elongated streaky structures and hairpin vortices dominate near wall flow dynamics in neutral flows; and intermittent small-scale structures control mixing in the stable regime. In the marine boundary layers, surface gravity waves are critical components. They are visible signatures of coupling between the atmosphere and ocean and promote global mixing in the OBL through the formation of Langmuir circulations and wave breaking. Progress in understanding boundary layer mechanics is being advanced through field observations, laboratory studies, and numerical simulations. High-Re (Reynolds number) large-eddy simulation is one of the important tools employed in current boundary layer research.

The enormous spectrum of scale interactions in high-Re turbulent flows and our inability to solve the governing dynamical equations require that empirical parameterizations be developed for boundary layer flows. In the case of large-scale climate and mesoscale codes, the parameterizations are by necessity ensemble average (or Reynolds average) closures that represent bulk features of the ABL and OBL, boundary layer depth, surface fluxes, and entrainment rates. In general, bulk boundary layer parameterizations are not well tested and can break catastrophically for certain flows—for example, stable boundary layers. The stable ABL parameterization is a prime candidate leading to the poor performance of large-scale numerical weather prediction models in cold regions. Also, new boundary layer dynamics—for example, combining Langmuir circulations and wave breaking in the OBL—can lead to nonlinear behavior. Thus boundary layer closures need to be tested over a wide set of mixed flow regimes.

LES is not immune from the subgrid-scale parameterization problem. However, by design LES computes the large-scale most energetic turbulent motions with a subgrid-scale closure for the small scales. In well-resolved regions of a turbulent flow the subgrid-scale motions are small and LES solutions are generally insensitive to the details of the closure. However, flows with laminar-to-turbulent transition, strong stable stratification, or near-solid boundaries, the subgrid motions can become large and their impact for LES solutions is generally poorly understood. Hence a deeper understanding of subgrid-scale parameterizations and their interactions with resolved scales is also required for LES. Improving LES parameterizations requires moving away from traditional eddy-viscosity approaches. Novel field campaigns, such as the Horizontal Array Turbulence Study (HATS), theoretical approaches, and direct numerical simulation can all be used to provide insight and databases to evaluate new subgrid-scale closures for LES. This research avenue is clearly exciting as it employs a beautiful blend of theory, experimentation, and numerical modeling.

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The Gap Between Simulation and Understanding in Climate Modeling

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January 13, 2004

ABSTRACT

The problem of creating truly convincing numerical simulations of our Earth's climate will remain a challenge for the next generation of climate scientists. Hopefully, the ever-increasing power of computers will make this task somewhat less frustrating than it is at present. But increasing computational power also raises issues as to how we would like to see climate modeling and the study of climate dynamics evolve in the 21st century. One of the key issues we will need to address is the widening gap between *simulation* and *understanding*. A change in emphasis in theoretical climate research is needed if we are to close this gap.

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

[1] Reprinted with permission from Isaac Held. A revised version of this paper can be found at *http://www.gfdl.noaa.gov/~ih/*. Accessed March 22, 2005.

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 in quality, they more and more often 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?

Without attempting an all-encompassing definition, 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 has often been emphasized. See Hoskins (1983) for a particularly eloquent discussion. But despite notable exceptions in a few subfields, climate theory has not, in my opinion, been very successful at hierarchy construction.

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 actions and 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 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 the biological sciences to be presented with precisely the kind of hierarchy needed to understand a complex system! Imagine how much progress would have been made if one 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 us with some insights into the range of behaviors possible, but they are few in number, and each planet has its own idiosyncrasies. While their study has connected to terrestrial climate theory on occasion, the influence has not been systematic. Laboratory simulations of rotating and/or convecting fluids remain a valuable and underutilized resource, but they cannot address many of 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 towards 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. One can justify studying *E. coli* not only because it shares many fundamental genetic mechanisms with all cells, but also because it exists, after all, and it and its close bacterial relatives affect the world in ways that are worth understanding at the molecular level in their own right. Elements of a climate model hierarchy are generally only of interest to climate theorists.

A biologist need not convince her colleagues that the model system 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 substantial parts of their careers 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 21st 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 some climate theorist?

Suppose that one can divide a climate model into many small distinct components and that one can devise a testing and development strategy for each of these modules in isolation (including the form of the interactions among these modules). 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 strategy is without doubt an appropriate and efficient approach to model development. Understanding is needed at the level of the module in question, so as to ensure its fidelity to nature, but is there understanding to be gained as a higher, more holistic level, that is of value 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 (for example, with regard to water vapor continuum absorption), this new information makes its way more or less efficiently into the broadband climate model codes. Given this relatively convincing methodology, the (clear-sky) radiative flux component of climate models is generally treated with respect, evolving only when driven to do so by evidence of the sort outlined above.

Work towards devising similar methodologies for other model components is obviously of vital importance. But 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 towards appropriate closures for deep moist convection, as an important example, there is little sense of convergence among existing atmospheric models. A program in which cloud-resolving simulations are systematically used as a middle ground between closure schemes and observations promises to improve this situation in the future, but there is still a long way to go.

When a fully satisfactory systematic bottom-up approach to model building 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!) Various ideas are put forward by the team building the model, based on their wisdom and experience, as well as their idiosyncratic interests and prejudices. To the extent that a modification to the model based on these ideas helps ameliorate a significant model deficiency, even if it is, serendipitously, a different deficiency than the one providing the original motivation, it is accepted into the model. Generated by these informed random walks, and being evaluated with different criteria of merit, the comprehensive climate models developed by various groups around the world evolve along distinct paths.

The value of a holistic understanding of climate dynamics for model development is in making this process more informed and less random, and thereby more efficient. To the extent that we have little understanding of which aspects of a moist convection scheme are most important for exaggerating the double ITCZ in the East Pacific, or which help control the period of ENSO, then our search for ways to ameliorate our double ITCZ or improve our ENSO spectrum will be that much more random and less informed.

A holistic understanding of climate dynamics also helps in relating one comprehensive model to another. If stratosphere-troposphere interactions in one comprehensive model result in a trend in the North Atlantic Oscillation as a result of increasing carbon dioxide, but not in other models, how does one judge which is correct? Perhaps by confronting the models with observations—but precisely how does one do this? All models are imperfect, but which imperfections are most relevant to this problem? Inevitably, one needs to understand the differences between the models at some level. One can try to systematically and laboriously morph one model into the other, and heroic attempts of this kind have been attempted in various contexts and can be informative. Alternatively, one can construct more idealized models designed to capture the essence of the interaction in simpler systems, within which the climate dynamics community can focus more directly on the central issues. These idealized studies can then suggest optimal ways of categorizing or analyzing more comprehensive models.

THE FUTURE OF CLIMATE THEORY

Accepting that the kind of understanding that emerges from the construction and analysis of climate model hierarchies is important, and given the many efforts under way that are devoted to models of various levels of complexity, are there things we could do to make this effort more productive? I highlight two related tendencies that have slowed the systematic development of climate model hierarchies. (My own work illustrates these two tendencies nicely, and this discussion is as much a self-critique as it is one of the field more generally.)

Conceptual Research versus Model Design

There is a tendency in our field for a theoretically inclined researcher to design and build a model—on the basis of which he or she tries to create the case for some picture of a phenomenon or for the utility of some concept or approach—and then drop the model. The model is not intended, in many cases, to have a 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. This tendency exists in those working with models at all levels of complexity. The focus is on the concept being put forward, not the model itself.

I do not mean to minimize the importance of the search for new concepts of general utility—this will always be one of the primary goals of all theoretical work. But we cannot limit ourselves to this conceptual approach. The claim is that the complexity of the climate system is such that we cannot make systematic progress towards understanding climate dynamics other than through model hierarchies. The design and refinement of these hierarchies require the accumulated wisdom of an assortment of scientists with different skills and incentives, and must in themselves be a central goal of our research.

Elegance versus Realism

Our goal must be to reduce the number of models that we analyze. Otherwise we are left with a string of interesting results, few of which have been intensively examined by more than two or three people, and which we never quite manage to relate to each other. (The body of theoretical work on the Madden-Julian Oscillation provides a good example of this problem, in my opinion.) But how can this inefficient deployment of our theoretical resources be avoided? The key, I feel, is *elegance*.

An elegant model is as elaborate as it needs to be to capture the essence of a particular source of complexity, but 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? Why not change the model to better suit by specific interests?

Over-elaboration results in part from the pressure we all feel for our work to be relevant to the big issues in climate dynamics. This relevance generally requires a certain level of realism in one's simulations, and this pressure to reach the required level of realism often pushes models towards ever-increasing elaboration. Yet, in the process one's model often loses much of its attraction to other researchers.

We justify our research, to ourselves and others, by appealing to 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 given existing resources. These simulations need be of no lasting value, as they will be supplanted by ever more comprehensive models as computer resources increase. When global nonhydrostatic atmospheric models resolving deep moist convection become common in future decades, the global warming simulations obtained with the current generation of models 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, as a large number of details may very well be needed to get things right quantitatively.

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, partly for the sake of a 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 computational 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.

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 will have become obsolete.

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 it is evident that research with a hierarchy of models is needed. But to be successful, this work must make progress towards 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 built on our ability to simulate. On the other hand, it must proceed more systematically towards 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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Workshop Agenda

Challenges in Representing Physical Processes in Coupled Atmosphere-Land-Ocean Models: A Workshop

July 12-13, 2004
J. Erik Jonsson Woods Hole Center of the
National Academy of Sciences

MONDAY, JULY 12, 2004

8:30 AM	Welcome, Introduction, and Purpose of Workshop, Kerry Emanuel and John Wyngaard, *Co-chairs*
9:15 AM	**GAP BETWEEN SIMULATION AND UNDERSTANDING IN CLIMATE MODELING**, Isaac Held, *NOAA-GFDL/Princeton University*
10:15 AM	Break
10:30 AM	**DISCUSSION SESSION 1—CONVECTION AND CLOUDS** Rapporteur: Dave Randall Discussion leaders: • Convection—Leo Donner, *NOAA-GFDL/Princeton University* • Clouds—Steve Sherwood, *Yale University*
NOON	Lunch
1:00 PM	**DISCUSSION SESSION 2—GRAVITY WAVES** Rapporteur: Yuk Yung Discussion leader: • Atmosphere—Joan Alexander, *Colorado Research Associates*

2:30 PM **DISCUSSION SESSION 3—OCEAN MIXING**
Rapporteur: Jim McWilliams
Discussion leaders:

- Eddy fluxes—Raffaele Ferrari, *Massachusetts Institute of Technology*
- Diapycnal material fluxes—Raymond Schmitt, *Woods Hole Oceanographic Institution*

4:00 PM Break

4:15 PM **DISCUSSION SESSION 4—PLANETARY BOUNDARY LAYER**
Rapporteur: John Wyngaard
Discussion leaders:

- Cloud-topped PBLs—Bjorn Stevens, *UCLA*
- Dry PBL—Peter Sullivan, *NCAR-MMM*

5:45 PM Adjourn

TUESDAY, JULY 13, 2004

8:30 AM Brief description of day's events, Emanuel and Wyngaard

8:45 AM **DISCUSSION SESSION 5—LAND-ATMOSPHERE INTERACTIONS**
Rapporteur: Julie Demuth
Discussion leaders:

- Carbon cycles—Scott Denning, *Colorado State University*
- Hydrologic cycles—Alan Betts, *Atmospheric Research*

10:15 AM Break

10:30 AM **DISCUSSION SESSION 6—ATMOSPHERE-OCEAN INTERACTIONS**
Rapporteur: Kerry Emanuel
Discussion leader:

- Chris Fairall, *NOAA-ETL*

NOON Lunch

1:00 PM **GUIDANCE FOR THE FUTURE**, Dennis Hartmann, *University of Washington*

1:45 PM	**DISCUSSION—WHERE DO WE GO NOW?** • Lessons learned • Guidance for future action
3:30 PM	Break
3:45 PM	**CONTINUED DISCUSSION**
5:30 PM	Adjourn

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Workshop Participants

Committee

Kerry Emanuel (*Co-chair*), *Massachusetts Institute of Technology, Cambridge*
John Wyngaard (*Co-chair*), *Pennsylvania State University, University Park*
James C. McWilliams, *University of California, Los Angeles*
David A. Randall, *Colorado State University, Fort Collins*
Yuk L. Yung, *California Institute of Technology, Pasadena*

Guests

Joan Alexander, *Colorado Research Associates, Boulder*
Robert Beardsley, *Woods Hole Oceanographic Institution, Massachusetts*
Alan Betts, *Atmospheric Research, Pittsford, Vermont*
Antonio Busalacchi, Jr., *University of Maryland, College Park*
Walter Dabberdt, *Vaisala Inc., Boulder, Colorado*
Scott Denning, *Colorado State University, Fort Collins*
Leo Donner, *NOAA-Geophysical Fluid Dynamics Laboratory and Princeton University, New Jersey*
James Edson, *Woods Hole Oceanographic Institution, Massachusetts*
Chris Fairall, *NOAA-Environmental Technology Laboratory, Boulder, Colorado*
Raffaele Ferrari, *Massachusetts Institute of Technology, Cambridge*
Colleen Hartman, *NOAA-National Environmental Satellite and Data Information Service, Camp Springs, Maryland*
Dennis Hartmann, *University of Washington, Seattle*

Isaac Held, *NOAA-Geophysical Fluid Dynamics Laboratory and Princeton University, New Jersey*
Rui Xin Huang, *Woods Hole Oceanographic Institution, Massachusetts*
Houshuo Jiang, *Woods Hole Oceanographic Institution, Massachusetts*
Stephen Lord, *NOAA-National Centers for Environmental Prediction, Camp Springs, Maryland*
Robert Schiffer, *University of Maryland, College Park*
Raymond Schmitt, *Woods Hole Oceanographic Institution, Massachusetts*
Robert Serafin, *National Center for Atmospheric Research, Boulder, Colorado*
Steven Sherwood, *Yale University, New Haven, Connecticut*
Bjorn Stevens, *University of California, Los Angeles*
Peter Sullivan, *National Center for Atmospheric Research, Boulder, Colorado*
Lynne Talley, *Scripps Institution of Oceanography, San Diego, California*
Samuel Williamson, *Federal Coordinator for Meteorology, Washington, D.C.*

Staff

Julie Demuth, Study Director
Sheldon Drobot, Program Officer
Chris Elfring, Board Director
Diane Gustafson, Administrative Coordinator
Amanda Staudt, Program Officer

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Abbreviations and Acronyms

ABL	atmospheric boundary layer
AGCM	Atmospheric General Circulation Model
A-L-O	atmosphere-land-ocean
AM2	Geophysical Fluid Dynamics Laboratory Atmospheric Model 2
AMS	American Meteorological Society
AO	Arctic Oscillation
ARM	Atmospheric Radiation Measurement
BASC	Board on Atmospheric Sciences and Climate
CAM	Community Atmosphere Model
CCM2	Community Climate Model-2
CCSM	Community Climate System Model
CLIVAR	Climate Variability and Predictability
CO_2	carbon dioxide
CPT	Climate Process and Modeling Team
CRM	Community Regional Model
CRYSTAL-FACE	Cirrus Regional Study of Tropical Anvils and Cirrus Layers-Florida Area Cirrus Experiment
DMS	dimethyl sulphide
DNS	direct numerical simulation
DYCOMS-II	Dynamics and Chemistry of Marine Stratocumulus Experiment

ECMWF	European Centre for Medium-Range Weather Forecasts
ENSO	El Niño-Southern Oscillation
GCM	General Circulation Model
GCSS	GEWEX Cloud Systems Study
GEWEX	Global Energy and Water Vapor Experiment
GFDL	Geophysical Fluid Dynamics Laboratory
GISS	NASA Goddard Institute for Space Studies
IR	infrared
ITCZ	intertropical convergence zone
LES	large-eddy simulation
NASA	National Aeronautics and Space Administration
NCAR	National Center for Atmospheric Research
NOAA	National Oceanic and Atmospheric Administration
NSF	National Science Foundation
NWP	numerical weather prediction
OBL	oceanic boundary layer
ONR	Office of Naval Research
PBL	planetary boundary layer
QBO	quasi-biennial oscillation
Re	Reynolds number
RICO	Rain in Cumulus over the Ocean Experiment
SOLAS	Surface Ocean Lower Atmosphere Study
TES	Tropospheric Emission Spectrometer
TKE	turbulent kinetic energy
TWP-ICE	ARM’s Tropical Warm Pool International Cloud Experiment

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Committee and Staff Biographies

KERRY A. EMANUEL (*Co-chair*) is a professor in the Program in Atmospheres, Oceans, and Climate with the Department of Earth, Atmospheric and Planetary Sciences at the Massachusetts Institute of Technology (MIT). Dr. Emanuel is interested in the dynamics and climate of the tropics, with a particular focus on moist convection and the various circulations that arise due to the interaction of the atmosphere and the ocean. His current research involves theoretical and modeling studies of air-sea interaction in tropical cyclones, coupled atmosphere-ocean models of hurricanes, the dynamics of cumulus convection and large-scale circulations, and the control of atmospheric water vapor by convection. Dr. Emanuel is a member of the National Research Council's (NRC) Board on Atmospheric Sciences and Climate; a former member of the NRC's Committee on Meteorological Analysis, Prediction, and Radar Research; and a fellow of the American Meteorological Society. He received his Ph.D. in meteorology from MIT.

JOHN C. WYNGAARD (*Co-chair*) is a professor of meteorology, mechanical engineering, and geoenvironmental engineering at Pennsylvania State University. Dr. Wyngaard studies turbulence in the atmosphere through direct observations and supercomputer simulation. He is interested in new observational approaches, including ground-based remote sensing, as well as measurements from towers and aircraft, and he studies the dynamic performance of turbulence sensors. Using the large-eddy simulation technique, he is developing new representations of turbulence effects in meteorological and oceanographic models of local to global scales. Dr. Wyngaard is a member of the National

Research Council's Board on Atmospheric Sciences and Climate and recently served on its Committee on the Atmospheric Dispersion of Hazardous Material Releases. He received his Ph.D. in mechanical engineering from Pennsylvania State University.

JAMES C. MCWILLIAMS is a professor of earth sciences at the University of California, Los Angeles, and a research scientist at the National Center for Atmospheric Research. As a geophysical fluid dynamicist, he explores how material advection in a fluid produces the peculiar combination of global order and local chaos, and vice versa, evident in oceanic currents, as well as analogous phenomena in atmospheric and astrophysical flows. Some of his particular interests are coherent vortices in turbulence, interactions among surface waves and nearby winds and currents, general circulation of the ocean, Earth's climate and its intrinsic variability, coastal physical-biogeochemical dynamics, and computational simulations of natural systems. Dr. McWilliams is a fellow of the American Geophysical Union and a member of the National Academy of Sciences and has served on several committees of the National Research Council, including the Committee on the Science of Climate Change. Dr. McWilliams received his Ph.D. in applied mathematics from Harvard University.

DAVID A. RANDALL has been a professor in the Department of Atmospheric Science at Colorado State University since 1988. Prior to that, he held positions at the Massachusetts Institute of Technology and the National Aeronautics and Space Administration. Most of his research centers around further development and applications of a global atmospheric model, which is coupled with a land-surface model and an ocean model. His current research focus is on modeling studies of clouds and their role in the global climate system. Ongoing projects include development of improved cloud parameterization methods and an investigation of the role of clouds in climate dynamics. Dr. Randall is chief editor of the *Journal of Climate*. He received his Ph.D. in atmospheric sciences from the University of California, Los Angeles.

YUK L. YUNG is a professor in the Division of Geological and Planetary Sciences at the California Institute of Technology. He is interested in the chemistry, radiation, and transport in the terrestrial and planetary atmospheres and the application of global datasets to study climate change. His current research includes planets, high-precision remote sensing from space, isotopic atmospheric chemistry, and what influences and is influenced by the ozone layer. Dr. Yung is a fellow of the American Geophysical Union and a member of Academia Sinica's Advisory Panel, the American Astronomical Society (Division of Planetary Science), and the National Research Council's Climate Research Committee. Dr. Yung has authored two books and more than 160 professional papers. He holds a Ph.D. in physics from Harvard University.

Staff

JULIE DEMUTH is a program officer for the Board on Atmospheric Sciences and Climate. She received her B.S. in meteorology from the University of Nebraska-Lincoln and her M.S. in atmospheric science from Colorado State University. Her master's research focused on developing techniques for objectively estimating the intensity and wind structure of tropical cyclones in the Atlantic and East Pacific basins using microwave sounding data. The intensity estimation algorithm is now being run operationally by the National Oceanic and Atmospheric Administration's National Hurricane Center during the tropical season. Since joining BASC in March 2003, Ms. Demuth has worked on studies involving atmospheric dispersion of hazardous materials, weather modification, and road weather research, and the use of NEXRADs sited in complex terrain for flash flood forecasting.

DIANE GUSTAFSON is an Administrative Coordinator in the Board on Atmospheric Sciences and Climate. Since joining BASC in 1998, she has worked on studies involving climate change, the transition from research to operations, climate services, communicating uncertainties in weather and climate, and the atmospheric dispersion of hazardous material releases. Ms. Gustafson received her B.S. in biology from Marymount University.